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# Three Essays on Education, Wages, and the Labour Market in Mexico

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Thesis submitted for the degree of Doctor of Philosophy Department of Economics University of Sussex 3<sup>rd</sup> August 2021 Declaration

I hereby declare that this thesis has not been and will not be, submitted in whole or in part to another University for the award of any other degree.

Signature

Eréndira León Bravo

# **UNIVERSITY OF SUSSEX**

# ERÉNDIRA LEÓN BRAVO

# DOCTOR OF PHILOSOPHY IN ECONOMICS

# THREE ESSAYS ON EDUCATION, WAGES AND THE LABOUR MARKET IN MEXICO

# Abstract

This thesis comprises three empirical chapters on the labour market outcomes of education, minimum wages, and public sector wage policies in Mexico.

The first chapter estimates the impact of the 1993 educational reform in Mexico, which raised compulsory schooling to the age of 15, on labour market outcomes, particularly earnings. The reform raised average years of schooling by between 0.26 and 0.32 years. However, the evidence suggests that this reform did not have an effect on earnings or other labour market outcomes.

The second chapter analyses the role of the 2012 Minimum Wage (MW) intervention in Mexico on the wage distribution. The results provide evidence that exposure to an up-rating in the MW increased wages from the 10<sup>th</sup> to the 90<sup>th</sup> percentiles by between 3.3% to 5.5%, respectively. Overall, the MW policy is not associated with reducing inequality. The MW up-rating appears to be a mechanism that plays a crucial role in wage inequality for public sector workers. This may be due to the use of the MW as a numeraire for increments in salary payments across the public sector pay scales. However, the influence of other confounders in the labour market may likely have coincided with the introduction of this MW policy, thus, challenging its interpretation as a causal effect in this case.

The third chapter exploits the implementation of the 2018 Federal Law on the remuneration of public servants in Mexico to examine the impact of a government wage policy on the public–private sector wage gap. The results report that the wage gap was reduced, both at the mean and across the wage distribution. The policy is found to reduce the wage gap between these two sectors. In particular, the findings suggest that at the median the policy is responsible for reducing the public sector pay premium by about 11%.

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# Dedication: To Jalil, the joy of my life

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## **CHAPTER ONE: Introduction**

The Mexican government in recent decades has introduced a number of separate policy reforms designed to encourage the accumulation of human capital, increase the earnings of low-paid workers, and ensure more sustainable public sector pay from the perspective of the government's fiscal position. The three governmental interventions analysed in this thesis explore the above themes focusing on: (i) a compulsory educational reform introduced in 1993, (ii) a minimum wage (MW) up-rating intervention implemented in 2012, (iii) and a pay reform introduced for the public sector in 2018. The empirical analysis aims to investigate whether these reforms in Mexico accomplished their objectives and had meaningful implications for the labour market. The studies undertaken in this thesis fill a gap among the studies conducted to date not only for Mexico but also to broader policy debates in other developed and developing countries.

In analysing the impacts or implications of these types of governmental policies, an important characteristic is successfully identifying the target population or treatment group, which is a crucial feature in designing and assessing the impact of any policy. The first empirical chapter estimates the impact on schooling attainment and labour market earnings of the 1993 educational reform in Mexico, which subjected the school-going population to a rise in compulsory schooling age from 12 to 15. The motivation for the Mexican government's introduction of this reform was primarily related to the schooling benefits that this policy could potentially have for the population. Specifically, growth in enrolment rates for secondary schooling was anticipated to increase the accumulation of human capital, strengthen the acquisitions of qualifications among the population, and consequently, raise labour market earnings.

Despite the extensive literature on returns to education, the studies that focused on returns to changes in compulsory education to causally identify the effect of interest are more limited. The analysis in this thesis contributes to this scant literature with a focus on a developing country. The impact of this policy is studied using a fuzzy Regression Discontinuity Design (RDD) for the period 2009 to 2017. This methodology is implemented to address the conventional problem of endogeneity that exists between schooling and labour market

outcomes by exploiting the age cohort discontinuities created by the policy change. A key feature of the study is its use of months of birth as a measure of age, in contrast to most of the existing literature, which uses the year of birth.

Although the results suggest that the reform raised average schooling, and therefore accomplished one of the policy's key objectives, the reform did not induce the anticipated effects on labour market outcomes. Specifically, it did not impact earnings or sectoral employment choices. Therefore, the 1993 compulsory schooling law was insufficient as a policy in itself to impact labour market outcomes in Mexico during the period analysed.

The second study investigates the role of the Minimum Wage (MW) up-rating intervention introduced in 2012 in Mexico on the wage distribution and wage inequality. The analysis focuses on the formal sector as the MW legislation is binding in this segment of the labour market. The MW was seen by the Mexican government as a tool to increase the earnings of low-income workers, reduce inequality, and assist in poverty alleviation. This empirical approach explores an institutional change in the MW that exploits geographic zones, one of which was subject to the MW up-rating.

The research uses data from 2010 to 2015 to estimate both mean and unconditional quantile (UQ) regressions to evaluate this intervention on both the wage level and wage inequality. The empirical evidence in developed and developing countries regarding the MW impacts on wage inequality is limited. The existing research has generally focused on the direct effects on employment and the level of wages. This study contributes to the growing literature that evaluates the impacts of MW interventions on both levels and the distribution of earnings using UQ regressions and a Gini-based Re-centred Influence Function (RIF) approach.

The results provide evidence that although the MW up-rating increased wages in the treated geographical zone, it also raised wages across the unconditional hourly wage distribution for this zone. In particular, the effects of the MW policy appear to persist higher up the unconditional wage distribution than generally found in the literature. This effect in the public sector workers is conjectured that may be attributable to the role that the MW plays as a numeraire in the award of increments in the public sector payments. Finally, the MW was found to exert no impact on wage inequality in the private formal sector.

The final empirical chapter exploits the implementation of the 2018 Federal Pay Reform on the remuneration of public sector workers in Mexico. This institutional change is used to examine the impact of a governmental wage policy on the public sector pay premium across the unconditional wage distribution, and consequently, on the public–private sector wage differential. In Mexico, higher wages in the public sector have been of concern among policy-makers since at least 2009, and this has exerted pressure on the Federal government to impose limits and regulate them. The 2018 Mexican public sector policy reform used both payment cuts and wage freezes for public sector workers in an attempt to get the objective of achieving more sustainable pay awards in the public sector.

Using data from 2017 to 2019, mean and unconditional quantile (UQ) regression models are estimated within a difference-in-differences (D-i-D) framework. The empirical findings suggest that the policy was responsible for reducing the public sector pay premium and this led to a reduction in the public–private sector wage gap. In particular, the effects were strongly concentrated among the lower-paid workers below the median of the unconditional pay distribution. Overall, the analysis reported here contributes to the broader literature for both developed and developing countries in its attempt to provide a causal impact of a policy change on the public–private sectoral wage gap. However, there is some evidence that the introduction of the policy was anticipated by public sector employers, thus posing a challenge for the causal identification of the key effect.

A common theme that emerges from the analysis of the three policies explored in this thesis is that sometimes there are unintended implications. For instance, the first policy raises schooling but has no discernible effect in the labour market; the second policy appears to increase rather than reduce inequality for a subset of workers, and the third policy designed to restrict excessive public sector pay had stronger adverse effects on the lower than the higher paid public sector workers. In spite of this, the findings presented in this thesis contribute to an understanding and appreciation of the broader implications of government policies on the labour market in Mexico.

The structure of this thesis is as follows, it comprises three empirical analyses presented in chapters Two, Three and Four, and these correspond to each of the three separate policies evaluated. Chapter Five provides some concluding and summary remarks.

**CHAPTER TWO: Does Compulsory Schooling Impact Labour Market Outcomes?** Evidence from the 1993 Educational Reform in Mexico

# 2.1 Introduction

The accumulation of human capital through education is widely perceived to develop the skills of individuals, provide higher qualifications for the labour market, and consequently, increase their lifetime earnings (e.g., Card, 2001; Duflo, 2001; Oreopoulos, 2006b). Hence, compulsory education has been introduced around the world as a government policy to increase human capital accumulation in the long term. However, school attendance may not necessarily impact school attainment; combinations of internal factors such as family background and external factors on the supply side of the educational system could influence school achievements and, therefore, the future earnings of individuals.

Empirical evidence in high-income economies suggests that compulsory schooling laws increase schooling. However, the small size effect does not translate into a systematic improvement in the labour market prospects of the people exposed to these reforms, where most of the estimated returns to the additional schooling are reported to be low or even zero (Devereux and Hart, 2010; Grenet, 2013; Pischke and Von Wachter, 2008). Among the explanations for these low returns is that the laws do not require obtaining the certificates that demonstrate the qualifications achieved through schooling, which play a key role in labour market signalling (Grenet, 2013); or because students are much better prepared in basic academic skills even before compulsory schooling is established (Pischke and Von Wachter, 2008).

The empirical studies on compulsory schooling available for developing countries report higher returns to compulsory education than to developed economies. The estimates range from 6% in Taiwan to 20% in China (Fang et al., 2012; Spohr, 2003).<sup>1</sup> It could be because people at the bottom of the schooling distribution acquired higher education, increased their qualifications, improved their employment status, and received higher salaries (Fang et al., 2012). However, Aydemir and Kirdar (2017) found low returns for Turkey, similar to those

<sup>&</sup>lt;sup>1</sup> Taiwan was a developing country at the time of the introduction of compulsory education in 1968 and during most of the period analysed by Spohr (2003).

observed in developed countries. The authors argued that higher returns to education are linked to higher schooling levels, more than the compulsory schooling levels promoted by the Turkish reform.<sup>2</sup>

The analysis of the effects of compulsory schooling is potentially important for Mexico which introduced an Educational Reform in 1993 that raised compulsory schooling from 12 to 15 years. This reform was introduced to increase the accumulation of human capital, strengthen the skills and qualifications among the population, and raise earnings.

The information about school attendance gathered by different sources suggests the implementation of compulsory schooling in 1993 had a marginal effect in increasing degree attainment and that it was poorly enforced for the urban youth. On the one hand, the Mexican National Urban Employment Survey (ENEU -Spanish acronym) reported in 1992 and 1993 an average of 47% of urban adolescents, ages 12-15, enrolled at secondary education, from those aged 15 years (i.e., age at which the students should finish secondary schooling), 33% on average completed secondary schooling. From 1994 to 1998 the rates slightly change. On the other hand, in 1995, the World Bank (2016) reported 21% of Mexican adolescents dropped out of secondary schooling compared to 26% in 1990.

The reform compelled the population to remain at school regardless of individual-level decisions. Hence, the analysis of the impact of this compulsory schooling law requires examination not only to disentangle the education attendance effects but also to explore the impacts on labour market prospects. Therefore, this study aims to shed light on the long-run effects of compulsory education on male labour market earnings and their selection of employment into the informal, formal, and self-employment sectors in Mexico.<sup>3</sup>

The research uses various rounds (2009-2017) of the National Occupations and Employment Survey. Following Aydemir and Kirdar (2017) and Grenet (2013), a Regression Discontinuity Design (RDD) is used to interrogate the research questions. Specifically, a fuzzy RDD is applied to address endogeneity between schooling and the set of labour market outcomes. It exploits a measure of age in months of birth as an exogenous source for the increase in education. Then, the cohorts exposed to compulsory schooling are compared to

<sup>&</sup>lt;sup>2</sup> Table B2.1 in Appendix B summarizes some of the literature estimates.

<sup>&</sup>lt;sup>3</sup> Information regarding labour market is less available for females.

similar cohorts that had the opportunity to leave schooling before the age of 15. The fuzziness in the method accounts for the imperfect compliance with the policy.

The empirical evidence reveals that the 1993 school-leaving age reform in Mexico marginally increased the years of schooling. Still, the size effect was weak, and it was an insufficient policy by itself to impact hourly wages or sectoral employment choices such as self-employment, formal or informal employment. These results are quite similar to those found for high-income economies. It could be explained by a number of reasons such as the quality of education; the so-called degraded tertiary effect;<sup>4</sup> skills mismatch; the role of institutional wage setting; the large child labour force participation rates; the large fraction of informal sector workers; or the relevance of the skills acquired at school for the labour market.

The Mexican compulsory schooling law might effectively raise earnings if it induces a significant fraction of the population to complete higher levels of education and if the government implements other labour market and educational (e.g., training programmes, wage-setting reforms).

The contribution of this research to the literature is in several ways. First, it is among few studies for developing countries and the Latin American region that explore the returns to compulsory schooling. Second, it uses a rigorous RDD approach to address the endogeneity of schooling by exploiting a measure of age cohort discontinuities in months of birth, which arguably provides more accurate estimates than the year of birth measure used in most of the literature. Third, to the best of my knowledge, it is the first study for Mexico that uses the 1993 compulsory education reform to analyse the returns.

The present study is structured as follows. The next section presents a review of the related literature. A subsequent section provides the institutional context for the 1993 Compulsory Schooling Reform implemented in Mexico. It is followed by the data and the empirical

<sup>&</sup>lt;sup>4</sup> The degraded tertiary effect in education implies that the tertiary education institutions after the 1993 educational reform were not prepared for the spill-over effects that kept the students at higher levels of education rather than the one mandated by the reform. Thus, schooling became of lower quality (Campos-Vázquez, 2016).

strategy. The empirical results and robustness checks are then presented. Additional discussions on the estimates and the conclusions are contained in the final sections.

#### 2.2 Literature Review

The study of returns to education is one of the most important motivations for the economic analyses of education policies. There is a long debate on whether schooling is a mechanism to increase wages in adulthood and/or to have an impact on labour market prospects, such as the type of employment or the labour sector where individuals work. Generally, the literature is based on the empirical returns to schooling rather than schooling policies themselves and is mostly focused on developed countries (e.g., Cameron and Taber, 2004; Card, 2001), although, there are some interesting studies centred on developing economies (e.g., Duflo, 2001; Estrada and Gignoux, 2017; Patrinos and Psacharopoulos, 2004; Patrinos and Psacharopoulos, 2010).

Most studies use the traditional Mincerian earnings equation. Nevertheless, some use institutional features of the schooling system to tackle endogeneity issues and identify returns to schooling through instrumental variables such as a measure of age based on the quarter of birth (Acemoglu and Angrist, 2001); the distance to the nearest college (Card, 1995);<sup>5</sup> or the variation in schooling based on a school construction policy (Duflo, 2001). These instruments are used as they are independent of ability and generate an exogenous source of variation in education, which is important for capturing the causal effects on the returns.

Average returns to education are often higher in developing countries (low -or middleincome economies) than in industrialized countries (Card, 2001; Duflo, 2001). Duflo (2001) found economic returns to education in a range of 6.8%–10.6% for Indonesia. The returns per additional year of schooling are found to be, on average, higher in the Latin America and the Caribbean region (9.3%) and for the Sub-Saharan African region (12.5%) than for highincome economies (10%) (Montenegro and Patrinos, 2014).<sup>6</sup> Moreover, in the Latin

<sup>&</sup>lt;sup>5</sup> Cited in Oreopoulos, 2006b.

<sup>&</sup>lt;sup>6</sup> The returns refer to the private rate of returns, which take into account earnings after taxes, and because of the public subsidisation of education, the main part of the cost is foregone earnings while studying.

American region, the average returns of secondary schooling are around 7%, but significantly higher at 18% for tertiary level education. The returns are lower for elementary schooling.

In Mexico, the existing literature is focused on returns to education and its effects on inequality (López-Acevedo, 2001); returns to private education (Patrinos and Psacharopoulos, 2010); returns to misallocation of labour markets (Levy and López-Calva, 2016); or returns to the elite public schooling system (Estrada and Gignoux, 2017). For López-Acevedo (2001) inequality based on education accounted for the largest share of earnings inequality during the first part of the 1990s. Patrinos and Psacharopoulos (2010) reported returns to private education of 21.6% for primary schooling and 15% for secondary schooling.<sup>7</sup> Using metropolitan cities, Estrada and Gignoux (2017) found increased returns from elite public schools at the high-school level by 15 percentage points relative to other public high schools.<sup>8</sup>

Despite the extensive literature on returns to education, the studies focused on returns to education based on changes to compulsory schooling are limited. Most of them are centred on developed countries, where the results generally suggest that compulsory schooling laws do not systematically improve the earnings of those who are affected by the institutional changes since the returns of secondary education are low or even zero compared to the on-the-job training available to workers. This literature essentially analyses two types of institutional changes: the minimum school-leaving age or the mandatory years of schooling.

On the one hand, Pischke and Von Wachter (2008) found zero returns from raising the minimum school-leaving age in West Germany during the period from 1947 to 1969 by using a difference in differences (D-i-D) analysis. Kamhöfer and Schmitz (2016) re-analysed the Pischke and Von Wachter (2008) study for Germany using a Two-Stage Least Squares (2SLS) method with additional instruments for the supply of schools and confirmed their zero returns for higher track schools. These results support the hypothesis that wage-relevant skills are learned at an earlier stage, through on-the-job apprenticeship or training.

<sup>&</sup>lt;sup>7</sup> The social returns reported by the authors are 19% and 10% in primary and secondary education, respectively. The social rate of return is based on pre-tax earnings and considers the full cost of education.

<sup>&</sup>lt;sup>8</sup> The elite public schools that belong to the National Polytechnic Institute (Instituto Politécnico Nacional or IPN) are compared to other public high schools.

On the other hand, Oreopoulos (2006a) compared birth cohorts affected by changes in schooling reforms with birth cohorts not affected. The compulsory schooling laws studied are the 1947 and 1957 reforms for Britain and Northern Ireland, the 1915 and 1970 laws for the US, and the Canadian legislations in 1925 and 1970. The overall returns for males are, on average, 10% to 14%. Devereux and Hart (2010) analysed the 1947 British law and find more nuanced results that suggest zero returns for women and returns for men in the range of 4%–7%. Both analyses exploit a fuzzy Regression Discontinuity Design (RDD) to account for imperfect compliance with the policy, using a fourth-order polynomial of the year of birth to control for any underlying heterogeneity across the sample.

Dolton and Sandi (2017) re-assessed the robustness, the specification, and the estimation method of previous studies focused on the UK school-leaving age reforms. The authors used the month of birth in the RDD analysis as a more accurate instrumental variable for the variation in education, highlighting the sensitivity of the results to different specifications. They found robust returns of 6% for males, a significantly lower rate than Oreopoulos (2006b) findings. Similarly, Grenet (2013) found returns to education of 6% for the UK using the 1972 reform and close to zero returns for France with the 1967 reform. A fuzzy RDD with months and years of birth is employed for evaluating the reforms' impacts.

Thus, the results for the UK, among developed countries, suggests statistically significant returns to compulsory education above 4%, Oreopoulos (2006b) obtained the highest returns. The low or zero returns in high-income economies are, on the one hand, based on the fact that the reforms targeted students much better prepared in the basic academic skills, which matters in the labour market, by the time they reach compulsory schooling age (e.g., Pischke and Von Wachter, 2008 for Germany). On the other hand, the laws did not encourage the students to enrol in grades that lead to obtaining academic or vocational diplomas. It could certify the knowledge and skills achieved through studying, signalling in the labour market, rather than marginally increasing the years of education (e.g., Grenet, 2013 for France).

Among the studies for developing countries, Spohr (2003) explored the impact of the 1968 reform in Taiwan and found that earnings increased by 5.8% per additional year of schooling for males from 1979-1996. Fang et al. (2012) studied the 1986 compulsory education law in

China and reported overall returns of 20% for the period 1997-2006. Both analyses used Mincerian equations with instrumental variables in a 2SLS approach.

Higher returns are generally found for developing countries where educational attendance increases over time and translates into higher earnings. The returns in China at the bottom of the schooling distribution are argued because the reform compelled the individuals to acquire higher education, which increased their qualifications and allowed them to change their employment status from unskilled to semi-skilled workers, thus receiving higher salaries (Fang et al., 2012). In Taiwan, the individuals in the lower tail of the schooling distribution faced the worst occupational conditions but increased schooling, and consequently, their earnings due to the reform (Spohr, 2003).

Nevertheless, Aydemir and Kirdar (2017) found low returns to schooling for young males in Turkey within a range of 2%–2.5% for the period 2002-2013, although not statistically significant for most specifications. The authors used exogenous variation in schooling across birth cohorts induced by the 1997 compulsory schooling reform within a fuzzy RDD framework exploiting the year of birth as a measure of age. The findings in this study contrast with the higher returns found for the other developing countries. The authors suggest that higher returns are linked to higher schooling levels, more than the levels encouraged by the Turkish reform, which did not have spill-over effects above the schooling level promoted by the policy.

Aside from analysing the returns to compulsory education, some studies examined the impacts on other labour market outcomes. Spohr (2003) found increases in workforce participation of 1%, for the male sub-sample, of which schooling credentials (i.e., getting a diploma) were particularly important for gaining public sector employment. Aydemir and Kirdar (2017) found that the Turkish policy did not impact male employment status. Oreopoulos (2006b) identified significant gains of the reform on health, employment, and other socioeconomic outcomes, such as welfare on high-income economies, whereas, Pischke and Von Wachter (2008) and Grenet (2013) reported no effects on employment nor self-employed indicators.

Therefore, the empirical evidence is varied for both developed and developing countries. The heterogeneity may potentially be driven by the different contexts and identification strategies

used. Most studies use the RDD approach with higher-order polynomial (i.e., cubic or quartic order) of the year of birth to address the endogeneity issue. Few studies use months of birth with the fuzzy RDD method for more accurate and robust estimates of the returns to education, since months of birth allows for more variation in the birth cohorts' schooling within a year (Dolton and Sandi, 2017; Grenet, 2013).

### 2.3 The 1993 Educational Reform in Mexico

The literature stated above highlights that some countries experienced growth in their enrolment rates for secondary and post-secondary schooling, including Germany, the United Kingdom, China, Taiwan, and Turkey. These growths were driven by the perceptions of the economic benefits of schooling. Governments of developing countries share these perceptions. They directly invest in schooling or enact regulations that encourage individuals to invest in education. However, there is heterogeneity in the evidence of the effectiveness of these policies, more so at improving labour market outcomes.

The Mexican government introduced compulsory education for long-term school benefits. In the country, most children start school at the age of six. The Constitution specifies six years of elementary schooling followed by three years of secondary schooling, and three years of high-school education before entering the university level for (approximately) five years. The students receive their certificates at each academic level after completing the last year (or grade) at each level: sixth grade of elementary education, ninth grade of secondary schooling, and twelfth grade of high–school level. The bachelor's degree is received on completing between 17 to 18 years of education.<sup>9</sup>

Before 1993, the legislation had established a minimum school-leaving age at 12 years. Generally, at this age, the student attended all six years of elementary schooling. In July 1993, the government approved the Educational Reform that extended compulsory schooling until the age of 15, regardless of the level of education that children would complete up to 15 years. The reform primarily compelled the students to complete secondary schooling level.

<sup>&</sup>lt;sup>9</sup> Mexico employs a numerical system for grading each year of education. A mark of six is sufficient for passing the grade.

The reform was not implemented in conjunction with other laws that would have had a bearing on schooling outcomes (e.g., *Prospera* is a social programme introduced in 1997 that assists to provide children access to basic services such as food, health and school). The policy was established in a stable macroeconomic context that began in the early 1990s (i.e., prior to the adoption of trade liberalization policies in 1994) and its timing was not influenced by the December 1995 financial crisis.

The intervention was first implemented nationwide in September 1993, at the start of the academic year.<sup>10</sup> It established that all children under age 16 must be enrolled at their respective schooling levels. At the age of six, by the 1<sup>st</sup> of September, children must be registered into elementary schooling, and by the age of 12, most children should be enrolled at the secondary schooling level. The high school level generally begins at the age of 15. However, it may be the case that some students enrolled at an early age in the school system (e.g., at the age of four or five years).<sup>11</sup>

The main goal of the government with the introduction of this reform was to increase the accumulation of human capital in the long term, and consequently, to raise educational qualifications. More educated people would obtain better jobs and enjoy economic opportunities later in life, such as a positive impact on life-cycle earnings. In addition, social and cultural returns were also anticipated. Therefore, education itself was viewed as a powerful tool to lift children and adults out of poverty and, to achieve lasting economic development for the country.

Aside from extending compulsory schooling, the 1993 reform changed the curricula and updated the study programmes and textbooks for each grade of elementary and secondary schooling levels to provide students with the skills and qualifications required by a changing labour market context.

The statistics of school attendance around the time that the compulsory law was implemented were not promising. The law appears to be not well enforced. The Mexican National Urban Employment Survey (ENEU -Spanish acronym) reported by the second quarter of 1992 and

<sup>&</sup>lt;sup>10</sup> Nowadays the academic year starts in the middle of August.

<sup>&</sup>lt;sup>11</sup> People above 15 years of age that are enrolled at elementary or secondary schooling levels attend adult literacy schools, in which the classes are provided at evenings or on weekends.

1993, before the introduction of the reform, 47% of urban adolescents, ages 12-15, enrolled in secondary education each year. From the adolescents aged 15 years (i.e., age at which the students should finish secondary schooling), 32% and 33.7% completed secondary schooling, respectively for 1992 and 1993.

After the introduction of the reform, the enrolment rate of urban adolescents slightly decreased to 46.5% by 1994, whereas in 1995 it was the same as in previous years with 47%. Among those students aged 15 years, 32.6% and 32.4% completed secondary schooling in 1994 and 1995, respectively. However, in 1997 and 1998, the percentage of the students enrolled at secondary education reached respectively 49.4% and 48.8%. The proportion of adolescents with completed secondary schooling level at the age of 15 slightly increased relative to previous years to 35.6% and 34.8%, respectively for 1997 and 1998. On the other hand, the World Bank (2016) reported in 1995 a reduction of five percentage points in the rate of Mexican adolescents that dropped out of secondary schooling compared to the percentage in 1990 (i.e., from 26% to 21%).

Thus, according to the information gathered by different organisations, there appears to have been a marginal effect on increasing degree attendance after the implementation of compulsory schooling in 1993, and it was poorly enforced for the urban youth. Hence, the analysis of the impacts of this universal compulsory schooling law is of paramount importance in Mexico, not only regarding school attendance but also for exploring the impacts and consequences on labour market outcomes. The next section describes the data used in the analysis.

# 2.4 Data

The empirical work uses the Mexican National Occupations and Employment Survey (ENOE -Spanish acronym) available from the Statistics, Geography, and Informatics Institute (INEGI -Spanish acronym). The survey encompasses information for gainfully occupied and unoccupied individuals aged 12 years and over. The ENOE is a nationally representative survey. It reports, *inter alia*, the age in months, birth states, the years of schooling, weekly hours worked, monthly earnings, formal and informal job activities derived from the main and secondary occupations, and residential areas.

The second quarter interview period of the ENOE is used primarily to avoid any seasonality in earnings since higher expenses are reported during the first and fourth quarters of the year because extra bonuses are received by most people in December and January (mainly for wage premia, dividends and shares of the company, etc.). The sample is restricted to male observations because the information reported of earnings and other labour market variables are complete and comprehensive than for the female sub-sample. Accurate and comprehensive information for female participants in the labour market is not available.<sup>12</sup>

The sample consists of a pooled cross-sectional dataset of 148,964 observations from the period 2009 to 2017. Since this survey is a quarterly rotating panel dataset that encompasses five continuous quarters, the fifth interview period of the survey is excluded from those individuals whose first interview was in the second quarter period of the ENOE one year prior. This is to avoid repetition in the same observations throughout the survey periods.

The sample is limited to full-time workers, which are the observations that reported between 30 and 90 weekly hours worked during the reference week.<sup>13</sup> Full-time workers are included to avoid people studying and working while being surveyed since the research focuses on the effects of completed education on labour market outcomes.

# 2.4.1 Outcome variables

The main outcome variable used in this research is the natural logarithm of hourly earnings. The definition of earnings in the survey refers to a monthly payment received from the main job net of all labour taxes and social security contributions.<sup>14</sup> The hours worked are reported weekly in the survey and are multiplied by 4.3 to obtain the monthly hours worked. Hence,

<sup>&</sup>lt;sup>12</sup> The inherent difficulty of including women in the analysis without modelling selection into the labour market and labour supply dynamics would complicate the analysis for a pooled sample (Dolton and Sandi, 2017). The point of concern would be how much of the estimated effect from the pooled sample is due to education *per se* rather than job selection.

<sup>&</sup>lt;sup>13</sup> This study follows Campos-Vázquez (2016) for the full-time workers definition used for Mexico and Aydemir and Kirdar (2017) for Turkey.

<sup>&</sup>lt;sup>14</sup> If the interviewees report weekly payments, the Mexican Statistics, Geography, and Informatics Institute transforms this into monthly earnings by multiplying the former times 4.3. The information of earnings is reported for all the employment sectors.

the hourly wage variable is computed by dividing monthly earnings from the main occupation by monthly hours worked.<sup>15</sup>

This hourly earnings measure is important to compare earnings across individuals. For example, people working a greater number of hours with lower wages can be compared to people working a smaller number of hours but receiving higher wages. The hourly earnings measure is in real Mexican pesos as of December 2010. Then, logarithms are applied to hourly earnings for getting the key-dependent variable conventionally used in Mincerian-type equations.

This research uses the definitions established by INEGI and identified in the survey (ENOE) for formality, informality and self-employment through the following information: the economic sector of work (e.g., services, agriculture, manufacturing); the type of occupations and activities undertaken (e.g., masonry, professional activities); the number of employees in the firm; if people own their businesses (or are farmers); and the type of rights the individuals are entitled to through their work contract.

Formal employment is mainly related to employment activities that provide access to social security or medical health services, which coincide with work under a contract that is subject to employment rights. On the other hand, informal employment is associated with labour activities that do not provide access to social security, health services, or a contract that includes employment rights.<sup>16</sup> Self-employment is related to job activities performed by individuals that work alone or are associated with others and may have unpaid workers in their business.

These employment sectors are used to construct the dependent variables for the analysis. Summary statistics of the wage variable for the pooled sample, the self-employed observations, and the formal and informal sectors are reported in Table 2.1. Informal sector

<sup>&</sup>lt;sup>15</sup> The main occupation is identified entirely by the interviewees as the one in which the respondent spends most of their time during the day and which provides the highest remuneration. In the sample, the individuals with a secondary occupation are slightly above 6%. Earnings and wages definitions are used indistinctively in this study.

<sup>&</sup>lt;sup>16</sup> Working in the informal sector and having informal employment are treated as the same in this analysis.

workers represent 44% of the sample, and their hourly wages are the lowest among all observations. Formal sector employees are 40% of the sample and report the highest earnings.

| Log of hourly wages | Obs.    | Mean  | Std. Dev. | Min   | Max   |
|---------------------|---------|-------|-----------|-------|-------|
| Pooled sample       | 148,964 | 3.105 | 0.617     | 0.003 | 7.640 |
| Formal sector       | 82,794  | 3.278 | 0.584     | 0.007 | 7.640 |
| Informal sector     | 66,170  | 2.889 | 0.588     | 0.003 | 6.611 |
| Self-employed       | 23,711  | 3.048 | 0.835     | 0.003 | 7.017 |

Table 2. 1: Summary statistics of hourly wages

Source: Mexican National Occupations and Employment Survey (2009-2017).

Figure 2.1 contains the trends for the average log of hourly wages by formal, informal, employed and self-employed workers. These graphs reveal that the average wages decreased from 2009 to 2014 and increased thereafter. The largest decline in average earnings for the pooled sample was in 2011 (3.08 log of hourly wages) and the peak in 2016 (3.13 log of hourly wages). The formal sector workers reported earnings above average, whereas workers in the informal sector reported the lowest. The earnings of the self-employed, although below average, grew rapidly after 2014 compared to the rest of the employment sectors.



Figure 2. 1: Trends of hourly wages by employment sector

#### 2.4.2 Other variables in the analysis

Among the explanatory variables included are years of schooling, areas of residence, and the type of economic sector. Table 2.2 displays the summary statistics for these covariates. The variable years of schooling is related to the highest academic level accomplished. Most of

Source: Mexican National Occupations and Employment Survey (2009-2017).

the observations in the sample reported completed secondary schooling, the average is 10.4 years which is equivalent to the attendance of at least 1.4 years of high school level. The majority of the observations work in the services (39%) and manufacturing (19%) sectors. The individuals residing in urban settlement areas are 60% of the sample.

Regarding the age of the observations, the sample includes males aged between 24 to 40 years when surveyed, the average age is 31 years. The individuals were born between 1975 and 1987 and were aged in a range of 6–18 years at the time of the 1993 educational reform, which is around the mandatory age of the intervention. The measure of age in months of birth is used in the analysis for the 'running variable', which determines the treatment of the policy according to the specific threshold.<sup>17</sup>

| Variable           | Mean  | Std. Dev. | Min | Max |
|--------------------|-------|-----------|-----|-----|
| Age                | 31.30 | 4.10      | 24  | 40  |
| Urban status       | 0.60  | 0.49      | 0   | 1   |
| Years of schooling | 10.36 | 4.01      | 0   | 24  |
| Economic sector:   |       |           |     |     |
| Agriculture        | 0.09  | 0.28      | 0   | 1   |
| Commerce           | 0.16  | 0.37      | 0   | 1   |
| Construction       | 0.15  | 0.36      | 0   | 1   |
| Manufacturing      | 0.19  | 0.40      | 0   | 1   |
| Services           | 0.39  | 0.49      | 0   | 1   |

Table 2. 2: Summary statistics of explanatory variables

*Source*: Mexican National Occupations and Employment Survey (2009-2017). Total observations 148,964.

Figure 2.2 illustrates the average log of hourly wages by the highest academic level reported over the period analysed.<sup>18</sup> The highest wages, above the average, reported high-school, college (undergraduate) and postgraduate schooling levels. Conversely, the lowest earnings, below the average, are exhibited by individuals with lower academic levels.<sup>19</sup>

<sup>&</sup>lt;sup>17</sup> All the observations with missing values for the month of birth are excluded.

<sup>&</sup>lt;sup>18</sup> Figure C2.1 in Appendix C illustrates the average log of hourly wages by economic sector.

<sup>&</sup>lt;sup>19</sup> People who had an elementary schooling degree or less reported a 0.3% increase in their wages, while people who had a secondary schooling certificate reported a 0.7% decrease in their earnings.



Figure 2. 2: Average hourly wages by academic levels

Source: Mexican National Occupations and Employment Survey (2009-2017).

#### 2.4.3 Treatment and Control Groups

The 1993 reform generated two groups of people according to their birth cohorts, the treatment and control groups. The treatment group comprises people aged 12 years at the time of the reform that were born on and after September 1981 and therefore exposed to compulsory schooling until the age of 15. The control group includes those born before September 1981 that could leave school before the age of 15. Table 2.3 reports the selected birth cohorts, 75 months before and after September 1981.<sup>20</sup>

The distributions of the log hourly wages and years of schooling per treatment and control groups are illustrated in Figures 2.3 and 2.4, respectively. For the younger cohorts, born on and after September 1981 and exposed to the reform, both distributions shift to the right suggesting higher earnings and years of education compared to the control group. Although the average earnings for both groups are quite similar in the period analysed, the average years of schooling are greater for the treatment group, indicating the policy might have increased schooling.

The peaks in Figure 2.4 report the highest academic level achieved by the sample individuals, which coincides with the certificates obtained around the last year of schooling in each

<sup>&</sup>lt;sup>20</sup> Tables B2.2 and B2.3 in Appendix B present statistics for the cohorts born before and after September 1981.

academic level: 6<sup>th</sup> year in elementary schooling, 9<sup>th</sup> year in secondary schooling, 12<sup>th</sup> year for high-school level and undergraduate between the year 17<sup>th</sup> and 18<sup>th</sup>.

| Group      | Age when<br>Survey year |          | Age in            | Year of hirth | Obs         | Total Obs. |
|------------|-------------------------|----------|-------------------|---------------|-------------|------------|
|            | Survey year             | surveyed | September 1993    |               | 0.053.      |            |
|            | 2009                    | 27-34    |                   | 1975-1981     | 8,563       | 74,618     |
|            | 2010                    | 28-35    |                   |               | 8,946       |            |
|            | 2011                    | 29-36    | 12 10             |               | 8,549       |            |
|            | 2012                    | 30-37    |                   |               | 8,219       |            |
| Control    | 2013                    | 31-38    | 12 - 18           |               | 8,103       |            |
|            | 2014                    | 32-39    |                   |               | 8,554       |            |
|            | 2015                    | 33-40    |                   |               | 8,495       |            |
|            | 2016                    | 34-40    | 12 - 17 1976-1981 |               | 8,421       |            |
|            | 2017                    | 35-40    |                   | 6,768         |             |            |
|            | 2009                    | 24-27    | 8 - 12            | 1981-1985     | 5,450       |            |
|            | 2010                    | 24-28    | 7 - 12            | 1981-1986     | 7,298       |            |
|            | 2011                    | 24-29    |                   | 1981-1987     | 8,589       | 74,356     |
|            | 2012                    | 24-30    |                   |               | 9,297       |            |
| Treatment  | 2013                    | 25-31    |                   |               | 8,877       |            |
|            | 2014                    | 26-32    | 6 - 12            |               | 8,904       |            |
|            | 2015                    | 27-33    | 0 1               |               | 9,025       |            |
|            | 2016                    | 28-34    |                   |               | 8,744       |            |
|            | 2017                    | 29-35    |                   |               | 8,162       |            |
| Total Obs. |                         |          |                   |               |             | 148,964    |
|            |                         | 1.0      |                   |               | (0000 0015) |            |

Table 2. 3: Birth cohorts by control and treatment groups

Source: Mexican National Occupations and Employment Survey (2009-2017).



Figure 2. 3: Distribution of hourly wages by birth cohorts

*Source*: Mexican National Occupations and Employment Survey (2009-2017). The vertical lines represent the average hourly wages.



Figure 2. 4: Distribution of years of schooling by birth cohorts



Table 2.4 reports the summary statistics of the sample by academic attainments. The people who hold a secondary schooling certificate as the highest educational attainment level are the majority with approximately 37%. The individuals holding at least a secondary schooling diploma are 80% in the treatment group and 74% in the control group. The 'never-takers', who are the people bound by the policy but illiterate, represent 1.2% of the sample, while 3.6% of the observations reported incomplete secondary schooling.<sup>21</sup>

| Academic attainment          | Treatment group<br>percentage | Control group<br>percentage |  |
|------------------------------|-------------------------------|-----------------------------|--|
| Elementary schooling or less | 20.3                          | 26.1                        |  |
| Secondary schooling          | 36.8                          | 36.7                        |  |
| High-school                  | 24.6                          | 20.4                        |  |
| Undergraduate level          | 17.3                          | 15.2                        |  |
| Graduate level               | 1.0                           | 1.7                         |  |
| Obs.                         | 74,356                        | 74,618                      |  |

Table 2. 4: Summary statistics by academic attainment

Source: Mexican National Occupations and Employment Survey (2009-2017).

<sup>&</sup>lt;sup>21</sup> The the Mexican National Urban Employment Survey (ENEU) survey reported an average of 1% of illiterate children aged between 12 and 15 years in the second quarters of the survey from 1990 to 1999.

Figure 2.5 contains the trends of holding a specific academic certificate. The graph on the right-hand side indicates the trends by year of birth, whereas the one on the left-hand side uses the months of birth. In general, an upward trend is shown for the cohorts holding high-school and college academic certificates. The birth cohorts with secondary schooling degrees are relatively constant over the period analysed, while those with elementary schooling diplomas or less are decreasing along with the birth cohorts.

These tendencies confirm Figure 2.4, younger cohorts reported more years of schooling compared to elder cohorts. They also emphasize the information reported in Table 2.4, the proportion of people who only had a secondary certificate did not change drastically among the treatment and control groups.



Figure 2. 5: Trends of academic certificates by month and year of birth

Source: Mexican National Occupations and Employment Survey (2009-2017).

#### **2.5 Empirical Strategy**

A new thrust of research has emerged, following the series of policies that encourage investment in education, where the institutional features of the education system can be used to establish credible instrumental variables for schooling outcomes that can cut through the gordian knot of endogenous schooling and unobserved ability. The use of supply-side variables to help solve the identification on the demand-side of the education market is a natural feature of standard econometric practice given the well-known 'paradox of identification'. There are also, theoretical underpinnings and econometric tools that explicitly recognize the possibility that returns to education may vary across the population, depending on such characteristics as family background and abilities (Card, 2001).

The 1993 educational reform in Mexico is a natural experiment, which can be used to estimate the long-run effects of this institutional change on labour market prospects. The main purpose of this study is to shed light on the impacts of compulsory schooling on earnings. In addition, the analysis studies to what extent the intervention influences employment sectoral choices, such as working in the formal sector.

The well-known problem in establishing this causal link is the endogeneity of schooling because there are unobserved omitted variables that could affect earnings as well as educational attainment or employment choices.<sup>22</sup> Earnings and schooling are potentially jointly determined by these unobserved factors, where also there is a concern about reverse causality.

The introduction of compulsory schooling laws creates an exogenous source of variation in educational attendance where the individuals are compelled to remain at school regardless of individual-level decisions. This exogenous source, which increases education, addresses the endogeneity of schooling by exploiting the age cohort discontinuities measured by the month of birth. Incorporating the within-year variation in month of birth would more accurately reflect the amount of extra-compulsory schooling faced by different cohorts in the sample (Clark and Royer, 2013).<sup>23</sup> The returns to education are then estimated with a Regression Discontinuity Design (RDD) approach.

The 1993 reform was mandatorily applied to compel a minimum school-leaving age of 15 years regardless of the educational attainment the children could accomplish. It was expected that the policy would extend schooling, even though the students could repeat some grades of elementary or secondary schooling, which naturally increases the years of schooling but not necessarily the educational attainment.

Admittedly, imperfect compliance is an issue with this reform because some factors could affect the exposure to the policy leading to imperfect enforcement (i.e., children not treated

<sup>&</sup>lt;sup>22</sup> The potential endogeneity problem has been stated in the literature by many authors (e.g., Acemoglu and Angrist, 2001; Duflo, 2001; Pischke and Von Wachter, 2008). Educational attendance and attainment depend on people's own decisions, abilities, motivations, learning dispositions, and parental characteristics, among other factors.

<sup>&</sup>lt;sup>23</sup> This is among the first studies using months of birth as a measure of age for the UK, although it focuses on health outcomes.

but that should be treated, in contrast to children that should not be treated but are actually treated). For example, geographical characteristics in Mexico complicate the desire and opportunity to go to school (e.g., people living in rural areas face different obstacles to commute to schools, such as walking long distances, even across rivers or mountains). For some households, children are an important source of work and therefore do not attend school regularly (e.g., those children working in domestic activities or agriculture). These should be children treated that possibly are not treated. <sup>24</sup>

Furthermore, the reform could induce children to continue with their education and obtain higher academic certificates, even for students over the age of 15 (i.e., spill-over effects on the students not targeted by the reform).<sup>25</sup> For example, on the one hand, children that started school earlier than the normal age of six, by the time of the policy implementation would have already completed secondary schooling and are bound by the reform until the age of 15. They could continue in higher education. On the other hand, children that started school later than the normal age of six are bounded by the policy until the age of 15, after this age they could either drop out of school without a secondary schooling certificate or stay at school.

Unfortunately, the survey does not provide information on when the people started school, completed an academic level or dropped out of school. These factors affect the exposure to the policy, the possibility of increasing education, and the unobserved characteristics among the individuals exposed and not exposed to the reform because of the imperfect enforcement of the legislation (Aydemir and Kirdar, 2017).

Similar to other countries, Mexican compulsory education affected a large fraction of the population. Possibly, as Aydemir and Kirdar (2017) argued, this reform made the local average treatment effect (LATE) likely to be closer to an average treatment effect (ATE) of the returns to compulsory education since the marginal individual affected by this policy becomes progressively likely to the average individual in the population.<sup>26</sup>

<sup>&</sup>lt;sup>24</sup> Urban settlement status and birth states are used in the analysis to control for these types of socio-demographic and economic conditions.

<sup>&</sup>lt;sup>25</sup> Children aged 6-12 years are mainly enrolled at elementary schooling, and aged between 12-15 are studying secondary schooling. Students over the age of 15 attend adult literacy schools if they are enrolled at elementary or secondary schooling levels.

<sup>&</sup>lt;sup>26</sup> This analysis focuses on a local average treatment effect (LATE), which is relevant to the subset that has been subject to the policy reform.

Therefore, whether compulsory school attendance raised the educational attainment and consequently had a positive economic impact in adulthood needs to be tested empirically. Although educational attainment depends on people's own decisions, abilities, motivations, learning dispositions, and parental characteristics, among other factors, increasing compulsory schooling until the age of 15 might increase attainment and therefore earnings. It might also impact employment choices, such as working in the formal sector.

Then, using the fuzzy RDD approach, the years of schooling and labour market outcomes of birth cohorts exposed to the reform are compared to those not exposed. This approach is similar in spirit to the Grenet (2013) and Aydemir and Kirdar (2017) studies. It employs a running variable, which is the age measured in months of birth from the cohort born in September 1981. This variable determines the treatment according to the specific threshold set. The treatment assignment is defined with a policy dummy variable (age in months of birth), which is equal to one for the birth cohorts born on and after September 1981, and zero for earlier birth cohorts.

The treatment variable is specified as follows:

$$Treatment_i \begin{cases} 1, & \text{if cohort born} \ge \text{September 1981} \\ 0, & \text{if cohort born} < \text{September 1981} \end{cases}$$

This methodology addresses imperfect compliance by using the random assignment of the instrumental variable, which is the policy dummy, as the determinant of the exposure to the 1993 educational reform. To estimate (a) the effects of the exposure to compulsory schooling until the age of 15 on years of schooling; (b) the effects of exposure to the compulsory schooling on labour market outcomes (earnings and employment sectoral choices); and (c) the effects of years of schooling (instrumented by the policy dummy) on labour market outcomes, this study uses the fuzzy RDD approach with the following Two-Stage Least Squares estimation (2SLS).<sup>27</sup>

The First Stage that estimates the causal link between schooling and the policy is defined in equation (1):

<sup>&</sup>lt;sup>27</sup> Card (2001) suggests that the use of alternative methods to Ordinary Least Squares (OLS), such as instrumental-variables, for estimating returns to compulsory education may reveal higher marginal returns to schooling since they provide a better approximation of the exposure to these mandatory laws.
Years of Schooling<sub>i</sub> = 
$$\alpha_0 + \alpha_1(Treatment_i) + \alpha_2 F(Age in months_i) + \alpha_3 X_i + \varepsilon_i$$
 (1)

The specification regresses the years of schooling on the treatment variable, a functional form of the running variable (*Age in months<sub>i</sub>*), a set of covariates ( $X_i$ ) and a random error term  $\varepsilon_i$ . The functional form is re-centred with the running variable at the cut-off point. Thus, the estimated effect represents the effect of the treatment precisely at the threshold. The covariates included are the urban settlement status, the economic sector, the survey year dummies, and birth states dummies clustered by regions.<sup>28</sup>

The estimated coefficient,  $\widehat{\alpha_1}$ , of the treatment variable captures the reform's average causal effect on years of schooling at the treatment assignment threshold. It is expected to be a positive effect, which indicates that the males exposed to the policy increased, on average, their years of schooling compared to the ones not exposed.

The reduced-form is implemented through equation (2):

$$LMkt outcomes_i = \beta_0 + \beta_1(Treatment_i) + \beta_2 F(Age in months_i) + \beta_3 X_i + \omega_i$$
(2)

where LMkt outcomes<sub>i</sub> represents the following labour market outcomes: 1) Log of hourly wages<sub>i</sub>; 2) Formal employment<sub>i</sub>; 3) Informal employment<sub>i</sub>; and 4) Self-Employment<sub>i</sub>. The last three outcomes are implemented as dummy variables. The random error term is  $\omega_i$ . The estimated coefficient  $\widehat{\beta_1}$  of the treatment variable measures the reform's average causal effect on each labour market prospect at the treatment assignment threshold.

The second stage equation, equivalent to a 2SLS is expressed as follows:

$$LMkt outcomes_{i} = \delta_{0} + \delta_{1} \left( Years \ of \ Schooling_{i} \right) + \delta_{2} F(Age \ in \ months_{i}) + \delta_{3}X_{i} + \mu_{i}$$

$$(3)$$

The specification regresses the labour market outcomes  $(LMkt outcomes_i)$  on years of schooling predicted from the first stage (Years of Schooling<sub>i</sub>) and instrumented by the

<sup>&</sup>lt;sup>28</sup> The INEGI clusters the 32 Mexican States by seven socioeconomic regions that reflect population's levels of welfare, including aspects such as education, employment, housing, and health. Therefore, this analysis uses seven birth region dummy variables and nine survey year dummies. Table B2.4 in Appendix B lists the 32 Mexican States by each region.

treatment assignment (*Treatment<sub>i</sub>*), the functional form of the running variable (*Age in months<sub>i</sub>*), the covariate vector  $X_i$ , and the random error term  $\mu_i$ .

The estimated coefficient  $\hat{\delta}_1$  is interpreted as a measure of the labour market outcomes specified in equation (2) for the treatment group who were compelled to increase their years of schooling as a result of the amendment in the Mexican legislation. For the four outcomes, the coefficients are interpreted as follows: 1) the returns to compulsory schooling; 2) the probability of working in the formal sector; 3) the probability of working in the informal sector, and 4) the probability of working as self-employed.

A positive coefficient is expected for  $\hat{\delta}_1$  indicating that improvement in school attendance would translate into causal effects on hourly earnings. For the other labour market outcomes, the anticipated sign is positive for the formal employment sector because as the intervention increases schooling, this would increase the probability of working in this specific sector. Whereas the signs would be negative for the informal employment sector and working as self-employed because the reform would increase years of education, and this would decrease the probability of working in these types of employment. More educated people would have the opportunity to improve their welfare by preferring to work in the formal sector where they can be provided with additional benefits such as social security and health services. The other sectors would rarely provide the workers with such benefits.

The fixed effects of birth states clustered by region are included, as baseline covariates, to control for socio-demographic and economic conditions in which the individuals were born and which may have different impacts on labour market outcomes. This is particularly important for the quality of education and school sizes as they could be different depending on the degree of development of each Mexican state and region. Survey year dummies are included to control for macroeconomic shocks, such as the effects that the 2008 financial crisis could have had in the period analysed. The urban settlement status variable is used to control for the current employment and settlement conditions.

Time trends in the labour market outcome variables are considered through the polynomial terms of the running variable (*Age in months*<sub>*i*</sub>). However, following Gelman and Imbens (2017), the analysis avoids higher-order polynomials in the running variable to circumvent

assigning higher weights to observations far from the threshold. Therefore, all the specifications use polynomial order two.

In addition, to conduct the estimation of equation (3), this study uses non-parametric and parametric methods. This is mainly for comparison purposes. In the non-parametric analysis, there is no need to make any assumptions regarding the population and the results cannot be seriously affected by outliers. The local polynomial method follows Calonico et al. (2018) and Calonico et al. (2014) for the selection of the optimal bandwidth, based on a local linear regression, and for the calculation of robust bias-corrected method, which provides the bias-corrected regression discontinuity (RD) estimates with robust variance estimators. The estimation also uses the triangular kernel to weight the observations.

The parametric analysis delineates a spline interacted quadratic specification for the running variable. To avoid constraining the effects to be identical on both sides of the threshold,<sup>29</sup> interacted variables of the treatment dummy variable with the running variable are included, and polynomial functions are allowed on either side of the cut-off as suggested by Gelman and Imbens (2017). This parametrisation represents the difference in the effects between the treated and non-treated observations.

In both analyses, the polynomial order two is used. The Eicker-Huber-White (EHW) robust standard error correction is also used in the spirit of Kolesár and Rothe (2018). The argument is that clustering the standard errors at the running variable level does not prevent model misspecification and yields confidence intervals that exhibit poor empirical coverage properties well below their nominal target values; whereas EHW correction generates wider confidence intervals and it is more conventional for inferential purposes. Clustering at the running variable understates the statistical uncertainty associated with the treatment estimates to a greater degree than when using the EHW correction. This could lead to an over-rejection of the null hypothesis of no treatment effects.<sup>30</sup>

<sup>&</sup>lt;sup>29</sup> Regarding the relationship of the labour market outcomes with the running variable.

 $<sup>^{30}</sup>$  Alternatively, the estimations for the equations (1), (2) and (3) using the standard errors clustered at the running variable level as recommended by Lee and Lemieux (2010) are conducted, however, the estimates are erratic compared to the results from the specification using EHW correction.

#### 2.5.1 RDD Internal validity

The 1993 educational reform creates a discontinuity between birth cohorts by compelling the treatment group to raise years of schooling compared to the control group. Figures 2.6 and 2.7 provide evidence of the discontinuity around the cut-off point in years of schooling, hourly earnings, formal and informal employment dummy variables. The plot of the self-employment dummy variable remains with a constant downward curve, this means that there is no visible discontinuity around the threshold (see Figure 2.8). <sup>31</sup>



Figure 2. 6: Discontinuity plots of years of schooling and log of hourly wages

Figure 2. 7: Discontinuity plots of formal and informal employment sectors



<sup>&</sup>lt;sup>31</sup> The plots use a polynomial order 2 for illustrating the discontinuity around the threshold. Figures C2.2, C2.3 and C2.4 in Appendix C show the graphs for the polynomial order 1.



Figure 2. 8: Discontinuity plots of the self-employment sector

The younger cohorts are located on the left-hand side of each of the plots presented above. Although the downward slope for the treatment group suggests a decrease in earnings in Figure 2.6, it reflects the earnings of the younger cohorts in the sample, thus, the farther to the left from the discontinuity, the younger the birth cohorts (i.e., the youngest cohort in the treatment group has 24 years whereas in the control group has 27 years). Around the threshold, the ages are between 28 to 36.

Finally, the possibility of the respondents manipulating their reported age in the survey (which is the basis for the construction of the running variable) is examined by conducting a McCrary test. The results of the test using the optimal bandwidth of 42 months (used in the parametric analysis), graphically displayed in Figure C2.5 in Appendix C, reveal that the density of the age in months for the observations does not change significantly at the cut-off point. The discontinuity in the frequency of the running variable at the threshold is not statistically significant at a p-value>0.1. This suggests that the values for the age just below and above the cut-off point are not systematically misreported in the survey.

An alternative test, following Cattaneo et al. (2018), for the density discontinuity based on the local polynomial density estimation is used. The value of the t-statistic is -1.4 and the associated p-value is 0.16. This means that under the continuity-based approach, the null hypothesis of no difference in the density of treated and control observations at the cut-off point is not rejected. The result is consistent with the McCrary test.

#### **2.6 Empirical Results**

The results presented for the non-parametric analysis consider 75 months on either side of the threshold (or cut-off point), which corresponds to the birth cohorts (measured in months) of the observations included in the sample. The parametric analysis uses a 42-months window, which is equivalent to 3.5 years before and after the reform. The primary sample used for both analyses comprises 145,035 observations. It excludes individuals with more than 17 years of schooling to avoid driving the estimates upwards as higher earnings are reported at higher academic levels.<sup>32</sup> It also trimmed the top and bottom 1% of the hourly wage distribution within each academic level for avoiding outliers that could drive the estimates upwards or downwards.<sup>33</sup>

The non-parametric analysis selects the optimal bandwidth based on a local linear regression and the results report the robust bias-corrected estimates. The parametric analysis allows polynomial functions on either side of the cut-off point as suggested by Gelman and Imbens (2017). It also uses a spline interacted quadratic specification for the running variable. In both analyses, the polynomial order two is used. The explanatory variables are included to control for macroeconomic and socio-demographic shocks. The first section (2.6.1) reports the results on the returns to compulsory education, whereas the second section (2.6.2) presents the estimates on the employment sectoral choices.

#### 2.6.1 Returns to Compulsory Education

The standard OLS approach, based on the traditional Mincerian equation for the returns to education, reports increasing hourly wages, on average, between 5.8–6.3% per additional year of schooling, *ceteris paribus* (see Table 2.5).<sup>34</sup> The OLS coefficients may underestimate the returns, for example, if there are diminishing marginal returns to education, because of over-reported years of schooling, or due to measurement error on earnings. Hence, the naïve estimates are not accounting for the endogeneity of schooling.

<sup>&</sup>lt;sup>32</sup> Individuals with more than 18 years of schooling, such as Master and Doctoral degrees, represent 1.35% of the sample. As argued by Aydemir and Kirdar (2017), the policy would be unlikely to have had spill-over effects far from the high-school or undergraduate academic level.

<sup>&</sup>lt;sup>33</sup> The literature on wage inequality generally follows this convention, see Katz and Autor (1999), and Autor et al. (2008) cited in Campos-Vázquez et al. (2016).

<sup>&</sup>lt;sup>34</sup> See Appendix A2.1 for the model specification.

|                      |                     | Log of ho           | urly wage           |                     |
|----------------------|---------------------|---------------------|---------------------|---------------------|
|                      | (1)                 | (2)                 | (3)                 | (4)                 |
| Years of schooling   | 0.062***<br>(0.000) | 0.063***<br>(0.000) | 0.061***<br>(0.000) | 0.058***<br>(0.000) |
| Obs.                 | 145,035             | 145,035             | 145,035             | 145,035             |
| Survey year dummies  | No                  | Yes                 | Yes                 | Yes                 |
| Birth region dummies | No                  | No                  | Yes                 | Yes                 |
| Urban status         | No                  | No                  | No                  | Yes                 |

Table 2. 5: OLS results of the returns to education

*Notes:* \*p<0.1, \*\* p<0.05, \*\*\* p<0.01

Standard errors in parentheses. The sample is constructed from the 2009-2017 Mexican National Occupations and Employment Survey.

#### a) Non-Parametric analysis

The estimated effects of the returns to compulsory education for the three relevant parameters discussed previously  $(\widehat{\alpha_1}, \widehat{\beta_1} \text{ and } \widehat{\delta_1})$  are reported in Table 2.6. The discontinuity at the mean of the years of schooling around the threshold of the running variable outlined in section 2.5.1 is validated by this identification strategy.

The estimated parameter  $\widehat{\alpha_1}$  is statistically significant in four different specifications. These estimates indicate that the exposure to compulsory schooling until the age of 15, increased years of education, on average, within a range of 0.24–0.29 of a year. The effects become less significant when the urban settlement status variable is added to the model (see column d of Table 2.6).

These estimates are smaller compared to other developing economies that increased similarly three years of compulsory schooling. For example, the average years of schooling increased by more than half of a year in Turkey (Aydemir and Kirdar, 2017) and 0.4 of a year in Taiwan (Spohr, 2003).<sup>35</sup> Although the effects are similar to the ones reported for France by Grenet (2013), which are in the range of 0.26–0.30 of a year, the reform in France increased two additional years of compulsory schooling compared to three years in Mexico.

<sup>&</sup>lt;sup>35</sup> The effects are smaller also compared to China and the UK, although the reforms increased mandatory education by one additional year in this countries, it increased average years of education by 0.8 in China (Fang et al., 2012) and between 0.36–0.48 of a year in the UK (Devereux and Hart, 2010).

The estimates for the reduced-form model (2),  $\widehat{\beta_1}$ , do not reveal any impact on earnings. Although Figure 2.6 illustrates a discontinuity in hourly wages for the treated group around the cut-off point, the 2SLS of the fuzzy RDD approach displays positive estimates of the returns to compulsory education ( $\widehat{\delta_1}$ ) that do not exhibit a statistically significant effect on earnings. Thus, no impacts on the individuals exposed to the 1993 educational reform compared to those not exposed (see Table 2.6).

Aydemir and Kirdar (2017) found similar non-statistically significant returns to compulsory schooling for Turkey. The estimates reported in this analysis (6%–8.6% see Table 2.6) are in the range of the returns to education found by Duflo (2001) for Indonesia (6.8%–10.6%).

| Estimation method     |             | First   | -stage    |         |         | Reduc     | ed-form   |         |         | 2         | SLS       |         |
|-----------------------|-------------|---------|-----------|---------|---------|-----------|-----------|---------|---------|-----------|-----------|---------|
| Dependent variable    | -           | Yearsof | schooling |         |         | Log of ho | urly wage | s       |         | Log of ho | urly wage | s       |
|                       | (a)         | (b)     | (c)       | (d)     | (a)     | (b)       | (c)       | (d)     | (a)     | (b)       | (c)       | (d)     |
| Treatment             | 0.288**     | 0.277*  | 0.275**   | 0.236*  | 0.024   | 0.024     | 0.016     | 0.015   |         |           |           |         |
|                       | (0.142)     | (0.145) | (0.125)   | (0.132) | (0.020) | (0.021)   | (0.018)   | (0.019) |         |           |           |         |
| Years of schooling    |             |         |           |         |         |           |           |         | 0.086   | 0.085     | 0.060     | 0.062   |
|                       |             |         |           |         |         |           |           |         | (0.068) | (0.073)   | (0.063)   | (0.080) |
| Obs.                  | 145,035     | 145,035 | 145,035   | 145,035 | 145,035 | 145,035   | 145,035   | 145,035 | 145,035 | 145,035   | 145,035   | 145,035 |
| Eff. Number of obs.   | 37,447      | 35,442  | 47,611    | 39,454  | 37,447  | 35,442    | 47,611    | 39,454  | 37,447  | 35,442    | 47,611    | 39,454  |
| Optimal bandwidth     | 32.13       | 31.25   | 38.64     | 33.90   | 32.13   | 31.25     | 38.64     | 33.90   | 32.13   | 31.25     | 38.64     | 33.90   |
| Survey year dummies   | 5 No        | Yes     | Yes       | Yes     | No      | Yes       | Yes       | Yes     | No      | Yes       | Yes       | Yes     |
| Birth region dummies  | s No        | No      | Yes       | Yes     | No      | No        | Yes       | Yes     | No      | No        | Yes       | Yes     |
| Urban status          | No          | No      | No        | Yes     | No      | No        | No        | Yes     | No      | No        | No        | Yes     |
| Notes: *p<0.1. ** p<0 | .05. *** p< | <0.01   |           |         |         |           |           |         |         |           |           |         |

Table 2. 6: Non-parametric results of the returns to compulsory schooling

The sample is constructed from the 2009-2017 Mexican National Occupations and Employment Survey. Following Calonico et al. (2018) and Calonico et al. (2014) for the optimal bandwidth. Robust standard errors using EHW correction as recommended by Kolesár and Rothe (2018) in parentheses.

#### b) Parametric analysis

Table 2.7 reports the estimates of the relevant parameters  $(\widehat{\alpha_1}, \widehat{\beta_1} \text{ and } \widehat{\delta_1})$ . The estimated effects reported in the first stage,  $\widehat{\alpha_1}$ , increases average years of schooling by 0.14 to 0.15 years. These magnitudes are almost halved compared to the non-parametric estimates. The effects vanish when the urban residing status is included. The estimates are smaller compared to those found for other developing countries, but similar to the ones found for the US and

Canada, which increased average years of schooling by 0.10 and 0.13 years, respectively (Oreopoulos, 2006a).<sup>36</sup>

The reduced-form yields statistically insignificant estimates for  $\widehat{\beta_1}$ . The returns to compulsory schooling,  $\widehat{\delta_1}$ , are consistent with the non-parametric results. Although, the magnitudes here are greater. Thus, the parametric analysis confirms that the 1993 educational reform improved school attendance marginally, but the size effect was weak enough that it did not translate into a statistically significant improvement in hourly wages per additional year of schooling for the people exposed to the reform.

Table 2. 7: Parametric results of the returns to compulsory schooling

| Estimation method    |         | First    | -stage    |         |         | Reduc    | ed-form   |         |         | 2        | SLS       |         |
|----------------------|---------|----------|-----------|---------|---------|----------|-----------|---------|---------|----------|-----------|---------|
| Dependent variable   | -       | Years of | schooling | -       | l       | og of ho | ourly wag | es      | L       | og of ho | urly wage | 25      |
|                      | (a)     | (b)      | (c)       | (d)     | (a)     | (b)      | (c)       | (d)     | (a)     | (b)      | (c)       | (d)     |
|                      |         |          |           |         |         |          |           |         |         |          |           |         |
| Treatment            | 0.147*  | 0.147*   | 0.137*    | 0.116   | 0.016   | 0.016    | 0.015     | 0.012   |         |          |           |         |
|                      | (0.082) | (0.082)  | (0.081)   | (0.079) | (0.012) | (0.012)  | (0.011)   | (0.011) |         |          |           |         |
| Years of schooling   |         |          |           |         |         |          |           |         | 0.110   | 0.109    | 0.110     | 0.106   |
| Ũ                    |         |          |           |         |         |          |           |         | (0.075) | (0.075)  | (0.080)   | (0.094) |
|                      |         |          |           |         |         |          |           |         |         |          |           |         |
| Obs.                 | 85,890  | 85,890   | 85,890    | 85,890  | 85,890  | 85,890   | 85,890    | 85,890  | 85,890  | 85,890   | 85,890    | 85,890  |
| Survey year dummies  | No      | Yes      | Yes       | Yes     | No      | Yes      | Yes       | Yes     | No      | Yes      | Yes       | Yes     |
| Birth region dummies | No      | No       | Yes       | Yes     | No      | No       | Yes       | Yes     | No      | No       | Yes       | Yes     |
| Urban status         | No      | No       | No        | Yes     | No      | No       | No        | Yes     | No      | No       | No        | Yes     |

Notes: \*p<0.1, \*\* p<0.05, \*\*\* p<0.01

The sample is constructed from the 2009-2017 Mexican National Occupations and Employment Survey. Robust standard errors using EHW correction as recommended by Kolesár and Rothe (2018) in parentheses.

#### 2.6.2 Effects on Employment Sectoral Choices

In light of the foregoing results, this analysis seeks to explore to what extent the educational reform that increased schooling influenced individual choices regarding employment sectors. Dummy variables for working in the formal, informal and self-employment sectors are used. The results reveal that the 1993 intervention in Mexico increased education, but it did not impact the employment sector decisions.

Table 2.8 reports the results for the standard OLS specification that uses a Mincerian equation with dummy variables for sectoral attachment as dependent variables.<sup>37</sup> The estimates

<sup>&</sup>lt;sup>36</sup> The study analyses the raised in minimum school-leaving age by one year in the US and Canada.

<sup>&</sup>lt;sup>37</sup> See Appendix A2.2 for the model specification.

indicate that the probability of working in the formal sector increases by 4.4 percentage points with an additional year of schooling. Conversely, the probability of working in the informal sector and as self-employed decreases by 4.4 and 0.4 percentage points respectively. Nonetheless, these estimates do not address endogeneity in education.

|                      | (1)       | (2)         | (3)          | (4)       |  |
|----------------------|-----------|-------------|--------------|-----------|--|
|                      |           | Formal em   | ployment (=1 | L)        |  |
| Years of schooling   | 0.044***  | 0.044***    | 0.044***     | 0.041***  |  |
|                      | (0.000)   | (0.000)     | (0.000)      | (0.000)   |  |
|                      |           | Informal em | ployment (=  | :1)       |  |
| Years of schooling   | -0.044*** | -0.044***   | -0.044***    | -0.041*** |  |
|                      | (0.000)   | (0.000)     | (0.000)      | (0.000)   |  |
|                      |           | Self-empl   | oyment (=1)  |           |  |
| Years of schooling   | -0.004*** | -0.004***   | -0.004***    | -0.003*** |  |
|                      | (0.000)   | (0.000)     | (0.000)      | (0.000)   |  |
| Obs.                 | 145,035   | 145,035     | 145,035      | 145,035   |  |
| Survey year dummies  | No        | Yes         | Yes          | Yes       |  |
| Birth region dummies | No        | No          | Yes          | Yes       |  |
| Urban status         | No        | No          | No           | Yes       |  |

Table 2. 8: OLS results of the employment sectoral choices

Notes: \*p<0.1, \*\* p<0.05, \*\*\* p<0.01

Standard errors in parentheses. The sample is constructed from the 2009-2017 Mexican National Occupations and Employment Survey.

#### a) Non-Parametric analysis

Using non-parametric analysis, the reform increased average years of schooling by between 0.25 and 0.32 of a year among people working in the formal, informal and self-employment sectors (see Table 2.9). The estimated effects for the reduced-form and the 2SLS do not yield a well-determined causal impact of the increased schooling policy on the probability of working in a specific employment sector.

The signs of the magnitudes are positive for the formal sector and negative for the informal sector and self-employment as anticipated, although neither are statistically significant. Similar results for the self-employed workers were found by Pischke and Von Wachter (2008) for Germany, and by Grenet (2013) for France and the UK.

# b) Parametric analysis

Table 2.10 displays the results for the parametric analysis. In the first stage, the estimates are the same as for the returns to compulsory education reported in Table 2.7, which reveal positive effects of the reform on years of schooling. Although the signs of the coefficients in the reduced-form and the 2SLS are as anticipated, they are not statistically significant. As with the effects of the reform on earnings, the schooling size effect was weak and it did not translate into causal effects on the selection of the employment sectors.

# Table 2. 9: Non-parametric results of the employment sectoral choices

|                      |          |   |           | For     | mal emplo  | yment (=   | 1)       |         |         |            |          |         |
|----------------------|----------|---|-----------|---------|------------|--|----------|---------|---------|------------|----------|---------|
| Estimation method    |          | First   | -stage    |         |            | Reduc  | ed-form  |         |         | 2          | SLS      |         |
| Dependent variable   | _        | Years of  | schooling | -       |            | Formal e   | nploymen | it      |         | Formal er  | nploymen | t       |
|                      | (a)      | (b)   | (c)       | (d)     | (a)        | (b)  | (c)      | (d)     | (a)     | (b)        | (c)      | (d)     |
| Treatment            | 0.317**  | 0.314**   | 0.292**   | 0.252*  | 0.022      | 0.022  | 0.019    | 0.015   |         |            |          |         |
|                      | (0.137)  | (0.138)   | (0.132)   | (0.130) | (0.017)    | (0.017)  | (0.017)  | (0.017) |         |            |          |         |
| Years of schooling   |          |   |           |         |            |  |          |         | 0.070   | 0.071      | 0.065    | 0.060   |
| Total of Sensoning   |          |   |           |         |            |  |          |         | (0.056) | (0.056)    | (0.059)  | (0.069) |
| Obs.                 | 145,035  | 145,035   | 145,035   | 145,035 | 145,035    | 145,035  | 145,035  | 145,035 | 145,035 | 145,035    | 145,035  | 145,035 |
| Eff. Number of obs.  | 41,528   | 41,528  | 43,522    | 43,522  | 41,528     | 41,528   | 43,522   | 43,522  | 41,528  | 41,528     | 43,522   | 43,522  |
| Bandwidth            | 32.42    | 31.89   | 33.91     | 33.48   | 32.42      | 31.89  | 33.91    | 33.48   | 32.42   | 31.89      | 33.91    | 33.48   |
| Survey year dummies  | No       | Yes   | Yes       | Yes     | No         | Yes  | Yes      | Yes     | No      | Yes        | Yes      | Yes     |
| Birth region dummies | No       | No  | Yes       | Yes     | No         | No   | Yes      | Yes     | No      | No         | Yes      | Yes     |
| Urban status         | No       | No  | No        | Yes     | No         | No   | No       | Yes     | No      | No         | No       | Yes     |
|                      |          |   |           | Info    | rmal emple | ovment (:  | =1)      |         |         |            |          |         |
| Estimation method    |          | First   | -stage    |         |            | Reduc  | ed-form  |         |         | 2          | SLS      |         |
| Dependent variable   | _        | Years of  | schooling | -       |            | Informal e   | mployme  | _<br>nt |         | Informal e | mployme  | _<br>nt |
|                      | (a)      | (b)   | (c)       | (d)     | (a)        | (b)  | (c)      | (d)     | (a)     | (b)        | (c)      | (d)     |
| Treatment            | 0.295**  | 0.290**   | 0.28/**   | 0.245*  | -0.021     | -0.022   | -0.018   | -0.015  |         |            |          |         |
| neatment             | (0.140)  | (0.142)   | (0.134)   | (0.131) | (0.017)    | (0.018)  | (0.016)  | (0.017) |         |            |          |         |
| Vears of schooling   | (0.2.10) | (012.12)  | (0.20.1)  | (0.202) | (0.027)    | (0.020)  | (0.020)  | (0.01)  | -0.073  | -0.077     | -0.065   | -0.060  |
| rears of schooling   |          |   |           |         |            | ) (0.017) (0.017) (0.017)<br>5 145,035 145,035 145,<br>41,528 43,522 43,5<br>31.89 33.91 33.4<br>Yes Yes Yi<br>No Yes Yi<br>No No Yi<br>bloyment (=1)<br>Reduced-form<br>Informal employment<br>(b) (c) (c)<br>-0.022 -0.018 -0.02<br>) (0.018) (0.016) (0.02<br>5 145,035 145,035 145,0<br>37,447 41,528 41,5<br>31.89 33.91 33.4<br>Yes Yes Yi<br>No Yo Yi<br>yment (=1)<br><u>Reduced-form</u><br>Self-employment<br>(b) (c) (c)<br>-0.000 -0.000 -0.000<br>) (0.013) (0.012) (0.02<br>5 145,035 |          |         | (0.061) | (0.063)    | (0.060)  | (0.071) |
|                      |          | Years of schooling         (a)         (b)         (c)         (d)         (a)           (a)         (b)         (c)         (d)         (a)         (a)           0.317**         0.314**         0.292**         0.252*         0.022           (0.137)         (0.138)         (0.132)         (0.130)         (0.017           145,035         145,035         145,035         145,035         145,035           145,28         41,528         43,522         43,522         41,528           32.42         31.89         33.91         33.48         32.42           No         Yes         Yes         No         No         No           No         No         Yes         Yes         No         No           No         No         No         Yes         Yes         No           No         No         No         Yes         Yes         No           No         No         No         Yes         Yes         No           (a)         (b)         (c)         (d)         (a)           0.295**         0.290**         0.284**         0.245*         -0.021           (0.140)         (0.142) |           |         |            |  |          |         |         |            |          |         |
| Obs.                 | 145,035  | 145,035   | 145,035   | 145,035 | 145,035    | 145,035  | 145,035  | 145,035 | 145,035 | 145,035    | 145,035  | 145,035 |
| Eff. Number of obs.  | 37,447   | 37,447  | 41,528    | 41,528  | 37,447     | 37,447   | 41,528   | 41,528  | 37,447  | 37,447     | 41,528   | 41,528  |
| Bandwidth            | 32.42    | 31.89   | 33.91     | 33.48   | 32.42      | 31.89  | 33.91    | 33.48   | 32.42   | 31.89      | 33.91    | 33.48   |
| Survey year dummies  | No       | Yes   | Yes       | Yes     | No         | Yes  | Yes      | Yes     | No      | Yes        | Yes      | Yes     |
| Birth region dummies | No       | No  | Yes       | Yes     | No         | No   | Yes      | Yes     | No      | No         | Yes      | Yes     |
| Urban status         | No       | No  | No        | Yes     | No         | No   | No       | Yes     | No      | No         | No       | Yes     |
|                      |          |   |           | Se      | elf-employ | ment (=1)  |          |         |         |            |          |         |
| Estimation method    |          | First   | -stage    |         |            | Reduc  | ed-form  |         |         | 2          | SLS      |         |
| Dependent variable   | _        | Years of  | schooling | -       |            | Self-em  | ployment | -       |         | Self-em    | ployment | -       |
|                      | (a)      | (b)   | (c)       | (d)     | (a)        | (b)  | (c)      | (d)     | (a)     | (b)        | (c)      | (d)     |
| Treatment            | 0.312**  | 0.310**   | 0.293**   | 0.256** | -0.001     | -0.000   | -0.000   | -0.000  |         |            |          |         |
|                      | (0.139)  | (0.139)   | (0.131)   | (0.128) | (0.012)    | (0.013)  | (0.012)  | (0.012) |         |            |          |         |
| Years of schooling   |          |   |           |         |            |  |          |         | -0.002  | -0.003     | -0.002   | -0.000  |
| 5                    |          |   |           |         |            |  |          |         | (0.044) | (0.045)    | (0.045)  | (0.052) |
|                      |          |   |           |         |            |  |          |         |         |            |          |         |
| Obs.                 | 145,035  | 145,035   | 145,035   | 145,035 | 145,035    | 145,035  | 145,035  | 145,035 | 145,035 | 145,035    | 145,035  | 145,035 |
| Eff. Number of obs.  | 41,528   | 41,528  | 43,522    | 43,522  | 41,528     | 41,528   | 43,522   | 43,522  | 41,528  | 41,528     | 43,522   | 43,522  |
| Bandwidth            | 31.40    | 31.57   | 34.36     | 34.57   | 31.40      | 31.57  | 34.36    | 34.57   | 31.40   | 31.57      | 34.36    | 34.57   |
| Survey year dummies  | No       | Yes   | Yes       | Yes     | No         | Yes  | Yes      | Yes     | No      | Yes        | Yes      | Yes     |
| Birth region dummies | No       | No  | Yes       | Yes     | No         | No   | Yes      | Yes     | No      | No         | Yes      | Yes     |
| Urban status         | No       | No  | No        | Yes     | No         | No   | No       | Yes     | No      | No         | No       | Yes     |
| Note: **             |          |   |           |         |            |  |          |         |         |            |          |         |

Notes: \*p<0.1, \*\* p<0.05, \*\*\* p<0.01

The sample is constructed from the 2009-2017 Mexican National Occupations and Employment Survey. Following Calonico et al. (2018) and Calonico et al. (2014) for the optimal bandwidth. Robust standard errors using EHW correction as recommended by Kolesár and Rothe (2018) in parentheses.

|                      |         |          |           | Forma   | al employ | ment (=1  | )                   |         |         |           |          |          |
|----------------------|---------|----------|-----------|---------|-----------|-----------|---------------------|---------|---------|-----------|----------|----------|
| Estimation method    |         | First    | -stage    |         |           | Reduce    | ed-form             |         |         | 25        | SLS      |          |
| Dependent variable   |         | Years of | schooling | 5       | F         | ormal en  | nploymer            | nt      | F       | ormal er  | nployme  | nt       |
|                      | (a)     | (b)      | (c)       | (d)     | (a)       | (b)       | (c)                 | (d)     | (a)     | (b)       | (c)      | (d)      |
| Treatment            | 0.147*  | 0.147*   | 0.137*    | 0.116   | 0.011     | 0.011     | 0.010               | 0.008   |         |           |          |          |
|                      | (0.082) | (0.082)  | (0.081)   | (0.079) | (0.010)   | (0.010)   | (0.010)             | (0.010) |         |           |          |          |
| Years of schooling   | . ,     | . ,      | . ,       | . ,     |           | . ,       | . ,                 |         | 0.073   | 0.075     | 0.076    | 0.072    |
| 0                    |         |          |           |         |           |           |                     |         | (0.067) | (0.067)   | (0.071)  | (0.084)  |
|                      |         |          |           |         |           |           |                     |         |         |           |          |          |
| Obs.                 | 85,890  | 85,890   | 85,890    | 85,890  | 85,890    | 85,890    | <mark>85,890</mark> | 85,890  | 85,890  | 85,890    | 85,890   | 85,890   |
| Survey year dummies  | No      | Yes      | Yes       | Yes     | No        | Yes       | Yes                 | Yes     | No      | Yes       | Yes      | Yes      |
| Birth region dummies | No      | No       | Yes       | Yes     | No        | No        | Yes                 | Yes     | No      | No        | Yes      | Yes      |
| Urban status         | No      | No       | No        | Yes     | No        | No        | No                  | Yes     | No      | No        | No       | Yes      |
|                      |         |          |           |         |           |           |                     |         |         |           |          |          |
|                      |         |          |           | Inform  | al employ | yment (=: | 1)                  |         |         |           |          |          |
| Estimation method    |         | First    | -stage    |         |           | Reduce    | ed-form             |         |         | 25        | SLS      |          |
| Dependent variable   | _       | Years of | schooling | -       | In        | formal e  | mployme             | nt      | Ir      | nformal e | mployme  | -<br>ent |
|                      | (a)     | (b)      | (c)       | (d)     | (a)       | (b)       | (c)                 | (d)     | (a)     | (b)       | (c)      | (d)      |
| Treatment            | 0.147*  | 0147*    | 0 137*    | 0 1 1 6 | -0.011    | -0.011    | -0.010              | -0.008  |         |           |          |          |
| Hodemone             | (0.082) | (0.082)  | (0.081)   | (0.079) | (0.010)   | (0.010)   | (0.010)             | (0.010) |         |           |          |          |
| Years of schooling   | (01002) | (0.002)  | (0.001)   | (0.075) | (0.010)   | (0.010)   | (01010)             | (0.010) | -0.073  | -0.075    | -0.076   | -0.072   |
|                      |         |          |           |         |           |           |                     |         | (0.067) | (0.067)   | (0.071)  | (0.084)  |
|                      |         |          |           |         |           |           |                     |         | . ,     | . ,       | . ,      |          |
| Obs.                 | 85,890  | 85,890   | 85,890    | 85,890  | 85,890    | 85,890    | 85,890              | 85,890  | 85,890  | 85,890    | 85,890   | 85,890   |
| Survey year dummies  | No      | Yes      | Yes       | Yes     | No        | Yes       | Yes                 | Yes     | No      | Yes       | Yes      | Yes      |
| Birth region dummies | No      | No       | Yes       | Yes     | No        | No        | Yes                 | Yes     | No      | No        | Yes      | Yes      |
| Urban status         | No      | No       | No        | Yes     | No        | No        | No                  | Yes     | No      | No        | No       | Yes      |
|                      |         |          |           |         |           |           |                     |         |         |           |          |          |
|                      |         |          |           | Self-   | employm   | nent (=1) |                     |         |         |           |          |          |
| Estimation method    |         | First    | -stage    | _       |           | Reduce    | ed-form             | _       |         | 25        | SLS      | _        |
| Dependent variable   | _       | Years of | schooling | 5       |           | Self-emp  | oloyment            |         |         | Self-emp  | ployment | t        |
|                      | (a)     | (b)      | (c)       | (d)     | (a)       | (b)       | (c)                 | (d)     | (a)     | (b)       | (c)      | (d)      |
| Treatment            | 0.147*  | 0.147*   | 0.137*    | 0.116   | -0.004    | -0.005    | -0.005              | -0.005  |         |           |          |          |
|                      | (0.082) | (0.081)  | (0.081)   | (0.079) | (0.007)   | (0.007)   | (0.007)             | (0.007) |         |           |          |          |
| Years of schooling   | . ,     | . ,      |           | . ,     |           |           |                     |         | -0.033  | -0.034    | -0.036   | -0.041   |
| Ū                    |         |          |           |         |           |           |                     |         | (0.052) | (0.052)   | (0.057)  | (0.069)  |
| Obs.                 | 85,890  | 85,890   | 85,890    | 85,890  | 85,890    | 85,890    | 85,890              | 85,890  | 85,890  | 85,890    | 85,890   | 85,890   |
| Survey year dummies  | No      | Yes      | Yes       | Yes     | No        | Yes       | Yes                 | Yes     | No      | Yes       | Yes      | Yes      |
| Birth region dummies | No      | No       | Yes       | Yes     | No        | No        | Yes                 | Yes     | No      | No        | Yes      | Yes      |
| Urban status         | No      | No       | No        | Yes     | No        | No        | No                  | Yes     | No      | No        | No       | Yes      |
|                      |         |          |           |         |           |           |                     |         |         |           |          |          |

# Table 2. 10: Parametric results of the employment sectoral choices

*Notes:* \*p<0.1, \*\* p<0.05, \*\*\* p<0.01

The sample is constructed from the 2009-2017 Mexican National Occupations and Employment Survey. Robust standard errors using EHW correction as recommended by Kolesár and Rothe (2018) in parentheses.

Therefore, the results do not report a causal effect of the intervention on both the returns to compulsory education and the employment sectoral outcomes. Although the reform increased school attendance, as shown in the first stage estimates of the fuzzy RDD, there

are no causal impacts on earnings nor on the probability of working in the formal sector (informal sector or as self-employed).

The estimates reported could be a consequence of the "degraded tertiary effect" in schooling, where the students do not receive the skills and qualifications at school that are subsequently required in the Mexican labour market (Campos-Vázquez et al., 2016). Thus, the tertiary education institutions in Mexico after the 1993 educational reform were not prepared for the spill-over effect that kept the students at higher schooling levels. Consequently, the education provided did not have the same quality, on average, as for older cohorts and the skills acquired through schooling could have been of lower quality, either because the new postsecondary schooling institutions were of lower quality (due to the size-class ratio of teachers per students) and/or because the marginal student had lower abilities.<sup>38</sup>

This hypothesis is strengthened by Aydemir and Kirdar (2017). The authors suggest that the low returns to compulsory education in Turkey may reflect the low quality of education that prevailed before the reform. However, there is a debate in the literature regarding the implications of quality in education on the returns to schooling. On the one hand, Duflo (2001) found higher returns to education for Indonesia, where the schooling quality was worse than in Turkey according to international maths and science tests. On the other hand, for England and Germany, where the international tests indicate a better quality of schooling, Devereux and Hart (2010), and Pischke and Von Wachter (2008) found quite low returns to compulsory education.

Pischke and Von Wachter (2008) argued that rather than the quality of schooling, it may be the labour market relevance of the skills acquired at school, encouraged for a particular policy, which determines the returns to education. Aydemir and Kirdar (2017) pointed out that a potential confounder in the analysis is the required labour market experience as these types of policies focus on inducing only more years of education.

In some developing countries, employment occurs around the age of 15 (which is the age targeted by the 1993 educational reform) and Mexico is not an exception. This work experience could be rewarded in the labour market. For the Mexican context, this means that

<sup>&</sup>lt;sup>38</sup> There may also be variability in quality of school if many children are undernousished or inadequate suppliers are provided.

those individuals exposed to the reform and compelled to attend school perhaps increased their schooling, but reduced their labour market experience at a given age, which could have reduced earnings later in life.

The ENOE does not report information of the age when the people started to work or regarding their first employment that could shed light on this potential confounder. However, the Mexican population census (CONAPO –Spanish acronym) reported an increase in the incidence of child labour among children aged between 12–14 years from 6.6% in 1990 to 7.9% in 2000 (i.e., it would be expected the incidence of child labour to have fallen with the 1993 policy but in fact, it increased).<sup>39</sup>

Another possible confounder pointed out by Campos-Vázquez et al. (2016) is the large proportion of informality in developing countries, exceptionally in Latin America and Mexico. Thus, earnings could be misreported in the survey because informal enterprise owners may not keep accurate earning records, or they may not report the official number of workers and their salary payments or firms may have unpaid family members working in it.

The estimates of the returns to compulsory education presented in this study could be capturing the effects of confounders. Further research is needed for disentangling these effects. Nevertheless, the lack of data regarding other possible confounders in Mexico limits these types of studies (e.g., child labour force participation, informality).

Among the non-parametric and parametric analyses, the sizable estimates are provided by non-parametric methods. The following section seeks to corroborate the stability of previous results.

# **2.7 Robustness Checks**

The analysis is centred on considering variations of the sample, sub-samples, and other specifications (i.e., trimming earnings, excluding or including years of schooling.) as robustness checks for both, the returns to compulsory education and the employment sectoral

<sup>&</sup>lt;sup>39</sup> In 1962 the Labour Law established a minimum legal working age of 14 years. In 2014, the Law raised the legal working age to 15 years, the age targeted by the educational reform in 1993. The Law changed the minimum legal age to 16 years in 2016. CONAPO reported a 2.6 percentage points increase of children aged 13-15 who had completed secondary schooling between 1990 and 2000. This low percentage reported could also reflect children withdraw from the informal labour market to be enrolled into school.

outcomes. In addition, the parametric analysis is conducted with alternative 39 –and 48– months windows. A placebo test in time is also implemented.

Most robustness checks reinforce the results for the core analysis. There is a marginal improvement in school attendance due to the educational policy that does not impact labour market outcomes. However, using sub-samples for formal and informal sectors, the results do not report effects on schooling, whereas the estimates reveal some effects on earnings for the self-employed sub-sample. Regarding the employment sectoral selections, the policy did not influence decisions concerning the sector in which the individuals work.

#### 2.7.1 Returns to Compulsory Education

The results of the returns to compulsory education generally suggest that the impacts of the policy on school attendance do not translate into higher earnings.

## a) Non-Parametric analysis

#### *Pooled* sample

This sample includes 148,964 observations without trimming the wages and not restricting it to the observations with less than 18 years of schooling. Table 2.11 reports average increases in education between 0.20–0.30 years as a consequence of the 1993 reform. These magnitudes are slightly different to those reported in Table 2.6. The policy did not raise earnings as revealed by the 2SLS estimates. These findings are consistent with previous results reported in Table 2.6.<sup>40</sup>

<sup>&</sup>lt;sup>40</sup> Table B2.5 in Appendix B reports the estimates for the parametric analysis considering a sample of 88,227 observations with a 42-months window, without trimming the wages and not restricting the sample to the observations with less than 18 years of schooling. The results are less statistically significant.

| Estimation method    | _       | First    | -stage    | _       |         | Reduc     | ed-form   | _       |         | 2         | SLS       | _       |
|----------------------|---------|----------|-----------|---------|---------|-----------|-----------|---------|---------|-----------|-----------|---------|
| Dependent variable   |         | Years of | schooling |         |         | Log of ho | urly wage | s       |         | Log of ho | urly wage | s       |
|                      | (a)     | (b)      | (c)       | (d)     | (a)     | (b)       | (c)       | (d)     | (a)     | (b)       | (c)       | (d)     |
| Treatment            | 0.262** | 0.295**  | 0.263**   | 0.195*  | 0.015   | 0.025     | 0.021     | 0.006   |         |           |           |         |
|                      | (0.120) | (0.133)  | (0.133)   | (0.114) | (0.018) | (0.020)   | (0.020)   | (0.017) |         |           |           |         |
| Years of schooling   |         |          |           |         |         |           |           |         | 0.059   | 0.085     | 0.082     | 0.033   |
|                      |         |          |           |         |         |           |           |         | (0.066) | (0.067)   | (0.075)   | (0.086) |
| Obs.                 | 148,964 | 148,964  | 148,964   | 148,964 | 148,964 | 148,964   | 148,964   | 148,964 | 148,964 | 148,964   | 148,964   | 148,964 |
| Eff. Number of obs.  | 53,259  | 42,719   | 42,719    | 55,293  | 53,259  | 42,719    | 42,719    | 55,293  | 53,259  | 42,719    | 42,719    | 55,293  |
| Bandwidth            | 44.45   | 37.15    | 36.44     | 45.56   | 44.45   | 37.15     | 36.44     | 45.56   | 44.45   | 37.15     | 36.44     | 45.56   |
| Survey year dummies  | No      | Yes      | Yes       | Yes     | No      | Yes       | Yes       | Yes     | No      | Yes       | Yes       | Yes     |
| Birth region dummies | No      | No       | Yes       | Yes     | No      | No        | Yes       | Yes     | No      | No        | Yes       | Yes     |
| Urban status         | No      | No       | No        | Yes     | No      | No        | No        | Yes     | No      | No        | No        | Yes     |

Table 2. 11: Non-parametric results of the returns to compulsory schooling: pooled sample

Notes: \*p<0.1, \*\* p<0.05, \*\*\* p<0.01

The sample is constructed from the 2009-2017 Mexican National Occupations and Employment Survey. Following Calonico et al. (2018) and Calonico et al. (2014) for the optimal bandwidth. Robust standard errors using EHW correction as recommended by Kolesár and Rothe (2018) in parentheses.

#### Variations in years of schooling and trimming earnings

Table 2.12 reports consistent estimates with a sample that does not trim earnings and includes only observations with less than 18 years of schooling (see columns a-c). The policy increased the average years of schooling within a range of 0.21–0.29 years but did not have an impact on earnings. Table 2.12 also reports no effects on the returns to compulsory education, although schooling raised, on average, between 0.22 and 0.33 of a year (see columns d-f). The sample only trims hourly wages and does not restrict it to specific years of schooling. These magnitudes are slightly bigger than those reported in Table 2.6.<sup>41</sup>

<sup>&</sup>lt;sup>41</sup> Table B2.6 in Appendix B reports the estimates for the parametric analysis considering two samples with variations in the years of schooling and trimming or not the hourly wages. The results are mostly not significant.

| Estimation method     | _       |         | First    | -stage    |         | _       |         |         | Reduc     | ed-form   |         | _       |         |         | 2         | SLS       |         | _       |
|-----------------------|---------|---------|----------|-----------|---------|---------|---------|---------|-----------|-----------|---------|---------|---------|---------|-----------|-----------|---------|---------|
| Dependent variable    | -       |         | Years of | schooling | 5       | _       |         |         | Log of ho | urly wage | 25      | -       |         |         | Log of ho | urly wage | es      | -       |
|                       | (a)     | (b)     | (c)      | (d)       | (e)     | (f)     | (a)     | (b)     | (c)       | (d)       | (e)     | (f)     | (a)     | (b)     | (c)       | (d)       | (e)     | (f)     |
| Treatment             | 0.290** | 0.272** | 0.208*   | 0.327**   | 0.287** | 0.220*  | 0.023   | 0.022   | 0.010     | 0.023     | 0.017   | 0.008   |         |         |           |           |         |         |
|                       | (0.124) | (0.127) | (0.115)  | (0.133)   | (0.128) | (0.120) | (0.019) | (0.019) | (0.018)   | (0.018)   | (0.018) | (0.017) |         |         |           |           |         |         |
| Years of schooling    |         |         |          |           |         |         |         |         |           |           |         |         | 0.080   | 0.082   | 0.049     | 0.071     | 0.060   | 0.038   |
|                       |         |         |          |           |         |         |         |         |           |           |         |         | (0.064) | (0.070) | (0.082)   | (0.056)   | (0.061) | (0.076) |
| Obs.                  | 146,954 | 146,954 | 146,954  | 147,015   | 147,015 | 147,015 | 146,954 | 146,954 | 146,954   | 147,015   | 147,015 | 147,015 | 146,954 | 146,954 | 146,954   | 147,015   | 147,015 | 147,015 |
| Eff. Number of obs.   | 48,268  | 44,132  | 52,512   | 44,151    | 48,296  | 52,561  | 48,268  | 44,132  | 52,512    | 44,151    | 48,296  | 52,561  | 48,268  | 44,132  | 52,512    | 44,151    | 48,296  | 52,561  |
| Band width            | 40.49   | 38.37   | 43.75    | 36.19     | 38.03   | 40.74   | 40.49   | 38.37   | 43.75     | 36.19     | 38.03   | 40.74   | 40.49   | 38.37   | 43.75     | 36.19     | 38.03   | 40.74   |
| Less than 18 years of | Vee     | Vee     | Var      | Ne        | Ne      | Ne      | Ver     | Ver     | Ver       | Ne        | Ne      | Ma      | Ver     | Vec     | Ver       | Ne        | Ne      | Ne      |
| schooling             | res     | res     | res      | NO        | NO      | NO      | res     | Tes     | res       | NO        | NO      | NO      | 162     | Tes     | 162       | NO        | NO      | NO      |
| Trimmed hourly wages  | No      | No      | No       | Yes       | Yes     | Yes     | No      | No      | No        | Yes       | Yes     | Yes     | No      | No      | No        | Yes       | Yes     | Yes     |
| Survey year and birth | Ne      | Var     | Var      | Ne        | Vec     | Ver     | Ne      | Ver     | Var       | Ne        | Vec     | Ver     | Ne      | Vac     | Ver       | Ne        | Ver     | Ver     |
| region dummies        | NO      | res     | res      | NO        | Tes     | 165     | NO      | Tes     | res       | NO        | res     | res     | NO      | Tes     | 165       | NO        | Tes     | res     |
| Urban status          | No      | No      | Yes      | No        | No      | Yes     | No      | No      | Yes       | No        | No      | Yes     | No      | No      | Yes       | No        | No      | Yes     |

 Table 2. 12: Non-parametric results of the returns to compulsory schooling: variations in schooling and earnings

The sample is constructed from the 2009-2017 Mexican National Occupations and Employment Survey. Following Calonico et al. (2018) and Calonico et al. (2014) for the optimal bandwidth. Robust standard errors using EHW correction as recommended by Kolesár and Rothe (2018) in parentheses.

# Sub-samples: self-employment, formal and informal sectors

Additional sub-samples are used for different employment sectors in conjunction with previous specifications that trimmed the top and bottom 1% of hourly wages and include or exclude those observations with higher education. The estimates for the formal and informal sectors do not reveal statistically significant effects of the 1993 policy on schooling or causal impacts on earnings (see Table B2.7 and B2.8 in Appendix B).<sup>42</sup>

The impacts of the policy on schooling are particularly surprising for the formal employment sector that uses schooling for signalling in the labour market. Perhaps these effects are influenced by other confounders in schooling such as quality of education, the degraded tertiary effect in education; or other labour market factors, such as minimum wage settings.

Table 2.13 reports that the policy raised years of education, on average, by between 0.59-0.69 years for the self-employed. These coefficients are higher than previous estimates. For the reduced-form and the 2SLS, some estimates are statistically significant, thus indicating that the intervention boosts wages, on average, between 13.6%-27.2% for an additional year of schooling, *ceteris paribus* (see Table B2.11 in Appendix B for additional specifications).<sup>43</sup>

<sup>&</sup>lt;sup>42</sup> Table B2.9 and B2.10 in Appendix B report the estimated effects for the parametric analysis using subsamples for the formal and informal employment sectors.

<sup>&</sup>lt;sup>43</sup> Technically the estimated coefficients  $\hat{\delta}_1$  are 14.57% [exp (0.136)-1\*100]; 31.26% [exp (0.272)-1\*100]. Table B2.12 in Appendix B reports the estimates for the parametric analysis.

These estimates that reveal effects on earnings, through increases in years of schooling, may be due to the different impacts that attendance in education has on individuals who have their own business since they do not need to demonstrate their school skills to employers. It is also true that there are fewer observations in this sub-sample that could drive these results.

Table 2. 13: Non-parametric results of the returns to compulsory schooling: self-employed

| Estimation method                       |         |         | First    | -stage    |         |         |          |          | Reduced    | l-form   |         |         |         |         | 25        | SLS       |         |         |
|---|---------|---------|----------|-----------|---------|---------|----------|----------|------------|----------|---------|---------|---------|---------|-----------|-----------|---------|---------|
| Dependent variable                      | -       |         | Years of | schooling |         | -       |          | L        | og of hour | ly wages |         | -       |         | 1       | .og of ho | urly wage | s       | -       |
|   | (a)     | (b)     | (c)      | (d)       | (e)     | (f)     | (a)      | (b)      | (c)        | (d)      | (e)     | (f)     | (a)     | (b)     | (c)       | (d)       | (e)     | (f)     |
| Treatment                               | 0.666** | 0.616** | 0.614**  | 0.686**   | 0.646** | 0.594*  | 0.175*** | 0.164*** | 0.167***   | 0.093*   | 0.087   | 0.076   |         |         |           |           |         |         |
|   | (0.316) | (0.296) | (0.297)  | (0.330)   | (0.328) | (0.328) | (0.060)  | (0.055)  | (0.055)    | (0.093)  | (0.053) | (0.053) |         |         |           |           |         |         |
| Years of schooling                      |         |         |          |           |         |         |          |          |            |          |         |         | 0.263** | 0.267** | 0.272*    | 0.136*    | 0.135   | 0.128   |
|   |         |         |          |           |         |         |          |          |            |          |         |         | (0.131) | (0.132) | (0.139)   | (0.083)   | (0.086) | (0.093) |
| Obs.                                    | 23,711  | 23,711  | 23,711   | 22,437    | 22,437  | 22,437  | 23,711   | 23,711   | 23,711     | 22,437   | 22,437  | 22,437  | 23,711  | 23,711  | 23,711    | 22,437    | 22,437  | 22,437  |
| Eff. Number of obs.                     | 8,739   | 9,656   | 9,051    | 7,624     | 7,624   | 7,293   | 8,739    | 9,656    | 9,051      | 7,624    | 7,624   | 7,293   | 8,739   | 9,656   | 9,051     | 7,624     | 7,624   | 7,293   |
| Bandwidth                               | 41.98   | 47.60   | 44.22    | 40.20     | 39.54   | 37.50   | 41.98    | 47.60    | 44.22      | 40.20    | 39.54   | 37.50   | 42      | 48      | 44        | 40.20     | 39.54   | 37.50   |
| Less than 18 years of schooling         | No      | No      | No       | Yes       | Yes     | Yes     | No       | No       | No         | Yes      | Yes     | Yes     | No      | No      | No        | Yes       | Yes     | Yes     |
| Trimmed hourly wages                    | No      | No      | No       | Yes       | Yes     | Yes     | No       | No       | No         | Yes      | Yes     | Yes     | No      | No      | No        | Yes       | Yes     | Yes     |
| Survey year and birth<br>region dummies | No      | Yes     | Yes      | No        | Yes     | Yes     | No       | Yes      | Yes        | No       | Yes     | Yes     | No      | Yes     | Yes       | No        | Yes     | Yes     |
| Urban status                            | No      | No      | Yes      | No        | No      | Yes     | No       | No       | Yes        | No       | No      | Ves     | No      | No      | Vec       | No        | No      | Yes     |

The sample is constructed from the 2009-2017 Mexican National Occupations and Employment Survey. Following Calonico et al. (2018) and Calonico et al. (2014) for the optimal bandwidth. Robust standard errors using EHW correction as recommended by Kolesár and Rothe (2018) in parentheses.

# a) Parametric analysis

# Additional windows employed

The results for the analysis conducted with 39 –and 48–month windows are reported in Table 2.14. Most specifications reveal in the first stage increases in years of schooling, on average, in a range of 0.13–0.17 years. Consistently, there are no effects on earnings through schooling.<sup>44</sup>

In general, most of the findings presented in this section reinforce the fact that the policy increased schooling. The results reveal that Mexico behaves similarly to other developed countries, in which compulsory education policies do not impact earnings, even if they boost school attendance, as has been pointed out in the literature.

<sup>&</sup>lt;sup>44</sup> Table B2.13 in Appendix B reports the results for additional sub-samples.

|   |                    |         |          |                    |                   |                   | 39-r             | nonths        | window        |                  |                  |               |                  |                  |                  |                  |                  |                  |
|---|--------------------|---------|----------|--------------------|-------------------|-------------------|------------------|---------------|---------------|------------------|------------------|---------------|------------------|------------------|------------------|------------------|------------------|------------------|
| Estimation method                       |                    |         | Firs     | t-stage            |                   |                   |                  |               | Reduc         | ced-form         |                  | _             |                  |                  | 2                | 2SLS             |                  |                  |
| Dependent variable                      | _                  |         | Years of | fschooling         | 5                 | -                 |                  |               | Log of h      | ourly wag        | es               | -             |                  |                  | Log of h         | ourly wag        | es               | -                |
|   | (a)                | (b)     | (c)      | (d)                | (e)               | (f)               | (a)              | (b)           | (c)           | (d)              | (e)              | (f)           | (a)              | (b)              | (c)              | (d)              | (e)              | (f)              |
| Treatment                               | 0.169**<br>(0.085) | 0.162*  | 0.145*   | 0.166**<br>(0.085) | 0.161*<br>(0.084) | 0.143*<br>(0.082) | 0.016<br>(0.012) | 0.015 (0.012) | 0.012 (0.012) | 0.017<br>(0.013) | 0.016<br>(0.013) | 0.014 (0.012) |                  |                  |                  |                  |                  |                  |
| Years of schooling                      |                    |         |          |                    |                   |                   |                  |               |               |                  |                  |               | 0.093<br>(0.066) | 0.091<br>(0.067) | 0.086<br>(0.075) | 0.102<br>(0.073) | 0.102<br>(0.074) | 0.097<br>(0.083) |
| Obs.                                    | 79,969             | 79,969  | 79,969   | 82,125             | 82,125            | 82,125            | 79,969           | 79,969        | 79,969        | 82,125           | 82,125           | 82,125        | 79,969           | 79,969           | 79,969           | 82,125           | 82,125           | 82,125           |
| Less than 18 years of schooling         | Yes                | Yes     | Yes      | No                 | No                | No                | Yes              | Yes           | Yes           | No               | No               | No            | Yes              | Yes              | Yes              | No               | No               | No               |
| Trimmed hourly wages                    | Yes                | Yes     | Yes      | No                 | No                | No                | Yes              | Yes           | Yes           | No               | No               | No            | Yes              | Yes              | Yes              | No               | No               | No               |
| Survey year and birth<br>region dummies | No                 | Yes     | Yes      | No                 | Yes               | Yes               | No               | Yes           | Yes           | No               | Yes              | Yes           | No               | Yes              | Yes              | No               | Yes              | Yes              |
| Urban status                            | No                 | No      | Yes      | No                 | No                | Yes               | No               | No            | Yes           | No               | No               | Yes           | No               | No               | Yes              | No               | No               | Yes              |
|   |                    |         |          |                    |                   |                   | 48-r             | nonths        | window        |                  |                  |               |                  |                  |                  |                  |                  |                  |
| Estimation method                       | _                  |         | Firs     | t-stage            |                   | _                 |                  |               | Reduc         | ced-form         |                  | _             |                  |                  | 2                | 2SLS             |                  | _                |
| Dependent variable                      |                    |         | Years of | fschooling         | 5                 |                   |                  |               | Log of h      | ourly wag        | es               |               |                  |                  | Log of h         | ourly wag        | es               |                  |
|   | (a)                | (b)     | (c)      | (d)                | (e)               | (f)               | (a)              | (b)           | (c)           | (d)              | (e)              | (f)           | (a)              | (b)              | (c)              | (d)              | (e)              | (f)              |
| Treatment                               | 0.143*             | 0.132*  | 0.118    | 0.135*             | 0.124             | 0.110             | 0.012            | 0.011         | 0.010         | 0.019*           | 0.018            | 0.016         |                  |                  |                  |                  |                  |                  |
| Years of schooling                      | (0.0.07            | (0.0.0) | (0.07.1) | (0.011)            | (0.07.07          | (0.07.1)          | ()               | ()            | (0.011)       | (/               | (0.022)          | (0.022)       | 0.086<br>(0.069) | 0.087<br>(0.074) | 0.081<br>(0.082) | 0.144<br>(0.090) | 0.149<br>(0.098) | 0.150<br>(0.111) |
| Obs.                                    | 97,661             | 97,661  | 97,661   | 100,296            | 100,296           | 100,296           | 97,661           | 97,661        | 97,661        | 100,296          | 100,296          | 100,296       | 97,661           | 97,661           | 97,661           | 100,296          | 100,296          | 100,296          |
| Less than 18 years of<br>schooling      | Yes                | Yes     | Yes      | No                 | No                | No                | Yes              | Yes           | Yes           | No               | No               | No            | Yes              | Yes              | Yes              | No               | No               | No               |
| Trimmed hourly wages                    | Yes                | Yes     | Yes      | No                 | No                | No                | Yes              | Yes           | Yes           | No               | No               | No            | Yes              | Yes              | Yes              | No               | No               | No               |
| Survey year and birth<br>region dummies | No                 | Yes     | Yes      | No                 | Yes               | Yes               | No               | Yes           | Yes           | No               | Yes              | Yes           | No               | Yes              | Yes              | No               | Yes              | Yes              |
| Urban status                            | No                 | No      | Yes      | No                 | No                | Yes               | No               | No            | Yes           | No               | No               | Yes           | No               | No               | Yes              | No               | No               | Yes              |
| Notes: *or0.1 ** or0.05                 | 5 *** oct (        | 01      |          |                    |                   |                   |                  |               |               |                  |                  |               |                  |                  |                  |                  |                  |                  |

Table 2. 14: Parametric results of the returns to compulsory schooling: 39 -and 48-month windows

The sample is constructed from the 2009-2017 Mexican National Occupations and Employment Survey. Robust standard errors using EHW correction as recommended by Kolesár and Rothe (2018) in parentheses.

# 2.7.2 Effects on Employment Sectoral Choices

The results of the analysis on the sectoral employment choices for the parametric and nonparametric methodologies suggest policy impacts on schooling that do not translate into changes in the employment sectors. The dependent variables used in this section are dummy variables for each of the employment choices.

## a) Non-Parametric analysis

## Pooled sample

Table 2.15 reports that the 1993 intervention increased the average schooling by between 0.2–0.3 years. These magnitudes are slightly smaller than those reported in Table 2.9. The policy did not influence the probability of selecting a specific employment sector as revealed in the estimates of the 2SLS, although the signs are as anticipated, positive for the formal sector and negative for the self-employed and informal sector.<sup>45</sup>

<sup>&</sup>lt;sup>45</sup> Table B2.14 in Appendix B reports the results for the parametric analysis.

Table 2. 15: Non-parametric results of the employment sectoral choices: pooled sample

|                      |          |          |           | For         | mal employ  | yment (=1  | .)       |         |         |            |          |             |
|----------------------|----------|----------|-----------|-------------|-------------|------------|----------|---------|---------|------------|----------|-------------|
| Estimation method    |          | First    | -stage    | _           |             | Reduc      | ed-form  | _       |         | 2          | SLS      |             |
| Dependent variable   | _        | Years of | schooling | -           |             | Formal e   | mploymer | it      |         | Formal e   | mploymer | nt          |
|                      | (a)      | (b)      | (c)       | (d)         | (a)         | (b)        | (c)      | (d)     | (a)     | (b)        | (c)      | (d)         |
| Treatment            | 0.292**  | 0.260**  | 0.276**   | 0.236*      | 0.027*      | 0.025*     | 0.024    | 0.019   |         |            |          |             |
|                      | (0.174)  | (0.120)  | (0.131)   | (0.131)     | (0.016)     | (0.015)    | (0.016)  | (0.016) |         |            |          |             |
| Voars of schooling   |          | . ,      |           |             |             |            |          |         | 0.092   | 0.096      | 0.087    | 0.082       |
| rears of schooling   |          |          |           |             |             |            |          |         | (0.060) | (0.062)    | (0.063)  | (0.075)     |
|                      |          |          |           |             |             |            |          |         |         |            | . ,      | . ,         |
| Obs.                 | 148,964  | 148,964  | 148,964   | 148,964     | 148,964     | 148,964    | 148,964  | 148,964 | 148,964 | 148,964    | 148,964  | 148,964     |
| Eff. Number of obs.  | 51,109   | 55,293   | 46,850    | 44,768      | 51,109      | 55,293     | 46,850   | 40,590  | 51,109  | 55,293     | 46,850   | 40,590      |
| Bandwidth            | 37.66    | 42.48    | 35.31     | 33.61       | 37.66       | 42.48      | 35.31    | 30.97   | 37.66   | 42.48      | 35.31    | 30.97       |
| Survey year dummies  | No       | Yes      | Yes       | Yes         | No          | Yes        | Yes      | Yes     | No      | Yes        | Yes      | Yes         |
| Birth region dummies | No       | No       | Yes       | Yes         | No          | No         | Yes      | Yes     | No      | No         | Yes      | Yes         |
| Urban status         | No       | No       | No        | Yes         | No          | No         | No       | Yes     | No      | No         | No       | Yes         |
|                      |          |          |           | Info        |             |            | 1)       |         |         |            |          |             |
|                      |          |          |           | Into        | rmai empio  | yment (-   | 1)       |         |         |            |          |             |
| Estimation method    | _        | First    | -stage    | -           |             | Reduc      | ed-form  | -       |         | 2          | SLS      | -           |
| Dependent variable   |          | Years of | schooling |             |             | Informal e | employme | nt      |         | Informal e | mployme  | nt          |
|                      | (a)      | (b)      | (c)       | (d)         | (a)         | (b)        | (c)      | (d)     | (a)     | (b)        | (c)      | (d)         |
| Treatment            | 0.297**  | 0.266**  | 0.277**   | 0.225*      | -0.027*     | -0.025*    | -0.024   | -0.019  |         |            |          |             |
|                      | (0.128)  | (0.121)  | (0.132)   | (0.132)     | (0.016)     | (0.015)    | (0.016)  | (0.016) |         |            |          |             |
| Veam of achaoling    | (0.1220) | (0.222)  | (01202)   | (0.202)     | (0.010)     | (0.010)    | (0.020)  | (01020) | -0.091  | -0 094     | -0.085   | -0.084      |
| rears of schooling   |          |          |           |             |             |            |          |         | (0.059) | (0.060)    | (0.063)  | (0.079)     |
|                      |          |          |           |             |             |            |          |         | (/      | (0.000)    | (/       | (           |
| Obs.                 | 148,964  | 148,964  | 148,964   | 148,964     | 148,964     | 148,964    | 148,964  | 148,964 | 148,964 | 148,964    | 148,964  | 148,964     |
| Eff. Number of obs.  | 48,960   | 53,259   | 44,768    | 42,719      | 48,960      | 53,259     | 44,768   | 42,719  | 48,960  | 53,259     | 44,768   | 42,719      |
| Bandwidth            | 37.66    | 42.48    | 35.31     | 33.606      | 37.66       | 42.48      | 35.31    | 33.606  | 37.66   | 42.48      | 35.31    | 33.606      |
| Survey year dummies  | No       | Yes      | Yes       | Yes         | No          | Yes        | Yes      | Yes     | No      | Yes        | Yes      | Yes         |
| Birth region dummies | No       | No       | Yes       | Yes         | No          | No         | Yes      | Yes     | No      | No         | Yes      | Yes         |
| Urban status         | No       | No       | No        | Yes         | No          | No         | No       | Yes     | No      | No         | No       | Yes         |
|                      |          |          |           | _           |             |            |          |         |         |            |          |             |
|                      |          |          |           | Se          | elt-employn | nent (=1)  |          |         |         |            |          |             |
| Estimation method    | _        | First    | -stage    | _           |             | Reduc      | ed-form  | _       |         | 2          | SLS      | _           |
| Dependent variable   | ( )      | Years of | schooling | <i>(</i> 1) |             | Self-em    | ployment | 6.05    |         | Self-em    | ployment | <i>(</i> 1) |
|                      | (a)      | (0)      | (C)       | (a)         | (a)         | (D)        | (C)      | (a)     | (a)     | (0)        | (C)      | (a)         |
| Treatment            | 0.267**  | 0 287**  | 0 278**   | 0 203*      | -0.000      | -0.002     | -0.001   | 0.000   |         |            |          |             |
| medement             | (0 122)  | (0.126)  | (0 132)   | (0 117)     | (0.011)     | (0.011)    | (0.012)  | (0.011) |         |            |          |             |
|                      | (0.122)  | (0.120)  | (0.102)   | (0.117)     | (0.011)     | (0.011)    | (0.012)  | (0.011) | -0.002  | -0.005     | -0.004   | 0.000       |
| Years of schooling   |          |          |           |             |             |            |          |         | (0.044) | (0.044)    | (0.047)  | (0.057)     |
|                      |          |          |           |             |             |            |          |         | (0.044) | (0.044)    | (0.047)  | (0.037)     |
| Obs.                 | 148,964  | 148,964  | 148,964   | 148,964     | 148,964     | 148,964    | 148,964  | 148,964 | 148,964 | 148,964    | 148,964  | 148,964     |
| Eff. Number of obs.  | 53,259   | 51,109   | 46,850    | 53,259      | 53,259      | 51,109     | 46,850   | 53,259  | 53,259  | 51,109     | 46,850   | 53,259      |
| Bandwidth            | 42.06    | 38.93    | 34.98     | 42.151      | 42.06       | 38.93      | 34.98    | 42.151  | 42.06   | 38.93      | 34.98    | 42.151      |
| Survey year dummies  | No       | Yes      | Yes       | Yes         | No          | Yes        | Yes      | Yes     | No      | Yes        | Yes      | Yes         |
| Birth region dummies | No       | No       | Yes       | Yes         | No          | No         | Yes      | Yes     | No      | No         | Yes      | Yes         |
| Urban status         | No       | No       | No        | Yes         | No          | No         | No       | Yes     | No      | No         | No       | Yes         |
|                      | ***      |          |           |             |             |            |          |         |         |            |          |             |

Notes: \*p<0.1, \*\* p<0.05, \*\*\* p<0.01

The sample is constructed from the 2009-2017 Mexican National Occupations and Employment Survey. Following Calonico et al. (2018) and Calonico et al. (2014) for the optimal bandwidth. Robust standard errors using EHW correction as recommended by Kolesár and Rothe (2018) in parentheses.

#### Variations in years of schooling and trimming earnings

The policy increased average schooling in a range of 0.21-0.29 of a year, in a sample that does not trim earnings and includes only observations with less than 18 years of schooling (see columns a–c of Table 2.16). These estimates, similar to the ones reported in Table 2.9, did not have causal effects on the employment sectoral choices. The results considering a sample that only trims earnings are presented in columns d–f in the same Table 2.16. The policy increased years of schooling, on average, between 0.25–0.32 years, and consistent with previous results did not impact employment choices. <sup>46</sup>

#### a) Parametric analysis

#### Additional windows employed

The analysis is conducted with 39 –and 48–month windows. Nevertheless, only the estimates with a 39–month window, reported in Table 2.17, reveal significant effects in the first stage. School attendance increases in the range of 0.14 and 0.17 years, similar to the parameters reported previously in Table 2.14, and it did not influence the employment sector choices.<sup>47</sup>

The general, the results presented in this section are consistent again with the estimates reported for the core analysis in section 2.6.2. The 1993 reform did not influence the decision of working in a specific employment sector, although it increased school attendance.

<sup>&</sup>lt;sup>46</sup> Table B2.15 in Appendix B reports the estimates for the parametric analysis considering similar samples.

<sup>&</sup>lt;sup>47</sup> Table B2.16 in Appendix B reports the results for a 39-months windows with the use of other sub-samples.

# Table 2. 16: Non-parametric results of the employment sectoral choices: variations in schooling and earnings

|                           |              |          |           |            |            |            | Formal em  | ploymer    | ıt (=1)    |              |           |               |              |          |           |            |           |          |
|---------------------------|--------------|----------|-----------|------------|------------|------------|------------|------------|------------|--------------|-----------|---------------|--------------|----------|-----------|------------|-----------|----------|
| Estimation method         |              |          | First     | -stage     |            |            |            |            | Reduce     | ed-form      |           |               |              |          | 25        | SLS        |           |          |
| Dependent variable        | -            |          | Years of  | schooling  |            | -          |            |            | Formal er  | nploymen     | t         | -             |              |          | Formal en | nploymer   | it        | -        |
|                           | (a)          | (b)      | (c)       | (d)        | (e)        | (f)        | (a)        | (Ь)        | (c)        | (d)          | (e)       | (f)           | (a)          | (b)      | (c)       | (d)        | (e)       | (f)      |
| Treatment                 | 0.29/**      | 0 27/**  | 0.229*    | 0 377**    | 0 282**    | 0.259**    | 0.024      | 0.021      | 0.017      | 0.024        | 0.022     | 0.018         |              |          |           |            |           |          |
| rieschiene                | (0.138)      | (0.134)  | (0132)    | (0137)     | (0127)     | (0127)     | (0.017)    | (0.017)    | (0.017)    | (0.017)      | (0.016)   | (0.016)       |              |          |           |            |           |          |
| Vegre of echooling        | (0.150)      | (0.134)  | (0.252)   | (0.257)    | (0.227)    | (0.227)    | (0.027)    | (0.017)    | (0.027)    | (0.017)      | (0.010)   | (0.010)       | 0.087        | 0.075    | 0.073     | 0.075      | 0.080     | 0.068    |
| rears of schooling        |              |          |           |            |            |            |            |            |            |              |           |               | (0.062)      | (0.063)  | (0.077)   | (0.055)    | (0.060)   | (0.067)  |
|                           |              |          |           |            |            |            |            |            |            |              |           |               | (0.002)      | (0.003)  | (0.077)   | (0.000)    | (0.000)   | (0.007)  |
| Obs.                      | 146,954      | 146,954  | 146,954   | 147,015    | 147,015    | 147,015    | 146,954    | 146,954    | 146,954    | 147,015      | 147,015   | 147,015       | 146,954      | 146,954  | 146,954   | 147,015    | 147,015   | 147,015  |
| Eff. Number of obs.       | 42,117       | 44,132   | 42,117    | 42,123     | 48,296     | 44,151     | 42,117     | 44,132     | 42,117     | 42,123       | 48,296    | 44,151        | 37,986       | 44,132   | 42,117    | 42,123     | 48,296    | 44,151   |
| Bandwidth                 | 31.68        | 33.24    | 32.08     | 33.39      | 37.46      | 34.02      | 31.68      | 33.24      | 32.08      | 33.39        | 37.46     | 34.02         | 29.33        | 33.24    | 32.08     | 33.39      | 37.46     | 34.02    |
| Less than 18 years of     |              |          |           |            |            |            |            |            |            |              |           |               |              |          |           |            |           |          |
| schooling                 | Yes          | Yes      | Yes       | No         | No         | No         | Yes        | Yes        | Yes        | NO           | NO        | NO            | Yes          | Yes      | Yes       | No         | No        | No       |
| Trimmed hourly wages      | No           | No       | No        | Yes        | Yes        | Yes        | No         | No         | No         | Yes          | Yes       | Yes           | No           | No       | No        | Yes        | Yes       | Yes      |
| Survey year and birth     |              |          |           |            |            |            |            |            |            |              |           |               |              |          |           |            |           |          |
| region dummies            | NO           | res      | res       | NO         | res        | res        | NO         | res        | res        | NO           | res       | res           | NO           | res      | res       | NO         | res       | res      |
| Urban status              | No           | No       | Yes       | No         | No         | Yes        | No         | No         | Yes        | No           | No        | Yes           | No           | No       | Yes       | No         | No        | Yes      |
|                           |              |          |           |            |            |            |            |            |            |              |           |               |              |          |           |            |           |          |
|                           |              |          |           |            |            | 1          | nformal er | nployme    | nt (=1)    |              |           |               |              |          |           |            |           |          |
| Estimation method         |              |          | First     | stare      |            |            |            |            | Reduc      | ed-form      |           |               |              |          | 25        | sis        |           |          |
| Dependent variable        | -            |          | Years of  | schooling  |            | -          |            |            | nformal e  | molovme      | nt        | -             |              |          | nformal e | molovme    | nt        | -        |
| Dependent vanable         | (a)          | (b)      | (c)       | (d)        | (e)        | (f)        | (a)        | (ь)        | (c)        | (d)          | (e)       | (f)           | (a)          | (ь)      | (c)       | (d)        | (e)       | (f)      |
| Treatment                 | 0.270*       | 0.259*   | 0.214*    | 0.298**    | 0.284**    | 0.252*     | -0.024     | -0.020     | -0.016     | -0.023       | -0.022    | -0.017        | 1-1          | 1-1      |           |            | (-)       |          |
|                           | (0.141)      | (0.135)  | (0.134)   | (0.140)    | (0.127)    | (0.132)    | (0.017)    | (0.016)    | (0.017)    | (0.017)      | (0.015)   | (0.016)       |              |          |           |            |           |          |
| Years of schooling        |              |          |           |            |            |            |            |            |            |              |           | · · ·         | -0.089       | -0.078   | -0.076    | -0.079     | -0.078    | -0.068   |
|                           |              |          |           |            |            |            |            |            |            |              |           |               | (0.069)      | (0.067)  | (0.084)   | (0.060)    | (0.059)   | (0.069)  |
|                           |              |          |           |            |            |            |            |            |            |              |           |               |              |          |           |            |           |          |
| Obs.                      | 146.954      | 146,954  | 146,954   | 147.015    | 147.015    | 147.015    | 146.954    | 146,954    | 146.954    | 147.015      | 147.015   | 147.015       | 146.954      | 146.954  | 146.954   | 147.015    | 147.015   | 147.015  |
| Eff. Number of obs        | 37.986       | 42 117   | 40.020    | 40.017     | 48.296     | 42 1 23    | 37 986     | 42 117     | 40.020     | 40.017       | 48.296    | 42 123        | 37 986       | 42 1 17  | 40.020    | 40.017     | 48.296    | 42 123   |
| Bandwidth                 | 31.68        | 32.24    | 32.08     | 22.29      | 37.46      | 34.02      | 31.68      | 32.24      | 32.08      | 22.29        | 37.46     | 34.02         | 31.68        | 33.24    | 32.08     | 22 29      | 37.46     | 34.02    |
| Less than 18 years of     | 51.00        | 33.24    | 52.00     | 55.55      | 57.40      | 34.02      | 51.00      | 33.24      | 52.00      | 55.55        | 37.40     | 34.02         | 51.00        | 33.24    | 52.00     |            | 57.40     | 34.02    |
| schooling                 | Yes          | Yes      | Yes       | No         | No         | No         | Yes        | Yes        | Yes        | No           | No        | No            | Yes          | Yes      | Yes       | No         | No        | No       |
| Trimmed hourly wages      | No           | No       | No        | Yes        | Yes        | Yes        | No         | No         | No         | Yes          | Yes       | Yes           | No           | No       | No        | Yes        | Yes       | Yes      |
| Survey year and birth     |              |          |           |            |            |            |            |            |            |              |           |               |              |          |           |            |           |          |
| region dummies            | No           | Yes      | Yes       | No         | Yes        | Yes        | No         | Yes        | Yes        | No           | Yes       | Yes           | No           | Yes      | Yes       | No         | Yes       | Yes      |
| Urban status              | No           | No       | Yes       | No         | No         | Yes        | No         | No         | Yes        | No           | No        | Yes           | No           | No       | Yes       | No         | No        | Yes      |
|                           |              |          |           |            |            |            |            |            |            |              |           |               |              |          |           |            |           |          |
|                           |              |          |           |            |            | Robu       | ust metho  | d: Self-en | ploymen    | t            |           |               |              |          |           |            |           |          |
| Estimation method         |              |          | Einet     |            |            |            |            |            | Peduc      | ad form      |           |               |              |          | ~         | 212        |           |          |
| Dependent variable        | -            |          | Vears of  | schooling  |            | -          |            |            | Self-em    | nlovment     |           | -             |              |          | Self-em   | nlovment   |           | -        |
| Dependent variable        | (=)          | (b)      | (c)       | (d)        | (e)        | (f)        | (=)        | (b)        | (c)        | (d)          | (e)       | (f)           | (=)          | (b)      | (c)       | (d)        | (e)       | (f)      |
| Trantmont                 | 0 795**      | 0 727**  | 0 7 7 0*  | 0 2 2 2 ** | 0.290**    | 0 247**    | 0.002      | 0.002      | 0.002      | 0.000        | 0.000     | 0.000         | (9/          | (0)      | (~/       | (0)        | (-/       | W        |
| reatment                  | (0.141)      | (0.117)  | (0.117)   | (0.120)    | (0.127)    | (0.124)    | (0.012)    | (0.010)    | (0.012)    | (0.012)      | (0.011)   | (0.012)       |              |          |           |            |           |          |
|                           | (0.141)      | (0.117)  | (0.117)   | (0.156)    | (0.127)    | (0.124)    | (0.012)    | (0.010)    | (0.012)    | (0.012)      | (0.011)   | (0.012)       | 0.010        | 0.000    | 0.000     | 0.002      | 0.001     | 0.004    |
| Years of schooling        |              |          |           |            |            |            |            |            |            |              |           |               | -0.010       | -0.008   | -0.005    | (0.002     | (0.045)   | (0.004   |
|                           |              |          |           |            |            |            |            |            |            |              |           |               | (0.040)      | (0.040)  | (0.000)   | (0.042)    | (0.045)   | (0.033)  |
| Obs.                      | 146,954      | 146,954  | 146,954   | 147,015    | 147,015    | 147,015    | 146,954    | 146,954    | 146,954    | 147,015      | 147,015   | 147,015       | 146,954      | 146,954  | 146,954   | 147,015    | 147,015   | 147,015  |
| Eff. Number of obs.       | 40.020       | 52,512   | 48.268    | 42.123     | 50.429     | 48.296     | 40.020     | 52.512     | 48.268     | 42.123       | 50.429    | 48.296        | 40.020       | 52.512   | 48.268    | 42.123     | 50.429    | 48.296   |
| Bandwidth                 | 31.48        | 44.12    | 38.27     | 32.38      | 37.59      | 37.12      | 31.48      | 44.12      | 38.27      | 32.38        | 37.59     | 37.12         | 31.48        | 44.12    | 38.27     | 32.38      | 37.59     | 37.12    |
| Lessthan 18 years of      |              |          |           |            |            |            |            |            |            |              |           |               |              |          |           |            |           |          |
| schooling                 | Yes          | Yes      | Yes       | No         | No         | No         | Yes        | Yes        | Yes        | No           | No        | No            | Yes          | Yes      | Yes       | No         | No        | No       |
| Trimmed hourly wages      | No           | No       | No        | Yes        | Yes        | Yes        | No         | No         | No         | Yes          | Yes       | Yes           | No           | No       | No        | Yes        | Yes       | Yes      |
| Survey year and birth     |              |          |           |            |            | N          |            |            | N.         |              |           | N             |              |          |           |            |           |          |
| region dummies            | No           | Yes      | Yes       | No         | Yes        | Yes        | No         | Yes        | Yes        | No           | Yes       | Yes           | No           | Yes      | Yes       | No         | Yes       | Yes      |
| Urban status              | No           | No       | Yes       | No         | No         | Yes        | No         | No         | Yes        | No           | No        | Yes           | No           | No       | Yes       | No         | No        | Yes      |
| Notes: *p<0.1, ** p<0.05  | i, *** p<0.0 | 01       |           |            |            |            |            |            |            |              |           |               |              |          |           |            |           |          |
| The sample is constructed | from the     | 2009-201 | 7 Mexican | National   | Occupation | s and Empl | loyment Su | vey. Follo | wing Calon | ico et al. ( | 2018) and | l Calonico et | al. (2014) f | ortheopt | imal band | width. Rob | ust stand | ardemors |

The sample is constructed from the 2009-2017 Mexican National Occupations and Employment Survey. Following Calonico et al. (2018) and Calonico et al. (2014) for the optimal bandwidth. Robust standard errors using EHW correction as recommended by Kolesár and Rothe (2018) in parentheses.

|                       |         |         |          |                  |         |         | Formal e | mploym  | nent (=1)  | )         |         |         |         |           |           |           |         |         |
|-----------------------|---------|---------|----------|------------------|---------|---------|----------|---------|------------|-----------|---------|---------|---------|-----------|-----------|-----------|---------|---------|
| Estimation method     |         |         | First    | -stage           |         |         |          |         | Reduce     | ed-form   |         |         |         |           | 2         | SLS       |         |         |
| Dependent variable    | -       |         | Years of | schooling        |         | -       |          | 1       | Formal en  | nploymer  | nt      | -       |         |           | Formal er | mp loy me | nt      | -       |
|                       | (a)     | (b)     | (c)      | (d)              | (e)     | (f)     | (a)      | (b)     | (C)        | (d)       | (e)     | (f)     | (a)     | (b)       | (c)       | (d)       | (e)     | (f)     |
| Treatment             | 0.166*  | 0 161*  | 0143*    | 0 169 **         | 0 162*  | 0 145*  | 0 014    | 0.013   | 0.011      | 0.011     | 0.011   | 0.009   |         |           |           |           |         |         |
|                       | (0.085) | (0.085) | (0.083)  | (0.085)          | (0.084) | (0.082) | (0.010)  | (0.010) | (0.010)    | (0 0 1 1) | (0.010) | (0.010) |         |           |           |           |         |         |
| Vears of schooling    | (,      | (,      | (,       | (,               | (       | (,      | (/       | (/      | (,         | (,        | (,      | (/      | 0.082   | 0.081     | 0.079     | 0.068     | 0.067   | 0.064   |
| rears of schooling    |         |         |          |                  |         |         |          |         |            |           |         |         | (0.062) | (0.063)   | (0.071)   | (0.060)   | (0.062) | (0.069) |
|                       |         |         |          |                  |         |         |          |         |            |           |         |         |         |           |           |           |         |         |
| Obs.                  | 82,125  | 82,125  | 82,125   | 79,969           | 79,969  | 79,969  | 82,125   | 82,125  | 82,125     | 79,969    | 79,969  | 79,969  | 82,125  | 82,125    | 82,125    | 79,969    | 79,969  | 79,969  |
| Less than 18 years of | No      | No      | No       | Ves              | Ves     | Vec     | No       | No      | No         | Vec       | Vec     | Ves     | No      | No        | No        | Ves       | Vec     | Vec     |
| schooling             | 140     | 140     | 140      | 103              | 103     | 10      | 140      | 140     | 140        | 10        | 103     | 103     | 140     | 140       | 140       | 10        | 10      | 103     |
| Trimmed hourly wages  | No      | No      | No       | Yes              | Yes     | Yes     | No       | No      | No         | Yes       | Yes     | Yes     | No      | No        | No        | Yes       | Yes     | Yes     |
| Survey year and birth | No      | Yes     | Yes      | No               | Yes     | Yes     | No       | Yes     | Yes        | No        | Yes     | Yes     | No      | Yes       | Yes       | No        | Yes     | Yes     |
| region dummies        |         |         |          |                  |         |         |          |         |            |           |         |         |         |           |           |           |         |         |
| Urban status          | NO      | NO      | res      | NO               | NO      | res     | NO       | NO      | 16         | NO        | NO      | res     | NO      | NO        | res       | NO        | NO      | res     |
|                       |         |         |          |                  |         |         |          |         |            |           |         |         |         |           |           |           |         |         |
|                       |         |         |          |                  |         |         | Informal | employr | nent (=1   | .)        |         |         |         |           |           |           |         |         |
| Estimation method     | _       |         | First    | -stage           |         | -       |          |         | Reduce     | ed-form   |         | -       |         |           | 2         | SLS       |         | _       |
| Dependent variable    |         |         | Years of | schooling        |         |         |          | Ir      | nformal er | mployme   | nt      |         |         | li<br>a s | nformal e | mploym    | ent     |         |
|                       | (a)     | (b)     | (C)      | (d)              | (e)     | (f)     | (a)      | (b)     | (C)        | (d)       | (e)     | (f)     | (a)     | (b)       | (c)       | (d)       | (e)     | (f)     |
| Treatment             | 0.166*  | 0.161*  | 0.143*   | 0.169 **         | 0.162*  | 0.145*  | -0.014   | -0.013  | -0.011     | -0.011    | -0.011  | -0.009  |         |           |           |           |         |         |
|                       | (0.085) | (0.085) | (0.083)  | (0.085)          | (0.084) | (0.082) | (0.010)  | (0.010) | (0.010)    | (0.011)   | (0.011) | (0.010) |         |           |           |           |         |         |
| Years of schooling    |         |         |          |                  |         |         |          |         |            |           |         |         | -0.082  | -0.081    | -0.079    | -0.068    | -0.067  | -0.064  |
|                       |         |         |          |                  |         |         |          |         |            |           |         |         | (0.062) | (0.063)   | (0.071)   | (0.060)   | (0.062) | (0.069) |
|                       |         |         |          |                  |         |         |          |         |            |           |         |         |         |           |           |           |         |         |
| Obs.                  | 82,125  | 82,125  | 82,125   | 79,969           | 79,969  | 79,969  | 82,125   | 82,125  | 82,125     | 79,969    | 79,969  | 79,969  | 82,125  | 82,125    | 82,125    | 79,969    | 79,969  | 79,969  |
| Less than 18 years of | No      | No      | No       | Vec              | Vec     | Vec     | No       | No      | No         | Vec       | Vec     | Vec     | No      | No        | No        | V~~       | Vec     | Vec     |
| schooling             | NO      | NO      | NO       | 165              | 165     | ies     | NO       | NO      | NO         | res       | 165     | ies -   | NO      | NO        | NO        | 16        | Tes     | res     |
| Trimmed hourly wages  | No      | No      | No       | Yes              | Yes     | Yes     | No       | No      | No         | Yes       | Yes     | Yes     | No      | No        | No        | Yes       | Yes     | Yes     |
| Survey year and birth | No      | Yes     | Yes      | No               | Yes     | Yes     | No       | Yes     | Yes        | No        | Yes     | Yes     | No      | Yes       | Yes       | No        | Yes     | Yes     |
| region dummies        |         |         |          |                  |         |         |          |         |            |           |         |         |         |           |           |           |         |         |
| Urban status          | No      | No      | Yes      | No               | No      | Yes     | No       | No      | Yes        | No        | No      | Yes     | No      | No        | Yes       | No        | No      | Yes     |
|                       |         |         |          |                  |         |         |          |         |            |           |         |         |         |           |           |           |         |         |
|                       |         |         |          |                  |         |         | Self-en  | nployme | nt (=1)    |           |         |         |         |           |           |           |         |         |
| Estimation method     | _       |         | First    | -stage           |         | -       |          |         | Reduce     | ed-form   |         | -       |         |           | 2         | SLS       |         | -       |
| Dependent variable    | (-)     | (b)     | Years of | schooling<br>(d) | (0)     | (#)     | (-)      | (b)     | ntormal er | mployme   | nt (a)  | (#)     | (-)     | (b)       | ntormal e | mployme   | ent (c) | (#)     |
|                       | (q)     | (0)     | (C)      | (u)              | (=)     | (1)     | (9)      | (0)     | (C)        | (u)       | (9)     | (0)     | (q)     | (0)       | (0)       | (u)       | (=)     | (1)     |
| Treatment             | 0.166*  | 0.161*  | 0.143*   | 0.169 **         | 0.162*  | 0.145*  | -0.003   | -0.003  | -0.003     | -0.006    | -0.006  | -0.005  |         |           |           |           |         |         |
| ~                     | (0.085) | (0.085) | (0.083)  | (0.085)          | (0.084) | (0.082) | (0.008)  | (0.008) | (0.008)    | (0.008)   | (0.008) | (0.008) |         | _         | _         | _         |         |         |
| Years of schooling    |         |         |          |                  |         |         |          |         |            |           |         |         | -0.017  | -0.017    | -0.018    | -0.034    | -0.035  | -0.038  |
|                       |         |         |          |                  |         |         |          |         |            |           |         |         | (0.047) | (0.048)   | (0.054)   | (0.048)   | (0.050) | (0.056) |
| Obs.                  | 82,125  | 82,125  | 82,125   | 79,969           | 79,969  | 79,969  | 82,125   | 82,125  | 82,125     | 79,969    | 79,969  | 79,969  | 82,125  | 82,125    | 82,125    | 79,969    | 79,969  | 79,969  |
| Less than 18 years of | No      | Ne      | No       | Vec              | Vec     | Voc     | Ne       | No      | Ne         | Vec       | Vec     | Vec     | Ne      | Ne        | Ne        | Vor       | Vec     | Vec     |
| schooling             | NO      | NO      | INO      | res              | res     | 165     | INO      | NO      | NO         | Tes       | Tes     | TES     | NO.     | NO        | INO       | 16        | TES     | TES     |
| Trimmed hourly wages  | No      | No      | No       | Yes              | Yes     | Yes     | No       | No      | No         | Yes       | Yes     | Yes     | No      | No        | No        | Yes       | Yes     | Yes     |
| Survey year and birth | No      | Yes     | Yes      | No               | Yes     | Yes     | No       | Yes     | Yes        | No        | Yes     | Yes     | No      | Yes       | Yes       | No        | Yes     | Yes     |
| region dummies        |         |         |          |                  |         |         |          |         |            |           |         |         |         |           |           |           |         |         |
| Urban status          | No      | No      | Yes      | No               | No      | Yes     | No       | No      | Yes        | No        | No      | Yes     | No      | No        | Yes       | No        | No      | Yes     |

Notes: \*p<0.1, \*\* p<0.05, \*\*\* p<0.01

The sample is constructed from the 2009-2017 Mexican National Occupations and Employment Survey. Robust standard errors using EHW correction as recommended by Kolesár and Rothe (2018) in parentheses.

### 2.7.3 Placebo Test

The placebo in time tests estimates jumps at points where there should be no discontinuities. The analyses are conducted for non-parametric and parametric methodologies. The results are not statistically significant as expected. Therefore, there are no other discontinuities on the variables tested besides the ones generated by the 1993 educational reform. The selected results are presented below.

# a) Returns to Compulsory Education

#### Non-parametric analysis

Table 2.18 presents the results of a placebo test that uses a false 12-months cut-off before and after the original implementation of the 1993 reform. The birth cohorts tested were born before and after September 1980 and 1982. The analysis uses 75-months on either side of the threshold and reveals no statistically significant coefficients on schooling or earnings. Thus, other birth cohorts, rather than the ones affected by the reform, are not being impacted by the educational intervention established in 1993.<sup>48</sup>

#### Parametric analysis

A 24-months cut-off before and after the original introduction of the policy in 1993 is tested with a sample of a 42-months window. The birth cohorts tested were born before and after September 1979 and 1983. The results again, are not statistically significant as anticipated and are reported in Table 2.19.<sup>49</sup>

<sup>&</sup>lt;sup>48</sup> To test if the policy could have effects far from the birth cohorts subjected to this reform, a longer cut-off of 48-months from the original implementation of the 1993 reform is tested. This analysis includes birth cohorts that born before and after September 1977 and 1985. The results are reported in Table B2.17 in Appendix B.

<sup>&</sup>lt;sup>49</sup> A 48-months cut-off from the original implementation of the 1993 reform is tested with a 75-months window for the birth cohorts born before and after September 1975 and 1987. The results are reported in Table B2.18 in Appendix B.

| Table 2.  | 18: Placebo te | st. Birth | cohorts borr | n 12 months | s before and | l after the  | 1993 cut-off  |
|-----------|----------------|-----------|--------------|-------------|--------------|--------------|---------------|
| 1 uoic 2. | 10.11000000    | or. Dhui  | conorto com  | 1 12 monun  | , ourore une | a uniter the | 1)))) out off |

|                       |         |          |           |          |            |           |           | -       |         |           |           |         |
|-----------------------|---------|----------|-----------|----------|------------|-----------|-----------|---------|---------|-----------|-----------|---------|
| Estimation method     |         | First    | -stage    |          |            | Reduce    | ed-form   |         |         | 2         | SLS       |         |
| Dependent variable    | -       | Years of | schooling | <u>,</u> |            | Log of ho | urly wage | s       |         | Log of ho | urly wage | s       |
|                       | (a)     | (b)      | (c)       | (d)      | (a)        | (b)       | (c)       | (d)     | (a)     | (b)       | (c)       | (d)     |
| Treatment             | 0.213   | 0.103    | 0.195     | 0.116    | 0.012      | 0.000     | 0.01      | 0.011   |         |           |           |         |
|                       | (0.140) | (0.133)  | (0.140)   | (0.134)  | (0.021)    | (0.020)   | (0.020)   | (0.019) |         |           |           |         |
| Years of schooling    |         |          |           |          |            |           |           |         | 0.056   | 0.002     | 0.094     | 0.096   |
| Ū                     |         |          |           |          |            |           |           |         | (0.091) | (0.176)   | (0.088)   | (0.140) |
| Obs.                  | 148.964 | 146.954  | 147.015   | 145.035  | 148.964    | 146.954   | 147.015   | 145.035 | 148,964 | 146.954   | 147.015   | 145.035 |
| Eff. Number of obs    | 39 977  | /1 37/   | /1 /73    | /2 781   | 39 972     | /1 37/    | 11 / 73   | /2 781  | 39 972  | /1 37/    | 11 173    | /2 781  |
| Bandwidth             | 31.91   | 32,887   | 31.87     | 33.23    | 31.91      | 32,887    | 31.87     | 33.23   | 31.91   | 32,887    | 31.87     | 33.23   |
| Less than 18 years of | 51.51   | 52.007   | 51.07     | 55.25    | 51.51      | 52.007    | 51.07     | 55125   | 51.51   | 52.007    | 51.07     | 00.20   |
| schooling             | No      | Yes      | No        | Yes      | No         | Yes       | No        | Yes     | No      | Yes       | No        | Yes     |
| Trimmed hourly        |         |          |           |          |            |           |           |         |         |           |           |         |
| wages                 | No      | No       | Yes       | Yes      | No         | No        | Yes       | Yes     | Nó      | Nó        | Yes       | Yes     |
| Survey year and birth |         | N        | N         | N        |            |           |           |         |         | N         | N         | N       |
| region dummies        | NO      | res      | res       | res      | NO         | res       | res       | Yes     | NO      | res       | res       | res     |
|                       |         |          |           |          |            |           |           |         |         |           |           |         |
|                       |         |          | Bi        | rth coho | ort born S | epteml    | ber 198   | 2       |         |           |           |         |
| Estimation method     | _       | First    | -stage    | _        |            | Reduce    | ed-form   | _       |         | 2         | SLS       |         |
| Dependent variable    | _       | Years of | schooling | 5        |            | Log of ho | urly wage | s       |         | Log of ho | urly wage | 2S      |
|                       | (a)     | (b)      | (c)       | (d)      | (a)        | (b)       | (c)       | (d)     | (a)     | (b)       | (c)       | (d)     |
| Treatment             | 0.027   | 0.054    | 0.006     | 0.035    | -0.008     | 0.014     | 0.003     | 0.013   |         |           |           |         |
|                       | (0.142) | (0.123)  | (0.129)   | (0.122)  | (0.022)    | (0.019)   | (0.018)   | (0.017) |         |           |           |         |
| Years of schooling    | (,      | (,       | (,        | (,       | (,         | (,        | (/        | (,      | -0.314  | 0.259     | 0.336     | 0.392   |
|                       |         |          |           |          |            |           |           |         | (2.127) | (0.497)   | (1.44)    | (1.007) |
|                       |         |          |           |          |            |           |           |         | (,      | (/        | (,        | (/      |
| Obs.                  | 148,964 | 146,954  | 147,015   | 145,035  | 148,964    | 146,954   | 147,015   | 145,035 | 148,964 | 146,954   | 147,015   | 145,035 |
| Eff. Number of obs.   | 38,563  | 48,402   | 46,248    | 49,903   | 38,563     | 48,402    | 46,248    | 49,903  | 38,563  | 48,402    | 46,248    | 49,903  |
| Bandwidth             | 29.12   | 36.247   | 34.43     | 37.39    | 29.12      | 36.247    | 34.43     | 37.39   | 29.12   | 36.247    | 34.43     | 37.39   |
| Less than 18 years of | No      | Yes      | No        | Yes      | No         | Yes       | No        | Yes     | No      | Yes       | No        | Yes     |
| schooling             | 110     | 105      | 110       | 105      |            | 105       |           | 105     |         | 105       | 110       | 105     |
| Trimmed hourly        | No      | No       | Yes       | Yes      | No         | No        | Yes       | Yes     | No      | No        | Yes       | Yes     |
| wages                 |         |          |           |          |            |           |           |         |         |           |           |         |
| Survey year and birth | No      | Yes      | Yes       | Yes      | No         | Yes       | Yes       | Yes     | No      | Yes       | Yes       | Yes     |

Birth cohort born September 1980

Notes: \*p<0.1, \*\* p<0.05, \*\*\* p<0.01

region dummies

The sample is constructed from the 2009-2017 Mexican National Occupations and Employment Survey. Following Calonico et al. (2018) and Calonico et al. (2014) for the optimal bandwidth. Robust standard errors using EHW correction as recommended by Kolesár and Rothe (2018) in parentheses.

| Estimation method First-stage Reduced-form 2SIS |                               |          |          |          |           |           |          |         |                |           |          |         |  |
|---|-------------------------------|----------|----------|----------|-----------|-----------|----------|---------|----------------|-----------|----------|---------|--|
| Estimation method                               | First-stage Reduced-form 2SLS |          |          |          |           |           |          |         |                |           |          |         |  |
| Dependent variable                              | _                             | Years of | schoolin | g        | L         | og of ho  | urly wag | es      | l              | og of ho  | urly wag | es      |  |
|   | (a)                           | (b)      | (c)      | (d)      | (a)       | (b)       | (c)      | (d)     | (a)            | (b)       | (c)      | (d)     |  |
| Treatment                                       | 0.077                         | 0.076    | 0.044    | 0.032    | -0.003    | -0.003    | -0.000   | -0.002  |                |           |          |         |  |
|   | (0.112)                       | (0.109)  | (0.113)  | (0.109)  | (0.017)   | (0.016)   | (0.016)  | (0.015) |                |           |          |         |  |
| Years of schooling                              |                               |          |          |          |           |           |          |         | -0.042         | -0.051    | -0.013   | -0.056  |  |
| 0   |                               |          |          |          |           |           |          |         | (0.251)        | (0.254)   | (0.384)  | (0.595) |  |
| Obs.  | 88,22 <b>7</b>                | 87,011   | 87,090   | 85,890   | 88,227    | 87,011    | 87,090   | 85,890  | 88,22 <b>7</b> | 87,011    | 87,090   | 85,890  |  |
| Less than 18 years of schooling                 | No                            | Yes      | No       | Yes      | No        | Yes       | No       | Yes     | No             | Yes       | No       | Yes     |  |
| Trimmed hourly wages                            | No                            | No       | Yes      | Yes      | No        | No        | Yes      | Yes     | No             | No        | Yes      | Yes     |  |
| Survey year and birth region dummies            | No                            | Yes      | No       | Yes      | No        | Yes       | No       | Yes     | No             | Yes       | No       | Yes     |  |
|   |                               |          | Birth    | n cohort | t born Se | ptemb     | er 198   | 3       |                |           |          |         |  |
| Estimation method                               | _                             | First    | -stage   | _        |           | Reduce    | ed-form  | _       |                | 2         | SLS      | _       |  |
| Dependent variable                              |                               | Years of | schoolin | g        | L         | .og of ho | urly wag | es      | l              | .og of ho | urly wag | es      |  |
|   | (a)                           | (b)      | (c)      | (d)      | (a)       | (b)       | (c)      | (d)     | (a)            | (b)       | (c)      | (d)     |  |
| Treatment                                       | 0.028                         | 0.053    | 0.045    | 0.061    | 0.001     | 0.008     | 0.011    | 0.013   |                |           |          |         |  |
|   | (0.087)                       | (0.086)  | (0.087)  | (0.086)  | (0.013)   | (0.013)   | (0.012)  | (0.012) |                |           |          |         |  |
| Years of schooling                              |                               |          |          |          |           |           |          |         | 0.047          | 0.149     | 0.240    | 0.215   |  |
|   |                               |          |          |          |           |           |          |         | (0.424)        | (0.265)   | (0.428)  | (0.284) |  |
|   |                               |          |          |          |           |           |          |         |                |           |          |         |  |

| Table 2, 19; Pla | acebo test. Birth | cohort born 24 | months before and | 1 after the | 1993 cut-off |
|------------------|-------------------|----------------|-------------------|-------------|--------------|
| 14010 2. 17. 14  | accob test. Dhim  |                | months berore and | a union uno |              |

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4070

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|   |        |        |        |        |        |        |        |        | (0.424) | (0.265) | (0.428) | (0.284) |
|---|--------|--------|--------|--------|--------|--------|--------|--------|---------|---------|---------|---------|
| Obs.                                    | 88,227 | 87,011 | 87,090 | 85,890 | 88,227 | 87,011 | 87,090 | 85,890 | 88,227  | 87,011  | 87,090  | 85,890  |
| Less than 18 years of schooling         | No     | Yes    | No     | Yes    | No     | Yes    | No     | Yes    | No      | Yes     | No      | Yes     |
| Trimmed hourly wages                    | No     | No     | Yes    | Yes    | No     | No     | Yes    | Yes    | No      | No      | Yes     | Yes     |
| Survey year and birth<br>region dummies | No     | Yes    | Yes    | Yes    | No     | Yes    | Yes    | Yes    | No      | Yes     | Yes     | Yes     |

Notes: \*p<0.1, \*\* p<0.05, \*\*\* p<0.01

The sample is constructed from the 2009-2017 Mexican National Occupations and Employment Survey. Robust standard errors using EHW correction as recommended by Kolesár and Rothe (2018) in parentheses.

#### a) Effects on Employment Sectoral Choices

#### Non-parametric analysis

This section focuses on the analysis for the three dependent variables based on the selected employment sectors. The placebo test that uses a false 12-months birth cut-off before and after the implementation of the 1993 reform with 75-months on either side of the threshold is conducted. Tables 2.20 and 2.21 present the results, and these are not statistically significant.<sup>50</sup>

<sup>&</sup>lt;sup>50</sup> Tables B2.19 and B2.20 in Appendix B report the results of a 48-months cut-off before and after the introduction of the policy in 1993. The estimates reveal that the birth cohorts, not exposed to the reform, are not impacted by the educational intervention.

### Table 2. 20: Placebo test. Birth cohorts born 12 months before the 1993 cut-off point

|                       |         |          |           | FO      | mai empio   | yment (=.  | .,       |         |         |            |          |          |
|-----------------------|---------|----------|-----------|---------|-------------|------------|----------|---------|---------|------------|----------|----------|
| Estimation method     |         | First    | -stage    |         |             | Reduc      | ed-form  |         |         | 2          | SLS      |          |
| Dependent variable    | _       | Years of | schooling |         |             | Formaler   | nploymer | nt      |         | Formal er  | nploymer | nt       |
|                       | (a)     | (b)      | (c)       | (d)     | (a)         | (b)        | (c)      | (d)     | (a)     | (b)        | (c)      | (d)      |
| Treatment             | 0.222   | 0.085    | 0.183     | 0.122   | 0.016       | 0.008      | 0.013    | 0.012   |         |            |          |          |
|                       | (0.149) | (0.133)  | (0.140)   | (0.124) | (0.018)     | (0.017)    | (0.017)  | (0.016) |         |            |          |          |
| Years of schooling    | (/      | (/       | (/        | (,      | (/          | (,         | (,       | (/      | 0.069   | 0.097      | 0.070    | 0.096    |
| rear of bene only     |         |          |           |         |             |            |          |         | (0.106) | (0.171)    | (0.087)  | (0.123)  |
|                       |         |          |           |         |             |            |          |         | (,      | ()         | (,       | (/       |
| Obs.                  | 148,964 | 146,954  | 147,015   | 145,035 | 148,964     | 146,954    | 147,015  | 145,035 | 148,964 | 146,954    | 147,015  | 145,035  |
| Eff. Number of obs.   | 60,670  | 59,758   | 59,758    | 58,972  | 60,670      | 59,758     | 59,758   | 58,972  | 60,670  | 59,758     | 59,758   | 58,972   |
| Bandwidth             | 27.81   | 30.301   | 29.81     | 34.90   | 27.81       | 30.301     | 29.81    | 34.90   | 27.81   | 30.301     | 29.81    | 34.90    |
| Less than 18 years of | No      | Vor      | No        | Vor     | No          | Vor        | No       | Vor     | No      | Vor        | No       | Voc      |
| schooling             | NO      | 165      | NO        | 165     | NO          | 165        | 140      | 165     |         | 165        | 140      | 163      |
| Trimmed hourly wages  | No      | No       | Yes       | Yes     | No          | No         | Yes      | Yes     | No      | No         | Yes      | Yes      |
| Survey year and birth | No      | Yes      | Yes       | Yes     | No          | Yes        | Yes      | Yes     | No      | Yes        | Yes      | Yes      |
| region dummies        |         |          |           |         |             |            |          |         |         |            |          |          |
|                       |         |          |           |         |             |            |          |         |         |            |          |          |
|                       |         |          |           | Info    | rmal emplo  | yment (=   | 1)       |         |         |            |          |          |
| Estimation method     |         | First    | -stage    |         |             | Reduc      | ed-form  |         |         | 2          | SLS      |          |
| Dependent variable    |         | Years of | schooling | 5       | I           | Informal e | mp loyme | nt      |         | Informal e | mp loyme | nt       |
|                       | (a)     | (b)      | (c)       | (d)     | (a)         | (b)        | (c)      | (d)     | (a)     | (b)        | (c)      | (d)      |
| Treatment             | 0.225   | 0104     | 0 107     | 0 1 2 4 | 0.016       | 0.010      | 0.015    | 0.015   |         |            |          |          |
| Treatment             | 0.225   | 0.104    | 0.197     | 0.124   | -0.010      | -0.010     | -0.015   | -0.015  |         |            |          |          |
| V                     | (0.147) | (0.129)  | (0.144)   | (0.155) | (0.018)     | (0.017)    | (0.017)  | (0.017) | -0.071  | -0.098     | -0.075   | -0 118   |
| Years of schooling    |         |          |           |         |             |            |          |         | (0.081) | (0.159)    | (0.083)  | (0.144)  |
|                       |         |          |           |         |             |            |          |         | (0.001) | (0.100)    | (0.000)  | (0.2.1.) |
| Obs.                  | 148,964 | 146,954  | 147,015   | 145,035 | 148,964     | 146,954    | 147,015  | 145,035 | 148,964 | 146,954    | 147,015  | 145,035  |
| Eff. Number of obs.   | 42,011  | 41,374   | 41,473    | 40,843  | 42,011      | 41,374     | 41,473   | 40,843  | 42,011  | 41,374     | 41,473   | 40,843   |
| Bandwidth             | 27.81   | 30.301   | 29.81     | 34.90   | 27.81       | 30.301     | 29.81    | 34.90   | 27.81   | 30.301     | 29.81    | 34.90    |
| Less than 18 years of | No      | Voc      | No        | Voc     | No          | Vos        | No       | Voc     | No      | Vos        | No       | Vec      |
| schooling             |         | 105      | 140       | 105     | 100         | 105        | 140      | 105     | 140     | 105        | 140      | 105      |
| Trimmed hourly wages  | No      | No       | Yes       | Yes     | No          | No         | Yes      | Yes     | No      | No         | Yes      | Yes      |
| Survey year and birth | No      | Yes      | Yes       | Yes     | No          | Yes        | Yes      | Yes     | No      | Yes        | Yes      | Yes      |
| region dummies        |         |          | 105       | 105     |             |            | 105      |         |         |            | 105      |          |
|                       |         |          |           |         |             |            |          |         |         |            |          |          |
|                       |         |          |           | S       | elf-employn | nent (=1)  |          |         |         |            |          |          |
| Estimation method     |         | First    | -stage    |         |             | Reduc      | ed-form  |         |         | 2          | SLS      |          |
| Dependent variable    | _       | Years of | schooling | -       |             | Self-em    | ployment | -       |         | Self-em    | ployment | -        |
|                       | (a)     | (b)      | (c)       | (d)     | (a)         | (b)        | (c)      | (d)     | (a)     | (b)        | (c)      | (d)      |
| <b>T</b>              | 0.101   | 0.000    | 0.470     | 0.455   | 0.000       | 0.000      | 0.010    | 0.000   |         |            |          |          |
| Treatment             | 0.181   | 0.092    | 0.1/0     | 0.120   | 0.002       | 0.001      | 0.010    | 0.003   |         |            |          |          |
|                       | (0.137) | (0.122)  | (0.135)   | (0.119) | (0.013)     | (0.012)    | (0.020)  | (0.011) |         |            |          |          |

#### Birth cohort born September 1980

The sample is constructed from the 2009-2017 Mexican National Occupations and Employment Survey. Following Calonico et al. (2018) and Calonico et al. (2014) for the optimal bandwidth. Robust standard errors using EHW correction as recommended by Kolesár and Rothe (2018) in parentheses.

50,458

30.39

No

No

No

148,964 146,954 147,015 145,035

49,816

31.87

No

Yes

Yes

49,059

47.16

Yes

Yes

Yes

49,693

40.569

Yes

No

Yes

0.009

50,458

No

No

No

30.39

0.011

49,693

40.569

Yes

No

Yes

0.025

49,816

31.87

No

Yes

Yes

(0.073) (0.134) (0.072) (0.106)

148,964 146,954 147,015 145,035

0.021

49,059

47.16

Yes

Yes

Yes

Notes: \*p<0.1, \*\* p<0.05, \*\*\* p<0.01

Years of schooling

Eff. Number of obs.

Less than 18 years of

Trimmed hourly wages

Survey year and birth

Obs.

Bandwidth

schoo ling

148,964 146,954 147,015 145,035

49,816

31.87

No

Yes

Yes

49,059

47.16

Yes

Yes

Yes

50,458 49,693

40.569

Yes

No

Yes

30.39

No

No

No

region dummies

# Table 2. 21: Placebo test. Birth cohorts born 12 months after the 1993 cut-off point

|   |         |          |           | FUI     | nai empioy | ment (-   | -,       |         |          |           |          |         |
|---|---------|----------|-----------|---------|------------|-----------|----------|---------|----------|-----------|----------|---------|
| Estimation method                       |         | First    | -stage    |         |            | Reduce    | ed-form  |         |          | 2         | SLS      |         |
| Dependent variable                      |         | Years of | schooling | 3       | l l        | Formal en | nploymer | nt      |          | Formal er | nploymen | t       |
|   | (a)     | (b)      | (c)       | (d)     | (a)        | (b)       | (c)      | (d)     | (a)      | (b)       | (c)      | (d)     |
| Treatment                               | -0.026  | 0.031    | -0.000    | 0.030   | -0.012     | -0.007    | -0.017   | -0.005  |          |           |          |         |
|   | (0.125) | (0.109)  | (0.115)   | (0.119) | (0.015)    | (0.014)   | (0.125)  | (0.014) |          |           |          |         |
| Years of schooling                      |         |          |           |         |            |           |          |         | -35.827  | -0.234    | -1.221   | -0.148  |
|   |         |          |           |         |            |           |          |         | (140.35) | (0.980)   | (7.621)  | (0.754) |
| Obs.                                    | 148,964 | 146,954  | 147,015   | 145,035 | 148,964    | 146,954   | 147,015  | 145,035 | 148,964  | 146,954   | 147,015  | 145,035 |
| Eff. Number of obs.                     | 61,967  | 61,175   | 61,131    | 54,229  | 61,967     | 59,758    | 61,131   | 54,229  | 61,967   | 59,758    | 61,131   | 54,229  |
| Bandwidth                               | 33.54   | 47.663   | 41.34     | 37.51   | 33.54      | 30.301    | 41.34    | 37.51   | 33.54    | 30.301    | 41.34    | 37.51   |
| Less than 18 years of                   | No      | Voc      | No        | Vos     | No         | Vos       | No       | Voc     | No       | Voc       | No       | Voc     |
| schooling                               | NO      | 165      | NO        | 165     | NO         | rea       | NO       | 165     | NO       | 165       | NO       | 165     |
| Trimmed hourly wages                    | No      | No       | Yes       | Yes     | No         | No        | Yes      | Yes     | No       | No        | Yes      | Yes     |
| Survey year and birth<br>region dummies | No      | Yes      | Yes       | Yes     | No         | Yes       | Yes      | Yes     | No       | Yes       | Yes      | Yes     |
|   |         |          |           | Info    | mal emplo  | yment (=  | =1)      |         |          |           |          |         |
| Estimation method                       |         | First    | -stage    |         |            | Reduce    | ed-form  |         |          | 2         | SLS      |         |
| Dependent variable                      | -       | Years of | schooling | -<br>3  | Ir         | nformal e | mployme  | nt      | li li    | nformal e | mploymer | -<br>nt |
|   | (a)     | (b)      | (c)       | (d)     | (a)        | (b)       | (c)      | (d)     | (a)      | (b)       | (c)      | (d)     |
| Treatment                               | -0.008  | 0.030    | 0.000     | 0.030   | 0.010      | 0.006     | 0.006    | 0.005   |          |           |          |         |
|   | (0.112) | (0.117)  | (0.115)   | (0.119) | (0.014)    | (0.015)   | (0.014)  | (0.014) |          |           |          |         |
| Years of schooling                      |         |          |           |         |            |           |          |         | 4.264    | 0.207     | 1.135    | 0.149   |
|   |         |          |           |         |            |           |          |         | (20.638) | (1.025)   | (6.875)  | (0.749) |
| Obs.                                    | 148,964 | 146,954  | 147,015   | 145,035 | 148,964    | 146,954   | 147,015  | 145,035 | 148,964  | 146,954   | 147,015  | 145,035 |
| Eff. Number of obs.                     | 66,164  | 57,070   | 61,131    | 54,229  | 66,164     | 57,070    | 61,131   | 54,229  | 66,164   | 57,070    | 61,131   | 54,229  |
| Bandwidth                               | 43.55   | 38.696   | 41.34     | 37.51   | 43.55      | 38.696    | 41.34    | 37.51   | 43.55    | 38.696    | 41.34    | 37.51   |
| 1 1 10 1                                |         |          |           |         |            |           |          |         |          |           |          |         |

#### Birth cohort born September 1982

Formal employment (=1)

| Informal employment (=1)                |         |          |           |         |         |           |         |         |          |           |          |         |  |
|---|---------|----------|-----------|---------|---------|-----------|---------|---------|----------|-----------|----------|---------|--|
| Estimation method                       |         | First    | -stage    | _       |         | Reduce    | ed-form | _       |          | 2         | SLS      | _       |  |
| Dependent variable                      |         | Years of | schooling | 5       | Ir      | nformal e | mployme | nt      | Ir       | nformal e | mploymer | nt      |  |
|   | (a)     | (b)      | (c)       | (d)     | (a)     | (b)       | (c)     | (d)     | (a)      | (b)       | (c)      | (d)     |  |
| Treatment                               | -0.008  | 0.030    | 0.000     | 0.030   | 0.010   | 0.006     | 0.006   | 0.005   |          |           |          |         |  |
|   | (0.112) | (0.117)  | (0.115)   | (0.119) | (0.014) | (0.015)   | (0.014) | (0.014) |          |           |          |         |  |
| Years of schooling                      |         |          |           |         |         |           |         |         | 4.264    | 0.207     | 1.135    | 0.149   |  |
| 0                                       |         |          |           |         |         |           |         |         | (20.638) | (1.025)   | (6.875)  | (0.749) |  |
| Obs.                                    | 148,964 | 146,954  | 147,015   | 145,035 | 148,964 | 146,954   | 147,015 | 145,035 | 148,964  | 146,954   | 147,015  | 145,035 |  |
| Eff. Number of obs.                     | 66,164  | 57,070   | 61,131    | 54,229  | 66,164  | 57,070    | 61,131  | 54,229  | 66,164   | 57,070    | 61,131   | 54,229  |  |
| Bandwidth                               | 43.55   | 38.696   | 41.34     | 37.51   | 43.55   | 38.696    | 41.34   | 37.51   | 43.55    | 38.696    | 41.34    | 37.51   |  |
| Less than 18 years of schooling         | No      | Yes      | No        | Yes     | No      | Yes       | No      | Yes     | No       | Yes       | No       | Yes     |  |
| Trimmed hourly wages                    | No      | No       | Yes       | Yes     | No      | No        | Yes     | Yes     | No       | No        | Yes      | Yes     |  |
| Survey year and birth<br>region dummies | No      | Yes      | Yes       | Yes     | No      | Yes       | Yes     | Yes     | No       | Yes       | Yes      | Yes     |  |

| Self-employment (=1)                 |         |          |           |         |         |         |          |         |          |         |          |         |  |  |
|--------------------------------------|---------|----------|-----------|---------|---------|---------|----------|---------|----------|---------|----------|---------|--|--|
| Estimation method                    |         | First    | -stage    |         |         | Reduce  | ed-form  |         |          | 2       | SLS      |         |  |  |
| Dependent variable                   | _       | Years of | schooling | 5       |         | Self-em | ployment | _       |          | Self-em | oloyment | -       |  |  |
|                                      | (a)     | (b)      | (c)       | (d)     | (a)     | (b)     | (c)      | (d)     | (a)      | (b)     | (c)      | (d)     |  |  |
| Treatment                            | -0.012  | 0.033    | -0.006    | 0.034   | -0.005  | -0.004  | -0.002   | 0.000   |          |         |          |         |  |  |
|                                      | (0.115) | (0.118)  | (0.117)   | (0.113) | (0.011) | (0.011) | (0.010)  | (0.010) |          |         |          |         |  |  |
| Years of schooling                   |         |          |           |         |         |         |          |         | -35.985  | -0.123  | -2.937   | -0.017  |  |  |
| -                                    |         |          |           |         |         |         |          |         | (278.12) | (0.378) | (23.912) | (0.287) |  |  |
| Obs.                                 | 148,964 | 146,954  | 147,015   | 145,035 | 148,964 | 146,954 | 147,015  | 145,035 | 148,964  | 146,954 | 147,015  | 145,035 |  |  |
| Eff. Number of obs.                  | 59,957  | 54,968   | 57,032    | 58,396  | 59,957  | 54,968  | 57,032   | 58,396  | 59,957   | 54,968  | 57,032   | 58,396  |  |  |
| Bandwidth                            | 42.11   | 38.582   | 40.83     | 42.81   | 42.11   | 38.582  | 40.83    | 42.81   | 42.11    | 38.582  | 40.83    | 42.81   |  |  |
| Less than 18 years of                | No      | Vac      | No        | Vac     | No      | Vac     | No       | Vac     | No       | Vac     | No       | Vac     |  |  |
| schooling                            | NO      | res      | INO       | res     | NO      | res     | NO       | res     | INO      | res     | NO       | res     |  |  |
| Trimmed hourly wages                 | No      | No       | Yes       | Yes     | No      | No      | Yes      | Yes     | No       | No      | Yes      | Yes     |  |  |
| Survey year and birth region dummies | No      | Yes      | Yes       | Yes     | No      | Yes     | Yes      | Yes     | No       | Yes     | Yes      | Yes     |  |  |

*Notes:* \*p<0.1, \*\* p<0.05, \*\*\* p<0.01

The sample is constructed from the 2009-2017 Mexican National Occupations and Employment Survey. Following Calonico et al. (2018) and Calonico et al. (2014) for the optimal bandwidth. Robust standard errors using EHW correction as recommended by Kolesár and Rothe (2018) in parentheses.

#### Parametric analysis

A 24-months cut-off before and after the 1993 policy is tested with a sample of a 42-months window on either side of the threshold. The estimates for the cohorts born 24-months before the policy reported in Table 2.22 are not statistically significant.<sup>51</sup> The results in Table 2.23 are for the cohorts born 24-months after the policy in 1993. Although the estimates of the reduced-form in the formal and informal sectors are significant, the policy did not impact school attendance or earnings for these birth cohorts.

<sup>&</sup>lt;sup>51</sup> A second false cut-off of 48-months before and after the implementation of the 1993 policy with a 75-months window is employed to confirm the not statistically significant results, which are reported in Tables B2.21 and B2.22 in Appendix B.

# Table 2. 22: Placebo test. Birth cohort born 24 months before the 1993 cut-off

Birth cohort born September 1979

|   |         |          |           | Form    | nal employ | ment (=1  | L)      |         |         |           |         |         |
|---|---------|----------|-----------|---------|------------|-----------|---------|---------|---------|-----------|---------|---------|
| Estimation method                       |         | First    | -stage    |         |            | Reduc     | ed-form |         |         | 2         | SLS     |         |
| Dependent variable                      | _       | Years of | schooling | g       | F          | ormal er  | nployme | nt      | I       | Formal er | nployme | nt      |
|   | (a)     | (b)      | (c)       | (d)     | (a)        | (b)       | (c)     | (d)     | (a)     | (b)       | (c)     | (d)     |
| Treatment                               | 0.078   | 0.076    | 0.044     | 0.032   | 0.003      | 0.001     | 0.006   | 0.004   |         |           |         |         |
|   | (0.112) | (0.109)  | (0.113)   | (0.110) | (0.014)    | (0.014)   | (0.014) | (0.014) |         |           |         |         |
| Years of schooling                      |         |          |           |         |            |           |         |         | 0.033   | 0.008     | 0.137   | 0.126   |
|   |         |          |           |         |            |           |         |         | (0.162) | (0.173)   | (0.370) | (0.481) |
| Obs.                                    | 88,227  | 87,011   | 87,090    | 85,890  | 88,227     | 87,011    | 87,090  | 85,890  | 88,227  | 87,011    | 87,090  | 85,890  |
| Less than 18 years of                   | No      | Yes      | No        | Yes     | No         | Yes       | No      | Yes     | No      | Yes       | No      | Yes     |
| schooling                               |         |          |           |         |            |           |         |         |         |           |         |         |
| I rimmed hourly wages                   | No      | No       | Yes       | Yes     | No         | No        | Yes     | Yes     | No      | No        | Yes     | Yes     |
| Survey year and birth                   | No      | Yes      | Yes       | Yes     | No         | Yes       | Yes     | Yes     | No      | Yes       | Yes     | Yes     |
| region dummies                          |         |          |           |         |            |           |         |         |         |           |         |         |
|   |         |          |           | Infor   | mal emplo  | yment (=  | :1)     |         |         |           |         |         |
| Estimation method                       |         | First    | -stage    |         |            | Reduc     | ed-form |         |         | 2         | SLS     |         |
| Dependent variable                      | _       | Years of | schooling | -<br>g  | Ir         | nformal e | mployme | ent     | Ir      | nformal e | mployme | ent     |
|   | (a)     | (b)      | (c)       | (d)     | (a)        | (b)       | (c)     | (d)     | (a)     | (b)       | (c)     | (d)     |
| Treatment                               | 0.078   | 0.076    | 0.044     | 0.032   | -0.003     | -0.001    | -0.006  | -0.004  |         |           |         |         |
|   | (0.112) | (0.109)  | (0.113)   | (0.110) | (0.014)    | (0.014)   | (0.014) | (0.014) |         |           |         |         |
| Years of schooling                      |         |          |           |         |            |           |         |         | -0.033  | -0.008    | -0.137  | -0.126  |
|   |         |          |           |         |            |           |         |         | (0.162) | (0.173)   | (0.370) | (0.481) |
| Obs.                                    | 88,227  | 87,011   | 87,090    | 85,890  | 88,227     | 87,011    | 87,090  | 85,890  | 88,227  | 87,011    | 87,090  | 85,890  |
| Less than 18 years of                   | No      | Yes      | No        | Yes     | No         | Yes       | No      | Yes     | No      | Yes       | No      | Yes     |
| Trimmed hourly wages                    | No      | No       | Yes       | Yes     | No         | No        | Yes     | Yes     | No      | No        | Yes     | Yes     |
| Survey year and birth<br>region dummies | No      | Yes      | Yes       | Yes     | No         | Yes       | Yes     | Yes     | No      | Yes       | Yes     | Yes     |

| Estimation method                    | First-stage<br>Years of schooling |         |         |         |         | Reduced-form<br>Self-employment |         |         |         | 2SLS<br>Self-employment |         |         |  |
|--------------------------------------|-----------------------------------|---------|---------|---------|---------|---------------------------------|---------|---------|---------|-------------------------|---------|---------|--|
| Dependent variable                   |                                   |         |         |         |         |                                 |         |         |         |                         |         |         |  |
|                                      | (a)                               | (b)     | (c)     | (d)     | (a)     | (b)                             | (c)     | (d)     | (a)     | (b)                     | (c)     | (d)     |  |
| Treatment                            | 0.078                             | 0.076   | 0.044   | 0.032   | -0.002  | -0.002                          | -0.002  | -0.002  |         |                         |         |         |  |
|                                      | (0.112)                           | (0.109) | (0.113) | (0.110) | (0.010) | (0.010)                         | (0.010) | (0.010) |         |                         |         |         |  |
| Years of schooling                   |                                   |         |         |         |         |                                 |         |         | -0.020  | -0.032                  | -0.037  | -0.065  |  |
|                                      |                                   |         |         |         |         |                                 |         |         | (0.136) | (0.143)                 | (0.252) | (0.387) |  |
| Obs.                                 | 88,227                            | 87,011  | 87,090  | 85,890  | 88,227  | 87,011                          | 87,090  | 85,890  | 88,227  | 87,011                  | 87,090  | 85,890  |  |
| Less than 18 years of                | Ne                                | Vac     | Ne      | Ver     | Ne      | Vac                             | Ne      | Vec     | Na      | Vac                     | Ne      | Vac     |  |
| schooling                            | NO                                | res     | NO      | res     | NO      | res                             | NO      | res     | NO      | res                     | NO      | res     |  |
| Trimmed hourly wages                 | No                                | No      | Yes     | Yes     | No      | No                              | Yes     | Yes     | No      | No                      | Yes     | Yes     |  |
| Survey year and birth region dummies | No                                | Yes     | Yes     | Yes     | No      | Yes                             | Yes     | Yes     | No      | Yes                     | Yes     | Yes     |  |

Notes: \*p<0.1, \*\* p<0.05, \*\*\* p<0.01

The sample is constructed from the 2009-2017 Mexican National Occupations and Employment Survey. Robust standard errors using EHW correction as recommended by Kolesár and Rothe (2018) in parentheses.

### Table 2. 23: Placebo test. Birth cohort born 24 months after the 1993 cut-off

|                                       |                    |         |         | Form                | nal employ | ment (=1          | .)                  |         |          |                   |         |         |  |
|---------------------------------------|--------------------|---------|---------|---------------------|------------|-------------------|---------------------|---------|----------|-------------------|---------|---------|--|
| Estimation method                     | First-stage        |         |         |                     |            | Reduce            | ed-form             |         | 2SLS     |                   |         |         |  |
| Dependent variable                    | Years of schooling |         |         |                     |            | Formal employment |                     |         |          | Formal employment |         |         |  |
|                                       | (a)                | (b)     | (c)     | (d)                 | (a)        | (b)               | (c)                 | (d)     | (a)      | (b)               | (c)     | (d)     |  |
| Treatment                             | 0.028              | 0.053   | 0.035   | 0.061               | 0.021*     | 0.023**           | 0.022*              | 0.024** |          |                   |         |         |  |
|                                       | (0.088)            | (0.086) | (0.088) | (0.086)             | (0.011)    | (0.011)           | (0.011)             | (0.011) |          |                   |         |         |  |
| Years of schooling                    |                    |         | . ,     |                     |            |                   |                     |         | 0.726    | 0.435             | 0.619   | 0.391   |  |
| -                                     |                    |         |         |                     |            |                   |                     |         | (2.141)  | (0.659)           | (1.489) | (0.518) |  |
|                                       |                    |         |         |                     |            |                   |                     |         |          |                   |         |         |  |
| Obs.                                  | 88,227             | 87,011  | 87,090  | 85,890              | 88,227     | 87,011            | 87,090              | 85,890  | 88,227   | 87,011            | 87,090  | 85,890  |  |
| Less than 18 years of                 | No                 | Vos     | No      | Voc                 | No         | Voc               | No                  | Voc     | No       | Voc               | No      | Voc     |  |
| schooling                             | NO                 | Tes     | NO      | Tes                 | NO         | Tes               | NO                  | Tes     | NO       | Tes               | NO      | Tes     |  |
| Trimmed hourly wages                  | No                 | No      | Yes     | Yes                 | No         | No                | Yes                 | Yes     | No       | No                | Yes     | Yes     |  |
| Survey year and birth                 | No                 | Yes     | Yes     | Yes                 | No         | Yes               | Yes                 | Yes     | No       | Yes               | Yes     | Yes     |  |
| region dummies                        |                    |         |         |                     |            |                   |                     |         |          |                   |         |         |  |
|                                       |                    |         |         | Infor               | mal emplo  | yment (=          | 1)                  |         |          |                   |         |         |  |
| Estimation method                     | First-stage        |         |         |                     |            | Reduced-form      |                     |         |          | 2                 | SLS     |         |  |
| Dependent variable                    | Years of schooling |         |         | Informal employment |            |                   | Informal employment |         |          |                   |         |         |  |
|                                       | (a)                | (b)     | (c)     | (d)                 | (a)        | (b)               | (c)                 | (d)     | (a)      | (b)               | (c)     | (d)     |  |
| Tanatan                               | 0.028              | 0.053   | 0.025   | 0.061               | 0.001*     | 0.000**           | * 0.000*            | 0.024** |          |                   |         |         |  |
| rreatment                             | (0.028)            | (0.035  | (0.035  | (0.086)             | -0.021     | -0.025            | (0.011)             | -0.024  |          |                   |         |         |  |
| Vears of schooling                    | (0.000)            | (0.000) | (0.0007 | (0.000)             | (0.011)    | (0.011)           | (0.011)             | (0.011) | -0 726   | -0.435            | -0.619  | -0 391  |  |
| rears of schooling                    |                    |         |         |                     |            |                   |                     |         | (2.141)  | (0.659)           | (1.489) | (0.518) |  |
|                                       |                    |         |         |                     |            |                   |                     |         | (212.12) | (0.000)           | (1      | (0.010) |  |
| Obs.                                  | 88,227             | 87,011  | 87,090  | 85,890              | 88,227     | 87,011            | 87,090              | 85,890  | 88,227   | 87,011            | 87,090  | 85,890  |  |
| Less than 18 years of                 | No                 | Voc     | No      | Voc                 | No         | Voc               | No                  | Voc     | No       | Voc               | No      | Voc     |  |
| schooling                             | NO                 | res     | NO      | Tes                 | NO         | Tes               | NO                  | Tes     | NO       | Tes               | NO      | Tes     |  |
| Trimmed hourly wages                  | No                 | No      | Yes     | Yes                 | No         | No                | Yes                 | Yes     | No       | No                | Yes     | Yes     |  |
| Survey year and birth                 | No                 | Yes     | Yes     | Yes                 | No         | Yes               | Yes                 | Yes     | No       | Yes               | Yes     | Yes     |  |
| region dummies                        |                    |         |         |                     |            |                   |                     |         |          |                   |         |         |  |
|                                       |                    |         |         | Se                  | lf-employn | nent (=1)         |                     |         |          |                   |         |         |  |
| Estimation method                     |                    | First   | -stage  |                     |            | Reduced-form      |                     |         |          | 2                 | SLS     |         |  |
| Dependent variable Years of schooling |                    |         |         | Self-employment     |            |                   | Self-employment     |         |          | t                 |         |         |  |
| ·                                     | (a)                | (b)     | (c)     | (d)                 | (a)        | (b)               | (c)                 | (d)     | (a)      | (b)               | (c)     | (d)     |  |
| Transforment                          | 0.020              | 0.053   | 0.025   | 0.064               | 0.000      | 0.004             | 0.004               | 0.000   |          |                   |         |         |  |
| Treatment                             | 0.028              | 0.053   | 0.035   | 0.061               | -0.002     | -0.004            | -0.001              | -0.002  |          |                   |         |         |  |
| Voora of schoolin -                   | (0.088)            | (0.086) | (0.088) | (0.080)             | (0.008)    | (0.008)           | (0.008)             | (0.008) | 0.066    | 0.069             | 0.019   | 0.030   |  |
| rears of schooling                    |                    |         |         |                     |            |                   |                     |         | (0 335)  | (0.178)           | -0.018  | (0 131) |  |
|                                       |                    |         |         |                     |            |                   |                     |         | (0.555)  | (0.170)           | (0.223) | (0.131) |  |
| Obs.                                  | 88,227             | 87,011  | 87,090  | 85,890              | 88,227     | 87,011            | 87,090              | 85,890  | 88,227   | 87,011            | 87,090  | 85,890  |  |

| Birth | cohort | born | Septem | ber | 1983 |
|-------|--------|------|--------|-----|------|

Less than 18 years of

Trimmed hourly wages

schooling

The sample is constructed from the 2009-2017 Mexican National Occupations and Employment Survey. Robust standard errors using EHW correction as recommended by Kolesár and Rothe (2018) in parentheses.

No

No

No

Yes

No

Yes

No

Yes

Yes

Yes

Yes

Yes

No

No

No

Yes

No

Yes

No

Yes

Yes

Yes

Yes

Yes

Survey year and birth No Yes Yes Yes

No

Yes

Yes

Yes

Yes

No

region dummies Notes: \*p<0.1, \*\* p<0.05, \*\*\* p<0.01

No

No

#### 2.8 Effects of Holding Schooling Certificates

In order to find an explanation for the evidence of no causal effect of the policy on returns to compulsory schooling, this section examines the effects of holding academic certificates for different schooling levels. An alternative explanation for the not statistically significant returns of compulsory education exhibited in the literature by Grenet (2013) is the repetition in grades within the academic level, which could explain why the reform failed to improve peoples' qualifications and therefore earnings in France.

Unfortunately, this analysis cannot use repetition in school grades data because the ENOE does not collect this information. It only reports the highest completed grades within the academic level. Also, the survey does not report the school-leaving age or the age at which the people completed and secured their grades.

Furthermore, on the one hand, if the students have repetitions in school grades, the school attendance might not translate into higher attainment, higher qualifications, higher earnings, or any changes in employment sector prospects. On the other hand, raising the minimum school-leaving age until the age of 15 might increase attendance but also attainment, and therefore earnings, or it might increase the probability of working in the formal sector.

Thus, the actual years of schooling without repetitions are important in assessing the effects of compulsory education on the selected labour market outcomes of interest. However, if this variable is not available, a measure of holding academic certificates at different educational levels could provide further insights on the returns to schooling. Then, the estimates would provide the average causal effects of the qualification attainments.

Particularly, this is important if the certificate for the accomplishment of an academic level (e.g., elementary, secondary, or high schooling) plays a significant role in the labour market by providing signals of the skills and qualifications learned at school compared to incomplete grades. These qualifications and the certificates could be relevant in the job-hiring process as a way of verifying the level of competency, and therefore on earnings.

Figure 2.5 illustrates that after the implementation of the 1993 Educational Reform, the distribution of the younger birth cohorts with high-school and college academic degrees

exhibits an upward trend. Individuals holding secondary schooling certificates are relatively constant across the birth cohorts. In contrast, those holding a primary schooling degree (or less) decreased, whereas younger birth cohorts significantly reduced their proportions.

The following reduced-form is used to test if the policy increased the probability of holding a specific degree:

Academic degree<sub>i</sub> = 
$$\gamma_0 + \gamma_1(Treatment_i) + \gamma_2 F(Age in months_i) + \gamma_3 X_i + \eta_i$$
 (4)

The dependent variables, *Academic degree<sub>i</sub>*, are the following dummy variables for holding the highest academic degree: 1) *No certificate<sub>i</sub>*, 2) *Primary schooling<sub>i</sub>*, 3) *Secondary schooling<sub>i</sub>*, 4) *High* – *school level<sub>i</sub>*, 5) *College* – *level<sub>i</sub>*, and 6) *Post* – *graduate level<sub>i</sub>*. The explanatory variables represented by  $X_i$  are the same as in equation (3). The random error term is  $\eta_i$ . The estimated coefficient of the treatment variable,  $\hat{\gamma}_1$ , measures the reform's average causal effect on holding a specific academic degree at the treatment assignment threshold.

The results of each dependent variable, which come from separate regressions, are reported in Tables 2.24 and 2.25 for both non-parametric and parametric analyses. The probability of not having schooling qualifications decreased for both analyses by between 1.4 and two percentage points, on average. For the non-parametric analysis, the policy increased the probability of holding a college-level degree by about two percentage points (see Table 2.24).

In the parametric analysis with a window of 24 months on either side of the cut-off point, the intervention decreased the probability of holding a secondary schooling certificate by two percentage points, albeit marginally statistically significant. However, with a 42-months window, the probability of holding a high-school level degree increased by 1.7 percentage points, revealing a possible spill-over effect. The policy reduced the probability of not having a schooling certificate by 0.8 of a percentage point with a sample of 75-months window (see Table 2.25).

These estimates reveal a small improvement in holding academic certificates induced by the 1993 Reform. Moreover, children that left the school right after primary education around the age of 12 or 13 years, before 1993, with the implementation of this reform they were

subject to it until the age of 15, but perhaps they did not obtain their secondary schooling diploma. Perhaps, the reform focused on keeping children at school instead of compelling them to conclude secondary schooling and obtain their certificates.

|                     | No certificates (=1) | Primary<br>schooling (=1) | Secondary<br>schooling (=1) | High-school (=1) | College (=1)      | Graduate (=1)     |
|---------------------|----------------------|---------------------------|-----------------------------|------------------|-------------------|-------------------|
| Treatment           | -0.021***<br>(0.007) | 0.008<br>(0.009)          | -0.019<br>(0.014)           | 0.015<br>(0.012) | 0.019*<br>(0.011) | -0.000<br>(0.003) |
| Obs.                | 148,964              | 148,964                   | 148,964                     | 148,964          | 148,964           | 148,964           |
| Eff. Number of obs. | 53,259               | 73,618                    | 57,333                      | 53,259           | 53,259            | 63,450            |
| Bandwidth           | 40.07                | 49.82                     | 42.94                       | 40.32            | 40.045            | 47.025            |

 Table 2. 24: Non-parametric results. Reduced-form for holding the highest certificate

 Holding the highest academic degree

*Notes:* \*p<0.1, \*\* p<0.05, \*\*\* p<0.01

The sample is constructed from the 2009-2017 Mexican National Occupations and Employment Survey. Following Calonico et al. (2018) and Calonico et al. (2014) for the optimal bandwidth. Robust standard errors using EHW correction as recommended by Kolesár and Rothe (2018) in parentheses.

| Holding the highest academic degree |          |         |                        |              |         |         |                          |         |               |         |         |         |
|-------------------------------------|----------|---------|------------------------|--------------|---------|---------|--------------------------|---------|---------------|---------|---------|---------|
|                                     |          | P       | Primary schooling (=1) |              |         |         | Secondary schooling (=1) |         |               |         |         |         |
|                                     | (a)      | (b)     | (c)                    | (d)          | (a)     | (b)     | (c)                      | (d)     | (a)           | (b)     | (c)     | (d)     |
| Treatment                           | -0.014** | -0.009* | -0.009*                | -0.008**     | -0.006  | -0.001  | -0.000                   | -0.002  | -0.021*       | -0.014  | -0.012  | -0.012  |
|                                     | (0.006)  | (0.005) | (0.004)                | (0.003)      | (0.009) | (0.007) | (0.007)                  | (0.005) | (0.012)       | (0.009) | (0.009) | (0.007) |
| Obs.                                | 51,109   | 88,227  | 100,296                | 148,964      | 51,109  | 88,227  | 100,296                  | 148,964 | 51,109        | 88,227  | 100,296 | 148,964 |
| window (months                      | ) 24     | 42      | 48                     | 75           | 24      | 42      | 48                       | 75      | 24            | 42      | 48      | 75      |
|                                     |          |         |                        |              |         |         |                          |         |               |         |         |         |
| High-school (=1)                    |          |         |                        | College (=1) |         |         |                          |         | Graduate (=1) |         |         |         |
|                                     | (a)      | (b)     | (c)                    | (d)          | (a)     | (b)     | (c)                      | (d)     | (a)           | (b)     | (c)     | (d)     |
| Treatment                           | 0.017    | 0.017** | 0.015*                 | 0.007        | 0.014   | 0.006   | 0.007                    | 0.010*  | -0.001        | -0.001  | -0.000  | 0.000   |
|                                     | (0.011)  | (0.008) | (0.007)                | (0.006)      | (0.010) | (0.007) | (0.007)                  | (0.005) | (0.003)       | (0.002) | (0.002) | (0.001) |
|                                     |          |         |                        |              |         |         |                          |         |               |         |         |         |
| Obs.                                | 51,109   | 88,227  | 100,296                | 148,964      | 51,109  | 88,227  | 100,296                  | 148,964 | 51,109        | 88,227  | 100,296 | 148,964 |
| window (months                      | ) 24     | 42      | 48                     | 75           | 24      | 42      | 48                       | 75      | 24            | 42      | 48      | 75      |

| Table 2. 25: Parametric analysis: | Reduced-form for | or holding the l | highest certificate |
|-----------------------------------|------------------|------------------|---------------------|
|-----------------------------------|------------------|------------------|---------------------|

Notes: \*p<0.1, \*\* p<0.05, \*\*\* p<0.01

The sample is constructed from the 2009-2017 Mexican National Occupations and Employment Survey. Reduced-Form estimation. Robust standard errors using EHW correction as recommended by Kolesár and Rothe (2018) in parentheses.

In Mexico, there are no national examinations at the end of secondary schooling. The completion of the three schooling grades at this academic level is sufficient to obtain the diploma. The unique measure of the qualifications is the average score held during the three

years that this academic level endured.<sup>52</sup> Then, signalling in the labour market depends on schooling scores, school's reputation, type of schools attended by the students such as public or private, rural or urban settlement types, among other characteristics. Thus, heterogeneity could arise through these characteristics. In addition, the quality of education also varies across the country (e.g., low quality tends to be a feature of schools in rural areas).

An alternative reason is perhaps the achievement of a higher level of education, beyond secondary schooling, which may be required to raise earnings. Grenet (2013) argued that compulsory schooling laws could be effective only to the extent that they encourage the student to become enrolled at grades that lead to academic certificates, which may act as a signalling device for the employers. This is also tested in this section for the individuals holding a secondary schooling diploma or high-school academic degree as the highest academic certificate held.<sup>53</sup>

The findings reported in Table 2.26 do not reveal a causal effect for the returns to education holding the secondary schooling degree or high-school level certificate.<sup>54</sup> However, the policy reduced the likelihood of holding a secondary schooling certificate, whereas it increased the probability of holding a high-school level degree. This provides suggestive evidence that the policy led to a small and significant spill-over effect in obtaining high-school level degrees. In addition, the estimates support the earlier results reported of the policy impacting on the average years of schooling. <sup>55</sup>

The policy perhaps would have impacted earnings if it had compelled students to obtain a high-school-level certificate instead. However, the reform of compulsory schooling until the age of 18 was implemented in 2012, since the information is more recent and the younger

<sup>&</sup>lt;sup>52</sup> As already noted, Mexico employs the numerical system for grading each year of education. From 0 to 10 to measure the students' scores. Generally, 0-5 is insufficient or failed; 6 is sufficient; 7 is average; 8 is good; 9 is very good; and 10 is excellent.

 $<sup>^{53}</sup>$  See Appendix A2.3 for the model specification.

<sup>&</sup>lt;sup>54</sup> Additional robustness checks are reported in table B2.23 in Appendix B.

<sup>&</sup>lt;sup>55</sup> An analysis holding at least a secondary schooling or at least high-school degree was tested using a similar specification as the one in Appendix A2.3. The results for the first stage are consistent with previous estimates, the probability of holding at least a high-school level certificate increased as a result of the reform for both non-parametric and parametric analyses; similarly, the estimates do not report impacts on hourly earnings.
birth cohorts would be less than 25 years old by the time this research was undertaken, this reform is not analysed in this study.

|                      |         |            |              | 24-m    | nonths wi           | ndow      |           |         |                     |                     |         |         |
|----------------------|---------|------------|--------------|---------|---------------------|-----------|-----------|---------|---------------------|---------------------|---------|---------|
| Estimation method    |         | First      | -stage       |         |                     | Reduce    | ed-form   |         |                     | 2                   | SLS     |         |
| Dependent variable   | Seco    | ndary scho | oling certi  | ificate | 1                   | Log of ho | urly wage | es      |                     | Log of hourly wages |         |         |
|                      | (a)     | (b)        | (c)          | (d)     | (a)                 | (b)       | (c)       | (d)     | (a)                 | (b)                 | (c)     | (d)     |
| Treatment            | -0.021* | -0.021*    | -0.021       | -0.020  | 0.006               | 0.006     | 0.003     | 0.000   |                     |                     |         |         |
|                      | (0.012) | (0.012)    | (0.012)      | (0.012) | (0.016)             | (0.016)   | (0.016)   | (0.015) |                     |                     |         |         |
| Years of schooling   |         |            |              |         |                     |           |           |         | -0.302              | -0.286              | -0.188  | -0.023  |
| 0                    |         |            |              |         |                     |           |           |         | (0.765)             | (0.756)             | (0.756) | (0.762) |
| Obs.                 | 51,109  | 51,109     | 51,109       | 51,109  | 51,109              | 51,109    | 51,109    | 51,109  | 51,109              | 51,109              | 51,109  | 51,109  |
| Survey year dummies  | No      | Yes        | Yes          | Yes     | No                  | Yes       | Yes       | Yes     | No                  | Yes                 | Yes     | Yes     |
| Birth region dummies | No      | No         | Yes          | Yes     | No                  | No        | Yes       | Yes     | No                  | No                  | Yes     | Yes     |
| Urban status         | No      | No         | No           | Yes     | No                  | No        | No        | Yes     | No                  | No                  | No      | Yes     |
|                      |         |            |              | 42-m    | nonths wi           | ndow      |           |         |                     |                     |         |         |
| Estimation method    |         | First      | -stage       |         |                     | Reduce    | ed-form   |         |                     | 2                   | SLS     |         |
| Dependent variable   | _       | High-schoo | l certificat | e       | Log of hourly wages |           |           |         | Log of hourly wages |                     |         |         |
|                      | (a)     | (b)        | (c)          | (d)     | (a)                 | (b)       | (c)       | (d)     | (a)                 | (b)                 | (c)     | (d)     |
| Treatment            | 0.017** | 0.017**    | 0.017**      | 0.016** | 0.019               | 0.020     | 0.019     | 0.015   |                     |                     |         |         |
|                      | (0.008) | (0.008)    | (0.008)      | (0.008) | (0.012)             | (0.012)   | (0.012)   | (0.012) |                     |                     |         |         |
| Years of schooling   |         |            |              |         |                     |           |           |         | 1.101               | 1.113               | 1.088   | 0.950   |
|                      |         |            |              |         |                     |           |           |         | (0.854)             | (0.862)             | (0.865) | (0.858) |
| Obs.                 | 88,227  | 88,227     | 88,227       | 88,227  | 88,227              | 88,227    | 88,227    | 88,227  | 88,227              | 88,227              | 88,227  | 88,227  |
| Survey year dummies  | No      | Yes        | Yes          | Yes     | No                  | Yes       | Yes       | Yes     | No                  | Yes                 | Yes     | Yes     |
| Birth region dummies | No      | No         | Yes          | Yes     | No                  | No        | Yes       | Yes     | No                  | No                  | Yes     | Yes     |
| Urban status         | No      | No         | No           | Yes     | No                  | No        | No        | Yes     | No                  | No                  | No      | Yes     |
|                      |         |            |              |         |                     |           |           |         |                     |                     |         |         |

Table 2. 26: Parametric results for holding secondary schooling and high-school certificates

*Notes:* \*p<0.1, \*\* p<0.05, \*\*\* p<0.01

The sample is constructed from the 2009-2017 Mexican National Occupations and Employment Survey. Robust standard errors using EHW correction as recommended by Kolesár and Rothe (2018) in parentheses.

# **2.9 Conclusions**

The major contribution of the analysis undertaken in this chapter is that it is among one of the few studies that provide empirical evidence of the returns to compulsory schooling for a developing country. Most of the extant literature has been carried out for high-income economies. In addition, this study uses a rigorous approach exploiting age cohort discontinuities measured in months of birth to provide estimates of the causal effects on earnings and other labour market outcomes.

The empirical evidence of this natural experiment suggests that the 1993 Mexican Educational Reform, which increased compulsory schooling from 12 to 15 years of age, was an effective strategy in enhancing educational attendance. The policy is associated with

average increases in the schooling of between 0.26-0.32 of a year for males exposed to the reform compared to those not exposed to the reform. Nevertheless, the reform by itself failed to have a measurable impact on labour market earnings. The evidence of the impacts on other labour market outcomes such as the likelihood of working in the formal or informal sectors, as well as being self-employed did not yield statistically significant effects either. An array of robustness checks support these findings.

These findings are broadly consistent with the literature for high-income economies, which generally reports that the compulsory schooling laws raise schooling but do not improve the labour-market prospects of the individuals exposed to these reforms. Specifically, the findings reported for this study are similar to those of Aydemir and Kirdar (2017) for Turkey suggesting that Mexico behaves as a developed country in terms of the impact on returns to compulsory schooling that is attributable to an educational reform policy.

Some plausible reasons could explain these findings for Mexico. The statistics about the school attendance reported before and after the policy implementation suggest the reform was poorly enforced. Less than half of the individuals within the age range compelled by the reform actually attended secondary schooling even five years after its introduction. In contrast to developed countries where the school leave age has been raised, it may take a longer period for greater compliance with such a reform in Mexico given weaker institutions. Thus, the subsequent effects on labour market outcomes may also take some time to be detected.

The educational reform may have the desired effect of increasing the labour market supplyside in raising enrollments of educated individuals. However, if the labour market demand for skilled workers rises in the same way that the supply increases, then, there would be no effect of the returns to human capital in the labour market. Hence, the null effect observed on earnings in this analysis.

In addition, the move towards liberalisation and trade openness in Mexico after 1994 may have enabled the greater exploitation of the comparative advantage of unskilled workers than skilled workers, thus raising the wages of the lower-skilled rather than higher-skilled. Thus, raising earnings through schooling would be possible if the demand for skilled workers increases faster than the rise of the supply of skilled workers. Mexican compulsory schooling laws might be effective at raising earnings if they induce a significant fraction of the population to complete higher levels of education, beyond secondary schooling, and/or if they boost the qualifications acquired at school. This would be useful for signalling in the labour market. However, in the Mexican context, it is necessary to acknowledge the role of different potential confounding factors that could drive the estimates reported in this chapter, such as the quality of education offered at school (which is different across private, public, rural, and urban schools); skills mismatch; the role of wage-setting mechanisms in the labour market; the large extent of child labour force participation; the large fraction of informal sector workers; and the nature of the skills acquired at school.

Further research on these factors would be useful to disentangle the effects of compulsory education on the labour market outcomes. Nevertheless, there is an opportunity for policy intervention in Mexico not just in terms of raising the age at which compulsory education is completed but also for improving the quality of education offered at school including redesigning the curriculum to match it with labour market demand and incentivising the acquisition of qualifications. Thus, a broader collection of educational policies with a common objective is more likely to improve education and to have the desired impact on earnings and other labour market outcomes.

# **CHAPTER THREE:** Impacts of the Minimum Wage Policy on Wage Level and Wage Inequality in Mexico

#### **3.1 Introduction**

The Minimum Wage (MW) has been a policy in the labour market used to influence wage levels and wage inequality in both developed and developing economies. Minimum wages are generally motivated by the government's objective of raising the earnings of the lower-paid workers, improving their welfare, and alleviating poverty among low-income households.

The final impacts of increasing MWs are difficult to predict as they depend on several factors. These include, *inter alia*, how the up-ratings of the MWs are determined, which workers are covered, how competitive the labour market is, the elasticities of demand and supply of relevant labour inputs, and to the extent to which MWs are complied with or enforced.

The impacts are part of an ongoing debate because, on the one hand, the policy could redistribute resources in a welfare-enhancing way, which has the potential to reduce poverty, boost productivity, and foster economic growth. If an up-rate in the MW (or an expansion in coverage) increases the wages of low-paid workers, it would compress wages between high-paid and low-paid workers and therefore decrease the measure of wage inequality.

On the other hand, MW interventions could misallocate labour in the formal sector and have spill-over effects for the high-paid workers. They could also lead to depressed wages for the low-paid employees in the informal sector, thus, wasting resources and reducing growth rates. A fall in the real value of the MW (or a narrowing of coverage) could potentially result in rising wage inequality (Bell, 1997).

In developed countries, MW policies have been interpreted as successful welfare-enhancing tools. The empirical evidence reveals that it raised the pay of a sizeable group of workers, especially at the lower tail of the wage distribution and may have assisted in reversing rising

wage inequality, albeit modestly.<sup>56</sup> However, the impacts are controversial and subject to an ongoing debate.

The empirical evidence for developing countries is varied as well. MW interventions may help compress the bottom of the wage distribution but do not reduce wage inequality or are implicated in the growth of wage inequality. Nevertheless, the evidence depends on the methodology used, the employment sector analysed, and the nature of the policy investigated, among other factors.

Understanding how the MWs affect labour markets and welfare is critical for developing effective labour market policies and poverty alleviation strategies (Ham, 2018). A common concern in Latin American countries is whether MW could be used as a tool to increase the earnings of (formal) low-income workers, reduce inequality, and thus assist lift households out of poverty (Cunningham, 2007).

For Mexico, existing studies mostly focus on the effects of the MW annual up-ratings on wages and employment with few focusing on wage inequality. The literature generally suggests that the growth in inequality is attributable to the deterioration in the real value of the MW over time and to the pay gap among public and private sector workers (Bell, 1997; Fairris et al., 2008; Gasparini et al., 2015). Bosch and Manacorda (2010) report an increase in wage inequality at the bottom of the wage distribution between 1989 and 2001 because of annual MW up-ratings. Moreover, none of the recent papers using the institutional MW change in 2012 has examined the effects on inequality (Campos-Vázquez et al., 2020).

This chapter evaluates the impact of the Federal mandated MW in 2012, which up-rated the MW for a specific geographic zone. The study tests whether the policy is associated with variations in formal sector wages across the unconditional wage distribution in Mexico over the period 2010-2015. Specifically, if up-ratings in the MW treated zone provide a mechanism that ultimately reduces wage inequality in Mexico.

<sup>&</sup>lt;sup>56</sup> For example, Autor et al. (2016), Dickens and Manning (2004), Stewart (2012), Dolton et al. (2012), Kambayashi et al. (2013).

The research exploits mean-based and unconditional quantile regression (UQ) methods to determine the effects of the MW policy on the unconditional wage distribution using cross-sectional data. Thus, the analysis goes beyond the mean impacts and provides an understanding of the distributional impacts of the intervention on the formal sector employees. The research also exploits a Recentred Influence Function (RIF) approach in conjunction with a Gini dispersion measure to assess the overall effects of the policy on wage inequality.

The evidence suggests the exposure to the MW up-rating in the treated MW zone increases wages across the unconditional hourly wage distribution. The impact is statistically stronger at the bottom end of the wage distribution, which is in comport with the findings for other developing countries. In particular, the effects persist higher up the unconditional wage distribution than generally found in the literature, indicating that wage inequality has not been reduced due to the MW policy. In the public sector, the rise in wages is conjectured to the MW's role as a numeraire in the award of increments in the public sector payments.

This research contributes to the empirical literature on minimum wages in several ways. First, it adds to the growing literature using unconditional quantile treatment effects (UQTE) to evaluate the impacts of the MW interventions for developing and developed countries on the distribution of earnings. Second, it is among the few studies assessing the effects of MW policies on wage inequality in a developing country context. Third, it updates previous results for wage inequality in Mexico and is the first study analysing the 2012 MW policy for this purpose.

This chapter is structured as follows. The next section presents the literature review followed by the institutional labour market context and the Minimum-Wage system in Mexico. A description of the data and the outlined empirical strategy are provided in Sections 3.4 and 3.5. The empirical results and robustness checks are then presented. The concluding remarks are contained in the final section.

#### **3.2 Literature Review**

The study of wage inequality in developing countries has generated extensive literature that attempts to understand what drives inequality. It has focussed on the relationship between wage inequality and its association with education, informality, and trade.

Wage inequality is primarily linked to the rise in the supply of skilled workers, which is also associated with the sustained effort made by developing country governments to expand basic education.<sup>57</sup> There is also evidence of an increased demand for labour that specializes in the production of low-skilled intensive goods for exports in developing countries.<sup>58</sup> Coexisting with skilled labour, a large informal sector in Latin America, where wages are lower and unregulated, is found as one mechanism that increases wage inequality. Informality generally attracts the least skilled, women and the youngest workers.<sup>59</sup> In contrast, formal sector wages are rendered more stable by employment protection legislation.

Labour market factors in the formal sector have been explored to examine their impacts on inequality, such as MW settings, unionizations, and pay-setting in the public sector. They are generally perceived to protect the lowest paid and narrow the bottom end of the pay distribution. Although the MW legislation is binding for the formal sector, some studies provide evidence of spill-over effects to the uncovered sector in developing countries (e.g., Campos-Vázquez and Rodas Milián, 2020 for Mexico; Leckcivilize, 2015 for Thailand).

This section reviews the evidence on the impacts of the MW policies on wages and the pay distribution. Most of the MW literature emphasizes the effects on employment and wages, and generally focuses on developed economies (e.g., the United States (US), the United Kingdom (UK), Germany and Canada). The analyses of the MW effects on wage inequality are less common and usually centred around the up-rating impacts, or the effects of introducing new MW legislation.

On the one hand, the objective of the MW policies is to redistribute resources in a welfareenhancing way to increase productivity and foster economic growth. On the other hand, the

<sup>&</sup>lt;sup>57</sup>Campos-Vázquez et al. (2014); Binelli (2016); Lustig et al. (2013).

<sup>&</sup>lt;sup>58</sup> Acosta et al. (2017); Binelli (2016); Esquivel et al. (2010).

<sup>&</sup>lt;sup>59</sup> Rosser et al. (2000); Binelli and Attanasio (2010); Binelli (2016).

empirical work suggests these policies increase the costs for employers, potentially reducing their labour force size, leading to higher unemployment rates and worsening low-income households.

For developed countries, Autor et al. (2016) found growth inequality in the lower tail of the wage distribution for the US to range between 30%–55% from 1979 to 1989. This finding was argued to be attributable to a decline in the real value of the MW (i.e., inequality measured by the differential between the 50<sup>th</sup> and 10<sup>th</sup> percentiles of the log wage). Although, the authors found the declining MW made a meaningful contribution to female inequality but a negligible contribution to male lower tail inequality during the entire period analysed from 1979 to 2012. In the UK, the National Minimum Wage (NMW), introduced in 1999 to reverse the trend in wage inequality, has been limited to be below the 10<sup>th</sup> percentile without influencing inequality in the lower half of the wage distribution (i.e., the 50<sup>th</sup>/10<sup>th</sup> wage ratio) (Dickens and Manning, 2004; Stewart, 2012).

Notwithstanding this, Dickens and Manning (2004) argue that the NMW was effective in raising the earnings of the lowest-paid workers between 1999 and 2001. The work of Stewart (2012) underlines continued inequality growth in the upper half of the wage distribution compared to the growth in the lower half between 1997 and 2008. However, Dolton et al. (2012), using data from 1997 to 2007 across local labour markets, concluded that an increase in the bite of the NMW is associated with falls on earnings in the lower tail of the wage distribution. Stewart (2012) applied a D-i-D estimator with individual panel data to evaluate the NMW, whereas Dolton et al. (2012) use pooled cross-sectional data within an incremental D-i-D (ID-i-D) approach with yearly interactions for each MW up-rating.

The empirical research on wage inequality due to changes in labour market institutions has been attracting growing attention in developing countries, for which there are additional issues to consider. Minimum wages tend to be set generally at a higher rate (Maloney and Nuñez, 2003) and are less likely to be rigorously enforced (Kanbur and Ronconi, 2016). In addition, labour markets are often segmented into formal and informal sectors (Ham, 2018;

Wong, 2019).<sup>60</sup> There is a high level of wage inequality, where the MW is used as a wage floor system to lessen inequality and reduce poverty.

Existing research highlights the shortcomings of the MW in reducing inequality. For example, Yamada (2016) found for Indonesia that even though a rise in the MW had a positive and significant effect on earnings for male formal workers, it had negative effects on the number of hours worked. It did not close gender pay gaps and consequently did not improve living standards or reduce inequality.

In Thailand, Leckcivilize (2015) argued that the MW appears to help compress the lower part of the wage distribution for formal low-paid employees in large businesses. However, the effect does not extend to small and medium firms. Likewise, the MW role as a benchmark for wage adjustment does not reduce overall wage inequality in the informal sector. This may be due to the high non-compliance rate and the weak MW law enforcement.<sup>61</sup>

Gasparini et al. (2015) evaluated wage inequality by employment sector (i.e., public and private) for 15 Latin American countries during 1992 and 2012. The Gini coefficients suggest, on average, a public sector with a satisfactory level of earnings equality, whereas higher levels of inequality for the private sector.<sup>62</sup> The study highlights the pay disparities between these sectors in the region.

The studies for Mexico have largely focused on the MW annual up-ratings. Bell (1997) reported for the period 1984-1990 that the federal mandated MW was too low to influence formal manufacturing wages for skilled workers.<sup>63</sup> Thus, the deterioration in the MW real value could not be responsible for the subsequent increase in wage inequality or unemployment. There is also a significant non-compliance with the MW in the formal sector

<sup>&</sup>lt;sup>60</sup> Latin American countries have a labour force that includes a high percentage of low-earners and informal workers.

<sup>&</sup>lt;sup>61</sup> The wage is concentrated more around the MW in the informal sector compared to the formal sector. However, this is something of a paradox because the informal sector of the economy is thought to be mostly immune to government regulations of this sort. Thus, for Leckcivilize (2015) the MW acts as a benchmark in wage negotiations.

<sup>&</sup>lt;sup>62</sup> The Gini coefficient for hourly earnings in the public sector was, on average, 0.369 in 2012 compared to 0.387 for private sector workers in large firms, and 0.494 for other private sector workers.

<sup>&</sup>lt;sup>63</sup> Bell (1997) and Fairris et al. (2008) suggest that wage levels tend to be well above the MW, and there is no clustering in the distribution around the level at which it is set.

for women and unskilled workers, who are the most affected because are paid at or below the MW.

Fairris et al. (2008) argued that changes in MWs over the period from 1984 to 1992 have their greatest impact on the mid-to-lower tail of the wage distribution, but mostly at the lower tail of the informal sector pay distribution. This implies that the pay of low-skilled workers is more likely to be influenced by MW norms than the pay of high-skilled workers in Mexico.

Bosch and Manacorda (2010) use data between 1989 and 2001 to analyse the effective MW up-rating in each Mexican state and provide evidence of an increase in wage inequality at the bottom end of the wage distribution (i.e., the 10<sup>th</sup>–70<sup>th</sup> and 20<sup>th</sup>–70<sup>th</sup> wage percentile gap of the logarithm of wages). In addition, a 10 percentage points increase in the effective MW raises earnings at the bottom decile by almost seven percentage points, and median earnings by around three percentage points relative to the 7<sup>th</sup> decile. They suggest that the direct and indirect impact of the opening of the economy (e.g., through trade, foreign direct investment, and other liberalization policies) contributed to the rise in the wage gap by skill group.

The Gini values for Mexico in 2012 were 0.449 and 0.451 for public and private sector workers, respectively (Gasparini et al., 2015). It suggests significant pay disparities, and thus inequality within these employment sectors. The foregoing evidence indicates the growth in inequality might be due to the steep decline and deterioration in the real value of the MW, and the stabilization programs that strengthened the link between wage levels, wage changes, and minimum wages in Mexico over the period from 1984 to 2012.

The effects of annual MW up-ratings within regions have been analysed through fixed-effects models with province-specific linear time trends (e.g., Leckcivilize, 2015; Yamada, 2016). The use of instrumental variables also features in this literature to capture the potential endogeneity between wages, MWs, and employment as they might be simultaneously determined (e.g., Bosch and Manacorda, 2010).<sup>64</sup>

<sup>&</sup>lt;sup>64</sup> Bosch and Manacorda (2010) use Instrumental variables with the ENEU survey and Social Security data (IMSS -Spanish acronym) from gross formal workers to provide error free estimates of average earnings across municipalities and time. Their procedure purges the estimates of the potential correlation between the included regressors and the error term due to potential measurement error.

The difference-in-differences (D-i-D) methodology has been also implemented when comparing observations initially earning below the MW to those above the MW (e.g., Dolton et al., 2012; and Stewart, 2012 for the UK) or for evaluating the different bites of an intervention across regions (e.g., Autor et al., 2016 for the US). A few studies have used the D-i-D method to analyse MW changes in developing countries (e.g., Campos-Vázquez et al., 2017 for Mexico; Ham, 2018 for Honduras; Wong, 2019 for Ecuador).

There are studies interested in understanding the distributional average impacts of treatment participation (e.g., in the lower, middle or upper tail of the wage distribution). However, the literature using conditional (Callaway and Li, 2019; Callaway et al., 2018) and unconditional (Firpo et al., 2009; Hernaes, 2018; Pérez, 2020) quantile regressions for analysing institutional changes in the labour market, such as the MW legislation, is limited.

In summary, the effects of MWs are somewhat controversial for developed countries. There are benefits of the earnings up-rating on low-paid workers with modest or ineffective impacts on wage inequality. The empirical evidence for developing countries is more varied. On the one hand, increases in MW help to compress the lower part of the wage distribution but do not reduce wage inequality. On the other hand, the decline of the MW real value accounted for the growing wage inequality observed in the lower tail of the distribution. Nevertheless, the results depend on the methodology used, the type of employment sector, and the type of policy analysed (annual up-ratings or changes in the legislation).

Hence, institutional factors in the labour market such as the minimum wages have been assumed of some importance in the determination of wage levels and wage inequality for developed and developing economies, although the literature on it remains limited. The next section describes these institutional features within the Mexican labour market context.

#### **3.3 Institutional Features of the Mexican Labour Market**

The Mexican labour market and industrial relations legislation are extremely detailed and complicated, as they are contained in several laws comprising thousands of legal articles. The legislation is also outdated because the Constitution dates back to 1917 and the main article related to labour (Article 123) has experienced only minor reforms since then. The Federal

Labour Law (LFT), enacted in 1970, has been subject to minimal amendments to increase benefits for workers and reduce the flexibility with which labour can be fired or dismissed.

As in many other Latin American countries, the Mexican legislation is characterized by generous mandated benefits and a high level of job protection. Employers should comply with several regulations, including minimum wages, a minimum age of employment, the maximum length of the working day, overtime pay, social security contributions, severance payments, seniority premia, maternity leave, on-the-job training provisions, non-discrimination policies and a provision for which the employers share profits with their employees. Nonetheless, there is likely to be a high degree of non-compliance with these regulations, and therefore a large share of workers that do not receive some or any of the required benefits (Castellanos et al., 2004).

Some features result in an explicit legal regulation of the labour market. For example, the prohibition of lowering nominal wages (LFT, Article 51). A worker whose nominal wage or benefits have been reduced can take legal action against the employer and request compensation for having been dismissed without just cause. Thus, downward nominal wage rigidities are well-known —albeit little understood— fact in the Mexican labour market (Castellanos et al., 2004).

One important feature of Mexican minimum wages is that they serve as a norm for wage settings at the micro-level, where many prices are fixed or tied to multiples or fractions of the MW such as fines, bails, pensions, income brackets for income tax rates, eligibility for certain social services (e.g., housing credits), bonuses as part of fringe benefits, and productivity bonuses for teachers and university professors (Bosch and Manacorda, 2010; Castellanos et al., 2004; Fairris et al., 2008).

The use of multiples of the monthly MW is consistent with the role of a numeraire that it traditionally plays in the Mexican economy. It is a common practice among workers and employers to report wages in multiples of MW even in the informal sector. Legislated occupational MWs are also expressed as multiples of the general MW in each area. The MW appears to have become, over time, an important benchmark or social custom for a "fair" remuneration in relative pay across occupations and is acknowledged as such by workers.

Linking wages to multiples of the MW is a mechanism for indexing wages to changes in the cost of living, although not enforced by law (Azuela de la Cueva et al., 2014; Fairris et al., 2008; Maloney and Nuñez, 2003). Labour unions, firms and workers explicitly or implicitly use the MW in their bargaining and to index contracts (Castellanos et al, 2004).

According to Azuela de la Cueva et al. (2014), even governmental payments, and public expenditures are expressed in multiples of the MW. The MW increases or up-ratings then could have a broader impact on the scale of public finances. By January 2014, approximately 280 Federal laws were indexed to the MW. The process to deindex them started in December 2014 for 149 Federal laws and 870 Constitutional articles. It was effectively de-linked from penalty payments by end of 2015.<sup>65</sup>

The effect of the numeraire in the region is far stronger than that found by Neumark and Wascher (2007) in the US suggesting that the MW induces far-reaching rigidities in the labour market. Possible trade-offs between poverty effects and reducing flexibility are likely to be more severe in Latin America. There is evidence for Brazil, Mexico, Argentina, and Uruguay that it serves as a reference throughout the economy, including sectors not legally bound by it, and influencing wage-setting in the informal sector (Maloney and Nuñez, 2003).

Nonetheless, Woodruff (1999) considered weak the effects of the MW as a reference price for labour in Mexico since its multiples are arguably more acceptable and meaningful for low-skill than high-skill workers. Bell (1997) and Fairris et al. (2008) also suggest the MW stopped exerting power in the Mexican economy because it has been at the bottom end of the wage distribution since the second half of the 1980s. Conversely, Bosch and Manacorda (2010) argued its use in urban municipalities as numeraire explains the cluster at monthly wages and the spill-over effects at higher percentiles of the pay distribution (up to the 60<sup>th</sup>-70<sup>th</sup> percentile).

At a macro level, the MW has played a role in stabilization policies throughout the decades of the 1980s and 1990s as a response to the high degree of volatility in inflation and GDP growth rates in Mexico. Its feature as a nominal anchor for the labour market and the overall

<sup>&</sup>lt;sup>65</sup> https://www.gob.mx/conasami/es/articulos/desindexacion-del-salario-minimo-68707?idiom=es

economy is also common in other Latin American countries (Bosch and Manacorda, 2010; Fairris et al., 2008).

### 3.3.1 The Minimum-Wage System in Mexico

The MW in Mexico is a constitutional right for all workers (in the formal sector). Article 123 specifies it needs to be sufficient to satisfy the elementary necessities covered by a head of household, such as the ability to purchase the basic alimentary basket of goods and to provide for the compulsory education of their children.

Since 1962, the daily MW setting has been assigned to a tripartite National Commission for Minimum Wages (CONASAMI) that comprised representatives from businesses, labour unions, and the government.<sup>66</sup> There are two different daily minimum wages, the "general" and the "occupational" wage. The latter is specified for low-wage occupations with a monetary value slightly higher than the general MW set for other professions.<sup>67</sup>

Minimum wages vary by MW geographic zones depending on their level of economic development. The zones are consolidated by groups of municipalities regardless of the state they belong to. Some Mexican states can set up different MW within the state because their municipalities could be assigned to different MW zones. The inclusion or exclusion of the municipalities in the MW zones have been changing over time.

In 1986, there were three MW zones represented by A, B, and C. The assignment intended to deliver approximately the same real value of the daily MW in each zone. However, zone A set the highest nominal and real MW, and zone C the lowest. Area A encompasses the capital city, cities close to the US border, some tourist resorts, and industrial hubs. The second and third most populated cities in Mexico (Guadalajara and Monterrey) belong to area B along with some other developed cities. The smaller and rural municipalities of the country in conjunction with a few developed cities and other important touristic resorts (e.g., Cancun) are consigned to area C.

<sup>&</sup>lt;sup>66</sup> <u>http://www.conasami.gob.mx/m/quienes\_somos.html</u>

<sup>&</sup>lt;sup>67</sup> These include construction workers, cashiers, truck drivers, operators of machinery, attendants at retailers, stonemasons, carpenters, and nurses, among others. In 2010, there were 73 occupations listed whereas in 2015 there were only 59.

According to CONASAMI, in 2010 there were 2,462 municipalities in Mexico; 65 belonged to zone A, 55 to zone B, and 2,342 to zone C. Area C accounted for 63% of the workforce, while areas A and B accounted for 11% and 26%, respectively.

In November 2012, the legislation streamlined the geographic variation in the MW areas from three to two zones. Therefore, the new zone A included the municipalities of the previous areas A and B, whereas the new zone B was thereafter the municipalities that originally were in zone C. Thus, zone C never mixed its municipalities with any other zone. By October 2015, the legislation established a unique MW zone and by January 2019 there were two zones once again with one specifically for the municipalities at the US border. The intervention analysed in this study, which provides the basis for the empirical strategy, is the one introduced in 2012.

Fairris et al. (2008) and Moreno-Brid et al. (2014) argue that there have been annual upratings in the nominal MW in each zone based on the anticipated and not the current inflation rate since 1984. This was the unique criteria for the up-ratings which have had more of an inflation stabilization function (based on aggregate economic conditions) rather than one designed to preserve the minimum purchasing power of workers. Moreno-Brid et al. (2014) argued that the 2012 MW up-rating was less than the inflationary impact and therefore occurred only to maintain the purchasing power of 2005, suggesting that the MW up-rating was not sufficient for its main function of purchasing a basic alimentary basket of goods.

Table 3.1 reports the MW around the time of the 2012 intervention, the MW values have the same pattern established back in 1986, zone A held the highest MW and zone C the lowest.

| Period        | Min   | Minimum wage zone |       |  |  |  |  |  |  |
|---------------|-------|-------------------|-------|--|--|--|--|--|--|
| Fellou        | Α     | В                 | С     |  |  |  |  |  |  |
| January 2012  | 62.33 | 60.57             | 59.08 |  |  |  |  |  |  |
| November 2012 | 62.33 | 60.57             | -     |  |  |  |  |  |  |
| January 2013  | 64.76 | 61.38             | -     |  |  |  |  |  |  |

Table 3. 1: Daily General Minimum wages in Mexico

*Source*: CONASAMI, 2012-2013. Nominal Mexican pesos. 4.35 US dollars in zone C in January 2012 and 4.82 US dollars in January 2013 in zone B.

The institutional change in the MW setting creates two groups. The treatment group consists of those individuals in the municipalities that used to be in zone C in 2012, which became zone B by November 2012, for which the policy increased their daily general MW in nominal values. The municipalities in this zone were not combined with any other MW zone. It is a clearly defined treatment group (72.8% of the sample). The control group comprises the individuals in the original zone A, prior to and after the policy in 2012, where the intervention is not directly binding. The MW did not change nor the geographic zone (14.5% of the sample).<sup>68</sup> The next section describes the data used in this analysis.

#### 3.4 Data

The study exploits cross-sectional data obtained from various rounds (2010–2015) of the Mexican National Occupations and Employment Survey (ENOE -Spanish acronym) available from the Statistics, Geography, and Informatics Institute (INEGI-Spanish acronym), which contains information for gainfully occupied and unoccupied individuals aged 12 years and over. The ENOE is a nationally representative survey of individuals in Mexico. The survey reports, *inter alia*, weekly hours worked, monthly earnings, formal and informal job activities derived from the main and secondary occupations, and the type of employment (public or private). The second quarter interview period of the survey is primarily used to avoid any seasonality in earnings since higher expenses are reported during the first and fourth quarters of the year because of extra bonuses received for the employees in those quarters.

The sample is restricted to male workers because the information reported in terms of earnings and other labour market variables are more complete and comprehensive for this gender group. The questions related to the labour market apply only to the employed and gainfully occupied individuals during the week of reference. The main occupation is

<sup>&</sup>lt;sup>68</sup> The municipalities that belonged to Zone B before the intervention in 2012 are considered as the miscellaneous group as they are combined with the original zone A after the policy and, thus, are potentially contaminated by the treatment effect. This comprises 12.7% of the sample (22,100 observations). By January 2013 the usual annual up-rating was applied to the two MW zones.

identified by the interviewees as the one in which they spend most of their time during the day and which provides the highest remuneration.<sup>69</sup>

The questionnaire of the ENOE captures aspects of formal and informal employment. Among the information collected are the following: the economic sector where the individual works (e.g., services, agriculture, manufacturing); the type of occupation and activities undertaken (e.g., masonry, agricultural activities); the number of employees in the firm they are working in; if people own their businesses (or are farmers); and the type of rights the individuals are entitled to through their work contract.

This study uses the variable established by INEGI to define formal employment, which as mentioned above is mainly related to employment activities that provide access to social security or medical health services, which mostly coincide with work under a contract subject to employment rights. To ensure an accurate measure of the wage, all those individuals who are informal workers, self-employed or unpaid workers are excluded from the sample because the MW laws are not binding for this group of workers.<sup>70</sup>

The data for the daily general MW, as well as the municipalities that integrate each MW geographic zone, are obtained from the CONASAMI. This information allows the association of every observation to the corresponding MW zone depending on the municipality in which the individual resides.

The sample is a pooled cross-sectional database of 173,669 individuals from the period 2010 to 2015 with an average of 28,900 observations per year. It comprises 780 municipalities from the 32 Mexican States for the whole period with 500 municipalities per year on average. The minimum number of observations per cell is five (observations per municipality per year), the maximum is 982 and 382 on average. Approximately, 73% of the sample belong to the MW zone C, 14% to zone A, and 13% to zone B.

<sup>&</sup>lt;sup>69</sup> It depends entirely on the individuals what is the occupation that they perceived as the main or principal job. People with a secondary job are 6.2% of the sample used in this analysis.

<sup>&</sup>lt;sup>70</sup> Approximately 0.25% of the observations reported zero wages, which are excluded from the analysis.

#### **3.4.1** Outcome variable: log of hourly wage

The outcome variable of interest is the natural logarithm of the hourly wage. The earnings, in the survey, refer to a monthly payment received from the main job net of all labour taxes and social security contributions.<sup>71</sup> The hours worked reported weekly in the survey are multiplied by 4.3 to obtain the monthly values.<sup>72</sup> The hourly wages are computed from the reported monthly earnings divided by the computed monthly hours worked; the measure is in real Mexican pesos as of December 2010. This hourly wage variable is important to compare earnings among individuals. The analysis includes full-time and part-time workers.

Summary statistics of the wage variable within MW zones presented in Table 3.2 suggest wage gaps between the geographic MW zones, where zone C reported the lowest hourly wages. The Gini index, commonly used as an indicator for inequality and aggregate wage dispersion, yields values below 0.4 suggesting a not very unequal level of earnings (see Table 3.2). Similar values are reported for the Latin American region by Gasparini et al. (2015). Zone A has the highest Gini value, 0.379, and the highest mandated daily nominal MW.

|                  | Log hourly | 10 <sup>th</sup> | 25 <sup>th</sup> | 50 <sup>th</sup> | 75 <sup>th</sup> | 90 <sup>th</sup> | Cini  | Obc     |  |
|------------------|------------|------------------|------------------|------------------|------------------|------------------|-------|---------|--|
|                  | wage       | percentile       | percentile       | percentile       | percentile       | percentile       | Gilli | Obs.    |  |
| All observations | 3.30       | 2.60             | 2.87             | 3.21             | 3.66             | 4.16             | 0.374 | 173,669 |  |
| MW Zone A        | 3.33       | 2.65             | 2.91             | 3.23             | 3.69             | 4.22             | 0.379 | 25,094  |  |
| MW Zone B        | 3.36       | 2.68             | 2.97             | 3.30             | 3.68             | 4.16             | 0.360 | 22,100  |  |
| MW Zone C        | 3.28       | 2.57             | 2.85             | 3.19             | 3.65             | 4.15             | 0.375 | 126,475 |  |

Table 3. 2: Summary statistics of average wages

Source: Mexican National Occupations and Employment Survey (2010-2015).

Figure 3.1 illustrates the kernel density distribution of log hourly wages by MW zones for the full period analysed. The wage distributions of zone B and C are farther left from the distribution of zone A, although workers in zone C retain the lowest average wages than the employees in the other zones (represented by the vertical lines). The average of the mandatory hourly MW over the entire period is at the bottom end of the distribution and

<sup>&</sup>lt;sup>71</sup> If the interviewees report weekly payments, the Mexican Statistics, Geography, and Informatics Institute transforms this into monthly earnings by multiplying the former times 4.3.

<sup>&</sup>lt;sup>72</sup> The individuals work 48.7 hours per week, on average, and about 1% work between six and 18 hours per week in the sample.

below the average wages of the three zones (1.95 log points).<sup>73</sup> There are no clusters around the MW in the wage distribution, similar to what Bell (1997) and Fairris et al. (2008) observed for Mexico in the 1980s; and Bosch and Manacorda (2010) reported for 2001.<sup>74</sup>



Figure 3. 1: Distribution of log hourly wage by MW zones

*Source*: Mexican National Occupations and Employment Survey (2010-2015). *Notes*: Real values at 2010 Mexican pesos. The vertical lines represent the average log hourly MW in the three zones over the entire period (1.95 log points) and the average wages for each zone.

Figure 3.2 exhibits the daily general MW for each geographic zone. The values are deflated by the national consumer price index (INPC) and converted to December 2010 prices. The highest MW was assigned to zone A and the lowest to zone C. The vertical dashed line indicates the time when the amalgamation of zones A and B was undertaken. In the case of zone C, it became zone B after this merger.<sup>75</sup> This graph reveals common trends for the MW in each zone across the period analysed for both real and nominal values.

<sup>&</sup>lt;sup>73</sup> The log of hourly MW is computed as the logarithm of the daily MW divided by eight as it is the stipulated length of the workday in hours in Mexican legislation. See Table 3.3.

<sup>&</sup>lt;sup>74</sup> Esquivel (2015) argued that in 2015 the MW was below the market levels, even for unskilled workers, and below the accepted poverty threshold.

 $<sup>^{75}</sup>$  For the growth rate of the daily MW, see Figure C3.1 in Appendix C.



Figure 3. 2: Nominal and Real values of Daily Minimum Wage

Source: CONASAMI 2010-2015, January of each year. Real values in 2010 Mexican pesos. Daily general MW refers to eight daily hours worked.

Although there have been up-ratings in the daily MW by zones, the plot above reveals relatively stable real values between 2010-2014 that slightly increased in 2015. This trend in the real values has been noted extensively in the literature (Bosch and Manacorda, 2010; Campos-Vázquez et al., 2014; Moreno-Brid et al., 2014).

Table 3.3 reports the summary statistics for the daily general MW. The streamlined into two zones was occurred in November 2012 (A and B). The hourly MW is computed as the logarithm of the mandated daily MW divided by eight, the number of hours stipulated in the Mexican legislation for a workday, the average is 1.95 log points.

The last columns of Table 3.3 report the real log of hourly wages for the 10<sup>th</sup>, 50<sup>th</sup> and 90<sup>th</sup> percentiles from the sample for each MW zone, which generally decreased after 2012. By 2013, zone B decreased by 0.14 and 0.10 log points at the 10<sup>th</sup> and 50<sup>th</sup> percentiles compared to unchanged log points in zone A for the same percentiles. In the 90<sup>th</sup> percentile, there was a decrease of 0.02 and 0.06 log points in zone B and A, respectively.

|      | ו<br>Me) | Daily MW<br>xican pe | sos)  | Log  | hourly I | ww   | 10t<br>log l | h percer<br>hourly v | ntile<br>vage | 50th<br>Iog h | percen<br>ourly w | tile<br>/age | 90ti<br>log i | n percen<br>hourly w | itile<br>/age |
|------|----------|----------------------|-------|------|----------|------|--------------|----------------------|---------------|---------------|-------------------|--------------|---------------|----------------------|---------------|
| Year |          |                      |       |      |          |      | MW ge        | ographi              | c zone        |               |                   |              |               |                      |               |
|      | Α        | В                    | С     | Α    | в        | С    | Α            | В                    | С             | Α             | В                 | С            | А             | В                    | С             |
| 2010 | 57.46    | 55.84                | 54.47 | 1.97 | 1.94     | 1.92 | 2.68         | 2.69                 | 2.59          | 3.31          | 3.35              | 3.23         | 4.28          | 4.26                 | 4.21          |
| 2011 | 57.62    | 55.99                | 54.61 | 1.97 | 1.95     | 1.92 | 2.67         | 2.69                 | 2.59          | 3.28          | 3.27              | 3.20         | 4.30          | 4.14                 | 4.17          |
| 2012 | 57.97    | 56.33                | 54.95 | 1.98 | 1.95     | 1.93 | 2.67         | 2.70                 | 2.58          | 3.25          | 3.29              | 3.20         | 4.24          | 4.12                 | 4.15          |
| 2013 | 57.93    | 54.90                | -     | 1.98 | 1.93     | -    | 2.66         | 2.56                 | -             | 3.25          | 3.19              | -            | 4.18          | 4.14                 | -             |
| 2014 | 57.83    | 54.80                | -     | 1.98 | 1.92     | -    | 2.64         | 2.55                 | -             | 3.22          | 3.18              | -            | 4.15          | 4.12                 | -             |
| 2015 | 58.99    | 55.92                | -     | 2.00 | 1.94     | -    | 2.66         | 2.58                 | -             | 3.23          | 3.17              | -            | 4.14          | 4.13                 | -             |

Table 3. 3: Real Minimum wage and wages in Mexico

Source: Mexican National Occupations and Employment Survey (2nd quarter 2010-2015).

National Commission for Minimum Wages (2010-2015). January of each year (April in 2015). Real values in December 2010 Mexican pesos.

The distribution of log hourly wages before and after the MW up-rate in November 2012 is illustrated in Figure 3.3. The distribution and its average wage value shift slightly to the left after the MW up-rating, suggesting that the policy did not increase wages or offset the erosion of the real wages due to inflation over the period analysed.

Figure 3. 3: Distribution of log hourly wage pre-and post- the MW up-rate



*Source*: Mexican National Occupations and Employment Survey (2010-2015). Real values at 2010 Mexican pesos.

The wage policy in Mexico has been conceived as a mechanism to contain inflation. During the 1980s and 1990s the general rise in prices was counteracted, in part, by a strategy that

limited wage increases below the price increases, this scheme was not designed to preserve the minimum purchasing power of workers. Since the 2000s, the real MW has been remained constant and its purchasing power has drastically decreased (Esquivel, 2015; Fairris et al., 2008; Moreno-Brid et al., 2014).<sup>76</sup>

Esquivel (2015) argued that although there is no evidence that the wage increases would have inflationary effects in the 2010s, as in previous decades, the wage policy has not changed in favour of increasing real wages. In addition, even though the annual inflation rate of 2.1% in 2015 was the lowest rate, between 1974 and 2020, the average during the period analysed (2010-2015) was 3.7%, similar to 3.6% in 2012 when the MW policy was implemented, suggesting that the wage polices have not been containing inflation.<sup>77</sup>

Figures 3.4 to 3.6 present the wage distribution before and after the MW change by MW geographic zones. All zones present similar distributions for both periods with slight shifts to the left for the post-treatment period, suggesting that the policy did not increase wages.



Figure 3. 4: Distribution of log hourly wage pre-and post- MW up-rate, Zone A.

*Source*: Mexican National Occupations and Employment Survey (2010-2015). Real values at 2010 Mexican pesos.

<sup>&</sup>lt;sup>76</sup> By 2014, the MW was a quarter of its purchasing power in 1976 and only a third of its value in 1969.

<sup>&</sup>lt;sup>77</sup> The annual inflation rate in 2010 was 4.4%.



Figure 3. 5: Distribution of log hourly wage pre-and post- MW up-rate, Zone B.

*Source*: Mexican National Occupations and Employment Survey (2010-2015). Real values at 2010 Mexican pesos.

Figure 3. 6: Distribution of log hourly wage pre-and post- MW up-rate, Zone C.



*Source*: Mexican National Occupations and Employment Survey (2010-2015). Real values at 2010 Mexican pesos.

# 3.4.2 Treatment and control groups

The institutional change in the MW setting creates two groups, the treatment group subject to a change in the MW and the control group for which the intervention in 2012 did not change the MW rate or the geographic zone. Tables 3.4, 3.5 and 3.6 report statistics for hourly wages of treatment and control groups before and after the intervention. Higher average wages are reported for the control group and before the intervention for both groups.

| Variable                    | Treatment group | Control group |
|-----------------------------|-----------------|---------------|
| Log hourly wage             | 3.28            | 3.33          |
| 10 <sup>th</sup> percentile | 2.57            | 2.65          |
| 25 <sup>th</sup> percentile | 2.85            | 2.91          |
| 50 <sup>th</sup> percentile | 3.19            | 3.23          |
| 75 <sup>th</sup> percentile | 3.65            | 3.69          |
| 90 <sup>th</sup> percentile | 4.15            | 4.22          |
| Gini                        | 0.38            | 0.38          |
| Obs.                        | 126,475         | 25,094        |

Table 3. 4: Mean values of hourly wages by treatment and control groups

Source: Mexican National Occupations and Employment Survey (2010-2015).

| Table 3. | 5: Summary | statistics for | treatment g | group before | e and after | the interv | vention |
|----------|------------|----------------|-------------|--------------|-------------|------------|---------|
|          | •          |                |             |              |             |            |         |

| Variable                    | Before treatment | After treatment |
|-----------------------------|------------------|-----------------|
| Log hourly wage             | 3.29             | 3.26            |
| 10 <sup>th</sup> percentile | 2.58             | 2.56            |
| 25 <sup>th</sup> percentile | 2.86             | 2.83            |
| 50 <sup>th</sup> percentile | 3.21             | 3.18            |
| 75 <sup>th</sup> percentile | 3.66             | 3.63            |
| 90 <sup>th</sup> percentile | 4.18             | 4.13            |
| Gini                        | 0.38             | 0.37            |
| Obs.                        | 62,541           | 63,934          |

Source: Mexican National Occupations and Employment Survey (2010-2015).

Table 3. 6: Summary statistics for control group before and after the intervention

| Variable                    | Before treatment | After treatment |
|-----------------------------|------------------|-----------------|
| Log hourly wage             | 3.38             | 3.29            |
| 10 <sup>th</sup> percentile | 2.67             | 2.63            |
| 25 <sup>th</sup> percentile | 2.95             | 2.87            |
| 50 <sup>th</sup> percentile | 3.28             | 3.19            |
| 75 <sup>th</sup> percentile | 3.74             | 3.64            |
| 90 <sup>th</sup> percentile | 4.27             | 4.16            |
| Gini                        | 0.39             | 0.37            |
| Obs.                        | 12,226           | 12,868          |

Source: Mexican National Occupations and Employment Survey (2010-2015).

Figures 3.7 and 3.8 illustrate the distribution of log hourly wages by the treatment and control groups for selected years (2010–2015). The mandatory MW at the bottom end of the distribution in both graphs has not substantially changed over this period. The wage distribution in the treatment group in 2015 is farther left compared to 2010, indicating decreasing real hourly wages throughout the period analysed, even though the other distributions were similar (see Figure 3.7).

For the control group, the wage distribution in 2015 shifted slightly to the right compared to the distributions for the other years, indicating a marginal increase in the hourly wages of the control group compared to the fall reported for the treatment group (see Figure 3.8).<sup>78</sup>



Figure 3. 7: Distribution of log hourly wage by the treatment group

*Source*: Mexican National Occupations and Employment Survey (2010-2015). Real values at 2010 Mexican pesos. The vertical lines represent the log hourly MW.

Figure 3. 8: Distribution of log hourly wage by the control group



*Source*: Mexican National Occupations and Employment Survey (2010-2015). Real values at 2010 Mexican pesos. The vertical lines represent the log hourly MW.

Figure 3.9 contains the annual average log hourly wages for the 10<sup>th</sup> and 50<sup>th</sup> percentiles by treatment and control groups. The wage gap between these groups for the 10<sup>th</sup> percentile does

<sup>&</sup>lt;sup>78</sup> See Figure C3.2 in Appendix C for the kernel density of the miscellaneous group. There are no apparent changes in the wage distributions of the selected years.

not show any significant change before and after the intervention (represented by the vertical line). However, for the 50<sup>th</sup> percentile, the wage gap decreased from 2011, and after the reform, it narrowed sharply.<sup>79</sup> The average values of the treatment group are similar to the pooled sample as this group represents 72.3% of the sample.



Figure 3. 9: Trends of the log hourly wages for the 10<sup>th</sup> and 50<sup>th</sup> percentiles by group

*Source*: Mexican National Occupations and Employment Survey (2010-2015). The vertical line represents the time of the intervention.

Table 3.7 provides the standard difference in means of the log hourly wages between individuals treated and those not exposed to the MW up-rating, both before and after the intervention. <sup>80</sup> The effects reveal how the wages in these two groups changed before and after the policy implementation. The individuals in the treatment group have, on average, statistically significant lower real hourly wages compared to the control group for both pre-treatment and post-treatment periods, indicating that even before the implementation of the policy, the wages of the treated group were lower than that in the control group.

However, the difference-in-differences (D-i-D) is positive and statistically significant indicating that the intervention may have positive effects in raising wages of the treated group. In the absence of covariates, there is a 2.5% rise in hourly wages, on average, as a consequence of this policy. This is subsequently analysed in more detail using a variety of econometric methods in the next sections.

<sup>&</sup>lt;sup>79</sup> For the 90<sup>th</sup> percentile the annual average wage gaps narrow from 2011 (see Figure C3.3 in Appendix C).

<sup>&</sup>lt;sup>80</sup> The standard difference in means reported here does not include fixed effects or robust standard errors.

| Outcome var. | Obs.   | log hourly wage | S.Err. | [t]    | P> t     |
|--------------|--------|-----------------|--------|--------|----------|
| Before       |        |                 |        |        |          |
| Control      | 23397  | 3.376           |        |        |          |
| Treated      | 62541  | 3.294           |        |        |          |
| Diff (T-C)   |        | -0.082          | 0.005  | -17.00 | 0.000*** |
| After        |        |                 |        |        |          |
| Control      | 23797  | 3.319           |        |        |          |
| Treated      | 63934  | 3.262           |        |        |          |
| Diff (T-C)   |        | -0.057          | 0.005  | 12.02  | 0.000*** |
| Diff-in-Diff | 173669 | 0.025           | 0.007  | 3.61   | 0.000*** |

Table 3. 7: Difference-in-Differences estimation of wages for treated and control groups

The observations in the control group are 47,194 and 126,475 in the treatment group. Means and Standard Errors are estimated by linear regression Inference: \* p<0.1, \*\* p<0.05, \*\*\* p<0.01

#### **3.4.3** Explanatory variables

The explanatory variables included in the specifications are age and its quadratic, years of education, urban or rural settlement area, marital status, the economic sector, and the employment sector. Table 3.8 presents the summary statistics for the covariates used.

The sample includes individuals aged between 15 to 65 years, capturing the minimum legal working age and excluding retirees or individuals of pensionable age. The average age in the sample is 36 years. The married status encompasses married people and individuals living together; single status includes widows, and divorced status incorporates separated people. The public sector workers include individuals at a parastatal company; social services run by the government; as well as workers at federal, state, and municipal levels. The private sector comprises workers in organizations run for private profits and not controlled by the federal government or its agencies.

Most of the respondents reside in urban areas (69%), are married (72%), employed in the services sector (46%), and working in the public sector (22%). These individuals enjoy higher average wages. The average years of schooling in the sample is 11, less than the 12 years necessary to complete high school.

| Variable                 | Mean  | Std. Dev. | Min | Max |
|--------------------------|-------|-----------|-----|-----|
| Age                      | 36    | 11.40     | 15  | 65  |
| Years of schooling       | 11.07 | 3.92      | 0   | 24  |
| Urban status             | 0.69  | 0.46      | 0   | 1   |
| Marital status:          |       |           |     |     |
| Married                  | 0.72  | 0.45      | 0   | 1   |
| Divorced                 | 0.04  | 0.19      | 0   | 1   |
| Single                   | 0.24  | 0.43      | 0   | 1   |
| Working in Public sector | 0.22  | 0.41      | 0   | 1   |
| Economic sector:         |       |           |     |     |
| Agriculture              | 0.03  | 0.16      | 0   | 1   |
| Commerce                 | 0.17  | 0.38      | 0   | 1   |
| Construction             | 0.07  | 0.25      | 0   | 1   |
| Manufacturing            | 0.25  | 0.43      | 0   | 1   |
| Sevices                  | 0.46  | 0.50      | 0   | 1   |
| Mining and energy        | 0.03  | 0.18      | 0   | 1   |

Table 3. 8: Summary statistics of the explanatory variables

*Source*: Mexican National Occupations and Employment Survey (2010-2015). The total observations are 173,669.

Table 3.9 reports the statistics disaggregated by treatment and control groups. The magnitudes of these statistics are similar to those reported in Table 3.8.<sup>81</sup> Table 3.10 provides the t-test difference in means of the explanatory variables between individuals treated and not treated before the intervention.

|                          |       | Treatmer  | nt group |     |       | Control   | group |     |
|--------------------------|-------|-----------|----------|-----|-------|-----------|-------|-----|
| Variable                 | Mean  | Std. Dev. | Min      | Max | Mean  | Std. Dev. | Min   | Max |
| Age                      | 36    | 11.38     | 15       | 65  | 36    | 11.35     | 15    | 65  |
| Years of schooling       | 11.15 | 3.98      | 0        | 24  | 10.78 | 3.83      | 0     | 24  |
| Urban status             | 0.69  | 0.46      | 0        | 1   | 0.57  | 0.49      | 0     | 1   |
| Marital status:          |       |           |          |     |       |           |       |     |
| Married                  | 0.74  | 0.44      | 0        | 1   | 0.69  | 0.46      | 0     | 1   |
| Divorced                 | 0.03  | 0.18      | 0        | 1   | 0.04  | 0.20      | 0     | 1   |
| Single                   | 0.23  | 0.42      | 0        | 1   | 0.27  | 0.44      | 0     | 1   |
| Working in Public sector | 0.24  | 0.43      | 0        | 1   | 0.20  | 0.40      | 0     | 1   |
| Economic sector:         |       |           |          |     |       |           |       |     |
| Agriculture              | 0.03  | 0.16      | 0        | 1   | 0.03  | 0.17      | 0     | 1   |
| Commerce                 | 0.17  | 0.38      | 0        | 1   | 0.17  | 0.37      | 0     | 1   |
| Construction             | 0.07  | 0.25      | 0        | 1   | 0.06  | 0.24      | 0     | 1   |
| Manufacturing            | 0.24  | 0.43      | 0        | 1   | 0.26  | 0.44      | 0     | 1   |
| Sevices                  | 0.47  | 0.50      | 0        | 1   | 0.45  | 0.50      | 0     | 1   |
| Mining and energy        | 0.03  | 0.18      | 0        | 1   | 0.03  | 0.16      | 0     | 1   |

Table 3. 9: Summary statistics of the explanatory variables by treatment and control groups

*Source*: Mexican National Occupations and Employment Survey (2010-2015). The observations in the treatment group are 126,475 while there are 25,094 in the control group.

<sup>&</sup>lt;sup>81</sup> Tables B3.1 and B3.2 in appendix B report the statistics for each group pre-treatment and post-treatment.

| Variables (s)     | Mean Control | Mean Treated | Diff. | [t]   | Pr( T > t ) |
|-------------------|--------------|--------------|-------|-------|-------------|
| lg hourly wages   | 3.38         | 3.29         | -0.08 | 16.77 | 0.0000***   |
| schooling         | 10.80        | 11.10        | 0.30  | 9.70  | 0.0000***   |
| age               | 35.95        | 36.42        | 0.48  | 5.42  | 0.0000***   |
| age square        | 1425.93      | 1458.07      | 32.14 | 4.70  | 0.0000***   |
| urban             | 0.75         | 0.69         | -0.06 | 16.08 | 0.0000***   |
| public sector     | 0.19         | 0.26         | 0.07  | 20.53 | 0.0000***   |
| married           | 0.68         | 0.73         | 0.05  | 14.11 | 0.0000***   |
| divorced          | 0.04         | 0.03         | -0.01 | 4.80  | 0.0000***   |
| single            | 0.28         | 0.24         | -0.04 | 12.67 | 0.0000***   |
| Construction      | 0.07         | 0.06         | -0.01 | 5.15  | 0.0000***   |
| Manufacturing     | 0.26         | 0.23         | -0.03 | 8.90  | 0.0000***   |
| Commerce          | 0.17         | 0.17         | 0.00  | 0.19  | 0.8459      |
| Services          | 0.44         | 0.48         | 0.04  | 10.87 | 0.0000***   |
| Agriculture       | 0.03         | 0.03         | -0.01 | 5.08  | 0.0000***   |
| Mining and energy | 0.03         | 0.03         | 0.01  | 3.15  | 0.0000***   |
| Obs.              | 23,397       | 62,541       |       |       |             |

Table 3. 10: Two-sample t-tests of means of the explanatory variables pre-treatment

Total of observations (baseline): 85,938

Notes: \*p<0.1, \*\* p<0.05, \*\*\* p<0.01

# **3.5 Empirical Strategy**

This study exploits the geographic variation of the 2012 MW policy setting in the formal Mexican labour market to analyse its impacts across the wage distribution and identify its implications for wage inequality. To determine the impacts, the analysis uses the unconditional quantile (UQ) regression approach based on the Re-centred Influence Function (RIF) originally popularized by Firpo et al. (2009).

The implementation of the wage policy creates an exogenous source of variation in the wages of the municipalities exposed to the policy compared to those not exposed. Thus, a conventional methodology for comparing observations pre-treatment and post-treatment is a difference-in-differences (D-i-D) approach, which is used in this study.

A regression model using a D-i-D framework would be valid if, for example, the path of earnings in the absence of the treatment does not evolve differently across the treated and untreated groups prior to the intervention. In this context, it is also more relevant to look at the effects of the MW policy on the lower quantiles relative to the top quantiles of the unconditional distribution. The unconditional quantile approach provides a simple and direct

way to estimate the treatment effects at all points of the unconditional wage distribution, and potentially inform on the presence of inequality effects (Firpo et al., 2009).<sup>82</sup>

This paper focuses on identifying and estimating the specific distributional treatment effect parameter in the spirit of Hernaes (2018) in applying the RIF procedure based on the Firpo et al. (2009) methodology for unconditional outcomes, which assumes linearity with respect to the covariates. Hernaes (2018) uses UQ regression with a linear D-i-D model following the Firpo et al. (2009) methodology to estimate the average distributional effects of conditional outcomes based on a welfare policy change in Norway on the treated group compared to the control group. The OLS estimation of the UQ regressions allows it to be situated within a D-i-D framework.

The point of departure for understanding the RIF-based approach is the Influence Function (IF), extensively used in applied statistics, which represents the influence of an individual observation on the distributional statistic of interest  $v(F_Y)$ .<sup>83</sup> If the IF is centred around zero and the distributional statistic of interest is then added back to the IF and thus centred around the statistic of interest (e.g., the population mean " $\mu$ " E(Y)) and not zero (i.e., re-weighting the observations), then the RIF is generated (see Firpo et al., 2009).<sup>84</sup>

The expected UQ regression for quantiles is expressed in equation (1):

$$E[RIF(Y;q_{\tau};F_Y)|X] = m_{\tau}(X) \tag{1}$$

which is the conditional expectation of the  $RIF(Y; q_{\tau}; F_Y)$  modelled as a linear function of the explanatory variables where  $q_{\tau}$  is the population of the  $\tau^{\text{th}}$  quantile of the unconditional distribution of the outcome variable (Y), and where  $m_{\tau}$  represents the effect of the X covariates on the unconditional  $\tau^{\text{th}}$  quantile of the outcome variable (Y).

As this study uses the quantiles of the log hourly wages (w) as an outcome variable, therefore:

$$E[RIF(w; q_{\tau}; F_w)|X] = \gamma_{\tau}(X)$$
<sup>(2)</sup>

<sup>&</sup>lt;sup>82</sup> The quantile regression method relaxes the assumption of homogeneity in the effects of covariates across the unconditional distribution of the dependent variable.

<sup>&</sup>lt;sup>83</sup> Corresponding to the observable log of hourly wages.

<sup>&</sup>lt;sup>84</sup> In the case of the mean, since the RIF is simply the outcome variable *Y*, a regression of RIF (*Y*;  $\mu$ ) on *X* is the same as an OLS regression of *Y* on *X* (Firpo et al., 2009).

where  $\gamma$  represents the effect of the X covariates on the unconditional  $\tau^{th}$  quantile of the log hourly wages (*w*).

The corresponding RIF quantile for the log wage dependent variable can be expressed as:

$$RIF(w; q_{\tau}) = q_{\tau} + [\tau - I(w \le q_{\tau})] / f_w(q_{\tau})$$
(3)

where  $q_{\tau}$  is the population quantile of the  $\tau^{\text{th}}$  quantile of interest, and  $f_w(q_{\tau})$  is the density of the marginal distribution of the log hourly wages evaluated at  $q_{\tau}$  (using kernel density estimation techniques). I(.) is an indicator function, which takes the value of 1 if the expression in parentheses is satisfied and zero otherwise. Thus, the RIF takes the value of  $\tau/f_w(q_{\tau})$  if the log hourly wage (w) is above the quantile value and a value of  $[\tau - 1]/f_w(q_{\tau})$  if the log hourly wage (w) is below or equal to the quantile value.

A key feature of this approach is that the mean of the RIF provides the quantile statistic of interest (e.g., 5<sup>th</sup>, 10<sup>th</sup>, 50<sup>th</sup> or 90<sup>th</sup> percentiles, or other selected quantiles) and can be approximated by a linear function. The OLS-RIF estimates first are derived from a linear probability model and are transformed into quantile effects using the reciprocal of the kernel density estimates at the relevant quantile.<sup>85</sup> This means transforming probabilities into unconditional quantiles using the inverse of the probability density function of the log hourly wage distribution at each quantile. The RIF quantiles of the wage distribution are the dependent variables for each  $\tau$ <sup>th</sup> quantiles from the 5<sup>th</sup> to the 95<sup>th</sup> percentiles in this study.

The approach involves regressing the empirical RIF quantiles, constructed using expression (3), on a set of covariates using a standard OLS procedure within a D-i-D framework. Two different groups are used to assess the impact of the policy intervention: the "treatment group" likely affected by the policy, and a suitably chosen "control group". The latter is unaffected by the intervention, but otherwise similar to the treatment group (e.g., other jurisdictions or states where the policy did not change, high and low wage regions and/or demographic groups).<sup>86</sup>

<sup>&</sup>lt;sup>85</sup> In this analysis, the probability that the log hourly wage is below some pre-stated quantile value.

<sup>&</sup>lt;sup>86</sup> The parallel trends assumption cannot be tested in this specific case because in the absence of the intervention, the outcomes in the treatment and control groups would have not necessarily evolve in a parallel fashion

The extent of variability in minimum wages across geographic zones is important for the empirical identification of wage inequality (treatment and control groups). The use of differences in the average wages across zones and municipalities with different MW incidence provided by the policy, which are assumed to be exogenous MW shocks, induces useful variation in the real bite of the MW net of the other confounding forces (see Ham, 2018 for Honduras).

This study seeks to determine the impacts of exposure to the MW intervention. The RIF unconditional quantiles treatment effects, of the observed log of hourly wages given the explanatory variables, compared to the impacts on the control group.

The RIF quantile regressions within a D-i-D approach using cross-sectional data are implemented using equation (4):

$$\widehat{RIF} (w_i, \widehat{q_\tau})_i = \beta_{0\tau} + \beta_{1\tau} (POST12_i) + \beta_{2\tau} (POST12_i * TREAT_i) + \beta_{3\tau} (POST12_i * Misc_i) + \beta_{4\tau} X_i + \delta_{k\tau} + e_{i\tau}$$
(4)

In this regression, the RIF provides the quantiles for each percentile of  $w_i$ , which is based on the log hourly wage of individual *i* at the quantile  $\tau$ . *POST*12<sub>*i*</sub> is a dummy variable assuming the value of one for all the observations on and after November 2012. *TREAT*<sub>*i*</sub> is a dummy variable indicating whether the individual *i* belongs to the municipalities treated after November 2012. *Misc*<sub>*i*</sub> takes the value of one if individual *i* belongs to the municipalities in the miscellaneous composite group after the policy in 2012.

As already noted,  $X_i$  comprises a set of covariates consisting of years of education, age and its square, urban settlement status, marital status, the economic sector, and the employment sector.  $\delta_k$  are municipality fixed effects to control for macroeconomic shocks at the municipal level, and  $e_i$  is a random idiosyncratic error term.

The terms  $(POST12_i * TREAT_i)$  and  $(POST12_i * Misc_i)$  are the interactions terms between the dummy variables for the MW intervention and each of the treatment and miscellaneous

<sup>(</sup>Neumark et al., 2014 and Allegretto et al., 2017 cited in Dube (2019)). However, Figure 3.2 illustrates the assumption of parallel trends graphically.

groups respectively. The parameter of the former,  $\widehat{\beta_{2\tau}}$ , represents the treatment effect due to the MW policy at each  $\tau^{\text{th}}$  quantile and is the key parameter of interest in this specification and for the empirical analysis undertaken here. It measures the effect of increasing the MW incidence on the unconditional wage distribution using the RIF transformation.

The RIF-based regressions can also be extended to a variety of inequality measures. The RIF Gini approach provides a linear approximation of highly non-linear functionals such as the Gini coefficient. It offers insights on wage dispersion and is also estimated in this analysis. Only a few papers have used the RIF-Gini regressions to date to investigate changes in income (or wage) inequality (Firpo et al., 2018).<sup>87</sup>

The Gini coefficient is defined in equation (5):

$$v^G(F_W) = 1 - 2\mu^{-1} R(F_W) \tag{5}$$

where  $R(F_W) = \int_0^1 GL(p; F_W) dp$  with  $p(W) = F_W(W)$  and where  $GL(p; F_W)$  is the generalized Lorenz ordinate of  $F_W$  given  $GL(p; F_W) = \int_{-\infty}^{F^{-1}(p)} z dF_W(z)$ . The generalized Lorenz curve tracks the cumulative total of y (i.e., the outcome variable) divided by total population size against the cumulative distribution function (CDF). This can be interpreted as the proportion of hourly earnings going to a specific per cent of the lowest wage earners.

The RIF of the Gini coefficient following Firpo et al. (2018) can be written as:

$$RIF(W; v^{G}, F_{W}) = 2\frac{y}{\mu} \left[ F_{W}(W) - \frac{(1+v^{G})}{2} \right] + 2\left[ \frac{(1-v^{G})}{2} - GL(p; F_{W}) \right] + v^{G}$$
(6)

where  $\frac{(1+v^G)}{2}$  and  $\frac{(1-v^G)}{2}$  correspond to the areas above and below the Lorenz curve, respectively. The first term is unbounded because it increases by the factor W/µ, while the second is bounded between  $v^G - 1$  and  $1 + v^G$ . Thus, the  $RIF(W; v^G, F_W)$  is continuous and convex in W, the hourly wages. The function is theoretically unbounded from above, but in practice, it reaches its maximum at the upper bound of the empirical support of the

<sup>&</sup>lt;sup>87</sup> Such as Gradín (2016), and Choe and Van Kerm (2014) cited in Firpo et al. (2018).

distribution. This implies that the Gini coefficient is not robust to measurement error at higher earnings.

The RIF-Gini within a D-i-D approach is implemented using the specification reported in equation (7) below:

$$\widehat{RIF} \left( v^{G}_{i}, F_{W} \right)_{i} = \theta_{0} + \theta_{1} \left( POST12_{i} \right) + \theta_{2} \left( POST12_{i} * TREAT_{i} \right) + \theta_{3} \left( POST12_{i} * Misc_{i} \right)$$
$$+ \theta_{4}X_{i} + \delta_{k} + \eta_{i}$$
(7)

where the dummy variables  $POST12_i$ ,  $TREAT_i$  and  $Misc_i$  are analogous to those used in equation (4). Likewise,  $X_i$  comprises the set of covariates, similar to the ones used in equation (4).  $\delta_k$  are the municipality's fixed effects and  $\eta_i$  is a random idiosyncratic error term.

The estimated coefficient  $\widehat{\theta_2}$  of the interaction between the dummy variables for the MW intervention and the treatment group (*POST*12<sub>*i*</sub> \* *TREAT*<sub>*i*</sub>) represents the treatment effects due to the policy on the RIF-Gini coefficient, which is of primary interest in this specification.

It is worth noting that the Gini index cannot distinguish between the decline in wage inequality in the lower or the upper part of the wage distribution. This limitation emphasizes why the analysis reported here primarily focuses on quantiles. Nonetheless, the RIF-Gini complements the results derived from the UQ regressions, which provides an insight into the impacts of the MW policy on overall wage inequality.

#### **3.6 Empirical Results**

The empirical results shed light on the wage inequality patterns observed in Mexico from 2010 to 2015 regarding the role of the 2012 MW up-rating implemented. The estimated effects of key interest for the RIF-quantiles and the RIF-Gini specifications are captured by  $\widehat{\beta_{2\tau}}$  and  $\widehat{\theta_2}$ .

# 3.6.1 Unconditional Quantile Treatment Effects

The standard OLS estimate for the logarithm of hourly wages reports, on average, an increase of 5.6% in wages as a consequence of the MW up-rating (see Table 3.11).<sup>88</sup> It is acknowledged that this average MW effect appears somewhat on the high side given the average increase of 2.5% reported earlier in Table 3.7. The estimated UQ effects of  $\hat{\beta}_{2\tau}$  from equation (4) for the 5<sup>th</sup> to the 95<sup>th</sup> percentiles of the log hourly wage distribution provide evidence that the exposure to an up-rating for formal employees in the treated MW zone increases wages across the entire unconditional wage distribution (see Figures 3.10 and 3.11).

The treatment effects in the lower tail of the wage distribution range from 3.2% at the 5<sup>th</sup> percentile to 5.7% at the 30<sup>th</sup> percentile. These effects subsequently oscillate around the mean value of 5.6% up to 6% at the 50<sup>th</sup> percentile (see Figure 3.10). Then they increase from 5.2% at the 51<sup>st</sup> percentile up to 7.9% at the 82<sup>nd</sup> percentile, after which they range from 4.2% to 8.9% at the 95<sup>th</sup> percentile, the peak (see Figure 3.11).<sup>89</sup>

Estimated treatment effects are most precise below the 85<sup>th</sup> percentile. As the effects fluctuate more in the upper part of the unconditional wage distribution. Thus, the increase in wages associated with the policy was not only for the lower-paid workers but also experienced up to the higher-paid workers, with sharper impacts for the latter. The UQ estimates suggest the impacts of the policy vary at different parts of the unconditional wage distribution, something that has been found in the literature to date.<sup>90</sup> Almost all the confidence intervals for the quantile estimates contain the OLS estimate, suggesting there is very little heterogeneity in the MW effect across the unconditional wage distribution (see Figures 3.10 and 3.11).

<sup>&</sup>lt;sup>88</sup> See Appendix A3.1 for details of this model specification. Table B3.3 in Appendix B reports all the coefficient results of the standard OLS regression with log of hourly wages as dependent variable.

<sup>&</sup>lt;sup>89</sup> See Table B3.4 in Appendix B for all the coefficient results of selected percentiles.

<sup>&</sup>lt;sup>90</sup> The estimated effects for years of schooling shows that for the first lower half of the distribution all the values are below the standard OLS estimation of log hourly wages (7.2%), they remain positive and statistically significant all the way up to the top of the distribution, suggesting that there is a fanning out effect of education on the wage distribution. The effects are considerably lower (higher) at the 10<sup>th</sup> (90<sup>th</sup>) percentile of the unconditional hourly wage distribution, 3% and 13%, respectively. This reinforces previous findings in the literature on the role of education in widening wage inequality.



Figure 3. 10: Effects of the MW up-rating on the lower half of the wage distribution

Figure 3. 11: Effects of the MW up-rating on the upper half of the wage distribution



Table 3.11 reports the estimates of the standard OLS regression for the log of hourly wages, the RIF-Gini, and selected RIF-quantiles estimates. The treatment effects reflect the results
contained in the previous graphs. The estimates rise steadily up from the  $10^{\text{th}}$  (3.3%) to the  $75^{\text{th}}$  percentile (6.3%) of the distribution and then decline at the 90<sup>th</sup> percentile (5.5%). The mean effect (5.6%) is somewhat homogeneous across the wage distribution, and thus, the MW up-rating is increasing wages similarly across the entire unconditional distribution. Therefore, the MW policy does not have any particular effect on wage inequality, as confirmed by the statistically insignificant coefficient of the RIF-Gini (see column 2 of Table 3.11).

|                   | (1)                | (2)      | (3)      | (4)      | (5)      | (6)      | (7)     |
|-------------------|--------------------|----------|----------|----------|----------|----------|---------|
|                   | Log hourly<br>wage | RIF-Gini | RIF 10   | RIF 25   | RIF 50   | RIF 75   | RIF 90  |
| Treatment effects | 0.056***           | 0.011    | 0.033*** | 0.050*** | 0.057*** | 0.063*** | 0.055** |
|                   | (0.010)            | (0.009)  | (0.012)  | (0.015)  | (0.017)  | (0.016)  | (0.023) |
| Obs.              | 173,669            | 173,669  | 173,669  | 173,669  | 173,669  | 173,669  | 173,669 |
| Covariates        | Yes                | Yes      | Yes      | Yes      | Yes      | Yes      | Yes     |

Table 3. 11: MW Treatment effects for selected percentiles

*Notes:* \*\* p<0.05, \*\*\* p<0.01

The sample is constructed from the 2010-2015 Mexican National Occupations and Employment Survey. Municipality Fixed effects. Standard errors adjusted for 780 clusters at municipality level in parentheses. Model (1) OLS standard estimation.

The policy does benefit the individuals in the treated group by increasing their wages. The treatment effects observed are consistent with the positive impacts of the MWs on the mid-to-lower tail of the wage distribution found in the literature (e.g., Bosch and Manacorda, 2010; Campos-Vázquez et al., 2017; Fairris et al., 2008). Although, the average effects reported here are larger than the average increase of 2.5% reported in Table 3.7.<sup>91</sup> It is in comport with the MW intervention's goal of raising the earnings of the individuals receiving the lowest wages. However, the MW's role as a benchmark for wage adjustment does not appear to reduce overall wage inequality in Mexico.

The mid-to-lower tail effect of the wage distribution could be a consequence of the low MW value, located at the bottom end of the distribution. Therefore, it could influence the pay of workers positioned below the median of the wage distribution. It highlights a similar

<sup>&</sup>lt;sup>91</sup> Although, the values are sizable in this analysis compared to the magnitude of previous effects found for Mexico, which were of the order of 1.6%-2.6%.

argument offered by Bell (1997) and Fairris et al. (2008) that the deterioration in the real value of the MW could reduce the potential ability to compress the earnings distribution.

The positive effect at the mean-based may reflect that the MW up-rating is successful to some extent in offsetting the eroding effects of inflation on real wages in the treated zone. Although Figure 3.6 suggests, the MW level is likely to be too low to influence wages for the mid-to-upper tail of the pay distribution. The impacts at the top of the wage distribution have not been alluded to extensively in the literature. <sup>92</sup> Some of these effects are perhaps due to the strong role that the numeraire plays in the Mexican economy, and the use of the MW as a benchmark for bargaining over workers' payment even in the uncovered informal sector (Azuela de la Cueva et al., 2014; Fairris et al., 2008; Maloney and Nuñez, 2003; Moreno-Brid et al., 2014).

The sizable impacts beyond the median of the wage distribution in light of the 2012 MW uprating are questionable as these are higher compared to those found in the literature to date. Bosch and Manacorda (2010) argued the spill-over effects to higher percentiles of the earnings distribution up to the 60<sup>th</sup> percentile, in their analysis, is due to the role of the numeraire in the Mexican economy. Their effects were found stronger at the bottom rather than at the top end of the wage distribution.

Hence, this analysis acknowledges that there must be other institutional factors and events that were concurred in the Mexican labour market with the MW up-ratings during 2010-2015, and that exerted an influence on the entire unconditional wage distribution, especially at the top end. Thus, the interpretation of the estimates is questionable, and the role of such confounders challenge the causal identification of the key effect of interest.

### **3.6.2 RIF-Gini coefficients**

The RIF-Gini approach assesses the overall effects of the MW policy on the average wage inequality. The estimated effects,  $(\widehat{\theta}_2)$ , for specification (7) complement the UQ estimates presented above. The analysis uses the pooled sample and two additional sub-samples that

<sup>&</sup>lt;sup>92</sup> Autor et al. (2016) found for the US over the period 1979-2012 effects on the entire wage distribution that were explained mainly by the presence of measurement error in earnings when reporting the information in the survey.

comprise the public and private sectors. These subsamples are used as the literature emphasizes that the employment differences could lead to widening inequality in Mexico.<sup>93</sup>

The results in Table 3.12 suggest that exposure to a certain MW up-rating does not reduce wage inequality. This corroborates the RIF quantile results of not decreasing inequality. On the contrary, the positive coefficients suggest that the MW policy could increase inequality, although only statistically significant in the public sector. The MW up-rating appears to be a mechanism that plays a crucial role in widening wage inequality for public sector workers (i.e., about 4.4 percentage points).<sup>94</sup>

|                  | RIF-Gini approach<br>Hourly wage |                  |                    |  |  |  |  |
|------------------|----------------------------------|------------------|--------------------|--|--|--|--|
|                  | (1)                              | (2)              | (3)                |  |  |  |  |
| Sample           | Pooled sample                    | Private sector   | Public sector      |  |  |  |  |
| Treament effects | 0.011<br>(0.009)                 | 0.000<br>(0.006) | 0.044**<br>(0.021) |  |  |  |  |
| Obs.             | 173,669                          | 135,379          | 38,290             |  |  |  |  |
| Covariates       | Yes                              | Yes              | Yes                |  |  |  |  |

Table 3. 12: RIF-Gini coefficients of unconditional estimates

*Notes:* \*\* p<0.05, \*\*\* p<0.01

The sample is constructed from the 2010-2015 Mexican National Occupations and Employment Survey. Municipality Fixed effects. Standard errors adjusted at municipality level in parentheses.

The estimates in the RIF-quantiles are mirrored in the RIF-Gini coefficients for the subsample of public sector workers with a strong effect on inequality. Potentially, some quantile effects observed between 2010 to 2015 reflect the changes in the wage dispersion within the public sector. However, the public sector is 22% of the total labour market, thus the overall effect on inequality is attenuated by this percentage. In addition, the effects may also be driven coincidently by other factors that occurred in the labour market over the same period.

<sup>&</sup>lt;sup>93</sup> Gasparini et al. (2015); Mora-Salas and de Oliveira (2009); Pagán et al. (2002); Panizza et al. (2001).

Figure C3.4 in Appendix C illustrates the trends of log hourly wages by employment sector.

<sup>&</sup>lt;sup>94</sup> Table B3.5 in Appendix B reports all the coefficients of the RIF-Gini specification.

For instance, it is arguable that the RIF-Gini impacts possibly coincide with the spending boom introduced by the Federal government in its efforts to mitigate the impacts of the financial crises in 2008, part of which found its way into public sector pay awards. The next section investigates further the effects of the MW up-rating on private and public sector workers as a robustness check.

#### **3.7 Robustness checks**

The 2012 MW intervention may have had an impact on the structure of the labour market in Mexico in terms of the changes in relative employment and earnings separately for the public sector and private sector workers. The effects of this policy are now analysed for these sector workers.<sup>95</sup>

#### 3.7.1 Private sector

The sub-sample for the private sector workers comprises 135,379 observations (78% of the total sample). The treatment effects are reported in Figures 3.12 and 3.13. Compared to the mean-based impacts of 5% (see Table 3.13),<sup>96</sup> in the lower tail of the wage distribution, the MW policy raises wages by between 4%–6.7% from the 5<sup>th</sup> to the 43<sup>rd</sup> percentiles. The latter is the peak across the entire unconditional wage distribution, while the lowest effect of 3% is at the 16<sup>th</sup> percentile. The impacts subsequently fluctuate more in the second half of the distribution, between 6.5% at the 61<sup>st</sup> percentile and 3.3% at the 68<sup>th</sup> percentile.

Table 3.13 reports the OLS, the RIF-Gini and unconditional estimated effects for selected percentiles. Changes in the MW have strong and positive effects on private-sector wages, although the magnitude decreases by the 75<sup>th</sup> percentile and the statistical significance weaken afterwards. It suggests the policy increases wages similarly across the unconditional wage distribution and thus, not widening inequality among private-sector employees.

<sup>&</sup>lt;sup>95</sup> A placebo in time test is implemented for the pre-treatment period with a false year for the introduction of the Federal Law in 2011 and 85,938 observations. The results are not statistically significant as expected.

<sup>&</sup>lt;sup>96</sup> The estimated effect comes from the OLS regression of the log hourly wages.



Figure 3. 12: Treatment effects on the lower half of the wage distribution in the private sector

Figure 3. 13: Treatment effects on the upper half of the wage distribution in the private sector



|                   | (1)                 | (2)              | (3)                 | (4)                 | (5)                 | <mark>(</mark> 6)   | (7)              |
|-------------------|---------------------|------------------|---------------------|---------------------|---------------------|---------------------|------------------|
|                   | Log hourly<br>wage  | RIF-Gini         | RIF 10              | RIF 25              | RIF 50              | RIF 75              | RIF 90           |
| Treatment effects | 0.050***<br>(0.011) | 0.000<br>(0.006) | 0.042***<br>(0.014) | 0.055***<br>(0.016) | 0.054***<br>(0.018) | 0.052***<br>(0.015) | 0.031<br>(0.017) |
| Obs.              | 135,379             | 135,379          | 135,379             | 135,379             | 135,379             | 135,379             | 135,379          |
| Covariates        | Yes                 | Yes              | Yes                 | Yes                 | Yes                 | Yes                 | Yes              |

Table 3. 13: Unconditional estimates in the private sector for selected percentiles

Notes: \*\* p<0.05, \*\*\* p<0.01

The sample is constructed from the 2010-2015 Mexican National Occupations and Employment Survey. Municipality Fixed effects. Standard errors adjusted for 769 clusters at municipality level in parentheses. Model (1) OLS standard estimation.

In both the pooled and the private sector sub-sample, the estimates are stronger at the bottom end than at the top end of the unconditional pay distribution. However, the mean estimate falls within the confidence intervals of most of the quantile regression estimates suggesting the mean effect exhibits some degree of homogeneity across the distribution. It is worth noting that some of the impacts at the top of the wage distribution in the private sector could be related to the role that the numeraire plays in the Mexican labour market as a benchmark for wage adjustments, but most probably other different economic factors were coincidental with the MW up-rating in the private sector between 2010 and 2015. They could also increase wages across the pay distribution.

#### 3.7.2 Public sector

The quantile estimates for a sub-sample of 38,290 public sector workers (22% of the sample) are contained in Figures 3.14 and 3.15. The up-ratings in the MW have positive and statistically significant effects on wages mostly after the 17<sup>th</sup> percentile and further up the distribution until the 78<sup>th</sup> percentile. The stronger effects are centred around the median. In the lower part of the pay distribution, all the estimated effects are below the mean-based coefficient of 6.5% (see Table 3.14). The 69<sup>th</sup> percentile goes beyond this value for increasing wages by 8.2% and 9.2% at the 72<sup>nd</sup> percentile. The highest estimate is at the 94<sup>th</sup> percentile with 19.7%.



Figure 3. 14: Treatment effects on the lower half of the wage distribution in the public sector

Figure 3. 15: Treatment effects on the upper half of the wage distribution in the public sector



These impacts are also reported in Table 3.14 for selected percentiles. At the median, the treatment effect raises earnings by 5.8%. The RIF-Gini estimates reported that the MW may be implicated in driving wage inequality in the public sector (4.4 percentage points).

|                   | (1)                 | (2)                | (3)              | (4)                | (5)                 | <mark>(</mark> 6) | (7)              |
|-------------------|---------------------|--------------------|------------------|--------------------|---------------------|-------------------|------------------|
|                   | Log hourly<br>wage  | RIF-Gini           | RIF 10           | RIF 25             | RIF 50              | RIF 75            | RIF 90           |
| Treatment effects | 0.065***<br>(0.018) | 0.044**<br>(0.021) | 0.019<br>(0.013) | 0.041**<br>(0.017) | 0.058***<br>(0.018) | 0.056<br>(0.032)  | 0.102<br>(0.062) |
| Obs.              | 38,290              | 38,290             | 38,290           | 38,290             | 38,290              | 38,290            | 38,290           |
| Covariates        | Yes                 | Yes                | Yes              | Yes                | Yes                 | Yes               | Yes              |

Table 3. 14: Unconditional estimates in the public sector for selected percentiles

*Notes:* \*\* p<0.05, \*\*\* p<0.01

The sample is constructed from the 2010-2015 Mexican National Occupations and Employment Survey. Municipality Fixed effects. Standard errors adjusted for 733 clusters at municipality level in parentheses.

unicipality Fixed effects. Standard errors adjusted for 733 clusters at municipality level in parenti

Model (1) OLS standard estimation.

Overall, the MW policy is not associated with reducing inequality. It raises wages, though this effect is found to be stronger in the public sector. The impacts beyond the median of the wage distribution are higher compared to those generally found in the literature to date (e.g., Autor et al., 2016; Bosch and Manacorda, 2010). This is not entirely implausible, especially for the public sector, which uses the MW as a numeraire for increments in salary payments, although higher spending for skilled public sector labour after the 2008 financial crisis may have coincided with these effects.

#### **3.8 Conclusions**

This chapter examines the role of an institutional feature of the Mexican labour market that potentially impacts the wage distribution in the country's formal sector. The 2012 Minimum Wage (MW) policy was introduced as a wage floor system to protect the low-paid, narrow the pay distribution at the bottom end, lessen inequality, and reduce poverty.

The study provides evidence that the MW up-rating in 2012 increased both the average wage and wages across the unconditional hourly wage distribution by comparable amounts over the period from 2010 to 2015. The average effect was over 5%. Although the results of a positive effect on wages are consistent with earlier findings for Mexico, the magnitudes of

these effects are found to be larger than in the literature to date and this raises questions regarding the identification strategy used here.

In addition, there is little evidence that the MW affected wage inequality overall. However, there is some evidence that it widened inequality in the public sector according to the RIF-Gini estimates. These findings may reflect the role of the MW as numeraire (i.e., the MW is used as a norm for indexing prices to multiples or fractions of the MW such as fines, pensions, extra bonuses, etc.). It is a strong institutional feature in Mexico and it may explain the uprating effects that resonate across the unconditional distribution. However, a very high degree of re-benchmarking wages, beyond the median of the wage distribution, would not fully explain the effects detected at the top end of the unconditional wage distribution observed. The findings higher up the distribution are somewhat questionable as, for instance, the spill-over effects found by Bosch and Manacorda (2010) between 1989 to 2001 persisted only up to the 60<sup>th</sup> percentile.

There are likely to be confounders in the labour market that coincided with the introduction of this MW intervention around the time it was implemented. It could explain the magnitude of the estimates obtained and their persistence across the distribution. This was a period of macroeconomic turbulence and real wage decline, and this context may be of some relevance here. In addition, the size of the treated group is large relative to the control group and this may render the treatment group more heterogeneous and prone to the influence of other confounding labour market factors.

Overall, the empirical findings are suggesting that the internal validity of the estimate is likely compromised and that the effects reported are not interpretable as representing the causal effects of the 2012 MW up-rating. An investigation to identify and isolate the putative confounders is confined to an agenda for future research for trying to explain and understand the MW effects detected at the top end of the pay distribution in this study. Nevertheless, it has demonstrated how RIF-based methods that exploit quantile and Gini-based regressions can potentially enhance the distributional analysis of MW effects in both developing and developed countries' labour markets.

Finally, a key sub-theme of this research has been that the public sector pay-setting mechanism appears to play some role in raising inequality. The debate about the nature and design of the public sector pay-setting mechanism has been a persistent feature of debate in Mexico since the turn of the 20<sup>th</sup> century. The issue of higher public sector wages is the subject of greater discussion and more detailed analysis in the next empirical chapter.

# **CHAPTER FOUR:** The Impact of a Government Pay Reform in Mexico on the Public Sector Wage Gap

#### **4.1 Introduction**

The size of the public–private sector pay gap has been the subject of extensive research over the past decades across both developed and developing countries. The existing studies have found a generally significant public sector premium in developing countries than in developed countries. This premium typically drives widening effects on pay dispersion between public and private sector workers (Finan et al., 2015; Gasparini et al., 2015; Gindling et al., 2019; Lausev, 2014).

In Latin America, Mizala et al. (2011) have provided empirical evidence of positive sectoral wage gaps due to higher premia for public sector workers. There is evidence of widening wage gaps in developed economies (e.g., Cai and Liu, 2011 for Australia; Hospido and Moral-Benito, 2016 for Spain), and also sectoral pay gap reductions (Bargain et al., 2018 for France; Christofides and Michael, 2020 for European countries).

Notwithstanding the extensive literature on public–private wage differentials, studies analysing the impacts of wage policies in the public sector, which are designed to narrow the gap between the two sectors, are limited. This scarcity may be because the implementation of these policies has been undetermined by a failure to implement the recommended changes effectively, or it may be due to the difficulties in providing evidence with a precise scientific measure of their success (Ampofo and Doko Tchatoka, 2019). Among the available studies exploring public sector pay policies on the public–private sector pay gap are Ampofo and Doko Tchatoka (2019) for Ghana; Telegdy (2018) for Hungary; and Vladisavljević (2020) for Serbia.

In Latin America, reforms of the state apparatus for renewing and modernizing the civil service administration generally began in the 1990s. The policies could partly reflect the government's concerns regarding the presence of inefficiencies in the public sector. They constituted an effort to attract and retain highly skilled personnel to enhance efficiency and

transparency in the government, which indirectly impacted the public and private sector wages (Lah and Perry, 2008).

In Mexico, higher wages in the public sector have been of concern among policy-makers since 2009, which exerted pressure on the Federal government to impose limits and regulate them. In late 2017, higher public sector wages assumed primary importance during the Mexican presidential election campaign and served as a political strategy. It would advocate that government finances are brought under control by getting more wage parity for public sector work. It would ultimately provide an effective and efficient public administration and it might also secure votes and political support.

Despite the political discourse, after the elections in July 2018, the Federal Pay Reform on the Remuneration of Public Servants was enacted in November 2018 by the new government to gain credibility and increase its popularity among certain voters. This policy set a salary cut for senior management positions and froze wage payments for lower and middle management jobs. This reform was designed to contract the pay gap with respect to the private sector, particularly at the top end of the wage distribution.

The current work aims to assess the impact of this legislative change and shed light on the magnitude of the wage gap between public and private sector workers in Mexico. There are two motivations for the empirical analysis undertaken in this chapter. One is to contribute to the empirical evidence of the public sector pay gap, which suggests relatively higher wage levels, ultimately leading to wage differentials between the public and private sectors. The second comes from assessing the recent introduction of the Federal Law in Mexico, which regulates the remuneration of public sector employees.

Therefore, this study investigates the impact of the 2018 Pay Reform across the pay distribution using cross-sectional data (2017-2019) from the National Occupations and Employment Survey (ENOE). It uses both mean and unconditional quantile (UQ) regression approaches and exploits an exogenous source of variation in wages provided by the policy. The use of UQ regressions within a Difference-in-Differences (D-i-D) framework potentially provides an understanding of the effects of the reform on the wage gap between public and private sector workers, before and after the policy intervention.

The study acknowledges that an important consideration for the analysis is the role of unobservables in the employment selection choice. However, since the D-i-D approach takes into account different unobservable effects in the public and the private employment sector selection choice pre-treatment and post-treatment, such unobservables are differenced out, and the concerns about a potential selection bias are mitigated.

The empirical results provide evidence that the exposure to a regulation on the salary introduced in the public sector is implicated in reducing the public–private sector wage gap across the unconditional wage distribution, with the effects generally stronger below the median than above it. The estimates suggest reductions of the public sector pay premium by about 24% and 11% at the bottom end and the median of the pay distribution, respectively. Thus, more potent effects for lower-paid public sector workers than higher-paid employees.

The analysis undertakes robustness checks to assess the stability of the results. In particular, a placebo test in time is also conducted using an earlier time for the introduction of the reform. The test reveals the presence of anticipation effects by public sector employees which undermines the internal validity of the exercise, though plausible in the current context.

The contribution of this study is across several dimensions. First, to the limited literature analysing wage policies for the public–private wage gaps in developing countries. Second, it is among the few studies using unconditional quantiles regressions for evaluating the public sector pay premium and the wage differential between public and private sector workers. Third, it is the first study that uses the 2018 Federal Pay Reform on the remuneration of public servants in an attempt to identify the causal effect of this public sector wage policy.

The present study is structured as follows. The next section presents the literature review. It is followed by the institutional context for the public sector wage-setting in Mexico. Then, a description of the data and the empirical strategy. The empirical results and robustness checks are then presented. The last sections discuss the policy implications and the concluding remarks.

#### **4.2 Literature Review**

The public–private sector wage gap has been explored extensively in developed and developing countries. This type of analysis is significant for the public sector in the labour market as both an employer and a source of government spending. Commonly, the public sector offers a non-competitive work environment, a secure job, and considerable fringe benefits that generally cannot be matched in the private sector in many countries.<sup>97</sup>

Public sector employment represents an indicator of state participation in the economy that provides essential services such as education, health, security, and justice. This involvement makes it a central actor within labour markets and hence the broader economy. It impacts the aggregate outcomes of employment, wages, and other welfare variables (Azuela de la Cueva et al., 2014; Gindling et al., 2019). There is also a perception that scarcity of skilled public servants may have detrimental effects on the ability of a country to cope with the increasing demand of its citizens (Christofides and Michael, 2020).

Hospido and Moral-Benito (2016) argue that public and private employees are paid differently –and at a higher level in the public sector– for many reasons: (i) the monopolistic power of governments in the provision of public services which enables setting non-competitive wages; (ii) the public sector objectives could sometimes be motivated by vote maximization rather than traditional profit maximization as in the private sector; (iii) union density is often higher in the public sector with contracts that provide valuable insurance against uncertain economic events; (iv) higher levels of productivity-enhancing employee characteristics such as education and/or labour force experience.

It is also argued that wage differentials can be driven by the competition in attracting and retaining high-qualified workers, especially in the case of skill shortages (Lausev, 2014; Mizala et al., 2011; Van Dooren et al., 2009). The empirical evidence suggests a positive and more significant public sector wage premium in developing and low-income economies. It may drive positive sectoral wage differentials between these sectors, compared to positive but small or modest – or, in some cases, zero or negative – wage premia in developed

<sup>&</sup>lt;sup>97</sup> Although the public sector is more likely to provide fringe benefits to its workers in developing economies than developed ones, somewhat paradoxically, the quality of government services is generally viewed as lower in developing rather than in developed countries (Finan et al., 2015).

countries. The highest public sector premia are generally reported in South Asia, Sub-Saharan Africa, and Latin America and the Caribbean countries. The lowest compensations are reported in Eastern and Western Europe, Central Asia, the US, and Canada (Finan et al., 2015).

The literature for both developed and developing countries provides evidence of positive public–private wage differentials and proof of negative pay gaps in some rarer cases (Bargain et al., 2018; Cai and Liu, 2011; Christofides and Michael, 2013; Finan et al., 2015; Hospido and Moral-Benito, 2016; Lausev, 2014; Mizala et al., 2011). Although the literature that exploits public or private sector wage policies -or reforms in the pay-setting mechanism- in analysing their ultimate impacts on the public–private wage gap is limited (e.g., Ampofo and Doko Tchatoka, 2019; Telegdy, 2018; Vladisavljević, 2020).

The public–private pay gap has been commonly analysed using a Mincerian wage equation with a dummy variable indicating whether the workers are employed in the public sector to account for differences in job characteristics between the two sectors. The estimate for such a dummy variable within this framework generally provides the public–private wage differential (Lausev, 2014).

The analysis around the mean of the wage distribution can not capture the part of the distribution that exhibits the most significant wage gap, which has important policy implications in the labour market. The differentials could be attributable, inter alia, to individual heterogeneity. Productivity gaps may differ by demographic characteristics, occupation, level of education, or union bargaining power.<sup>98</sup> Hence, quantile regressions based on the work of Firpo et al. (2009) are used to compute the effects across either the conditional or unconditional wage distributions.

Some studies exploit panel datasets including individual-specific effects when estimating the public–private wage differentials at different points of the wage distribution to net out individual time-invariant unobservable characteristics that could generate employment

<sup>&</sup>lt;sup>98</sup> Union status represents a strong bargaining power in the public sector for developing countries; changes in unionism relative to that obtaining in the private sector may alter the estimated public–private sector wage differentials. However, many countries do not provide information on unions (Lausev, 2014).

selection bias. Others analyses use decomposition techniques to divide the sectoral wage gap according to the differences in observed and in unobserved characteristics in conjunction with quantiles or individual fixed effects regressions.<sup>99</sup>

For a group of European countries, Christofides and Michael (2020) used Oaxaca-Ransom decomposition techniques along with unconditional quantiles in exploiting the impact of the international and sovereign crises (2007) on wage gaps between 2006-2013. Their average estimates suggest a reduction in the public–private sector gap, particularly evident at the 50<sup>th</sup> (-2%) and the 90<sup>th</sup> (-2.5%) percentiles, whereas the 90<sup>th</sup>–10<sup>th</sup> percentile effects were -6%, arguably reflecting attempts by many countries to protect low-paid workers. Bargain et al. (2018) used a panel dataset (1988-2013) with individual fixed effects for France, where the public sector wages have been nominally frozen since 2010 and reported a decline in the wage gaps from -3% at the 10<sup>th</sup> percentile to -7% at the 90<sup>th</sup> percentile of the wage distribution.<sup>100</sup>

Cai and Liu (2011) using the Oaxaca-Blinder (O-B) decomposition, reported for Australian men between 2001 and 2006 a wage gap of 5.3% at the 10<sup>th</sup> percentile, which became a negative gap at the median of 0.5% and to -13.2% gap at the 90<sup>th</sup> percentile. Likewise, Hospido and Moral-Benito (2016) used the O-B decomposition with individual-specific effects for Spain and data from 2005 to 2012 to report a wage gap of 18% for high-skilled men in the public sector compared to the private sector workers at the 10<sup>th</sup> percentile, and a gap of -21% at the 90<sup>th</sup> percentile. Thus, a widening wage gap is generally found higher at the lower end of the wage distribution that decreases across the distribution.

For developing countries, Keskin et al. (2020) use panel data and fixed effects to find persistent negative wage gaps for Egypt in 1998–2018, from 6% at the 10<sup>th</sup> percentile to

<sup>&</sup>lt;sup>99</sup> Workers do not randomly select into either the public or private sectors; this self-selection can result from an individual's utility maximization based on observable and time-invariant unobservable characteristics such as ability, motivation, risk aversion and family socioeconomic status (Keskin et al., 2020).

<sup>&</sup>lt;sup>100</sup> Over the long-run, the French public sector pays less –with the penalty increasing with the wage level– but succeeds in attracting workers with 'good' unobserved characteristics across the whole distribution. This result may be related to elements of intrinsic motivation (a sense of the public service mission) or reflects the relative efficiency of the national examination process in selecting the most talented workers from the pool of applicants (The École Nationale d'Administration (ENA)).

10.7% at the 90<sup>th</sup> percentile (10% at the mean and the 50<sup>th</sup> percentile).<sup>101</sup> Whereas Emilio et al. (2012) use a rotating household panel to analyse the transition of individuals between jobs in both sectors in Brazil between 2002 and 2004. The authors reported a 3.9% wage differential at the mean.

Mizala et al. (2011) investigated the public–private wage gap evolution for urban workers on the conditional wage distribution during the period 1992-2007 for eleven Latin American countries. The authors report positive wage gaps for all the economies up to the 50<sup>th</sup> percentile, after which some shifted into negative gaps for some countries. At the highest percentiles (the 90<sup>th</sup> and 95<sup>th</sup>), reductions in wage differentials were primarily reported. The most qualified public sector workers faced wage penalties, which explains their findings.

A limited number of studies analyse the public–private wage differentials in Mexico, some studies explore each gender separately (e.g., Panizza and Qiang, 2005; Rodríguez-Pérez, 2019). Pagán et al. (2002) exploit a decomposition method in 16 metropolitan areas.<sup>102</sup> The study accounts for relative changes in individual endowments and their prices to suggest that wage differentials increased on average by 44% from 1987 to 1997, widening the sectoral gap.<sup>103</sup> These results can be explained by increases in the price of skills and changes in sorting across sectors.<sup>104</sup> Besides, this period had a historically high level of government spending due to the decentralization and privatization measures implemented in Mexico.

The analysis of Panizza and Qiang (2005) found a 0.7% public–private wage gap at the mean for male public sector workers in 1994, albeit not statistically significant,<sup>105</sup> and a 9% raw wage differential applying the O-B decomposition. Using decomposition methods and Firpo et al. (2009) methodology, Rodríguez-Pérez (2019) found in 2017 a decreasing wage gap across the unconditional wage distribution between these sector workers, from -0.2 log points of hourly wages at the 10<sup>th</sup> percentile to approximately -0.5 log points at the 90<sup>th</sup> percentile. The wage gap was attributed mainly to the differential in the endowments of productive

<sup>&</sup>lt;sup>101</sup> The researchers prove that the public sector fails to attract better quality men across the conditional wage distribution. The results raised concerns about whether the provision of public services is indeed efficient.

<sup>&</sup>lt;sup>102</sup> The authors used the Juhn, Murphy and Pierce decomposition method (JMP, 1991).

 $<sup>^{\</sup>rm 103}$  The sample includes male and female observations.

<sup>&</sup>lt;sup>104</sup> Mexico introduced compulsory secondary education in 1993.

<sup>&</sup>lt;sup>105</sup> The estimated public sector effect was negative in 1994 but not statistically significant.

characteristics (personal and labour attributes). The public sector workers enjoyed a relatively higher appreciation of their productive qualifications, resulting in salary advantages over comparable private-sector workers, especially in the lower half of the wage distribution.

Therefore, using an array of approaches, the evidence suggests that some developed and developing countries experience wider sectoral pay gaps (e.g., Cai and Liu, 2011 for Australia; Hospido and Moral-Benito, 2016 for Spain; Mizala et al, 2011 for Latin American countries). There is also evidence of wage gap reductions (e.g., Bargain et al., 2018 for France; Christofides and Michael, 2020 for Norway; Keskin et al., 2020 for Egypt; Rodríguez-Pérez, 2019 for Mexico).

Some studies have provided evidence on the effects of public sector austerity due to the global economic crisis using a variety of methodologies. However, they either use separate regressions for before and after the financial crisis or analyse the entire period using a dummy variable for the pre-crisis and post-crisis periods (e.g., Bargain et al., 2018; Christofides and Michael, 2020; Hospido and Moral-Benito, 2016).

#### 4.2.1 Public Sector Wage Policy Reforms

Notwithstanding the literature presented above, studies analysing the impacts of government wage policies aiming to reduce the pay gap among public and private sectors are limited. This type of analysis is more in line with the study presented in this chapter. Governmental reforms in the public sector administration are generally driven by the need for good governance indicators such as efficiency, the inclusion of high-skilled workers, and to provide fair remuneration to the public servants that deliver public services in both developed and developing countries (Lah and Perry, 2008; Mizala et al., 2011).

In 2008, around 80% of the economies that belong to the Organisation for Economic Cooperation and Development (OECD) had implemented a public sector pay system linking payments to the individual's performance based on merit. It was motivated to address wage differentials between public and private sector workers and provide performance-related rewards that could change the servants' behaviour and increase their productivity and efficiency in the public sector (Bajorek and Bevan, 2015).<sup>106</sup> In 1978, the US implemented these pay settings in the Civil Service Reform Act (CSRA). Among the components, it had merit payment, performance appraisal and separating regulatory personnel functions for senior management (Lah and Perry, 2008).

This type of analysis is important, primarily in developing countries where a hefty premium in public sector wages is found, leading to the perception of inefficiencies in providing public services and yielding only economic benefits for public sector employees. However, there is little evidence of the effectiveness of such wage policies for reducing the magnitude of the public–private pay differentials. In general, they rely on weak research designs without counterfactuals for inferences, which cannot assess the potential programme effects,<sup>107</sup> or they do not explore how these policies reduce the wage disparities.<sup>108</sup> Some focus only on the health care and education sectors.<sup>109</sup>

Among the studies that attempt to identify a causal effect of public sector wage policies on the wage gaps in developing countries are Ampofo and Doko Tchatoka (2019) that analyse the intervention introduced in 2010 for Ghana, and Telegdy (2018) that explores the effects of the 2002 wage policy in Hungary. In addition, the study of Vladisavljević (2020) examines the policy implemented in Serbia in 2014.

The Single Spine Pay Policy (SSPP) introduced in 2010 in Ghana to reduce strikes in the public sector, retain skilled workers, and raise their wages, was exploited by Ampofo and Doko Tchatoka (2019). The study examines the policy effectiveness in addressing the public–private sector wage gap using a pseudo panel dataset and cohort fixed effects for males and females. It employs conditional quantile regressions based on a difference-in-differences (D-i-D) estimation.<sup>110</sup> The results report a negative and insignificant effect at the mean-based estimate, but a reduction of the public–private wage gap up to the 50<sup>th</sup> percentile of the hourly earnings distribution from 33% to 3%. The gap then widens beyond the median by between

<sup>&</sup>lt;sup>106</sup> Payments in the form of permanent supplements or lump-sum bonuses.

<sup>&</sup>lt;sup>107</sup> For example, Marsden (2009) cited in Hasnain et. al. (2012).

<sup>&</sup>lt;sup>108</sup> Such as Bryson et al. (2012) and Lucifora and Origo (2015) cited in Ampofo and Doko Tchatoka (2019).

<sup>&</sup>lt;sup>109</sup> Hasnain et. al. (2012) analyse 127 studies done for OECD countries and 26 studies for developing economies.

<sup>&</sup>lt;sup>110</sup> Since 1992 the wages in Ghana of the private sector had been higher than the public sector employees.

0.8% to 2%. Thus, the public sector wages increased at the lower tail of the wage distribution up to the median, whilst reducing earnings at the 75<sup>th</sup> and 90<sup>th</sup> quantiles.<sup>111</sup>

In Hungary, the wages of public sector employees lagged behind private-sector earnings between 1998 and 2001. They increased only to keep the public–private pay gap stable. In 2002, the public sector base wage increased by 50%. Telegdy (2018) analysed this intervention with fixed effects and proxy variables for the exposure to the policy based on the variation of the share in public sector employment. The results reported a change in the public wage premium from -17% to +7.5% from one month to the next. The wage differential increased by 9.6% between two workers exposed to the measure situated at the 25<sup>th</sup> and the 75<sup>th</sup> percentiles of the wage distribution. The spill-over effects to the private sector affected primarily young, more mobile, and highly educated workers.

Vladisavljević (2020) investigates the impact of wage austerity measures introduced in Serbia at the end of 2014 on the differences in the wage premium and compliance between two public subsector workers (i.e., the state sector and state-owned enterprises). Before the intervention, the wages of state-owned enterprises workers were higher, on average, than in the state sector and especially from the median to the top of the wage distribution. The findings reveal that after the policy was introduced, the wage differences between the subsectors intensified due to lower compliance of state-owned enterprises with the wage cut.

Many Latin American countries introduced reforms since the 1990s to implement responsible governments and modernize the civil service administration with efficient and effective public sector workers; the reforms regulated their wages (e.g., Chile in 1990, Brazil in 1995, and Uruguay in 1997). Although, research is not exploiting these institutional changes to investigate their success or failure in reducing the public sector pay differentials.

Mexico also has been implementing and amending Constitutional reforms in the public sector since the 1990s. In 2018 a Federal Law that regulates the remuneration of public sector workers was introduced. Nonetheless, there is no empirical evidence regarding the

<sup>&</sup>lt;sup>111</sup> Moreover, the SSPP has decreased workers' productivity across the distribution of earnings, mainly due to a decrease in the effort of male workers in the administration sector.

effectiveness of this particular intervention to date and this motivates the research undertaken in this chapter. The next section describes the public sector wage setting in Mexico.

#### **4.3 Public Sector Pay Setting in Mexico**

Mexico partially implemented reforms back in 1994 following the civil servant meritpayment component of the CSRA. However, the public sector wages grew faster relative to those in the private sector by 1997, motivated in part by a conscious government effort to attract higher-skilled workers through the payment of higher wages (Pagán et al., 2002).

Consequently, in 2003, it consolidated the Professional Career Service Law in the Federal Public Administration (Ley de Servicio Profesional de Carrera -SPC- in Spanish).<sup>112</sup> This Law provides a mechanism to guarantee equal access to work in the public sector for middle management or trusted workers and ensure transparent recruitment processes. It had the objective to attract, maintain, and train public employees for their careers as public servants.

The Law also ensures that the public administration operates independently of political change, where the public servants are not appointed politically but rather by demonstrating relative administrative skills, experience, and qualifications. However, it did not establish any regulations regarding the public sector wages.

Public sector employees in Mexico perform activities at the federal, state, or municipal government level that directly undertake the production of goods, merchandise, commercialization, or the provision of services using the exclusive assets of the public sector. It includes activities in the offices and dependencies of the three government levels dedicated to the administration of the resources and patrimony of the nation, both directly and through the regulation and legislation of economic, social, and political practices. The chambers of legislators, the armed forces, and state-owned enterprises such as para-statal firms, in which the government exercises control, are also included.<sup>113</sup> The private sector is that part of the economy which is both run for private profits and is not controlled by the Federal government or other state agencies.

<sup>112</sup> http://www.diputados.gob.mx/LeyesBiblio/pdf/260.pdf

<sup>&</sup>lt;sup>113</sup> Definition in the glossary of the Mexican Statistics, Geography and Informatic Institute (INEGI-Spanish acronym).

After the 2008 financial crisis, the Mexican government engineered a public spending boom through different social programmes to support the population to overcome the crisis. These included a temporary employment programme; reduction in utilities' prices (electricity and LP Gas) through subsidies; providing employees earlier access to long-term savings accounts if they lost formal jobs; and the universalization of social protection programmes.<sup>114</sup>

In light of this boom in government expenditure, the demand for transparency and accountability in regard to the government's budget, income, and expenses increased. It exerted pressure on the government to impose regulations on the determination of public sector wages. Hence, in 2009 it implemented reforms to Constitutional articles mainly to transparent the remuneration of the civil servants (federal, state, and municipal levels).<sup>115</sup>

By 2010, Mexico faced a large contraction in its real GDP (-5.3%), tax revenues (including from oil) were less than 18% of its GDP, many formal jobs were lost, and economic activity declined (Moreno-Brid, 2010). The economic environment was unstable, and the public sector pay-setting did not adjust to the changing conditions. In 2012, monthly public sector wages were on average 43.3% higher than in the private sector and almost triple that in the informal sector. Public sector employees earned 29% more per hour than formal private-sector workers with similar characteristics. Consequently, with higher salaries, the unobserved skills of workers in the public sector were improved and less-skilled workers were forced out (Gasparini et al., 2015).

The higher salaries in the public sector and higher public spending may have been actions consciously introduced by the government to accomplish its political objectives –vote maximization from public sector workers and more generally society– particularly in 2012 with the presidential election, and in 2015 with statal elections (Gutiérrez-Rodríguez, 2015).

In 2015, the fiscal deficit of 3.5% of the GDP was the highest in three decades. The international collapse of oil in 2014-2015 precipitated a fall in oil revenues by over a third

<sup>&</sup>lt;sup>114</sup> The Temporary Employment Program (PET) provides financial support and contributes to the population's well-being who faced a reduction in their income because of national emergencies. The programme offers temporary financial support for their participation in family or community projects.

<sup>&</sup>lt;sup>115</sup> These reforms applied to the articles 75, 115, 116, 122, 123 and 127. Essentially, the reforms stated that the public sector salaries would be of the public domain, and proportional to the positions' responsibilities.

during this period. Thus, the excessive historical dependence on oil export revenues for its public spending was seen as unsustainable (Gutiérrez-Rodríguez, 2015). The much-needed austere fiscal policy was implemented in 2016. It was mainly designed to reduce the expenditure on social programmes.

During the presidential election campaign in 2017, the higher level of public wages compared to the private sector became a political issue, for which one of the main proposals of the current government campaign focused on addressing these historically higher remunerations. To achieve this goal, the political party included strategies that would force public servants to comply with the existing remuneration reforms, such as removing discretionary or arbitrary use of public expenditure and cutting the wages of public sector workers.

In addition, it intended that the public resources freed from reducing public sector wages would be re-directed towards public spending to reactivate the economy, target poverty reduction, and fund the country's development.<sup>116</sup> The proposal of getting the government's finances under control through greater sustainable wage parity for the public sector workers ultimately leading to an effective public administration served as an attractive political strategy for the election campaign.

Thus, after the elections in July 2018, the Federal Pay Reform on the Remuneration of Public Servants was enacted in November 2018 by the new government to gain credibility and further increase its popularity. The Federal Law contains 17 articles designed to regulate the remuneration of public sector workers of the three government levels.<sup>117</sup> The policy was designed to be implemented through reductions in the salary of senior management positions up to 40%; freezes on wage increases for other public sector workers; in some cases permitting increases in the wages of the servants who earned the least.<sup>118</sup> This labour market

<sup>&</sup>lt;sup>116</sup> <u>https://transparencia.info.jalisco.gob.mx/sites/default/files/Plan-de-Nacion-de-Morena.pdf</u>

<sup>&</sup>lt;sup>117</sup> <u>http://www.diputados.gob.mx/LeyesBiblio/ref/lfremsp/LFRemSP\_orig\_05nov18.pdf</u>

The Law stipulates a set of guidelines for providing an adequate remuneration for public sector workers reflecting their performance with the minimum and maximum limit earnings. The Law also establishes the Federal Penal Code for the sanctions applied to the public servants that try to evade compliance.

<sup>&</sup>lt;sup>118</sup> <u>https://www.gob.mx/sfp/prensa/ajustes-salariales-en-la-administracion-publica-federal-apegados-a-politica-de-austeridad-republicana?idiom=es</u>

According to the 2019 report of the Ministry of Finance, the spending on salaries was 2.0% lower than the approved budget and fell by 2.6% in real terms compared to 2018. https://www.cuentapublica.hacienda.gob.mx/es/CP/2019

reform is empirically assessed in this chapter to provide evidence regarding its effectiveness. The following section describes the data used in the analysis.

## 4.4 Data

The empirical work uses repeated cross-sectional data obtained from various rounds (2017-2019) of the Mexican National Occupations and Employment Survey (ENOE -Spanish acronym) available from the Statistics, Geography, and Informatics Institute (INEGI-Spanish acronym), containing information for gainfully occupied and unoccupied individuals aged 12 years and over. The ENOE is a nationally representative survey.

The survey reports, *inter alia*, weekly hours worked, monthly earnings, formal and informal job activities derived from the main and secondary occupations, and the type of employment (public or private). The second quarter interview period of the ENOE is used primarily to avoid any seasonality in earnings since higher expenses are reported during the first and fourth quarters of the year because of extra bonuses provided in those quarters. The sample is restricted to male observations because the information reported of earnings and other labour market variables are more complete and comprehensive for this gender group.

The questions related to the labour market apply only to the employed and gainfully occupied individuals during the reference week. Formal employment is captured through different information: the economic sector where the individual works (e.g., services, agriculture, manufacturing); the employment sector (e.g., public or private); the type of occupation and activities undertaken (e.g., masonry, agricultural activities, professional activities.); the number of employees in the firm they are working in; if people own their businesses (or are farmers); and the type of rights the individuals are entitled to through their work contract.

This study uses the variable established by INEGI for formal employment, which mainly provides access to social security, medical health services, and a contract subject to employment rights. In a similar spirit, the ENOE employs different questions to differentiate among the individuals that work in the public and private sectors. For this research, the definition of public sector workers includes people working at the judicial authority, in

parastatal companies; schools, hospitals, healthcare centres run by the government, and workers at federal, state, and municipal levels. The private sector workers are not controlled by the Federal government or other state agencies and run for private profits.

The analysis uses information on the main occupation, which the interviewees identify as the one where they spend most of their time during the day and which provides the highest remuneration.<sup>119</sup> To ensure an accurate measure of the wage, all those individuals who are informal workers, self-employed or unpaid workers are excluded from the sample because labour laws regarding public sector workers are not binding for this group of workers.

The sample comprises a pooled cross-sectional database of 85,867 formal male sector employees from 2017 to 2019 with an average of 28,600 observations per year. It comprises 645 municipalities across the 32 Mexican States for the whole period, with 480 municipalities per year on average. The minimum number of observations per cell is five (observations per municipality per year), the maximum is 1,023 and 376 on average. Approximately 17% of the observations are public sector workers, and 83% are private-sector employees.

#### 4.4.1 The Outcome Variable: log of hourly wage

The outcome variable used in the analysis is the natural logarithm of the hourly wage. The definition of earnings in the survey refers to a monthly payment received from the main job net of all labour taxes and social security contributions.<sup>120</sup> Thus, no fringe benefits, tips, bonuses, or commissions are included in the wage measure.

The sample includes both full-time and part-time workers. The individuals work 48.7 hours per week on average, and slightly above 1% are working between 6 and 20 hours per week in the sample. The hours worked are reported weekly in the survey, and this study multiplies these values by 4.3 to obtain the monthly hours worked. The hourly wages are thus computed

<sup>&</sup>lt;sup>119</sup> In the survey, it depends entirely on the individuals' responses the occupation they perceived as the main or principal job.

<sup>&</sup>lt;sup>120</sup> If the interviewees report weekly payments, the Mexican Statistics, Geography, and Informatics Institute transforms this into monthly earnings by multiplying the former times 4.3.

from the reported monthly earnings divided by the calculated monthly hours worked. The measure is expressed in real Mexican pesos as of June 2018.<sup>121</sup>

Table 4.1 reports the summary statistics of the wage variable for the employment sector (i.e., public or private sector) and selected quantiles. Public sector earnings are higher than the private sector wages at all reported percentiles. The raw gap is 0.52 log points. The statistics reveal pronounced wage differentials between both employment sectors and potentially indicate disparities in pay determination arrangements.

|                             | Pooled<br>sample | Public<br>sector | Private<br>sector |
|-----------------------------|------------------|------------------|-------------------|
| Log hourly wage             | 3.59             | 4.02             | 3.50              |
| 10 <sup>th</sup> percentile | 2.95             | 3.10             | 2.92              |
| 25 <sup>th</sup> percentile | 3.22             | 3.43             | 3.17              |
| 50 <sup>th</sup> percentile | 3.51             | 3.90             | 3.43              |
| 75 <sup>th</sup> percentile | 3.91             | 4.58             | 3.78              |
| 90 <sup>th</sup> percentile | 4.38             | 5.17             | 4.22              |
| Obs.                        | 85,867           | 14,717           | 71,150            |

Table 4. 1: Summary statistics of wages

*Source*: Mexican National Occupations and Employment Survey (2017-2019). *Note*: Mean values of the wage variables.

Figure 4.1 illustrates the kernel density distributions of log hourly wages by public and private sectors. The average wages are higher for the public sector workers, and its wage distribution is above the private sector at the bottom and top of the distribution.

The compression of wages at the bottom end of the wage distribution in the public sector has been found in many studies for developed and developing countries such as Cai and Liu (2011), Hospido and Moral-Benito (2016), Keskin et al. (2020), and Bargain et at. (2018). For Latin America, workers at the lower part of the wage distribution have a positive differential with respect to the private sector, whilst workers at the higher part of the wage distribution face a penalty (Mizala et al., 2011). However, Figure 4.1 does not seem to exhibit a penalty at the top end of the distribution for public sector employees in Mexico.

 $<sup>^{121}</sup>$  A small 0.25% of the observations reported zero wages. Thus, these observations are eliminated from the analysis.



Figure 4. 1: Distribution of log hourly wage by employment sector

*Source*: Mexican National Occupations and Employment Survey (2017-2019). Real values 2018 Mexican pesos.

The distribution of log hourly wages, in both employment sectors, before and after the 2018 Federal Pay Reform is illustrated in Figure 4.2. These figures reveal distributions quite similar to Figure 4.1. They indicate that the objective of reducing wages in the public sector due to the policy is not strongly reflected in the wage distribution post-intervention. The foregoing corroborates Gasparini et al. (2015) findings for Mexico of higher average hourly wages for the public sector workers compared to the private sector workers. A similar pay premium is found for Latin American countries as well (Finan et al., 2015; Gindling et al., 2019).

Figure 4.3 presents the wage distribution by each employment sector for the pre -and postintervention period. Private sector post-intervention distributions are shifted slightly to the right compared to the public sector. Thus, public sector wage growth was more constrained in the post-intervention period than the private sector wages, in which wage growth occurred. Hence, this implies the reform had a potential role in reducing the public sector pay gap, which needs to be analysed further using econometric methods.



Figure 4. 2: Distribution of wages pre -and post-the Federal Pay Reform in 2018

*Source*: Mexican National Occupations and Employment Survey (2017-2019). Real values 2018 Mexican pesos.

Figure 4. 3: Distribution of wages by employment sector and pre -and post-the Federal Pay Reform in 2018



*Source*: Mexican National Occupations and Employment Survey (2017-2019). Real values 2018 Mexican pesos

#### 4.4.2 Treatment and control groups

The 2018 reform for the remuneration of public servants represents an institutional change that allows the creation of two separate groups. One is subject to a wage regulation due to the Pay Reform and another that is not directly impacted by the intervention. Hence, the treatment group consists of those individuals working in the public sector for which the policy mandates a regulatory change to their wages (14,717 observations, 17% of the sample). The control group comprises the employees in the private sector, their wages are not regulated (71,150 observations, 83% of the sample).

Table 4.2 reports the treatment and control groups' hourly wages pre-treatment and postintervention. The treated group reports similar average salaries post-intervention at the 10<sup>th</sup> and 25<sup>th</sup> percentiles, while slightly higher salaries for the 75<sup>th</sup> percentile. In contrast, it reports lower wages at the median and the 90<sup>th</sup> percentile. These statistics indicate partial accomplishment with the intervention wage cuts at the top percentiles and frozen wage payments at the bottom percentiles. In contrast, the control group exhibited greater average wages at the post-treatment period across the whole distribution, reflecting that the private sector is not subject to the public sector wage-setting mechanism.

|                             | Treatme       | ent group      | Contro        | ol group       |
|-----------------------------|---------------|----------------|---------------|----------------|
| Variable                    | Pre-treatment | Post-treatment | Pre-treatment | Post-treatment |
| Log hourly wage             | 4.02          | 4.01           | 3.49          | 3.52           |
| 10 <sup>th</sup> percentile | 3.10          | 3.10           | 2.90          | 2.95           |
| 25 <sup>th</sup> percentile | 3.43          | 3.44           | 3.16          | 3.19           |
| 50 <sup>th</sup> percentile | 3.91          | 3.88           | 3.42          | 3.45           |
| 75 <sup>th</sup> percentile | 4.57          | 4.60           | 3.77          | 3.78           |
| 90 <sup>th</sup> percentile | 5.19          | 5.13           | 4.21          | 4.23           |
| Obs.                        | 9,889         | 4,828          | 46,093        | 25,057         |

Table 4. 2: Mean values of hourly wages by treatment and control groups

Source: Mexican National Occupations and Employment Survey (2017-2019).

Figures 4.4 and 4.5 illustrate the treatment and control groups' distribution of log hourly wages for the three survey years (2017-2019). For the treatment group (Figure 4.4), the wage distributions shifted to the right after 2017, showing higher hourly wages. The distribution in 2019 reflects a wage growth constrained with respect to 2018. For the control group, Figure 4.5, reveals a wage growth in 2019 compared to 2018, not restricted to this group.



Figure 4. 4: Distribution of log hourly wage by the treatment group, 2017-2019

*Source*: Mexican National Occupations and Employment Survey (2017-2019). Real values 2018 Mexican pesos

Figure 4. 5: Distribution of log hourly wage by the control group, 2017-2019



*Source*: Mexican National Occupations and Employment Survey (2017-2019). Real values 2018 Mexican pesos

Table 4.3 provides the standard difference in means of the log hourly wages between people treated and not treated by the Pay Reform, before and after the policy.<sup>122</sup> The estimated effects reveal how the wages in these two groups changed with the reform. The treatment group has, on average, statistically significantly higher hourly wages than the control group for both

<sup>&</sup>lt;sup>122</sup> The difference in means reported does not include fixed effects or use robust standard errors.

pre-treatment and post-treatment periods. Thus, even before the policy was enacted, the public sector workers' wages were better off than private-sector workers' wages.

The D-i-D estimate is negative and statistically significant, indicating that the intervention may decrease wages of the treated group. In the absence of covariates, there is, on average, a 3.7% public sector pay gap reduction due to this reform. This wage gap decrease is further analysed with econometric methods.

 Table 4. 3: Difference-in-Differences estimation of log hourly wages between treated and control groups, pre -and post-treatment

| Outcome var. | Obs.  | log hourly wage | S.Err. | t     | P> t     |
|--------------|-------|-----------------|--------|-------|----------|
| Before       |       |                 |        |       |          |
| Control      | 46093 | 3.494           |        |       |          |
| Treated      | 9889  | 4.019           |        |       |          |
| Diff (T-C)   |       | 0.525           | 0.006  | 85.58 | 0.000*** |
| After        |       |                 |        |       |          |
| Control      | 25057 | 3.522           |        |       |          |
| Treated      | 4828  | 4.01            |        |       |          |
| Diff (T-C)   |       | 0.488           | 0.009  | 56.12 | 0.000*** |
| Diff-in-Diff | 85867 | -0.037          | 0.011  | 3.45  | 0.001*** |

The observations in the control group are 71,150 and 14,717 in the treatment group. Means and Standard Errors are estimated by linear regression Inference: \* p<0.1, \*\* p<0.05, \*\*\* p<0.01

#### 4.4.3 Explanatory variables

The explanatory variables used are age and its quadratic, years of education, whether residing in an urban or rural settlement area, marital status, head of the household status, and the type of economic sector. The sample includes individuals aged between 15 and 65 years when surveyed; aged 15 years because it is the minimum legal working age. It excludes people over 65 years to avoid retirees or pensionable. The average age in the sample is 37 years.

The married status encompasses married people and individuals living together; single status includes widows, and divorced status incorporates separated individuals. Table 4.4 displays the statistics for the covariates. The average years of schooling are 11 years, which in Mexico is equivalent to incomplete high school (i.e., completed high school is 12 years of education). Most of the respondents reside in urban areas (71%), are married (71%), and are the head of the household (65%). The service sector employs 43% of the respondents. Likewise, the more

educated people, married, residing in urban areas, head of the household, and working in the public sphere enjoy the higher average wages.<sup>123</sup>

| Variable           | Mean  | Std. Dev. | Min | Max |
|--------------------|-------|-----------|-----|-----|
| Age                | 37    | 11.57     | 15  | 65  |
| Years of schooling | 11.32 | 3.75      | 0   | 24  |
| Urban status       | 0.71  | 0.45      | 0   | 1   |
| Head of household  | 0.65  | 0.48      | 1   | 1   |
| Marital status     |       |           |     |     |
| Married            | 0.71  | 0.45      | 0   | 1   |
| Divorced           | 0.04  | 0.21      | 0   | 1   |
| Single             | 0.24  | 0.43      | 0   | 1   |
| Economic sector    |       |           |     |     |
| Agriculture        | 0.03  | 0.16      | 0   | 1   |
| Commerce           | 0.17  | 0.37      | 0   | 1   |
| Construction       | 0.07  | 0.26      | 0   | 1   |
| Manufacturing      | 0.28  | 0.45      | 0   | 1   |
| Sevices            | 0.43  | 0.49      | 0   | 1   |
| Mining and energy  | 0.03  | 0.16      | 0   | 1   |

Table 4. 4: Summary statistics of the explanatory variables

Source: Mexican National Occupations and Employment Survey (2017-2019). Total observations 85,867.

Table 4.5 reports the statistics disaggregated by treatment and control groups, with present similar magnitudes as Table 4.4. Tables 4.6 and 4.7 provide the t-test difference in means of the covariates between individuals treated and not treated pre -and post-intervention. <sup>124</sup>

| Treatment group    |       |           |     | Control group |       |           |     |     |
|--------------------|-------|-----------|-----|---------------|-------|-----------|-----|-----|
| Variable           | Mean  | Std. Dev. | Min | Max           | Mean  | Std. Dev. | Min | Max |
| Age                | 42    | 10.72     | 17  | 65            | 36    | 11.46     | 15  | 65  |
| Years of schooling | 13.66 | 3.75      | 0   | 23            | 10.84 | 3.57      | 0   | 24  |
| Urban status       | 0.73  | 0.45      | 0   | 1             | 0.70  | 0.46      | 0   | 1   |
| Head of household  | 0.76  | 0.43      | 0   | 1             | 0.63  | 0.48      | 0   | 1   |
| Marital status     |       |           |     |               |       |           |     |     |
| Married            | 0.78  | 0.42      | 0   | 1             | 0.70  | 0.46      | 0   | 1   |
| Divorced           | 0.05  | 0.21      | 0   | 1             | 0.04  | 0.20      | 0   | 1   |
| Single             | 0.17  | 0.38      | 0   | 1             | 0.26  | 0.44      | 0   | 1   |
| Economic sector    |       |           |     |               |       |           |     |     |
| Agriculture        | 0.00  | 0.01      | 0   | 1             | 0.03  | 0.17      | 0   | 1   |
| Commerce           | 0.00  | 0.06      | 0   | 1             | 0.20  | 0.40      | 0   | 1   |
| Construction       | 0.00  | 0.02      | 0   | 1             | 0.09  | 0.28      | 0   | 1   |
| Manufacturing      | 0.01  | 0.12      | 0   | 1             | 0.34  | 0.47      | 0   | 1   |
| Sevices            | 0.91  | 0.29      | 0   | 1             | 0.33  | 0.47      | 0   | 1   |
| Mining and energy  | 0.07  | 0.26      | 0   | 1             | 0.02  | 0.13      | 0   | 1   |

Table 4. 5: Summary statistics of the explanatory variables by treatment and control groups

*Source*: Mexican National Occupations and Employment Survey (2017-2019). The total observations in the treatment group are 14,717, while there are 71,150 in the control group.

<sup>&</sup>lt;sup>123</sup> Those employed in the judicial authority received the highest average pay among public sector workers, while individuals working in municipal governments obtained the lowest. In the private sector, workers employed in the mining and energy sector are the highest paid.

<sup>&</sup>lt;sup>124</sup> Tables B4.1 and B4.2 in appendix B report the statistics for each group pre -and post- treatment.

| Variables (s)     | Mean Control | Mean Treated | Diff.  | t      | Pr( T > t ) |
|-------------------|--------------|--------------|--------|--------|-------------|
| Ig hourly wages   | 3.49         | 4.02         | 0.53   | 85.08  | 0.0000***   |
| schooling         | 10.79        | 13.65        | 2.87   | 71.61  | 0.0000***   |
| age               | 35.78        | 41.88        | 6.10   | 48.81  | 0.0000***   |
| age square        | 1409.98      | 1867.93      | 457.95 | 46.84  | 0.0000***   |
| urban             | 0.69         | 0.72         | 0.04   | 6.85   | 0.0000***   |
| head of household | 0.64         | 0.76         | 0.13   | 23.93  | 0.0000***   |
| married           | 0.70         | 0.78         | 0.08   | 15.29  | 0.0000***   |
| divorced          | 0.04         | 0.05         | 0.01   | 2.16   | 0.0308**    |
| single            | 0.25         | 0.17         | -0.08  | 17.17  | 0.0000***   |
| Construction      | 0.09         | 0.00         | -0.09  | 31.29  | 0.0000***   |
| Manufacturing     | 0.33         | 0.01         | -0.32  | 66.28  | 0.0000***   |
| Commerce          | 0.21         | 0.00         | -0.20  | 49.63  | 0.0000***   |
| Services          | 0.33         | 0.91         | 0.58   | 118.64 | 0.0000***   |
| Agriculture       | 0.03         | 0.00         | -0.03  | 17.68  | 0.0000***   |
| Mining and energy | 0.02         | 0.07         | 0.06   | 32.63  | 0.0000***   |
| Obs.              | 46,093       | 9,889        |        |        |             |

Table 4. 6: Two-sample t-test of the explanatory variables pre-treatment

Total observations (baseline): 55,982

*Notes:* \*p<0.1, \*\* p<0.05, \*\*\* p<0.01

| Table 4. 7: Two-sam | ple t-test of the e | xplanatory variable | es post-treatment |
|---------------------|---------------------|---------------------|-------------------|
|---------------------|---------------------|---------------------|-------------------|

| Variables (s)     | Mean Control | Mean Treated | Diff.  | t     | Pr( T > t ) |
|-------------------|--------------|--------------|--------|-------|-------------|
| lg hourly wages   | 3.52         | 4.01         | 0.49   | 56.75 | 0.0000***   |
| schooling         | 10.93        | 13.69        | 2.76   | 49.06 | 0.0000***   |
| age               | 36.09        | 42.56        | 6.48   | 36.03 | 0.0000***   |
| age square        | 1435.83      | 1927.77      | 491.94 | 34.82 | 0.0000***   |
| urban             | 0.73         | 0.74         | 0.01   | 0.85  | 0.3934      |
| head of household | 0.61         | 0.76         | 0.14   | 19.16 | 0.0000***   |
| married           | 0.68         | 0.78         | 0.09   | 12.85 | 0.0000***   |
| divorced          | 0.05         | 0.05         | 0.00   | 0.71  | 0.4794      |
| single            | 0.27         | 0.18         | -0.09  | 13.89 | 0.0000***   |
| Construction      | 0.08         | 0.00         | -0.08  | 20.31 | 0.0000***   |
| Manufacturing     | 0.35         | 0.01         | -0.34  | 48.92 | 0.0000***   |
| Commerce          | 0.20         | 0.01         | -0.19  | 33.55 | 0.0000***   |
| Services          | 0.32         | 0.91         | 0.59   | 83.94 | 0.0000***   |
| Agriculture       | 0.03         | 0.00         | -0.03  | 12.22 | 0.0000***   |
| Mining and energy | 0.01         | 0.07         | 0.05   | 23.11 | 0.0000***   |
| Obs.              | 25,057       | 4,828        |        |       |             |

Total observations (baseline): 29,885

*Notes:* \*p<0.1, \*\* p<0.05, \*\*\* p<0.01

# 4.5 Empirical strategy

To determine how the 2018 Federal Law on the Remuneration of the Public Servants impacted not just wages at the mean but across the unconditional wage distribution among public and private sector workers in Mexico and to shed light on whether or not this policy ultimately has implications for the public–private wage gap across the wage distribution, this

study uses an unconditional quantile regression (UQR) approach based on the Re-centred Influence Function (RIF) and initially popularized by Firpo et al. (2009).

The implementation of this wage policy creates an exogenous source of variation in wages of the public sector compared to the private sector. A conventional methodology for comparing observations pre-treatment and post-treatment is a Difference-in-Differences (D-i-D) approach used in this study. The analysis identifies the effects of the intervention regulating wages in the public sector on the pay differentials among public and private sector employees.

It is anticipated that the wage policy decreases the public sector wage premium across the pay distribution, especially of higher-paid servants, as a consequence of the salary cuts applied to these job positions, given that the intervention unconstrained private-sector wages. This mechanism, a combination of salary cuts at the top end and frozen wage payments at the lower end in the public sector, subsequently leads to a contraction in the pay gap with respect to the private sector counterparts. More substantial negative effects are anticipated at the top end of the unconditional wage distribution due to wage payment cuts.

There are two main issues highlighted in the literature when estimating wage gaps. First, the mean estimation of the wage gap represents a drawback due to the potential heterogeneity in the effects of covariates on wages across the wage distribution. A way to address this issue is to analyse the wage gap at different points of the unconditional wage distribution. The UQ regression approach allows for the control of heterogeneity that can emerge at different percentiles providing a straightforward way to estimate the treatment effects of this policy at all points of the unconditional wage distribution.<sup>125</sup>

A second issue is the endogeneity of employment selection because there are important determinants of workers' sectoral choices such as skills, motivations and productivity not observed in the data. However, the D-i-D approach in this analysis takes into account the difference in the public sector and the private sector effects pre-treatment and post-treatment. Assuming constant unobservables across time that determine public or private selection

<sup>&</sup>lt;sup>125</sup> The quantile regression method relaxes the assumption of homogeneity in the effects of covariates across the unconditional distribution of the dependent variable.

employment, time-invariant unobservable effects are eliminated with the D-i-D method. Then, the concerns about potential selection bias are mitigated. Therefore, the empirical analysis uses UQ regression models within a D-i-D framework to provide an understanding of the effects of the reform on the public–private sector wage gap in Mexico.

In the literature, there are a small number of studies that combine quantile regressions with a D-i-D specification. Among these analyses, Hernaes (2018) and Pérez (2020) use UQ regressions within a linear D-i-D framework exploiting the Firpo et al. (2009) methodology to estimate the average distributional effects of unconditional outcomes based on a welfare policy change in Norway and a minimum wage up-rating in Colombia, respectively, on the treated group compared to the control group.

Few studies among developed and developing countries use conditional quantile regressions within a D-i-D approach exploiting a wage policy in the public sector to investigate the public–private pay gaps (e.g., the Ampofo and Doko Tchatoka (2019) study for Ghana). Nevertheless, studies using unconditional quantile regressions within a D-i-D framework for examining the public–private sector pay differentials are limited.

This paper focuses on identifying and estimating a distributional treatment effect parameter called the unconditional quantile (UQ) regression in the spirit of Hernaes (2018) in applying the RIF procedure based on Firpo et al. (2009) methodology for unconditional outcomes with linearity in covariates. This mean-based estimation of the UQ regressions allows for the use of a D-i-D framework.

The RIF procedure allows the average effects to be interpreted at different quantiles of the marginal or unconditional log of the hourly wage distribution. According to the literature, the effects are different across the unconditional wage distribution. The average impact of the RIF provides the quantile value of interest (e.g., the 5<sup>th</sup>, 95<sup>th</sup> percentiles or other intermediate quantiles).

Further, following Firpo et al. (2009) this study estimates a linear regression based on the RIF of the UQ of the log hourly wage. The methodology is laid out in Chapter Three for the

equations (1), (2) and (3) as the analysis in this chapter uses the same UQ technique within a D-i-D framework as in Chapter Three.<sup>126</sup>

The approach involves regressing the empirical RIF quantiles, constructed using expression (3), on a set of covariates using standard OLS procedure within a D-i-D specification. Two different groups are used to assess the impact of the policy intervention: the treatment group likely affected by the wage policy, and the control group which is unaffected by the intervention, but otherwise comparable to the treatment group (e.g., demographic groups, high or low wage regions, other jurisdictions where the policy did not change).<sup>127</sup>

Moreover, with the use of the D-i-D approach, the estimates eliminate the differences in the time-invariant unobservables in the wages that potentially determine employment selection pre-and post-treatment assuming they are constant across time. Therefore, this study seeks to determine the impacts of the public sector workers' exposure to the wage policy in 2018 on the RIF unconditional quantile log hourly wages given the explanatory variables and compared to the impacts with the non-treated private sector employees. These estimates that provide the treatment effects indicate the public–private wage gaps attributable to the policy reform.

The RIF quantile regressions with the wage policy intervention in a D-i-D approach using cross-sectional data are implemented through equation (4):

$$\widehat{RIF} (w_i, \widehat{q_\tau})_i = \beta_{0\tau} + \beta_{1\tau} (POST18_i) + \beta_{2\tau} (POST18_i * TREAT_i) + \beta_{3\tau} TREAT_i + \beta_{4\tau} X_i + \delta_{k\tau} + e_{i\tau}$$
(4)

In this regression, the RIF provides the quantiles for each percentile of  $w_i$ , which is the log hourly wage of individual *i* at the quantile  $\tau$ . *POST*18<sub>*i*</sub> is the policy implementation dummy variable assuming the value of one for the observations on and after November 2018. *TREAT*<sub>*i*</sub>

<sup>&</sup>lt;sup>126</sup> The methodology is not repeated here to conserve space.

<sup>&</sup>lt;sup>127</sup> The parallel trends assumption cannot be tested in this type of labour market scenario because, in the absence of the intervention, the outcomes associated with the treatment and control groups would have not necessarily evolved in a parallel fashion (Neumark et al., 2014 and Allegretto et al., 2017 cited in Dube (2019)).
is a dummy variable indicating whether the individual i works in the public sector, representing the treated group.

The control covariates are contained in  $X_i$  and consist of years of education, age and its square, urban reside status, marital status, head of household status and the economic sector in which the individual works.  $\delta_k$  are municipality fixed effects to control for macroeconomic shocks at the municipal level, which induces useful variation in the real effect of the intervention that allows the empirical identification nets out of other confounding forces. The idiosyncratic error term is  $e_i$ .

The parameter  $\beta_{2\tau}$  of the interaction term between the dummy variables for the policy and the treatment group,  $(POST18_i * TREAT_i)$ , represents the treatment effect due to the intervention at each  $\tau^{\text{th}}$  quantile and captures the public–private wage differential estimate, which is of primary interest in this specification. The next section presents the results for the specifications outlined above.

#### **4.6 Empirical Results**

The estimated treatment effects,  $\widehat{\beta}_{2\tau}$ , for the RIF-quantile regressions are presented in this section. The mean-based estimate reports, on average, a decrease of 2.9 percentage points reduction in the wage gap (see Table 4.8), which represents a 5.6% reduction in the average public sector pay premium overall (i.e., 0.029/-0.52) given the raw gap reported of about 0.52 in Table 4.1.<sup>128</sup> Although the policy had an effect reducing the public sector pay gap overall at the mean by about 6%, the reduction was achieved through cutting public sector pay of the lower-paid workers than through payment cuts of the higher-paid workers as originally anticipated in the policy.

Figure 4.6 illustrates the pay gaps at the bottom end of the wage distribution (i.e., from the  $5^{th}$  to the  $50^{th}$  percentiles). It provides evidence of statistically significant negative impacts on the unconditional wage distribution. The reduction in the wage varies in absolute values from 3.4 percentage points at the  $6^{th}$  percentile to 6.2 percentage points at the  $22^{nd}$ 

<sup>&</sup>lt;sup>128</sup> See Appendix A4.1 for details of this OLS model specification. Table B4.3 in Appendix B reports all the coefficient results of the OLS regression with log of hourly wages as dependent variable. The mean-based estimate of 2.9 percentage points is smaller compared to the 3.7 percentage points reported in Table 4.3, indicating the covariates as individual factors play a role in determining earnings.

percentile.<sup>129</sup> The latter represents the most significant decrease in the wage gap within the first half of the unconditional wage distribution.

The coefficients are generally below the mean-based values (-2.9 percentage points) and negative statistically significant up to the  $50^{\text{th}}$  percentile. Those estimates close to zero are not statistically significant from zero (see Figure 4.6).



Figure 4. 6: Public sector pay gap in the lower half of the wage distribution

Figure 4.7 contains the unconditional effects across the top half of the wage distribution (i.e., from the 51<sup>st</sup> to the 95<sup>th</sup> percentiles). The estimated effects increase in absolute magnitude across the upper half of the unconditional wage distribution from 3.9 at the 51<sup>st</sup> percentile to 13 percentage points at the 93<sup>rd</sup> percentile, only a few percentiles yield statistically significant estimates. Most of the wage reductions are below the mean-based values of -2.9 percentage points. Those estimates above zero are not statistically significant. It is worth noting that the policy was primarily designed to reduce the wages of the top wage earners. However, the results presented here do not reveal evidence of these wage cuts in Mexico.

<sup>&</sup>lt;sup>129</sup> The estimated effect at the 5<sup>th</sup> percentile is not statistically significant (0.9 percentage points).

Therefore, most of the public sector pay reductions associated with this policy were obtained through cuts in payments of the lower-paid workers and not by cutting wages of the higher-paid earners. Hence, the effects of the policy were more nuanced than its design expected and were more heavily concentrated among the lower-paid public sector workers.



Figure 4. 7: Public sector pay gap in the upper half of the wage distribution

The estimated unconditional quantile effects,  $\hat{\beta}_{2\tau}$ , for selected percentiles are reported in Table 4.8. The negative statistically significant effects at the bottom end of the pay distribution reduce wages by 4.3 percentage points at the 10<sup>th</sup> percentile, and by 5 percentage points at the 50<sup>th</sup> percentile.<sup>130</sup> As mentioned in Figures 4.6 and 4.7, the estimates generally suggest stronger effects in reducing the wage gap below the median of the unconditional wage distribution than above it.

Hence, the policy is responsible for reducing the public sector pay premium by about 24% at the bottom end of the pay distribution (i.e., -0.043/0.18). Given the estimates at the  $10^{\text{th}}$  percentile which suggests a contraction in the wage gap of 0.043 (in Table 4.8) and the raw

<sup>&</sup>lt;sup>130</sup> Table B4.4 in Appendix B reports all the coefficient results for selected percentiles. The estimates for *TREAT<sub>i</sub>* also report a statistically significant wage premia across the wage distribution for working in the public sector compared to their private sector counterpart wages by between 24% to 55%.

gap reported for the 10<sup>th</sup> percentile of 0.18 (in Table 4.1). At the median (i.e., the 50<sup>th</sup> percentile) the public sector pay premium contraction is 10.6% (i.e., -0.05/0.47). It suggests that the policy adversely affected more the wages of the lowest-paid, and below the median, compared to the highest-paid public sector workers (i.e., the effects above the median are mostly not statistically significant).

|                   | (1)                  | (2)                  | (3)                 | (4)                  | (5)              | (6)               |
|-------------------|----------------------|----------------------|---------------------|----------------------|------------------|-------------------|
|                   | Log hourly<br>wage   | RIF 10               | RIF 25              | RIF 50               | RIF 75           | RIF 90            |
| Treatment effects | -0.029***<br>(0.009) | -0.043***<br>(0.012) | -0.015<br>(0.009)   | -0.050***<br>(0.010) | 0.022<br>(0.019) | -0.060<br>(0.034) |
| Obs.              | 85,867               | 85,867               | <mark>85,867</mark> | 85,867               | 85,867           | 85 <i>,</i> 867   |
| Covariates        | Yes                  | Yes                  | Yes                 | Yes                  | Yes              | Yes               |

Table 4. 8: Unconditional estimates of the public sector pay gap for selected percentiles

Notes: \*\* p<0.05, \*\*\* p<0.01

The sample is constructed from the 2017-2019 Mexican National Occupations and Employment Survey. Municipality Fixed effects. Robust standard errors adjusted for 645 clusters at municipality level in parentheses.

Model (1) OLS standard estimation.

The pay differentials reported are similar to those found in the studies for France (Bargain et al., 2018) and Egypt (Keskin et al., 2020)<sup>131</sup> regarding the broad gap reductions at the bottom end of the wage distribution.<sup>132</sup> As the literature on the impacts of a policy change on the public–private wage differentials is limited, this analysis provides new evidence for both developed and developing countries focusing on the effects across the unconditional wage distribution. A set of robustness checks are explored in the next section.

#### 4.7 Robustness checks

In order to test the robustness of the foregoing results, the analysis in this section includes the use of a pseudo panel dataset with cohort fixed effects and subsequently a placebo in time

<sup>&</sup>lt;sup>131</sup> Even though, the literature does not refer to the wage gap effects as a result of institutional changes such as a wage reform in the public sector like in the present study.

<sup>&</sup>lt;sup>132</sup> The values at the 10<sup>th</sup> percentile in this analysis range from -3% found for France and -6% for Egypt.

test. The cohort fixed effects analysis supports the key findings of the study, which are that the policy did reduce the pay differentials between public and private sector workers and exerted a sharper effect at the bottom end of the wage distribution rather than at the top end during the period analysed. The results are not sensitive to the inclusion of cohort fixed effects.

The placebo in time test estimates whether there are effects of the pay reform where there should not be effects with a specification that uses a 'false' year for the introduction of the Federal Law. However, the results suggest anticipated effects that seem to be a consequence of the policy change adopted by the government in advance of its implementation. This result weakens the interpretation of the core estimates as representing causal effects.<sup>133</sup>

#### 4.7.1 Cohort Fixed Effects

This approach uses a pseudo panel for examining the impact of cohort-specific time-invariant unobservables that may potentially bias the intervention effects on the public–private sector pay-differentials. By constructing a pseudo panel dataset this study also addresses the lack of longitudinal panel data for Mexico. The sample uses the ENOE information from 2017 to 2019 following the approach of Ampofo and Doko Tchatoka (2019).

This pseudo panel dataset groups the respondents into cohorts with the same time-invariant characteristics that identify them, in this case, only the year of birth. The literature also uses gender and ethnicity for the cohorts. Still, this analysis is restricted to males and the survey does not report ethnicity. The cohort fixed effects identify the unobserved heterogeneity over time to infer individual behaviour from a group with similar characteristics.

The sample uses 82,835 individuals, 18% of them working in the public sector and 82% in the private industry.<sup>134</sup> There are 46 birth cohorts per survey year that correspond to the years

<sup>&</sup>lt;sup>133</sup>A two-step model within a Heckman framework for dealing with employment sectoral attachment that may potentially bias the intervention effects on the public–private sector wage differentials has little validity in this case. The Hansen J-statistic suggests the instruments used are not orthogonal to the wage outcome despite their relevance (i.e., head of household status, the number of children, elderly people in the household, and other household members working in the public sector). An IV-based model confirms this result. Besides, a two-step selection model is more applicable for mean-based analyis than at the quantile levels where such correction procedures remain an unsettled issue when using either conditional or unconditional pay distributions.

<sup>&</sup>lt;sup>134</sup> The wage distributions for both sectors are illustrated in Figure C4.1 in Appendix C.

of birth from 1954 to 1999. The cohort sizes, apart from two exceptions with less than 100 observations, <sup>135</sup> have at least 107 observations per cohort per survey year and, on average, 686 with a maximum of 947 observations. In addition, this analysis computes averages of continuous time-varying variables for each cohort across each survey year and uses these as variables (i.e., age, its quadratic, and years of schooling). Discrete time-varying variables such as marital status, urban residential area, and economic sector are used as reported.

The effects of the 2018 Federal Pay Reform are estimated with cohort effects in equation (4) for the mean and unconditional quantiles along with a D-i-D estimation. The RIF quantile regressions using the pseudo panel dataset are implemented through the equation (5):

$$\widehat{RIF} (w_i, \widehat{q_\tau})_i = \varphi_{0\tau} + \varphi_{1\tau} (POST18_i)(U_{ct}) + \varphi_{2\tau} (POST18_i * TREAT_i)(U_{ct}) + \varphi_{3\tau} TREAT_i (U_{ct}) + \varphi_{4\tau} X_i (U_{ct}) + s_{k\tau} (U_{ct}) + \delta_{k\tau} (U_{ct}) + \varepsilon_{i\tau} (U_{ct}) (5)$$

where the RIF provides the quantiles for each percentile of the individual log hourly wage,  $w_i$ , at the quantile  $\tau$ . *POST*18<sub>*i*</sub> and *TREAT*<sub>*i*</sub> are similar dummy variables to equation (4).  $X_i$  represents the covariates. Likewise, the parameter  $\varphi_{2\tau}$  corresponding to the interaction term between the dummy variables for the policy and the treatment group, (*POST*18<sub>*i*</sub> \* *TREAT*<sub>*i*</sub>), is of interest in this specification because it denotes the public–private sector wage differentials due to the intervention at each  $\tau$ <sup>th</sup> quantile.

The function  $U_{ct}$  denotes time-invariant and time-varying characteristics, modelled as a function of the cohort fixed effects and the idiosyncratic shocks. It is often called the rank variable and indicates the variation in the coefficients  $\varphi_{2\tau}$  and  $\varphi_{4\tau}$  at the RIF-quantiles. The assumption on the rank structure is useful to define the distribution of the potential outcomes. Thus, workers at higher wage quantiles have higher values of  $U_{ct}$ . The estimated effect is the treatment effect at the quantile of the potential outcome distributions (Ampofo and Doko Tchatoka, 2019).

The survey year fixed effects are symbolized by  $s_{k\tau}$  and the regional fixed effects are represented by  $\delta_{k\tau}$ . These fixed effects are incorporated into the model to address any potential endogeneity in the variable of interest. Survey year effects capture the economic

<sup>&</sup>lt;sup>135</sup> 85 and 91 observations for the individuals born in 1954 and surveyed in 2018 and 2019, respectively.

and political events that have evolved and could influence wages, affecting the policy's identification. Regional effects account for the economic activities that developed before and after the policy in similar economic areas.<sup>136</sup> The inclusion of the cohort fixed effects plays a fundamental role in not attributing the effect of time-invariant traits to the policy and overcoming any potential employment sector selection bias.

The RIF-quantile treatment effects from 5<sup>th</sup> to 95<sup>th</sup> percentiles are contained in Figures 4.8 and 4.9. The magnitudes and statistical significance are not different from those initially reported for the core analysis. Most of the estimates are statistically significant below the median, whereas the effects are mostly not statistically significant above the median. Thus, the policy constrains the public sector pay growth and exert more effect on the lower-paid public sector workers than the higher-paid ones.

For selected RIF-quantiles, the estimates are reported in Table 4.9, the results confirm the magnitude of the impacts reported previously in Table 4.8. The mean-based fixed effects estimate remains negative, suggesting a similar wage gap reduction between the public and private sectors of 2.9 percentage points and a 5.7% public sector pay premium (i.e., the raw gap is 0.51). The wage at the bottom end and the median of the unconditional wage distribution is contracted approximately by 4 percentage points.

The pay differentials are not sensitive to the inclusion of cohort fixed effects. It is a salutary finding. However, it may be that the cohorts are not playing a crucial role in determining the wage gap between these two sectors due to the introduction of this pay reform. Perhaps, it might be that the year of birth cohorts are capturing labour market effects that change slowly across cohorts while not capturing other unobservables that may potentially be important for the formal labour market.<sup>137</sup>

<sup>&</sup>lt;sup>136</sup> INEGI clusters the 32 Mexican States by seven economic regions that reflect population's levels of welfare, including aspects such as education, employment, housing, and health. Therefore, this analysis uses seven dummy variables.

 $<sup>^{137}</sup>$  In addition, using individual-level fixed effects (FE) model with a five-quarter panel dataset of the ENOE to explore the public–private wage differentials limits the empirical identification of the public sector effect. In this case, only 1.1% of the observations switch jobs between sectors. Thus, the estimated effect may reflect the behaviour of a minimal unrepresentative number of workers. The wage distributions and the results are reported in Table B4.5 in Appendix B and in Figures C4.2 to C4.4 in Appendix C.



Figure 4. 8: Public sector pay gap with cohorts fixed effects in the lower half of the wage distribution

Figure 4. 9: Public sector pay gap with cohorts fixed effects in the upper half of the distribution



|                           | (1)                | (2)      | (3)     | (4)       | (5)     | (6)     |
|---------------------------|--------------------|----------|---------|-----------|---------|---------|
|                           | Log hourly<br>wage | RIF 10   | RIF 25  | RIF 50    | RIF 75  | RIF 90  |
| Treatment effects         | -0.029**           | -0.039** | -0.013  | -0.041*** | -0.005  | -0.062  |
|                           | (0.013)            | (0.013)  | (0.010) | (0.013)   | (0.019) | (0.033) |
| Obs.                      | 82,835             | 82,835   | 82,835  | 82,835    | 82,835  | 82,835  |
| Covariates                | Yes                | Yes      | Yes     | Yes       | Yes     | Yes     |
| Cohort Fixed Effects      | Yes                | Yes      | Yes     | Yes       | Yes     | Yes     |
| Survey Year Fixed Effects | Yes                | Yes      | Yes     | Yes       | Yes     | Yes     |
| Regional Fixed Effects    | Yes                | Yes      | Yes     | Yes       | Yes     | Yes     |

Table 4. 9: Unconditional estimates of the public sector pay gap with cohort fixed effects

*Notes:* \*\* p<0.05, \*\*\* p<0.01

The sample is constructed from the 2017-2019 Mexican National Occupations and Employment Survey. Municipality Fixed effects. Robust standard errors adjusted for 46 cohorts in parentheses. Model (1) Mean Fixed Effects estimation.

#### 4.7.2 Placebo in time test

An important threat to identifying the effects of the Pay Reform is that the policy might not be exogenous. Thus, this placebo in time test examines the robustness of the identification used in equation (4) with a false dummy variable specifying the policy being implemented in November 2017 instead of in 2018 (as was the actual case). The coefficient of the interaction term between the intervention and the treatment group, which is of interest, is expected not to be statistically significant.

The sample uses 55,982 observations for the original pre-treatment period (2017-2018). Table 4.10 reports the results for selected percentiles revealing significant estimates for the placebo test at the 10<sup>th</sup>, 25<sup>th</sup>, and 75<sup>th</sup> percentiles and non-significant estimates on the public wage for the remainder percentiles.

The use of this placebo in time suggests there are significant effects where they should not be. These results may reveal anticipated effects that are a consequence of the policy change adopted by the government in advance of the introduction of the Federal Law in the Mexican Constitution in 2018.

It might be feasible because this reform proposal was in the public domain during the presidential election campaign in 2017, which coincides with the period used for the placebo test. It may be the case that the government in charge, following the strategy outlined in the

campaign by the opposite political party, began to implement some actions ahead of the presidential election to reduce the discretionary diversion of public resources. Perhaps, enforce public servants to comply with the existing remuneration reforms, and induce wage parity for public sector work.

If these are anticipated effects, they suggest strong contractions of the wage at the bottom end of the wage distribution by 3.4 percentage points, on average (10<sup>th</sup> and 25<sup>th</sup> percentiles) (see Table 4.10).<sup>138</sup> The anticipated effects disproportionately affected the lower-paid public sector workers, which represent the higher proportion of workers within the sector and not the higher-paid.

|                   | (1)                  | (2)      | (3)       | (4)     | (5)      | (6)     |
|-------------------|----------------------|----------|-----------|---------|----------|---------|
|                   | Log hourly           | RIF 10   | RIF 25    | RIF 50  | RIF 75   | RIF 90  |
|                   | wage                 |          |           |         |          |         |
|                   |                      |          |           |         |          |         |
| Treatment effects | -0.014               | -0.035** | -0.033*** | -0.007  | 0.073*** | 0.001   |
|                   | (0.012)              | (0.011)  | (0.010)   | (0.011) | (0.017)  | (0.045) |
|                   |                      |          |           |         |          |         |
| Obs.              | <mark>5</mark> 5,982 | 55,982   | 55,982    | 55,982  | 55,982   | 55,982  |
| Covariates        | Yes                  | Yes      | Yes       | Yes     | Yes      | Yes     |

Table 4. 10: Unconditional estimates of the public sector pay gap with placebo in time test

*Notes:* \*\* p<0.05, \*\*\* p<0.01

The sample is constructed from the 2017-2019 Mexican National Occupations and Employment Survey. Municipality Fixed effects. Robust standard errors adjusted for 567 clusters at municipality level in parentheses.

Model (1) OLS standard estimation.

The placebo test weakens any interpretation of the estimates from equation (4) as representing causal effects. However, even if the policy was introduced ahead of time and the governing authorities had the power to do so, the impact still reduced the public sector wage growth. Therefore, the implementation of the policy before its formal adoption through a Constitutional reform had efficacious effects from the government's perspective.

<sup>&</sup>lt;sup>138</sup> The graphs showing these estimates from 5<sup>th</sup> to 95<sup>th</sup> percentiles are in Appendix C (Figures C4.5 and C4.6).

#### **4.8 Policy implications**

The results presented in this study have important policy implications since they suggest the 2018 policy intervention in Mexico (but possibly implemented earlier by the relevant authorities) constrained public sector wage growth, and disproportionately reduced the earnings of the lower-paid public sector workers compared to the higher-paid workers.

Nevertheless, although the policy reduced the public sector pay gap, the effects were more strongly felt among the lower-paid workers below the median. It could be argued that unduly low wages in the public sector could affect the competitiveness, performance, and recruitment of public sector employers. It in turn may be detrimental for the achievement of governmental objectives, such as the high-quality provision of essential public services. This is particularly important for Mexico where there is a higher proportion of public sector workers located at the lower half of the wage distribution. Therefore, the efficient provision and delivery of public goods are heavily reliant on these workers, a reduction in pay may reduce the incentives for these public servants to work more productively.

#### **4.9 Conclusions**

The literature commonly refers to the fact that public sector employment is associated with very large benefits, making the sector very attractive to employees. In the case of Mexico, many studies have implied that the public sector pay arrangements are set well above the minimum wage and pay more than the private sector to recruit and retain high-skilled workers. These pay settings arrangements may potentially widen the public–private wage gap (Finan et. al., 2015; Gasparini et al., 2015; Guindling et al., 2019).

In November 2018, the Federal Pay Reform on the Remuneration of Public Servants was enacted by the Government of Mexico to address historically high public sector pay remuneration levels. The proposed mechanism was intended to reduce the salary of senior management positions by up to 40% and freeze wages at lower points of the pay distribution. The research in this chapter investigates the impact of the introduction of this policy on the magnitude of the public–private wage differentials both at the mean and across the unconditional wage distribution. The evidence suggests that the regulation of the public sector wages in Mexico constrained its wage growth and subsequently led to a contraction in the wage gap between the employment sectors at the mean and with heterogeneous effects across the unconditional pay distribution.

The empirical findings suggest that the reform reduced the public sector pay gap by cutting the public sector pay of the lower-paid workers than through pay cuts in the payment of the higher-paid workers, as originally was proposed. The higher-paid public sector earners do not appear to have incurred any pay penalties due to the introduction of this policy. Thus, most of the public sector pay reductions are restricted to the bottom end of the pay distribution and with little evidence of the effects of this policy at the top end of it.

It appears the policy was implemented in advance of its adoption within the Mexican Constitution. The presence of anticipated effects undermine its interpretation as a causal effect, however, the proposal and discussion around this reform were probably within the public domain during the presidential election campaign of 2017 and was motivated by a political debate that prioritised the need for such reform. If its introduction was indeed by stealth, the evidence reveals that the policy reform served as a mechanism that constrained public sector wage growth over the period analysed. Specifically, it was responsible for reducing the public sector pay premium by 24% at the bottom end of the pay distribution and about 11% at the median.

Nevertheless, it more adversely and disproportionately affected the wages of those low-paid workers below the median. Given the size of the public sector workforce, the constraint on pay at the bottom end of the distribution may well have reduced overall public pay expenditure more than selective reductions in pay at the very top end of it. It raises an important issue around the design of policies and the need to consider the broader (sometimes unintended) consequences that the introduction of such policies may induce.

As an agenda for future research, it would be worth exploring why and how the policy was implemented ahead of time in the first place. In addition, it would also be informative to examine in more granular detail the pay adjustment decisions made, and determine why better paid public sector workers avoided the cuts in pay initially intended as part of the proposed reform.

#### **CHAPTER FIVE: Concluding remarks**

This thesis provides an empirical analysis of the effects of three separate government policy reforms, which were implemented in Mexico over the last 20 years or so. The three reforms focused on (i) raising the compulsory schooling leaving age to 15 years, (ii) up-rating the federal minimum wage, (iii) reforming the remuneration structure for public sector workers.

In the first policy, the Government of Mexico introduced an educational reform in 1993 with the aim of extending nationwide compulsory schooling from 12 to 15 years of age in order to compel the population to complete a secondary schooling level equivalent to nine years of education. The main goal of the government's education policy was to raise educational qualifications and increase human capital accumulation in the long run. The policy also anticipated that a more educated population would access better jobs and avail of enhanced economic opportunities later in life, thus exerting a positive impact on their earnings. Therefore, the schooling reform was viewed as a powerful tool for achieving lasting and sustainable economic development for the country going forward.

The econometric results suggest that the intervention represented an efficient strategy to enhance the level of educational attendance during the period analysed. It was associated with small average increases in the years of schooling for those exposed to the reform compared to those that were not. However, the introduction of the 1993 compulsory schooling law appears to have had no effect on the labour market outcomes of those subject to the treatment. It is conjectured that the failure to detect any meaningful labour market effects in terms either of earnings or the employment sector may be linked to a number of factors.

First, in the Mexican system, completing nine years of compulsory schooling does not imply the acquisition of a formal educational qualification that could either represent a signal to employers in the labour market or enhance productivity. Second, in contrast to developed economies, there may not have been complete compliance with the legislation from the start and its effect may have taken a protracted period of time to bed in beyond the period of analysis here. Third, although the relative supply of educated works may have increased as a consequence of this policy reform, the relative demand for such educated workers may not have increased by more to generate higher returns to education in the form of earnings. All these considerations merit attention as part of an agenda for future research. Specifically, further research could explore other possible channels linking compulsory schooling with wages in the labour market, by using additional measures related to educational attainments such as quality in education, levels of competencies in qualifications, and the role of the curriculum.

For the second policy reform, the Mexican legislation introduced in November 2012 streamlined the minimum wage (MW) areas from three to two geographic zones and implemented an up-rating in the zone that exhibited the lowest wages. This was also the zone with the largest workforce. The objective of this policy was to raise wages for the lowest-paid workers. This institutional factor in the labour market was seen as a mechanism through which wage levels of the lower-paid workers could be raised to improve their welfare and alleviate poverty among low-income households. The objective of the research was to determine if the up-rating raised the level of wages across the unconditional distribution and affected the overall distribution of wages.

The evidence suggests that this policy increased wages at the average by over 5% and across the unconditional hourly wage distribution. These results were found to be larger in magnitude compared to previous studies for Mexico and were also found to echo higher up the pay distribution than that suggested in other studies for Mexico. Overall, the up-rating was not found to reduce earnings inequality as the increase in wages appeared to be fairly homogeneous across the distribution.

The interpretation of the role of the MW as a numeraire for wage payments at a very high degree of re-benchmarking wages does not fully explain these large effects, and that they persisted well up the distribution. Hence, there may be other confounders in the labour market concurrent with the implementation of the MW intervention that exerted effects on the entire unconditional wage distribution. Further research on the possible confounders may be useful in explaining and understanding the MW effects detected at the end of the pay distribution in this study. Nevertheless, the research undertaken here suggested a useful methodological approach based on Recentred Influence Functions (RIFs) that could be used to evaluate the distributional effects of MW up-ratings in other contexts.

In November 2018, the Federal Pay Reform on the Remuneration of Public Servants was enacted by the Government of Mexico to address historically high levels of public sector pay remuneration. The proposed mechanism through which the policy was intended to be implemented was by a way of reductions in the salary of senior management positions of up to 40% and a freeze on wage increases at lower points of the pay distribution.

The econometric evidence suggests that the regulation of the wages constrained public sector wage growth and was responsible for reducing the public sector pay premium for workers paid below the median. However, the findings suggest the policy affected more the earnings of the lower-paid public sector workers compared to the higher-paid workers, whose earnings appeared to be largely unaffected by the reform in spite of the stated objectives regarding the higher paid. Although the use of a placebo in time revealed the presence of anticipated effects undermining its interpretation as a causal effect, it is clear that the public sector wage growth was constrained over the period analysed. It is conjectured that the early introduction of the reforms prior to the constitutional amendment was motivated by political debate within the context of the 2017 election that prioritized the need for such reform.

The findings for the three empirical chapters confirm that the policies established by the Mexican government accomplished their objectives: to increase years of schooling, to increase the wages of the lower-paid workers in a specific MW zone, and to constrain wage growth in the public sector.

These effects, however, especially for the two first policies did not have additional implications for the labour market outcomes as intended. The compulsory schooling policy did not raise earnings, and the MW up-rating intervention did not reduce wage inequality. In the case of the public pay reform, it reduced the public sector pay premium and also contracted the public–private sector wage gap, but it more adversely affected the wages of the low-paid workers compared to the higher-paid workers. This raises an important issue around the design of policies and the need to consider the broader (sometimes unintended) consequences that the introduction of such policies may induce.

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#### **Appendix A. Supplementary Material**

A2.1 Standard OLS Mincerian model for returns to education

Log hourly wages<sub>i</sub>

$$= \sigma_0 + \sigma_1(Years \ of \ schooling_i) + \sigma_2(Age_i) + \sigma_3(Age \ square_i) + \varphi_i$$

where the coefficient  $\widehat{\sigma_1}$  is the estimate of interest for the returns to education and it is compared to the 2SLS estimate of the Fuzzy RDD.

A2.2 Standard OLS Mincerian model for the employment sectoral choices

Labour market sector<sub>i</sub>

$$= \sigma_0 + \sigma_1(Years \ of \ schooling_i) + \sigma_2(Age_i) + \sigma_3(Age \ square_i) + \varphi_i$$

where the coefficient  $\widehat{\sigma_1}$  is the estimate of interest for the influence of the Reform on the probability of working in a specific *Labour market sector*: formal, informal, and self-employment. The coefficients are compared to the 2SLS of the Fuzzy RDD model.

A2.3 Model specification for holding a secondary schooling or high-school degree <u>First stage</u>

Academic certificate<sub>i</sub>

 $= \alpha_0 + \alpha_1(Treatment_i) + \alpha_2 F(Age in months_i) + \alpha_3 X_i + \varepsilon_i$ 

where Academic certificate<sub>i</sub> stands for: 1) Secondary schooling<sub>i</sub>, and 2)  $High - school_i$  as the highest academic degree that the individuals hold.

Reduced-form

Log hourly wages<sub>i</sub> =  $\beta_0 + \beta_1(Treatment_i) + \beta_2 F(Age in months_i) + \beta_3 X_i + \omega_i$ 

<u>2SLS</u>

Log hourly wages<sub>i</sub> =  $\delta_0 + \delta_1 (Academic \ certificate_i) + \delta_2 F(Age \ in \ months_i) + \delta_3 X_i + \mu_i$ 

A3.1 Standard OLS model for the logarithm of hourly wages

$$\begin{aligned} \text{Log hourly } w_i &= \gamma_0 + \gamma_1 \left( \text{POST12}_i \right) + \gamma_2 \left( \text{POST12}_i * \text{TREAT}_i \right) + \\ \gamma_3 \left( \text{POST12}_i * \text{Misc}_i \right) + \gamma_4 \text{Public}_i + \gamma_5 X_i + \delta_k + \omega_i \end{aligned}$$

where the coefficients,  $\hat{\gamma}_2$  and  $\hat{\gamma}_4$ , represent the treatment effects due to the MW policy and the participation in the public sector. These estimates are of interest for comparing them with the UQ regressions' results. Same covariates,  $X_i$ , described in equation (4).

**A4.1** Standard OLS model for the logarithm of hourly wages  $Log hourly w_i = \gamma_0 + \gamma_1 (POST18_i) + \gamma_2 (POST18_i * TREAT_i) + \gamma_3 TREAT_i + \gamma_5 X_i + \delta_k + \omega_i$ 

where the coefficient,  $\hat{\gamma}_2$ , represents the treatment effects due to the policy in the public sector. These robust estimates are of interest for comparing them with the UQ regressions' results. Same covariates,  $X_i$ , described in equation (4) were used in this specification.

## **Appendix B. Tables**

| Author(s)                         | Country          | Year of the   | Compulsory schooling                           | Empirical    | Earning Returns  |
|-----------------------------------|------------------|---------------|--|--------------|--|
| Developing countri                | es               |               |  | 51101087     |  |
| Aydemir and<br>Kirdar (2017)      | Turkey           | 1997          | from 5 to 8 years of schooling                 | RDD          | 2–2.5% for men (not statistically significant in most specifications) and 7–8% for women |
| Fang et. al. (2012)               | China            | 1986          | from 8 to 9 years of<br>schooling              | IV           | overall returns of 20%   |
| Spohr (2003)                      | Taiwan           | 1968          | from 6 to 9 years of<br>schooling              | IV           | 5.8% for men and 16.7% for women   |
| Developed countri                 | es               |               |  |              |  |
| Devereux and Hart<br>(2010)       | UK               | 1947          | aged from 14 to 15 years                       | RDD          | zero returns for women and 4–7% for men  |
| Dolton and Sandi<br>(2017)        | UK               | 1947-1972     | aged from 14 to 15, and 15 to 16 years         | RDD          | 6% returns for men   |
| Grenet (2013)                     | France and<br>UK | 1967 and 1972 | aged from 14 to 16, and<br>from 15 to 16 years | RDD          | close to zero returns for France and 6–7% for<br>England                                 |
| Oreopolus (2006)                  | UK               | 1947          | aged from 14 to 15 years                       | RDD          | overall returns of 15%   |
| Pischke and Von<br>Wächter (2008) | Germany          | 1947 -1969    | from 8th to 9th schooling<br>grade             | Diff-in-Diff | zero returns   |

Table B2. 1: Summary of some available estimates for the returns to compulsory schooling

Table B2. 2: Summary statistics for the cohort born before September 1981

| Variable            | Mean  | Std. Dev. | Min   | Max   |
|---------------------|-------|-----------|-------|-------|
| Birth cohort        | 1978  | 1.83      | 1975  | 1981  |
| Age                 | 34.16 | 3.08      | 27    | 40    |
| Urban status        | 0.59  | 0.49      | 0     | 1     |
| Years of schooling  | 10.08 | 4.11      | 0     | 24    |
| Log of hourly wages | 3.12  | 0.64      | 0.009 | 7.640 |
| Informal employment | 0.45  | 0.5       | 0     | 1     |
| Self-employment     | 0.19  | 0.39      | 0     | 1     |
| Economic sector:    |       |           |       |       |
| Agriculture         | 0.09  | 0.29      | 0     | 1     |
| Commerce            | 0.16  | 0.36      | 0     | 1     |
| Construction        | 0.15  | 0.36      | 0     | 1     |
| Manufacturing       | 0.19  | 0.39      | 0     | 1     |
| Services            | 0.39  | 0.49      | 0     | 1     |

Source: Mexican National Occupations and Employment Survey (2009-2017). Total observations 74,618.

| Variable            | Mean  | Std. Dev. | Min   | Max   |
|---------------------|-------|-----------|-------|-------|
| Birth cohort        | 1984  | 1.78      | 1981  | 1987  |
| Age                 | 28.44 | 2.78      | 24    | 35    |
| Urban status        | 0.60  | 0.49      | 0     | 1     |
| Years of schooling  | 10.64 | 3.88      | 0     | 22    |
| Log of hourly wages | 3.09  | 0.59      | 0.003 | 7.017 |
| Informal employment | 0.44  | 0.50      | 0     | 1     |
| Self-employment     | 0.13  | 0.34      | 0     | 1     |
| Economic sector:    |       |           |       |       |
| Agriculture         | 0.08  | 0.27      | 0     | 1     |
| Commerce            | 0.17  | 0.38      | 0     | 1     |
| Construction        | 0.15  | 0.36      | 0     | 1     |
| Manufacturing       | 0.20  | 0.40      | 0     | 1     |
| Services            | 0.38  | 0.49      | 0     | 1     |

Table B2. 3: Summary statistics for the cohort born on and after September 1981

Source: Mexican National Occupations and Employment Survey (2009-2017). Total observations 74,346.

State Region State Region State Region Aguascalientes Chiapas Colima 1 Guerrero Coahuila Mexico 6 Oaxaca Morelos Jalisco Nayarit Nuevo Leon 4 Campeche Queretaro Hidalgo Quintana Roo **Distrito Federal** 7 Puebla Sinaloa 2 San Luis Potosí Yucatán Tabasco Baja California Veracruz Baja California Sur Chihuahua 5 Durango Sonora Guanajuato Michoacan 3 Tamaulipas Tlaxcala Zacate cas

Table B2. 4: Mexican Socioeconomic regions

*Notes*: Mexican Statistics, Geography, and Informatics. Institute Socioeconomic regions of Mexico. The first region represents the stratus associated with the lowest level of welfare and the seventh with the highest level.

| Estimation method    |         | First    | -stage    |         |         | Reduc     | ed-form    |         |         | 2         | SLS       |         |
|----------------------|---------|----------|-----------|---------|---------|-----------|------------|---------|---------|-----------|-----------|---------|
| Dependent variable   | -       | Years of | schooling |         |         | Log of ho | ourly wage | 25      |         | Log of ho | urly wage | s       |
|                      | (a)     | (b)      | (c)       | (d)     | (a)     | (b)       | (c)        | (d)     | (a)     | (b)       | (c)       | (d)     |
| Treatment            | 0.139*  | 0.138*   | 0.131     | 0.109   | 0.019   | 0.019     | 0.019      | 0.015   |         |           |           |         |
|                      | (0.082) | (0.082)  | (0.082)   | (0.080) | (0.012) | (0.012)   | (0.012)    | (0.012) |         |           |           |         |
| Years of schooling   |         |          |           |         |         |           |            |         | 0.142   | 0.143     | 0.145     | 0.146   |
| -                    |         |          |           |         |         |           |            |         | (0.093) | (0.094)   | (0.099)   | (0.119) |
| Obs.                 | 88,227  | 88,227   | 88,227    | 88,227  | 88,227  | 88,227    | 88,227     | 88,227  | 88,227  | 88,227    | 88,227    | 88,227  |
| Survey year dummies  | No      | Yes      | Yes       | Yes     | No      | Yes       | Yes        | Yes     | No      | Yes       | Yes       | Yes     |
| Birth region dummies | No      | No       | Yes       | Yes     | No      | No        | Yes        | Yes     | No      | No        | Yes       | Yes     |
| Urban status         | No      | No       | No        | Yes     | No      | No        | No         | Yes     | No      | No        | No        | Yes     |
|                      |         |          |           |         |         |           |            |         |         |           |           |         |

Table B2. 5: Parametric results of the returns to compulsory schooling: pooled sample

Notes: \*p<0.1, \*\* p<0.05, \*\*\* p<0.01

The sample is constructed from the 2009-2017 Mexican National Occupations and Employment Survey. Robust standard errors using EHW correction as recommended by Kolesár and Rothe (2018) in parentheses.

# Table B2. 6: Parametric results of the returns to compulsory schooling: variations in years of schooling and earnings

| Estimation method                        |        |        | First    | stage     |        |        |        |        | Reduo     | ed-form   |        |        |                   |                  | 2                | SLS              |                  |                  |
|--|--------|--------|----------|-----------|--------|--------|--------|--------|-----------|-----------|--------|--------|-------------------|------------------|------------------|------------------|------------------|------------------|
| Dependent variable                       | -      |        | Years of | schooling |        | -      |        |        | Log of ho | urly wage | s      | -      |                   |                  | Log of ho        | urly wage        | 5                | -                |
|  | (a)    | (b)    | (c)      | (d)       | (e)    | (f)    | (a)    | (b)    | (c)       | (d)       | (e)    | (f)    | (a)               | (b)              | (c)              | (d)              | (e)              | (f)              |
| Treatment                                | 0.153* | 0.143* | 0.120    | 0.136     | 0.128  | 0.107  | 0.022* | 0.021* | 0.018     | 0.014     | 0.014  | 0.011  |                   |                  |                  |                  |                  |                  |
| Years of schooling                       | (,     | ()     | ()       | (,        | (,     | (,     | (,     | (,     | (,        | (,        | (,     | (,     | 0.143*<br>(0.085) | 0.147<br>(0.092) | 0.148<br>(0.109) | 0.106<br>(0.080) | 0.106<br>(0.084) | 0.101<br>(0.100) |
| Obs.                                     | 87,011 | 87,011 | 87,011   | 87,090    | 87,090 | 87,090 | 87,011 | 87,011 | 87,011    | 87,090    | 87,090 | 87,090 | 87,011            | 87,011           | 87,011           | 87,090           | 87,090           | 87,090           |
| Less than 18 years of schooling          | Yes    | Yes    | Yes      | No        | No     | No     | Yes    | Yes    | Yes       | No        | No     | No     | Yes               | Yes              | Yes              | No               | No               | No               |
| Trimmed hourly wages                     | No     | No     | No       | Yes       | Yes    | Yes    | No     | No     | No        | Yes       | Yes    | Yes    | No                | No               | No               | Yes              | Yes              | Yes              |
| Survey year and birth<br>region dum mies | No     | Yes    | Yes      | No        | Yes    | Yes    | No     | Yes    | Yes       | No        | Yes    | Yes    | No                | Yes              | Yes              | No               | Yes              | Yes              |
| Urban status                             | No     | No     | Yes      | No        | No     | Yes    | No     | No     | Yes       | No        | No     | Yes    | No                | No               | Yes              | No               | No               | Yes              |

Notes: \*p<0.1, \*\* p<0.05, \*\*\* p<0.01 The sample is constructed from the 2009-2017 Mexican National Occupations and Employment Survey. Robust standard errors using EHW correction as recommended by Kolesár and Rothe (2018) in parentheses.

| Estimation method                       |                  |                  | First            | -stage           |                 |                  |                   |                 | Reduc             | ed-form           |                   |                   |                   |                   | 2                 | SLS               |                   |                   |
|---|------------------|------------------|------------------|------------------|-----------------|------------------|-------------------|-----------------|-------------------|-------------------|-------------------|-------------------|-------------------|-------------------|-------------------|-------------------|-------------------|-------------------|
| Dependent variable                      | -                |                  | Years of         | schoolin         | g               | -                |                   |                 | Log of ho         | urly wage         | es                | -                 |                   |                   | Log of ho         | ourly wag         | es                | -                 |
|   | (a)              | (b)              | (c)              | (d)              | (e)             | (f)              | (a)               | (b)             | (c)               | (d)               | (e)               | (f)               | (a)               | (b)               | (C)               | (d)               | (e)               | (f)               |
| Treatment                               | 0.110<br>(0.141) | 0.082<br>(0.141) | 0.140<br>(0.150) | 0.144<br>(0.180) | 0.100 (0.147)   | 0.130<br>(0.152) | -0.028<br>(0.022) | -0.028 (0.022)  | -0.027<br>(0.024) | -0.011<br>(0.019) | -0.014<br>(0.022) | -0.011<br>(0.023) |                   |                   |                   |                   |                   |                   |
| Years of schooling                      |                  |                  |                  |                  |                 |                  |                   |                 |                   |                   |                   |                   | -0.254<br>(0.432) | -0.343<br>(0.678) | -0.195<br>(0.298) | -0.080<br>(0.203) | -0.137<br>(0.328) | -0.083<br>(0.271) |
| Obs.                                    | 82,794           | 82,794           | 82,794           | 80,096           | 80,096          | 80,096           | 82,794            | 82,794          | 82,794            | 80,096            | 80,096            | 80,096            | 82,794            | 82,794            | 82,794            | 80,096            | 80,096            | 80,096            |
| Eff. Number of obs.<br>Bandwidth        | 34,272<br>48.03  | 33,180<br>47.53  | 28,450<br>41.21  | 28,750<br>41.72  | 29,836<br>41.71 | 27,537<br>38.22  | 34,272<br>48.03   | 33,180<br>47.53 | 28,450<br>41.21   | 28,750<br>41.72   | 29,836<br>41.71   | 27,537<br>38.22   | 34,272<br>48.03   | 33,180<br>47.53   | 28,450<br>41.21   | 28,750<br>41.72   | 29,836<br>41.71   | 27,537<br>38.22   |
| Less than 18 years of<br>schooling      | No               | No               | No               | Yes              | Yes             | Yes              | No                | No              | No                | Yes               | Yes               | Yes               | No                | No                | No                | Yes               | Yes               | Yes               |
| Trimmed hourly<br>wages                 | No               | No               | No               | Yes              | Yes             | Yes              | No                | No              | No                | Yes               | Yes               | Yes               | No                | No                | No                | Yes               | Yes               | Yes               |
| Survey year and birth<br>region dummies | No               | Yes              | Yes              | No               | Yes             | Yes              | No                | Yes             | Yes               | No                | Yes               | Yes               | No                | Yes               | Yes               | No                | Yes               | Yes               |
| Urban status                            | No               | No               | Yes              | No               | No              | Yes              | No                | No              | Yes               | No                | No                | Yes               | No                | No                | Yes               | No                | No                | Yes               |
| Estimation method                       |                  |                  | First            | -stage           |                 |                  |                   |                 | Reduc             | ed-form           |                   |                   |                   |                   | 2                 | SLS               |                   |                   |

Table B2. 7: Non-parametric results of the returns to compulsory schooling: formal sector

| Estimation method                       | _       |         | First    | -stage   |         | _       |         |         | Reduc     | ed-form    |         | -       |         |         | 2         | SLS       |         | _       |
|---|---------|---------|----------|----------|---------|---------|---------|---------|-----------|------------|---------|---------|---------|---------|-----------|-----------|---------|---------|
| Dependent variable                      |         |         | Years of | schoolin | g       |         |         |         | Log of ho | ourly wage | 25      |         |         |         | Log of ho | ourly wag | es      |         |
|   | (g)     | (h)     | (i)      | (i)      | (k)     | (I)     | (g)     | (h)     | (i)       | (j)        | (k)     | (I)     | (g)     | (h)     | (1)       | (i)       | (k)     | (1)     |
| Treatment                               | 0.133   | 0.118   | 0.171    | 0.130    | 0.059   | 0.097   | -0.029  | -0.030  | -0.024    | -0.010     | -0.013  | -0.013  |         |         |           |           |         |         |
|   | (0.143) | (0.146) | (0.155)  | (0.171)  | (0.140) | (0.147) | (0.022) | (0.023) | (0.025)   | (0.020)    | (0.021) | (0.022) |         |         |           |           |         |         |
| Years of schooling                      |         |         |          |          |         |         |         |         |           |            |         |         | -0.222  | -0.257  | -0.140    | -0.080    | -0.226  | -0.129  |
|   |         |         |          |          |         |         |         |         |           |            |         |         | (0.320) | (0.383) | (0.254)   | (0.233)   | (0.716) | (0.346) |
| Obs.                                    | 81,055  | 81,055  | 81,055   | 81,816   | 81,816  | 81,816  | 81,055  | 81,055  | 81,055    | 81,816     | 81,816  | 81,816  | 81,055  | 81,055  | 81,055    | 81,816    | 81,816  | 81,816  |
| Eff. Number of obs.                     | 31,266  | 29,054  | 25,506   | 31,623   | 34,994  | 30,490  | 31,266  | 29,054  | 25,506    | 31,623     | 34,994  | 30,490  | 31,266  | 29,054  | 25,506    | 31,623    | 34,994  | 30,490  |
| Bandwidth                               | 45.48   | 43.11   | 37.10    | 45.44    | 47.07   | 42.03   | 45.48   | 43.11   | 37.10     | 45.44      | 47.07   | 42.03   | 45.48   | 43.11   | 37.10     | 45.44     | 47.07   | 42.03   |
| Less than 18 years of<br>schooling      | Yes     | Yes     | Yes      | No       | No      | No      | Yes     | Yes     | Yes       | No         | No      | No      | Yes     | Yes     | Yes       | No        | No      | No      |
| Trimmed hourly                          | No      | No      | No       | Yes      | Yes     | Yes     | No      | No      | No        | Yes        | Yes     | Yes     | No      | No      | No        | Yes       | Yes     | Yes     |
| Survey year and birth<br>region dummies | No      | Yes     | Yes      | No       | Yes     | Yes     | No      | Yes     | Yes       | No         | Yes     | Yes     | No      | Yes     | Yes       | No        | Yes     | Yes     |
| Urban status                            | No      | No      | Yes      | No       | No      | Yes     | No      | No      | Yes       | No         | No      | Yes     | No      | No      | Yes       | No        | No      | Yes     |

 
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 standard errors using EHW correction as recommended by Kolesár and Rothe (2018) in parentheses.

| Estimation method                       | ation method<br>(dent variable         First-stage<br>Years of school<br>(a)         First-stage<br>(b)         (c)         (d)         (d) <th></th> <th></th> <th></th> <th></th> <th>Reduc</th> <th>ed-form</th> <th></th> <th></th> <th></th> <th></th> <th>2</th> <th>SLS</th> <th></th> <th></th> |                 |                 |                 |                 |                 |                 |                 | Reduc           | ed-form         |                 |                 |                  |                  | 2                | SLS              |                  |                  |
|---|---|-----------------|-----------------|-----------------|-----------------|-----------------|-----------------|-----------------|-----------------|-----------------|-----------------|-----------------|------------------|------------------|------------------|------------------|------------------|------------------|
| Dependent variable                      | -   |                 | Years of        | schoolin        | g               | -               |                 |                 | Log of ho       | urly wag        | 25              | -               |                  |                  | Log of ho        | ourly wag        | es               | -                |
|   | (a)   | (b)             | (c)             | (d)             | (e)             | (f)             | (a)             | (b)             | (c)             | (d)             | (e)             | (f)             | (a)              | (b)              | (C)              | (d)              | (e)              | (f)              |
| Treatment                               | 0.046   | 0.024           | 0.012           | 0.151           | 0.115           | 0.091           | 0.060           | 0.055           | 0.051           | 0.034           | 0.030           | 0.027           |                  |                  |                  |                  |                  |                  |
| Years of schooling                      | (0.200)   | (0.201)         | (0.227)         | (0.220)         | (0.207)         | (0.202)         | (0.000)         | (0.002)         | (0.001)         | (0.001)         | (0.000)         | (0.000)         | 0.423<br>(0.200) | 0.461<br>(0.235) | 0.494<br>(0.268) | 0.222<br>(0.264) | 0.259<br>(0.372) | 0.290<br>(0.504) |
| Obs.                                    | 66,170  | 66,170          | 66,170          | 64,939          | 64,939          | 64,939          | 66,170          | 66,170          | 66,170          | 64,939          | 64,939          | 64,939          | 66,170           | 66,170           | 66,170           | 64,939           | 64,939           | 64,939           |
| Eff. Number of obs.<br>Bandwidth        | 28,925<br>28.88   | 28,925<br>29.05 | 28,925<br>29.22 | 15,780<br>28.90 | 15,780<br>28.58 | 15,780<br>29.02 | 28,925<br>28.88 | 28,925<br>29.05 | 28,925<br>29.22 | 15,780<br>28.90 | 15,780<br>28.58 | 15,780<br>29.02 | 28,925<br>28.88  | 28,925<br>29.05  | 28,925<br>29.22  | 15,780<br>28.90  | 15,780<br>28.58  | 15,780<br>29.02  |
| Less than 18 years of<br>schooling      | No  | No              | No              | Yes             | Yes             | Yes             | No              | No              | No              | Yes             | Yes             | Yes             | No               | No               | No               | Yes              | Yes              | Yes              |
| Trimmed hourly<br>wages                 | No  | No              | No              | Yes             | Yes             | Yes             | No              | No              | No              | Yes             | Yes             | Yes             | No               | No               | No               | Yes              | Yes              | Yes              |
| Survey year and birth<br>region dummies | No  | Yes             | Yes             | No              | Yes             | Yes             | No              | Yes             | Yes             | No              | Yes             | Yes             | No               | Yes              | Yes              | No               | Yes              | Yes              |
| Urban status                            | No  | No              | Yes             | No              | No              | Yes             | No              | No              | Yes             | No              | No              | Yes             | No               | No               | Yes              | No               | No               | Yes              |
|   |   |                 |                 |                 |                 |                 |                 |                 |                 |                 |                 |                 |                  |                  |                  |                  |                  |                  |
| Estimation method                       | _   |                 | First           | -stage          |                 | _               |                 |                 | Reduc           | ed-form         |                 | _               |                  |                  | 2                | SLS              |                  | _                |
| Dependent variable                      |   |                 | Vegrs of        | rchoolin        | -               |                 |                 |                 | Log of he       | univ wom        | 20              |                 |                  |                  | Log of he        | our la surar     | OF.              |                  |

### Table B2. 8: Non-parametric results of the returns to compulsory schooling: informal sector

|        |   | 11120   | -zage   |  | Reduced-form  |  |  |  |  |  |  | 2SLS   |  |  |  |  |   |
|--------|---|---|---|--|---|--|--|--|--|--|--|--|--|--|--|--|---|
|        |   | Years of  | schooling   | g  | -   |  |  | Log of ho  | urly wage  | 25   | -  |  |  | Log of ho  | urly wag   | es   | -   |
| (g)    | (h)   | (i)   | (j)   | (k)  | (I)   | (g)  | (h)  | (i)  | (i)  | (k)  | (I)  | (g)  | (h)  | ()   | (j)  | (k)  | (1)   |
| .091   | 0.073   | 0.055   | 0.123   | 0.085  | 0.097   | 0.071  | 0.066  | 0.063  | 0.026  | 0.023  | 0.019  |  |  |  |  |  |   |
| 0.208) | (0.202)   | (0.196)   | (0.209)   | (0.207)  | (0.147)   | (0.033)  | (0.032)  | (0.031)  | (0.030)  | (0.030)  | (0.029)  |  |  |  |  |  |   |
|        |   |   |   |  | · · ·   |  |  |  |  |  |  | 0.401  | 0.428  | 0.457  | 0.210  | 0.260  | 0.282   |
|        |   |   |   |  |   |  |  |  |  |  |  | (0.176)  | (0.200)  | (0.223)  | (0.293)  | (0.457)  | (0.606)   |
| 5,899  | 65,899  | 65,899  | 65,199  | 65,199   | 65,199  | 65,899   | 65,899   | 65,899   | 65,199   | 65,199   | 65,199   | 65,899   | 65,899   | 65,899   | 65,199   | 65,199   | 65,199  |
| 8,795  | 28,795  | 28,795  | 15,859  | 15,859   | 15,859  | 28,795   | 28,795   | 28,795   | 15,859   | 15,859   | 15,859   | 28,795   | 28,795   | 28,795   | 15,859   | 15,859   | 15,859  |
| 8.56   | 28.84   | 28.92   | 29.68   | 29.26  | 29.75   | 28.56  | 28.84  | 28.92  | 29.68  | 29.26  | 29.75  | 28.56  | 28.84  | 28.92  | 29.68  | 29.26  | 29.75   |
| Yes    | Yes   | Yes   | No  | No   | No  | Yes  | Yes  | Yes  | No   | No   | No   | Yes  | Yes  | Yes  | No   | No   | No  |
| No     | No  | No  | Yes   | Yes  | Yes   | No   | No   | No   | Yes  | Yes  | Yes  | No   | No   | No   | Yes  | Yes  | Yes   |
| No     | Yes   | Yes   | No  | Yes  | Yes   | No   | Yes  | Yes  | No   | Yes  | Yes  | No   | Yes  | Yes  | No   | Yes  | Yes   |
| No     | No  | Yes   | No  | No   | Yes   | No   | No   | Yes  | No   | No   | Yes  | No   | No   | Yes  | No   | No   | Yes   |
|        | (g)<br>091<br>0.208)<br>5,899<br>3,795<br>3.56<br>Yes<br>No<br>No<br>No | (g) (h)<br>091 0.073<br>1.208) (0.202)<br>3,899 65,899<br>3,795 28,795<br>3.56 28,84<br>Yes Yes<br>No No<br>No Yes<br>No No | (g)         (h)         (i)           091         0.073         0.055           1.208)         (0.202)         (0.196)           3,795         28,795         28,795           3,56         28.84         28.92           Yes         Yes         Yes           No         No         No           No         Yes         Yes           No         No         Yes | (g)         (h)         (i)         (ii)         (iii)         (iii)         (iii)         (iii)         (iii)         (iii)         (iiii)         (iiiii)         (iiii)         (iiiii)         (iiii)         (iiii)         (iiiiiiii)         (iiiiiiiiii)         (iiiii) | (g)         (h)         (i)         (i)         (k)           091         0.073         0.055         0.123         0.085           1.208)         (0.202)         (0.196)         (0.209)         (0.207)           3,899         65,899         65,199         65,199         65,199           3,795         28,795         15,859         15,859         15,859           3,56         28.84         28,92         29.68         29.26           Yes         Yes         Yes         Yes         No         No           No         No         No         Yes         Yes         No           No         Yes         Yes         Yes         No         No           No         No         No         Yes         No         No | (g)         (h)         (i)         (i)         (k)         (l)           091         0.073         0.055         0.123         0.085         0.097           1.208         (0.202)         (0.196)         (0.209)         (0.207)         (0.147)           3.899         65,899         65,199         65,199         65,199         65,199           3.795         28,795         15,859         15,859         15,859         15,859           3.56         28.84         28.92         29.68         29.26         29.75           Yes         Yes         Yes         Yes         No         No           No         No         No         Yes         Yes         Yes           No         No         No         Yes         Yes         No           No         No         Yes         No         Yes         Yes | (g)         (h)         (i)         (i)         (k)         (i)         (g)           091         0.073         0.055         0.123         0.085         0.097         0.071           1.208)         (0.202)         (0.196)         (0.209)         (0.207)         (0.147)         (0.033)           3.899         65,899         65,199         65,199         65,199         65,899         3.55         28,795         28,795           3.56         28.84         28.92         29.68         29.75         28.56         Yes         Yes         Yes         No         No         Yes         No         No         Yes         Nes         No         No         Yes         No         No         Yes         No         No         No         Yes         No         No         No         Yes         No         No         Yes         No         No         Yes         No         No         Yes         No         No         No         Yes         No         No         No         Yes         No         Yes         No         N | (g)         (h)         (i)         (i)         (k)         (i)         (g)         (h)           091         0.073         0.055         0.123         0.085         0.097         0.071         0.066           1208)         (0.202)         (0.196)         (0.209)         (0.207)         (0.147)         0.066           1208)         (0.202)         (0.196)         (0.209)         (0.207)         (0.147)         0.066           1208)         65,899         65,199         65,199         65,199         65,899         65,899         58,895         28,795         28,795         28,795         28,795         28,795         28,795         28,795         28,795         28,795         28,795         28,795         28,879         28,795         28,879         28,795         28,879         28,879         28,879         28,879         28,879         28,879         28,879 </td <td>(g)         (h)         (i)         (i)         (k)         (i)         (g)         (h)         (i)         (i)         (k)         (i)         (g)         (h)         (i)         (i)<td>(g)         (h)         (i)         (i)         (ii)         (k)         (ii)         (g)         (h)         (ii)         (iii)         (iiii)         (iii)         (iiiiii)         (iiii)         (iiii)</td><td>(g)         (h)         (i)         (i)         (k)         (i)         (k)         (k)<td>(g)         (h)         (i)         (i)         (k)         (i)         (g)         (h)         (i)         (k)         (i)         (i)         (k)         (i)         (i)         (k)         (k)<td>(g)         (h)         (i)         (i)<td>(g)         (h)         (i)         (i)         (k)         (i)         (g)         (h)         (i)         (k)         (i)         (g)         (h)         (g)<td>(g)         (h)         (i)         (i)         (k)         (i)         (g)         (h)         (i)         (k)         (i)         (k)         (i)         (i)<td>(g)         (h)         (i)         (i)<td>(g)         (h)         (i)         (k)         (i)         (g)         (h)         (h)         (g)</td></td></td></td></td></td></td></td> | (g)         (h)         (i)         (i)         (k)         (i)         (g)         (h)         (i)         (i)         (k)         (i)         (g)         (h)         (i)         (i) <td>(g)         (h)         (i)         (i)         (ii)         (k)         (ii)         (g)         (h)         (ii)         (iii)         (iiii)         (iii)         (iiiiii)         (iiii)         (iiii)</td> <td>(g)         (h)         (i)         (i)         (k)         (i)         (k)         (k)<td>(g)         (h)         (i)         (i)         (k)         (i)         (g)         (h)         (i)         (k)         (i)         (i)         (k)         (i)         (i)         (k)         (k)<td>(g)         (h)         (i)         (i)<td>(g)         (h)         (i)         (i)         (k)         (i)         (g)         (h)         (i)         (k)         (i)         (g)         (h)         (g)<td>(g)         (h)         (i)         (i)         (k)         (i)         (g)         (h)         (i)         (k)         (i)         (k)         (i)         (i)<td>(g)         (h)         (i)         (i)<td>(g)         (h)         (i)         (k)         (i)         (g)         (h)         (h)         (g)</td></td></td></td></td></td></td> | (g)         (h)         (i)         (i)         (ii)         (k)         (ii)         (g)         (h)         (ii)         (iii)         (iiii)         (iii)         (iiiiii)         (iiii)         (iiii) | (g)         (h)         (i)         (i)         (k)         (i)         (k)         (k) <td>(g)         (h)         (i)         (i)         (k)         (i)         (g)         (h)         (i)         (k)         (i)         (i)         (k)         (i)         (i)         (k)         (k)<td>(g)         (h)         (i)         (i)<td>(g)         (h)         (i)         (i)         (k)         (i)         (g)         (h)         (i)         (k)         (i)         (g)         (h)         (g)<td>(g)         (h)         (i)         (i)         (k)         (i)         (g)         (h)         (i)         (k)         (i)         (k)         (i)         (i)<td>(g)         (h)         (i)         (i)<td>(g)         (h)         (i)         (k)         (i)         (g)         (h)         (h)         (g)</td></td></td></td></td></td> | (g)         (h)         (i)         (i)         (k)         (i)         (g)         (h)         (i)         (k)         (i)         (i)         (k)         (i)         (i)         (k)         (k) <td>(g)         (h)         (i)         (i)<td>(g)         (h)         (i)         (i)         (k)         (i)         (g)         (h)         (i)         (k)         (i)         (g)         (h)         (g)<td>(g)         (h)         (i)         (i)         (k)         (i)         (g)         (h)         (i)         (k)         (i)         (k)         (i)         (i)<td>(g)         (h)         (i)         (i)<td>(g)         (h)         (i)         (k)         (i)         (g)         (h)         (h)         (g)</td></td></td></td></td> | (g)         (h)         (i)         (i) <td>(g)         (h)         (i)         (i)         (k)         (i)         (g)         (h)         (i)         (k)         (i)         (g)         (h)         (g)<td>(g)         (h)         (i)         (i)         (k)         (i)         (g)         (h)         (i)         (k)         (i)         (k)         (i)         (i)<td>(g)         (h)         (i)         (i)<td>(g)         (h)         (i)         (k)         (i)         (g)         (h)         (h)         (g)</td></td></td></td> | (g)         (h)         (i)         (i)         (k)         (i)         (g)         (h)         (i)         (k)         (i)         (g)         (h)         (g) <td>(g)         (h)         (i)         (i)         (k)         (i)         (g)         (h)         (i)         (k)         (i)         (k)         (i)         (i)<td>(g)         (h)         (i)         (i)<td>(g)         (h)         (i)         (k)         (i)         (g)         (h)         (h)         (g)</td></td></td> | (g)         (h)         (i)         (i)         (k)         (i)         (g)         (h)         (i)         (k)         (i)         (k)         (i)         (i) <td>(g)         (h)         (i)         (i)<td>(g)         (h)         (i)         (k)         (i)         (g)         (h)         (h)         (g)</td></td> | (g)         (h)         (i)         (i) <td>(g)         (h)         (i)         (k)         (i)         (g)         (h)         (h)         (g)</td> | (g)         (h)         (i)         (k)         (i)         (g)         (h)         (h)         (g) |

The sample is constructed from the 2009-2017 Mexican National Occupations and Employment Survey. Following Calonico et al. (2018) and Calonico et al. (2014) for the optimal bandwidth. Robust standard errors using EHW correction as recommended by Kolesár and Rothe (2018) in parentheses.

|                                 |         |         |            |             |         |         | 42-month | windo   | W         |          |         |         |         |         |          |          |         |         |
|---------------------------------|---------|---------|------------|-------------|---------|---------|----------|---------|-----------|----------|---------|---------|---------|---------|----------|----------|---------|---------|
| Estimation method               | _       |         | First      | -stage      | _       |         |          |         | Reduc     | ed-form  | _       |         |         |         | 25       | SLS      | _       |         |
| Dependent variable              | _       |         | Years of s | schoo lin g |         |         |          | 1       | Log of ho | urly wag | es      |         |         | Lo      | g of hou | rly wage | is      |         |
|                                 | (a)     | (b)     | (c)        | (d)         | (e)     | (f)     | (a)      | (b)     | (c)       | (d)      | (e)     | (f)     | (a)     | (b)     | (c)      | (d)      | (e)     | (f)     |
| <b>-</b>                        | 0.075   | 0.050   | 0.050      | 0.000       | 0.074   | 0.070   | 0.004    | 0.000   | 0.010     | 0.005    | 0.007   | 0.007   |         |         |          |          |         |         |
| Treatment                       | 0.075   | 0.063   | 0.060      | 0.088       | 0.074   | 0.070   | 0.004    | 0.006   | 0.016     | 0.005    | 0.007   | 0.007   |         |         |          |          |         |         |
| ~ .                             | (0.102) | (0.102) | (0.101)    | (0.101)     | (0.100) | (0.099) | (0.016)  | (0.016) | (0.016)   | (0.016)  | (0.016) | (0.016) | 0.050   | 0.004   | 0.000    | 0.050    | 0.000   | 0.005   |
| Years of schooling              |         |         |            |             |         |         |          |         |           |          |         |         | 0.058   | 0.094   | 0.095    | 0.062    | 0.096   | 0.095   |
|                                 |         |         |            |             |         |         |          |         |           |          |         |         | (0.189) | (0.226) | (0.239)  | (0.162)  | (0.196) | (0.206) |
| Obs.                            | 49,112  | 49,112  | 49,112     | 48,068      | 48,068  | 48,068  | 49,112   | 49,112  | 49,112    | 48,068   | 48,068  | 48,068  | 49,112  | 49,112  | 49,112   | 48,068   | 48,068  | 48,068  |
| Less than 18 years of schooling | No      | No      | No         | Yes         | Yes     | Yes     | No       | No      | No        | Yes      | Yes     | Yes     | No      | No      | No       | Yes      | Yes     | Yes     |
| Trimmed hourly wages            | No      | No      | No         | No          | No      | No      | No       | No      | No        | No       | No      | No      | No      | No      | No       | No       | No      | No      |
| Survey year and birth region    | No      | Var     | Var        | No          | Var     | Vec     | No       | Var     | Var       | No       | Var     | Ver     | No      | Vac     | Var      | No       | Ver     | Var     |
| dummies                         | NO      | Tes     | res        | NO          | Tes     | Tes     | NO       | Tes     | Tes       | NO       | res     | 165     | NO      | Tes     | res      | NO       | res     | res     |
| Urban status                    | No      | No      | Yes        | No          | No      | Yes     | No       | No      | Yes       | No       | No      | Yes     | No      | No      | Yes      | No       | No      | Yes     |
| Estimation method               |         |         | First      | -stage      |         |         |          |         | Reduc     | ed-form  |         |         |         |         | 25       | 15       |         |         |
| Dependent variable              | -       |         | Years of s | schoo line  | -       |         |          |         | Log of ho | urly was | es      |         |         | Lo      | e of hou | rlv wage | -       |         |
|                                 | (g)     | (h)     | (i)        | (j)         | (k)     | (1)     | (g)      | (h)     | (i)       | (j)      | (k)     | (I)     | (g)     | (h)     | (i)      | (j)      | (k)     | (1)     |
| Treatment                       | 0.052   | 0.043   | 0.036      | 0.065       | 0.054   | 0.047   | 0.007    | 0.008   | 0.007     | 800.0    | 0.009   | 0.008   |         |         |          |          |         |         |
|                                 | (0.103) | (0.102) | (0.101)    | (0.102)     | (0.101) | (0.100) | (0.015)  | (0.015) | (0.015)   | (0.015)  | (0.015) | (0.015) |         |         |          |          |         |         |
| Years of schooling              |         |         |            |             |         |         |          |         |           |          |         |         | 0.130   | 0.187   | 0.202    | 0.123    | 0.170   | 0.179   |
| -                               |         |         |            |             |         |         |          |         |           |          |         |         | (0.279) | (0.409) | (0.521)  | (0.221)  | (0.308) | (0.366) |
| Obs.                            | 48,571  | 48,571  | 48,571     | 47,539      | 47,539  | 47,539  | 48,571   | 48,571  | 48,571    | 47,539   | 47,539  | 47,539  | 48,571  | 48,571  | 48,571   | 47,539   | 47,539  | 47,539  |
| Less than 18 years of schooling | No      | No      | No         | Yes         | Yes     | Yes     | No       | No      | No        | Yes      | Yes     | Yes     | No      | No      | No       | Yes      | Yes     | Yes     |
| Trimmed hourly wages            | Yes     | Yes     | Yes        | Yes         | Yes     | Yes     | Yes      | Yes     | Yes       | Yes      | Yes     | Yes     | Yes     | Yes     | Yes      | Yes      | Yes     | Yes     |
| Survey year and birth region    |         | Mar.    | N          |             | Mara    | Mara    |          | Maria   | N         |          |         | N       |         | Mar     | Mara     |          | ×       | Mara 1  |
| dummies                         | NO      | Yes     | Yes        | NO          | Yes     | Yes     | NO       | res     | Yes       | NO       | Yes     | Yes     | NO      | Yes     | Yes      | NO       | Yes     | Yes     |
| Urban status                    | No      | No      | Yes        | No          | No      | Yes     | No       | No      | Yes       | No       | No      | Yes     | No      | No      | Yes      | No       | No      | Yes     |
| Notes: *p<0.1, ** p<0.05, *** p | <0.01   |         |            |             |         |         |          |         |           |          |         |         |         |         |          |          |         |         |

#### Table B2. 9: Parametric results of the returns to compulsory schooling: formal sector

The sample is constructed from the 2009-2017 Mexican National Occupations and Employment Survey. Robust standard errors using EHW correction as recommended by Kolesár and Rothe (2018) in parentheses.

### Table B2. 10: Parametric results of the returns to compulsory schooling: informal sector

|   |                  |                  |            |                  |        | 4      | 12-month | windo  | w         |          |                  |        |                  |                  |                  |                 |                   |                    |
|---|------------------|------------------|------------|------------------|--------|--------|----------|--------|-----------|----------|------------------|--------|------------------|------------------|------------------|-----------------|-------------------|--------------------|
| Estimation method                       |                  |                  | First      | -stage           |        |        |          |        | Reduc     | ed-form  |                  |        |                  |                  | 2                | SLS             |                   |                    |
| Dependent variable                      | -                |                  | Years of s | chooling         | _      |        |          |        | Log of ho | urly wag | es               |        |                  | L.               | og of hou        | urly wage       | 25                |                    |
|   | (a)              | (b)              | (c)        | (d)              | (e)    | (f)    | (a)      | (b)    | (c)       | (d)      | (e)              | (f)    | (a)              | (b)              | (c)              | (d)             | (e)               | (f)                |
| Treatment                               | 0.131<br>(0.116) | 0.109            | 0.083      | 0.150            | 0.126  | 0.100  | 0.028    | 0.023  | 0.019     | 0.032*   | 0.027            | 0.022  |                  |                  |                  |                 |                   |                    |
| Years of schooling                      |                  |                  |            |                  |        |        |          |        |           |          |                  |        | 0.214<br>(0.204) | 0.216<br>(0.245) | 0.229<br>(0.334) | 0.210<br>(0.175 | 0.211<br>) (0.208 | 0.220<br>) (0.269) |
| Obs.                                    | 39,115           | 39,115           | 39,115     | 38,943           | 38,943 | 38,943 | 39,115   | 39,115 | 39,115    | 38,943   | 38,943           | 38,943 | 39,115           | 39,115           | 39,115           | 38,943          | 38,943            | 38,943             |
| Less than 18 years of schooling         | No               | No               | No         | Yes              | Yes    | Yes    | No       | No     | No        | Yes      | Yes              | Yes    | No               | No               | No               | Yes             | Yes               | Yes                |
| Trimmed hourly wages                    | No               | No               | No         | No               | No     | No     | No       | No     | No        | No       | No               | No     | No               | No               | No               | No              | No                | No                 |
| Survey year and birth region<br>dummies | No               | Yes              | Yes        | No               | Yes    | Yes    | No       | Yes    | Yes       | No       | Yes              | Yes    | No               | Yes              | Yes              | No              | Yes               | Yes                |
| Urban status                            | No               | No               | Yes        | No               | No     | Yes    | No       | No     | Yes       | No       | No               | Yes    | No               | No               | Yes              | No              | No                | Yes                |
| Estimation method                       |                  |                  | First      | -stage           |        |        |          |        | Reduc     | ed-form  |                  |        |                  |                  | 2                | SLS             |                   |                    |
| Dependent variable                      | -                |                  | Years of s | chooling         | -      |        |          |        | Log of ho | urly wag | es               |        |                  | L.               | og of hou        | urly wage       | 25                |                    |
|   | (a)              | (b)              | (c)        | (d)              | (e)    | (f)    | (a)      | (b)    | (c)       | (d)      | (e)              | (f)    | (a)              | (b)              | (c)              | (d)             | (e)               | (f)                |
| Treatment                               | 0.168            | 0.146<br>(0.115) | 0.126      | 0.177<br>(0.115) | 0.153  | 0.132  | 0.015    | 0.012  | 0.008     | 0.018    | 0.014<br>(0.016) | 0.010  |                  |                  |                  |                 |                   |                    |
| Years of schooling                      |                  |                  |            |                  |        |        |          |        |           |          |                  |        | 0.091<br>(0.101) | 0.079<br>(0.112) | 0.066<br>(0.126) | 0.100<br>(0.098 | 0.089<br>) (0.109 | 0.078<br>) (0.123) |
| Obs.                                    | 38,519           | 38,519           | 38,519     | 38,351           | 38,351 | 38,351 | 38,519   | 38,519 | 38,519    | 38,351   | 38,351           | 38,351 | 38,519           | 38,519           | 38,519           | 38,351          | 38,351            | 38,351             |
| Less than 18 years of schooling         | No               | No               | No         | Yes              | Yes    | Yes    | No       | No     | No        | Yes      | Yes              | Yes    | No               | No               | No               | Yes             | Yes               | Yes                |
| Trimmed hourly wages                    | Yes              | Yes              | Yes        | Yes              | Yes    | Yes    | Yes      | Yes    | Yes       | Yes      | Yes              | Yes    | Yes              | Yes              | Yes              | Yes             | Yes               | Yes                |
| Survey year and birth region<br>dummies | No               | Yes              | Yes        | No               | Yes    | Yes    | No       | Yes    | Yes       | No       | Yes              | Yes    | No               | Yes              | Yes              | No              | Yes               | Yes                |
| Urban status                            | No               | No               | Yes        | No               | No     | Yes    | No       | No     | Yes       | No       | No               | Yes    | No               | No               | Yes              | No              | No                | Yes                |

Notes: \*p<0.1, \*\* p<0.05, \*\*\* p<0.01 The sample is constructed from the 2009-2017 Mexican National Occupations and Employment Survey. Robust standard errors using EHW correction as recommended by Kolesár and Rothe (2018) in parentheses.

| Estimation method     |         |         | First    | t-stage   |         |         |          |          | Reduce     | d-form    |         |         |         |         | 25        | ils.      |         |         |
|-----------------------|---------|---------|----------|-----------|---------|---------|----------|----------|------------|-----------|---------|---------|---------|---------|-----------|-----------|---------|---------|
| Dependent variable    | -       |         | Years of | schooling | 5       | -       |          | 1        | .og of hou | rly wages |         | -       |         |         | .og of ho | urly wage | 25      | -       |
|                       | (a)     | (b)     | (c)      | (d)       | (e)     | (f)     | (a)      | (b)      | (c)        | (d)       | (e)     | (f)     | (a)     | (b)     | (c)       | (d)       | (e)     | (f)     |
| Treatment             | 0.585*  | 0.551*  | 0.543*   | 0.786**   | 0.753** | 0.693** | 0.174*** | 0.171*** | 0.170***   | 0.101*    | 0.095*  | 0.082   |         |         |           |           |         |         |
|                       | (0.303) | (0.295) | (0.292)  | (0.330)   | (0.328) | (0.328) | (0.058)  | (0.056)  | (0.055)    | (0.055)   | (0.053) | (0.053) |         |         |           |           |         |         |
| Years of schooling    |         |         |          |           |         |         |          |          |            |           |         |         | 0.298*  | 0.311*  | 0.314*    | 0.129*    | 0.126*  | 0.118   |
|                       |         |         |          |           |         |         |          |          |            |           |         |         | (0.156) | (0.166) | (0.172)   | (0.071)   | (0.072) | (0.078) |
| Obs.                  | 23,431  | 23,431  | 23,431   | 22,705    | 22,705  | 22,705  | 23,431   | 23,431   | 23,431     | 22,705    | 22,705  | 22,705  | 23,431  | 23,431  | 23,431    | 22,705    | 22,705  | 22,705  |
| Eff. Number of obs.   | 9,237   | 9,535   | 9,237    | 7,721     | 7,721   | 7,386   | 9,237    | 9,535    | 9,237      | 7,721     | 7,721   | 7,386   | 9,237   | 9,535   | 9,237     | 7,721     | 7,721   | 7,386   |
| Band width            | 44.64   | 45.57   | 44.38    | 40.62     | 40.78   | 38.88   | 44.64    | 45.57    | 44.38      | 40.62     | 40.78   | 38.88   | 44.64   | 45.57   | 44.38     | 40.62     | 40.78   | 38.88   |
| Less than 18 years of |         | N       | Marc     |           | Ne      | Ne      |          |          |            |           | N       |         |         |         |           | No        |         |         |
| schoo ling            | res     | res     | res      | NO        | NO      | NO      | res      | res      | res        | NO        | NO      | NO      | res     | res     | res       | NO        | NO      | NO      |
| Trimmed hourly        | No      | No      | No       | Ver       | Ver     | Voc     | No       | No       | No         | Ver       | Var     | Ver     | No      | No      | No        | Vac       | Ver     | Ver     |
| wages                 | NO      | NO      | NO       | 165       | 165     | 162     | NO       | NO       | NO         | 165       | 162     | 165     | NO      | NO      | NO        | 162       | 165     | 165     |
| Survey year and birth | No      | Ver     | Ver      | No        | Ver     | Ver     | Ne       | Ver      | Ver        | No        | Var     | Ver     | No      | Ver     | Ver       | No        | Ver     | Ver     |
| region dummies        | 140     | 165     | 165      | 140       | 165     | 165     | 110      | 165      | 165        | 140       | 165     | 165     | 140     | 165     | 165       | 140       | 165     | 165     |
| Urban status          | No      | No      | Yes      | No        | No      | Yes     | No       | No       | Yes        | No        | No      | Yes     | No      | No      | Yes       | No        | No      | Yes     |

Table B2. 11: Non-parametric results of the returns to compulsory schooling: self-employment

Notes: \* px0.1, \*\* p<0.05, \*\*\* p<0.01 The sample is constructed from the 2009-2017 Mexican National Occupations and Employment Survey. Following Calonico et al. (2018) and Calonico et al. (2014) for the optimal bandwidth. Robust standard errors using EHW correction as recommended by Kolesár and Rothe (2018) in parentheses.

| Table B2. 12: Parametric results of the returns to compulsory schooling: self-employment | ıt |
|--|----|
|--|----|

| Estimation method                       |                    |         | First              | -stage    |                    |                     |                    |                    | Reduce             | d-form           |                  |        |                   |                   | 25                | ils              |                  |                  |
|---|--------------------|---------|--------------------|-----------|--------------------|---------------------|--------------------|--------------------|--------------------|------------------|------------------|--------|-------------------|-------------------|-------------------|------------------|------------------|------------------|
| Dependent variable                      | -                  |         | Years of           | schooling |                    | -                   |                    | L.                 | og of hour         | ly wages         |                  | -      |                   | L                 | og of hoi         | urly wag         | es               | -                |
|   | (a)                | (b)     | (c)                | (d)       | (e)                | (f)                 | (a)                | (b)                | (c)                | (d)              | (e)              | (f)    | (a)               | (b)               | (c)               | (d)              | (e)              | (f)              |
| Treatment                               | 0.424**<br>(0.199) | 0.421** | 0.418**<br>(0.193) | 0.460**   | 0.459**<br>(0.199) | 0.436 **<br>(0.196) | 0.078**<br>(0.039) | 0.083**<br>(0.038) | 0.082**<br>(0.036) | 0.024<br>(0.035) | 0.025<br>(0.034) | 0.020  |                   |                   |                   |                  |                  |                  |
| Years of schooling                      |                    |         |                    |           |                    |                     |                    |                    |                    |                  |                  |        | 0.184*<br>(0.102) | 0.197*<br>(0.105) | 0.197*<br>(0.106) | 0.052<br>(0.070) | 0.055<br>(0.068) | 0.047<br>(0.071) |
| Obs.                                    | 15,802             | 15,802  | 15,802             | 14,981    | 14,981             | 14,981              | 15,802             | 15,802             | 15,802             | 14,981           | 14,981           | 14,981 | 15,802            | 15,802            | 15,802            | 14,981           | 14,981           | 14,981           |
| Less than 18 years of schooling         | No                 | No      | No                 | Yes       | Yes                | Yes                 | No                 | No                 | No                 | Yes              | Yes              | Yes    | No                | No                | No                | Yes              | Yes              | Yes              |
| Trimmed hourly wages                    | No                 | No      | No                 | Yes       | Yes                | Yes                 | No                 | No                 | No                 | Yes              | Yes              | Yes    | No                | No                | No                | Yes              | Yes              | Yes              |
| Survey year and birth<br>region dummies | No                 | Yes     | Yes                | No        | Yes                | Yes                 | No                 | Yes                | Yes                | No               | Yes              | Yes    | No                | Yes               | Yes               | No               | Yes              | Yes              |
| Urban status                            | No                 | No      | Yes                | No        | No                 | Yes                 | No                 | No                 | Yes                | No               | No               | Yes    | No                | No                | Yes               | No               | No               | Yes              |
|   |                    |         |                    |           |                    |                     |                    |                    |                    |                  |                  |        |                   |                   |                   |                  |                  |                  |
| Estimation method                       |                    |         | First              | -stage    |                    |                     |                    |                    | Reduce             | d-form           |                  |        |                   |                   | 25                | ils              |                  |                  |

| Estimation method                       |                    |                    | FILS     | t-stage            |                    |                    |                   |                    | Reduce             | a-torm    |        |                  |                   |                   | 2                 | 51.5             |                  |                  |
|---|--------------------|--------------------|----------|--------------------|--------------------|--------------------|-------------------|--------------------|--------------------|-----------|--------|------------------|-------------------|-------------------|-------------------|------------------|------------------|------------------|
| Dependent variable                      | _                  |                    | Years of | schooling          |                    | _                  |                   | L                  | og of hou          | rly wages |        | -                |                   | L                 | og of ho          | urly wag         | es               | -                |
|   | (g)                | (h)                | (i)      | (j)                | (k)                | (1)                | (g)               | (h)                | (i)                | (j)       | (k)    | (1)              | (g)               | (h)               | (i)               | (j)              | (k)              | (1)              |
| Treatment                               | 0.402**<br>(0.197) | 0.402**<br>(0.194) | 0.395**  | 0.512**<br>(0.205) | 0.507**<br>(0.082) | 0.485**<br>(0.198) | 0.076*<br>(0.039) | 0.081**<br>(0.038) | 0.079**<br>(0.037) | 0.030     | 0.032  | 0.027<br>(0.033) |                   |                   |                   |                  |                  |                  |
| Years of schooling                      |                    |                    |          |                    |                    |                    |                   |                    |                    |           |        |                  | 0.191*<br>(0.110) | 0.201*<br>(0.112) | 0.200*<br>(0.114) | 0.059<br>(0.062) | 0.063<br>(0.062) | 0.056<br>(0.063) |
| Obs.                                    | 15,614             | 15,614             | 15,614   | 15,160             | 15,160             | 15,160             | 15,614            | 15,614             | 15,614             | 15,160    | 15,160 | 15,160           | 15,614            | 15,614            | 15,614            | 15,160           | 15,160           | 15,160           |
| Less than 18 years of schooling         | Yes                | Yes                | Yes      | No                 | No                 | No                 | Yes               | Yes                | Yes                | No        | No     | No               | Yes               | Yes               | Yes               | No               | No               | No               |
| Trimmed hourly wages                    | No                 | No                 | No       | Yes                | Yes                | Yes                | No                | No                 | No                 | Yes       | Yes    | Yes              | No                | No                | No                | Yes              | Yes              | Yes              |
| Survey year and birth<br>region dummies | No                 | Yes                | Yes      | No                 | Yes                | Yes                | No                | Yes                | Yes                | No        | Yes    | Yes              | No                | Yes               | Yes               | No               | Yes              | Yes              |
| Urban status                            | No                 | No                 | Yes      | No                 | No                 | Yes                | No                | No                 | Yes                | No        | No     | Yes              | No                | No                | Yes               | No               | No               | Yes              |
| Notes: *p<0.1, ** p<0.0                 | 5, *** p<0         | .01                |          |                    |                    |                    |                   |                    |                    |           |        |                  |                   |                   |                   |                  |                  |                  |

The sample is constructed from the 2009-2017 Mexican National Occupations and Employment Survey. Robust standard errors using EHW correction as recommended by Kolesár and Rothe (2018) in parentheses.

# Table B2. 13: Parametric results of the returns to compulsory schooling: 39 -and 48-months window

|                                 |         |         |            |          |         |         | 39-mont | hs win  | dow       |           |         |         |         |         |           |            |         |         |
|---------------------------------|---------|---------|------------|----------|---------|---------|---------|---------|-----------|-----------|---------|---------|---------|---------|-----------|------------|---------|---------|
| Estimation method               |         |         | First      | stage    |         |         |         |         | Reduc     | ed-form   |         |         |         |         | 2         | SLS        |         |         |
| Dependent variable              | -       |         | Years of s | chooling | -       |         |         |         | Log of ho | urly wage | 25      |         |         |         | Log of ho | ourly wage | 25      |         |
|                                 | (a)     | (b)     | (c)        | (d)      | (e)     | (f)     | (a)     | (b)     | (c)       | (d)       | (e)     | (f)     | (a)     | (b)     | (c)       | (d)        | (e)     | (f)     |
| Treatment                       | 0.175** | 0.167** | 0.149*     | 0.161*   | 0.157*  | 0.139*  | 0.019   | 0.018   | 0.016     | 0.014     | 0.013   | 0.011   |         |         |           |            |         |         |
|                                 | (0.084) | (0.083) | (0.081)    | (0.086)  | (0.085) | (0.083) | (0.013) | (0.013) | (0.013)   | (0.012)   | (0.012) | (0.012) |         |         |           |            |         |         |
| Years of schooling              |         |         |            |          |         |         |         |         |           |           |         |         | 0.109   | 0.109   | 0.105     | 0.088      | 0.085   | 0.080   |
| -                               |         |         |            |          |         |         |         |         |           |           |         |         | (0.071) | (0.073) | (0.081)   | (0.068)    | (0.069) | (0.077) |
| Obs.                            | 80,994  | 80,994  | 80,994     | 81,086   | 81,086  | 81,086  | 80,994  | 80,994  | 80,994    | 81,086    | 81,086  | 81,086  | 80,994  | 80,994  | 80,994    | 81,086     | 81,086  | 81,086  |
| Less than 18 years of schooling | Yes     | Yes     | Yes        | No       | No      | No      | Yes     | Yes     | Yes       | No        | No      | No      | Yes     | Yes     | Yes       | No         | No      | No      |
| Trimmed hourly wages            | No      | No      | No         | Yes      | Yes     | Yes     | No      | No      | No        | Yes       | Yes     | Yes     | No      | No      | No        | Yes        | Yes     | Yes     |
| Survey year and birth region    | No      | Yes     | Yes        | No       | Yes     | Yes     | No      | Yes     | Yes       | No        | Yes     | Yes     | No      | Yes     | Yes       | No         | Yes     | Yes     |
| Urban status                    | No      | No      | Yes        | No       | No      | Yes     | No      | No      | Yes       | No        | No      | Yes     | No      | No      | Yes       | No         | No      | Yes     |
|                                 |         |         |            |          |         |         | 48-mont | hs win  | dow       |           |         |         |         |         |           |            |         |         |
| Estimation method               |         |         | First      | stage    |         |         |         |         | Reduc     | ed-form   |         |         |         |         | 2         | SLS        |         |         |
| Dependent variable              | -       |         | Years of s | chooling | -       |         |         |         | Log of ho | urly wage | 25      |         |         |         | Log of ho | ourly wage | es      |         |
|                                 | (a)     | (b)     | (c)        | (d)      | (e)     | (f)     | (a)     | (b)     | (c)       | (d)       | (e)     | (f)     | (a)     | (b)     | (c)       | (d)        | (e)     | (f)     |
| Treatment                       | 0147*   | 0.136*  | 0120       | 0136*    | 0125    | 0 112   | 0.021*  | 0 020*  | 0.018     | 0 011     | 0 010   | 0.009   |         |         |           |            |         |         |
|                                 | (0.076) | (0.075) | (0.073)    | (0.078)  | (0.077) | (0.075) | (0.012) | (0.011) | (0.011)   | (0.011)   | (0.011) | (0.011) |         |         |           |            |         |         |
| Years of schooling              |         |         |            |          |         |         |         |         |           |           |         |         | 0 144*  | 0149    | 0150      | 0.081      | 0.082   | 0 076   |
| -                               |         |         |            |          |         |         |         |         |           |           |         |         | (0.083) | (0.091) | (0.103)   | (0.072)    | (0.078) | (0.086) |
| Obs.                            | 98,928  | 98,928  | 98,928     | 99,009   | 99,009  | 99,009  | 98,928  | 98,928  | 98,928    | 99,009    | 99,009  | 99,009  | 98,928  | 98,928  | 98,928    | 99,009     | 99,009  | 99,009  |
| Less than 18 years of schooling | Yes     | Yes     | Yes        | No       | No      | No      | Yes     | Yes     | Yes       | No        | No      | No      | Yes     | Yes     | Yes       | No         | No      | No      |
| Trimmed hourly wages            | No      | No      | No         | Yes      | Yes     | Yes     | No      | No      | No        | Yes       | Yes     | Yes     | No      | No      | No        | Yes        | Yes     | Yes     |
| Survey year and birth region    | No      | Yes     | Yes        | No       | Yes     | Yes     | No      | Yes     | Yes       | No        | Yes     | Yes     | No      | Yes     | Yes       | No         | Yes     | Yes     |
| Urban status                    | No      | No      | Yes        | No       | No      | Yes     | No      | No      | Yes       | No        | No      | Yes     | No      | No      | Yes       | No         | No      | Yes     |

Notes: \*p<0.1, \*\* p<0.05, \*\*\* p<0.01 The sample is constructed from the 2009-2017 Mexican National Occupations and Employment Survey. Robust standard errors using EHW correction as recommended by Kolesár and Rothe (2018) in , parentheses.

| Table B2. 14: Parametric results of the | he employment sectoral  | choices: pooled | sample |
|---|-------------------------|-----------------|--------|
| Tuble D2: 11: Turumetrie results of th  | the employment sectoral | enoices. poolea | sumple |

|                      |         |          |           | Form    | nal emplo | oyment     | (=1)     |         |         |            |          |         |
|----------------------|---------|----------|-----------|---------|-----------|------------|----------|---------|---------|------------|----------|---------|
| Estimation method    | _       | First    | -stage    | _       |           | Reduc      | ed-form  | _       |         | 2          | SLS      | _       |
| Dependent variable   |         | Years of | schooling | ;       |           | Formal e   | mploymer | nt      |         | Formal e   | mployme  | nt      |
|                      | (a)     | (b)      | (c)       | (d)     | (a)       | (b)        | (c)      | (d)     | (a)     | (b)        | (c)      | (d)     |
| Treatment            | 0 139*  | 0 138*   | 0 131     | 0 109   | 0.013     | 0.013      | 0.013    | 0.010   |         |            |          |         |
| neutrient            | (0.082) | (0.082)  | (0.082)   | (0.080) | (0.010)   | (0.010)    | (0.010)  | (0.010) |         |            |          |         |
| Years of schooling   | (0.002) | (0.002)  | (0.002)   | (0.000) | (0.010)   | (0.010)    | (0.010)  | (0.010) | 0.093   | 0.095      | 0.096    | 0.096   |
| 0                    |         |          |           |         |           |            |          |         | (0.073) | (0.074)    | (0.078)  | (0.094) |
|                      |         |          |           |         |           |            |          |         |         | . ,        |          |         |
| Obs.                 | 88,227  | 88,227   | 88,227    | 88,227  | 88,227    | 88,227     | 88,227   | 88,227  | 88,227  | 88,227     | 88,227   | 88,227  |
| Survey year dummies  | No      | Yes      | Yes       | Yes     | No        | Yes        | Yes      | Yes     | No      | Yes        | Yes      | Yes     |
| Birth region dummies | No      | No       | Yes       | Yes     | No        | No         | Yes      | Yes     | No      | No         | Yes      | Yes     |
| Urban status         | No      | No       | No        | Yes     | No        | No         | No       | Yes     | No      | No         | No       | Yes     |
|                      |         |          |           | Inform  | nal empl  | ovmen      | t (=1)   |         |         |            |          |         |
| Estimation method    |         | First    | -stage    |         |           | Reduc      | ed-form  |         |         | 2          | SLS      |         |
| Dependent variable   | _       | Years of | schooling | ;       |           | Informal e | employme | nt      |         | Informal e | employme | ent     |
|                      | (a)     | (b)      | (c)       | (d)     | (a)       | (b)        | (c)      | (d)     | (a)     | (b)        | (c)      | (d)     |
|                      |         |          |           |         |           |            |          |         |         |            |          |         |
| Ireatment            | 0.139*  | 0.138*   | 0.131     | 0.109   | -0.013    | -0.013     | -0.013   | -0.010  |         |            |          |         |
| Very Feeling         | (0.082) | (0.082)  | (0.082)   | (0.080) | (0.010)   | (0.010)    | (0.010)  | (0.010) | 0.007   | 0.005      | 0.000    | 0.000   |
| rears of schooling   |         |          |           |         |           |            |          |         | -0.093  | -0.095     | -0.096   | -0.096  |
|                      |         |          |           |         |           |            |          |         | (0.075) | (0.074)    | (0.078)  | (0.094) |
| Obs.                 | 88,227  | 88,227   | 88,227    | 88,227  | 88,227    | 88,227     | 88,227   | 88,227  | 88,227  | 88,227     | 88,227   | 88,227  |
| Survey year dummies  | No      | Yes      | Yes       | Yes     | No        | Yes        | Yes      | Yes     | No      | Yes        | Yes      | Yes     |
| Birth region dummies | No      | No       | Yes       | Yes     | No        | No         | Yes      | Yes     | No      | No         | Yes      | Yes     |
| Urban status         | No      | No       | No        | Yes     | No        | No         | No       | Yes     | No      | No         | No       | Yes     |
|                      |         |          |           | Sel     | f-employ  | ment (:    | =1)      |         |         |            |          |         |
| Estimation method    |         | First    | -stage    |         |           | Reduc      | ed-form  |         |         | 2          | SLS      |         |
| Dependent variable   | _       | Years of | schooling | 5       |           | Self-em    | ployment |         |         | Self-em    | ployment |         |
|                      | (a)     | (b)      | (c)       | (d)     | (a)       | (b)        | (c)      | (d)     | (a)     | (b)        | (c)      | (d)     |
| <b>T</b>             | 0.120*  | 0.120*   | 0.121     | 0.100   | 0.001     | 0.001      | 0.001    | 0.001   |         |            |          |         |
| Ireatment            | 0.139*  | 0.138*   | 0.131     | 0.109   | -0.001    | -0.001     | -0.001   | -0.001  |         |            |          |         |
| Very Feeling         | (0.082) | (0.082)  | (0.082)   | (0.080) | (0.007)   | (0.007)    | (0.007)  | (0.007) | 0.000   | 0.010      | 0.010    | 0.000   |
| Years of schooling   |         |          |           |         |           |            |          |         | -0.009  | -0.010     | -0.010   | -0.009  |
|                      |         |          |           |         |           |            |          |         | (0.053) | (0.054)    | (0.057)  | (0.068) |
| Obs.                 | 88,227  | 88,227   | 88,227    | 88,227  | 88,227    | 88,227     | 88,227   | 88,227  | 88,227  | 88,227     | 88,227   | 88,227  |
| Survey year dummies  | No      | Yes      | Yes       | Yes     | No        | Yes        | Yes      | Yes     | No      | Yes        | Yes      | Yes     |
| Birth region dummies | No      | No       | Yes       | Yes     | No        | No         | Yes      | Yes     | No      | No         | Yes      | Yes     |
| Urban status         | No      | No       | No        | Yes     | No        | No         | No       | Yes     | No      | No         | No       | Yes     |
|                      |         |          |           |         |           |            |          |         |         |            |          |         |

Notes: \*p<0.1, \*\* p<0.05, \*\*\* p<0.01

The sample is constructed from the 2009-2017 Mexican National Occupations and Employment Survey. Robust standard errors using EHW correction as recommended by Kolesár and Rothe (2018) in parentheses.

# Table B2. 15: Parametric results of the employment sectoral choices: variations in schooling and earnings

|                       |               |         |          |          |           |         | Forma   | l emplo | yment     | (=1)     |          |         |         |         |           |          |         |         |
|-----------------------|---------------|---------|----------|----------|-----------|---------|---------|---------|-----------|----------|----------|---------|---------|---------|-----------|----------|---------|---------|
| Estimation method     | _             |         | First    | -stage   |           | _       |         |         | Reduc     | ed-form  |          | _       |         |         | 2         | SLS      |         | -       |
| Dependent variable    |               |         | Years of | schoolin | g         |         |         |         | Formal er | nployme  | nt       |         |         |         | Formal er | nployme  | nt      |         |
|                       | (a)           | (b)     | (c)      | (d)      | (e)       | (f)     | (a)     | (b)     | (c)       | (d)      | (e)      | (f)     | (a)     | (b)     | (C)       | (d)      | (e)     | (f)     |
| Treatment             | 0.153*        | 0.143*  | 0.120    | 0.136    | 0.128     | 0.107   | 0.013   | 0.012   | 0.010     | 0.011    | 0.010    | 0.008   |         |         |           |          |         |         |
| -                     | (0.081)       | (0.080) | (0.078)  | (0.083)  | ) (0.082) | (0.080) | (0.010) | (0.010) | (0.010)   | (0.010)  | (0.010)  | (0.010) |         |         |           |          |         |         |
| Years of schooling    |               |         |          |          |           |         |         |         |           |          |          |         | 0.084   | 0.087   | 0.085     | 0.078    | 0.080   | 0.077   |
|                       |               |         |          |          |           |         |         |         |           |          |          |         | (0.065) | (0.070) | (0.082)   | (0.073)  | (0.077) | (0.091) |
|                       |               |         |          |          |           |         |         |         |           |          |          |         |         |         |           |          |         |         |
| Obs.                  | 87,011        | 87,011  | 87,011   | 87,090   | 87,090    | 87,090  | 87,011  | 87,011  | 87,011    | 87,090   | 87,090   | 87,090  | 87,011  | 87,011  | 87,011    | 87,090   | 87,090  | 87,090  |
| Less than 18 years of | Yes           | Yes     | Yes      | No       | No        | No      | Yes     | Yes     | Yes       | No       | No       | No      | Yes     | Yes     | Yes       | No       | No      | No      |
| schooling             | Ne            | Ne      | Ne       | Vee      | Ver       | Vee     | Ne      | Ne      | Ne        | Ver      | Ver      | Ver     | Ne      | Ne      | Ne        | Vee      | Vee     | Vee     |
| Survey year and birth | NO            | NO      | NO       | Tes      | 16        | TES     | NO      | NO      | NO        | 16       | 16       | 16      | NO      | NO      | NO        | TES      | 16      | 16      |
| region dummies        | No            | Yes     | Yes      | No       | Yes       | Yes     | No      | Yes     | Yes       | No       | Yes      | Yes     | No      | Yes     | Yes       | No       | Yes     | Yes     |
| Urban status          | No            | No      | Yes      | No       | No        | Yes     | No      | No      | Yes       | No       | No       | Yes     | No      | No      | Yes       | No       | No      | Yes     |
|                       |               |         |          |          |           |         | Informa | lomn    | ovmon     | + (-1)   |          |         |         |         |           |          |         |         |
| Estimation method     |               |         | First    | state    |           |         | monia   | nempi   | Peduc     | - 1)     |          |         |         |         | 2         | 212      |         |         |
| Dependent variable    | -             |         | Years of | schoolin | ρ         | -       |         | h       | nformal e | mploym   | ant      | -       |         | Ir      | nformal e | mployme  | ant     | -       |
| bependent variable    | (a)           | (b)     | (c)      | (d)      | (e)       | (f)     | (a)     | (b)     | (c)       | (d)      | (e)      | (f)     | (a)     | (b)     | (c)       | (d)      | (e)     | (f)     |
| Treatment             | 0.153*        | 0.143*  | 0.120    | 0.136    | 0.128     | 0.107   | -0.013  | -0.012  | -0.010    | -0.011   | -0.010   | -0.008  | 1-1     | 1-1     | 1-1       | 1-1      | (-/     | 1.1     |
|                       | (0.081)       | (0.080) | (0.078)  | (0.083)  | ) (0.082) | (0.080) | (0.010) | (0.010) | (0.010)   | (0.010)  | (0.010)  | (0.010) |         |         |           |          |         |         |
| Years of schooling    | (/            |         | (        |          | , ,,      | (/      | (,      | (,      | (,        | (,       | (/       | (/      | -0.084  | -0.087  | -0.085    | -0.078   | -0.080  | -0 077  |
| Ŭ                     |               |         |          |          |           |         |         |         |           |          |          |         | (0.065) | (0.070) | (0.082)   | (0.073)  | (0.077) | (0.091) |
|                       |               |         |          |          |           |         |         |         |           |          |          |         |         |         |           |          |         |         |
| Obs.                  | 87,011        | 87,011  | 87,011   | 87,090   | 87,090    | 87,090  | 87,011  | 87,011  | 87,011    | 87,090   | 87,090   | 87,090  | 87,011  | 87,011  | 87,011    | 87,090   | 87,090  | 87,090  |
| Less than 18 years of | Ves           | Ves     | Ves      | No       | No        | No      | Ves     | Ves     | Ves       | No       | No       | No      | Ves     | Ves     | Ves       | No       | No      | No      |
| schooling             |               |         | .0       | 140      |           | 140     |         | 100     |           |          |          |         |         |         |           | 140      |         |         |
| Trimmed hourly wages  | No            | No      | No       | Yes      | Yes       | Yes     | No      | No      | No        | Yes      | Yes      | Yes     | No      | No      | No        | Yes      | Yes     | Yes     |
| Survey year and birth | No            | Yes     | Yes      | No       | Yes       | Yes     | No      | Yes     | Yes       | No       | Yes      | Yes     | No      | Yes     | Yes       | No       | Yes     | Yes     |
| Index status          | No            | No      | Ver      | No       | No        | Vec     | No      | No      | Vec       | No       | No       | Ver     | No      | No      | Vec       | No       | No      | Ver     |
| orbaristatus          | NO            | NO      | 10       | NO       | NU        | 103     | NO      | 140     | 10        | NO       | NO       | 10      | NO      | NO      | 10        | NO       | NO      | 18      |
|                       |               |         |          |          |           |         | Self-e  | employ  | ment (    | =1)      |          |         |         |         |           |          |         |         |
| Estimation method     | -             |         | First    | -stage   |           | -       |         |         | Reduc     | ed-form  |          | -       |         |         | 2         | SLS      |         | -       |
| Dependent variable    | (-)           | (1-)    | Years of | schoolin | g<br>(=)  | (8)     | (-)     | (1-)    | Self-em   | ployment | :<br>(~) | (0)     | (=)     | (1-)    | Self-em   | ployment | (-)     | (6)     |
| Treatment             | (d)<br>0.152* | (0)     | 0 120    | 0.126    | 0 129     | 0.107   | (a)     | -0.007  | -0.007    | -0.00/   | -0.00/   | -0.003  | (d)     | (0)     | (C)       | (u)      | (e)     | (1)     |
| reachene              | (0.081)       | (0.080) | (0.078)  | 0.150    | 1 (0.027) | (0.090) | (0.002) | (0.002) | (0.002)   | (0.007)  | (0.007)  | (0.003) |         |         |           |          |         |         |
| Vears of schooling    | (0.001)       | (0.080) | (0.078)  | (0.085)  | (0.082)   | (0.080) | (0.007) | (0.007) | (0.007)   | (0.007)  | (0.007)  | (0.007) | -0.013  | -0.015  | -0.015    | -0.076   | -0.029  | -0.037  |
|                       |               |         |          |          |           |         |         |         |           |          |          |         | (0.049) | (0.053) | (0.062)   | (0.056)  | (0.059) | (0.072) |
|                       |               |         |          |          |           |         |         |         |           |          |          |         | (0.015) | (0.033) | (0.002)   | (0.050)  | (0.055) | (0.072) |
| Obs.                  | 87,011        | 87,011  | 87,011   | 87,090   | 87,090    | 87,090  | 87,011  | 87,011  | 87,011    | 87,090   | 87,090   | 87,090  | 87,011  | 87,011  | 87,011    | 87,090   | 87,090  | 87,090  |
| Less than 18 years of | Yes           | Yes     | Yes      | No       | No        | No      | Yes     | Yes     | Yes       | No       | No       | No      | Yes     | Yes     | Yes       | No       | No      | No      |
| schooling             | Ne            | Nie     | Ne       | Marc     | Ver       | Vee     | Ne      | Nie     | Nie       | Ver      | Mar      | Ver     | Ne      | Ne      | Nie       | Max      | Ver     | ¥       |
| Supravyer and birth   | NO            | NO      | NO       | res      | 16        | res     | NO      | NO      | NO        | 16       | 16       | 165     | NO      | NO      | NO        | res      | 16      | 16      |
| region dummies        | No            | Yes     | Yes      | No       | Yes       | Yes     | No      | Yes     | Yes       | No       | Yes      | Yes     | No      | Yes     | Yes       | No       | Yes     | Yes     |
| Urban status          | No            | No      | Yes      | No       | No        | Yes     | No      | No      | Yes       | No       | No       | Yes     | No      | No      | Yes       | No       | No      | Yes     |

Notes: \*p<0.1, \*\* p<0.05, \*\*\* p<0.01 The sample is constructed from the 2009-2017 Mexican National Occupations and Employment Survey. Robust standard errors using EHW correction as recommended by Kolesár and Rothe (2018) in parentheses.

| Table | B2. | 16: | Parametric | results | of the | emplor | vment | sectoral | choices: | 39-month | s window |
|-------|-----|-----|------------|---------|--------|--------|-------|----------|----------|----------|----------|
|       |     |     |            |         |        |        |       |          |          |          |          |

|                                 |               |          |            |            |           | For       | mal emp    | loymer   | nt (=1)   |            |          |           |            |         |           |           |           |          |
|---------------------------------|---------------|----------|------------|------------|-----------|-----------|------------|----------|-----------|------------|----------|-----------|------------|---------|-----------|-----------|-----------|----------|
| Estimation method               | _             |          | First      | -stage     |           | _         |            | _        | Reduce    | ed-form    |          | _         |            |         | 2         | SLS       |           | _        |
| Dependent variable              |               |          | Years of   | schooling  |           |           |            |          | Formal en | nploymen   | it       |           |            |         | Formaler  | nploymer  | nt        |          |
|                                 | (a)           | (b)      | (c)        | (d)        | (e)       | (f)       | (a)        | (b)      | (c)       | (d)        | (e)      | (f)       | (a)        | (b)     | (c)       | (d)       | (e)       | (f)      |
| Treatment                       | 0 175**       | 0 167**  | 0 149*     | 0.161*     | 0 157*    | 0.139*    | 0.013      | 0.013    | 0.011     | 0.011      | 0.011    | 0.009     |            |         |           |           |           |          |
| - eschenc                       | (0.084)       | (0.083)  | (0.081)    | (0.086)    | (0.085)   | (0.083)   | (0.011)    | (0.010)  | (0.010)   | (0.011)    | (0.010)  | (0.010)   |            |         |           |           |           |          |
| Years of schooling              | (0.001)       | (0.000)  | (0.001)    | (0.000)    | (0.000)   | (0.000)   | (0.011)    | (0.010)  | (0.010)   | (0.011)    | (0.010)  | (0.010)   | 0 077      | 0.077   | 0.075     | 0 0 7 0   | 0.068     | 0.065    |
| Tears a schooling               |               |          |            |            |           |           |            |          |           |            |          |           | (0.058)    | (0.060) | (0.067)   | (0.062)   | (0.063)   | (0.071)  |
|                                 |               |          |            |            |           |           |            |          |           |            |          |           | (/         | (/      | (         | (/        | (/        | (        |
| Obs.                            | 80,994        | 80,994   | 80,994     | 81,086     | 81,086    | 81,086    | 80,994     | 80,994   | 80,994    | 81,086     | 81,086   | 81,086    | 80,994     | 80,994  | 80,994    | 81,086    | 81,086    | 81,086   |
| Less than 18 years of schooling | Yes           | Yes      | Yes        | No         | No        | No        | Yes        | Yes      | Yes       | No         | No       | No        | Yes        | Yes     | Yes       | No        | No        | No       |
| Trimmed hourly wages            | No            | No       | No         | Yes        | Yes       | Yes       | No         | No       | No        | Yes        | Yes      | Yes       | No         | No      | No        | Yes       | Yes       | Yes      |
| Survey year and birth region    | No            | Yes      | Yes        | No         | Yes       | Yes       | No         | Yes      | Yes       | No         | Yes      | Yes       | No         | Yes     | Yes       | No        | Yes       | Yes      |
| dummies                         |               |          |            |            |           |           |            |          |           |            |          |           |            |         |           |           |           |          |
| Urban status                    | No            | No       | Yes        | No         | No        | Yes       | No         | No       | Yes       | No         | No       | Yes       | No         | No      | Yes       | No        | No        | Yes      |
|                                 |               |          |            |            |           | Info      | rmal emi   | olovme   | nt (=1)   |            |          |           |            |         |           |           |           |          |
| Estimation method               |               |          | First      | -stage     |           |           |            |          | Reduce    | ed-form    |          |           |            |         | 2         | SLS       |           |          |
| Dependent variable              | _             |          | Years of   | schooling  |           | -         |            | 1        | nformal e | nployme    | nt       | -         |            | h       | nformale  | mployme   | nt        | -        |
|                                 | (a)           | (b)      | (c)        | (d)        | (e)       | (f)       | (a)        | (b)      | (c)       | (d)        | (e)      | (f)       | (a)        | (b)     | (c)       | (d)       | (e)       | (f)      |
| Trastmost                       | 0 175+4       | 0 167**  | 0 149*     | 0.161*     | 0 157*    | 0.130*    | -0.013     | -0.013   | -0.011    | -0.011     | -0.011   | -0.009    |            |         |           |           |           |          |
| reacheric                       | (0.084)       | (0.083)  | (0.081)    | (0.086)    | (0.085)   | (0.083)   | (0.010)    | (0.010)  | (0.010)   | (0.011)    | (0.011)  | (0.010)   |            |         |           |           |           |          |
| Very of schooling               | (0.004)       | (0.000)  | (0.001)    | (0.000)    | (0.005)   | (0.000)   | (0.010)    | (0.010)  | (0.010)   | (0.011)    | (0.011)  | (0.010)   | -0.077     | -0.077  | -0.075    | -0.070    | -0.068    | -0.065   |
| Tears of schooling              |               |          |            |            |           |           |            |          |           |            |          |           | (0.058)    | (0.060) | (0.067)   | (0.062)   | (0.063)   | (0.071)  |
|                                 |               |          |            |            |           |           |            |          |           |            |          |           | (0.050)    | (0.000) | (0.0077   | (0.002)   | (0.000)   | (0.07 1) |
| Obs.                            | 80,994        | 80,994   | 80,994     | 81,086     | 81,086    | 81,086    | 80,994     | 80,994   | 80,994    | 81,086     | 81,086   | 81,086    | 80,994     | 80,994  | 80,994    | 81,086    | 81,086    | 81,086   |
| Less than 18 years of schooling | Yes           | Yes      | Yes        | No         | No        | No        | Yes        | Yes      | Yes       | No         | No       | No        | Yes        | Yes     | Yes       | No        | No        | No       |
| Trimmed hourly wages            | No            | No       | No         | Yes        | Yes       | Yes       | No         | No       | No        | Yes        | Yes      | Yes       | No         | No      | No        | Yes       | Yes       | Yes      |
| Survey year and birth region    | No            | Yes      | Yes        | No         | Yes       | Yes       | No         | Yes      | Yes       | No         | Yes      | Yes       | No         | Yes     | Yes       | No        | Yes       | Yes      |
| dummies                         |               |          |            |            |           |           |            |          |           |            |          |           |            |         |           |           |           |          |
| Urban status                    | No            | No       | Yes        | No         | No        | Yes       | No         | No       | Yes       | No         | No       | Yes       | No         | No      | Yes       | No        | No        | Yes      |
|                                 |               |          |            |            |           | S         | elf-emplo  | wment    | (=1)      |            |          |           |            |         |           |           |           |          |
| Estimation method               |               |          | First      | -stage     |           |           |            |          | Reduce    | ed-form    |          |           |            |         | 2         | SLS       |           |          |
| Dependent variable              |               |          | Years of   | schooling  |           | -         |            |          | Self-emp  | loyment    |          | -         |            |         | Self-em   | ployment  |           | -        |
|                                 | (a)           | (b)      | (c)        | (d)        | (e)       | (f)       | (a)        | (b)      | (c)       | (d)        | (e)      | (f)       | (a)        | (b)     | (c)       | (d)       | (e)       | (f)      |
| Treatment                       | 0 175**       | 0 167**  | 0 149*     | 0.161*     | 0.157*    | 0.139*    | -0.003     | -0.003   | -0.003    | -0.005     | -0.005   | -0.004    |            |         |           |           |           |          |
|                                 | (0.084)       | (0.083)  | (0.081)    | (0.086)    | (0.085)   | (0.083)   | (0.008)    | (0.008)  | (0.008)   | (0.008)    | (0.008)  | (0.008)   |            |         |           |           |           |          |
| Very of schooling               | (0.001)       | (0.000)  | (0.001)    | (0.000)    | (0.005)   | (0.000)   | (0.000)    | (0.000)  | (0.000)   | (0.000)    | (0.000)  | (0.000)   | -0.020     | -0.020  | -0.021    | -0.029    | -0.029    | -0.031   |
| Tears & Schooling               |               |          |            |            |           |           |            |          |           |            |          |           | (0.045)    | (0.047) | (0.053)   | (0.049)   | (0.050)   | (0.057)  |
|                                 |               |          |            |            |           |           |            |          |           |            |          |           | ()         | ( /     | (/        | (         | (,        | ()       |
| Obs.                            | 80,994        | 80,994   | 80,994     | 81,086     | 81,086    | 81,086    | 80,994     | 80,994   | 80,994    | 81,086     | 81,086   | 81,086    | 80,994     | 80,994  | 80,994    | 81,086    | 81,086    | 81,086   |
| Less than 18 years of schooling | Yes           | Yes      | Yes        | No         | No        | No        | Yes        | Yes      | Yes       | No         | No       | No        | Yes        | Yes     | Yes       | No        | No        | No       |
| Trimmed hourly wages            | No            | No       | No         | Yes        | Yes       | Yes       | No         | No       | No        | Yes        | Yes      | Yes       | No         | No      | No        | Yes       | Yes       | Yes      |
| Survey year and birth region    | No            | Yes      | Yes        | No         | Yes       | Yes       | No         | Yes      | Yes       | No         | Yes      | Yes       | No         | Yes     | Yes       | No        | Yes       | Yes      |
| dummies                         |               |          |            |            |           |           |            |          |           |            |          |           |            |         |           |           |           |          |
| Urban status                    | No<br>- c0.01 | No       | Yes        | No         | No        | Yes       | No         | No       | Yes       | No         | No       | Yes       | No         | No      | Yes       | No        | No        | Yes      |
| The sample is constructed from  | p<0.01        | 2017 M-  | vican Nati | ional Occu | nations - | nd Employ | mant Succe | Robert   | standars  | l errors : | sing ELM |           |            | manded  | by Koleri | ir and Po | the (201) | 9) in    |
| nesample is constructed from    | nine 2009     | -2017 Me | Arcan Mati | onar occu  | pauons a  | no employ | mencourve  | y. Noous | scanuaro  | renorsu    | ang criv | correctio | n as recom | menued  | oy kolesa |           | cne (201  | 21.00    |
| porent/leses.                   |               |          |            |            |           |           |            |          |           |            |          |           |            |         |           |           |           |          |
### Table B2. 17: Placebo test. Birth cohort born 48 months before and after the 1993 cut-off, nonparametric results of the returns to compulsory schooling

|   |         |          |           | Birth   | cohort Sept | tember 19 | 977       |         |         |           |           |          |
|---|---------|----------|-----------|---------|-------------|-----------|-----------|---------|---------|-----------|-----------|----------|
| Estimation method                       |         | First    | -stage    |         |             | Reduce    | ed-form   |         |         | 2         | SLS       |          |
| Dependent variable                      | -       | Years of | schooling | -       |             | Log of ho | urly wage | s       |         | Log of ho | urly wage | s        |
|   | (a)     | (b)      | (c)       | (d)     | (a)         | (b)       | (c)       | (d)     | (a)     | (b)       | (c)       | (d)      |
| Treatment                               | -0.281  | -0.255   | -0.347    | -0.334  | -0.050      | -0.049    | -0.077    | -0.087  |         |           |           |          |
|   | (0.347) | (0.347)  | (0.345)   | (0.345) | (0.055)     | (0.055)   | (0.050)   | (0.050) |         |           |           |          |
| Years of schooling                      |         |          |           |         |             |           |           |         | 0.177   | 0.192     | 0.222     | 0.258    |
| -                                       |         |          |           |         |             |           |           |         | (0.204) | (0.220)   | (0.182)   | (0.200)  |
|   | 440.054 | 446.054  | 447.045   | 445.025 | 440.004     | 445.054   | 447.045   | 445.005 | 440.054 | 446.054   | 447.045   | 445.005  |
| Obs.                                    | 148,964 | 146,954  | 147,015   | 145,035 | 148,964     | 146,954   | 147,015   | 145,035 | 148,964 | 146,954   | 147,015   | 145,035  |
| Eff. Number of obs.                     | 10,837  | 10,644   | 10,680    | 10,490  | 10,837      | 10,644    | 10,680    | 10,490  | 10,837  | 10,644    | 10,680    | 10,490   |
| Bandwidth                               | 11.42   | 10.27    | 11.52     | 10.87   | 11.42       | 10.27     | 11.52     | 10.87   | 11.42   | 10.27     | 11.52     | 10.87    |
| Less than 18 years of<br>schooling      | No      | Yes      | No        | Yes     | No          | Yes       | No        | Yes     | No      | Yes       | No        | Yes      |
| Trimmed hourly wages                    | No      | No       | Yes       | Yes     | No          | No        | Yes       | Yes     | No      | No        | Yes       | Yes      |
| Survey year and birth region dummies    | No      | Yes      | Yes       | Yes     | No          | Yes       | Yes       | Yes     | No      | Yes       | Yes       | Yes      |
|   |         |          |           | Birth   | cohort Sept | tember 19 | 985       |         |         |           |           |          |
| Estimation method                       | -       | First    | stage     | -       |             | Reduce    | ed-form   | -       |         | 2         | SLS       | -        |
| Dependent variable                      |         | Years of | schooling |         |             | Log of ho | urly wage | s       |         | Log of ho | urly wage | s        |
|   | (a)     | (b)      | (C)       | (d)     | (a)         | (b)       | (c)       | (d)     | (a)     | (b)       | (c)       | (d)      |
| Treatment                               | 0.152   | 0.167    | 0.151     | 0.170   | -0.046      | -0.049    | -0.038    | -0.041  |         |           |           |          |
|   | (0.296) | (0.293)  | (0.297)   | (0.293) | (0.046)     | (0.046)   | (0.043)   | (0.043) |         |           |           |          |
| Years of schooling                      |         |          |           |         |             |           |           |         | -0.302  | -0.296    | -0.253    | -0.240   |
| -                                       |         |          |           |         |             |           |           |         | (0.765) | (0.668)   | (0.659)   | (0.576)  |
|   |         |          |           |         |             |           |           |         |         |           | · · ·     | <u> </u> |
| Obs.                                    | 148,964 | 146,954  | 147,015   | 145,035 | 148,964     | 146,954   | 147,015   | 145,035 | 148,964 | 146,954   | 147,015   | 145,035  |
| Eff. Number of obs.                     | 11,038  | 10,940   | 10,895    | 10,799  | 11,038      | 10,940    | 10,895    | 10,799  | 11,038  | 10,940    | 10,895    | 10,799   |
| Bandwidth                               | 24.35   | 21.912   | 21.63     | 24.88   | 24.35       | 21.912    | 21.63     | 24.88   | 24.35   | 21.912    | 21.63     | 24.88    |
| Less than 18 years of<br>schooling      | No      | Yes      | No        | Yes     | No          | Yes       | No        | Yes     | No      | Yes       | No        | Yes      |
| Trimmed hourly wages                    | No      | No       | Yes       | Yes     | No          | No        | Yes       | Yes     | No      | No        | Yes       | Yes      |
| Survey year and birth<br>region dummies | No      | Yes      | Yes       | Yes     | No          | Yes       | Yes       | Yes     | No      | Yes       | Yes       | Yes      |

Notes: \*p<0.1, \*\* p<0.05, \*\*\* p<0.01

The sample is constructed from the 2009-2017 Mexican National Occupations and Employment Survey. Following Calonico et al. (2018) and Calonico et al. (2014) for the optimal bandwidth. Robust standard errors using EHW correction as recommended by Kolesár and Rothe (2018) in parentheses. For birth cohort in 1985 the specifications are not considering any covariates.

# Table B2. 18: Placebo test. Birth cohort born 48 months before and after the 1993 cut-off,parametric results of the returns to compulsory schooling

|                                      |         |          |           | Birth coh | ort Septem | ber 1977  |           |         |         |           |           |         |
|--------------------------------------|---------|----------|-----------|-----------|------------|-----------|-----------|---------|---------|-----------|-----------|---------|
| Estimation method                    | _       | First    | -stage    | _         |            | Reduce    | ed-form   | _       |         | 2         | SLS       | _       |
| Dependent variable                   |         | Years of | schooling |           |            | Log of ho | urly wage | s       |         | Log of ho | urly wage | 5       |
|                                      | (a)     | (b)      | (c)       | (d)       | (a)        | (b)       | (c)       | (d)     | (a)     | (b)       | (c)       | (d)     |
| Treatment                            | -0.045  | -0.034   | -0.088    | -0.071    | 0.007      | 0.014     | 0.004     | 0.008   |         |           |           |         |
|                                      | (0.089) | (0.086)  | (0.089)   | (0.086)   | (0.014)    | (0.014)   | (0.013)   | (0.013) |         |           |           |         |
| Years of schooling                   |         |          |           |           |            |           |           |         | -0.163  | -0.419    | -0.040    | -0.118  |
|                                      |         |          |           |           |            |           |           |         | (0.539) | (1.293)   | (0.170)   | (0.276) |
| Obs.                                 | 148,964 | 146,954  | 147,015   | 145,035   | 148,964    | 146,954   | 147,015   | 145,035 | 148,964 | 146,954   | 147,015   | 145,035 |
| Less than 18 years of schooling      | No      | Yes      | No        | Yes       | No         | Yes       | No        | Yes     | No      | Yes       | No        | Yes     |
| Trimmed hourly wages                 | No      | No       | Yes       | Yes       | No         | No        | Yes       | Yes     | No      | No        | Yes       | Yes     |
| Survey year and birth region dummies | No      | Yes      | No        | Yes       | No         | Yes       | No        | Yes     | No      | Yes       | No        | Yes     |
|                                      |         |          |           | Birth coh | ort Septem | ber 1985  |           |         |         |           |           |         |
| Estimation method                    | _       | First    | -stage    | _         |            | Reduce    | ed-form   | _       |         | 2         | SLS       | _       |
| Dependent variable                   | -       | Years of | schooling | -         |            | Log of ho | urly wage | s       |         | Log of ho | urly wage | 5       |
|                                      | (a)     | (b)      | (c)       | (d)       | (a)        | (b)       | (c)       | (d)     | (a)     | (b)       | (c)       | (d)     |
| Treatment                            | 0.074   | 0.044    | 0.085     | 0.053     | -0.001     | -0.004    | 0.003     | -0.000  |         |           |           |         |
|                                      | (0.072) | (0.071)  | (0.073)   | (0.072)   | (0.011)    | (0.011)   | (0.010)   | (0.010) |         |           |           |         |
| Years of schooling                   |         |          |           |           |            |           |           |         | -0.014  | -0.094    | 0.034     | -0.004  |
|                                      |         |          |           |           |            |           |           |         | (0.155) | (0.330)   | (0.111)   | (0.190) |
| Obs.                                 | 148,964 | 146,954  | 147,015   | 145,035   | 148,964    | 146,954   | 147,015   | 145,035 | 148,964 | 146,954   | 147,015   | 145,035 |
| Less than 18 years of schooling      | No      | Yes      | No        | Yes       | No         | Yes       | No        | Yes     | No      | Yes       | No        | Yes     |
| Trimmed hourly wages                 | No      | No       | Yes       | Yes       | No         | No        | Yes       | Yes     | No      | No        | Yes       | Yes     |
| Survey year and birth region dummies | No      | Yes      | No        | Yes       | No         | Yes       | No        | Yes     | No      | Yes       | No        | Yes     |

*Notes:* \*p<0.1, \*\* p<0.05, \*\*\* p<0.01

The sample is constructed from the 2009-2017 Mexican National Occupations and Employment Survey. Robust standard errors using EHW correction as recommended by Kolesár and Rothe (2018) in parentheses.

# Table B2. 19: Placebo test. Birth cohort born 48 months before the 1993 cut-off, non-parametric analysis of the employment sectoral choices

|                       |         |          | Birth co  | hort Septen | nber 1977 i | for forma | il employi | ment (=1) |         |           |          |         |
|-----------------------|---------|----------|-----------|-------------|-------------|-----------|------------|-----------|---------|-----------|----------|---------|
| Estimation method     |         | First    | -stage    |             |             | Reduc     | ed-form    |           |         | 25        | ils      |         |
| Dependent variable    | -       | Years of | schooling | -           |             | Formal er | nployment  | Ē         |         | Formal em | ployment | -       |
|                       | (a)     | (b)      | (c)       | (d)         | (a)         | (b)       | (c)        | (d)       | (a)     | (b)       | (c)      | (d)     |
| Treatment             | -1.077  | -1.424   | -1.112    | 0.122       | -0.412      | 490       | -0.456     | 0.012     |         |           |          |         |
|                       | (2.887) | (3.609)  | (3.687)   | (0.124)     | (0.348)     | (0.449)   | (0.445)    | (0.016)   |         |           |          |         |
| Years of schooling    |         |          |           |             |             |           |            |           | 5.752   | 7.342     | 4.491    | 0.096   |
|                       |         |          |           |             |             |           |            |           | (7.820) | (10.259)  | (6.711)  | (0.123) |
| Obs.                  | 148,964 | 146,954  | 147,015   | 145,035     | 148,964     | 146,954   | 147,015    | 145,035   | 148,964 | 146,954   | 147,015  | 145,035 |
| Eff. Number of obs.   | 56,113  | 55,161   | 55,366    | 54,426      | 56,113      | 55,161    | 55,366     | 54,426    | 56,113  | 55,161    | 55,366   | 54,426  |
| Bandwidth             | 10.31   | 9.797    | 9.85      | 9.62        | 10.31       | 9.797     | 9.85       | 9.62      | 10.31   | 9.797     | 9.85     | 9.62    |
| Less than 18 years of | No      | Vec      | No        | Vec         | No          | Vac       | No         | Vec       | No      | Vac       | No       | Vec     |
| schooling             | INO     | res      | INO       | res         | NO          | res       | NO         | res       | NO      | res       | NO       | res     |
| Trimmed hourly wages  | No      | No       | Yes       | Yes         | No          | No        | Yes        | Yes       | No      | No        | Yes      | Yes     |
| Survey year and birth | No      | Vec      | Vac       | Voc         | No          | Voc       | Voc        | Voc       | No      | Voc       | Vec      | Voc     |
| region dummies        | INO     | res      | res       | res         | NO          | res       | res        | res       | NO      | res       | res      | res     |
|                       |         |          |           |             |             |           |            |           |         |           |          |         |

|   |         |                   | Birth coh           | ort Septen | ber 1977 f | or inform           | al employ           | /ment (=1) |          |                 |                  |         |
|---|---------|-------------------|---------------------|------------|------------|---------------------|---------------------|------------|----------|-----------------|------------------|---------|
| Estimation method<br>Dependent variable | -       | First<br>Years of | -stage<br>schooling | -          |            | Reduc<br>Informal e | ed-form<br>mplovmen | īt         |          | 2<br>Informal e | SLS<br>mplovment | -       |
|   | (a)     | (b)               | (c)                 | (d)        | (a)        | (b)                 | (c)                 | (d)        | (a)      | (b)             | (c)              | (d)     |
| Treatment                               | -0.159  | -0.231            | -0.150              | -0.352     | 0.113      | 0.139               | 0.122               | -0.015     |          |                 |                  |         |
|   | (1.026) | (1.282)           | (1.309)             | (1.326)    | (0.124)    | (0.160)             | (0.158)             | (0.017)    |          |                 |                  |         |
| Years of schooling                      |         |                   |                     |            |            |                     |                     |            | -4.318   | -3.214          | -1.668           | -2.265  |
| 5                                       |         |                   |                     |            |            |                     |                     |            | (19.230) | (11.194         | ) (7.236)        | (5.806) |
| Obs.                                    | 148,964 | 146,954           | 147,015             | 145,035    | 148,964    | 146,954             | 147,015             | 145,035    | 148,964  | 146,954         | 147,015          | 145,035 |
| Eff. Number of obs,                     | 40,306  | 39,625            | 39,735              | 39,062     | 40,306     | 39,625              | 39,735              | 39,062     | 40,306   | 39,625          | 39,735           | 39,062  |
| Bandwidth                               | 10.31   | 9.797             | 9.85                | 9.62       | 10.31      | 9.797               | 9.85                | 9.62       | 10.31    | 9.797           | 9.85             | 9.62    |
| Less than 18 years of schooling         | No      | Yes               | No                  | Yes        | No         | Yes                 | No                  | Yes        | No       | Yes             | No               | Yes     |
| Trimmed hourly wages                    | No      | No                | Yes                 | Yes        | No         | No                  | Yes                 | Yes        | No       | No              | Yes              | Yes     |
| Survey year and birth region dummies    | No      | Yes               | Yes                 | Yes        | No         | Yes                 | Yes                 | Yes        | No       | Yes             | Yes              | Yes     |

|                                 |         |          | Birth c   | ohort Sept | ember 1977 | 7 for self- | employm  | ent (=1) |         |          |          |         |
|---------------------------------|---------|----------|-----------|------------|------------|-------------|----------|----------|---------|----------|----------|---------|
| Estimation method               |         | First    | -stage    |            |            | Reduc       | ed-form  |          |         | 2        | SLS      |         |
| Dependent variable              | -       | Years of | schooling | -          |            | Self-em     | ployment | -        |         | Self-emp | oloyment | -       |
|                                 | (a)     | (b)      | (C)       | (d)        | (a)        | (b)         | (c)      | (d)      | (a)     | (b)      | (c)      | (d)     |
| Treatment                       | -0.620  | -0.616   | -0.392    | -0.557     | 0.047      | 0.037       | 0.023    | 0.017    |         |          |          |         |
|                                 | (1.265) | (1.216)  | (1.645)   | (1.702)    | (0.123)    | (0.123)     | (0.160)  | (0.170)  |         |          |          |         |
| Years of schooling              |         |          |           |            |            |             |          |          | -0.420  | -0.330   | -0.079   | 0.042   |
|                                 |         |          |           |            |            |             |          |          | (1.295) | (1.136)  | (1.384)  | (1.394) |
| Obs.                            | 148,964 | 146,954  | 147,015   | 145,035    | 148,964    | 146,954     | 147,015  | 145,035  | 148,964 | 146,954  | 147,015  | 145,035 |
| Eff. Number of obs.             | 48,016  | 44,198   | 47,352    | 46,545     | 48,016     | 44,198      | 47,352   | 46,545   | 48,016  | 44,198   | 47,352   | 46,545  |
| Bandwidth                       | 11.28   | 11.293   | 10.52     | 10.33      | 11.28      | 11.293      | 10.52    | 10.33    | 11.28   | 11.293   | 10.52    | 10.33   |
| Less than 18 years of schooling | No      | Yes      | No        | Yes        | No         | Yes         | No       | Yes      | No      | Yes      | No       | Yes     |
| Trimmed hourly wages            | No      | No       | Yes       | Yes        | No         | No          | Yes      | Yes      | No      | No       | Yes      | Yes     |
| Survey year and birth           | No      | Yes      | Yes       | Yes        | No         | Yes         | Yes      | Yes      | No      | Yes      | Yes      | Yes     |

*Notes:* \*p<0.1, \*\* p<0.05, \*\*\* p<0.01

The sample is constructed from the 2009-2017 Mexican National Occupations and Employment Survey. Following Calonico et al. (2018) and Calonico et al. (2014) for the optimal bandwidth. Robust standard errors using EHW correction as recommended by Kolesár and Rothe (2018) in parentheses.

# Table B2. 20: Placebo test. Birth cohort born 48 months after the 1993 cut-off, non-parametric results of the employment for sectoral choices

|                        |         |                 | Birth coh  | ort Septerr | nber 1985 fo | or formal  | employm         | nent (=1) |         |           |           |         |
|------------------------|---------|-----------------|------------|-------------|--------------|------------|-----------------|-----------|---------|-----------|-----------|---------|
| Estimation method      |         | First           | -stage     |             |              | Reduo      | ed-form         |           |         | 2         | SLS       |         |
| Dependent variable     | -       | Years of        | schooling  | -           |              | Formal er  | nployment       |           |         | Formal er | nployment |         |
|                        | (a)     | (b)             | (c)        | (d)         | (a)          | (b)        | (c)             | (d)       | (a)     | (b)       | (c)       | (d)     |
| Treatment              | 0 288   | 0 240           | 0 292      | 0 2 7 1     | 0.017        | 0.029      | 0.030           | 0.012     |         |           |           |         |
| nearment               | (0 157) | (0 105)         | (0.200)    | (0.106)     | (0.021)      | (0.025)    | (0.026)         | (0.016)   |         |           |           |         |
| Veam of ash asling     | (0.157) | (0.155)         | (0.200)    | (0.150)     | (0.021)      | (0.020)    | (0.020)         | (0.010)   | 0.074   | 0.152     | 0 1 2 0   | 0.125   |
| rears of schooling     |         |                 |            |             |              |            |                 |           | 0.074   | 0.155     | 0.130     | 0.135   |
|                        |         |                 |            |             |              |            |                 |           | (0.094) | (0.138)   | (0.115)   | (0.133) |
| Obs.                   | 148.964 | 146.954         | 147.015    | 145.035     | 148,964      | 146.954    | 147.015         | 145.035   | 148,964 | 146,954   | 147.015   | 145.035 |
| Eff. Number of obs.    | 54,320  | 53,807          | 53,664     | 53,162      | 54,320       | 53,807     | 53,664          | 53,162    | 54,320  | 53,807    | 53,664    | 53,162  |
| Bandwidth              | 25.72   | 22.915          | 22.77      | 22.90       | 25.72        | 22.915     | 22.77           | 22.90     | 25.72   | 22.915    | 22.77     | 22.90   |
| Less than 18 years of  |         |                 |            |             |              |            |                 |           |         |           |           |         |
| schooling              | No      | Yes             | No         | Yes         | No           | Yes        | No              | Yes       | No      | Yes       | No        | Yes     |
| Trimmed hourly wages   | No      | No              | Yes        | Yes         | No           | No         | Yes             | Yes       | No      | No        | Yes       | Yes     |
| Survey year and birth  |         |                 |            |             |              |            |                 |           |         |           |           |         |
| region dummies         | No      | No              | No         | Yes         | No           | No         | No              | Yes       | No      | No        | No        | Yes     |
| -                      |         |                 |            |             |              |            |                 |           |         |           |           |         |
| Follow Managements and |         |                 | Birth coho | ort Septem  | ber 1985 to  | r informa  | l employr       | ment (=1) |         |           |           |         |
| Estimation method      | -       | First           | -stage     | -           |              | Reduo      | ed-form         |           |         | 2         | SLS       |         |
| Dependent variable     | (a)     | rears or<br>(h) | (c)        | (d)         | (2)          | /h)        | mpioymen<br>(c) | (d)       | (2)     | (h)       | (c)       | (d)     |
|                        | (a)     | (6)             | (9)        | (4)         | (a)          | (6)        | (9              | (4)       | (a)     | (0)       | (0)       | (4)     |
| Treatment              | 0.287   | 0.250           | 0.304      | 0.278       | -0.013       | -0.016     | -0.016          | -0.014    |         |           |           |         |
|                        | (0.145) | (0.152)         | (0.154)    | (0.152)     | (0.019)      | (0.020)    | (0.020)         | (0.020)   |         |           |           |         |
| Years of schooling     |         |                 |            |             |              |            |                 |           | -0.045  | -0.067    | -0.053    | -0.054  |
| 0                      |         |                 |            |             |              |            |                 |           | (0.069) | (0.083)   | (0.068)   | (0.077) |
|                        |         |                 |            |             |              |            |                 |           |         |           |           |         |
| Obs.                   | 148,964 | 146,954         | 147,015    | 145,035     | 148,964      | 146,954    | 147,015         | 145,035   | 148,964 | 146,954   | 147,015   | 145,035 |
| Eff. Number of obs.    | 39,722  | 39,364          | 39,230     | 38,883      | 39,722       | 39,364     | 39,230          | 38,883    | 39,722  | 39,364    | 39,230    | 38,883  |
| Bandwidth              | 25.72   | 22.915          | 22.77      | 22.90       | 25.72        | 22.915     | 22.77           | 22.90     | 25.72   | 22.915    | 22.77     | 22.90   |
| Less than 18 years of  | No      | Yes             | No         | Yes         | No           | Yes        | No              | Yes       | No      | Yes       | No        | Yes     |
| schooling              |         |                 |            |             |              |            |                 |           |         |           |           |         |
| Trimmed hourly wages   | No      | No              | Yes        | Yes         | No           | No         | Yes             | Yes       | No      | No        | Yes       | Yes     |
| Survey year and birth  | No      | No              | No         | Yes         | No           | No         | No              | Yes       | No      | No        | No        | Yes     |
| region dummies         |         |                 |            |             |              |            |                 |           |         |           |           |         |
|                        |         |                 | Birth co   | hort Septe  | mber 1985    | for self-e | mployme         | ent (=1)  |         |           |           |         |
| Estimation method      |         | First           | -stage     |             |              | Reduo      | ed-form         |           |         | 2         | SLS       |         |
| Dependent variable     | -       | Years of        | schooling  | -           |              | Self-em    | oloyment        | -         |         | Self-em   | oloyment  | -       |
|                        | (a)     | (b)             | (c)        | (d)         | (a)          | (b)        | (c)             | (d)       | (a)     | (b)       | (c)       | (d)     |
| Treatment              | 0.001   | 0.001           | 0 160      | 0.094       | 0.012        | 0.012      | 0.010           | 0.014     |         |           |           |         |
| reachient              | (0.007) | (0.001          | (0.104)    | (0.004      | (0.012)      | (0.012)    | (0.015)         | (0.019)   |         |           |           |         |
| · · · · ·              | (0.207) | (0.200)         | (0.194)    | (0.211)     | (0.010)      | (0.017)    | (0.010)         | (0.018)   | 0.000   | 0.050     | 0.007     | 0.000   |
| Years of schooling     |         |                 |            |             |              |            |                 |           | -0.066  | -0.069    | -0.087    | -0.082  |
|                        |         |                 |            |             |              |            |                 |           | (0.081) | (0.091)   | (0.071)   | (0.089) |
| Oha                    | 140.064 | 146.054         | 147.015    | 145.025     | 149.064      | 146.054    | 147.015         | 145.025   | 149.064 | 146.054   | 147.015   | 145.025 |
| CDS.                   | 148,964 | 140,954         | 147,015    | 145,035     | 148,964      | 146,954    | 147,015         | 145,035   | 148,964 | 46 534    | 147,015   | 145,035 |
| EII. NUMBER OT ODS.    | 40,952  | 40,524          | 40,388     | 45,972      | 40,952       | 40,524     | 40,388          | 45,972    | 40,952  | 40,524    | 40,388    | 45,972  |
|                        | 19.51   | 19.193          | 20.22      | 19.29       | 19.51        | 19.193     | 20.22           | 19.29     | 19.51   | 19.793    | 20.22     | 19.29   |
| Less than 18 years of  | No      | Yes             | No         | Yes         | No           | Yes        | No              | Yes       | No      | Yes       | No        | Yes     |
| Trimmed hourly wages   | No      | No              | Voc        | Vec         | No           | No         | Voc             | Ver       | No      | No        | Voc       | Ver     |
| Survey year and hirth  | NO      | NO              | ies        | 162         | NO           | NU         | 162             | 125       | NO      | NO        | 162       | 162     |
| i a part and an an all |         |                 |            |             |              |            |                 |           |         |           |           |         |

Notes: \*p<0.1, \*\* p<0.05, \*\*\* p<0.01

region dummies

No

No

No

Yes

The sample is constructed from the 2009-2017 Mexican National Occupations and Employment Survey. Following Calonico et al. (2018) and Calonico et al. (2014) for the optimal bandwidth. Robust standard errors using EHW correction as recommended by Kolesár and Rothe (2018) in parentheses.

No

No

No

Yes

No

No

No

Yes

# Table B2. 21: Placebo test. Birth cohort born 48 months before the 1993 cut-off, parametric results of the employment sectoral choices

|   |         |                          | Birth coh  | ort Septen | nber 1977 fo           | or formal  | employm   | ent (=1)  |         |            |           |          |
|---|---------|--------------------------|------------|------------|------------------------|------------|-----------|-----------|---------|------------|-----------|----------|
| Estimation method                       | _       | First-stage Reduced-form |            |            |                        |            |           |           |         | 25         | SLS       | _        |
| Dependent variable                      | -       | Years of                 | schooling  | •          |                        | Formal en  | nployment |           |         | Formal en  | nployment |          |
|   | (a)     | (b)                      | (c)        | (d)        | (a)                    | (b)        | (c)       | (d)       | (a)     | (b)        | (c)       | (d)      |
| T                                       | 0.045   | 0.024                    | 0.000      | 0.071      | 0.015                  | 0.017      | 0.012     | 0.015     |         |            |           |          |
| freatment                               | -0.045  | -0.034                   | -0.000     | -0.071     | 0.015                  | (0.017     | 0.013     | 0.013     |         |            |           |          |
|   | (0.089) | (0.086)                  | (0.089)    | (0.086)    | (0.011)                | (0.011)    | (0.011)   | (0.011)   |         |            |           |          |
| Years of schooling                      |         |                          |            |            |                        |            |           |           | -0.325  | -0.505     | -0.148    | -0.215   |
|   |         |                          |            |            |                        |            |           |           | (0.763) | (1.439)    | (0.227)   | (0.350)  |
| Obs.                                    | 148,964 | 146,954                  | 147,015    | 145,035    | 148,964                | 146,954    | 147,015   | 145,035   | 148,964 | 146,954    | 147,015   | 145,035  |
| Less than 18 years of                   | Ne      | Var                      | Ne         | Vee        | N                      | Vee        | Ne        | Vaa       | Ma      | Vee        | Ne        | Vee      |
| schooling                               | NO      | res                      | NO         | res        | NO                     | res        | NO        | res       | NO      | res        | INO       | res      |
| Trimmed hourly wages                    | No      | No                       | Yes        | Yes        | No                     | No         | Yes       | Yes       | No      | No         | Yes       | Yes      |
| Survey year and birth<br>region dummies | No      | Yes                      | No         | Yes        | No                     | Yes        | Yes       | Yes       | No      | Yes        | Yes       | Yes      |
|   |         |                          | Birth coho | rt Septem  | ber 1977 fo            | r informa  | l employr | nent (=1) |         |            |           |          |
| Estimation method                       |         | First                    | -stage     |            |                        | Reduo      | ed-form   | √ =/      |         | 25         | SLS       |          |
| Dependent variable                      | -       | Years of                 | schooling  | -          |                        | Informal e | mplovmen  | ŧ         |         | Informal e | mplovmen  | ŧ        |
|   | (a)     | (b)                      | (c)        | (d)        | (a)                    | (b)        | (c)       | (d)       | (a)     | (b)        | (c)       | (d)      |
| Trantariat                              | 0.045   | 0.024                    | 0.000      | 0.071      | 0.015                  | 0.017      | 0.012     | 0.015     |         |            |           |          |
| Treatment                               | -0.045  | -0.034                   | -0.088     | -0.071     | -0.015                 | -0.017     | -0.015    | -0.015    |         |            |           |          |
| · · · ·                                 | (0.089) | (0.086)                  | (0.089)    | (0.086)    | (0.011)                | (0.011)    | (0.011)   | (0.011)   |         |            |           |          |
| Years of schooling                      |         |                          |            |            |                        |            |           |           | 0.325   | 0.505      | 0.148     | 0.215    |
|   |         |                          |            |            |                        |            |           |           | (0.763) | (1.439)    | (0.227)   | (0.350)  |
| Obs.                                    | 148,964 | 146,954                  | 147,015    | 145,035    | 148,964                | 146,954    | 147,015   | 145,035   | 148,964 | 146,954    | 147,015   | 145,035  |
| Less than 18 years of                   | No      | Yes                      | No         | Yes        | No                     | Yes        | No        | Yes       | No      | Yes        | No        | Yes      |
| schooling                               |         |                          |            |            |                        |            |           |           |         |            |           |          |
| Irimmed hourly wages                    | No      | No                       | Yes        | Yes        | No                     | No         | Yes       | Yes       | No      | No         | Yes       | Yes      |
| Survey year and birth                   | No      | Yes                      | No         | Yes        | No                     | Yes        | Yes       | Yes       | No      | Yes        | Yes       | Yes      |
| region dummies                          |         |                          |            |            |                        |            |           |           |         |            |           |          |
|   |         |                          | Birth      | cohort Sep | tember 197             | 7 for self | -employn  | nent      |         |            |           |          |
| Estimation method                       |         | First                    | -stage     |            |                        | Reduo      | ed-form   |           |         | 25         | SLS       |          |
| Dependent variable                      | -       | Years of                 | schooling  | -          |                        | Self-em    | oloyment  | -         |         | Self-emp   | oloyment  | -        |
|   | (a)     | (b)                      | (c)        | (d)        | (a)                    | (b)        | (c)       | (d)       | (a)     | (b)        | (c)       | (d)      |
|   |         |                          |            |            |                        |            |           |           |         |            |           |          |
| Treatment                               | -0.045  | -0.034                   | -0.088     | -0.071     | 0.003                  | 0.001      | 0.005     | 0.004     |         |            |           |          |
|   | (0.089) | (0.086)                  | (0.089)    | (0.086)    | (0.009)                | (0.009)    | (0.009)   | (0.009)   |         |            |           |          |
| Years of schooling                      |         |                          |            |            |                        |            |           |           | -0.060  | -0.036     | -0.052    | -0.050   |
|   |         |                          |            |            |                        |            |           |           | (0.220) | (0.271)    | (0.108)   | (0.134)  |
|   |         |                          |            |            |                        |            |           |           | (0.220) | (0.272)    | (0.100)   | (0.10 .) |
| Obs.                                    | 148,964 | 146,954                  | 147,015    | 145,035    | 148 <mark>,</mark> 964 | 146,954    | 147,015   | 145,035   | 148,964 | 146,954    | 147,015   | 145,035  |
| Less than 18 years of                   | No      | Yes                      | No         | Yes        | No                     | Yes        | No        | Yes       | No      | Yes        | No        | Yes      |
| schooling                               | No      | No                       | Vec        | Voc        | No                     | No         | Voc       | Vec       | No      | No         | Voc       | Ves      |
| Survey year and hirth                   | 110     | 110                      | 162        | 165        | NO                     | 110        | 105       | 100       | NO      | 110        | 162       | 105      |
| region dummies                          | No      | Yes                      | No         | Yes        | No                     | Yes        | Yes       | Yes       | No      | Yes        | Yes       | Yes      |

*Notes:* \*p<0.1, \*\* p<0.05, \*\*\* p<0.01

The sample is constructed from the 2009-2017 Mexican National Occupations and Employment Survey. Robust standard errors using EHW correction as recommended by Kolesár and Rothe (2018) in parentheses.

# Table B2. 22: Placebo test. Birth cohort born 48 months after the 1993 cut-off, parametric results of the employment sectoral choices

|  |         |          | Birth coh  | ort Septerr | nber 1985 fo | or formal  | employm   | ent (=1)  |         |            |           |         |
|--|---------|----------|------------|-------------|--------------|------------|-----------|-----------|---------|------------|-----------|---------|
| Estimation method  | _       | First    | -stage     |             |              | Reduce     | ed-form   | _         |         | 25         | SLS       |         |
| Dependent variable   | -       | Years of | schooling  |             |              | Formal en  | nployment |           |         | Formal en  | nployment | -       |
|  | (a)     | (b)      | (c)        | (d)         | (a)          | (b)        | (c)       | (d)       | (a)     | (b)        | (c)       | (d)     |
| Trootmont  | 0.074   | 0 044    | 0.085      | 0.053       | 0.003        | 0.001      | 0.002     | -0.000    |         |            |           |         |
| reatment   | (0.072) | (0.071)  | (0.005)    | (0.072)     | (0,000)      | (0.000)    | (0.002    | (0.000)   |         |            |           |         |
| Variation of the section of the sect | (0.072) | (0.071)  | (0.073)    | (0.072)     | (0.009)      | (0.009)    | (0.009)   | (0.009)   | 0.042   | 0.010      | 0.024     | 0.005   |
| rears of schooling   |         |          |            |             |              |            |           |           | 0.042   | 0.018      | 0.024     | -0.005  |
|  |         |          |            |             |              |            |           |           | (0.121) | (0.204)    | (0.106)   | (0.1/8) |
| Obs.   | 148,964 | 146,954  | 147,015    | 145,035     | 148,964      | 146,954    | 147,015   | 145,035   | 148,964 | 146,954    | 147,015   | 145,035 |
| Less than 18 years of  | No      | Yes      | No         | Yes         | No           | Yes        | No        | Yes       | No      | Yes        | No        | Yes     |
| Trimmed hourly wages   | No      | No       | Vos        | Vos         | No           | No         | Vec       | Vos       | No      | No         | Voc       | Vec     |
| Survey year and Birth  | NO      | NO       | ies        | ies         | NU           | NO         | Tes       | Tes       | NU      | NO         | Tes       | Tes     |
| region dummies   | No      | Yes      | No         | Yes         | No           | Yes        | No        | Yes       | No      | Yes        | No        | Yes     |
|  |         |          | Rirth coho | rt Sontom   | har 1985 fo  | rinforma   | lomploy   | mont (-1) |         |            |           |         |
| Estimation method  |         | First    | stago      | rt septem   | ber 1965 fü  | Poduce     |           | nent (-1) |         | 2(         | 21.0      |         |
| Dependent variable   | -       | Years of | schooling  |             |              | Informal e | mplovmen  | ŧ         |         | Informal e | nplovment | ŧ       |
|  | (a)     | (b)      | (c)        | (d)         | (a)          | (b)        | (c)       | (d)       | (a)     | (b)        | (c)       | (d)     |
|  |         |          | 0.005      | 0.050       | 0.000        |            |           |           |         |            |           |         |
| Treatment  | 0.074   | 0.044    | 0.085      | 0.053       | -0.003       | -0.001     | -0.002    | 0.000     |         |            |           |         |
| _  | (0.072) | (0.071)  | (0.073)    | (0.072)     | (0.009)      | (0.009)    | (0.009)   | (0.009)   |         |            |           |         |
| Years of schooling   |         |          |            |             |              |            |           |           | -0.042  | -0.018     | -0.024    | 0.005   |
|  |         |          |            |             |              |            |           |           | (0.121) | (0.204)    | (0.106)   | (0.178) |
| Obs.   | 148,964 | 146,954  | 147,015    | 145,035     | 148,964      | 146,954    | 147,015   | 145,035   | 148,964 | 146,954    | 147,015   | 145,035 |
| Less than 18 years of  | No      | Yes      | No         | Yes         | No           | Yes        | No        | Yes       | No      | Yes        | No        | Yes     |
| schooling  |         |          |            |             |              |            |           |           |         |            |           |         |
| Trimmed hourly wages   | No      | No       | Yes        | Yes         | No           | No         | Yes       | Yes       | No      | No         | Yes       | Yes     |
| Survey year and Birth<br>region dummies  | No      | Yes      | No         | Yes         | No           | Yes        | No        | Yes       | No      | Yes        | No        | Yes     |
|  |         |          | Birth co   | hort Sente  | mber 1985    | for self-e | mployme   | ot (=1)   |         |            |           |         |
| Estimation method  |         | First    | -stage     | noreocpie   | 111501 2505  | Reduce     | -d-form   |           |         | 25         | SLS       |         |
| Dependent variable   | -       | Years of | schooling  |             |              | Self-em    | olovment  | -         |         | Self-emr   | oloyment  | -       |
| •  | (a)     | (b)      | (c)        | (d)         | (a)          | (b) '      | ,<br>(c)  | (d)       | (a)     | (b)        | (c)       | (d)     |
|  |         |          |            |             |              |            |           |           |         |            |           |         |
| Treatment  | 0.074   | 0.044    | 0.085      | 0.053       | 0.002        | -0.001     | 0.002     | -0.001    |         |            |           |         |
| ~  | (0.072) | (0.071)  | (0.073)    | (0.072)     | (0.006)      | (0.006)    | (0.006)   | (0.006)   |         |            |           |         |
| Years of schooling   |         |          |            |             |              |            |           |           | 0.029   | -0.014     | 0.020     | -0.019  |
|  |         |          |            |             |              |            |           |           | (0.089) | (0.140)    | (0.073)   | (0.116) |
| Obs.   | 148,964 | 146,954  | 147,015    | 145,035     | 148,964      | 146,954    | 147,015   | 145,035   | 148,964 | 146,954    | 147,015   | 145,035 |
| Less than 18 years of  |         |          |            |             |              |            |           |           |         |            |           |         |
| schooling  | No      | Yes      | No         | Yes         | No           | Yes        | No        | Yes       | No      | Yes        | No        | Yes     |
| Trimmed hourly wages   | No      | No       | Yes        | Yes         | No           | No         | Yes       | Yes       | No      | No         | Yes       | Yes     |
| Survey year and Birth  | No      | Yes      | No         | Yes         | No           | Yes        | No        | Yes       | No      | Yes        | No        | Yes     |
| region dummies   |         |          |            |             |              |            |           |           |         |            |           |         |

Notes: \*p<0.1, \*\* p<0.05, \*\*\* p<0.01

The sample is constructed from the 2009-2017 Mexican National Occupations and Employment Survey. Robust standard errors using EHW correction as recommended by Kolesár and Rothe (2018) in parentheses.

|   |                    |                    |                    |                    |                    |                    | 24-moi | nths win | dow       |                  |        |        |                   |                   |                   |                   |                   |                   |
|---|--------------------|--------------------|--------------------|--------------------|--------------------|--------------------|--------|----------|-----------|------------------|--------|--------|-------------------|-------------------|-------------------|-------------------|-------------------|-------------------|
| Estimation method                       |                    |                    | First              | -stage             |                    |                    |        |          | Reduce    | ed-form          |        |        |                   |                   | 23                | SLS               |                   |                   |
| Dependent variable                      | -                  | 5                  | econdary           | certifica          | te                 | -                  |        | -        | Log of ho | urly wage        | 25     | -      |                   | L                 | og of ho          | urly wag          | es                | -                 |
|   | (a)                | (b)                | (c)                | (d)                | (e)                | (f)                | (a)    | (b)      | (C)       | (d)              | (e)    | (f)    | (a)               | (b)               | (c)               | (d)               | (e)               | (f)               |
| Treatment                               | -0.022*<br>(0.013) | -0.021*<br>(0.013) | -0.024*<br>(0.013) | -0.024*<br>(0.013) | -0.025*<br>(0.013) | -0.026*<br>(0.013) | 0.009  | 0.006    | 0.007     | 0.005<br>(0.015) | 0.009  | 0.009  |                   |                   |                   |                   |                   |                   |
| Years of schooling                      |                    |                    |                    |                    |                    |                    |        |          |           |                  |        |        | -0.411<br>(0.747) | -0.299<br>(0.733) | -0.310<br>(0.626) | -0.214<br>(0.615) | -0.365<br>(0.612) | -0.344<br>(0.598) |
| Obs.                                    | 50,389             | 50,389             | 50,429             | 50,429             | 49,716             | 49,716             | 50,389 | 50,389   | 50,429    | 50,429           | 49,716 | 49,716 | 50,389            | 50,389            | 50,429            | 50,429            | 49,716            | 49,716            |
| Less than 18 years of<br>schooling      | Yes                | Yes                | No                 | No                 | Yes                | Yes                | Yes    | Yes      | No        | No               | Yes    | Yes    | Yes               | Yes               | No                | No                | Yes               | Yes               |
| Trimmed hourly wages                    | No                 | No                 | Yes                | Yes                | Yes                | Yes                | No     | No       | Yes       | Yes              | Yes    | Yes    | No                | No                | Yes               | Yes               | Yes               | Yes               |
| Survey year and birth<br>region dummies | No                 | Yes                | No                 | Yes                | No                 | Yes                | No     | Yes      | No        | Yes              | No     | Yes    | No                | Yes               | No                | Yes               | No                | Yes               |
|   |                    |                    |                    |                    |                    |                    | 42-mo  | nths win | dow       |                  |        |        |                   |                   |                   |                   |                   |                   |
| Estimation method                       | _                  |                    | First              | -stage             |                    | _                  |        |          | Reduce    | ed-form          |        | _      |                   |                   | 23                | SLS               |                   | -                 |
| Dependent variable                      |                    | н                  | igh-schoo          | ol certifica       | ate                |                    |        |          | Log of ho | urly wage        | 25     |        |                   | L                 | og of ho          | urly wag          | es                |                   |
|   | (a)                | (b)                | (c)                | (d)                | (e)                | (f)                | (a)    | (b)      | (C)       | (d)              | (e)    | (f)    | (a)               | (b)               | (c)               | (d)               | (e)               | (f)               |
| Treatment                               | 0.017**            | 0.017**            | 0.020**            | 0.020**            | 0.020**            | 0.019**            | 0.022* | 0.021*   | 0.014     | 0.013<br>(0.011) | 0.016  | 0.015  |                   |                   |                   |                   |                   |                   |
| Years of schooling                      |                    |                    |                    |                    |                    |                    |        |          |           |                  |        |        | 1.217<br>(0.881)  | 1.209<br>(0.904)  | 0.705<br>(0.636)  | 0.679<br>(0.637)  | 0.784<br>(0.639)  | 0.757<br>(0.646)  |
| Obs.                                    | 87,011             | 87,011             | 87,090             | 87,090             | 85,890             | 85,890             | 87,011 | 87,011   | 87,090    | 87,090           | 85,890 | 85,890 | 87,011            | 87,011            | 87,090            | 87,090            | 85,890            | 85,890            |
| Less than 18 years of<br>schooling      | Yes                | Yes                | No                 | No                 | Yes                | Yes                | Yes    | Yes      | No        | No               | Yes    | Yes    | Yes               | Yes               | No                | No                | Yes               | Yes               |
| Trimmed hourly wages                    | No                 | No                 | Yes                | Yes                | Yes                | Yes                | No     | No       | Yes       | Yes              | Yes    | Yes    | No                | No                | Yes               | Yes               | Yes               | Yes               |
| Survey year and birth<br>region dummies | No                 | Yes                | No                 | Yes                | No                 | Yes                | No     | Yes      | No        | Yes              | No     | Yes    | No                | Yes               | No                | Yes               | No                | Yes               |

Table B2. 23: Parametric results of holding secondary schooling and high-school certificates

Notes: \*p<0.1, \*\* p<0.05, \*\*\* p<0.01

The sample is constructed from the 2009-2017 Mexican National Occupations and Employment Survey. Robust standard errors using EHW correction as recommended by Kolesár and Rothe (2018) in parentheses.

|                          | After treatment |           |     |     |       |           |     |     |
|--------------------------|-----------------|-----------|-----|-----|-------|-----------|-----|-----|
| Variable                 | Mean            | Std. Dev. | Min | Max | Mean  | Std. Dev. | Min | Max |
| Age                      | 36              | 11.46     | 15  | 65  | 36    | 11.30     | 15  | 65  |
| Years of schooling       | 11.10           | 4.05      | 0   | 24  | 11.20 | 3.90      | 0   | 24  |
| Urban status             | 0.69            | 0.46      | 0   | 1   | 0.68  | 0.47      | 0   | 1   |
| Marital status:          |                 |           |     |     |       |           |     |     |
| Married                  | 0.73            | 0.44      | 0   | 1   | 0.74  | 0.44      | 0   | 1   |
| Divorced                 | 0.03            | 0.18      | 0   | 1   | 0.03  | 0.18      | 0   | 1   |
| Single                   | 0.24            | 0.42      | 0   | 1   | 0.22  | 0.42      | 0   | 1   |
| Working in Public sector | 0.26            | 0.44      | 0   | 1   | 0.22  | 0.42      | 0   | 1   |
| Economic sector:         |                 |           |     |     |       |           |     |     |
| Agriculture              | 0.03            | 0.16      | 0   | 1   | 0.02  | 0.15      | 0   | 1   |
| Commerce                 | 0.17            | 0.37      | 0   | 1   | 0.17  | 0.38      | 0   | 1   |
| Construction             | 0.06            | 0.25      | 0   | 1   | 0.07  | 0.25      | 0   | 1   |
| Manufacturing            | 0.23            | 0.42      | 0   | 1   | 0.25  | 0.43      | 0   | 1   |
| Sevices                  | 0.48            | 0.50      | 0   | 1   | 0.46  | 0.50      | 0   | 1   |

Table B3. 1: Summary statistics of the covariates by treatment group

Source: Mexican National Occupations and Employment Survey (2010-2015). Total observations 126,475

|                          |       | Before tre | atment |     | After treatment |           |     |     |  |  |
|--------------------------|-------|------------|--------|-----|-----------------|-----------|-----|-----|--|--|
| Variable                 | Mean  | Std. Dev.  | Min    | Max | Mean            | Std. Dev. | Min | Max |  |  |
| Age                      | 36    | 11.44      | 15     | 65  | 36              | 11.20     | 15  | 65  |  |  |
| Years of schooling       | 10.77 | 3.91       | 0      | 24  | 10.80           | 3.74      | 0   | 24  |  |  |
| Urban status             | 0.63  | 0.48       | 0      | 1   | 0.52            | 0.50      | 0   | 1   |  |  |
| Marital status:          |       |            |        |     |                 |           |     |     |  |  |
| Married                  | 0.69  | 0.46       | 0      | 1   | 0.70            | 0.46      | 0   | 1   |  |  |
| Divorced                 | 0.04  | 0.20       | 0      | 1   | 0.04            | 0.21      | 0   | 1   |  |  |
| Single                   | 0.27  | 0.44       | 0      | 1   | 0.26            | 0.44      | 0   | 1   |  |  |
| Working in Public sector | 0.23  | 0.42       | 0      | 1   | 0.17            | 0.38      | 0   | 1   |  |  |
| Economic sector:         |       |            |        |     |                 |           |     |     |  |  |
| Agriculture              | 0.03  | 0.18       | 0      | 1   | 0.03            | 0.16      | 0   | 1   |  |  |
| Commerce                 | 0.17  | 0.37       | 0      | 1   | 0.17            | 0.38      | 0   | 1   |  |  |
| Construction             | 0.06  | 0.24       | 0      | 1   | 0.06            | 0.24      | 0   | 1   |  |  |
| Manufacturing            | 0.23  | 0.42       | 0      | 1   | 0.29            | 0.45      | 0   | 1   |  |  |
| Sevices                  | 0.47  | 0.50       | 0      | 1   | 0.43            | 0.49      | 0   | 1   |  |  |

### Table B3. 2: Summary statistics of covariates by the control group

Source: Mexican National Occupations and Employment Survey (2010-2015). Total observations 25,094

Table B3. 3: OLS estimates of the log of hourly wages

|                                |                   | Log of hourly wage |                   |
|--------------------------------|-------------------|--------------------|-------------------|
|                                | (1)               | (2)                | (3)               |
| Sample                         | Pooled sample     | Private sector     | Public sector     |
| Time                           | -0.090***         | -0.090***          | -0.087***         |
|                                | (0.008)           | (0.009)            | (0.017)           |
| Treatment effects              | 0.056***          | 0.050***           | 0.068***          |
|                                | (0.010)           | (0.011)            | (0.019)           |
| Effects on miscellaneous group | 0.062***          | 0.063***           | 0.076***          |
|                                | (0.017)           | (0.015)            | (0.029)           |
| Years of schooling             | 0.072***          | 0.066***           | 0.087***          |
|                                | (0.001)           | (0.001)            | (0.001)           |
| Age                            | 0.027***          | 0.031***           | 0.025***          |
| Age coulored                   | 0.000***          | 0.000***           | 0.000***          |
| Age squared                    | (0.000)           | (0.000)            | (0.000)           |
| Urban reside                   | 0.051***          | 0.050***           | 0.084***          |
|                                | (0.015)           | (0.014)            | (0.026)           |
| Working in Public sector       | 0.314***          |                    |                   |
|                                | (0.009)           |                    |                   |
| Economic sector                | 0.407848          | 0.000***           | 0.450             |
| Construction                   | (0.012)           | (0.012)            | (0.152)           |
| Commerce                       | -0.1/0***         | -0.125***          | -0.426***         |
| commerce                       | (0.010)           | (0.009)            | (0.065)           |
| Services                       | -0.040***         | -0.019**           | -0.333***         |
|                                | (0.010)           | (0.009)            | (0.054)           |
| Agriculture                    | -0.040***         | -0.036***          | -0.682***         |
|                                | (0.013)           | (0.012)            | (0.131)           |
| Other                          | 0.186***          | 0.229***           | -0.106**          |
|                                | (0.025)           | (0.029)            | (0.043)           |
| Marital status                 |                   |                    |                   |
| Married                        | 0.051***          | 0.067***           | 0.006             |
|                                | (0.000)           | 0.009              | (0.014)           |
| Single                         | -0.012<br>(0.008) | -0.004<br>(0.010)  | -0.013<br>(0.015) |
| Constant                       | 1 750***          | 1 75 4***          | 2 072***          |
| Constant                       | (0.024)           | (0.030)            | (0.059)           |
| Obc                            | 172 660           | 125 270            | 28 200            |

Notes: \*\* p<0.05, \*\*\* p<0.01

The sample is constructed from the 2010-2015 Mexican National Occupations and Employment Survey. Municipality Fixed effects. Standard errors adjusted at municipality level in parentheses.

|                                | (1)       | (2)       | (3)       | (4)       | (5)       |
|--------------------------------|-----------|-----------|-----------|-----------|-----------|
|                                | RIF 10    | RIF 25    | RIF 50    | RIF 75    | RIF 90    |
| Time                           | -0.058*** | -0.079*** | -0.083*** | -0.087*** | -0.119*** |
|                                | (0.010)   | (0.014)   | (0.016)   | (0.013)   | (0.020)   |
| Treatment effects              | 0.033***  | 0.050***  | 0.057***  | 0.063***  | 0.055**   |
|                                | (0.012)   | (0.015)   | (0.017)   | (0.016)   | (0.023)   |
| Public sector                  | 0.218***  | 0.218***  | 0.303***  | 0.478***  | 0.444***  |
|                                | (0.013)   | (0.011)   | (0.009)   | (0.015)   | (0.026)   |
| Effects on miscellaneous group | 0.031     | 0.069***  | 0.052***  | 0.060***  | 0.08      |
|                                | (0.017)   | (0.016)   | (0.017)   | (0.020)   | (0.041)   |
| Years of schooling             | 0.030***  | 0.040***  | 0.064***  | 0.109***  | 0.131***  |
|                                | (0.002)   | (0.001)   | (0.001)   | (0.002)   | (0.003)   |
| Age                            | 0.025***  | 0.035***  | 0.040***  | 0.030***  | 0.005**   |
|                                | (0.002)   | (0.001)   | (0.001)   | (0.002)   | (0.002)   |
| Age squared                    | -0.000*** | -0.000*** | -0.000*** | -0.000*** | 0.000***  |
|                                | (0.000)   | (0.000)   | (0.000)   | (0.000)   | (0.000)   |
| Urban reside                   | 0.024     | 0.063***  | 0.077***  | 0.066***  | 0.012     |
|                                | (0.018)   | (0.014)   | (0.017)   | (0.022)   | (0.031)   |
| Economic sector                |           |           |           |           |           |
| Construction                   | 0.141***  | 0.209***  | 0.269***  | 0.237***  | 0.123***  |
|                                | (0.014)   | (0.015)   | (0.016)   | (0.019)   | (0.020)   |
| Commerce                       | -0.156*** | -0.142*** | -0.130*** | -0.143*** | -0.143*** |
|                                | (0.011)   | (0.012)   | (0.013)   | (0.012)   | (0.014)   |
| Services                       | -0.142*** | -0.066*** | 0.006     | 0.012     | -0.015    |
|                                | (0.010)   | (0.011)   | (0.013)   | (0.012)   | (0.017)   |
| Agriculture                    | -0.204*** | -0.172*** | -0.059*** | 0.083***  | 0.153***  |
|                                | (0.027)   | (0.021)   | (0.016)   | (0.018)   | (0.024)   |
| Other                          | -0.023    | 0.079***  | 0.199***  | 0.339***  | 0.366***  |
|                                | (0.016)   | (0.016)   | (0.020)   | (0.036)   | (0.076)   |
| Marital status                 |           |           |           |           |           |
| Married                        | 0.039***  | 0.046***  | 0.059***  | 0.061***  | -0.031    |
|                                | (0.009)   | (0.008)   | (0.009)   | (0.013)   | (0.020)   |
| Single                         | -0.020*   | -0.018**  | 0.001     | 0.010     | -0.051**  |
|                                | (0.011)   | (0.009)   | (0.010)   | (0.012)   | (0.020)   |
| Constant                       | 1.800***  | 1.653***  | 1.483***  | 1.461***  | 2.187***  |
|                                | (0.059)   | (0.040)   | (0.028)   | (0.045)   | (0.076)   |
| Obs.                           | 173,669   | 173,669   | 173,669   | 173,669   | 173,669   |

Table B3. 4: Unconditional quantile estimates for selected percentiles

The sample is constructed from the 2010-2015 Mexican National Occupations and Employment Survey. Municipality Fixed effects. Standard errors adjusted for 780 clusters at municipality level in parentheses.

|                                | RIF-Gini approach<br>Hourly wage |                |               |  |  |
|--------------------------------|----------------------------------|----------------|---------------|--|--|
|                                | (1)                              | (2)            | (3)           |  |  |
| Sample                         | Pooled sample                    | Private sector | Public sector |  |  |
| Time                           | -0.022**                         | -0.006         | -0.078***     |  |  |
|                                | (0.009)                          | (0.005)        | (0.021)       |  |  |
| Treatment effects              | 0.011                            | 0.000          | 0.044**       |  |  |
|                                | (0.009)                          | (0.006)        | (0.021)       |  |  |
| Effects on miscellaneous group | 0.017                            | 0.005          | 0.070         |  |  |
|                                | (0.014)                          | (0.009)        | (0.041)       |  |  |
| Years of schooling             | 0.018***                         | 0.012***       | 0.037***      |  |  |
|                                | (0.001)                          | (0.001)        | (0.002)       |  |  |
| Age                            | -0.010***                        | -0.007***      | -0.005**      |  |  |
|                                | (0.001)                          | (0.001)        | (0.002)       |  |  |
| Age squared                    | 0.000***                         | 0.000***       | 0.000***      |  |  |
|                                | (0.000)                          | (0.000)        | (0.000)       |  |  |
| Urban reside                   | -0.011                           | -0.011**       | 0.005         |  |  |
|                                | (0.007)                          | (0.005)        | (0.017)       |  |  |
| Public sector                  | 0.006                            |                |               |  |  |
|                                | (0.006)                          |                |               |  |  |
| Economic sector                |                                  |                |               |  |  |
| Construction                   | -0.028***                        | -0.025***      | -0.052        |  |  |
|                                | (0.004)                          | (0.004)        | (0.165)       |  |  |
| Commerce                       | -0.001                           | 0.004          | -0.124***     |  |  |
|                                | (0.004)                          | (0.003)        | (0.047)       |  |  |
| Services                       | 0.013***                         | 0.027***       | -0.062        |  |  |
|                                | (0.004)                          | (0.003)        | (0.042)       |  |  |
| Agriculture                    | 0.062***                         | 0.056***       | -0.416***     |  |  |
|                                | (0.006)                          | (0.006)        | (0.058)       |  |  |
| Other                          | 0.066***                         | 0.039**        | 0.027         |  |  |
|                                | (0.019)                          | (0.018)        | (0.034)       |  |  |
| Marital status                 |                                  |                |               |  |  |
| Married                        | -0.002                           | -0.003         | -0.003        |  |  |
|                                | (0.006)                          | (0.005)        | (0.012)       |  |  |
| Single                         | -0.011                           | -0.002         | -0.038***     |  |  |
|                                | (0.006)                          | (0.006)        | (0.013)       |  |  |
| Constant                       | 0.292***                         | 0.320***       | -0.031        |  |  |
|                                | (0.026)                          | (0.025)        | (0.057)       |  |  |
| Obs.                           | 173,669                          | 135,379        | 38,290        |  |  |

Table B3. 5: RIF-Gini estimates for unconditional quantiles

The sample is constructed from the 2010-2015 Mexican National Occupations and Employment Survey. Municipality Fixed effects. Standard errors adjusted at municipality level in parentheses.

|                    |       | Pre-treatment |     |     |       | Post-treatment |     |     |  |  |
|--------------------|-------|---------------|-----|-----|-------|----------------|-----|-----|--|--|
| Variable           | Mean  | Std. Dev.     | Min | Max | Mean  | Std. Dev.      | Min | Max |  |  |
| Age                | 42    | 10.69         | 18  | 65  | 43    | 10.78          | 17  | 65  |  |  |
| Years of schooling | 13.65 | 3.76          | 0   | 23  | 13.69 | 3.73           | 0   | 22  |  |  |
| Urban status       | 0.72  | 0.45          | 0   | 1   | 0.74  | 0.44           | 0   | 1   |  |  |
| Head of household  | 0.76  | 0.43          | 0   | 1   | 0.76  | 0.43           | 0   | 1   |  |  |
| Marital status     |       |               |     |     |       |                |     |     |  |  |
| Married            | 0.78  | 0.41          | 0   | 1   | 0.78  | 0.42           | 0   | 1   |  |  |
| Divorced           | 0.05  | 0.21          | 0   | 1   | 0.05  | 0.22           | 0   | 1   |  |  |
| Single             | 0.17  | 0.38          | 0   | 1   | 0.18  | 0.38           | 0   | 1   |  |  |
| Economic sector    |       |               |     |     |       |                |     |     |  |  |
| Agriculture        | 0.00  | 0.02          | 0   | 1   | 0.00  | 0.00           | 0   | 1   |  |  |
| Commerce           | 0.00  | 0.06          | 0   | 1   | 0.01  | 0.07           | 0   | 1   |  |  |
| Construction       | 0.00  | 0.02          | 0   | 1   | 0.00  | 0.01           | 0   | 1   |  |  |
| Manufacturing      | 0.01  | 0.12          | 0   | 1   | 0.01  | 0.12           | 0   | 1   |  |  |
| Sevices            | 0.91  | 0.29          | 0   | 1   | 0.91  | 0.29           | 0   | 1   |  |  |
| Mining and energy  | 0.07  | 0.26          | 0   | 1   | 0.07  | 0.25           | 0   | 1   |  |  |

Table B4. 1: Summary statistics of the covariates by treatment group pre -and post-treatment

Source: Mexican National Occupations and Employment Survey (2017-2019). Total observations 14, 717.

Table B4. 2: Summary statistics of the covariates by control group pre -and post-treatment

|                    |       | Pre-treatment |     |     |       | Post-treatment |     |     |  |  |
|--------------------|-------|---------------|-----|-----|-------|----------------|-----|-----|--|--|
| Variable           | Mean  | Std. Dev.     | Min | Max | Mean  | Std. Dev.      | Min | Max |  |  |
| Age                | 36    | 11.40         | 15  | 65  | 36    | 11.56          | 15  | 65  |  |  |
| Years of schooling | 10.79 | 3.58          | 0   | 24  | 10.93 | 3.55           | 0   | 24  |  |  |
| Urban status       | 0.69  | 0.46          | 0   | 1   | 0.73  | 0.44           | 0   | 1   |  |  |
| Head of household  | 0.64  | 0.48          | 0   | 1   | 0.61  | 0.49           | 0   | 1   |  |  |
| Marital status:    |       |               |     |     |       |                |     |     |  |  |
| Married            | 0.70  | 0.46          | 0   | 1   | 0.68  | 0.47           | 0   | 1   |  |  |
| Divorced           | 0.04  | 0.20          | 0   | 1   | 0.05  | 0.21           | 0   | 1   |  |  |
| Single             | 0.25  | 0.43          | 0   | 1   | 0.27  | 0.44           | 0   | 1   |  |  |
| Economic sector:   |       |               |     |     |       |                |     |     |  |  |
| Agriculture        | 0.03  | 0.17          | 0   | 1   | 0.03  | 0.17           | 0   | 1   |  |  |
| Commerce           | 0.21  | 0.40          | 0   | 1   | 0.20  | 0.40           | 0   | 1   |  |  |
| Construction       | 0.09  | 0.29          | 0   | 1   | 0.08  | 0.27           | 0   | 1   |  |  |
| Manufacturing      | 0.33  | 0.47          | 0   | 1   | 0.35  | 0.48           | 0   | 1   |  |  |
| Sevices            | 0.33  | 0.47          | 0   | 1   | 0.32  | 0.47           | 0   | 1   |  |  |
| Mining and energy  | 0.02  | 0.13          | 0   | 1   | 0.01  | 0.12           | 0   | 1   |  |  |

Source: Mexican National Occupations and Employment Survey (2017-2019). Total observations 71, 150.

|                              | Log of hourly wage |
|------------------------------|--------------------|
|                              | (1)                |
| Time                         | 0.014 ***          |
|                              | (0.005)            |
| Trastmant offacts            | 0.000***           |
| freatment effects            | -0.029             |
|                              | (0.005)            |
| Working in the public sector | 0.349***           |
|                              | (0.012)            |
| Years of schooling           | 0.064***           |
| lears of sensoring           | (0.001)            |
|                              | ()                 |
| Age                          | 0.019***           |
|                              | (0.001)            |
| Age square                   | -0.000***          |
|                              | (0.000)            |
| Urban reside                 | 0.047***           |
|                              | (0.010)            |
| Head of household            | 0.089***           |
|                              | (0.005)            |
|                              |                    |
| Economic sector              |                    |
| Construction                 | 0.189***           |
|                              | (0.012)            |
| Commoreo                     | Ó 100***           |
| commerce                     | -0.128             |
|                              | (0.000)            |
| Services                     | -0.035***          |
|                              | (0.008)            |
| Agriculture                  | -0.020             |
|                              | (0.017)            |
| Mining and energy            | 0 173***           |
| Winning and chergy           | (0.019)            |
|                              |                    |
| Marital status               |                    |
| Married                      | 0.007              |
|                              | (0.010)            |
| et                           | 0.010              |
| SuBie                        | -0.018             |
|                              | (0.010)            |
| Constant                     | 2.285***           |
|                              | (0.027)            |
| Obs.                         | 85.867             |

Table B4. 3: OLS estimates of the log of hourly wages

The sample is constructed from the 2017-2019 Mexican National Occupations and Employment Survey. Municipality Fixed effects. Robust standard errors adjusted for 645 clusters at municipality level in parentheses.

|                              | (1)<br>RIF 10 | (2)<br>RIE 25 | (3)<br>RIE 50 | (4)<br>RIF 75 | (5)<br>RIF 90 |
|------------------------------|---------------|---------------|---------------|---------------|---------------|
| Time                         | 0.036***      | 0.013**       | 0.023***      | -0.006        | -0.011        |
|                              | (0.008)       | (0.006)       | (0.006)       | (0.009)       | (0.009)       |
| Treatment effects            | -0.043***     | -0.015        | -0.050***     | 0.022         | -0.060        |
|                              | (0.012)       | (0.009)       | (0.011)       | (0.019)       | (0.034)       |
| Working in the public sector | 0.256***      | 0.237***      | 0.324***      | 0.474***      | 0.553***      |
|                              | (0.017)       | (0.012)       | (0.012)       | (0.015)       | (0.032)       |
| Years of schooling           | 0.028***      | 0.034***      | 0.053***      | 0.093***      | 0.118***      |
|                              | (0.002)       | (0.002)       | (0.001)       | (0.002)       | (0.004)       |
| Age                          | 0.017***      | 0.024***      | 0.030***      | 0.026***      | 0.006         |
|                              | (0.002)       | (0.002)       | (0.002)       | (0.002)       | (0.003)       |
| Age square                   | -0.000***     | -0.000***     | -0.000***     | -0.000***     | 0.000**       |
|                              | (0.000)       | (0.000)       | (0.000)       | (0.000)       | (0.000)       |
| Head of household            | 0.054***      | 0.071***      | 0.089***      | 0.107***      | 0.122***      |
|                              | (0.007)       | (0.006)       | (0.006)       | (0.008)       | (0.013)       |
| Urban reside                 | 0.023         | 0.042***      | 0.054***      | 0.056***      | 0.063***      |
|                              | (0.015)       | (0.014)       | (0.015)       | (0.016)       | (0.019)       |
| Economic sector              |               |               |               |               |               |
| Construction                 | 0.114***      | 0.169***      | 0.242***      | 0.270***      | 0.142***      |
|                              | (0.012)       | (0.012)       | (0.015)       | (0.024)       | (0.024)       |
| Commerce                     | -0.175***     | -0.164***     | -0.119***     | -0.100***     | -0.107***     |
|                              | (0.013)       | (0.010)       | (0.012)       | (0.013)       | (0.013)       |
| Services                     | -0.156***     | -0.086***     | -0.001        | 0.034***      | 0.010         |
|                              | (0.011)       | (0.008)       | (0.011)       | (0.012)       | (0.016)       |
| Agriculture                  | -0.091***     | -0.134***     | -0.071***     | 0.063***      | 0.100***      |
|                              | (0.026)       | (0.028)       | (0.023)       | (0.020)       | (0.028)       |
| Mining and energy            | -0.042**      | 0.049***      | 0.164***      | 0.315***      | 0.382***      |
|                              | (0.017)       | (0.017)       | (0.020)       | (0.033)       | (0.066)       |
| Marital status               |               |               |               |               |               |
| Married                      | 0.018         | 0.018*        | 0.011         | 0.000         | 0.014         |
|                              | (0.013)       | (0.010)       | (0.010)       | (0.017)       | (0.030)       |
| Single                       | -0.020        | -0.015        | -0.017*       | -0.007        | -0.021        |
|                              | (0.013)       | (0.011)       | (0.010)       | (0.016)       | (0.028)       |
| Constant                     | 2.315***      | 2.304***      | 2.132***      | 2.013***      | 2.481***      |
| constant                     |               |               |               |               |               |
| constant                     | (0.050)       | (0.042)       | (0.033)       | (0.048)       | (0.081)       |

Table B4. 4: Unconditional quantile estimates for selected percentiles

The sample is constructed from the 2017-2019 Mexican National Occupations and Employment Survey. Municipality Fixed effects. Robust standard errors adjusted for 645 clusters at municipality level in parentheses.

|                           | (1)                | (2)                           | (3)                 | (4)               | (5)               | (6)              | (7)              |
|---------------------------|--------------------|-------------------------------|---------------------|-------------------|-------------------|------------------|------------------|
|                           | Log hourly<br>wage | Mean-FE<br>Log hourly<br>wage | RIF 10              | RIF 25            | RIF 50            | RIF 75           | RIF 90           |
| Treatment effects         | -0.020<br>(0.039)  | -0.021<br>(0.027)             | -0.092**<br>(0.037) | -0.021<br>(0.027) | -0.013<br>(0.029) | 0.077<br>(0.054) | 0.066<br>(0.113) |
| Obs.                      | 6720               | 6720                          | 6720                | 6720              | 6720              | 6720             | 6720             |
| Covariates                | Yes                | Yes                           | Yes                 | Yes               | Yes               | Yes              | Yes              |
| Survey Round Fixed Effect | No                 | Yes                           | Yes                 | Yes               | Yes               | Yes              | Yes              |
| Individua Fixed Effects   | No                 | Yes                           | Yes                 | Yes               | Yes               | Yes              | Yes              |

Table B4. 5: Individual fixed effects estimates for the public sector pay gap

The sample is constructed from the 2017-2019 Mexican National Occupations and Employment Survey. Municipality Fixed effects. Robust standard errors adjusted at municipality level in parentheses.

Model (1) OLS standard estimation. Model (2) Mean Fixed Effects standard estimation.



Figure C2. 1: Average hourly wages by economic sector

Source: Mexican National Occupations and Employment Survey (2009-2017).



Figure C2. 2: Discontinuity plots of schooling and hourly wages, polynomial order 1

Figure C2. 3: Discontinuity plots of informal and formal employment, polynomial order 1





Figure C2. 4: Discontinuity plots of self-employed, polynomial order 1





Figure C3. 1: The growth rate of daily Minimum Wages 2005-2018



*Source*: Mexican National Occupations and Employment Survey (2005-2018). Real values 2010 Mexican pesos



Figure C3. 2: Distribution of log hourly wage for the Miscellaneous group

Source: Mexican National Occupations and Employment Survey (2010-2015).





*Source*: Mexican National Occupations and Employment Survey (2010-2015). The vertical line represents the time of the intervention.





*Source*: Mexican National Occupations and Employment Survey (2010-2015). Real values in 2010 Mexican pesos.



*Source*: Mexican National Occupations and Employment Survey (2017-2019). Real values in 2010 Mexican pesos.

Figure C4. 2: Panel data wage distributions



*Source*: Mexican National Occupations and Employment Survey (2017-2019). Real values in 2010 Mexican pesos.

Figure C4. 1: Cohorts wage distributions



Figure C4. 3: Public sector pay gap with individual fixed effects in the lower half of the wage distribution

Figure C4. 4: Public sector pay gap with individual fixed effects in the upper half of the wage distribution





Figure C4. 5: Public sector pay gap with a placebo test in the lower half of the wage distribution

Figure C4. 6: Public sector pay gap with a placebo test in the upper half of the wage distribution

