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

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

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Abstract

This study estimates the impacts on selected labour market outcomes of the 1993 educational reform in Mexico, which subjected the school-going population to compulsory schooling until the age of 15. The impact of the education attendance policy is analysed through a fuzzy Regression Discontinuity Design (RDD) approach for the period 2009 to 2017.

The methodology addresses endogeneity between schooling and labour market outcomes by exploiting the age cohort discontinuities in months of birth compared to most of the literature, which uses the year of birth. The analysis contributes to the limited literature on returns to compulsory schooling in developed and developing countries.

The empirical evidence suggests that the 1993 compulsory schooling law, although raising average school attendance, was an insufficient policy to exert impacts on labour market earnings in Mexico during the period analysed. The reform also did not translate into causal effects on individual-level employment decisions (i.e., self-employment, formal and informal sectors). Probably a broader collection of educational policies with a common objective is more likely to have the desired impact on earnings and other labour market outcomes.

Keywords: Compulsory Schooling, Fuzzy Regression Discontinuity Design, Labour Market, Mexico.

JEL classification: I21; I28; J24; J31

1. Introduction

The study of returns to education is one of the most important motivations for the economic analyses of education policies. The accumulation of human capital through education is widely perceived to develop the skills of individuals, provide higher qualifications for the labour market, impact labour market prospects, such as the type of employment or the labour sector, and consequently increase their lifetime earnings (e.g., Card, 2001; Duflo, 2001; Oreopoulos, 2006b).

Generally, the literature is based on the empirical returns to schooling rather than schooling policies themselves, mostly focused on developed countries, and some using the traditional Mincerian earnings equation (e.g., Cameron and Taber, 2004; Card, 2001). Although, there are some interesting studies centred on developing economies which use institutional features of the schooling system to tackle down endogeneity issues (e.g., Acemoglu and Angrist, 2001; Card, 1995[1]; Duflo, 2001[2]; Estrada and Gignoux, 2017; Levy and López-Calva, 2016; López-Acevedo, 2001; Patrinos and Psacharopoulos, 2004; Patrinos and Psacharopoulos, 2010). Average returns to human capital are often higher in low -or middle-income economies than in industrialised countries (Card, 2001; Duflo, 2001).

Compulsory education has been introduced worldwide as a policy for achieving long term benefits. Nevertheless, the studies focused on returns to these type of education interventions are limited, most of them centred on high-income economies. The empirical evidence suggests that although these laws increase schooling, the small size effect does not translate into a systematic improvement in the labour market prospects, such as earnigns, of those who are affected by the institutional changes. Most of the estimated returns are reported to be low or even zero compared to the on-the-job training available to workers (Devereux and Hart, 2010; Grenet, 2013; Pischke and Von Wachter, 2008). However, Oreopoulos (2006a) found overall returns for males, on average, between 10% to 14% from compulsory schooling laws in Britain and Northern Ireland, the US, and Canada. Dolton and Sandi (2017) and Grenet (2013) reported more nounce returns to education of 6% for the UK.

[1] Cited in Oreopoulos, 2006b.
[2] The study found economic returns to human capital in a range of 6.8%–10.6% for Indonesia.

The low or zero returns in high-income economies are, on the one hand, because the reforms targeted students much better prepared in the basic academic skills which play a key role in labour market signalling, through on-the-job apprenticeship or training, by the time they reached compulsory schooling age (e.g., Pischke and Von Wachter, 2008 for Germany). On the other hand, the laws did not encourage the students obtain the certificates or vocational diplomas that demonstrate the qualifications achieved through schooling, rather than marginally increasing years of education (e.g., Grenet, 2013 for France).

For developing countries, the evidence reports higher returns to compulsory education than developed economies, where educational attendance increases over time and translates into higher earnings. The estimates range from 6% in Taiwan to 20% in China (Fang et al., 2012; Spohr, 2003).[3] People at the bottom of the schooling distribution acquired higher education, increased their qualifications, improved their employment status, and therefore, received higher salaries (Spohr, 2003; Fang et al., 2012). However, Aydemir and Kirdar (2017) found low returns for Turkey between 2%–2.5%, although not statistically significant for most specifications. The authors argue that higher returns to education are linked to higher schooling levels, and not the schooling levels promoted by the reform.[4]

The heterogeneity in the empirical evidence of the effectiveness of these policies, may potentially be driven by the different contexts and identification strategies used. Most studies use a Regression Discontinuity Design (RDD) approach with different order polynomial of the year of birth to address the endogeneity issues (i.e., cubic or quartic order, such as Devereux and Hart (2010), Oreopoulos (2006a) and Aydemir and Kirdar (2017)). Few studies use months of birth with the fuzzy RDD method for more accurate and robust estimates of the returns to education as it allows more birth cohorts' schooling variation within a year (Dolton and Sandi, 2017; Grenet, 2013). Difference in differences (D-i-D) and Two-Stage Least Squares (2SLS) methods are also employed (Pischke and Von Wachter, 2008; Kamhöfer and Schmitz, 2016; Spohr, 2003; Fang et al., 2012). The studies highlight the sensitivity of the results to different specifications.

[3] Taiwan was a developing country by 1968 and during most of the period analysed by Spohr (2003).
[4] Table B1 in Appendix B summarizes some of the literature studies.

Additionally, some studies examined the impacts on other labour market outcomes aside from the returns to compulsory education such as employment status, workforce participation in the public sector, on health, welfare, and other (Spohr, 2003; Aydemir and Kirdar, 2017; Oreopoulos, 2006b; Pischke and Von Wachter, 2008; Grenet, 2013).

The analysis of compulsory schooling is potentially important for Mexico, which introduced an Educational Reform in 1993. It raised schooling from 12 to 15 years and compelled the population to remain at school regardless of individual-level decisions. The main objectives are human capital accumulation, strengthen the skills and qualifications, and consequently raise earnings of the individuals. 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, future earnings.

This study sheds light on the long-run labour market effects, primarly earnings but as well to what extent the reform influenced employment selection choices in Mexico between 2009 to 2017. Following Aydemir and Kirdar (2017) and Grenet (2013), a Fuzzy Regression Discontinuity Design (RDD) is applied using a non-parametric analysis to address endogeneity between schooling and a set of labour market outcomes. It exploits a measure of age in months of birth as an exogenous source for increasing education. The fuzziness of the methodology also accounts for the imperfect compliance with the policy.

The empirical evidence reveals marginally increases on the years of schooling. Still, the weak size effect was insufficient by itself to impact hourly wages or sectoral employment choices such as self-employment, formal or informal employment. The law might effectively raise earnings by inducing a significant fraction of the population to complete higher levels of education, or by implementing other labour market and educational policies (e.g., training programmes, wage-setting, quality in educational programmes).

The contribution of this research to the literature is in several ways. First, it is among the few studies for developing countries that explore the returns to compulsory schooling. Second, it uses a rigorous RDD approach to address the endogeneity of schooling by exploiting age cohort discontinuities in months of birth for a 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 analysis the returns to compulsory education of the 1993 Reform. It is also among the few ones analysing three years of compulsory schooling compared to the majority of the studies that analyse one year.

The next section presents the institutional context for the 1993 Compulsory Schooling Reform implemented in Mexico. It is followed by the data and the empirical 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. The 1993 Educational Reform in Mexico

The Mexican government introduced compulsory education for long-term school benefits. More educated people would get better jobs and economic opportunities with positive impacts on life-cycle earnings. 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.

The Mexican Constitution specifies that at the age of six, children must be registered into elementary schooling for six years, followed by three years of secondary schooling, and then three years of high-school education before entering to the university level for approximately five years. The students receive their certificates of each academic level after completing satisfactorily the last year of each level. There are not national examinations.[5]

Before 1993, the legislation had established a minimum school-leaving age at 12 years. Generally, the students have attended all the six years of elementary schooling by the age of 12. In July 1993, the Educational Reform extended the compulsory schooling until the age of 15, regardless of the level of education that children would complete up to this age. It primarily compelled the students to complete the three years of secondary schooling. However, it also changed the curricula and updated the study programmes and textbooks for the elementary and secondary schooling levels for providing the qualifications required in a changing labour market.

[5] Mexico employs a numerical system for grading each year of education. 0-5 is insufficient or failed; 6 is sufficient; 7 is average; 8 is good; 9 is very good; and 10 is excellent. The certificate's grade is the average held during the academic level endured.

The reform was not implemented in conjunction with other laws that would have had a bearing on schooling outcomes (e.g., *Progresa* is a social programme introduced in 1997 that assists to provide children access to basic services such as food, health and school).[6] It 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 national financial crisis.

The intervention was implemented nationwide in September 1993 when the academic year started.[7] At the age of 12, by the 1st of September, the children would have been 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 would be enrolled at an early age in the school system due to different circumstances that would be discussed in section 4.[8]

The statistics of school attendance around the time that the compulsory law was implemented were not promising. On the one hand, the Mexican National Urban Employment Survey (ENEU -Spanish acronym) reported by the second quarter of 1992 and 1993, before the introduction of the reform, 47% of urban adolescents (i.e., ages 12-15) enrolled in secondary education. From the adolescents aged 15 years (i.e., age at which they should finish secondary schooling), approximately 33% completed secondary schooling. By 1994 and 1995, after the reform, the enrolment rates were similar to previous years. In 1997 and 1998, the rates slightly increased by 2 percentage points. On the other hand, the World Bank (2016) reported in 1995 a reduction of five percentage points in the rate of adolescents that dropped out of secondary schooling compared to the percentage in 1990 (i.e., from 26% to 21%).

The law appears to bear a marginal effect on attendance and attainment, and it was poorly enforced for the urban youth. Hence, the impacts of this schooling law is of paramount importance in Mexico, not only regarding school attendance but also for exploring the consequences on labour market outcomes.

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[6] *Progresa* was later called *Oportunidades* in 2002.

[7] Nowadays the academic year starts in the middle of August. Although some Constitutional articles in Mexico mention some sanctions, there are no penalties for the parents if their children do not attend school regularly.

[8] 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 in the evenings or on weekends.

3. 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. It is a nationally representative survey, which reports, *inter alia*, age in months, birth states, years of schooling, weekly hours worked, monthly earnings, formal and informal job activities, 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 due to extra bonuses received during December and January (e.g., wage premia, dividends and shares of the company). 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 previous year. This is to avoid repetition throughout the survey periods.

The sample is a pooled cross-sectional dataset of 148,964 observations from 2009 to 2017. It is restricted to male observations because the information reported of earnings and labour market variables are complete and comprehensive than for female observations. [9] It is also limited to full-time workers, which reported between 30 and 90 weekly hours worked during the week of reference to avoid people studying and working while being surveyed since this research analyses the effects of completed education on labour market outcomes. [10]

3.1 Outcome variables

The main outcome variable used is the natural logarithm of hourly earnings. The definition of earnings in the survey refers to a monthly payment received from the main occupation net of all labour taxes and social security contributions.[11] The hours worked are reported weekly

[9] 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 concern would be how much of the estimated effect from the pooled sample is due to education *per se* rather than job selection.
[10] Full-time workers definition in spirit of Campos-Vázquez (2016) and Aydemir and Kirdar (2017).
[11] If the interviewees report weekly payments, INEGI transforms them into monthly earnings by multiplying the former times 4.3. The information of earnings is reported for all the employment sectors.

in the survey and are multiplied by 4.3 to obtain the monthly hours worked. Hence, the hourly wage variable is computed by dividing monthly earnings by monthly hours worked.[12] Then, logarithms are applied to hourly earnings for getting the key-dependent variable.

Formality, informality and self-employment definitions, used also as dependent variables, are identified in the ENOE through 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 provided by the work contract.

Formal employment is mainly related to activities that offer access to social security, medical health services, and employment rights set in the work contract. Informal employment is associated with activities that do not provide access to social security, health services, or a contract with employment rights. Self-employment is performed by individuals that work alone or are associated with others and may have unpaid workers in their businesses.

Summary statistics of the outcome variables are reported in Table 1. The informal sector represents 44% of the sample, and their hourly wages are the lowest among all the observations, whereas the formal sector is 40% and reports the highest earnings. Figure 1 contains the trends for the average log of hourly wages by type of employment. It reveals that the average wages decreased from 2009 to 2014 and increased thereafter, the peak was in 2016 (3.13 log of hourly wages). Self-employed earnings grew rapidly after 2014 compared to the other employment sectors.

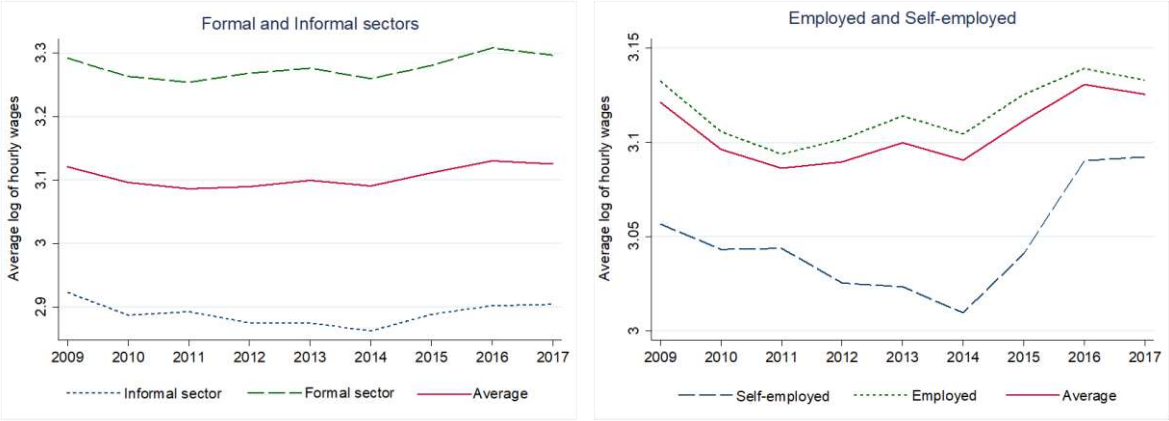
Table 1: Summary statistics of hourly wages

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

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

[12] The interviewees identify the main occupation as the one in which they spend 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%. Hourly wage is measure in real Mexican pesos as of December 2010.

Figure 1: Trends of hourly wages by type of employment



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

3.2 Other variables in the analysis

Table 2 displays the summary statistics of the explanatory variables, which include years of schooling, areas of residence, and the type of economic sector. The average years of schooling is 10.4 which is equivalent to at least 1.4 years of high-school level. The individuals aged between 24 to 40 years when surveyed, the average is 31 years. They were born between 1975 and 1987 and were aged in a range of 6–18 years at the time of the 1993 reform.[13] Figure 2 illustrates that the higher wages are reported for higher schooling levels.[14]

Table 2: Summary statistics of explanatory variables

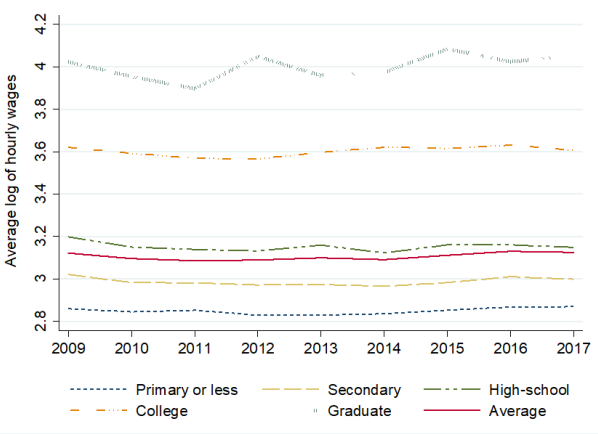
Variable	Mean	Std. Dev.	Min	Max
Age	31.30	4.10	24	40
Urban status	0.60	0.49	0	1
Years of schooling	10.36	4.01	0	24
Economic sector:				
Agriculture	0.09	0.28	0	1
Commerce	0.16	0.37	0	1
Construction	0.15	0.36	0	1
Manufacturing	0.19	0.40	0	1
Services	0.39	0.49	0	1

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

[13] All the observations with missing values for the month of birth are excluded.

[14] People who had an elementary schooling degree or less reported a 0.3% increase in their wages, while people with secondary schooling certificate reported a 0.7% decrease in their earnings. Figure C 1 in Appendix C illustrates the average log of hourly wages by economic sector.

Figure 2: Average hourly wages by academic levels



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

3.3 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 the school before the age of 15 (see Table 3). [15]

The distributions of the log hourly wages and years of schooling per group are illustrated in Figure 3. For the younger cohorts, 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 years of schooling are greater for the treatment group, possibly indicating the policy might have increased schooling. The peaks in years of schooling coincides with the certificates obtained at the last year of each academic level: 6^{th} in elementary schooling, 9^{th} in secondary schooling, 12^{th} for high-school level and undergraduate around the year 17^{th}.

Table 4 reports the summary statistics by academic attainments. The people holding a secondary schooling certificate are approximately 37% of the sample. The 'never-takers',

[15] Tables B2 and B3 in Appendix B present statistics for the cohorts born before and after September 1981.

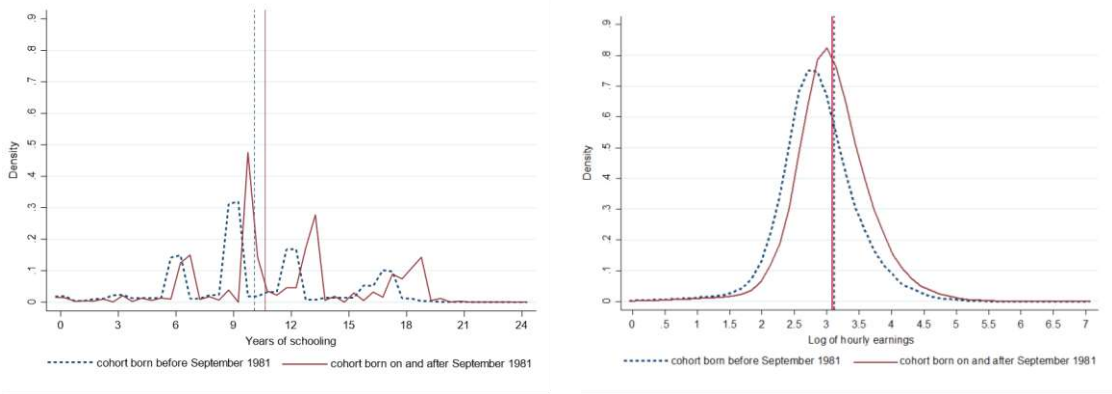
who are bound by the policy but illiterate, represent 1.2% of the sample, while 3.6% of the observations reported incomplete secondary schooling.[16]

Table 3: Birth cohorts by control and treatment groups

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

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

Figure 3: Distribution of years of schooling and hourly earnings by birth cohorts



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

[16] 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.

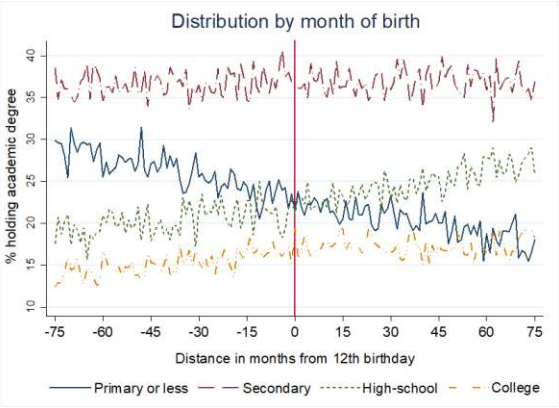
Table 4: Summary statistics by academic attainment

Academic attainment	Treatment group percentage	Control group 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

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

Figure 4 contains the trends of holding academic certificates by months of birth. The cohorts holding secondary schooling degrees are relatively constant and did not change drastically among the treatment and control groups. In contrast, the cohorts holding a primary schooling degree (or less) decreased along with the birth cohorts, whereas higher academic certificates showed upward trends. Thus, the younger cohorts reported further years of schooling.

Figure 4: Trends of academic certificates by months of birth



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

4. Empirical Strategy

The institutional features of the education system can be used as credible instrumental variables for schooling outcomes that can cut through the gordian knot of endogeneity of schooling. There are unobserved variables that could determine jointly earnings, educational attainment and employment choices. [17] There is also concern about reverse causality. Thus,

[17] The potential endogeneity problem has been widely stated in the literature (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.

theoretical underpinnings explicitly recognize the possibility that returns to human capital may vary across the population, depending on such characteristics as family background and abilities (Card, 2001). The use of supply-side variables is a natural feature of standard econometric practice given the well-known 'paradox of identification'.

The 1993 reform in Mexico is a natural experiment, which can be used to estimate the long-run effects of compulsory schooling on the labour market prospects. It creates an exogenous source of variation in educational attendance where the individuals are compelled to remain at school regardless of individual-level decisions. It addresses the endogeneity of schooling by exploiting the age cohort discontinuities measured by the month of birth. Incorporating the within-year variation by months of birth reflects the amount of extra-compulsory schooling that faced the different birth cohorts in the sample (Clark and Royer, 2013[18]).

It was expected that the policy would extend schooling, even though the students could repeat some grades of elementary or secondary schooling level, which naturally increases the years of education 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 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., children working in domestic activities or agriculture). These children should be treated but possibly are not treated. [19]

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

[18] The study uses months of birth as a measure of age for the UK and focuses on health outcomes.
[19] Urban settlement status and birth states are used to control for socio-demographic and economic conditions (e.g., 1994 NAFTA effect on the states close to the border with the U.S.)

the students not targeted by the reform).[20] On the one hand, children that started school earlier than the normal age of six, would have already completed secondary schooling by the time of the introduction of the policy, yet still bound by the reform until the age of 15.

On the other hand, children that started school later than the age of six are bounded by the policy until the age of 15, after which they could either drop out of school without a secondary schooling certificate or stay at school. Thus, the unobserved characteristics affect the exposure to the policy, which creates an imperfect enforcement of the legislation.[21]

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

Therefore, using the fuzzy Regression Discontinuity Design (RDD) approach, the years of schooling and labour market outcomes of the birth cohorts exposed to the reform are compared to those not exposed. The approach is similar in spirit to Grenet (2013) and Aydemir and Kirdar (2017). It employs a running variable, which is the age measured in months of birth from the cohort born in September 1981, to determines the exposure to the 1993 reform (i.e., the treatment). The treatment assignment is a dummy variable equals to one for the birth cohorts born on and after September 1981, and zero for earlier birth cohorts. It is specified as follows:

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

The fuzzy RDD method addresses imperfect compliance by using the random assignment of the instrumental variable (i.e., the treatment dummy). It uses a Two-Stage Least Squares (2SLS) approach to estimate (a) the effects of the exposure to compulsory schooling on years

[20] Children aged 6-12 years are mainly enrolled at elementary schooling, whereas between 12-15 years are studying secondary schooling. Students over the age of 15 generally attend adult literacy schools if they are enrolled at elementary schooling.

[21] The ENOE does not provide information on when the people started school, completed an academic level or dropped out of school.

of schooling; (b) the effects of the exposure to compulsory schooling on labour market outcomes (earnings and employment sectoral choices); and (c) the effects of years of schooling (instrumented by the treatment dummy) on labour market outcomes.[22]

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

$$Years\ of\ Schooling_i = \alpha_0 + \alpha_1(Treatment_i) + \alpha_2 F(Age\ in\ months_i) + \alpha_3 X_i + \varepsilon_i \quad (1)$$

It regresses schooling on the treatment variable ($Treatment_i$), a functional form of the running variable ($Age\ in\ months_i$) re-centred at the cut-off point (threshold), a set of covariates (X_i) and a random error term ε_i. The covariates included are the urban settlement status, the economic sector, the survey year dummies, and birth states dummies clustered by regions.[23] The estimated coefficient, $\widehat{\alpha_1}$, captures the reform's average causal effect on years of schooling at the treatment assignment threshold. It is expected a positive effect indicating that the exposure to the policy increased, on average, years of schooling.

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

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

where $LMkt\ outcomes_i$ represents the following labour market outcomes: 1) $Log\ of\ hourly\ wages_i$; 2) $Formal\ employment_i$; 3) $Informal\ employment_i$; and 4) $Self-Employment_i$. The last three outcomes are implemented as dummy variables. The random error term is ω_i. The estimated coefficient, $\widehat{\beta_1}$, 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:

$$LMkt\ outcomes_i = \delta_0 + \delta_1\left(\widehat{Years\ of\ Schooling_i}\right) + \delta_2 F(Age\ in\ months_i) + \delta_3 X_i + \mu_i \quad (3)$$

[22] Card (2001) suggests that the use of alternative methods to Ordinary Least Squares (OLS), such as instrumental-variables, for estimating the returns to compulsory schooling may reveal higher marginal returns since they provide a better approximation of the exposure to these mandatory laws.

[23] 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. This analysis uses seven birth region dummy variables and nine survey year dummies. See Table B 4 in Appendix B.

It regresses the labour market outcomes ($LMkt\ outcomes_i$) on years of schooling predicted from the first stage $\left(\widehat{Years\ of\ Schooling_i}\right)$, the functional form of the running variable ($Age\ in\ months_i$), the covariate vector X_i, and the random error term μ_i. The estimated coefficient $\widehat{\delta_1}$ is interpreted as a measure of the labour market outcomes for the treatment group exposed to the reform: 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 to indicate that increasing school attendance would translate into causal effects on hourly earnings and it would increase the probability of working in the formal sector. Whereas the signs would be negative for the informal employment sector and working as self-employed, suggesting that as the reform increases schooling, it would decrease the probability of working in these types of employment. More educated people would improve their welfare by working in the formal sector where they receive additional benefits such as social security and health services. The other sectors would rarely provide the workers with such benefits.

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 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 controls 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 ($Age\ in\ months_i$). Following Gelman and Imbens (2017), the analysis uses polynomial order two and avoids higher-order polynomials in the running variable to circumvent assigning higher weights to observations far from the threshold.

The estimation of equation (3) is conducted using a non-parametric method, where the results are not seriously affected by outliers. [24] 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 Eicker-Huber-White (EHW) robust standard error correction is also used in the spirit of Kolesár and Rothe (2018) to generate wider confidence intervals which are more conventional for inferential purposes. 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. It also understates the statistical uncertainty associated with the treatment estimates to a greater degree than the EHW correction leading to an over-rejection of the null hypothesis of no treatment effects.

4.1 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 5 and 6 provide evidence of the discontinuity around the cut-off point in years of schooling, hourly earnings, formal and informal employment. [25]

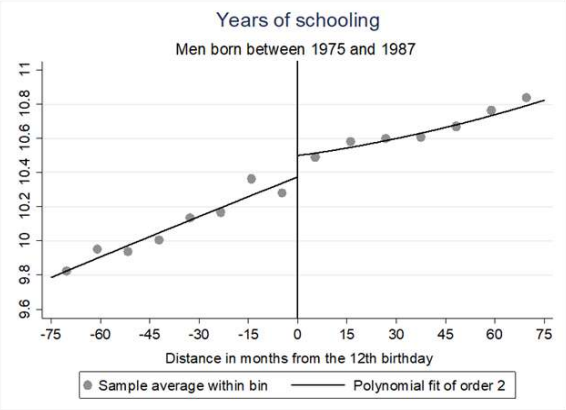
The younger cohorts are located on the left-hand side of each of the plots. Although the downward slope for the treatment group suggests a decrease in earnings in Figure 5, it reflects the earnings of the younger birth cohorts in the sample as they are farther to the left from the discontinuity. The ages are between 28 to 36 years around the threshold.

[24] Parametric analysis is also implemented, some results are presented in the Appendices, others upon request. Both Non-parametric and parametric analyses are performed with the use of the Statistical software for data science (Stata).

[25] Figures C2 and C3 in Appendix C show the graphs using a polynomial order 1. Even using observations enrolled from the first year of the secondary school level to the last year of the high-school level, the discontinuity is visible, although more smoothly (graphs upon request). The plot of the self-employment dummy variable remains with a constant downward curve with no visible discontinuity around the threshold (see Figure C4 in Appendix).

The age reported just below and above the cut-off point are not systematically misreported in the survey (which is the basis for the construction of the running variable). The McCrary test, graphically displayed in Figure C5 in Appendix C, reveals 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. In addition, following Cattaneo et al. (2018) for the density discontinuity based on the local polynomial density estimation, the t-statistic is -1.4 and the associated p-value is 0.16. This indicates 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.

Figure 5: Discontinuity plots years of schooling and log of hourly earnings



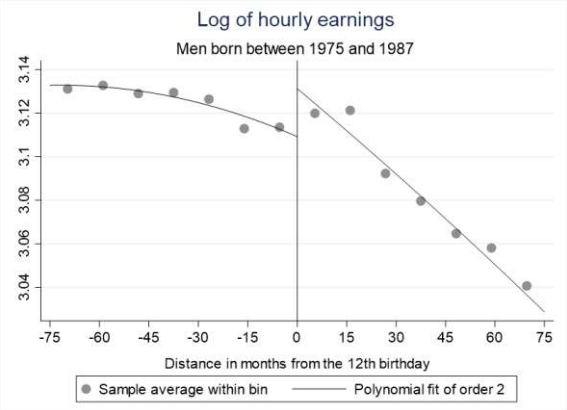
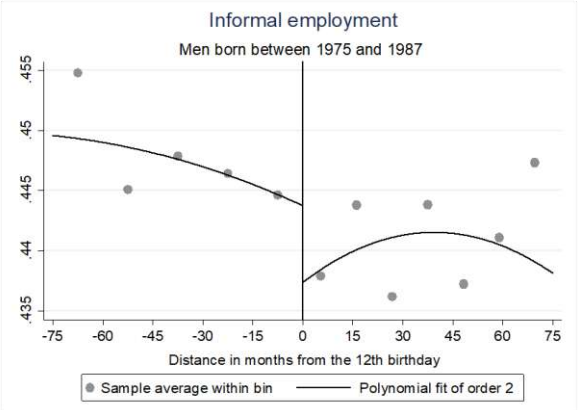
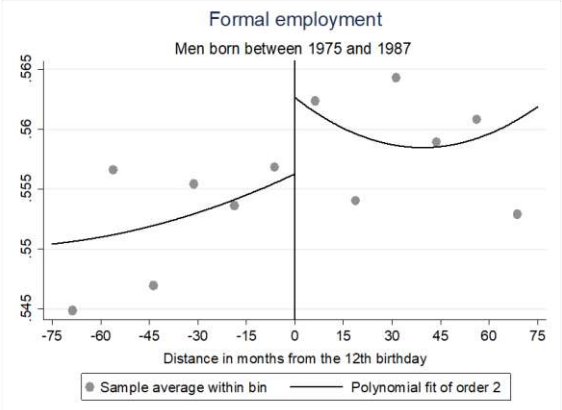


Figure 6: Discontinuity plots of formal and informal employment sectors





5. Empirical Results

The primary sample 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.[26] It trims 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.[27] The 75-months on either side of the threshold correspond to the birth cohorts used in the analysis. [28]

5.1 Returns to Compulsory Education

The standard OLS approach, based on the traditional Mincerian equation, reports an increase in hourly wages, on average, between 5.8–6.3% per additional year of schooling, *ceteris paribus*.[29] These coefficients may be underestimated if there are diminishing marginal returns to education due to over-reported years of schooling or measurement error on earnings. Hence, the naïve estimates are not accounting for the endogeneity of schooling.

The estimated effects of the relevant parameters ($\widehat{\alpha_1}$, $\widehat{\beta_1}$ and $\widehat{\delta_1}$) are reported in Table 5. The parameter $\widehat{\alpha_1}$ is statistically significant and validates the discontinuity on the years of schooling around the threshold outlined in section 4.1. The exposure to compulsory schooling until the age of 15 increased education, on average, within a range of 0.24–0.29 of a year.

The estimates are smaller compared to other developing economies that implemented similar policies. Average schooling increased by more than half of a year in Turkey (Aydemir and Kirdar, 2017) and by 0.4 of a year in Taiwan (Spohr, 2003).[30] Although the effects are similar

[26] Individuals holding master and doctoral degrees represent 1.35% of the sample. As argued by Aydemir and Kirdar (2017), the policy would be unlikely to have spill-over effects far from the high-school level.
[27] 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).
[28] See Appendix A1 for details on the parametric analysis.
[29] See Appendix A2 for the model specification and Table B5 for the results. Table B7 shows the results of the parametric analysis.
[30] The effects are smaller also compared to China and the UK, although their reforms increased mandatory education by one additional year. By 0.8 average years of education in China (Fang et al., 2012) and between 0.36–0.48 of a year in the UK (Devereux and Hart, 2010).

to the ones reported for France by Grenet (2013), in the range of 0.26–0.30 of a year, the reform increased two years of compulsory schooling compared to three years in Mexico.

The estimates for the reduced-form model (2), $\widehat{\beta_1}$, are not statistically significant. Although, Figure 5 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 ($\widehat{\delta_1}$) that do not exhibit statistically significant effects on earnings (see Table 5). However, the estimated magnitudes are in the range of the returns found by Duflo (2001) for Indonesia and are similar to the non-statistically results found by Aydemir and Kirdar (2017) for Turkey.

Table 5: Returns to compulsory schooling

Estimation method		First-stage				Reduced-form				2SLS			
Dependent variable		Years of schooling				Log of hourly wages				Log of hourly wages			
	(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	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.

5.2 Effects on Employment Sectoral Choices

The probability of working in the formal (informal) sector increases (decreases) by 4.4 percentage points, whereas it decreases for working as self-employed by 0.4 percentage points with an additional year of schooling, based on the standard OLS specification.[31] Nonetheless, these estimates do not address endogeneity in education.

The fuzzy RDD approach reports the reform increased average schooling attendance by between 0.25 and 0.32 of a year (see Table 6). However, the reduced-form and the 2SLS estimates do not yield a well-determined causal impact of the schooling policy on the

[31] See Appendix A3 for the model specification and Table B6 for the results. Table B8 shows the results of the parametric analysis.

probability of working in a specific employment sector. Similar results were found by Pischke and Von Wachter (2008) and Grenet (2013).

Therefore, there are no causal impacts on earnings nor on the decisions regarding the employment sector. This result 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).

Table 6: Results of the employment sectoral choices

Formal employment (=1)

Estimation method		First-stage				Reduced-form				2SLS			
Dependent variable		Years of schooling				Formal employment				Formal employment			
	(a)	(b)	(c)	(d)	(a)	(b)	(c)	(d)	(a)	(b)	(c)	(d)
Treatment	0.317**	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)				
$\widehat{\text{Years of schooling}}$									0.070	0.071	0.065	0.060
									(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

Informal employment (=1)

Estimation method		First-stage				Reduced-form				2SLS			
Dependent variable		Years of schooling				Informal employment				Informal employment			
	(a)	(b)	(c)	(d)	(a)	(b)	(c)	(d)	(a)	(b)	(c)	(d)
Treatment	0.295**	0.290**	0.284**	0.245*	-0.021	-0.022	-0.018	-0.015				
	(0.140)	(0.142)	(0.134)	(0.131)	(0.017)	(0.018)	(0.016)	(0.017)				
$\widehat{\text{Years of schooling}}$									-0.073	-0.077	-0.065	-0.060
									(0.061)	(0.063)	(0.060)	(0.071)
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

Self-employment (=1)

Estimation method		First-stage				Reduced-form				2SLS			
Dependent variable		Years of schooling				Self-employment				Self-employment			
	(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)				
$\widehat{\text{Years of schooling}}$									-0.002	-0.003	-0.002	-0.000
									(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

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.

The education institutions in Mexico after the 1993 reform were not prepared for the effect that kept the students at school. Consequently, for younger cohorts the skills acquired through schooling could have been of lower quality, on average, either because the secondary and

new post-secondary 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.[32]

This hypothesis is strengthened by Aydemir and Kirdar (2017) whom suggest that the low returns to compulsory education in Turkey may reflect the low quality of education that prevailed before the reform as these types of policies could induce only more school attendance, and pointed out the work experience as a potential confounder. However, there is a debate in the literature between higher returns to human capital with low schooling quality (e.g., Duflo, 2001) and higher quality of schooling with quite low returns to compulsory education (e.g., Devereux and Hart, 2010 and Pischke and Von Wachter, 2008).

Regarding job experience, in some developing countries employment occurs around the age of 15 (the age targeted by the 1993 reform), this work experience could be rewarded in the labour market. Therefore, the individuals exposed to the reform could have increased their schooling, reduced their work experience at a given age and their earnings later in life. The incidence of child labour in Mexico increased from 6.6% in 1990 to 7.9% in 2000 as reported by the population census. [33] However, the ENOE does not report the work-starting age nor the first employment that could shed light on this potential confounder.

Another possible confounder pointed out by Campos-Vázquez et al. (2016) is the informality in developing countries, exceptionally in Latin America region. Thus, earnings could be misreported in the survey because informal enterprise owners may not keep accurate earning records, they may not report the official number of workers or their salary payments. Further research is needed for disentangling the possible effects of confounders.

6. Robustness Checks

To tests the stability of previous results, different variations of the sample and the use of control variables are employed, as well as a placebo test in time. The robustness checks reinforce the results for the core analysis of a marginal improvement in school attendance, due to the educational policy, that does not impact labour market outcomes.

[32] There may also be variability in schooling due to undernousished or inadequate suppliers.
[33] In 1962 the Labour Law established a minimum legal working age of 14 years. In 2014, it raised it to 15 years, the age targeted by the reform in 1993. By 2016, the minimum legal age increased to 16 years.

6.1 Returns to Compulsory Education

Using the pooled sample that includes 148,964 observations without trimming the wages and not restricted to individuals with less than 18 years of schooling, the results are consistent with Table 5, years of education increases marginally, albeit there are no effects on earnings (see Table 7). Table 8 also reports no returns to education using different sub-samples that trimmed the top and bottom 1% of hourly wages and restrict the years of schooling.

Table 7: Returns to compulsory schooling: pooled sample

Estimation method		First-stage				Reduced-form				2SLS			
Dependent variable		Years of schooling				Log of hourly wages				Log of hourly wages			
	(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)				
$\widehat{\text{Years of schooling}}$									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

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 8: Returns to compulsory schooling: variations in schooling and earnings

Estimation method		First-stage					Reduced-form					2SLS					
Dependent variable		Years of schooling					Log of hourly wages					Log of hourly wages					
	(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)						
$\widehat{\text{Years of schooling}}$													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
Bandwidth	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 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

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.

Additional sub-samples are used for different employment sectors in conjunction with the previous specifications. The estimates do not reveal causal impacts on earnings either (see Table B9 and B10 in Appendix B). However, Table 9 reports that the policy boosts wages in

some specifications for the self-employed sub-sample. [34] It might be because the fewer observations in this sub-sample and/or these individuals do not need to demonstrate their school skills to employers if they have their own businesses.

Table 9: Returns to compulsory schooling: self-employed

Estimation method		First-stage						Reduced-form						2SLS					
Dependent variable		Years of schooling						Log of hourly wages						Log of hourly wages					
	(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 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. 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.

6.2 Effects on Employment Sectoral Choices

Table 10 and 11 reports the estimates for the pooled sample and for other sub-samples. The policy did not influence the probability of selecting a specific employment sector as revealed in the coefficients for the 2SLS, although it increased average schooling.

6.3 Placebo Test

The results of a placebo test that uses a false 12-months cut-off before and after the original implementation of the 1993 reform are not statistically significant as expected. This means that there are no other discontinuities on the variables besides the ones generated by the reform. Thus, other birth cohorts are not impacted by the intervention. The results are reported in Tables 12, 13 and 14. [35]

[34] See Table B11 in Appendix B for additional specifications.

[35] 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 also not statistically significant.

Table 10: Results of the employment sectoral choices: pooled sample

Formal employment (=1)

Estimation method		First-stage				Reduced-form				2SLS			
Dependent variable		Years of schooling				Formal employment				Formal employment			
	(a)	(b)	(c)	(d)	(a)	(b)	(c)	(d)	(a)	(b)	(c)	(d)
Treatment	0.292**	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)				
$\widehat{\text{Years of schooling}}$									0.092	0.096	0.087	0.082
									(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

Informal employment (=1)

Estimation method		First-stage				Reduced-form				2SLS			
Dependent variable		Years of schooling				Informal employment				Informal employment			
	(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)				
$\widehat{\text{Years of schooling}}$									-0.091	-0.094	-0.085	-0.084
									(0.059)	(0.060)	(0.063)	(0.079)
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

Self-employment (=1)

Estimation method		First-stage				Reduced-form				2SLS			
Dependent variable		Years of schooling				Self-employment				Self-employment			
	(a)	(b)	(c)	(d)	(a)	(b)	(c)	(d)	(a)	(b)	(c)	(d)
Treatment	0.267**	0.287**	0.278**	0.203*	-0.000	-0.002	-0.001	0.000				
	(0.122)	(0.126)	(0.132)	(0.117)	(0.011)	(0.011)	(0.012)	(0.011)				
$\widehat{\text{Years of schooling}}$									-0.002	-0.005	-0.004	0.000
									(0.044)	(0.044)	(0.047)	(0.057)
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.

Table 11: Results of the employment sectoral choices: variations in schooling and earnings

Formal employment (=1)

Estimation method		First-stage						Reduced-form						2SLS					
Dependent variable		Years of schooling						Formal employment						Formal employment					
	(a)	(b)	(c)	(d)	(e)	(f)	(a)	(b)	(c)	(d)	(e)	(f)	(a)	(b)	(c)	(d)	(e)	(f)
Treatment	0.294**	0.274**	0.229*	0.322**	0.282**	0.259**	0.024	0.021	0.017	0.024	0.022	0.018						
	(0.138)	(0.134)	(0.132)	(0.137)	(0.127)	(0.127)	(0.017)	(0.017)	(0.017)	(0.017)	(0.016)	(0.016)						
$\widehat{\text{Years of schooling}}$													0.082	0.075	0.073	0.075	0.080	0.068
													(0.062)	(0.063)	(0.077)	(0.055)	(0.060)	(0.067)
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	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

Informal employment (=1)

Estimation method		First-stage						Reduced-form						2SLS					
Dependent variable		Years of schooling						Informal employment						Informal employment					
	(a)	(b)	(c)	(d)	(e)	(f)	(a)	(b)	(c)	(d)	(e)	(f)	(a)	(b)	(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						
	(0.141)	(0.135)	(0.134)	(0.140)	(0.127)	(0.132)	(0.017)	(0.016)	(0.017)	(0.017)	(0.015)	(0.016)						
$\widehat{\text{Years of schooling}}$													-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	42,123	37,986	42,117	40,020	40,017	48,296	42,123	37,986	42,117	40,020	40,017	48,296	42,123
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	31.68	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	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

Robust method: Self-employment

Estimation method		First-stage						Reduced-form						2SLS					
Dependent variable		Years of schooling						Self-employment						Self-employment					
	(a)	(b)	(c)	(d)	(e)	(f)	(a)	(b)	(c)	(d)	(e)	(f)	(a)	(b)	(c)	(d)	(e)	(f)
Treatment	0.286**	0.237**	0.229*	0.322**	0.280**	0.247**	-0.003	-0.002	-0.002	0.000	0.000	0.000						
	(0.141)	(0.117)	(0.117)	(0.138)	(0.127)	(0.124)	(0.012)	(0.010)	(0.012)	(0.012)	(0.011)	(0.012)						
$\widehat{\text{Years of schooling}}$													-0.010	-0.008	-0.009	0.002	0.001	0.004
													(0.048)	(0.048)	(0.055)	(0.042)	(0.045)	(0.053)
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
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	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. 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 12: Placebo test. Birth cohorts born 12 months before and after the 1993 cut-off

Birth cohort born September 1980

Estimation method	First-stage				Reduced-form				2SLS			
Dependent variable	Years of schooling				Log of hourly wages				Log of hourly wages			
	(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)				
$\widehat{\text{Years of schooling}}$									0.056	0.002	0.094	0.096
									(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.	39,972	41,374	41,473	42,781	39,972	41,374	41,473	42,781	39,972	41,374	41,473	42,781
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 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 born September 1982

Estimation method	First-stage				Reduced-form				2SLS			
Dependent variable	Years of schooling				Log of hourly wages				Log of hourly wages			
	(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)				
$\widehat{\text{Years of schooling}}$									-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 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

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 13: Placebo test. Birth cohorts born 12 months before the 1993 cut-off point

Birth cohort born September 1980

Formal employment (=1)

Estimation method		First-stage				Reduced-form				2SLS			
Dependent variable		Years of schooling				Formal employment				Formal employment			
	(a)	(b)	(c)	(d)	(a)	(b)	(c)	(d)	(a)	(b)	(c)	(d)
Treatment	0.222	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)				
$\widehat{\text{Years of schooling}}$									0.069	0.097	0.070	0.096
									(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 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

Informal employment (=1)

Estimation method		First-stage				Reduced-form				2SLS			
Dependent variable		Years of schooling				Informal employment				Informal employment			
	(a)	(b)	(c)	(d)	(a)	(b)	(c)	(d)	(a)	(b)	(c)	(d)
Treatment	0.225	0.104	0.197	0.124	-0.016	-0.010	-0.015	-0.015				
	(0.147)	(0.139)	(0.144)	(0.135)	(0.018)	(0.017)	(0.017)	(0.017)				
$\widehat{\text{Years of schooling}}$									-0.071	-0.098	-0.075	-0.118
									(0.081)	(0.159)	(0.083)	(0.144)
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 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

Self-employment (=1)

Estimation method		First-stage				Reduced-form				2SLS			
Dependent variable		Years of schooling				Self-employment				Self-employment			
	(a)	(b)	(c)	(d)	(a)	(b)	(c)	(d)	(a)	(b)	(c)	(d)
Treatment	0.181	0.092	0.170	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)				
$\widehat{\text{Years of schooling}}$									0.009	0.011	0.025	0.021
									(0.073)	(0.134)	(0.072)	(0.106)
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.	50,458	49,693	49,816	49,059	50,458	49,693	49,816	49,059	50,458	49,693	49,816	49,059
Bandwidth	30.39	40.569	31.87	47.16	30.39	40.569	31.87	47.16	30.39	40.569	31.87	47.16
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

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 14: Placebo test. Birth cohorts born 12 months after the 1993 cut-off point

Birth cohort born September 1982

Formal employment (=1)

Estimation method		First-stage				Reduced-form				2SLS			
Dependent variable		Years of schooling				Formal employment				Formal employment			
	(a)	(b)	(c)	(d)	(a)	(b)	(c)	(d)	(a)	(b)	(c)	(d)
Treatment	-0.026	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)				
$\widehat{\text{Years of schooling}}$									-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 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

Informal employment (=1)

Estimation method		First-stage				Reduced-form				2SLS			
Dependent variable		Years of schooling				Informal employment				Informal employment			
	(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)				
$\widehat{\text{Years of schooling}}$									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
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

Self-employment (=1)

Estimation method		First-stage				Reduced-form				2SLS			
Dependent variable		Years of schooling				Self-employment				Self-employment			
	(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)				
$\widehat{\text{Years of schooling}}$									-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 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

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.

7. Effects of Holding Schooling Certificates

Alternative explanations for the not statistically significant returns of compulsory education exhibited in the literature by Grenet (2013) are the repetition in grades within the academic level that do not translate into higher attainment. Then, compulsory schooling laws could be effective to the extent that they encourage the population to achieve academic certificates, which may signal to employers.

Unfortunately, the ENOE does not collect repetition in school grades, the school-leaving age or the grade-completed age. It only reports the highest completed academic level. Then, this section examines the effects of the policy on the academic certificate achieved, which eventually would provide further insights on the returns to compulsory schooling.[36]

The results in Table 15, from separate regressions, report that the probability of holding (not holding) a college-level certificate (schooling qualifications) increased (decreased) with the implementation of the reform, on average, by two percentage points. These estimates reveal a small improvement in holding academic certificates induced by the 1993 Reform. Perhaps, the intervention focused on keeping children at school instead of compelling them to obtain their secondary schooling certificates.

Table 15: Reduced-form for holding the highest certificate

	Holding the highest academic degree					
	No certificates (=1)	Primary schooling (=1)	Secondary schooling (=1)	High-school (=1)	College (=1)	Graduate (=1)
Treatment	-0.021***	0.008	-0.019	0.015	0.019*	-0.000
	(0.007)	(0.009)	(0.014)	(0.012)	(0.011)	(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

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.

In Mexico, there are not national examinations for measuring the qualifications, only the average score held during the period each academic level endures. Then, signalling in the

[36] As an alternative measure for the years of schooling without repetitions. See Appendix A4 for the model specification.

labour market also depends on schooling scores, school's reputation, type of schools attended by the students such as public or private, rural or urban settlement types (where quality of education varies, e.g., low quality tends to be a feature of schools in rural areas), among other factors.

Earnings could raise by achieving a higher level of education, beyond secondary schooling. Table 16 reports the findings when testing by individuals holding secondary schooling or high-school academic degrees on labour market earnings.[37] The estimates would provide the average causal effects of the qualification attainments on earnings compared to incomplete grades. However, the results do not reveal a causal effect for the returns to human capital either. [38]

Table 16: Results for holding secondary schooling and high-school certificates

Holding secondary schooling certificate

Estimation method	First-stage				Reduced-form				2SLS			
Dependent variable	Secondary schooling certificate				Log of hourly wages				Log of hourly wages			
	(a)	(b)	(c)	(d)	(a)	(b)	(c)	(d)	(a)	(b)	(c)	(d)
Treatment	-0.018	-0.018	-0.018	-0.017	0.013	0.011	0.008	0.004				
	(0.014)	(0.014)	(0.014)	(0.014)	(0.018)	(0.018)	(0.017)	(0.017)				
$\widehat{\text{Years of schooling}}$									-0.690	-0.615	-0.435	-0.218
									(1.070)	(1.036)	(1.009)	(0.984)
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.	59,454	61,437	63,450	61,437	59,454	61,437	63,450	61,437	59,454	61,437	63,450	61,437
Bandwidth	44.63	47.30	47.93	47.49	44.63	47.30	47.93	47.49	44.63	47.30	47.93	47.49
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

Holding high-school certificate

Estimation method	First-stage				Reduced-form				2SLS			
Dependent variable	High-school certificate				Log of hourly wages				Log of hourly wages			
	(a)	(b)	(c)	(d)	(a)	(b)	(c)	(d)	(a)	(b)	(c)	(d)
Treatment	0.015	0.015	0.014	0.013	0.016	0.014	0.011	0.006				
	(0.012)	(0.012)	(0.012)	(0.012)	(0.019)	(0.018)	(0.018)	(0.017)				
$\widehat{\text{Years of schooling}}$									1.042	0.937	0.761	0.490
									(1.393)	(1.327)	(1.359)	(1.352)
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	55,293	57,333	57,333	53,259	55,293	57,333	57,333	53,259	55,293	57,333	57,333
Bandwidth	43.46	44.53	48.07	48.54	43.46	44.53	48.07	48.54	43.46	44.53	48.07	48.54
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.

Perhaps the intervention would have impact on earnings if it had compelled the achievement of high-school-level certificate. However, the Mexican government established the

[37] See Appendix A5 for the model specification.

[38] An analysis holding at least secondary schooling or high-school degree was tested as well. Similarly, the estimates do not report impacts on hourly earnings.

compulsory schooling for this academic level in 2012. Since the younger birth cohorts would have been less than 25 years old by the time this research was undertaken, this reform is not analysed here.

8. Conclusions

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

The empirical evidence suggests that the 1993 Mexican 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 male observations exposed to the reform compared to those not exposed. Nevertheless, the reform by itself failed to have a measurable impact on labour market earnings or on the likelihood of working in the formal or informal sectors, as well as being self-employed as they did not yield statistically significant effects either. An array of robustness checks supports these findings.

The reults are broadly consistent with the literature for high-income economies, which generally reports that compulsory schooling laws raise schooling but do not improve the labour-market prospects of the individuals exposed to these reforms. Specifically, the findings reported here are similar to those of Aydemir and Kirdar (2017) for Turkey.

Some plausible reasons could explain these evidence for Mexico. The school attendance' statistics reported before and after the policy implementation suggest it 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-leaving age has been raised, it may take a longer period for greater compliance with such 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 supply-side by raising education enrollments of individuals. However, if the labour market demand for skilled workers rises in the same way that the supply does, 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. Then, 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, it is necessary to acknowledge the role of different potential confounding factors that could drive the estimates reported in this analysis, 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 school-leaving age for compulsory education but also for improving the quality of education offered at school including re-designing 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.

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Appendix A. Supplementary Material

A1 The parametric analysis delineates a spline interacted quadratic specification for the running variable and avoids higher-order polynomial. To avoid constraining the effects to be identical on both sides of the threshold,[39] 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. It uses a 42-months window, equivalent to 3.5 years before and after the refom.

[39] Regarding the relationship of the labour market outcomes with the running variable.

A2 Standard OLS-mincerian model for returns to education:

$$Log\ hourly\ wages_i = \sigma_0 + \sigma_1(Years\ of\ schooling_i) + \sigma_2(Age_i) + \sigma_3(Age\ square_i) + \varphi_i$$

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.

A3 Standard OLS-mincerian model for the employment sectoral choices:

$$Labour\ market\ sector_i = \sigma_0 + \sigma_1(Years\ of\ schooling_i) + \sigma_2(Age_i) + \sigma_3(Age\ square_i) + \varphi_i$$

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.

A4 The reduced-form tests if the policy increased the probability of holding an academic certificate:

$$Academic\ certificate_i = \gamma_0 + \gamma_1(Treatment_i) + \gamma_2 F(Age\ in\ months_i) + \gamma_3 X_i + \eta_i$$

where $Academic\ degree_i$ represents the following dummy variables: 1) $No\ certificate_i$, 2) $Primary\ schooling_i$, 3) $Secondary\ schooling_i$, 4) $High-school\ level_i$, 5) $College-level_i$, and 6) $Post-graduate\ level_i$. The explanatory variables, X_i, are the same as in equation (3). The random error term is η_i. The estimated coefficient of the treatment variable, $\widehat{\gamma_1}$, measures the reform's average causal effect of holding a specific academic degree at the treatment assignment threshold.

A5 Model specification for holding a secondary schooling or high-school academic degree:

<u>First stage</u>

$$Academic\ certificate_i = \alpha_0 + \alpha_1(Treatment_i) + \alpha_2 F(Age\ in\ months_i) + \alpha_3 X_i + \varepsilon_i$$

where $Academic\ certificate_i$ stands for: 1) $Secondary\ schooling_i$, and 2) $High-school_i$ as the highest academic degree that the individuals hold.

<u>Reduced-form</u>

$$Log\ hourly\ wages_i = \beta_0 + \beta_1(Treatment_i) + \beta_2 F(Age\ in\ months_i) + \beta_3 X_i + \omega_i$$

<u>2SLS</u>

$$Log\ hourly\ wages_i = \delta_0 + \delta_1\left(\widehat{Academic\ certificate_i}\right) + \delta_2 F(Age\ in\ months_i) + \delta_3 X_i + \mu_i$$

Appendix B. Tables

Table B 1: Summary of some available estimates for the returns to compulsory schooling

Author(s)	Country	Year of the reform	Compulsory schooling	Empirical strategy	Earning Returns
Developing countries					
Aydemir and 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 schooling	IV	overall returns of 20%
Spohr (2003)	Taiwan	1968	from 6 to 9 years of schooling	IV	5.8% for men and 16.7% for women
Developed countries					
Devereux and Hart (2010)	UK	1947	aged from 14 to 15 years	RDD	zero returns for women and 4–7% for men
Dolton and Sandi (2017)	UK	1947-1972	aged from 14 to 15, and 15 to 16 years	RDD	6% returns for men
Grenet (2013)	France and UK	1967 and 1972	aged from 14 to 16, and from 15 to 16 years	RDD	close to zero returns for France and 6–7% for England
Oreopolus (2006)	UK	1947	aged from 14 to 15 years	RDD	overall returns of 15%
Pischke and Von Wächter (2008)	Germany	1947 -1969	from 8th to 9th schooling grade	Diff-in-Diff	zero returns

Table B 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.

Table B 3: Summary statistics for the cohort born on and after September 1981

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

Source: Mexican National Occupations and Employment Survey (2009-2017). Total observations 74,346.

Table B 4: Mexican Socioeconomic regions

State	Region	State	Region	State	Region
Chiapas	1	Colima	4	Aguascalientes	6
Guerrero		Mexico		Coahuila	
Oaxaca		Morelos		Jalisco	
		Nayarit		Nuevo Leon	
Campeche	2	Queretaro			
Hidalgo		Quintana Roo		Distrito Federal	7
Puebla		Sinaloa			
San Luis Potosí		Yucatán			
Tabasco					
Veracruz		Baja California	5		
		Baja California Sur			
Durango	3	Chihuahua			
Guanajuato		Sonora			
Michoacan		Tamaulipas			
Tlaxcala					
Zacatecas					

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.

Table B 5: OLS results of the returns to education

	Log of hourly wage			
	(1)	(2)	(3)	(4)
Years of schooling	0.062***	0.063***	0.061***	0.058***
	(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

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.

Table B 6: OLS results of the employment sectoral choices

	(1)	(2)	(3)	(4)
	Formal employment (=1)			
Years of schooling	0.044***	0.044***	0.044***	0.041***
	(0.000)	(0.000)	(0.000)	(0.000)
	Informal employment (=1)			
Years of schooling	-0.044***	-0.044***	-0.044***	-0.041***
	(0.000)	(0.000)	(0.000)	(0.000)
	Self-employment (=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

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.

Table B 7: Parametric results of the returns to compulsory schooling

Estimation method	First-stage				Reduced-form				2SLS			
Dependent variable	Years of schooling				Log of hourly wages				Log of hourly wages			
	(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)				
$\widehat{\text{Years of schooling}}$									0.110	0.109	0.110	0.106
									(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.

Table B 8: Parametric results of the employment sectoral choices

Formal employment (=1)

Estimation method	First-stage				Reduced-form				2SLS			
Dependent variable	Years of schooling				Formal employment				Formal employment			
	(a)	(b)	(c)	(d)	(a)	(b)	(c)	(d)	(a)	(b)	(c)	(d)
Treatment	0.147*	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)				
$\widehat{\text{Years of schooling}}$									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

Informal employment (=1)

Estimation method	First-stage				Reduced-form				2SLS			
Dependent variable	Years of schooling				Informal employment				Informal employment			
	(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)				
$\widehat{\text{Years of schooling}}$									-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-employment (=1)

Estimation method	First-stage				Reduced-form				2SLS			
Dependent variable	Years of schooling				Self-employment				Self-employment			
	(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)				
$\widehat{\text{Years of schooling}}$									-0.033	-0.034	-0.036	-0.041
									(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

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 B 9: Non-parametric results of the returns to compulsory schooling: formal sector

Estimation method	First-stage						Reduced-form						2SLS					
Dependent variable	Years of schooling						Log of hourly wages						Log of hourly wages					
	(a)	(b)	(c)	(d)	(e)	(f)	(a)	(b)	(c)	(d)	(e)	(f)	(a)	(b)	(c)	(d)	(e)	(f)
Treatment	0.110	0.082	0.140	0.144	0.100	0.130	-0.028	-0.028	-0.027	-0.011	-0.014	-0.011						
	(0.141)	(0.141)	(0.150)	(0.180)	(0.147)	(0.152)	(0.022)	(0.022)	(0.024)	(0.019)	(0.022)	(0.023)						
$\widehat{\text{Years of schooling}}$													-0.254	-0.343	-0.195	-0.080	-0.137	-0.083
													(0.432)	(0.678)	(0.298)	(0.203)	(0.328)	(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.	34,272	33,180	28,450	28,750	29,836	27,537	34,272	33,180	28,450	28,750	29,836	27,537	34,272	33,180	28,450	28,750	29,836	27,537
Bandwidth	48.03	47.53	41.21	41.72	41.71	38.22	48.03	47.53	41.21	41.72	41.71	38.22	48.03	47.53	41.21	41.72	41.71	38.22
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 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						Reduced-form						2SLS					
Dependent variable	Years of schooling						Log of hourly wages						Log of hourly wages					
	(g)	(h)	(i)	(j)	(k)	(l)	(g)	(h)	(i)	(j)	(k)	(l)	(g)	(h)	(i)	(j)	(k)	(l)
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)						
$\widehat{\text{Years of schooling}}$													-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 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

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 B 10: Non-parametric results of the returns to compulsory schooling: informal sector

Estimation method	First-stage						Reduced-form						2SLS					
Dependent variable	Years of schooling						Log of hourly wages						Log of hourly wages					
	(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						
	(0.208)	(0.204)	(0.197)	(0.210)	(0.207)	(0.202)	(0.033)	(0.032)	(0.031)	(0.031)	(0.030)	(0.030)						
$\widehat{\text{Years of schooling}}$													0.423	0.461	0.494	0.222	0.259	0.290
													(0.200)	(0.235)	(0.268)	(0.264)	(0.372)	(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.	28,925	28,925	28,925	15,780	15,780	15,780	28,925	28,925	28,925	15,780	15,780	15,780	28,925	28,925	28,925	15,780	15,780	15,780
Bandwidth	28.88	29.05	29.22	28.90	28.58	29.02	28.88	29.05	29.22	28.90	28.58	29.02	28.88	29.05	29.22	28.90	28.58	29.02
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 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						Reduced-form						2SLS					
Dependent variable	Years of schooling						Log of hourly wages						Log of hourly wages					
	(g)	(h)	(i)	(j)	(k)	(l)	(g)	(h)	(i)	(j)	(k)	(l)	(g)	(h)	(i)	(j)	(k)	(l)
Treatment	0.091	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)						
$\widehat{\text{Years of schooling}}$													0.401	0.428	0.457	0.210	0.260	0.282
													(0.176)	(0.200)	(0.223)	(0.293)	(0.457)	(0.606)
Obs.	65,899	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
Eff. Number of obs.	28,795	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
Bandwidth	28.56	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
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	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. 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.

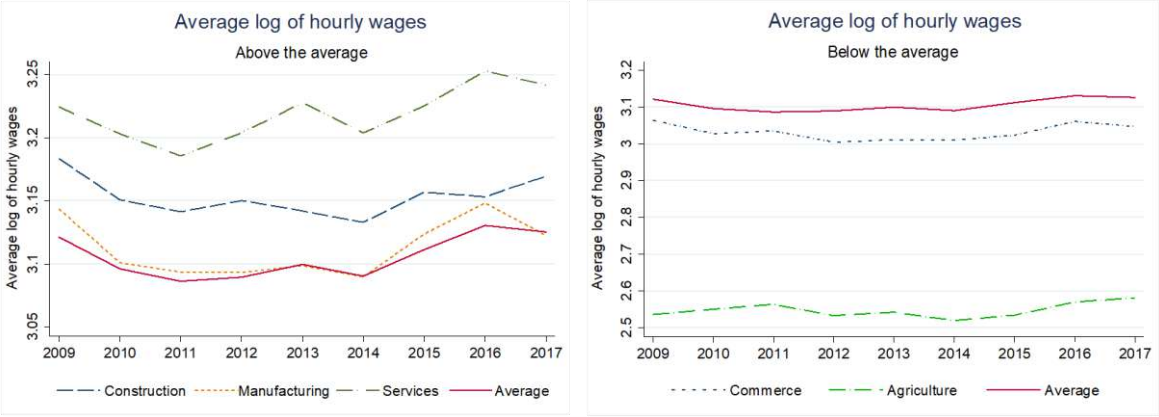
Table B 11: Non-parametric results of the returns to compulsory schooling: self-employment

Estimation method	First-stage						Reduced-form						2SLS					
Dependent variable	Years of schooling						Log of hourly wages						Log of hourly wages					
	(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
Bandwidth	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 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

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.

Appendix C. Figures

Figure C 1: Average hourly wages by economic sector



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

Figure C 2: Discontinuity plots of schooling and hourly wages, polynomial order 1

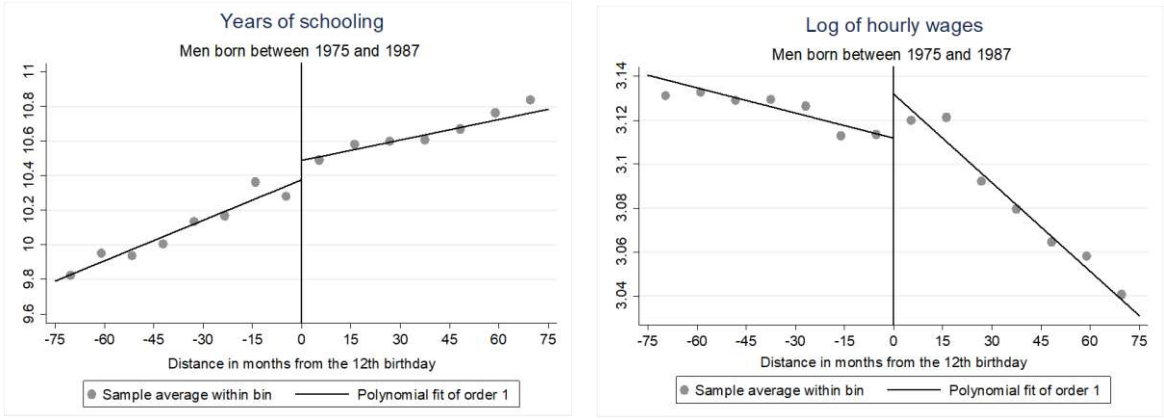


Figure C 3: Discontinuity plots of informal and formal employment, polynomial order 1

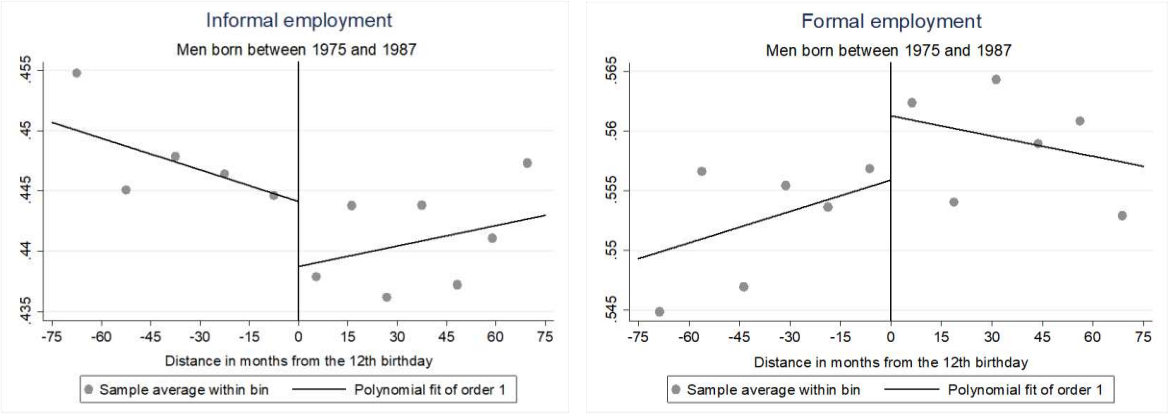


Figure C 4: Discontinuity plots of self-employed, polynomial order 1 and 2

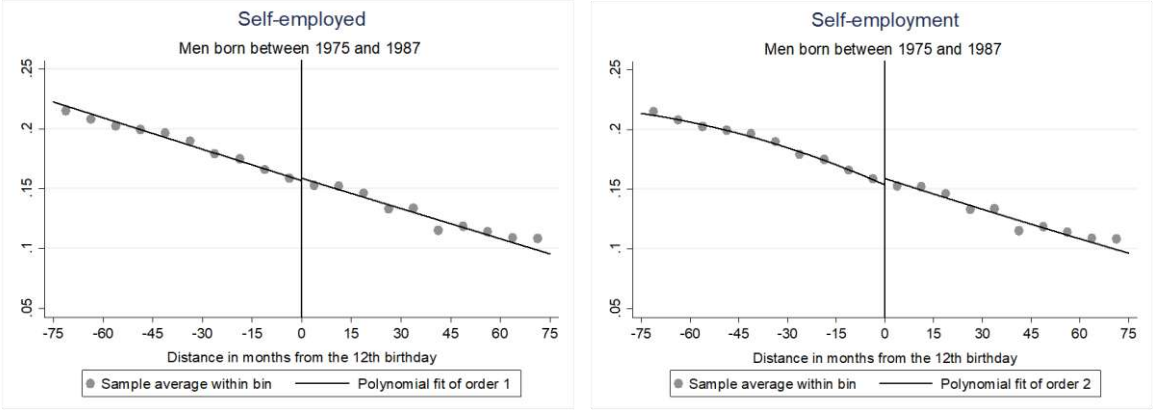
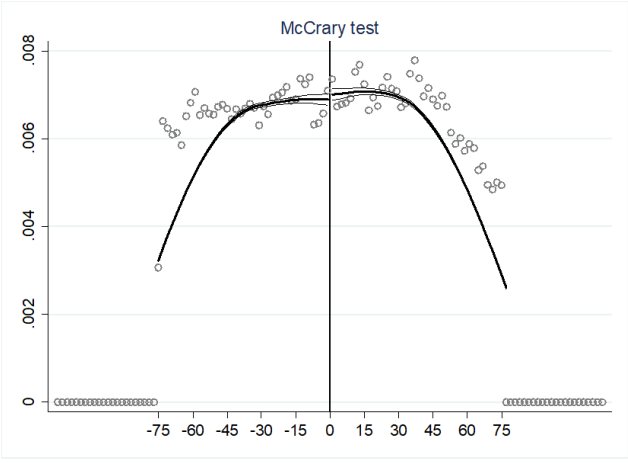


Figure C 5: McCrary test of the running variable



Note: The test uses an optimal bandwidth of 42 months (employed in the parametric analysis)