The effects of a change in the minimum wage on schooling, employment and informality: the case of Mexico

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Este documento es parte de un estudio preparado por el Banco Mundial — *El Salario Mínimo y la Productividad: un Enfoque en el Caso de México* —.Los siete documentos que conforman este estudio, presentado abajo, exploran la relación entre el salario mínimo y la productividad de la empresa, la productividad individual, así como la productividad laboral agregada. Los principales mensajes del estudio están consignados en un documento de resumen. El equipo principal del Banco Mundial fue conformado por la Dra. Wendy Cunningham, Dra. Ximena del Carpio, Dr. Leonardo Iacovone, Dr. Juan Martín Moreno, Lic. Laura Pabón y Dra. Elizaveta Perova, en colaboración con el Lic. Luis Munguia, Lic Juan Diego Trujillo, Lic. Brenda Samaniego, y Lic. Enrique Seira y bajo la orientación de la Dra. Margaret Grosh[1].

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THE EFFECTS OF A CHANGE IN THE MINIMUM WAGE ON SCHOOLING, EMPLOYMENT AND INFORMALITY: THE CASE OF MEXICO

Elizaveta Perova y Juan Diego Trujillo

Although the research of the impacts of changes in minimum wages on the labor market variables, such as employment and informality, and on human capital accumulation has been extensive, up to date it has not led to a consensus neither in theoretical, nor in empirical literature.

In theoretical literature, predictions of the effects on employment and informality point in different directions, depending on the assumptions about characteristics of the market and the size of the change in the minimum wage. Under perfect competition, a minimum wage exceeding marginal productivity, results in unemployment. In a monopsonic model, introduction of or an increase in a minimum wage may trigger increase in employment (Boeri and van Ours, 2013). Increase in employment (along with increase in productivity and decrease in monitoring costs) is also consistent with efficiency wage model (Rebitzer and Taylor, 1995).

Theoretical predictions on human capital accumulation also vary. Some models (Cahuc and Michel, 1996; Cubitt and Heap, 1999) suggest that increase in the minimum wage creates incentives to acquire more education as a countermeasure for an anticipated decrease in labor demand. Other models (Agell and Lommerud, 1997; Ravn and Sorensen, 1999) predict an ambiguous effect that depends on the distribution of talent in the population: while higher ability workers may respond to increase in minimum wage by acquiring higher qualifications to reduce the risk of being laid off, for lower ability workers the costs of acquiring more education may exceed the benefits.

Empirical studies have not generated consistent evidence to assert supremacy of any of the theoretical explanations. The debate in the US on the effects of minimum wage increase on employment has been ongoing for several decades, with some studies finding positive or no effects (Katz and Krueger, 1992; Card and Krueger, 1994), while others significant negative effects (Neumark and Wascher, 2000; Burkhauser, Couch and Wittenburg, 2000). Recent literature focused on the contribution of methodological choices to the disparity in results (Dube et al., 2010; Allegretto et al., 2013). Similarly, empirical studies focused on the impact of changes in minimum wage on schooling[2] also found the entire spectrum of effects: positive (Baker, 2005), none (Card, 1992; Warren and Hamrock, 2010) and negative (Chaplin, Turner and Pape, 2003, Crofton, Anderson and Rawe, 2009).

[2] A range of dependent variables are used to capture schooling: enrollment rates (Baker, 2005; Card, 1992), dropout rates (Crofton, Anderson and Rawe, 2009), completion rates (Warren and Hamrock, 2010), and likelihood to continue to the next grade (Chaplin, Turner and Pape, 2003).

In recent years the study of the effects of minimum wages has been receiving greater attention in developing countries. However, no consensus on the effects on employment has emerged in this literature, and to our knowledge no studies have explored the effects on human capital accumulation in the developing countries. A number of studies find negative and significant impacts of the increase in minimum wages on employment: Grau and Landerretche (2011) in Chile, Maloney and Nunez Mendez (2004) in Colombia, Cespedes (2005) in Peru, Del Caprio et al. (2014) in Vietnam. Cunningham and Siga (2013), Fajnzylber (2001), Neumark et al. (2006) and Lemos (2004) all find evidence of negative impact of minimum wage increase in Brazil; however, their estimates vary in magnitude.

El-Hamidi and Terrell (2001) find evidence of increase in employment in Costa Rica. Yet another set of studies suggest that changes in minimum wage may have opposing effects in different subsets of population within one country. Ginding and Terrell (2009) find positive effects on employment in small firms, and negative in large firms in Honduras. Lemos (2007) estimates positive long-term effects on employment in public sector and negative in private sector in Brazil. Using data from Nicaragua, Alaniz et al. (2011) find evidence on decrease in employment for the private sector workers, whose initial wage is within 20 percent of the minimum wage. In South Africa, Bhorat et al. (2014) estimates positive and negative effects on employment, depending on the sector.

In developing countries, where informality is high, changes in the minimum wage are likely to also affect informality, however, evidence on the magnitude and direction of this effect is not consistent. A number of papers find that increase in minimum wage triggers decrease in informal employment: Fajnzylber (2001) and Lemos (2004) in Brazil, Magruder (2013) in Indonesia, and Campos et al. (2015) in Mexico. Other studies find a positive effect on informal employment: Bird and Manning (2002) and Comola and de Mello (2011) in Indonesia.

This paper builds on several earlier studies of the effects of the minimum wages in Mexico (Bell, 2013; Cunningham and Siga, 2013, Campos et al., 2015), and contributes to this debate through the analysis of impacts on school attendance, which to our knowledge, has not been studied earlier and analysis of heterogeneous effects across several groups: men and women, different age and income groups for both labor market and schooling.

Our paper closely follows Campos et al. (2015) in that we take advantage of a change in the minimum wage legislation in 2012[3], which resulted in differential increase in the minimum wage across Mexican municipalities, to identify the impacts. However, we expand their methodology through first, employing additional treatment variables based on the firm level data and second, limiting the sample to include only municipalities that were affected and those that share a border with affected municipalities.

[3] Specifically, 2 minimum wage zones were merged, resulting in increase of 6.47% in 50 municipalities while in the remaining municipalities minimu m wage increased by 3.75%.

Our results suggest that small increases in minimum wage, similar to the increase in 2012, are likely to have small or no effects on employment of individuals in the lower end of income distribution, no impact on informality, and gender differentiated effects on school attendance. In the following sections, we present empirical strategy and data, followed by discussion of results and conclusions.

Empirical Strategy and Data

To explore the impacts of minimum wage on labor market and human capital accumulation variables we take advantage of the natural experiment. On November 27, 2012 two out of three minimum wage zones in Mexico were merged. Before November 27, 2012 Mexico had three minimum wage zones: zone A with the daily minimum wage equal to 62.33 pesos, zone B with the daily minimum wage of 60.57 pesos, and zone C where daily minimum wage was 59.08 pesos. As a result of the reform, zones A and B were merged, and received zone A minimum wage of 60.57 pesos. Zone C became the new zone B, however, there was no change in minimum wage. On January 1st, there was a 3.75% increase in minimum wage across the country; however, individuals in former zone B saw their wages increase by 6.47% due to the merger with the zone A.

We use these differential increase in the minimum wage across Mexican municipalities and the national occupation and employment survey (*ENOE - Encuesta Nacional de Ocupacion y Empleo)* to identify the impacts of changes in minimum wage on labor market outcomes and human capital accumulation. ENOE is a nationally representative survey, with a rotating panel structure: individuals are interviewed for 5 trimesters before being replaced. *ENOE* data have been collected since 2005. For this study, we will be using two samples: a panel spanning the period from July, 2012 to September, 2013, and a cross section, which includes the first quarter observations for five years: from 2011 to 2015. Both data sets exclude incomplete interviews and are limited to working age individuals, 14 to 65.

ENOE has a high fraction of individuals who do not report income when employed and remunerated. For example, in the panel 36 percent of interviewed individuals did not report income at least in one of the five trimesters, although they were employed, remunerated and reported income in prior or subsequent periods. We consider this not reporting to be an indication of poor data quality and exclude these individuals from the sample. In the cross-sectional data, 10.48 percent of observations are for individuals who do not report income when employed and remunerated. These observations are dropped from the analysis. We also exclude all individuals whose earnings exceeded 99th percentile of the earnings distribution in at least one trimester. As a result, we are left with a balanced panel of 122,466 observations: 24,543 individuals, each of them interviewed for 5 periods. Cross-section includes 1,319,402 observations (approximately 250,000 per year from 2011 to 2015, using only data from the first trimester).

In addition to carrying out the analysis with all the municipalities in Mexico, we employ samples limited to only municipalities in the former zone B, i.e. municipalities which experienced an increase of 6.47% in the minimum wage, and municipalities from other zones which share a border with zone B

municipalities. Municipalities in former zone B which did not share any of its borders with municipalities from other zones are also excluded from these samples (from now on referred to as partial samples).

There are 39 municipalities in the zone B which share a border with 61 municipalities from other zones (5 from A and 56 from C). Another 16 municipalities are surrounded by zone B municipalities only. Out of these 100 neighboring municipalities, only 37 are captured in the ENOE panel: 23 from zone B, and 2 from zone A and 12 from zone C. Zone B municipalities appear more populous compared to municipalities from other zones: they account for 62 percent of municipalities, but 81 percent of individuals in the sample. We will be referring to the sample limited to municipalities which share border as a partial sample. In the panel data, the partial sample includes 11,811 observations (2,369 each trimester). In the cross-section, the partial sample includes 131,745 observations.

We analyze four outcomes: school attendance, monthly earnings, employment and informality. All these outcomes, except for monthly earnings, are dummy variables, equal to one if a respondent is currently attending an educational establishment, is employed and is informal[4], respectively, and zero otherwise. While ENOE provides a rich set of labor market characteristics, other socio-demographic variables are not abundant in this survey. Hence, we are able to control for age, age squared, dummy to denote economically active population, marital status, occupational category, household size, number of individuals under 12 and dummy equal to 1 if the household head is working, and zero otherwise. Summary statistics for these variables in all the four samples used are presented in Table 1.

In addition to the ENOE we employ the IMSS data set[5], which provides us with the universe of all formal employees in Mexico. Based on IMSS, we construct a measure of the magnitude of the shock, created by the increase in the minimum wage at the municipal level: the ratio of workers whose earnings fell between the old and the new minimum wage before the reform (on November 2012) to all formally employed workers. Summary statistics for this measure are also included in Table 1.

We use both panel and cross-sectional samples to identify the impact of the change in the minimum wage on the outcomes of interest in the difference-in-difference framework, with individual and municipal fixed effects, respectively. While identification strategy based on the panel data relies on less stringent identification assumptions, the cross sectional data spans a longer time horizon, thus offering an opportunity to explore longer term effects.

[4] We use INEGI's definition of informality; specifically, an individual qualifies as an informal worker if one of the following two conditions is satisfied: (1) an individual is employed in informal sector either as an independent worker, or an employee; (2) an individual is employed informally, but outside of informal sector, which includes: (a) working as a subsistence farmer; (b) not having social security while working outside of informal sector (for example, in private households, farms or even formal establishments); (c) being an unpaid workers outside of informal sector in both agricultural and non-agricultural activities.

[5] While working with this dataset, all the confidentiality requirements needed to ensure that no economic agent (a worker or a firm) could be identified, were respected.

Additionally, we follow Card (1992) to explore heterogeneity in the magnitude of the shock at the municipal level, captured in the differential fraction of workers likely to be affected by the legislative change, i.e. workers who earned more than the old minimum wage but less than the new one, among all formally employed workers. This fraction ranged from 0 to 63 percent.

All these specifications are described in greater detail below.

Identification based on the individual panel data

To estimate the impacts of minimum wage on school attendance, incomes, employment and informality, based on the panel data we estimate the following regression:

$$y_{itm} = \beta_0 + \beta_1\lambda_t + \beta_2 T_m + \beta_3\lambda_t * T_m + \beta_4 X_{itm} + \xi_i + \theta_t + \epsilon_{itm} \quad (1)$$

where y_{itm} is the outcome of individual i at time t in municipality m. We capture the effect of the change in the minimum wage in the coefficient $\beta_3$on the interaction between two dummies: λ_t and T_m. λ_t takes the value of 1 for periods after November 27, 2012, when the minimum wage increased in zone B by over 3 percentage points, compared to other municipalities in the country, and is zero otherwise. T_m is equal to 1 for the former zone B municipalities, and is zero otherwise. ξ_i and θ_t are individual and quarter fixed effects, respectively, and X_{itm} is a vector of time variant individual controls. . Specifically, X_{itm} includes age at the time of the interview, age squared, number of persons in the household, number of individuals younger than 12 years old living in the household, and a dummy equal to one if a household head is working. Regressions with schooling as a dependent variable, include additional control variables: a dummy equal to one if a respondent is economically active, and a dummy equal to 1 if an individual is a child in the household (as opposed to household head, or spouse of the household head), Regressions with employment as a dependent variable include the number of hours usually worked and dummies denoting family status (household head, spouse of the household head, child or other) as additional regressors. Regressions with informality and hourly income as dependent variables, in addition to the set of regressors in the employment regressions, also include dummies that denote whether a respondent is an employee, employer, or self-employed.

This specification will provide an unbiased estimate of β_3 as long as the change in minimum wage is not correlated with unobservable individual characteristics in the error term, conditional on fixed effects and time variant controls. Formally:

$$Cov\left(\lambda_t T_m, \epsilon_{itm} \middle| X_{itm}, \xi_i, \theta_t\right) = 0 \quad (2)$$

This assumption could be violated if individuals selectively migrated in anticipation of the change in the minimum wage policy or in response to it. However, the change was unexpected and low to warrant such possibility, and at the individual level may be safely treated as an exogenous shock. We also assume mean zero errors.

Table 1: Descriptive Statistics

	Panel				Cross-section			
	Full		Partial		Full		Partial	
	no change	change	no change	change	no change	change	no change	change
Household size	4.56***	-0.17***	4.48***	-0.14*	4.58***	-0.16***	4.42***	-0.02
	(0.01)	(0.04)	(0.08)	(0.09)	(0.0)	(0.01)	(0.02)	(0.02)
Indiviudals under 12 years old	0.93***	-0.09***	0.92***	-0.10**	0.91***	-0.12***	0.89***	-0.11***
	(0.01)	(0.03)	(0.04)	(0.05)	(0.0)	(0.01)	(0.01)	(0.01)
HH head working	0.79***	0.00	0.79***	0.00	0.79***	-0.01***	0.80***	-0.02***
	(0.0)	(0.01)	(0.02)	(0.02)	(0.0)	(0.0)	(0.0)	(0.01)
Household income	7,326.10***	1,614.40***	7,503.13***	1,594.72**	7,060.20***	940.16***	6,996.49***	1,076.09***
	(56.36)	(194.14)	(649.09)	(734.92)	(11.29)	(36.42)	(99.59)	(107.15)
Years of education	8.84***	0.55***	8.79***	0.65***	9.37***	0.77***	8.81***	1.34***
	(0.03)	(0.09)	(0.16)	(0.18)	(0.01)	(0.02)	(0.05)	(0.05)
Female	0.60***	-0.01	0.58***	0.01	0.52***	-0.01***	0.51***	0.00
	(0.0)	(0.01)	(0.02)	(0.02)	(0.0)	(0.0)	(0.01)	(0.01)
Age	34.85***	0.52	34.82***	0.63	34.80***	0.66***	34.98***	0.47**
	(0.1)	(0.35)	(0.67)	(0.76)	(0.02)	(0.07)	(0.17)	(0.18)
Still a kid	0.33***	0.01	0.31***	0.03	0.33***	0.01***	0.31***	0.02***
	(0.0)	(0.01)	(0.02)	(0.02)	(0.0)	(0.0)	(0.01)	(0.01)
Marital status	0.57***	-0.03**	0.58***	-0.05*	0.56***	-0.02***	0.59***	-0.05***
	(0.0)	(0.01)	(0.02)	(0.02)	(0.0)	(0.0)	(0.01)	(0.01)
Economically active population	0.49***	-0.02**	0.49***	-0.02	0.62***	0.02***	0.60***	0.03***
	(0.0)	(0.01)	(0.02)	(0.02)	(0.0)	(0.0)	(0.01)	(0.01)
Ocupational status: Not employed	0.55***	0.03**	0.55***	0.02	0.42***	-0.01***	0.43***	-0.03***
	(0.0)	(0.01)	(0.02)	(0.02)	(0.0)	(0.0)	(0.01)	(0.01)
Ocupational status: Salaried	0.35***	-0.01	0.36***	-0.01	0.05***	0.41***	0.41***	0.05***
	(0.0)	(0.01)	(0.02)	(0.02)	(0.0)	(0.0)	(0.01)	(0.01)
Ocupational status: Employer	0.01***	-0.01**	0.01**	0.00	-0.00	0.03***	0.02***	0.00
	(0.0)	(0.0)	(0.0)	(0.0)	(0.0)	(0.0)	(0.0)	(0.0)
Ocupational status: Self-employed	0.09***	-0.02**	0.08***	-0.01	-0.02***	0.12***	0.11***	-0.01***
	(0.0)	(0.01)	(0.01)	(0.01)	(0.0)	(0.0)	(0.0)	(0.0)
Hours worked	18.60***	-2.06***	17.37***	-0.57	0.18	24.49***	23.28***	1.37***
	(0.16)	(0.55)	(1.01)	(1.14)	(0.12)	(0.04)	(0.29)	(0.31)
Scholar attendance	0.21***	0.02*	0.22***	0.01	0.00**	0.18***	0.16***	0.03***
	(0.0)	(0.01)	(0.02)	(0.02)	(0.0)	(0.0)	(0.0)	(0.0)
Income per hour	10.77***	0.80**	12.44***	-0.53	1.30***	11.85***	12.33***	0.99***
	(0.11)	(0.38)	(0.81)	(0.91)	(0.09)	(0.03)	(0.24)	(0.25)
Employment	0.45***	-0.03**	0.45***	-0.02	0.01***	0.58***	0.57***	0.03***
	(0.0)	(0.01)	(0.02)	(0.02)	(0.0)	(0.0)	(0.01)	(0.01)
Informality	0.27***	-0.05***	0.24***	-0.02	-0.06***	0.33***	0.31***	-0.05***
	(0.0)	(0.01)	(0.02)	(0.02)	(0.0)	(0.0)	(0.01)	(0.01)
Number of observations:	24,543		2,369		531,897		53,031	

The literature in developing countries suggests that the impacts vary across different population groups. A number of studies found significant differences in impacts across gender, with women experiencing either greater reductions in employment, compared to men, or being the only group affected, in most countries studies: Brazil (Fajnzylber, 2001; Cunningham and Siga, 2013), Mexico (Feliciano, 1998; Cunningham and Siga, 2013), Colombia (Arango and Pachon, 2004), Tailand (Del Carpio et al., 2014). However, there are some exceptions: in Chile, Montenegro and Pages (2004) find positive effects on female employment. Other groups that are more likely to bear the brunt of negative impact are low-qualified workers (Del Carpio et al., 2014; Gindling and Terrell, 2005; Gindling and Terrell, 2009) and youth (Majchrowska and Zolkiewski, 2012; Fajnzylber, 2001, Lemos, 2004). Notably, some studies find heterogeneous effects within youth: Pereira (2003) estimates a reduction in employment among youth aged 18 to 19, but an increase among those aged 20 to 25. Identifying such differential effects is particularly important from a policy perspective, as it provides information to design programs and policies for the protection of

vulnerable groups, likely to bear the negative consequences of changes in the minimum wage policy.

We explore differences in the effect of the minimum wage change between men and women, age, education and income groups. We do so by estimating equation (1) with interactions terms added:

$$y_{itm} = \rho_0 + \rho_1 \lambda_t R_i + \rho_2 T_m R_i + \rho_3 R_i \lambda_t T_m + \rho_4 R_{\ i} + \rho_4 X_{itm} + \xi_i + \theta_t + \epsilon_i \quad (3)$$

where R_i is some characteristic along the distribution of which the impacts may vary. Specifically, we use: a dummy equal to one if a respondent is female, a dummy equal to one if a respondent is aged between 14 and 18, and household monthly income. The vector of control variables is the same as in regression (1). As a robustness check, we also re-run regression (1) on subsamples limited to specific values of R_i: men, women, individuals aged between 14 and 18, individuals aged 19 and older, individuals from the households with monthly income below the median, and individuals from the households with monthly income above the median.

Identification based on repeated cross-sections

In addition to the panel of 5 quarters we also attempt to identify the impacts of the change in the minimum wage using cross sectional data which spans 5 years: 2011 through 2015. For each year, we only use the data from the first trimester. We continue to use the difference-in-difference framework, however, are only able to control for time-invariant heterogeneity at the municipal level. Specifically, we estimate:

$$y_{itm} = \beta_0 + \beta_1 \lambda_t + \beta_2 T_m + \beta_3 \lambda_t * T_m + \beta_4 X_{itm} + \mu_m + \theta_t + \epsilon_{itm} \quad (4)$$

Where μ_m denotes municipal fixed effect. While this approach allows for exploring longer term impacts, it requires more restrictive identification assumption.

$$Cov(\lambda_t T_m, \epsilon_{itm} | X_{itm}, \mu_m, \theta_t) = 0 \quad (5)$$

The equation (5) would not hold if the change in the minimum wage policy coincided with the introduction of other programs, potentially correlated with the outcomes but not observed in our data, or if there were significant differences in the dynamics of outcomes of interest between affected and not affected municipalities prior to the change in the minimum wage policy. We will test whether the latter possibility indeed materialized using a placebo test. We run regression (4) on the sample limited to 2011 and 2012, and assigning to λ_t the value of 1 in 2012, and 0 in 2011. The results of this placebo test are presented in Table 2.

Notably, parallel trends assumption does not hold for all the indicators. For school attendance, it is violated in the partial sample, and for employment in the full. Given that the assumption is valid for the majority of outcomes, we do not discard completely the difference in difference specification with municipal variables, but discard the results for school attendance in the partial sample and employment in the full.

We cannot test for the presence of time-variant unobserved variables, potentially correlated with the outcome of interest..

Table 2: Testing parallel trends assumption

	full	partial
School attendance	-0.00	0.03*
	(0.01)	(0.02)
Per hour earnings	-0.40	-0.59
	(0.29)	(0.42)
Employment	0.01*	-0.01
	(0.00)	(0.01)
Informality	-0.00	-0.00
	(0.00)	(0.01)

* denotes 0.1 significance level; ** denotes 0.05 significance level and *** denotes 0.01 significance level

Identification based on repeated cross-sections

Taking advantage of the IMSS data, we explore the municipal level relationship between changes in the outcomes of interest: school attendance, earnings, employment and informality - and the fraction of individuals affected by the change in minimum wages legislation among all formally employed in November, 2012. We define affected by the change as those who received more than the old minimum wage but less than the new one. We merge municipal level IMSS data with ENOE cross-section and estimate:

$$\Delta Y_m = \alpha_0 + \alpha_1 F_m + \alpha_2 \Delta X_m + \varepsilon_m \quad (6)$$

where ΔY_m denotes difference in municipal level outcomes before and after the change in the minimum wage legislation, F_m is the fraction of those who received more than the old minimum wage but less than the new one among all employed in November 2012, and ΔX_m includes municipal level differences in individual and household socio-demographic and economic control variables, also used in regressions (1) and (4).

We use two periods to explore longer and shorter time effects and estimate (6) with $\Delta Y_m = Y_{m2013} - Y_{m2012}$ as well as $\Delta Y_m = Y_{m2014} - Y_{m2012}$. For comparability and consistency, we limit the panel to the third trimester in this estimation.

Results

As discussed in the data section, the ENOE survey and IMSS dataset may suffer from a number of limitations. For example, only a subset of municipalities that experienced a higher change in minimum wage is included into the ENOE sample. Moreover, not trivial fraction of not reported incomes by individuals who work and receive remuneration casts a shadow on overall quality of the data. IMSS data does not include any information on the sizable informal sector in Mexico.

Although we employ several identification strategies, each of them relies on not testable assumptions.

The limitations of the data and identification strategy define our approach to interpreting the regression results. Regression (1) run on the partial sample is our preferred specification. First, it relies on the assumption that, in our view, is more likely to hold compared to the assumptions required for identification using municipal fixed effects regression and regressing differences in outcomes on the fraction of affected among employed (regressions (4) and (6), respectively.

In addition to that we interpret consistency in significance of estimates across several regressions as indicative of detecting an impact; we treat significant results present only in estimation of either regression (4) or regression (6) as an artifact of the data.

Impacts in per hour earnings

We do not find average impact on earnings in either of specifications (Table 3). The results from our preferred specification (individual level panel) suggest that earnings of individuals from households with low income increased. Specifically, in the regression which includes interactions with pre-intervention household income level the coefficient on the treatment is positive and significant, while the coefficient on the interaction is negative and also significant. Simple calculations suggest that the increase in minimum wage positively affected per hour earnings of individuals from the households with pre-intervention income below 10,685 pesos. This threshold exceeds the sample average by approximately 20 percent. If we limit the sample to households with pre-intervention income below the sample median, we also find a significant and positive effect.

Increase in per hour earnings is an expected effect: as long as the minimum wage policy was implemented, we should be automatically observing increase in incomes of the qualifying individuals. We interpret the fact that only individual panel regression (our preferred specification) suggests that the policy increased per hour earnings of low income individuals as indicative of its relative strength compared to other specifications.

Impacts on employment

Estimation of the individual panel regressions and cross-sectional regressions with municipal fixed effects on a partial sample suggest the possibility of a small negative impact on employment. Table 4shows that introduction of approximately 3 percent minimum wage increase is associated with an increase of 2 percent in the probability of becoming unemployed in the affected municipalities. In alignment with earlier literature in developing countries, women and those in the lower half of the income distribution appear disproportionately affected, at least in some regressions with interactions or run on the subsamples of the data corresponding to population subgroups. However, these results are not consistent across specifications.

Table 3: Hourly Income

	Panel		Cross section	
	full	partial	full	partial
No controls	-0.05	-0.57	0.02	-0.29
	(0.27)	(0.73)	(0.35)	(0.68)
Controls	0.06	0.41	0.11	0.21
	(0.21)	(0.50)	(0.28)	(0.67)
	Interactions			
Gender: base effect	0.66	0.74	0.50	-0.01
	(0.40)	(0.96)	(0.40)	(0.82)
Gender: female dummy	-1.01**	-0.57	-0.77**	0.43
	(0.40)	(0.92)	(0.33)	(0.61)
Age: base effect	0.11	0.47	0.20	0.21
	(0.27)	(0.59)	(0.35)	(0.71)
Age: young dummy	-0.29	-0.31	-0.59	-0.01
	(0.35)	(0.68)	(0.63)	(0.76)
Income: base effect	-0.31	1.55**	-	-
	(0.39)	(0.57)	-	-
Income: coefficient on pre-intervention household income	-0.00*	-0.00**	-	-
	(0.00)	(0.00)	-	-
	Group-level observations			
Women	-0.33*	0.20	0.18	0.51
	(0.17)	(0.33)	(0.26)	(0.66)
Men	0.66*	0.82	0.01	-0.15
	(0.40)	(0.94)	(0.34)	(0.75)
Young (14 to 18)	-0.16	0.09	0.02	0.02
	(0.16)	(0.38)	(0.09)	(0.24)
Older (19 to 25)	0.12	0.51	0.09	0.21
	(0.27)	(0.59)	(0.32)	(0.77)
Households with income below median	0.29	1.11**	-	-
	(0.27)	(0.45)	-	-
Households with income above median	0.14	-0.08	-	-
	(0.34)	(0.80)	-	-

* denotes 0.1 significance level; ** denotes 0.05 significance level and *** denotes 0.01 significance level

Table 4: Employment

	Panel		Cross section	
	full	parcial	full	parcial
No controls	-0.00	-0.03**	-0.00	-0.02**
	(0.01)	(0.02)	(0.00)	(0.01)
Controls	-0.01	-0.02**	-0.00	-0.00
	(0.00)	(0.01)	(0.00)	(0.01)
Interactions				
Gender: base effect	0.00	-0.01	0.00	-0.01**
	(0.01)	(0.02)	(0.00)	(0.01)
Gender: female dummy	-0.01	-0.02	-0.00	0.02**
	(0.01)	(0.02)	(0.01)	(0.01)
Age: base effect	-0.01	-0.03**	0.00	0.00
	(0.00)	(0.01)	(0.00)	(0.01)
Age: young dummy	0.00	0.02	-0.02***	-0.02***
	(0.01)	(0.03)	(0.00)	(0.01)
Income: base effect	-0.01*	-0.03**	-	-
	(0.00)	(0.01)	-	-
Income: coefficient on pre-intervention hh income	0.00**	0.00	-	-
	(0.00)	(0.00)	-	-
Group-level observations				
Women	-0.01**	-0.03***	0.00	0.00
	(0.00)	(0.01)	(0.00)	(0.01)
Men	0.00	-0.01	-0.00	-0.01
	(0.01)	(0.02)	(0.00)	(0.01)
Young (14 to 18)	-0.01	-0.01	-0.01**	0.00
	(0.01)	(0.03)	(0.00)	(0.01)
Older (19 to 25)	-0.01	-0.03**	0.00	-0.00
	(0.00)	(0.01)	(0.00)	(0.01)
Households with income below median	-0.00	-0.02	-	-
	(0.01)	(0.01)	-	-
Households with income above median	-0.01	-0.02	-	-
	(0.00)	(0.01)	-	-

* denotes 0.1 significance level; ** denotes 0.05 significance level and *** denotes 0.01 significance level

Impacts on informality

The results from the regressions with informality as a dependent variable highly depend on the specification used. Regressions on individual panel data suggest that changes in minimum wages did not affect informality. When carried out on a sample of youth (under 18), the results suggest reduction of informality, but only with a full sample used. This result also does not hold in the regression with interactions. The regression with municipal fixed effects, however, suggest differential significant effects across several groups: informality appears to increase among men, and decrease among women. Such effects in the opposite directions may account for not significant average effects in the partial sample.

Regressions on subgroups and with interactions also suggest that informality increased among younger people (aged 14 to 18) and decreased in the older group.

Impacts in school attendance

Table 6 shows the effects of the increase in the minimum wage on the likelihood that youth aged 14 to 25 attend an educational establishment at the time of the interview. We do not find average effect in any of the difference-in-difference regression: with panel or cross-sectional data, run on full or on partial sample.

However, once we explore heterogeneous impacts by gender, the data suggest that increase in minimum wage positively affected women. The result is consistent across specifications. In some regressions we also find evidence of negative impact on men; however, the significance of impact is less robust to changes in specification, compared to the result for women.

Table 5: Informality

	Panel		**Cross section**	
	full	partial	full	partial
No controls	-0.00	-0.02	-0.01***	-0.01
	(0.01)	(0.01)	(0.00)	(0.01)
Controls	-0.00	0.01	-0.01***	-0.00
	(0.00)	(0.01)	(0.00)	(0.01)
	Interactions			
Gender: base effect	-0.00	0.01	-0.04***	-0.02
	(0.01)	(0.03)	(0.01)	(0.02)
Gender: female dummy	0.00	-0.00	0.07***	0.03*
	(0.01)	(0.03)	(0.01)	(0.02)
Age: base effect	-0.00	0.01	-0.02***	-0.01
	(0.01)	(0.02)	(0.00)	(0.01)
Age: young dummy	-0.01	-0.01	0.07***	0.03***
	(0.01)	(0.02)	(0.01)	(0.01)
Income: base effect	-0.01	-0.00	-	-
	(0.00)	(0.01)	-	-
Income: coefficient on pre-intervention household income	0.00	0.00*	-	-
	(0.00)	(0.00)	-	-
	Group-level observations			
Women	-0.00	0.01	-0.00	-0.00
	(0.00)	(0.01)	(0.00)	(0.01)
Men	-0.00	0.01	-0.01***	0.00
	(0.01)	(0.03)	(0.01)	(0.01)

Young (14 to 18)	-0.01*	0.01	0.01**	0.01*
	(0.01)	(0.01)	(0.00)	(0.00)
Older (19 to 25)	-0.00	0.01	-0.01***	-0.00
	(0.01)	(0.02)	(0.00)	(0.01)
Households with income below median	-0.01	-0.01	-	-
	(0.00)	(0.01)	-	-
Households with income above median	0.00	0.03	-	-
	(0.01)	(0.02)	-	-

* denotes 0.1 significance level; ** denotes 0.05 significance level and *** denotes 0.01 significance level

Table 6: School Attendance

	Panel		**Cross section**	
	full	partial	full	partial
No controls	0.01	0.02	0.01	0.01
	(0.01)	(0.02)	(0.01)	(0.02)
Controls	0.01	0.01	0.00	-0.01
	(0.01)	(0.01)	(0.00)	(0.01)
	Interactions			
Gender: base effect	-0.02*	-0.01	-0.00	-0.03**
	(0.01)	(0.02)	(0.01)	(0.01)
Gender: female dummy	0.05***	0.04	0.01*	0.03**
	(0.01)	(0.03)	(0.01)	(0.01)
Age: base effect	-0.01	0.02	0.02**	0.00
	(0.01)	(0.03)	(0.01)	(0.01)
Age: young dummy	0.03	-0.02	-0.03**	-0.03
	(0.02)	(0.03)	(0.01)	(0.02)
Income: base effect	0.01	0.03	-	-
	(0.01)	(0.03)	-	-
Income: coefficient on pre-intervention household income	0.00	-0.00	-	-
	(0.00)	(0.00)	-	-
	Regressions by group			
Women	0.04**	0.04*	0.00	-0.00
	(0.01)	(0.02)	(0.01)	(0.01)
Men	-0.02*	-0.01	0.01	-0.03**
	(0.01)	(0.02)	0.00	(0.01)
Young (14 to 18)	0.02**	0.00	0.01	-0.01
	(0.01)	(0.02)	(0.01)	(0.02)
Older (19 to 25)	-0.01	0.02	0.00	-0.02
	(0.01)	(0.03)	(0.01)	(0.01)

Households with income below median	0.02**	0.05	-	-
	(0.01)	(0.03)	-	-
Households with income above median	0.00	-0.01	-	-
	(0.01)	(0.01)	-	-

* denotes 0.1 significance level; ** denotes 0.05 significance level and *** denotes 0.01 significance level

Impacts with fraction of affected as a treatment variable

To further explore robustness of results, we create a panel of municipal level averages and regress changes in outcomes of interest before and after the increase in minimum wage on the fraction of affected individuals and control variables. The results from these regressions are presented in Table 7for the shorter term and longer term impacts.

In the shorter term, this method corroborates the results based on our preferred specification, individual panel regressions: higher fraction of affected workers is associated with an increase in per hour earnings. We also find that greater share of affected workers is associated with increase in informality, contrary to the results based on municipal level fixed effects regression, which relies on more restrictive assumptions.

Notably, in the longer run, when we explore differences between 2012 and 2014 outcomes, none of the coefficients is significant, which may suggest adjustment at the individual and firm level.

Table 7: Impacts of change in minimum wage – regressions with IMSS data

	Change between 2012-2013		**Change between 2012-2014**	
	full	parcial	full	parcial
School attendance	-0.13	-0.43	0.11	-0.36
	(0.30)	(0.31)	(0.18)	(0.26)
Per hour earnings	16.52**	22.95***	1.38	-2.28
	(6.50)	(6.18)	(7.25)	(10.25)
Employment	-0.00	-0.00	-0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)
Informality	0.17**	-0.09	-0.09	-0.23
	(0.09)	(0.20)	(0.12)	(0.27)

* denotes 0.1 significance level; ** denotes 0.05 significance level and *** denotes 0.01 significance level

Conclusions

The task of exploring the impacts of the increase in the minimum wage in labor market and human capital accumulation variables in Mexico presents a formidable challenge. The ENOE and the IMSS provide the data that can be used to identify impacts through several methodological approaches, with relative strength and weaknesses.

Individual fixed effects framework offers, probably, the most convincing identification strategy. IMSS data allows us for a finer definition of the shock: this census of all formal employees allows us to calculate the fraction of workers affected by the intervention among all workers in the formal sector. Difference-in-difference approach with repeated cross-sections offers an advantage of exploring longer term effects, although is the weakest identification strategy.

In interpreting the results from all these approaches, we rank their credibility based on restrictiveness of the assumptions needed for the impacts to be identified, and consider as more reliable results consistent across different specifications. We interpret significant results in one specification only, if it is not the preferred specification, as a potential consequence of a data aberration or a randomly significant result.

With this lens, our paper suggests that the impacts of an increase in the minimum wage around 3 percentage points is likely to have small, if at all existent, impacts on the labor market variables and human capital accumulation. Two specifications suggest that an increase in minimum wage in zone B triggered higher increase in earnings, compared to zones A and C. Individual panel regressions suggest that the impacts were concentrated in the bottom half of the income distribution, as expected.

Other results (employment, informality, school attendance) are less consistent across specifications. Although our preferred specification, based on individual level panel, suggest a decrease in employment, also concentrated in the bottom end of the income distribution, this result is not corroborated by regressions with treatment variables based on IMSS data, or by difference-in-difference with municipal fixed effects.

We do not find significant impacts on informality in the individual panel regressions. The next preferred identification – regression with IMSS data – suggest an increase in the short run, and no impact in the longer run. Contrary to this result, municipal fixed effects regression suggest a decrease in informality. Given these discrepancy, we hesitate to conclude that the change in minimum wage triggered any impact on informality.

The combination of impacts on employment and informality may be interpreted as suggestive of the channels through which the firms absorb an increase in the costs of labor. Depending on the channels, the impact on productivity may vary. Del Carpio and Pabon (2015) outline 5 channels studied in the literature: (1) absorbing costs and accepting lower earnings, (2) increase in prices, reduction of non-salary costs and quality, (3) changes in human resources, (4) increasing informality completely or partially, (5) increase in physical capital, technology and processes. The combination of a low impact on employment and no discernable impact on informality suggests that the firms are

most likely adjusting to the increase in minimum wage through making changes in human resources, primarily, through firing less productive workers; there is no evidence of becoming more informal. Theoretical literature (Cahuc y Michel (1996), Cubitt y Heap (1999), Ravn y Sorensen (1999)) suggest that in the long run such firing of less productive workers may have positive impacts on productivity through generating incentives to increase productivity for individuals, for example, through getting more schooling.

Notably, while none of the three specifications detect any impact on average school attendance, there is evidence of significant heterogeneous results. Increase in minimum wage appears associated with increase in school attendance for girls, and decrease for boys in the individual panel regressions. This result is corroborated by municipal level fixed effect regressions, at least in some of the specifications. Other theoretical studies suggest that the impact of increase in minimum wage on schooling depends on a number of parameters, including how costly it is for the individuals to receive additional education (Agell y Lommerud, 1997). The costs of remaining at school may vary by gender, and given differentiated results for boys and girls, a more profound analysis into the contributing factors to school dropout would be helpful for deciding on the policy options to mitigate effects of minimum wage increase on boys.

Overall, our results suggest that small increases in the minimum wage are likely to trigger small or no changes in the labor market. The most robust evidence, consistent across specifications, suggests increase in per hour wages (at least in the shorter term) and small decrease in employment, likely to be concentrated among the bottom half of the population. The changes in minimum wage may also have gender specific effects on human capital accumulation, with likely negative consequences for boys. In conjunction, our results suggest desirability of policies which complementing the minimum wage reforms with policies aimed at protecting vulnerable groups.

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