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Article

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The effects of a place-based tax cut and minimum wage increase on labor market outcomes

Abstract

The benefits of place-based policies are still under debate. In this study, we analyze what is probably one of the boldest interventions in the recent history of Mexico and the rest of the world: the Northern Border Free Zone (NBFZ). Launched in January 2019, this program doubles the minimum wage and substantially lowers taxes in 43 municipalities along the border with the United States, aiming to improve living standards for low-wage workers and foster economic activity within the region. Given the unique features of the NBFZ, we estimate its short-run effects on labor outcomes: employment, wages, and formality. Our primary identification strategy follows a synthetic control method employing monthly administrative data at the municipality level for the period 2015–2019. Using administrative data for formal employment, we find that the policy substantially increased labor income in the NBFZ by approximately 9% over the control municipalities. The results for employment are less clear. Formal employment showed 1.6% less growth in the NBFZ than in the control municipalities, but the estimate is imprecise and we cannot reject a null impact of the program on employment. These results are robust to alternative control groups, including metropolitan areas in the United States. We also use the labor force survey to estimate the effects on formality at the individual level and find results closer to a null effect. These two results suggest that the NBFZ did not substantially affect employment, and the intersection of confidence intervals for the two estimates implies a maximum loss of employment of approximately 24,000 jobs.

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1 Introduction

Most of the world's countries currently show substantial and increasing disparities in living standards, income, and socioeconomic conditions between different regions, cities, and neighborhoods. Such differences have motivated governments to design “place-based” policies targeting public resources to specific locations rather than specific groups of people, aiming to foster economic performance where it is most needed.¹ However, it is possible that such policies merely shift economic activity from one place to another (Glaeser and Gottlieb, 2008; Kline and Moretti, 2014), so their effectiveness thus far remains unclear.² This study addresses this empirical question by analyzing one of the boldest place-based initiatives in the recent history of Mexico: the Northern Border Free Zone (NBFZ).³ This intervention combines a substantial increase in the minimum wage with a reduction in taxes, with the goal of improving living standards for low-wage workers, fostering economic activity, reestablishing competitiveness, and diminishing crime within the region (Presidencia de la República, 2018).

The newly elected government launched the NBFZ in January 2019, assigning a “Free Zone” status to 43 municipalities along the northern border with the United States. There are two fundamental features of this zone. The first is a 100% increase in the daily minimum wage, from MXN 88.40 in 2018 to MXN 176.20 per day in 2019 (from USD 4.50 to USD 9.00 in PPP), with an increase in the rest of the country of 16% (to MXN 102.68). This measure stands out for its magnitude in the NBFZ and the short period over which it was implemented. Only Latvia and Russia have raised their minimum wages by a similar amount, in 1993 and 2007. The ratio of the minimum wage to average monthly earnings in Mexico was 26% before the intervention, one of the lowest among OECD countries, and just above the ratio for the United States. In early 2019, it had increased to 45%, comparable with Australia (46%) and the United Kingdom (44%).

The second feature of this Free Zone is tax incentives. Firms within the zone can take advantage of a one-third reduction in the corporate income tax (CIT) if they comply with specific requirements. Once approved, their CIT rate is 20%, below the combined federal and state CIT rate in the US South.⁴ The value-added tax (VAT) rate is also lowered from 16% to 8%, comparable with the sales tax in the US South.⁵ In practice, the main incentive is the reduction in VAT, as only 3% of firms qualified for the reduction in the CIT.

This Mexican place-based policy is different from policies that have been implemented elsewhere, such as the US Empowerment Zones or the French Urban Zones, which target distressed areas. The NBFZ is an affluent region in Mexico. Moreover, the US and French programs are restricted to tax incentives to firms, without requirements to increase wages (Busso et al., 2013; Givord et al., 2013). Since the incentives involve different taxes and different

1 Neumark and Simpson (2015) distinguished three types of policies. “People-based policies” are aimed at disadvantaged people independent of their location. “Place-based policies” target a geographical area or region regardless of the profile of people living in it. “Place-based people policies” target specific individuals within a delimited region.

2 Glaeser and Gottlieb (2008) also argued that by spending resources on disadvantaged places, governments encourage people to stay in deprived areas.

3 See Glaeser and Gottlieb (2008) and Kline and Moretti (2014) for theoretical perspectives on place-based policies.

4 The Tax Foundation reports the combined federal and state corporate income tax rates in Texas (21%), New Mexico (25.7%), Arizona (24.9%), California (28%), and an average nationwide rate (25.7%) for 2018. See <https://taxfoundation.org/us-corporate-income-tax-more-competitive/>.

5 The Tax Foundation reports the combined state and average local sales tax rates for Texas (8.19%), New Mexico (7.82%), Arizona (8.37%), and California (8.56%). See <https://taxfoundation.org/sales-tax-rates-2019>.

targets, there is no systematic way to compare them with the NBFZ. It is, however, important to study the effects of the NBFZ on economic activity, employment, and earnings at the bottom of the income distribution.

In this study, we exploit the fact that the NBFZ program offers a nearly ideal setting to estimate the causal implications and determine whether the program is fulfilling its goals. First, the place-based policy targets a specific area. Second, there is an exogenous variation deriving from whether a municipality borders the United States; since these municipalities have neither the healthiest nor the least healthy economies in the country, random policy placement is likely to be achieved. One limitation, however, is that we are not able to separate the effects of the minimum wage increase and the reduction in taxes.

Our identification strategy relies on the synthetic control method (SCM; Abadie and Gardeazabal, 2003; Abadie et al., 2010), using monthly formal employment administrative data aggregated at the municipality level. These data allow us to disaggregate employment by demographic group (age and sex), type of firm (small, medium, or large), and sector at the municipality level. We construct a synthetic control for each subgroup such that trends in the pretreatment period (before January 2019) between the treated municipalities (those on the northern border) and controls are similar. As a robustness check, we reestimate the SCM using metropolitan areas in the United States and estimating employment effects using the quarterly household employment labor survey. Finally, we implement an instrumental variable approach, as in Card (1992), to exploit the exogenous variation derived from the fact that the intervention is effective in specific municipalities to estimate the elasticity in labor demand.

The results are as follows. Using administrative data, we find that this place-based policy resulted in a clear increase in labor incomes, especially at the bottom of the distribution. Average labor income increased by 9% in the treated municipalities than in the controls. We find no substantial evidence of short-run adverse effects on employment outcomes as a result of this policy, and it is centered around an effect of -1.6%. The confidence intervals are wide, ranging from an implied 96,000 jobs lost to an implied gain of 35,000, centered around a loss of 33,000 (of a total 2 million jobs in the border region). The confidence interval of the labor demand elasticity is between -0.5 and 0. Since few firms use the reduction in the CIT, the estimate is driven mainly by the increase in the minimum wage and the reduction in the VAT.

The results are generally heterogeneous across demographic groups and by firm size, but in general they are close to zero and nonsignificant. Using the instrumental variable approach, we find that the policy affects relatively more small firms (fewer than five employees): their average wages increase by 25% and they show a small reduction in employment (elasticity around -0.1), in particular for women (although imprecisely estimated). Using the labor force survey, the results regarding formality are positive (the opposite sign as the administrative data) but close to zero and not statistically significant. Overall, the results using different datasets and methodologies indicate that the policy has close to zero effect on employment and a positive effect on labor income. If both results are correct, the intersection of confidence intervals would imply an impact of -1.2% or less, which implies a net loss of approximately 24,000 jobs in the NBFZ. Data at the establishment level are critical in explaining the channels behind the near-zero effect on formal employment.

This study relates to two types of literature. First, it relates to recent evidence showing that increases in the minimum wage have little or no effect on employment, especially if the minimum wage is low and the increase is relatively small (Belman and Wolfson, 2014). For the United States, Dube et al. (2010) followed the seminal work of Card and Krueger (1994) and compared adjoining counties in different states with different changes in the minimum wage between 1996 and 2006. They found no negative impact of such changes on employment outcomes. More recently, Cengiz et al. (2019) focused on the effect of a minimum wage increase on earnings and employment outcomes of low-wage workers. They found that although earnings increase an average of 7%, there are no adverse effects on employment.⁶ Recent studies for other countries, including Brazil (Saltiel and Urzua, 2018), Chile (Grau et al., 2018), Germany (Ahlfeldt et al., 2018; Caliendo et al., 2018), and the United Kingdom (Hafner et al., 2017; Riley and Bondibene, 2017) also support the notion of zero or small effects of minimum wage increases on employment.⁷

The second type of literature deals with tax changes and economic activity. Most recent influential studies examined historical tax changes in the United States to estimate the effect of tax policies on economic activity, and they found a positive effect of tax cuts on output (Mertens and Ravn, 2013; Romer and Romer, 2010).⁸ The underlying mechanism seems to involve labor market outcomes and consumption. For instance, Zidar (2019) estimated that a state income tax cut of 1% for low-income workers results in a 3.4% point increase in employment over 2 years, with a negligible negative effect on real wages. Suárez-Serrato and Zidar (2016) showed that firm owners bear 40%, and workers 30–35%, of the burden of state corporate tax changes in the United States. Similarly, Fuest et al. (2018) showed that wages decreased significantly following an income tax increase in Germany, and 51% of the corporate tax burden was passed onto workers. Although the NBFZ includes reductions in corporate taxes, in practice they were limited to very few firms. The reduction in VAT was more salient: energy prices dropped immediately in January 2019. However, the literature concerning the implication of VAT cuts on labor outcomes is still limited. This policy seems to have little effect on employment, since the benefits are not necessarily passed onto consumers (Benzarti and Carloni, 2019; Kosonen, 2015).

The remainder of this article is organized as follows. Section 2 briefly describes the evolution of the minimum wage and tax incentives on the northern border. Section 3 describes the data and presents descriptive statistics related to employment and income for the average municipality on the northern border, as compared with the other municipalities. The section also explains the empirical strategy to identify the impact of the place-based policy. Section 4 presents the results. Section 5 discusses potential confounding factors. Finally, Section 6 offers some concluding observations.

⁶ Despite recent evidence suggesting a negligible effect of minimum wage on employment, there is still debate about the magnitude of its elasticity with respect to employment (see Allegretto et al. 2017; Neumark et al. 2014).

⁷ The literature also suggests potential explanations as to why increases in minimum wage levels yield little or no effect on employment. These include improvements in productivity (Mayneris et al. 2018; Riley and Bondibene 2017), reductions in job turnover (Dube et al. 2016), and the translation of higher wages into increased prices or labor-capital substitution (Harasztosi and Lindner 2019; Leung 2018 and the references cited therein). We are not able to test these mechanisms.

⁸ In their baseline specification, Romer and Romer (2010) showed that an exogenous tax increase of 1% of GDP yields a 3% decrease in real GDP. Mertens and Ravn (2013) estimated that a decrease of 1% point in the average personal income tax rate increases per capita GDP by 1.4%, while the same cut in corporate income tax increases per capita GDP by 0.4%.

2 Background

2.1 Minimum wage

The decline, stagnation, and sudden rise in the value of the minimum wage in Mexico can be described in light of the economic events of the last few decades. Between 1980 and 1995, Mexico faced two adverse economic events: the 1982 debt crisis and the 1994–1995 financial crisis (better known as the Tequila Crisis). In an attempt to preserve macroeconomic stability, the government did not peg the minimum wage to the inflation rate; it lost 72% of its value and there was a substantial increase in inequality (Bosch and Manacorda, 2010). Between 1996 and 2012, the real minimum wage was kept relatively constant to avoid inflationary pressures and reflect labor productivity.

From 1987 to 2012, the government divided the country into three economic regions, each with its own minimum wage, and recent efforts to increase it have relied on reducing the number of regions: from three to two in 2012 and one in 2015. The latter reduction meant an increase of approximately 3% in some regions in that year.⁹ Finally, in January 2019, the government increased the daily minimum wage from MXN 88.36 to MXN 176.72 in 43 municipalities along the northern border (from USD 4.5 in PPP to USD 9 in PPP) and to MXN 102.68 in the rest of the country (a 16% increase).

The increase in the minimum wage in Mexico is one of the largest recorded since 1960, according to the OECD databases, which include OECD countries and others such as Costa Rica, Brazil, Russia, and Colombia. Between 1960 and 2018, only three countries raised their minimum wage more than 50% in 1 year (in constant monetary units): New Zealand in 1973 (54%), Latvia in 1993 (103%), and Russia in 2007 (92%).¹⁰ Only two countries in Latin America have substantially increased their real minimum wage over a relatively short period: Uruguay had an increase of 100% from 2004 to 2007 and Honduras had an increase of 70% from 2008 to 2009.¹¹ Other countries that substantially increased their minimum wages over short period are Peru (1996–2000), the Czech Republic (1998–2002), Hungary (2000–2002), Serbia (2003–2008), Haiti (2008–2010), Bulgaria (2011–2016), and Romania (2013–2018).

The increase is also meaningful in terms of the ratio of the monthly minimum wage to the average monthly wage. In 2018, this ratio was 26% for formal workers on the northern border of Mexico. Among OECD countries, only the United States had a lower ratio (24% in 2017). After the minimum wage increase in Mexico, the ratio reached 45% in the border region, similar to that of Australia (46%), Chile (49%), and the United Kingdom (44%), but lower than those of Colombia (58%), France (50%), and New Zealand (52%).¹²

⁹ Using this exogenous variation in the minimum wage by region, Campos-Vazquez et al. (2017) and Bouchot (2018) found no effect on employment outcomes. However, the increase in the minimum wage in these studies is small (less than 5%).

¹⁰ Muravyev and Oshchepkov (2015) found that the increase in the minimum wage in Russia decreased employment, especially among youth. In Mexico, however, the doubling of the minimum wage targets a specific region, which allows for a better identification.

¹¹ Between 2003 and 2005, the real minimum wage doubled in Argentina. However, it is not clear whether the inflation statistics in that period are correct. The OECD and the World Bank do not report inflation data for that period. In Uruguay, the increase in the minimum wage decreased wage inequality, but mainly in the formal sector. For the country as a whole, there was no discernible effect on employment, but excluding Montevideo there was a 10% decrease in employment (Borraz and Gonzalez-Pampillon 2017). In Honduras, Ham (2015, 2018) found that the increase in the minimum wage caused an increase in noncompliance and a decrease in employment in the formal sector. Other Latin American countries have taken longer to double the minimum wage: Bolivia doubled it between 2010 and 2017, Brazil between 2000 and 2013, and Ecuador between 2000 and 2015. See, for example, Saltiel and Urzua (2018) and Wong (2019).

¹² All data for minimum wages in specific countries is obtained from ECLAC (2019), Eurostat (2019), and OECD (2019).

The importance of the minimum wage increase on the northern border is also in the proportion of workers affected. In 2018, about 0.1% of workers enrolled in social security (Instituto Mexicano del Seguro Social [IMSS]) earned the minimum wage. However, about 40% of workers earned between one and two times the minimum wage, the largest earnings bracket in terms of the number of minimum wages.¹³

In December 2019, the government decided to increase the minimum wage 5% in the NBFZ and 20% in the rest of the country for 2020. Given this change, we focus only on the short-run effects of the policy implemented in 2019.

2.2 Tax incentives at the northern border

The VAT reduction at the northern border is not a new incentive. In the late 1970s, the Mexican government introduced the VAT at an initial rate of 10%, and in 1983 it introduced a preferential rate of 6% for consumers within 20 km of the United States, Belize, and Guatemala. In 1995 the VAT increased to 15% and 10% at the border, and in 2010 the rates were increased to 16% and 11%. In 2014, after almost 20 years of differential rates, the government unified the VAT to 16% throughout the country.¹⁴

In January 2019, a presidential decree began to apply tax incentives to businesses and taxpayers with entrepreneurial activity residing in municipalities at the northern border. These incentives, which will remain until 2020, are a one-third discount in the CIT and a reduction in the VAT from 16% to 8%. With these measures, the government intends to restore investment, employment, and competitiveness, and to reduce crime and the high level of violence. Both incentives exclude specific sectors, taxpayers, and income sources: the financial and real estate sectors, *maquiladoras*, taxpayers under audit, income obtained from intangible goods or assets, e-commerce, digital content, salaries, dividends, professional services, and others (Presidencia de la República, 2018).

For businesses to take advantage of the CIT discount, they had to submit a request to the Mexican tax authority (Servicio de Administración Tributaria, SAT) and fulfill specific requirements. Businesses resident in the region for at least 18 months before the request, that generated at least 90% of their income within the region, that were not receiving any other fiscal benefit, and that had no relation with firms issuing false invoices were enrolled in a Registry of Tax Incentives for the Northern Border Region. Businesses initiating operations in the region could also request enrollment in the Registry if they could prove they would make new investments in fixed assets and facilities, and would generate at least 90% of their total income in the region.

In practice, the policy operated mainly through a reduction in the VAT and an increase in the minimum wage. By the end of 2019, among firms that applied, only 3% obtained the CIT reduced rate. In contrast, the reduction in VAT could be seen immediately through lower energy prices from the state-owned oil company Petróleos Mexicanos (PEMEX), which is the primary retailer and distributor of gasoline to private retailers. Data collected for the Consumer

¹³ See Figures A1 and A2 in Supplementary Materials.

¹⁴ Although there have been some attempts to explore the effects of this preferential VAT rate (e.g., Davis 2011; Fuentes Flores et al. 2016), there is, to the best of our knowledge, no substantial evidence linking the preferential rate at the northern border to economic outcomes.

Price Index show that in northern cities, energy prices, including those set by the government, fell by 7% (Campos-Vazquez and Esquivel, 2020). The reduction in energy prices has had an effect on firms' costs in the area.

3 Data, descriptive statistics, and methods

3.1 Data sources

This study uses administrative and household survey data to provide a fuller assessment of how a minimum wage increase coupled with tax incentives affected labor market outcomes. We employ administrative records made public on a monthly basis by the Mexican Institute of Social Security (IMSS, 2015–2019) and survey data from the National Survey on Employment and Occupations (ENOE) published quarterly by the National Institute of Geography and Statistics (INEGI, 2015–2019).

The IMSS keeps records of the number of workers registered for social security and the earnings reported by both employers and the self-employed, as required by law. This database contains mostly employees from the formal sector, whose registration is mandatory.¹⁵ These administrative records provide rich information about the number of workers according to firm size (1, 2–5, 6–50, 51–250 employees and so on), economic sector, sex, age, and other characteristics at the municipality level. These data are published every month on the IMSS website (<http://datos.imss.gob.mx/>). We show results by these characteristics for the period January 2015–December 2019 (balanced panel). As variations in employment in small municipalities can be large and we wish to avoid empty cells for some groups, we restrict the analysis to municipalities in the urban sector with a population of at least 50,000.¹⁶ In the northern border region, there are 18 municipalities meeting this criterion.

The labor force survey (ENOE), similar to the US Current Population Survey, is used in the analysis for robustness checks and to include effects not only on the formal sector, but also on the population as a whole. The sample consists of people aged 15 or older in randomly selected households at the national level, and it is representative at the national and state levels, and for some urban areas. The data are published on a quarterly basis and each household in the sample is interviewed for five consecutive quarters. The ENOE provides information about the main socioeconomic and demographic characteristics of the Mexican labor force, with detailed information concerning earnings, occupation, economic activity, and working conditions.

Both datasets have advantages and disadvantages. The main advantage of the IMSS sample is its extensive coverage, which allows the measurement of formal employment and earnings at the municipality level on a nationwide, monthly basis. However, employers might have an incentive to report fewer employees or lower salaries to evade payment of their social security contributions. If this incentive varies with the economic stimulus package, the labor force survey may find different estimates. Therefore, incorporating the ENOE sample into the analysis provides a robustness check and further support for the results observed with the IMSS sample.

¹⁵ According to the Mexican social security law, there are two types of affiliations: the compulsory regime and the voluntary regime. Employers are required to register employees in the compulsory regime. Workers without formal employment, such as domestic workers and the self-employed, are eligible to register in the voluntary regime.

¹⁶ Although approximately 22% of the Mexican population lives in municipalities with less than 50,000 inhabitants, their share of formal sector employment registered at IMSS is only 5%.

3.2 Descriptive statistics

Table 1 shows the main descriptive statistics using IMSS data for municipalities in the border and non-border regions. There are 18 municipalities (those with at least 50,000 inhabitants) in the treatment group and 395 in the control group. The border region represents close to 11% of total employment. The proportion of men in the formal sector is higher than that of women, but the border region employs relatively more women than the non-border region. Most of the

Table 1 Descriptive statistics using IMSS data (September 2018)

Variables	All	Border region	Non-border region
No. of municipalities	413	18	395
Employment: All	19,035,280	2,050,744	16,984,536
% Employment: Men	62.09	59.85	62.37
% Employment: Women	37.91	40.15	37.63
% Employment: Firm size 1–5	6.77	5.31	6.95
% Employment: Firm size 6–50	20.89	15.96	21.48
% Employment: Firm size 51+	72.34	78.73	71.57
% Employment: Age < 30	33.37	35.83	33.07
% Employment: Age 30–59	63.75	61.91	63.97
% Employment: Age 60+	2.88	2.26	2.95
% Earning < MXN 180 daily	39.83	30.64	40.94
Avg. daily income < MXN 180 daily	USD 121.6	USD 129.4	USD 120.9
Avg. monthly income: All	9,427	10,131	9,365
Avg. monthly income: Men	9,949	11,006	9,856
Avg. monthly income: Women	8,470	8,819	8,440
Avg. monthly income: Firm size 1–5	4,897	5,281	4,863
Avg. monthly income: Firm size 6–50	6,992	7,769	6,924
Avg. monthly income: Firm size 51+	10,890	11,018	10,878
Avg. monthly income: Age < 30	7,116	7,716	7,063
Avg. monthly income: Age 30–59	10,626	11,547	10,545
Avg. monthly income: Age 60+	9,192	9,196	9,191
Avg. population	1,351,532	927,028	1,388,767
% People in poverty	38.1	30.8	38.8
# Social needs (poverty measure)	2.0	1.8	2.0
% People in extreme poverty	5.2	2.3	5.5
% People in income poverty	8.8	10.0	8.7
% People behind in school	15.0	14.3	15.1
% People attending school (15–24 years old)	46.9	44.6	47.1
% People (25+ years old) with at least college degree	15.7	12.5	16.0
% Households receiving remittances	9.5	10.4	9.4
% Women who work (20–65 years old)	44.6	49.0	44.2
% Men who work (20–65)	82.5	83.6	82.4
% Employment in manufacturing (20–65)	16.4	31.2	15.1
% Informal employment (20–65)	45.5	32.2	46.7

Notes: Authors' calculations. Statistics related to employment and earnings are from IMSS data, and statistics on poverty, school attendance, remittances, share in manufacturing, and informality are from the Encuesta Intercensal (2015) and Coneval (2019).

formal sector employment is generated by large firms, especially in the border region. Less than 3% of workers are 60 years of age or older. The proportion of workers earning less than MXN 180 daily is higher in the non-border cities. Approximately 31% earned less than MXN 180 daily in the border municipalities in September: these are the workers directly affected by the new minimum wage. The average wage for workers in the border region is USD 129.4, so the new minimum wage means an average increase of 36%. The average monthly labor income for all workers is MXN 9,427 (approximately USD 1,050 in PPP).¹⁷ The border region has a higher labor income across different groups than the non-border region, and it has also lower levels of poverty (across different measures), a higher proportion of workers employed in the manufacturing sector, and lower levels of informality.

3.3 Empirical strategy

This study focuses on the short-run effect on labor market outcomes of a stimulus package of location-based policies in the Mexican northern border region. Since the policy affected only this region, which includes a specific set of municipalities, it is possible to identify the impact of the intervention. However, it is necessary to compare it with a group of municipalities that have had the same secular trends in the outcome variables, but did not receive the stimulus. To build such a group, we employ the SCM, which relies on a set of predictors, in our case pre-intervention outcomes that are used to assign weights to unaffected municipalities, such that the trends of the weighted average of pre-intervention outcomes best represent the trends of the counterfactual of the treated unit.

Following standard notation (e.g., Abadie et al., 2010; Cavallo et al., 2013), we consider $J + 1$ municipalities. Without a loss of generality, the first unit is a municipality in the northern border region, where the stimulus package was implemented in January 2019. The remaining J municipalities constitute the donor pool, which is the set of municipalities that did not receive any benefit from the stimulus package. Then, the effect of the intervention on the outcome variables for the treated municipality, for example, formal employment, E_{it} , can be written as follows:

$$\alpha_{it} = E_{it}^I - E_{it}^N \quad (1)$$

where E_{it}^N is the outcome that would be observed in the affected municipality in the absence of the intervention and E_{it}^I the outcome in the same region with the minimum wage increase and tax incentives. Since E_{it}^I is observed, to estimate the effect on employment we need to estimate E_{it}^N . The SCM constructs a vector of weights \mathbf{W} over J donor municipalities, such that the weighted combination closely matches the treated municipality in pre-intervention formal employment.

Following Abadie and Gardeazabal (2003), Abadie et al. (2010), and Abadie et al. (2015), we proceed to define the optimization problem to compute the synthetic control. Formal employment (or earnings) is observed for T periods for the Mexican northern border region

¹⁷ In Supplementary Material, we include IMSS data showing that in 2018 most formal sector workers in the country earned between one and two minimum wages, the same income as approximately 40% of all workers; this share increased to 43.8% in 2019. Of this share, only 0.1% earned only one minimum wage in both years (Figure A1). According to the ENOE, in 2018 the proportion of workers earning at most one minimum wage was about 16.1%—3.3% in formal and 25.9% in informal employment.

(or a municipality within a region) $E_{1t'}$ and for the remaining unaffected municipalities E_{jt} for $t = 1, \dots, T$ and $j = 2, \dots, J + 1$. Intervention happens at time $t = t'$ (January 2019). We define a vector of pretreatment characteristics for the treated region (4 years of monthly observations, or up to 48 observations for each characteristic) formed only by pre-intervention outcomes or predictors $\mathbf{X}_1 = (E_{1,t'-48}, \dots, E_{1,t'-1})$. Similarly, we define a matrix $\mathbf{X}_0 = (E_{j,t'-48}, \dots, E_{j,t'-1})$ containing the same pre-intervention outcomes or predictors for the J donor municipalities. Defining a $(J \times 1)$ vector of positive weights $(J \times 1)$ whose sum is 1, with $\mathbf{W} = (w_2, \dots, w_{J+1})$ as the weighted average of available donors, the following optimization problems determine the optimal weights that describe the synthetic control:

$$\begin{aligned} \min_w \|\mathbf{X}_1 - \mathbf{X}_0 \mathbf{W}\|_v \quad \text{s.t.} \quad & i) w_2 \geq 0, \dots, w_{J+1} \geq 0; \\ & ii) w_2 + \dots + w_{J+1} = 1 \end{aligned} \quad (2)$$

where v is a symmetric and positive semidefinite matrix whose elements reflect the importance of each predictor according to its predictive power for the outcome variable. Given the optimal weights $\mathbf{W} = (w_2^*, \dots, w_{J+1}^*)$ for each donor, the synthetic control at any time t is the weighted combination of donor employment $\sum_{j=2}^{J+1} w_j^* E_{jt}$; the estimate of the effect on employment is therefore:

$$\alpha_{1t} = E_{1t} - \sum_{j=2}^{J+1} w_j^* E_{jt} \quad \forall t \geq t' \quad (3)$$

The optimization problem covers the period from January 2015 to December 2018 with monthly observations of formal employment (or earnings), and we estimate the effect on formal employment for the period from January to December 2019. Although it is clear how to compute matrices \mathbf{n} and \mathbf{W} , there is little guidance and a lack of consensus about the predictors used to construct a valid synthetic control other than a good pretreatment fit (Abadie et al., 2010), which runs the risk of specification searching to find significant estimates (Ferman et al., 2017). We therefore allow the algorithm to choose the best fitting model. Like Abadie et al. (2010), we use the minimum value of the root mean square prediction error (RMSPE) as the key criterion in the pretreatment period. To find the best model for each outcome variable, we consider a set of up to 50 different specifications to determine which model meets this criterion. These specifications include lags of the outcome variable and covariates that do not vary over time (like the poverty head count ratio and other variables described in Table 1, as well as average labor income in different periods). We report as the main results the output produced by the model with the minimum RMSPE.

An additional complication in the analysis is that we have 18 municipalities as treated units. We thus estimate 18 different synthetic controls, one for each municipality, and obtain a treatment effect for each municipality and outcome variable. To obtain the aggregate effect for the northern border region, we use a weighted average using population in treated municipalities in 2015 as weights. The results are similar if we use one aggregated treated unit instead of the 18 individual municipalities and build its synthetic control accordingly (Figure A6 in Supplementary Materials).

To make inferences from this estimate, we provide two results. First, we follow Abadie et al. (2010, 2015) and Cavallo et al. (2013) and show p -values using placebo tests, calculated by

repeating the synthetic control procedure for each donor unit excluding the treatment units. The p -value is obtained as the proportion of cases in the placebo tests whose effects are as large as the estimated treatment effect.¹⁸ The intuition behind this procedure is that if many placebo tests are as large as the main effect (i.e., there is a large p -value), then it is likely that the effect is due to random factors instead of the intervention. Although this first method is the most important, it is also useful to show confidence intervals. The second method, following Peri and Yasenov (2019), is to calculate these using a regression analysis with the weights obtained in the synthetic control procedure.¹⁹

To implement the SCM, we normalize the value of all outcome variables (employment and earnings) in September 2018 for each municipality. This allows us to compare employment and earnings trends across different municipalities and increases the number of potential donors who can be included in the synthetic control group. For example, the method can consider donors with trends similar to those in the treatment group regardless of their level of employment.

A related approach, similar to that of Card (1992), is to estimate the elasticity of labor demand using the local stimulus package in the northern border region as the instrumental variable. The regression to estimate is:

$$E_{mt} = \theta W_{mt} + \delta_m + \delta_t + \pi \mathbf{X}_m \times Trend_t + \sigma Fraction_{m,2015} \times Trend_t + \varepsilon_{mt} \quad (4)$$

where the dependent variable is the index of employment level (E_{mt}) and the main independent variable is the index of the earnings level (W_{mt}). Parameters δ_m and δ_t refer to municipality and year-month fixed effects. The first controls are for unobserved characteristics that are constant over time for each municipality, and the second controls are for effects that affect all municipalities at the same time (like macroeconomic effects that vary seasonally throughout the country). The matrix \mathbf{X}_m contains control variables that do not vary over time, like the poverty rate in 2015 and other variables shown in Table 1. The variable $Fraction_{m,2015}$ refers to the fraction of workers in 2015 earning less than MXN 180 (close to the new minimum wage enacted in 2019 for the northern border region) in municipality m . Standard errors are clustered at the municipality level. The instrumental variable for W_{mt} is a dummy variable with the value of 1 if the municipality is in the northern border region and observed in 2019, the period of the local stimulus package. We show regressions using all municipalities and using municipalities in the synthetic control group (in which case we include only time fixed effects and treatment group with no further controls).

To ensure the robustness of the results using the SCM, we implement other strategies and econometric methods. First, the SCM relies on finding a valid counterfactual. It is possible that the northern border in Mexico depends much more on the United States than on the Mexican economy. We calculate the SCM using only metropolitan areas in the United States from the Current Employment Statistics of the US Bureau of Labor Statistics. Second, we obtain the treatment effect using the procedure described in Doudchenko and Imbens (2017). Finally,

18 We use the Stata `synth_runner` package to implement this procedure (see Galiani and Quistorff 2017). The p -value is calculated for those placebo tests with a certain value of RMSPE to ensure that they are not noisy and that good controls are found for each placebo. We modify the package to calculate p -values using a stricter criterion for the RMSPE to ensure that the p -values are not sensitive to bad controls.

19 We estimate a regression with year-month fixed effects and municipalities fixed effects. The key independent variable is a dummy variable that takes the value of 1 for a municipality in the northern region observed in 2019. Standard errors are clustered at the municipality level.

to corroborate the results using the full population we rely on the labor force survey (ENOE) for the same period as before: 2015–2019. The dataset has various disadvantages: the sample does not cover all municipalities, and labor income suffers from measurement error and a low response rate (Campos-Vazquez and Lustig, 2017). However, the main advantage is that it covers the full working population, including the formal and informal sectors, and includes information at the individual level. This allows us to include individuals' age, gender, schooling, and marital status as control variables.

4 Results

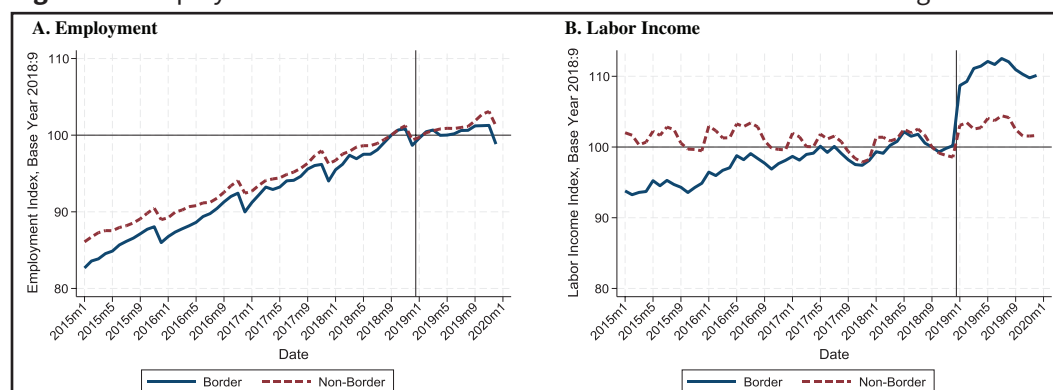
We start by describing trends in employment using administrative data. Figure 1 shows the trends in total employment and labor income, normalized to September 2018, of the border and non-border regions. Panel A shows that employment is growing faster in the border region than in the rest of the country before the stimulus was implemented. Panel B shows the growth in average labor income (in real terms) for the two regions. For 2019, employment growth slowed in the border region while earnings substantially increased: average earnings in the formal sector increased approximately 11%, as compared with 3% in the non-border region. The trends in the figure suggest the need to find a valid control group that shows a similar trend as the border region.²⁰

4.4 Administrative data: formal sector employment

4.4.1 Results using synthetic control

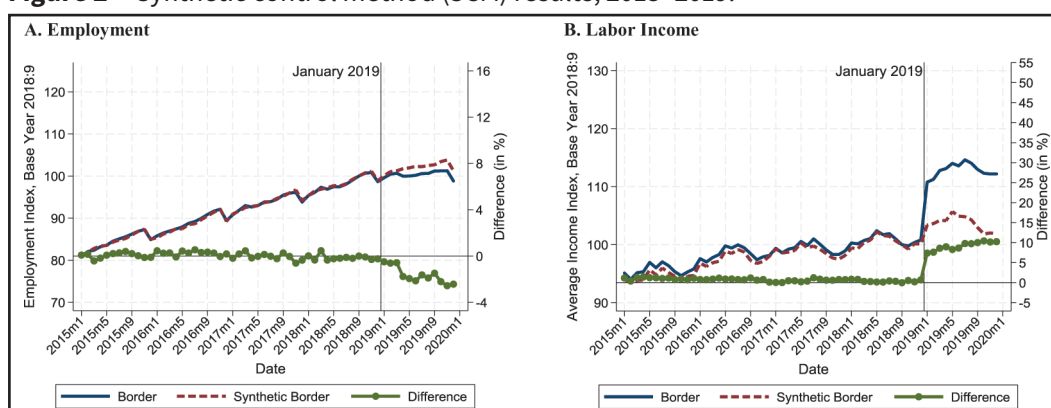
The main results of the study are shown in Figure 2. Panel A describes the trends in employment and panel B the trends in labor income using IMSS administrative data for formal sector workers. The border region is represented by the solid line and the synthetic control by the dashed line. The SCM is successful in matching the trend in the pretreatment period for the

Figure 1 Employment and labor income between border and non-border regions.



Notes: Authors' calculations using IMSS data restricted to municipalities with at least 50,000 inhabitants. Border refers to the 18 municipalities in the treatment group. Labor income is deflated using the same Consumer Price Index for both regions.

²⁰ In Supplementary Material, we show that running an event study with municipality controls does not show similar trends before the implementation of the stimulus package. This suggests the need to use other econometric methods to find a valid control group (Figure A3). All point estimates are provided in Tables A1–A7 in Supplementary Materials.

Figure 2 Synthetic control method (SCM) results, 2015–2019.

Notes: Authors' calculations. The SCM is applied to each of the 18 municipalities in the treated group. The trends for the treatment and synthetic group are then obtained using the 2015 population as weights. The base period is September 2018; the policy was implemented in January 2019.

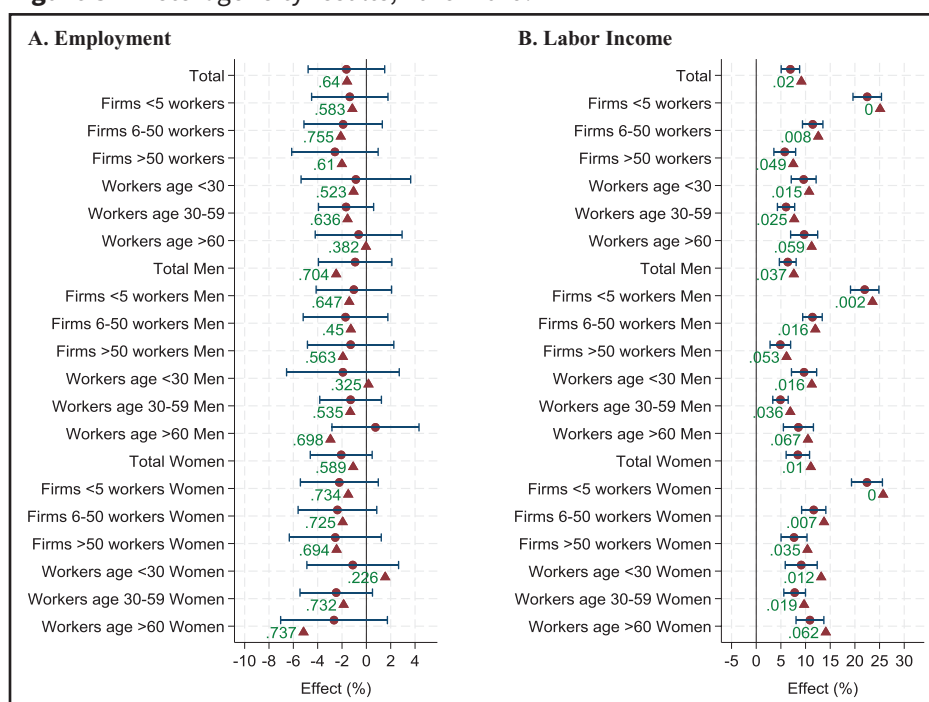
border region. The difference between the border region and its synthetic control is depicted in green dots on the second y -axis. As the base period is September 2018, each dot is a difference-in-difference estimate expressed as a percentage. Employment declines slightly in the first 3 months of 2019, and then there is a larger disemployment effect in the second to fourth quarters. In September 2019, the border region has grown 1.5% less than its synthetic control. Panel B depicts the trends in labor income and clearly shows that the border region had a greater increase than its synthetic control. Labor income in the border region after the policy is implemented is approximately 9% higher than its comparison.

To assess whether the estimate is statistically significant, we provide two comparisons in Figure 3. We show the mean effect (triangles) in the treatment period and the p -values from the placebo tests (next to each triangle), as suggested by Abadie et al. (2010). We also show estimates (dots) and 95% confidence intervals from a regression with fixed effects for time and treatment group (18 municipalities and their respective synthetic controls) using the weights obtained in the SCM (interacted with 2015 population). We estimate the effect for different demographic groups (by gender and age groups) and by firm size: small firms (5 employees or fewer), medium firms (5–50 employees), and large firms (at least 51 employees). Each outcome variable has its own synthetic control group.

Panel A shows the results for employment and panel B for labor income. All estimates by subgroups and the total effect on employment are negative, and the average effect is -1.6% (with the exception of male workers at least 60 years old in a regression framework and young female workers using the SCM), but they are not statistically significant using either confidence intervals from a regression estimate or placebo tests. Moreover, the largest negative effects are close to 4% (older women), which indicates a limited effect on employment with respect to the stimulus package. Panel B shows large increases in labor income across subgroups. Workers in small firms and in the border regions increase their labor income by approximately 25% with respect to the synthetic control. Overall, the stimulus package increases labor income by approximately 9%. Workers in large firms show the smallest increase, approximately 5%.

The confidence intervals in the regression imply that the results of the policy were between a decline in total employment of 4.7% or an increase of 1.7%, which correspond to an effect

Figure 3 Heterogeneity results, 2015–2019.



Notes: Authors’ calculations. The synthetic control method (SCM) is implemented for each outcome variable on the y-axis for the 18 municipalities in the treated group. The 95% confidence intervals (robust and clustered at the municipality level) are obtained from a regression with fixed effects for time and treatment group (18) using the weights obtained in the SCM. The p-values from the placebo tests are shown next to the triangles. Placebos with a RMSPE larger than 2.5 times the treated unit are omitted.

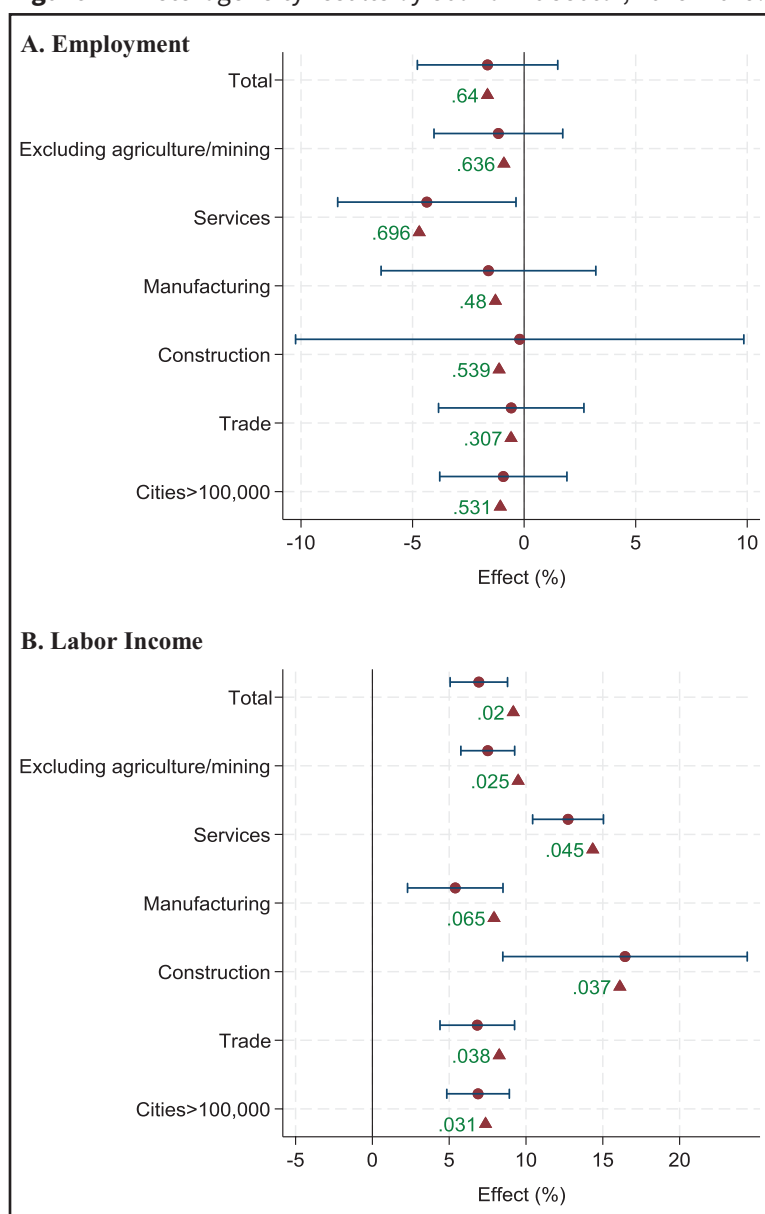
ranging between the loss of 96,000 jobs and the creation of 35,000 jobs (in the context of a total employment in the region of close to 2 million; see Table 1). This implies that the labor demand elasticity is between -0.5 and 0.

Figure 4 shows estimates by industry as reported in IMSS administrative data, as well as estimates using data restricted to municipalities with at least 100,000 inhabitants (instead of 50,000). The figure shows the results for services, the results with agriculture and mining (industries with more volatile employment) excluded, for manufacturing, construction, retail, and wholesale trade, and for municipalities with at least 100,000 inhabitants. The results cluster around zero: the stimulus package does not seem to increase or reduce total employment. While all groups benefited from an increase in labor income, the groups with the largest decrease in employment are small firms and services (around 4%), although the effect is imprecise and not statistically significant.

4.4.2 Robustness checks

Figure 2 shows that the synthetic control closely matches the trend of the treatment group. However, it could be that unobserved components in 2019 (such as a change in US economic activity) affect the border region differently than the non-border region. We obtain monthly employment for all metropolitan areas in the United States using the Current Employment Statistics for the period 2015–2019 and use them to build a different synthetic control. In Figure 5, panel A uses all metropolitan areas in the United States as the control, and panel B uses not only the municipalities in Mexico, but also metropolitan areas in the border states

Figure 4 Heterogeneity results by economic sector, 2015–2019.



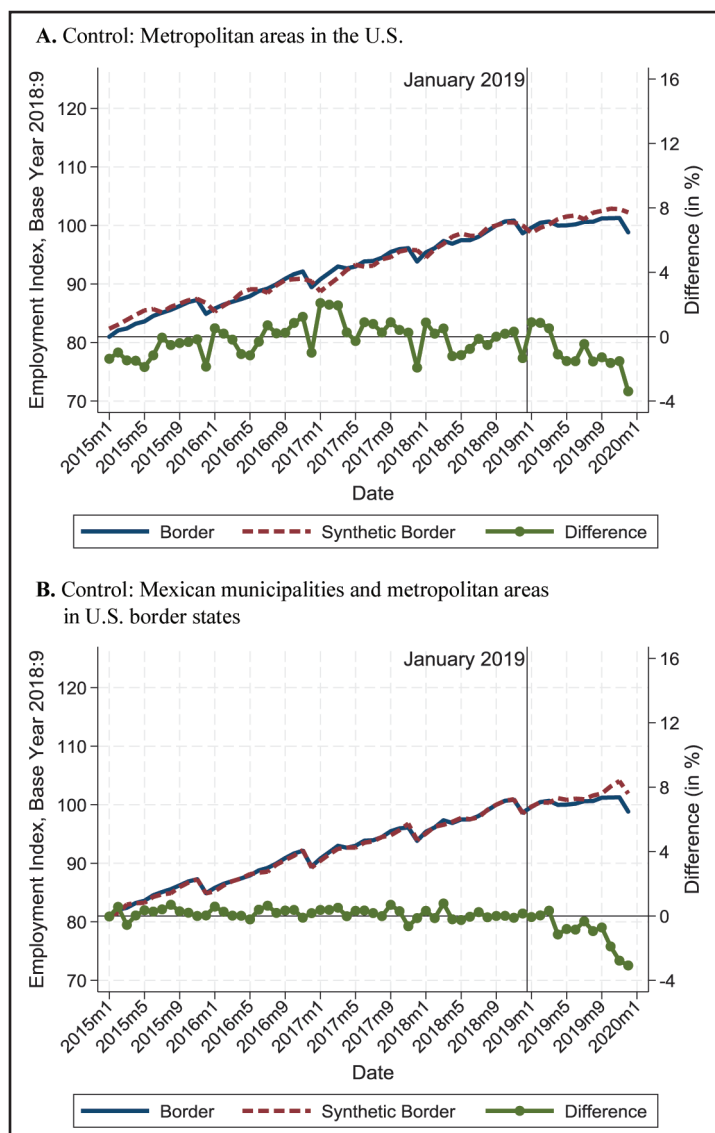
Notes: Authors’ calculations. The synthetic control method (SCM) is implemented for each outcome variable on the y-axis for the 18 municipalities in the treated group. The 95% confidence intervals (robust and clustered at the municipality level) are obtained from a regression with fixed effects for time and treatment group (18), using the weights obtained in the SCM. The *p*-values from the placebo tests are shown besides the triangles. Placebos with a RMSPE larger than 2.5 times the treated unit are omitted.

of the United States. The synthetic control using all metropolitan areas in the United States is noisier than in Figure 2, but it does not show a clear negative effect once the stimulus package is implemented. Panel B shows similar results. Hence, it does not seem that differential trends for reasons other than the stimulus package explain the lack of effect on employment.²¹

We implement two other methods to construct a counterfactual for the border region (the results are shown in Figure A7 in Supplementary Materials): the methods described in Doudchenko

²¹ Instead of using municipalities, we also implemented the synthetic control using 1 treatment unit (border region) and 31 donors (each of the remaining states in Mexico). The results are similar and are shown in Figure A8 in Supplementary Materials. Results by municipality are shown in Figures A4 and A9.

Figure 5 Robustness test using metropolitan areas in the United States as controls, 2015–2019.



Notes: Authors’ calculations. The synthetic control method is implemented for each outcome variable on the x-axis for the 18 municipalities in the treated group. Control group in panel A constructed from metropolitan areas in the US control group in panel B constructed from municipalities in Mexico and all metropolitan areas in US border states. The difference is not statistically significant.

and Imbens (2017) and Li and Bell (2017). The first uses a Lasso regression to select the control municipalities for the treatment group. The second relies on a time-series approach and model fitting in the pretreatment period. This procedure uses only aggregated data in the border and non-border regions, and assumes that seasonality is the same in the treatment period as the estimate for the pretreatment period.²² The results are similar to the main SCM.

22 Many models can be tested. We estimate models similar to $Y_{i,t} = f(Y_{0,t}, Seasonality) + \varepsilon_t$, but the one selected most often was a simple one, $Y_{i,t} = \beta Y_{0,t} \mu_{month} + \varepsilon_t$ in which 1 refers to the treatment group, 0 to the control group, and μ_{month} to fixed effects by month. This equation is estimated, for example, for the period 2015–2017; we then calculate the RMSE using the prediction for 2018. We select the model with the lowest RMSE in 2018, and then the model is re-estimated for the period 2015–2018 to obtain its prediction $\hat{Y}_{i,t,treatment}$ in the treatment period. We then contrast the observed outcome in the treatment period and its counterfactual ($\hat{Y}_{i,t,treatment}$). Standard errors are calculated using a bootstrap procedure.

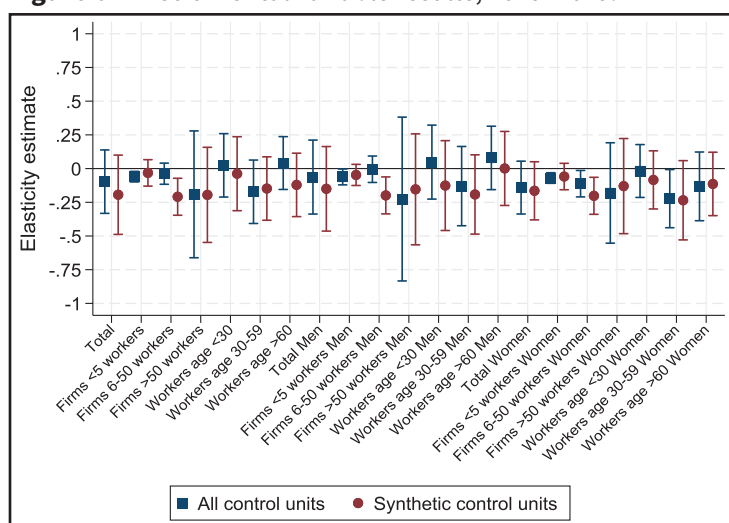
4.4.3 Elasticity of labor demand: results using an instrumental variable approach

The elasticity of labor demand is estimated using either all control units or the units in the synthetic control for each outcome variable. It is obtained from a regression of the employment index against the labor income index, instrumented by a dummy variable for a municipality within the NBFZ in 2019, the period in which the minimum wage increases and taxes decrease. The regression controls for fixed effects of time and municipality, and the municipality controls are interacted with trends plus state trends. Standard errors are clustered at the municipality level. In the synthetic control, we use fixed effects for time and for the 18 treatment groups without further controls. Figure 6 shows the results and confirms the findings that the elasticity is close to zero. It also shows that both estimation procedures have similar results. In general, the elasticity of labor demand is small, negative, and statistically significant (at the 95% level) for workers in small and medium-sized firms (ranging from -0.05 to -0.1 in small firms using OLS and from -0.17 to -0.22 in medium-sized firms using the synthetic control). Other estimates are highly imprecise.²³

4.5 Household survey data: employment and other social outcomes

In this section, we describe the results from using the labor force survey (ENOE) for the period 2015–2019. The advantage of this dataset is that we can analyze patterns for total employment

Figure 6 Instrumental variable results, 2015–2019.



Notes: Authors’ calculations. 95% confidence intervals. “All control units” is a regression for all units in the treatment and control groups; the dependent variable is the employment index and the independent variable is the labor income index in 2019, instrumented with a dummy variable for the municipalities in the border region. The regression includes fixed effects of time and municipality as well as the control variables summarized in Table 1 interacted with a trend and state trends, and the fraction of workers in the municipality with daily earnings less than MXN 180 interacted with a trend. Regression is weighted by the population in 2015. The results for “synthetic control units” uses only fixed effects for time and the 18 treatment groups, but weighted by the synthetic control weights interacted with population in 2015.

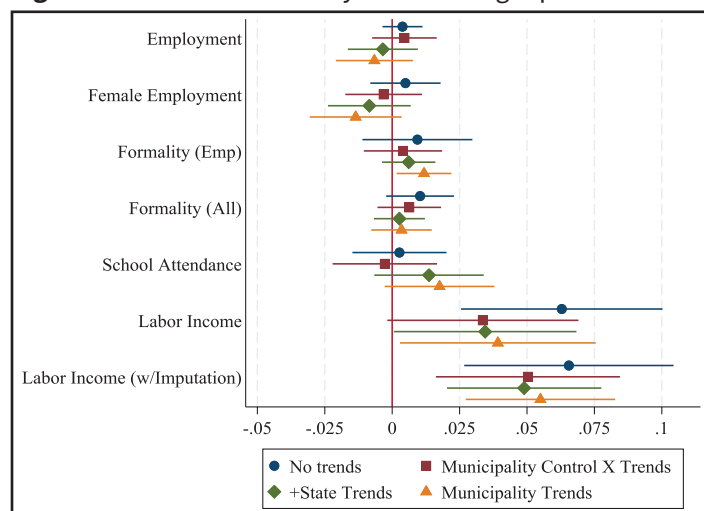
²³ In Supplementary Material (Table A8), we take advantage of the variation in the fraction of workers earning less than USD 180 MXN as an additional instrumental variable. We estimate regressions at the municipality and municipality X sector level. Results are similar to those shown in Figure 6.

including the informal sector. We focus on the following outcome variables (for individuals 20–65 years old): the employment rate (number of employed individuals divided by the population), the female employment rate, the share of workers in the formal sector (Formality (Emp)), the share of formal sector workers in the population (Formality (All)), labor income as observed and using imputation to avoid the low response rate, and school attendance (for individuals 15–24 years old).²⁴ School attendance is relevant, as there could be an increase in the dropout rate if the increase in the minimum wage acts as a pull factor.

In this case, it is not possible to use the synthetic control methodology, as the sample size within municipalities is too small to calculate trends at that level. We rely on simple OLS regressions using the same control variables as earlier, but with the unit of observation at the individual level. This allows us to also include controls like individual age, gender, schooling, and marital status. The key regressor is a dummy variable for the border region in 2019. The results include the estimation of individual control variables only, and also with controls at the municipality interacted with a trend, with state-level trends, and finally municipality fixed effects interacted with a trend. The results are plotted in Figure 7 (results for different subsamples and restricting for the period 2017–2019 are shown in Figures A10–A16 in Supplementary Materials).²⁵

The results are broadly consistent with the previous findings using IMSS administrative data. There is a small negative effect on total employment (using state trends or municipality trends), although the estimate is not statistically significant and its magnitude is relatively

Figure 7 Labor force survey results using repeated cross-sections, 2015–2019.



Notes: Authors' calculations using ENOE data. Each dot is the coefficient for the border region in 2019 for different regressions; the outcome variables are shown on the y-axis. The regression is at the individual level. Control variables include fixed effects by time and municipality, individual age, gender, schooling, and marital status. Municipality Control X Trends: Trends are interacted with controls at the municipality level. Municipality Trends: municipality fixed effects are interacted with a trend.

²⁴ We use a hot-deck procedure to impute labor income when there is no response. The imputation is within year, quarter, gender, age groups, schooling groups (less than primary, primary, junior high school, high school, and college), formal sector status, state, and border region. We calculate the average labor income across five imputations.

²⁵ For example, we estimate models for different age groups, for workers with less than high school, and for workers in the cities selected by the synthetic control method. All results are similar.

small. There is a small nonsignificant negative effect on female employment (consistent with the finding that the elasticity of labor demand is negative for women, especially in small firms). In contrast with the results using administrative data, when all control variables are included there is a positive although non-statistically significant effect on formality (rows with the variables Formality (Emp) and Formality (All)), suggesting that employment decreases mainly through a decrease in informality.²⁶ The effect on school attendance is close to zero, meaning that the stimulus package is not encouraging school dropouts. Finally, there is a positive effect of the stimulus package on labor income, but it is less than that found with IMSS data: 3%–5% as opposed to the 8%–9% found in Figure 1. Combining the confidence intervals of the results using IMSS data and those using Formality (Emp), the maximum negative effect is approximately 1.2%. This implies that at most there was a net loss of approximately 24,000 jobs.

4.6 Summary

The findings using administrative data suggest a null to small negative effect on total employment due to the place-based policy. But using the variation of wages within demographic groups, the findings suggest that the elasticity of labor demand is negative and significant for small firms, and especially for women.

Using household survey data, the findings suggest that the estimates for total employment and those for the proportion of formal sector workers in the total population are close to zero. The estimates are slightly more negative for women, but they are still nonsignificant. In sum, labor income increases substantially for workers in the northern border municipalities, with no large effects (positive or negative) on employment outcomes.

Both datasets suggest that if there is a negative effect of doubling the minimum wage, it is likely to be relatively small at the aggregate level. To understand these results, we search for all newspaper articles published in or mentioning the border municipalities that use the term “minimum wage.” There were 39 articles from January to September 2019, and only 6 were about the potential negative effects of the minimum wage (mainly in January and February). However, there were no articles documenting firm closures or employment contractions due to the new minimum wage. On the contrary, recent articles document how Central American migrant workers are being hired in the maquila and restaurant industries at the new minimum wage.²⁷ There were four articles documenting that firms were cutting benefits and bonuses to decrease the impact of the minimum wage.²⁸

26 To corroborate this claim, we estimate transition probabilities using a multinomial logit, taking advantage of the panel structure in the labor force survey. For the period 2015–2019, we take the observations for the first and fifth quarters for the same individual, and estimate a multinomial logit conditioned on being out of the workforce, being in the formal sector, or being in the informal sector in the first quarter. The model thus estimates the transition probability 1 year later of being out of the workforce, having formal employment, or having informal employment. The key regressor is a dummy variable for the northern border cities interacted with a dummy variable for the year 2019. The results clearly indicate that the transition from formal to informal employment is reduced and the transition from informal to formal employment increases. The other effects are close to zero. See Figure A17 in Supplementary Materials.

27 *Voz de América*, June 28, 2019, “México ofrecerá 40.000 empleos a migrantes en su frontera con EE.UU.”; *Los Angeles Times*, September 4, 2019, “Ante una larga espera en Tijuana algunos migrantes encuentran trabajo”; *El Mañana*, September 27, 2019, “Contratan restauranteros a la comunidad migrante”; *El Mañana*, September 29, 2019, “Dan trabajo maquilas a unos 120 migrantes.”

28 *El Mexicano*, January 7, 2019, “Quitán bonos a empleados de maquiladoras”; *El Mexicano*, January 16, 2019, “Es contra la ley quitar prestaciones a trabajadores”; *El Imparcial*, May 23, 2019, “Analizaron empresas quitar bonos por doble salario mínimo”; *El Diario MX*, July 24, 2019, “Salario al doble amenaza prestaciones.”

5 Potential explanations for the lack of substantial employment effects

5.1 Prices

One explanation for the virtually null effect of the policy on employment is that firms might be offsetting labor costs through higher prices. With the simultaneous decrease in VAT and increase in the minimum wage, real wages are likely to rise along with aggregate demand. In fact, analyzing previous VAT reforms in Mexico, Mariscal and Werner (2018) found that the pass-through from VAT to inflation is approximately 20%. A potential decrease in inflation in the northern border municipalities is thus about 1.4% points (VAT cut from 16% to 8%, $1.08/1.16 \times 20\%$). In 2018, average annual inflation in both the border region and the rest of the country was 4.9%, while in September 2019 it was 2.4% and 4%, respectively.²⁹ This estimate is close to the pass-through effect estimated by Mariscal and Werner (2018). Hence, it seems that the minimum wage is not playing an important role in how firms adjust prices.

5.2 Correlation with other government programs

The government also introduced other new programs in January 2019. The most relevant is the program “Young People Building the Future” (“Jóvenes construyendo el futuro”), which provides a monthly stipend of MXN 3,600 (approximately USD 360 in PPP) to individuals 18–29 years old who are not currently employed and are willing to work as apprentices. In the IMSS administrative data they are not counted as employed, since the government, not the employer, pays their social security contributions. Their presence in the labor force survey depends on how they interpret the question “During the past week, did you work at least one hour?” But even if they answer no, there is a filter for training courses that considers the person employed. Hence, IMSS data do not take into account the individuals in the program, but the labor force survey does.

The main results using administrative data show that restricting for age groups does not change this null to small negative effect. When we estimate all effects, omitting individuals younger than 30, the results are approximately the same. Moreover, for the youth program to bias the estimates, enrollment would have to be larger in the northern border municipalities. However, using administrative data we find that enrollment in these municipalities is close to 70 apprentices per 100,000 inhabitants, while in the rest of the country it is close to 600. The youth program thus does not seem to be affecting the main results of the study.

5.3 Compensating costs

It is possible that the lack of an employment effect is due to a compensation of costs from the VAT reduction and the increase in labor costs. This is difficult to ascertain, as not all of the information needed to compute different scenarios is available. The Mexican Economic Census is available for information on costs for all firms in 2018. In the NBFZ, labor costs account for 26.9% of total costs, energy costs account for 5.3%, and other costs for 67.8%. We also need an

²⁹ Results are approximately the same using a regression framework. Campos-Vazquez and Esquivel (2020) obtained a similar conclusion.

estimate of how prices change. Previous results indicate that labor income increased by 9%. Campos-Vazquez and Esquivel (2020) show that energy prices fell by 7%, and core inflation (which excludes energy prices) fell by 0.9%. With quantities fixed, labor costs increase by 2.4% and energy costs decrease by 0.4%. The rest is difficult to estimate, as we do not know how much of inputs is bought in local markets (with a VAT of 8%) as opposed to the rest of the country (with a VAT of 16%). If we assume, for example, that only 10% of nonlabor, non-energy costs is due to inputs bought locally (and 57.8% from those bought in the rest of Mexico or abroad), we obtain a reduction in total costs of 0.09%. Under this scenario, total costs increase by 1.9%. From this calculation, it is clear that the policy has heterogeneous effects, depending on an establishment's share of labor, energy, and nonlabor and non-energy costs on total costs, as well as on how much of inputs is bought locally (and subject to lower prices). In any case, the reduction in VAT partially or completely compensates for the increase in labor costs. Matched employer–employee data might provide a clearer picture on how costs were modified under the policy and its effects on employment outcomes at the individual and establishment level.

6 Concluding remarks

The primary contribution of this article relies on the short-run evaluation of one of the bold-est place-based policies in recent history: the NBFZ in Mexico. The intervention combined a doubling of the minimum wage and a 50% reduction in VAT (only 3% of firms were able to take advantage of a CIT reduction). This place-based policy was restricted to 43 municipalities sharing a border with the United States and began in January 2019. Using monthly administrative data from the IMSS and quarterly data from the labor force survey (ENOE), we estimate the impact of the policy on labor market outcomes. The identification strategy is based on constructing a synthetic control that matches the trends in the pretreatment period to municipalities along the border with the United States. We find substantial labor income increases with close to zero effect on employment, using either the administrative data or the labor force survey. The significance of our results is robust to alternative methodologies and control groups. However, the results should be interpreted with caution, since the estimates are imprecise and for some subgroups and specifications (labor demand elasticity for small and medium-sized firms) the effect is negative and statistically significant. Finally, combining the confidence intervals of the results using both administrative and household survey data, the maximum negative effect is around 1.2%. This implies that at most there was a net loss of approximately 24,000 jobs. Future research needs to use establishment-level data to examine how firms adjust, as well as lighthouse effects and changes in the wages of higher-paid workers independent of the number of workers earning a minimum wage.

Abbreviations

CFE, Federal Electric Commission; CIT, Corporate Income Tax; Coneval, National Council for the Evaluation of Social Development Policy; ECLAC, Economic Commission for Latin America and the Caribbean; ENOE, National Survey on Employment and Occupations; Eurostat, Statistical Office of the European Communities; GDP, Gross Domestic Product; IMSS, Mexican Institute of Social Security; INEGI, National Institute of Geography and Statistics; MXN, ISO 4217 code for the Mexican peso; NBFZ, Northern Border Free Zone; OECD, Organization for Economic Cooperation and Development; PEMEX, Mexican Petroleum Company; PPP, Purchasing Power Parity; RMSPE, root mean square prediction error; SAT, Tax Administration Service; SCM, synthetic control method; SEZ, Special Economic Zones; US, United States of America; USD, ISO 4217 code for the United States dollar; VAT, value-added tax.

Availability of data and materials

The datasets used and/or analyzed during the current study are available from the corresponding author on reasonable request.

Competing interests

The IZA Journal of Labor Policy is committed to the IZA Guiding Principles of Research Integrity. The authors declare that they have observed these principles. Moreover, the opinions expressed herein must in no way be considered to reflect the official position of Banco de México, El Colegio de México, or the Secretaría de Hacienda y Crédito Público.

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Authors' contributions

All authors discussed the results and contributed to the final manuscript. All authors read and approved the final manuscript. RMC-V cleaned datasets, designed tests, and interpreted results, and was a major contributor in writing the manuscript. VD analyzed datasets, previous literature, and the institutional design of the place-based policy, and was a minor contributor in writing the manuscript. AR analyzed datasets, performed the statistical programming, prepared the figures and tables, and carried out robustness tests.

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