DOI: 10.1111/1475-5890.12400

CONTRIBUTED PAPER

Fiscal Studies
The Journal of Applied Public Economics
IFS Institute for Fiscal Studies

Who pays for a VAT hike at an international border?

Emmanuel Chávez[1] | **Cristóbal Domínguez**[2]

[1]Centro de Investigación y Docencia Económicas

[2]Comisión Nacional Bancaria y de Valores

Correspondence
Emmanuel Chávez, CIDE, Carretera México-Toluca 3655, Mexico City, 01210, Mexico.
Email: emmanuel.chavez@cide.edu

Submitted: December 2023

Abstract

This research studies the effects of a value added tax (VAT) reform that raised the rate from 11 to 16 per cent at localities close to international borders in Mexico. Using difference-in-differences, we find that the VAT hike increased prices by one-third the size of the full-passthrough counterfactual. Moreover, we find that workers absorbed part of the VAT hike, as the reform had a negative effect on workers' compensation. Informality, competition across the border, worker mobility and earnings level drive the effects we find.

KEYWORDS
international borders, tax incidence, value added tax

JEL CLASSIFICATION
H22, H24, H73

1 | INTRODUCTION

Who pays for taxes is a longstanding question in economic thought. Consumption taxes, and in particular value added tax (VAT), are not exempt from this analysis, as recent studies on the incidence of VAT find that it directly impacts outcomes beyond prices (Kosonen, 2015; Harju, Kosonen and Skans, 2018; Benzarti and Carloni, 2019; Benzarti et al., 2020). We build on this literature by analysing a reform that increased the VAT rate *only* at localities close to Mexico's international frontiers while leaving the rest of the country unaffected.[1] Specifically, the VAT rate increased from 11 per cent to 16 per cent at the borders. Before the reform, the rate was set at 16 per cent in the rest of the country. So the reform standardised the rate across Mexico. This natural experiment allows us to analyse the effects of raising VAT in a context where the neighbouring jurisdiction is unaffected.

We obtain a series of relevant findings regarding the incidence and the effects of VAT. On the consumer side, we find the VAT hike increased prices. However, the average price increase is around one-third of the full-passthrough counterfactual, and prices do not catch up with the full passthrough in the period we analyse. This is relevant, as previous research has found that VAT hikes are often fully passed to consumers (Benzarti et al., 2020). We detect two factors that drive our price effects. First, in line with evidence presented by Bachas, Jensen and Gadenne (2024), we find that the goods most likely

[1] In this paper, we refer to two types of local geographic divisions. A *locality* is an agglomeration of housing units, either small (town) or large (city). A *municipality* is a local administrative division possessing its own government. In Mexico, municipalities are usually composed of many localities.

consumed at informal establishments experience a null VAT passthrough to prices. On the other hand, the goods least consumed at informal establishments experience a positive VAT passthrough to prices. However, the effect for the latter goods is about half of the full-passthrough counterfactual, indicating that the VAT passthrough determinants are multidimensional. The second relevant mechanism behind our price results is the competition that firms at the border face from the neighbouring jurisdiction. If a country raises the VAT rate in the whole territory, consumers have limited options to find better prices if the hike is fully passed to the consumer. However, if the VAT rate is raised at international borders, consumers have more opportunities to search for better prices. We find that goods more strongly demanded by cross-border shoppers saw no price increase due to the VAT hike, while goods mostly consumed internally experienced a positive price increase.[2]

On the labour side, we provide compelling evidence that the VAT reform had a negative impact – of around 2 per cent – on the cost of labour in the formal sector, with no effect on the level of employment. This negative effect on worker compensation is not strong enough to lead to an absolute decrease. Instead, compensation kept nominally increasing but at a lower rate than it would have without the reform. Although the administrative labour data we use do not allow us to ascertain whether the negative effect on labour cost comes from price (wage) or quantity (hours) changes, we present evidence that the number of hours worked did not change due to the VAT reform. Moreover, Mexican labour law makes it difficult for firms to adjust working hours downward. This implies that wages rather than hours drive the effect on labour costs. Our findings on prices and labour costs are consistent with literature showing that firms tend to preserve or increase their profits due to VAT reforms (Kosonen, 2015; Benzarti and Carloni, 2019; Benzarti et al., 2020). In our study setting, firms are cautious about raising prices fully following the VAT hike. However, they appear to be able to adjust labour costs by taking a (counterfactual) bite on wages. We detect two mechanisms that drive the labour compensation effect. First, workers in sectors with low worker mobility (across regions) experience a statistically significant negative impact on their compensation due to the VAT hike. Meanwhile, worker compensation in high-mobility sectors shows a null effect. Second, we find suggestive evidence that the reform affects workers with lower earnings more. These findings indicate that the VAT hike harmed relatively more the earnings of workers with less bargaining power, as mobility and earnings have been linked with workers' ability to set their compensation (Yamaguchi, 2010; Piketty, Saez and Stantcheva, 2014; Jinkins and Morin, 2018; Rolim, Baltar and Lima, 2023).

To complement the effects on labour, we find that the VAT hike had a negative impact on payroll credit; this is granted explicitly to formal sector workers. Banks can collect repayments of payroll credit by directly discounting them from workers' payrolls. This credit reflects the credit market most accessible to workers. This is consistent with workers adjusting their borrowing downwards due to a permanent negative income shock caused by the VAT hike. These findings reinforce the effects of the reform on labour, as the impact of the VAT hike on workers negatively affected not only their compensation but also their probability of contracting credit.

We use a difference-in-differences strategy to estimate the impacts of the VAT hike. The geographic zone subject to the VAT discount rate before the 2014 VAT reform was mainly a 20-kilometre strip from the international frontiers. Our treatment area comprises the municipalities where most of the population lived in the VAT discount zone. The control area includes the municipalities located inside states at the international borders but outside the VAT discount zone. In addition, our estimation strategy focuses on the goods and sectors subject to VAT to estimate the treatment effect on the treated more precisely.

[2] Supporting this mechanism, exploratory evidence suggests that the VAT hike did not shift consumption to the US side of the border. The cross-border shopping literature generally finds that raising taxes in a jurisdiction causes demand to increase in a neighbouring jurisdiction, as the savings compensate for the costs of travelling to the other jurisdiction (Walsh and Jones, 1988; Asplund, Friberg and Wilander, 2007; Leal, Lopez-Laborda and Rodrigo, 2010). In the reform we study, the absence of consumption shifting to the US following the VAT hike could be due to the relatively small price increase by firms on the Mexican side.

1.1 | Related literature

Our research builds on literature that analyses the differential incidence of VAT on prices. In France, Carbonnier (2007) provides early evidence that the incidence of a VAT rate change on prices differs according to the affected sector. Gaarder (2018) finds that a VAT cut in Norway is passed to the treated sector with no spillovers to other sectors. This result aligns with our findings, as we see no effect of the VAT reform we study on the prices of goods not subject to VAT. Harju, Kosonen and Skans (2018) study restaurant VAT cuts in Finland and Sweden and find no reduction of prices in independent restaurants due to these cuts, while chains choose a complete passthrough. Benedek et al. (2020) find that the incidence of VAT reforms on prices in the Eurozone is smaller when special rates are changed. Benzarti et al. (2020) use a series of convincing estimation strategies and find that the incidence of VAT in Europe depends on the direction of the reform. While the incidence of VAT cuts is low, VAT hikes are regularly fully passed to consumers. Our results contrast with those of Benzarti et al., as we find that a VAT hike is not fully passed to consumers. We argue that this is due to the studied setting. While Benzarti et al. (2020) study VAT reforms that take place in the whole country, we analyse a VAT hike in a highly competitive setting with a sizeable informal economy. In this sense, our findings align with those of Carbonnier (2007), as he argues that the price incidence depends on the degree of competition in the market.

We also build on literature that studies the effects of VAT reforms on outcomes beyond prices. Kosonen (2015) finds that a VAT cut in Finland enabled firms to increase profits, as only about half of the VAT cut was passed to prices, while input costs were unaffected. Results by Benzarti et al. (2020) are in the same line. They find that VAT hikes may lead firms to increase prices at a rate exceeding the full passthrough, while VAT cuts are not passed to consumers. Firms thereby increase profits. Regarding the labour market, a causal impact of VAT on labour costs has been previously documented by Benzarti and Carloni (2019) in France. Their results indicate that firms share part of a profit windfall that follows a VAT cut with workers in the form of higher wages. To our knowledge, our paper is the first to show that a VAT rate change may impact labour in the opposite direction: a VAT hike can be shared with workers through lower labour compensation.

In addition, we contribute to the literature on the effects of income shocks on the credit market. Horvath, Kay and Wix (2023) find that the negative income shocks due to the COVID-19 pandemic led households in the United States to decrease their consumer credit use sharply. Hundtofte, Olafsson and Pagel (2019) find that negative income shocks due to unemployment in Iceland and in the US do not lead households to increase their credit balances. Braxton, Herkenhoff and Phillips (2020) show that the net borrowing effect across workers facing unemployment in the US is zero. Our results tend to be in line with these papers, as we find that a negative income shock coming from a tax hike negatively impacts the credit market. Moreover, to our knowledge, our paper is the first to analyse the credit effects of an income shock coming from a tax hike.

Finally, we expand previous literature on the effects of VAT reforms in Mexico. Aportela and Werner (2002) and Mariscal and Werner (2018) study the effects of VAT hikes on inflation. They find that VAT hikes lead to a small positive short-lived inflationary effect. We complement these findings with the approach we use to measure the effect of the VAT hike: as we focus on the prices of products and services treated by the reform (instead of general inflation), we find that the reform had a lasting effect on prices.[3] We also focus on sectors subject to VAT to study the effects of the reform on labour outcomes. This allows us to get new findings for the VAT incidence literature in Mexico. In particular, our findings stand in contrast to those of Núñez Joyo (2017). He finds that the reform had a positive effect on the level of employment, while we find no effect

[3] Our approach resembles an estimation done by Campos-Vazquez and Esquivel (2020). They study a package of tax and wage changes at Mexico's northern border in 2018, including a VAT cut. They find that the policy package did not affect general prices (inflation). However, the prices of groups of products prone to be taxed by the VAT were affected. Calderón et al. (2023) also use this approach to explore the effects of the same policy package on prices.

on employment. This discrepancy probably comes from two crucial differences in the estimation strategy: (1) we exclude border municipalities that were largely untreated by the reform; and (2) we exclude sectors that do not pay VAT. Finally, our findings support those of Davis (2011) on the real economic effects of the VAT discount in the border regions. He indicates that the VAT discount may (moderately) encourage economic activity. Our evidence complements the picture with labour market effects.

The remainder of the paper is organised as follows: in Section 2, we describe the 2014 reform that raised the rate at the borders, as well as the context of the border region and Mexico's labour market; in Section 3, we describe our data; Section 4 details the methodology; Section 5 presents our main results; Section 6 explores mechanisms that drive our findings; finally, Section 7 concludes.

2 | CONTEXT AND INSTITUTIONAL SETTING

VAT was introduced in Mexico in 1979. In 2013, the general VAT rate stood at 16 per cent, with a lower rate of 11 per cent in a geographic area that mainly comprised a 20-kilometre strip from the international borders. These rates were in effect since 2010. In September 2013, the president in office presented a tax reform to Congress to increase the VAT rate at the 20km strip, standardising it at 16 per cent nationwide. This represents a 45 per cent increase in the previously discounted areas. Congress approved the reform in October 2013, taking effect in January 2014. The reform included several measures besides the VAT hike at the international borders.[4] All other measures were introduced across the country and are accounted for in the estimation strategy we explain in Section 4. The primary justification the government used to introduce the 2014 tax reform was to increase tax collection, as Mexico's tax revenues are low, not only by OECD standards but also compared with other Latin American countries.[5]

Panel (a) of Figure 1 later shows the VAT rate discount area before the 2014 reform. The area included all localities 20km or less from the international borders. However, the discount area in some places exceeded the 20km limit. Some states – as well as some municipalities – were completely included in the VAT discount area. Around 9.9 million people lived in the VAT discount zone in 2010, i.e. nearly 9 per cent of the country's population at the time. According to the VAT law, the VAT discount was determined at the establishment (shop) level, based on the address registered with the tax authority (Congreso de la Unión, 2009). This implies that, when the VAT discount was in place, some neighbourhoods were split by the 20km strip, leading to a situation where some businesses charge a 16 per cent VAT rate and others across the street charge an 11 per cent rate. However, the delimitation of the VAT discount zone made this event extremely rare, as the vast majority of localities were placed completely inside the VAT discount line or completely outside. Only four localities with at least 1,000 inhabitants were split by the VAT discount line shown in Panel (a) of Figure 1. The combined population of these localities is around 80,000 people, i.e. only about 0.8 per cent of the population living in the VAT discount zone.[6]

Note that this VAT reform differs from that of other studies outside Mexico. Previous papers have mainly studied changes in the VAT rate in a particular sector (or sectors) across a whole country

[4] Among these are measures to incorporate informal firms into the formal sector, a special tax on stock exchange transactions, a slight increase in the income tax for the top brackets and a special tax on mining companies' revenues.

[5] In 2010, tax revenues in Mexico (not including oil revenues) stood at nearly 14.5 per cent of GDP. The average in Latin America at that time was close to 19 per cent of GDP. Meanwhile, the OECD average stood at 26 per cent (Clavellina-Miller et al., 2016). Arguably, the tax reform had some success in its goal to increase tax collection. Tax revenues increased from an average of 14.2 per cent of GDP in the three years before the reform to an average of 17.3 per cent in the three years after the reform (Clavellina-Miller and Villarreal-Páez, 2016).

[6] We do not have the exact geographic polygons of localities with fewer than 1,000 inhabitants. Thus we cannot determine which localities with fewer than 1,000 people were split by the VAT discount line, and our calculation does not include those localities. However, Mexico is a predominantly urban country, with 87 per cent of its population living in localities with at least 1,000 inhabitants, according to the population census. So the exclusion of localities with fewer than 1,000 people should not significantly change the calculation.

(or countries), or a general VAT change in the entire country.[7] VAT reforms covering a whole country give consumers fewer opportunities to search for jurisdictions unaffected by the reform. As the VAT hike we study takes place at international borders, consumers may find that crossing to a neighbouring unaffected jurisdiction is relatively easy. Indeed, business owner associations and chambers of commerce intensely contested the 2014 VAT hike at Mexico's borders (Quesada, 2013). They organised demonstrations in bordering localities that registered attendance by the thousands (Villalpando, Breach and Figueroa, 2013). Their main concern was the loss of competitiveness relative to businesses on the other side of the international borders – especially on the US side, as the US sales tax in all bordering states stood below the discounted 11 per cent VAT rate. However, other concerns were mentioned, such as inflation or negative effects on the labour market (Fuentes, Brugues and Diaz-Bautista, 2013).

The social unrest regarding the VAT hike may be understood by looking at border region statistics. Let us focus on the Mexico–US border as more information is available on that than on the southern border. In 2013, there were close to 108 million (legal) crossings of people from Mexico to the United States (González-König, Fuentes Flores and Ruiz Ochoa, 2014). Data on the nationality of border crossers are not publicly available, but estimates indicate that around half of the crossings are made by Mexican nationals (Ghaddar and Brown, 2005a). Depending on the port of entry, from one-half to three-quarters of Mexican border crossers visit the US for shopping reasons (SANDAG, 2020). Most visits by Mexican border crossers last less than one day, and the visitor makes an average of 3.1 stops and returns to Mexico (Ghaddar and Brown, 2005b; SANDAG, 2020). Evidence of the importance of consumption by Mexicans in US border localities comes from the fact that these localities have relatively high per-capita sales compared with the national average but a relatively low per-capita income.[8] In this context, business owners on Mexico's side of the border will resent a shock that may be reflected in a rise in the price of their products, as competition on the other side of the border is heavy. Despite this, Mexico's Congress approved the VAT reform without significant changes. We know of no compensation offered to firms or residents at the border in the form of tax credits or price subsidies.

Regarding the context of the Mexican labour market, in the period we study, the conditions were relatively unfavourable toward workers. Historically, labour unions in Mexico have played an important role in raising workers' bargaining power, as indicated by the positive union wage premium shown in different studies (Fairris, 2007; Fairris, Popli and Zepeda, 2008; Varela Llamas et al., 2010; Gutiérrez Rufrancos, 2019). However, the strength of labour unions decreased during the 2000s and 2010s (Gutiérrez Rufrancos, 2019). In 2013, the share of union members in total employees stood at around 14 per cent in Mexico; this share experienced a declining trend in the 2010s. Panel (a) of Figure C1 in the online appendix shows Mexico's negative union density trend in the 2010s. Moreover, union density in Mexico is below the 17.5 per cent OECD average and well below the 35 per cent average of Western European countries. Another important determinant of Mexico's labour market is the minimum wage. Literature shows that the minimum wage in Mexico tends to push general wages upwards (Fairris et al., 2008; Campos-Vázquez and Rodas Milián, 2020; Campos-Vázquez, Delgado and Rodas, 2020). Many wages in the economy are set as multiples of the minimum wage. Thus, periods with rising minimum wages tend to be generally favourable for workers. Real minimum

[7] For instance, Kosonen (2015) studies a VAT reform that specifically targets hairdressers in Finland. Benzarti et al. (2020) study this reform plus reforms in the European Union. These affect most sectors of the economy or some commodities, or are sector-specific. Harju, Kosonen and Skans (2018), Ván and Olah (2018) and Benzarti and Carloni (2019) study a reform that targets restaurants across countries in Europe. Carbonnier (2008) examines a reform that changed the VAT rate across many sectors in France.

[8] Some examples are Calexico, CA: per-capita retail sales as a share of the national average = 1.41, per-capita income as a share of the national average = 0.46; Yuma, AZ: 1.29 versus 0.78; Nogales, AZ: 1.72 versus 0.47; El Paso, TX: 0.91 versus 0.67; McAllen, TX: 2.05 versus 0.69. In addition, estimates from surveys of Mexican shoppers indicate that the share of consumption from Mexicans in some border localities is important. Some examples are Yuma, AZ: 12.4% of total taxable sales; Cameron, TX: 16.6%; El Paso, TX: 12.7%; Webb, TX: 19.4% (Ghaddar and Brown, 2005a and 2005b).

wages stagnated in the 2000s and 2010s, as shown in Panel (b) of Figure C1. The minimum wage started to recover meaningfully only in 2018.

3 | DATA

This research relies on multiple (mainly) administrative data sources collected by Mexican and US agencies. Most data sets are publicly available. However, the data sets collected by *Banco de México* and *Comisión Nacional Bancaria y de Valores* are not (more on this below). Let us start by describing the Mexican data sets in this research.

Data on **prices** come from the *Índice de Precios al Consumidor* data set collected by *Instituto Nacional de Estadística y Geografía* (INEGI). This data set contains monthly information on the prices of nearly 300 products and services from 46 localities across Mexico. Of the primary Mexican data sources we use in this research, this is the only one that provides data at the *locality* level. The other data sets offer information at the *municipality* level. We discuss the implications of this in Section 4. In addition, as shown in Table C1 in the online appendix, the price data set is the most geographically constrained of the primary data sets we use.[9]

Data on **labour** outcomes come from the *Asegurados* data set collected by *Instituto Mexicano del Seguro Social* (IMSS). This data set contains monthly information on the universe of employees in formal private firms at the municipality level. We are especially interested in two variables in the data set: mean daily labour earnings and employment level. Daily earnings are obtained from the definition of *salario base de cotización* (SBC) in the data set. While *salario* is directly translated to English as 'wage', SBC is not precisely the usual price of labour per hour in the English-speaking world because, in Mexico, the shortest period of wage setting is on a daily basis. SBC does not include compensation from overtime work, bonuses or other types of compensation. Still, changes in SBC are compatible with changes in the price of labour (wage) or quantities (worked hours). Since the IMSS data set does not contain information on the number of hours worked, we complement our analysis with data from INEGI's *Encuesta Nacional de Ocupación y Empleo* (ENOE), the largest labour survey in Mexico, to study changes in wages versus quantities. We discuss this in more detail in Section 5.

Credit data come from the data sets collected by *Banco de México* and shared with *Comisión Nacional Bancaria y de Valores* (CNBV), which are the main financial market regulators in the country. The data sets contain data for every two months on the universe of active non-revolving consumer credit granted by financial institutions (commercial banks and Sofomes R).[10] A municipality-level data set of this credit is built using geographic information collected in administrative reports.[11] We are particularly interested in *créditos de nómina* (payroll credit) – i.e. credit whose repayments are discounted directly from workers' payrolls – as this closely reflects the credit market directly accessible to workers in the formal sector. However, we also analyse credit accessible to the general population. We focus on three outcomes: the number of awards of credit, their interest rate and the average amount of the credit.

Regarding the US data sets, we use annual data on **sales tax revenues** collected by the US Census Bureau. The data come from the Annual Survey of State and Local Government Finances. The survey covers all sources of revenue at the state, county and locality level. We use locality-level data. The survey covers all localities with more than 70,000 inhabitants. In addition, we use monthly

[9] We present a battery of robustness tests in Section B of the online appendix. Our main results are robust to a wide set of different specifications. This supports the results not being driven by the constraint of the price data set.

[10] Sofomes (*Sociedades Financieras de Objeto Múltiple*) are a specific kind of financial entity in Mexican financial regulation. These institutions can offer credit, leasing and factoring operations but cannot take deposits from the general public.

[11] We prefer to aggregate credit at the municipality level, not the locality level, because, according to CNBV personnel, banks in Mexico frequently register credit in the main municipality branch instead of the branch that granted the credit. So the locality level may be problematic for the estimation strategy we discuss in Section 4.

data on **border crossings** collected by the Bureau of Transportation Statistics. The data set contains information on all entries to the United States at the port-of-entry level.[12]

4 | METHODOLOGY

We use two variations of the difference-in-differences (DiD) methodology: the standard DiD described by Angrist and Krueger (1999) – we call this the 'static' difference-in-differences – and the 'dynamic' difference-in-differences. The static DiD allows us to get a point estimate of the policy effects. We compare this point estimate with the full-passthrough counterfactual. The dynamic DiD shows the difference in outcome between treatment and control groups at a given period. This is useful for examining how the effect is distributed across time and confirming common trends before the policy occurs.

Eligibility for the policy is conditioned on geographic location. Panel (a) of Figure 1 shows the geographic areas subject to the VAT discount before the 2014 reform.[13] The VAT reform affected the localities that lie inside those areas. The map shows that the VAT discount area in most regions consists of a 20km strip from the international borders. This challenges our estimations, as most of the Mexican data we use are provided at the municipality level, and the 20km VAT discount strip cuts through most municipalities at international borders. So for most municipalities at the border, part of the territory lies inside the discount area and another part lies outside. Suppose all the bordering municipalities were included in the treatment group. In that case, we risk including large swathes of economic activity not subject to the VAT discount before 2014 as part of the treatment. This would result in an underestimation of the true effects of the tax hike. Therefore, to form our treatment area, we keep just the municipalities at international borders where the majority (50 per cent or more) of the population live inside the VAT discount zone.[14] These municipalities are shown in red (darker greyscale shading) in Panel (b) of Figure 1.

The definition of the control area is also subject to consideration. The difference-in-differences methodology is valid only if the treatment and control groups show similar trends before the policy change. We expect common trends in places that are close to the treated municipalities. The area closest to a treated municipality is composed of the municipalities that are its neighbours. However, focusing on just the neighbours for the control group has some practical difficulties. As shown in Panel (b) of Figure 1, the municipalities contiguous to the treated municipalities contain relatively small urban areas. Outcome information from these municipalities is sometimes absent and highly volatile, leading to imprecise estimates. Moreover, people living in these municipalities may regularly visit the bordering municipalities, which may confound the effects we find. To overcome these problems, we extend the control area to all municipalities in states at the international frontiers where 0 per cent of the population lived in the VAT discount zone before the 2014 reform. The control municipalities are shown in blue (lighter greyscale shading) in Panel (b) of Figure 1.[15,16]

[12] Apart from the data sets mentioned above, we use INEGI's *Marco Geoestadístico* and the US Census Bureau's TIGER/Line Shapefiles to create the treatment and control areas described in Section 4 and Section A of the online appendix, as well as the maps included in this paper. Additionally, we get municipality- and locality-level control variables from INEGI's Population Census and the US American Community Survey for Mexico and the US, respectively.

[13] These areas are defined in Mexico's value added tax law, which can be consulted in Congreso de la Unión (2009).

[14] We show robustness tests using alternative treatment areas in Section B of the online appendix.

[15] We estimate our regressions with two alternative control areas: (1) the first- and second-degree neighbour municipalities of the treated municipalities; and (2) the municipalities located in international-bordering states but excluding the first- and second-degree neighbours of the treated. These alternative controls are shown in Figure C8. We explain this in more detail in Section B of the online appendix.

[16] We exclude the municipalities that display the largest variation in the employment outcome to address the noisiness in the employment series. The excluded municipality outliers are 24 out of 423 municipalities in the original treatment and control sample (6 per cent of the sample). The excluded outliers are relatively small, with an average population of 18,604 people (minimum of 2,235 and maximum of 71,432). The total population in these outlier municipalities is 446,503 people, i.e. 1.5 per cent of the 30,723,266 people living in the original treatment and control municipalities sample.

(a) VAT discount area before 2014

(b) Treatment and control areas

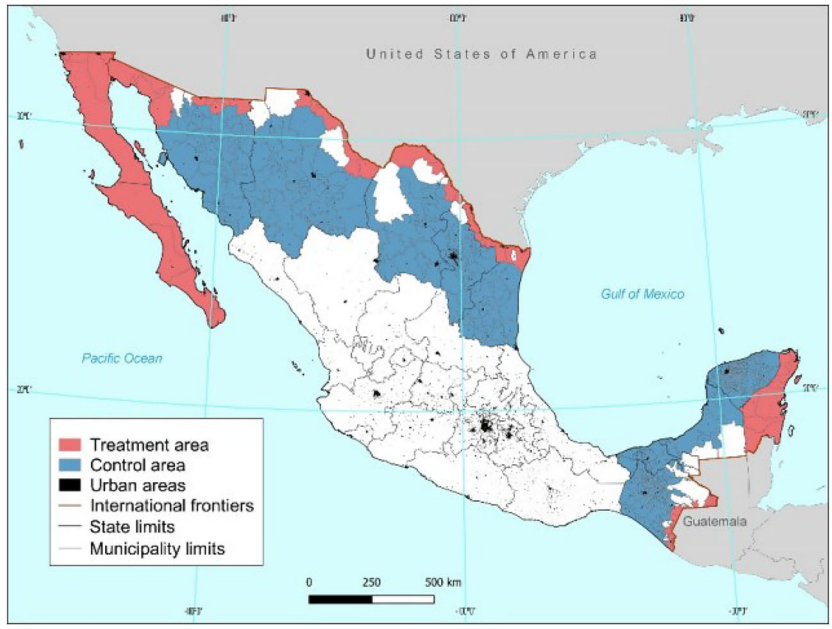

FIGURE 1 Geographical distribution of the policy. *Note*: Panel (a) shows the geographic areas subject to the VAT discount rate before 2014. The VAT rate was set at 11 per cent in the VAT discount areas and 16 per cent in the rest of the country. The 2014 VAT reform suppressed the discount zone and standardised the rate at 16 per cent nationwide. The reform took effect in January 2014. Panel (b) shows the treatment and control municipalities in our estimation strategy. The treatment municipalities (in red or darker greyscale shading) are those where 50 per cent or more of the municipality's population live in the VAT discount zone. Control municipalities (in blue or lighter greyscale shading) are those where 0 per cent of the population lives in the VAT discount zone and are located in international bordering states. *Source*: Value Added Tax Law (last reform: 7 December 2009) and *Marco Geoestadístico* from *Instituto Nacional de Estadística y Geografía* (INEGI). [Colour figure can be viewed at wileyonlinelibrary.com]

The static DiD equation we use to estimate the effect of the 2014 VAT tax hike is

$$Y_{jt} = \alpha + \beta M_j + \gamma D_t + \delta M_j \cdot D_t + \prod X_j \cdot T_t + \varepsilon_{jt}, \tag{1}$$

where Y_{jt} is the selected outcome at municipality j and time t.[17] $M = 1$ if municipality j is in the treated area, and $M = 0$ if municipality j is in the control area shown in Panel (b) of Figure 1. $D = 1$ if time $t \geq 2014$ (the VAT hike took effect in January 2014), and $D = 0$ otherwise. T_t are time dummies and X_j is a set of time-invariant municipality-level controls.[18] Standard errors ε_{jt} are clustered at the municipality level. The estimator δ from equation (1) provides a point estimate of the effect of the VAT hike on the outcomes we measure. In addition, we use the following equation to get the dynamic DiD estimators:

$$Y_{jt} = \alpha + \beta M_j + \gamma_t T_t + \sum_{t=\tau_0}^{t=-2} \delta_t T_t \cdot M_j + \sum_{t=0}^{t=\tau_1} \delta_t T_t \cdot M_j + \prod X_j \cdot T_t + \varepsilon_{jt}. \tag{2}$$

τ_0 and τ_1 are the lowest and highest number of lags and leads surrounding the period $t = 0$ when the VAT hike took effect. In this specification, coefficients δ_t capture the difference in the outcome between the treatment and control groups for a given time t. They also allow us to test for the parallel trends assumption before the VAT hike. The $t = -1$ time lag is dropped and used as the reference period to avoid perfect multicollinearity.[19]

5 | RESULTS

5.1 | Price effects

We start by describing the effect of the January 2014 VAT hike on prices. This effect has been previously analysed by Mariscal and Werner (2018).[20] They take the inflation rate as the outcome of interest and find that the reform had a positive but short-lived effect. We propose a different approach to study the impact of the VAT hike on prices, as not all products and services in Mexico are subject to VAT.[21] In general terms, the exemptions apply to food, non-alcoholic beverages, rent, mortgages, medicines, medical consultations, public transport, books and private schooling. So taking

[17] For the price outcome, j refers to the locality level. As for the units of time t, the price and labour outcome regressions are done quarterly and the credit regressions are done with half-year periods.

[18] Municipality-level control variables are the unemployment rate, the percentage of the total workforce employed in the formal sector and the total number of firms operating in a fixed address (public and private); and in the case of credit outcomes, the former plus the number of branches of financial institutions and the number of active credit cards. The municipality-level controls included in X_j are time-invariant as they are obtained from Census data updated every 10 years. We interact these time-invariant controls with time dummies. So, although we cannot estimate the coefficients of the time-invariant variables directly, we can account for the variables' time-varying effects.

[19] We study the period between the years 2012 and 2015, for the following reasons. (1) Pre-2012 data on credit outcomes are not fully comparable to the post-2012 period and present gaps. (2) The 2008–09 financial crisis impacted the border localities significantly more than the rest of the country due to the heavy presence on the border of the US-bound exporting *maquiladora* industry. Recovery from the crisis was slow, so pre-2012 data show a rebound effect from the aftermath. (3) In late 2015, the geographic zones that determined the minimum wage in Mexico were changed. These minimum wage zones overlap with an important share of municipalities with a discounted VAT rate before the 2014 reform. Hence, keeping the 2012–15 period allows us to single out the effect of the 2014 VAT hike in the treatment and control municipalities we use.

[20] The control group in Mariscal and Werner (2018) differs from ours. We take as control group the localities outside the VAT discount zone and inside states at the international borders. They take as control group all Mexico localities outside the VAT discount zone.

[21] Campos-Vazquez and Esquivel (2020) also use this approach to show that a mix of policies that included a VAT cut and minimum wage hikes in 2018 affected only the prices of products subject to VAT. The approach is also used by Calderón et al. (2023) to study the impact of the same policy package on prices.

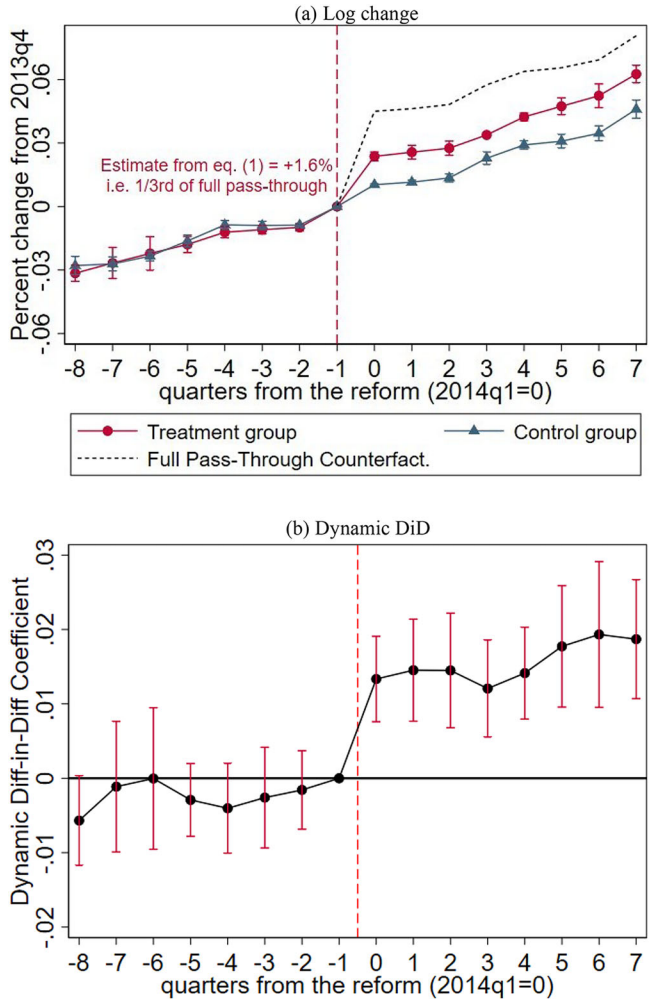

FIGURE 2 Graphical evidence of the effect of the VAT hike on prices of goods subject to VAT. *Note*: Panel (a) of this figure shows the log difference of the mean price of goods subject to VAT. The difference is taken with respect to the fourth quarter of 2013, i.e. the last period before the VAT hike took place. The means are taken at the municipality level across treatment and control areas. Panel (b) plots the estimates of parameters δ_t from equation (2) under the specification that includes control variables. The outcome is the logarithm of the mean price of goods subject to VAT at the municipality level. Treatment and control areas are shown in Panel (b) of Figure 1. Municipality-level control variables are the unemployment rate, the percentage of the total workforce employed in the formal sector and the total number of firms operating in a fixed address (public and private). *Source*: *Índice Nacional de Precios al Consumidor* database collected by *Instituto Nacional de Estadística y Geografía* (INEGI). [Colour figure can be viewed at wileyonlinelibrary.com]

the consumer price index (CPI) as an outcome may underestimate the actual effect of the tax hike.[22] To estimate the effect of the VAT increase on prices, our outcome is the average price of the products and services subject to VAT in the CPI data set.[23]

Panel (a) of Figure 2 shows visual evidence of the effect of the VAT hike on prices. It shows the average price log change with respect to the fourth quarter of 2013 – the period just before the VAT hike took place – of goods subject to VAT. Before the reform, we see non-statistically significant

[22] Indeed, around 68 per cent of the CPI is exempt from VAT. This does not mean that 68 per cent of the goods that compose the CPI are VAT-exempt. Instead, it means that 68 per cent of the goods' weights in the CPI are VAT-exempt.

[23] The list of all products and services included in this average price is shown in Section D of the online appendix.

TABLE 1 Effect of the VAT hike on prices and labour outcomes: static difference-in-differences.

	Outcome in logs		
	Prices	**Nominal labour compensation**	**Employment**
	(1)	**(2)**	**(3)**
Without controls			
Coefficient	0.016***	−0.021***	−0.002
	(0.003)	(0.008)	(0.021)
	0.009 to 0.022	−0.035 to −0.006	−0.044 to 0.040
R-squared	0.518	0.034	0.058
Observations	931	7,000	7,000
Including time-interacted controls			
Coefficient	0.018***	−0.018***	0.009
	(0.003)	(0.006)	(0.016)
	0.012 to 0.024	−0.031 to −0.006	−0.023 to 0.041
R-squared	0.827	0.464	0.886
Observations	931	7,000	7,000

Note: This table shows δ estimates from equation (1) for the price and labour outcomes. Column (1) refers to the mean price of goods subject to VAT. Column (2) refers to workers' mean nominal daily labour earnings in sectors whose final goods are subject to VAT. Column (3) refers to the level of employment in sectors whose final goods are subject to VAT. Municipality-level control variables are the unemployment rate, the percentage of the total workforce employed in the formal sector and the total number of firms operating in a fixed address (public and private). Panel (b) of Figure 1 shows the treatment and control groups. Standard errors are shown in parentheses. Confidence intervals are shown below standard errors.

differences in the log change between localities in the treatment and control areas. After the reform, the log change is larger in the treatment area than in the control area, and this effect lasts over time. In addition, the graph includes the estimate of δ from equation (1). The coefficient indicates that the VAT hike led to a 1.6 per cent price increase, as in column (1) of Table 1.

The table shows estimates of equation (1) under two specifications: without control variables and with time-interacted control variables.[24] In all specifications, the estimate of δ is positive and significant. The size of the effect is similar across specifications. Note that the VAT rate at the international borders went from 11 to 16 per cent in January 2014. Hence, full passthrough of the tax hike on prices would amount to a price increase of about 4.5 per cent.[25] So the reform's effect on prices is about one-third the size of the full-passthrough counterfactual. In addition, we show the δ_t estimates from equation (2) in Panel (b) of Figure 2. The graph shows that the coefficients are not statistically different from zero before the VAT hike of January 2014. After the reform, the dynamic DiD estimates are positive and statistically different from zero.

[24] Municipality-level control variables are the unemployment rate, the percentage of the total workforce employed in the formal sector and the total number of firms operating in a fixed address (public and private).

[25] Take y as the price including VAT and x as the non-VAT price. Take $t+1$ as the period after the VAT change and t as the period before the change. Then $y_t = 1.11x$ and $y_{t+1} = 1.16x$. The percentage change in y from period t to period $t+1$ is $\Delta\%y = 100 \times (y_{t+1} - y_t)/y_t = 100 \times (1.16x - 1.11x)/1.11x \approx 4.5$.

5.2 | Labour effects

For the labour outcomes, we take the workers employed in firms listed in sectors whose products and services are subject to VAT.[26] Using these workers, we construct the mean daily labour cost and the mean employment level in the treatment and control municipalities. Panel (a) of Figure 3 shows visual evidence of the effect of the January 2014 VAT hike on nominal mean labour cost. The left-hand plot displays the log change with respect to the fourth quarter of 2013. Before the reform, we see non-statistically significant differences in the log change between municipalities in the treatment and control areas. After the reform, the log change is larger in the control municipalities.[27]

This suggests a negative effect of the VAT hike on labour costs in sectors subject to VAT. The effect size from the static DiD estimator is around −2 per cent, as in column (2) of Table 1. Both specifications in the column are negative, significant and similar in size.[28] Note that the negative effect on labour cost is not as big as to lead to an *absolute decrease* in the treated areas. Instead, nominal labour costs increased, but more slowly than they counterfactually would have. This is in accordance with Mexican labour law, as wages in the country cannot be nominally adjusted downwards. In addition, the right-hand plot in Panel (a) of Figure 3 shows the dynamic DiD estimates from equation (2). Before the reform, the coefficients are not statistically different from zero, whereas the coefficients are negative in all periods after the reform.

We also have evidence of a negative effect of the VAT hike on *real* labour compensation. In Panel (b) of Figure 3, the left-hand plot shows that real labour compensation growth is slower in the treatment area than in the control area after the VAT reform.[29] As in the case of nominal earnings, the negative effect on real earnings does not translate into a decrease in real earnings after the VAT hike. Rather, real labour compensation remained stagnant in the treatment area for around one year following the VAT reform. The right-hand plot in Panel (b) of Figure 3 shows additional evidence of the negative effect of the VAT hike on real labour earnings. δ_t coefficients from equation (2) are negative after treatment and not different from zero before treatment. Panels (a) and (b) show compelling evidence of the negative effect of the VAT hike on labour cost. In Section 5.4, we present placebo tests that provide additional evidence.[30]

Moving to the employment outcome, Panel (c) of Figure 3 indicates no clear evidence of an effect on employment in sectors subject to VAT. All coefficients are not statistically different from zero in the pre- and post-treatment periods. This supports the assumption of common trends between the treatment and control groups. The right-hand plot shows an increasing trend in the dynamic DiD coefficients starting the second year following the reform. This may indicate some delayed effect on

[26] These sectors are listed in Section D of the online appendix.

[27] Note that the plot on the left-hand side shows that mean labour compensation increases at the start of the calendar year. Some reasons behind this are: (1) salaries are typically reviewed upward at the beginning of the year; (2) minimum wage increases occur in January; and (3) at the end of the year consumption surges and employment increases, while at the start of the year demand tightens, temporary end-of-the-year employees are not kept and mean labour earnings go up.

[28] For the labour cost and employment outcomes, municipality-level control variables are the same as we include in the price outcome. These are the unemployment rate, the percentage of the total workforce employed in the formal sector and the total number of firms operating in a fixed address (public and private).

[29] The distance between the treatment and control series after treatment is smaller for real labour earnings than for nominal labour earnings. The δ estimate from equation (1) is −1.5 per cent, i.e. about one-quarter smaller than the nominal earnings estimate. This is because inflation rates across the treatment and control areas differ. To obtain the inflation rate in each area, we took the mean price index for the localities that compose each area (weighted by locality population). This approximates the inflation rate in both areas because the price data set does not survey the universe of localities in the treatment and control areas.

[30] The VAT reform appears to have had an equalising effect regarding prices and labour compensation across the treatment and control areas. Panel (a) of Figure C2 in the online appendix shows the logarithm of the mean price of goods subject to VAT. After the VAT hike, mean prices in the treated localities start to catch up with those of the control localities. Panel (b) of Figure C2 shows the logarithm of mean nominal labour cost in sectors subject to VAT. The graph shows that labour compensation in the control municipalities was lower than in the treated municipalities before the reform. After the reform, labour compensation rose more rapidly in the control area than in the treatment area until the difference in labour compensation between the areas was not statistically significant. So the reform appears to have erased the relative attractiveness of the border region in terms of prices and labour compensation, bringing it in line with the rest of the country.

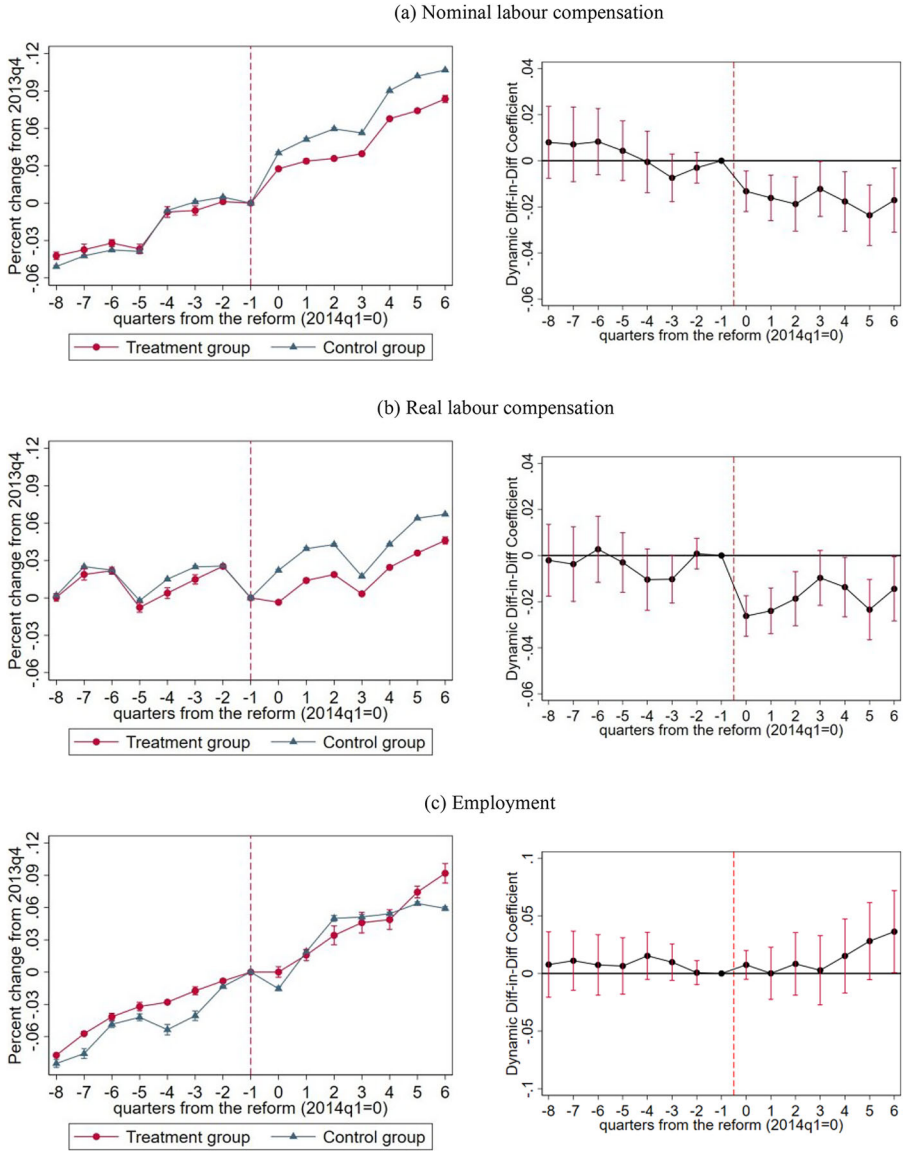

FIGURE 3 Graphical evidence of the effect of the VAT hike on labour outcomes in sectors subject to VAT. *Note*: This figure shows evidence of the effect of the 2014 VAT reform on nominal labour earnings in Panel (a), on real labour earnings in Panel (b) and on employment in Panel (c). Left-hand graphs show the log difference of the mean outcome value in sectors subject to VAT. The difference is taken with respect to the fourth quarter of 2013, i.e. the last period before the VAT hike. The means are taken at the municipality level across treatment and control areas. Right-hand graphs plot the estimates of parameters δ_t from equation (2) under the specification that includes control variables. Treatment and control areas are shown in Panel (b) of Figure 1. *Source*: *Índice Nacional de Precios al Consumidor* database collected by *Instituto Nacional de Estadística y Geografía* (INEGI) and *Asegurados* database collected by *Instituto Mexicano del Seguro Social* (IMSS). [Colour figure can be viewed at wileyonlinelibrary.com]

employment. However, the coefficients remain not different from zero. We take a cautious stance and interpret this as no evidence of an impact of the reform on employment. The results from equation (1) shown in column (3) of Table 1 support this. Neither of the coefficients in the column is statistically significant. This result gives evidence that the effect on labour earnings is not driven by workers moving between the treatment and control areas due to the reform, as the level of employment does

not change. In addition, as shown in Panel (b) of Figure C2 in the online appendix, mean daily labour earnings are higher in the treatment areas than in the control areas before and after the reform. So there is no earnings incentive for workers in the treatment areas to cross to the control areas around the time that the VAT reform took place.

Thus firms appear to have preferred to adjust labour compensation rather than employment following the VAT shock. Firms continued to hire the same number of positions as they would have without the VAT reform but paid lower compensation to workers than they counterfactually would. The negative effect on compensation may come from a negative effect on the price of labour (wage) or a negative impact on quantities (hours worked per employee). The labour cost variable we use refers to employees' daily earnings. It does not include compensation for overtime pay, bonuses or other types of compensation, i.e. it is the mean compensation to labour for a standard full-time or part-time working day. Firms in Mexico can only reduce the hours an employee works below the eight-hour full-time work shift by changing the full-time worker to a part-time worker. So, if the employment level remains constant, reductions in the labour cost are compatible with a decrease in the number of hours worked per employee – and not a decrease in the wage – if full-time jobs become part-time jobs.

The IMSS data set does not contain information on the number of hours worked or full-time versus part-time employment. To face this, we use data from *Encuesta Nacional de Ocupación y Empleo* (ENOE) produced by INEGI. This is the largest labour survey in Mexico. It is representative of a relatively small number of localities in our treatment and control areas.[31] However, it provides significantly more variables than the IMSS data set. In particular, it has information on the number of hours worked by the surveyed workers. In Panel (a) of Figure C3 in the online appendix, we show that the mean number of hours worked by employees in formal firms in sectors largely exposed to the VAT hike follows similar (flat) trends in the treatment and control areas, implying that labour supply in both the treatment and control areas was not vigorous. Panel (b) shows the dynamic DiD coefficients. These are not statistically different from zero both before and after the VAT hike. Therefore, as Mexican labour law makes it difficult for firms to adjust working hours downwards, and we have evidence that worked hours do not change, wages are the most likely channel that drives the negative effect on labour compensation. Specifically, firms appear to take a (counterfactual) bite on the wage they pay to workers.

5.3 | Credit effects

To complete our analysis of the internal effects of the reform, we study the credit specifically granted to workers in the formal sector – namely, payroll credit. Banks grant this credit to workers whose earnings are transferred to a salary account held in a bank, most commonly the same bank that grants the payroll credit. The bank discounts payments directly from the worker's account without her intervention. This credit reduces the problem of asymmetric information and the risk of default, as its granting is supported by a verifiable and steady flow of income in a salary account. Payroll credit is granted nearly exclusively to formal workers, as banks link their collection to an official payroll.[32]

We take as outcome the characteristics of the credit awarded in each period t (not the credit stock at period t). This allows us to analyse how credit is influenced by the context that happened at time t, and hence identify the effect of the VAT hike on the credit market. Panel A of Table 2 shows the δ estimate from equation (1) for payroll credit. Column (1) shows the effect of the VAT hike on the number of new awards of payroll credit. The effect is negative and significant in both equation (1) specifications. Column (2) shows the impact of the VAT hike on the average amount of these awards.

[31] Localities in the treatment area are Cancún, Ciudad Juárez, La Paz, Mexicali, Reynosa and Tijuana. Campeche, Chihuahua, Hermosillo, Mérida, Monterrey, Saltillo, Tampico, Torreón, Tuxtla Guitiérrez and Villahermosa are those in the control area.

[32] In the CNBV data sets, we cannot identify the economic sector of the firm where the credited worker is employed. Thus, our treatment group is the payroll credit awarded in the treated municipalities *regardless* of the worker's economic employment sector.

TABLE 2 Effect of the VAT hike on credits awarded at time *t*: static difference-in-differences.

	Outcome in logs		
	Number of awards	**Average amount of credit**	**Interest rate**
	(1)	**(2)**	**(3)**
Panel A. Payroll credit			
Without controls			
Coefficient	−0.111***	−0.011	0.002
	(0.015)	(0.010)	(0.004)
	−0.141 to −0.082	−0.030 to 0.008	−0.006 to 0.010
R-squared	0.079	0.234	0.076
Observations	4,143	4,143	4,143
Including time-interacted controls			
Coefficient	−0.090***	−0.022	0.001
	(0.015)	(0.012)	(0.006)
	−0.119 to −0.062	−0.046 to 0.002	−0.010 to 0.012
R-squared	0.829	0.407	0.197
Observations	4,135	4,135	4,135
Panel B. Personal credit			
Including time-interacted controls			
Coefficient	0.031	−0.031	−0.021
	(0.048)	(0.030)	(0.018)
	−0.064 to 0.125	−0.090 to 0.027	−0.056 to 0.013
R-squared	0.712	0.142	0.289
Observations	4,106	4,106	4,106

Note: This table shows δ estimates from equation (1). Column (1) refers to the number of new awards of credit at time *t*. Column (2) refers to the average amount of these awards at time *t*. Column (3) refers to the interest rate on these awards at time *t*. Panel A refers to payroll credit, i.e. credit whose repayments are discounted directly from workers' payrolls. Panel B refers to personal credit, i.e. credit granted to individuals by financial institutions where repayments are not discounted from workers' payrolls. Municipality-level control variables are the unemployment rate, the percentage of the total workforce employed in the formal sector, the total number of firms operating in a fixed address (public and private), the number of branches of financial institutions and the number of active credit cards. Panel (b) of Figure 1 shows the treatment and control groups. Standard errors are shown in parentheses. Confidence intervals are shown below standard errors.

Both specifications have coefficients that are not statistically different from zero. Finally, column (3) shows that the VAT hike did not affect the interest rate on these awards. Figure 4 shows the estimates from the dynamic DiD model of equation (2). Panels (a) and (b) show these estimates for payroll credit. In Panel (a), the outcome is the number of new awards of payroll credit. The graph shows that the DiD estimates are not statistically different from zero before the reform. After the reform takes place, these estimates become negative. Panel (b) indicates that the reform has no effect on the nominal interest rate on payroll credit.[33]

[33] The dynamic DiD effects shown in Figure 4 are more volatile than those of the price and wage outcomes. This is because of volatility in the credit due to two main factors. (1) the relatively low development of the credit market in Mexico. Only about 18 per cent of adults have borrowed from a financial institution, compared with about 65 per cent in the United States or around 30 per cent in Brazil or Chile (CNBV, 2021). Credit penetration in small localities is much lower due to the limited presence of bank branches and low internet connectivity. This leads to sporadic participation in credit markets. (2) The data we use are self-reported by banks. Personnel responsible for filling out the forms may not be correctly trained. We know of no sanctions for banks in the case of them misreporting data. These two factors may lead to measurement error. We estimate credit effects in half-year periods to reduce this volatility and weight our regressions by municipality population.

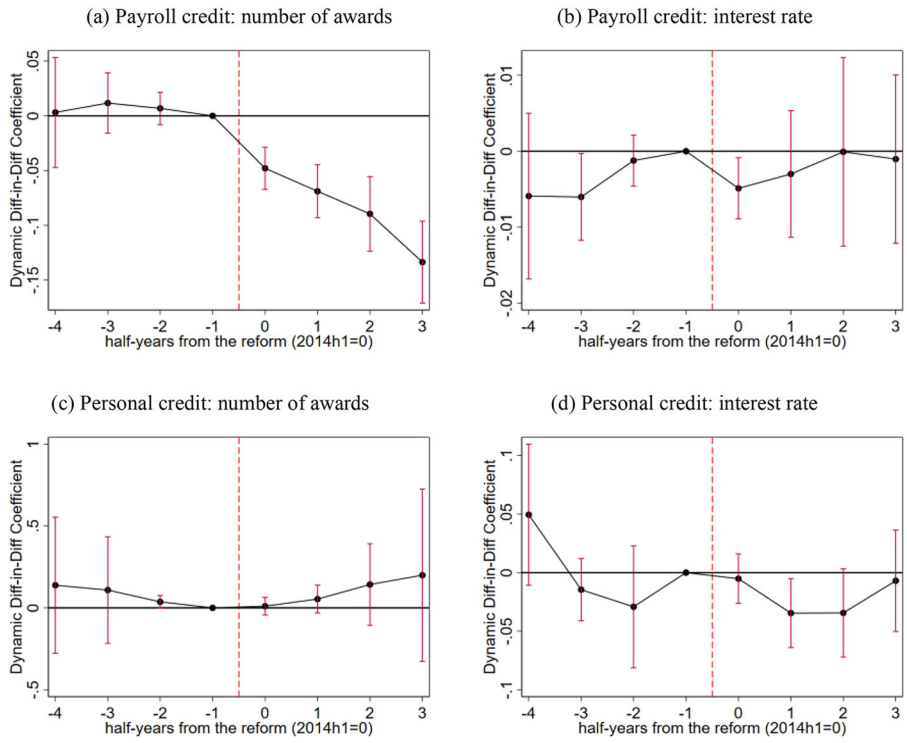

FIGURE 4 Graphical evidence of the effect of the VAT hike on credit awarded at time *t*. *Note*: This figure shows the effects of the VAT reform on credit awarded at time *t*. Panels (a) and (b) focus on payroll credit, Panels (c) and (d) on personal credit. Panels (a) and (c) show the effects on the number of credits granted in period *t*. Panels (b) and (d) show the effects on the interest rate for credit granted in period *t*. All panels show the estimates of parameters δ_t from equation (2) under the specification with control variables. Means are taken at the municipality level. Treatment and control areas are shown in Panel (b) of Figure 1. Municipality-level control variables are the unemployment rate, the percentage of the total workforce employed in the formal sector, the total number of firms operating in a fixed address (public and private), the number of branches of financial institutions and the number of active credit cards. *Source*: *Créditos al Consumo no Revolvente* database collected by *Comisión Nacional Bancaria y de Valores* (CNBV). [Colour figure can be viewed at wileyonlinelibrary.com]

In addition to payroll credit, we analyse the regular 'personal credit' granted by Mexico's financial institutions. Payment of this credit is not discounted from workers' payrolls. Any adult individual can demand or be offered these credits. Panel B of Table 2 shows the static DiD estimates for personal credit. The coefficients are not significant for the number, the amount or the interest rate for personal credit. Panels (c) and (d) of Figure 4 plot the dynamic DiD estimates and indicate that the reform does not affect this credit. This means that the impact of the reform on credit came from the payroll credit channel, i.e. credit that can be granted just to workers in the formal sector. The credit evidence we present reinforces the reform's effect on workers at the borders. The VAT hike can be interpreted as a negative permanent shock on workers' real income. From the demand side, workers may have adjusted their credit demand downwards to align with their new consumption and borrowing capacities. From the supply side, banks may see workers' payrolls as collateral for the payroll credit they grant.

We cannot deduce from our data whether the negative effect came from the demand or the supply side. However, we believe that the most likely scenario is the demand side, as workers' nominal compensation did not decrease due to the VAT hike; instead, it increased more slowly. So, on average, banks did not see a decrease in workers' payrolls, which may act as payroll credit collateral. From the point of view of a bank, it would be difficult to detect the damage in the collateral caused by the VAT reform. From either the demand or the supply side (or both), workers have fewer opportunities to

counteract the negative income shock (higher prices and counterfactually lower earnings) that appears to be caused by the VAT hike.

5.4 | Placebo estimations

To present additional evidence on the positive (negative) effect of the VAT hike on prices (labour compensation), we test whether the reform affected the sectors that are not subject to VAT. In the treatment area, these sectors do not receive treatment. So they act as a placebo group that serves as a point of comparison with the treated group. Panel (a) of Figure 5 shows visual evidence of the effect of the VAT hike on the prices of goods in these sectors. The left-hand graph shows that the percentage changes in prices are similar in the treatment and control areas before and after the reform, i.e. there is no jump in the log change in the treatment area at the time the reform takes place. This indicates that the reform did not affect the prices of goods not subject to VAT. The right-hand graph plots the δ_t parameters from the dynamic DiD. It confirms that the reform did not affect these prices, as the coefficients are not statistically different from zero both before and after the reform. In addition, Panel (b) of Figure 5 shows that the VAT hike did not affect the mean daily cost of workers employed in sectors not subject to VAT. The same is true for the level of employment, as shown in Panel (c). The static DiD estimates of these three outcomes are shown in Table 3. Coefficients in all specifications are not statistically different from zero.

The results from these placebo tests give empirical support to the choice of our treatment and control areas under the difference-in-differences empirical strategy. In the left-hand graphs of Figure 5, we see

TABLE 3 Effect of the VAT hike on prices of goods and labour outcomes in sectors that are not subject to VAT: static difference-in-differences.

	Outcome in logs		
	Prices	**Nominal labour compensation**	**Employment**
	(1)	**(2)**	**(3)**
Without controls			
Coefficient	0.001	−0.006	0.015
	(0.004)	(0.009)	(0.021)
	−0.007 to 0.010	−0.023 to 0.011	−0.026 to 0.056
R-squared	0.590	0.038	0.044
Observations	931	6,632	6,632
Including time-interacted controls			
Coefficient	0.002	−0.002	0.003
	(0.004)	(0.009)	(0.016)
	−0.007 to 0.010	−0.020 to 0.016	−0.028 to 0.033
R-squared	0.913	0.486	0.768
Observations	931	6,632	6,632

Note: This table shows δ estimates from equation (1) for the price and labour outcomes. Column (1) refers to the mean price of goods not subject to VAT. Column (2) refers to workers' mean nominal daily labour earnings in sectors whose final goods are not subject to VAT. Column (3) refers to the level of employment in sectors whose final goods are not subject to VAT. Municipality-level control variables are the unemployment rate, the percentage of the total workforce employed in the formal sector and the total number of firms operating in a fixed address (public and private). Panel (b) of Figure 1 shows the treatment and control groups. Standard errors are shown in parentheses. Confidence intervals are shown below standard errors.

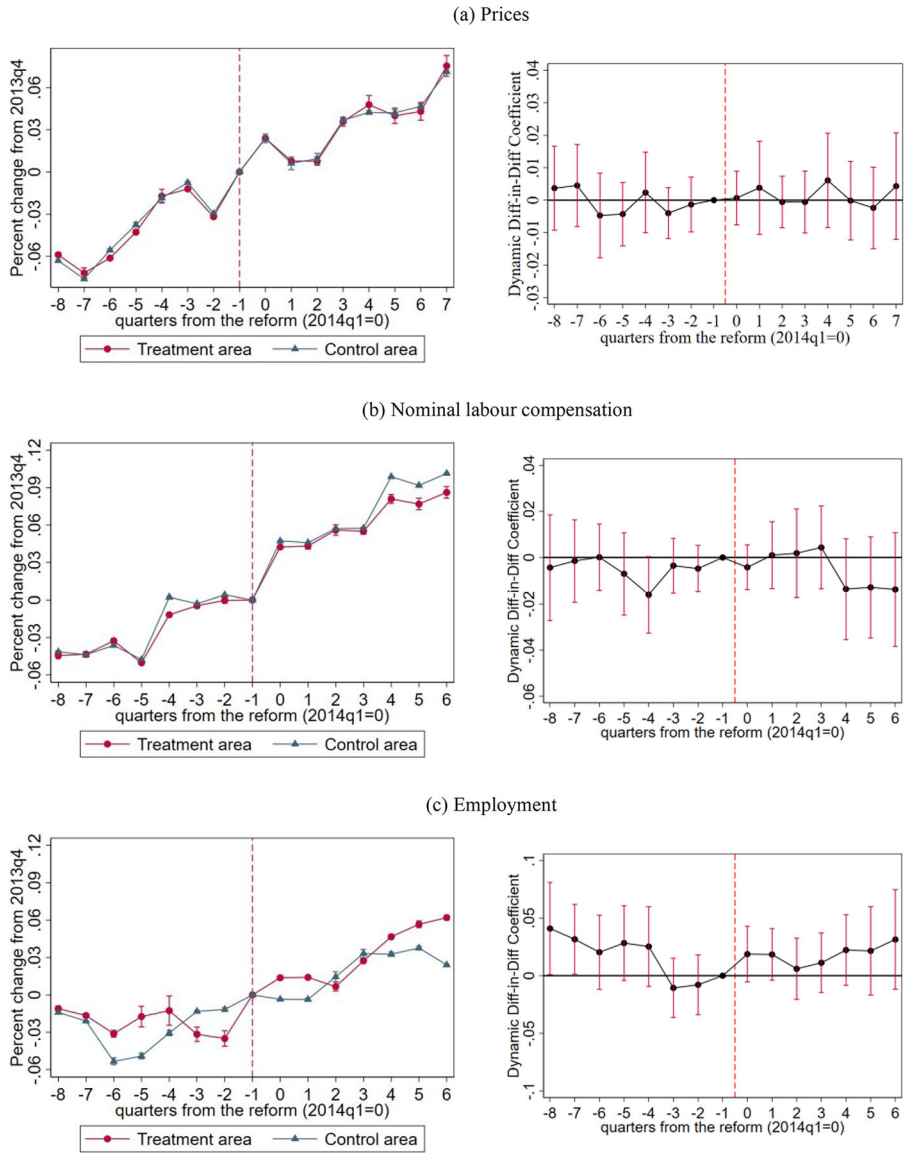

FIGURE 5 Graphical evidence of the effect of the VAT hike on goods and in sectors not subject to VAT. *Note*: This figure shows evidence of the effect of the 2014 VAT reform on prices in Panel (a), on labour earnings in Panel (b) and on employment in Panel (c). Left-hand graphs show the log difference of the mean outcome value in sectors not subject to VAT. The difference is taken with respect to the fourth quarter of 2013, i.e. the last period before the VAT hike. The means are taken at the municipality level across treatment and control areas. Right-hand graphs plot the estimates of parameters δ_t from equation (2) under the specification that includes control variables. Treatment and control areas are shown in Panel (b) of Figure 1. *Source*: *Índice Nacional de Precios al Consumidor* database collected by *Instituto Nacional de Estadística y Geografía* (INEGI) and *Asegurados* database collected by *Instituto Mexicano del Seguro Social* (IMSS). [Colour figure can be viewed at wileyonlinelibrary.com]

that, in the absence of treatment, the pre-treatment and post-treatment differences are the same for non-treated prices, non-treated labour cost and non-treated employment. So the common trends assumption on which the difference-in-differences estimation relies is empirically supported. In addition, the results from our placebo tests provide evidence in support of the post-treatment difference in Figure 2

(treated prices) and Panels (a) and (b) of Figure 3 (treated labour compensation) being caused by the VAT reform.[34] Additional robustness tests are presented in Section B of the online appendix.

5.5 | Incidence share decomposition

To put our price and labour effects into perspective, we follow Benzarti and Carloni (2019) and conduct an incidence share decomposition exercise to inspect how the VAT hike was shared between consumers, workers and business owners. We do not have firm data to inspect the effect of the VAT hike on profits, so our incidence decomposition exercise should be taken as a rough approximation. To approach the effect of the VAT hike on profits, we first take evidence from previous literature. We use three sources: (1) Benzarti and Carloni (2019) estimate a 9.3 per cent profit increase for a VAT cut of 14.1 percentage points (ppt); (2) Benzarti et al. (2020) estimate a 10 per cent decrease for a 14ppt VAT hike; and (3) Kosonen (2015) estimates a 15 per cent increase following a VAT cut of 14ppt. We adjust the effects these authors find proportionally to the 5ppt VAT hike we analyse. The proportional effects on profits are −3.3, −3.6 and −5.4 per cent. We use the effects on prices (1.6 per cent) and wages (−2.1 per cent) from our estimates in Table 1. To get the incidence share decomposition, we divide the absolute value of each coefficient by the sum of all coefficients in absolute value.

Panel A of Table 4 displays the VAT incidence share for consumers, workers and firm owners. The table indicates that consumers paid between 18 and 23 per cent of the VAT hike, workers paid 23 to

TABLE 4 Incidence share decomposition.

	Consumers	Workers	Firm owners
Panel A. Profit effect from previous literature			
Benzarti and Carloni, 2019	22.9	30.0	47.1
$d\ln r = 0.033$			
Benzarti et al., 2020	22.0	28.9	49.1
$d\ln r = 0.035$			
Kosonen, 2015	17.7	23.2	59.2
$d\ln r = 0.053$			
Panel B. Implied profit effect			
Ibarra and Ros, 2019	20.3	26.6	53.2
$\gamma = 0.296$			
Samaniego Breach, 2014	19.2	25.2	55.6
$\gamma = 0.418$			
Guerriero, 2019	18.5	24.2	57.3
$\gamma = 0.486$			

Note: This table presents how the VAT hike is split between consumers, workers and firm owners. Panel A obtains the incidence shares based on previous literature findings on the effect of VAT changes on profits. Panel B obtains the incidence shares using the relative loss formula in equation (3).

[34] Figure C4 in the online appendix shows evidence of the VAT hike effect on all goods and sectors of the economy. Panel (a) focuses on the price outcome. A comparison of Panel (a) in Figure C4, with respect to Panel (a) in Figures 2 and 5, indicates that the effect of the VAT hike on all goods is somewhere in between the effect on VAT-paying goods and that on non-VAT-paying goods. Indeed, the equation (1) point estimate for all goods is 1.1 per cent. This is smaller than the estimate of 1.8 per cent for goods subject to VAT (including controls). Panel (b) shows the effect on the labour compensation outcome in all sectors. The difference with respect to Panel (a) of Figure 3 is less noticeable than for the price case. However, the labour compensation equation (1) estimate for all sectors is −1.6 per cent. This is smaller than the −1.8 per cent estimate for sectors subject to VAT (including controls). These comparisons support that the effects we find in Figures 2 and 3 and Table 1 are due to the VAT hike. It is intuitive that the effects are largest for the VAT-paying goods and sectors, smaller for all sectors in the economy, and null for the sectors that do not pay VAT.

30 per cent and firm owners paid 47 to 59 per cent. This indicates that firm owners, followed by workers, bore the largest share of the VAT hike. However, the calculations should be taken cautiously, as we use the profit effects from VAT rate changes in contexts differing from ours in several dimensions: the country's income level, the geographic regions covered, the economic sectors, or the direction of the VAT change, among others.

A different approach to calculate the incidence share is to decompose the relative loss from the VAT hike for consumers, employees and capital owners as defined in Benzarti and Carloni (2019):[35]

$$\frac{d\ln p}{\Omega} - \frac{d\ln w}{\Omega} - (1-\gamma)\frac{d\ln r}{\Omega} = 1, \tag{3}$$

where $d\ln p = 0.016$, $d\ln w = -0.021$, γ is the labour share of total income, which we obtain from previous literature, and $\Omega = d\tau + \tau d\ln p$ scales the shares so that they add up to 1. The only unknown variable is $d\ln r$, which is the VAT hike's effect on profits. We can solve for $d\ln r$ in equation (3). We pick three different γ estimates: (1) Ibarra and Ros (2019) calculate $\gamma = 0.296$; (2) Samaniego Breach (2014) gets $\gamma = 0.418$; and (3) Guerriero (2019) estimates $\gamma = 0.486$.[36]

Panel B of Table 4 presents the incidence share decomposition when we obtain $d\ln r$ from equation (3) for different γs. The incidence shares are similar to those in Panel A. Consumers paid 19 to 20 per cent of the VAT hike, workers paid 24 to 27 per cent and firm owners paid 53 to 57 per cent. Thus, the calculations indicate that about half of the VAT hike was paid by firm owners, and the other half was split between workers and consumers. However, this evidence should be taken as suggestive because we do not know the exact effect of the 2014 VAT hike on profits; we only have rough approximations.

6 | MECHANISMS

Summing up the results: first, we find strong evidence that firms did not fully pass the VAT hike on to final prices; and second, we find that firms partly pass the VAT hike to labour via counterfactually lower compensation. This is probably facilitated by the relatively unfavourable labour market conditions for workers in the period we study. Let us discuss the mechanisms that may drive the results we find in our study context.

6.1 | Informality

We first describe the role of informality, a determinant of VAT incidence previously studied in middle-income countries (Bachas, Gadenne and Jensen, 2023).[37] Informal firms are not registered with the tax authority. So, when they make a sale, they do not collect VAT, which would later be passed on to the tax authority. Thus they may have fewer incentives to raise prices than their formal counterparts. Our price data are representative at the municipality level. Thus they include points of sale that may be informal.[38] To inspect informality as a mechanism, we conduct a heterogeneous analysis of the degree

[35] We adjust the formula slightly as Benzarti and Carloni (2019) also take into account sellers of material goods. We exclude them from our analysis.

[36] The labour share of income estimates vary widely mainly depending on how income from the self-employed is classified. The labour share increases the larger the earnings of the self-employed are assigned to labour.

[37] In Mexico, a firm (or a person) is considered formal if it is registered as a productive unit with *Servicio de Administración Tributaria* (SAT), the tax-collecting agency. All non-registered firms (or persons) are informal. Any worker engaged in labour activities in formal firms and registered at IMSS (the Social Security Institute) is a formal worker. Workers engaging at informal or formal firms but not registered at IMSS are informal (Ibarra-Olivo, Acuña and Espejo, 2021).

[38] The data for the other outcomes we measure – labour cost, employment and payroll credit – come entirely from the formal sector.

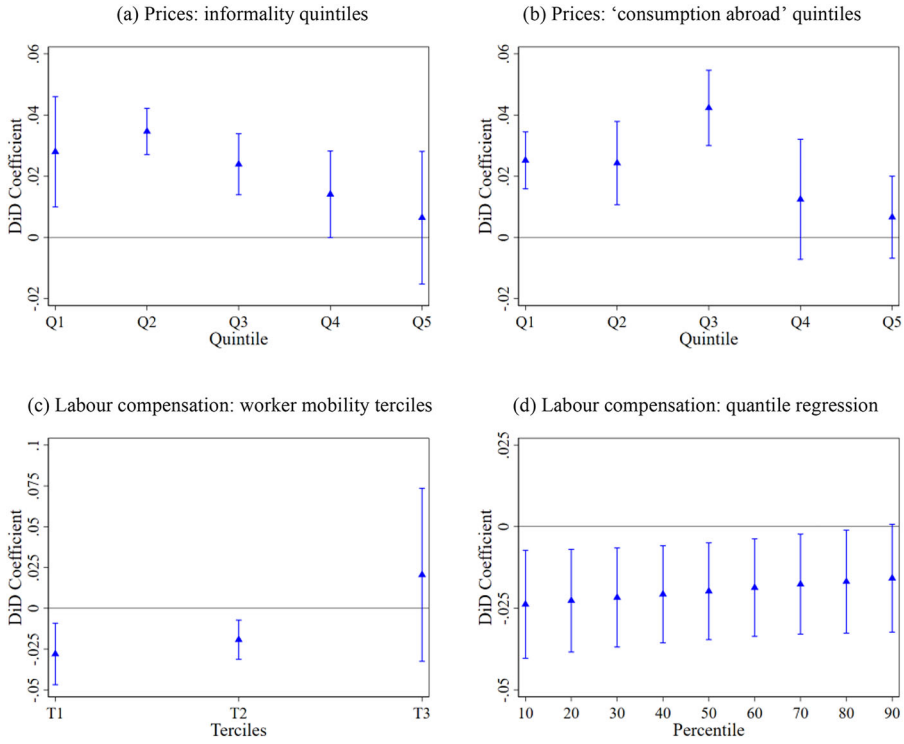

FIGURE 6 Passthrough determinants. *Note*: This figure plots the estimates of parameter δ_t from equation (1). In Panels (a) and (b), the outcome is the logarithm of prices of goods subject to VAT. In Panel (a), goods are ranked by quintile according to their likeliness of purchase at informal establishments. In Panel (b), goods are ranked by quintile according to their likeliness of purchase abroad. In Panels (c) and (d), the outcome is the logarithm of wages of workers employed in sectors subject to VAT. In Panel (c), economic sectors are ranked by tercile according to worker mobility. Panel (d) shows the estimates of a quantile difference-in-differences regression. *Source*: *Índice Nacional de Precios al Consumidor* database collected by *Instituto Nacional de Estadística y Geografía* (INEGI) and *Asegurados* database collected by *Instituto Mexicano del Seguro Social* (IMSS). [Colour figure can be viewed at wileyonlinelibrary.com]

of product exposure to purchase at informal establishments using data from *Encuesta Nacional de Ingresos y Gastos de los Hogares* (ENIGH), the largest household survey in Mexico. ENIGH asks households how frequently they consume products at different outlets. We obtain the percentage of times each product is purchased in flea markets or from street vendors in bordering states. This approximates each good's likeliness of being bought at informal establishments. We match the goods in ENIGH to those in *Índice Nacional de Precios al Consumidor* (INPC).[39] Then we rank INPC goods by 'informal outlet' quintile and estimate the effect of the VAT hike by quintile. If informality is a driving mechanism, we should find that goods in the highest quintiles see small or no price effects due to the VAT hike, while the lowest quintiles see relatively larger effects.

Panel (a) of Figure 6 shows the equation (1) DiD estimates for each quintile. The goods most likely purchased at informal establishments (fourth and fifth quintiles) do not show a statistically significant price effect due to the VAT hike. On the other hand, the lower quintiles (first to third) display a positive and statistically significant price effect. Our results support results by Bachas, Gadenne and Jensen (2023), indicating that informality is an important determinant of the VAT incidence in countries with high informality contexts. However, in all quintiles, the effect size is smaller than the full-passthrough

[39] We match 97 per cent of the INPC products subject to VAT with a corresponding product surveyed by ENIGH. ENIGH surveys 637 different products.

counterfactual. This means that firms do not pass on the VAT hike fully, even in the low informality extreme, which opens the door for other mechanisms driving our results.[40]

6.2 | Competition across the border

A relevant mechanism in the context we study relates to cross-border shopping: firms may resist increasing prices heavily, as the threat of losing consumption to competitors on the other side of the border is always looming. To inspect this mechanism, we follow a similar approach to the one we used for informality. For each surveyed good, ENIGH asks households how often they consume the good abroad. We obtain the percentage of times each product is consumed abroad by individuals in bordering states. Then we rank INPC goods by the 'consumption abroad' quintile.

Panel (b) of Figure 6 shows the DiD estimates from equation (1) by quintile. The estimates for products in the fourth and fifth quintiles are not statistically different from zero. These percentiles mostly comprise clothing, sports articles, computers, tablets and video games.[41] The first, second and third quintiles display a positive and statistically significant effect. These quintiles comprise goods mostly in the service sector (utilities, schooling, insurance, restaurants, beauty) and are thus not likely to be consumed across the border.[42]

The evidence presented here is consistent with the 'competition across the border' mechanism. Firms that are the least threatened by competition across the border respond to the VAT hike with the largest increases. Meanwhile, firms selling products that Mexican shoppers seek across the border are reluctant to increase prices. These results are consistent with the model presented by Carbonnier (2007). His model indicates that the price response due to VAT rate changes is decreasing on the product's demand elasticity with respect to prices. Firms selling goods with a high probability of being consumed abroad face a high elasticity of demand and are thus less responsive to changes in the VAT rate. Firms selling goods with low consumption abroad face a low elasticity of demand and are more responsive to changes in the VAT rate.

We can also inspect the cross-border competition mechanism by analysing variables related to cross-border shopping on the US side of the border.[43] We present this analysis in Section A of the online appendix. We use two outcomes that reflect changes in consumption: (1) sales tax revenues in US localities and (2) land crossings from Mexico to the United States. Our outcomes are proxies of consumption at US border localities, so the results should be taken as suggestive evidence. Nonetheless, we do not find evidence that the VAT hike impacted cross-border consumption. This is consistent with the small or null price increases due to the VAT hike for goods that cross-border shoppers demand.

[40] A back-of-the-envelope calculation can also indicate that informality is not the only determinant. In Mexico, the informal economy was responsible for around 25 per cent of the retail and wholesale sector output in our study period. If formal versus informal consumption in this sector is distributed as output, and informal firms do not move prices after the reform, then, to get the $\delta = 0.019$ equation (1) estimate on goods subject to VAT sold through retail or wholesale stores – which include formal and informal firms – formal firms would have to increase prices by about 2.5 per cent (we solve $0.75x + 0.25 \cdot 0 = 0.019 \Leftrightarrow x = 0.025$), which is only half of the 4.5 per cent full passthrough on prices.

[41] These coincide with the most desired products that Mexican cross-border shoppers cite in surveys that study their consumption habits at US ports of entry (Ghaddar and Brown, 2005a and 2005b; SANDAG, 2020).

[42] The goods in the consumption-abroad and informal-outlet quintiles are far from perfectly correlated. On average, across quintiles, 39 per cent of goods are in the same consumption-abroad and informal-outlet quintile. The lowest figure is 26 per cent and the highest is 45 per cent. Thus, we are not estimating the effect of the VAT hike on the same goods when we split them by exposure to cross-border competition or informality.

[43] We focus on the Mexico–US border because more data are available to study changes in demand in the US than in the two neighbouring countries of Mexico's southern border: Guatemala and Belize.

6.3 | Workforce mobility

We also study heterogeneous effects regarding the labour compensation outcome. Theoretical and applied research finds that workforce mobility relates positively to wages due to mobile workers having more bargaining power than less mobile workers and due to better employee–job matches in mobile economies or sectors (Yamaguchi, 2010; Eckstein, Ge and Petrongolo, 2011; Jinkins and Morin, 2018; Engbom, 2022). In the context we study, employees in sectors with low worker mobility may be relatively more exposed to a negative impact of the VAT hike on their wages, as they may have less bargaining power than employees in high-mobility sectors. To inspect this, we use the measure most related to worker mobility in *Encuesta Nacional de Ocupación y Empleo* (ENOE), the largest labour survey in Mexico. ENOE asks surveyed employees to declare if they moved from a different city to enter their current job. We rank ENOE's economic sectors by the share of employees declaring they moved to find their job. Then we divide the sectors into terciles, with the first tercile having the least worker mobility and the third tercile having the highest mobility.[44]

Panel (c) of Figure 6 shows the DiD effects on labour compensation by sector mobility tercile. The graph shows a negative and statistically significant effect on workers' compensation in the sectors with low mobility (first and second terciles). On the other hand, there is no effect for workers in high-mobility sectors (third tercile). This evidence supports worker mobility being a relevant mechanism for determining the impact of the VAT hike. Firms in mobile sectors may be reluctant to (counterfactually) decrease worker compensation following the VAT hike due to high exposure to workers changing jobs. Meanwhile, firms in low-mobility sectors are less exposed to workers quitting their jobs if wages (counterfactually) decrease.

6.4 | Worker earnings level

Another mechanism related to the wage outcome is explored using the differential effect of the VAT hike depending on workers' wage level. Literature indicates that higher-earning workers have more bargaining power to set their compensation (Piketty, Saez and Stantcheva, 2014; Rolim, Baltar and Lima, 2023). If this mechanism is relevant in our context, we should see that the effect of the VAT hike on wages is larger for low-wage workers. We use the difference-in-differences quantile regression proposed by Machado and Santos Silva (2019) to inspect this. Panel (d) of Figure 6 shows the DiD estimates for different percentiles of the wage distribution. The graph shows that the DiD point estimate is larger (in absolute terms) for the lower wage percentiles and smaller for the higher wage percentiles. The estimates across percentiles are not statistically different from each other. Nonetheless, our DiD quantile regressions give some evidence that workers with higher earnings are better able to stop the VAT hike from harming their compensation.

6.5 | Discussion

Informality is an important determinant of the VAT passthrough to prices. However, the determinants we find in our study context go beyond informality. Firms are reluctant to raise the prices of goods exposed to heavy cross-border competition. On the other hand, firms appear to be reluctant to (counterfactually) decrease wages in high-worker-mobility sectors. Workers with higher earnings also seem to be relatively more protected from the negative effects of the VAT hike. Previous literature indicates that firms use prices to preserve or increase their profits when they face changes in the VAT

[44] We divide sectors by tercile because sector disaggregation is low in ENOE, consisting of only 21 different sectors. Dividing sectors into more groups leads to loss of power to detect effects.

rate (Kosonen, 2015; Benzarti et al., 2020). The mechanisms we find are consistent with this, as firms appear to set the price of goods and labour differently according to their ability to do so.

7 | CONCLUSION

Overall, the findings in this paper support literature that indicates that changes in VAT affect outcomes beyond prices and quantities. In particular, we have shown compelling evidence that the VAT hike at the Mexican borders imposed costs on workers that they would otherwise not have endured. Lustig, Pessino and Scott (2014) show that indirect taxes in Mexico are regressive. So, once VAT is paid, the net negative effect on low-income workers may be higher than we find. Legislators and policymakers should weigh increased tax revenues from VAT hikes against possible pernicious effects on different social groups. The results from this research may be relevant not only to regions near borders but also to other contexts where firms are exposed to a high degree of competition.

ACKNOWLEDGEMENTS

We thank Aaron Zaragoza, Alejandro Lapuente and Miguel Rosales for their excellent research assistance. In addition, we appreciate insightful comments by Antoine Bozio, Jimmy Lopez, Loriane Py, Daniel Miranda, Nora Lustig, Clément Carbonnier, Susan Parker, Tuomas Kosonen, Raymundo Campos-Vázquez, Irvin Rojas, Fernanda Márquez, Fausto Hernández, David Margolis, Ariell Reshef, Oliver Vanden Enyde, Anne-Célia Disdier, François Fontaine, Elena Stancanelli, Clément Malgouyres, Laurent Gobillon, Stéphane Gauthier, Thomas Piketty, Guillermo Cruces, Vedanth Nair, Jaime Millán-Quijano, Pedro Hancevic, John Scott, Luz Marina Arias, Hernán Bejarano, Laura Juárez, Horacio Sobarzo, Anna Jacobs, Alberto Aguilar, Carmel Connell, Héctor Moreno, Christian Abele, Salvatore Carrozo, Alvaro Zuñiga-Cordero, Guillermo Woo-Mora and seminar and conference participants at various forums. Administrative data collected by Comisión Nacional Bancaria y de Valores (CNBV) were handled by Cristobal Domínguez, following CNBV's guidelines to protect data security. This paper's comments, opinions and conclusions are our own and do not reflect those of CNBV.

DATA AVAILABILITY STATEMENT

Most of the data used in this research are publicly available and can be shared upon request. However, the credit data have access restrictions. We cannot provide these data, but we can provide the codes created to obtain the calculations upon request.

ORCID

Emmanuel Chávez https://orcid.org/0000-0002-4387-5224

REFERENCES

Angrist, J. D. and Krueger, A. B. (1999), Chapter 23 - empirical strategies in labor economics. In O. C. Ashenfelter and D. Card (eds), *Handbook of Labor Economics*, 3, 1277–366, Elsevier.

Aportela, F. and Werner, A. (2002), La reforma al impuesto al valor agregado de 1995: efecto inflacionario, incidencia y elasticidades relativas. Documento de Investigación 2002-01, Banco de México.

Asplund, M., Friberg, R. and Wilander, F. (2007), Demand and distance: evidence on cross-border shopping. *Journal of Public Economics*, 91(1), 141–57.

Bachas, P., Gadenne, L. and Jensen, A. (2023), Informality, consumption taxes, and redistribution. *Review of Economic Studies*, 91(5), 2604–34.

Bachas, P., Jensen, A. and Gadenne, L. (2024), Tax equity in low- and middle-income countries. *Journal of Economic Perspectives*, 38(1), 55–80.

Benedek, D., De Mooij, R. A., Keen, M. and Wingender, P. (2020), Varieties of VAT pass through. *International Tax and Public Finance*, 27(4), 890–930.

Benzarti, Y. and Carloni, D. (2019), Who really benefits from consumption tax cuts? Evidence from a large VAT reform in France. *American Economic Journal: Economic Policy*, 11(1), 38–63.

Benzarti, Y., Carloni, D., Harju, J. and Kosonen, T. (2020), What goes up may not come down: asymmetric incidence of value-added taxes. *Journal of Political Economy*, 128(12), 4438–74.
Braxton, J. C., Herkenhoff, K. F. and Phillips, G. M. (2020), Can the unemployed borrow? Implications for public insurance. National Bureau of Economic Research, Working Paper 27026.
Calderón, M., Cortés, J., Pérez Pérez, J. and Salcedo, A. (2023), Disentangling the effects of large minimum wage and VAT changes on prices: evidence from Mexico. *Labour Economics*, 80, 102294.
Campos-Vazquez, R. M., Delgado, V. and Rodas, A. (2020), The effects of a place-based tax cut and minimum wage increase on labor market outcomes. *IZA Journal of Labor Policy*, 10(1).
Campos-Vazquez, R. M. and Esquivel, G. (2020), The effect of doubling the minimum wage and decreasing taxes on inflation in Mexico. *Economics Letters*, 189, 109051.
Campos-Vázquez, R. M. and Rodas Milián, J. A. (2020), El efecto faro del salario mínimo en la estructura salarial: evidencias para México. *El Trimestre Económico*, 87(345), 51–97.
Carbonnier, C. (2007), Who pays sales taxes? Evidence from French VAT reforms, 1987–1999. *Journal of Public Economics*, 91(5–6), 1219–29.
Carbonnier, C. (2008), Différence des ajustements de prix à des hausses ou baisses des taux de la TVA: un examen empirique à partir des reformes françaises de 1995 et 2000. *Economie et statistique*, 413, 3–20.
Clavellina-Miller, J. L., Maya-Hernandez, S. O., Morales-Cisneros, G. and Ramirez-Carvajal, L. E. (2016), *Retos para el Éxito de la Reforma Hacendaria*. Reformas Estructurales: Avances y Desafíos. Instituto Belisario Domínguez.
Clavellina-Miller, J. L. and Villarreal-Páez, H. J. (2016), A tres años de la reforma fiscal… los pendientes. *Pluralidad y Consenso*, 6(28).
CNBV (2021), Encuesta nacional de financiamiento de las empresas, 2021. Dataset, Comisión Nacional Bancaria y de Valores.
Congreso de la Unión (2009), Ley del impuesto al valor agregado. Ultima Reforma 07-12-2009, Cámara de Diputados del Congreso de la Unión.
Davis, L. W. (2011), The effects of preferential VAT rates near international borders: evidence from Mexico. *National Tax Journal*, 64(1), 85–104.
Eckstein, Z., Ge, S. and Petrongolo, B. (2011), Job and wage mobility with minimum wages and imperfect compliance. *Journal of Applied Econometrics*, 26(4), 580–612.
Engbom, N. (2022), Labor market fluidity and human capital accumulation. National Bureau of Economic Research, Working Paper 29698.
Fairris, D. (2007), ¿Qué hacen los sindicatos en México? *Estudios Económicos*, 22(2), 185–240.
Fairris, D., Popli, G. and Zepeda, E. (2008), Minimum wages and the wage structure in Mexico. *Review of Social Economy*, 66(2), 181–208.
Fuentes, N. A., Brugues, A. and Diaz-Bautista, A. (2013), Impactos de la homologación del IVA en baja california. Reporte, Colegio de la Frontera Norte.
Gaarder, I. (2018), Incidence and distributional effects of value-added taxes. *Economic Journal*, 129(618), 853–76.
Ghaddar, S. and Brown, C. (2005a), The cross-border Mexican shopper: a profile. *Research Review*, 12(2), 46–50.
Ghaddar, S. and Brown, C. J. (2005b), The economic impact of Mexican visitors along the US-Mexico border: a research synthesis. University of Texas-Pan American.
Gonzalez-König, G., Fuentes Flores, N. and Ruiz Ochoa, W. (2014), Fuga de consumidores en la franja fronteriza norte: tendencias a la luz de los ajustes fiscales recientes. *Realidad Económica*, 19(41), 9–14.
Guerriero, M. (2019), The labor share of income around the world: evidence from a panel dataset. In G. Fields and S. Paul (eds), *Labor Income Share in Asia: Conceptual Issues and the Drivers*, 39–79, Springer Singapore.
Gutiérrez Rufrancos, H. (2019), Are there gains to joining a union? Evidence from Mexico. *British Journal of Industrial Relations*, 57(3), 676–712.
Harju, J., Kosonen, T. and Skans, O. N. (2018), Firm types, price-setting strategies, and consumption-tax incidence. *Journal of Public Economics*, 165, 48–72.
Horvath, A., Kay, B. and Wix, C. (2023), The COVID-19 shock and consumer credit: evidence from credit card data. *Journal of Banking & Finance*, 152, 106854.
Hundtofte, C. S., Olafsson, A. and Pagel, M. (2019), Credit smoothing. National Bureau of Economic Research, Working Paper 26354.
Ibarra, C. A. and Ros, J. (2019), The decline of the labor income share in Mexico, 1990–2015. *World Development*, 122, 570–84.
Ibarra-Olivo, E., Acuña, J. A. and Espejo, A. (2021), Estimación de la informalidad en México a nivel subnacional. Documentos de Proyectos LC/TS.2021/19, Comisión Económica para América Latina y el Caribe (CEPAL).
Jinkins, D. and Morin, A. (2018), Job-to-job transitions, sorting, and wage growth. *Labour Economics*, 55, 300–27.
Kosonen, T. (2015), More and cheaper haircuts after VAT cut? On the efficiency and incidence of service sector consumption taxes. *Journal of Public Economics*, 131, 87–100.
Leal, A., López-Laborda, J. and Rodrigo, F. (2010), Cross-border shopping: a survey. *International Advances in Economic Research*, 16, 135–48.
Lustig, N., Pessino, C. and Scott, J. (2014), The impact of taxes and social spending on inequality and poverty in Argentina, Bolivia, Brazil, Mexico, Peru, and Uruguay: introduction to the special issue. *Public Finance Review*, 42(3), 287–303.
Machado, J. A. and Santos Silva, J. (2019), Quantiles via moments. *Journal of Econometrics*, 213(1), 145–73.

Mariscal, R. and Werner, A. (2018), The price and welfare effects of the value-added tax: evidence from Mexico. International Monetary Fund, Working Paper WP/18/240.
Núñez Joyo, R. H. (2017), *Ensayos sobre Economía Laboral Mexicana*. PhD thesis, Centro de Estudios Económicos, El Colegio de México.
Piketty, T., Saez, E. and Stantcheva, S. (2014), Optimal taxation of top labor incomes: a tale of three elasticities. *American Economic Journal: Economic Policy*, 6(1), 230–71.
Quesada, J. D. (2013), Peña nieto pone en jaque el régimen económico de las fronteras mexicana. *El País*, 30 October, https://elpais.com/internacional/2013/10/30/actualidad/1383116439_167910.html.
Rolim, L. N., Baltar, C. T. and Lima, G. T. (2023), Income distribution, productivity growth, and workers' bargaining power in an agent-based macroeconomic model. *Journal of Evolutionary Economics*, 33(2), 473–516.
Samaniego Breach, N. (2014), La participación del trabajo en el ingreso nacional: el regreso a un tema olvidado. *Economia UNAM*, 11(33), 52–77.
SANDAG (2020), Cross-border travel behavior survey: summary report. True North Research, Inc.
Ván, B. and Olah, D. (2018), Does VAT cut appear on the menu? The consumer price impact of Hungary's VAT decreases of 2016–2017. *Public Finance Quarterly*, 63(3), 355–75.
Varela Llamas, R., Ocegueda Hernández, J. M., Castillo Ponce, R. A. and Huber Bernal, G. (2010), Determinantes de los ingresos salariales en México: una perspectiva de capital humano. *Región y sociedad*, 22(49), 117–42.
Villalpando, R., Breach, M. and Figueroa, C. (2013), Integran un frente común contra el aumento al IVA en la frontera norte. *La Jornada*, 20 October, https://www.jornada.com.mx/2013/10/20/politica/004n2pol.
Walsh, M. J. and Jones, J. D. (1988), More evidence on the 'border tax' effect: the case of West Virginia, 1979–84. *National Tax Journal*, 41(2), 261–5.
Yamaguchi, S. (2010), Job search, bargaining, and wage dynamics. *Journal of Labor Economics*, 28(3), 595–631.

SUPPORTING INFORMATION

Additional supporting information can be found online in the Supporting Information section at the end of this article.

How to cite this article: Chávez, E., & Domínguez, C. (2025), Who pays for a VAT hike at an international border? *Fiscal Studies*, 46, 185–210. https://doi.org/10.1111/1475-5890.12400