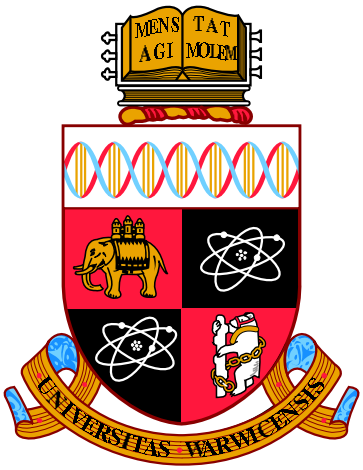

Three Essays in Applied Microeconomics

by

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Thesis

Submitted to the University of Warwick

for the degree of

Doctor of Philosophy in Economics (Research)

Department of Economics

October 2022

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Acknowledgments

I am indebted to my thesis advisors, Lucie Gadenne and James Fenske, for their constant guidance, patience and support throughout these challenging but rewarding years. Thank you once again.

I am grateful to the faculty of the Economics Department at Warwick for its insightful feedback and suggestions. I want to especially highlight the contributions of Roland Rathelot, Manuel Bagues, Robin Naylor and Emma Duchini. I would also like to thank Maryanne Heafey, Natalie Deven and Colin Ellis for their helpful administrative work and for always greeting us, miserable PhD students, with a smile.

One of the most exciting, and at times adventurous, parts of my PhD involved regularly traveling to Mexico, which gave me the opportunity to meet wonderful people who have contributed to this thesis in one way or another. Firstly, to my other two co-authors, Pierre Bachas and Anders Jensen, thank you for welcoming me to the project as an equal and for teaching me how to become a better researcher by being constantly curious. I am also grateful to Natalia Volkow and all her team at INEGI for providing vital data used in this study. I want to especially thank Lidia Hernández Hernández, Liliana Martínez Matías and María de Lourdes Garrido Blancas, as I could not have finished this project without their support. Within the Banco de México, where I spent a summer as an intern, I acknowledge the immense contributions of Fernando Pérez Cervantes, Gerardo Avilez Alonso, Santiago Bazdresch, Josué Cortés Espada, Cecilia García Medina, Rosa Marina González Sámano, Marine André, Jorge Pérez Pérez, Diego Solórzano and Claudia Velázquez Villegas.

To the friends I made at Warwick, I still cannot believe I found (semi-) sane people at a PhD in Economics. Thank you to my London crew: Ashish Aggarwal, Diego Calderón

Guajardo and Alexandra Muñoz. You have been my rock and the frequent providers of fun in this usually tense period. Thank you to Daniel Habermacher and his family for giving me perspective on what is important. Carlos Cardona Andrade and Apurav Bhatiya, I am grateful for your refreshing optimism in a place like Coventry. Karmini Sharma, you might be the only person in the world who thinks I am chill, thanks for your alternative point of view. To Andrés Lajer Baron, despite your political leanings, thank you for the regular doses of common sense. Giulia Vattuone, thank you for reminding me to be kind, for the laughter, the trips and the invaluable lessons in Italian culture. Riccardo Di Leo, thank you for understanding all my pop culture references and laughing about them with me. And thank you to the town of Grottaglie for being the birthplace of Eleonora Alabrese, a fantastic economist/chef/ambassador of Apulia.

I would also like to thank the friends who knew me before I started this PhD and who have stuck with me throughout this time. In particular, Martina Kljenak, Juan Martín Starobinsky and Mariano Molina, thank you for lending an ear when I needed to vent. Thanks to my friends from UdeSA and the IADB doing PhDs (or with partners doing PhDs) elsewhere for letting me know that we were all on the same boat, just many miles apart.

With this thesis, I finish a learning process I started on my first day of school when I was 4 years old. I would like to thank the little girls who grew up beside me and who became the women hugging me goodbye when I left for England 6 years ago. Thank you, Tortugas, for being there for me all along. I am especially grateful for the support, love and jokes provided by María Gracia Bianchi, Teresa Caride, Atalía María Delfino, María Sol Gilges, Victoria Kennedy and Agustina Ovejero. To Victoria Torrendell, thank you for cheering me up throughout this PhD and for always having faith that I would finish it; you helped in making it happen.

Finally, I need to acknowledge my family. To my brother Fernando and his partner Paula, thanks for distracting me away from my research with cute pictures of Matias, and thanks to my brother Martín for inviting himself over to lunch at my place and kindly lying about the quality of my cooking. To my parents, whose health has significantly deteriorated worrying about my poor living standards and my safety during this PhD, thank you. I am grateful for your love, your financial aid offers (I still do not need them!), the five-star treatment whenever I arrived in Buenos Aires and the meet-ups around Europe. I love you and I dedicate this thesis to you.

Declarations

This thesis is submitted to the University of Warwick in accordance with the requirements of the degree of Doctor of Philosophy. I declare that any material contained in this thesis has not been submitted for a degree to any other university. Chapter 1 is my own, single-authored work. Chapter 2 is collaborative work with Pierre Bachas, Lucie Gadenne and Anders Jensen. Chapter 3 has also been written solely by me.

Mariana Racimo

Abstract

This thesis consists of three chapters in Applied Economics, which address three different research questions, using government data from the United Kingdom and Mexico.

Chapter 1 examines the impact of an English university reform on educational choices. Using a difference-in-differences approach comparing English-born individuals to other UK nationals before and after the reform, I find that the probability of having vocational secondary school as the highest education level increases between 3 and 6 percentage points once tuition fees in England increase from £3,000 to £9,000. Additionally, I implement a regression discontinuity design to analyze how students in secondary school modify their education content in response to the reform. The first cohort exposed to the modified university fees increased the amount (0.165) and proportion (2.8 percentage points) of secondary school-level vocational qualifications, as well as the probability of attending a vocational course during this stage (7 percentage points). While existing studies have shown that tertiary education choices are responsive to university fees, my findings indicate that these price modifications also alter decisions earlier in the education path, presenting a broader scope of impact for higher education reforms.

In Chapter 2, along with my co-authors, I assess the incidence of tax changes in economies where a large share of agents are in the informal sector and therefore do not remit taxes. We study this issue by looking at a large increase in the Value-Added-Tax (VAT) in Mexico in 2014 which only affected areas close to international borders. Using a difference-in-differences approach and rich data on retailers and information on store type to proxy for informality, we consider how the tax increases affected outcomes in the informal and formal retail sectors. We find a 38% pass-through of taxes to formal prices that persists for at least two years, and a smaller (18%) pass-through of taxes to informal prices. Suggestive evidence indicates that informal firms' sales, profits and number of employees increased thanks to the reform, consistent with the idea that the informal sector gains when taxes increase.

Finally, in Chapter 3, I explore how the increase in Mexican VAT studied in Chapter 2 affected electoral outcomes. During 2013, the Mexican president, Enrique Peña Nieto, proposed the equalization of the VAT rate to the General Congress. His political party, the Institutional Revolutionary Party (PRI), held the majority in both chambers, which voted in favor of the law modification. Using a difference-in-differences specification, I examine whether the PRI experienced a change in support in the areas where the tax rate was increased from 11% to 16%. In particular, I analyze the change in vote shares in federal deputy elections, which take place every 3 years. I find that relative to areas where the VAT rate remained at 16%, support for the PRI did not significantly decline in treated regions a year and a half into its implementation. However, I present suggestive evidence that, 4 and a half years after the increase, vote shares for the PRI were reduced in treated areas by 3.2 percentage points.

Chapter 1

The Effect of Increasing University Fees on Vocational Education: Evidence from the UK

This work was produced using statistical data accessed via the ONS Secure Research Service. The use of this data in this work does not imply the endorsement of the ONS in relation to the interpretation or analysis of the statistical data. This work uses research datasets which may not exactly reproduce National Statistics aggregates.

1.1 Introduction

The debate on higher education funding has led a number of OECD countries to shift the burden of university funding from the taxpayer to the beneficiary (see for example, Hübner, 2012; Dearden, Fitzsimons & Wyness, 2014). However, the impact of these reforms on other types of education remains largely unexplored. In the UK, vocational qualifications have been shown to represent a profitable investment for many students, including those of lower ability (Dearden et al. 2002). As vocational qualifications become increasingly valuable in the labour market and resilient in times of economic downturns (OECD, 2019), it is important to assess how financial re-structuring of the education system can induce students into adopting alternative learning pathways and whether these changes affect their returns in the labour market.

This paper seeks to address this question by looking at an education reform that tripled the tuition fees that universities could charge students in England in 2012. I find that this change increased the probability of having a vocational qualification in upper secondary school by 3 to 6 percentage points. Additionally, I show that students increased the amount and proportion of vocational courses they took once the reform was announced.

The 2012 higher education reform constitutes the most recent example of a large increase in university fees within the UK and provides an ideal setting to understand how academic reforms affect vocational education. As a first prominent feature, this reform represented the highest increase in university tuition since fees were implemented in England in 1998. Until 2012, universities could charge English students up to £3,000 per year in fees.[1] From the academic year 2012/2013 onwards, English students starting a university degree could be charged up to £9,000 per year (Sá, 2019). In an effort to deter English students from over-crowding their higher education institutions, Scotland, Wales and Northern Ireland increased the university fees charged to all UK-born individuals except home students, for whom tuition levels were kept considerably low. This second characteristic of the reform diminishes the appeal of applying to schools in other parts of the UK and thus increases the potential for substitution away from university altogether. A third feature of the reform relies on the English higher education system being homogeneous, as fees do not vary substantially across institutions and most universities

[1] This cap was set in 2006 and was adjusted for inflation in subsequent years.

in the country are public (Azmat & Simion, 2020). This paper exploits these characteristics to provide a comprehensive analysis of the impact of a large raise in tuition fees on vocational education choices of individuals in upper secondary school.

Students wishing to attend university must fulfill traditional academic prerequisites during their time in upper secondary education. Consequently, individuals must decide whether they will attend university at the start of this particular stage of schooling, when they choose subjects that suit their educational interests. As an alternative to academic education, individuals over the age of 16 who have successfully completed lower secondary school may engage in vocational qualifications. I focus on the effect that the 2012 reform had on all types of vocational qualifications, but in particular on those that are close substitutes to academic ones: Applied Generals and Tech Levels. Both types of qualifications generally contain several subjects and can be pursued alongside academic modules.

To obtain causal estimates of the reform on upper secondary school enrollment, I apply two different empirical approaches. First, I use a national labour survey to compare individuals born in England to people born in the rest of the UK and create a difference-in-differences design, where English individuals aged 19 and under at the time of the reform are considered as treated. While students in England experienced a large change in university fees, students in the rest of the UK either faced no change in university fees or had their tuition subsidized by their home countries. Second, I use more detailed, restricted-access data to assess how qualification choices within upper secondary education were modified by the reform. Since this data only exists for English students, I compare similar academic cohorts, before and after the change in fees using a regression discontinuity design based on the students' date of birth.

I find that students more exposed to the reform had an increase of 3 to 6 percentage points in the probability of finishing their education with a vocational qualification in upper secondary level. Additionally, I show that exposed students are more likely to pursue Applied Generals and Tech Levels qualifications: the probability of enrolling in at least one of these courses increased by 7 percentage points and the average proportion of vocational courses, relative to the total number of upper secondary courses, increased by 3 percentage points. Students most likely to switch to vocational courses are those who were disproportionately more represented in this

educational pathway before the reform took place: men, individuals from poorer backgrounds and with lower performance in learning assessments. Whether these switches yield higher returns in the long run compared to a academic paths remains uncertain. The analysis performed in this study on medium-term labour outcomes suggests that the impact of the reform is limited, as individuals who choose the vocational pathway do not experience significant gains in either earnings or the probability of being employed.

Previous empirical research has mainly analyzed the effects of tuition and maintenance grants on university participation rates. These studies typically focus on continental Europe (Hübner, 2012; Kelchtermans and Verboven, 2010; Nielsen et al., 2010)[2] and the US (see, for example, Cameron and Heckman, 2001; Dynarski, 2003)[3], where authors find a more elastic response to tuition fees than to financial aid. In the UK, Dearden, Fitzsimons and Wyness (2014) use a reform in maintenance grants from 2004 to show that a £1,000 increase in this type of financial aid raised university enrollment by 3.95 percentage points. Azmat and Simion (2020) study the 2006 and 2012 higher education reform, which increased the caps on university tuition by £2,000 and almost £6,000, respectively, and find that enrollment fell by only 0.5 percentage points in both cases; a result driven by the highest socio-economic groups. Sá (2019) uses a difference-in-differences model comparing university applications for English and Scottish student before and after the 2012 change in fees and estimates the price elasticity of demand for higher education to be equal to -0.4. Despite all of these studies, little evidence has been documented regarding how higher university costs lead to substitution towards vocational education. This paper fills that gap in the literature and shows that, after a raise in university tuition fees, enrollment in non-academic qualifications increases for students attending upper secondary school.

Thus, I additionally contribute to the existing literature that explores substitution effects between academic and vocational education. A large number of studies have shown positive effects of delaying selection into a particular track on schooling, academic performance and labour

[2]These authors find that enrollment response rates to a €1,000 increase in fees range from approximately 1 percentage point in Belgium (Kelchtermans and Verboven, 2010), to 4.7 percentage points in Germany (Hübner, 2012), while responses on stipend of a similar magnitude in Denmark were shown to raise enrollment by only 1.35 percentage points (Nielsen et al., 2010).

[3]Dynarski (2003) finds that offering $1,000 in financial aid increases the probability of attending university by 4 percentage points. Cameron and Heckman (2001) present evidence that a $1,000 increase in grants raises enrollment by less than 1%, while a similar increase in tuition fees reduces enrollment by 6%.

market outcomes for different countries in Europe (Bellés-Obrero & Duchini, 2021; Malamud & Pop-Eleches, 2010, 2011; Meghir and Palme, 2005; Pekkala, Pekkarinen & Uusitalo, 2013). The reforms that generated these delays in track selection were accompanied by an increase in the academic content of compulsory education. Moreover, Bertrand, Mogstad and Mountjoy (2019) examine a reform that took place in Norway in 1994 that sought to improve the quality of vocational secondary school qualifications by adding to it academic courses and facilitating access into apprenticeships. These changes led to improved social mobility, but increased the gender gap in earnings, as they were more beneficial to men than women. I find that, in the UK, when the costs of academic education increase, substitution into vocational qualifications occurs and is largest among men, but effects in the labour market are limited.

This paper is structured as follows. Section 1.2 provides background on the English educational system and the 2012 higher education reform. Section 1.3 describes the data and the relevant samples. Section 1.4 explains the different empirical strategies used to identify reduce-form effects of the reform on upper secondary education decisions and labour outcomes. Results are presented in Section 1.5, with robustness checks shown in Section 1.6. Mechanisms appear in Section 1.7. Section 1.8 concludes.

1.2 Institutional Framework

1.2.1 Academic and Vocational Upper Secondary Education

For the period covered in this study, full-time education across the UK was compulsory for all children aged 5 to 16 years old. Although each country within the UK has its own system, education is generally organized in four Key Stages until age 16. I focus on educational decisions in Key Stage 5, when students are aged 16 to 18. This stage, also referred to as further education or upper secondary education, is not compulsory for the individuals in the sample.[4]

If students decide to continue their education into this stage, they can choose between a vocational or an academic track. The academic track is designed for people aiming to go to university: over two years, students take three or four subjects in order to gain their General

[4]The school leaving age in England was raised to 18 years old for people born after September 1st 1997 (Education and Skills Act 2008). For this reason, individuals born after that date are left out of the study.

Certificate of Education Advanced Level (A-levels). The subjects chosen during this period are related to the students' degree preferences and the test scores obtained in these national-level exams are used to determine university admission (Azmat & Simion, 2020).

The vocational track encompasses a large number of qualifications. Unlike the academic track, educational choices in this area are diverse and complex, with students being able to select among over 3,700 qualifications, depending on their local availability (Hupkau et al., 2017). For simplicity, these qualifications are generally grouped according to their awarding body and the type of skills that they develop. While students may decide to progress to vocational higher education, transitioning to academic degrees is also possible. Upper secondary education qualifications that provide a combination of practical skills and theoretical knowledge can be recognized for university entrance (e.g. Business and Technology Education Council qualifications), but students undertaking hands-on qualifications, such as apprenticeships, are less likely to enroll in a university degree. For this reason, I focus on vocational qualifications that are close substitutes to academic courses: Applied Generals and Tech Levels. While Applied Generals provide students with transferable skills and knowledge, Tech Levels have a more specialist approach to specific occupations (Hupkau et al., 2017).

1.2.2 The 2012 Higher Education Reform

Until 1998, the cost of a university degree for UK nationals was entirely supported by the government (Sá, 2014). From September 1998 onwards, students could be charged a maximum of £1,000 for higher education qualifications, where the tuition fee of each individual depended on their yearly family income.[5]

Additionally, in 1998, the UK Parliament devolved legislative power to Scotland, Northern Ireland and Wales by passing three new acts: the Scotland Act 1998, the Northern Ireland Act 1998, and the Government of Wales Act 1998. Since then, each country within the UK has independently established the level of tuition fees for higher education institutions located in their territory.

[5]Students with an income lower than £23,000 per year were exempt from paying, a reduced fee was granted to people with an income between £23,001 and £35,000, and those whose income was higher than £35,000 were charged the full tuition fee (Azmat & Simion, 2020).

In England, the 2004 Higher Education Act introduced a fee cap initially set at £3,000 (inflation-indexed) for students starting their degree in the academic year 2006/2007 (Wakeling & Jefferies, 2013). In 2010, the fee cap was increased once again[6] and set at £9,000 per year for UK students starting higher education in the academic year 2012/2013 (Azmat & Simion, 2020).[7] I use this change to assess how increases in university fees affected educational choices for people in upper secondary school. In this period, financial aid in England was also modified: maintenance loans became more generous; the annual earnings threshold to start repaying university loans was increased from £15,795 to £21,000 ; the repayment period of loans was extended (from 25 to 30 years); and the real interest rate of these loans increased from 0% to 3% (Sá, 2019).

For Scottish-domiciled students graduating from April 2007 onwards, full-time university education became free (Sá, 2019)[8]; although fees were technically still in place, the Student Awards Agency Scotland, an Executive agency of the Scottish Government, covered these costs for all eligible students. Wales generally mimicked the policies established in England. A fee cap of £3,000 per year, set in 2007, was followed by an increase of the cap equal to £9,000 in 2012. However, the Welsh government heavily subsidized fees for its nationally-domiciled students, ensuring that the effective fees they paid were pegged to the original £1,000 rate, plus inflation (Wakeling & Jefferies, 2013). Fees in Northern Ireland also followed the policies imposed in England until the academic year 2012/2013 (Wakeling & Jefferies, 2013). The university fee cap in Northern Ireland has been indexed to inflation since 2006/2007, when it was first set at £3,000.[9]

In order to discourage student mobility to countries with lower caps, fees for non-home students from the rest of the UK were set according to the English maximum. For example, starting in 2012, students from Wales, Northern Ireland and England attending Scottish universities could be charged a maximum of £9,000, while home students were fully subsidized by the government. Therefore, the new cap imposed by England in 2012 was effective for all British students attending English universities and all British students attending universities in

[6]This change occured as a result of the Browne review (2010), which proposed removing caps on university tuition fees.

[7]The fee cap was raised to £9,250 in the year 2017.

[8]Individuals are considered as Scottish-domiciled if they have been living in Scotland for at least three years before the start of university (Sá, 2019).

[9]As of the academic year 2020/2021, the cap for Northern Ireland-domiciled students is equal to £4,530.

countries other than their own. As a result, independently of where they studied within the UK, English students were exposed to a higher fee, while Scottish, Welsh and Northern Irish individuals were sheltered by their home countries.

These differences in tuition fees across countries within the UK reduce the likelihood that other UK policy changes drive the results of my work. The differences-in-differences approach detailed in Section 1.4.1 allows me to capture the effect of the reform to the extent that it applied to English individuals and not to the rest of the UK. However, nation-wide policy changes contemporary to the reform under study, such as the Education Act (2011) or the Wolf report (2011), could be confounders for the regression discontinuity design described in Section 1.4.2. On the one hand, the Education Act (2011) states that the government should prioritize the provision of proper facilities and funding for apprenticeships for people under the age of 25. By promoting apprenticeships, the Act could have generated a compositional change in upper secondary education, discouraging students from the preparing to attend university. I argue, however, that the students in the regression discontinuity analysis are not likely to substitute academic studies for apprenticeships. On the other hand, the Wolf report (2011) intended to improve the progression of vocational qualifications to either education or skilled employment and to reduce the information gaps that students have on these qualifications. While some of the recommendations proposed in the report started to be implemented in 2012, the more substantial changes occurred from August 2013 to September 2016 (Department for Education, 2015b), when all the students in the sample were at least 17 years old and would have already decided which courses to take in upper secondary school.

1.3 Data and Descriptive Statistics

1.3.1 Labour Force Survey

I use data from the UK Labour Force Survey (LFS), which is a quarterly survey representative of households. Besides labour outcomes, the LFS contains information on respondents' education, age, year and place of birth, gender, religion and ethnicity. It is carried out by the Office for National Statistics (ONS) and, in each period it covers an average of 38,000 households and 90,000 individuals. The analysis is conducted using data from the first quarter of

2013 to the third quarter of 2020. Households are interviewed in 5 successive waves. To avoid double-counting, I only consider information collected during the first wave each individual was surveyed.[10] I keep in the sample respondents born in the UK between the years 1988 and 1996 and who were at least 20 years old at the time of the interview.[11] After deleting observations with missing data on the variables of interest, the resulting sample size is equal to 41,966 individuals: 86% of the observations correspond to English-born respondents, while the remaining people in the analysis were born in either Scotland (8%), Wales (5%) or Northern Ireland (1%).[12]

I focus on individuals whose highest level of education is a vocational upper secondary school qualification. The LFS distinguishes between different types of vocational courses and I follow Dearden et al. (2002) to identify them within the British educational system: General National Vocational Qualifications (GNVQs) and General Scottish Vocational Qualifications (GSVQs); National Vocational Qualifications (NVQs); Royal Society of Arts certification (RSAs); City and Guilds qualifications; trade apprenticeships; Ordinary National Certificates/Diplomas (ONCs/ONDs); Business and Technology Education Council (BTEC) and Scottish Vocational Educational Council (SCOTVEC) qualifications are all considered as vocational education.

Table 1.1 shows the proportion of respondents whose highest qualification is in vocational upper secondary education. This proportion has increased over the years in England, but not for the rest of the UK, where the probability of finishing schooling with a vocational upper secondary education degree drops to less than 10% for individuals born after 1992.

1.3.2 National Pupil Database (NPD)

In addition to the LFS data, I use the National Pupil Database (NPD) provided by the English Department for Education to assess differences in upper secondary school choices before and after the increase in university fee caps. The NPD is a longitudinal database of all students enrolled in state schools in England. While schools are not the exclusive providers of Key Stage

[10] See Table A.6 for results using all waves of the LFS.

[11] By considering individuals over the age of 19 when surveyed, I am able to capture the effects of the reform on the portion of the population most likely to have finished upper secondary school.

[12] The representation in the sample of individuals born in Northern Ireland is small due to missing values for two of the variables used as controls in the main specification: religion and ethnicity.

Table 1.1: Mean Probability of Vocational Upper Secondary Education as Highest Qualification (1988-1996)

Country of birth	Year of Birth 1988-1992	Year of Birth 1993-1996	Total
England	14.324	15.949	14.975
N	22,611	13,509	36,120
Rest of the UK	11.700	9.904	10.996
N	3,663	2,183	5,846
Total	13.95	15.113	14.414
N	26,274	15,692	41,966

Notes: Population born in 1988-1996 and aged over 19 at time of the interview. Individual weights included. Country assignment according to place of birth.
Source: Author's own computations based on data from the Labour Force Survey (LFS), 2013-2020.

5 courses in England,[13] I focus on this subset of institutions because the majority of individuals undertaking academic courses on this level attend them (Hupkau et al., 2016).[14] By analyzing this subsample of individuals, I am able to capture the effect of the reform on students who are more likely to attend universities upon graduation.

I look at post-compulsory courses taken using upper secondary schooling data from the Post-16 Learning Aims (PLAMS) combined with data on attainment in lower secondary school (Key Stage 4) and the Spring Census, which contains information on student characteristics, such as month and year of birth, gender, ethnicity and student geographical residence. Although I do not have a direct measure of family income, I use the Income Domain Affecting Children Index to indicate socio-economic status. This variable measures the percentage of children aged 0 to 15 living in income deprived families within the student's area of residence (Ministry of Housing, Communities & Local Government, 2019). Table A.1 contains summary statistics on these variables, which are used as controls in my baseline specification.

[13]For example, further education colleges represent an alternative for students inclined to take vocational courses.

[14]The authors point out that 66% of students studying for A-levels attend schools or academies, 21% attend Sixth Form colleges, 10% attend Further Education colleges, while the remaining 3% are enrolled in other types of Further Education institutions (Hupkau et al., 2016).

The PLAMS dataset details the type of course students take each academic year. I code each course according to the categories detailed in Table 1.2, which is based in previous analysis made by Hupkau et al. (2016, 2017). The first group comprises academic courses that traditionally fulfill university entry requirements. Applied General qualifications enable learners to develop transferable knowledge and skills in a vocational area, while Tech Levels equip students with the knowledge they need for a specific occupation (Hupkau et al., 2017). Both Applied Generals and Tech Levels are vocational qualifications that can lead to higher education and that are considered on a par with academic courses by the department for Education (Department for Education, 2015a). They can also be taken alongside academic courses. National Vocational Qualifications (NVQs) are work-based qualifications relating to an industry or sector. Key Skills qualifications help students develop and put in practice a range of transferable, work-related skills and are commonly undertaken with other vocational courses. The final category in the table highlights the fact that not all 16 to 18-year-old students may be in upper secondary school, as some are taking remedial courses to access this educational stage.

Hence, 4 groups represent the different types of vocational education offered to students in upper secondary school, with academic courses and lower level education representing the only alternatives to the vocational track. Using this dataset, I look at the effect of the university fee cap increase on Applied Generals and Tech Levels as a separate group. Given that Applied Generals and Tech Levels are closer substitutes to academic education than other available qualifications, a change in educational choices should be detected in these areas if the reform had an effect on the students in the sample.

To assess if significant changes in educational tracks took place as a result of the reform, I use 3 measures of vocational uptake:

1. Indicator for the student having taken at least one Applied Generals or Tech Levels in any of the years she appears in the sample;

2. Total number of Applied Generals and Tech Levels;

3. Percentage of Applied Generals and Tech Levels relative to the total number of courses taken by the student, including those in academic tracks, other vocational pathways and

lower educational levels.

The first variable allows me to measure the extensive margin effect of the reform, while the other two show different intensive margin responses: the amount of vocational content students take and its share within the syllabus. I analyze the evolution of these variables for students born between September 1992 and August 1996, that is, individuals who were 16 to 19 years old when the reform was implemented. Academic years in England start in September of each year, with a cohort being defined between that month and August of the following year. Unlike the LFS, this dataset allows me to accurately allocate students to their corresponding academic cohort, as it contains information of the month and year of birth of the individuals in the sample.

Once the PLAMS for the academic years 2007/08 to 2014/15 is combined with the database on previous learning outcomes, as well as the corresponding Spring Censuses, the resulting sample contains 786,542 observations. Each observation corresponds to one individual. The average number of years individuals are present in the sample is 1.81, with 70% of them spending two years in upper secondary school.

1.4 Empirical Methodology

1.4.1 Probability of vocational upper secondary school as highest qualification

To measure the effect of the increase in university fee caps on vocational education attainment, I compare British individuals born in different years and countries within the UK using a difference-in-differences design on the LFS data. English students born after 1992 (aged 19 or younger in 2012) are more likely to be exposed to the reform, while non-English students and English individuals born before 1993 are not as likely to be exposed. To account for potential differences within these groups, my baseline specification includes controls for pre-determined characteristics.

I estimate the following equation:

$$Voc\ Upper\ Secondary_i = \sum_{t=1988}^{1996} \beta_t Eng_i \times Year\ of\ Birth_i^t + X_i'\lambda + \delta_c + \gamma_t + \varepsilon_i \qquad (1.1)$$

Table 1.2: Qualification Categorization for English Students Aged 16-18

Level-Stage	Track	Qualification	Description	Example
3-Upper Secondary School	Academic	A-levels	Academic qualifications; traditional prerequisites for university entrance.	A-level in Chemistry
	Vocational	Applied Generals	They provide learning in a vocational area rather than for a single occupation. They aim to facilitate progression to employment or tertiary education	Certificate in Applied Science
		Tech Levels	Specific technical vocational qualification. They aim to facilitate progression to employment.	Diploma in Pharmaceutical Science
		Key Skills/NVQs	Recognised vocational qualifications relating to an industry or sector.	Diploma in Laboratory Science
		Other Upper Secondary School	Apprenticeships and everything else not included above	Laboratory Technician Apprenticeship
2-Lower secondary school 1-Primary school		Other Lower Levels		

Notes: Information based on Hupkau et al. (2016, 2017).

where *Voc Upper Secondary*$_i$ is equal to 1 if the highest level of qualification for individual i is vocational upper secondary education and 0 otherwise. Eng_i is an indicator for English-born students. I create one dummy variable per year of birth (*Year of Birth*$_i^t$), where each individual's year of birth is indicated by superscript t, starting from 1988 and finishing in 1996. The omitted year is 1992: students born that year were between 19 and 20 years old when the increase in fees took place. X_i is a vector of variables, including age at the time of the interview, gender, religion and ethnicity.[15] δ_c captures country fixed effects, while year fixed effects are denoted by γ_t. Standard errors are clustered at the area of residence and place of birth level to account for the possibility of correlation among people born in the same country and living in the same area (Bertrand, Duflo and Mullainathan, 2004). I use individual weights to obtain population-level estimations, though results are robust to unweighted specifications (see Table A.6). Figure 1.1 shows that trends in the outcome variable were not statistically different before 1993, supporting the assumption that in the absence of treatment, the probability of finishing education in vocational upper secondary school would have continued to be statistically similar between groups in later periods.

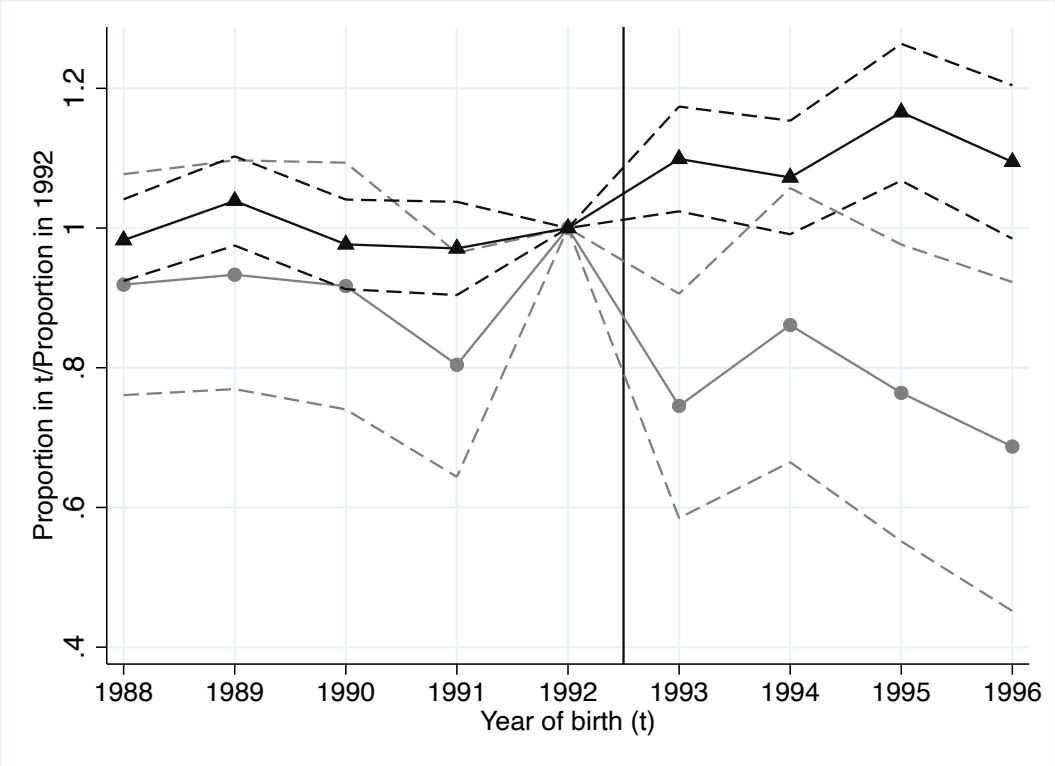
Besides estimating the effect of increasing university fees on vocational education in upper secondary school, I use the empirical strategy expressed in equation (1.1) to calculate the impact this reform had on labour market outcomes. In particular, I consider hourly wages, the probability of being employed, the probability of having permanent employment and the probability of having a full-time job as dependent variables for this specification. The impact on hourly wages is measured only for those individuals who reported a positive wage. This regression uses income weights, instead of individual weights, to better represent the population examined, which excludes individuals without a reported measure of earnings. I study the effect on the remaining labour outcome variables using all respondents in the sample and individual weights.

The proposed empirical strategy in this section considers respondents' year of birth, instead of the academic cohort of birth. An academic cohort is comprised of individuals born between September 1st of a given year and August 31st of the following year. Since the LFS only reports the year of birth of respondents, the misalignment of the academic and calendar years means that

[15]Both age and age squared are included in vector X_i. I use 4 groups to create ethnicity dummies and interactions between ethnicity and cohort: White, Asian, Black and other, including mixed ethnic groups.

some individuals are allocated to the wrong cohort. This issue is particularly severe for those respondents born between September and December 1992, who are assigned to a non-treated year of birth while corresponding to a treated academic year. If there is indeed an effect for this particular quarter, then my results are underestimations of the impact of the reform, and can therefore be considered as lower bounds of the true effect of the reform.

Figure 1.1: Proportion of Students with Vocational Upper Secondary Education as Highest Qualification (1988-1996)



Notes: The plot shows the average proportion of individuals whose highest qualification is vocational upper secondary education, relative to 1992. The sample includes respondents born in the UK between 1988 and 1996 who were over 19 years old at time of interview. Averages for English-born individuals are shown in black (treatment group). Averages for people born in the rest of the UK are presented in gray (control group). The 95% confidence intervals are reported as dashed lines. Observations are weighted at the individual level.
Source: Author's own computations based on data from the Labour Force Survey (LFS), 2013-2020.

1.4.2 Content of upper secondary education courses

I additionally estimate the effect of increasing university fee caps on the content of upper secondary education for English students using a regression discontinuity design on the NPD database. I consider two cohorts as treated:

1. Individuals born between September of 1993 and August 1994. This cohort was 18 years

old when reform was implemented. In other words, this is the first cohort to experience the change in university fees. In addition, since the announcement of the reform occurred two years prior to its implementation, the start of upper secondary education for this cohort coincides with the announcement that university fees would be higher. That is, this cohort was aware that the increase would take place when deciding which courses to take in upper secondary school. I compare this group to the set of individuals who were 19 years old at the time of the reform. This cohort was the last to have the opportunity to pay lower fees, as long as they started university before the academic year 2012/13. By comparing these two cohorts, I am capturing the effect of facing higher university fees on upper secondary school choices.

2. Individuals born between September 1995 and August 1996. This cohort was 16 years old at the time the reform was put in place and, as a result, it was the first to start upper secondary education with the higher university fee caps in place. I compare this group to the set of individuals who were 17 years old at the time of the reform. When this cohort started upper secondary education, the fees modification was not in place, but these individuals were aware that a change in university fees would happen. Prior to the start of the academic year 2012/13, the reform could have been postponed or modified between its announcement and its implementation. Thus, by comparing the 16-year-old cohort with the 17-year-old cohort, I am estimating the effect of the reform being in place. Since both types of students would have faced higher university fees if they had continued their education, I expect estimates in this exercise to be lower than those obtained in the comparison between 18 and 19-year-olds.

The baseline analysis in this section estimates the following regression model:

$$z_i = \alpha_1 Post_i + \sum_{p=1}^{F} \mu_p Time_i^p + \sum_{p=1}^{F} \kappa_p Post_i \times Time_i^p + \Pi_i' \eta + \tau_s + \sigma_n + \mu_m + \epsilon_i. \tag{1.2}$$

I estimate effects for enrollment in Applied Generals and Tech Levels, both of which are a subset of vocational education. I assess the impact of the reform on whether students attend any of these courses, as well as the number and the percentage of vocational courses taken.

These outcomes, represented by z_i in equation (1.2), are measured for each student i. The variable $Post_i$ is an indicator that takes value 1 if the student was born during what I define as a treated period: September 1993 to August 1994 in the comparison of 18 and 19-year-olds and September 1995 to August 1996 in the analysis of 16 and 17-year-old students. I use a linear piece-wise trend in my main specification to account for discontinuities in the uptake of vocational education between cohorts, where each point in the trend is represented by a month of a given year. $Time_i^p$ represents the linear trend when p is equal to 1, while $Post_i \times Time_i^p$ is an interaction between the time trend and the $Post_i$ indicator.

Using the NPD, I am able to control for several individual characteristics that may affect educational choices (Π_i). Besides gender and ethnicity, I control for English as a first language, achievement in lower secondary school exams (called GCSEs[16]) and the Income Deprivation Affecting Children Index (IDACI) score for each student's neighborhood. Descriptive statistics on these variables can be found in Table A.1.

Fixed effects are also included at the neighborhood-of-residence (σ_n) and school (τ_s) level.[17] Figure 1.2 presents the density of the running variable used in this specification: date of birth. While the number of students born in each month varies considerably, the pattern of births is consistent for every month over the years in the sample (for example, September always has the highest rate of births, February has the lowest). To account for this variation, as well as for differences in cognitive development within students of an academic cohort (Crawford, Dearden and Meghir, 2007), I include month-of-the-year fixed effects. Finally, standard errors are clustered at the month and year of birth. Since in each specification, I only take into account 24 months, I implement the wild cluster bootstrap method proposed by Cameron, Gelbach and Miller (2008) for small numbers of clusters.

The graphs in Figure 1.3 present the mean number of students per month of birth that attended at least one Applied Generals or Tech Levels during upper secondary school. Students born between September 1993 and August 1994, aged 18 years old at the time of the reform, experienced a large increase in vocational education enrollment relative to students in the previous

[16] I include a dummy for whether students have achieved 5 GCSEs, which is a threshold that determines educational pathways in upper secondary education (Chowdry et al., 2013).

[17] As shown in Crawford (2014), school characteristics are relevant determinants of higher education participation in the UK.

cohort. People born in this time frame were the first to face the changed fees once the reform was implemented. Given the fee cap modification was announced when they had just started upper secondary education, it is important to assess whether awareness of the future increase had an effect on this group's educational choices. The rise in vocational education enrollment of this cohort is even larger than that of the second treated cohort, born after August 1995. In other words, the changed fees affected more the educational choices of the first cohort of students to face this increase than those starting upper secondary school the moment the new fees were put in place. This is due to the fact that the reform was announced two years before it was implemented, when that first cohort started upper secondary education. The differential effect for the first treated cohort relative to the second treated group is also present in the other two measures of vocational education that I use: number of Applied Generals and Tech Levels (Figure 1.4), and Percentage of Applied Generals and Tech Levels (Figure 1.5).

The identifying assumption of this regression discontinuity design states that being born around the academic year threshold, September 1st, cannot be manipulated. I perform the McCrary (2008) test for smoothness of the density function around the threshold on the two samples in my database: 1992-1994 (19 and 18-year-olds) and 1994-1996 (17 and 16-year-olds). I collapse the data by month and year and use the observation count in each period as the dependent variable of equation (1.2), excluding controls and fixed effects. There is no evidence for a discontinuous jump of student date of birth at either threshold. The null hypothesis of continuity of this function at the threshold cannot be rejected, as the p-values are equal to 0.205 and 0.384 for the first and second sample, respectively.[18]

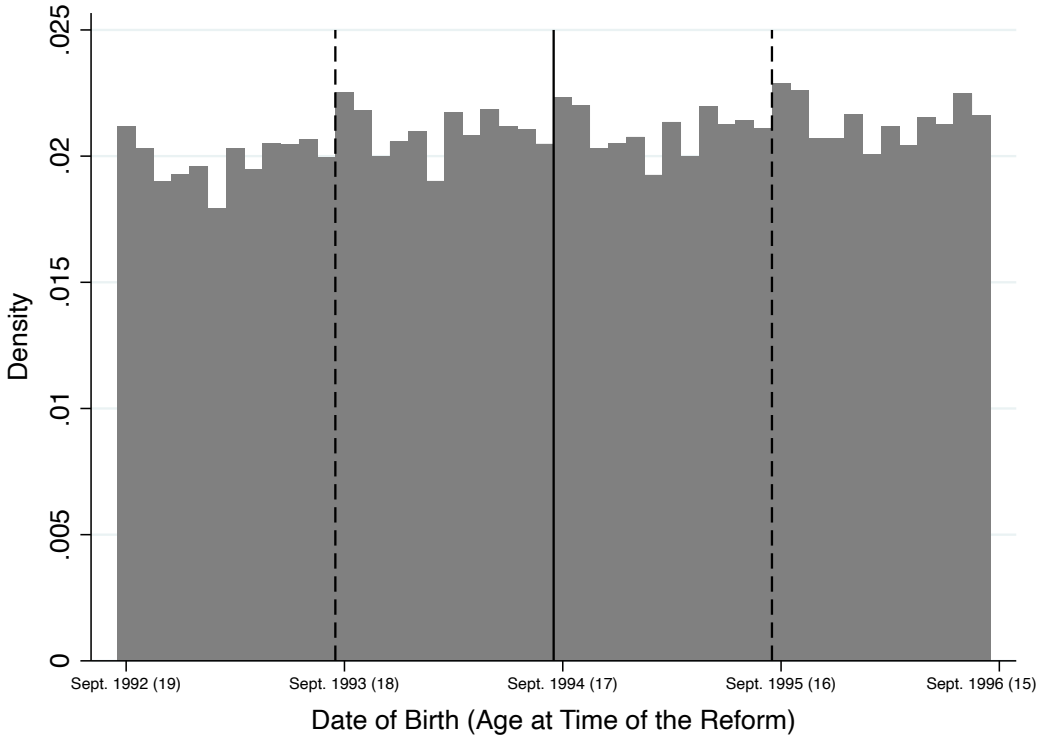
1.5 Results

1.5.1 Probability of vocational upper secondary qualifications

Figure 1.6 presents results for the baseline specification in Section 1.4 (see Table A.2 for point estimates). For cohorts born before 1992, the probability of having vocational upper secondary school as the highest qualification was not significantly different between students from England and the rest of the UK. However, for English people born between 1993 and

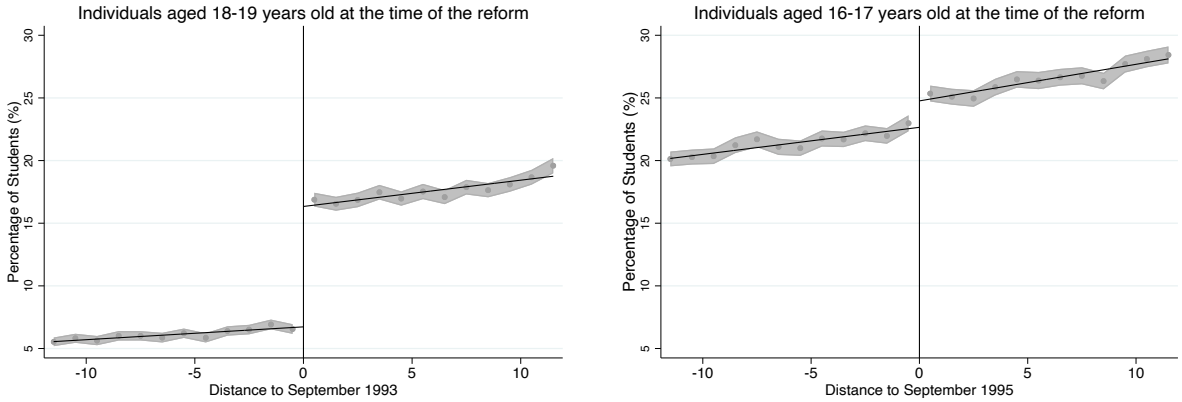
[18]In Table A.1, I report the result of similar tests performed on the dependent variables included in equation (1.2) . Figures A.1 and A.2 present monthly averages of the control variables.

Figure 1.2: Density Distribution of the Running Variable (1992-1996)



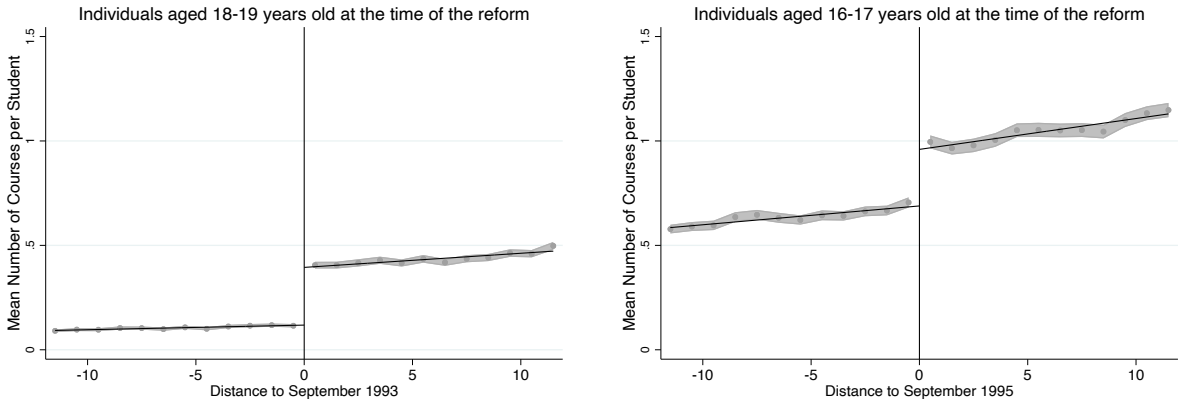
Notes: The plot shows the density of students born in each month in the sample. The start of each academic year is represented by the lines between the months of August and September. Dashed lines separate control from treatment cohorts. The solid line separates the two samples used in this study.
Source: Author's own computations based on data from the from the National Pupil Database (NPD), 2007-2015.

Figure 1.3: Monthly Averages of the Probability of Taking Applied Generals and Tech Levels (1992-1996)



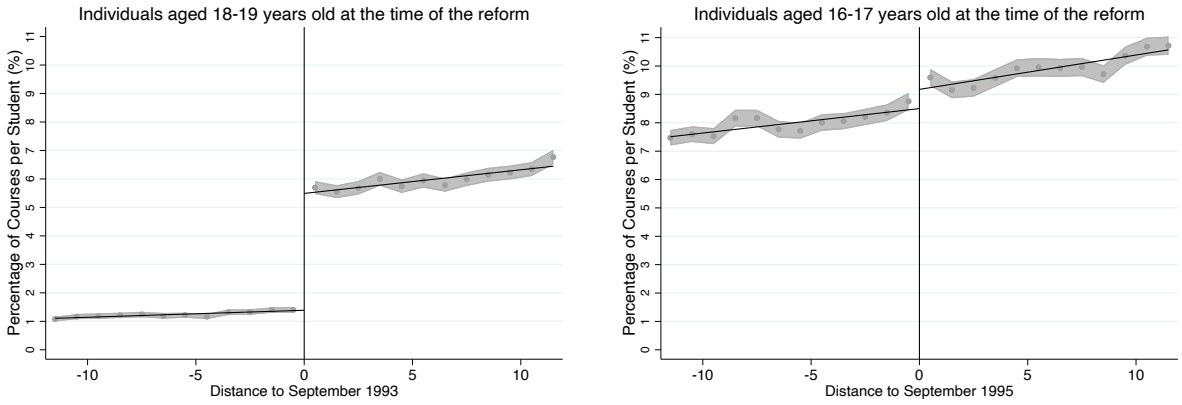
Notes: The figure plots monthly averages of an indicator for the student having taken at least one Applied Generals or Tech Levels. The solid line plots predicted values, with linear trends on either side of the start of the academic year threshold. The shaded areas show estimates within the 95% confidence intervals.
Source: Author's own computations based on data from the National Pupil Database, 2007-2015.

Figure 1.4: Monthly Averages of the Number of Applied Generals and Tech Levels (1992-1996)



Notes: The figure plots monthly averages of the total number of Applied Generals and Tech Levels courses taken per student. The solid line plots predicted values, with linear trends on either side of the start of the academic year threshold. The shaded areas show estimates within the 95% confidence intervals.
Source: Author's own computations based on data from the National Pupil Database, 2007-2015.

Figure 1.5: Monthly Averages of the Percentage of Applied Generals and Tech Levels (1992-1996)



Notes: The figure plots monthly averages of the percentage of Applied Generals and Tech Levels relative to the total number of courses taken by each student. The solid line plots predicted values, with linear trends on either side of the start of the academic year threshold. The shaded areas show estimates within the 95% confidence intervals.
Source: Author's own computations based on data from the National Pupil Database, 2007-2015.

1996, there is a significant increase of 3 to 6 percentage points in the probability of a vocational qualification. This is equivalent to a 26-47% change relative to the control mean in the pre-reform period. Furthermore, these results are in line with those found in studies that analyze the impact of the 2012 higher education reform on university enrollment (Azmat and Simion, 2020; Sá, 2019).

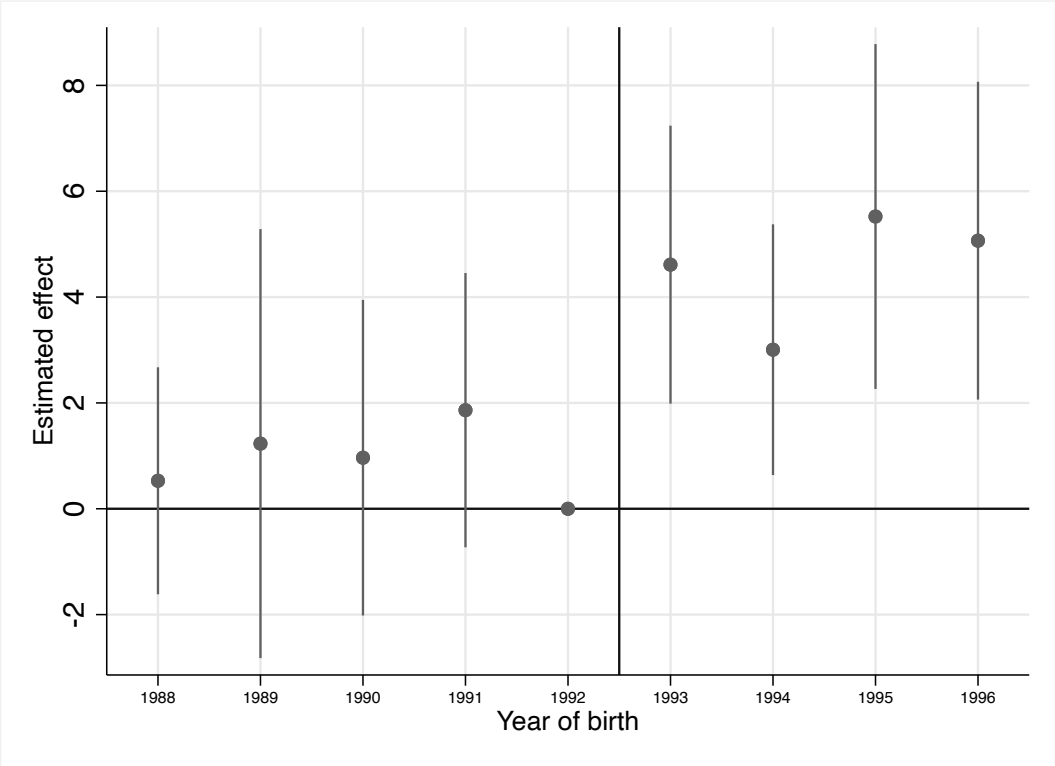
Since estimates are significant not only for students aged 16 at the time of the reform, but also for older cohorts, the increase in university fees deters older students from adopting alternative paths to vocational education. This effect can be explained by the fact that students were made aware that a change in fees would take place two years ahead of its implementation. The first group of individuals who would face the increase in fees was the academic cohort born between September 1993 and August 1994. Since the announcement of the change occurred when these students were 16 years old, they could modify their educational choices in upper secondary education to respond the university cost increase that would happen in 2012. Therefore, I observe a positive and significant anticipation effect to the change in fees in individuals who were older than 16 at the time of the reform.

1.5.2 Content of upper secondary school courses

Table 1.3 reports results for the NPD dataset, which only contains information on English students. For people born between September 1993 and August 1994, the probability of taking at least one Applied Generals or Tech Levels increased 7 percentage points, relative to individuals born a year earlier (Column (1)). For this cohort, the response to the reform along the intensive margin was also significant, with the representation of these vocational courses increasing by almost 3 percentage points (Column (3)) and the average of Applied Generals and Tech Levels taken per student increasing by 0.17 (Column (2)). The reported increases in vocational education enrollment for students in this cohort represent large changes relative to the control mean, ranging from 113%, in the case of the indicator for enrollment, to 223% for the percentage of courses taken.

The impact on students who were born between September 1995 and August 1996 is smaller at the extensive margin, as evidenced by Column (4) of Table 1.3: the probability

Figure 1.6: Difference-in-Differences Analysis of the Reform on the Probability of Vocational Upper Secondary Education as Highest Qualification (1988-1996)



Notes: The sample includes respondents born in the UK between 1988 and 1996 who were over 19 years old at time of interview. The outcome variable is an indicator for vocational upper secondary education as the highest qualification achieved. The reported estimates, represented by dots, correspond to interaction terms between year of birth categorical values and the indicator for English-born individuals. The 95% confidence intervals are reported as lines. Observations are weighted at the individual level. Controls for ethnicity (time trends), age, gender and religion are included in the regression, as well as country and year fixed effects. Standard errors are clustered at the region-of-residence and country-of-birth level.
Source: Author's own computations based on data from the Labour Force Survey (LFS), 2013-2020.

of studying at least one Applied Generals or Tech Levels increased by 4.5 percentage points, considerably less than the effect found for the first treated cohort. The number of courses taken by the second treated cohort increased by 0.31, relative to students a year older, while the percentage of Applied Generals and Tech Levels taken also grew for this group (2.1 percentage points). In the case of the second treated cohort, the effects are notably smaller than those found for the first treated cohort: the size of the impact ranges from 21% to 49% relative to the control mean. Moreover, the size of the effect for this last cohort is similar to the impact found in the previous section for the probability of having vocational upper secondary education as the highest qualification (24-47% relative to the control mean).

To further compare results between both empirical strategies, I modify equation (1.1) to have two interaction terms instead of one. The variable $Post_i \times English_i$ is an indicator for a respondent born in England after 1992. The interaction term $Second\ Year\ Post_i \times English_i$ takes value 1 if the respondent was born in England in either 1994 or 1996. This variable aims to identify the differential effect of the reform on the treated cohorts (i.e. those born in 1994 or 1996) in the regression discontinuity design. If the estimated coefficient for this variable is positive and significant, then treated cohorts in the regression discontinuity design have a higher probability than their counterparts born in 1993 or 1995 of having vocational upper secondary education as their highest qualification.

In contrast to the results found in Table 1.3, the impact of increasing university fees on English individuals born in 1994 and 1996 had a negative and non-significant impact on vocational education relative to the English cohorts born in 1993 and 1995, whose average effect is equal to a reduction of 4 percentage points in the probability of having upper secondary education as the highest qualification (Table A.3). When taken together, these results suggest that while English cohorts born after 1992 experienced a similar increase in the average probability of having vocational upper secondary education as the highest qualification, differences exists in the content of their courses, with treated students in the regression discontinuity design increasing their enrollment in vocational subjects.

Table 1.3: Regression Discontinuity Analysis of the Reform on Applied Generals and Tech Levels Enrollment (1992-1996)

	(1) Any Courses	(2) Number of Courses	(3) Percentage of Courses	(4) Any Courses	(5) Number of Courses	(6) Percentage of Courses
	1992-1994 Aged 18-19 during reform			1994-1996 Aged 16-17 during reform		
Post	6.965*	0.165*	2.780*	4.505*	0.314*	2.135*
	(1.032)	(0.038)	(0.300)	(0.493)	(0.024)	(0.418)
	[0.056]	[0.070]	[0.059]	[0.070]	[0.074]	[0.091]
Male	2.232***	0.059***	0.894***	4.012***	0.180***	2.247***
	(0.218)	(0.007)	(0.118)	(0.144)	(0.011)	(0.063)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Ethnicity: White	-0.278*	-0.019***	-0.064	0.251	0.011	0.345***
	(0.142)	(0.003)	(0.061)	(0.162)	(0.009)	(0.071)
	[0.055]	[0.000]	[0.321]	[0.126]	[0.244]	[0.001]
English Language	0.914***	0.006	0.611***	0.605**	-0.017	0.928***
	(0.246)	(0.007)	(0.129)	(0.255)	(0.012)	(0.106)
	[0.003]	[0.322]	[0.000]	[0.024]	[0.175]	[0.000]
Achieved 5 GCSEs	-17.773***	-0.464***	-6.774***	-30.927***	-1.374***	-16.181***
	(1.342)	(0.048)	(0.771)	(0.284)	(0.061)	(0.212)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Poverty Score	3.238***	0.069***	1.123***	6.548***	0.212***	2.629***
	(0.553)	(0.016)	(0.247)	(0.465)	(0.024)	(0.241)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Observations	385,915	385,915	385,915	400,627	400,627	400,627
R-squared	0.301	0.301	0.327	0.392	0.393	0.437
Control group mean	6.116	0.105	1.242	21.357	0.635	7.981

Notes: This table presents results from a regression discontinuity specification with a local polynomial of order 1. The running variable is the date of birth of the students in the sample. The outcome variables measure whether the students are enrolled in any Applied Generals or Tech Levels courses (Columns (1) and (4)), the number of Applied Generals and Tech Levels courses taken (Columns (2) and (5)) and the percentage of Applied Generals and Tech Levels relative to all courses taken in upper secondary school (Columns (3) and (6)). Columns (1)-(3) show results for regressions where the threshold is September 1st 1993. The threshold for Columns (4)-(6) is the September 1st 1995. Other independent variables include an index of neighborhood poverty, as well as indicators for gender, ethnicity, English as native language and achievement in lower secondary school exams. All regressions include neighborhood of residence, school and month-of-the-year fixed effects. Standard errors clustered by month and year are reported in parentheses. The p-value of the wild cluster bootstrap test of the hypothesis that each coefficient is equal to 0 is reported in brackets (number of clusters=24, bootstrap replications=999). *** significant at 1%, ** significant at 5%, * significant at 10%.
Source: Author's own computations based on data from the National Pupil Database, 2007-2015.

1.5.3 Labour Market Outcomes

As shown in Graph (a) of Figure 1.7, increasing university fees does not have a significant impact on the labour returns of individuals who were younger than 19 years old at the time of the reform (see Column (2) in Table A.2 for point estimates). Only the English cohort born in 1993 experienced a decrease in average hourly wages. In addition, the probability of being employed was not affected by the 2012 educational reform (Graph (b), Figure 1.7; Column (3), Table A.2). A similar point can be made regarding the probability of permanent employment (Graph (c), Figure 1.7; Column (4), Table A.2). Regarding full-time employment, only English individuals born in 1995 were affected by the reform: this cohort was 4 percentage points less likely to be working in full-time jobs than other British individuals born in the same year.

As pointed out in McIntosh (2006), stable returns combined with a rise in the proportion of adults with higher level qualifications suggest that demand for educated workers is increasing at a similar rate as its supply in affected areas. A parallel argument could be made regarding vocational degrees, given the results shown in this section. I do not make the claim, however, that the impact on returns is neutral, as I do not have the scope to estimate effects on lifetime earnings.[19]

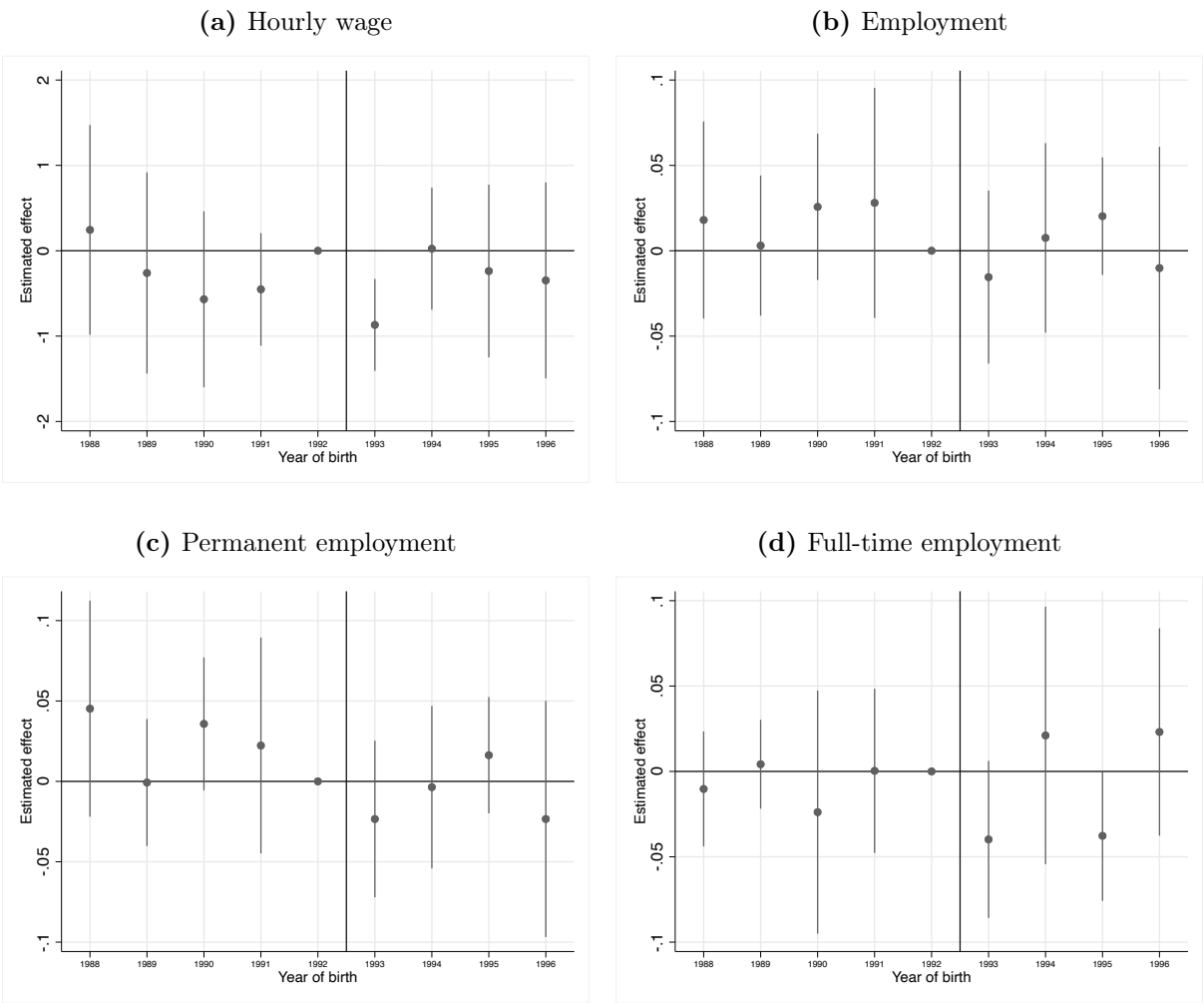
1.6 Robustness Checks

Alternative samples. Respondents of the LFS are interviewed for 5 successive waves at 3-month intervals, with 20% of the sample being replaced every quarter (Office for National Statistics, 2015). In order to avoid double-counting, baseline results only considered information provided during the first wave each individual was surveyed. Results when all five waves are included in the sample are slightly lower than those found at baseline (Table A.4), with the largest change shown for the coefficient that estimates the effect of being born in England in 1994.

Besides including all waves in the analysis, I additionally check that estimates are robust to another change in the main sample. Observations with missing values in any of the variables

[19]Data limitations prevent the long-run study of labour market effects, as the youngest cohort in the sample is only 24 years old in the last wave under analysis.

Figure 1.7: Difference-in-Differences Analysis of the Reform on Labour Outcomes (1988-1996)



Notes: The sample includes respondents born in the UK between 1988 and 1996 who were over 19 years old at time of interview. The sample for the results in Graph (a) only includes individuals who reported a positive wage. The outcome variable for this graph is average hourly wage. The results for Graphs (b), (c) and (d) include respondents with and without a positive wage. The outcome variables for Graphs (b), (c) and (d) are indicators for being employed, having permanent employment and having full-time employment, respectively. The reported estimates, represented by dots, correspond to interaction terms between year of birth categorical values and the indicator for English-born individuals. The 95% confidence intervals are reported as lines. Observations are weighted using income weights for Graph (a) and individual weights for Graphs (b)-(d). Controls for ethnicity, gender, age and religion are included in all regressions, as well as country and time fixed effects. Standard errors are clustered at the region-of-residence and country-of-birth level.
Source: Author's own computations based on data from the Labour Force Survey (LFS), 2013-2020.

detailed in equation (1.1) were excluded from the LFS sample, resulting in a loss of 2,644 observations (6% of the total number of observations). The majority of the missing values correspond to the ethnicity and religion controls, as the remaining independent variables do not report any missing values and the outcome variable, *Voc Upper Secondary*, is only missing in 129 cases. Columns (1) to (5) in Table A.5 show regression results for a specification that excludes religion and ethnicity controls and includes observations with missing data on these variables. Columns (6) to (10) present estimates on the main sample, but excluding religion and ethnicity controls as well. Estimates in Columns (1) to (5) do not differ considerably from those obtained in Columns (6) to (10), supporting the claim that results are not affected by attrition.

Unweighted observations. The baseline specification that uses the Labour Force Survey includes individual weights to obtain estimates at the population level. Table A.6 presents the unweighted impact of the reform on vocational education and labour outcomes. Results for the probability of having vocational upper secondary education as the highest qualification are slightly lower when weights are not applied. Most labour outcomes remain unaffected by the reform, with the exception of the probability of being employed, which is positive and significant for the 1995 cohort.

Exclusion of controls, fixed effects and time trends. In this section, I explore whether results are robust to removing controls, country-of-birth fixed effects and ethnicity time trends. As shown in Tables A.7-A.9, estimations from the difference-in-differences approach do not vary considerably from the ones obtained at baseline.

For the regression discontinuity design, removing month-of-year fixed effects or controls yields higher estimates for the older treated cohort and slightly lower results in all but one case for the younger group of treated students (see Table A.10). For the first treated group, the estimated effects when removing school fixed effects remain similar to the baseline specification. For the second treated cohort, however, the increase in the probability of taking a vocational course is equal to 8.3 percentage points, which is almost twice the effect found when school fixed effects are included. Thus, schools appear to be important determinants of the probability of studying Applied Generals and Tech Levels for individuals born between September 1994 and August 1996, though not for older cohorts.

Winsorization. Additionally, I winsorize outliers in outcome variables that are not indicators to assess whether results are driven by extreme values. The winsorization is done for each academic year in the sample and for the top and bottom 5% of the dependent variable distribution. Given that using this strategy results in similar estimates as the baseline calculations (see Table A.10), I can conclude that the impact on vocational courses is not driven by outliers.

Expanding the scope of analysis. This study assesses the effect of increasing university fees on different academic cohorts, which justifies the choice of 12 months before and after the beginning of an academic year as the criterion to select the samples used in the baseline specifications. As an alternative approach, I examine the effect of including in the sample students born 18 months before and after the start of each relevant academic year (Table A.10). For the 16-year-old cohort, I find that, when including more months in the sample, the probability of taking a vocational course is increased by only 1.4 percentage points, a 70% reduction relative to the baseline estimate. Additionally, the effect on the share of vocational courses taken by this group is very small and no longer statistically significant. Thus, for this particular treated group, expanding the scope of analysis reduces the impact of the reform. As a an additional robustness check, I limit the scope to only include students born within 9 months of the threshold and I also estimate results when excluding from the sample the months of August and September. Results are very similar to the ones found in Section 1.5.2.

Alternative polynomials. The baseline regression discontinuity specification included a linear time trend on each side of the month-of-birth threshold. In Table A.10, I present regression estimates when controlling for alternative orders of polynomials. I follow Gelman and Imbens (2019) and only use polynomials of a degree lower than 3 to assess the robustness of my findings: in one case, I do not control for trends at either side of the cutoff point and, in another, I control for a separate quadratic trend on each side of the month-of-birth threshold.

For the second treated cohort, estimates are similar to those found previously, with the only exception being the probability of taking a vocational course using a local polynomial of order 2. However, for both types of functional forms, results are considerably larger than those found using a local linear polynomial for the first treated cohort. Thus, for this group, baseline estimates can be thought of as a lower bound for the effect of the reform on vocational education

enrollment.

1.7 Mechanisms

1.7.1 Courses within upper secondary school

This study argues that the vocational qualifications that are most likely to respond to changes in university fees are those more similar to academic courses. Applied Generals provide a broader learning approach and, as a result, are more comparable to academic courses than Tech Levels, which are more targeted qualifications. The results shown in Table A.11 present support for this hypothesis: the increases in the number and percentage of Applied Generals taken after the reform more than triple those of the Tech Levels for the first group analyzed. These results are also present in the extensive margin, with the increase in uptake of Applied Generals being larger than that of Tech Levels. For the second group, results show that while the enrollment in Applied Generals increased for the treated cohort, the response on Tech Levels was not statistically significant in any of the outcome variables.

As shown in Table 1.2, the amount of vocational courses available to secondary school students in the UK expands beyond Applied Generals and Tech Levels. Key Skills, National Vocational Qualifications and apprenticeships are some of the alternatives that individuals can choose from if they want to continue their studies in areas other than the academic path, which largely consists of A-level qualifications. I construct variables that measure the uptake of these alternative qualifications by dividing them in three groups: A-levels and other academic courses; vocational qualifications different from Applied Generals and Tech Levels; and remedial courses corresponding to lower levels of education. In Table A.12, I find that the average number of academic courses is significantly reduced for the first treated cohort. For these students, I also find that the increase in the proportion of Applied Generals and Tech Levels evidenced in Table 1.3 is attributed to reductions in the proportion of courses taken in all other types of qualifications, with academic courses accounting for over 51% of the change. In the case of the second affected cohort, however, it is the loss in the average percentage of other vocational qualifications that accounts for large part of the increase in the proportion of Applied Generals and Tech Levels.

When taken together, these results show that the increase in uptake of vocational upper secondary education courses can be attributed to different types of students: those on academic tracks, those within the vocational track and those in lower levels of education.

1.7.2 Demographic variables

The literature on vocational education in Britain has highlighted the differences in qualification returns for men and women, with each gender earning their highest premia from different types of qualifications. Men are better rewarded when they study Applied Generals and Tech Levels, while women achieve higher returns from teaching and nursing, which are higher education vocational qualifications (Dearden et al., 2002).[20] Given these disparities, it is relevant to assess whether the impact of the reform varied by gender. Table A.13 shows that male students were more affected by the reform. Difference-in-differences estimates for male respondents report an increase of 7 to 8 percentage points in the probability of having upper secondary vocational education as the highest qualification, whereas the effect for women is considerably lower and sometimes negative (-2 to 4 percentage points). I find similar results when using the regression discontinuity design: in both the extensive and the intensive margins, the increase in vocational education enrollment is larger for men (Table A.14). Regarding labour outcomes, all estimates reported for women are negative, though not always statistically significant. In contrast, results are more varied for men. Thus, despite the large increase in vocational participation, men do not experience significantly positive returns in the labour market as a result of this change.

Regarding ethnicity, Sá (2014) finds that the effect of the 2012 tuition fee changes on university attendance was stronger for White students than non-White individuals. She attributes this finding to two factors: the provision of government funded student loans and the ability of universities to charge higher fees conditional on having agreements with Office for Fair Access (OEFA) that can attract and support students of disadvantaged backgrounds. I find similar results when assessing the impact of the reform on the probability of having vocational education as the highest qualification, as White individuals in all treated cohorts present higher effects than non-White respondents. (Table A.13). Results on labour outcomes, however, are more

[20]The authors find that the highest returns for men correspond to what were previously called ONCs (Ordinary National Certificates) and ONDs (Ordinary National Diplomas), as well as City and Guilds qualifications. In the LFS, ONCs and ONDs are grouped in the same category as the current classification of Applied Generals. City and Guilds qualifications are classified as Tech Levels according to Hupkau et al. (2016).

varied, with very few results being statistically significant.

Using the ONS database, I am able to disaggregate effects by 4 types of ethnicity: White, Asian, Black and an additional category containing all other ethnic backgrounds. I find that Black students in the 18-year-old cohort had a positive and significant response to the reform, relative to their White counterparts: the probability of enrolling in a vocational education course and the percentage of vocational courses within the syllabus increased by 2 percentage points (Table A.15). Relative to White students, individuals from Asian and other ethnic backgrounds belonging to the first treated cohort had a decline in vocational education enrollment. Asian students in the second treated cohort do not appear to have a response to the increase in fees different from White students, while individuals from other ethnic backgrounds in this cohort only decreased the average amount of vocational education they took, relative to the omitted category.

Additionally, I show the differential effects of the reform on academic performance levels. While previous research suggests that selection according to cognitive ability determines educational pathways (Malamud & Pop-Eleches, 2010), I argue that changes in educational costs can also induce switches in qualification tracks. English students usually take national examinations at the end of lower secondary school called General Certificate of Secondary Education qualifications, or GCSEs. A common requisite to enroll in academic courses during upper secondary school is that students pass at least 5 of these examinations. Individuals who accomplished this goal were, on average, less likely to enroll in vocational education (see Table 1.3). However, Table A.16 shows that there is an additional effect of passing 5 GCSEs for students born after the reform, especially for the 18-year-old cohort, as the probability of enrolling in these courses declined by 22 percentage points. The impact of the reform is also large in the intensive margin, as the share of vocational education courses of these students is drastically reduced for students with 5 GCSEs (12 percentage points). Therefore, as a result of an increase in the cost of university, it is low-achieving students that switched to vocational education, perpetuating the ability gap between academic and vocational qualifications.

Finally, I find that poorer students are more sensitive to changes in university costs, using the neighborhood poverty index as a measure of economic status. These results, as well as those

described in the paragraph above, are in line with those presented in Chowdry et al. (2013), who find that students from lower socio-economic backgrounds are less likely to participate in higher education due to differences in academic performance in secondary school. Splitting students according to the median of the poverty index (the IDACI score), my results not only show that poorer students are more likely to take vocational education courses than richer students (Table 1.3), but that they are also more responsive to the 2012 higher education reform: economically disadvantaged individuals in the first treated cohort are 4 percentage points more likely to take a vocational education course than the rest of the students in this group (Table A.17). As a result of the change in university fees, they also increase the percentage of vocational courses they take during secondary school by 2 percentage points. A similar, though smaller effect, is detected for the second treated cohort.

1.8 Conclusion

So far, the literature on the impact of increasing university costs has focused on academic enrollment in higher education and labour outcomes. However, a large part of the education literature ignores the effect that these types of reform can have on other types of human capital accumulation. In this paper, I estimate the effect of increasing university fees on vocational education enrollment, as well as labour market outcomes. By looking at a large change in the cost of acquiring an academic degree that took place in England in 2012, I am able to understand how students in upper secondary school modify their educational choices according to this increase in fees. I argue that the decision to attend university is made when students enter upper secondary education and not when they are close to finishing it, as university entrance depends on taking specific academic courses during the entire extent of this educational stage.

Overall, I find a large effect in vocational education attainment and enrollment, but a limited impact on wages and the probability of being employed. Using a difference-in-differences approach, I show that, relative to students in the rest of the UK, English individuals exposed to the reform had a significant increase (3-6 percentage points) in the probability of ending their education in vocational upper secondary school.

The results from the regression discontinuity approach show a mixed substitution pattern

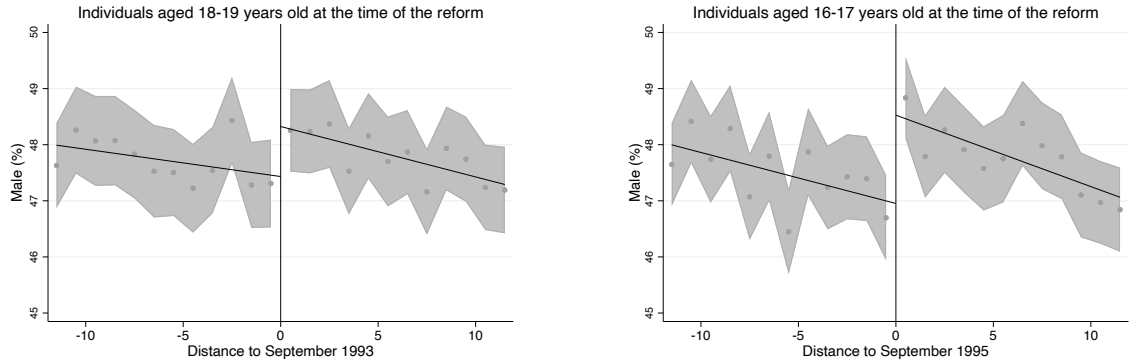
within upper secondary education students: part of the increase in Applied Generals and Tech Levels enrollment is the result of a substitution away from academic courses while another is attributed to students reducing the amount of other vocational courses they take. The first cohort to face the change in university fees had a significant increase in the number (0.17) and the percentage of Applied Generals and Tech Levels (2.8 percentage points) it took during upper secondary school, as well as in the probability of taking at least one of these vocational qualification (7 percentage points). For the cohort that started upper secondary school when the reform was put in place, results are smaller, though still significant.

Given that students from poorer backgrounds and lower academic performance are more responsive to the reform, the limited results on labour outcomes appear to indicate that the change in university costs was not economically beneficial for students belonging to these groups. Recent research supports the claim that income-based loans can help mitigate the impact of increasing university fees on enrollment of students belonging to lower socio-economic groups (Cabrales et al., 2019; Diris & Ooghe, 2018). However, little has been stated about how re-financing the higher education system can affect other schooling choices. Despite the continuing availability of financial aid for university students across the UK, my analysis shows that individuals are responsive to increasing fees, but do not appear to experience changes in employment or earnings in the short run once they switch to vocational education. An important next step would be to further analyze how this educational reform affected labour outcomes in the long run. Data limitations prevent me from assessing this phenomenon, as the cohorts studied in this paper are still very young.

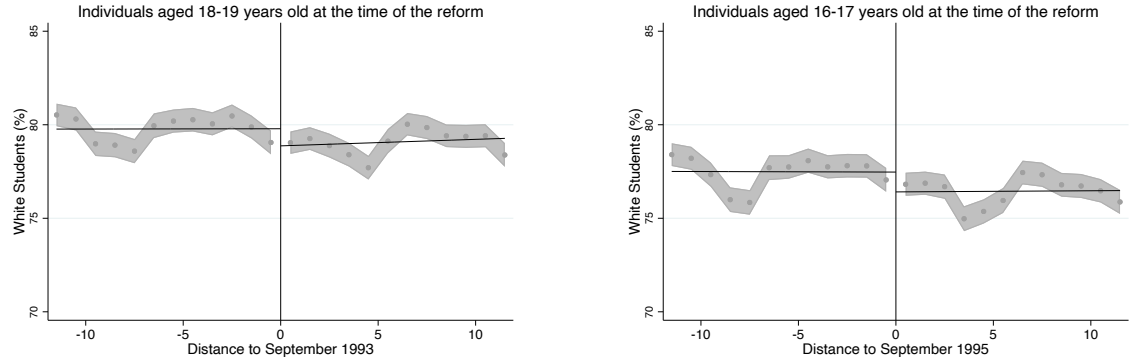
Appendix A: Chapter 1

Figure A.1: Monthly Averages of Regression Discontinuity Controls: Gender, Ethnicity (1992-1996)

(a) Male



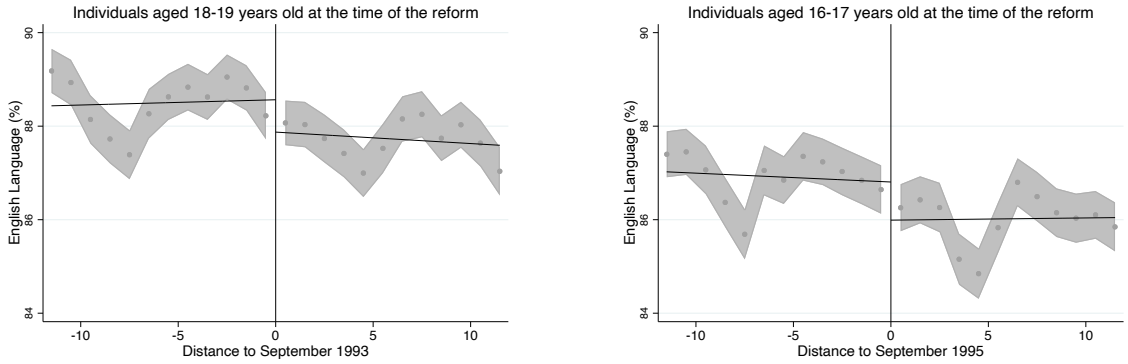
(b) White



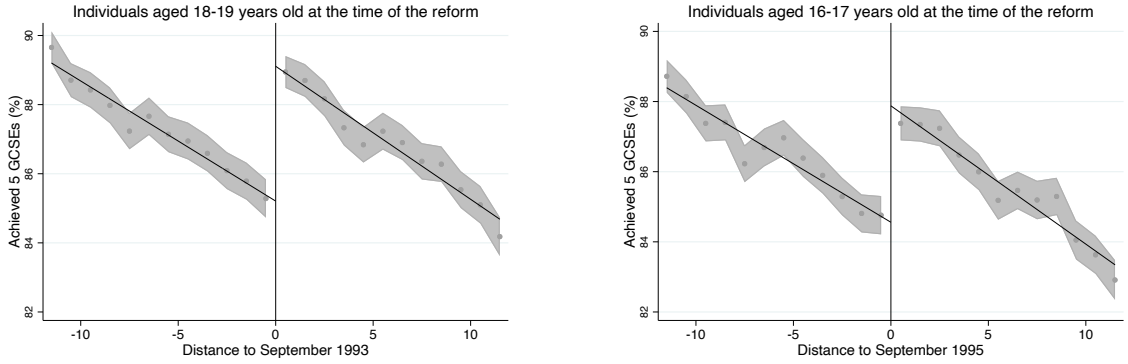
Notes: The figure plots monthly averages of two indicators used as controls in the main regression discontinuity specification: gender (male) and ethnic background (White). The solid line plots predicted values, with separate linear trends on either side of the start of the academic year threshold. The shaded areas show estimates within the 95% confidence intervals.
Source: Author's own computations based on data from the National Pupil Database, 2007-2015.

Figure A.2: Monthly Averages of Regression Discontinuity Controls: Language, Previous Education, Poverty (1992-1996)

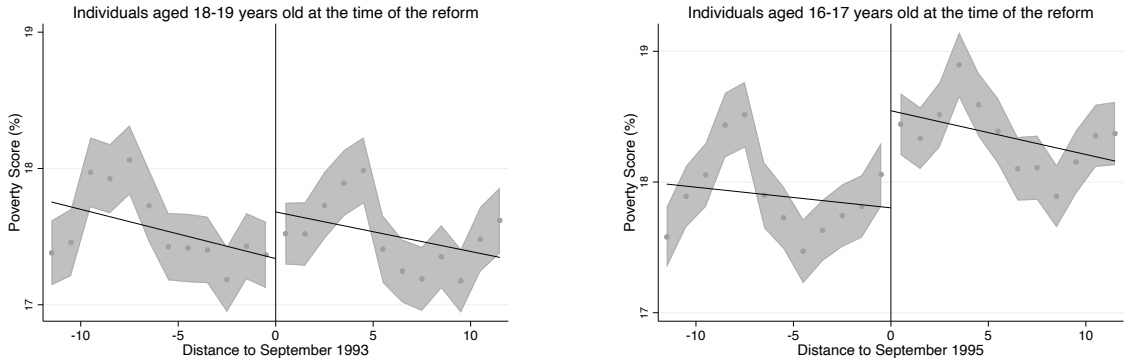
(a) English as Native Language



(b) Achieved 5 GCSEs



(c) Neighbourhood Poverty Index



Notes: The figure plots monthly averages of three variables used as controls in the main regression discontinuity specification: an indicator for English as native language, an indicator for having achieved 5 GCSEs and the neighborhood poverty score. The solid line plots predicted values, with separate linear trends on either side of the start of the academic year threshold. The shaded areas show estimates within the 95% confidence intervals. *Source:* Author's own computations based on data from the National Pupil Database, 2007-2015.

Table A.1: Variable Means for Regression Discontinuity Controls (1992-1996)

Panel A	Academic Year				
	1992-1993	1993-1994	RD estimate		
	Age during reform		Polynomial	Polynomial	Polynomial
	19	18	Degree: 1	Degree: 2	Degree: 3
	(1)	(2)	(3)	(4)	(5)
Male	0.477	0.478	0.049	-0.006	0.680
			[0.291]	[0.838]	[0.466]
Ethnicity: White	0.798	0.791	-0.094	-0.028	-0.265
			[0.145]	[0.686]	[0.759]
English Language	0.885	0.877	-0.042*	-0.026	-0.182
			[0.068]	[0.488]	[0.685]
Achieved 5 GCSEs	0.873	0.868	0.057**	0.020**	0.061
			[0.044]	[0.088]	[0.230]
Poverty score	0.175	0.175	0.031	0.009	0.176
			[0.106]	[0.130]	[0.113]
Number of schools	1,949	1,982			
Number of neighbourhoods	151	151			
Observations	187,691	198,224			
Panel B	Academic Year				
	1994-1995	1995-1996	RD estimate		
	Age during reform		Polynomial	Polynomial	Polynomial
	17	16	Degree: 1	Degree: 2	Degree: 3
	(1)	(2)	(3)	(4)	(5)
Male	0.475	0.478	-0.065**	0.052	-1.416
			[0.030]	[0.739]	[0.573]
Ethnicity: White	0.775	0.764	-0.046	-0.062	0.404
			[0.166]	[0.283]	[0.723]
English Language	0.869	0.860	-0.060*	-0.034	0.064
			[0.075]	[0.615]	[0.869]
Achieved 5 GCSEs	0.866	0.855	0.003	0.041	0.004
			[0.865]	[0.250]	[0.944]
Poverty score	0.179	0.183	0.012	0.013	-0.026
			[0.474]	[0.244]	[0.848]
Number of schools	2,020	2,082			
Number of neighbourhoods	151	151			
Observations	198,415	202,212			

Notes: Columns (1) and (2) present averages for the controls used in the regression discontinuity design. Means for the cohorts 1992-1993 and 1993-1994 are presented in Panel (A), while means for the cohorts 1994-1995 and 1995-1996 are shown in Panel (B). Columns (3) to (5) report the coefficients on the indicator for students born after September 1993 (panel (A)) or September 1995 (Panel (B)) from standard RD specifications where the respective characteristic is used as the dependent variable. The p-value of the wild cluster bootstrap test of the hypothesis that each coefficient is equal to 0 is reported in brackets (number of clusters=24, bootstrap replications=999). *** significant at 1%, ** significant at 5%, * significant at 10%.
Source: Author's own computations based on data from the National Pupil Database, 2007-2015

Table A.2: Difference-in-Differences Analysis of the Reform on the Probability of Vocational Upper Secondary Education as Highest Qualification and on Labour Outcomes (1988-1996)

	Voc Upper Secondary (1)	Hourly Wage (2)	Employment (3)	Permanent Employment (4)	Full-time Employment (5)
Eng x 1988	0.529	0.244	0.018	0.045	-0.010
	(1.077)	(0.616)	(0.029)	(0.034)	(0.017)
Eng x 1989	1.231	-0.261	0.003	-0.001	0.004
	(2.035)	(0.591)	(0.021)	(0.020)	(0.013)
Eng x 1990	0.965	-0.568	0.026	0.036*	-0.024
	(1.497)	(0.517)	(0.022)	(0.021)	(0.036)
Eng x 1991	1.863	-0.452	0.028	0.022	0.000
	(1.301)	(0.331)	(0.034)	(0.034)	(0.024)
Eng x 1993	4.613***	-0.869***	-0.015	-0.023	-0.040*
	(1.318)	(0.270)	(0.025)	(0.024)	(0.023)
Eng x 1994	3.006**	0.024	0.008	-0.004	0.021
	(1.189)	(0.358)	(0.028)	(0.025)	(0.038)
Eng x 1995	5.523***	-0.237	0.020	0.016	-0.038*
	(1.636)	(0.507)	(0.017)	(0.018)	(0.019)
Eng x 1996	5.066***	-0.347	-0.010	-0.023	0.023
	(1.507)	(0.575)	(0.036)	(0.037)	(0.030)
Eng	10.523***	-1.146	-0.011	-0.017	0.040
	(1.825)	(1.186)	(0.034)	(0.040)	(0.048)
Welsh	12.680***	-2.281*	-0.034	-0.058	0.014
	(1.620)	(1.231)	(0.030)	(0.037)	(0.046)
Scottish	6.146***	-1.682	-0.021	-0.006	-0.004
	(1.350)	(1.161)	(0.029)	(0.037)	(0.047)
Age	2.594**	0.698**	0.240***	0.253***	0.233***
	(1.182)	(0.331)	(0.016)	(0.016)	(0.018)
Age Squared	-0.054**	0.005	-0.004***	-0.004***	-0.004***
	(0.023)	(0.007)	(0.000)	(0.000)	(0.000)
Male	1.838***	-1.074***	-0.057***	-0.016***	-0.162***
	(0.437)	(0.064)	(0.005)	(0.004)	(0.009)
Religion	-0.231**	0.037	-0.001	-0.005**	-0.004**
	(0.111)	(0.066)	(0.002)	(0.002)	(0.002)
Observations	41,966	21,539	41,966	41,966	32,110
R-squared	0.009	0.181	0.061	0.046	0.068
Control group mean	11.700	11.404	0.783	0.675	0.788

Notes: This table presents results from a difference-in-differences specification. The sample includes respondents born in the UK between 1988 and 1996 who were over 19 years old at time of interview. The outcome variable in Column (1) measures whether the respondent's highest qualification is vocational upper secondary education. The sample for Column (2) only includes individuals who reported a positive wage. The outcome variable for this column is average hourly wage. The results for Columns (3)-(5) include respondents with and without a positive wage. The outcome variables for these columns are indicators for being employed (Column (3)), having permanent employment (Column (4)) and having full-time employment (Column (5)). Observations are weighted using income weights for Column (2) and individual weights for all other columns. The first reported estimates correspond to interaction terms between year of birth categorical values and the indicator for English-born individuals. Controls for gender, ethnicity (time trends), age and religion are included in all regressions, as well as country and year fixed effects. Standard errors are clustered at the region-of-residence and country-of-birth level. *** significant at 1%, ** significant at 5%, * significant at 10%.
Source: Author's own computations based on data from the Labour Force Survey (LFS), 2013-2020.

Table A.3: Difference-in-Differences Analysis of the Reform on Vocational Secondary Education and Labour Outcomes, Alternative Specification (1988-1996)

	Voc Upper Secondary (1)	Hourly Wage (2)	Employment (3)	Permanent Employment (4)	Full-time Employment (5)
Second Year Post x English	-1.197	0.497	0.001	-0.004	0.061***
	(1.003)	(0.545)	(0.025)	(0.023)	(0.021)
Post x English	4.065***	-0.398	-0.016	-0.028**	-0.033**
	(1.334)	(0.369)	(0.010)	(0.013)	(0.014)
English	11.552***	-1.352	0.005	0.005	0.035
	(1.535)	(1.211)	(0.031)	(0.038)	(0.046)
Welsh	12.788***	-2.261*	-0.033	-0.057	0.015
	(1.596)	(1.207)	(0.031)	(0.038)	(0.047)
Scottish	6.247***	-1.683	-0.020	-0.005	-0.003
	(1.316)	(1.144)	(0.030)	(0.038)	(0.047)
Age	2.596**	0.699**	0.240***	0.253***	0.232***
	(1.182)	(0.330)	(0.016)	(0.016)	(0.018)
Age Squared	-0.054**	0.005	-0.004***	-0.004***	-0.004***
	(0.023)	(0.007)	(0.000)	(0.000)	(0.000)
Male	1.840***	1.072***	0.057***	0.016***	0.162***
	(0.436)	(0.064)	(0.005)	(0.004)	(0.009)
Religion	-0.230**	0.037	-0.001	-0.005**	-0.004**
	(0.111)	(0.066)	(0.002)	(0.002)	(0.002)
Observations	41,966	21,539	41,966	41,966	32,110
R-squared	0.009	0.180	0.061	0.046	0.068
Control group mean	11.700	11.404	0.783	0.675	0.788

Notes: This table presents results from a difference-in-differences specification. The sample includes respondents born in the UK between 1988 and 1996 who were over 19 years old at time of interview. The outcome variable for Column (1) is an indicator for having vocational upper secondary education as the highest qualification. The sample for Column (2) only includes individuals who reported a positive wage. The outcome variable for this column is average hourly wage. The results for the remaining columns include respondents with and without a positive wage. The outcome variables for these columns are indicators for being employed (Column (3)), having permanent employment (Column (4)) and having full-time employment (Column (5)). Observations are weighted using income weights for Column (2) and individual weights for all other columns. The first reported estimates correspond to the interaction between the indicator for being born in England (*English*) and in either 1994 or 1996 (*Second Year Post*). The second line of estimates corresponds to the interaction between being born in England (*English*) after 1992 (*Post*). Controls for gender, ethnicity (time trends), age and religion are included in all regressions, as well as country and year fixed effects. Standard errors are clustered at the region-of-residence and country-of-birth level. *** significant at 1%, ** significant at 5%, * significant at 10%.
Source: Author's own computations based on data from the Labour Force Survey (LFS), 2013-2020.

Table A.4: Difference-in-Differences Analysis of the Reform on Vocational Secondary Education and Labour Outcomes, Using All Waves in the LFS (1988-1996)

	Voc Upper Secondary (1)	Hourly Wage (2)	Employment (3)	Permanent Employment (4)	Full-time Employment (5)
Eng x 1988	0.618	-0.557	0.017	0.032***	0.008
	(1.181)	(0.563)	(0.015)	(0.011)	(0.017)
Eng x 1989	1.379	-0.663	0.017	0.012	0.006
	(0.946)	(0.407)	(0.018)	(0.014)	(0.020)
Eng x 1990	-0.150	-0.712**	0.022*	0.016	-0.006
	(1.197)	(0.317)	(0.012)	(0.016)	(0.027)
Eng x 1991	0.394	-0.425*	0.022*	0.015	0.022
	(1.077)	(0.214)	(0.011)	(0.019)	(0.017)
Eng x 1993	3.520***	-1.179***	-0.010	-0.012	-0.001
	(1.102)	(0.248)	(0.012)	(0.012)	(0.010)
Eng x 1994	1.478	-0.276	-0.006	-0.007	0.034***
	(1.290)	(0.298)	(0.013)	(0.012)	(0.012)
Eng x 1995	3.206*	-0.396	0.020	0.031**	-0.000
	(1.648)	(0.471)	(0.015)	(0.014)	(0.020)
Eng x 1996	4.569***	-0.766**	-0.003	-0.004	0.021
	(1.094)	(0.337)	(0.012)	(0.015)	(0.020)
English	11.818***	-1.017	0.018	-0.001	0.001
	(1.681)	(1.048)	(0.034)	(0.039)	(0.033)
Welsh	11.791***	-2.402**	0.002	-0.032	-0.010
	(1.308)	(1.067)	(0.033)	(0.039)	(0.030)
Scottish	6.065***	-1.772*	0.023	0.021	-0.023
	(1.137)	(1.010)	(0.032)	(0.039)	(0.030)
Age	0.759	0.768**	0.219***	0.250***	0.269***
	(1.014)	(0.325)	(0.016)	(0.014)	(0.018)
Age Squared	-0.019	0.004	-0.004***	-0.004***	-0.005***
	(0.019)	(0.006)	(0.000)	(0.000)	(0.000)
Male	1.646***	1.085***	0.057***	0.015***	0.163***
	(0.440)	(0.056)	(0.004)	(0.004)	(0.009)
Religion	-0.348***	0.029	-0.000	-0.004**	-0.004**
	(0.110)	(0.033)	(0.002)	(0.002)	(0.001)
Observations	188,685	33,009	188,685	188,685	145,321
R-squared	0.008	0.186	0.057	0.045	0.074
Control group mean	12.055	11.156	0.797	0.688	0.790
Sample	All waves	All waves	All waves	All waves	All waves

Notes: This table presents results from a difference-in-differences specification using all the waves in the LFS. The sample includes respondents born in the UK between 1988 and 1996 who were over 19 years old at time of interview. The outcome variable in Column (1) is an indicator for having upper secondary school as the highest qualification. The sample for Column (2) only includes individuals who reported a positive wage. The outcome variable for this column is average hourly wage. The results for Columns (3)-(5) include respondents with and without a positive wage. The outcome variables for these columns are indicators for being employed (Column (3)), having permanent employment (Column (4)) and having full-time employment (Column (5)). The first reported estimates correspond to interaction terms between year of birth categorical values and the indicator for English-born individuals. Observations are weighted using income weights for Column (2) and individual weights for all other columns. Controls for gender, ethnicity (time trends), age and religion are included in all regressions, as well as country and year fixed effects. Standard errors are clustered at the region-of-residence and country-of-birth level. *** significant at 1%, ** significant at 5%, * significant at 10%.
Source: Author's own computations based on data from the Labour Force Survey (LFS), 2013-2020.

Table A.5: Difference-in-Differences Analysis of the Reform on Vocational Secondary Education and Labour Outcomes, Including Observations with Missing Values (1988-1996)

	Voc Upper Secondary (1)	Hourly Wage (2)	Employment (3)	Permanent Employment (4)	Full-time Employment (5)	Voc Upper Secondary (6)	Hourly Wage (7)	Employment (8)	Permanent Employment (9)	Full-time Employment (10)
Eng x 1988	-0.823	0.249	0.020	0.053*	-0.013	0.738	0.201	0.027	0.058*	-0.006
	(1.926)	(0.523)	(0.027)	(0.029)	(0.016)	(1.003)	(0.611)	(0.029)	(0.033)	(0.017)
Eng x 1989	0.243	-0.241	0.007	0.007	-0.010	1.270	-0.275	0.010	0.009	0.004
	(2.123)	(0.513)	(0.019)	(0.019)	(0.018)	(2.038)	(0.588)	(0.021)	(0.021)	(0.013)
Eng x 1990	-0.306	-0.418	0.030	0.044**	-0.025	1.099	-0.588	0.034	0.049**	-0.023
	(1.874)	(0.447)	(0.023)	(0.021)	(0.030)	(1.520)	(0.512)	(0.022)	(0.022)	(0.036)
Eng x 1991	1.891	-0.388	0.038	0.039	-0.009	1.939	-0.504	0.033	0.031	-0.001
	(1.209)	(0.320)	(0.030)	(0.030)	(0.022)	(1.289)	(0.315)	(0.034)	(0.034)	(0.024)
Eng x 1993	3.580**	-0.762***	-0.016	-0.013	-0.042**	4.826***	-0.899***	-0.016	-0.024	-0.042*
	(1.798)	(0.279)	(0.023)	(0.023)	(0.021)	(1.327)	(0.263)	(0.026)	(0.025)	(0.023)
Eng x 1994	1.632	-0.143	0.007	0.008	0.014	2.967**	0.015	0.006	-0.004	0.011
	(1.768)	(0.333)	(0.025)	(0.022)	(0.034)	(1.183)	(0.354)	(0.028)	(0.025)	(0.039)
Eng x 1995	4.531***	-0.307	0.016	0.016	-0.044**	5.203***	-0.270	0.014	0.014	-0.043**
	(1.702)	(0.426)	(0.015)	(0.016)	(0.018)	(1.577)	(0.508)	(0.017)	(0.017)	(0.019)
Eng x 1996	4.180**	-0.392	-0.021	-0.025	0.006	5.328***	-0.346	-0.019	-0.029	0.013
	(1.832)	(0.473)	(0.030)	(0.031)	(0.025)	(1.509)	(0.573)	(0.035)	(0.035)	(0.030)
English	2.758*	1.271***	0.005	0.033*	0.049***	9.822***	-1.089	-0.023	-0.030	0.040
	(1.549)	(0.473)	(0.020)	(0.018)	(0.015)	(1.855)	(1.180)	(0.034)	(0.040)	(0.048)
Welsh	4.652***	0.126	-0.007	0.008	0.019*	12.652***	-2.287*	-0.034	-0.057	0.014
	(1.570)	(0.606)	(0.010)	(0.011)	(0.010)	(1.615)	(1.225)	(0.030)	(0.037)	(0.046)
Scottish	-1.823	0.697	0.007	0.061***	0.002	6.145***	-1.692	-0.021	-0.005	-0.003
	(1.225)	(0.461)	(0.008)	(0.011)	(0.011)	(1.340)	(1.154)	(0.029)	(0.037)	(0.047)
Age	2.540**	0.675**	0.242***	0.256***	0.236***	2.680**	0.693**	0.243***	0.255***	0.232***
	(1.170)	(0.331)	(0.017)	(0.017)	(0.019)	(1.187)	(0.339)	(0.017)	(0.017)	(0.019)
Age Squared	-0.052**	0.006	-0.004***	-0.004***	-0.004***	-0.055**	0.005	-0.004***	-0.004***	-0.004***
	(0.023)	(0.007)	(0.000)	(0.000)	(0.000)	(0.023)	(0.007)	(0.000)	(0.000)	(0.000)
Male	1.850***	1.065***	0.056***	0.016***	0.164***	1.859***	1.071***	0.057***	0.017***	0.163***
	(0.422)	(0.066)	(0.005)	(0.004)	(0.008)	(0.431)	(0.065)	(0.005)	(0.004)	(0.009)
Observations	44,481	22,664	44,610	44,610	34,079	41,966	21,539	41,966	41,966	32,110
R-squared	0.004	0.179	0.053	0.040	0.067	0.005	0.179	0.053	0.040	0.066
Control group mean	11.722	11.156	0.782	0.669	0.789	11.700	11.404	0.783	0.675	0.788
Sample	Including missing values	Including missing values	Including missing values	Including missing values	Including missing values	Excluding some controls	Excluding some controls	Excluding some controls	Excluding some controls	Excluding some controls

Notes: This table presents results from difference-in-differences specifications. The sample includes respondents born in the UK between 1988 and 1996 who were over 19 years old at time of interview. Columns (1)-(5) present results for a sample that includes observations with missing values in some of the variables in the specification. Columns (6)-(10) show estimates for the baseline specification, excluding controls for ethnicity and religion. The outcome variable in Columns (1) and (6) is an indicator for having upper secondary school as the highest qualification. The samples for Columns (2) and (7) only include individuals who reported a positive wage. The outcome variable for these columns is average hourly wage. The results for Columns (3)-(5) include respondents with and without a positive wage. The outcome variables for these columns are indicators for being employed (Columns (3) and (8)), having permanent employment (Columns (4) and (9)) and having full-time employment (Columns (5) and (10)). The first reported estimates correspond to interaction terms between year of birth categorical values and the indicator for English-born individuals. Observations are weighted at the individual level, except for estimates in Columns (2) and (7), which use income weights. Controls for gender and age are included in all regressions, as well as country and year fixed effects. Standard errors are clustered at the region-of-residence and country-of-birth level. *** significant at 1%, ** significant at 5%, * significant at 10%.
Source: Author's own computations based on data from the Labour Force Survey (LFS), 2013-2020.

Table A.6: Difference-in-Differences Analysis of the Reform on Vocational Secondary Education and Labour Outcomes, Unweighted Observations (1988-1996)

	Voc Upper Secondary (1)	Hourly Wage (2)	Employment (3)	Permanent Employment (4)	Full-time Employment (5)
Eng x 1988	-0.029	0.127	-0.010	0.012	-0.010
	(1.183)	(0.598)	(0.028)	(0.032)	(0.015)
Eng x 1989	0.748	-0.190	0.005	-0.004	0.002
	(2.005)	(0.405)	(0.018)	(0.016)	(0.016)
Eng x 1990	0.687	-0.569	0.020	0.024	-0.017
	(1.932)	(0.387)	(0.018)	(0.018)	(0.035)
Eng x 1991	1.477	-0.350	0.028	0.015	0.002
	(1.405)	(0.277)	(0.034)	(0.034)	(0.024)
Eng x 1993	3.973***	-0.831***	-0.028	-0.040	-0.031
	(1.144)	(0.247)	(0.026)	(0.024)	(0.023)
Eng x 1994	2.540**	-0.027	-0.007	-0.015	0.018
	(1.138)	(0.437)	(0.027)	(0.024)	(0.035)
Eng x 1995	4.793***	-0.285	0.028*	0.017	-0.029
	(1.612)	(0.323)	(0.015)	(0.015)	(0.018)
Eng x 1996	4.646***	-0.072	-0.004	-0.030	0.025
	(1.562)	(0.610)	(0.040)	(0.038)	(0.031)
English	10.995***	-0.998	-0.021	-0.006	0.049
	(1.766)	(0.907)	(0.035)	(0.043)	(0.052)
Welsh	12.692***	-2.023**	-0.051*	-0.060	0.024
	(1.473)	(0.910)	(0.029)	(0.039)	(0.049)
Scottish	5.897***	-1.372	-0.031	-0.002	0.007
	(1.287)	(0.861)	(0.030)	(0.040)	(0.050)
Age	2.354**	0.617**	0.229***	0.245***	0.242***
	(0.981)	(0.282)	(0.015)	(0.016)	(0.018)
Age squared	-0.048**	0.007	-0.004***	-0.004***	-0.004***
	(0.023)	(0.029)	(0.045)	(0.050)	(0.057)
Male	1.913***	1.133***	0.061***	0.019***	0.171***
	(0.461)	(0.064)	(0.004)	(0.005)	(0.007)
Religion	-0.257*	0.075	-0.001	-0.004*	-0.004**
	(0.131)	(0.046)	(0.002)	(0.002)	(0.002)
Observations	41,966	22,283	41,966	41,966	32,110
R-squared	0.009	0.174	0.060	0.046	0.069
Control group	11.700	11.404	0.783	0.675	0.788

Notes: This table presents results from a difference-in-differences specification. The sample includes respondents born in the UK between 1988 and 1996 who were over 19 years old at time of interview. The outcome variable in Column (1) is an indicator for having upper secondary school as the highest qualification. The sample for Column (2) only includes individuals who reported a positive wage. The outcome variable for this column is average hourly wage. The results for Columns (3)-(5) include respondents with and without a positive wage. The outcome variables for these columns are indicators for being employed (Column (3)), having permanent employment (Column (4)) and having full-time employment (Column (5)). The first reported estimates correspond to interaction terms between year of birth categorical values and the indicator for English-born individuals. Controls for gender, ethnicity (time trends), age and religion are included in all regressions, as well as country and year fixed effects. Standard errors are clustered at the region-of-residence and country-of-birth level. *** significant at 1%, ** significant at 5%, * significant at 10%.
Source: Author's own computations based on data from the Labour Force Survey (LFS), 2013-2020.

Table A.7: Difference-in-Differences Analysis of the Reform on the Probability of Vocational Upper Secondary Education as Highest Qualification, Alternative Specifications (1988-1996)

	Voc Upper Secondary (1)	Voc Upper Secondary (2)	Voc Upper Secondary (3)
Eng x 1988	0.789	0.783	0.615
	(0.938)	(0.983)	(1.012)
Eng x 1989	1.416	1.291	1.221
	(1.976)	(2.036)	(2.012)
Eng x 1990	0.727	1.147	0.984
	(1.308)	(1.501)	(1.483)
Eng x 1991	2.089*	1.984	1.948
	(1.236)	(1.280)	(1.277)
Eng x 1993	4.689***	4.945***	4.925***
	(1.375)	(1.334)	(1.316)
Eng x 1994	2.823**	2.987**	3.087***
	(1.110)	(1.178)	(1.167)
Eng x 1995	5.414***	5.189***	5.240***
	(1.631)	(1.583)	(1.582)
Eng x 1996	5.373***	5.475***	5.420***
	(1.507)	(1.531)	(1.485)
English	1.604	9.807***	10.449***
	(2.684)	(1.848)	(1.819)
Welsh		12.665***	12.646***
		(1.596)	(1.624)
Scottish		6.172***	6.145***
		(1.303)	(1.345)
Age			2.534**
			(1.186)
Age squared			-0.053**
			(0.023)
Male			1.829***
			(0.440)
Religion			-0.230**
			(0.113)
Asian			-7.407***
			(0.926)
Black			-5.185***
			(1.084)
Other ethnicity			-4.900***
			(1.410)
Observations	41,966	41,966	41,966
R-squared	0.002	0.004	0.008
Control group mean	11.700	11.700	11.700
Fixed Effects	No	Country of birth	Country of birth
Time trends	No	No	No
Controls	No	No	Yes

Notes: This table presents results from a difference-in-differences specification. The sample includes respondents born in the UK between 1988 and 1996 who were over 19 years old at time of interview. The outcome variable in Columns (1)-(3) is an indicator for having vocational upper education as the highest qualification. Column (1) does not include fixed effects, time trends or controls. Column (2) includes country-of-birth fixed effects. Column (3) additionally includes controls. Observations are weighted using individual weights. Standard errors are clustered at the region-of-residence and country-of-birth level. *** significant at 1%, ** significant at 5%, * significant at 10%.
Source: Author's own computations based on data from the Labour Force Survey (LFS), 2013-2020.

Table A.8: Difference-in-Differences Analysis of the Reform on Wages and Employment, Alternative Specification (1988-1996)

	Hourly Wage (1)	Hourly Wage (2)	Hourly Wage (3)	Employment (4)	Employment (5)	Employment (6)
Eng x 1988	-0.035	-0.017	0.199	0.025	0.025	0.025
	(0.583)	(0.568)	(0.612)	(0.028)	(0.028)	(0.029)
Eng x 1989	-0.526	-0.496	-0.286	0.009	0.009	0.009
	(0.677)	(0.672)	(0.587)	(0.022)	(0.022)	(0.020)
Eng x 1990	-0.598	-0.614	-0.582	0.037	0.037	0.033
	(0.520)	(0.492)	(0.514)	(0.023)	(0.023)	(0.022)
Eng x 1991	-0.641*	-0.602*	-0.519	0.036	0.036	0.033
	(0.325)	(0.326)	(0.318)	(0.037)	(0.037)	(0.034)
Eng x 1993	-0.740**	-0.743**	-0.905***	-0.013	-0.013	-0.015
	(0.327)	(0.321)	(0.263)	(0.028)	(0.028)	(0.026)
Eng x 1994	0.039	0.019	0.008	0.006	0.006	0.008
	(0.223)	(0.237)	(0.356)	(0.030)	(0.030)	(0.028)
Eng x 1995	-0.587	-0.521	-0.278	0.005	0.006	0.015
	(0.564)	(0.598)	(0.511)	(0.019)	(0.019)	(0.018)
Eng x 1996	-0.093	-0.096	-0.366	-0.008	-0.008	-0.018
	(0.516)	(0.498)	(0.576)	(0.032)	(0.032)	(0.035)
English	0.862**	-0.748	-1.124	0.004	-0.007	-0.012
	(0.370)	(1.316)	(1.183)	(0.024)	(0.036)	(0.034)
Welsh		-1.881	-2.288*		-0.017	-0.033
		(1.371)	(1.227)		(0.030)	(0.030)
Scottish		-1.524	-1.688		-0.008	-0.020
		(1.299)	(1.157)		(0.029)	(0.029)
Age			0.697**			0.240***
			(0.337)			(0.016)
Age squared			0.005			-0.004***
			(0.007)			(0.000)
Male			1.075***			0.057***
			(0.064)			(0.005)
Religion			0.037			-0.001
			(0.064)			(0.002)
Asian			0.547**			-0.111***
			(0.267)			(0.023)
Black			0.368			-0.108***
			(0.547)			(0.008)
Other ethnicity			0.719			-0.098***
			(0.523)			(0.011)
Observations	21,539	21,539	21,539	41,966	41,966	41,966
R-squared	0.044	0.045	0.180	0.013	0.013	0.059
Control group mean	11.404	11.404	11.404	0.783	0.783	0.783
Fixed Effects	No	Country of birth	Country of birth	No	Country of birth	Country of birth
Time trends	No	No	No	No	No	No
Controls	No	No	Yes	No	No	Yes

Notes: This table presents results from a difference-in-differences specification. The sample includes respondents born in the UK between 1988 and 1996 who were over 19 years old at time of interview. The outcome variable in Columns (1)-(3) is average hourly wage. The sample for these columns only includes individuals who reported a positive wage. The results for Columns (4)-(6) include respondents with and without a positive wage. The outcome variable for these columns is an indicator for being employed. Columns (1) and (4) do not include fixed effects, time trends or controls. Columns (2) and (5) include country-of-birth fixed effects. Columns (3) and (6) additionally include controls. Observations are weighted using income weights for Columns (1)-(3) and individual weights for all other columns. Standard errors are clustered at the region-of-residence and country-of-birth level. *** significant at 1%, ** significant at 5%, * significant at 10%.
Source: Author's own computations based on data from the Labour Force Survey (LFS), 2013-2020.

Table A.9: Difference-in-Differences Analysis of the Reform on Permanent and Full-Time Employment, Alternative Specification (1988-1996)

	Permanent Employment (1)	Permanent Employment (2)	Permanent Employment (3)	Full-time Employment (4)	Full-time Employment (5)	Full-time Employment (6)
Eng x 1988	0.056*	0.055*	0.056*	-0.001	-0.001	-0.007
	(0.032)	(0.032)	(0.033)	(0.024)	(0.024)	(0.017)
Eng x 1989	0.007	0.008	0.008	0.006	0.006	0.004
	(0.022)	(0.021)	(0.020)	(0.015)	(0.015)	(0.013)
Eng x 1990	0.053**	0.050**	0.047**	-0.014	-0.013	-0.024
	(0.021)	(0.022)	(0.022)	(0.038)	(0.037)	(0.035)
Eng x 1991	0.033	0.033	0.031	0.003	0.003	-0.001
	(0.036)	(0.036)	(0.034)	(0.023)	(0.023)	(0.024)
Eng x 1993	-0.022	-0.024	-0.023	-0.028	-0.027	-0.042*
	(0.026)	(0.026)	(0.025)	(0.024)	(0.024)	(0.023)
Eng x 1994	-0.004	-0.005	-0.002	0.019	0.020	0.011
	(0.022)	(0.022)	(0.025)	(0.039)	(0.039)	(0.038)
Eng x 1995	0.005	0.005	0.015	-0.048**	-0.049**	-0.043**
	(0.018)	(0.018)	(0.017)	(0.021)	(0.021)	(0.019)
Eng x 1996	-0.018	-0.019	-0.028	0.030	0.031	0.013
	(0.031)	(0.032)	(0.036)	(0.030)	(0.030)	(0.030)
English	-0.005	-0.013	-0.021	0.035	0.046	0.042
	(0.028)	(0.040)	(0.040)	(0.021)	(0.048)	(0.048)
Welsh		-0.040	-0.058		0.027	0.014
		(0.036)	(0.037)		(0.046)	(0.046)
Scottish		0.008	-0.006		0.003	-0.004
		(0.036)	(0.037)		(0.046)	(0.046)
Age			0.253***			0.232***
			(0.016)			(0.019)
Age squared			-0.004***			-0.004***
			(0.000)			(0.000)
Male			0.017***			0.163***
			(0.004)			(0.009)
Religion			-0.005**			-0.004**
			(0.002)			(0.002)
Asian			-0.071***			0.010
			(0.020)			(0.011)
Black			-0.107***			-0.081***
			(0.014)			(0.024)
Other ethnicity			-0.101***			-0.052***
			(0.015)			(0.016)
Observations	41,966	41,966	41,966	32,110	32,110	32,110
R-squared	0.011	0.011	0.044	0.005	0.005	0.067
Control group mean	0.675	0.675	0.675	0.788	0.788	0.788
Fixed Effects	No	Country of birth	Country of birth	No	Country of birth	Country of birth
Time trends	No	No	No	No	No	No
Controls	No	No	Yes	No	No	Yes

Notes: This table presents results from a difference-in-differences specification. The sample includes respondents born in the UK between 1988 and 1996 who were over 19 years old at time of interview. The outcome variable in Columns (1)-(3) is an indicator for permanent employment. The outcome variable for Columns (4)-(6) is an indicator for full-time employment. Columns (1) and (4) do not include fixed effects, time trends or controls. Columns (2) and (5) include country-of-birth fixed effects. Columns (3) and (6) additionally include controls. Observations are weighted using individual weights. Standard errors are clustered at the region-of-residence and country-of-birth level. *** significant at 1%, ** significant at 5%, * significant at 10%.
Source: Author's own computations based on data from the Labour Force Survey (LFS), 2013-2020.

Table A.10: Regression Discontinuity Analysis of the Reform on Applied Generals and Tech Levels Enrollment, Alternative Specifications (1992-1996)

	Any Courses (1)	Number of Courses (2)	Percentage of Courses (3)	Any Courses (4)	Number of Courses (5)	Percentage of Courses (6)
	1992-1994 Aged 18-19 during reform			1994-1996 Aged 16-17 during reform		
No controls	7.842*	0.189*	3.098**	3.286*	0.261**	1.432*
	(0.965)	(0.038)	(0.248)	(0.655)	(0.028)	(0.496)
	[0.051]	[0.082]	[0.040]	[0.074]	[0.039]	[0.089]
No month-of-year fixed effects	10.099***	0.287***	4.229***	3.380***	0.323***	1.121***
	(0.251)	(0.005)	(0.074)	(0.170)	(0.008)	(0.093)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
No school fixed effects	6.606*	0.180*	2.857*	8.395**	0.460	3.754*
	(1.936)	(0.065)	(0.705)	(0.557)	(0.060)	(0.788)
	[0.055]	[0.066]	[0.063]	[0.018]	[0.102]	[0.083]
Winsorized by year at the top and bottom 5%	6.965**	0.170**	2.716**	4.505*	0.289*	2.188*
	(1.032)	(0.025)	(0.232)	(0.493)	(0.029)	(0.367)
	[0.042]	[0.047]	[0.042]	[0.064]	[0.071]	[0.070]
Excluding August and September	6.916*	0.166	2.784*	4.362*	0.310*	2.073*
	(1.330)	(0.044)	(0.327)	(0.585)	(0.036)	(0.459)
	[0.080]	[0.101]	[0.065]	[0.073]	[0.073]	[0.093]
18-month sample	6.802**	0.157*	2.673*	4.244**	0.299	2.100
	(1.153)	(0.044)	(0.419)	(0.423)	(0.050)	(0.581)
	[0.048]	[0.093]	[0.091]	[0.043]	[0.122]	[0.194]
36-month sample	7.139***	0.187***	3.260***	1.390***	0.220***	0.021
	(0.239)	(0.006)	(0.047)	(0.173)	(0.010)	(0.091)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.847]
Local polynomial of order 0	10.766***	0.310***	4.438***	4.271***	0.374***	1.369***
	(0.109)	(0.004)	(0.043)	(0.075)	(0.005)	(0.038)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Local polynomial of order 2	12.696**	0.411*	5.737**	2.517*	0.416	1.275
	(2.079)	(0.072)	(0.479)	(0.641)	(0.032)	(0.509)
	[0.029]	[0.057]	[0.039]	[0.078]	[0.103]	[0.292]

Notes: This table presents results from a regression discontinuity specification with a local polynomial of order 1. The running variable is the date of birth of the students in the sample. The outcome variables measure whether the students are enrolled in any Applied Generals or Tech Levels courses (Columns (1) and (4)), the number of Applied Generals and Tech Levels courses taken (Columns (2) and (5)) and the percentage of Applied Generals and Tech Levels relative to all courses taken in upper secondary school (Columns (3) and (6)). Columns (1)-(3) show results for regressions where the threshold is September 1st 1993. The threshold for Columns (4)-(6) is the September 1st 1995. Rows shows point estimates and standard errors for the variable *Post* in equation (1.2) using alternative specifications: excluding controls, excluding month-of-year fixed effects, excluding school fixed effects, winsorizing outliers in the top and bottom 5% of the outcome variable distribution, excluding the months of August and September, performing analysis on a scope of 18 and 36 months, including local polynomials of order 0 and 2. Standard errors are clustered at the school level. *** significant at 1%, ** significant at 5%, * significant at 10%.
Source: Author's own computations based on data from the National Pupil Database, 2007-2015.

Table A.11: Regression Discontinuity Analysis of the Reform on Applied Generals and Tech Levels Enrollment Separately (1992-1996)

	(1) Any Courses	(2) Number of Courses	(3) Percentage of Courses	(4) Any Courses	(5) Number of Courses	(6) Percentage of Courses
	1992-1994 Aged 18-19 during reform			1994-1996 Aged 16-17 during reform		
Applied Generals						
Post	4.723**	0.126*	2.229**	4.779*	0.329*	1.922**
	(1.177)	(0.024)	(0.180)	(0.618)	(0.029)	(0.289)
	[0.046]	[0.051]	[0.033]	[0.067]	[0.092]	[0.049]
Observations	385,915	385,915	385,915	400,627	400,631	400,627
R-squared	0.273	0.270	0.287	0.357	0.357	0.384
Tech Levels						
Post	2.487**	0.039	0.551*	0.237	-0.015	0.213
	(0.922)	(0.016)	(0.200)	(0.548)	(0.016)	(0.446)
	[0.030]	[0.143]	[0.057]	[0.752]	[0.454]	[0.628]
Observations	385,915	385,915	385,915	400,627	400,631	400,627
R-squared	0.157	0.153	0.157	0.196	0.191	0.208

Notes: This table presents results from a regression discontinuity specification with a local polynomial of order 1. The running variable is the date of birth of the students in the sample. The outcome variables measure whether the students are enrolled in any Applied Generals or Tech Levels courses (Columns (1) and (4)), the number of Applied Generals and Tech Levels courses taken (Columns (2) and (5)) and the percentage of Applied Generals and Tech Levels relative to all courses taken in upper secondary school (Columns (3) and (6)). Columns (1)-(3) show results for regressions where the threshold is September 1st 1993. The threshold for Columns (4)-(6) is the September 1st 1995. Other independent variables include an index of neighborhood poverty, as well as indicators for gender, ethnicity, English as native language and achievement in lower secondary school exams. All regressions include neighborhood of residence, school and month-of-the-year fixed effects. Standard errors clustered by month and year are reported in parentheses. The p-value of the wild cluster bootstrap test of the hypothesis that each coefficient is equal to 0 is reported in brackets (number of clusters=24, bootstrap replications=999). *** significant at 1%, ** significant at 5%, * significant at 10%.
Source: Author's own computations based on data from the National Pupil Database, 2007-2015.

Table A.12: Regression Discontinuity Analysis of the Reform on Other Courses (1992-1996)

	Any courses			Number of courses			Percentage of courses		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	A-levels	Other Vocational	Lower Levels	A-levels	Other Vocational	Lower Levels	A-levels	Other Vocational	Lower Levels
Panel A: Individuals aged 18-19 during reform (1992-1994)									
Post	0.488	-0.511	0.099	-0.790*	0.007	-0.051	-1.418	-0.919	-0.560
	(0.385)	(3.745)	(0.500)	(0.204)	(0.112)	(0.033)	(2.029)	(1.462)	(0.558)
	[0.431]	[0.841]	[0.842]	[0.083]	[0.918]	[0.449]	[0.633]	[0.596]	[0.527]
Observations	385,915	385,915	385,915	385,915	385,915	385,915	385,915	385,915	385,915
R-squared	0.318	0.324	0.411	0.442	0.391	0.392	0.523	0.358	0.401
Panel B: Individuals aged 17-16 during reform (1994-1996)									
Post	0.041	-1.572	1.299	3.171*	-0.023	0.078*	-0.646	-1.691*	0.303
	(0.780)	(1.609)	(0.564)	(0.168)	(0.070)	(0.024)	(0.482)	(0.544)	(0.289)
	[0.928]	[0.594]	[0.170]	[0.053]	[0.785]	[0.063]	[0.338]	[0.097]	[0.495]
Observations	400,627	400,627	400,627	400,627	400,627	400,627	400,627	400,627	400,627
R-squared	0.348	0.274	0.422	0.443	0.425	0.341	0.546	0.323	0.372

Notes: This table presents results from a regression discontinuity specification with a local polynomial of order 1. The running variable is the date of birth of the students in the sample. The outcome variables measure whether the students are enrolled in any other courses besides Applied Generals and Tech Levels (Columns (1)-(3)), the number of courses taken (Columns (4)-(6)) and the percentage of courses besides Applied Generals and Tech levels relative to all courses taken in upper secondary school (Columns (7)-(9)). The courses that are not Applied Generals and Tech Levels are divided into three categories: A-levels and other academic courses, other vocational qualifications and lower level courses. The latter category refers to remedial courses from previous educational stages. Panel A shows results for regressions where the threshold is September 1st 1993. The threshold for Panel B is the September 1st 1995. Other independent variables include an index of neighborhood poverty, as well as indicators for gender, ethnicity, English as native language and achievement in lower secondary school exams. All regressions include neighborhood of residence, school and month-of-the-year fixed effects. Standard errors clustered by month and year are reported in parentheses. The p-value of the wild cluster bootstrap test of the hypothesis that each coefficient is equal to 0 is reported in brackets (number of clusters=24, bootstrap replications=999). *** significant at 1%, ** significant at 5%, * significant at 10%.
Source: Author's own computations based on data from the National Pupil Database, 2007-2015.

Table A.13: Difference-in-Differences Analysis of Reform on Vocational Secondary Education and Labour Outcomes, by Gender and Ethnicity (1988-1996)

	Voc Upper Secondary (1)	Voc Upper Secondary (2)	Hourly Wage (3)	Hourly Wage (4)	Employment (5)	Employment (6)	Permanent Employment (7)	Permanent Employment (8)	Full-time Employment (9)	Full-time Employment (10)
Panel A: Regressions by Gender										
Eng x 1993	1.574	7.610**	-1.268***	-0.482	-0.055	0.028	-0.057	0.011	-0.091***	0.012
	(1.622)	(3.043)	(0.459)	(0.467)	(0.051)	(0.033)	(0.051)	(0.033)	(0.024)	(0.031)
Eng x 1994	-1.827	7.476**	-0.446	0.388	-0.007	0.020	-0.047	0.037	-0.029	0.065***
	(1.699)	(2.892)	(0.492)	(0.710)	(0.051)	(0.021)	(0.042)	(0.032)	(0.063)	(0.024)
Eng x 1995	3.735***	7.318***	-0.849	0.326	-0.002	0.039	-0.000	0.033	-0.112***	0.026
	(1.293)	(2.695)	(0.513)	(0.541)	(0.027)	(0.036)	(0.037)	(0.043)	(0.026)	(0.033)
Eng x 1996	2.889*	6.981***	-0.631	-0.005	-0.008	-0.009	-0.050	0.003	-0.014	0.055
	(1.553)	(2.623)	(0.490)	(0.855)	(0.063)	(0.029)	(0.054)	(0.035)	(0.036)	(0.040)
Observations	21,812	20,154	11,477	10,062	21,812	20,154	21,812	20,154	16,092	16,018
R-squared	0.010	0.009	0.162	0.190	0.044	0.076	0.043	0.051	0.023	0.056
Sample	Female	Male	Female	Male	Female	Male	Female	Male	Female	Male
Panel B: Regressions by Ethncity										
Eng x 1993	3.308	4.632***	-1.973	-0.849***	0.158	-0.020	0.080	-0.025	-0.322**	-0.035
	(2.045)	(1.353)	(2.340)	(0.280)	(0.298)	(0.030)	(0.160)	(0.026)	(0.146)	(0.023)
Eng x 1994	1.263	3.041**	1.093	0.027	0.157***	0.004	-0.012	-0.003	-0.128	0.025
	(1.690)	(1.221)	(1.000)	(0.350)	(0.053)	(0.029)	(0.094)	(0.025)	(0.155)	(0.038)
Eng x 1995	-11.572	5.872***	-0.788	-0.223	0.132	0.017	-0.166	0.019	-0.278	-0.033
	(12.059)	(1.605)	(1.293)	(0.521)	(0.162)	(0.018)	(0.133)	(0.018)	(0.191)	(0.021)
Eng x 1996	3.851*	5.087***	-1.187	-0.329	0.308	-0.019	0.231	-0.032	0.003	0.024
	(2.111)	(1.501)	(1.139)	(0.588)	(0.258)	(0.034)	(0.173)	(0.036)	(0.222)	(0.032)
Observations	3,671	38,295	1,312	20,227	3,671	38,295	3,671	38,295	2,409	29,701
R-squared	0.009	0.006	0.179	0.180	0.116	0.047	0.105	0.035	0.092	0.066
Sample	Non-White	White	Non-White	White	Non-White	White	Non-White	White	Non-White	White

Notes: This table presents results from a difference-in-differences specification. The sample includes respondents born in the UK between 1988 and 1996 who were over 19 years old at time of interview. Panel A presents results by gender. Panel B reports estimates by ethnicity. The outcome variable in Columns (1) and (2) is an indicator for having upper secondary school as the highest qualification. The sample for Columns (3) and (4) only includes individuals who reported a positive wage. The outcome variable for these columns is average hourly wage. The results for Columns (5)-(10) include respondents with and without a positive wage. The outcome variables for these columns are indicators for being employed (Columns (5) and (6)), having permanent employment (Columns (7) and (8)) and having full-time employment (Columns (9)-(10)). The reported estimates correspond to interaction terms between year of birth categorical values and the indicator for English-born individuals. Controls for gender (in Panel B), ethnicity (in Panel A), age and religion are included in all regressions, as well as country and year fixed effects. Observations are weighted using income weights for Columns (3) and (4) and individual weights for all other columns. Standard errors are clustered at the region-of-residence and country-of-birth level. *** significant at 1%, ** significant at 5%, * significant at 10%.
Source: Author's own computations based on data from the Labour Force Survey (LFS), 2013-2020.

Table A.14: Regression Discontinuity Analysis of the Reform on Applied Generals and Tech Levels Enrollment, by Gender (1992-1996)

	Any Courses	Number of Courses	Percentage of Courses	Any Courses	Number of Courses	Percentage of Courses
	(1)	(2)	(3)	(4)	(5)	(6)
	1992-1994			1994-1996		
	Aged 18-19 during reform			Aged 16-17 during reform		
Post x Male	2.363***	0.091***	1.412***	0.736***	0.117***	0.468***
	(0.203)	(0.006)	(0.079)	(0.229)	(0.008)	(0.088)
	[0.000]	[0.000]	[0.000]	[0.008]	[0.000]	[0.000]
Post	8.337**	0.276**	3.403*	-1.566*	0.096	-1.982*
	(0.572)	(0.023)	(0.653)	(0.502)	(0.079)	(0.820)
	[0.033]	[0.037]	[0.085]	[0.074]	[0.490]	[0.075]
Observations	385,915	385,915	385,915	400,627	400,627	400,627
R-squared	0.301	0.301	0.327	0.392	0.393	0.436
Control group mean	6.116	0.105	1.242	21.357	0.635	7.981

Notes: This table presents results from a regression discontinuity specification with a local polynomial of order 1. The running variable is the date of birth of the students in the sample. The outcome variables measure whether the students are enrolled in any Applied Generals or Tech Levels courses (Columns (1) and (4)), the number of Applied Generals and Tech Levels courses taken (Columns (2) and (5)) and the percentage of Applied Generals and Tech Levels relative to all courses taken in upper secondary school (Columns (3) and (6)). Columns (1)-(3) show results for regressions where the threshold is September 1st 1993. The threshold for Columns (4)-(6) is the September 1st 1995. Each threshold is interacted with the gender indicator. All regressions include controls, as well as neighborhood of residence, school and month-of-the-year fixed effects. Standard errors clustered by month and year are reported in parentheses. The p-value of the wild cluster bootstrap test of the hypothesis that each coefficient is equal to 0 is reported in brackets (number of clusters=24, bootstrap replications=999). *** significant at 1%, ** significant at 5%, * significant at 10%.
Source: Author's own computations based on data from the National Pupil Database, 2007-2015.

Table A.15: Regression Discontinuity Analysis of the Reform on Applied Generals and Tech Levels Enrollment, by Ethnicity (1992-1996)

	Any Courses	Number of Courses	Percentage of Courses	Any Courses	Number of Courses	Percentage of Courses
	(1)	(2)	(3)	(4)	(5)	(6)
	1992-1994			1994-1996		
	Aged 18-19 during reform			Aged 16-17 during reform		
Post x Asian	-1.484***	-0.008	-0.263**	0.080	-0.012	-0.082
	(0.243)	(0.008)	(0.098)	(0.318)	(0.015)	(0.139)
	[0.000]	[0.296]	[0.016]	[0.797]	[0.450]	[0.582]
Post x Black	2.333***	0.105***	2.067***	-0.189	0.051**	-0.097
	(0.402)	(0.011)	(0.162)	(0.544)	(0.023)	(0.249)
	[0.000]	[0.000]	[0.000]	[0.759]	[0.042]	[0.714]
Post x Other	-1.416***	-0.030***	-0.365***	0.153	-0.089***	0.107
	(0.352)	(0.008)	(0.096)	(0.284)	(0.016)	(0.167)
	[0.001]	[0.004]	[0.000]	[0.601]	[0.000]	[0.517]
Post	9.718*	0.320**	4.037	-1.245	0.158	-1.810
	(0.524)	(0.024)	(0.627)	(0.456)	(0.076)	(0.807)
	[0.069]	[0.048]	[0.100]	[0.113]	[0.184]	[0.145]
Observations	385,915	385,915	385,915	400,627	400,627	400,627
R-squared	0.301	0.301	0.327	0.392	0.393	0.437
Control group mean	6.116	0.105	1.242	21.357	0.635	7.981

Notes: This table presents results from a regression discontinuity specification with a local polynomial of order 1. The running variable is the date of birth of the students in the sample. The outcome variables measure whether the students are enrolled in any Applied Generals or Tech Levels courses (Columns (1) and (4)), the number of Applied Generals and Tech Levels courses taken (Columns (2) and (5)) and the percentage of Applied Generals and Tech Levels relative to all courses taken in upper secondary school (Columns (3) and (6)). Columns (1)-(3) show results for regressions where the threshold is September 1st 1993. The threshold for Columns (4)-(6) is the September 1st 1995. Each threshold is interacted with ethnicity indicators. All regressions include controls, as well as neighborhood of residence, school and month-of-the-year fixed effects. Standard errors clustered by month and year are reported in parentheses. The p-value of the wild cluster bootstrap test of the hypothesis that each coefficient is equal to 0 is reported in brackets (number of clusters=24, bootstrap replications=999). *** significant at 1%, ** significant at 5%, * significant at 10%.
Source: Author's own computations based on data from the National Pupil Database, 2007-2015.

Table A.16: Regression Discontinuity Analysis of the Reform on Applied Generals and Tech Levels Enrollment, by Academic Performance (1992-1996)

	Any Courses	Number of Courses	Percentage of Courses	Any Courses	Number of Courses	Percentage of Courses
	(1)	(2)	(3)	(4)	(5)	(6)
	1992-1994			1994-1996		
	Aged 18-19 during reform			Aged 16-17 during reform		
Post x 5 GCSE	-21.839***	-0.797***	-12.336***	-3.863***	-0.857***	-2.473***
	(0.393)	(0.016)	(0.144)	(0.251)	(0.020)	(0.154)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Post	28.784**	1.022**	14.914*	2.201*	0.915**	0.378
	(0.536)	(0.025)	(0.580)	(0.512)	(0.073)	(0.804)
	[0.043]	[0.020]	[0.080]	[0.078]	[0.042]	[0.768]
Observations	385,915	385,915	385,915	400,627	400,627	400,627
R-squared	0.283	0.291	0.318	0.352	0.359	0.391
Control group mean	6.116	0.105	1.242	21.357	0.635	7.981

Notes: This table presents results from a regression discontinuity specification with a local polynomial of order 1. The running variable is the date of birth of the students in the sample. The outcome variables measure whether the students are enrolled in any Applied Generals or Tech Levels courses (Columns (1) and (4)), the number of Applied Generals and Tech Levels courses taken (Columns (2) and (5)) and the percentage of Applied Generals and Tech Levels relative to all courses taken in upper secondary school (Columns (3) and (6)). Columns (1)-(3) show results for regressions where the threshold is September 1st 1993. The threshold for Columns (4)-(6) is the September 1st 1995. Each threshold is interacted with an indicator for the student having passed 5 GCSEs. All regressions include controls, as well as neighborhood of residence, school and month-of-the-year fixed effects. Standard errors clustered by month and year are reported in parentheses. The p-value of the wild cluster bootstrap test of the hypothesis that each coefficient is equal to 0 is reported in brackets (number of clusters=24, bootstrap replications=999). *** significant at 1%, ** significant at 5%, * significant at 10%.
Source: Author's own computations based on data from the National Pupil Database, 2007-2015.

Table A.17: Regression Discontinuity Analysis of the Reform on Applied Generals and Tech Levels Enrollment, by Neighborhood Poverty (1992-1996)

	Any Courses	Number of Courses	Percentage of Courses	Any Courses	Number of Courses	Percentage of Courses
	(1)	(2)	(3)	(4)	(5)	(6)
	1992-1994			1994-1996		
	Aged 18-19 during reform			Aged 16-17 during reform		
Post x Poor	3.812***	0.135***	2.022***	1.344***	0.151***	0.515***
	(0.481)	(0.017)	(0.258)	(0.168)	(0.020)	(0.098)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Post	7.631**	0.252*	3.033*	-1.873**	0.081	-2.066
	(0.569)	(0.030)	(0.620)	(0.484)	(0.087)	(0.837)
	[0.036]	[0.051]	[0.078]	[0.039]	[0.665]	[0.102]
Observations	385,915	385,915	385,915	400,627	400,627	400,627
R-squared	0.281	0.286	0.311	0.353	0.357	0.391
Control group mean	6.116	0.105	1.242	21.357	0.635	7.981

Notes: This table presents results from a regression discontinuity specification with a local polynomial of order 1. The running variable is the date of birth of the students in the sample. The outcome variables measure whether the students are enrolled in any Applied Generals or Tech Levels courses (Columns (1) and (4)), the number of Applied Generals and Tech Levels courses taken (Columns (2) and (5)) and the percentage of Applied Generals and Tech Levels relative to all courses taken in upper secondary school (Columns (3) and (6)). Columns (1)-(3) show results for regressions where the threshold is September 1st 1993. The threshold for Columns (4)-(6) is the September 1st 1995. Each threshold is interacted with an indicator for above median neighborhood poverty (based on the IDACI score). All regressions include controls, as well as neighborhood of residence, school and month-of-the-year fixed effects. Standard errors clustered by month and year are reported in parentheses. The p-value of the wild cluster bootstrap test of the hypothesis that each coefficient is equal to 0 is reported in brackets (number of clusters=24, bootstrap replications=999). *** significant at 1%, ** significant at 5%, * significant at 10%.
Source: Author's own computations based on data from the National Pupil Database, 2007-2015.

Chapter 2

VAT Pass-through with an Informal Sector: Evidence from Mexico

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We would like to thank INEGI officials for granting on-site access to the firm-level data used in this study under the commitment of complying with the confidentiality requirements set by the Mexican laws. We gratefully acknowledge funding from the Banco de México Summer Research Program and from the IFS' TaxDev.

2.1 Introduction

The Value-Added Tax (VAT) is one of the most important sources of government revenue, particularly in developing countries (Gordon and Li 2009). Understanding who effectively bears the incidence of VAT increases is thus of major interest for public policy. The existing literature has studied the effect of VAT increases on consumer prices and firm outcomes in rich countries (see Benzarti and Carloni 2019 for a recent example), but little is known on how the burden of taxes is shared across consumers, firms and employees in the developing world where the existence of large informal sectors potentially complicate incidence patterns. Firms in the informal sector do not remit taxes - do they gain when their competitors in the formal sector face higher taxes? If so, how are these gains passed-through to their customers and employees?

This paper seeks to answer these questions and fill a gap in the literature by looking at a policy reform that increased VAT from 11% to 16% in areas close to international borders (hereafter 'border areas') in Mexico in 2014. We find that the change in VAT increased formal retail prices by 2 percentage points (a 38% pass-through) whilst informal prices increased by a less precisely estimated 0.9 percentage points. Informal retailers seemed to have gained when their competitors faced higher taxes: suggestive evidence from the firm census show an increase in profits and numbers of employees amongst informal retailers, though we also see a puzzling increase in the number of employees in formal firms.

Mexico is a good setting in which to study this question, for three reasons. First, the Mexican economy, like most emerging economies, is characterized by a large informal sector, which represents roughly one-fourth of its GDP and one-fourth of its retail consumption (Instituto Nacional de Estadística y Geografía, 2016b; Bachas et al., 2022). This makes it a good context in which to understand how interactions between formal and informal firms affect the overall distribution of the tax burden. Second, the availability of rich micro-data on retailers of all store types enables us to observe the evolution of a wide range of outcomes in the formal and informal sector before and after the reform. Third, the 2014 tax increase is both large (45%) and, somewhat uniquely, applies to only some areas of the country which had, until then, faced a lower VAT rate due to concerns regarding cross-border shopping. This provides us with research design with which to estimate causal effects of the change in the tax.

Our analysis leverages very rich Mexican data from two sources. The first is restricted access micro-data from Mexico's National Consumer Price Index (CPI) over the period 2013-2015, containing over 1.5 million observations for a wide variety of 142 different products collected from a wide range of establishments. The second consists of two rounds (2013 and 2018) of Mexico's Economic Census which covers the universe of firms in the country and includes information on 1,588,210 retailers. Both data-sets contain information on store type, which allows us to proxy for the formality status of each firm (and price quote) in our data, following the methodology developed in Bachas et al. (2022): in a nutshell, we allocate supermarkets and department stores to the formal sector and corner stores, public markets and street stalls to the informal sector.

We estimate the causal effect of the increase in the VAT rate on retail product prices and firm-level outcomes using a difference-in-differences approach. The rate increase only applied to border areas, which had until 2014 benefited from lower VAT rates. Our approach therefore compares price quotes and firms before and after the tax reform in border and non-border areas. Because our source of variation is geographical, the set of products in our control and treatment groups are the same so our results cannot be due to product-specific shocks. The price data is a panel, which enables us to check for evidence of different pre-trends in border and non-border areas prior to the reform. We find no such evidence. This is reassuring regarding the plausibility of our identifying assumption, we note however that no such data on pre-trends is available for firm-level outcomes (obtained from a repeated cross-section of the Economic Census), so results on these outcomes must be treated with more caution.

Our estimate of the average pass-through of taxes to formal retail prices, 38%, is well within the range of estimates of pass-through of VAT increases to prices in rich countries (see Benzarti et al., 2017). We find a positive effect on informal prices too, albeit of a much smaller size. This is consistent with an estimated increase in revenue of informal retailers, suggesting they may have gained market shares. This, together with evidence in Bachas et al. (2022) that Informality Engel Curves are downward sloping in developing countries (including Mexico), indicates that the burden of the reform was borne by richer households more than the average. Suggestive firm-level evidence indicates that informal firm owners may even have gained from

the tax increase, as we find a positive effect on their profits.

We additionally find that, when we divide our sample according to product type, formal pass-through of services more than doubles the estimates for merchandise goods. Since services are non-tradable, we argue that pricing strategies of retailers in this market are less constrained by the potential cross-location shopping that may arise when the prices of tradables are increased.

The next section of the paper reviews the related literature, while Section 2.3 details the context under which the VAT increase took place. Sections 2.4 and 2.5 describe the data and the empirical methodology employed, respectively. Results on retail prices are presented in Section 2.6 whilst Section 2.7 presents heterogeneous effects. Section 2.8 provides evidence on the effect of the reform on other firm outcomes. Section 2.9 concludes.

2.2 Related Literature

Taxes on the consumption of goods and services exist in countries all around the world. As a result, there is an extensive empirical literature which studies whether, and to what extent, changes in the tax rates on these goods and services are passed on to final consumer prices. The US has a sales tax, rather than a VAT. Many studies have focused on estimating the pass-through of sales tax rate changes, leveraging the fact that individual states in the US have full discretion over how and when to change their sales tax rates. Haig and Shoup (1934) calculated the sales tax pass-through for different states, with varying results across locations. Poterba (1996) focused on the clothing industry and found under-shifting (less than 100% pass-through to consumers) for the 1925-1939 period and full shifting (100% pass-through) for the 1947-1977 era. Focusing on a different sample of commodities and time-periods, Besley and Rosen (1999) found evidence of a 100% pass-through for 12 commodities sold across 155 cities between 1975 and 1998. More recent studies have also found that the extent of pass-through varies significantly by type of good, including: full pass-through and under-shifting for gas (see Chouinard and Perloff, 2004, for both effects; Doyle and Samphantharak, 2008, for under-shifting) and over-shifting for tobacco (Hanson and Sullivan, 2009) and alcohol (Kenkel, 2005).

While the US has a sales tax, most of the remaining countries in the world deploy a

value-added tax. The empirical evidence on pass-through of VAT rate changes has largely been focused on developed countries. Similarly to the US studies, there is evidence of under-shifting, full pass-through, and over-shifting. Carbonnier (2007) studied two VAT sector-specific reforms in France and found that a reduction in the rate of car sales, in 1987, and housing repairs services, in 1999, led to under-shifting of the tax to prices. In particular, he found that the consumer's share of the marginal tax burden created by the reform was 57% for the 1987 reform and 77% for the 1999 reform. Gautier and Lalliard (2013) undertook a broader scope and analyzed all VAT changes in France that occurred since 1995. They found that the average pass-through to consumer prices was equal to 80%. In the UK, Crossley, Low and Sleeman (2014) showed evidence of pass-through reversal after a few months of the implementation of a VAT reduction in 2008. Additionally, Kosonen (2013) analyzed a tax reduction for hairdressing services in Finland, which resulted in a pass-through close to 50%. Finally, Benedek et al. (2015), using a sample of 17 European countries between 1999 and 2013, showed that results vary by the type of VAT rate: reforms to standard rates produce a close to full pass-through, while reforms to differentiated rates (mostly reduced rates relative to the standard rate) led to pass-through of around 30%.

In an important recent study, Benzarti et al. (2017) found that the extent of pass-through is asymmetric across VAT rate increases and decreases. They establish this result using reforms to VAT rate changes in Finland and remaining EU member states between 1996 and 2015. For VAT rate increases, the estimated price pass-through varied between between 29% to 98%. Finally, Benzarti and Carloni (2019) study the tax burden incidence of a reform in France, where the tax rate was cut in sit-down restaurants. Using a difference-in-differences design, where non-restaurant market services serve as a control group, the authors find that only 10% of the reduction in the VAT burden was passed on to consumers in the form of lower prices. Instead, the authors find that firm owners were able to capture a large share of the reduced VAT burden, in the form of larger profits.

Despite the fact that the VAT is often the largest source of revenue in developing countries, there are fewer studies in these countries on the extent of pass-through from consumption tax reforms. Politi and Mattos (2011) focus on reforms to 10 basic products across 16 states in

Brazil. Similarly to the US studies, for empirical estimation the authors leverage the fact that the VAT rates are set by each state, rather than at the federal level. Consistent with the findings of Benzarti et al. (2017), this paper also provides evidence that the direction of the VAT change matters: the extent of pass-through based on rate increases differs, in absolute magnitude, from the extent of pass-through based on rate decreases. Additionally, the authors find significant heterogeneity by type of product: less than full pass-through existed for beef, bread, coffee, milk, rice and soybean; full pass-through existed for beans, butter and flour; only sugar exhibited more than full pass-through. Using the same setting as ours, Mariscal and Werner (2018) study VAT rate changes in Mexico. They find that the overall pass-through of VAT rate changes is limited, similarly to us, though their results are based on a larger set of reforms than our estimates.

When taken together, these reviewed studies suggest that the extent to which consumers bear the impact of consumption tax rate changes is strongly dependent on the environment in which reform occurs. The magnitude of pass-through differs, even within the same country setting, across geographical areas, across goods, and across types of firms. The limited evidence on pass-through in developing countries is surprising, given that the VAT is one of the most important tax instruments in these settings. Our paper contributes to this small set of studies which estimates VAT pass-through outside of high-income countries. Our study differs from Mariscal and Werner (2018) in several important ways. First, we estimate pass-through separately by formal and informal stores, which matters for the distributional implications of the VAT. Second, we study the impacts of the VAT reform on a wider range of outcomes than prices, which allows us to investigate how the reform-induced tax burden was shared between consumers, firm owners and other economic agents.

In our study, we focus on a VAT rate reform which was limited to certain geographical regions within the Mexican territory. Specifically, the reform removed the previous preferential tax treatment to border zones by equalizing the VAT rate in those areas to the standard VAT rate in the country's non border areas. Davis (2011) exploits the preferential tax zone that was in place until the reform in 2014; using a regression discontinuity design, the author finds evidence of sizable distortions in economic activity towards the border areas that benefit from the lower VAT rate. Of interest to our study, Davis finds a statistically significant impact for

retail, which is the main focus in our estimation. However, to the best of our knowledge, no other study has investigated how the removal of this preferential tax zone treatment impacted prices and other economic outcomes across formal and informal stores.

The existence of informal firms and their effects on taxation have been analyzed in the literature on public finance and development. Several studies have focused on how the existence of informality constrains revenue collection - including Bird and Gendron (2006), Gordon and Li (2009), Besley and Persson (2014) and Jensen (2022). While most prior studies have focused on the impacts of informality for the ability of governments to raise revenues, less attention has been devoted to studying how informality affects the equity of tax systems in developing countries (Arunatilake, Inchauste and Lustig, 2017). Bachas et al. (2022) use expenditure diaries from developing countries to show that poorer people are more likely to shop in informal stores where VAT is not levied on purchases; in contrast, richer people shop to a larger extent in formal, modern stores where it is most likely that purchases are subject to VAT. The existence of an 'informality engel curve' implies that the VAT is, in fact, redistributive and leads the share of taxes paid to rise with income. Using input-output matrices and making assumptions about the informality status of store types, this point has also been previously noted in the study by Jenkins, Jenkins and Kuo (2006) in the Dominican Republic. This paper contributes by directly estimating the (marginal) tax burden separately for formal and informal stores in a large, developing country. The estimates in this paper can serve as an important input to rigorously assess the progressivity of the VAT in developing countries.

2.3 Background on the VAT in Mexico

In order to replace the prevalent sales tax, known as *Impuesto Sobre Ingresos Mercantiles* (ISIM), the Mexican government introduced the VAT in 1980. This tax had historically had a lower rate in areas close to the border than in the rest of the territory, in an effort to discourage citizens living close to the frontier from shopping in neighboring countries. The original preferential rate imposed in the border regions was 6%, 4 percentage points lower than in the rest of the country. In 1983, the interior rate was increased from 10% to 15%, but no change took place in the border. The first equalization of the VAT across the country took place in 1991,

when the general rate was set at 10%. However, after the Tequila crisis, in 1995, the interior rate was once again raised to 15%, while no change took place in border regions (Lustig, 2010). Finally, in 2010, both rates were increased by one percentage point, establishing the VAT at 11% in border areas and 16% in the rest of the country (Abramovsky and Phillips, 2015).

During September 2013, in an effort to stimulate the economy, president Peña Nieto proposed a large fiscal reform to the Congress, which included the equalization of VAT rates. The bill was passed in October of the same year and was put in place on January 1st 2014, when a general VAT rate of 16% was established across Mexico. In what follows, we consider the possibility that the VAT increase was anticipated by firms as early as September 2013 when looking at dynamic effects.

The original preferential tax zone was defined as areas within 20 kilometers of both borders (north and south). Over time, the classification of the preferential zone expanded to include the entire territory of the states of Baja California, Baja California Sur, Quintana Roo and some additional municipalities in the state of Sonora (Davis, 2011). Since these regions lobbied for and were granted preferential tax treatment, they differ from other areas in the country. This, in turn, raises endogeneity concerns (Davis, 2011). However, only two of the eight treated cities in our sample are located beyond the 20-kilometer line. Figure 2.1 shows a map of the country, where the sections in black denote the areas considered as part of the border under the 2013 legislation (Mexican Federal Government, 2016).

2.4 Data

2.4.1 Price data

Our analysis uses restricted-access price data obtained from Mexico's National Consumer Price Index, which is published by the National Institute of Statistics and Geography (INEGI). This data includes price quotes for 142 products, which are collected in cities with at least 20,000 inhabitants and spread across the 32 states of the country. Stores and products are selected according to household surveys and market studies (Instituto Nacional de Estadística y Geografía, 2013). Each price observation contains information on the product, date and store from where it was extracted. Our final sample averages weekly-reported data by month for a

period of 3 years: one before the reform (2013) and the first two after the VAT was put in place (2014-2015).

We classify goods in the sample as taxed or non-taxed following the Mexican Federal VAT law and consultations with researchers at the Banco de México.[4] The same approach was used to classify cities in border and non-border regions. We first restrict the sample to taxed products only, as we are interested in measuring the effect of the reform on the goods that were directly affected. Secondly, within taxed products we only consider goods which are considered as part of Banco de Mexico's 'core inflation' group, which includes services and merchandise products. Additionally, we exclude products with an indexed price above 1,000 in at least one period of the sample.[5] Finally, we eliminate products that disappear from the sample over time (i.e. long-distance communications). Note that product characteristics may change over time: a product's content may change, and INEGI occasionally changes the brand of the price quotes it collects. To prevent large jumps in prices due to these issues, we use a price variable generated by INEGI that takes into account changes in quality and adjusts prices accordingly[6].

Information is collected for 46 cities in the country, with 8 of those located in border areas. Figure 2.1 shows a map of the country, where the gray dots represent cities in the non-border region and the white dots denote the cities in the sample located in preferential tax areas, under the 2013 legislation (Mexican Federal Government, 2016).

We follow the methodology developed by Bachas et al. (2022) to allocate stores in the our data to the formal or informal sector. As they show in their paper, 'traditional' stores - labor-intensive, small retailers - have a very low probability of paying any taxes to the government, whilst 'modern' stores - capital-intensive, large retailers - are much more likely to pay consumption taxes, such as VAT. We follow their methodology and consider as formal establishments

[4]Products that are not taxed with the 16% rate can either be classified as tax-exempt or zero-taxed. The difference in these categories lies in the right to claim tax credits for the purchases performed to suppliers (Ahmad, Best and Pöschl, 2012). To the effects of this study, both types of goods are considered as non-taxed and excluded from the analysis.

[5]Indexed prices were set to 100 at the end of 2010.

[6]The price variable taken into account for this task is called *relativo* and consists on a price index, whose reference base period is the last fortnight of 2010. When a product change occurs in period t, instead of calculating the change between the price of the old product in *t-1* and the new product in t, the *relativo* value for that period considers the price change between t and *t-1* of the new product. In this way, the estimated change is not due to a modification in the quality of the good.

Table 2.1: Summary Statistics for Prices, by Treatment and Formality Status (2013-2015)

		Non-Border		Border		Full sample
		Informal	Formal	Informal	Formal	
Full sample						
Raw Price Quote	*Mean*	10,202.080	850.722	9,267.551	777.184	4,471.697
	Median	200	174.5	194.9	169	180
Price Variable	*Mean*	112.535	111.486	111.759	111.494	111.849
	Median	110.050	109.051	108.969	108.520	109.371
Merchandise	*Mean*	0.635	0.986	0.663	0.986	0.849
	Median	1	1	1	1	1
Observations		521,316	819,612	103,860	140,112	1,584,900

Notes: This table shows summary statistics for the variables of interest in the treatment and control groups, where the treatment group is denoted as *Border* and the control group as *Non-border*.
Source: Authors' own computations based on data from the Consumer Price Index, 2013-2015.

department stores, price clubs[7], supermarkets and places that offer online purchases. Every other type of store is classified as informal, including public markets, *tianguis* (street stalls), specialized shops, convenience stores and grocery stores.

The resulting sample consists of 44,025 unique products and 1,584,900 observations, 15% of which correspond to treated regions. Table B.1 of the appendix names all the cities in the sample, by treatment status.

Table 2.1 shows summary statistics for the log prices in the sample. While raw price quotes vary considerably between the formal and the informal sector, average and median indexed prices (the variable we use in our analysis below) are very similar across regions and sectors. A similar share of observations (39% and 42%) come from informal stores in border and non-border regions. From the 6,365 stores studied in this sample, 1,079 are located in border areas, with 91% of them belonging to the informal sector, a figure comparable to the share of informal firms found in the rest of the country (91%). In addition, while the formal sector is composed almost entirely of merchandise goods, the informal sector is more evenly distributed among services and merchandise.

[7]Price clubs are stores in which customers are charged a fee to have full access to low-price products.

Figure 2.1: Preferential Tax Area and Cities in Sample, by Treatment Group

Notes: The map shows the formal preferential tax areas and the Mexican cities used in the sample. Areas that had a VAT rate of 11% until 2014 are shown in black. White dots denote preferential tax cities in the sample, which are referred to as *border cities*. They represent the treatment group. Gray dots indicate cities in the rest of the country (control group). Information based on Mexican Federal Government (2016).

2.4.2 Firm-level data

We use store-level outcomes obtained from the Economic Census, which is conducted by INEGI every 5 years. It collects information on the universe of Mexican retailers, regardless of whether they are formally registered (Higgins, 2019).[8] We use data from the the 2014 and 2019 census rounds, which refer to data for 2013 and 2018, respectively: we use the last round before the reform, and the first round after the reform. Each store is identified with a unique code such that we can track firms over time. This allows us to study the effects of the reform on firm entry-exit (Section 2.8). We define our sample and allocate stores to the formal or informal sector using the six-digit North American Industry Classification System (NAICS) codes in the data. To restrict our analysis to consumer-facing firms we only consider establishments that are part of the following sectors: retail trade (code 46); arts, entertainment, and recreation (code 71); accommodation and food services (code 72), other services except public administration (code 81). As an additional restriction, we exclude from the sample stores where the largest revenue sources are non-taxed products (for example, greengrocers), as well as stores that are not present in both waves or whose store type code has changed over time. Within the remaining sample, we classify every store as informal with the exception of supermarkets, mini-markets, department stores, pharmacies, gas stations and online stores. In this sample, only 6% of firms are considered formal.

Table 2.2 presents descriptive statistics on the set of firm-level variables included in our analysis. To consider the effect of the tax increase on firms' total sales, we look at the revenue variable, which is equal to sales at the pre-tax price. For the effect on labor variables, we consider total labor costs (cost of employees), as well as the number of employees in the firm. To consider potential spillover effects upstream in supply chains, we consider total costs of materials (intermediate inputs) and finally we use two different variables (before-tax profits and return on assets) to proxy for returns to capital.[9] We see that formal stores are substantially larger than informal stores in both regions (for all variables considered), as expected, and that stores in border regions are slightly bigger than those in non-border regions, in both sectors.

[8]The Economic Census covers all fixed establishments, therefore stores that do not fulfill this characteristic are excluded from the analysis.

[9]We define return on assets as profits over the sum of total stock, financial income and total fixed assets.

Table 2.2: Summary Statistics for Firm Outcomes, by Treatment and Formality Status (2014-2019)

		Non-Border		Border		Total
		Informal	Formal	Informal	Formal	
Full sample						
Revenue	*Mean*	1,109.135	17,089.010	1,930.731	20,488.870	2,205.483
	Median	185	756	240	4468	200
Cost of employees	*Mean*	137.057	1,445.899	278.626	1,697.681	231.907
	Median	30	98.173	40.123	329	33
Number of employees	*Mean*	2.429	5.196	3.178	6.106	2.664
	Median	2	2	2	2	2
Cost of materials	*Mean*	652.697	12,668.620	960.307	15,022.140	1,453.570
	Median	96	480	100	2800	96
Before-tax profits	*Mean*	281.949	3,412.007	524.513	4,138.133	502.937
	Median	33.817	135.840	44	529	36.682
Return on assets	*Mean*	7.729	17.765	8.491	12.128	8.407
	Median	1.191	1.115	1.189	1.160	1.184
Observations		2,750,369	176,803	224,871	24,377	3176,420

Notes: This table shows summary statistics for the Economic Census variables of interest in the treatment and control groups, where the treatment group is denoted as *Border* and the control group as *Non-border*. Information on costs, profits, revenue and return to assets is expressed in thousands of pesos.
Source: Authors' own computations based on data from the Economic Census, 2014-2019.

2.5 Empirical Methodology

2.5.1 Main specification

In order to estimate the average effect of the VAT change on consumer prices, we use a difference-in-differences estimation and compare the selected taxed products sold in Mexican border cities (our treatment group) to similar goods sold in the rest of the country (our control group). The analysis we perform focuses on how the differences between these regions varied between the pre-reform period, 2013, and the post reform period, 2014-2015. We define our baseline specification as follows:

$$Log(price)_{glmy} = \beta_1 Border_l \times Post_{my} + \beta_2 Post_{my} + \gamma_g + \delta_m + \epsilon_{glmy}, \tag{2.1}$$

where $Log(price)_{glym}$ denotes the log price of unique good g, sold in city l during year y and month m. A unique good is understood as a product sold in a specific store within a city. The variable $Border_l$ is an indicator for treated goods, while $Post_{ym}$ is equal to 1 if the price was collected from 2014 onwards, and 0 otherwise. $Border_l \times Post_{ym}$ measures the interaction of both variables. More specifically, β_1 calculates the impact of the rate change on prices for the goods directly affected by the reform, relative to products that had no change in the VAT. Hence, the estimated coefficient for β_1 is used to measure pass-through. Unique product and month fixed effects are denoted by γ_g and δ_m, respectively. Since unique products are identified for a given store and city, the inclusion of city fixed effects would be correlated with unique goods fixed effects and are therefore excluded from the identification.

We use product weights that reflect the representation of goods on aggregate household expenditure following the calculations made by Mariscal and Werner (2018). While the authors estimate product weights for generic products, we divide the generic product weights over the number of observations to create unique weights by store-product combination.

The error term, ϵ_{glmy}, is clustered by city and generic product to control for the possibility of within-correlation among similar goods sold in the same location (Bertrand, Duflo and Mullainathan, 2004).

Equation (2.1) is performed separately for formal and informal stores. Since formal stores are more likely to be directly affected by the tax, we expect the pass-through for this group to be considerably larger relative to the informal sector. In other words, β_1 is expected to be larger for formal stores than informal stores.

To examine the dynamics of how prices adjust over time to the 2014 VAT change, we modify the model in equation (2.1) to include leads and lags for the period 2013-2015:

$$Log(price)_{glj} = \sum_{j=-k}^{q} [\beta_j Period_j \times Border_l + \eta_j Period_j] + \gamma_g + \epsilon_{glj}, \tag{2.2}$$

where k is an index for months prior to announcement of the reform and q is an index for months after the announcement. Since the project to increase the VAT rate in border areas was presented to Congress in September, we impose $\beta_{\text{August2013}} = 0$ and $\eta_{\text{August2013}} = 0$. The dynamic coefficients of interest, β_{j}, allow us to estimate the effect of the reform every month before and after it was announced: coefficients for September 2013 until December of the same year measure the impact of the announcement, while every coefficient for 2014 and 2015 shows the percent change in log prices relative to August 2013. This interpretation assumes that in the absence of the tax change, the difference between border and non-border areas would have been the same as in the reference period. This specification allows us to quantify the impact of every month on price changes and to distinguish between short-run and long-run effects of the VAT equalization.

Given the difference-in-differences approach employed, the main identification assumption is that in the absence of the tax change, there would have been no change in the prices of products in the border regions relative to those in the non-border regions, besides those already captured by the fixed effects. Although we cannot test this assumption directly, Figure 2.2 appears to support it. The graph illustrates the evolution of weighted mean prices by sector for the taxed products included in the sample between 2013 and 2015 (see Figure B.1 for the price evolution of merchandise goods and services separately). Prices are set to 0 during August 2013, a month before president Peña Nieto introduced the bill to Congress. Since log prices during the months leading up to the reform are not statistically different between groups, we assume that this trend would have persisted if the VAT had not been equalized across the country.

The January 2014 reform could have coincided with other significant economic events, and as a result, they could be confounding factors in our specification. This scenario is unlikely, given the demarcation of the preferential tax areas. Firstly, these areas are close to several different countries, reducing the chances that foreign policy in one particular nation drives the results. Secondly, proximity to any country is not necessarily a characteristic of the preferential tax regions, as some treated cities are located far from any actual border (e.g. La Paz, in Baja California Sur). Additionally, pre-2014 VAT rates were assigned using an arbitrary 20-kilometer threshold with respect to the border, and do not correspond to state limits (Davis, 2011). Consequently, for some regions, state legislation affects both treated and non-treated cities. This helps mitigate concerns about unobserved differences across locations.

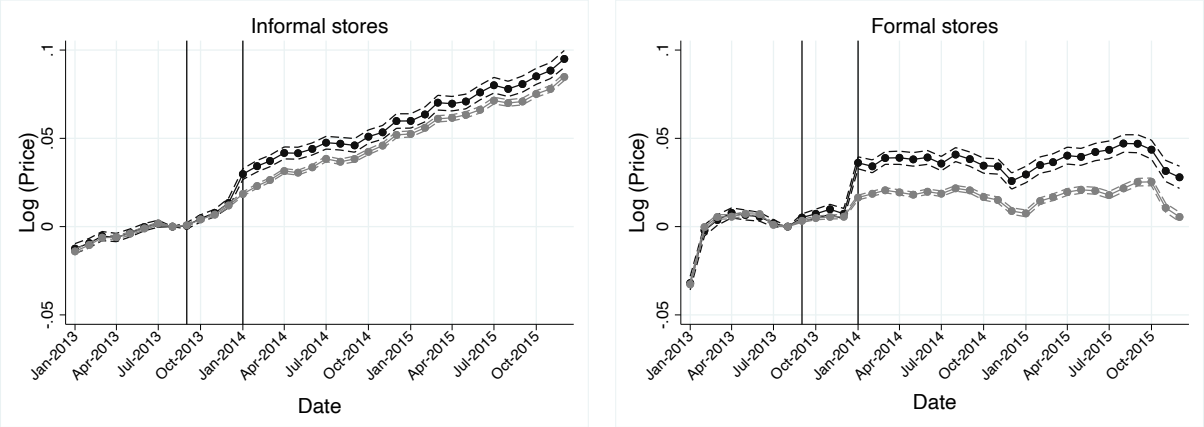
Moreover, even though the VAT equalization was part of a larger economic reform, the rest of the changes made to the fiscal system were performed either at a national or state level, which reduces the likelihood that other components of the reform are driving the results. Some products became taxed goods (chewing gum, pet food), while others had a new tax applied to them: 8% levy on non-basic food items with a caloric density of 275 kilo-calories per 100 grams, 3% tax on sale of fossil fuel except natural gas, application of the IEPS (Special Tax on Production and Services) to sugary drinks (Reuters, 2013). Since these policy changes were all enforced at the national level during the start of 2014, their impact would be reflected in the estimated coefficient for *Post*, instead of the interaction between *Post* and *Border*.

2.6 Results

2.6.1 Main estimates

Table 2.3 presents estimates for the impact of the 2014 VAT equalization on prices obtained following specification (2.1) estimated separately on price quotes of formal and informal stores. Results for formal and informal stores are presented in Table 2.3. Columns (1) and (2) show results for all taxed products in our sample. In the remaining columns of Table 2.3, we follow the literature (Mariscal and Werner, 2018) and present results by the two key components of core taxed products: merchandise and services. Given a 5 percentage-point increase in the VAT rate in Mexico, full pass-through would imply an estimate for β_1 equal to 0.05. Hence, we calculate

Figure 2.2: Average Log Prices, by Sector (2013-2015)



Notes: The figure shows the weighted average price evolution by area and sector, before and after the VAT rate increase of 5 percentage points in border regions during January 2014. The panel on the left shows the evolution for products sold in informal stores, while the panel on the right shows the time series for goods sold in formal retailers. Averages for border areas are shown in black, mean prices for non-border areas are displayed in gray. Log prices are normalized to 0 for August 2013. 95% confidence intervals for the mean in each period are presented in dashed lines.
Source: Authors' own computations based on data from the Consumer Price Index, 2013-2015.

pass-through as the estimate for β_1 divided by 0.05.

We see that the average increase in prices set by informal stores was small (0.9 percentage points) relative to the VAT increase (Column (1)), and only marginally statistically significant at the 10% level. This corresponds to an 18% pass-through. The average increase in formal stores, in contrast, was twice as large (1.9 percentage points), though we cannot rule out that the two estimates are the same in this sample. The implied pass-through to formal retail prices is over one-third, at 38%. This estimate is of similar magnitude to the results in Benzarti et al. (2017), who find a 29% pass-through to prices of large VAT changes in Europe from 1996 to 2015. Looking at large product types separately in columns (3) to (6) we see that most of the difference in pass-through rates between the formal and informal sectors comes from services, for which the pass-through to formal prices is much higher (perhaps because services are non-tradable, so that retailers' pricing strategies are less constrained by potential cross-location shopping).

Figure 2.3 presents the dynamic effects estimates of the reform on formal prices (right panel) and informal prices (left panel), first for all products and then separately for merchandise and services. Looking at informal prices, we see that the VAT increase had a small effect in the first panel, which is no longer significant after August 2014. These results are driven by

Table 2.3: Difference-in-Differences Analysis of the VAT Increase on Prices (2013-2015)

	Core products		Merchandise		Services	
	Informal Log price	Formal Log price	Informal Log price	Formal Log price	Informal Log price	Formal Log price
	(1)	(2)	(3)	(4)	(5)	(6)
Border x Post	0.009*	0.019***	0.010**	0.014***	0.008	0.035**
	(0.005)	(0.007)	(0.005)	(0.004)	(0.007)	(0.014)
Post	0.053***	0.016***	0.047***	0.036***	0.057***	-0.064***
	(0.002)	(0.004)	(0.003)	(0.002)	(0.003)	(0.008)
Observations	625,176	959,724	399,744	946,008	225,432	13,716
R-squared	0.147	0.015	0.127	0.043	0.162	0.226
Number of unique products	17,366	26,659	11,104	26,278	6,262	381
Fixed effects	Month & unique product	Month & unique product	Month & unique product	Month & unique product	Month & unique product	Month & unique product

Notes: This table compares retail prices in *border* and *non-border* areas around the January 2014 VAT equalization. Each column shows estimates for a separate difference-in-differences specification. Columns (1), (3) and (5) report results for the effect on prices of products sold in informal stores, while Columns (2), (4) and (6) show estimates for products sold in formal stores. Columns (1)-(2) present results for the entire sample of products. Columns (3)-(4) report estimates for merchandise goods. Columns (5)-(6) present results for services. Product weights are included in all specifications, as well as month and unique product fixed effects. Standard errors are clustered at the city-generic product level. *** significant at 1%, ** significant at 5%, * significant at 10%.
Source: Authors' own computations based on data from the Consumer Price Index, 2013-2015.

the sample of merchandise products only: there is no significant effect of the tax increase on informal service prices in any period (bottom left graph). Interestingly, we see some evidence of a possibly anticipatory effect for merchandise products, with informal prices increasing as soon as the reform is announced, prior to the increase in the tax.

The graphs on the left show that the reform has a persistent effect on formal retail prices, with the magnitude of the estimates being similar throughout the months of 2014 and most of 2015. Thus, the formal sector presents evidence of a long-term effect of the VAT change.

2.6.2 Robustness checks

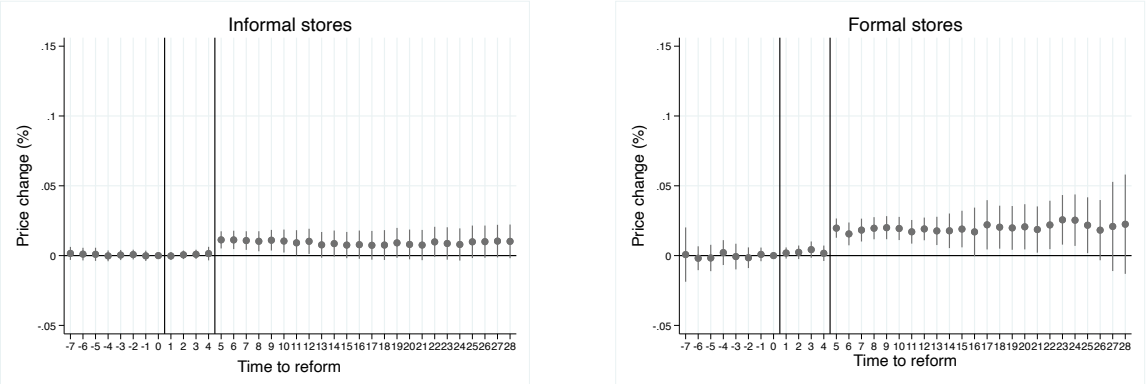
Outliers. We winsorize outliers in the top and bottom 5% of the price distribution by border and formality status for every month and year in the sample. The estimates presented in Figure B.2 and Table 2.4 are similar to those found in Figure 2.3, suggesting that the baseline results are not driven by outliers. The only notable change is found in informal services, where the pass-through estimation becomes marginally significant for the first 9 months after the reform was implemented.

Unweighted observations. Our baseline specification includes weights that reflect the share of each product in households' expenditures, as explained above. The majority of the products included in the sample correspond to merchandise goods, but services have, on average, larger weights. In this section, we present results for unweighted estimations. Put differently, we assess whether giving more weight to merchandise goods modifies our results. Columns (3) and (4) of Table 2.4 show that unweighted estimates for pass-through are smaller than those found in our baseline results (see Figure B.3 for dynamic effects). Since services in border areas, on average, experienced a larger increase in prices than merchandise goods, assigning less weight to the former reduces overall pass-through. Within each product category, we also document lower estimates in both formal and informal stores.

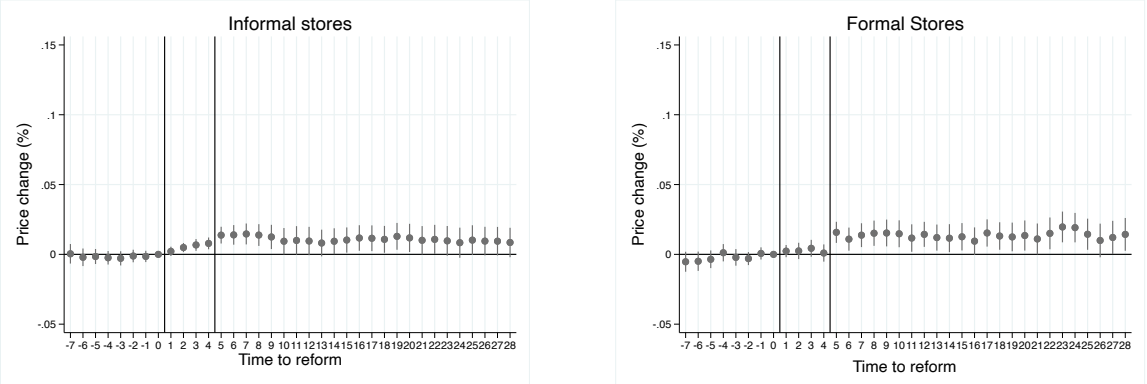
Only border states. Finally, we restrict our sample to only consider cities located in states that include border (treated) areas. This sample includes states that entirely belong to border regions (i.e. Baja California, Baja California Sur and Quintana Roo), as well as those whose territory is only partially considered a border area (see Table B.1 for the sample

Figure 2.3: Monthly Effects of the VAT Increase on Prices (2013-2015)

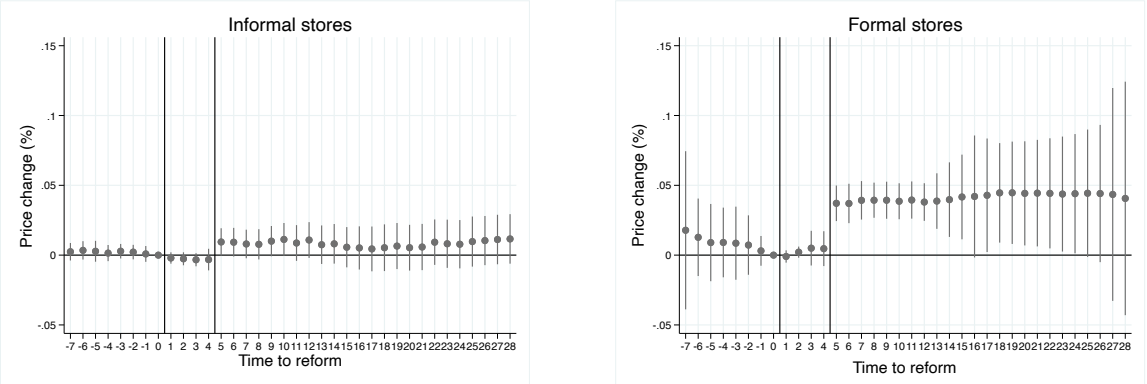
(a) Core products



(b) Merchandise



(c) Services



Notes: This figure shows the monthly impact of increasing the VAT rate on prices in border areas. The reported estimates, represented by dots, correspond to interaction terms between month-year indicators and the *Border* variable. The 95% confidence intervals are reported as lines. Product weights are included in all specifications, as well as month and unique product fixed effects. Standard errors are clustered at the city-generic product level. *Source:* Authors' own computations based on data from the Consumer Price Index, 2013-2015.

of cities in border states). By removing some cities from the sample, we compare regions that are geographically nearby, that is, areas that are more similar to one another. Our results for this exercise, however, show that estimates do not differ from those found using the main specification (see Table 2.4, Columns (5)-(6); Figure B.4).

Tax status by store type. Our assignment of stores to informal or formal status was based on the store type (traditional versus modern), rather than any information directly related to tax-status of each individual store (Section 2.4.1). This assignment has two advantages. First, it assigns treatment on a plausibly less endogenous variable - to the extent that stores can more easily manipulate whether they pay VAT than they can misreport or genuinely change their store type. Second, as discussed in Bachas et al. (2022), the store-type classification is strongly comparable both across countries and over time - which increases the external validity of the results in the paper for other settings.

At the same time, assigning treatment based on store-type rather than directly on tax status impacts the interpretation of the pass-through results. Specifically, if the mapping from store-type to tax-status is imperfect, then the pass-through estimates based on store-type will overestimate or underestimate the pass-through based on tax-status. To gauge the importance of this point, Table B.2 shows summary statistics for tax-status characteristics by store-type based on the 2014 Census. We see that only 9% of stores that are classified as informal levy any VAT on goods sold, reflecting a strong overlap between the traditional store-type and the absence of tax payments. However, only 45% of modern stores report in the Census levying VAT on consumer sales. Since we selected the Census to only include stores that sell taxed products, the low share of modern stores levying VAT is likely to be due to evasion or avoidance of the legally due VAT. Moreover, this statistic implies that the pass-through estimated in formal (modern) stores underestimates the pass-through that would be obtained based on tax-status.

Table B.2 also shows the share of informal and formal stores that purchase inputs from suppliers that levy VAT. This share is 16% in informal (traditional) stores and 51% in formal (modern) stores. Regardless of the (output) tax status of a store, this characteristic matters for the interpretation of pass-through. Per example, Bachas et al. (2022) show that, even if informal stores levy no VAT on their outputs, they may still have a positive pass-through of a

VAT increase to their consumers' final prices if they source inputs from VAT suppliers. This is because the informal stores may seek to pass on the input cost arising from the VAT rate increase to their consumers in the form of higher prices. In the model developed in Bachas et al. (2022), the authors show that the pass-through in this case is equal to the share of inputs where VAT is levied. Relative to our main results, the positive VAT input-share in informal stores could partly explain the positive pass-through that we estimate in these stores.

For stores surveyed in the Consumer Price Index, the share that levy any VAT is larger in both formal and informal establishments, relative to the retailers present in the 2014 Census. This discrepancy is due to the CPI sample being reduced to only include information on products that are taxed and part of the core inflation estimation. Differences between sectors, however, are maintained, with the share of formal stores levying VAT reaching 90%, compared to only 42% for informal firms. Such a large proportion of formal stores levying VAT suggests a lower level of evasion or avoidance from firms compared to the larger Census sample. For informal firms, however, the presence of considerable amount of establishments levying VAT on consumer sales suggests that the estimates provided in the Section 2.6.1 represent an upper bound on informal pass-through. Finally, Table B.2 shows that in the CPI sample there is also a gap between sectors when comparing the proportion of establishments that purchase inputs from suppliers who levy VAT.

2.7 Heterogeneous Effects

North and south border cities. Border areas are geographically divided into two distinctive categories: north and south. The southern area of Mexico is considerably less developed than the rest of the country, including the northern region, which has a GDP slightly above the national average (Banco de México, 2019). In our sample, only 2 out of the 8 treated cities are located in the south border areas: Chetumal and Tapachula. These two cities represent 21% of the observations for border regions and only 3% of the full sample.

Table B.3 and Figures B.5-B.6 show that overall informal pass-through is larger for firms in the south, while the documented formal pass-through is slightly larger in the north border due to the increase in service prices, equal to 4.2 percentage points. As stated in Bachas et al.

Table 2.4: Difference-in-Differences Analysis of the VAT Increase on Prices, Robustness Checks (2013-2015)

	Informal Log(Price) (1)	Formal Log(Price) (2)	Informal Log(Price) (3)	Formal Log(Price) (4)	Informal Log(Price) (5)	Formal Log(Price) (6)
Core products						
Border x Post	0.009**	0.018***	0.007***	0.013***	0.010*	0.014*
	(0.004)	(0.007)	(0.002)	(0.002)	(0.006)	(0.008)
Observations	625,176	959,724	625,176	959,724	200,412	304,812
R-squared	0.227	0.016	0.089	0.035	0.152	0.027
Merchandise						
Border x Post	0.008*	0.014***	0.006**	0.013***	0.008	0.012**
	(0.005)	(0.004)	(0.003)	(0.002)	(0.007)	(0.005)
Observations	399,744	946,008	399,744	946,008	128,376	300,780
R-squared	0.187	0.060	0.078	0.035	0.156	0.058
Services						
Border x Post	0.009*	0.035**	0.008**	0.028**	0.012	0.036*
	(0.006)	(0.014)	(0.004)	(0.013)	(0.008)	(0.019)
Observations	225,432	13,716	225,432	13,716	72,036	4,032
R-squared	0.256	0.230	0.114	0.073	0.151	0.205
Winsorized	Yes	Yes	No	No	No	No
Weights	Yes	Yes	No	No	Yes	Yes
Sample	All	All	All	All	Border States	Border States

Notes: This table compares retail prices in *border* and *non-border* areas around the January 2014 VAT equalization. Each column shows estimates for a separate difference-in-differences specification. Columns (1), (3) and (5) report results for the effect on prices of products sold in informal stores, while Columns (2), (4) and (6) show estimates for products sold in formal stores. Columns (1)-(2) present winsorized and weighted results for all the cities in the sample. Columns (3)-(4) report unweighted estimates. Columns (5)-(6) present results excluding states that do not have any border areas. Month and unique product fixed effects are included in all specifications. Standard errors are clustered at the city-generic product level. *** significant at 1%, ** significant at 5%, * significant at 10%.
Source: Authors' own computations based on data from the Consumer Price Index, 2013-2015.

(2022), informal consumption patterns can contribute to the progressivity of the VAT. Despite finding a lower pass-through for the informal sector relative to the formal one in both areas, the fact that informal store prices increased more in the south suggests that the progressivity of the VAT is more affected in the less-developed region. However, further information on household expenditure is needed to justify this claim.

Store types. The CPI classifies stores according to 8 categories that we group into formal and informal establishments. In Table B.4 and Figure B.7, we report regression estimates according to 4 store types. Public markets and street stalls represent less than 1% of our sample. Unlike other store types, this sector experienced a large and statistically significant decline in prices in border areas after the reform. In the case of services, the percentage-point reduction in prices is equal to the size of the VAT rate increase. Other informal establishments such as specialized, convenience and grocery stores report an overall increase in prices as a result of the reform, though the estimate is smaller than those found for formal stores. Within formal stores, we find that prices increased more in online and department stores than in supermarkets and price clubs. For the latter category, there is a pass-through equivalent to 320% in services, which is driven by changes in food and phone services. In the department stores category we also find that the services pass-through (70%) is larger than that of merchandise goods (18%), but considerably smaller than that of supermarkets.

2.8 Effects on firms

In this section, we consider the incidence of the VAT increase on firm-level outcomes reported in the Economic Census and which were described in Section 2.4. We begin by examining whether the reform affected the exit and entry of firms in the retail sector. We then investigate whether the reform affected firm profits, labor costs and intermediate input costs; the rich data allows us to study if there are differential effects on these outcomes by store-type. For all of these economic outcomes, we use the following specification:

$$X_{slt} = \alpha_1 Border_l \times Year2019_t + \alpha_2 Year2019_t + \delta_l + \epsilon_{slt}, \tag{2.3}$$

where X_{slt} is an outcome variable for firm s, in municipality l and year t. The variable $Border_l$ is equal to 1 if the store is located in a treated municipality, while $Year2019_t$ takes value 1 if the observation corresponds to 2019 and 0 if it belongs to 2014. Municipality fixed effects are denoted by δ_l. The error term is represented by ϵ_{slt} and clustered at the municipality level.

Impact of the reform on firm presence Table 2.5 presents results of estimating specification (3) using an indicator for whether a firm is operating (present in the data) in year t. If a store is present during a given year in the sample, the indicator takes value 1; the indicator takes a value of 0 otherwise. The mean of this indicator variable is equal to 0.70 for informal stores and 0.72 for formal establishments. To maintain a parallel with the estimation sample for the price results, we restrict our sample to stores that sell taxed products. For firms that are present in both years, we exclude stores that change store type or formality status over time. This happens infrequently, but ensures that there is no composition change over time when we study effects separately by store-type.

We find that the reform did not have an effect on firm presence. Column (1) in Table 2.5 shows estimates for the entire sample of firms, while Columns (2) and (3) present results for informal and formal firms, respectively. In all cases, there was a small and non-significant reduction in the number of firms in border areas after the reform. While we acknowledge that there could have been differential impacts on entry and exit of firms during this period, the focus of this exercise is to examine the overall behavior of the market, which does not show a significant change in terms of firm presence.

Impact of the reform on other economic outcomes The null results in Table 2.5 suggest that the reform-induced tax burden did not have impacts on the extensive margin; instead, the tax burden is likely to have been shared on the intensive margin amongst firms, workers and consumers. We turn to this investigation in Table 2.6.

Given the absence of any impact on entry-exit in Table 2.5, we restrict the sample in Table 2.6 to those firms that are present in both rounds of the Census. Using the specification in equation (2.3) we start by looking at the effect of the reform on firms' total sales (net of taxes)

Table 2.5: Difference-in-Differences Analysis of the VAT Increase on Firm Presence (2014-2019)

	Presence All firms (1)	Presence Informal firms (2)	Presence Formal firms (3)
Border x Year 2019	-0.010	-0.011	-0.008
	(0.008)	(0.008)	(0.017)
Year 2019	0.090***	0.088***	0.134***
	(0.004)	(0.004)	(0.005)
Fixed effects	Muncipality	Muncipality	Muncipality
R-squared	0.012	0.012	0.029
Observations	7,844,170	7,403,716	440,454

Notes: This table presents difference-in-differences estimations of the effect of the VAT on firm presence, where presence is an indicator for the firm being present in the Economic Census. Column (1) shows results for all firms present in either 2014 or 2019. Column (2) shows results for informal firms, while Column (3) presents estimates for formal stores. All regressions include municipality fixed effects. Clustered errors at the municipality level are reported in parentheses. *denotes significance at the 10%, ** at the 5% and *** at the 1% levels.
Source: Authors' own computations based on data from the Economic Census, 2014-2019.

to see if formal and informal retail market shares were affected by the reform. We then consider effects of the reforms on firms' employees (both their total number and their total remuneration), on their total intermediate input costs, then on two proxies for firms' returns to capital. All variables are in logs.

We see a statistically significant increase in sales for informal firms but no significant change for formal firms. The effects cannot statistically be distinguished across store-types, but the effect on formal firms is less than half that on informal firms. This is consistent with the idea that the change in prices observed in the previous section lead to informal firms gaining market shares. There is also a positive effect on the number of employees working in informal firms. As shown in Table 2.5, the number of firms in border areas was not modified as a result of the reform, therefore the increase in employment is due to the firms already in existence hiring more personnel. The effect on the total cost of employees in informal firms is slightly larger than that on the number of employees, albeit not statistically significant - this suggests that informal sector workers may have benefited from the reform, at least via stronger employment opportunities. Surprisingly, there is also a positive effect in formal firms. This effect is puzzling; we also see a 12

percent decrease in total labor costs, suggesting formal firms may have substituted lower-skilled employees for higher-skilled ones. We note that the employment effects in formal stores are marginally significant at the 10 percent level. Nonetheless, investigating these formal effects in more detail is an important task for future research.

Finally, we find positive and strongly significant effects of the reform on the total input costs and profits of informal firms. The magnitude of these effects is roughly in line with the effect on revenues, suggesting informal firms 'passed-through' the positive effect of the reform in roughly equal measure to their owners and their suppliers.

Investigation of pre-trends As in all difference-in-differences specifications, the causality of these results rests upon the assumption that, in absence of the reform, there would have been no differential trend in outcomes between border and non-border areas. Given that the economic outcomes are collected in the Census but only every 5 years, our ability to gauge the validity of this assumption is limited. We consider results from three separate exercises to make progress on this concern. First, recall that we found strongly similar trends in monthly prices between border and non-border areas in the year preceding the reform, both for formal stores and for informal stores. Insofar as prices reflect (in part) market conditions, these results suggest that stores in border and non-border areas faced similar changes in the market environment – thus making it more likely that changes in revenue, profits and employment would also be common across border and non-border areas in the year preceding the reform.

Our second exercise is to estimate the same regression as for Table 2.6, but between two periods prior to the reform. Indeed, the Census was also conducted in 2009 such that we can investigate if there are long-run differential changes between 2014 and 2009 in border versus non border areas. In Table B.5, we perform this pre-trend analysis on entry-exit. Similar to the main Table 2.5, we find no differential pattern in entry-exit between border and non-border areas, both for formal and for informal stores. In other words, the parallel trends assumption may hold on the extensive margin. In Table B.6, we investigate pre-trends for the economic outcomes in Table 2.6. For these outcomes, Table B.6 shows that the relative change in border areas, when compared to non-border areas is negative and, for the most part, statistically significant. This negative pre-trend for outcomes on the intensive margin holds for both formal and informal

firms, suggesting the pre-trend is common to all firms in border areas. Given that these data are only conducted every 5 years, we have limited ability to further investigate why there appears to be a pre-trend in the Census data when there were no pre-trends in the price data. With this caveat in mind, we note that, if anything, the pre-trend would lead us to understate the positive effects of the VAT reform on outcomes in informal stores.

To further probe the difference in pre-trends between the price data and the Census data, we leverage National Survey of Occupation and Employment (ENOE) data in our third exercise. Similarly to the Census, the ENOE data contains nationally representative data on economic outcomes; unlike the Census, ENOE usefully contains data on a quarterly basis. In the ENOE, the raw data is available at the individual level, but the surveyed individuals are only present in the panel for a limit number of quarters. We therefore aggregate individual responses to the municipality level. This yields a total of 2,542 municipalities for which we observe outcomes in 30 consecutive quarters between 2008 and 2015. Out of the 2,542 municipalities present in the sample, 137 correspond to treated (border) areas. We focus on three economic outcomes: number of workers in place of work; weekly hours worked; and, monthly income. While the ENOE data does not measure these outcomes separately by the formality-status of the store, it does measure formal and informal outcomes by the status of the respondent's main job. We can therefore separately study these three outcomes for formal workers and informal workers.

To gauge pre-trends, we estimate the dynamic event-study regression which is analogous to equation (2.2), but at the quarter-municipality level. We include quarter and municipality fixed effects, and cluster standard errors at the municipality level. The reference period is the third quarter of 2013. The results are shown in Figure B.8. We find no differential pre-trend between border and non-border areas over a period of 5 years prior to the VAT reform. This holds for the three different outcomes and both in the sample of formal and informal respondents. The absence of pre-trends over a long range of years for economic outcomes that should be closely related to the firm outcomes in the Census further raise the intrigue over the pre-trends in Table B.6.

When taken together, these results lead us to conclude there is suggestive but not decisive evidence that the VAT reform positively impacted economic outcomes in informal stores. Results

Table 2.6: Difference-in-Differences Analysis of the VAT Increase on Firm Outcomes (2014-2019)

		Informal firms (1)	Formal firms (2)
Revenue	Estimated Coefficient	0.050**	0.021
	S.E.	(0.025)	(0.022)
	N	2,975,240	201,180
Cost of employees	Estimated Coefficient	0.028	-0.124*
	S.E.	(0.027)	(0.063)
	N	2,975,240	201,180
Number of employees	Estimated Coefficient	0.013**	0.083*
	S.E.	(0.006)	(0.048)
	N	2,975,240	201,180
Cost of materials	Estimated Coefficient	0.067***	-0.001
	S.E.	(0.026)	(0.025)
	N	2,975,240	201,180
Before-tax profits	Estimated Coefficient	0.081***	-0.042
	S.E.	(0.027)	(0.085)
	N	2,975,240	201,180
Return on assets	Estimated Coefficient	0.065**	0.058
	S.E.	(0.025)	(0.040)
	N	2,739,783	196,154

Notes: This table presents difference-in-differences estimations of the effect of the VAT on firm outcomes. The figures reported correspond to the estimated coefficient and standard errors for the interaction of variables $Border_l$ and $Year2019_t$. All outcome variables are expressed in logs. Column (1) shows results for informal firms, while Column (2) presents results for formal firms. All regressions include municipality fixed effects. Clustered standard errors at the municipality level are reported in parentheses. *denotes significance at the 10%, ** at the 5% and *** at the 1% levels.
Source: Authors' own computations based on data from the Economic Census, 2014-2019.

from several data-sets indicate that border and non-border areas are on parallel trends in multiple years prior to the reform; moreover, the sign of the pre-trend estimated in the Census data suggests that, if anything, the positive impacts we uncover in the informal stores are under-estimated.

2.9 Conclusion

This study sheds light on the pass-through of the VAT to consumers in a developing country. The results presented are comparable to the estimates obtained in the literature that examines VAT changes in Europe. However, our analysis additionally takes into account informal markets, which represent a large sector of the economy in developing countries.

While formal markets show a significant pass-through of 38%, the estimate for informal retailers is much lower and equal to 18%. These estimates are robust to a set of alternative specifications, including winsorizing outliers, removing product weights and changing the control sample.

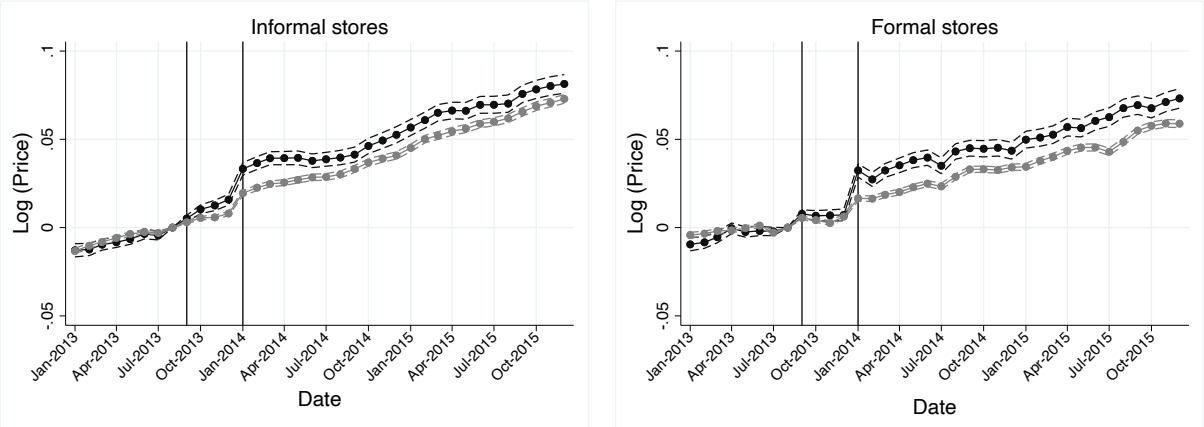
Furthermore, the results presented provide evidence in favor of a progressive component of the Mexican VAT. Since lower income tax groups are more reliant on unregulated markets than the rich, this sector can shelter the poor from adverse effects of fiscal measures. In order to properly assess the net effect of the rate change on consumers by income group, a broader scope is needed to account for the effect of the newly introduced distortions, the allocation of increased government revenue and the changes in utility of all the individuals involved. As pointed out by Ahmad, Best and Pöschl (2012), it is also important to consider the interactions between different taxes and potential evasion sources when applying changes to the VAT. Future exploration will focus on these topics.

In conclusion, the results so far are suggestive of strong differences in pricing behavior between formal and informal markets. However, many additional avenues of research remain unexplored on this particular subject. Further investigation should entail extending the number of products included in the sample to verify whether VAT modifications impact non-taxed goods. Additionally, it would be useful to expand the time frame of the analysis to measure the long-term effect of the VAT equalization and to explore how the pass-through evolves differentially over time. Finally, measures of formal and informal competition should be added to the regressions, to account for potential effects of the market structure on pricing decisions. We intend to address all of these approaches in future research.

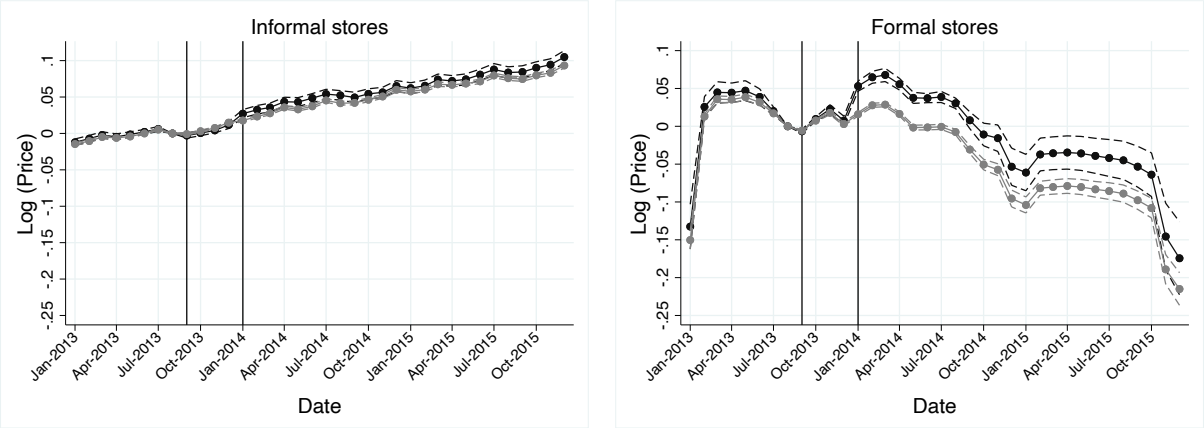
Appendix B: Chapter 2

Figure B.1: Average Log Prices, by Sector and Product Types (2013-2015)

(a) Merchandise



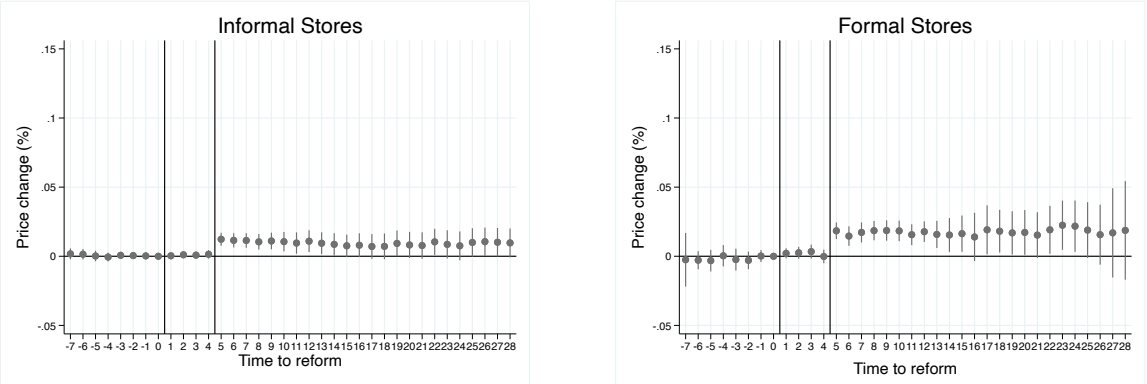
(b) Services



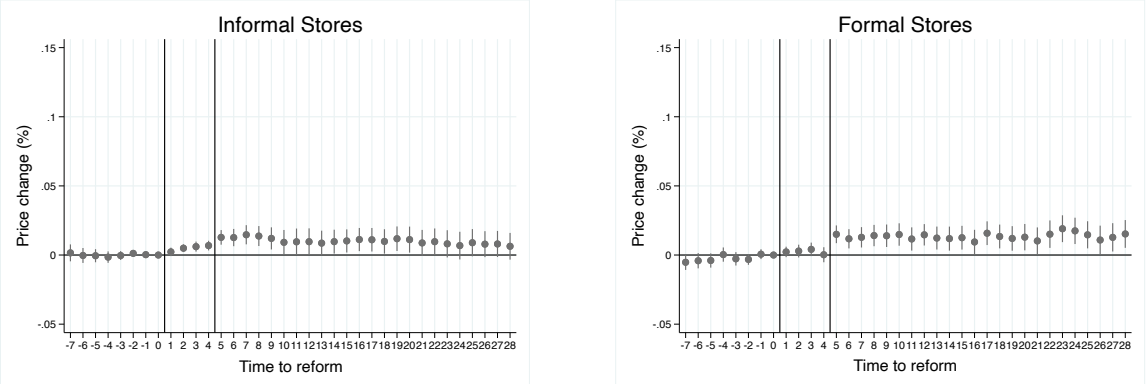
Notes: The figure shows the weighted average price evolution by area and sector, before and after the VAT rate increase of 5 percentage points in border regions during January 2014. The upper panel reports averages for merchandise goods; the lower panel reports means for services. The panels on the left show the evolution for products sold in informal stores, while the panels on the right show the time series for goods sold in formal retailers. Averages for border areas are shown in black, mean prices for non-border areas are displayed in gray. Log prices are normalized to 0 for August 2013. 95% confidence intervals for the mean in each period are presented in dashed lines.
Source: Authors' own computations based on data from the Economic Census, 2013-2015.

Figure B.2: Monthly Effects of the VAT Increase on Winsorized Prices (2013-2015)

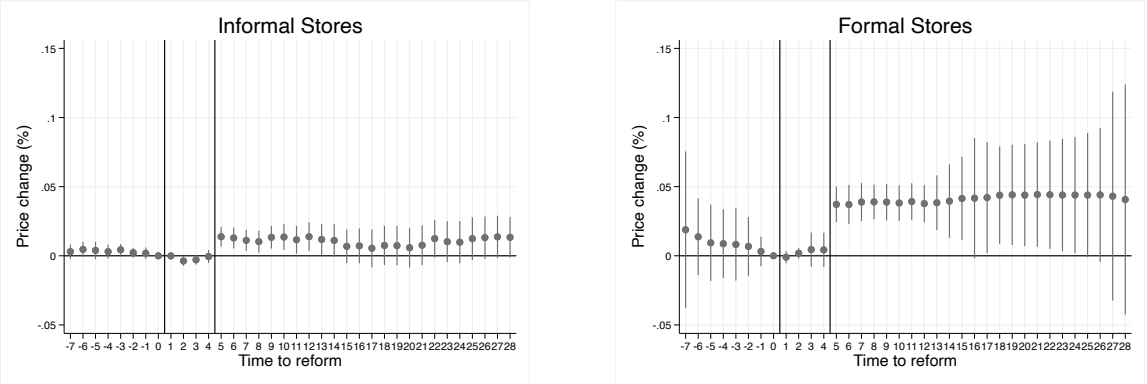
(a) Core products



(b) Merchandise



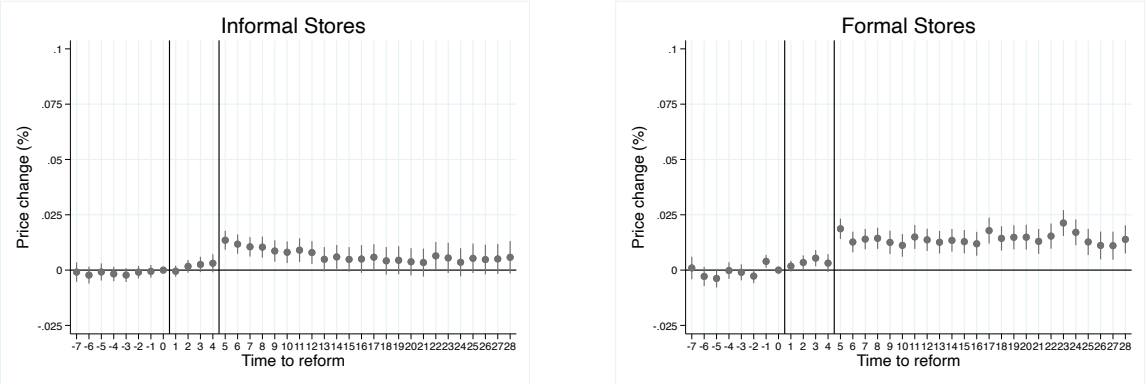
(c) Services



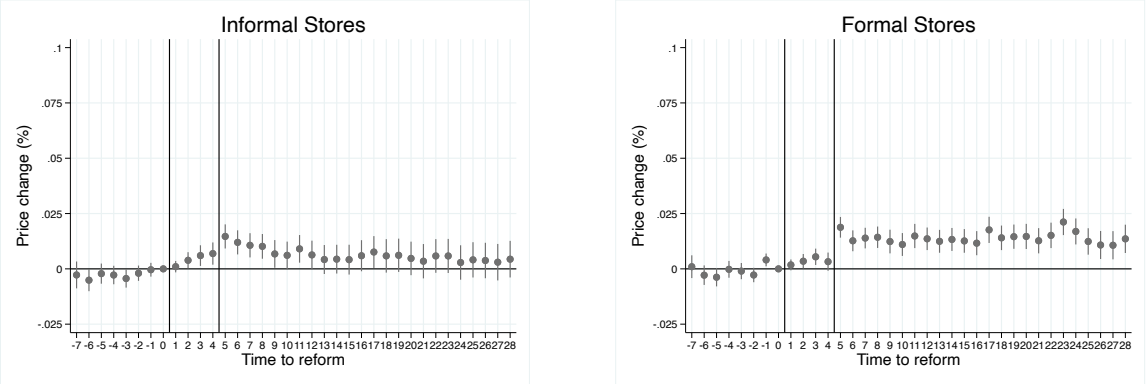
Notes: This figure shows the monthly impact of increasing the VAT rate on prices in border areas. Prices are winsorized for the top and bottom 5% of the distribution by border and formality status for every month in the sample. The reported estimates, represented by dots, correspond to interaction terms between month-year indicators and the *Border* variable. The 95% confidence intervals are reported as lines. Product weights are included in all specifications, as well as month and unique product fixed effects. Standard errors are clustered at the city-generic product level.
Source: Authors' own computations based on data from the Economic Census, 2013-2015.

Figure B.3: Unweighted Monthly Effects of the VAT Increase on Prices (2013-2015)

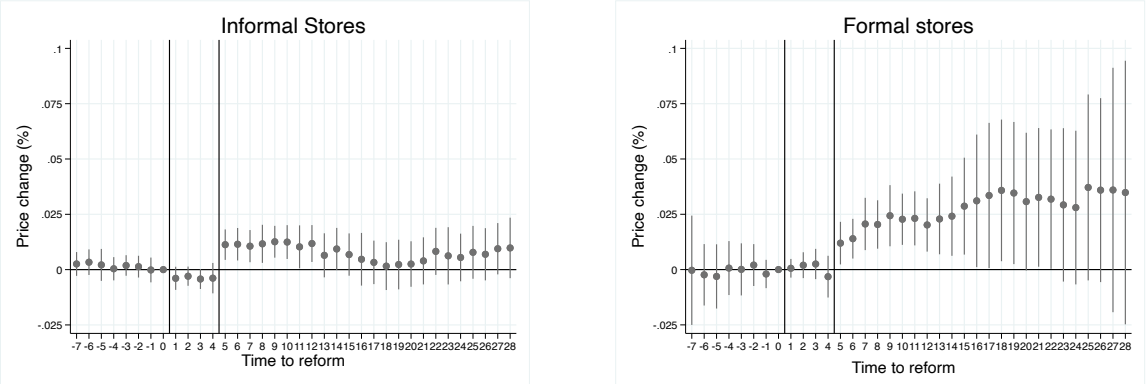
(a) Core products



(b) Merchandise



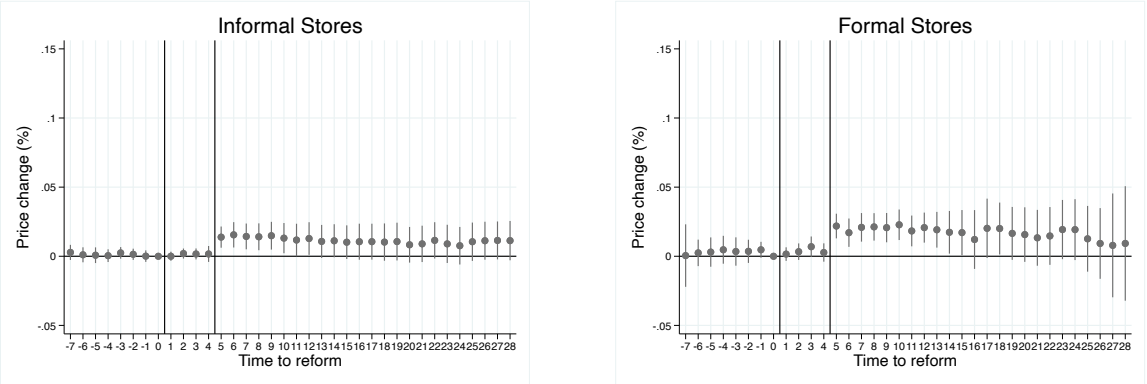
(c) Services



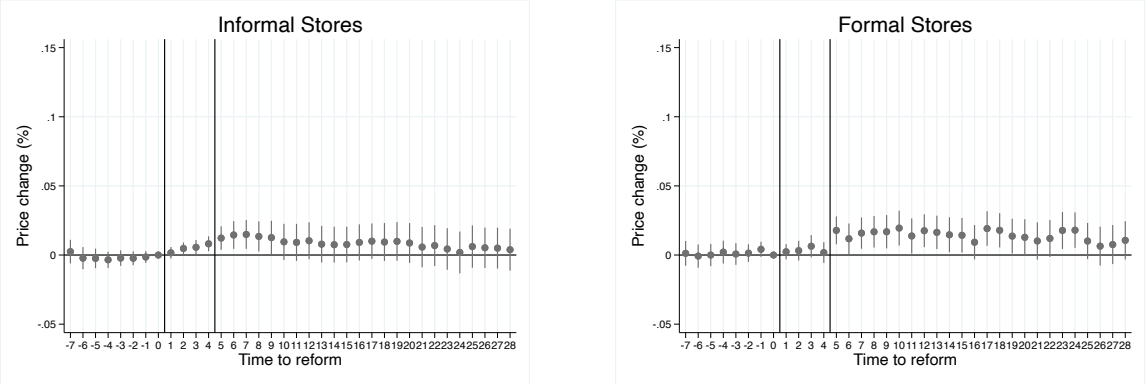
Notes: This figure shows the monthly impact of increasing the VAT rate on prices in border areas. The reported estimates, represented by dots, correspond to interaction terms between month-year indicators and the *Border* variable. The 95% confidence intervals are reported as lines. Month and unique product fixed effects are included in all specifications. Standard errors are clustered at the city-generic product level.
Source: Authors' own computations based on data from the Economic Census, 2013-2015.

Figure B.4: Monthly Effects of the VAT Increase on Prices, Border States Only (2013-2015)

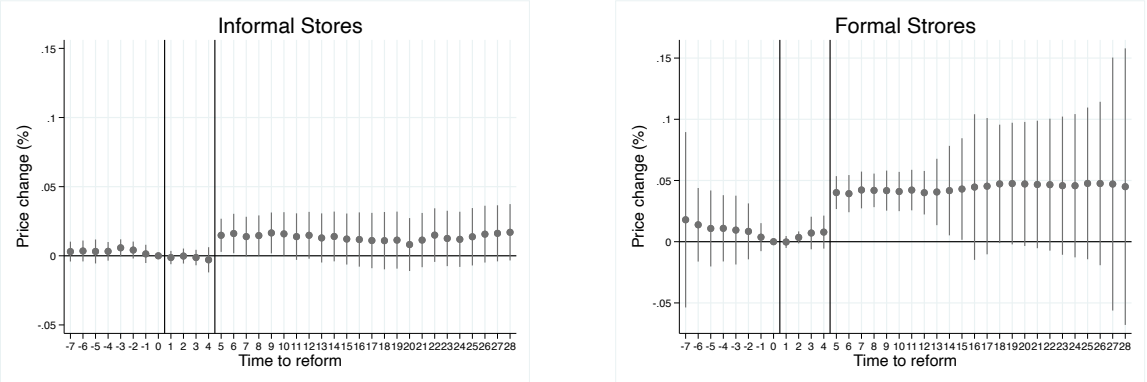
(a) Core products



(b) Merchandise



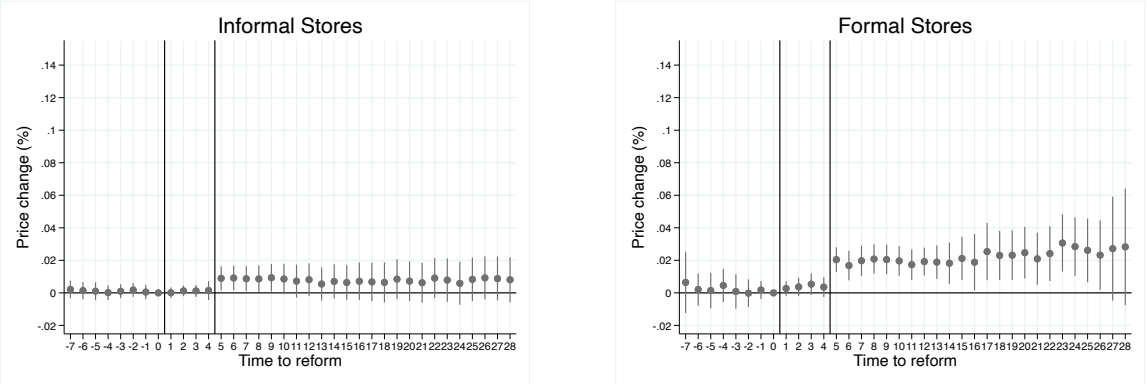
(c) Services



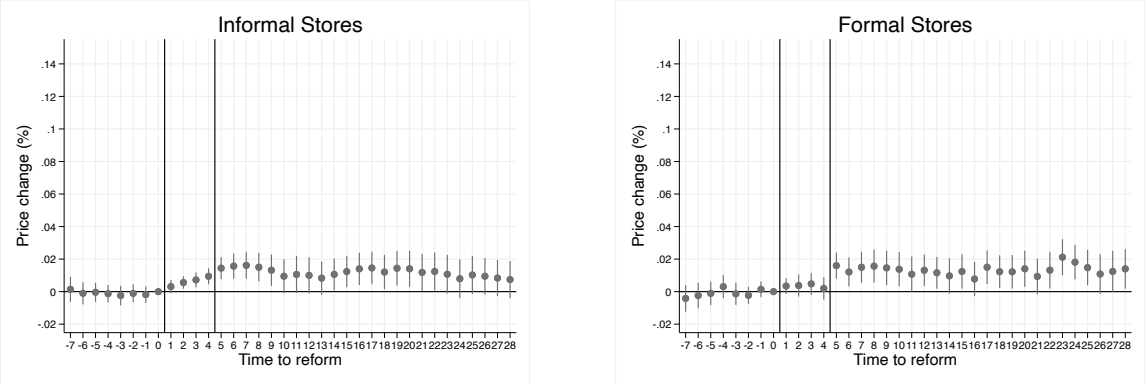
Notes: This figure shows the monthly impact of increasing the VAT rate on prices in border areas. We exclude from analysis states that do not have any border regions. The reported estimates, represented by dots, correspond to interaction terms between month-year indicators and the *Border* variable. The 95% confidence intervals are reported as lines. Product weights are included in all specifications, as well as month and unique product fixed effects. Standard errors are clustered at the city-generic product level.
Source: Authors' own computations based on data from the Economic Census, 2013-2015.

Figure B.5: Monthly Effects of the VAT Increase on Prices in the North Border (2013-2015)

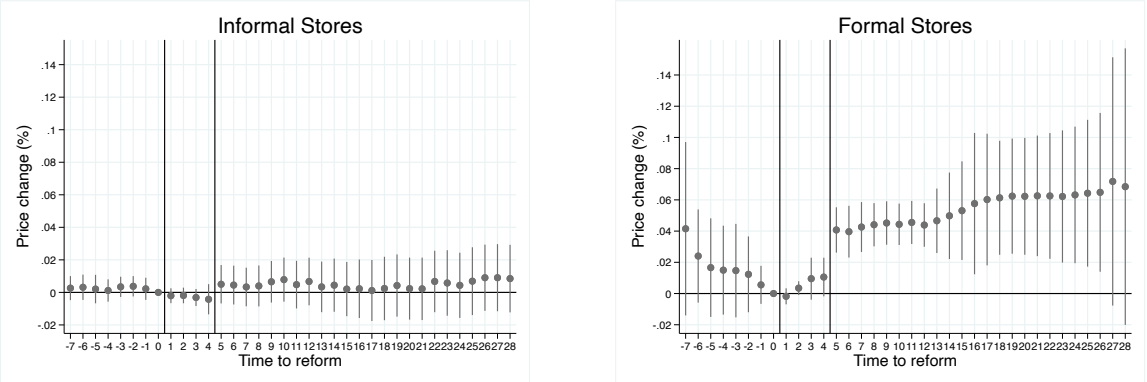
(a) Core products



(b) Merchandise



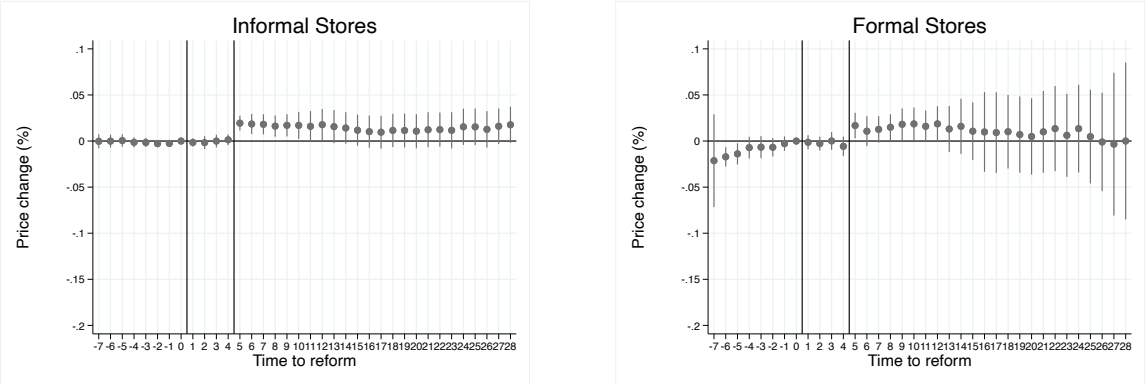
(c) Services



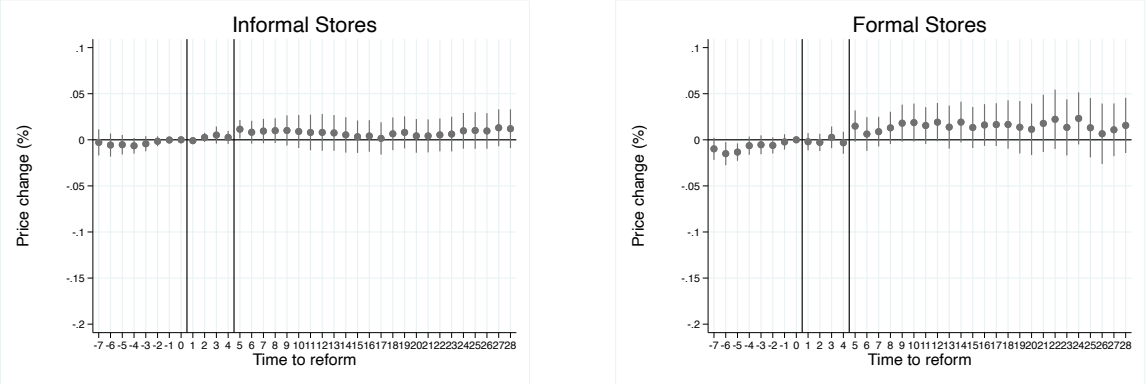
Notes: This figure shows the monthly impact of increasing the VAT rate on prices in northern border areas. The reported estimates, represented by dots, correspond to interaction terms between month-year indicators and the *Border* variable. The 95% confidence intervals are reported as lines. Product weights are included in all specifications, as well as month and unique product fixed effects. Standard errors are clustered at the city-generic product level.
Source: Authors' own computations based on data from the Economic Census, 2013-2015.

Figure B.6: Monthly Effects of the VAT Increase on Prices in the South Border (2013-2015)

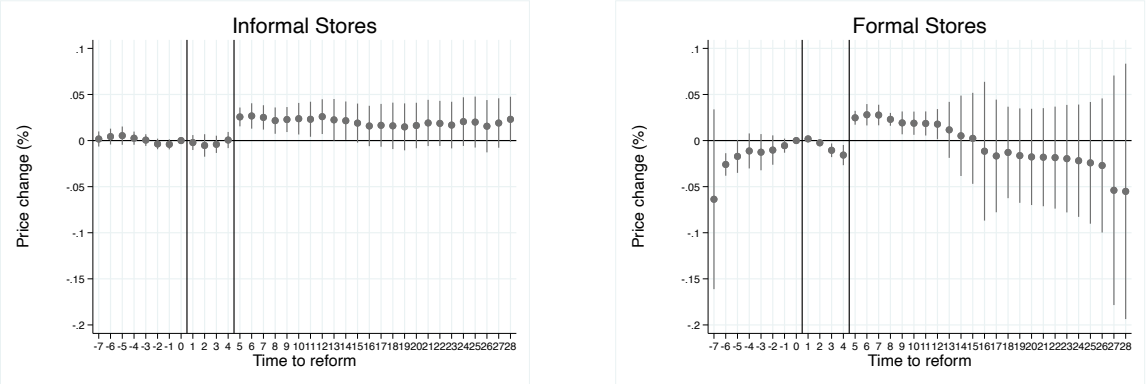
(a) Core products



(b) Merchandise



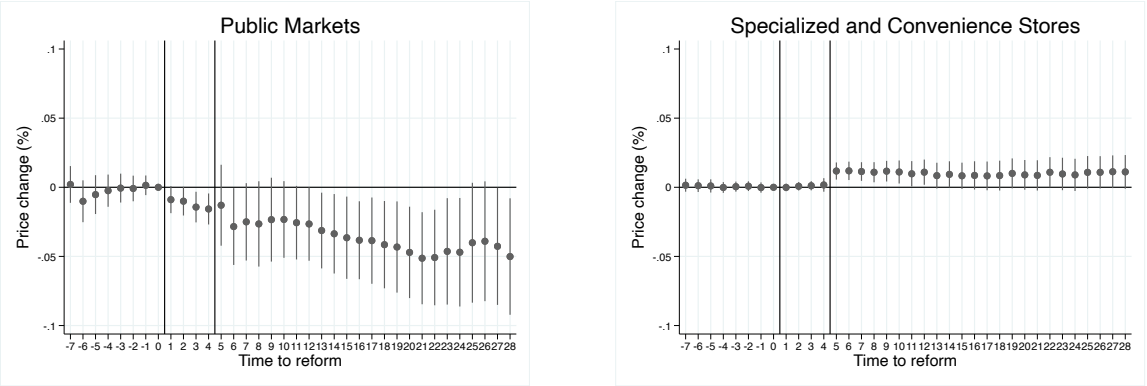
(c) Services



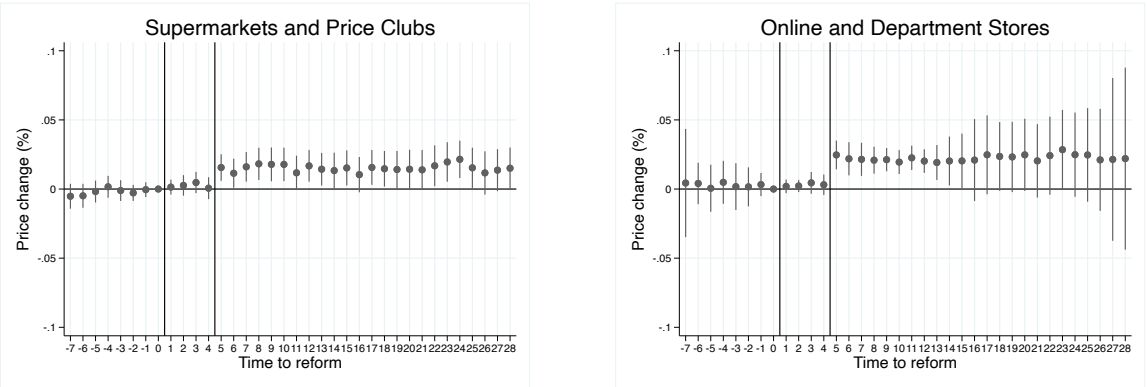
Notes: This figure shows the monthly impact of increasing the VAT rate on prices in southern border areas. The reported estimates, represented by dots, correspond to interaction terms between month-year indicators and the *Border* variable. The 95% confidence intervals are reported as lines. Product weights are included in all specifications, as well as month and unique product fixed effects. Standard errors are clustered at the city-generic product level.
Source: Authors' own computations based on data from the Economic Census, 2013-2015.

Figure B.7: Monthly Effects of the VAT Increase on Prices in the Border, by Store Type (2013-2015)

(a) Informal Stores



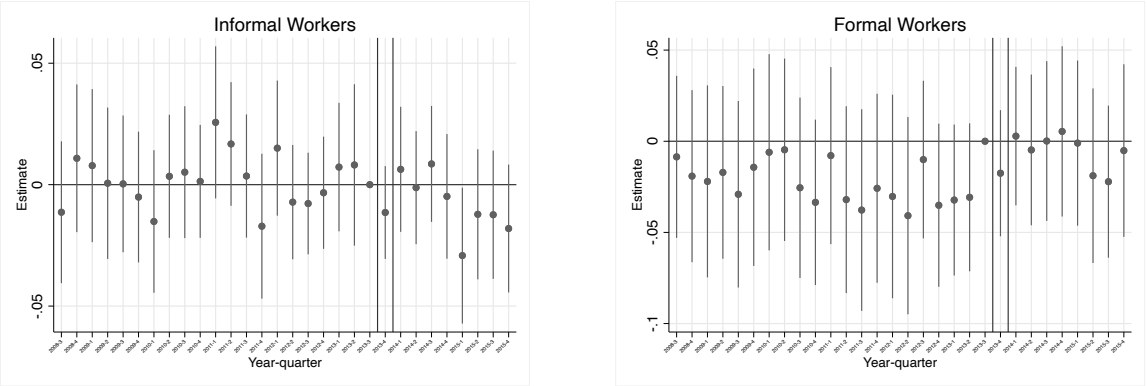
(b) Formal Stores



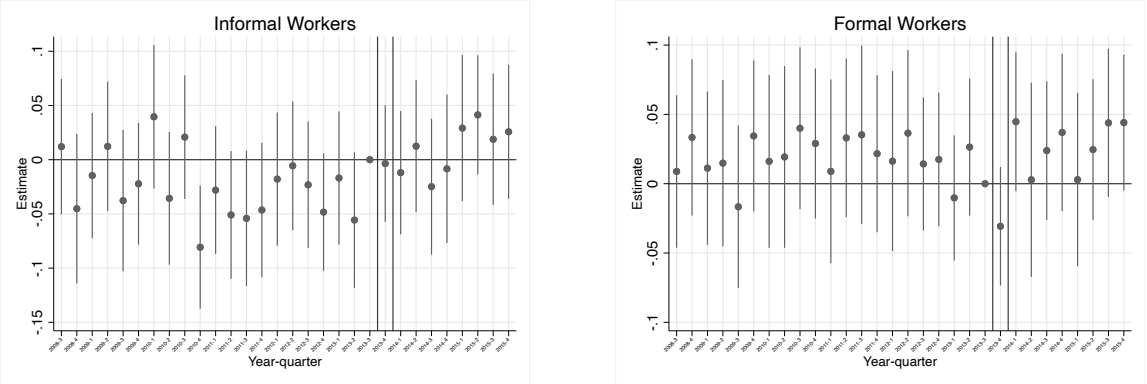
Notes: This figure shows the monthly impact of increasing the VAT rate on prices in border areas by store types. The sample includes merchandise goods and services. The reported estimates, represented by dots, correspond to interaction terms between month-year indicators and the *Border* variable. The 95% confidence intervals are reported as lines. Product weights are included in all specifications, as well as month and unique product fixed effects. Standard errors are clustered at the city-generic product level.
Source: Authors' own computations based on data from the Economic Census, 2013-2015.

Figure B.8: Quarterly Effects of the VAT Increase on Labour Outcomes in the Border, by Type of Employment (2008-2015)

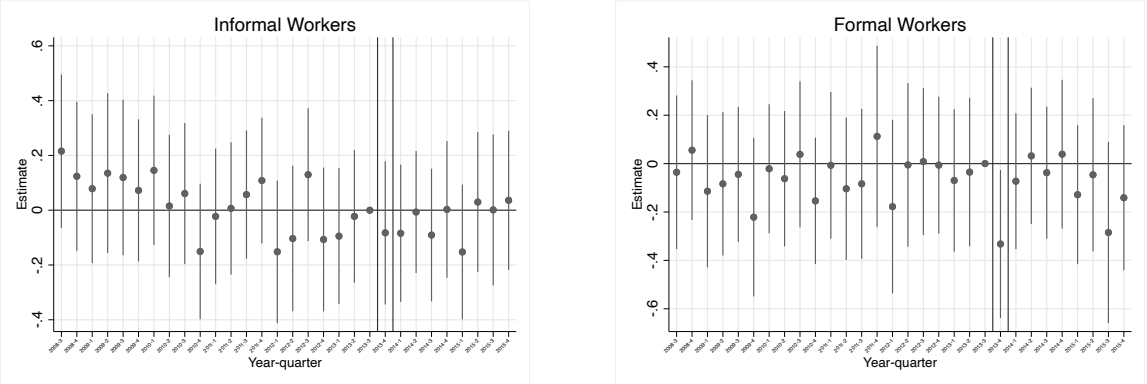
(a) Number of Workers



(b) Hours Worked per Week



(c) Monthly Income



Notes: This figure shows the quarterly impact of increasing the VAT rate on labour outcomes in border areas by type of worker. Each panel shows results for a different dependent variable: number of workers in the place of work (panel (a)), number of hours worked per week (panel (b)) and monthly income (panel (c)). All outcomes are presented in logs. The reported estimates, represented by dots, correspond to interaction terms between year-quarter indicators and the *Border* variable. The 95% confidence intervals are reported as lines. Municipality and year-quarter fixed effects are included in all specifications. Errors are clustered at the municipality level.
Source: Authors' own computations based on data from the National Survey of Occupation and Employment (ENOE), 2008-2015.

Table B.1: Cities in Sample, by Treatment Group

Border cities	**Non-border cities**			
Cd. Acuña, Coah.	Acapulco, Gro.	Durango, Dgo.	**Monclova, Coah.**	Tepatitlán, Jal.
Cd. Juárez, Chih.	Aguascalientes, Ags.	Fresnillo, Zac.	**Monterrey, N.L.**	Tepic, Nay.
Chetumal, Q. Roo.	Metropolitan Area of Mexico City	Guadalajara, Jal.	Morelia, Mich.	Tlaxcala, Tlax.
La Paz, B.C.S.	**Campeche, Camp.**	**Hermosillo, Son.**	Oaxaca, Oax.	Toluca, Edo. de Méx.
Matamoros, Tamps.	**Chihuahua, Chih.**	**Huatabampo, Son.**	Puebla, Pue.	**Torreón, Coah.**
Mexicali, B.C.	Colima, Col.	Iguala, Gro.	Querétaro, Qro.	Tulancingo, Hgo.
Tapachula, Chis.	Córdoba, Ver.	Jacona, Mich.	San Andrés Tuxtla, Ver.	Veracruz, Ver.
Tijuana, B.C.	Cortazar, Gto.	**Jiménez, Chih.**	San Luis Potosí, S.L.P.	Villahermosa, Tab.
	Cuernavaca, Mor.	León, Gto.	**Tampico, Tamps.**	
	Culiacán, Sin.	Mérida, Yuc.	Tehuantepec, Oax.	

Notes: This table presents the name of the cities in the sample, by treatment status. The name of the states where the cities are located appear abbreviated. Border cities are considered as treated, while Non-border cities are included in the control group. Non-border cities in border states are shown in bold.
Source: Authors' own classification based on data from the Consumer Price Index, 2013-2015.

Table B.2: Summary Characteristics Based on Economic Census and Consumer Price Index (2013-2015)

Year	Variable	Informal firms		Formal firms	
		N	Mean	N	Mean
Panel A: Consumer Price Index merged with Economic Census 2014					
2013-2015	Firms with VAT on inputs (%)	5,438	48.049	654	92.173
	VAT on inputs over cost of materials (%)	5,217	37.710	650	15.185
	Firms that charged VAT to consumers (%)	5,438	41.827	654	89.778
	VAT charged over revenue before taxes (%)	5,378	4.627	649	11.497
	VAT charged over revenue after taxes (%)	5,377	5.301	649	13.206
Panel B: Economic Census					
2014	Firms with VAT on inputs (%)	1,487,620	16.145	100,590	50.905
	VAT on inputs over cost of materials (%)	1,464,046	3.926	99,174	16.891
	Firms that charged VAT to consumers (%)	1,487,620	9.913	100,590	44.529
	VAT charged over revenue before taxes (%)	1,475,453	0.934	98,957	5.208
	VAT charged over revenue after taxes (%)	1,475,448	1.062	98,955	5.974

Notes: This table shows summary statistics for variables with information on VAT payments. Panel A shows weighted statistics for the Consumer Price Index database (2013-2015) merged with the 2014 Economic Census. Panel B presents statistics for the Economic Census in 2014.
Source: Authors' own computations based on data from the Economic Census, 2013-2015.

Table B.3: Difference-in-Differences Analysis of the VAT Increase on Prices, North and South (2013-2015)

	North Border		South Border	
	Informal Log(Price)	Formal Log(Price)	Informal Log(Price)	Formal Log(Price)
	(1)	(2)	(3)	(4)
Core products				
Border x Post	0.007	0.020***	0.015*	0.018
	(0.005)	(0.007)	(0.008)	(0.016)
Observations	601,200	931,608	545,292	847,728
R-squared	0.144	0.014	0.146	0.011
Merchandise				
Border x Post	0.010**	0.013***	0.009	0.020*
	(0.005)	(0.004)	(0.010)	(0.011)
Observations	383,184	918,216	347,400	835,668
R-squared	0.126	0.042	0.123	0.040
Services				
Border x Post	0.004	0.042***	0.021**	0.010
	(0.008)	(0.016)	(0.010)	(0.021)
Observations	218,016	13,392	197,892	12,060
R-squared	0.158	0.227	0.164	0.233

Notes: This table compares retail prices in *border* and *non-border* areas around the January 2014 VAT equalization. Each column shows estimates for a separate difference-in-differences specification. Columns (1) and (3) report results for the effect on prices of products sold in informal stores, while Columns (2) and (4) show estimates for products sold in formal stores. Columns (1)-(2) present results when only border cities in the north border are included in the treatment group. Columns (3)-(4) report estimates using border cities in the south as the treatment group. Product weights are included in all specifications, as well as month and unique product fixed effects. Standard errors are clustered at the city-generic product level. *** significant at 1%, ** significant at 5%, * significant at 10%.
Source: Authors' own computations based on data from the Consumer Price Index, 2013-2015.

Table B.4: Difference-in-Differences Analysis of the VAT Increase on Prices, by Store Type (2013-2015)

	Public Markets	Specialized and Convenience Stores	Supermarkets and Price Clubs	Online and Department Stores
	Log(Price) (1)	Log(Price) (2)	Log(Price) (3)	Log(Price) (4)
Core products				
Border x Post	-0.031**	0.009**	0.016***	0.020*
	(0.015)	(0.005)	(0.005)	(0.011)
Observations	10,080	615,096	513,900	445,824
R-squared	0.350	0.144	0.055	0.030
Merchandise				
Border x Post	-0.024	0.011**	0.015***	0.009**
	(0.018)	(0.005)	(0.005)	(0.004)
Observations	9,396	390,348	513,576	432,432
R-squared	0.342	0.122	0.055	0.021
Services				
Border x Post	-0.050***	0.008	0.160***	0.035**
	(0.016)	(0.007)	(0.038)	(0.014)
Observations	684	224,748	324	13,392
R-squared	0.392	0.160	0.245	0.237

Notes: This table compares retail prices in *border* and *non-border* areas around the January 2014 VAT equalization. Each column shows estimates for a separate difference-in-differences specification. Each column reports results for products sold in a particular store type: public markets (Column (1)); specialized and convenience stores (Column (2)); supermarkets and price clubs (Column (3)); and online purchases and department stores (Column (4)). Product weights are included in all specifications, as well as month and unique product fixed effects. Standard errors are clustered at the city-generic product level. *** significant at 1%, ** significant at 5%, * significant at 10%.
Source: Authors' own computations based on data from the Consumer Price Index, 2013-2015.

Table B.5: Difference-in-Differences Analysis of the VAT Increase on Firm Presence (2009-2014)

	Presence All firms (1)	Presence Informal firms (2)	Presence Formal firms (3)
Border x Year 2014	-0.002	-0.004	0.003
	(0.012)	(0.012)	(0.016)
Year 2014	0.094***	0.089***	0.182***
	(0.004)	(0.005)	(0.004)
Fixed effects	Muncipality	Muncipality	Muncipality
R-squared	0.013	0.012	0.046
Observations	7,066,866	6,699,490	367,376

Notes: This table presents difference-in-differences estimations of the effect of the VAT on firm presence, where presence is an indicator for the firm being present in the Economic Census. Column (1) shows results for all firms present in either 2009 or 2014. Column (2) shows results for informal firms, while Column (3) presents estimates for formal stores. All regressions include municipality fixed effects. Clustered errors at the municipality level are reported in parentheses. *denotes significance at the 10%, ** at the 5% and *** at the 1% levels.
Source: Authors' own computations based on data from the Economic Census, 2009-2014.

Table B.6: Difference-in-Differences Analysis of the VAT Increase on Firm Outcomes (2009-2014)

		Informal firms (1)	Formal firms (2)
Revenue	Estimated Coefficient	-0.072***	-0.095***
	S.E.	(0.019)	(0.029)
	N	2,433,200	144,236
Cost of employees	Estimated Coefficient	-0.059***	-0.093***
	S.E.	(0.020)	(0.032)
	N	2,433,200	144,236
Number of employees	Estimated Coefficient	-0.012	0.015
	S.E.	(0.007)	(0.019)
	N	2,433,200	144,236
Cost of materials	Estimated Coefficient	-0.045**	-0.100***
	S.E.	(0.018)	(0.033)
	N	2,433,200	144,236
Before-tax profits	Estimated Coefficient	0.048	-0.060
	S.E.	(0.036)	(0.058)
	N	2,433,200	144,236
Return on assets	Estimated Coefficient	-0.055***	-0.064***
	S.E.	(0.015)	(0.023)
	N	2,276,895	139,013

Notes: This table presents difference-in-differences estimations of the effect of the VAT on firm outcomes. The figures reported correspond to the estimated coefficient and standard errors for the interaction of variables $Border_l$ and $Year2014_t$. All outcome variables are expressed in logs. Column (1) shows results for informal firms, while Column (2) presents results for formal firms. All regressions include municipality fixed effects. Clustered errors at the municipality level are reported in parentheses. *denotes significance at the 10%, ** at the 5% and *** at the 1% levels.
Source: Authors' own computations based on data from the Economic Census, 2009-2014.

Table B.7: Difference-in-Differences Analysis of the VAT Increase on Labour Outcomes (2008-2015)

	Number of Workers (1)	Number of Workers (2)	Weekly Hours (3)	Weekly Hours (4)	Monthly Income (5)	Monthly Income (6)
Year 2011-Q2 x Border	0.017	-0.032	-0.051*	0.033	0.007	-0.104
	(0.013)	(0.026)	(0.030)	(0.029)	(0.123)	(0.150)
Year 2011-Q3 x Border	0.004	-0.038	-0.054*	0.035	0.057	-0.083
	(0.013)	(0.028)	(0.032)	(0.033)	(0.119)	(0.158)
Year 2011-Q4 x Border	-0.017	-0.026	-0.046	0.022	0.108	0.113
	(0.015)	(0.026)	(0.032)	(0.029)	(0.117)	(0.191)
Year 2012-Q1 x Border	0.015	-0.030	-0.018	0.016	-0.152	-0.178
	(0.014)	(0.028)	(0.031)	(0.033)	(0.133)	(0.183)
Year 2012-Q2 x Border	-0.007	-0.041	-0.006	0.036	-0.103	-0.005
	(0.012)	(0.028)	(0.030)	(0.031)	(0.135)	(0.172)
Year 2012-Q3 x Border	-0.008	-0.010	-0.023	0.014	0.130	0.008
	(0.011)	(0.022)	(0.030)	(0.024)	(0.124)	(0.155)
Year 2012-Q4 x Border	-0.003	-0.035	-0.048*	0.017	-0.107	-0.006
	(0.012)	(0.023)	(0.028)	(0.025)	(0.134)	(0.144)
Year 2013-Q1 x Border	0.007	-0.032	-0.017	-0.010	-0.095	-0.070
	(0.013)	(0.021)	(0.031)	(0.023)	(0.126)	(0.150)
Year 2013-Q2 x Border	0.008	-0.031	-0.056*	0.026	-0.023	-0.035
	(0.017)	(0.021)	(0.032)	(0.025)	(0.123)	(0.157)
Year 2013-Q4 x Border	-0.011	-0.018	-0.004	-0.031	-0.083	-0.332**
	(0.010)	(0.018)	(0.027)	(0.022)	(0.133)	(0.156)
Year 2014-Q1 x Border	0.006	0.003	-0.012	0.045*	-0.084	-0.073
	(0.013)	(0.019)	(0.029)	(0.026)	(0.128)	(0.143)
Year 2014-Q2 x Border	-0.001	-0.005	0.012	0.003	-0.007	0.032
	(0.012)	(0.021)	(0.031)	(0.036)	(0.113)	(0.144)
Year 2014-Q3 x Border	0.009	0.000	-0.025	0.024	-0.091	-0.037
	(0.012)	(0.022)	(0.032)	(0.025)	(0.123)	(0.139)
Year 2014-Q4 x Border	-0.005	0.005	-0.008	0.037	0.003	0.039
	(0.013)	(0.024)	(0.035)	(0.029)	(0.127)	(0.157)
Year 2015-Q1 x Border	-0.029**	-0.001	0.029	0.003	-0.152	-0.129
	(0.014)	(0.023)	(0.034)	(0.032)	(0.125)	(0.147)
Year 2015-Q2 x Border	-0.012	-0.019	0.041	0.025	0.030	-0.046
	(0.014)	(0.024)	(0.028)	(0.026)	(0.130)	(0.162)
Year 2015-Q3 x Border	-0.012	-0.022	0.019	0.044	0.001	-0.285
	(0.013)	(0.021)	(0.031)	(0.027)	(0.140)	(0.191)
Year 2015-Q4 x Border	-0.018	-0.005	0.026	0.044*	0.036	-0.141
	(0.013)	(0.024)	(0.031)	(0.025)	(0.129)	(0.153)
Observations	44,131	41,008	44,133	41,216	44,133	41,216
R-squared	0.467	0.484	0.391	0.236	0.612	0.389
Formality of Workers	Informal	Formal	Informal	Formal	Informal	Formal

Notes: This table presents difference-in-differences estimations of the effect of the VAT on labour outcomes (in logs). The figures reported correspond to the estimated coefficient and standard errors for the interaction of variables $Border_m$ and $Quarter_q$. Columns (1), (3) and (5) show results for informal workers, while Columns (2), (4) and (6) present results for workers in the formal sector. All regressions include municipality fixed effects. Clustered standard errors at the municipality level are reported in parentheses. *denotes significance at the 10%, ** at the 5% and *** at the 1% levels.
Source: Authors' own computations based on data from the National Survey of Occupation and Employment (ENOE), 2008-2015.

Chapter 3

Do Voters Respond to Tax Increases? The Effect of Equalizing the Value Added Tax on Mexican Elections

3.1 Introduction

It is well documented in the literature on economic voting that citizens hold incumbents accountable for economic performance (Stegmaier, Lewis-Beck and Park, 2017). Understanding which policy decisions are the ones the electorate prioritizes thus becomes a major interest for the parties in a given political system. While studies have shown how individuals respond to nation-wide fiscal adjustments (for example, Arias and Stasavage, 2016; Ardanaz et al., 2020; Hübscher et al., 2021; Talving, 2017), little is known about how centrally-mandated tax changes made in specific regions affect voting. Do incumbents lose political support in areas where they increase taxes?

This study addresses the question of whether a fiscal reform that modified the VAT rate from 11% to 16% in preferential areas (also known as 'border areas') in Mexico in 2014 had any impact on the electoral results of the political party responsible for its implementation: the Institutional Revolutionary Party (PRI). I show that the PRI party vote share in these preferential areas was not affected by an increase in the VAT rate a year and a half after its implementation. I additionally find a reduction of 3.2 percentage points in PRI vote shares 4 and a half years after the VAT change took place. The evidence presented in this study suggests that the decline in PRI support is the result of a new political party being able to attract voters by promoting the implementation of a lower VAT rate in preferential areas.

Mexico presents a good setting to explore this issue for a number of reasons. First, the unique geographical distribution of preferential areas allows me to isolate the effect of increasing VAT rates on electoral outcomes from the impact of other policies undertaken during the period analyzed in this paper. Preferential areas expand across the northern and southern border of the country, but they also cover municipalities far from any international limit. Most notably, some states contain both preferential and non-preferential areas within their territory, thus reducing the likelihood that effects found for preferential areas can be attributed to state-specific policies. Second, electoral calendars are fixed and therefore are not subject to endogenous electoral timing. Third, the highest share of tax revenues in the country is derived from the VAT (24%, OECD et al., 2022). The reliance on this tax and the large increase it experienced in preferential areas in 2014 (45%) contribute to its salience during the electoral periods studied in this paper.

My analysis uses disaggregated data on electoral results from over 60,000 small regions across the country between the years 2009 and 2018 to estimate the impact of the reform on the vote shares of the PRI. The main sample of this study includes votes obtained in the election of national deputies, which occur every 3 years, but additional analysis also considers results in presidential and senatorial elections, which take place every 6 years.

I estimate the causal effect of increasing the VAT on the vote shares of the PRI using a difference-in-differences specification. The tax was increased in some regions of the country, the preferential areas, which had benefited from a lower rate until 2014. Therefore, my approach compares the PRI electoral support before and after the implementation of the new tax in preferential areas and the rest of the country. I find no difference in the behavior of PRI vote shares between the two regions in 2009 and 2012, before the VAT increase,which helps support the assumption that, in the absence of the reform, PRI vote shares would have continued to behave similarly in later elections.

Despite not finding an effect in the short run, my results provide evidence of the existence of a lagged response to the VAT increase more than 4 years after it was put in place. The 3 percentage-point decline in PRI vote shares detected in the 2018 election of deputies is also present when analyzing the same year's presidential election, although, puzzlingly, no effect is found for PRI vote shares in the senate. Further analysis shows that the reduction in support shown for PRI candidates in the chamber of deputies during 2018 is not large enough to have an impact on party turnover within preferential areas, a result that is in line with those found by Alesina et al. (2012) for developed economies, where no effect is detected from applying fiscal adjustments on electoral turnover.

When analyzing another VAT change of a similar size that the PRI implemented in 1995, I find null results on PRI vote shares in the short and medium term for border areas. I argue that the significant decline in PRI support that occurred during 2018 is due to the emergence of a political party, the National Regeneration Movement (MORENA), that was able to garner more votes in preferential zones by proposing a reduction in the VAT rate. A similar proposal made by the PRI's historic rival, the National Action Party (PAN), was unsuccessful in achieving an increase in vote shares after the reform for this particular region. Thus, estimates suggests that

parties that implement large VAT increases may experience a loss in support if younger parties take advantage of such fiscal reforms to steal votes away from the politicians who put them in place.

This paper is related to several strands in the literature. Firstly, it contributes to the literature that studies the common assumption that voters dislike fiscal adjustments. While research has shown that austerity can lead to social unrest (Ponticelli and Voth, 2020) and mass protests (Genovese et al. 2016), evidence on the impact of austerity on electoral outcomes has been mixed. Alesina et al. (1998) study fiscal adjustments in 19 OECD from 1960 to 1995 and find that governments that implement them do not lose electoral support or popularity, even when elections occur three years after austerity measures are put in place. Arias and Stasavage (2019) similarly find that Western countries do not punish governments that engage in austerity.[1] More recently, however, research has documented that while austerity can be preferred to other economic measures (Bansak et al. 2021), European political parties can suffer a loss in support if they pursue it after an economic crisis (Talving, 2017; Fetzer, 2019; Barnes and Hicks, 2021).[2] Thus, the timing and context of fiscal reforms become relevant determinants of political support and validate the decision to approach austerity after elections instead of at the end of term (Persson and Tabellini, 2003), especially if electoral margins for the party in government are small (Hübscher and Sattler, 2017). Moreover, Ardanaz et al. (2020) argue that in Latin America tax-based fiscal adjustment have a large effect on the probability of electoral turnover for both stable and weak governments. I add to this literature by focusing on a tax increase that applied to a specific area of Mexico and compare the effect on vote shares for the incumbent party in treated and non-treated areas. Since the tax change was part of a national fiscal reform, I am able to exploit the geographical variation in the implementation of economic measures to isolate the impact of the tax change over other fiscal consolidation policies.

A second contribution of this paper is related to the literature on yardstick competition, which claims voters use fiscal choices from other jurisdictions as benchmarks to judge the performance of incumbent governments (Besley and Case, 1995). Under this theory, votes are reduced

[1] Arias and Stasavage (2019) present evidence of a null effect for both OLS and IV estimations, in which expenditure cuts are instrumented by exogenous trade and financial shocks.

[2] Fetzer (2019) and Galofré-Vilà et al. (2021) study the effect of fiscal austerity measures on the rise of populist parties in the UK and Germany, respectively.

by a tax increase but they can also be gained if increases are implemented in neighboring jurisdictions. In the setting of my paper, a tax increase is analyzed to detect whether federal politicians are differentially judged by voters in areas where the change took place. I also consider whether the effect varies if the control group is exclusively composed of neighboring regions to those treated. Unlike previous studies, the tax mimicking I examine is not attributed to interactions between the members of the same political party (Santolini, 2008, 2009), but part of a fiscal reform that equalized the VAT rate across the country. Instead of exploring whether tax-mimicking within a country exists (see Delgado et al. 2014; Redoano, 2007; Brueckner, 2003), I follow Bosch and Solé-Ollé (2007) and examine how this strategy affects voting behavior. They find that increasing property taxes in Spanish municipalities bears a large cost on incumbent vote shares in local elections. For federal elections, I detect no significant short-term impact of equalizing the VAT rate on the vote shares for the party that put it in place.

This paper is structured as follows. Section 3.2 provides background on the 2012 election and the 2014 VAT change. Section 3.3 describes the data and the relevant samples. Section 3.4 explains the empirical strategy used to identify reduce-form effects of the reform on PRI vote shares. Main results are presented in Section 3.5, with robustness checks shown in Section 3.6. Mechanisms appear in Section 3.7. Section 3.8 concludes.

3.2 The 2012 election and the VAT reform of 2014

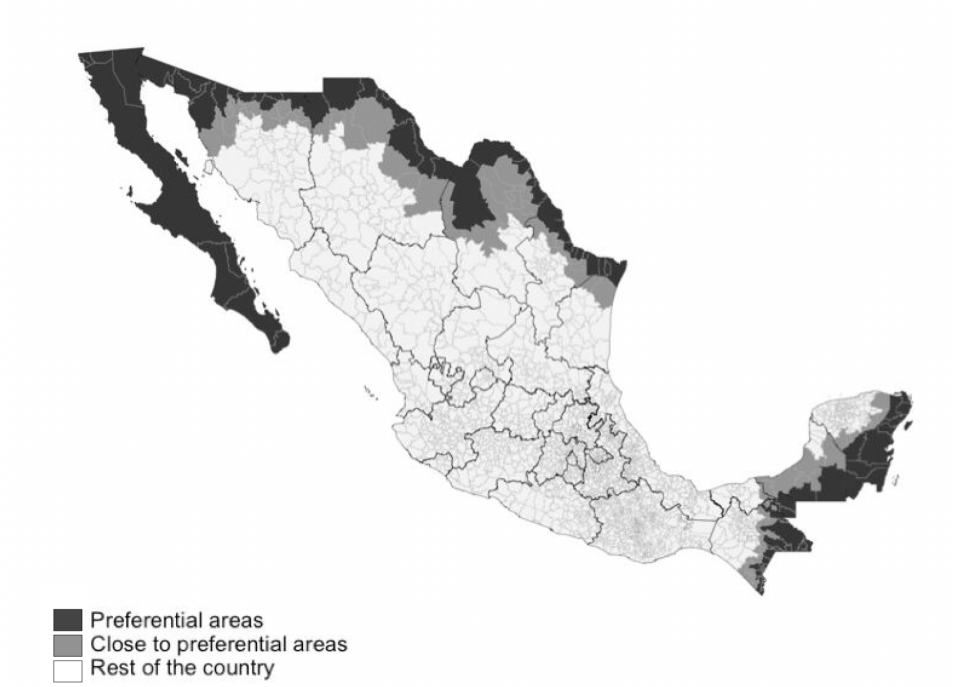
The PRI dominated Mexican politics for most of the twentieth century (Dell, 2015). In 2000, for the first time in 70 years, Mexico elected a canditate from the opposition party: the PAN (Flores-Macías, 2013). After another successful election by the PAN in 2006, the PRI finally returned to power in 2012. The newly elected president, Enrique Peña Nieto, announced a fiscal reform at the end of 2013, which included an equalization of the Value-Added Tax (VAT) rate across the nation. Even though the VAT equalization was part of a larger economic reform, fiscal changes other than the one studied in this paper were performed at a national or state level,[3] reducing the likelihood that these modifications drive the results in this study. The border

[3]Some products became taxed goods (for example, chewing gum, pet food) and others had additional taxes applied to them: 8% levy on non-basic food items with a caloric density of 275 kilo-calories per 100 grams, 3% tax on sale of fossil fuel except natural gas, application of the IEPS (Special Tax on Production and Services) to sugary drinks (Reuters, 2013).

reform implied a 5 percentage-point modification in the tax rate levied in the preferential zones, where the rate increased from 11% to 16%. Both chambers of the federal Congress approved the bill in October 2013 and, from January 2014 onward, the new tax rate was implemented in border areas.

The map shown in Figure 3.1 displays in black the preferential municipalities in the country. The original preferential tax zone was defined as all the towns within 20 kilometers of both borders (north and south). Over time, the classification of this region expanded to include the entire territory of the states of Baja California, Baja California Sur, Quintana Roo and some additional municipalities in the state of Sonora (Davis, 2011).

Figure 3.1: Preferential Tax Areas in Mexico



Notes: The map shows the Mexican preferential tax areas. Areas that had a VAT rate of 11% until 2014 are shown in black. Municipalities that are considered close to preferential areas are denoted in gray. Information based on Mexican Federal Government (2016).

3.3 Data

The Mexican president and the members of the national Congress are selected through federal elections. The national Congress is divided into two chambers: the chamber of senators and the chamber of deputies. While the presidential and senatorial elections take place every

Table 3.1: Descriptive Statistics, Federal Deputies Elections (2009-2018)

	Proportional Representation		First Past the Post	
	Non-preferential (1)	Preferential (2)	Non-preferential (3)	Preferential (4)
PRI vote share	33.485	30.632	33.338	30.546
Year	2013.176	2013.384	2013.176	2013.384
Number of parties	10.325	10.558	10.370	10.616
Number of votes	645.641	563.633	644.701	561.147
Lagged PRI share	35.636	34.943	35.602	34.900
N	220,144	21,533	220,103	21,528

Notes: This table shows summary statistics for the variables of interest in the treatment and control groups, where the treatment group is denoted as *Preferential* and the control group as *Non-preferential*. Columns (1)-(2) show statistics for the proportional representation system, while Columns (3)-(4) show statitistics for the first-past-the-post system.
Source: Author's own computations based on data from the National Electoral Institute (INE), 2009-2018.

six years, deputies are chosen every three years. The higher frequency in the election of deputies allows me to estimate the effect of the VAT equalization in two separate periods: a year and a half and four years and a half into its implementation.

I use data on electoral outcomes from Mexico's National Electoral Institute (INE). Results for years 2009 to 2018 are reported at the section (*sección*) level, a geographical subdivision of electoral districts in which individuals are registered to vote. These sections have a minimum of 50 and a maximum of 1,500 voters (*Código federal de instituciones y procedimientos electorales*, 2008). The main sample contains information on an average of 60,419 sections per year.

The INE reports data separately for the different systems employed to elect deputies: the relative majority (or first-past-the-post) system and the proportional representation system. In each of the 300 Mexican electoral districts, a deputy is chosen via simple majority. The other 200 deputies that are part of the chamber are appointed proportionally according to the number of votes each party obtains in each of the five circumscriptions (or *circunscripciones*) in the country.[4] In the interest of simplicity, I use the proportional representation database as my main sample and show results for the first-past-the-post system as robustness checks.

Table 3.1 presents descriptive statistics for PRI vote shares in border and non-border areas

[4]See Table C.1 for a list of states belonging to the each circumscription.

during the 2009, 2012, 2015 and 2018 elections of deputies. In both types of electoral system, preferential areas show a lower support for the PRI and tend to have a lower number of votes per section than the rest of the country. The similar number of parties competing per area and election in both groups suggests that the lower average support for PRI in preferential areas may not be attributed to a larger set of choices in these regions.

3.4 Empirical Methodology

In order to estimate the average effect of the 2014 VAT increase on electoral outcomes, I use a difference-in-differences estimation and compare preferential regions (the treatment group) to the rest of the country (the control group). The analysis I perform studies how the differences between these two areas varied between the pre-reform elections, in 2009 and 2012, and the post reform elections, in 2015 and 2018. I define the baseline specification of this study as follows:

$$PRIShare_{msy} = \sum_{y=2009}^{2018} [\alpha_y Year_y \times Pref_m + \beta_y Year_y] + X'_{msy}\gamma + \delta_m + \epsilon_{msy} \tag{3.1}$$

The dependent variable, $PRIShare_{msy}$, denotes the vote share of the PRI in the election held during year y, in section s (*sección*) located in municipality m. In particular, I study the election of deputies in the national Congress. During the period under analysis, the PRI formed political alliances with other parties, mainly the Ecologist Green Party of Mexico (PVEM). The Mexican electoral system allows individuals to vote for one or more political parties as long as they belong to the same coalition.[5] The INE provides information on the number of votes each party obtained individually and as a part of a political alliance. To capture the direct impact of the reform on the party that promoted this increase, $PRIShare_{msy}$ includes votes obtained only by the PRI, that is, as a coalition or individually. In other words, it excludes votes obtained solely by its political allies. In 2012, for example, the outcome variable does not include votes obtained by the Green Party. However, results are robust to $PRIShare_{msy}$ also including votes exclusively attributed to PRI's political allies (Tables C.7 and C.8).

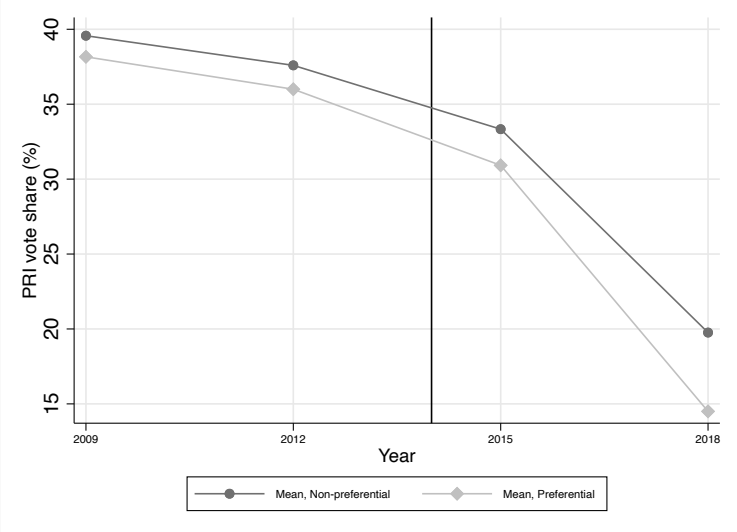
$Year_y \times Pref_m$ captures the effect of the interaction between the variables $Year_y$, a dummy for each year in the sample, and $Pref_m$, which indicates whether the municipality is

[5] Votes assigned to opposing parties are annulled (Vázquez Alfaro, 2012).

located in a preferential area. Since the election of deputies takes place every three years, there are three estimated coefficients for the interaction terms in the sample analyzed: $\hat{\alpha}_{2009}$, $\hat{\alpha}_{2015}$, $\hat{\alpha}_{2018}$. As the election of 2012 is the one that immediately precedes the reform, the interaction of the year 2012 dummy and the preferential area dummy, $Year_{2012} \times Pref_m$ is omitted from the regression. Negative estimates for α_{2015} and α_{2018} would represent a political backlash against the party that promoted the increase in the VAT rate in the treated areas. The vector X'_{msy} contains variables including the number of competing parties in the election, the number of votes per section and a lag for the outcome variable. δ_m denotes municipality fixed effects. Standard errors are clustered at the municipal level to control for the possibility of within-correlation among sections in the same location (Bertrand, Duflo and Mullainathan, 2004).

The identifying assumption of this empirical strategy states that in the absence of treatment, the average change in PRI vote share for the border areas equals the average change in the same outcome for the non-border regions. Figure 3.2 shows PRI vote share trends by area (see Figure C.1 for first-past-the-post averages). Before the reform, average shares in both regions present a similar pattern of declining support for PRI, including a sharp decline in preferential zones in 2018.

Figure 3.2: PRI Vote Share Averages in Proportional Representation System, by Area (2009-2018)



Notes: The figure shows the average PRI vote share by area and electoral year, before and after the VAT rate increase of 5 percentage points in preferential regions during January 2014.
Source: Author's own computations based on data from the National Electoral Institute (INE), 2009-2018.

To measure the total effect of the reform in preferential areas once the new VAT rate was implemented, instead of using year dummies interacted with the location indicator, I create a variable equal to 1 for elections that took place after 2014, $After_y$, and interact it with $Pref_m$. I use a similar empirical strategy for additional regressions where only one election before and after the reform is available, such as presidential and senatorial elections.

Between January 2014 and the 2015 elections, held in June, there were many political and socioeconomic events that could have had a differential impact across regions in Mexico. This analysis argues that the demarcation of preferential areas reduces the likelihood of confounding factors driving the estimates detailed in equation (3.1). Firstly, these areas are close to different countries, reducing the possibility that foreign policy in one particular nation drives the results. Secondly, proximity to any country is not necessarily a characteristic of the preferential tax regions, as some treated municipalities are located far from either international border (e.g. La Paz, in Baja California Sur). Additionally, pre-2014 VAT rates were assigned using an arbitrary 20-kilometer threshold with respect to the border, and do not correspond to state limits (Davis, 2011). Consequently, for some regions, state legislation affected both treated and non-treated zones, which helps mitigate concerns regarding unobserved differences across locations. These three characteristics of preferential regions suggest that changes exclusively related to the VAT rate drive the estimated coefficients $\hat{\alpha}_{2009}$, $\hat{\alpha}_{2015}$ and $\hat{\alpha}_{2018}$.

3.5 Results

Panel (a) of Table 3.2 shows results for the basic difference-in-differences specification (see Table C.2 and Figure C.2 for first-past-the-post results). Columns (1)-(4) present evidence of a negative but non-significant effect of the reform on vote shares in preferential areas. However, results differ by electoral year. Panel (b) and Figure 3.3 present estimates for the impact of the reform on each year in the sample: one year and a half after the increase in VAT was put in place in border areas, there was no significant effect on PRI vote shares. In 2018, 4 years and a half after the reform, I find a significant loss of 3.2 percentage points in vote shares, which is equivalent to almost 10% of the control mean before 2014.

The null results found in preferential areas for 2015 appear to be in line with those found in

Alesina et al. (1998) and Arias and Stasavage (2019): voters do not punish the party responsible for fiscal austerity less than two years into its implementation. However, I also find that, despite the lack of political backlash in the election immediately following a VAT increase, there is a statistically significant negative effect of this reform in the second election after it was put in place. This puzzling result implies that there is a delayed response from voters to tax changes.

A reverse causation claim could be made regarding the null results present in preferential areas for the 2015 elections: the PRI equalized the VAT across the country because it knew it was a strong enough party to perform the change without a significant loss in support. While I am not able to measure the political strength of this party, I argue that the existence of a coalition between the PRI and other parties in every year but 2009 supports the claim that the PRI, during the period under analysis, was not as strong as it was during the 20th century, when no presidential alternations occurred for 70 years. In a later section, I analyze the impact of another VAT change when the PRI ran without being part of a coalition, from 1991 to 2000, and detect no significant effect either for elections immediately after the VAT change. These results are in line with those of Alesina et al. (2012), who find no evidence of different voting behavior in terms of fiscal adjustments of coalition versus single-party governments.

3.6 Robustness checks

3.6.1 Effects by area

Panel (A) in Figure 3.4 shows the effect of the VAT equalization using different geographical areas as part of the control group (see Figure C.3 for fist-past-the-post results). The graph on the left presents the estimates for a specification in which the control group only includes municipalities adjacent to the affected regions. The graph on the right shows results when neighboring municipalities are excluded from the sample. In both years, results are larger when only neighboring municipalities are considered as part of the control group (see Tables C.3-C.4 in the appendix for point estimates). The evidence presented implies that the reform was not as negatively perceived in areas close to border zones. Put differently, municipalities close to preferential areas rewarded the PRI for equalizing the VAT rate.

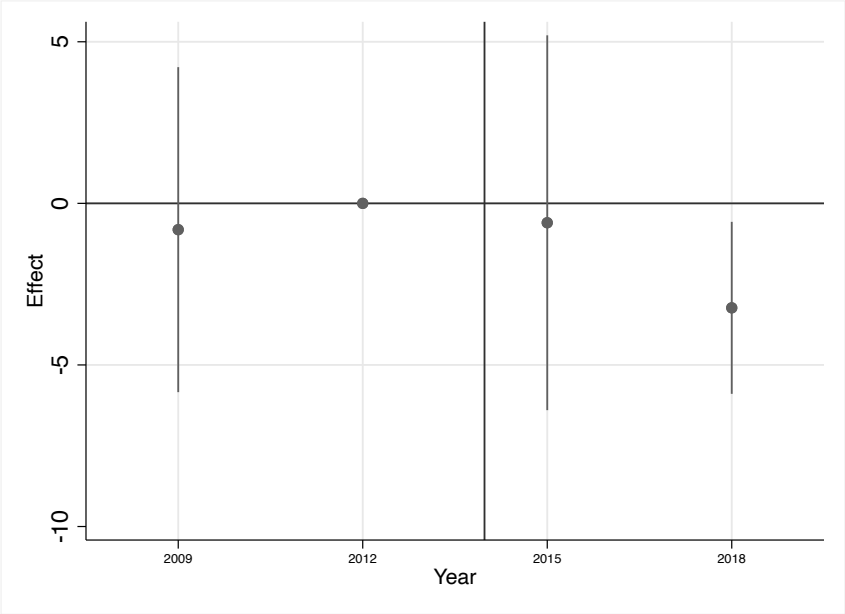
Besides estimating the effect of the VAT equalization relative to other control groups, I

Table 3.2: Difference-in-Differences Analysis of the Reform on PRI Vote Shares in the Proportional Representation System, Federal Deputies Election (2009-2018)

	PRI vote share (1)	PRI vote share (2)	PRI vote share (3)	PRI vote share (4)
Panel A: Effect before and after 2014				
After 2014 x Pref.	-1.871	-1.535	-1.815	-1.163
	(1.354)	(1.287)	(1.277)	(1.384)
After 2014	-10.980***	-10.715***	-5.762***	-5.794***
	(0.355)	(0.357)	(0.441)	(0.447)
Preferential	-1.540			
	(1.382)			
Number of parties			-1.816***	-1.836***
			(0.080)	(0.087)
Number of votes			0.000	0.000***
			(0.000)	(0.000)
Lagged PRI Share				0.272***
				(0.015)
R-squared	0.120	0.495	0.544	0.580
Panel B: Effect by year				
Year 2009 x Pref.	0.185	0.071	0.084	-0.814
	(2.363)	(2.256)	(2.348)	(2.564)
Year 2015 x Pref.	-0.819	-0.684	-0.744	-0.600
	(2.451)	(2.392)	(2.458)	(2.957)
Year 2018 x Pref.	-3.667**	-3.298**	-3.399**	-3.233**
	(1.658)	(1.324)	(1.425)	(1.357)
Year 2009	1.978***	1.955***	0.374	2.619***
	(0.517)	(0.519)	(0.541)	(0.655)
Year 2015	-4.258***	-4.182***	-4.167***	-3.651***
	(0.465)	(0.465)	(0.482)	(0.554)
Year 2018	-17.832***	-17.824***	-15.562***	-14.257***
	(0.486)	(0.419)	(0.517)	(0.495)
Preferential	-1.593			
	(0.986)			
Number of parties			-0.621***	-0.547***
			(0.069)	(0.059)
Number of votes			-0.001***	-0.000***
			(0.000)	(0.000)
Lagged PRI Share				0.251***
				(0.016)
R-squared	0.205	0.578	0.582	0.610
Observations	241,677	241,677	241,677	241,677
Fixed Effects	-	Municipality	Municipality	Municipality
Controls	-	-	Yes	Yes
Clusters	2,448	2,448	2,448	2,448

Notes: This table compares PRI vote shares in preferential and non-preferential areas around the January 2014 VAT equalization. Panel (A) shows estimates for the average effect of the reform after it was implemented. Panel (B) presents results for the effects on each year in the sample. Each column shows estimates for a separate difference-in-differences specification. Column (1) reports results without the inclusion of controls or fixed effects. Column (2) shows estimates for regressions with municipality fixed effects. Column (3) reports results with municipality fixed effects and two controls: the number of parties participating in the election and the number of votes within a section. Column (4) additionally includes a lag of the dependent variable. Standard errors are clustered at the municipality level. *** significant at 1%, ** significant at 5%, * significant at 10%.
Source: Author's own computations based on data from the National Electoral Institute (INE), 2009-2018.

Figure 3.3: Difference-in-Differences Analysis of the Reform on PRI Vote Shares in the Proportional Representation System (2009-2018)



Notes: This figure shows the impact of increasing the VAT rate on PRI vote shares. The reported estimates, represented by dots, correspond to interaction terms between year categorical variables and the *Preferential* variable. The 95% confidence intervals are reported as lines. Controls for the number of competing parties, number of votes and lagged PRI vote shares are included in the regression, as well as municipality fixed effects. Standard errors are clustered at the municipality level.
Source: Author's own computations based on data from the National Electoral Institute (INE), 2009-2018.

estimate the impact of the reform separately for north and south preferential areas. Almost 76% of the treated municipalities are located in the north of the country, close to the border with the United States, where state sales tax rates do not exceed 9%.[6] The remaining treated municipalities are found close to Mexico's southern border, near Belize and Guatemala, whose VAT rates ranged between 12% to 12.5% in 2014 (Congreso de la República de Guatemala, 2001; Law revision commissioner of Belize, 2011). Although the estimated effects of the reform on both types of preferential areas during 2018 are negative, in 2015, southern areas had a non-significant increase in PRI vote shares, while the PRI lost support in the north (see Tables C.5-C.6 in the appendix for point estimates). Thus, despite displaying varying results in 2015, southern and northern border areas reacted similarly towards the PRI in 2018.

3.6.2 Coalition parties as alternative outcome

With the exception of the elections in 2009, the PRI was part of a political coalition in the legislative and presidential elections that took place during the period under analysis. In 2012 and 2015, it formed an alliance with the Green Party, PVEM, and in 2018 these two parties were also joined by the New Alliance Party (PANAL). Previous estimations excluded from the PRI vote share count votes that were exclusively cast in favor of the other members of the PRI coalition. The inclusion of these additional votes increases the average PRI vote share in border and non-border areas by 8 and 6 percentage points, respectively.[7]

Figure 3.5 and Table C.7 show regression estimates for equation (3.1) when the dependent variable, *PRIShare*, includes all the coalition votes (see Table C.8 and Figure C.4 for first-past-the-post results). In this case, the effect for 2015 is positive, but non-significant, suggesting that the PRI allies had a positive, though negligible, effect in vote shares for the coalition in border areas. Estimates for 2018 are slightly larger than baseline results when taking into account votes cast in favour of PRI allies: vote shares were reduced by 4.24 percentage points in preferential areas. The size of the effect relative to the control mean is close to 10%, a similar figure to the one reported in the main results section.

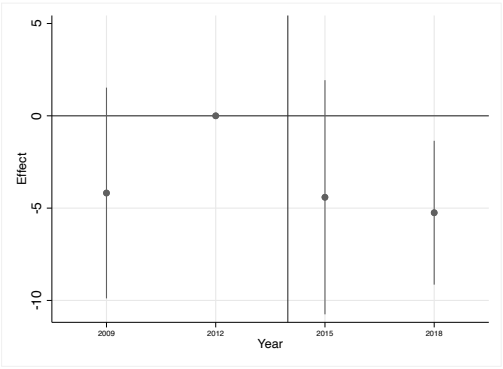
[6]Sales tax rates vary in states that share a border with Mexico: Arizona (5.6%), California (6.5%), New Mexico (5.125%), and Texas (6.25%) (Arizona Department of Revenue, 2022; California Department of Tax and Fee Administration, 2022; New Mexico Taxation and Revenue Department, 2022; Texas Comptroller of Public Accounts, 2022).

[7]The average PRI coalition vote share per section is 38.8% in preferential areas and 39.4% in the rest of the country.

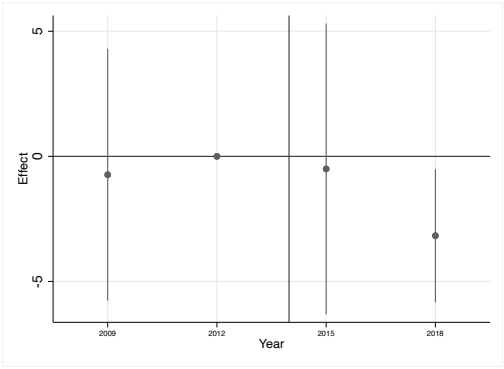
Figure 3.4: Difference-in-Differences Analysis of the Reform on PRI Vote Shares in the Proportional Representation System, by Area (2009-2018)

(A) Alternative control groups

I. Close Municipalities

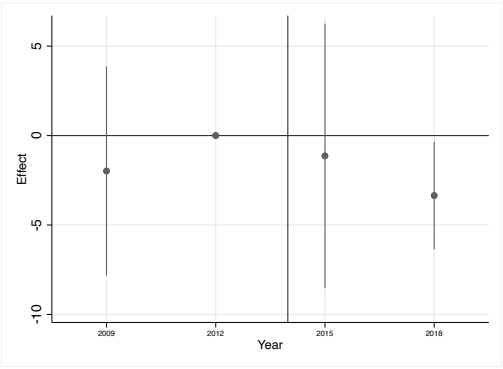


II. Rest of the Country

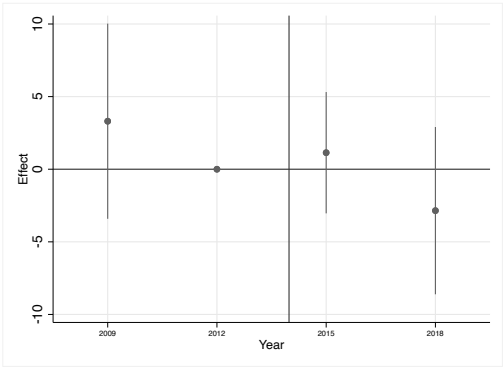


(B) By Border

III. North Border

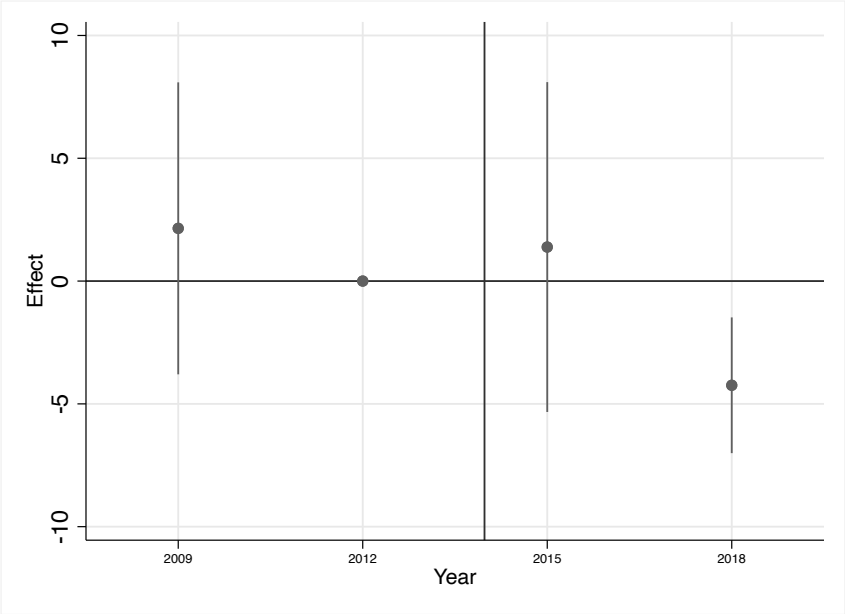


IV. South Border



Notes: This figure shows the impact of increasing the VAT rate on PRI vote shares in different geographical areas. Panel (A) shows results using alternative control groups: neighboring and non-neighboring municipalities to the treated regions. Panel (B) shows results separately for northern and southern preferential areas. The reported estimates, represented by dots, correspond to interaction terms between year categorical variables and the *Preferential* variable. The 95% confidence intervals are reported as lines. Controls for the number of competing parties, number of votes and lagged PRI vote shares are included in all regressions, as well as municipality fixed effects. Standard errors are clustered at the municipality level.
Source: Author's own computations based on data from the National Electoral Institute (INE), 2009-2018.

Figure 3.5: Difference-in-Differences Analysis of the Reform on PRI Coalition Vote Shares in the Proportional Representation System, (2009-2018)



Notes: This figure shows the impact of increasing the VAT rate on PRI coalition vote shares. The reported estimates, represented by dots, correspond to interaction terms between year categorical variables and the *Preferential* variable. The 95% confidence intervals are reported as lines. Controls for the number of competing parties, number of votes and lagged PRI coalition vote shares are included in the regression, as well as municipality fixed effects. Standard errors are clustered at the municipality level.
Source: Author's own computations based on data from the National Electoral Institute (INE), 2009-2018.

3.6.3 Elected Deputies

In the Mexican Congress, 200 out of 500 deputies are elected via the proportional representation system. States within the country are grouped into 5 circumscriptions, with each circumscription appointing 40 deputies per electoral process (see Table C.1 for a list of the states corresponding to each circumscription). Thus, it is possible to estimate, within each state, the share of PRI deputies elected every year under analysis. The remaining 300 deputies of the chamber are elected through simple majority, with each deputy representing a district in the country. In this case, I am able to observe whether a PRI candidate was elected in a given district using information available at the Chamber of Deputies website (Honorable Cámara de Diputados, 2020).

Using this information, I am able to estimate the impact of the VAT increase on the election of PRI deputies in both types of electoral systems. Columns (1) and (2) of Table 3.3 present results when the outcome variable is the share of PRI deputies elected in a given state through the proportional representation system. While the effect for both 2015 and 2018 in preferential areas is negative, it is not statistically significant in either case. Columns (3) and (4) of Table 3.3 show estimates when the dependent variable is an indicator for the elected deputy of a given district belonging to the PRI party. The probability of selecting a PRI deputy in border areas decreased in 2015 and increased in 2018, relative to 2012, although in both cases the effect is not statistically significant. Given that PRI vote shares were reduced for these regions after the reform, results suggest that winning margins were reduced. Thus, while the VAT had an impact on PRI vote shares in preferential municipalities, this change was not large enough to affect the election of federal deputies. This evidence is consistent with those found in Arias and Stasavage (2019) where fiscal adjustments do not alter political turnover.

3.6.4 The 2012 legislature and the approval of the VAT change

In October 2013, the chamber of deputies passed the bill that would increase the VAT rate in preferential areas from 11% to 16%. Out of 500 deputies that were elected in 2012, 317 voted in favor of the change, with 109 positive votes attributed to deputies elected via proportional representation. Column (1) of Table 3.4 presents results for equation (3.1) in states where a small share of deputies voted in favor of the reform, while Column (2) presents estimates in

Table 3.3: Difference-in-Differences Analysis of the Reform on Elected Federal Deputies (2009-2018)

	Proportional Representation		First Past the Post	
	Share Elected (PRI) (1)	Share Elected (PRI) (2)	Elected Deputy (PRI) (3)	Elected Deputy (PRI) (4)
Year 2009 x Pref.	5.911	3.481	-0.106	-0.070
	(5.211)	(3.554)	(0.152)	(0.142)
Year 2015 x Pref.	-3.743	-5.654	-0.158	-0.142
	(6.267)	(4.979)	(0.141)	(0.114)
Year 2018 x Pref.	-3.226	-6.384	0.019	0.006
	(4.008)	(4.488)	(0.074)	(0.072)
Year 2009	2.283**	0.291	0.083***	0.009
	(1.065)	(1.136)	(0.028)	(0.028)
Year 2015	0.537	-0.446	0.007	-0.009
	(1.395)	(1.213)	(0.028)	(0.026)
Year 2018	-2.948***	-5.570***	-0.512***	-0.529***
	(1.069)	(1.205)	(0.027)	(0.029)
Number of parties		0.408***		0.001
		(0.103)		(0.003)
Number of votes		-0.000*		-0.000
		(0.000)		(0.000)
Lagged Elected PRI Share		-0.386***		
		(0.044)		
Lagged Elected PRI Deputy				-0.192***
				(0.031)
Observations	241,677	241,677	241,631	241,631
R-squared	0.396	0.470	0.513	0.532
Fixed Effects	Municipality	Municipality	Municipality	Municipality
Controls	-	Yes	-	Yes
Clusters	2,448	2,448	2,448	2,448

Notes: This table compares elected PRI deputies in preferential and non-preferential areas around the January 2014 VAT equalization. Each column shows estimates for a separate difference-in-differences specification. Columns (1)-(2) report results for the proportional representation system. The outcome variable is the share of elected deputies that belong to the PRI party in a state. Columns (3)-(4) shows estimates for the first-past-the-post system. The outcome variable is an indicator for whether the deputy elected in the district is a member of the PRI party. Columns (1) and (3) present results for regressions that do not include controls. Columns (2) and (4) show estimates for regressions with three controls: a lag of the dependent variable, the number of parties participating in the election and the number of votes within a section. All regressions include municipality fixed effects. Standard errors are clustered at the municipality level. *** significant at 1%, ** significant at 5%, * significant at 10%.
Source: Author's own computations based on data from the National Electoral Institute (INE), 2009-2018.

states where a large proportion of deputies voted in favor of the VAT increase. The division of the sample is done according to the median share of deputies that voted in favor of the reform for a given state, where the median is equal to 46.41%. For border areas in Column (1), there is a large increase in PRI support in the election after the reform followed by a reduction in average PRI vote shares of a similar size to the effect found at baseline. For preferential regions in the second group, the impact of the reform is negative in both elections after 2014 and considerably larger in 2015 than in 2018. However, the existence of differing trends in PRI vote shares before the VAT suggests caution is needed when interpreting these results. A similar case can be made when splitting the sample according to the median share of elected PRI deputies within a state in 2012, which is equal to 59.81% (see Columns (3)-(4) of Table 3.4 for estimates). The final column of Table 3.4 shows the estimated impact of the reform in states that had a large share of elected PRI members and of deputies who voted in favor of the VAT change, while Column (5) includes the remaining areas. Results in both cases are similar to the ones presented Columns (1) through (4) of Table 3.4.

In Panel A of Table C.9, I use the same strategy to divide the first-past-the-post sample and find results that are in line with those found in Table 3.4. However, since deputies elected via the relative majority system correspond to single-member district, I am able to perform an additional exercise on this sample. In Panel B of Table C.9, instead of dividing the sample according to state shares, I split the sample according to whether the deputy elected within a district in 2012 was a PRI member and whether she voted in favor of the VAT change in 2013.[8] In this case, PRI vote share trends do not differ before the reform between border and non-border areas. The estimated effect in preferential areas for 2015 is not significant in any of the regressions, while the impact of the VAT change in preferential areas for 2018 is slightly larger when a district's elected deputy during 2012 voted in favor of the VAT change (Column(2)), is a PRI member (Column(4)), or both (Column (6)). These last results suggest that areas where deputies were PRI members and/or voted in favor of the VAT change had a larger decline in PRI vote shares after the reform than the rest of the country, though these differences are small.

[8]For this restricted sample of 300 deputies elected per term through relative majority, 208 of them voted in favor of the VAT change when the bill was passed in 2013.

Table 3.4: Difference-in-Differences Analysis of the Reform on PRI Vote Shares in the Proportional Representation System, VAT Votes and 2012 Deputies (2009-2018)

	VAT Vote		PRI Deputy in 2012		VAT Vote & PRI Deputy in 2012	
	PRI vote share (1)	PRI vote share (2)	PRI vote share (3)	PRI vote share (4)	PRI vote share (5)	PRI vote share (6)
Year 2009 x Pref.	8.309***	-7.185**	7.478**	-6.525**	7.671**	-6.820**
	(3.074)	(2.907)	(3.083)	(2.906)	(3.072)	(2.914)
Year 2015 x Pref.	8.447***	-7.052*	8.046***	-6.768*	8.041***	-6.847*
	(2.623)	(3.917)	(2.627)	(3.918)	(2.614)	(3.924)
Year 2018 x Pref.	-3.863**	-2.374	-3.678*	-2.680	-4.054**	-2.113
	(1.881)	(1.939)	(1.881)	(1.942)	(1.875)	(1.949)
Year 2009	0.451	4.489***	1.274	3.800***	1.057	4.075***
	(0.968)	(0.823)	(1.030)	(0.801)	(0.972)	(0.837)
Year 2015	-5.241***	-2.439***	-4.902***	-2.744***	-4.882***	-2.634***
	(0.768)	(0.744)	(0.810)	(0.727)	(0.764)	(0.762)
Year 2018	-12.753***	-15.519***	-13.096***	-15.200***	-12.630***	-15.713***
	(0.643)	(0.690)	(0.659)	(0.687)	(0.628)	(0.708)
Number of parties	-0.456***	-0.573***	-0.430***	-0.577***	-0.448***	-0.596***
	(0.080)	(0.090)	(0.081)	(0.087)	(0.077)	(0.094)
Number of votes	-0.001***	-0.000*	-0.001***	-0.000**	-0.001***	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Lagged PRI Share	0.262***	0.259***	0.255***	0.259***	0.257***	0.259***
	(0.028)	(0.017)	(0.030)	(0.016)	(0.028)	(0.017)
Observations	103,601	138,076	100,622	141,055	107,704	133,973
R-squared	0.589	0.615	0.608	0.604	0.602	0.612
Fixed Effects	Municipality	Municipality	Municipality	Municipality	Municipality	Municipality
Controls	Yes	Yes	Yes	Yes	Yes	Yes
VAT vote	Below Median	Above Median	- -	- -	Rest	Above Median
Deputies in 2012	- -		Below PRI Median	Above PRI Median	Rest	Above Median
Clusters	1,147	1,301	1,195	1,253	1,253	1,195

Notes: This table compares PRI vote shares in preferential and non-preferential areas around the January 2014 VAT equalization. Each column shows estimates for a separate difference-in-differences specification. Columns (1)-(2) report results according to the share of deputies that voted in favor of the VAT change within a state. Columns (3)-(4) shows estimates according to the share of PRI deputies elected within a state in 2012. Column (6) presents results for states with a large share of PRI deputies who also voted in favor of the VAT change in 2012, while Column (5) shows estimates for the remaining states. All regressions include municipality fixed effects and three controls: a lag of the dependent variable, the number of parties participating in the election and the number of votes within a section. Standard errors are clustered at the municipality level. *** significant at 1%, ** significant at 5%, * significant at 10%.
Source: Author's own computations based on data from the National Electoral Institute (INE), 2009-2018.

Table 3.5: Difference-in-Differences Analysis of the Reform on PRI Vote Shares, Federal Senators and President (2009-2018)

	Presidential Elections		Senatorial Elections			
			Proportional Representation		First Past the Post	
	PRI vote share (1)	PRI vote share (2)	PRI vote share (3)	PRI vote share (4)	PRI vote share (5)	PRI vote share (6)
After 2014 x Pref.	-3.402***	-2.951**	-0.945	-0.158	-0.676	0.096
	(1.086)	(1.368)	(1.547)	(1.699)	(1.574)	(1.718)
After 2014	-23.090***	-24.784***	-17.129***	-16.867***	-17.461***	-17.210***
	(0.418)	(0.560)	(0.421)	(0.599)	(0.453)	(0.621)
Number of parties		-0.392***		-0.415***		-0.389***
		(0.043)		(0.069)		(0.074)
Number of votes		-0.001***		-0.001***		-0.001***
		(0.000)		(0.000)		(0.000)
Lagged PRI Share		0.298***		0.278***		0.279***
		(0.015)		(0.026)		(0.025)
Observations	110,606	110,606	110,638	110,638	110,636	110,636
R-squared	0.751	0.787	0.677	0.715	0.679	0.717
Fixed Effects	Municipality	Municipality	Municipality	Municipality	Municipality	Municipality
Controls	-	Yes	-	Yes	-	Yes
Clusters	2,444	2,444	2,441	2,441	2,441	2,441

Notes: This table compares PRI vote shares in preferential and non-preferential areas around the January 2014 VAT equalization. Each column shows estimates for a separate difference-in-differences specification. Columns (1)-(2) report results for presidential elections. Columns (3)-(4) show estimates for senatorial elections under the proportional representation system. Columns (5)-(6) report results for senatorial elections under the first-past-the-post system. Columns (1), (3) and (5) present results for regressions that do not include controls. Columns (2), (4) and (6) show estimates for regressions with three controls: a lag of the dependent variable, the number of parties participating in the election and the number of votes within a section. All regressions include municipality fixed effects. Standard errors are clustered at the municipality level. *** significant at 1%, ** significant at 5%, * significant at 10%.
Source: Author's own computations based on data from the National Electoral Institute (INE), 2009-2018.

3.6.5 Senatorial and presidential elections

In this section, I assess if the equalization of the VAT rate had an impact on other elections. Unlike the case of federal deputies, presidential and senatorial elections are held every 6 years, instead of 3, and on the same day as the election of deputies. In the sample analyzed, presidential and senatorial elections took place in 2012 and 2018. Given the negative and significant effect present in PRI vote shares for deputies in 2018, I expect a similar effect on presidential and senatorial elections, the latter of which also use proportional representation and first-past-the-post systems to appoint seats in the federal Congress. Table 3.5 shows that there is a significant impact of the reform for presidential elections, where the loss in average PRI vote share is similar in size to the one reported for federal deputies and equal to 2.95 percentage points. Interestingly, the estimated effect on senatorial elections is much smaller and not statistically significant. That is, in a year where 3 different elections took place simultaneously, voters in preferential areas only reacted against the PRI in 2 of those elections. Thus, voters only held deputies and the president accountable for the tax increase that took place in 2014.

3.6.6 PRI incumbency

In this section, I analyze the effect of the 2014 VAT reform taking into account whether jurisdictions with previously elected PRI deputies had a differential impact on PRI vote share. Columns (1) and (2) of Table C.10 show results for the proportional representation system, where year dummies are interacted with the indicator for preferential area and the number of elected PRI deputies over all deputies within a given state and year (*Share of PRI Deputies*). When including controls in the regression, this triple interaction dummies have negative and non-significant coefficients. Furthermore, results for the coefficients of *Year 2015* $\times$ *Pref* and *Year 2018* $\times$ *Pref* are very similar to the ones found at baseline. For the first-past-the-post system, instead of interacting *Year 2018* $\times$ *Pref* by the proportion of elected deputies, I apply an indicator for whether the district elected a PRI deputy. In this case, the triple interaction coefficients are positive, although they are not significant for the years after the reform. Thus, preferential areas with PRI representation in the national Congress did not experience an electoral advantage after the reform was implemented.

3.6.7 Attrition concerns

Observations that had missing values in any of the variables included in the baseline specification were removed from the main sample. This resulted in the attrition of 24,476 observations in the proportional representation sample, with 82% of them corresponding to the year 2018. All of these observations only have missing values in the lagged PRI vote share variable, as information is not available for some sections before 2018. In Tables C.11-C.12, I report estimates of equation (3.1) when these observations are included in the sample and the lagged PRI vote share is excluded from the specification. For both types of electoral systems, results are similar to the ones found in Table 3.2: the VAT increase caused a reduction in PRI votes shares in border areas by 3.5-3.9 percentage points in 2018, while no significant effect is detected for 2015. The inclusion of these additional observations, however, renders the estimated coefficients of the variable *After 2014* $\times$ *Pref* significant (Panel A in Tables C.11-C.12). Thus, baseline results can be interpreted as cautious estimations of the impact of the reform.

3.7 Mechanisms

3.7.1 Understanding null results in the short-term

Reaction to another VAT change

The literature on electoral cycles argues that fiscal reforms that may be negatively perceived by the public, such as tax increases, are performed early in the political term to reduce the backlash against these measures in the following election (Persson and Tabellini, 2003). The evidence presented above for the year 2015 empirically supports this hypothesis. However, there may also exists an issue of reverse causation, in which the government pursued austerity measures because it knew it would not suffer a large electoral loss. While I am not able to instrument for the implementation of the 2014 VAT equalization, I follow Alesina et al. (2012) and compare similar fiscal reforms performed by weak and strong governments. Parties that resort to coalitions to gain support are considered weaker than those who run for elections as single-party units and are therefore less likely to implement austerity.

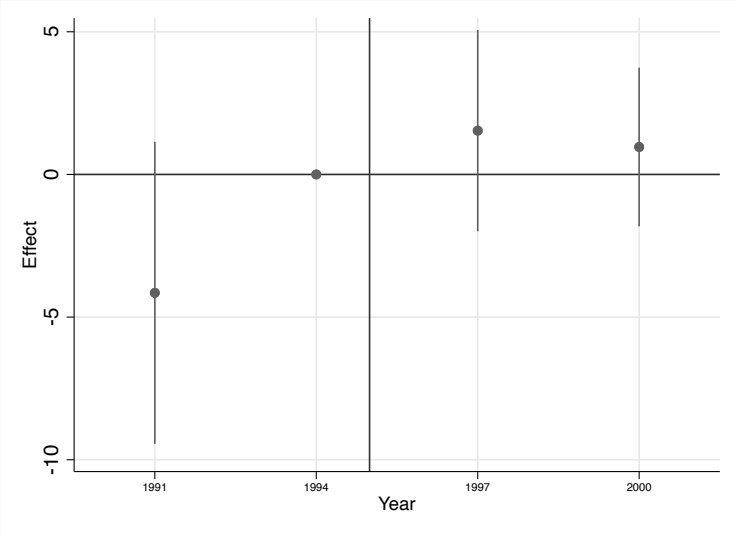
Until the year 2000, the PRI was the dominant party in Mexico, having held the presidency since 1929 (Langston, 2017). During the 21st century, however, electoral turnover pushed the PRI to find political allies in order to maintain its support across the country. In previous sections, I showed that during the period where the PRI was part of a coalition the party did not suffer a significant political loss in the first election held after the VAT change, but it did experience a decline in support in 2018. In this section, I study a reform that occurred during the PRI's single-party era.

In 1994, Ernesto Zedillo, the PRI presidential candidate was elected into office. In 1995, a few months into his term, the Congress approved his proposal of increasing the VAT rate from 10% to 15% across the country, with the exception of preferential areas, who would maintain a rate of 10% (Lustig, 2010). Thus, the 1995 VAT rate increase was similar to that of 2014: a recently elected PRI president increased the rate by 5 percentage points in part of the country. These reforms differed, however, in the part of the country affected.

Figure 3.6 shows that, unlike the 2014 reform, this earlier fiscal adjustment had a positive but non-significant effect two years after it was carried out (see Table C.13 for estimates; Table

C.14 and Figure C.5 for first-past-the-post coefficients). That is, in areas where the VAT rate was increased (the non-border zones) political support for the PRI was reduced relative to those where no change took place (border regions). The lack of significance for estimates in the first election held after the VAT change is also found in the main specification. That is, I detect no difference in preferential area voting between single-party and coalition elections. However, the negative and statistically significant effect found more than four years into the 2014 VAT increase appears to support the claim that weakened parties are more likely to suffer electoral losses when implementing fiscal reforms.

Figure 3.6: Difference-in-Differences Analysis of the Reform on PRI Vote Shares in the Proportional Representation System (1991-2000)



Notes: This figure shows the impact of increasing the VAT rate on PRI vote shares. The reported estimates, represented by dots, correspond to interaction terms between year categorical variables and the *Preferential* variable. The 95% confidence intervals are reported as lines. Controls for the number of competing parties and the number of votes are included in the regression, as well as municipality fixed effects. Standard errors are clustered at the municipality level.
Source: Author's own computations based on data from the National Electoral Institute (INE), 1991-2000.

3.7.2 Medium-term effects: a lagged response?

Effect on opposition party vote shares

In October 2013, both chambers of the Mexican national Congress approved the equalization of the VAT rate across the country. No senator or deputy from the PAN, the PRI's

historic rival and the country's largest right-wing party, voted in favor of the project (Chamber of Deputies, 2013). In this section, I examine if standing against the new bill helped the PAN increased its support in affected areas after the change in VAT was in place.

Results in Figure 3.7 show that PAN vote shares experienced a non-significant increase in preferential areas in the 2015 elections (see Tables C.15 for points estimates; Table C.16 and Figure C.6 for first-past-the post results). The fact that no elected deputy belonging to the PAN party voted in favor of the VAT rate increase did not improve PAN vote shares considerably in treated areas.

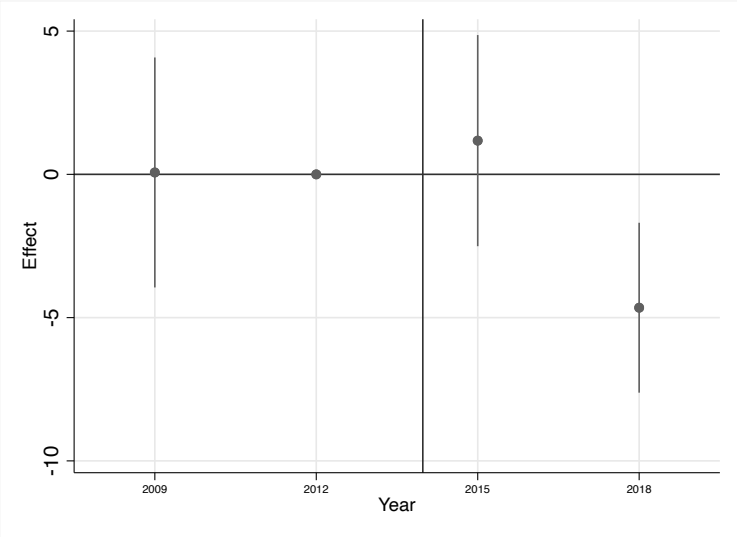
In 2018, however, PAN experienced a reduction in vote shares larger than that found for the PRI in the same period and equal to 4.7 percentage votes, an effect that represents 17% of the pre-reform control mean. This estimate shows that despite having opposing views on the fiscal adjustment approved in 2013 and put in place in 2014, both the PRI and the PAN had a similar vote share loss in preferential zones relative to the rest of the country in 2018. In further sections, I show that the decline in support for these two historically predominant parties during the 2018 election in preferential areas is due to the increased popularity of another political force: the MORENA party.

Campaign for a new VAT rate

In 2015, a new left-wing party, the MORENA faced its first election and garnered 8.39% of votes under the first-past-the-post system (INE, 2015). Only 3 years later, the coalition formed by the MORENA, the Party of the Democratic Revolution (PRD) and the Social Encounter Party was the most voted party at the national level in all the federal elections that took place in 2018, including the presidency, which was gained with 53.19% of the votes (INE, 2018).

One of the campaign proposals made by the MORENA's presidential candidate, Andrés Manuel López Obrador, included reducing the VAT rate to 8% in border areas. The PAN's presidential candidate, Ricardo Anaya Cortés, mimicked MORENA's strategy and promised to cut the VAT rate by the same amount, a political move that was perceived as plagiarism by the MORENA (Forbes, 2018). Anaya countered that, as a member of Congress in 2013, he had rejected the PRI's project to increase the VAT rate and that he was therefore being consistent

Figure 3.7: Difference-in-Differences Analysis of the Reform on PAN Vote Shares in the Proportional Representation System (2009-2018)



Notes: This figure shows the impact of increasing the VAT rate on PAN vote shares. The reported estimates, represented by dots, correspond to interaction terms between year categorical variables and the *Preferential* variable. The 95% confidence intervals are reported as lines. Controls for a lag of the dependent variable, the number of competing parties and the number of votes are included in the regression, as well as municipality fixed effects. Standard errors are clustered at the municipality level.
Source: Author's own computations based on data from the National Electoral Institute (INE), 2009-2018.

with a political stand that preceded the 2018 presidential campaign (Sánchez, 2018). On the one hand, this conflict shed light on the importance of the VAT during the 2018 electoral campaign. On the other hand, given that the MORENA and the PAN had similar proposals regarding this tax, changes in vote shares for these parties in preferential areas should also be similar, unless additional factors determine electoral support in these regions.

Table 3.6 shows the estimates of a difference-in-differences regression similar to the one presented in equation (3.1). Since the MORENA only participated in the 2015 and the 2018 elections, the main coefficient of interest is the interaction term between the preferential area indicator and the dummy variable for the year 2018. This interaction term captures the differential effect that the MORENA party had in preferential areas during the 2018 election. The demarcation of preferential areas reduces the likelihood of factors other than VAT policy driving the results of this specification, as the delimitation of these regions does not always coincide with other political jurisdictions. Furthermore, the possibility of foreign policy in a neighbouring country affecting the results is reduced, given that border regions can be close to three different countries (i.e. the United States, Guatemala and Belize) or far from any international borders (e.g. Baja California Sur). Since VAT policy in these areas was a salient campaign issue for the MORENA party in 2018, I expect the estimate for the interaction between the 2018 indicator and the preferential region indicator to be significant and positive if the party was successful in proposing an new VAT tax scheme.

Columns (1) and (2) show that, relative to the rest of the country, the MORENA had an increase in vote shares of almost 9 percentage points in preferential areas. Despite having a similar proposal to the one presented by MORENA regarding VAT changes, the PAN lost an average of 5.4 percentage points in vote shares in preferential areas. Thus, the announcement of a VAT cut only proved to be a successful strategy for MORENA. Given the evidence presented, it is possible to attribute the decline in PRI and PAN support to a differential capacity of this younger party to politically benefit from the tax increase implemented by PRI and the proposal of a tax cut in preferential areas.

Table 3.6: Difference-in-Differences Analysis of the VAT Campaign on MORENA and PAN Vote Shares in the Proportional Representation System (2015-2018)

	MORENA vote share (1)	MORENA vote share (2)	PAN vote share (3)	PAN vote share (4)
Year 2018 x Pref.	8.795***	8.882***	-5.367***	-5.435**
	(1.455)	(1.544)	(2.047)	(2.111)
Year 2018	28.349***	25.433***	-1.928***	-0.399
	(0.628)	(0.672)	(0.575)	(0.561)
Number of parties		0.817***		-0.480***
		(0.078)		(0.085)
Number of votes		0.001***		0.000***
		(0.000)		(0.000)
Observations	112,934	112,934	112,934	112,934
R-squared	0.817	0.823	0.651	0.653
Fixed Effects	Municipality	Municipality	Municipality	Municipality
Controls	-	Yes	-	Yes
Clusters	2,426	2,426	2,426	2,426

Notes: This table compares vote shares in preferential and non-preferential areas around the proposal of a VAT reduction in 2018. Each column shows estimates for a separate difference-in-differences specification. Columns (1)-(2) report results for MORENA vote shares. Columns (3)-(4) shows estimates for PAN vote shares. Columns (1) and (3) present results for regressions that do not include controls. Columns (2) and (4) show estimates for regressions with two controls: the number of parties participating in the election and the number of votes within a section. All regressions include municipality fixed effects. Standard errors are clustered at the municipality level. *** significant at 1%, ** significant at 5%, * significant at 10%.
Source: Author's own computations based on data from the National Electoral Institute (INE), 2015-2018.

3.7.3 Effects on expenditure and income

The introduction of a preferential VAT area was justified as a deterrent to shopping abroad (Davis, 2011). In this section, I use the National Survey of Household Income and Expenditure (ENIGH) to explore if the change in the preferential VAT rate had any impact on income or expenditure for households living in the treated regions. The ENIGH is collected every two years and contains nationally representative cross-sectional data on urban and rural households in Mexico.

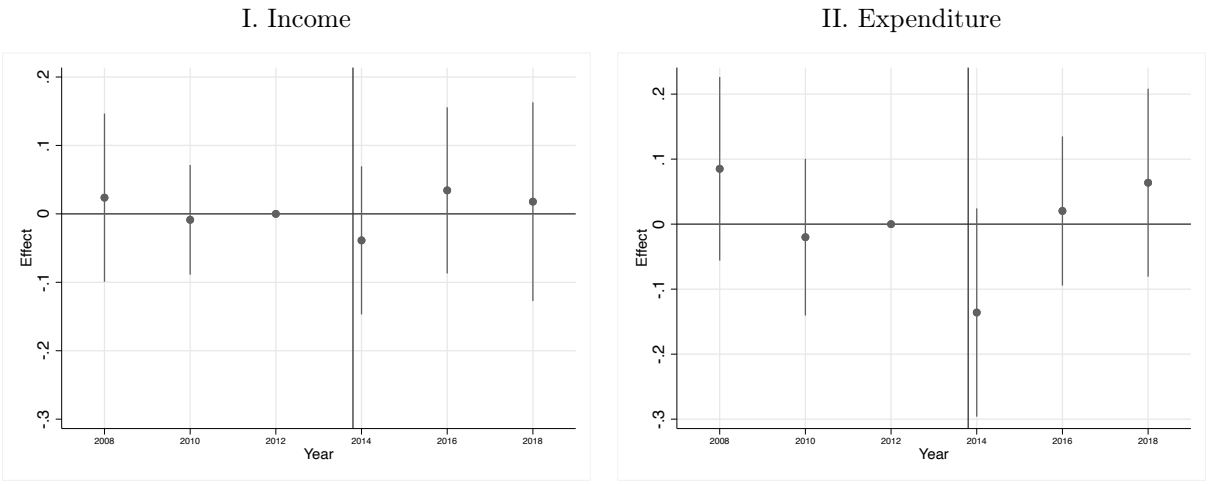
To examine the dynamics of how income and consumption patterns change over time, I use the following specification:

$$Expenditure_h = \sum_{y=2008}^{2018} [\alpha_y Year_h^y \times Pref_m + \beta_y Year_h^y] + X_h^{'}\gamma + \delta_m + \epsilon_h \tag{3.2}$$

I measure the effect of the reform on three outcome variables measured for each household h, living in municipality m surveyed during year y: log-income, log-expenditure and share of expenditure in products purchased abroad, relative to total household expenditure. I use the interaction between the year dummies in which the households were surveyed ($Year_h^y$) and the preferential area indicator ($Pref_m$) to estimate the effect of the VAT increase in border areas. I use three surveys before and after the reform to estimate its impact on household consumption. In all the specifications, I apply household weights and control for the number of household members, as well as the age and gender of the head of household. I include municipality fixed effects and cluster standard errors at the same level.

As shown in Figure 3.8, increasing the VAT rate by 5 percentage points did not have an effect on total household income or expenditure in any of the years after the reform was put in place (see Table C.18 for point estimates). However, the average share of household expenditure devoted to purchases made abroad experienced a significant decline starting in 2014 (-1.1 percentage points), when the new VAT rate was implemented in preferential areas (Figure 3.9). This reduction in the share of purchases made abroad became larger after 2015, a trend that is consistent with the results found for PRI vote shares. Thus, the estimates presented in this section not only provide further evidence of a lagged response in treated areas to the 2014

Figure 3.8: Difference-in-Differences Analysis of the Reform on Income and Expenditure (2008-2018)



Notes: This figure shows the impact of increasing the VAT rate on household log-income and log-expenditure. The reported estimates, represented by dots, correspond to interaction terms between year categorical variables and the *Preferential* variable. The 95% confidence intervals are reported as lines. Controls for the number of household members, gender and age of the head of household are included in the regressions, as well as municipality fixed effects. Household weights are applied to all observations. Standard errors are clustered at the municipality level.
Source: Author's own computations based on data from the National Survey of Household Income and Expenditure (ENIGH), 2008-2018.

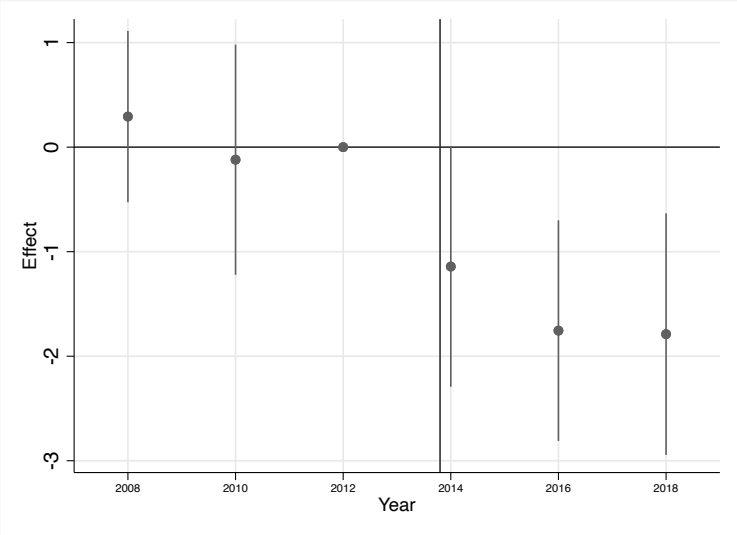
reform, but they also appear to contradict the claim that higher VAT rates encourage shopping abroad.

3.8 Conclusion

This paper analyses the effect of a large increase in the Mexican Value-Added tax rate on the electoral vote share of the political party that put it in place. First, difference-in-differences estimates show that a year and a half into its implementation, the reform did not significantly reduce the PRI vote shares or deputy turnover in treated areas. Additional results suggest that PRI vote shares do not change in the short run either if instead of equalizing the tax rate across the country, a PRI government imposes a larger tax rate in non-preferential areas.

I find a sizeable loss in vote shares (3 percentage points) for the PRI in preferential areas during the 2018 deputy election, which occurred more than 4 years after the VAT change. A similar reduction in electoral support is observed in this period for the PAN, a party that did not vote in favor of the VAT increase in 2013 and that even proposed to reduce it as part of its

Figure 3.9: Difference-in-Differences Analysis of the Reform on Foreign Expenditure (2008-2018)



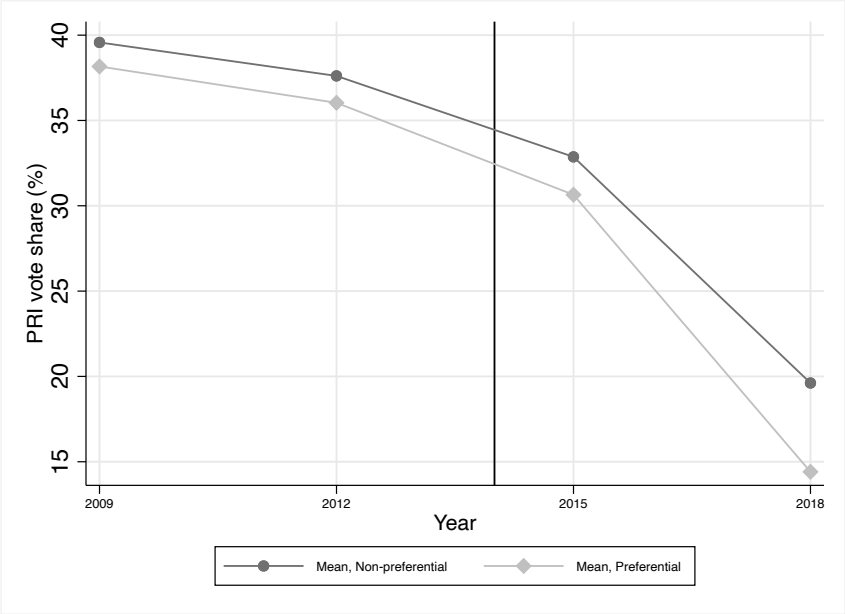
Notes: This figure shows the impact of increasing the VAT rate on the household share of foreign expenditure. The reported estimates, represented by dots, correspond to interaction terms between year categorical variables and the *Preferential* variable. The 95% confidence intervals are reported as lines. Controls for the number of household members, gender and age of the head of household are included in the regressions, as well as municipality fixed effects. Household weights are applied to all observations. Standard errors are clustered at the municipality level.
Source: Author's own computations based on data from the National Survey of Household Income and Expenditure (ENIGH), 2008-2018.

electoral campaign in 2018. Instead, a new political force, the MORENA was able to increase its vote shares in preferential areas at the expense of the other two parties by also proposing a VAT cut in 2018. Thus, lagged responses may occur if opposition parties know how to take advantage of adversely-perceived policy choices.

While further research is needed to identify causal effects on other types of fiscal adjustments and in other political contexts, the estimates found in this study suggest that voters can have a lagged response to changes in the VAT, although the effect may not be large enough to generate an impact on party turnover. Thus, despite previous studies on Latin America showing that voters respond to tax-based fiscal consolidations by replacing incumbents (Ardanaz et al., 2020), I present evidence that changing the VAT only causes a small effect on vote share in treated regions.

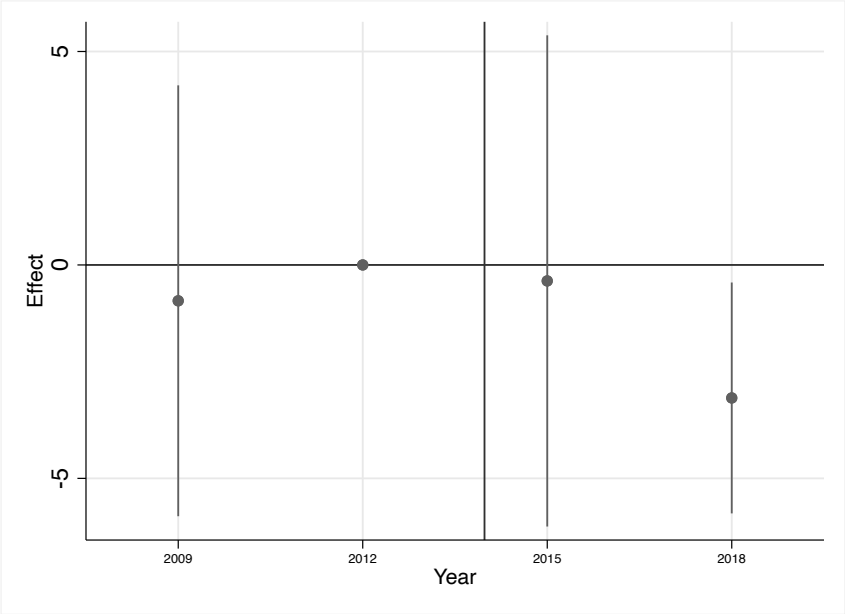
Appendix C: Chapter 3

Figure C.1: PRI Vote Share Averages in First-Past-the-Post System, by Area (2009-2018)



Notes: The figure shows the average PRI vote share by area and electoral year, before and after the VAT rate increase of 5 percentage points in preferential regions during January 2014.
Source: Author's own computations based on data from the National Electoral Institute (INE), 2009-2018.

Figure C.2: Difference-in-Differences Analysis of the Reform on PRI Vote Shares in the First-Past-the-Post System (2009-2018)

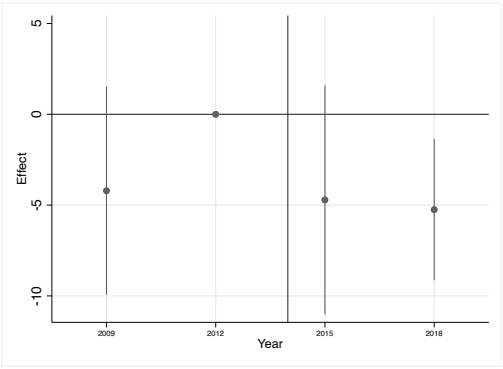


Notes: This figure shows the impact of increasing the VAT rate on PRI vote shares. The reported estimates, represented by dots, correspond to interaction terms between year categorical variables and the *Preferential* variable. The 95% confidence intervals are reported as lines. Controls for the number of competing parties, number of votes and lagged PRI vote shares are included in the regression, as well as municipality fixed effects. Standard errors are clustered at the municipality level.
Source: Author's own computations based on data from the National Electoral Institute (INE), 2009-2018.

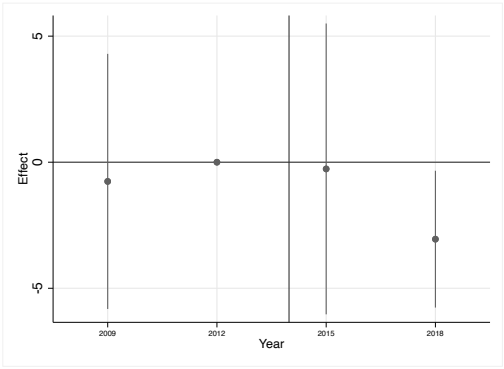
Figure C.3: Difference-in-Differences Analysis of the Reform on PRI Vote Shares in the First-Past-the-Post System, by Area (2009-2018)

(A) Alternative control groups

I. Close Municipalities

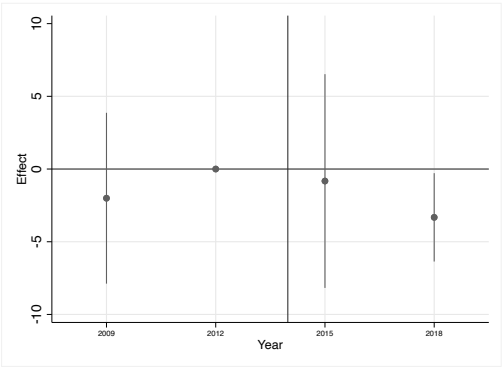


II. Rest of the Country

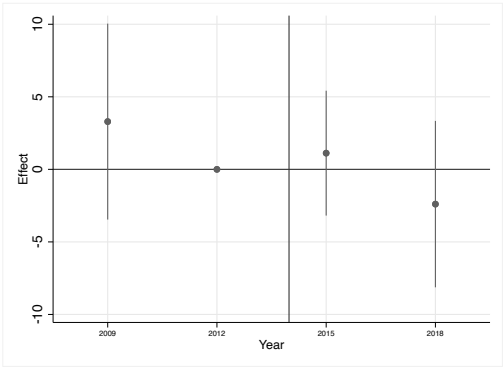


(B) By Border

III. North Border

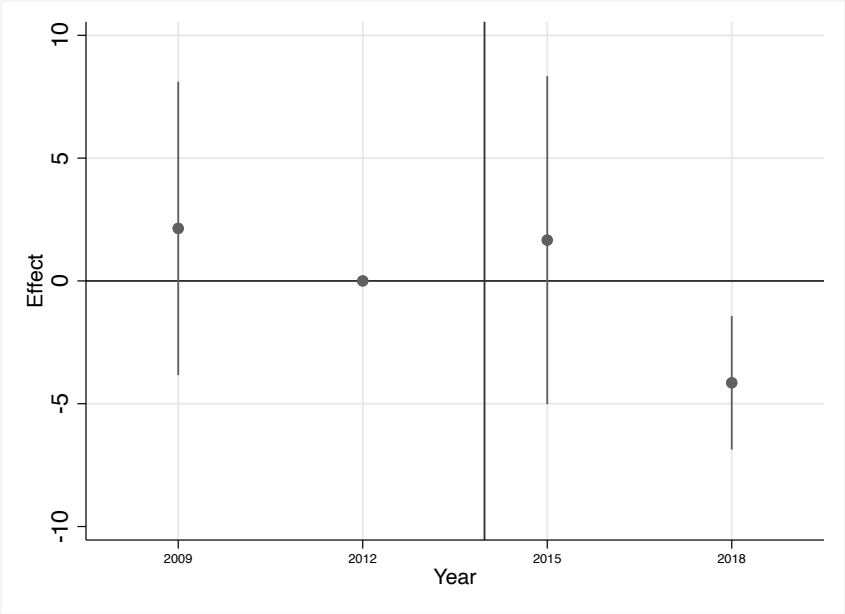


IV. South Border



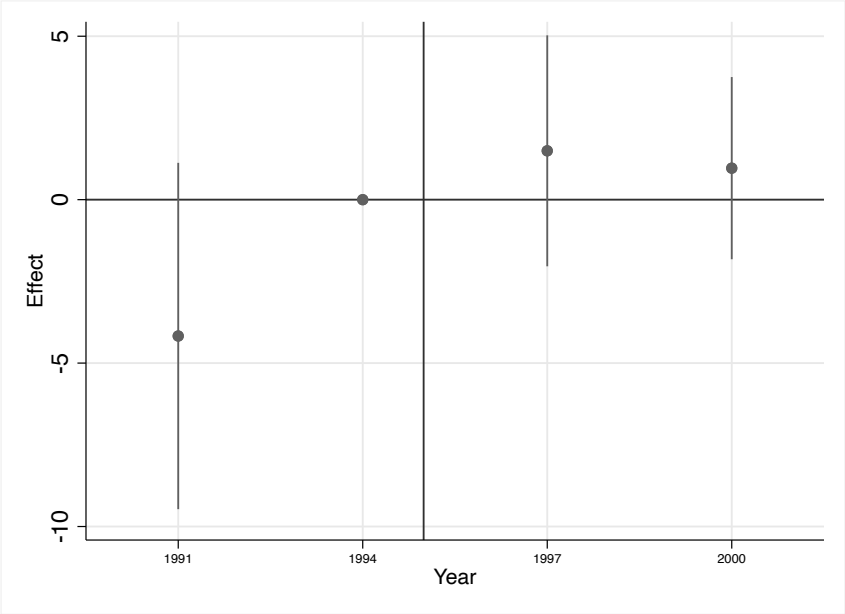
Notes: This figure shows the impact of increasing the VAT rate on PRI vote shares in different geographical areas. Panel (A) shows results using alternative control groups: neighboring and non-neighboring municipalities to the treated regions. Panel (B) shows results separately for northern and southern preferential areas. The reported estimates, represented by dots, correspond to interaction terms between year categorical variables and the *Preferential* variable. The 95% confidence intervals are reported as lines. Controls for the number of competing parties, number of votes and lagged PRI vote shares are included in all regressions, as well as municipality fixed effects. Standard errors are clustered at the municipality level.
Source: Author's own computations based on data from the National Electoral Institute (INE), 2009-2018.

Figure C.4: Difference-in-Differences Analysis of the Reform on PRI Coalition Vote Shares in the First-Past-the-Post System (2009-2018)



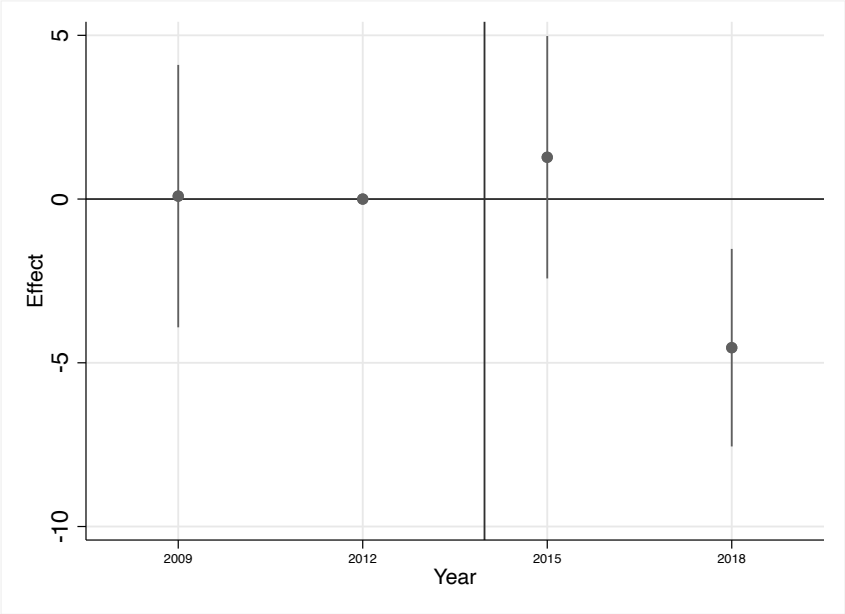
Notes: This figure shows the impact of increasing the VAT rate on PRI coalition vote shares. The reported estimates, represented by dots, correspond to interaction terms between year categorical variables and the *Preferential* variable. The 95% confidence intervals are reported as lines. Controls for the number of competing parties, number of votes and lagged PRI coalition vote shares are included in the regression, as well as municipality fixed effects. Standard errors are clustered at the municipality level.
Source: Author's own computations based on data from the National Electoral Institute (INE), 2009-2018.

Figure C.5: Difference-in-Differences Analysis of the Reform on PRI Vote Shares in the First-Past-the-Post System (1991-2000)



Notes: This figure shows the impact of increasing the VAT rate on PRI vote shares. The reported estimates, represented by dots, correspond to interaction terms between year categorical variables and the *Preferential* variable. The 95% confidence intervals are reported as lines. Controls for the number of competing parties and the number of votes are included in the regression, as well as municipality fixed effects. Standard errors are clustered at the municipality level.
Source: Author's own computations based on data from the National Electoral Institute (INE), 1991-2000.

Figure C.6: Difference-in-Differences Analysis of the Reform on PAN Vote Shares in the First-Past-the-Post System (2009-2018)



Notes: This figure shows the impact of increasing the VAT rate on PAN vote shares. The reported estimates, represented by dots, correspond to interaction terms between year categorical variables and the *Preferential* variable. The 95% confidence intervals are reported as lines. Controls for a lag of the dependent variable, the number of competing parties and the number of votes are included in the regression, as well as municipality fixed effects. Standard errors are clustered at the municipality level.
Source: Author's own computations based on data from the National Electoral Institute (INE), 2009-2018.

Table C.1: Electoral Circumscriptions of Mexico

Number	Head of Circumscription	States
1	Guadalajara	Baja California, Baja California Sur, Chihuahua, Durango, Jalisco, Nayarit, Sinaloa and Sonora.
2	Monterrey	Aguascalientes, Coahuila, Guanajuato, Nuevo León, Querétaro, San Luis Potosí, Tamaulipas and Zacatecas.
3	Xalapa	Campeche, Chiapas, Oaxaca, Quintana Roo, Tabasco, Veracruz and Yucatán.
4	Mexico City	Mexico City, Guerrero, Morelos, Puebla and Tlaxcala.
5	Toluca	Colima, Estado de México, Hidalgo and Michoacán.

Notes: This table includes the states that belong to each of the 5 Mexican electoral circumscriptions (*circunscripciones.*)
Source: Olguín (2016).

Table C.2: Difference-in-Differences Analysis of the Reform on Vote Shares in the First-Past-the-Post System, Federal Deputies Election (2009-2018)

	PRI vote share (1)	PRI vote share (2)	PRI vote share (3)	PRI vote share (4)
Panel A: Effect before and after 2014				
After 2014 x Pref.	-1.745	-1.376	-1.607	-0.939
	(1.316)	(1.243)	(1.287)	(1.402)
After 2014	-11.323***	-11.099***	-5.966***	-6.017***
	(0.357)	(0.338)	(0.405)	(0.425)
Preferential	-1.531			
	(1.385)			
Number of parties			-1.815***	-1.818***
			(0.076)	(0.084)
Number of votes			0.000	0.000***
			(0.000)	(0.000)
Lagged PRI Share				0.274***
				(0.016)
R-squared	0.128	0.497	0.546	0.583
Year 2009 x Pref.	0.171	0.066	0.078	-0.840
	(2.364)	(2.261)	(2.357)	(2.573)
Year 2015 x Pref.	-0.641	-0.502	-0.513	-0.374
	(2.401)	(2.340)	(2.431)	(2.934)
Year 2018 x Pref.	-3.627**	-3.180**	-3.302**	-3.116**
	(1.659)	(1.326)	(1.440)	(1.379)
Year 2009	1.961***	1.937***	0.253	2.555***
	(0.517)	(0.519)	(0.540)	(0.655)
Year 2015	-4.745***	-4.674***	-4.582***	-4.074***
	(0.452)	(0.451)	(0.462)	(0.537)
Year 2018	-17.998***	-18.073***	-15.536***	-14.209***
	(0.495)	(0.432)	(0.533)	(0.511)
Preferential	-1.578			
	(0.990)			
Number of parties			-0.674***	-0.584***
			(0.071)	(0.062)
Number of votes			-0.000***	-0.000***
			(0.000)	(0.000)
Lagged PRI Share				0.254***
				(0.016)
R-squared	0.210	0.577	0.581	0.610
Observations	241,631	241,631	241,631	241,631
Fixed Effects	-	Municipality	Municipality	Municipality
Controls	-	-	Yes	Yes
Clusters	2,448	2,448	2,448	2448

Notes: This table compares PRI vote shares in preferential and non-preferential areas around the January 2014 VAT equalization. Panel (A) shows estimates for the average effect of the reform after it was implemented. Panel (B) presents results for the effects on each year in the sample. Each column shows estimates for a separate difference-in-differences specification. Column (1) reports results without the inclusion of controls or fixed effects. Column (2) shows estimates for regressions with municipality fixed effects. Column (3) reports results with municipality fixed effects and two controls: the number of parties participating in the election and the number of votes within a section. Column (4) additionally includes a lag of the dependent variable. Standard errors are clustered at the municipality level. *** significant at 1%, ** significant at 5%, * significant at 10%.
Source: Author's own computations based on data from the National Electoral Institute (INE), 2009-2018.

Table C.3: Difference-in-Differences Analysis of the Reform on PRI Vote Shares in the Proportional Representation System, Alternative Control Groups (2009-2018)

	Close municipalities		Rest of the Country	
	PRI vote share	PRI vote share	PRI vote share	PRI vote share
	(1)	(2)	(3)	(4)
Year 2009 x Pref.	-3.738	-4.181	0.160	-0.735
	(2.681)	(2.890)	(2.258)	(2.570)
Year 2015 x Pref.	-3.784	-4.411	-0.613	-0.502
	(2.899)	(3.211)	(2.394)	(2.964)
Year 2018 x Pref.	-5.193**	-5.249***	-3.253**	-3.173**
	(2.023)	(1.973)	(1.326)	(1.361)
Year 2009	5.764***	6.550***	1.866***	2.530***
	(1.537)	(1.761)	(0.530)	(0.671)
Year 2015	-1.083	-0.188	-4.254***	-3.738***
	(1.700)	(1.841)	(0.473)	(0.565)
Year 2028	-15.929***	-14.423***	-17.869***	-14.267***
	(1.585)	(1.673)	(0.427)	(0.503)
Number of parties		-0.102		-0.557***
		(0.153)		(0.061)
Number of votes		-0.001***		-0.000***
		(0.000)		(0.000)
Lagged PRI Share		0.159***		0.253***
		(0.050)		(0.016)
Observations	26,452	26,452	236,758	236,758
R-squared	0.641	0.652	0.576	0.608
Fixed Effects	Municipality	Municipality	Municipality	Municipality
Controls	-	Yes	-	Yes
Clusters	162	162	2,367	2,367

Notes: This table compares PRI vote shares in preferential and non-preferential areas around the January 2014 VAT equalization. Each column shows estimates for a separate difference-in-differences specification. Columns (1)-(2) report results when the control group only includes municipalities geographically close to the treated areas. Columns (3)-(4) shows estimates for regressions where the control group are municipalities that are not geographically adjacent to the treated areas. Columns (1) and (3) present results for regressions that do not include controls. Columns (2) and (4) show estimates for regressions with three controls: a lag of the dependent variable, the number of parties participating in the election and the number of votes within a section. All regressions include municipality fixed effects. Standard errors are clustered at the municipality level. *** significant at 1%, ** significant at 5%, * significant at 10%.

Source: Author's own computations based on data from the National Electoral Institute (INE), 2009-2018.

Table C.4: Difference-in-Differences Analysis of the Reform on Vote Shares in the First-Past-the-Post System, Alternative Control Groups (2009-2018)

	Close municipalities		Rest of the Country	
	PRI vote share	PRI vote share	PRI vote share	PRI vote share
	(1)	(2)	(3)	(4)
Year 2009 x Pref.	-3.751	-4.206	0.154	-0.761
	(2.686)	(2.899)	(2.263)	(2.580)
Year 2015 x Pref.	-4.078	-4.712	-0.420	-0.266
	(2.860)	(3.191)	(2.341)	(2.941)
Year 2018 x Pref.	-5.311***	-5.251***	-3.130**	-3.052**
	(2.025)	(1.969)	(1.328)	(1.383)
Year 2009	5.754***	6.667***	1.848***	2.466***
	(1.538)	(1.754)	(0.530)	(0.671)
Year 2015	-1.097	-0.182	-4.756***	-4.171***
	(1.702)	(1.848)	(0.458)	(0.547)
Year 2018	-15.941***	-14.596***	-18.123***	-14.221***
	(1.589)	(1.703)	(0.440)	(0.519)
Number of parties		-0.060		-0.594***
		(0.152)		(0.063)
Number of votes		-0.001***		-0.000**
		(0.000)		(0.000)
Lagged PRI Share		0.165***		0.257***
		(0.049)		(0.016)
Observations	26,447	26,447	236,712	236,712
R-squared	0.642	0.654	0.574	0.608
Controls	-	Municipality	-	Municipality
Clusters	162	162	2,367	2,367

Notes: This table compares PRI vote shares in preferential and non-preferential areas around the January 2014 VAT equalization. Each column shows estimates for a separate difference-in-differences specification. Columns (1)-(2) report results when the control group only includes municipalities geographically close to the treated areas. Columns (3)-(4) shows estimates for regressions where the control group are municipalities that are not geographically adjacent to the treated areas. Columns (1) and (3) present results for regressions that do not include controls. Columns (2) and (4) show estimates for regressions with three controls: a lag of the dependent variable, the number of parties participating in the election and the number of votes within a section. All regressions include municipality fixed effects. Standard errors are clustered at the municipality level. *** significant at 1%, ** significant at 5%, * significant at 10%.

Source: Author's own computations based on data from the National Electoral Institute (INE), 2009-2018.

Table C.5: Difference-in-Differences Analysis of the Reform on PRI Vote Shares in the Proportional Representation System, North and South (2009-2018)

	North Border		South Border	
	PRI vote share	PRI vote share	PRI vote share	PRI vote share
	(1)	(2)	(3)	(4)
Year 2009 x Pref.	-1.413	-1.980	5.640**	3.306
	(2.610)	(2.984)	(2.463)	(3.427)
Year 2015 x Pref.	-0.814	-1.137	-0.291	1.139
	(3.081)	(3.771)	(1.753)	(2.130)
Year 2018 x Pref.	-2.865*	-3.353**	-4.996*	-2.853
	(1.567)	(1.533)	(2.721)	(2.937)
Year 2009	1.934***	2.603***	1.888***	2.514***
	(0.525)	(0.667)	(0.523)	(0.666)
Year 2015	-4.170***	-3.639***	-4.265***	-3.731***
	(0.471)	(0.562)	(0.468)	(0.560)
Year 2018	-17.846***	-14.256***	-17.847***	-14.099***
	(0.424)	(0.502)	(0.423)	(0.500)
Number of parties		-0.556***		-0.584***
		(0.062)		(0.060)
Number of votes		-0.000***		-0.000***
		(0.000)		(0.000)
Lagged PRI Share		0.252***		0.258***
		(0.016)		(0.016)
Observations	233,839	233,839	223,063	223,063
R-squared	0.577	0.609	0.572	0.606
Fixed Effects	Municipality	Municipality	Municipality	Municipality
Controls	-	Yes	-	Yes
Clusters	2,377	2,377	2,359	2,359

Notes: This table compares PRI vote shares in preferential and non-preferential areas around the January 2014 VAT equalization. Each column shows estimates for a separate difference-in-differences specification. Columns (1)-(2) report results for northern preferential areas. Columns (3)-(4) shows estimates for southern preferential areas. Columns (1) and (3) present results for regressions that do not include controls. Columns (2) and (4) show estimates for regressions with three controls: a lag of the dependent variable, the number of parties participating in the election and the number of votes within a section. All regressions include municipality fixed effects. Standard errors are clustered at the municipality level. *** significant at 1%, ** significant at 5%, * significant at 10%.
Source: Author's own computations based on data from the National Electoral Institute (INE), 2009-2018.

Table C.6: Difference-in-Differences Analysis of the Reform on Vote Shares in the First-Past-the-Post System, North and South (2009-2018)

	North Border		South Border	
	PRI vote share	PRI vote share	PRI vote share	PRI vote share
	(1)	(2)	(3)	(4)
Year 2009 x Pref.	-1.424	-2.006	5.666**	3.292
	(2.615)	(2.995)	(2.477)	(3.443)
Year 2015 x Pref.	-0.541	-0.825	-0.366	1.118
	(3.017)	(3.748)	(1.816)	(2.194)
Year 2018 x Pref.	-2.794*	-3.322**	-4.679*	-2.395
	(1.555)	(1.549)	(2.729)	(2.924)
Year 2009	1.916***	2.532***	1.870***	2.433***
	(0.526)	(0.667)	(0.523)	(0.666)
Year 2015	-4.667***	-4.066***	-4.761***	-4.153***
	(0.457)	(0.545)	(0.453)	(0.542)
Year 2018	-18.098***	-14.201***	-18.097***	-14.019***
	(0.437)	(0.518)	(0.435)	(0.516)
Number of parties		-0.596***		-0.629***
		(0.064)		(0.063)
Number of votes		-0.000***		-0.000**
		(0.000)		(0.000)
Lagged PRI Share		0.256***		0.261***
		(0.017)		(0.017)
Observations	233,795	233,795	223,020	223,020
R-squared	0.576	0.610	0.571	0.607
Fixed Effects	Municipality	Municipality	Municipality	Municipality
Controls	-	Yes	-	Yes
Clusters	2,377	2,377	2,359	2,359

Notes: This table compares PRI vote shares in preferential and non-preferential areas around the January 2014 VAT equalization. Each column shows estimates for a separate difference-in-differences specification. Columns (1)-(2) report results for northern preferential areas. Columns (3)-(4) shows estimates for southern preferential areas. Columns (1) and (3) present results for regressions that do not include controls. Columns (2) and (4) show estimates for regressions with three controls: a lag of the dependent variable, the number of parties participating in the election and the number of votes within a section. All regressions include municipality fixed effects. Standard errors are clustered at the municipality level. *** significant at 1%, ** significant at 5%, * significant at 10%.
Source: Author's own computations based on data from the National Electoral Institute (INE), 2009-2018.

Table C.7: Difference-in-Differences Analysis of the Reform on PRI Coalition Vote Shares in the Proportional Representation System (2009-2018)

	PRI coalition vote share (1)	PRI coalition vote share (2)	PRI coalition vote share (3)	PRI coalition vote share (4)
Year 2009 x Pref.	2.154	2.072	2.088	2.147
	(2.633)	(2.587)	(2.660)	(3.031)
Year 2015 x Pref.	0.643	0.867	0.812	1.385
	(3.064)	(3.004)	(3.063)	(3.424)
Year 2018 x Pref.	-6.509***	-4.663***	-4.769***	-4.243***
	(1.682)	(1.478)	(1.598)	(1.409)
Year 2009	4.519***	4.499***	2.719***	6.318***
	(0.511)	(0.513)	(0.547)	(0.717)
Year 2015	-1.801***	-1.735***	-1.692***	-0.609
	(0.498)	(0.500)	(0.519)	(0.555)
Year 2018	-14.228***	-14.288***	-11.700***	-10.373***
	(0.492)	(0.430)	(0.522)	(0.508)
Preferential	0.323			
	(1.146)			
Number of parties			-0.714***	-0.660***
			(0.070)	(0.063)
Number of votes			-0.000***	-0.000***
			(0.000)	(0.000)
Lagged PRI Coalition Share				0.238***
				(0.016)
Observations	241,677	241,677	241,677	241,677
R-squared	0.176	0.508	0.513	0.540
Fixed Effects	-	Municipality	Municipality	Municipality
Controls	-	-	Yes	Yes
Clusters	2,448	2,448	2,448	2,448

Notes: This table compares PRI coalition vote shares in preferential and non-preferential areas around the January 2014 VAT equalization. Each column shows estimates for a separate difference-in-differences specification. Column (1) reports results without the inclusion of controls or fixed effects. Column (2) shows estimates for regressions with municipality fixed effects. Column (3) reports results with municipality fixed effects and two controls: the number of parties participating in the election and the number of votes within a section. Column (4) additionally includes a lag of the dependent variable. Standard errors are clustered at the municipality level. *** significant at 1%, ** significant at 5%, * significant at 10%.
Source: Author's own computations based on data from the National Electoral Institute (INE), 2009-2018.

Table C.8: Difference-in-Differences Analysis of the Reform on PRI Coalition Vote Shares in the First-Past-the-Post System (2009-2018)

	PRI coalition vote share (1)	PRI coalition vote share (2)	PRI coalition vote share (3)	PRI coalition vote share (4)
Year 2009 x Pref.	2.143	2.071	2.087	2.139
	(2.636)	(2.594)	(2.671)	(3.046)
Year 2015 x Pref.	0.854	1.084	1.085	1.662
	(3.017)	(2.956)	(3.035)	(3.405)
Year 2018 x Pref.	-6.485***	-4.566***	-4.696***	-4.148***
	(1.686)	(1.461)	(1.603)	(1.386)
Year 2009	4.500***	4.479***	2.567***	6.248***
	(0.512)	(0.513)	(0.547)	(0.720)
Year 2015	-2.353***	-2.291***	-2.157***	-1.072**
	(0.484)	(0.485)	(0.500)	(0.532)
Year 2018	-14.453***	-14.602***	-11.676***	-10.311***
	(0.504)	(0.448)	(0.548)	(0.525)
Preferential	0.337			
	(1.149)			
Number of parties			-0.781***	-0.712***
			(0.074)	(0.067)
Number of votes			-0.000***	-0.000**
			(0.000)	(0.000)
Lagged PRI Coalition Share				0.241***
				(0.016)
Observations	241,631	241,631	241,631	241,631
R-squared	0.181	0.507	0.512	0.540
Fixed Effects	-	Municipality	Municipality	Municipality
Controls	-	-	Yes	Yes
Clusters	2,448	2,448	2,448	2,448

Notes: This table compares PRI coalition vote shares in preferential and non-preferential areas around the January 2014 VAT equalization. Each column shows estimates for a separate difference-in-differences specification. Column (1) reports results without the inclusion of controls or fixed effects. Column (2) shows estimates for regressions with municipality fixed effects. Column (3) reports results with municipality fixed effects and two controls: the number of parties participating in the election and the number of votes within a section. Column (4) additionally includes a lag of the dependent variable. Standard errors are clustered at the municipality level. *** significant at 1%, ** significant at 5%, * significant at 10%.
Source: Author's own computations based on data from the National Electoral Institute (INE), 2009-2018.

Table C.9: Difference-in-Differences Analysis of the Reform on Vote Shares in the First-Past-the-Post System, VAT vote and 2012 Deputies (2009-2018)

	VAT Vote		PRI Deputy in 2012		VAT Vote & PRI Deputy in 2012	
	PRI vote share (1)	PRI vote share (2)	PRI vote share (3)	PRI vote share (4)	PRI vote share (5)	PRI vote share (6)
Panel A: Choices by the Deputies within a State						
Year 2009 x Pref.	8.303***	-7.222**	7.474**	-6.561**	7.666**	-6.857**
	(3.081)	(2.927)	(3.091)	(2.925)	(3.079)	(2.933)
Year 2015 x Pref.	7.958***	-6.337	7.555***	-6.072	7.550***	-6.112
	(2.590)	(3.937)	(2.593)	(3.937)	(2.581)	(3.944)
Year 2018 x Pref.	-3.440*	-2.513	-3.252*	-2.815	-3.645*	-2.244
	(1.900)	(1.879)	(1.899)	(1.880)	(1.893)	(1.890)
Observations	103,577	138,054	100,596	141,035	107,676	133,955
R-squared	0.590	0.615	0.609	0.604	0.603	0.612
VAT vote	Below Median	Above Median	- -	- -	Rest	Above Median
Deputies in 2012	- -		Below PRI Median	Above PRI Median	Rest	Above Median
Clusters	1,147	1,301	1,195	1,253	1,253	1,195
Panel B: Individual Choices in Single-Member Districts						
Year 2009 x Pref.	1.831	-3.424	1.241	-2.986	2.070	-3.715
	(3.297)	(2.546)	(2.947)	(2.735)	(2.934)	(2.707)
Year 2015 x Pref.	3.655	-3.369	2.748	-3.131	3.134	-3.507
	(2.873)	(3.664)	(2.857)	(3.683)	(2.797)	(3.731)
Year 2018 x Pref.	-3.198*	-3.964**	-3.051*	-3.793***	-2.604	-3.899***
	(1.697)	(1.618)	(1.696)	(1.367)	(1.686)	(1.375)
Observations	72,883	168,748	105,738	135,893	110,781	130,850
R-squared	0.582	0.620	0.600	0.576	0.583	0.591
VAT vote	Negative	Positive	-	-	Rest	Positive
Deputy in 2012	-	-	Non-PRI	PRI	Rest	PRI
Clusters	662	1,814	1,022	1,448	1,062	1,408

Notes: This table compares PRI vote shares in preferential and non-preferential areas around the January 2014 VAT equalization. Each column shows estimates for a separate difference-in-differences specification. Panel A shows results by splitting the sample according to states. Panel B shows results splitting the sample according to districts. Columns (1)-(2) report results according to the deputies that voted in favor of the VAT change within a state or district. Columns (3)-(4) shows estimates according to the PRI deputies elected within a state or district in 2012. Column (6) presents results for states or districts with PRI deputies who also voted in favor of the VAT change in 2012, while Column (5) shows estimates for the remaining states or districts. All regressions include municipality fixed effects and three controls: a lag of the dependent variable, the number of parties participating in the election and the number of votes within a section. Standard errors are clustered at the municipality level. *** significant at 1%, ** significant at 5%, * significant at 10%.
Source: Author's own computations based on data from the National Electoral Institute (INE), 2009-2018.

Table C.10: Difference-in-Differences Analysis of the Reform on PRI Vote Shares, PRI Incumbency (2009-2018)

	Proportional Representation		First Past the Post	
	PRI vote share (1)	PRI vote share (2)	PRI vote share (3)	PRI vote share (4)
Year 2009 x Pref. x Share of PRI Deputies	-0.097	-0.094		
	(0.106)	(0.124)		
Year 2015 x Pref. x Share of PRI Deputies	-0.055	-0.110		
	(0.091)	(0.116)		
Year 2018 x Pref. x Share of PRI Deputies	0.118	-0.009		
	(0.124)	(0.140)		
Year 2009 x Pref. x Elected PRI Deputy			6.291*	5.084
			(3.512)	(3.791)
Year 2015 x Pref. x Elected PRI Deputy			2.491	3.472
			(2.651)	(3.221)
Year 2018 x Pref. x Elected PRI Deputy			0.718	0.461
			(3.377)	(3.514)
Year 2009 x Pref.	1.122	0.569	-2.574	-2.897
	(3.637)	(4.366)	(2.207)	(2.546)
Year 2015 x Pref.	-0.145	1.381	-1.199	-1.276
	(3.231)	(4.277)	(2.404)	(2.805)
Year 2018 x Pref.	-6.977*	-3.551	-4.146*	-4.037*
	(4.007)	(4.689)	(2.181)	(2.341)
Observations	241,677	241,677	241,631	241,631
R-squared	0.584	0.619	0.587	0.628
Fixed Effects	Municipality	Municipality	Municipality	Municipality
Controls	-	Yes	-	Yes
Clusters	2,448	2,448	2,448	2,448

Notes: This table compares PRI vote shares in preferential and non-preferential areas around the January 2014 VAT equalization. Each column shows estimates for a separate difference-in-differences specification. Columns (1)-(2) show estimates for deputies elected under the proportional representation system. The estimates correspond to interaction terms between year categorical variables, the *Preferential* variable and the share of PRI deputies within a circumscription and state elected in the previous period. Columns (3)-(4) report results for deputies elected under the first-past-the-post system. The estimates correspond to interaction terms between year categorical variables, the *Preferential* variable and an indicator for a PRI deputy elected in the district in the previous period. Columns (1) and (3) present results for regressions that do not include controls. Columns (2) and (4) show estimates for regressions with three controls: a lag of the dependent variable, the number of parties participating in the election and the number of votes within a section. All regressions include municipality fixed effects. Standard errors are clustered at the municipality level. *** significant at 1%, ** significant at 5%, * significant at 10%.
Source: Author's own computations based on data from the National Electoral Institute (INE), 2009-2018.

Table C.11: Difference-in-Differences Analysis of the Reform on PRI Vote Shares in the Proportional Representation System, with Additional Observations (2009-2018)

	PRI vote share (1)	PRI vote share (2)	PRI vote share (3)
Panel A: Effect before and after 2014			
After 2014 x Pref.	-1.936*	-1.907*	-2.334**
	(1.063)	(1.061)	(1.110)
After 2014	-12.132***	-12.024***	-6.336***
	(0.338)	(0.338)	(0.440)
Preferential	-1.616		
	(1.355)		
Number of parties			-1.878***
			(0.078)
Number of votes			0.000**
			(0.000)
R-squared	0.145	0.495	0.551
Panel B: Effect by year			
Year 2009 x Pref.	0.655	0.440	0.488
	(2.227)	(2.306)	(2.385)
Year 2015 x Pref.	-0.062	-0.301	-0.315
	(2.373)	(2.354)	(2.415)
Year 2018 x Pref.	-3.448***	-3.461***	-3.617***
	(1.311)	(1.315)	(1.362)
Year 2009	1.967***	1.967***	0.445
	(0.517)	(0.521)	(0.542)
Year 2015	-4.371***	-4.258***	-4.223***
	(0.465)	(0.466)	(0.481)
Year 2018	-17.901***	-17.809***	-15.596***
	(0.406)	(0.405)	(0.492)
Preferential	-1.869*		
	(0.990)		
Number of parties			-0.609***
			(0.064)
Number of votes			-0.000***
			(0.000)
R-squared	0.239	0.589	0.593
Observations	266,153	266,153	266,153
Fixed Effects	-	Municipality	Municipality
Controls	-	-	Yes
Clusters	2,463	2,463	2,463

Notes: This table compares PRI vote shares in preferential and non-preferential areas around the January 2014 VAT equalization. Panel (A) shows estimates for the average effect of the reform after it was implemented. Panel (B) presents results for the effects on each year in the sample. Each column shows estimates for a separate difference-in-differences specification. Column (1) reports results without the inclusion of controls or fixed effects. Column (2) shows estimates for regressions with municipality fixed effects. Column (3) reports results with municipality fixed effects and two controls: the number of parties participating in the election and the number of votes within a section. Standard errors are clustered at the municipality level. *** significant at 1%, ** significant at 5%, * significant at 10%.
Source: Author's own computations based on data from the National Electoral Institute (INE), 2009-2018.

Table C.12: Difference-in-Differences Analysis of the Reform on Vote Shares in the First-Past-the-Post System, with Additional Observations (2009-2018)

	PRI vote share (1)	PRI vote share (2)	PRI vote share (3)
Panel A: Effect before and after 2014			
After 2014 x Pref.	-1.968**	-1.948*	-2.248**
	(0.996)	(0.995)	(1.104)
After 2014	-12.448***	-12.340***	-6.490***
	(0.343)	(0.343)	(0.412)
Preferential	-1.609		
	(1.358)		
Number of parties			-1.872***
			(0.073)
Number of votes			0.000**
			(0.000)
R-squared	0.145	0.495	0.551
Panel B: Effect by year			
Year 2009 x Pref.	0.645	0.429	0.481
	(2.227)	(2.305)	(2.390)
Year 2015 x Pref.	0.138	-0.109	-0.080
	(2.326)	(2.305)	(2.388)
Year 2018 x Pref.	-3.721***	-3.747***	-3.859***
	(1.224)	(1.227)	(1.324)
Year 2009	1.950***	1.948***	0.338
	(0.517)	(0.521)	(0.541)
Year 2015	-4.854***	-4.743***	-4.638***
	(0.452)	(0.453)	(0.462)
Year 2018	-18.065***	-17.972***	-15.526***
	(0.417)	(0.416)	(0.518)
Preferential	-1.858*		
	(0.994)		
Number of parties			-0.654***
			(0.066)
Number of votes			-0.000***
			(0.000)
R-squared	0.239	0.589	0.593
Observations	266,153	266,153	266,153
Fixed Effects	-	Municipality	Municipality
Controls	-	-	Yes
Clusters	2,463	2,463	2,463

Notes: This table compares PRI vote shares in preferential and non-preferential areas around the January 2014 VAT equalization. Panel (A) shows estimates for the average effect of the reform after it was implemented. Panel (B) presents results for the effects on each year in the sample. Each column shows estimates for a separate difference-in-differences specification. Column (1) reports results without the inclusion of controls or fixed effects. Column (2) shows estimates for regressions with municipality fixed effects. Column (3) reports results with municipality fixed effects and two controls: the number of parties participating in the election and the number of votes within a section. Standard errors are clustered at the municipality level. *** significant at 1%, ** significant at 5%, * significant at 10%.
Source: Author's own computations based on data from the National Electoral Institute (INE), 2009-2018.

Table C.13: Difference-in-Differences Analysis of the Reform on PRI Vote Shares in the Proportional Representation System (1991-2000)

	PRI vote share (1)	PRI vote share (2)	PRI vote share (3)
Year 1991 x Pref.	-3.610	-3.737	-4.150
	(2.898)	(2.953)	(2.698)
Year 1997 x Pref	1.851	1.894	1.536
	(1.821)	(1.861)	(1.799)
Year 2000 x Pref	0.956	0.874	0.962
	(1.429)	(1.438)	(1.418)
Year 1991	11.334***	11.266***	10.245***
	(0.469)	(0.469)	(0.463)
Year 1997	-10.606***	-10.601***	-12.139***
	(0.361)	(0.366)	(0.377)
Year 2000	-11.445***	-11.470***	-14.382***
	(0.540)	(0.555)	(0.650)
Preferential	-1.132		
	(1.247)		
Number of parties			-1.503***
			(0.081)
Sum of votes			-0.005***
			(0.000)
Observations	251,029	251,029	251,029
R-squared	0.212	0.582	0.598
Fixed Effects	-	Municipality	Municipality
Controls	-	-	Yes
Clusters	2,434	2,434	2,434

Notes: This table compares PRI vote shares in preferential and non-preferential areas around the 1995 VAT increase. Each column shows estimates for a separate difference-in-differences specification. Column (1) reports results without the inclusion of controls or fixed effects. Column (2) shows estimates for regressions with municipality fixed effects. Column (3) reports results with municipality fixed effects and two controls: the number of parties participating in the election and the number of votes within a section. Standard errors are clustered at the municipality level. *** significant at 1%, ** significant at 5%, * significant at 10%.
Source: Author's own computations based on data from the National Electoral Institute (INE), 1991-2000.

Table C.14: Difference-in-Differences Analysis of the Reform on PRI Vote Shares in the First-Past-the-Post System (1991-2000)

	PRI vote share (1)	PRI vote share (2)	PRI vote share (3)
Year 1991 x Pref.	-3.624	-3.755	-4.171
	(2.901)	(2.957)	(2.702)
Year 1997 x Pref.	1.889	1.883	1.495
	(1.826)	(1.864)	(1.802)
Year 2000 x Pref.	0.968	0.882	0.964
	(1.433)	(1.442)	(1.421)
Year 1991	11.327***	11.260***	10.228***
	(0.470)	(0.470)	(0.464)
Year 1997	-10.620***	-10.605***	-12.157***
	(0.362)	(0.367)	(0.376)
Year 2000	-11.433***	-11.456***	-14.372***
	(0.540)	(0.555)	(0.650)
Preferential	-1.133		
	(1.250)		
Number of parties			-1.502***
			(0.081)
Number of votes			-0.005***
			(0.000)
Observations	250,847	250,847	250,847
R-squared	0.212	0.582	0.598
Fixed Effects	-	Municipality	Municipality
Controls	-	-	Yes
Clusters	2,434	2,434	2,434

Notes: This table compares PRI vote shares in preferential and non-preferential areas around the 1995 VAT increase. Each column shows estimates for a separate difference-in-differences specification. Column (1) reports results without the inclusion of controls or fixed effects. Column (2) shows estimates for regressions with municipality fixed effects. Column (3) reports results with municipality fixed effects and two controls: the number of parties participating in the election and the number of votes within a section. Standard errors are clustered at the municipality level. *** significant at 1%, ** significant at 5%, * significant at 10%.
Source: Author's own computations based on data from the National Electoral Institute (INE), 1991-2000.

Table C.15: Difference-in-Differences Analysis of the Reform on PAN Vote Shares in the Proportional Representation System, Federal Deputies Election (2009-2018)

	PAN vote share (1)	PAN vote share (2)	PAN vote share (3)	PAN vote share (4)
Year 2009 x Pref.	0.374	-0.086	-0.058	0.066
	(1.900)	(1.905)	(1.830)	(2.046)
Year 2015 x Pref.	0.879	0.989	1.026	1.177
	(1.540)	(1.502)	(1.477)	(1.879)
Year 2018 x Pref.	-2.407*	-4.088***	-4.114***	-4.654***
	(1.334)	(1.443)	(1.481)	(1.512)
Year 2009	1.731***	1.738***	0.379	-1.008*
	(0.426)	(0.428)	(0.414)	(0.555)
Year 2015	-5.532***	-5.624***	-5.428***	-4.867***
	(0.449)	(0.453)	(0.465)	(0.514)
Year 2018	-7.200***	-7.464***	-5.236***	-2.896***
	(0.510)	(0.482)	(0.512)	(0.543)
Preferential	3.074*			
	(1.701)			
Number of parties			-0.633***	-0.621***
			(0.073)	(0.075)
Number of votes			0.001***	0.001***
			(0.000)	(0.000)
Lagged PAN Share				0.328***
				(0.032)
Observations	241,677	241,677	241,677	241,677
R-squared	0.059	0.573	0.575	0.623
Fixed Effects	-	Municipality	Municipality	Municipality
Controls	-	-	Yes	Yes
Clusters	2,448	2,448	2,448	2,448

Notes: This table compares PAN vote shares in preferential and non-preferential areas around the January 2014 VAT equalization. Each column shows estimates for a separate difference-in-differences specification. Column (1) reports results without the inclusion of controls or fixed effects. Column (2) shows estimates for regressions with municipality fixed effects. Column (3) reports results with municipality fixed effects and two controls: the number of parties participating in the election and the number of votes within a section. Column (4) additionally includes a lag of the dependent variable. Standard errors are clustered at the municipality level. *** significant at 1%, ** significant at 5%, * significant at 10%.
Source: Author's own computations based on data from the National Electoral Institute (INE), 2009-2018.

Table C.16: Difference-in-Differences Analysis of the Reform on PAN Vote Shares in the First-Past-the-Post System, Federal Deputies Election (2009-2018)

	PAN vote share (1)	PAN vote share (2)	PAN vote share (3)	PAN vote share (4)
Year 2009 x Pref.	0.386	-0.065	-0.041	0.090
	(1.901)	(1.904)	(1.828)	(2.043)
Year 2015 x Pref.	0.943	1.050	1.124	1.277
	(1.560)	(1.519)	(1.486)	(1.887)
Year 2018 x Pref.	-2.307*	-3.970***	-4.017***	-4.539***
	(1.304)	(1.459)	(1.498)	(1.538)
Year 2009	1.738***	1.746***	0.340	-1.041*
	(0.427)	(0.428)	(0.408)	(0.553)
Year 2015	-5.724***	-5.816***	-5.567***	-5.004***
	(0.462)	(0.466)	(0.477)	(0.520)
Year 2018	-7.381***	-7.677***	-5.294***	-2.919***
	(0.523)	(0.498)	(0.540)	(0.555)
Preferential	3.059*			
	(1.700)			
Number of parties			-0.653***	-0.642***
			(0.075)	(0.076)
Number of votes			0.001***	0.001***
			(0.000)	(0.000)
Lagged PAN Share				0.328***
				(0.032)
Observations	241,631	241,631	241,631	241,631
R-squared	0.062	0.573	0.576	0.624
Fixed Effects	-	Municipality	Municipality	Municipality
Controls	-	-	Yes	Yes
Clusters	2,448	2,448	2,448	2,448

Notes: This table compares PAN vote shares in preferential and non-preferential areas around the January 2014 VAT equalization. Each column shows estimates for a separate difference-in-differences specification. Column (1) reports results without the inclusion of controls or fixed effects. Column (2) shows estimates for regressions with municipality fixed effects. Column (3) reports results with municipality fixed effects and two controls: the number of parties participating in the election and the number of votes within a section. Column (4) additionally includes a lag of the dependent variable. Standard errors are clustered at the municipality level. *** significant at 1%, ** significant at 5%, * significant at 10%.
Source: Author's own computations based on data from the National Electoral Institute (INE), 2009-2018.

Table C.17: Difference-in-Differences Analysis of the Reform on MORENA and PAN Vote Shares in the First-Past-the-Post System (2015-2018)

	MORENA vote share (1)	MORENA vote share (2)	PAN vote share (3)	PAN vote share (4)
Year 2018 x Pref.	8.458***	8.630***	-5.267**	-5.372**
	(1.518)	(1.539)	(2.070)	(2.129)
Year 2018	28.072***	24.995***	-2.005***	-0.498
	(0.628)	(0.672)	(0.571)	(0.552)
Number of parties		0.853***		-0.466***
		(0.078)		(0.085)
Number of votes		0.001***		0.000***
		(0.000)		(0.000)
Observations	112,888	112,888	112,888	112,888
R-squared	0.816	0.822	0.655	0.657
Fixed Effects	Municipality	Municipality	Municipality	Municipality
Controls	-	Yes	-	Yes
Clusters	2,426	2,426	2,426	2,426

Notes: This table compares vote shares in preferential and non-preferential areas around the proposal of a VAT reduction in 2018. Each column shows estimates for a separate difference-in-differences specification. Columns (1)-(2) report results for MORENA vote shares. Columns (3)-(4) shows estimates for PAN vote shares. Columns (1) and (3) present results for regressions that do not include controls. Columns (2) and (4) show estimates for regressions with two controls: the number of parties participating in the election and the number of votes within a section. All regressions include municipality fixed effects. Standard errors are clustered at the municipality level. *** significant at 1%, ** significant at 5%, * significant at 10%.
Source: Author's own computations based on data from the National Electoral Institute (INE), 2015-2018.

Table C.18: Difference-in-Differences Analysis of the Reform on Income and Expenditure (2008-2018)

	Income	Expenditure	Foreign Expenditure Share
	(1)	(2)	(3)
Year 2008 x Pref.	0.024	0.085	0.293
	(0.063)	(0.072)	(0.418)
Year 2010 x Pref.	-0.009	-0.020	-0.120
	(0.041)	(0.061)	(0.561)
Year 2014 x Pref.	-0.039	-0.136*	-1.142*
	(0.055)	(0.082)	(0.586)
Year 2016 x Pref.	0.034	0.020	-1.755***
	(0.062)	(0.058)	(0.538)
Year 2018 x Pref.	0.018	0.064	-1.789***
	(0.074)	(0.074)	(0.589)
Year 2008	-0.147***	-0.193***	0.057
	(0.019)	(0.029)	(0.040)
Year 2010	-0.137***	-0.085***	0.010
	(0.016)	(0.028)	(0.042)
Year 2014	0.011	-0.015	0.003
	(0.020)	(0.030)	(0.036)
Year 2016	0.181***	0.106***	-0.048
	(0.017)	(0.030)	(0.037)
Year 2018	0.297***	0.232***	-0.062*
	(0.019)	(0.034)	(0.036)
Number of members in household	0.103***	0.110***	-0.007
	(0.002)	(0.003)	(0.005)
Age of head of household	0.002***	-0.007***	-0.001
	(0.000)	(0.000)	(0.001)
Male head of household	0.109***	0.189***	0.023
	(0.006)	(0.010)	(0.035)
Observations	230,512	230,512	230,512
R-squared	0.324	0.209	0.112
Fixed Effects	Municipality	Municipality	Municipality
Controls	Yes	Yes	Yes
Clusters	1,482	1,482	1,482

Notes: This table compares household income and expenditure in preferential and non-preferential areas around the 2014 VAT equalization. Each column shows estimates for a separate difference-in-differences specification, with 3 different dependent variables: total household income (Column (1)), total household expenditure (Column (2)) and share of household foreign expenditure relative to total household expenditure (Column (3)). Controls for the number of household members, gender and age of the head of household are included in the regressions, as well as municipality fixed effects. Household weights are applied to all observations. Standard errors are clustered at the municipality level. *** significant at 1%, ** significant at 5%, * significant at 10%.
Source: Author's own computations based on data from the National Survey of Household Income and Expenditure (ENIGH), 2008-2018.

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