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Doctor of Philosophy in Economics

**Measuring causal impacts with natural and field experiments:
Evidence for firms, workers, consumers, and voters**

João Pereira dos Santos, Student Number 13155

A dissertation carried out on the PhD in Economics, under the supervision of
Professor José Tavares.

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Abstract

This thesis explores an array of natural and field experiments to produce causal estimates of the impact of interventions on four populations: firms, workers, consumers, and voters, making use of public and private micro-level data. The first chapter measures the effects on firm performance of a sharp increase in transportation costs on specific highways. The second chapter investigates the labor market consequences of an exogenous shock, the unexpected inflow of repatriates following the independence of former Portuguese African colonies. The third chapter analyzes one of the first soda taxes implemented worldwide that rises with sugar content to assess its effects on prices and consumption. The fourth chapter reports on a nationwide field experiment assessing, for the first time, the capacity of the entire network of a country's ATM to “get out the vote”.

Keywords: Evidence-based reforms, difference-in-differences, natural experiments, field experiments, (shift-share) instrumental variables, highways, repatriates, migration, consumption taxes, get out the vote, Portugal.

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¹This is one of the longest sentences in this thesis. Most of them studied law, they are used to it.

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Introduction

In economics, it is understood that the presence of market failures — like externalities, public goods, and imperfect competition — creates room for well-designed public interventions that enhance social welfare. The challenging part is then how to exactly define what “well-designed” means. Reforms implemented without a deep understanding of their economic and political consequences, rather than promoting economic efficiency, can remarkably reduce it (Acemoglu and Robinson, 2013).² Rigorous empirical methods can help determining which sorts of interventions are effective by providing a counterfactual or, put simply, what would have happened had the action not been taken.

John Hicks started his 1980 book writing that “Causality and economics, which I have joined in my title, are words that are not often found together. Causality, the relation between cause and effect, is thought to be the business of philosophers; economists, though they often talk about effects and sometimes (perhaps less frequently) about causes, are usually content to leave the question of the meanings of these terms to others” (Hicks, 1980, page 14). The relationship between economists and causality has, since then, completely changed. Inspired by the early works of statisticians such as, *inter alia*, Donald Rubin, Judea Pearl, and James Heckman, and the “credibility revolution” put forward by labor economists like Joshua Angrist, David

²Joseph Stiglitz argued that sometimes even Pareto improvements are difficult to implement (Stiglitz, 1998).

Card, and Alan Krueger, economists have become obsessed with estimating the causal impact of shocks and reforms (Angrist and Pischke, 2010). Also important in this pursuit were the experimental contributions of development and behavioral economists such as Daniel Kahneman, Abhijit Banerjee, Esther Duflo, and John List, as well as the increased availability of detailed micro-level data in the works of Daron Acemoglu, Emmanuel Saez, and Raj Chetty.

If all these methodological advances were important for academic reasons, they are even more central for improving policy prescriptions and reshaping how non-economist policymakers understand a given issue. Statistics, the “science of the state,” lies at the heart of policy-making, since every policy measure is constructed to manipulate certain parameters to achieve a goal. To make an informed, evidence-based decision, policy makers need to have *a priori* knowledge about the likely trade-offs of their actions, given the relevant theoretical framework, the available data, the institutional context, and the economic question of interest. However, it is also extremely important to revisit and monitor previous decisions *a posteriori* and measure their causal impact to fine-tune current policies and inform on future ones.

Most policymakers know that “correlation does not imply causality”, but sometimes fail to understand exactly why correlations, particularly in observational data, are unlikely to display a causal relationship. And this is for the simple reason that economic agents are not behaving randomly, but instead engaging in (what they perceive as) optimal behavior. Even more difficult is to gauge a causal relationship when that effect is not straightforwardly identifiable by simple descriptive statistics.

The causal interpretation of policy shocks and reforms requires meticulous and sound testing, and attentive consideration of direct and spillover effects. As Scott Cunningham put it, “The foundations of scientific knowledge are scientific methodologies. True scientists do not collect evidence in order to prove what they want to be true or what

others want to believe. That is a form of deception and manipulation called propaganda, and propaganda is not science. Rather, scientific methodologies are devices for forming a particular kind of belief. Scientific methodologies allow us to accept unexpected, and sometimes undesirable, answers. They are process oriented, not outcome oriented.”³

In this thesis, I present four examples that carefully estimate the causal inference of shocks and reforms using (quasi-) natural (Dunning, 2012) and field experiments (Duflo and Banerjee, 2017). I take advantage of different “mostly harmless” econometric methods such as difference-in-differences, (shift-share) instrumental variables, and randomized control trials to provide credible estimates of the true impacts (Angrist and Pischke, 2008, Cunningham, 2021, Wooldridge, 2010). I study these impacts on four key economic agents: firms, workers, consumers, and voters.

There is a growing global awareness that the private sector collects valuable data for social science research (Chetty et al., 2020). In the first two chapters, I use rich administrative datasets, while in the final two chapters, I collaborate with two of the most important Portuguese private firms.

In chapter one, “Transportation Costs and firm performance”, a joint work with Catarina Branco, Dirk Dohse, and José Tavares, we focus our attention on firms to analyze how they are effected by the introduction of tolls on previously toll-free highways. In recent decades, falling transportation costs and rapid technological progress have precipitated an explosion of trade and changed the spatial economic pattern of several regions. But what happens when transportation costs suddenly increase? To answer this question in a causal manner, we rely on a natural experiment, which resulted from Portuguese central authorities being forced to increase these transportation costs, in specific highways, during the European sovereign debt crisis (2010-2011). We use micro-level data encompassing the universe of Portuguese private

³For an interesting discussion on this topic see <https://mixtape.scunning.com/introduction.html>.

firms, for the period 2006-2016, with detailed accounting information to estimate difference-in-differences and event study with various vectors of firm and regional-year fixed effects.

Our results show a 10.8% decrease of turnover in firms located in affected municipalities *vis-à-vis* firms in the remaining areas. Expenses, in turn, increase, but by a smaller magnitude, which leads to a reduction of firms' profits by 3.4%, on average. Both domestic and international trade (sales and purchases), particularly with EU countries, was significantly affected. Furthermore, employment fell 2% in municipalities affected by the increased transportation costs. Importantly, our findings do not uncover induced inter-regional firm migration, suggesting a substantial net loss to the Portuguese economy as a whole.

In chapter two, "Cousins From Overseas: The Labor Market Impact of Half a Million Portuguese Repatriates", a joint project with Lara Bohnet and Susana Peralta, we zoom in on the labor market consequences of a large, exogenous, labor market shock, exploiting the unexpected inflow of repatriates to Portugal, following the end of the Portuguese Colonial War in 1974. We use detailed census data on all Portuguese repatriates living in Portugal in 1981, as well as regional level data for the Portuguese natives in 1960 and in 1981. The labor supply shock entails a composition dimension, as the repatriates were more than twice as likely to have secondary or higher education. We take advantage of the fact that most of the repatriates were Portuguese born to build novel shift-share instrumental variables based on their region of birth. We explore the impact on regional labor force participation, unemployment, employment and self-employment, for both male and female natives.

We find substantial gender differences in the effects, with females absorbing the bulk of the shock. Native workers are driven out of employment as employees, with a sizeable 22% decrease for males and 50% for females. Men compensate for this

loss by transitioning to low quality self-employment, while women move to inactivity. Our results are robust to changing the instrumental variable, the geographical unit of analysis, and to various sample restrictions.

In chapter three, “Brown Sugar, how come you taste so good? The impact of a soda tax on prices and consumption”, a joint work with Judite Gonçalves, we focus on the impact of a recent tax reform on consumers. Increasing obesity-related problems and rising healthcare expenditures have led governments in several countries to consider the introduction of soda taxes. We study such tax, implemented in Portugal in February 2017 - one of the first soda taxes worldwide that increases with sugar content (0.08 euros per liter for drinks with less than 80 grams of sugar per liter, and 0.16 euros per liter for drinks with 80 grams or more sugar per liter, plus VAT). We use extremely detailed panel data at the product level, from one of the two largest retailers in the country, covering the period between February 2015 and January 2018. We take advantage of the tax breakdown by sugar levels to examine how soda prices and quantities purchased reacted to this policy. For identification, we rely on difference-in-differences models with various vectors of fixed effects, comparing each group of products to bottled water.

For drinks with more than 80 grams of sugar per liter, results indicate close to 100% full price pass-through to the consumer. For drinks with less than 80 grams of sugar per liter, price pass-through surpassed 100%. Regarding consumption, our findings suggest stockpiling behavior in the quarter when the tax was approved and before it was actually implemented. In the implementation period, there are no significant changes in quantities purchased for most beverages *vis-à-vis* water, with the exception of soda drinks with comparatively low levels of sugar, and thus exempt from the high tax rate. This suggests that benefits of the soda tax, in terms of reducing sugar intake, are mainly due to reformulation, as producers dramatically reduced the sugar content of some drinks to fall below the 80-grams-per-liter threshold.

In the first three chapters, we rely on observational data, that is, data where policies were determined in a non-random way (Athey and Imbens, 2017). Finally, in chapter four, “Can ATMs Get Out the Vote? Evidence from a Nationwide Field Experiment,” jointly with José Tavares and Pedro Vicente, we conduct a randomized control trial and a post-election survey to study the effect of a heretofore unexploited method of voter mobilization to study voter turnout. Low and declining levels of voter turnout constitute a fundamental problem for democracies, raising questions of legitimacy and representativeness. We report on a large-scale field experiment we designed and conducted to assess ATMs’ (automatic teller machines) capacity to “get out the vote”. Our experimental design used the full universe of functioning ATMs in Portugal (more than 12 000), which benefits from a sophisticated world class system, with wide national coverage. We randomly selected a set of treatment civil parishes, where a civic message took over the totality of ad time in ATMs, which we compare with a set of control civil parishes where advertisements ran as usual. The ATM campaign we follow was active for almost three days immediately before and during the 2017 October local elections in Portugal.

We find no statistical significant impact of the treatment *per se*. However, when we consider the intensity of treatment, for both the entire campaign period and the weekend, results show a statistically significant increase in the likelihood of voting. All three proxies for treatment intensity we employ (number of unique card users, operations, and withdrawals) deliver consistent positive coefficients. The results are also robust to several checks. For example, falsification tests using turnout rates in previous elections do not show significant coefficients, strengthening our interpretation of the experimental results.

Chapter 1

Transportation Costs and Firm Performance¹

1.1 Introduction

Transport infrastructure is key for economic development. Not only does it allow for circulation of people, it is a fundamental piece in the exchange of goods. At the same time, transport infrastructure is rather expensive. Thus, it is essential to understand the relationship between transport infrastructure and economic outcomes in order to adequately design transport policy. Given the importance of this topic, studies on the effect of transport infrastructure on aggregate economic outcomes are quite abundant (Redding and Turner, 2015). However, micro level studies on the effect

¹This chapter is written in co-authorship with Catarina Branco, Dirk Dohse, and José Tavares. We are grateful to Samira Barzin, Dave Donovan, Ed Glaeser, Nezh Guner, Adelheid Holl, Gabriel Kreindler, Xiang Li, Teresa Molina, Susana Peralta, Alfredo M Pereira, Pedro Portugal, Francisco Queiró, and seminar participants at Nova SBE, the DUP meeting (Harvard University), the 3rd CompNet ProdTalks (IWH), SAEe 2020, and the 9th PhD Student Workshop on Industrial and Public Economics (ECO-SOS) for useful comments and suggestions. We would also like to thank *BPLim – Banco de Portugal* for providing the necessary data for this study.

of transport infrastructure on firms performance are rather limited (Holl, 2016).² This paper contributes to this still-growing literature by studying the impact of an exogenous increase in transportation costs on a series of financial indicators of firm performance using micro-level data. We build on earlier work by Audretsch et al. (2020), but we take advantage of micro firm-level data to gain a better understanding of the exact firm-level mechanisms at work, and shed light on which indicators of firm performance are most strongly affected by the introduction of road tolls.

Estimating the causal effect of transport infrastructure on economic outcomes is not straightforward, as this kind of infrastructure is usually not randomly assigned. This fact could lead to biased results as it would not be clear if firm-level outcomes are varying due to a change in response to lower transportation costs provided by this type of infrastructure or due to other unobserved characteristics. A common solution in the literature for this endogeneity problem is to use an Instrumental Variable (IV) or the inconsequential places approach (Redding and Turner, 2015). Although less common, some papers alternatively rely on a natural experiment.³ The latter is exactly what we do in this paper.

The SCUT highway system started being built in 1990 and came into completion in 2008 in Portugal. Portuguese authorities made this network toll free for its users, hence the name SCUT (“**S**em **C**usto para o **U**tilizador” or “Without Cost for the User”). One of the main motivations behind its conception was to create an alternative network to the old and deteriorated roads. This new and more modern system sought to make travelling safer and a lot faster. By the end of 2008, SCUTs accounted for

²Some studies explore the effect of transport infrastructure on micro-level outcomes, such as: firms’ exports (Martincus and Blyde, 2013, Martincus et al., 2017, 2012, 2014), inventories (Datta, 2012, Li and Li, 2013, Shirley and Winston, 2004), and productivity (Gibbons et al., 2019, Graham, 2007a,b, Holl, 2012, 2016, Lall et al., 2004, Martin-Barroso et al., 2015).

³For some examples of papers which also use a natural experiment as source of exogenous variation in infrastructure see Martincus et al. (2014) and Martincus and Blyde (2013). Pereira et al. (2021) and Audretsch et al. (2020) study the same natural experiment that we do in this paper, but rely on data aggregated at the municipal-level.

almost 1000km which was nearly a third of the Portuguese highway grid at that time (Statistics Portugal, 2008).

All of this changed in 2010. On the onset of the European sovereign debt crisis, the Portuguese government was forced to consolidate its financial position, cutting spending and increasing revenues (Financial Times, 2013). Thus, the national budget could no longer sustain the provision of a toll free network. Tolls were then introduced in two waves, first by the end of 2010 and, then, by the end of 2011.

This event provides a unique setting for a natural experiment, which allows one to study the impact of an exogenous variation of transportation costs on firm related outcomes.⁴ This is only possible because tolls were introduced purely out of the necessity to regulate government budget. In other words, this decision was made without special consideration for the firms in those regions. Two earlier studies have used the same natural experiment in their analyzes to assess the impact of the unexpected introduction of road tolls on other economic outcomes. Pereira et al. (2021) show that the introduction of tolls resulted in the increase of the number of accidents and road injuries. Moreover, another study by Audretsch et al. (2020) concludes that this shock had a significant negative impact on the number of firms and employment for the affected municipalities. While Audretsch et al. (2020) rely on municipality-level data, the current paper makes use of a new firm-level dataset, encompassing more than 300 000 firms, which allows us to investigate the impact of tolls on a wide array of firm-level performance indicators (including turnover, value-added, productivity, imports, and exports) and also to check whether the tolls have induced inter-regional firm migration.

The paper contributes to the literature in four different ways. First, it provides an in-depth analysis of the impact of interurban road tolls on a wide array of firm-level

⁴In Portugal, the transportation of goods is mainly done through road transports. According to Statistics Portugal (2015), in 2010, 76% of goods were delivered via road transportation. Moreover, there were no changes in the provision or in the capacity of railroads in this period.

performance measures. Second, it analyzes how the tolls affect labor productivity and how firms pass on the burden of the tolls to their employees (employment cuts or wage cuts). Third, it covers a relatively long time period after the introduction of tolls, which allows us to disentangle short-run and medium- and longer- run effects. And fourth, the rich firm-level dataset allows us to analyze whether the firms respond to the tolls by migration to other (less or non-affected) regions in Portugal. This is extremely important for the assessment of the overall welfare effects of the tolls, as losses in the (most) affected municipalities might be compensated by benefits in the remaining regions.

Using difference-in-differences and event study specifications, our findings show that the introduction of tolls caused a 0.8% decrease of turnover in firms located in affected municipalities *vis-à-vis* firms in the remaining areas. Both labor and non-labor related expenses are also negatively affected by the shock (around 9%, on average), but to a lesser extent than turnover. Hence, firm profits fall by 3.4%, on average. Value added reduces by more than 7% in these areas. Both sales and purchases to/from the domestic market and abroad (especially to/from partners in the European Union) were significantly hit.

Focusing on the effects of the tolls on the labor market, we find that paid employment fell, on average, 2% in the treatment areas. Full-time employees suffer more (in terms of employment losses) than part-time employees. The majority of effects are long term, i.e. still persistent six years after the introduction of tolls. Average wages and labor productivity go down as well, but the effect is not statistically significant.

Finally, we find that inter-regional firm migration has not played a major role in response to the introduction of tolls. This is a particularly important result as it allows to conclude that we observe a real deterioration of overall economic activity – and not just a relocation of firms from treated to non-treated municipalities.

The rest of the paper is structured as follows. Section 2 presents a brief literature review. Section 3 details the institutional background, whereas Section 4 presents the data and discusses the methodology and possible identification threats. Section 5 highlights the results related to firm-level performance and labor-market outcomes. Section 6 discusses if the previous results represent a relocation of economic activity from treated to comparison areas or an overall deterioration of the economic conditions. Section 7 concludes.

1.2 Literature Review

The majority of research on the economic impact of infrastructure provision and pricing focuses on the macro (country or region) level and looks at macroeconomic variables. Some of the most prominent papers study the impact of transport infrastructure on: population growth (Baum-Snow, 2007, Baum-Snow et al., 2017, Garcia-López et al., 2015, Michaels et al., 2012), aggregate trade (Donaldson, 2018, Duranton et al., 2014, Storeygard, 2016), and GDP (Banerjee et al., 2012, Faber, 2014).

However, studies carried out at aggregate levels of analysis provide little insights in the actual mechanisms by which improvements or deteriorations of infrastructure affect firm performance (Haughwout, 2002). Moreover, these studies ignore mobility of firms and responses to local price changes caused by modifications in infrastructure provision and pricing (Holl, 2006). Therefore, researchers have begun to analyze the effects of transportation infrastructure on firm level performance variables.

Holl (2012) investigates the influence of transportation infrastructure on firm-level productivity through its effects on market potential in Spain, finding that growth of market access has a positive impact on firm-level output growth. Holl and Mariotti (2018) use detailed geo-referenced data to analyze the effects of highway development

on firm level performance in the Spanish logistics sector. They find that highways have significant implications for logistics' firm performance, although with important spatial heterogeneity. While improved highway access increases the productivity performance of urban logistics firms, the impact on productivity growth for those rural logistics firms that remained outside of highway corridors are found to be negative. Gibbons et al. (2019) estimate the impact of new road infrastructure on employment and labor productivity using firm level longitudinal data for Britain. They measure exposure to transport improvements through changes in accessibility and find that improved accessibility increases output per worker, wages, and use of intermediate inputs for existing establishments. Several (recent) studies suggest that infrastructure provision positively affects firm performance in a developing country context. Chauvet and Ferry (2020) investigate the relationship between taxation and firm performance in developing countries, finding that taxation benefits firm growth in developing countries, especially in lower-income countries, through the financing of public infrastructure. Barzin et al. (2018) estimate the effect of roads on firms' output growth in Colombia, finding elasticities of output with respect to road infrastructure ranging from 0.13 to 0.15%, which is clearly larger than what is found in comparable work for developed countries.

While the recent literature has made substantial progress in addressing the possibility that infrastructure and transportation costs are not assigned to regions randomly,⁵ the majority of research is unable to distinguish between growth (or decline)

⁵The most recurrent strategy found in the literature to solve this problem is the use of an instrumental variable. According to Redding and Turner (2015) the use of instrumental variables in this literature can be categorized into three main strategies: planned route IV (Baum-Snow, 2007, Hornung, 2015, Jedwab and Moradi, 2016, Mayer and Trevien, 2017, Michaels, 2008, Michaels et al., 2012, Möller and Zierer, 2018), historical route IV (Baum-Snow et al., 2017, Duranton et al., 2014, Duranton and Turner, 2011, 2012, Garcia-López et al., 2015, Hsu and Zhang, 2014, Martincus et al., 2017, Percoco, 2016), and the inconsequential place approach (Ahlfeldt and Feddersen, 2018, Banerjee et al., 2012, Chandra and Thompson, 2000, Datta, 2012, Faber, 2014, Fretz et al., 2017, Ghani et al., 2016). An attractive alternative to these standard approaches is the use of natural experiments (Bröcker et al., 2019).

and reorganization of economic activity. As Gibbons et al. (2019) put it, "In common with all empirical work that estimates causal effects from statistical comparisons across time and place, it is impossible to know for sure whether these employment increases are additional to the economy as a whole" (Gibbons et al. (2019), p. 45).

However, as is well-known since the seminal paper by Fogel (1964), the assessment of the economic impact of infrastructure depends critically on whether transport cost changes the level of economic activity or just leads to a reorganization (or relocation) of existing activity. Quite obviously, the welfare implications of a new road that creates new firms and employment are quite different than those that just lead to a re-location of pre-existing economic activity (Redding and Turner, 2015). The same is true for the economic effects of road tolls. If the tolls lead to a deterioration of economic activities (losses in value added, employment etc.) this is – from an overall economic point of view – quite different from a mere relocation of economic activities to other (less or non-affected) regions.

We know from the pertinent literature that such relocation effects can be quite substantial. Duranton et al. (2014), who investigate the effects of within-city motorways on intercity trade within the US, find that the main effect of within-city highways is to relocate economic activity, and not to create it. Besides Chandra and Thompson (2000), who study the effects of access to the US interstate highways system in rural counties, find that access to these highways increases firm earnings in treated counties mainly at the expense of their untreated neighbor counties. It is thus important to not only tackle the endogeneity problem, but also to check whether the observed effect is a real growth or decline in economic activity or just the consequence of relocation of economic activity. Our experimental design and our rich firm-level data base that contains information about movement of firms allows us to do both: the natural experiment creates quasi-exogeneity of the introduction of tolls and the rich firm level dataset

allows us to analyze whether the firms respond to the tolls by migration to other (less or non-affected) regions in Portugal. This is most important for the assessment of the overall welfare effects of the tolls, as losses in the (most) affected municipalities might be compensated by benefits in the less or non-affected) regions.

1.3 Institutional Background

Since 1986, Portugal invested substantial resources to close the gap with the core of Europe in terms of road infrastructure (Fernandes and Viegas, 1999). During the 1990s, however, these investments became a heavy financial burden. The need to guarantee the necessary funds without breaching the EU rules on member state budget deficits spurred the cooperation with private enterprises through public private partnerships (PPPs) to expand and operate road infrastructures (Fernandes et al., 2005). In 1997, a new kind of PPP scheme was introduced: a system of modern, toll-free highways, the so-called SCUTs (acronym for "Sem Custos para o Utilizador"/without costs for the users).⁶ Private investors were ensured, based on the traffic volume and operation standards, a long-term rent paid by the central government budget (Sarmiento, 2010).⁷

The SCUT highways were constructed between 1998 and 2002 at a cost of about 3 billion Euro, and cover nearly 1000 km, i.e., about one-third of the total Portuguese highway system. This new PPP scheme allowed for a swift expansion of the highway system in Portugal at low initial costs for the public sector, and helped cut average travel time between Lisbon and the Spanish border (as well as between the capital

⁶According to the Court of Accounts, these projects were financed essentially through loans from commercial banks (45%), the European Investment Bank (40%), and equity (12%). Fernandes et al. (2016) compute that the financing costs of SCUT highways are, on average, 370 basis points above the cost of raising public debt. Moreover, they argue that the transaction costs (which include banking fees and diligence costs and the impact of all cash distribution traps, such as reserve accounts or minimum-level of debt ratios) account for around 40% of that financial premium.

⁷The same rationale can be found, for example, in the UK, Spain, and Australia.

and some areas) by more than 40%. However, these large investments also generated a severe pressure for the country's budget over the next 25-30 years (Santos and Santos, 2012, Sarmiento, 2010). Many industrial parks were built in the proximity of these infrastructures to take advantage of the improved transportation connection.

In the course of the European sovereign debt crisis, the financial strain on the central budget became so tough that the Portuguese authorities had no choice but to enforce sizeable tolls on the formerly toll-free SCUT highways. As the Financial Times wrote, "To help keep Portugal's 78bn bailout on track, the government has been forced to introduce charges on more than 900 km of roads where there was previously none" (Financial Times of 25 August 2013). When the tolls were established in 2011, the price was 9 cents/km. In many situations, this implied a shift from SCUT highways to slower alternative options (e.g., municipal roads), and thus negatively affected businesses' competitiveness. These toll prices were so high that they also had a substantial adverse impact on traffic. According to a study by the Institute for Road Infrastructures, traffic along the SCUT highways decreased substantially between the first quarter of 2011 and the first quarter of 2012. There were no noteworthy congestion cases on the SCUTs highways when they were introduced (INIR 2011). This shock was driven by purely financial reasons and did not consider local conditions. The mayors of the SCUTs regions were against the introduction of tolls (even those who belonged to the same party as the national government), and there were massive protests from the local populations.⁸

With the improvement of the financial conditions, and in reaction to the decrease in traffic and the widespread criticism in the public, the new Portuguese Socialist government decided to cut back the tolls on SCUT highways by 15% from 1 August 2016 onwards. This decision was supported by a report from the public entity responsible

⁸See, for instance, <https://www.jornaldenegocios.pt/empresas/transportes/detalhe/parlamento-rejeita-fim-das-portagens-na-via-do-infante>.

for managing the road infrastructure that estimated that decreasing the tolls by 15% would increase public revenues.⁹

1.4 Data and Empirical Approach

1.4.1 Data

We combine administrative firm with municipality-level data. The firm-level information comes from the *Central Balance Sheet* database provided by *Banco de Portugal*.¹⁰ It consists of economic and financial information on virtually all private firms (such as financial balance sheet indicators, location, number of employees, size, among other indicators). This is quite an extensive dataset, which comprises 554,497 firms during the period of 2006-2016, amounting to a total of 3,680,060 observations.¹¹ Note that it is an unbalanced dataset, as not all firms have observations for all the years in this period. Additionally, only firms in Portugal mainland were considered, hence firms from Madeira and Azores are not part of the analysis.

This unique dataset allows us to investigate a variety of firm-level performance variables. These include turnover – the amount of sales of goods and services after any allowances, discounts, and returns are considered; value added – the value of output minus the value of intermediate consumption; and a measure of profitability – EBITDA (earnings before interest, taxes, depreciation, and amortization). The purchases of raw and intermediate materials are also contemplated in our data. For both turnover

⁹See, for instance, <https://www.tsf.pt/economia/descer-portagens-nas-antigas-scut-e-bom-negocio.html>.

¹⁰The data in this database is collected through *Simplified Business Information (IES - Informação Estatística Simplificada)* since 2006. *IES* is an annual report that must be filled online by firms. This report is mandatory and non-compliance is penalized. The quality of this data is then monitored by Statistics Portugal who check with respondents on a regular basis.

¹¹This corresponds to the total number of observations after dropping firms with no municipality reported, non positive levels of turnover and non positive number of employees. This last drop is meant to eliminate cases of self-employment from the sample.

and purchases, we can distinguish between those to/from the domestic market and exports/imports, from the European Union (EU) and the rest of the world.

Furthermore, we consider the following labor-market outcomes: employment, yearly average wages, and labor productivity. Regarding employment, the dataset allows us to divide between paid and non-paid employment, and between full- and part-time paid employment. Labor productivity is computed as the ratio between turnover and the number of paid employees.

In addition, this rich dataset allows us to follow the location of firms across time to compute the variable *Moved*, a binary indicator that takes the value 1 when a firm changes municipality and 0 otherwise. Hence, we use this variable to analyze whether firms have tried to mitigate or circumvent the effects of the treatment by moving to municipalities less affected by the rapid transport cost increases.¹²

Table 1.1 presents the descriptive statistics on the firm-level dependent variables used in the study. Note that most variables were logarithmized with the intention of bringing skewed data closer to a normal distribution. To prevent the loss of observations equal to zero and thus avoiding biased estimates, an extra value of one was added when using the log transformation on these variables.

We complement the firm-level data with administrative municipal information which allows us to control for time-variant municipal-level covariates. Municipal socio-demographic characteristics, such as the population density and the age dependency ratio, were gathered from Statistics Portugal. Moreover, information on per capita electricity consumption was retrieved from *Direção Geral de Energia e Geologia* (DGEG) and data on municipal expenditures was acquired from *Direção Geral das Autarquias Locais* (DGAL) to proxy for the wealth of the region. With this data, the variables *age dependency ratio*, *population density*, *electricity consumption per capita*

¹²An important limitation of this strategy is that we are not able to observe if entrepreneurs decide to close a firm in a given municipality and open a *different* firm in another area.

and *municipal expenditures per capita* were created and later added to the model to control for municipal time-varying characteristics.

Table 1.1: Descriptive Statistics

Variable	Mean	Std. Dev.	Min	Max
Treated	0.244	0.430	0	1
Firm Performance				
Turnover	10.212	4.116	0	22.988
Value Added	9.982	2.967	-4.605	21.025
Profits	9.670	1.819	-31.192	22.558
Expenses	11.039	2.894	-3.912	23.000
Wage bill	8.378	4.269	0	19.977
Other non-wage expenses	10.690	2.917	0	22.985
Domestic vs. foreign				
Turnover – Domestic	9.985	4.236	-0.051	22.582
Turnover – Exports	1.504	3.817	0	22.029
Turnover – Exports EU	1.226	3.463	0	21.390
Turnover – Exports extra EU	0.644	2.570	0	21.449
Purchases	6.833	5.499	0	22.876
Purchases – Domestic	6.662	5.440	-0.094	22.336
Purchases – Imports	1.543	3.746	0	22.714
Purchases – Imports EU	1.491	3.672	0	22.714
Purchases – Imports extra EU	0.289	1.751	0	22.646
Labor-market outcomes				
Total Employment	1.263	0.983	0	10.114
Paid Employment	1.227	1.011	0	10.114
Full-Time Paid Employment	1.315	0.982	0	9.875
Part-Time Paid Employment	0.115	0.385	0	8.935
Moved	0.018	0.134	0	1
Controls				
Population density	1717.197	2131.935	4.2	7492.4
Age dependency ratio	0.534	0.095	0.371	1.108
Electricity consumption pc	4.880	3.421	1.527	83.860
Expenses pc	0.507	0.235	0.089	2.630

Notes: All variables are measured in ln. The exceptions are Treated and Moved (dummy variables) and the vector of controls.

1.4.2 Empirical Strategy

The validity of this model relies on the fact that the introduction of tolls on SCUT highways was forced by an exogenous shock (the sovereign debt crisis) upon the Portuguese political authorities. Being a national matter, municipal authorities played no role in this decision nor were they able to directly intervene.¹³ At the same time, there was no discrimination nor favoritism towards these municipalities.¹⁴

We follow the identification strategy proposed and implemented by Audretsch et al. (2020). They define treatment as the introduction of tolls in the SCUT highways. As such, municipalities are divided into a treatment group and a comparison group. All municipalities that have a segment of the SCUT highway network belong to the treatment group. These amount to 59 municipalities in the treatment group and 219 in the comparison group.¹⁵ Note that municipalities in the comparison group do have other non-SCUT highways. However, it is important to point out that these other highways were already subject to charges a long time before this crisis and that these charges remain unchanged by the shock. As far as the treatment period goes, note that in some treated municipalities, tolls were introduced on the 15th of October 2010 and in others, this happened on the 8th of December 2011.

The effect of an increase in transportation costs on outcome y is estimated using the following difference-in-differences specification for unit of analysis firm f , in municipality m , NUTS 2 or NUTS 3 region n , and year t , during the period 2006-2016:

$$y_{fmnt} = \alpha_f + \delta_m + \lambda_{nt} + \gamma Treated_m \times PostPeriod_{mt} + X'_{mt} \beta_1 + \epsilon_{fmnt} \quad (1.1)$$

¹³Even though, there were huge protests made by SCUT highway users and local mayors, they had no saying in this decision. (See https://www.jornaldenegocios.pt/empresas/transportes/detalhe/municipios_e_utentes_perdem_accoes_contra_portagens)

¹⁴Audretsch et al. (2020) show that there was no political attempt to favor municipalities of the same political party.

¹⁵In Table A.1, there's a list with the municipalities which were affected by this shock and in Figure B4, there's a geographical display of these municipalities.

where α_f denotes firm fixed effects, δ_m municipality fixed effects, and λ_{nt} represents NUTS 2 or NUTS 3 - year fixed effects. The coefficient of interest is γ as it gives us the treatment effect. $Treated \times PostPeriod$ represents the interaction between the *Treated* dummy and the *PostPeriod* treatment dummy. *Treated* variable takes the value one for municipalities in the treatment group and zero otherwise, while *PostPeriod* equals one for the treatment period, and zero otherwise. Note that for firms in municipalities where the tolls were introduced on the 15th of October 2010, *PostPeriod* dummy equals one from 2011 onwards. As for the firms in municipalities where the tolls were introduced on the 8th of December 2011, the *PostPeriod* dummy equals one from 2012 onwards.

Additionally, X'_{mt} is a vector of municipal-level controls. To control for socio-demographic characteristics, we include the age dependency ratio and population density. The model also includes electricity consumption per capita and municipal expenses per capita to control for municipal income.

Robust standard errors are clustered at the municipal level municipality to correct for heteroskedasticity and autocorrelation, since treatment varies at this level (Bertrand et al., 2004). However, a few firms change municipality during the period under study. Therefore, the municipality at the time of the treatment was used to cluster the standard errors, i.e., the location of the firm in the year 2009 just before the treatment. Additionally, for firms that do not have an observation for this year (since this is an unbalanced panel), the location of the firm at the time it first appears in the sample was used.

Furthermore, we compute a linear probability model to address the growth versus re-organization issue with the following equation for firm f , in municipality m , and year t :

$$Moved_{fmnt} = \delta_m + \lambda_{nt} + \gamma Treated_m \times PostPeriod_{mt} + X'_{mt}\beta + \epsilon_{fmnt} \quad (1.2)$$

where *Moved* takes the value of one if firm relocates to another municipality, and

zero otherwise. The remaining variables are defined as before. As can be seen, this model does not include firm fixed effects. This would have the advantage of capturing unobservable characteristics of firms that affect the likelihood of exit. However, it would also imply that the sample used in the estimation of this model could only include firms that did exit during the time interval covered in our sample (2006-2016). Therefore, our results would be conditional on the firm dying.¹⁶

1.4.3 Identification threats

The internal validity of a difference-in-differences estimation model relies on the parallel trends assumption (Angrist and Pischke, 2008). This assumption states that in absence of treatment, the average outcome of the treatment group would have changed in a similar trend as the average outcome of the comparison group. For a careful test on the validity of the parallel trends assumption, we rely on event study designs for the main outcome variables. An event study has two main advantages. First, it allows us to observe whether the strength of the treatment varies with time. Second, it provides a more rigorous test on whether the common trend assumption holds in the pre-treatment periods (i.e., 2006-2009 in our sample). The estimating equation for the event study of firm f , in municipality m , NUTS 2 region n , and year t is:

$$y_{fmnt} = \alpha_f + \delta m + \lambda_{nt} + \sum_{t=2006}^{2009} \delta_t Treated_m \cdot Year_t + \sum_{t=2011}^{2016} \delta_t Treated_i \cdot Year_t + \epsilon_{fmnt} \quad (1.3)$$

Notice that in equation 1.3 the interaction terms for all pre- and post-treatment years are included, except for 2010. This way, all the coefficients are estimated relative to this year.

¹⁶Results are very similar if we do add firm fixed effects and are available from the authors upon request.

An important threat to our identification strategy arises if there are any other contemporaneous shocks than the treatment occurring during the time period under analysis. In this study, our time period includes one of the greatest recessions in history, forcing the Portuguese government to request an international bailout. Given that this crisis might have affected municipalities differently, region-year fixed effects, either at the NUTS 2 or NUTS 3 levels, are used to mitigate this problem.¹⁷ In addition, we also include a rich vector of municipal-level yearly controls to take into consideration the socio-demographic and economic context of these regions.

We also perform a battery of robustness checks and exploit the heterogeneity in our sample to shed light on the mechanisms driving our results. Regarding the robustness tests, we show whether our findings hold if we exclude *i*) firms in the municipalities of the Lisbon metropolitan area; *ii*) firms in the 18 district capitals; *iii*) exclude firms in all municipalities without a highway.¹⁸ In the first exercise, we remove from our sample firms that are located in the more urban comparison areas, while in the third exercise we drop firms located in more rural comparison areas. The second exercise is inspired by the inconsequential place approach, described in Table B1, where we drop from our sample the most important cities in each area (district capitals), located close to a major transportation infrastructure, mainly because they happen to be located between two important local centers. In additional exercise, We also focus on a balanced sample of firms that persist between 2006 and 2016. Regarding heterogeneity, we divide the universe of Portuguese private firms between tradable and non-tradable, and between manufacturing and service sectors.

¹⁷Moreover, Tavares and Pereira dos Santos (2018) show that the allocation of European funds is important for business firms dynamics in Portugal. Since this allocation is done at the NUTS2-level, using regional-year fixed effects can help accounting for this effect.

¹⁸In 2003, the Portuguese municipalities were allowed to organize themselves into intermunicipal communities and the two metropolitan areas of Lisbon and Porto. Since then, administrative, financial and political competencies have been transferred to these entities. Districts in mainland Portugal still serve as a basis for electoral constituencies.

1.5 Results

In this section, we analyze the impact of the introduction of tolls in previous toll-free highways through different specifications. First, we consider the main financial indicators of firm performance. We then examine labor-market outcomes and zoom in on the margins of adjustment for sales and purchases on the domestic and foreign trade. Finally, we show that our results are not driven by inter-regional firm migration.

1.5.1 Firm performance

We estimate Equation (1.3) considering four important indicators of firm performance (measured in logarithms). Figure 1.1 summarizes the main finding for Turnover (in panel a), value added (in b), profits (in c), and total expenses (in d). For all outcomes, our results support the parallel trend assumption, i.e., there are no significant differences between the treatment and the comparison group in the years 2006-2009 before the treatment set in. Turnover and expenses suffered an increasing deterioration as time evolves, whereas profits (proxied by the EBITDA) experienced a small recovery after 2013. Value added decreased but results are only marginally significant.

The event study results are confirmed in the difference-in-differences specifications computed as in Equation (1.1). In our most demanding specifications with NUTS 3-year fixed effects, presented in Table 1.2, we find that firms located in treated municipalities experienced, on average, experienced a decrease of 10.8% in turnover *vis-à-vis* firms located in comparison regions. Value added and profits also decreased by 7.6% and 3.4%, respectively.

In addition, we investigate whether firm expenses adjusted in reaction to this shock in Table 1.3. The difference-in-differences results corroborate that total expenses decreased by, on average, 9.4%. We also divide firm expenses in two categories: wage

Figure 1.1: Event Studies – Firm Performance



Notes: Outcome variables are measured in logarithms. Graphs were computed with Firm, Municipality, and NUTS 2 \times Year fixed effects. The 90% confidence levels are calculated using clustered standard errors at the municipal level.

bill (in columns 3 and 4) and other non-wage expenses (in columns 5 and 6). The wage bill includes wages, social security expenses, and insurance schemes for accidents at work and occupational diseases. The other non-wage expenses include rents, taxes, purchases of raw and intermediate materials, and debt service. This distinction is important to understand how the managers of these firms reacted to the shock. Our findings suggest the adjustment from the expenditure side impacted labor-related costs with a similar magnitude (about 10%, on average) as the remaining expenses.

The results for the four main outcomes in this subsection are robust to removing

Table 1.2: Firm performance deteriorated with the introduction of tolls

	Turnover		Value Added		Profits	
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Treated</i> × <i>Post</i>	-0.075* (0.041)	-0.108*** (0.034)	-0.051 (0.033)	-0.076** (0.033)	-0.042*** (0.015)	-0.034* (0.018)
Firm FE	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>
Municipality FE	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>
NUTS 2 × Year FE	<i>Yes</i>	<i>No</i>	<i>Yes</i>	<i>No</i>	<i>Yes</i>	<i>No</i>
NUTS 3 × Year FE	<i>No</i>	<i>Yes</i>	<i>No</i>	<i>Yes</i>	<i>No</i>	<i>Yes</i>
Controls	<i>No</i>	<i>Yes</i>	<i>No</i>	<i>Yes</i>	<i>No</i>	<i>Yes</i>
R-squared	0.038	0.038	0.054	0.055	0.021	0.022
Observations	3,680,060		3,062,711		2,336,798	
Number of Firms	554,497		520,444		464,436	

Notes: Standard errors in parenthesis are clustered at the municipal level. The vector of socio-demographic and economic controls includes electricity consumption per capita, age dependency ratio, population density and expenses per capita. Asterisks indicate significance levels of 10% (*), 5% (**), and 1%(***), respectively.

Table 1.3: Firm expenses adjusted after the introduction of tolls

	Total Expenses		Wage bill		Other non-wage expenses	
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Treated</i> × <i>Post</i>	-0.065* (0.034)	-0.094*** (0.028)	-0.058 (0.036)	-0.099*** (0.030)	-0.068** (0.034)	-0.097*** (0.028)
Firm FE	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>
Municipality FE	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>
NUTS 2 × Year FE	<i>Yes</i>	<i>No</i>	<i>Yes</i>	<i>No</i>	<i>Yes</i>	<i>No</i>
NUTS 3 × Year FE	<i>No</i>	<i>Yes</i>	<i>No</i>	<i>Yes</i>	<i>No</i>	<i>Yes</i>
Controls	<i>No</i>	<i>Yes</i>	<i>No</i>	<i>Yes</i>	<i>No</i>	<i>Yes</i>
R-squared	0.053	0.054	0.038	0.038	0.055	0.055
Observations	3,680,060		3,678,707		3,678,229	
Number of Firms	554,497		554,497		554,488	

Notes: Standard errors in parenthesis are clustered at the municipal level. The vector of socio-demographic and economic controls includes electricity consumption per capita, age dependency ratio, population density and expenses per capita. Asterisks indicate significance levels of 10% (*), 5% (**), and 1%(***), respectively.

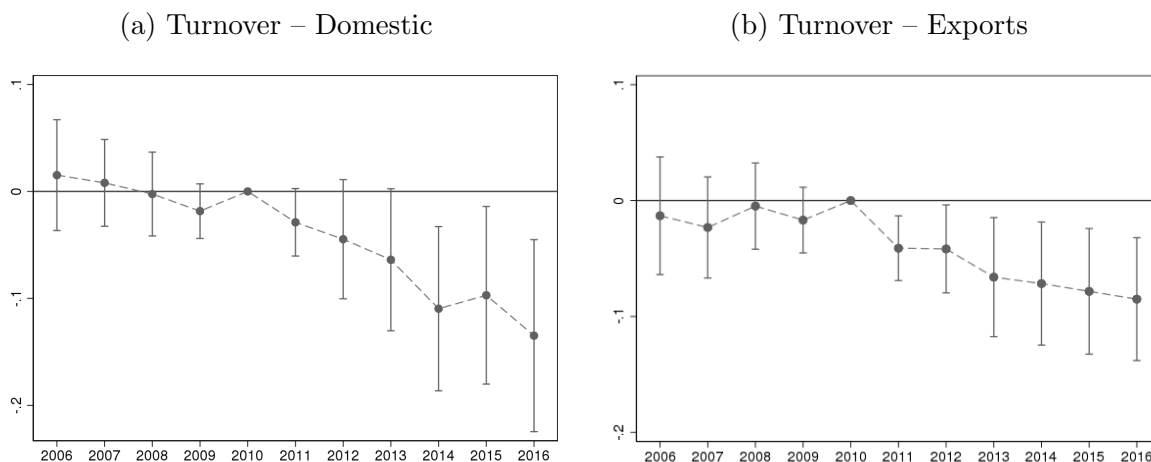
firms in municipalities in the Lisbon metropolitan area and in district capitals from the sample, as shown in Table AR.1 in the Appendix. These results also hold if we remove firms in comparison municipalities with no highway access and in a balanced sample, as displayed in Table AR.2. Moreover, results seem to be heterogeneous across firms since the shock affects more tradable than nontradable sectors (see Table AH.1 in the Appendix) and the manufacturing sector more than the services sector (see Table AH.2 in the Appendix).

1.5.2 Domestic vs. foreign: Turnover by destination and purchases by origin

We now analyze how the rise in transportation costs affected turnover with respect to the destination of goods and services: in the Portuguese market or abroad. Theoretically, it is not clear if sales to domestic buyers should be more or less affected than sales to foreign buyers by the transportation costs in Portugal. On the one hand, SCUT highways connect several locations in Portugal. On the other, some of these highways are the most important roads to Spain. Moreover, it is a well-known stylized fact that exporting firms usually constitute a small share in their own industry. They also tend to be larger and more productive than their domestic counterparts (Melitz, 2003). Hence, they may be more robust to adverse shocks.¹⁹ This is tested using Equation (1.3) and the results are presented in Figure 1.2 for domestic turnover (in panel a) and exports (in panel b). For both outcomes, we find an increasing deterioration in the treatment period with persistent effects. The reduction in economic activity is less negative for exports than for the turnover from the domestic market.

¹⁹In Portugal, Bastos and Silva (2010) demonstrate that, controlling for distance, exporters in 2005 charged higher prices for goods sold to richer destination markets within narrow product categories. Moreover, Bastos et al. (2018) show that exporting to richer countries leads Portuguese firms to pay higher prices for inputs, raising the average quality of goods they produce.

Figure 1.2: Event Studies – Turnover by destination



Notes: Outcome variables are measured in logarithms. Graph computed with Firm, Municipality, and NUTS 2 \times Year fixed effects. The 90% confidence levels are calculated using clustered standard errors at the municipal level.

We further decompose these effects computing difference-in-differences regressions as in Equation (1.1). We report the results on total firm turnover in Table 1.4 as a reference to columns 1 and 2 of Table 1.2, but we then further distinguish between those that are destined to the domestic market (in Columns 3 and 4) from Exports (columns 5 and 6). In fact, firms subject to this shock experienced a decrease in turnover from internal market of more than 7% and in turnover from exports of around 5%. Going one step further, columns (7) and (8) separately analyze the exports that were going to the EU market from the ones going to the rest of the world. One can see that exports to the EU area were significantly affected while no significant effect was found for the ones directed to the non-EU market. More precisely, exports to the EU decreased by 6.5% for firms in treated areas. Note that this decrease in exports coincides with the hypothesis previously made in regards to how the manufacturing sector, as well as large firms were affected by the shock. If indeed, these two types of firms tend to export more, then it is only natural that these firms were more vulnerable to this shock, given that exports were heavily affected. The results obtained for the sales to the EU and

the internal market can be explained by the dependence of these two markets on road transportation.

Table 1.4: Sales to domestic and foreign EU consumers were affected

	Turnover		Domestic		Exports		EU	extra EU
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
<i>Treated × Post</i>	-0.075* (0.041)	-0.108*** (0.034)	-0.073* (0.042)	-0.106*** (0.033)	-0.048* (0.029)	-0.004 (0.025)	-0.065** (0.026)	0.021 (0.017)
Firm FE	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>
Municipality FE	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>
NUTS 2 × Year FE	<i>Yes</i>	<i>No</i>	<i>Yes</i>	<i>No</i>	<i>Yes</i>	<i>No</i>	<i>Yes</i>	<i>Yes</i>
NUTS 3 × Year FE	<i>No</i>	<i>Yes</i>	<i>No</i>	<i>Yes</i>	<i>No</i>	<i>Yes</i>	<i>No</i>	<i>No</i>
Controls	<i>No</i>	<i>Yes</i>	<i>No</i>	<i>Yes</i>	<i>No</i>	<i>Yes</i>	<i>No</i>	<i>No</i>
R-squared	0.038	0.038	0.036	0.037	0.003	0.004	0.003	0.005
Observations	3,680,060		3,680,060		3,680,060			
Number of Firms	554,497		554,497		554,497			

Notes: Standard errors in parenthesis are clustered at the municipal level. The vector of socio-demographic and economic controls includes electricity consumption per capita, age dependency ratio, population density and expenses per capita. Asterisks indicate significance levels of 10% (*), 5% (**), and 1%(***), respectively.

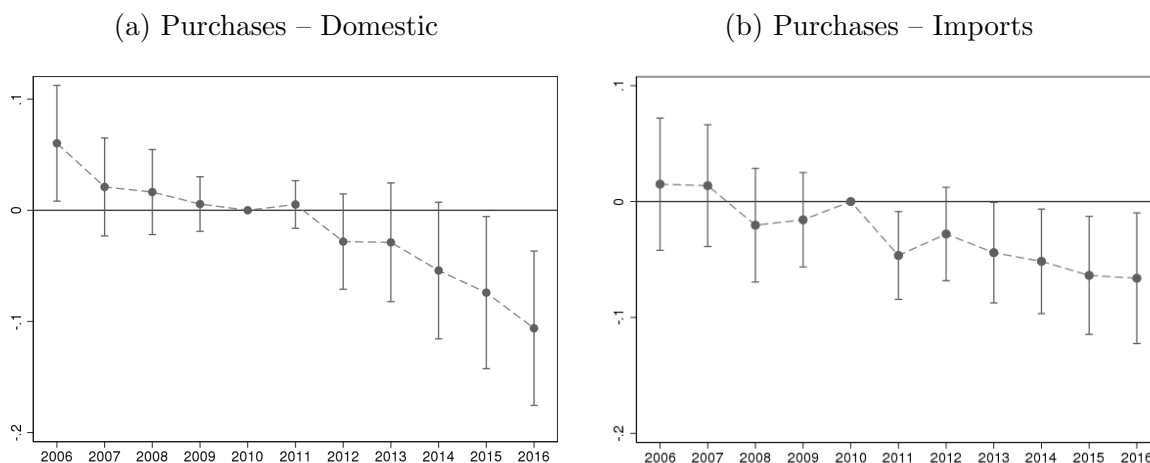
Regarding the EU market, it is important to highlight that Spain is one of the main trading partners of Portugal.²⁰ Spain’s proximity to Portugal makes trade between these two countries more reliable on road transportation. In addition, Spain’s relevance for Portuguese exports, makes Portuguese firms more vulnerable to an increase in transportation costs to this market. This could possibly explain the decrease in exports to the EU market.

We next investigate whether purchases of raw and intermediate materials were affected by the transportation shock. The event study graphs are displayed in Figure 1.3. We divide these inputs between domestic purchases (in panel a), and imports (in panel b). Our results show that estimates for domestic purchases should be interpreted with a grain of salt as the parallel trend assumption does not hold in 2006. Nevertheless, the estimated coefficients are very close to zero in the remaining years of

²⁰In 2010, Portugal’s exports to Spain accounted for 32% of the total exports made to the EU area.

the pre-treatment period.

Figure 1.3: Event Studies – Purchases by origin



Notes: Outcome variables are measured in logarithms. Graphs were computed with Firm, Municipality, and NUTS 2 \times Year fixed effects. The 90% confidence levels are calculated using clustered standard errors at the municipal level.

We also analyze the effect of an increase in transportation costs on firm's purchases using difference-in-differences regressions. The impact on total purchases is presented in columns (1) and (2) of Table 1.5. We find that purchases decreased between 7% to 10% more in firms located in municipalities with a SCUT highway. In addition, we further separate between purchases from the internal (in columns 3 and 4) or external market (in columns 5 and 6). We find that both were impacted: purchases from domestic products were reduced by 6.4% to 10.2% while total imports were reduced by 4.6%. The result is not statistically significant in our most demanding specification. However, if we divide imports between imports from the EU in column (7) and Extra EU in column (8), only imports from the EU seem to be affected (4%) as imports from outside the EU are less reliant on transportation by road.

The results for both the domestic turnover and purchases are robust to excluding all firms in the Lisbon metropolitan area, in district capitals, in municipalities without a highway access, and in a balanced sample. These results can be seen in Table AR.3

Table 1.5: Purchases from domestic and foreign EU consumers were affected

	Purchases		Domestic		Imports		EU	extra EU
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
<i>Treated</i> × <i>Post</i>	-0.066* (0.035)	-0.101*** (0.025)	-0.064* (0.035)	-0.102*** (0.026)	-0.046* (0.024)	-0.002 (0.020)	-0.040* (0.021)	-0.013 (0.009)
Firm FE	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>
Municipality FE	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>
NUTS 2 × Year FE	<i>Yes</i>	<i>No</i>	<i>Yes</i>	<i>No</i>	<i>Yes</i>	<i>No</i>	<i>Yes</i>	<i>Yes</i>
NUTS 3 × Year FE	<i>No</i>	<i>Yes</i>	<i>No</i>	<i>Yes</i>	<i>No</i>	<i>Yes</i>	<i>No</i>	<i>No</i>
Controls	<i>No</i>	<i>Yes</i>	<i>No</i>	<i>Yes</i>	<i>No</i>	<i>Yes</i>	<i>No</i>	<i>No</i>
R-squared	0.049	0.049	0.046	0.046	0.003	0.003	0.003	0.001
Observations	3,680,060		3,680,060		3,680,060			
Number of Firms	554,497		554,497		554,497			

Notes: Standard errors in parenthesis are clustered at the municipal level. The vector of socio-demographic and economic controls includes electricity consumption per capita, age dependency ratio, population density and expenses per capita. Asterisks indicate significance levels of 10% (*), 5% (**), and 1%(***), respectively.

and in Table AR.4 in the Appendix. However, trade-related results on exports and imports are less precisely estimated and are not statistically different from zero in these specifications. Finally, heterogeneity results highlight that negative effects are concentrated in the tradable sectors (in Table AH.3 in the Appendix) and in manufacturing firms (in Table AH.4 in the Appendix).

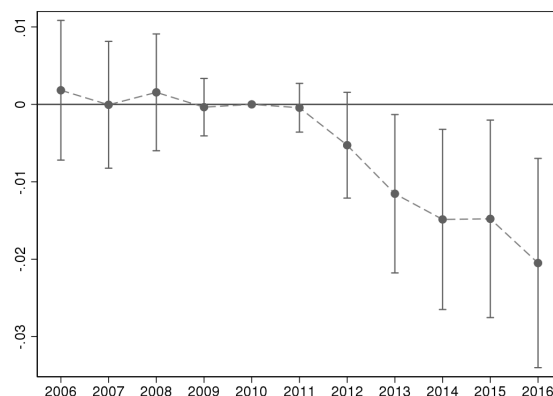
1.5.3 Employment

We now focus on the labor market impacts of this shock. So far, we found that employee expenses were dramatically reduced. In this subsection we try to distinguish between the extensive margin (i.e., if the shock caused a shortfall in employment) and the intensive margin (i.e., if those who stayed employed experienced a wage cut).

We present the event study results for labor related outcomes in Figure 1.4. One can observe that the introduction of tolls had a significantly negative effect on (paid) employment numbers (in panel a). These reductions are not only persistent but they seem to deteriorate further between 2013 and 2016.

Figure 1.4: Event Studies – Employment

(a) Paid Employment



Notes: The outcome variable is measured in logarithms. Graphs were computed with Firm, Municipality, and NUTS 2 \times Year fixed effects. The 90% confidence levels are calculated using clustered standard errors at the municipal level.

Regarding the difference-in-differences results, we find very similar decreases for total employment (in columns 1 and 2 of Table 1.6) and paid employment (in columns 3 and 4). The magnitudes are close to 2%, meaning that employment decreased 2%, on average, for firms in affected municipalities *vis-à-vis* firms in the comparison group. Total employment consists of paid and unpaid employment, the latter being performed by the company owners and their family. These results are consistent with the findings of Audretsch et al. (2020), who documented a significantly negative impact on employment at the municipal-level. In addition, our dataset allows us to distinguish between full-time and part-time employment. Our findings underline that the effects are concentrated on full-time workers (in column 5) with an estimated impact of 1.3%.

1.5.4 Wages and productivity

What happened to those who managed to keep their job in these firms? To answer this question we provide the event study specifications in Figure 1.5. We do not find statistically significant effects on average wages of those who stayed employed (in

Table 1.6: Employment decreased with the introduction of tolls

	Total Employment		Paid Employment		Full-Time	Part-Time
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Treated</i> × <i>Post</i>	-0.010* (0.006)	-0.019*** (0.005)	-0.011* (0.006)	-0.019*** (0.005)	-0.013** (0.006)	0.001 (0.003)
Firm FE	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>
Municipality FE	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>
NUTS 2 × Year FE	<i>Yes</i>	<i>No</i>	<i>Yes</i>	<i>No</i>	<i>Yes</i>	<i>Yes</i>
NUTS 3 × Year FE	<i>No</i>	<i>Yes</i>	<i>No</i>	<i>Yes</i>	<i>No</i>	<i>No</i>
Controls	<i>No</i>	<i>Yes</i>	<i>No</i>	<i>Yes</i>	<i>No</i>	<i>No</i>
R2	0.023	0.024	0.026	0.026	0.024	0.007
Observations	3,680,060		3,667,385		3,345,267	2,215,656
Number of Firms	554,497		554,131		540,269	478,542

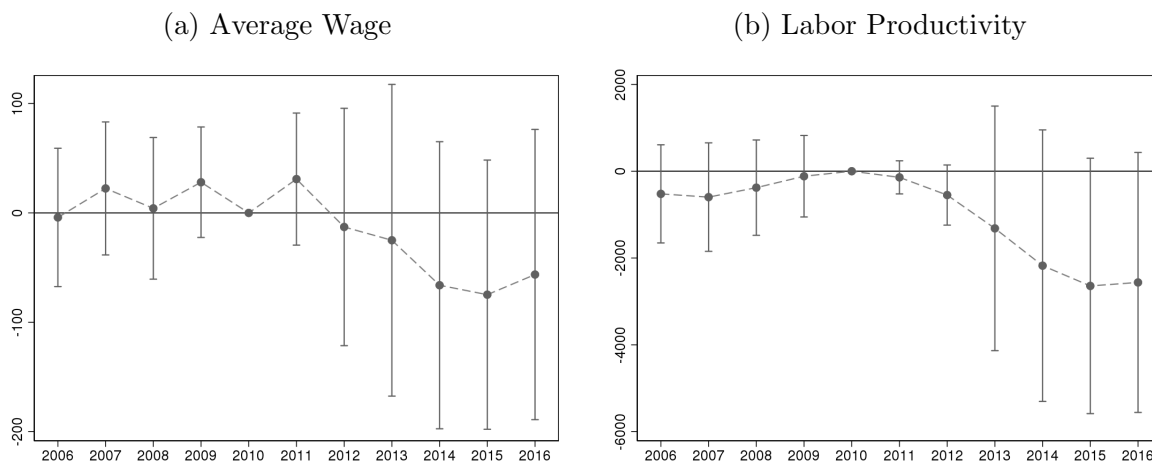
Notes: Standard errors in parenthesis are clustered at the municipal level. The vector of socio-demographic and economic controls includes electricity consumption per capita, age dependency ratio, population density and expenses per capita. Asterisks indicate significance levels of 10% (*), 5% (**), and 1%(***), respectively.

panel a). This can be explained by the fact that there is a strong downward nominal labor rigidity in Portugal. Legal restrictions on nominal wage cuts and periods of close-to-zero inflation leave employers with limited margin to adjust real wages. As a consequence, in periods of crisis, employment becomes the main margin of adjustment (Carneiro et al., 2014).²¹ With respect to the average labor productivity (in panel b), our findings suggest that this indicator does not seem to be impacted by the increase in transportation costs, which may possibly be due to the fact that workers with lower productivity were laid-off first. In any case, results are substantially noisier than in the pre-treatment years.

Furthermore, we report the difference-in-differences results for the yearly average wage (in columns 1 and 2) and labor productivity (in columns 3 and 4) of Table 1.7. These findings confirm the event study results as both variables present a negative,

²¹The minimum wage was frozen between 2011 and 2014 at 485 euros.

Figure 1.5: Event Studies – Wages and Labor Productivity



Notes: Graphs were computed with Firm, Municipality, and NUTS 2 \times Year fixed effects. The 90% confidence levels are calculated using clustered standard errors at the municipal level.

although not significant, impact caused by the introduction of tolls in treated SCUT highways. However, we would like to point out that the point estimate of the average yearly decrease is about 40 euros. This constitutes a small effect, even if we consider that, during this period, the minimum monthly wage was below 550 euros (Alexandre et al., 2020).

In Section 1.5.1, we find that the wage bill decreased by more than 9%, while in Section 1.5.3 we argue that employment fell by almost 2% and average wages reduced, even though this last indicator is not statistically significant. This is consistent with the fact that the reduction in paid employment comes from full-time workers that are more likely to be, on average, older, more experienced, and receive higher wages.

The results for the three main outcomes in this and in the previous subsection are robust to several exercises. First, removing firms in municipalities in the Lisbon metropolitan area and in district capitals from the sample, as shown in Table A.5 in the Appendix, does not seem to change our baseline results. Second, the same is true if we remove firms in rural comparison municipalities or if we consider a balanced sample, as displayed in Table A.6 in the Appendix. In all cases, we estimate that paid

Table 1.7: Average wages and productivity were less affected with the introduction of tolls

	Average Monthly Wage		Labor Productivity	
	(1)	(2)	(3)	(4)
<i>Treated</i> × <i>Post</i>	-38.510 (67.989)	-40.095 (62.325)	-514.679 (1,165.119)	-577.891 (1,818.932)
Firm FE	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>
Municipality FE	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>
NUTS 2 × Year FE	<i>Yes</i>	<i>No</i>	<i>Yes</i>	<i>No</i>
NUTS 3 × Year FE	<i>No</i>	<i>Yes</i>	<i>No</i>	<i>Yes</i>
Controls	<i>No</i>	<i>Yes</i>	<i>No</i>	<i>Yes</i>
R-squared	0.005	0.006	0.001	0.001
Observations	2,884,520		2,884,520	
Number of Firms	484,981		484,981	

Notes: Standard errors in parenthesis are clustered at the municipal level. The vector of socio-demographic and economic controls includes electricity consumption per capita, age dependency ratio, population density and expenses per capita. Asterisks indicate significance levels of 10% (*), 5% (**), and 1%(***), respectively.

employment falls, on average, 1% to 2%. Although negative, we do not find statistical evidence that average wages were affected. Furthermore, negative impacts seem to be, once again, concentrated in tradable sectors, as can be seen in Table AH.5 in the Appendix. Labor-related impact differences between manufacturing and service sectors are more pronounced as can be seen in Table AH.6 in the Appendix.

1.6 Economic deterioration or just re-location?

As Redding and Turner wrote in their chapter on the Handbook of Urban Economics: "While much effort has been directed to unraveling the problem of non-random assignment of infrastructure to places, much less has been directed to distinguishing between growth and reorganization. This distinction is clearly central to any understanding of the role of infrastructure and transportation costs in an economy"

(Redding and Turner, 2015, page 1394).

So far, we documented a strong impact of this transportation shock on firm related outcomes. In this section, we shed light on a key public policy question: do these results really indicate a drastic net-reduction of economic activity in Portugal as a whole, or are they just driven by a re-location of firms from the most affected to the least affected municipalities in Portugal? While, in the first scenario, this would imply a zero-sum game for regions, the second case would imply that more profound general equilibrium effects are present.

In any case, it is important to point out that, if there are non-negligible negative effects of the introduction of tolls that spillover to comparison municipalities, results in the previous sections constitute a lower-bound estimate for the causal impact.

Our unique data and setting allows us to convincingly make this distinction. Our dependent variable is now an indicator variable that takes value one if a firm decides to change to another municipality and zero otherwise. Deviating from the analysis so far, we do not include firm-fixed effects in this subsection due to the nature of the explanatory variable, which by definition has a very low variability. All remaining specifications will nevertheless continue to include municipal- and year-fixed effects.²²

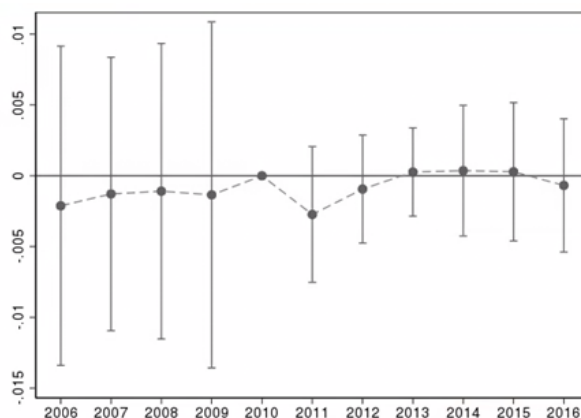
First, we show the event study results in Figure 1.6. We can observe that all point estimates are very close to zero. If anything, these zeros are even more precise after the implementation of tolls in SCUT highways indicating that re-location did not seem to take place.

Second, we confirm previous results examining the difference-in-differences obtained from estimating Equation (1.2) for the probability of firms changing municipality. We

²²That is, their values vary utmost once from 0 to 1 when a firm changes municipality or exits the market. As such, these variables have very low individual within variation. Therefore, adding firm fixed effects might not be appropriate in Equation (1.2) as it would condition the results for firms where we necessarily observe transition. Nevertheless, the results are very similar if we include firm fixed-effects.

Figure 1.6: Event Studies – Move

(a) Moving Municipality



Notes: Graph computed with Municipality, and NUTS 2 \times Year fixed effects. The 90% confidence levels are calculated using clustered standard errors at the municipal level.

find that all estimated coefficients are extremely small and not statistically significant. These precisely estimated zeros corroborate the idea that displacement effects played no significant role and that the tolls on SCUT highways led to a severe deterioration of economic activity in Portugal as a whole. The results are consistent with earlier findings suggesting that Portuguese entrepreneurs tend to have a strong home-bias in deciding the municipality where to locate their firms (Figueiredo et al., 2002).

Finally, these results are unchanged when we exclude all firms located in *i)* the Lisbon metropolitan area, *ii)* in the 18 district capitals, and *iii)* in the comparison municipalities without a highway access. This is also the case when we rely on a balanced panel covering the firms that remain in our sample throughout the period of analysis. All these results can be seen in Table A.7 in the Appendix. Exploring heterogeneity regarding firms in tradable and non-tradable sectors of activity or between those in manufacturing industries and services sectors does not change these conclusions. The results for these exercises are presented in Table AH.7 in the Appendix.

Table 1.8: Firms did not change their municipality in reaction to the shock

	Prob Change	
	(1)	(2)
<i>Treated</i> × <i>PostPeriod</i>	0.001 (0.004)	0.002 (0.003)
Municipality FE	<i>Yes</i>	<i>Yes</i>
NUTS 2 × Year FE	<i>Yes</i>	<i>No</i>
NUTS 3 × Year FE	<i>Yes</i>	<i>Yes</i>
Controls	<i>No</i>	<i>Yes</i>
R-squared	0,004	0,005
Observations	3,680,060	
Number of Firms	554,497	

Notes: Standard errors in parenthesis are clustered at the municipal level. The vector of socio-demographic and economic controls includes electricity consumption per capita, age dependency ratio, population density and expenses per capita. Asterisks indicate significance levels of 10% (*), 5% (**), and 1%(***), respectively.

1.7 Conclusion

Politicians in several countries argue that new highways are worth large amounts of public money because they stimulate, among other effects, private business sector activity and lead to economic development. In fact, however, it is very difficult to assert the efficacy of these efforts in a causal manner as roads are not assigned to locations randomly. We deal with this fundamental endogeneity problem examining the impact of a substantial reduction in transportation costs on the performance of private firms.

This paper is, to the best of our knowledge, one of the first that analyzes the effect of the introduction of road tolls on a variety of firm-level performance measures, using a rich administrative micro dataset covering the universe of private firms in a country. We take advantage of a quasi-natural experiment that happened in Portugal during the European debt crisis to identify causal parameters.

Our findings show that the tolls had a significant negative effect on firm turnover (especially serving the domestic market, but also on exports) and purchases (especially those from the domestic market, but also, to a lesser extent, on imports). The impact is more pronounced for turnover than for total expenses, and therefore, firm profits are negatively hit. Firm value-added and (paid) employment numbers also decrease more for firms located in affected areas impacted by this shock in transportation costs. In addition, average wages and labor productivity decline, but the effect is not statistically significant. Full-time employees suffer more (in terms of employment losses) than part-time employees. The majority of effects for all these outcomes are medium-term, i.e., still persistent six years after the introduction of tolls.

Critically, we find that inter-regional firm migration has not played a major role in response to the introduction of tolls. In other words, we observe a real deterioration of economic activity and not just a relocation of economic activity. This is extremely important for the assessment of the overall welfare effects of these tolls, as losses in the (most) affected municipalities were not compensated by benefits in the less or non-affected regions. All our results are robust to a plethora of exercises restricting both the treatment and the comparison group. In this regard, the exercise where we exclude all firms in the major cities of the country is especially interesting as it resembles the inconsequential places approach that is also common in the urban and trade literature.

Our results thus provide compelling evidence that the introduction of tolls on former SCUT highways – which may have been, in the short-run, inevitable for financial reasons – inflicted a substantial cost in terms of foregone firm performance and employment in the medium- to long-run. Future research can further disentangle the impact of this natural experiment on the financial conditions of firms, depending on their pre-shock level of debt and their relations with the banking system.

Chapter 2

Cousins From Overseas: The labor Market Impact of Half a Million Portuguese Repatriates¹

2.1 Introduction

Following a military coup in 1974, Portugal granted independence to its former colonies, Angola, Cape Verde, Guinea-Bissau, Mozambique and São Tomé and Príncipe. The civil wars that soon erupted in these territories induced close to half a million ethnic Portuguese living in Africa to flee to Portugal. In 1976, *The New York Times* described the situation in Lisbon as follows: “They are still coming here in droves from what was once Portuguese Africa, entire families with all their worldly belongings in plastic bags and tin trunks, and no place to go” and reported that “the absorption of this

¹This chapter was written in co-authorship with Lara Bohnet and Susana Peralta. We thank Morgane Delaunay, Elsa Peralta, and Rui Pena Pires for providing us with historical background and with data to be used in this paper. We are also grateful for comments and suggestions by Cátia Batista, Ana Rute Cardoso, Anthony Edo, Jules Gazeaud, Yajna Govind, Joël Machado, Sara Signorelli, and seminar participants at the 2021 IAAEU Workshop on labor Economics (Trier University) and the Economics of Migration - Junior Seminar.

mass of colonial refugees is one of the main difficulties facing Western Europe's poorest country."² Two years later, *Der Spiegel* states that the hundreds of thousands of refugees had been integrated faster than expected, describing how local employers were more prone to employ repatriates than natives because of their more conservative attitudes, making them less likely to be in labor unions.³ This paper provides quantitative evidence on the labor market impact of the Portuguese repatriation, which constituted a large, immigration-induced, one-time supply shock to the labor force.

The case of these so-called Portuguese "retornados" is a particularly interesting setting to study for several reasons. First, the inflow was large and concentrated, with close to half a million people arriving in Portugal within less than three years, increasing the workforce by about 3.9% on average, and up to more than 15% in some municipalities.⁴ Second, the timing of the inflow was largely unpredictable and hence provides a plausibly exogenous source of variation. Third, there was little selection among immigrants, as a large majority of the Portuguese living in the former colonies repatriated to Portugal, independent of social class, education, and other characteristics. Fourth, most repatriates were born in Portugal, therefore constituting a case of return migration of substitutes for the native population. This is opposed to other contexts where natives and immigrants are imperfect substitutes due to different language ability or religious preferences (Manacorda et al., 2012). The final reason is related with the skill composition of the repatriates. While the majority of existing literature on the labor market effects of migration has studied inflows of people who were less skilled than the native population, the repatriates were considerably more educated than natives (Pires et al., 2020).

²Howe, Marvine. 1976. "Chased From Africa, Adrift and Jobless in Portugal" *The New York Times*, Paragraph 3, March 7. <https://www.nytimes.com/1976/03/07/archives/chased-from-africa-adrift-and-jobless-in-portugal-the-excolonizers.html>.

³Der Spiegel. 1978. "Rechnungen bezahlt" *Der Spiegel*, July 24. <https://www.spiegel.de/spiegel/print/d-40694112.html>.

⁴In comparison, French repatriation increased the workforce by about 1.6%, on average (Edo, 2020).

Prior studies by Carrington and De Lima (1996) and Mäkelä (2017) have investigated the impact of the Portuguese repatriates. They were, however, unable to distinguish composition effects from effects on native workers. We overcome this issue by using a rich micro data set on the universe of Portuguese repatriates, which has not been previously exploited in an econometric analysis of the labor market; in particular, it allows us to retrieve native outcomes. We further extend their analysis by looking at a wider range of outcomes, including labor force participation (LFP), unemployment, employment, and entrepreneurship, and by investigating these outcomes separately for male and female natives.

To study the effect of the repatriates on these outcomes, we combine the natural experiment created by the end of the Portuguese Colonial War with a novel instrumental variable (IV) approach to mitigate potential endogeneity concerns in the location of repatriates. We use shift-share instruments based on places of birth of repatriates, thereby exploiting a peculiarity of the Portuguese repatriates: opposed to other repatriation flows in the literature, such as the French case (Edo, 2020), most Portuguese repatriates had still been born in Portugal.

For both male and female natives, we find a strong decrease in employment as employees. This effect is stronger for women (50%) than for men (21%). While men compensate for this loss by moving into low quality entrepreneurship (i.e., self-employment), displaced female natives move mainly to inactivity. This change is reflected in a decrease in female LFP and overall employment, opposed to no change in male LFP or overall employment. Our results are robust to changing the instrumental variable, the geographical unit of analysis, and to various sample restrictions.

We contribute to the literature on the effects of migration on native workers. Predictions from economic theory depend on the structure of the receiving country and the skill composition of immigrants relative to the native population (Dustmann

et al., 2005). In a closed economy with capital and one type of labor, immigrants reduce the capital-labor ratio and thus lower wages (Friedberg, 2001). If there is more than one type of labor, the wages of natives decrease when they are gross substitutes to immigrants in production, and raise them otherwise. If labor supply and demand are both elastic, native employment will move in the same direction as wages, with a smaller change in wages than in the case of an inelastic labor supply. In a small open economy, wages will remain at the world level in the long run. If capital is re-allocated to labor-intensive sectors with a delay, wages will be lower for some time. If wages are sticky, there will be a period of unemployment (Hunt, 1992). Recently, the literature has incorporated externalities of immigration, such as promoting learning, innovation, or agglomeration externalities (Peri, 2016).

Empirically, a wide range of studies has found modest or absent effects of immigration on average natives' wages and employment, while others have found sizeable impacts of immigration on labor market outcomes for natives. For instance, Borjas (2003) shows that wages of natives are harmed by immigration, and Ottaviano and Peri (2012) finds positive wage effects of immigration. In the light of these conflicting results, there is an ongoing debate about measurement and identification (Borjas, 2003). Dustmann et al. (2016) argue that different empirical specifications and assumptions can explain the contradicting findings.

Given the simultaneity between immigrants' location choice and local labor market conditions, one strand of literature has focused on large, unexpected increases in migration as natural experiments. They correspond more closely to exogenous increases in the supply of immigration to a specific labor market given that their timing is exogenous to local labor market conditions (Friedberg, 2001). A seminal contribution by Card (1990), for instance, studies the effect of a large inflow of Cubans into the labor market of Miami and finds virtually no effect on unemployment or wages of native

workers.⁵ We review the literature that uses natural experiments to identify the labor market consequences for natives in receiving economies in Table B1 in the Appendix.

The impacts of immigration to Portugal have been previously studied by Carrington and De Lima (1996) and Mäkelä (2017), who both investigate the effect of the Portuguese repatriation. The former provide ambiguous results: a comparison with Spain and France indicates no negative effect of the repatriates, while a comparison between districts within Portugal shows a substantial adverse impact on Portuguese wages. Mäkelä (2017) employs a synthetic control method and finds significant adverse effects on productivity and wages in the agricultural and construction industries.⁶ The data used in both studies is aggregated at the regional and national levels, and hence does not allow for distinguishing between composition effects from the inflow and impacts only on the native population. This study overcomes this issue by taking advantage of a large micro data set on the Portuguese repatriates. Finally, also related to this topic is Cardoso and Morin (2018). They study the effect of emigration from Portugal on native women, showing that the out-migration of men and war drafting in the 1960s and early 1970s led to a demand-driven increase in female LFP in Portugal. We investigate the impact of a subsequent increase in the labor force following the arrival of the repatriates.

The remainder of this paper is organized as follows. Section 2.2 provides historical background on the Portuguese Colonial War, and the repatriation to Portugal. Section 2.3 describes the data used, presents descriptive statistics, and information on the

⁵This finding is, however, subject to some debate, with, for instance, Borjas (2017) finding a large negative impact on wages of native high-school dropouts following the Mariel boatlift. Opposed to that, Clemens and Hunt (2019) and Peri and Yasenov (2019) agree with Card (1990), concluding that the boatlift had modest adverse effects on wages.

⁶Both studies use district-level wage data from Statistics Portugal, recording daily wages in the agriculture and construction industries. We do not use this data for two reasons. First, it is only available at the district level, while our main level of analysis are the (smaller) NUTS 3 (Nomenclature of Territorial Units for Statistics) regions. A more substantial caveat, given that this analysis focuses on native outcomes, is that it does not allow to distinguish between natives' and repatriates' wages.

spatial distribution of repatriates. Section 2.4 introduces the empirical strategy, before section 2.5 presents the results. Section 2.6 discusses and reconciles these results with prior studies. Section 2.7 shows various robustness checks implemented, and section 2.8 concludes.

2.2 Historical Background

2.2.1 A Brief Overview of the Portuguese Colonial War

During the 1960s and early 1970s, unrest caused by independence movements in Portugal's largest colonies, Angola and Mozambique, led the authoritarian Portuguese regime to increase the resources spent on colonial administration. In 1973, military expenditures made up close to 50% of government expenditures (Carrington and De Lima, 1996). These high monetary costs, coupled with a rising number of dead and injured in the Colonial War, and an increasing anti-colonisation sentiment, eventually culminated in the April 1974 military coup, which put an end to the authoritarian regime in Portugal (Kalter, 2018). Subsequently, the military withdrew its troops from the colonies and surrendered to the local independence movements. While initially it was expected that the white settler populations would be able to remain in Africa, soon conflicts erupted on the streets of the former colonies (Peralta, 2019). The anticipation of civil war caused by divisions among the African nationalists and meddling of foreign powers in Angola and Mozambique led hundreds of thousands of ethnic Portuguese to flee to Portugal as repatriates (Young and Hall, 1997), especially through a large airlift organized by Portugal with the assistance of several countries.

2.2.2 Repatriation to Portugal

The inflow of repatriates to Portugal was large and sudden, which was partly due to the unexpected timing of the military coup and subsequent independence of the colonies. The evidence provided by the 1981 census allows us to estimate that close to half a million repatriates arrived in Portugal between 1974 and 1976, making it the largest migration exodus resulting from decolonization in relative terms (Peralta, 2019).⁷ At the time, the native population accounted for about nine million people. In Figure B1 in the Appendix we display the relative growth of the overall population, with respect to 1970, for both males and females. Given that many of the repatriates arrived with few physical resources, the Portuguese government initiated a large-scale settlement program to assist them in their arrival, carried out by the Instituto de Apoio ao Retorno de Nacionais (IARN) (Carrington and De Lima, 1996). This state support included employing repatriates as public servants and giving cheap credits to small businesses (Peralta, 2019).⁸ In need for accommodation for the repatriates, the government rented all available places (sometimes even luxury hotels), in which some of the repatriates passed their first two years in Portugal. The settlement program accounted for 11% of total government spending in 1976 (Solsten, 1993) and amounted to roughly 5% of Portuguese GNP over the 1974-76 period (Carrington and De Lima, 1996). However, as early as 1981, IARN was dissolved, and the repatriates who remained in need of assistance were handed over to the social security system (Peralta, 2019).

This fast process may be one of the reasons why the integration of the repatriates is often remembered as a success that "may even be considered miraculous" (Peralta (2019), 6). This happened despite the fact that the post-revolutionary left-wing

⁷In July 1975, a new Nationality Law was enacted to prevent a mass inflow of Africans determining that only those who could prove an European lineage up to their grandfathers could apply for a Portuguese identity card.

⁸The public sector's employment share rose from 13.4% in 1973 to 23.7% in 1976 (Carrington and De Lima, 1996).

governments implemented income-leveling policies that included a large increase in the minimum wage for most of the workforce, the nationalization of many industries, and other restrictions, that made it difficult for firms to fire incumbents and therefore, indirectly, hire repatriates. However, the arrival of the repatriates was not without problems.⁹ Portugal was in the middle of a process of social and political change.¹⁰ This, coupled with a severe economic recession that culminated with the 1978 arrangement between the Portuguese authorities and the International Monetary Fund, contributed to repatriates being received with hostility, perceived by the native population as foreigners, or even invaders (Peralta, 2019). According to Lubkemann (2002), the media at the time contributed to the negative stereotyping of the "internal strangers", as he called them (p. 76). Press reports of the government assistance program included claims that repatriates were "stealing housing and jobs" from the Portuguese (Mäkelä (2017), 242).

2.3 Data and Descriptive Statistics

2.3.1 Data on Repatriates

Data on the repatriates was retrieved by the sociologist Rui Pena Pires from the Portuguese census of 1981. He defines a repatriate as someone who lived in Portugal in 1981 and had lived in an African country in December 1973. The data set contains

⁹A 1978 survey displayed that 68% of the respondents were in favour of the independence of the colonies, but 59% expressed their disagreement with the way the process had been conducted and stated that the Portuguese authorities should have defended more "the rights" of the Portuguese nationals (Oliveira, 2017). Figure B2 in the Appendix, retrieved from Lourenço (2018), we can see the number of news mentioning the repatriates in two daily newspapers (1974-1979): *Primeiro de Janeiro* (from Porto) and *Diário de Notícias* (from Lisbon). From this graph, two conclusions can be drawn. First, even though most news were neutral, there were more negative than positive news. Second, the number of articles was already small in 1979.

¹⁰The government changed four times in 1974 and three times in 1975. There was only one change of government in 1976 and 1977.

individual-level data on 471,427 repatriates, including demographic information such as sex and age, place of birth, place of residency in 1979 and 1981, as well as educational and employment information. In our sample, we consider repatriates who are between 20 and 69 years old in 1981. This range corresponds to an age of around 15 to 64 when arriving in Portugal, which is considered the most relevant range in inducing a shock to the labor market. This restriction reduces the sample to 339,868 repatriates. We furthermore limit the sample to individuals who migrated to Portugal before 1979 and to those who did not change NUTS 3 region since then. This exclusion leaves us with a sample size of 317,924 repatriates. The former restriction allows us to consider only those who moved to Portugal in response to an unanticipated political event, that is, in response to an exogenous push-factor, which facilitates identification. Restricting the sample to only those who did not change NUTS 3 region leads to the exclusion of roughly 20,000 repatriates (approx. 4% of all repatriates). It however aids to capture the initial shock rather than movements between regions at a later point, which may have been for economic reasons.¹¹

Moreover, we retrieved data on the white resident population in Angola and Mozambique from 1940 to 1970 from Statistics Portugal, as displayed in Table B2 in the Appendix. Movements to the colonies were relatively recent, as the number of white residents in Angola and Mozambique more than tripled between 1950 and 1970. There were 443,068 white residents in these colonies in 1970. This figure is very close to the total of repatriates, indicating that almost the entire Portuguese population residing abroad repatriated to Portugal. We will use this white population in the former colonies to construct the shift in one of the shift-share instruments.

¹¹As shown later, the results are robust to including those who changed region and to changing the age range considered.

2.3.2 Data on Portuguese Natives

To investigate the impact of the repatriates, we are using census data from 1960 as the pre-shock period, and census data from 1981 as the post-shock period. We use the 1960 census because it is the last census before the massive emigration of Portuguese residents in the 1960s.¹² The 1981 census is the first census after the inflow. Hence, the post-shock period takes place about six years after the shock, thereby measuring its impact after some time for adjustment.¹³ The censuses contain municipality-level data on demographics and employment. In most of our specifications, we aggregate the data to the NUTS 3 level, hence containing 30 regions.¹⁴ Since the census of 1981 includes repatriates and is aggregated at the municipality level, a distinction between repatriates and the native population is not readily available from the data. We define the native population in each region as the non-repatriate population, calculating them as the total population listed in the 1981 census minus the number of repatriates that lived in each region in that year, as taken from the data set on repatriates. We compute outcomes for the native population likewise: the number of unemployed natives, for instance, is derived from the total number of unemployed as defined in the census minus the number of unemployed repatriates. We focus the analysis on the impact of the repatriates on labor Force Participation, Unemployment Rate and the Employment Share.¹⁵ The census also provides data on different types of employment, among them the number of employees and entrepreneurs. Entrepreneurs are further divided into employers (those who have employees) and self-employed individuals (those who do not

¹²While there was a census in 1970, which is available in non-digital format at Statistics Portugal, it is considerably smaller than the 1960 and 1981 censuses, covering only 20% of the population and containing none of the labor market variables used in this analysis.

¹³In their study of the effect of skewed sex ratios on Portuguese women's labor market outcomes, Cardoso and Morin (2018) use an even longer time period between the shock and measured outcomes.

¹⁴We use the first definition for NUTS 3 in the country which was established in 2002.

¹⁵We do not analyze wages because the linked employer-employee dataset where they are available started in 1986 (Card and Cardoso, 2012).

employ others). We investigate these outcomes separately for male natives and female natives.

To construct a shift-share instrument based on educational levels, we decompose the native population and the repatriates across four educational groups, namely those with no education, primary education, secondary education, and higher education.¹⁶

2.3.3 Comparison of Repatriates and Natives

Approximately 77.8% of repatriates were born in Portugal, with the majority of the remaining repatriates born in former Portuguese colonies. This characteristic distinguishes the repatriates from other decolonization migrants to, for instance, France and The Netherlands, of whom most had been born in the colonies (Lubkemann, 2002). In line with this, the large majority of repatriates were Portuguese speaking (Pires et al., 2020).

What differentiated the Portuguese repatriates from the native population, however, was the fact that they were more likely to be of working age, as shown in Figure B3 in the Appendix. In Table 2.1, we report further differences. Compared to natives, repatriates were more likely to be male, more educated, and more likely to be employees or employers. Overall, the inflow of repatriates not only changed the size, but also the composition of the Portuguese labor force. The Table displays data on the place of residence in 1973, indicating that the majority of repatriates came from Angola and Mozambique.

Table 2.2 shows descriptive statistics for changes in native labor market outcomes between 1960 and 1981, natives' and repatriates' outcomes in 1981, and the difference

¹⁶Primary education includes those with Primário elementar or Preparatório. Secondary education includes those with Secundário unificado, Secundario complementar or Propedêutico ou 12.^o ano. Higher education includes those with Curso de índole profissional e artístico, Curso médio, enfermagem, professional, or Curso superior.

Table 2.1: Comparison Repatriates and Natives

	Natives (Sample)		Repatriates (Sample)		Repatriates (Above 15)	
	N	%	N	%	N	%
Gender						
Male	3,189,679	46.7%	173,382	54.5%	194,617	53.8%
Female	3,634,546	53.3%	144,542	45.5%	167,162	46.2%
Total	6,824,225	100.0%	317,924	100.0%	361,779	100.0%
Education						
None	2,612,630	38.3%	39,493	12.4%	45,328	12.5%
Primary	3,341,173	49.0%	185,816	58.4%	210,633	58.2%
Secondary	657,780	9.6%	65,460	20.6%	78,156	21.6%
Higher	212,642	3.1%	27,155	8.5%	27,662	7.6%
Total	6,824,225	100.0%	317,924	100.0%	361,779	100.0%
Profession						
Employee	2,808,796	76.8%	174,745	81.4%	181,912	81.7%
Self-employed	602,565	16.5%	25,839	12.0%	26,157	11.8%
Employer	118,985	3.3%	10,338	4.8%	10,441	4.7%
Stay-home parent	97,936	2.7%	1,673	0.8%	1,989	0.9%
Cooperative	16,496	0.5%	540	0.3%	564	0.3%
Other	13,235	0.4%	1,467	0.7%	1,520	0.7%
Total	3,658,013	100.0%	214,602	100.0%	222,583	100.0%
<i>Unemployed/Inactive</i>	3,166,212	100.0%	103,322	100.0%	139,196	100.0%
Residence in 1973						
Angola			195,206	61.4%	222,420	61.5%
Mozambique			106,242	33.4%	121,588	33.6%
Other			16,476	5.2%	17,771	4.9%
Total			317,924	100.0%	361,779	100.0%

Notes: The native sample is comprised of all non-repatriates above the age of 15. The sample of repatriates is comprised of all repatriates between 20 and 69 years old in 1981. For comparison, statistics of repatriates above 15 are displayed. Shares may not add up to 100% due to rounding. Source: census of 1981, Statistics Portugal, computations by the author.

between them. In addition, differences are considered separately for both males and females. We calculate all indicators, with the exception of the unemployment rate, as shares over the total population of working age, which we define as from 15 to 64 years old. The unemployment rate is the share of the labor force who is unemployed. The outcomes exhibit an increasing degree of granularity as one moves from the top to the bottom of the Table. The labor force encompasses all those who are unemployed and employed. Those employed encompass, among less relevant categories

not considered, employees, and entrepreneurs. Entrepreneurs, in turn, are made up of self-employed individuals (those who do not have employees), which consider low-quality entrepreneurship, and employers (those who have employees), which we consider to be high-quality entrepreneurship.

Table 2.2: Descriptive Statistics

Variable	Males				Females			
	Natives		Repatriates	Difference	Natives		Repatriates	Difference
	Δ_{81-60}	m_{81}	m_{81}	m_{81}	Δ_{81-60}	m_{81}	m_{81}	m_{81}
LFP	-0.100 (0.039)	0.889 (0.026)	0.878 (0.028)	0.011 (0.007)	0.223 (0.052)	0.392 (0.088)	0.483 (0.071)	-0.091*** (0.021)
Unemployment rate	0.011 (0.013)	0.040 (0.011)	0.073 (0.026)	-0.034*** (0.005)	0.120 (0.059)	0.133 (0.060)	0.224 (0.044)	-0.091*** (0.014)
Employment share	-0.105 (0.041)	0.854 (0.030)	0.814 (0.039)	0.040*** (0.009)	0.173 (0.050)	0.341 (0.085)	0.375 (0.061)	-0.034 (0.019)
<i>Share Employee</i>	-0.129 (0.089)	0.587 (0.078)	0.627 (0.075)	-0.040* (0.020)	0.095 (0.045)	0.242 (0.087)	0.320 (0.068)	-0.078*** (0.020)
<i>Share Entrepreneur</i>	-0.014 (0.056)	0.221 (0.068)	0.179 (0.047)	0.042** (0.015)	0.065 (0.044)	0.083 (0.049)	0.051 (0.021)	0.033** (0.010)
<i>Share Employer</i>	-0.038 (0.016)	0.031 (0.009)	0.046 (0.012)	-0.014*** (0.003)	0.001 (0.002)	0.005 (0.002)	0.008 (0.003)	0.003*** (0.001)
<i>Share Self-employed</i>	0.023 (0.058)	0.189 (0.072)	0.133 (0.044)	0.056*** (0.015)	0.064 (0.045)	0.079 (0.049)	0.042 (0.021)	0.036*** (0.010)

Notes: Standard deviations in parentheses. Δ_{81-60} refer to the change in each outcome between 1960 and 1981. m_{81} refers to the mean level in 1981 across the 30 NUTS 3 regions. LFP stands for labor force participation. The column *Difference* shows the difference between mean levels of natives and repatriates in 1981. The stars indicate significance of an unpaired t-test of the differences. All indicators except for the unemployment rate are calculated as shares over the total population of working age. I compute the unemployment rate as the share of the labor force that is unemployed. Source: census of 1981, computations by the author. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

The LFP of male natives decreased between 1960 and 1981, while female native LFP increased. We observe an increase in the male native unemployment rate, and a large drop in the male employment share. Male natives experienced a decrease in the share of employers, but an increase in the share of self-employed individuals. For

female natives, all these outcomes exhibit positive changes, indicating an increasing integration of women in the labor market.

In 1981, the LFP of both native and repatriate men is significantly higher than for women. Compared to natives, repatriates of both genders are more likely to be unemployed, indicating that they were not yet fully integrated into the Portuguese labor market. However, female repatriates seem to be better integrated than female natives, as reflected in their higher LFP.¹⁷

As already indicated in Table 2.1, both male and female repatriates are less likely to be self-employed, but more likely than natives to be employers.

2.3.4 Spatial distribution of repatriates

The total sample of repatriates accounts for 4.7%¹⁸ of the total natives above 15 in 1981, with considerable spatial variation between municipalities, as shown in Figure B4 in the Appendix. The highest density is observed in the North East of the country and around Lisbon. In 1981, there seems to be no major clustering of repatriates in certain regions.

Figure 2.1 shows the relative supply shock by gender and NUTS 3 level. The regional average supply shock for males is approximately 4.7% of the native population, while for females it accounts for about 3.3%, with a total average shock of 3.9%. The largest supply shock was induced in Serra da Estrela, followed by Grande Lisboa.

¹⁷Female repatriates exhibit a higher unemployment rate and a higher employment share than do native women. This means that they are more likely to either be employed or unemployed once they are of working age and less likely to, for instance, be stay-home parents. This is also reflected in the higher female LFP. For male repatriates, the LFP is lower than for natives. Once male repatriates are in the labor force, they are more likely to be unemployed and less likely to be employed than the native population.

¹⁸Calculated as 317,924/6,824,225, see Table 2.1 for the numbers of total natives and repatriates.

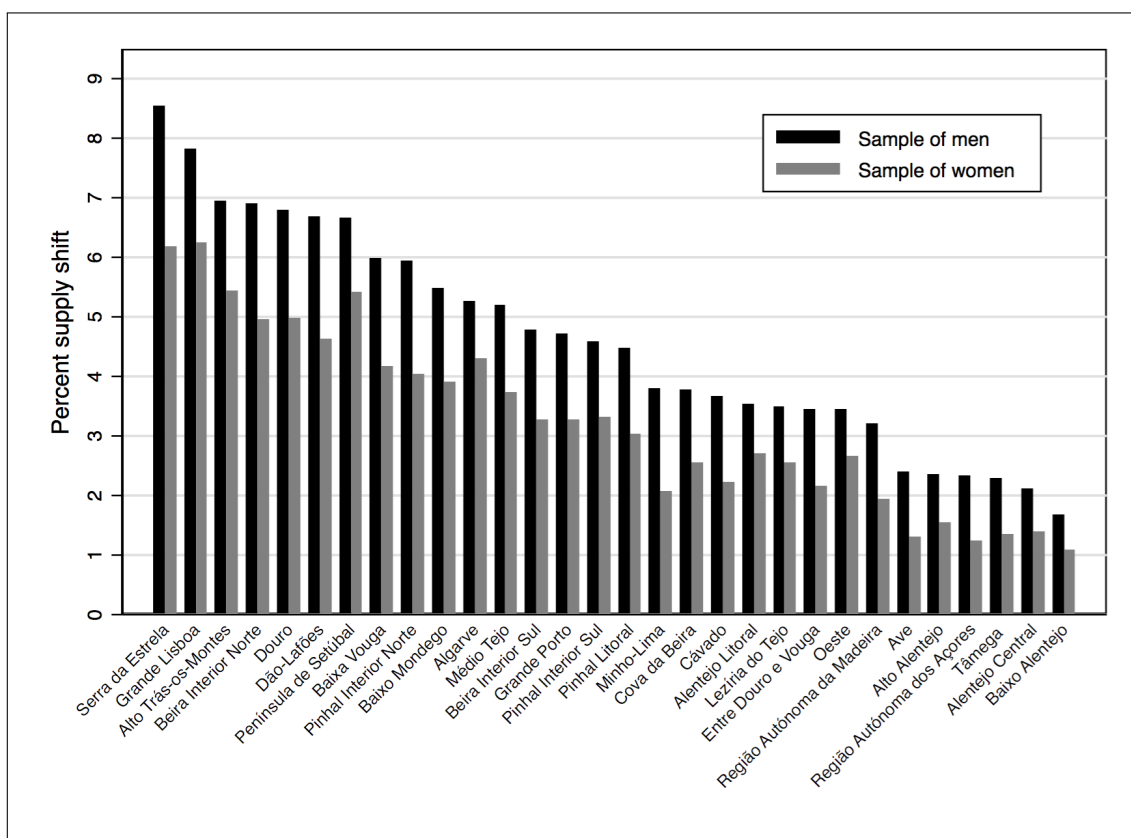


Figure 2.1 Percent supply shock by NUTS 3 regions. The supply shock is calculated as the number of repatriates in the sample over the number of natives above 15 in 1981. Source: census of 1981, Statistics Portugal, own construction.

2.4 Empirical Strategy and Identification

2.4.1 Main Econometric Equation

One of the most common approaches in economic literature to study the effect of migration on native outcomes is the spatial correlation approach (Glitz, 2012). In this approach, an outcome in a given area is regressed on the relative quantity of immigrants in that same area. This area is intended to correspond to a local labor market. We follow this approach, using as a source of variation the differential relative size of inflows of repatriates by region. As suggested by Dustmann et al. (2016), we investigate the effect of the overall (opposed to the group-specific) immigration shock on labor market

outcomes.¹⁹ We estimate the impact of the Portuguese repatriates on several labor market outcomes using the following specification:

$$\Delta Y_{nr} = \alpha_n + \beta m_r + X_r^{1960} + \epsilon_{nr} \quad (2.1)$$

where r stands for NUTS 3 region and n for NUTS 2 region. Figure B5 in the Appendix shows a map with these regions, Table B3 shows the size of these regions in terms of population and labor force. The analysis is at the geographical level of NUTS 3 since they are assumed to correspond to local labor markets.²⁰ ΔY_r denotes the change in the outcome Y from 1960 (the pre-shock period) to 1981 (the post-shock period) in each region. We investigate labor force participation, the unemployment rate, overall employment and employment as an employee or entrepreneur. m_r is the ratio of repatriates in the sample in 1981 to the natives above 15 in 1981, in region r . X_r^{1960} is a vector of controls, namely, the shares of unemployed, inactive, young, highly educated and entrepreneurs in 1960. We further include dummies for the seven NUTS 2 regions to focus our analysis on differences within these regions. To account for potential heteroskedasticity of the error term, all regressions use heteroskedasticity robust standard errors.

¹⁹Dustmann et al. (2016) argue that this specification is preferable over specifications using variation in immigrant inflows both across education groups and across regions for two reasons. First, the latter unduly rely on the assumption that an immigrant and a native with the same measured education and experience compete against each other. There is, however, strong evidence that immigrants "downgrade" upon their arrival. Second, they argue that the overall effect of the total inflow is easier to interpret and estimates a parameter with direct policy relevance.

²⁰See, for instance, Baptista et al. (2008).

2.4.2 Identification

Main Identification Issue

The aim of this analysis is to compare the economic outcomes of certain regions after immigration with the counterfactual outcome that would have been observed had migration not taken place (Dustmann et al., 2005). In an ideal empirical world, immigrants would be allocated randomly across labor markets. Any subsequent variation in economic outcomes would then be purely related to variation in immigrant densities. However, in the real world, migrants are not allocated randomly, and immigration densities may be spatially correlated with labor market outcomes because of common influences, which would bias OLS results.

Our outcome variables are first-differenced to account for omitted time-invariant characteristics of the regions. Two further endogeneity concerns relate to the measurement of the supply shock m_r , calculated as the ratio of repatriates to the pre-existing native workforce in 1981.²¹ This ratio poses two endogeneity concerns: natives may have moved in response to the inflow of repatriates and repatriates may locate endogenously.²² If immigration were to increase unemployment in certain areas, but natives would move to areas with lower migrant density in response, the impact of immigration would be dispersed through the national economy, leading to downward biased estimates of the effect of immigration on unemployment. The extent to which repatriates could actively base their location decision on economic considerations was limited by the unexpected timing of the end of the Portuguese Colonial War. Even when controlling for an extensive set of controls in the pre-shock period, we, however, cannot entirely exclude the possibility that repatriates moved to regions for unobserved factors that are correlated with changes in outcomes between 1960 and 1981, which

²¹Hunt (1992), Borjas (2003) and Edo (2020) likewise use this post-shock denominator.

²²These concerns are especially warranted in this case since data for the post-shock period comes from several years after the shock, giving natives sufficient time to adjust their location.

would again bias OLS estimates.

Shift-Share Instrument

To address the potential endogeneity in the location of repatriates, we use three variations of a shift-share instrument, which is the most common method applied in studies of this kind (Jaeger et al., 2018). A shift-share instrument is a weighted average of some shock, with weights reflecting heterogeneous shock exposure (Borusyak et al., 2018). That is, the spatial distribution of a certain shock (i.e. the shift) is instrumented by predicting regional shock exposure from some regional, exogenous characteristic (i.e. the share). Following the work of Bartel (1989), who showed that arriving migrants are more likely to settle in areas with higher previous migrant densities, the most common way to build a shift-share instrument in the migration literature is to use a measure of historical settlement patterns as share. This assumes network effects between current and past migrants. Our shift-share instruments resemble that of Edo (2020), who likewise uses past settlements as shares. We, however, adapt the shares given that we are dealing with a particular kind of migration, namely repatriation of people who had mostly still been born in the receiving country. The Portuguese repatriates are unlikely to have much in common with former migrants, making network effects between these two groups less plausible. A more suitable parameter to predict settlement patterns is the place of birth of repatriates since many of them returned to their region of birth.²³ Therefore, we use the share of Portuguese-born repatriates born in each region as a source of variation. We construct three alternative shift-share instruments, all based on birth places. For the first IV, we decompose the sample of repatriates across four educational levels to construct the instrument, assuming that network effects with other

²³In Figure B6 of the Appendix we show that a large Portuguese-born repatriates return to the same NUTS 3 where they were born. In our sample, and on average, about 40% of Portuguese-born repatriates lived in 1981, in the municipality they were born, with 51% residing in the NUTS 3 region they were born in.

repatriates are stronger between social classes, for which education serves as a proxy.

We then use the share of Portuguese-born repatriates of a certain education group born in each region to build the shift-share instrument. The underlying reasoning is the following: if more repatriates of a particular social class were born in a region, this region is predicted to attract more repatriates of the same social class. We compute the imputed number of repatriates for the first IV in the following way:

$$\widehat{Repatriates}_r = \sum_i \frac{Portuguese - born_{ir}}{Portuguese - born_i} \times Repatriates_i \quad (2.2)$$

where i stands for one of four education groups and r stands for one of 30 regions.

To show robustness to the assumption that network effects are stronger within education groups, we compute a variant of the repatriate prediction by defining i as origin group rather than education group, with origin referring to the place lived in before repatriation to Portugal. The three origin groups are Angola, Mozambique and other colonies. This instrument assumes that network effects are stronger between repatriates from the same colonies, which might be a more reasonable assumption for repatriates, who, for instance, migrated together with part of their African-born former employees.

Lastly, we compute a third prediction of repatriates to construct a more simple Bartik instrument, as first proposed by Bartik (1991) in the context of predicting employment growth. We interact the share of total births in each region with another proxy for the total inflow of repatriates, namely the number of white residents in Angola and Mozambique in 1970, using the following equation:

$$\widehat{Repatriates}_r = \frac{Portuguese - born_r}{Portuguese - born} \times White\ residents\ in\ colonies_{1970} \quad (2.3)$$

This instrument does not rely on network effects between certain education or origin groups and uses an alternative measure as a shift.

Following Edo (2020), we address the above described potential endogeneity of natives in 1981 by likewise predicting the number of pre-existing natives, i.e., those that do not move in response to the repatriate shock, in each region as follows:

$$\widehat{Natives}_r^{1981} = \sum_i \frac{Natives_{ir}(1960)}{Natives_i(1960)} \times Natives_i(1981) \quad (2.4)$$

where i again stands for education group and r stands for region.

After predicting the number of repatriates per region from (2.2), or from (2.3), and the number of natives from (2.4), we compute the three different shift-share instruments as follows:

$$m_r^{IV} = \frac{\widehat{Repatriates}_r}{\widehat{Natives}_r^{1981}} \quad (2.5)$$

The literature on shift-share instruments suggests that such an instrument will be invalid if conditions which influence the spatial distribution used as share are serially correlated over time and influence current outcomes (Borjas, 1999). According to Dustmann et al. (2005), this concern is mitigated by using a sufficient time lag to predict the regional distribution of migrants. As we are using places of birth of migrants of different ages, it seems plausible to assume that their places of birth in different years and subsequent decision to leave their home region in different years have no systematic impact on changes in outcomes between 1960 and 1981.²⁴ In addition, as noted by Peralta (2019), the repatriates were a socially diverse population. Some of them had left Portugal because they resided in impoverished regions and wanted to

²⁴Note that our outcomes use first differences. Therefore, as argued by Goldsmith-Pinkham et al. (2020), the question is not whether the shares influence levels of outcomes, but rather whether they influence changes in outcomes.

escape poverty, others were affluent settler families with affinities to colonial power. This diversity indicates that there is unlikely to be a systematic relationship between the places of birth and changes in labor market outcomes between 1960 and 1981. Nevertheless, we include a set of controls in 1960 to support this assumption. For our instrument to yield unbiased estimates, we then require the share of births of repatriates in each region to be exogenous to changes in labor market outcomes between 1960 and 1981, after controlling for several characteristics in the pre-shock period.

2.5 Empirical Results

The fact that the characteristics of female and male natives, as shown in Table 2.2, differ substantially motivates a separate analysis for male and female outcomes. Moreover, female labor supply has been found to be more elastic to shocks than male labor supply (Lloyd and Niemi, 1978). Therefore, it seems relevant to investigate the effect of the repatriate supply shock on both the male and the female labor markets separately.

Table 2.3 presents the OLS and IV estimated effects of the supply shock induced by the repatriates on the change in labor force participation, unemployment rate, employment and entrepreneurship for male natives. Table 2.4 presents the same for female natives. Specification (1) and (2) are OLS regressions of equation (2.1), without and with pre-shock controls, respectively. Specification (3) to (5) refer to IV regressions, with (3) using the IV based on educational network effects, (4) using the IV based on origin network effects, and (5) using the simple Bartik instrument. All first-stage coefficients of the instruments are large in magnitude, and the first-stage F-statistics comfortably pass the bound of 10 suggested by the literature on weak instruments (Stock et al., 2002). These results indicate that all instruments are relevant predictors of repatriate density and the IV estimates are unlikely to be subject to weak instrument

Table 2.3: Labor market effects of repatriates on male natives - baseline

Outcomes for male natives	OLS		IV		
	(1)	(2)	(3)	(4)	(5)
Δ LFP	0.228 (0.366)	0.526 (0.458)	-0.107 (0.293)	-0.111 (0.306)	-0.082 (0.296)
Δ Unemployment rate	-0.186 (0.137)	-0.044 (0.185)	0.214 (0.119)	0.216 (0.118)	0.210 (0.117)
Δ Employment share	0.397 (0.399)	0.527 (0.542)	-0.328 (0.319)	-0.334 (0.334)	-0.300 (0.321)
Δ Share Employee	-0.490 (0.936)	-1.865 (1.085)	-3.912*** (0.905)	-3.902*** (0.931)	-3.779*** (0.886)
Δ Share Entrepreneur	0.318 (0.573)	1.764* (0.809)	3.079*** (0.766)	3.059*** (0.774)	2.967*** (0.744)
Δ Share Employer	-0.396 (0.204)	-0.624*** (0.152)	-0.677*** (0.158)	-0.685*** (0.159)	-0.686*** (0.154)
Δ Share Self-employed	0.714 (0.566)	2.388** (0.854)	3.756*** (0.851)	3.744*** (0.860)	3.653*** (0.826)
Controls	NO	YES	YES	YES	YES
Instrument	-	-	IV1	IV2	IV3
First-stage coefficient	-	-	0.528	0.510	0.375
First-stage F-statistic	-	-	55.05	50.94	63.64
Observations	30	30	30	30	30

Notes: Robust standard errors in parentheses. The independent variable is the sample of repatriates over the native population above 15. All outcomes refer to changes between 1960 and 1981. LFP, Employment share, Share Employee, Share Entrepreneurship, Share Employer and Share Self-employed are calculated as shares over the native working age population. The unemployment rate refers to the share of unemployed over the native labor force. All regression contain dummies for NUTS 2 regions. Controls contain the following parameters in 1960: unemployed and entrepreneurs as share of working age population, inactive and population below 15 as share of total population, those with higher education as share of those above 15. IV1 is the instrument based on educational network effects, IV2 is the instrument based on network effects according to the colony lived in and IV3 is the basic Bartik instrument. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

bias. Notably, the first-stage coefficients for instrument 1 and 2 (i.e. specification (3) and (4)) are larger than for instrument 3 (i.e. specification (5)), indicating that the instruments based on network effects between subgroups predict more of the spatial

Table 2.4: Labor market effects of repatriates on female natives - baseline

Outcomes for female natives	OLS		IV		
	(1)	(2)	(3)	(4)	(5)
Δ LFP	-0.512 (0.581)	-1.082 (0.892)	-1.871** (0.691)	-1.845** (0.698)	-1.835** (0.690)
Δ Unemployment rate	-1.426 (0.733)	-0.655 (0.916)	0.069 (0.519)	0.073 (0.513)	0.084 (0.509)
Δ Employment share	0.087 (0.596)	-0.680 (0.906)	-1.587* (0.661)	-1.568* (0.663)	-1.562* (0.654)
Δ Share Employee	-0.771 (0.558)	-1.059 (0.731)	-1.919*** (0.573)	-1.900** (0.586)	-1.886*** (0.573)
Δ Share Entrepreneur	0.604 (0.500)	0.122 (0.497)	0.139 (0.458)	0.136 (0.459)	0.128 (0.452)
Δ Share Employer	-0.014 (0.031)	-0.050 (0.039)	-0.072* (0.034)	-0.071* (0.034)	-0.071* (0.034)
Δ Share Self-employed	0.618 (0.514)	0.171 (0.489)	0.211 (0.448)	0.207 (0.449)	0.199 (0.441)
Controls	NO	YES	YES	YES	YES
Instrument	-	-	IV1	IV2	IV3
First-stage coefficient	-	-	0.528	0.510	0.375
First-stage F-statistic	-	-	55.05	50.94	63.64
Observations	30	30	30	30	30

Notes: See notes in table 2.3. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

variation in repatriates' location than does the more general Bartik instrument. The estimated second-stage effects using the three instruments are all very similar both in terms of magnitude and significance.

OLS indicates a non-significant increase in male LFP, with a slight, non-significant decrease in unemployment and a non-significant rise in employment. IV reverses the signs of the estimates, suggesting positive selection: repatriates seem to settle in regions with better labor market prospects. The IVs mitigate this endogeneity, showing that a higher supply shock slightly decreases male LFP, decreases male employment, and

increases male unemployment. None of these effects is however, statistically significant. The effect on employment hides substantial heterogeneity between different types of employment. A higher supply shock leads to a substantial and significant decrease in male employment as employees. A 1 pp increase in the share of repatriates will lead to roughly a 4 pp reduction in the share of the labor force working as employees, on average. As the supply shock accounted for a 3.9 pp change in the share of repatriates, on average, the average decline in employment as employee caused by the shock amounts to 15.6 pp. This corresponds to an average reduction of about 22% compared to the pre-shock level of 71.6% in 1960. On the other hand, we observe a substantial increase in the share of male entrepreneurs. The supply shock increased the share of entrepreneurs by about 12 pp.²⁵ This corresponds to a rise of about 51.1% compared to the pre-shock level of 23.5%. An increase in self-employed individuals drives this increase: in the presence of relatively more repatriates, natives are more likely to be self-employed, but less likely to work as employers. All these effects are statistically significant at 10%.

For female natives, OLS indicates a non-significant decrease in LFP, unemployment rate and employment share following a higher shock. The changes in coefficients between OLS and IV estimates also suggest positive selection in the location of repatriates. The IV estimates indicate that the shock causes a statistically significant reduction in female LFP, stemming from a drop in the share of females employed, but no change in unemployment. On average, the shock introduced a 6.2 pp reduction in the share employed and a 7.4 pp reduction in the share working as employees. Given the low pre-shock level of 14.6% of women working as employees, the latter indicates a larger relative change than for men, corresponding to a decrease of about 50.1% (compared to 22% for men).²⁶ Contrary to men, women do not compensate for this loss

²⁵Calculated as 3.9×3.1 , i.e the average shock multiplied by the coefficient of the variable at hand.

²⁶As all these indicators recorded an increase between 1960 and 1981 (see Table 2.2), these negative effects imply that in regions with more repatriates, these indicator increased by less than they would have increased in the absence of the repatriates.

with an increase in self-employment. While we observe a slight, statistically significant drop in the share of employers, the overall increase in entrepreneurship is statistically non-significant. Females seem to move to inactivity, as reflected in the negative effect on female LFP.

2.6 Discussion of Results

Overall, the results imply that places of birth of repatriates are a strong predictor of settlement patterns of the Portuguese repatriates. Despite that, it seems that there is some positive selection, that is, that repatriates to some extent base their location decisions on economic fundamentals. This finding is in line with makela2017effect study of the Portuguese repatriates. In addition, while the repatriates caused no overall increase in unemployment several years after the inflow, there was some displacement of local workers following the arrival of the repatriates. Both male and female natives seem to be driven out of employment as employees by the shock, with a larger relative effect on females. This larger effect is consistent, with the findings of edo2017impact study of the French repatriation. While male natives manage to adjust and compensate for this loss by becoming self-employed, overall female employment is impacted negatively as female natives lose their employment as employees but do not record an increase in entrepreneurship. Instead, they leave the labor force altogether, with no significant increase in unemployment. This result is also consistent with Edo (2020).

These findings can be explained by a segmented labor market between male and female workers. The arrival of a massive number of working age adults is likely to decrease wages. Indeed, Carrington and De Lima (1996) document a sizeable decrease in real earnings. They then perform an exploratory regional analysis based on the construction sector wage (for both natives and repatriates), and suggest that the

decreases are related to the inflow of repatriates in each of the districts. Aggregated data from Statistics Portugal confirms that after a period of increases, real wages started to decrease in 1975, after the arrival of the repatriates (Pereirinha, 1980). Mäkelä (2017) finds that in 1977, actual average annual wages per worker were about 8% lower compared to the synthetic counterfactual, with a larger negative effect of -25% in 1970 and -55% in 1985, respectively. Although this is the overall average (i.e., including male and female, native and repatriate workers) wage, it is reasonable to assume that it reflects mostly the impact on male wages, given that the bulk of workers were males, on the one hand, and the supply shock was larger for males, as male repatriates were more likely to be part of the labor force than females. As of 1960, 71.6% of working age males worked as employees, compared to 16.4% of females. Given that the population of both genders was approximately the same, this implies that there were more than four times as many male employees *vis-à-vis* female employees. We also know, from de Carvalho (1980), that the gender wage gap was 64% in 1974, and 75% in 1978, a sharp change in just four years which can be explained by a decrease in male wages. These spare pieces of evidence confirm that following the inflow of the Portuguese repatriates, (i) real wages decreased, (ii) the gender wage gap decreased, driven by a decline in male wages, and, (iii) the market was segmented by gender, with significantly lower wages for women than for men.

With the decrease in male wages, some men were likely induced to voluntarily leave employment as employees to pursue self-employment. If employers in the late 1970s and early 1980s had a preference for male employees and employed women because they were relatively cheaper, the decrease in the gender wage gap reduced the relative price advantage of female labor, leading to female layoffs.²⁷

²⁷Even if both male and female wages decreased, the fact that the relative wage of females increased drives this effect. Moreover, a stronger displacement effect for women is consistent with the fact that female labor supply is generally more responsive to wage changes (Lloyd and Niemi, 1978).

Furthermore, Cardoso and Morin (2018) show that the relative scarcity of men in the Portuguese economy resulting from military drafting and emigration in the 1960s and early 1970s led to a demand-driven sharp increase in female LFP, making Portugal one of the European leaders in female LFP. The rise slowed down in the 1980s, coinciding with the arrival of the repatriates. Therefore, our results imply that after a demand-driven increase in female LFP, a supply shock leads to a slowdown in this trend. More specifically, the supply shock changed the relative wage of females, prompting a demand-driven decrease in female LFP. While Cardoso and Morin (2018) argue that the early increase in female LFP may have changed social norms in Portugal, our results question the persistence of the change. Once (predominately male) repatriates arrived, depressing male wages, women were once again driven out of the labor market, possibly due to a preference of employers for men. This indicates that there may still have been strong social norms against female employment.

Interestingly, Mäkelä (2017) finds a small short-run increase in unemployment in the first years after the repatriates' arrival, with no effect visible after 1980, and argues that this may be due to the low unemployment benefits at the time. These low benefits may have induced displaced females to move to inactivity rather than unemployment. Conversely, displaced male natives moved to self-employment. The increase in self-employment and decline in employers fits in with the fact that, while repatriates are less likely to be self-employed compared to natives, they are more likely to be employers (see Table 2.2). Therefore, repatriates seem to drive natives out of occupations in which they are more prevalent. This is what Peri (2016) calls margins of adjustment: native workers move away from tasks or skills provided by immigrants and towards tasks or skills complemented by them. We do not have sufficient information on the respective characteristics of self-employment and work as employers to make any conclusive remarks on their relative quality. Self-employment without having any

employees may however be an indicator for lower quality entrepreneurship than work as an employer. Thereby, male natives seem to be driven out of employment as employees and into, possibly, lower-quality entrepreneurship. This is bound to reflect an overall decrease in their position in the earnings ranking and may explain part of the negative sentiment described in Section 2.2.

2.7 Robustness

In this section, we once again take advantage of the fact that we have access to a individual level data covering the universe of repatriates in 1981 to show that our baseline results are robust to several checks.

A possible concern relates to the fact that, as we conduct our analysis at the NUTS 3 level, we have a small sample size. In Table 2.5 and Table 2.6 we run the regressions at the municipality level, with 303 observations, for males and females, respectively.²⁸ This exercise further allows us to mitigate another concern. If we think that the repatriates may have caused internal migration among natives, municipal level results should be less negative than those aggregated at the NUTS 3 regions, as the former would be contaminated by spatial spillovers due to internal migration. In any case, if anything, we observe the opposite: our results for the employment effects at the municipality level are more negative than those at NUTS 3-level.

In Tables 2.7 and 2.8, we display more robustness tests, using the education networks specification, to account for a number of possible remaining concerns related to our econometric analysis. More specifically, in column (1), we replace the instrumental

²⁸Braun et al. (2020) point out that the choice of spatial units can have an important impact on the estimated coefficients. While there were 305 municipalities in Portugal in 1960, the 1960 census misses data for two municipalities. The number of repatriates in the municipality-level regression is smaller, as we exclude all repatriates who moved municipalities (rather than NUTS 3 regions) from the sample.

Table 2.5: Labor market effects of repatriates on male natives - municipality level

Outcomes for male natives	OLS		IV		
	(1)	(2)	(3)	(4)	(5)
Δ LFP	-0.053 (0.110)	-0.054 (0.109)	-0.395 (0.243)	-0.353 (0.257)	-0.399** (0.154)
Δ Unemployment rate	-0.069 (0.040)	0.026 (0.035)	0.132 (0.069)	0.138 (0.075)	0.135** (0.051)
Δ Employment share	0.015 (0.114)	-0.087 (0.116)	-0.518* (0.256)	-0.481 (0.268)	-0.526** (0.165)
Δ Share Employee	-0.595** (0.227)	-1.247*** (0.181)	-2.829*** (0.413)	-2.894*** (0.442)	-2.170*** (0.238)
Δ Share Entrepreneur	-0.030 (0.174)	0.477** (0.174)	1.702*** (0.362)	1.819*** (0.408)	1.018*** (0.210)
Δ Share Employer	-0.188** (0.060)	-0.135* (0.061)	-0.110 (0.110)	-0.076 (0.119)	-0.166* (0.067)
Δ Share Self-employed	0.159 (0.173)	0.612** (0.189)	1.812*** (0.372)	1.896*** (0.410)	1.184*** (0.226)
Controls	NO	YES	YES	YES	YES
Instrument	-	-	IV1	IV2	IV3
First-stage coefficient	-	-	0.327	0.309	0.378
First-stage F-statistic	-	-	49.93	39.96	559.05
Observations	303	303	303	303	303

Notes: Robust standard error in parentheses. The independent variable is the sample of repatriates over the native population above 15. All outcomes refer to changes between 1960 and 1981. LFP, Employment share, Share Employee, Share Entrepreneurship, Share Employer and Share Self-employed are calculated as shares over the native working age population. The unemployment rate refers to the share of unemployed over the native labor force. All regression contain dummies for NUTS 2 regions. Controls contain the following parameters in 1960: unemployed and entrepreneurs as share of working age population, inactive and population below 15 as share of total population, those with higher education as share of those above 15. IV1 is the instrument based on educational network effects, IV2 is the instrument based on network effects according to the colony lived in and IV3 is the basic Bartik instrument. The regressions are run at the municipality level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table 2.6: Labor market effects of repatriates on female natives - municipality level

Outcomes for female natives	OLS		IV		
	(1)	(2)	(3)	(4)	(5)
Δ LFP	-0.231 (0.222)	-0.499* (0.198)	-2.199*** (0.494)	-2.414*** (0.556)	-1.130*** (0.262)
Δ Unemployment rate	-0.724*** (0.198)	-0.098 (0.160)	0.626* (0.263)	0.685* (0.290)	0.299 (0.167)
Δ Employment share	0.068 (0.227)	-0.379 (0.197)	-2.085*** (0.478)	-2.292*** (0.534)	-1.060*** (0.256)
Δ Share Employee	-0.252 (0.165)	-0.575*** (0.137)	-2.192*** (0.442)	-2.344*** (0.490)	-1.247*** (0.201)
Δ Share Entrepreneur	0.100 (0.158)	-0.004 (0.160)	0.019 (0.310)	-0.028 (0.317)	0.056 (0.238)
Δ Share Employer	-0.009 (0.010)	-0.019* (0.009)	-0.060** (0.019)	-0.061** (0.020)	-0.045*** (0.011)
Δ Share Self-employed	0.110 (0.159)	0.015 (0.160)	0.079 (0.310)	0.033 (0.318)	0.100 (0.239)
Controls	NO	YES	YES	YES	YES
Instrument	-	-	IV1	IV2	IV3
First-stage coefficient	-	-	0.327	0.309	0.378
First-stage F-statistic	-	-	49.93	39.96	559.05
Observations	303	303	303	303	303

Notes: Robust standard error in parentheses. The independent variable is the sample of repatriates over the native population above 15. All outcomes refer to changes between 1960 and 1981. LFP, Employment share, Share Employee, Share Entrepreneurship, Share Employer and Share Self-employed are calculated as shares over the native working age population. The unemployment rate refers to the share of unemployed over the native labor force. All regression contain dummies for NUTS 2 regions. Controls contain the following parameters in 1960: unemployed and entrepreneurs as share of working age population, inactive and population below 15 as share of total population, those with higher education as share of those above 15. IV1 is the instrument based on educational network effects, IV2 is the instrument based on network effects according to the colony lived in and IV3 is the basic Bartik instrument. The regressions are run at the municipality level.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

variable according to Kronmal (1993). In column (2) and (3), we exclude particular regions from the sample, while, in columns (4) and (5), we change and remove the regional fixed effects. Column (6) includes the subsample of repatriates who changed regions within Portugal before 1981. Lastly, in columns (7) and (8), we consider gender-specific shocks. We present the main tables with OLS and the three (shift-share) IV estimates, for all these exercises, in the Appendix.

Table 2.7: Labour market effects of repatriates on male natives - robustness

IV 1 - based on educational network effect								
Outcomes for male natives	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Δ LFP	-0.012 (0.011)	0.074 (0.278)	-0.171 (0.270)	-0.248 (0.317)	-0.590 (0.394)	-0.107 (0.294)	0.169 (0.577)	-0.218 (0.291)
Δ Unemployment rate	0.014* (0.006)	0.160 (0.123)	0.268* (0.117)	0.281 (0.161)	0.309 (0.161)	0.214 (0.119)	0.350 (0.242)	0.230 (0.120)
Δ Employment share	-0.026* (0.012)	-0.100 (0.309)	-0.439 (0.287)	-0.522 (0.356)	-0.874* (0.437)	-0.328 (0.320)	(0.650)	-0.449 (0.323)
Δ Share Employee	-0.186*** (0.036)	-3.384*** (0.790)	-4.186*** (0.924)	-3.965*** (0.891)	-3.484*** (0.964)	-3.915*** (0.918)	-6.875*** (2.501)	-3.915*** (1.009)
Δ Share Entrepreneur	0.141*** (0.034)	2.748*** (0.692)	3.260*** (0.812)	2.939*** (0.773)	1.969* (0.871)	3.082*** (0.774)	5.697** (2.085)	3.009*** (0.847)
Δ Share Employer	-0.027*** (0.007)	-0.635*** (0.150)	-0.687*** (0.167)	-0.538** (0.188)	-0.859*** (0.260)	-0.677*** (0.159)	-1.302*** (0.367)	-0.629*** (0.179)
Δ Share Self-employed	0.168*** (0.038)	3.383*** (0.769)	3.946*** (0.9222)	3.476*** (0.840)	2.829** (0.905)	3.759*** (0.861)	6.999*** (2.353)	3.638*** (0.967)
Controls	YES	YES	YES	YES	YES	YES	YES	YES
First-stage coefficient	0.507	0.569	0.501	0.517	0.371	0.527		0.745
First-stage F-statistic	19.75	87.06	65.28	75.84	13.00	46.80	55.05	9.99
Observations	30	27	25	30	30	30	30	30

Notes: Robust standard error in parentheses. For a description of outcomes, controls and instruments see Table 2.3. (1) uses the Krommal specification, that is, the independent variable is the log of repatriates, with the controls including the log of natives above 15 in 1981. (2) - (5) use the share of repatriates over natives as independent variable. (2) excludes Lisbon, Setúbal and Algarve from the regression, (3) includes only three regional dummies (4) runs the regression at municipality level (5) includes those repatriates who changed NUTS 3 regions in the sample. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table 2.8: Labour market effects of repatriates on female natives - robustness

	IV 1 - based on educational network effect							
Outcomes for female natives	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Δ LFP	-0.119*** (0.023)	-1.676* (0.654)	-2.038*** (0.270)	-2.319* (1.027)	-1.421 (1.475)	-1.872*** (0.690)	-3.427* (1.661)	-1.972*** (0.711)
Δ Unemployment rate	0.043 (0.024)	0.021 (0.527)	0.352 (0.468)	0.114 (0.536)	0.462 (0.594)	0.069 (0.519)	0.240 (0.959)	0.106 (0.511)
Δ Employment share	-0.114*** (0.024)	-1.439* (0.642)	-1.807*** (0.591)	-2.020* (0.970)	-1.432 (1.275)	-1.595* (0.675)	-2.972 (1.524)	-1.680* (0.676)
Δ Share Employee	-0.124*** (0.025)	-1.939*** (0.542)	-2.145*** (0.431)	-2.010*** (0.600)	-1.848* (0.779)	-1.921*** (0.576)	-3.511* (1.474)	-1.912*** (0.583)
Δ Share Entrepreneur	0.005 (0.026)	0.315 (0.452)	0.161 (0.465)	-0.193 (0.589)	0.218 (0.681)	0.139 (0.459)	0.176 (0.824)	0.014 (0.444)
Δ Share Employer	-0.004* (0.002)	-0.064 (0.033)	-0.076* (0.032)	-0.072 (0.038)	-0.091* (0.036)	-0.072* (0.034)	-0.127 (0.070)	-0.075* (0.035)
Δ Share Self-employed	0.009 (0.025)	0.378 (0.438)	0.237 (0.450)	-0.121 (0.568)	0.309 (0.667)	0.211 (0.448)	0.303 (0.824)	0.089 (0.434)
Controls	YES	YES	YES	YES	YES	YES	YES	YES
First-stage coefficient	0.507	0.569	0.237	0.517	0.371	0.527	1.010	
First-stage F-statistic	19.75	87.06	55.28	75.84	13	46.80	55.05	92.34
Observations	30	27	25	30	30	30	30	30

Notes: See notes in table 2.7. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Specification (1) in Table 2.7 and Table 2.8 shows that the results are robust to the specification of Kronmal (see Tables B4 and B5 in the Appendix). As pointed out by Clemens and Hunt (2019), the fact that the shift-share instrument and the endogenous variable have a common denominator has the potential to bias second-stage IV estimates.²⁹ To show that the IV results are not driven by the correlation between the predicted and actual number of natives, we follow their suggestion to apply the correction of Kronmal (1993). Consequently, we instrument for the log of repatriates with the log of the predicted repatriates based on places of birth, including the log of the native population in 1981 as a control. For males, there are some slight changes in significance, suggesting slightly more adverse effects. For females, all results are in line with the main specification.

We then take into consideration possible confounding contemporaneous economic shocks: the oil crises of 1973 and 1979, the end of the Portuguese Colonial War, which caused a reduction in military employment of about 200,000 people, the revolution and democratisation of the country, and the end of emigration to France and West Germany. These shocks may be a problem if they caused differential regional impacts, hampering our identification strategy, which is based on the regional distribution of the repatriates.

The return of soldiers from the Colonial War is unlikely to asymmetrically affect regions within Portugal. As noted by Card and Cardoso (2012), the options for self-selecting out of drafting for the Colonial War were severely limited, indicating that returning soldiers likely settled evenly across the country. The fact that the Colonial War had a relatively low death toll, with 8,290 dead soldiers recorded (Cardoso and Morin, 2018), adds additional confidence that the impacts of the war do not significantly bias our results.

On top of including dummy variables for the large NUTS 2 regions in our baseline

²⁹While we are not using the exact same denominator in the endogenous variable as we are using in the instruments, both denominators have some parts in common.

specification, the use of (shift-share) IVs should further reduce the concern for bias by confounding factors. We nevertheless demonstrate that the results are robust to excluding regions that could be more prone to such factors. As explained by Carrington and De Lima (1996), Lisbon and Setúbal were the center of the most dramatic political and economic conflicts following the democratization of Portugal, with communist-led unions effectively promulgating compulsory unionization in these areas, whereas there was a large drop in tourism in the Algarve after 1975. Specification (2) in Table 2.7 and Table 2.8 shows that the results are robust to excluding these three regions (see Tables B6 and B7 in the Appendix).³⁰ In column (3) of Table 2.7 and Table 2.8, we underline that the results are robust to excluding the NUTS 2 region of Alentejo (see Tables B8 and B9 in the Appendix). As described by Pires de Almeida (2016), the Alentejo region was subject to a profound agrarian reform following the military coup in Portugal, which altered many aspects of the region's political, economic, and social reality.

The inclusion of NUTS 2 region dummies, however, may increase the restrictiveness of the regression and capture part of the impact we want to estimate, as some repatriates may positively affect the growth of the NUTS 2 region, and can, in turn, influence native employment. We deal with this by showing that our baseline results remain unchallenged if we use a smaller set of fixed effects, or if we remove them. Specification (4) in Tables 2.7 and 2.8 confirms that the results are robust to including a less demanding set of three regional fixed effects, while, in Specification (5), we remove the NUTS 2 fixed effects (see Tables B10 and B11 for the first exercise and Tables B12 and B13 for the second).³¹

³⁰Since we are using different spatial units than these two studies, we exclude the NUTS 3 most closely corresponding to those excluded districts, namely Algarve, Grande Lisboa and the Peninsula of Setúbal.

³¹The three dummy variables are constructed as follows: the first dummy comprises the NUTS 3 regions Center and North; the second dummy comprises Alentejo, Algarve, and Lisbon; the third dummy comprises the islands of Azores and Madeira.

We now construct several subsamples of repatriates to show that the results do not depend on the previously applied sampling criteria. So far, all our specifications excluded all repatriates who changed NUTS 3 regions between 1979 and 1981. Specification (6) in Tables 2.7 and 2.8 highlights that including these individuals in our immigration shock does not change the baseline results. Moreover, Table B14 and Table B15 in the Appendix show that the findings for males and females, respectively, hold if we exclude students, adapt the age range of repatriates to 15 to 64 years old and to 25 to 59 year old in 1981, exclude all inactive repatriates, and if we include only Portuguese-born repatriates.³² Also related to how we measure the immigration shock, we show that results are robust to using the preexisting workforce as suggested by (Card and Peri, 2016), rather than the instrumented post-shock values, as denominator. This may be important as the native workforce in 1981 may be positively correlated with native labor force participation due to local demand shocks caused by the influx of repatriates. The results in column (7) of of Tables 2.7 and 2.8 are more negative than baseline (see Tables B16 and B17 in the Appendix).

Finally, and given that men and women could be imperfect substitutes in production (Edo and Toubal, 2017), we replace the ratio of repatriates m_r by a gender-specific repatriate share (i.e., we compute m_r in the sample of males when estimating its impact on the employment of native men, and compute m_r in the sample of females when estimating its impact on the employment of native women). The results, as shown in specification (8) of Tables 2.7 and 2.8, are very similar to our baseline (see Tables B18 and B19 in the Appendix).

³²For space constraints, we only show the results for IV1. Results for the other IVs are likewise robust and available upon request from the authors.

2.8 Conclusion

This paper offers new evidence on the effect of a large-scale, one-time supply shock on native labor market outcomes, exploiting the end of the Portuguese Colonial War and subsequent repatriation of half a million ethnic Portuguese. As documented by various scholars, (e.g. Borjas and Monras (2017), Friedberg and Hunt (1995)) such natural experiments offer an excellent setting to identify causality by combining an exogenous push factor with the use of an IV strategy to control for the endogenous allocation of migrants. We use a novel shift-share instrument, exploiting the unique characteristic of the Portuguese repatriates, namely the fact that they had still been born in Portugal and that many of them returned to their birthplace. The obtained results are in line with and extend upon Mäkelä (2017) study of the Portuguese repatriation.

We find robust evidence for adjustment in the labor market following the arrival of the repatriates, with a larger displacement of females. For both female and male natives, employment as employees decreases. Men compensate for this by becoming self-employed entrepreneurs, thereby not experiencing a decrease in overall employment. Women instead move to inactivity, which is reflected in the significant adverse effect on overall employment and LFP. A possible mechanism through which the inflow may have led to these outcomes is through a decrease in wages, which is supported by Mäkelä (2017) study. Furthermore, the fact that the inflow of the repatriates hampered the previous sharp increase in female LFP in Portugal may be an illustration of the persistence of social norms against female employment.

At the same time, we find no evidence for an increase in neither male nor female unemployment. Notably, we do not show any effects in the very short run, but measure outcomes several years after the inflow. While there may have been an increase in native unemployment in the first years after the arrival of the repatriates, we conclude that about 6 years later, the labor market had accommodated the large inflow, with

no increased native unemployment detectable. This result is striking given the size of the inflow and the negative stereotyping of the repatriates at the time. The lack of a large increase in unemployment may partly be attributable to the policy making by the Portuguese state to support the integration of the repatriates. The government rented hotels in different parts of the country to avoid clustering in certain regions, gave support to small businesses and provided jobs as public servants to repatriates.

Due to the peculiar nature of the repatriates, the findings of this study may not be applicable to all kinds of migration. The take-away for policymakers is, however, that lending support to arriving migrants may give the structure of the labor market sufficient time to adjust and accommodate even substantial inflows of migrants within just some years. The study further illustrates the importance of viewing female and male labor market outcomes separately to grasp the whole picture and to be able to design policy interventions to overcome potential challenges related to large-scale migration. Future research could explore the importance of the state support provided to the repatriates in hampering potential adverse effects of the repatriation, or look into the long-term effect of the repatriates on gender norms in Portugal.

Chapter 3

Brown Sugar, how come you taste so good? The impact of a soda tax on prices and consumption¹

3.1 Introduction

A growing number of governments around the world are introducing sugar-sweetened beverage taxes (SSB or soda taxes for short) to change consumer behavior, generate revenue, and incentivize manufacturers to reformulate products. In fact, the World Health Organization's Global Action Plan for the Prevention and Control of Non-Communicable Diseases 2013-2020 added soda taxes to their list of recommended

¹This chapter is written in co-authorship with Judite Gonçalves. The authors are grateful for comments from two anonymous reviewers, Pedro Pita Barros, Judit Vall Castello, Alexander Coutts, David Cutler, Randall Ellis, Jonathan Gruber, Albert Ma, Martin O'Connell, Jim Poterba, Nigel Rice, Susana Peralta, José Tavares, and participants at a Nova SBE-ISEG seminar, a Nova Health Economics and Management KC meeting, the VIII Taller EvaluAES (Universitat de les Illes Balears), the Workshop on Economics of Taxation Social Expenditure (Universitat de Barcelona), the XXXIX Jornadas de la Asociación de Economía de la Salud (Albacete), the 13th Annual Meeting of the PEJ (Évora), the International Health Economics Association Congress (Basel), the 2019 EuHEA PhD student-supervisor conference, and the conference of the Portuguese Health Economics Association. We thank Mafalda Luís for excellent technical assistance.

policies, as there is overwhelming evidence linking SSB consumption to diseases such as obesity and diabetes (see e.g. Malik et al., 2010a and Malik et al., 2010b for a review of the evidence).² As of July 2019, more than 40 countries have implemented or are on the verge of implementing SSB taxes, including for example Mexico, France, the UK, Portugal, South Africa, as well as Catalonia in Spain and several cities in the US (e.g. Berkeley, California, Boulder, Colorado, and Philadelphia, Pennsylvania; see Global Food Research Program UNC, 2019).

This study assesses the impacts of the Portuguese soda tax, implemented in February 2017, on soda prices and consumption. We use extremely rich product-store-month-level sales data from a large retailer with 400+ stores. To estimate the causal impacts of the tax, we adopt difference-in-differences and event study designs, using bottled water as the comparison group and controlling for several vectors of fixed effects. We explore the impacts of the soda tax on four distinct groups of soda products. This distinction takes into account the structure of the Portuguese soda tax, which taxes more heavily drinks with higher sugar content and has led manufacturers to alter the recipes of several drinks. All analyses are performed on both unbalanced and balanced panels; the latter including only the most popular drinks. Lastly, we study responses to the tax in different periods: when it was only being discussed in the media, when it was formally considered and debated in the parliament, and finally when it was enacted.

Our main findings include first, substantial pass-through of the tax to consumer prices, at almost 100% for high-sugar drinks and above 100% for drinks with less sugar; second, a substantial drop in consumption of drinks with relatively low sugar content, but otherwise limited impacts of the tax on consumption; and third, stockpiling in the quarter before the tax was implemented.

²Macro-level results of a recent study suggest that for each additional teaspoon of added sugar per person, spending on diabetes per capita rises by as much as 26.8% and the growth rate of total health care expenditure per capita increases by 1.8 percentage points in the long run (Castro, 2017).

The economic reasoning underlying SSB taxes is that of making consumers internalize the costs they impose on themselves (internalities) and on others (externalities) from consuming too much SSBs. Internalities have to do with individuals ignoring the effects of consuming SSBs on their health, because they are misinformed or because they fail to consider health problems that tend to appear far in the future.³ Externalities, in this case, are mainly healthcare costs of treating conditions related with SSB consumption, that are shared by everyone through public or private insurance.

SSB taxes have different welfare effects if we focus on internalities or externalities. If poorer individuals tend to consume more SSBs than richer ones, then the internality benefits of the tax are likely to be progressive, while the externality benefits will be regressive. In the end, the total regressivity of a SSB tax will also depend on the allocation of the tax revenues (see e.g. Allcott et al., 2019b for a detailed exposition of this issue). SSB taxes opponents' main argument is precisely regressivity; however, when internality benefits and tax revenue allocation are taken into account, the evidence suggests that the benefits of SSB taxes are likely to be flat across the income distribution, or possibly the highest for the lowest-income consumers, at least in the US (Allcott et al., 2019a; see also Dubois et al., 2018, Etilé and Sharma, 2015, and Etilé et al., 2018).

Soda taxes can decrease the intake of sugar from SSBs, and consequently lead to improvements in population health, through three channels. The first channel is by increasing prices. In principle, consumers respond negatively to higher soda prices. However, whether soda prices increase or not depends, first, on whether manufacturers and retailers pass on the tax to the consumer, or alternatively (partly) absorb it. This in turn depends on manufacturers' and retailers' market power as well as the price

³Pigouvian taxation of externalities has also been advocated in the context of unhealthy foods (Cremer et al., 2016, Haavio and Kotakorpi, 2011, O'Donoghue and Rabin, 2006), cigarette consumption (Gruber and Kőszegi, 2004), and energy markets (Allcott et al., 2014).

elasticity of demand for SSBs. Manufacturers may also reformulate recipes in order to avoid (higher) taxes. Thus, the first question to be addressed concerns the impact of soda taxes on consumer prices (i.e. price pass-through). Available evidence on enacted soda taxes shows significant heterogeneity in price pass-through across countries and specific drink groups, ranging from less than 40% to more than 100% (Aguilar et al., 2018; Arteaga et al., 2017; Berardi et al., 2016; Bollinger and Sexton, 2018; Capacci et al., 2019; Cawley and Frisvold, 2017; Cawley et al., 2018a; Cawley et al., 2018c; Etilé et al., 2018; Falbe et al., 2015; Grogger, 2017; Rojas and Wang, 2017; Seiler et al., 2019).

The second channel is precisely the incentive for manufacturers to reformulate recipes towards formulas with less added sugar. If SSBs contain less sugar, then sugar intake will be lower, by construction. Nevertheless, if consumers dislike the new recipes, they may substitute towards comparatively sweeter SSBs or other (unhealthy) products.

The third channel is increased consumer awareness. Regardless of any impact on price, media coverage and public debate around soda taxes may raise consumer awareness towards the detrimental effects of sugar intake and SSB consumption for health, and consequently decrease SSB consumption. Globally, salience is a key component of a soda tax, as there is robust evidence that consumers underreact to taxes that are not salient (Chetty et al., 2009). One paper finds that soda consumption at the University of California campus, in Berkeley, fell immediately after the Berkeley soda tax was passed, two years before prices increased on-campus (Taylor et al., 2019a).

Other factors to keep in mind are substitution towards untaxed products that also generate internalities and externalities, e.g. candy or beer, and leakage, namely the possibility to purchase soda outside of the taxed jurisdiction (Allcott et al., 2019b). There is suggestive evidence of substitution towards diet soda in countries where it is untaxed (e.g. Allcott et al., 2019a; Castelló and López-Casasnovas, 2018). For example

Finkelstein et al. (2013) don't find evidence of substitution towards sugary foods or pizza. As for leakage, Bergman and Hansen (2017) find that the tax pass-through for beer and soda in Denmark is an increasing function of distance to the German border. Bollinger and Sexton (2018), Cawley and Frisvold (2017), Cawley et al. (2018b), and Seiler et al. (2019) also find evidence of cross-border shopping as a response to the soda taxes implemented in Berkeley and Philadelphia.

Most existing studies on consumer responses to soda taxes enacted in Mexico, Chile, Catalonia, Berkeley, and Philadelphia find that consumption of soda decreased, from 6% in Mexico to more than 20% in Berkeley and Philadelphia (Aguilar et al., 2018; Arteaga et al., 2017; Castelló and López-Casasnovas, 2018; Cawley et al., 2018b; Falbe et al., 2016; Mora et al., 2018; Nakamura et al., 2018; Seiler et al., 2019; Taylor et al., 2019a). Due perhaps to specific data or methodologies, a few studies on the Berkeley soda tax don't find significant impacts on consumption (Bollinger and Sexton, 2018; Rojas and Wang, 2017) and one finds a small positive impact (Debnam, 2017).

The literature so far has mainly relied on survey or home-scan data (e.g. Kantar World Panel) or hand-collected data on a few products or stores, with only a few studies having access to retail data (Berardi et al. (2016); Castelló and López-Casasnovas (2018); Seiler et al., 2019). To try to estimate causal impacts, the vast majority of studies employ difference-in-differences designs, either comparing taxed products to untaxed ones (e.g. bottled water), or regions where soda is taxed to regions where it is not.

The studies cited above estimate the impacts of enacted soda taxes around the world ex-post. Several other studies provide ex-ante estimates of the impacts of soda taxes, by estimating demand systems for soda and related products and then simulating the impact of the introduction of a soda tax. For example, Finkelstein et al. (2013) and Xiang et al. (2018) estimate that a 20% tax-induced increase in SSB prices would

decrease per capita energy purchases by 24-29 kcal/day (see also Andalón and Gibson, 2018; Caro et al., 2018; Dubois et al., 2018; Etilé and Sharma, 2015; Gomo and Birg, 2018; Harding and Lovenheim, 2017).

To summarize, most studies on the ex-post impacts of soda taxes implemented in France, Mexico, Chile, Spain, and the US find increases in SSB prices and drops in SSB consumption. However, the magnitudes of the effects differ substantially across countries and studies, reflecting on the one hand the different tax rates and designs across countries, and on the other the different types of data and methodological approaches employed.

As for Portugal, non-scientific evidence indicates that between 2016 and 2017 alone, lemony drinks' and fruit-flavored soda's average sugar content decreased by 32.2% and 17.3% respectively (e.g. 7Up, Fanta; dos Santos, 2018). Overall, the change in the caloric content per 100 millilitres of non-alcoholic beverages was -11% from 2016 to 2017 (-21% from 2013 to 2017; Grupo de Trabalho, 2018). Until December 2017, the share of products with 80 grams of sugar or more per liter decreased from 61% to 37.9% of the sales volume (Grupo de Trabalho, 2018). Aggregate data also indicate a 15% reduction in the total amount of sugar intake in 2017, arising from a transfer of consumption from high-sugar drinks to drinks with less than 80 grams of sugar per liter (Goiana-da Silva et al., 2018). Of course, this mixes together the effect of recipe reformulations and any potential substitution effects.

This study adds to the previous literature by assessing for the first time the causal impacts of the Portuguese soda tax. We start by determining the extent of pass-through to consumer prices, and then analyze the impacts of the tax on soda consumption.

We make three main contributions. First, soda taxes that vary according to drinks' sugar content, penalizing more heavily drinks with a lot of sugar, are increasingly popular. In particular, this tax design led manufacturers in Portugal to change recipes

in order to pay a lower tax, and we distinguish between the effects of the soda tax on drinks that remained above the threshold and those that saw their sugar content reduced. This is an unexplored issue in previous studies.

Second, we have extremely detailed product-store-month-level data from a large retailer with more than 400 stores distributed across the country and 21% market share. Our data are nationally representative and allow us to estimate the causal impacts of the tax through a difference-in-differences design, using bottled water as the comparison group and controlling for several vectors of fixed effects. Few previous studies are able to control for potential confounders (e.g. preferences) as rigorously as we are. We also present event study specifications, providing evidence in favor of the parallel-trends assumption.

Third, we study the impacts of the soda tax before it was introduced, when it was only being discussed in the media and debated in the parliament, i.e. before and after prices changed due to the tax. This allows us to (partly) separate-out price effects from the other two channels (product reformulations and increased awareness). This is something that Taylor et al. (2019a) also explore, but our data are more detailed and representative, covering all soda products and the whole country, as opposed to a university campus. We also explore stockpiling effects in the quarter prior to the tax implementation.⁴

The remainder of this study is organized as follows: the next section presents the institutional background and sections 3.3 and 3.4 present the data and empirical strategy. Next, we present the results and finally, section 3.6 discusses the main findings and concludes.

⁴There is empirical evidence of stockpiling in the UK before the government levy on sugary drinks came into effect in 2018 (<https://www.bbc.com/news/uk-scotland-glasgow-west-42565363>).

3.2 Institutional Background

The Portuguese soda tax was implemented nationwide in February 2017 and received extensive media coverage. It was first mentioned almost one year earlier, in May 5, 2016.⁵ The tax proposal was included in the government budget proposal for 2017, submitted to the parliament for discussion in mid-October 2016. The soda tax was finally approved on December 28, 2016, together with the government budget for 2017 (Decree-law no. 42/2016).

The Portuguese soda tax applies to non-alcoholic drinks with added sugar or sweeteners, including liquid or powder concentrates.⁶ Tax-exempt products include (1) milk-, soy-, or rice-based drinks, (2) fruit-, algae-, or veggie-based juice and nectar, as well as cereal- and nut-based drinks, and (3) drinks considered essential for special dietary needs. Similarly to Catalonia, in Portugal there are different brackets defined based on drinks' sugar content. The amount of the tax is 0.08€ per liter for drinks with less than 80 grams of sugar per liter, and 0.16€ per liter for drinks with 80 grams or more sugar per liter. The usual 23% VAT adds up to the soda tax.⁷ So, unlike in Catalonia, neither drinks with relatively little added sugar nor light drinks are exempt, and drinks with a lot of sugar pay a comparatively higher tax.

The tax is levied on producers, not retailers. The different tax breaks are a way to incentivize producers to reduce drinks' sugar content in order to be subject to a lower tax (Allcott et al., 2019b; Cremer et al., 2019). In fact, several products that used to have more than 80 grams of sugar per liter now have 78-79 grams instead, paying a tax half as large. The UK, which also introduced a graduated soda tax, seems to be

⁵“Sumos e refrigerantes vão ter imposto extra”, in *Expresso* (<https://expresso.pt/sociedade/2016-05-28-Sumos-e-refrigerantes-vao-ter-imposto-extra#gs.ziLEots6>)

⁶Examples are Sunquick and Tang. In this case, the tax is calculated based on the sugar content of the final diluted mix.

⁷More precisely, the tax is 8.22 and 16.46 euro cents per liter, plus 23% VAT, which gives 10.11 and 20.25 euro cents per liter.

experiencing similar effects (Roache and Gostin, 2017). In Portugal, between 2016 and 2017 alone, lemony drinks' and fruit-flavored soda's average sugar content decreased by 32.2% and 17.3% respectively (e.g. 7Up, Fanta; dos Santos, 2018). The drinks that still have more than 80 grams of sugar per liter include cola-flavored and some energy drinks. Overall, the change in the caloric content per 100 millilitres of non-alcoholic beverages was -11% from 2016 to 2017 (-21% from 2013 to 2017; Grupo de Trabalho, 2018). Until December 2017, the share of products with 80 grams of sugar or more per liter decreased from 61% to 37.9% of the sales volume. Aggregate data also indicate a 15% reduction in the total amount of sugar intake in 2017, arising from a transfer of consumption from high-sugar drinks to drinks with less than 80 grams of sugar per liter (Goiana-da Silva et al., 2018). Of course, this mixes together the effect of recipe reformulations and any potential substitution effects.

One year after the tax was implemented, news reports suggest that consumer prices increased by about 25-30% and sales decreased by about 5%.⁸ Besides affecting recipes and prices, the soda tax may have changed consumer perception about soda. An online survey by Nielsen, conducted in the summer of 2017, revealed that 60% of Portuguese consumers pay attention to drinks' sugar content; 50% of respondents also admit that nutritional information may influence their purchasing behavior. In 2017, the Portuguese soda tax generated almost 70 million euros in revenue, 10 million euros less than expected.

⁸“Preços subiram 30% com imposto sobre refrigerantes”, in *Diário de Notícias* (<https://www.dn.pt/dinheiro/interior/precos-subiram-30-com-imposto-sobre-refrigerantes-9096084.html>)

3.3 Data

We use data from one of the two largest retailers in Portugal for the period February 2015-January 2018. This retailer has a share of 21% of the Portuguese retailer market and more than 400 stores that cover the mainland and Madeira territories comprehensively.⁹ Unlike in other countries, most retailers operating in Portugal (and all the main ones) are relatively similar in terms of the products/brands sold and price ranges.¹⁰ This contributes to the national representativeness of our analyses.

The dataset includes monthly information on sales and sales volume at the product and store levels, from which we can compute unit prices. Prices include VAT and other taxes; unlike in the US, in Portugal price tags include any applicable taxes. We can identify products by name/brand, and container size. A product corresponds to a specific bar code, meaning that a 1-liter bottle and a 33-centiliter can of Coca-Cola are two distinct products, for instance.

Geographically, we know only if a store is located in the North, Center, South, or Madeira regions. These regions display both between and within heterogeneity, e.g. they include both rural and urban areas. In addition, we can identify stores located in the two main metropolitan areas (Lisbon and Oporto), and stores located within 30 kilometers from the border with Spain. Unfortunately, we lack more detailed store locations or client information, preventing us from investigating potentially heterogeneous responses by local income level or other characteristics.

We define four main *treated* product groups, directly affected by the tax, based on the drink's sugar content. Information on each drink's sugar content was gathered from online sources and field visits to the supermarket in mid 2018, and validated,

⁹“Quota de mercado da Sonae MC aproxima-se dos 22%”, in *Jornal Económico* (<https://jornaleconomico.sapo.pt/noticias/quota-de-mercado-da-sonae-mc-aproxima-se-dos-22-350698>)

¹⁰“Preços nos supermercados: Jumbo volta a ganhar” (<https://www.deco.proteste.pt/familia-consumo/supermercado/noticias/precos-nos-supermercados-jumbo-volta-a-ganhar>)

for the most part, by the producers or the Portuguese Association of Non-Alcoholic Beverages (PROBEB) by email.¹¹ The first group includes drinks with more than 80 grams of sugar per liter (*High Sugar, HS*); examples are Coca-Cola and Red Bull. The second group includes drinks with sugar levels just below that threshold (*Medium Sugar, MS*); e.g. Fanta, 7Up. According to the available evidence, these drinks experienced reductions in their sugar contents; i.e. they contained more than 80 grams of sugar per liter before the tax was implemented (section 3.2). The third group includes drinks with lower sugar levels, mostly below 70 grams per liter, unlikely to have dropped from more than 80 grams per liter pre-tax (*Low Sugar, LS*), such as most iced tea and flavored water drinks. The fourth group includes zero-added sugar/artificially-sweetened drinks (*Zero Sugar, ZS*); e.g. Coca-Cola Zero, Diet 7up. Note that this classification is fixed over time —products do not move from one group to another. In sum, *High Sugar* drinks pay the highest tax (0.16€ per liter +VAT), and all other drinks pay the lowest tax (0.08€ per liter +VAT).

Our comparison group is *Water*. We follow for example Alsukait et al. (2020), Etilé et al. (2018), and Taylor et al. (2019b), who also consider bottled water as a comparison product. There are several reasons why we believe that water is a good comparison group in our setting. First, water is neither taxed nor likely to be indirectly affected by the tax. Cawley et al. (2019) and Seiler et al. (2019) explore the impacts of the Philadelphia soda tax on water consumption and find no evidence of substitution of soda for bottled water. Some other studies from completely different geographies, including Saudi Arabia or France, also find no evidence of such substitution (Alsukait et al., 2020; Capacci et al., 2019). Second, with the exception of sugar, the water-bottling industry uses the same inputs as the soda industry (e.g. machines, electricity, water, plastic/glass). So, water and soda are likely to share similar cost structures in packaging,

¹¹A few drinks with unknown sugar content that accounted for very few observations or total sales were excluded. Liquid and powder concentrates were also dropped.

marketing, and logistics (Etilé et al., 2018). Recent trends against plastic packaging should also affect both soda and bottled water in a similar manner. Third, soda brands have very low market shares in the bottled water segment, which mitigates any strategic manipulation of prices. Coca-Cola Portugal and Sumol-Compal are the main competitors in the Portuguese soda market, while the bottled water market is highly fragmented, with more than 30 brands/firms competing. In our data, the two water brands distributed by Coca-Cola Portugal are not represented, and the one brand commercialized by Sumol-Compal, *Serra da Estrela*, represents only 1% of sales volume. Fourth, most people simply drink water from the tap, making substitution of soda for bottled water less likely (Instituto Ricardo Jorge, 2016).

We conduct our analyses on the full sample (Unbalanced Panel) that includes all products, sold in any store in any month over the period February 2015-January 2018. For comparison, we also estimate our models on a restricted sample (Balanced Panel) that includes only products available in *all* stores in *every* month over the period February 2015-January 2018, i.e. the most popular drinks in common sizes. Descriptive statistics for both samples are presented in Table 3.1.

Figures C2 to C5 in the Appendix show the evolution of prices and quantities sold for each treatment group versus water, in the unbalanced panel (Panel A) and the balanced one (Panel B). The graphs display similar patterns for water and each of the treatment groups in the pre-tax period, suggesting that the parallel-trends assumption holds (i.e. that water is a good comparison group). Overall, the price of water is constant over the entire period under analysis (left-hand side graphs), at slightly less than 50 euro cents per liter, on average; quantity of water sold also exhibits a flat trend but with some seasonality (right-hand side graphs). All product groups show clear increases in prices when the tax is implemented —especially in the unbalanced panel—, but no clear changes in quantities sold.

Table 3.1: Descriptive statistics

	Full	Before	Discuss Price in euros	Approval	Implement	Full	Before	Discuss Quantity sold (liters)	Approval	Implement
<u>A. Unbalanced Panel</u>										
Comparison group: <i>Water</i>										
Avg	0.42	0.41	0.42	0.42	0.43	1899.53	1863.68	2273.06	1479.55	1872.23
SD	0.30	0.27	0.28	0.28	0.33	5344.12	5354.86	6127.92	4232.96	5184.60
Obs	295359	114319	48036	26012	106992	295359	114319	48036	26012	106992
High Sugar (<i>HS</i>)										
Avg	1.84	1.72	1.69	1.76	2.06	216.73	237.15	276.45	207.09	170.00
SD	1.61	1.60	1.62	1.56	1.61	1257.36	977.60	1980.30	1471.38	1045.77
Obs	352607	139868	57137	27071	128531	352607	139868	57137	27071	128531
Medium Sugar (<i>MS</i>)										
Avg	1.00	0.95	0.91	0.96	1.10	228.25	221.28	251.94	227.33	224.03
SD	0.50	0.47	0.46	0.48	0.53	503.33	494.38	522.35	539.00	495.14
Obs	272427	109342	49568	19928	93589	272427	109342	49568	19928	93589
Low Sugar (<i>LS</i>)										
Avg	0.96	0.89	0.89	0.90	1.10	188.24	195.77	200.54	177.60	174.31
SD	0.62	0.59	0.58	0.59	0.65	420.58	445.79	446.95	374.06	380.54
Obs	812070	339566	145032	66716	260756	812070	339566	145032	66716	260756
Zero Sugar (<i>ZS</i>)										
Avg	1.56	1.39	1.43	1.75	1.77	114.88	104.48	138.38	97.52	119.78
SD	1.10	0.98	1.10	1.17	1.18	308.02	248.93	381.67	258.54	339.73
Obs	203740	84955	32262	13436	73087	203740	84955	32262	13436	73087
<u>B. Balanced Panel</u>										
Comparison group: <i>Water</i>										
Avg	0.43	0.43	0.43	0.43	0.43	2653.14	2622.62	3095.95	2217.50	2578.79
SD	0.27	0.27	0.27	0.27	0.27	6385.42	6411.21	7182.98	5383.30	6144.02
Obs	163881	68268	27318	13659	54636	163881	68268	27318	13659	54636
High Sugar (<i>HS</i>)										
Avg	1.85	1.80	1.78	1.77	1.97	179.34	189.78	218.41	181.66	146.17
SD	1.77	1.82	1.81	1.69	1.70	363.29	395.26	485.00	334.04	230.60
Obs	137194	57142	22872	11436	45744	137194	57142	22872	11436	45744
Medium Sugar (<i>MS</i>)										
Avg	1.03	1.00	0.95	0.98	1.14	304.21	287.46	354.79	294.26	302.32
SD	0.50	0.49	0.48	0.47	0.52	582.63	574.87	609.33	585.92	576.33
Obs	153730	64039	25626	12813	51252	153730	64039	25626	12813	51252
Low Sugar (<i>LS</i>)										
Avg	0.74	0.69	0.67	0.68	0.85	334.52	356.07	376.74	313.00	291.88
SD	0.31	0.30	0.28	0.29	0.30	574.21	613.89	627.50	503.57	504.43
Obs	346183	144184	57714	28857	115428	346183	144184	57714	28857	115428
Zero Sugar (<i>ZS</i>)										
Avg	1.33	1.31	1.27	1.30	1.41	115.43	115.96	134.28	100.74	109.01
SD	1.08	1.11	1.09	0.96	1.05	187.40	209.89	191.91	138.82	163.70
Obs	61079	25442	10182	5091	20364	61079	25442	10182	5091	20364

Notes: *Full*: February 2015-January 2018; *UnderDiscussion*: May-October 2016; *UnderApproval*: November 2016-January 2017; *Implementation*: February 2017-January 2018.

3.4 Empirical Strategy

3.4.1 Difference-in-Differences Model

We apply difference-in-differences models to compare each treated product group (PG) to *Water*, the comparison group (section 3.3). In the following econometric specification, q denotes the quarter (Feb-Apr 2015 through Nov 2017-Jan 2018), i the product (e.g. 33-centiliter can of Pepsi, 1.5-liter bottle of Luso water), s the store (each of 400+ stores), m the month of the year (Jan, Feb, ..., Dec), and r the region (North, Center, South, or Madeira):

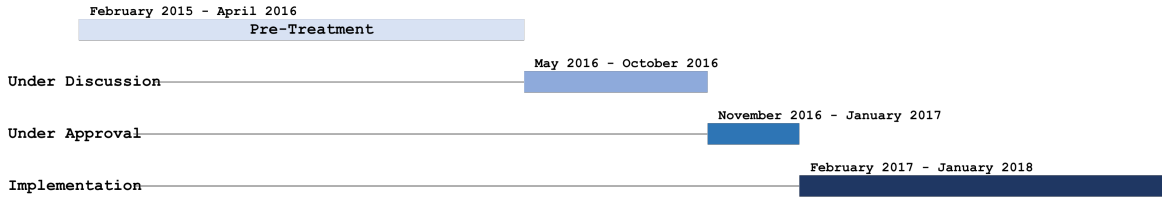
$$y_{q,i,s,m,r} = \beta_1 \text{Under Discussion}_q \times PG_i + \beta_2 \text{Under Approval}_q \times PG_i + \beta_3 \text{Implementation}_q \times PG_i + \lambda_q + \alpha_{i,s} + \delta_{m,r} + \varepsilon_{q,i,s,m,r} \quad (3.1)$$

The dependent variable, y , is either price (in euros per liter) or the natural logarithm of quantity sold. The natural logarithm accounts for the skewness in the distribution of sales volume and allows us to interpret consumption responses in percentage.

We compare the pre-treatment period (Feb 2015-Apr 2016) with three distinct post-treatment periods: 1) *Under Discussion*, the two quarters between May and October 2016, when the tax was only being discussed in the media, 2) *Under Approval*, the quarter between November 2016 and January 2017, when the tax was formally proposed and debated in the parliament, and 3) *Implementation*, from February 2017 onwards, when the tax was actually in place (Figure 3.1). The parameters of interest are represented by β_j .

In the previous section, we motivate the use of bottled water as the comparison group. We also test the parallel-trends assumption formally (see next section). In

Figure 3.1: **Timeline of Events**



addition, the different vectors of fixed effects included in Equation 3.1 control for potential confounders that may hinder the identification of the causal impact of the soda tax. Quarter fixed effects (λ_q) control for aggregate trends related for example to the business cycle. Product-store fixed effects ($\alpha_{i,s}$) account for unobserved factors that may impact specific products or stores, such as preferences, competition, and other local characteristics. We also include month-region fixed effects ($\delta_{m,r}$) to control for seasonality. In the tables presented in the results section and in the Appendix, Equation 3.1 corresponds to specification (3). Specification (1) controls only for quarter, product, and store fixed effects (not product-store fixed effects), and specification (2) controls only for quarter and product-store fixed effects.

Standard errors are clustered at the brand level to accommodate any serial correlation across different container sizes of the same product, that may for example be substitutes (Bertrand et al., 2004).

3.4.2 Event Study Design

We complement the previous strategy with event studies. The econometric specification is similar, except that it includes interactions between the treatment group and every quarter:

$$y_{q,i,s,m,r} = \sum (\beta_q Quarter_q \times PG_i) + \lambda_q + \alpha_{i,s} + \delta_{m,r} + \varepsilon_{q,i,s,m,r} \quad (3.2)$$

The omitted quarter is Feb-Apr 2016, before the first news piece on the soda tax. Again, the parameters of interest are represented by β_j and standard errors are clustered at the brand level.

The event study design presents two key advantages beyond the difference-in-differences model. First, it is a way of formally testing the parallel-trends assumption. That is, we may test if prices or consumption of soda and water displayed similar patterns in the pre-tax period. If so, then it is reasonable to believe that prices or consumption patterns of bottled water in the post-tax period represent a good counterfactual for what would be the price or consumption patterns of soda, had there been no tax. Second, with event study specifications we may look at the dynamics in more detail, distinguishing between short- and medium-run responses to the tax.

3.5 Results

3.5.1 Difference-in-differences Baseline Results

We present the difference-in-differences results for price (in euros) and $\ln(\text{quantity of liters sold})$, based on three alternative specifications.¹² Overall, all specifications give similar results. We focus on the most conservative one, specification (3), which includes quarter, product-store, and month-region fixed effects, as specified in Equation (3.1). We present results based on the unbalanced panel (Panel A) and the balanced one (Panel B).

Starting with *High Sugar* drinks, we find that when the tax was implemented, and compared to one year earlier, before the tax was ever mentioned, prices increased by 16 cents on average, *vis-à-vis* water prices (Panel A of Table 3.2). The price increase is slightly larger, at 17 cents, when considering only the most popular products (Panel

¹²Results for $\ln(\text{price})$ are presented in Table C1 in the Appendix, for comparison.

B). This increase is consistent with pass-through to consumer prices below but not too far from 100%.

Regarding sales, the point estimates suggest a 8% drop in the *Under Discussion* period, consistent with an awareness effect, and a 6% drop in the *Implementation* period. However, these effects are imprecisely estimated and not statistically different from zero. In the balanced panel, which includes only the most popular products, we find a statistically significant 19% increase in sales in the *Under Approval* period, right before the tax was implemented. This suggests a stockpiling effect, whereby consumers may have purchased large quantities of these drinks in anticipation of the price increase due to the tax in the following quarter.

Moving on to *Medium Sugar* drinks, which saw their recipes reformulated to fall below the 80 grams of sugar per liter threshold, we see that the average increase in prices is less pronounced than in the case of *High Sugar* drinks, at 15 cents per liter (Panel A of Table 3.3). In this case, the price increase corresponds to pass-through to consumer prices well above 100% (the tax amounts to about 10 cents when including VAT). This may reflect, at least in part, additional costs borne by producers related to product reformulation, repackaging, and brand repositioning. Regarding sales, the only estimate worth notice is a 24% increase in the quarter prior to the tax implementation in the balanced panel (again, a stockpiling effect; Panel B).

Regarding *Low Sugar* drinks, prices increased by 15-16 cents per liter, on average, which again corresponds to more than the amount of the tax (Table 3.4). In this case, we do find significant drops in consumption in both the *Under Discussion* and *Implementation* periods; as much as a 18% drop in the unbalanced panel. One possible explanation is increased awareness that drinks with added sugar in general are bad for health, whereas Cola-flavored drinks (in the *High Sugar* group) have always been perceived as unhealthy. An alternative explanation is that drinks with comparatively

Table 3.2: **Difference-in-differences Baseline Results: *High Sugar (HS)***

	Price (in euros)			ln(Quantity of liters sold)		
	(1)	(2)	(3)	(1)	(2)	(3)
	<u>A. Unbalanced Panel</u>					
<i>UnderDiscussion</i> × <i>HS</i>	-0.043** (0.018)	-0.040** (0.015)	-0.040** (0.015)	-0.087 (0.051)	-0.084 (0.053)	-0.084 (0.051)
<i>UnderApproval</i> × <i>HS</i>	-0.009 (0.046)	-0.015 (0.048)	-0.016 (0.048)	0.019 (0.086)	-0.003 (0.093)	-0.005 (0.093)
<i>Implementation</i> × <i>HS</i>	0.156*** (0.039)	0.159*** (0.034)	0.160*** (0.034)	-0.071 (0.075)	-0.059 (0.080)	-0.065 (0.080)
<i>N</i>	647966	647966	647966	647966	647966	647966
adj. <i>R</i> ²	0.974	0.980	0.980	0.846	0.899	0.904
	<u>B. Balanced Panel</u>					
<i>UnderDiscussion</i> × <i>HS</i>	-0.028* (0.015)	-0.028* (0.015)	-0.027* (0.015)	-0.034 (0.059)	-0.033 (0.059)	-0.034 (0.059)
<i>UnderApproval</i> × <i>HS</i>	-0.029 (0.057)	-0.029 (0.057)	-0.029 (0.057)	0.190*** (0.054)	0.190*** (0.054)	0.189*** (0.054)
<i>Implementation</i> × <i>HS</i>	0.173*** (0.052)	0.173*** (0.052)	0.173*** (0.052)	-0.056 (0.090)	-0.056 (0.090)	-0.056 (0.090)
<i>N</i>	301075	301075	301075	301075	301075	301075
adj. <i>R</i> ²	0.983	0.983	0.983	0.890	0.936	0.940
Quarter FE	✓	✓	✓	✓	✓	✓
Product FE	✓			✓		
Store FE	✓			✓		
Product-store FE		✓	✓		✓	✓
Month-region FE			✓			✓

Notes: Standard errors in parentheses are clustered by brand. Stars indicate significance levels of 10% (*), 5% (**), and 1%(***). *UnderDiscussion*: May-October 2016; *UnderApproval*: November 2016-January 2017; *Implementation*: February 2017-January 2018.

less sugar may be less addictive, which may translate into a more elastic demand compared to drinks with more sugar. In the balanced panel, once again we find a surge in sales in the quarter prior to implementation.

Lastly, for *Zero Sugar* drinks, we find about 100% price pass-through (Table 3.5). Looking at the balanced panel, we find a statistically significant increase in consumption in the *Under Approval* period, in line with our findings for the other drink groups. In addition, the point estimate for the *Implementation* interaction term indicates a 12% increase in sales, suggestive of a substitution effect towards artificially-sweetened beverages, but it is not statistically different from zero due to a large standard error.

Table 3.3: **Difference-in-differences Baseline Results: *Medium Sugar (MS)***

	Price (in euros)			ln(Quantity of liters sold)		
	(1)	(2)	(3)	(1)	(2)	(3)
	<u>A. Unbalanced Panel</u>					
<i>UnderDiscussion</i> × <i>MS</i>	-0.033** (0.015)	-0.032** (0.014)	-0.031** (0.014)	-0.009 (0.110)	-0.022 (0.107)	-0.026 (0.107)
<i>UnderApproval</i> × <i>MS</i>	0.010 (0.027)	0.004 (0.025)	0.003 (0.025)	0.081 (0.179)	0.042 (0.184)	0.041 (0.184)
<i>Implementation</i> × <i>MS</i>	0.154*** (0.023)	0.152*** (0.021)	0.152*** (0.021)	0.013 (0.104)	0.010 (0.112)	0.003 (0.113)
<i>N</i>	567786	567786	567786	567786	567786	567786
adj. <i>R</i> ²	0.950	0.963	0.963	0.785	0.849	0.855
	<u>B. Balanced Panel</u>					
<i>UnderDiscussion</i> × <i>MS</i>	-0.050*** (0.015)	-0.050*** (0.015)	-0.050*** (0.015)	0.049 (0.098)	0.049 (0.098)	0.049 (0.098)
<i>UnderApproval</i> × <i>MS</i>	-0.017 (0.017)	-0.017 (0.017)	-0.017 (0.017)	0.239*** (0.078)	0.239*** (0.078)	0.239*** (0.079)
<i>Implementation</i> × <i>MS</i>	0.143*** (0.018)	0.143*** (0.018)	0.143*** (0.018)	0.037 (0.086)	0.037 (0.086)	0.038 (0.086)
<i>N</i>	317611	317611	317611	317611	317611	317611
adj. <i>R</i> ²	0.970	0.971	0.971	0.858	0.908	0.913
Quarter FE	✓	✓	✓	✓	✓	✓
Product FE	✓			✓		
Store FE	✓			✓		
Product-store FE		✓	✓		✓	✓
Month-region FE			✓			✓

Notes: Standard errors in parentheses are clustered by brand. Stars indicate significance levels of 10% (*), 5% (**), and 1%(***). *UnderDiscussion*: May-October 2016; *UnderApproval*: November 2016-January 2017; *Implementation*: February 2017-January 2018.

3.5.2 Internal Validity: Event Study and Falsification Tests

We test the internal validity of our results by estimating event study specifications, as shown in Equation (3.2), and by conducting a placebo test.

Event study results are shown in Figures 3.2 through 3.5, where the top panels display the results for the unbalanced panel and the bottom panels display the results for the balanced panel. We present both 90% and 95% confidence intervals. As a reminder, the omitted quarter is February-April 2016, before the first news piece on the soda tax.

Overall, we find support for the parallel-trends assumption and the validity of

Table 3.4: **Difference-in-differences Baseline Results: *Low Sugar (LS)***

	Price (in euros)			ln(Quantity of liters sold)		
	(1)	(2)	(3)	(1)	(2)	(3)
A. Unbalanced Panel						
<i>UnderDiscussion</i> × <i>LS</i>	-0.017** (0.006)	-0.016** (0.006)	-0.016** (0.006)	-0.144** (0.069)	-0.150** (0.065)	-0.150** (0.066)
<i>UnderApproval</i> × <i>LS</i>	-0.009 (0.013)	-0.011 (0.011)	-0.011 (0.011)	0.132 (0.079)	0.088 (0.089)	0.085 (0.089)
<i>Implementation</i> × <i>LS</i>	0.154*** (0.026)	0.154*** (0.026)	0.154*** (0.025)	-0.183** (0.074)	-0.184** (0.077)	-0.185** (0.077)
<i>N</i>	1107429	1107429	1107429	1107429	1107429	1107429
adj. <i>R</i> ²	0.943	0.956	0.957	0.813	0.875	0.879
B. Balanced Panel						
<i>UnderDiscussion</i> × <i>LS</i>	-0.019** (0.007)	-0.019** (0.007)	-0.019** (0.007)	-0.053 (0.058)	-0.053 (0.058)	-0.052 (0.058)
<i>UnderApproval</i> × <i>LS</i>	-0.011 (0.012)	-0.011 (0.012)	-0.011 (0.012)	0.238* (0.120)	0.238* (0.120)	0.237* (0.119)
<i>Implementation</i> × <i>LS</i>	0.162*** (0.025)	0.162*** (0.025)	0.162*** (0.025)	-0.135* (0.065)	-0.134* (0.065)	-0.134* (0.065)
<i>N</i>	510064	510064	510064	510064	510064	510064
adj. <i>R</i> ²	0.930	0.931	0.932	0.844	0.904	0.908
Quarter FE	✓	✓	✓	✓	✓	✓
Product FE	✓			✓		
Store FE	✓			✓		
Product-store FE		✓	✓		✓	✓
Month-region FE			✓			✓

Notes: Standard errors in parentheses are clustered by brand. Stars indicate significance levels of 10% (*), 5% (**), and 1%(***). *UnderDiscussion*: May-October 2016; *UnderApproval*: November 2016-January 2017; *Implementation*: February 2017-January 2018.

Table 3.5: **Difference-in-differences Baseline Results: Zero Sugar (ZS)**

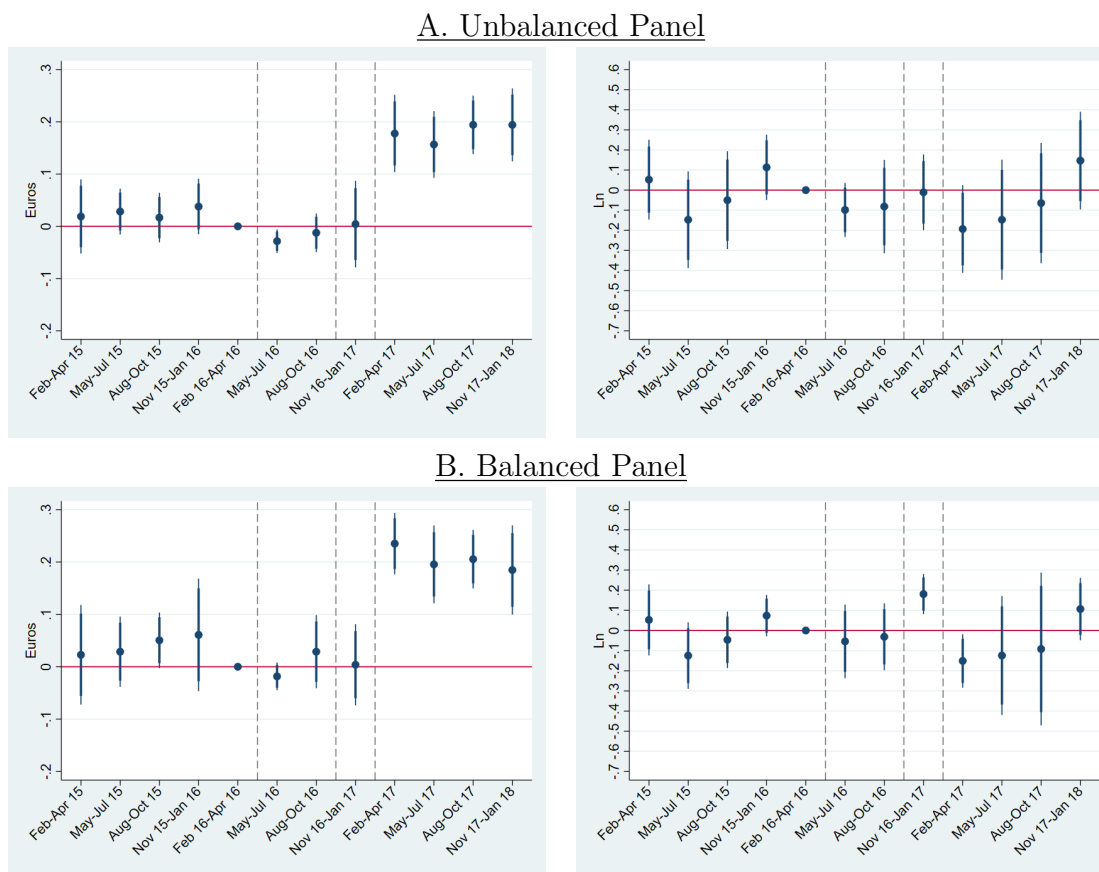
	Price (in euros)			ln(Quantity of liters sold)		
	(1)	(2)	(3)	(1)	(2)	(3)
<u>A. Unbalanced Panel</u>						
<i>UnderDiscussion</i> × ZS	-0.038*** (0.011)	-0.042*** (0.010)	-0.042*** (0.010)	-0.041 (0.049)	-0.044 (0.048)	-0.041 (0.048)
<i>UnderApproval</i> × ZS	-0.019 (0.061)	-0.021 (0.056)	-0.022 (0.055)	-0.003 (0.132)	-0.041 (0.134)	-0.040 (0.127)
<i>Implementation</i> × ZS	0.091*** (0.022)	0.093*** (0.020)	0.093*** (0.020)	0.020 (0.086)	0.034 (0.090)	0.027 (0.091)
<i>N</i>	499099	499099	499099	499099	499099	499099
adj. <i>R</i> ²	0.977	0.984	0.984	0.853	0.902	0.906
<u>B. Balanced Panel</u>						
<i>UnderDiscussion</i> × ZS	-0.042*** (0.010)	-0.042*** (0.010)	-0.041*** (0.010)	0.010 (0.097)	0.010 (0.097)	0.009 (0.097)
<i>UnderApproval</i> × ZS	-0.013 (0.046)	-0.013 (0.046)	-0.013 (0.046)	0.172** (0.066)	0.172** (0.066)	0.171** (0.066)
<i>Implementation</i> × ZS	0.102** (0.043)	0.102** (0.042)	0.102** (0.042)	0.119 (0.156)	0.119 (0.155)	0.119 (0.155)
<i>N</i>	224960	224960	224960	224960	224960	224960
adj. <i>R</i> ²	0.985	0.985	0.985	0.895	0.935	0.939
Quarter FE	✓	✓	✓	✓	✓	✓
Product FE	✓			✓		
Store FE	✓			✓		
Product-store FE		✓	✓		✓	✓
Month-region FE			✓			✓

Notes: Standard errors in parentheses are clustered by brand. Stars indicate significance levels of 10% (*), 5% (**), and 1%(***). *UnderDiscussion*: May-October 2016; *UnderApproval*: November 2016-January 2017; *Implementation*: February 2017-January 2018.

our difference-in-differences results, as confidence intervals for pre-treatment period interactions include the value zero. In addition, we highlight two main findings, looking at these plots. First, for any group of drinks, price increases appear mostly stable along the four quarters of the *Implementation* period. Second, in the *High Sugar* group, we find that consumption did decrease in the first quarter of the *Implementation* period, specially in the balanced panel, where we also find evidence of stockpiling in the pre-implementation quarter. However, consumption quickly returned to previous levels, resulting overall in the non-significant 6% drop that we find in the

difference-in-differences results.¹³

Figure 3.2: Event Study Results: *High Sugar*



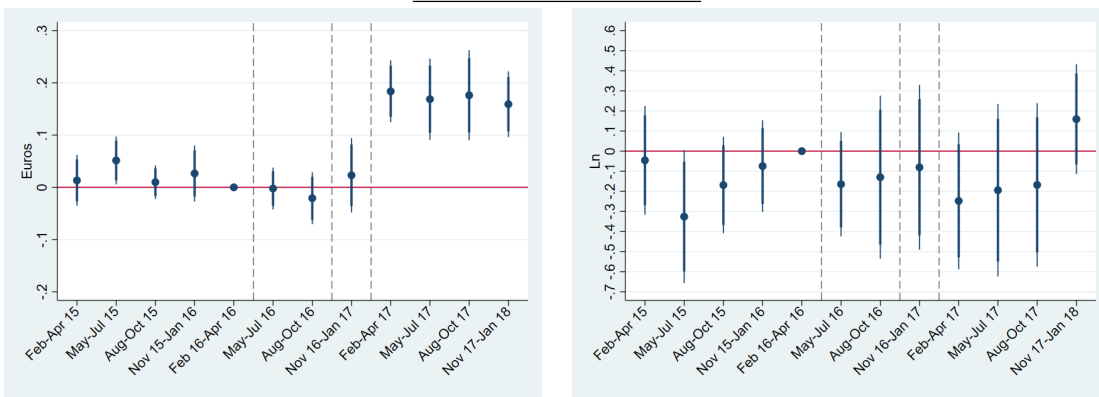
Notes: 90% and 95% confidence intervals using standard errors clustered by brand.

Our placebo test consists in estimating Equation (3.1) only with data for the pre-treatment period (i.e., February 2015-April 2016). We estimate the difference-in-differences model pretending the tax is implemented in the quarter before the first news piece on the soda tax. As expected, results indicate non-significant impacts of the fake tax introduction (Table C2).

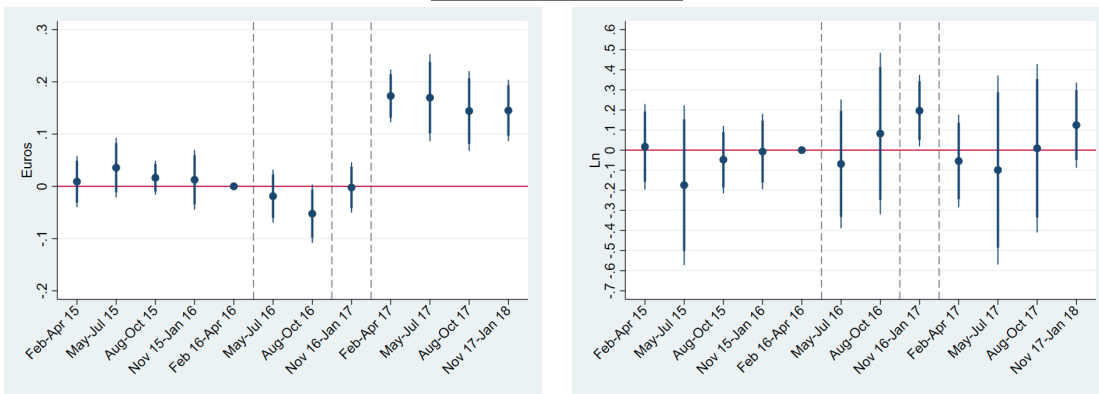
¹³We also considered quantities sold at the brand level, i.e. aggregating sales volume of all container sizes of the same drink (and in some cases, different flavors) in the same store and month, to account for potential substitution between sizes/flavors. Results shown in Figure C6 in the Appendix are very similar to those presented in this section, but noisier given the smaller sample. We explore further potential heterogeneous effects by size in Section 3.5.4.

Figure 3.3: Event Study Results: *Medium Sugar*

A. Unbalanced Panel



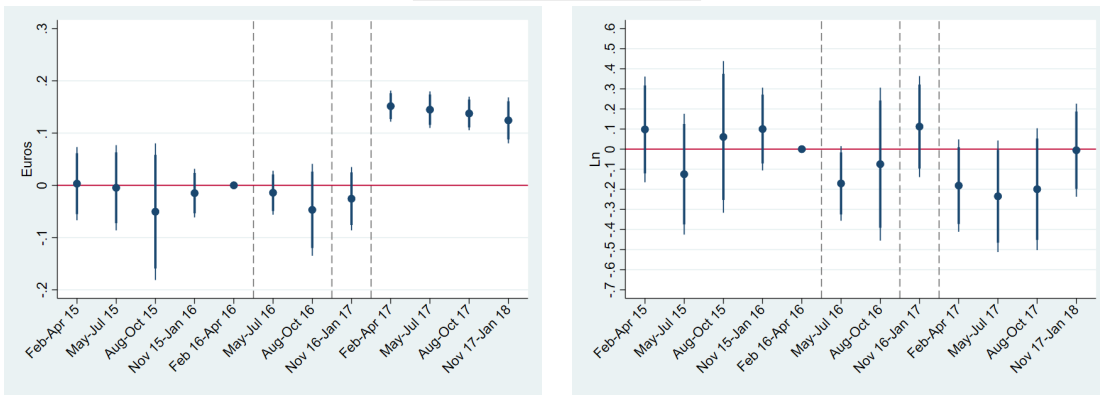
B. Balanced Panel



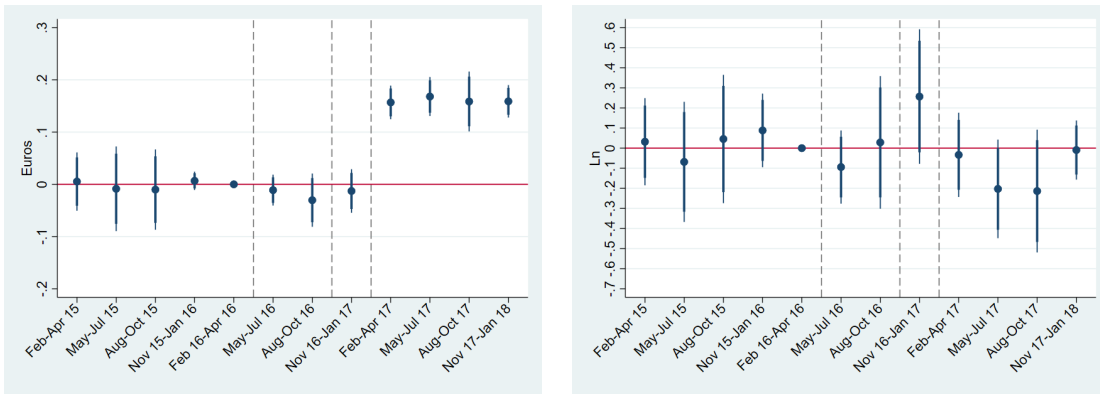
Notes: 90% and 95% confidence intervals using standard errors clustered by brand.

Figure 3.4: **Event Study Results: *Low Sugar***

A. Unbalanced Panel



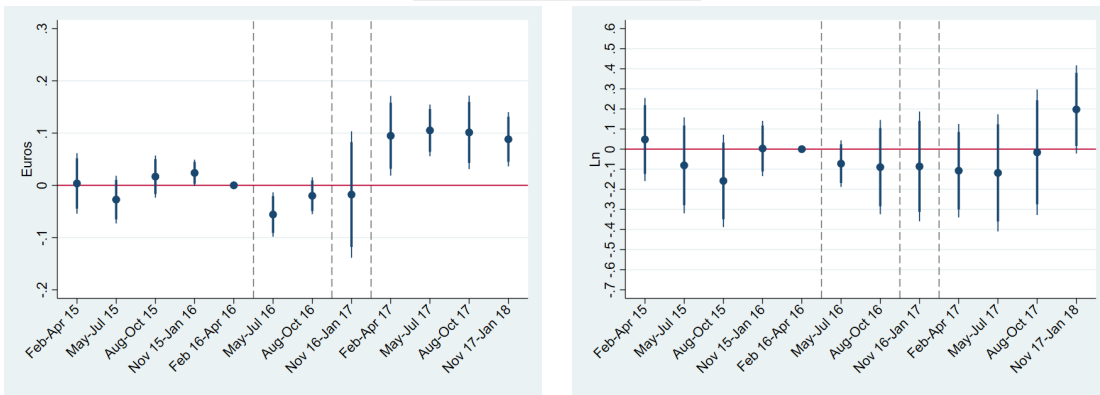
B. Balanced Panel



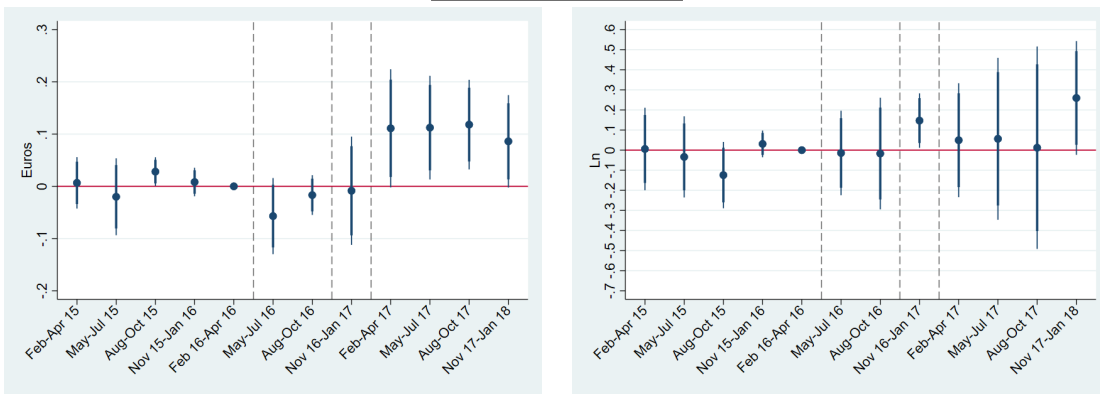
Notes: 90% and 95% confidence intervals using standard errors clustered by brand.

Figure 3.5: Event Study Results: *Zero Sugar*

A. Unbalanced Panel



B. Balanced Panel



Notes: 90% and 95% confidence intervals using standard errors clustered by brand.

3.5.3 Robustness Checks

Our first robustness check consists in adding sparkling water to the comparison group, as a means to enlarge sample sizes and eventually obtain more precise estimates. Our go-to comparison group did not include sparkling water because, contrary to other countries, in Portugal sparkling water has little expression. Results are virtually unchanged by this enlargement of the comparison group (Table C3).

Second, we acknowledge that consumers may have different elasticities regarding popular multinational brands and the retailer's own brand products. We test the sensitivity of our findings to the exclusion of the retailer's own brand products. Third, we consider potential cross-border shopping as a means to avoid higher tax-induced prices in Portugal, and exclude from the sample stores close to the border with Spain (within a 30km distance). Some studies on soda taxes in the US find evidence of cross-border shopping.¹⁴ Neither excluding own brand products nor excluding stores close to Spain impacts visibly our main findings (Table C4).

Fourth, we distinguish between the impacts of the soda tax in the Lisbon and Oporto metropolitan areas versus the rest of the country. Excluding observations from the most urban and densely populated areas in the country also gives very similar results (Table C5).

3.5.4 Is There Substitution from Large to Small Container Sizes?

Consumers may react to the introduction of the soda tax by substituting from larger to smaller container sizes, compensating for the increase in price by reducing quantity

¹⁴See also Beatty et al. (2009) on the impacts of differentials in alcohol and tobacco taxes near an international border, or Hindriks and Serse (2019) on the importance of considering spatial heterogeneity in tax incidence for homogeneous products due to proximity to borders.

purchased, in liters. To test this hypothesis, we split the treatment groups into <1 liter and 1+ liters container sizes. The comparison group is unaltered. Results are reported in Table C6. For all product groups, consumption of larger packages is hit in a more severe way; the exception is *Low Sugar* products in the balanced panel, where point estimates are virtually the same. These findings are in line with the findings of Castelló and López-Casasnovas (2018). In some cases, the differential impact of the tax implementation on consumption may partly reflect different price increases in the two groups. Yet, even in the *Under Discussion* period, when prices don't change substantially in economic terms, we see larger drops in the consumption of large container sizes. This suggests that heavier soda consumers are the main ones reducing consumption. For more sugary drinks (*HS* and *MS*), consumption tends to increase in the *Under Approval* period, especially for larger packages, consistent with the idea of stockpiling.

3.6 Concluding Remarks

A recent WHO report called for the introduction of taxes on SSBs in developed countries (WHO, 2017). In particular, WHO recommended that to be effective in reducing consumption, a soda tax should result in at least 20% increase in retail prices. One of the arguments to introduce soda taxes is to improve consumer diets through the reduction of sugar intake from soda. Determining the impact of soda taxes on consumption can only be done empirically, as in theory there are several mechanisms that may entail both positive and negative effects. Those mechanisms include the elasticity of consumption to soda prices, changes in recipes, and amplified consumer awareness of the detrimental health effects of sugar and soda. Understanding better these mechanisms is also key to inform the design of more efficient public policies (Cawley and Ruhm, 2011; Cornelsen

and Smith, 2018).

Portugal introduced a soda tax in February 2017, levied on producers. The amount of the tax is 0.08€ (+VAT) per liter for drinks with less than 80 grams of sugar per liter, and 0.16€ (+VAT) per liter for drinks with more than 80 grams of sugar per liter. We study the impacts of this tax on prices and consumers' purchasing behavior. First, we find substantial pass-through of the tax to consumer prices: almost 100% for drinks with more than 80 grams of sugar per liter, more than 100% for drinks with less sugar, and about 100% for artificially-sweetened beverages (average price increases of about 16%, 19%, and 8% respectively, compared to water). One possible reason underlying price overshooting for drinks with less than 80 grams of sugar per liter is costs associated with product reformulations. Another explanation may be that producers don't think consumers pay attention to the sugar threshold and thus raise the price of all SSBs equally.

Second, regarding consumption, our results suggest limited impacts of the tax. We do find a significant and substantial drop in consumption of drinks with comparatively lower sugar content (-18%). For drinks with very high sugar content, the point estimate suggests a 6% drop in consumption, but it is not estimated with enough precision to be statistically significant. Moreover, event study results show an immediate drop in consumption that quickly rebounds. This suggests that the main benefits of the soda tax in terms of reducing sugar intake are mainly through product reformulations, as producers reduced the sugar content of several drinks to fall below the 80 grams per liter threshold. In fact, the Portuguese government introduced a new soda tax breakdown in 2019.¹⁵ In short, drinks with relatively less sugar, *light*, and *zero* products, are now subject to a lower tax, whereas drinks with high sugar content pay an aggravated

¹⁵Drinks with less than 25 grams of sugar per liter now pay a tax of 0.01€ per liter, drinks with 25 grams or more and less than 50 grams of sugar per liter pay a tax of 0.06€ per liter, drinks with 50 grams or more and less than 80 grams of sugar per liter pay a tax of 0.08€ per liter, and drinks with 80 grams or more sugar per liter pay a tax of 0.20€ per liter (+VAT).

tax. This new design is expected to further promote product reformulations towards lower sugar content. These changes are outside of our period of analysis but are worth studying in future research. Possibly because in Portugal artificially-sweetened beverages are also taxed, we find no significant evidence of substitution towards this type of drinks.

It is worth mentioning that even though it appears that producers are able to increase prices without significantly impacting demand (i.e., demand for most soda products appears rather inelastic), they still bother with product reformulations. This may be to accommodate consumers' increasing preferences for healthier options, as well as to come progressively closer to targets negotiated with the government or even in anticipation of the new tax brackets.¹⁶

As far as awareness is concerned, in most cases we find a slight decrease in consumption when the tax first started to be mentioned in the media, but the estimates are not precisely estimated and are not significantly different from zero.

Lastly, consumers appear to have engaged in stockpiling of the most widely consumed products in the quarter prior to the tax implementation. This is not a surprising finding in the context of Portugal. For example, in 2019 fuel truck drivers have been striking and when a new strike is announced, consumers run to gas stations.¹⁷

The comparison between previous findings and ours is not straightforward, as we split drinks into different groups. The drop in consumption of low sugar drinks is somewhere in the middle of the range of prior studies' estimates. Overall, it is important to note that although we do not find statistically significant results for most drink groups, our estimates are more conservative than most, as we include in our main specification product-store fixed effects, and our standard errors are clustered at the

¹⁶In May 2019, the government and several industry representatives signed a compromise to cut sugar, salt, and fat content in thousands of products until 2022.

¹⁷<https://sicnoticias.pt/economia/2019-04-16-Corrída-as-bombas-de-gasolina>.

brand level.

To conclude, the Portuguese soda tax has two main distinctive features: it depends on sugar content, taxing more heavily drinks with a lot of sugar, and artificially-sweetened beverages are also taxed. Due to its structure, the Portuguese soda tax led producers to reformulate many recipes towards lower sugar content. This seems to have been the main channel through which the tax reduced sugar intake from soda (Goiana-da Silva et al., 2018). This finding is in line with recent developments in soda taxes worldwide, with not only Portugal but also France introducing more tax brackets in 2019 and 2018 respectively, and for example the UK structuring its 2018 soda tax in a similar manner. We believe that our results are of practical relevance not only for policymakers planning to implement similar taxes in other countries but also for countries or regions that have very recently introduced bracketed soda taxes.

Chapter 4

Can ATMs Get Out the Vote?

Evidence from a Nationwide Field

Experiment¹

4.1 Introduction

Low and declining levels of voter turnout constitute a fundamental problem for democracies, raising questions of legitimacy and representativeness, as well as challenging the idea of political accountability. Why a rational individual would spend time and resources to become informed and vote constitutes a classical puzzle,

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which has attracted a significant body of work in economics and political science.² In this context, studies examining which factors influence electoral participation have gained relevance. Observational studies, as surveyed in Cancela and Geys (2016), have addressed the question by establishing positive correlations of voter turnout with campaign expenditures and election closeness. In a more recent generation of studies, surveyed by Gerber and Green (2017), experimental methods have allowed researchers to credibly estimate the causal effects of specific interventions. Randomized field experiments, taking place unobtrusively in real-world settings, are better designed to address inference identification problems such as self-selection, strategic targeting, or unobserved confounders. This literature documents large impacts of relatively expensive personal contact and of interventions activating peer pressure.

This paper contributes to the literature on electoral participation using experimental methods through an experiment employing the highly granular universe of ATM (automatic teller machines) terminals in Portugal.³ Our aim is to determine whether low-cost, impersonal reminders which emphasize the idea of civic duty, when exhibited in the days leading to local elections, can mobilize voters to vote, and if yes, by how much. The ATM network in Portugal, *Multibanco*, is known to be a credible

²Several distinct theories have tried to rationalize the act of voting. According to rational choice theory, the positive expected utility from participating is associated with the possibility of one's vote being decisive or pivotal – typically, a probability close to zero. See Downs (1957), and Dhillon and Peralta (2002) for a survey. A more general approach contemplates ideological and valence elements to voter's preferences, as in Feddersen and Pesendorfer (1997). The leading alternative to these instrumental voting models is ethical models, starting with Riker and Ordeshook (1968). This strand of the literature argues that voters derive utility from the act of turning out to vote (through a general sense of duty), separate from the consequences of their vote, as discussed, for instance, in Feddersen and Sandroni (2006). According to Hillman (2010), voting creates positive expressive utility, independent from the outcome, derived from a conception of civic duty or expressive confirmation of identity. Habit is also regarded as causing political action (Gerber et al., 2003; Bechtel et al., 2018). There is also evidence that we vote to tell others (DellaVigna et al., 2016) and we like to hear gratitude for voting in previous elections (Panagopoulos, 2011).

³The number of ATM machines increased steadily since the 1990s until 2010 (see Figure D1 in the Appendix to this paper). In per capita terms, ATM machines per 100 thousand inhabitants reached the record of 135.4 in that year. After that, the number of ATMs slightly decrease to 11570 in 2018 (112.6 per 100 thousand inhabitants). However, ATMs do not seem to be less used over time. Figure D2 displays the amount of withdrawals in ATMs (in €), which depicts an upward trend.

communication channel, associated with high levels of security, performance and reliability. Besides account information and withdrawals, the ATM network has the largest number of functionalities worldwide – 60 innovative operations including mobile top-ups, the possibility of buying transportation and music festival tickets, as well as performing instantaneous interpersonal transfers between accounts of different individuals, and paying for an array of government taxes and licences.⁴ It is one of the largest interbank networks within Europe, operating over 11,700 terminals and processing over 75 million transactions worth €4.8 billion per month.⁵ To contextualize, in 2017, there were more than 21,18 million of payment cards (Banco de Portugal, 2019) for a population of about 10,31 million citizens (Statistics Portugal).

Our ATM treatment was implemented in a randomly selected sample of municipalities, where a “get out the vote” (GOTV) advertisement reached potential voters using ATM machines. This message was activated during the two and a half days leading up to the election, in three different moments: before and after ATM users introduced their banking card, while they waited to withdraw cash, and while they waited to perform other operations.

We combine official turnout records for treated and control civil parishes (*freguesias*)⁶ with descriptive information provided by the ATM company on cards, operations, and withdrawals. We account for potential confounding factors using detailed socio-economic and political information for a cross-section of more than 1700 civil parishes. One week after the local elections, we conducted a follow-up survey in neighboring treated and control parishes in Lisbon, interviewing more than 200 ATM

⁴A proof of the credibility and granularity of the Multibanco system is the 2011 public discussion of its potential use as a voting network (see https://www.rtp.pt/noticias/economia/voto-simplex-via-caixas-multibanco-distinguido-no-movimento-milenio_n430575).

⁵The only competitor of the dominant Multibanco network has 300 terminals, concentrated in tourist areas and thus directed at non-voters. See <https://www.publico.pt/2017/01/02/economia/noticia/euronet/a-unica/alternativa/a-tradicional-rede-multibanco-da-sibs-ja-tem-300-caixas-em-portugal-1756507>.

⁶Civil parishes (*freguesias*) are the lowest local administrative unit in the country.

users, in order to gauge whether they recalled the treatment messages. The post-election survey showed that the treated civil parish had a significantly higher level of recall than the control civil parish.

We find no statistical significance impact of the treatment *per se*, but our results on the intensity of treatment, for either the entire campaign period or the weekend period (knowing that the election was on a Sunday), show the campaign leads to a statistically significant increase in the average turnout rate. Our findings are robust to three alternative measures of user intensity: number of cards used, operations conducted, and withdrawals. Placebo tests employing turnout rates in previous elections show no impact whatsoever of the intensity of treatment on turnout rates.

The application of field experiments to the study of electoral participation has its origins in the early contributions by Gosnell (1927) and Eldersveld (1956). However, this literature was only re-activated in the late 1990s. In this context, a plethora of (i) communication modes and (ii) message contents to encourage citizens to go to the polls were studied using field experiments.

Communication modes tested in the literature range from the highly personal to the highly impersonal: results suggest that impersonal and passive methods of contact are less effective at mobilizing voter turnout than personal interactions. For example, Gerber and Green (2000) find that nonpartisan face-to-face canvassing increased turnout in an uncontested American election by five to eight percentage points, compared to less than 1 percentage point for live phone calls and mailings.⁷ Message contents tested in the literature explored various dimensions like social norms, explicit peer pressure, and reciprocity. In an influential study of American elections,

⁷This relative effectiveness has been replicated in local (Gerber et al., 2003) and federal elections in the U.S. (Nickerson et al., 2006). The findings of Gerber and Green (2000) have been contested by Imai (2005) who demonstrates that telephone canvassing increased turnout by five percentage points while employing matching techniques (see Gerber and Green (2005) for an additional discussion). In Europe, Bhatti et al. (2016) show that the effects of door-to-door canvassing are substantially smaller than the ones found for the U.S.

Gerber et al. (2008) highlight that, even in one-way communications, showing citizens their voting record, or that of their neighbours, which are likely to activate existing social norms related to peer pressure, can render campaigns more effective.

Several one-way means of GOTV contact were also tested in the literature. Dale and Strauss (2009) show that text messages on mobile phones reminding recipients about the election day can succeed in increasing the turnout of registered voters, that is, those that have signalled their interest in voting. Other modes include radio – Panagopoulos and Green (2008), newspapers – Gerber et al. (2009), street signs – Panagopoulos (2009), TV – Gerber et al. (2011), and social media platforms such as facebook or whatsapp – Arias et al. (2019). Although this literature became most developed for the US, a number of different geographical contexts have expanded the scope and range of interventions studied.⁸ Our experiment has the benefit of estimating the impact of a low-cost one-way mode of communication – through ATM messaging, on the full universe of voters in Portugal, while using a message that targets the social norm of civic duty related to electoral participation.

The remainder of the paper is organized as follows. Section 2 details the institutional background whereas Section 3 discusses the empirical strategy and data. Section 4 presents the results. Section 5 concludes.

⁸Other examples of randomized voter mobilization applications include studies in the U.K. (John and Brannan, 2008), Mexico (Arias et al., 2019, Chong et al., 2014), São Tomé and Príncipe (Vicente, 2014), Nigeria (Collier and Vicente, 2014), Sweden (Nyman, 2017), Mozambique (Aker et al., 2017), Perú (León, 2017), and France (Pons and Liegey, 2019). Large-scale partisan interventions have also been analyzed in Benin (Wantchekon, 2003), Italy (Kendall et al., 2015), and France (Pons, 2018). Recent quasi-experimental studies have also looked at different types of voter mobilization: Barone et al. (2015) and Ellingsena and Hernæs (2018) looked at the case of access to digital/cable TV in Italy and Norway (respectively). Using similar methodologies, Shue and Luttmer (2009) for the U.S., and Hodler et al. (2015) for Switzerland, analyze the impacts of different voting technologies. Card and Moretti (2007) test whether electronic voting technology affected electoral outcomes in the 2000 and 2004 US presidential elections and find a positive correlation between use of electronic voting and the Republican vote share.

4.2 Institutional background

Local administration elected democratically exists in Portugal since the 1976 Constitution came into force, identifying three administrative divisions (Articles 235-262): civil parishes (*freguesias*), municipalities (*municípios*), and administrative regions (*regiões administrativas*). Civil parishes are the lowest administrative unit, ruled by an executive body, the civil parish board (*junta de freguesia*), and a deliberative body, the civil parish assembly (*assembleia de freguesia*). Local elections are exogenously fixed every four years for the civil parish assembly, whose winner is elected president.⁹ The lists are closed, and the seats assigned according to the D'Hondt proportionality method. Unlike in national legislative elections, independent lists can run. The official campaign period happens during the two weeks before election. The exception is the day before the election, when the campaign is suspended. Moreover, debates on TV are organized around one month before the election, only for the races of the most populous municipalities.

Before 2013, the 308 municipalities were subdivided into 4259 civil parishes. However, In the aftermath of bailout negotiations during the financial crisis, the Portuguese government was forced to reduce the number of these units. This way, the number of parishes was reduced from 4259 to 3091.¹⁰

Our field experiment took place in the days leading up to the 2017 municipal elections, on October 1, 2017. Figure D3 in the Appendix to this paper presents turnout rates for this election. We exclude from our experimental sample all civil parishes with no ATM machines in the days before the 2017 local election. The official turnout rate in 2017 (which includes all civil parishes in Portugal) was 55% while this number was slightly lower in 2013 (the previous local election), i.e., 52.6% . In our sample,

⁹Simultaneously, elections are held for Municipal Town Halls (Câmara Municipal), and Municipal Assemblies.

¹⁰Some of these amalgamations, especially in the Lisbon district, implied significant border changes.

these numbers are very similar: 61% in 2017 vs. 59.5% in 2013. Internationally, these numbers are comparable to 65.2% (65%) in the 2019 Spanish local (regional) elections, and 63.5% at the first round of voting during the 2014 French local elections. Turnout is substantially lower at the 2018 England local elections (34.6%).

4.3 Experimental design

4.3.1 Treatment

Our treatment consisted on the exhibition, on all ATM machines of treated civil parishes, of an advert reminding voters that voting is a civic duty, as well as reminding them of the election day.¹¹ This happened just before and on the election day for a period of three days, between Friday and Sunday (the election day), until 3 p.m. So, in succession, the messages pointed “Vote is a Civic Duty – Vote Sunday” presented Friday, “Vote is a Civic Duty – Vote Tomorrow” presented Saturday, and finally, “Vote is a Civic Duty – Vote Today” presented on election day.¹² The advertisement is shown in Figure D4 of the Appendix to this paper. We obtained authorization to use the official layout – images, lettering, and official seal of the National Electoral Commission. ATMs in control civil parishes displayed publicity for a TV soap-opera and car commercials. This message was activated in three different moments: before and after ATM users introduced their banking card (around 3 seconds), while they waited to withdraw cash (around 6 seconds), and while they waited to perform other operations (around 6 seconds).

The Electoral Commission spent most of their funds in TV commercials and

¹¹Dale and Strauss (2009) show that, for certain citizens, a noticeable reminder is enough to drive them to cast a vote.

¹²Nickerson (2007) presents evidence on timing effects, namely that phone calls made more than one week before the election are ineffective.

newspaper ads. Both are targeted to reach potential voters in the entire country and were broadcasted eight to two weeks before the local elections. In the last two weeks, the Electoral Commission did not make any efforts to publicize the election. Finally, it should be highlighted that we assured they did not modify what they typically did in previous elections to encourage turnout.

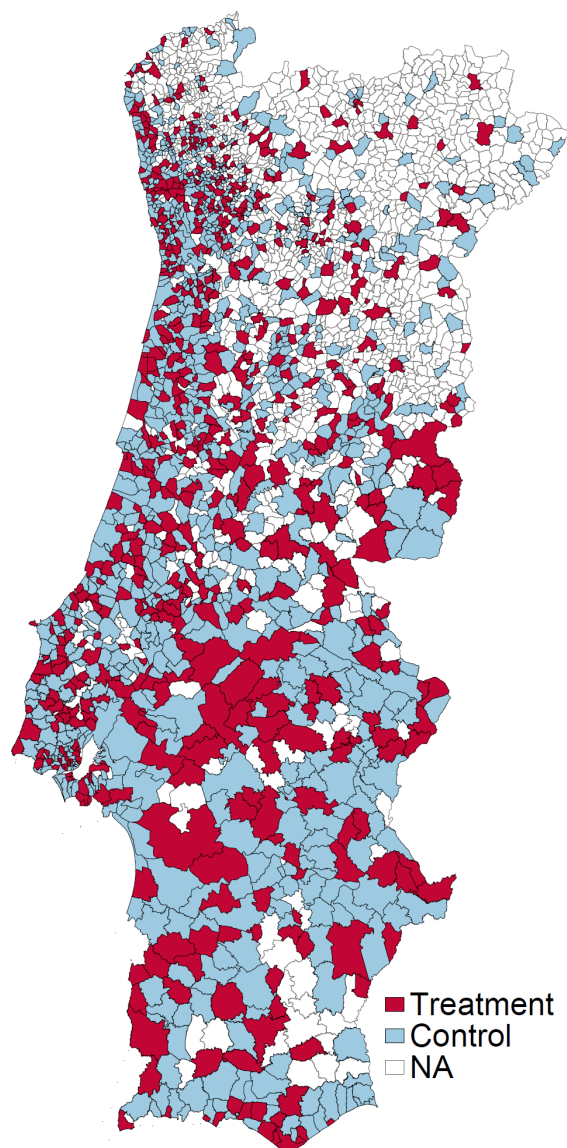
4.3.2 Sampling, randomization, and measurement

We restrict our attention to the 1704 civil parishes that have at least one ATM, a geographical area comprising more than 92% of the population of Portugal according to the 2011 census. The allocation of treatment and control conditions to the set of civil parishes followed a standard randomization procedure in two steps: *i*) we first formed blocks of civil parishes within each municipality, conditional on observables; *ii*) we then randomly assigned the treatment and control conditions to civil parishes within each block. The referred observables, allowing for *a priori* balance between treatment and control, were: voters' density (i.e., the number of registered voters divided by the civil parish area), the number of well-functioning ATM devices, the number of commercial bank agencies, the turnout rate for the previous local elections in 2013, and a term limit dummy variable taking value one if the civil parish president cannot run for another term.¹³ As voter turnout tends to be highly persistent, controlling for pre-treatment records of the outcome variable is especially important, as pointed in McKenzie (2012). Figure 4.1 shows the spatial allocation of treatment and control groups.

During the 63 hours of the treatment, our campaign reached more than 1.5 million unique cards in 682 civil parishes, i.e., 40% of all the civil parishes.

¹³Veiga and Veiga (2018) study the impact of the 2013 introduction of mayoral term limits on turnout and show that presence of term-limited incumbents has a positive impact on voter participation. For more information about the impact of this reform on incumbency advantage see Fonseca (2017).

Figure 4.1: Treated vs. Control Civil Parishes
2017 Local Elections - Mainland Portugal



4.3.3 Data

Outcome variables, composed from the number of registered and actual voters for the 2017 local elections and for previous elections, are obtained from official turnout records for civil parishes. These data are combined with treatment assignment and descriptive information provided by the ATM company on three treatment intensity measures:

the number of cards, the number of operations, and the number of withdrawals.¹⁴ Recall that withdrawals are just one of the possible operations that can be done in the Portuguese ATM network.¹⁵ All these numbers do not include foreign credit or debit cards. We can distinguish between number of operations during the three days of the campaign and during the weekend of the elections. We also collected data on a series of potential socio-demographic, political, and economic controls. Table 4.1 presents the descriptive statistics of our sample.

Besides the variables considered as part of the randomization procedure, we add a vector of socio-demographic covariates to our analysis. Education is one of the strongest predictors of voter turnout, so we include the shares of the population with no primary education and with tertiary education. We use the unemployment rate and the mean value withdrawn in ATMs on September 2016 as proxies for the economic environment.¹⁶ Both the education measures and the unemployment rate were obtained from the 2011 census operation of the Portuguese National Institute of Statistics (*INE*).

Our analysis also considers a vector of institutional variables. Political competition has been shown to positively affect turnout, as in Gerber and Green (2017) and others.¹⁷ We take two variables proxying for local competitiveness: the percentage difference in the vote on the two largest parties in the 2013 local elections, i.e., the winning margin in those elections, and the number of candidates in the 2017 municipal election. We

¹⁴Unique cards as well as the other intensity measures are counted per civil parish in the three days before the 2017 local election. We do not know the origin of viewers (they could even be from civil parishes in areas without ATMs). Therefore, this may be overstating the actual number of reached potential voters if someone used the same card in different civil parishes or if there are some clients of the ATM machines with less than 18 years old, the minimum required age to vote.

¹⁵In 2017, withdrawals accounted for less than 30% of the operations in Multibanco (Banco de Portugal, 2017).

¹⁶Martins and Veiga (2012), using panel datasets covering all mainland municipalities, from 1979 to 2005, and cross-sections of civil parishes for 2011 show that turnout in legislative and local elections react to the state of the economy.

¹⁷This is consistent with evidence from lab experiments. Levine and Pelfrey (2007), Duffy and Tavits (2008), Agranov et al. (2017) find that a higher chance of being pivotal, as in smaller elections or when elections are closely contested, leads to higher voter turnout.

Table 4.1: Descriptive Statistics

Variable	Mean	Std. Dev.	Min	Max
Dependent Variable (%)				
Turnout Rate 2017	61.222	9.266	34.61	89.154
Turnout Rate EU 2014	33.833	7.278	0	64.63
Turnout Rate Legislative 2015	56.17	7.591	22.423	80.335
Turnout Rate Presidential 2016	48.957	8.025	17.09	74.36
Experimental Variables				
Treatment	0.4	0.49	0	1
Intensity Measures: Total				
Number of Cards (/100)	22.573	49.317	0	532.59
Number of Operations (/100)	38.522	84.409	0	869.99
Number of Withdrawals (/100)	19.305	41.691	0	467.8
Intensity Measures: Weekend				
Number of Cards (/100)	12.525	26.317	0	259.34
Number of Operations (/100)	19.123	40.695	0	399.11
Number of Withdrawals (/100)	10.042	20.884	0	224.09
Stratification Controls				
Voters Density	512.904	1292.266	1.729	14081.45
Number of ATM Devices	7.006	14.334	1	162
Number of Commercial Banks	1.19	3.225	0	36
Turnout 2013 (%)	59.509	9.913	5.869	100
Term Limit Dummy 2017	0.086	0.28	0	1
Other Controls				
Socio-demographic and Economic				
Population Below 15 Share 2011 (%)	24.658	4.973	5.941	47.09
Population Above 70 Share 2011 (%)	16.532	7.335	3.564	54.525
New Citizens Share 2011 (%)	2.143	1.258	0	12.209
Until Primary Education Share 2011 (%)	39.4	8.03	17.087	78.96
Tertiary Education Share 2011 (%)	7.763	5.172	0.301	41.829
Mean Value September 2016	40162.93	82599.15	0	825553.8
Unemployment Rate 2011 (%)	12.395	3.920	0.990	31.299
Service Workers Share 2011 (%)	61.828	14.019	23.696	91.474
Fiscal and Political				
Transfer to Civil Parishes per capita	0.031	0.028	0	0.246
Winning Margin 2013 (%)	21.71	18.523	0.045	100
Number of Candidates 2017	3.585	1.303	1	10
Independent Mayor Dummy	0.098	0.297	0	1
Leftist Share 2013 (%)	48.924	22.586	0	98.587
Central Government Alignment Dummy	0.447	0.497	0	1

Notes N= 1703.

also include whether the incumbent mayor has independently run for office and the share of leftist mandates in the Municipal Assembly as defined in the 2013 elections. These variables are provided by *Direção Geral Autarquias Locais (DGAL)*.

Balance tests on the variables described in this subsection are presented in Table D1 of the Appendix to this paper: they show that randomization was successful in creating comparable treatment and control groups.

4.3.4 Econometric specifications

We estimate the intent-to-treat (average treatment effects) impact of the campaign using the following specification:

$$\text{Turnout Rate}_{im} = \alpha_m + \gamma \text{Treatment}_i + \beta X_i + \varepsilon_i \quad (1)$$

where the outcome variable is the Turnout Rate for the 2017 local elections, determining who will become president of the civil parish board. i denotes a Civil Parish. α_m includes binary variables for each of the 308 Portuguese municipalities (denoted by m). $Treatment$ is a binary indicator that takes value one if the civil parish was treated. γ is our coefficient of interest, capturing the effect of being assigned to the treatment group. It captures both the direct impact of the campaign on voters who saw it and indirect spillover effects stemming from interactions between voters who have seen the campaign. X is a vector of covariates including the stratification controls, as well as the socio-demographic and institutional variables specified above. ε_i accounts for robust standard errors given that the unit of observation and the unit of randomization are the same.

As it is the case in several GOTV efforts, our campaign does not reach everyone assigned to the treatment group and may reach people in the control group as a spillover

effect. This happens because some of the voters in the treated civil parish may not use the ATM machines, nor interact with people who use them. In fact, it could happen that some of these voters are users of ATM machines in control locations (e.g., where they work, study, or shop). At the same time, voters in non-treated civil parishes, may have used ATM machines in treated civil parishes. As pointed out by Arceneaux and Nickerson (2009), “the failure to treat problem does not bias the estimates of the empirical model (...), because random assignment ensures that (within sampling variability) the treatment and control group have an equal proportion of contactable individuals.” Nevertheless, while the intent-to-treat effect allows us to evaluate the effects of a program, it is not suitable to estimate the behavioral response of individuals to the actual program intervention. Both these possibilities, if real in our experiment, contribute to bias treatment effects towards zero. Some of the robustness tests we show below attempt to minimize the extent that these biases are at work.

We also estimate the following equation considering three measures of campaign intensity:

$$\text{Turnout Rate}_{im} = \alpha_m + \gamma_1 \text{Treatment}_i + \gamma_2 \text{Intensity}_i + \gamma_3 \text{Treatment}_i \text{Intensity}_i + \beta X_i + \epsilon_i \quad (2)$$

where we consider three different measures of *Intensity* provided by the ATM company: the number of cards, the number of operations, and the number of withdrawals. γ_3 is our coefficient of interest, which tests whether more intensely treated civil parishes are associated with significantly higher turnout rates. Control variables are particularly important in this setting. This is the reason we selected a set of control variables that features prominently in non-experimental turnout studies.

Finally, we run a specification to examine whether ATM user j recalled seeing the

campaign in two similar civil parishes, but just one of them treated, as follows:

$$Recall_j = \alpha + \gamma \text{ Treatment}_j + \beta X_j + \varepsilon_j \quad (3)$$

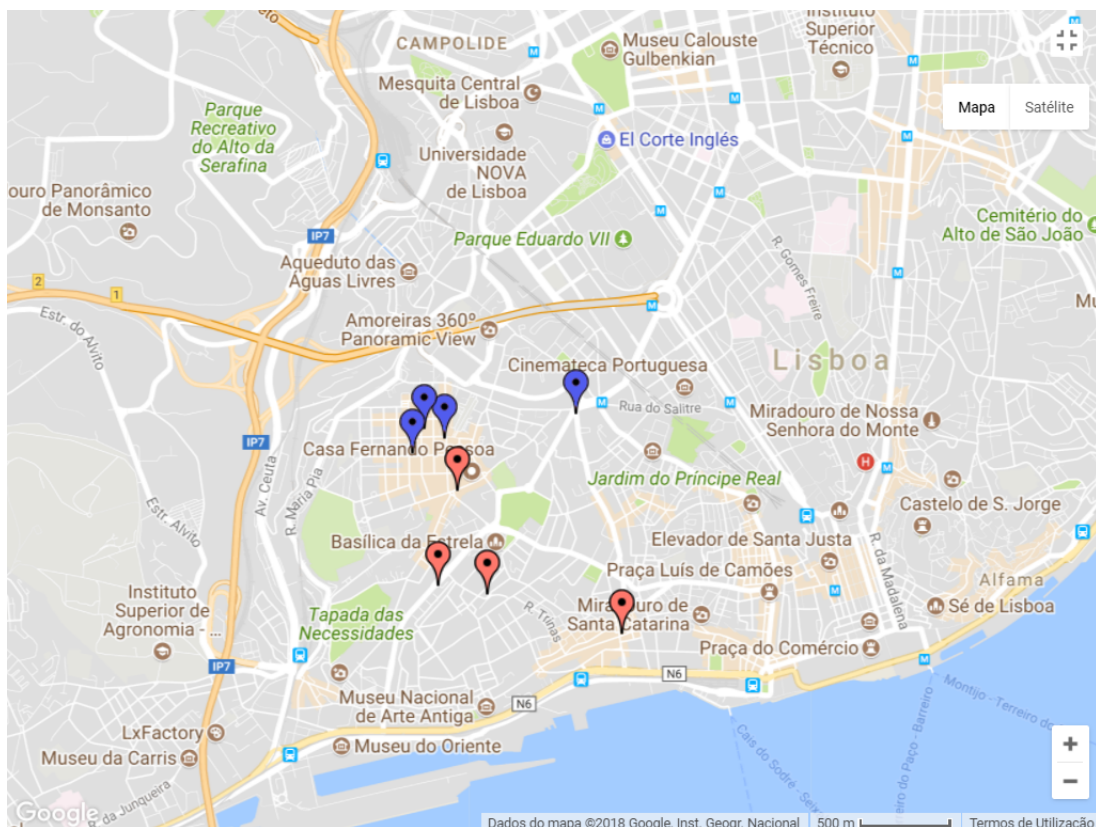
Here, *Recall* is a binary indicator taking value one if the subject recognizes the image of the campaign. γ is our coefficient of interest. X is a vector of control variables such as gender, age, self-reported education level, and self-reported interest in politics. We present the summary statistics for these variables in Table D2 of the Appendix to this paper.

4.3.5 Treatment adherence: the post-treatment recall survey

We conducted a post-treatment survey in eight ATMs in two contiguous civil parishes in Lisbon on Sunday October 8, 2017, one week after the local elections, between 10 a.m. and 1 p.m. One of the civil parishes belonged to the treatment group and the other to the control, respectively *Campo de Ourique* and *Estrela*. Figure 4.2 presents a map of the exact location of the eight ATMs that were targeted by surveyors, four in the treated (in blue) and four in the control (in red) civil parishes.

All individuals who used these ATMs in the referred period were approached by the enumeration team, leading to slightly less than 200 valid interviews. All enumerators received detailed training and advice on how to start and lead the questionnaire. Moreover, enumerators were not told about whether they were in a treated or control area. The goal of this exercise was to assess whether there were significant differences in recall for treated and control groups. Enumerators confronted ATM clients with a visual copy of our campaign (see Figure D4), asked whether they recalled seeing the image, and further collected information on socio-demographic characteristics of

Figure 4.2: Recall: Treated and Control ATMs



the interviewees such as age, gender, self-reported education level, and self-reported interest in politics.¹⁸ As a placebo, and before confronting ATM clients with the image of our campaign, we asked if they recalled seeing any particular ad in the ATM.

Table 4.2 presents the results using a linear probability model for the likelihood of recall using equation (3).¹⁹ The results show a large and statistically significant difference in recall between the treatment and the control civil parishes. Moreover, with respect to recalling past ATM campaigns, there are no reported differences as displayed in column (4).

¹⁸Table D2 of the Appendix to this paper presents the summary statistics for these control variables.

¹⁹We examined results using a Probit specification and obtained very similar findings.

Table 4.2: **Self-Reported Recall Results of the post-treatment recall survey**

	Recall GOTV Campaign			Recall Any Campaign
	(1)	(2)	(3)	(4)
Treatment	0.113** (0.054)	0.118** (0.056)	0.092* (0.055)	0.016 (0.078)
Gender		0.078 (0.058)	0.063 (0.061)	-0.158** (0.077)
Age		0.002 (0.002)	0.002 (0.002)	0.004* (0.002)
Primary Education			-0.145 (0.152)	-0.093 (0.222)
Secondary Education			-0.024 (0.158)	0.104 (0.208)
Tertiary Education			0.022 (0.154)	0.142 (0.202)
Low Interest in Politics			-0.035 (0.072)	0.034 (0.095)
Medium Interest in Politics			-0.020 (0.105)	-0.063 (0.123)
_cons	0.111*** (0.037)	-0.029 (0.108)	0.012 (0.171)	0.257 (0.238)
Mean of Control Group	0.111	0.113	0.113	0.486
N	188	176	175	175
Adjusted R2	0.015	0.025	0.017	0.011

Notes The main dependent variable, Recall GOTV Campaign, is a binary indicator that takes value one if the ATM user reported to recall seeing our treatment message. Recall Any Campaign is a binary indicator that takes value one if the ATM user reported to recall seeing any advertising campaign in the ATM. Treatment is a binary indicator that takes value one if the ATM user was asked in one of the four ATMs in *Campo de Ourique*. The omitted categories in the control variables are No Primary Education and High Interest in Politics. Robust standard errors are depicted in parenthesis. Stars indicate significance levels of 10% (*), 5% (**), and 1% (***).

4.4 Results

4.4.1 Average treatment effects

In Table 4.3 we present the results for the intent-to-treat estimates from equation (1). Across specifications, our findings suggest that treatment causes an increase in turnout, although it never reaches standard levels of statistical significance. It is reassuring to see that the magnitude of the effect is stable as controls are added across specifications, while the precision of the treatment effect increases.²⁰

Table 4.3: **Treatment effects**

	Turnout Rate 2017			
	(1)	(2)	(3)	(4)
Treatment	0.119 (0.458)	0.109 (0.322)	0.101 (0.178)	0.091 (0.175)
Municipal dummies	No	Yes	Yes	Yes
Stratification Controls	No	No	Yes	Yes
Other Controls	No	No	No	Yes
Number of observations	1 703	1 703	1 703	1 703
Mean of Control Group	61.173	61.173	61.173	61.173
Adjusted R2	0.001	0.574	0.870	0.876

Notes : The dependent variable, Turnout Rate 2017, is measured in percentage terms. Treatment is a binary indicator that takes value one if all ATMs in the civil parish displayed the voting campaign. There are 308 municipal dummies. The vector of Stratification Controls includes registered voters' density, the number of ATM devices, the number of commercial banks in 2016, and turnout in 2013 municipal elections. The vector of Other Controls includes the share population below 15 years old in 2011, the share of population above 70 years old in 2011, the share of new citizens in 2011 (since 2007), the share of citizens with primary education and below in 2011, the share of citizens with tertiary education in 2011, the mean value withdrawn in ATMs in September 2016, the unemployment rate in 2011, transfers to civil parishes per capita, the winning margin share in 2013 Local Elections, the number of candidates in the 2017 local election, an independent mayor dummy for 2017, the share leftist votes in the 2013 local election, a central government alignment dummy. Robust standard errors are depicted in parenthesis. Stars indicate significance levels of 10% (*), 5% (**), and 1% (***) .

²⁰Assuming for a moment the point estimate from column (3) as a treatment effect size, considering the mean turnout for the control group is 61.17% , , and taking the average number of voters per municipality, our treatment would increase turnout by 2.77 (=0.101/61.173 * 1677.65) voters, on average, per civil parish). Since our treatment was implemented in 682 civil parishes, our campaign would have a causal impact of 1889 more voters in the 2017 local elections.

4.4.2 Heterogeneous effects and Robustness

We now focus on estimating how the intensity of treatment affects voter turnout, making use of information provided by the ATM company, including the number of cards, operations, and withdrawals by civil parish. In addition to the binary treatment variable, we now include, successively, these three different indicators of intensity of ATM usage during the campaign, as well as the interaction term between intensity and treatment. Table 4.4 presents estimates for equation (2) using information on the intensity of treatment for the entire campaign period. Odd columns show results with fixed effects for the 308 municipalities, whereas even columns show similar results after adding the vector of stratification and additional controls.

Our results in Table 4.4 suggest that the wide use of ATMs can be a powerful tool for rallying voters. Considering the average ATM usage for the sample of civil parishes and the point estimate in Column (2), our results translate into an increase in the average turnout rate by 0.14 percentage points ($22.573 * 0.006$), thus indicating that people who live in areas with high ATM usage are mobilized by our treatment. These results are statistically significant and stable across the three proxies of intensity of ATM usage.²¹

In face of our concern that there could be a downward bias in treatment effects due to a mismatch between voting and treatment locations for a share of the voters, we restrict our attention to the intensity derived from ATM usage during the weekend, when voters

²¹In Figure D5 of the Appendix to this paper we provide a graph of the estimated treatment effects in Table 4.3. As expected, we see no differences for areas with limited ATM usage while we observe sizable effects in the areas with many ATM users. We also relax the functional form by separating our sample by each tercile of parishes in terms of ATM usage (measured by the number of unique cards). The results are presented in Table D3 of the Appendix to this paper and show that the tercile with highest ATM usage seem to be most responsive to our treatment. In addition, we also test the robustness of our findings with a difference-in-differences exercise using the turnout for the 2017 and the 2013 local elections. The results in Table D4 of the Appendix to this paper are very similar to our baseline results.

Table 4.4: Intensity Effects: Entire Campaign

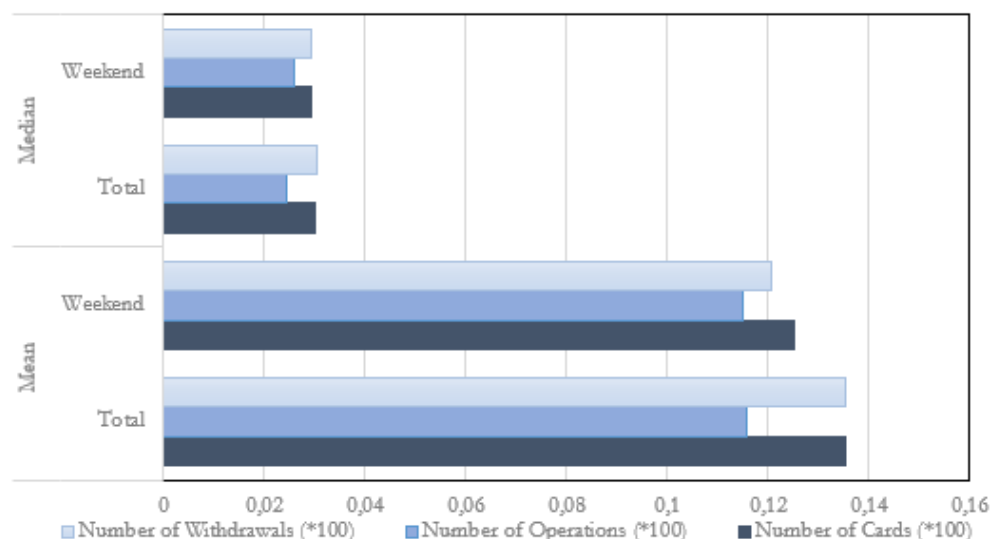
	Turnout Rate 2017					
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	-0.244 (0.350)	-0.035 (0.196)	-0.232 (0.349)	-0.034 (0.195)	-0.254 (0.351)	-0.039 (0.195)
Treatment * Number of Cards (*100)	0.016** (0.007)	0.006** (0.003)				
Number of Cards (*100)	-0.058*** (0.006)	-0.012 (0.010)				
Treatment * Number of Operations (*100)			0.009** (0.004)	0.003** (0.002)		
Number of Operations (*100)			-0.034*** (0.003)	-0.009 (0.006)		
Treatment * Number of Withdrawals (*100)					0.020** (0.009)	0.007** (0.003)
Number of Withdrawals (*100)					-0.069*** (0.007)	-0.020 (0.013)
Municipal Dummies	Yes	Yes	Yes	Yes	Yes	Yes
Stratification Controls	No	Yes	No	Yes	No	Yes
Other Controls	No	Yes	No	Yes	No	Yes
N	1 703	1 703	1 703	1 703	1 703	1 703
Adjusted R2	0.617	0.876	0.618	0.876	0.619	0.876

The dependent variable, Turnout Rate 2017, is measured in percentage terms. Treatment is a binary indicator that takes value one if all ATMs in the civil parish displayed the voting campaign. There are 308 municipal dummies. The vector of Stratification Controls includes registered voters' density, the number of ATM devices, the number of commercial banks in 2016, and turnout in 2013 municipal elections. The vector of Other Controls includes the share population below 15 years old in 2011, the share of population above 70 years old in 2011, the share of new citizens in 2011 (since 2007), the share of citizens with primary education and below in 2011, the share of citizens with tertiary education in 2011, the mean value withdrawn in ATMs in September 2016, the unemployment rate in 2011, transfers to civil parishes per capita, the winning margin share in 2013 Local Elections, the number of candidates in the 2017 local election, an independent mayor dummy for 2017, the share leftist votes in the 2013 local election, a central government alignment dummy. Robust standard errors are depicted in parenthesis. Stars indicate significance levels of 10% (*), 5% (**), and 1% (***)

moving across parishes for employment reasons is likely to be smaller. Table D5 of the Appendix to this paper presents results using weekend intensity measures, which confirm our previous findings.

For an easier interpretation of results, we compare the magnitudes for the mean and the median values of the three intensity measures. Figure 4.3 presents the results. We can see that point estimates for the total campaign period and weekend yield very similar results.

Figure 4.3: **Interpretation of intensity effects: mean and median**



In addition, we also run a battery of robustness exercises. First, we exclude the large urban areas of Lisbon and Oporto (in Table D6 of the Appendix to this paper), as well as the autonomous regions of Azores and Madeira (in Table D7 of the Appendix to this paper). These specifications aim to disregard possible concerns with the results being driven by more urban areas or insular regions. The results remain unchallenged. Finally, we run a horse race to test if our main result is affected by differences in

important control variables in the treatment and control areas. For that we modify equation (2) by adding an interaction term between the treatment status dummy and the top 5 most highly correlated control variables with ATM usage.²² These results (reported in Table D8 of the Appendix to this paper) confirm our main conclusions.

4.4.3 Falsification tests

In Table D9 of the Appendix to this paper we present again estimates considering the intensity of treatment for the entire campaign period, but employ instead the turnout rates for *(i)* the 2014 European elections, *(ii)* the 2015 legislative elections, and *(iii)* the 2016 presidential elections as the dependent variable. This constitutes a placebo test for our parameters of interest. We find no statistically significant effect of the campaign on turnout using the alternative placebo dependent variables, further strengthening our interpretation of the results as causal, associated with the specific timing and scope of the nationwide field experiment.

4.4.4 Cost-effectiveness discussion

So far, we have analyzed the potential benefits of using ATMs to get out the vote. But how much does it cost to make someone vote instead of abstaining? Is it cost-effective? These are precisely the questions that we tackle in this subsection.

The cost of our campaign, in terms of revenue that was not received from selling the ad space to other clients of the ATM system was estimated, by the partner organization, at around 35,000€ (2017 current prices). In addition, since we used the official layout

²²For space considerations, we present the results for the number of cards as our intensity measure. Results for the other intensity measures are available from the authors upon request.

– images, lettering, and official seal of the National Electoral Commission, no further costs on creating the visual aspects of the campaign should be imputed. Taking this into consideration we can reach two conclusions with back-of-the-envelope computations: 1) the cost of reaching an additional potential voter was below 3 cents ($35,000\text{€}/1,547,197$ unique card users in the treated area); and 2) the cost effectiveness of our campaign, considering the numbers presented in Section IV. 1, was 18.53€ ($35,000\text{€}/1,889$ new voters) or $\$ 20.81$ (at September 2017 exchange rates).

We compare these numbers with the benchmark on other modes of communication provided by Green and Gerber (2019). For ease of comparison, we present their conclusions Table D10 of the Appendix to this paper, adapted from Green and Gerber (2019). Two caveats should be mentioned. First, they only report cost-effectiveness estimates of tactics whose average impact has been demonstrated, in their survey of the literature, to be significantly greater than zero. Second, dollars-per-vote exclude start-up and management costs (which are included in our assessment). Regarding other modes of communication, costs range between $\$ 31$ (for door-to-door) and $\$ 91$ (for nonpartisan direct mail).

Hence, costs of converting get-out-the-vote efforts into voting, for the other means of communication reviewed by Green and Gerber (2019), seem to be substantially higher than the cost-benefit of our treatment. So, despite the relative modest effects of the treatment, it probably helps that the ATM campaign involved reaching people frequently in moments where they are, naturally, paying attention to a screen before withdrawing their own money or using the other wide-ranging services of the Portuguese ATMs.

4.5 Concluding Remarks

We conducted a field experiment that encompassed the entire universe of ATM machines in a country to assess whether treating a subset of civil parishes with a get-out-the-vote message based on activating the social norm of civic duty delivers higher turnout. Portugal is an appropriate testing ground as there is an ATM system that is both virtually universal, and particularly credible – it is the base for a wide array of sensitive financial and tax operations in the country. ATMs are so far an unexploited mode of communication for voter mobilization. All ATM users in treated civil parishes were subject to a message encouraging turnout based for three consecutive days leading to the day of the local elections in 2017. In the treated civil parishes, ATM users were exposed to no other message in ATMs, while in non-treated civil parishes voters were exposed to the usual advertisement messages. Considering the number of unique cards who saw our treatment in the 63 hours of the campaign, the cost of reaching an additional potential voter was below 3 cents.

While the estimated impact of treatment on turnout, despite the stability of the estimates, is not significant, results that consider the intensity of treatment measured by the number of ATM users, the number of operations and the number of withdrawals show a statistically significant effect on turnout. This is true for the whole treatment period, as well as for the weekend, when contamination between treatment and control is less likely. A placebo test using turnout for the previous election and a plethora of robustness checks further strengthen our causal interpretation.

The short time frame for which the experiment was run, and the low-cost and wide dissemination of the communication tool suggest a great potential of ATMs for channelling get-out-the-vote campaigns. The fact that voters were approached

electronically at a moment when they are likely to be paying attention to the means of communication provides an indication of what can be done through the internet employing ads that are strategically placed. Based on our findings, it is likely that ads linked to the financial movements of voters are effective at increasing electoral participation. These results can also potentially be generalizable to the use of automated kiosks that are being more and more used for transactions worldwide. Less closely related to ATMs, the (cost-)efficacy of ads in Youtube, Spotify, and other related digital platforms in get-out-the-vote efforts can also constitute an interesting avenue for future research.

Moreover, as voting moves to electronic platforms in many countries, and ATMs represent a highly secure network with unique capillarity, one can envision that electoral communication and procedures can increasingly be taken to these networks.

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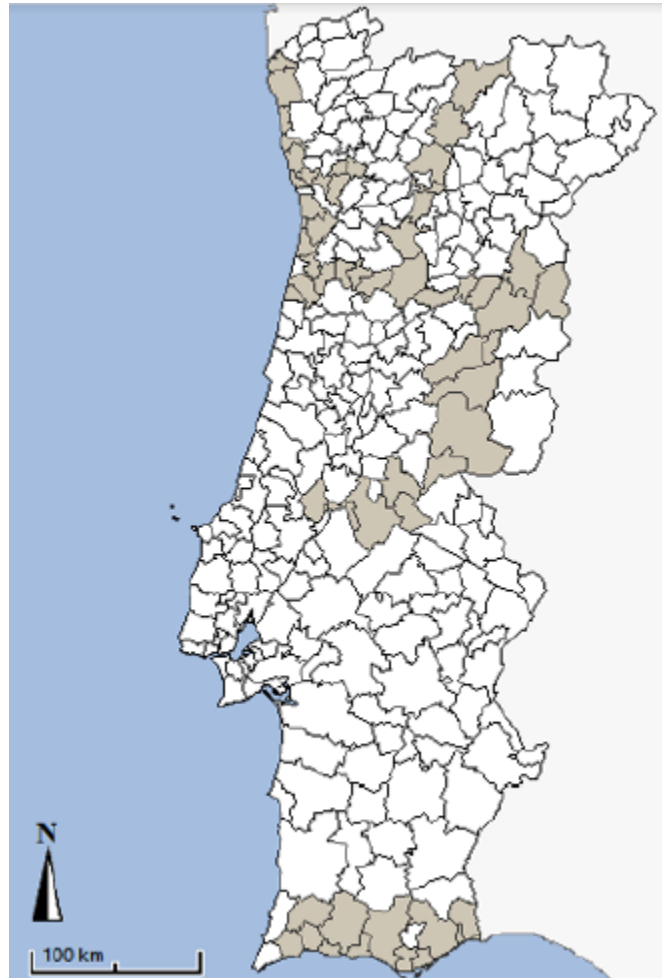
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Appendix A

Firms

Figures

Figure A.1: Geographical Distribution of Affected Municipalities



Note: Darkened regions represent treated municipalities whereas light regions depict municipalities in the comparison group.

Tables

Table A.1: Municipalities Affected by the Introduction of Tolls in the SCUT Highways

SCUT Highway	Municipalities Affected
Tolls introduced on the 15 th October 2010	
SCUT Grande Porto - 79 Km	
A4: AE Transmontana	Matosinhos, Maia.
A41: CREP - Circular Regional Exterior do Porto	Matosinhos, Valongo, Santa Maria da Feira, Espinho.
A42: AE Douro Litoral	Valongo, Paços de Ferreira, Paredes, Lousada.
SCUT Litoral Norte -113 Km	
A28	Matosinhos, Vila do Conde, Póvoa de Varzim, Esposende, Viana do Castelo, Caminha.
SCUT Costa da Prata – 110 Km	
A29	Estarreja, Ovar, Espinho, Vila Nova de Gaia.
Tolls introduced on the 8 th December 2011	
SCUT Algarve – 133 Km	
A22	Lagos, Monchique, Portimão, Lagoa, Silves, Albufeira, Loulé, Faro, Olhão, Tavira, Castro Marim, Vila Real de Sto. António.
SCUT Beira Interior – 217 Km	
A23	Torres Novas, Entroncamento, Constancia, Abrantes, Marvão, Gavião, Vila Velha de Rodão, Vila Nova da Barquinha, Castelo Branco, Fundão, Belmonte, Covilha, Guarda.
SCUT Interior Norte – 162 Km	
A24	Viseu, Castro Daire, Lamego, Peso da Régua, Vila Real, Vila Pouca de Aguiar, Chaves.
SCUT Beiras Litoral e Alta – 173 Km	
A25	Íhavo, Aveiro, Albergaria-a-Velha, Sever do Vouga, Oliveira de Frades, Vouzela, Viseu, Mangualde, Fornos de Algodres, Celorico da Beira, Guarda, Pinhel, Almeida.

Robustness Tests

Table AR.1: Firm performance Robustness: excluding i) the Lisbon metropolitan area and ii) district capitals

	Turnover		Total Expenses		Value Added		Profits	
	no Lisbon	no Capitals	no Lisbon	no Capitals	no Lisbon	no Capitals	no Lisbon	no Capitals
<i>Treated × Post</i>	-0.122*** (0.032)	-0.109*** (0.038)	-0.103*** (0.029)	-0.084*** (0.031)	-0.082*** (0.033)	-0.085*** (0.039)	-0.036*** (0.017)	-0.049*** (0.020)
Firm FE	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>
Municipality FE	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>
NUTS 3 × Year FE	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>
Controls × Year FE	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>
R-squared	0.036	0.041	0.051	0.058	0.052	0.058	0.022	0.023
Observations	2,461,358	2,592,200	2,460,662	2,591,414	2,077,192	2,182,062	1,597,723	1,663,291
Number of Firms	371,225	403,036	371,218	403,028	350,049	378,460	313,346	337,342

Notes: Standard errors in parenthesis are clustered at the municipal level. The vector of socio-demographic and economic controls includes electricity consumption per capita, age dependency ratio, population density and expenses per capita. Asterisks indicate significance levels of 10% (*), 5% (**), and 1%(***) , respectively.

Table AR.2: Firm performance Robustness: excluding i) municipalities without highways in the comparison group and ii) balanced sample

	Turnover		Total Expenses		Value Added		Profits	
	no Highways	Balanced	no Highways	Balanced	no Highways	Balanced	no Highways	Balanced
<i>Treated × Post</i>	-0.066* (0.039)	-0.068** (0.035)	-0.067** (0.032)	-0.054** (0.025)	-0.054 (0.037)	-0.034 (0.029)	-0.025 (0.020)	-0.034* (0.018)
Firm FE	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>
Municipality FE	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>
NUTS 3 × Year FE	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>
Controls × Year FE	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>
R-squared	0.039	0.059	0.055	0.075	0.055	0.064	0.022	0.031
Observations	3,356,784	1,668,469	3,355,520	1,668,010	2,789,530	1,505,498	2,119,118	1,222,294
Number of Firms	511,124	151,679	511,118	151,679	478,668	151,017	425,842	148,818

Notes: Standard errors in parenthesis are clustered at the municipal level. The vector of socio-demographic and economic controls includes electricity consumption per capita, age dependency ratio, population density and expenses per capita. Asterisks indicate significance levels of 10% (*), 5% (**), and 1% (***) , respectively.

Table AR.3: Trade Robustness: excluding i) the Lisbon metropolitan area and ii) district capitals

Exclude	Domestic Turnover		Exports		Domestic Purchases		Imports	
	no Lisbon	no Capitals	no Lisbon	no Capitals	no Lisbon	no Capitals	no Lisbon	no Capitals
<i>Treated × Post</i>	-0.120*** (0.031)	-0.104*** (0.037)	0.005 (0.025)	-0.018 (0.029)	-0.111*** (0.032)	-0.079** (0.031)	-0.003 (0.020)	-0.031 (0.022)
Firm FE	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>
Municipality FE	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>
NUTS 3 × Year FE	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>
Controls × Year FE	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>
R-squared	0.035	0.039	0.004	0.003	0.047	0.050	0.004	0.003
Observations	2,461,358	2,592,200	2,461,358	2,592,200	2,461,358	2,592,200	2,461,358	2,592,200
Number of Firms	371,225	403,036	371,225	403,036	371,225	403,036	371,225	403,036

Notes: Standard errors in parenthesis are clustered at the municipal level. The vector of socio-demographic and economic controls includes electricity consumption per capita, age dependency ratio, population density and expenses per capita. Asterisks indicate significance levels of 10% (*), 5% (**), and 1%(***), respectively.

Table AR.4: Trade Robustness: excluding i) municipalities without highways in the comparison group and ii) balanced sample

	Domestic Turnover		Exports		Domestic Purchases		Imports	
	no Highways	Balanced	no Highways	Balanced	no Highways	Balanced	no Highways	Balanced
<i>Treated × Post</i>	-0.069* (0.037)	-0.068** (0.034)	0.010 (0.028)	0.030 (0.031)	-0.078*** (0.029)	-0.062** (0.029)	0.014 (0.021)	0.010 (0.026)
Firm FE	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>
Municipality FE	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>
NUTS 3 × Year FE	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>
Controls × Year FE	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>
R-squared	0.038	0.056	0.004	0.007	0.047	0.053	0.003	0.003
Observations	3,356,784	1,668,469	3,356,784	1,668,469	3,356,784	1,668,469	3,356,784	1,668,469
Number of Firms	511,124	151,679	511,124	151,679	511,124	151,679	511,124	151,679

Notes: Standard errors in parenthesis are clustered at the municipal level. The vector of socio-demographic and economic controls includes electricity consumption per capita, age dependency ratio, population density and expenses per capita. Asterisks indicate significance levels of 10% (*), 5% (**), and 1% (***) respectively.

Table A.5: Labor Robustness: excluding i) the Lisbon metropolitan area and ii) district capitals

Exclude	Paid Employment		Average Wage		Labor Productivity	
	no Lisbon	no Capitals	no Lisbon	no Capitals	no Lisbon	no Capitals
<i>Treated</i> × <i>Post</i>	-0.020*** (0.005)	-0.018*** (0.006)	-57.490 (57.815)	-96.577 (68.583)	-730.971 (1,996.758)	1,854.510 (2,240.964)
Firm FE	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>
Municipality FE	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>
NUTS 3 × Year FE	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>
Controls × Year FE	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>
R-squared	0.025	0.029	0.010	0.008	0.000	0.001
Observations	2,453,617	2,583,610	1,969,938	2,069,516	1,969,938	2,069,516
Number of Firms	370,992	402,744	329,512	355,804	329,512	355,804

Notes: Standard errors in parenthesis are clustered at the municipal level. The vector of socio-demographic and economic controls includes electricity consumption per capita, age dependency ratio, population density and expenses per capita. Asterisks indicate significance levels of 10% (*), 5% (**), and 1%(***), respectively.

Table A.6: Labor Robustness: excluding i) municipalities without highways in the comparison group and ii) balanced sample

	Paid Employment		Average Wage		Labor Productivity	
	no Highways	Balanced	no Highways	Balanced	no Highways	Balanced
<i>Treated</i> × <i>Post</i>	-0.013** (0.006)	-0.019*** (0.007)	4.358 (68.447)	-70.668 (71.816)	38.339 (2,225.762)	-295.368 (2,715.546)
Firm FE	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>
Municipality FE	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>
NUTS 3 × Year FE	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>
Controls × Year FE	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>
R-squared	0.027	0.051	0.005	0.007	0.000	0.000
Observations	3,345,013	1,667,255	2,624,574	1,484,934	2,624,574	1,484,934
Number of Firms	510,766	151,679	445,748	148,360	445,748	148,360

Notes: Standard errors in parenthesis are clustered at the municipal level. The vector of socio-demographic and economic controls includes electricity consumption per capita, age dependency ratio, population density and expenses per capita. Asterisks indicate significance levels of 10% (*), 5% (**), and 1%(***), respectively.

Table A.7: Moved Robustness: i) excluding the Lisbon metropolitan area, ii) excluding district capitals, iii) municipalities without Highways in the comparison group, and iv) balanced sample

	Moved			
	no Lisbon	no Capitals	no Highways	Balanced
<i>Treated</i> × <i>Post</i>	0.002 (0.004)	0.003 (0.002)	0.002 (0.005)	0.002 (0.003)
Firm FE	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>
Municipality FE	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>
NUTS 3 × Year FE	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>
Controls × Year FE	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>
Observations	2,461,358	2,592,200	3,356,784	1,668,469
R-squared	0.004	0.005	0.005	0.005

Notes: Standard errors in parenthesis are clustered at the municipal level. The vector of socio-demographic and economic controls includes electricity consumption per capita, age dependency ratio, population density and expenses per capita. Asterisks indicate significance levels of 10% (*), 5% (**), and 1%(***), respectively.

Heterogeneity Tests

Table AH.1: Firm performance Heterogeneity: Tradables vs non-tradables

	Turnover		Total Expenses		Value Added		Profits	
	T	NT	T	NT	T	NT	T	NT
<i>Treated × Post</i>	-0.100** (0.042)	-0.050 (0.044)	-0.063* (0.034)	-0.062 (0.039)	-0.067* (0.035)	-0.035 (0.037)	-0.056*** (0.019)	-0.026* (0.014)
Firm FE	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>
Municipality FE	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>
NUTS 2 × Year FE	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>
R-squared	0.035	0.043	0.041	0.067	0.047	0.064	0.019	0.025
Observations	1,631,134	2,048,926	1,630,511	2,048,196	1,392,655	1,670,056	1,067,094	1,269,704
Number of Firms	250,819	313,966	250,808	313,966	236,883	292,236	212,782	259,061

Notes: Standard errors in parenthesis are clustered at the municipal level. Asterisks indicate significance levels of 10% (*), 5% (**), and 1%(***), respectively.

Table AH.2: Firm performance Heterogeneity: Manufacturing vs Services

	Turnover		Total Expenses		Value Added		Profits	
	M	S	M	S	M	S	M	S
<i>Treated × Post</i>	-0.102* (0.053)	-0.073* (0.038)	-0.079* (0.045)	-0.055* (0.031)	-0.079 (0.049)	-0.039 (0.029)	-0.099*** (0.031)	-0.025*** (0.013)
Firm FE	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>
Municipality FE	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>
NUTS 2 × Year FE	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>
R-squared	0.046	0.036	0.049	0.047	0.050	0.049	0.026	0.020
Observations	439,117	2,682,601	439,007	2,681,700	397,209	2,216,182	307,898	1,665,234
Number of Firms	63,702	413,960	63,701	413,954	61,711	383,957	56,429	337,388

Notes: Standard errors in parenthesis are clustered at the municipal level. Asterisks indicate significance levels of 10% (*), 5% (**), and 1% (***), respectively.

Table AH.3: Trade Heterogeneity: Tradables vs non-tradables

	Domestic Turnover		Exports		Domestic Purchases		Imports	
	T	NT	T	NT	T	NT	T	NT
<i>Treated × Post</i>	-0.101** (0.044)	-0.045 (0.045)	-0.057* (0.031)	-0.039 (0.031)	-0.066* (0.036)	-0.050 (0.043)	-0.039* (0.023)	-0.047 (0.033)
Firm FE	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>
Municipality FE	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>
NUTS 2 × Year FE	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>
R-squared	0.033	0.041	0.004	0.003	0.024	0.068	0.003	0.004
Observations	1,631,134	2,048,926	1,631,134	2,048,926	1,631,134	2,048,926	1,631,134	2,048,926
Number of Firms	250,819	313,966	250,819	313,966	250,819	313,966	250,819	313,966

Notes: Standard errors in parenthesis are clustered at the municipal level. Asterisks indicate significance levels of 10% (*), 5% (**), and 1%(***) , respectively.

Table AH.4: Trade Heterogeneity: Manufacturing vs Services

	Domestic Turnover		Exports		Domestic Purchases		Imports	
	M	S	M	S	M	S	M	S
<i>Treated × Post</i>	-0.101*	-0.074*	-0.113*	-0.020	-0.134**	-0.038	-0.079	-0.039
	(0.052)	(0.041)	(0.061)	(0.022)	(0.056)	(0.028)	(0.056)	(0.028)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
NUTS 2 × Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.045	0.034	0.009	0.003	0.051	0.038	0.007	0.003
Observations	439,117	2,682,601	439,117	2,682,601	439,117	2,682,601	439,117	2,682,601
Number of Firms	63,702	413,960	63,702	413,960	63,702	413,960	63,702	413,960

Notes: Standard errors in parenthesis are clustered at the municipal level. Asterisks indicate significance levels of 10% (*), 5% (**), and 1%(***) , respectively.

Table AH.5: Labor Heterogeneity: Tradables vs non-tradables

	Paid Employment		Average Wage		Labor Productivity	
	T	NT	T	NT	T	NT
<i>Treated</i> × <i>Post</i>	-0.019*** (0.007)	-0.003 (0.006)	-18.980 (94.855)	-44.687 (61.769)	-1,907.124 (3,075.788)	-386.813 (254.791)
Firm FE	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>
Municipality FE	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>
NUTS 2 × Year FE	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>
R-squared	0.017	0.039	0.005	0.007	0.000	0.001
Observations	1,626,134	2,041,251	1,318,925	1,565,595	1,190,473	1,377,044
Number of Firms	250,660	313,744	223,720	269,179	210,802	249,917

Notes: Standard errors in parenthesis are clustered at the municipal level. Asterisks indicate significance levels of 10% (*), 5% (**), and 1%(***), respectively.

Table AH.6: Labor Heterogeneity: Manufacturing vs Services

	Paid Employment		Average Wage		Labor Productivity	
	M	S	M	S	M	S
<i>Treated</i> × <i>Post</i>	-0.029* (0.015)	-0.010* (0.005)	-117.152 (80.965)	0.408 (70.031)	-3,739.623 (7,374.162)	-368.516 (678.758)
Firm FE	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>
Municipality FE	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>
NUTS 2 × Year FE	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>
R-squared	0.043	0.017	0.013	0.005	0.001	0.001
Observations	438,265	2,672,654	383,527	2,071,259	361,750	1,829,826
Number of Firms	63,676	413,636	59,820	356,512	57,939	329,408

Notes: Standard errors in parenthesis are clustered at the municipal level. Asterisks indicate significance levels of 10% (*), 5% (**), and 1%(***), respectively.

Table AH.7: Moved Heterogeneity: Tradables vs non-tradables and Manufacturing vs Services

	Moved			
	T	NT	M	S
<i>Treated</i> × <i>Post</i>	0.001 (0.003)	0.001 (0.003)	0.001 (0.002)	0.001 (0.003)
Firm FE	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>
Municipality FE	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>
NUTS 2 × Year FE	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>	<i>Yes</i>
Observations	1,631,134	2,048,926	887,783	2,682,601
R-squared	0.005	0.004	0.005	0.005

Notes: Standard errors in parenthesis are clustered at the municipal level. Asterisks indicate significance levels of 10% (*), 5% (**), and 1%(***), respectively.

Appendix B

Workers

Figures

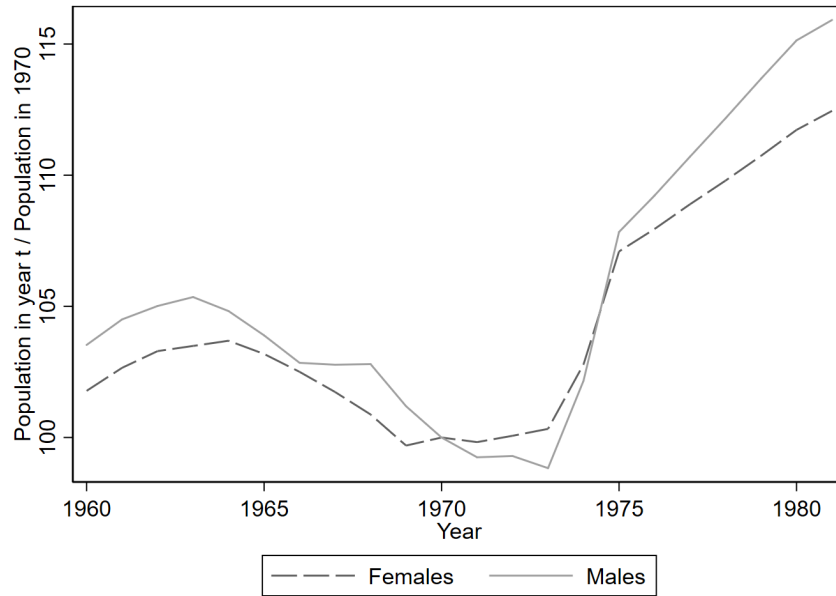


Figure B1 Relative Population.

Notes: Female population in 1970: 4,546 millions. Male population in 1970: 4,078 millions.

Source: Statistics Portugal, own construction.

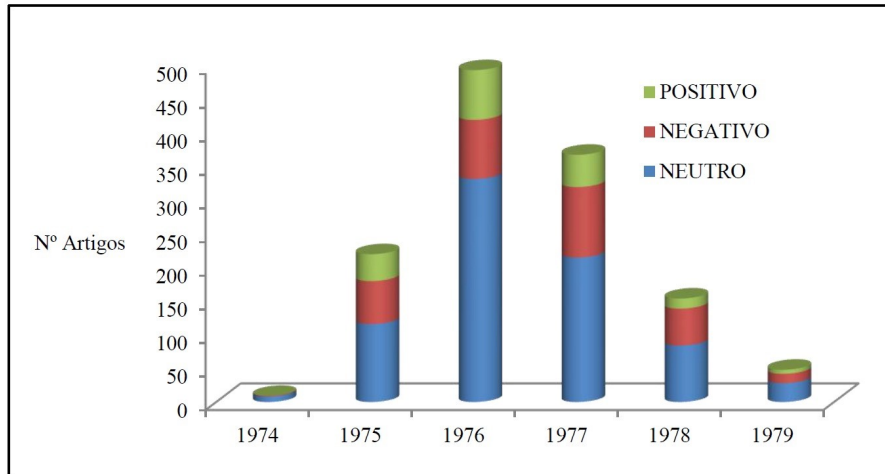


Figure B2 Number of references to repatriates in two Portuguese daily newspapers. Notes: This figure is retrieved from Lourenço (2018). News are collected from two newspapers: Primeiro de Janeiro (from Porto) and Diário de Notícias (from Lisbon).

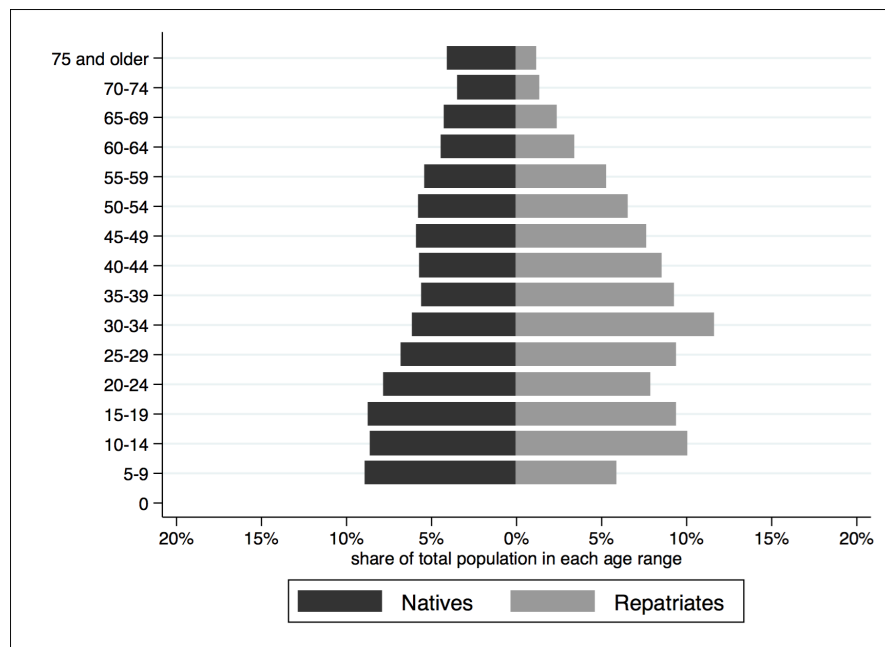


Figure B3 Age pyramid natives vs repatriates in 1981. Notes: The age range below five is not displayed as the data set on repatriates only contains repatriates above the age of seven. Source: census of 1981, Statistics Portugal, own construction.

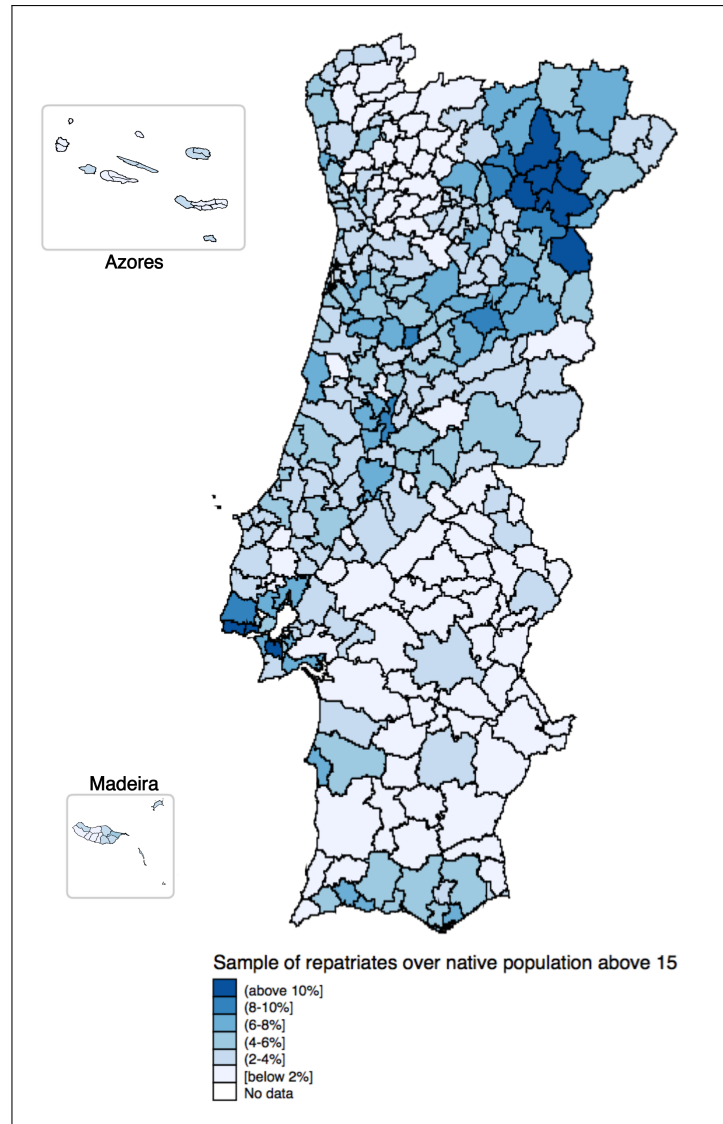


Figure B4 Repatriate settlement across municipalities in 1981.

Source: census of 1981, Statistics Portugal, own construction.

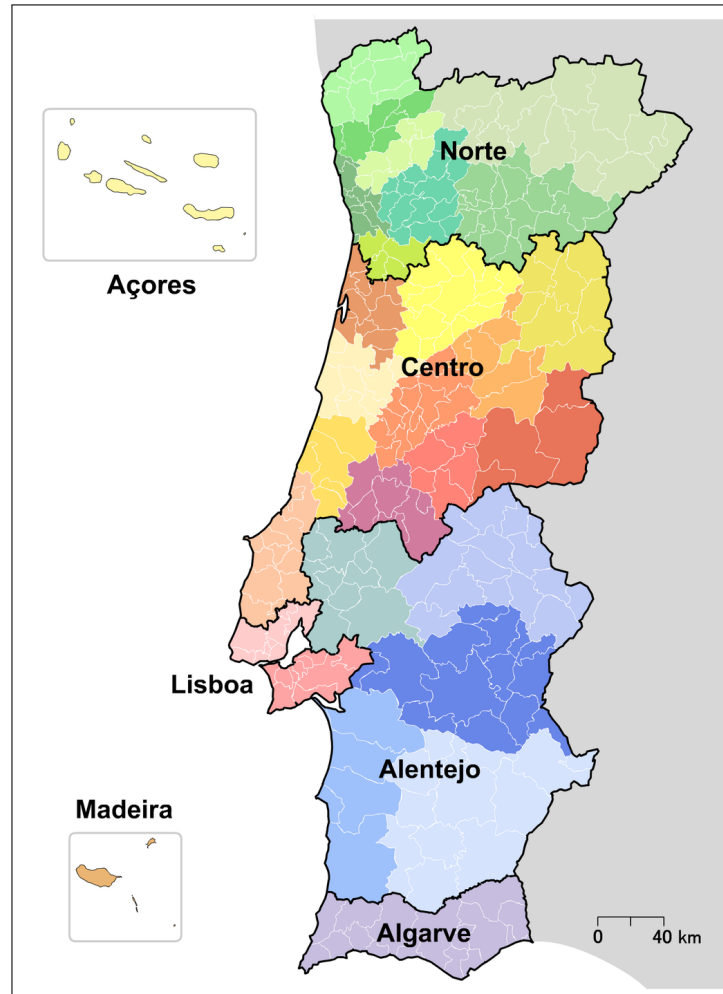


Figure B5 NUTS 3 and NUTS 2 regions according to the 2002 definition.

Notes: Different colors indicate different NUTS 3 regions, while the black outlines show NUTS 2 regions.

Source: Statistics Portugal.

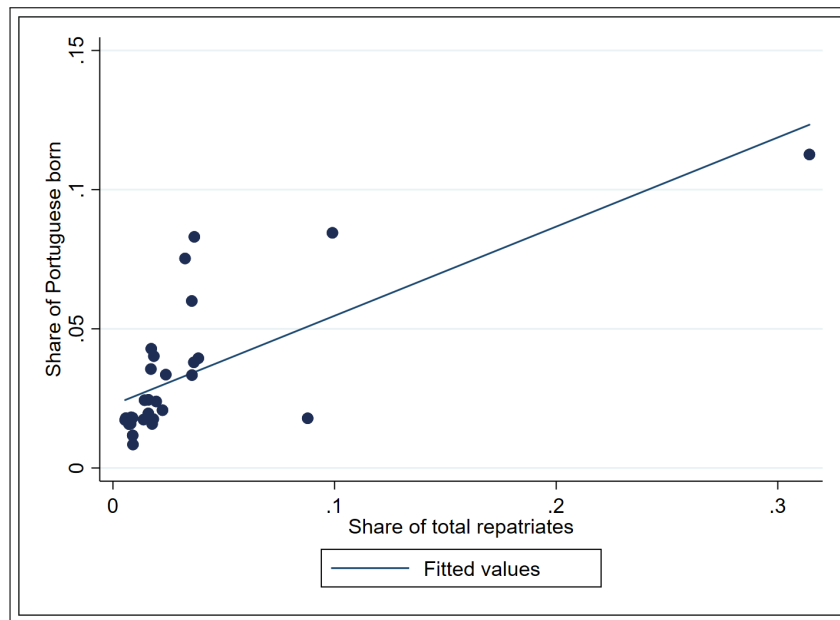


Figure B6 Are repatriates returning to their place of birth?

Notes: The share of Portuguese born is measured, for each NUTS 3 region, from those that migrated to Africa and are in Portugal in 1981. The most prominent outlier is Grande Lisboa.

Source: census of 1981, Statistics Portugal.

Tables

Table B1: Survey on the labor market consequences for natives in receiving countries using natural experiments

Authors	Event	Period	Origin	Destination	Main Results
Angrist and Kugler (2003)	Balkan Wars	1973–1991	Former Yugoslavia	West. Europe	Employment (-)
Beerli et al. (2021)	Cross-border reform	1999–2013	European Union	Switzerland	Wages of highly educated (+)
Braun and Mahmoud (2014); Braun and Weber (2021)	Expulsion of Germans after World War II	1944–1946	Eastern Europe	Germany (West)	Employment (-); Employment and wages (-), regional migration (+)
Calderón-Mejía and Ibáñez (2016)	Internal conflict	1998–2013	Colombia	Colombia	Wages (-) for low-skilled and in informal sector
Cohen-Goldner and Paserman (2011)	Migration restriction lifted in the former U.S.S.R.	1990–1999	Former U.S.S.R.	Israel	Employment (0), wages (-)
Dustmann et al. (2017)	Commuting policy reform	1944–1946	Czech Republic	Germany	Employment (-) sharply; Wages (-)
Edo (2020)	Algerian War	1962–1976	Algeria	France	Employment (-), especially for females, wages (-) in the short-run but recover in 15 years
Foged and Peri (2016)	Wars	1994–2008	Bosnia, Afghanistan, Somalia, and Iraq	Denmark	Employment and Wages of low-skilled (+)
Friedberg (2001)	Migration restriction lifted	1990–1994	Former U.S.S.R.	Israel	Employment and wages (0)
Glitz (2012)	Fall of the Berlin Wall	1996–2001	East. Europe and former U.S.S.R.	Germany	Employment (-), wages (0)
Hunt (1992)	Algerian War	1962–1968	Algeria	France	Employment (-), wages (-)
Labanca (2020)	Arab Spring	2011	Egypt, Tunisia and Yemen	Italy	Employment (0), but considerable heterogeneity per sector
Monras (2020)	Mexican Peso Crisis	1994–1999	Mexico	U.S.A.	Native low-skilled wages (-)
Morales (2018)	Internal conflict	1960s–2008	Colombia	Colombia	Wages (-) in short-run, wages in long-run for men (0) and women (-), outmigration (+)
Ruiz and Vargas-Silva (2016)	Hutu-Tutsi conflict	1990s	Burundi and Rwanda	Tanzania	Employment (-)
Schumann (2014)	Expulsion of Germans	1944–1946	Eastern Europe	Germany	Industry employment (+)
Tabellini (2020)	WWI, Immigration Acts	1910–1930	Europe	U.S.A	Employment (+)
Tumen (2016)	Syrian civil war	2012–2014	Syria	Turkey	Informal employment (-), long-term unemployment (+), wages (0)

Table B2: White resident population in Angola and Mozambique, 1940 - 1970

Year	White resident population		
	Angola	Mozambique	Total
1940	44,083	27,438	71,521
1950	78,826	48,213	127,039
1960	172,529	97,245	269,774
1970	280,101	162,967	443,068

Notes: Source: Statistics Portugal, Recenseamentos Gerais da População de Angola e Moçambique.

Table B3: Population and Labor Force in 1981 by NUTS 3 and NUTS 2 regions

Region	Native Population above 15	Native Labor Force
Grande Porto	773,788	480,728
Tâmega	328,242	175,560
Ave	289,346	190,280
Cávado	212,680	122,738
Alto Trás-os-Montes	181,544	80,195
Mínho-Lima	179,634	87,503
Douro	172,304	81,556
Entre Douro e Vouga	161,565	101,354
Região do Norte	2,299,103	1,319,914
Grande Lisboa	1,299,030	798,476
Península de Setúbal	394,596	230,053
Região de Lisboa	1,693,626	1,028,529
Baixo Mondego	235,406	126,627
Baixa Vouga	229,908	133,111
Oeste	226,672	118,855
Dão-Lafões	199,922	104,982
Médio Tejo	164,753	80,345
Pinhal Litoral	150,897	83,332
Pinhal Interior Norte	110,555	49,663
Beira Interior Norte	93,514	42,640
Cova da Beira	74,185	37,953
Beira Interior Sul	66,234	28,063
Pinhal Interior Sul	45,798	19,751
Serra da Estrela	39,473	19,688
Região do Centro	1,637,317	845,010
Lezíria do Tejo	173,980	95,960
Alentejo Central	138,687	76,565
Baixo Alentejo	122,216	58,792
Alto Alentejo	111,167	54,377
Alentejo Litoral	77,280	41,502
Alentejo	623,330	327,196
Algarve	244,654	123,987
Algarve	244,654	123,987
Região Autónoma da Madeira	170,975	93,907
Região Autónoma da Madeira	170,975	93,907
Região Autónoma dos Açores	155,220	71,728
Região Autónoma dos Açores	155,220	71,728
Portugal	6,824,225	3,810,271

Notes: The regions in bold are NUTS 2 regions. The Labor Force is defined as all those who are employed (i.e. in paid employment) and unemployed. Source: census of 1981, computations by the author.

Table B4: Labor market effects of repatriates on male natives - Kronmal specification

Outcomes for male natives	OLS		IV		
	(1)	(2)	(3)	(4)	(5)
Δ LFP	-0.002 (0.015)	0.018 (0.016)	-0.012 (0.011)	-0.012 (0.011)	-0.011 (0.010)
Δ Unemployment rate	-0.008 (0.005)	-0.005 (0.008)	0.014* (0.006)	0.014* (0.006)	0.014* (0.006)
Δ Employment share	0.006 (0.017)	0.021 (0.020)	-0.026* (0.012)	-0.026* (0.012)	-0.024* (0.011)
Δ Share Employee	-0.028 (0.037)	-0.058 (0.039)	-0.186*** (0.036)	-0.185*** (0.035)	-0.179*** (0.033)
Δ Share Entrepreneur	0.014 (0.022)	0.056 (0.031)	0.141*** (0.034)	0.140*** (0.033)	0.136*** (0.032)
Δ Share Employer	-0.017** (0.005)	-0.021** (0.007)	-0.027*** (0.007)	-0.027*** (0.007)	-0.027*** (0.007)
Δ Share Self-employed	0.030 (0.019)	0.078* (0.035)	0.168*** (0.038)	0.167*** (0.037)	0.163*** (0.036)
Controls	NO	YES	YES	YES	YES
Instrument	-	-	IV1	IV2	IV3
First-stage coefficient	-	-	0.507	0.505	0.519
First-stage F-statistic	-	-	19.75	20.12	23.56
Observations	30	30	30	30	30

Notes: Robust standard errors in parentheses. The independent variable is the log of repatriates. All outcomes refer to changes between 1960 and 1981. LFP, Employment share, Share Employee, Share Entrepreneurship, Share Employer and Share Self-employed are calculated as shares over the native working age population. The unemployment rate refers to the share of unemployed over the native labor force. All regression contain dummies for NUTS 2 regions. Controls contain the following parameters in 1960: unemployed and entrepreneurs as share of working age population, inactive and population below 15 as share of total population, those with higher education as share of those above 15 and the native population above 15 in 1981. IV1 is the instrument based on educational network effects, IV2 is the instrument based on network effects according to the colony lived in and IV3 is the basic Bartik instrument. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table B5: Labor market effects of repatriates on female natives - Kronmal specification

Outcomes for male natives	OLS		IV		
	(1)	(2)	(3)	(4)	(5)
Δ LFP	-0.0139 (0.023)	-0.0382 (0.033)	-0.119*** (0.023)	-0.118*** (0.022)	-0.116*** (0.022)
Δ Unemployment rate	-0.057 (0.029)	-0.036 (0.038)	0.043 (0.024)	0.042 (0.024)	0.040 (0.023)
Δ Employment share	0.010 (0.023)	-0.019 (0.035)	-0.114*** (0.024)	-0.113*** (0.023)	-0.110*** (0.022)
Δ Share Employee	-0.029 (0.020)	-0.039 (0.026)	-0.124*** (0.025)	-0.123*** (0.024)	-0.121*** (0.023)
Δ Share Entrepreneur	0.031 (0.015)	0.010 (0.019)	0.005 (0.026)	0.005 (0.026)	0.005 (0.025)
Δ Share Employer	-0.001 (0.001)	-0.002 (0.001)	-0.004* (0.002)	-0.004* (0.002)	-0.004* (0.001)
Δ Share Self-employed	0.031 (0.015)	0.012 (0.019)	0.009 (0.025)	0.009 (0.025)	0.009 (0.024)
Controls	NO	YES	YES	YES	YES
Instrument	-	-	IV1	IV2	IV3
First-stage coefficient	-	-	0.507	0.505	0.519
First-stage F-statistic	-	-	19.75	20.12	23.56
Observations	30	30	30	30	30

Notes: Robust standard errors in parentheses. The independent variable is the log of repatriates. All outcomes refer to changes between 1960 and 1981. LFP, Employment share, Share Employee, Share Entrepreneurship, Share Employer and Share Self-employed are calculated as shares over the native working age population. The unemployment rate refers to the share of unemployed over the native labor force. All regression contain dummies for NUTS 2 regions. Controls contain the following parameters in 1960: unemployed and entrepreneurs as share of working age population, inactive and population below 15 as share of total population, those with higher education as share of those above 15 and the native population above 15 in 1981. IV1 is the instrument based on educational network effects, IV2 is the instrument based on network effects according to the colony lived in and IV3 is the basic Bartik instrument. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table B6: Labor market effects of repatriates on male natives - 27 regions

Outcomes for male natives	OLS		IV		
	(1)	(2)	(3)	(4)	(5)
Δ LFP	0.271 (0.465)	0.481 (0.458)	0.074 (0.278)	0.077 (0.286)	0.086 (0.284)
Δ Unemployment rate	-0.182 (0.175)	-0.031 (0.176)	0.160 (0.123)	0.160 (0.122)	0.160 (0.121)
Δ Employment share	0.450 (0.497)	0.471 (0.533)	-0.100 (0.309)	-0.098 (0.318)	-0.089 (0.316)
Δ Share Employee	-0.231 (1.108)	-1.995 (1.059)	-3.384*** (0.790)	-3.353*** (0.806)	-3.289*** (0.790)
Δ Share Entrepreneur	0.257 (0.702)	1.846* (0.788)	2.748*** (0.692)	2.715*** (0.695)	2.661*** (0.681)
Δ Share Employer	-0.699*** (0.119)	-0.635*** (0.142)	-0.635*** (0.150)	-0.641*** (0.150)	-0.647*** (0.147)
Δ Share Self-employed	0.957 (0.666)	2.481** (0.822)	3.383*** (0.769)	3.356*** (0.771)	3.308*** (0.757)
Controls	NO	YES	YES	YES	YES
Instrument	-	-	IV1	IV2	IV3
First-stage coefficient	-	-	0.569	0.553	0.401
First-stage F-statistic	-	-	87.06	81.55	96.26
Observations	27	27	27	27	27

Notes: Robust standard error in parentheses. The independent variable is the sample of repatriates over the native population above 15. All outcomes refer to changes between 1960 and 1981. LFP, Employment share, Share Employee, Share Entrepreneurship, Share Employer and Share Self-employed are calculated as shares over the native working age population. The unemployment rate refers to the share of unemployed over the native labor force. All regression contain dummies for NUTS 2 regions. Controls contain the following parameters in 1960: unemployed and entrepreneurs as share of working age population, inactive and population below 15 as share of total population, those with higher education as share of those above 15. IV1 is the instrument based on educational network effects, IV2 is the instrument based on network effects according to the colony lived in and IV3 is the basic Bartik instrument. The regressions exclude Grande Lisboa, the Peninsula of Setúbal and Algarve from the NUTS 3 regions. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table B7: Labor market effects of repatriates on female natives - 27 regions

Outcomes for female natives	OLS		IV		
	(1)	(2)	(3)	(4)	(5)
Δ LFP	-0.510 (0.745)	-1.129 (0.854)	-1.676* (0.654)	-1.644* (0.659)	-1.654* (0.655)
Δ Unemployment rate	-1.618 (0.895)	-0.646 (0.902)	0.021 (0.527)	0.023 (0.520)	0.039 (0.516)
Δ Employment share	0.228 (0.768)	-0.715 (0.870)	-1.439* (0.642)	-1.415* (0.642)	-1.425* (0.636)
Δ Share Employee	-1.203 (0.600)	-1.049 (0.725)	-1.939*** (0.542)	-1.921*** (0.551)	-1.904*** (0.543)
Δ Share Entrepreneur	1.301* (0.552)	0.074 (0.453)	0.315 (0.452)	0.319 (0.453)	0.293 (0.446)
Δ Share Employer	-0.054 (0.026)	-0.052 (0.038)	-0.064 (0.033)	-0.063 (0.033)	-0.063 (0.033)
Δ Share Self-employed	1.355* (0.549)	0.126 (0.449)	0.378 (0.438)	0.381 (0.439)	0.356 (0.433)
Controls	NO	YES	YES	YES	YES
Instrument	-	-	IV1	IV2	IV3
First-stage coefficient	-	-	0.569	0.553	0.401
First-stage F-statistic	-	-	87.06	81.55	96.26
Observations	27	27	27	27	27

Notes: Robust standard error in parentheses. The independent variable is the sample of repatriates over the native population above 15. All outcomes refer to changes between 1960 and 1981. LFP, Employment share, Share Employee, Share Entrepreneurship, Share Employer and Share Self-employed are calculated as shares over the native working age population. The unemployment rate refers to the share of unemployed over the native labor force. All regression contain dummies for NUTS 2 regions. Controls contain the following parameters in 1960: unemployed and entrepreneurs as share of working age population, inactive and population below 15 as share of total population, those with higher education as share of those above 15. IV1 is the instrument based on educational network effects, IV2 is the instrument based on network effects according to the colony lived in and IV3 is the basic Bartik instrument. The regressions exclude Grande Lisboa, the Peninsula of Setúbal and Algarve from the NUTS 3 regions.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table B8: Labor market effects of repatriates on male natives - 25 regions

Outcomes for male natives	OLS		IV		
	(1)	(2)	(3)	(4)	(5)
Δ LFP	-0.225 (0.361)	0.476 (0.496)	-0.171 (0.270)	-0.170 (0.283)	-0.137 (0.269)
Δ Unemployment rate	-0.044 (0.129)	0.077 (0.176)	0.268* (0.117)	0.270* (0.117)	0.263* (0.116)
Δ Employment share	-0.191 (0.332)	0.372 (0.567)	-0.439 (0.287)	-0.440 (0.301)	-0.402 (0.285)
Δ Share Employee	-2.205** (0.787)	-2.255 (1.185)	-4.186*** (0.924)	-4.168*** (0.937)	-4.059*** (0.893)
Δ Share Entrepreneur	1.179 (0.609)	2.028* (0.871)	3.260*** (0.812)	3.236*** (0.813)	3.159*** (0.784)
Δ Share Employer	-0.329 (0.240)	-0.633** (0.181)	-0.687*** (0.167)	-0.695*** (0.169)	-0.697*** (0.166)
Δ Share Self-employed	1.508* (0.623)	2.661** (0.949)	3.946*** (0.922)	3.931*** (0.924)	3.856*** (0.894)
Controls	NO	YES	YES	YES	YES
Instrument	-	-	IV1	IV2	IV3
First-stage coefficient	-	-	0.501	0.484	0.355
First-stage F-statistic	-	-	55.28	50.92	65.83
Observations	25	25	25	25	25

Notes: Robust standard error in parentheses. The independent variable is the sample of repatriates over the native population above 15. All outcomes refer to changes between 1960 and 1981. LFP, Employment share, Share Employee, Share Entrepreneurship, Share Employer and Share Self-employed are calculated as shares over the native working age population. The unemployment rate refers to the share of unemployed over the native labor force. All regression contain dummies for NUTS 2 regions. Controls contain the following parameters in 1960: unemployed and entrepreneurs as share of working age population, inactive and population below 15 as share of total population, those with higher education as share of those above 15. IV1 is the instrument based on educational network effects, IV2 is the instrument based on network effects according to the colony lived in and IV3 is the basic Bartik instrument. The regressions exclude the NUTS 2 regions Alentejo (corresponding NUTS 3 regions: Alentejo Central, Alentejo Litoral, Baixo Alentejo, Lezíria do Tejo, Alto Alentejo) from the regions. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table B9: Labor market effects of repatriates on female natives - 25 regions

Outcomes for female natives	OLS		IV		
	(1)	(2)	(3)	(4)	(5)
Δ LFP	-0.572 (0.693)	-1.405 (0.914)	-2.038*** (0.594)	-2.003*** (0.600)	-2.002*** (0.593)
Δ Unemployment rate	-0.002 (0.368)	0.179 (0.622)	0.352 (0.468)	0.364 (0.461)	0.370 (0.458)
Δ Employment share	-0.470 (0.622)	-1.192 (0.893)	-1.807** (0.591)	-1.785** (0.593)	-1.782** (0.585)
Δ Share Employee	-1.223 (0.639)	-1.570* (0.613)	-2.145*** (0.431)	-2.120*** (0.440)	-2.121*** (0.428)
Δ Share Entrepreneur	0.405 (0.575)	0.163 (0.542)	0.161 (0.465)	0.155 (0.464)	0.158 (0.460)
Δ Share Employer	-0.001 (0.036)	-0.056 (0.044)	-0.076* (0.032)	-0.075* (0.033)	-0.075* (0.033)
Δ Share Self-employed	0.406 (0.590)	0.219 (0.519)	0.237 (0.450)	0.231 (0.449)	0.234 (0.445)
Controls	NO	YES	YES	YES	YES
Instrument	-	-	IV1	IV2	IV3
First-stage coefficient	-	-	0.501	0.484	0.355
First-stage F-statistic	-	-	55.28	50.92	65.83
Observations	25	25	25	25	25

Notes: Robust standard error in parentheses. The independent variable is the sample of repatriates over the native population above 15. All outcomes refer to changes between 1960 and 1981. LFP, Employment share, Share Employee, Share Entrepreneurship, Share Employer and Share Self-employed are calculated as shares over the native working age population. The unemployment rate refers to the share of unemployed over the native labor force. All regression contain dummies for NUTS 2 regions. Controls contain the following parameters in 1960: unemployed and entrepreneurs as share of working age population, inactive and population below 15 as share of total population, those with higher education as share of those above 15. IV1 is the instrument based on educational network effects, IV2 is the instrument based on network effects according to the colony lived in and IV3 is the basic Bartik instrument. The regressions exclude the NUTS 2 regions Alentejo (corresponding NUTS 3 regions: Alentejo Central, Alentejo Litoral, Baixo Alentejo, Lezíria do Tejo, Alto Alentejo) from the regions. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table B10: Labor market effects of repatriates on male natives - different region fixed effects

Outcomes for male natives	OLS		IV		
	(1)	(2)	(3)	(4)	(5)
Δ LFP	0.228 (0.366)	0.455 (0.353)	-0.248 (0.317)	-0.247 (0.329)	-0.213 (0.316)
Δ Unemployment rate	-0.186 (0.137)	-0.180 (0.224)	0.281 (0.161)	0.281 (0.160)	0.272 (0.158)
Δ Employment share	0.397 (0.399)	0.588 (0.492)	-0.522 (0.356)	-0.522 (0.369)	-0.479 (0.356)
Δ Share Employee	-0.490 (0.936)	-0.912 (1.166)	-3.965*** (0.891)	-3.941*** (0.901)	-3.766*** (0.855)
Δ Share Entrepreneur	0.318 (0.573)	0.926 (0.849)	2.939*** (0.773)	2.911*** (0.769)	2.779*** (0.731)
Δ Share Employer	-0.396 (0.204)	-0.427* (0.201)	-0.538** (0.188)	-0.541** (0.189)	-0.526** (0.181)
Δ Share Self-employed	0.714 (0.566)	1.353 (0.962)	3.476*** (0.840)	3.452*** (0.836)	3.304*** (0.793)
Controls	NO	YES	YES	YES	YES
Instrument	-	-	IV1	IV2	IV3
First-stage coefficient	-	-	0.517	0.501	0.370
First-stage F-statistic	-	-	75.84	73.86	83.79
Observations	30	30	30	30	30

Notes: Robust standard error in parentheses. The independent variable is the sample of repatriates over the native population above 15. All outcomes refer to changes between 1960 and 1981. LFP, Employment share, Share Employee, Share Entrepreneurship, Share Employer and Share Self-employed are calculated as shares over the native working age population. The unemployment rate refers to the share of unemployed over the native labor force. Controls contain the following parameters in 1960: unemployed and entrepreneurs as share of working age population, inactive and population below 15 as share of total population, those with higher education as share of those above 15. IV1 is the instrument based on educational network effects, IV2 is the instrument based on network effects according to the colony lived in and IV3 is the basic Bartik instrument. All regressions contain three regional dummies: a dummy containing the NUTS 3 regions Centre and North, a dummy comprising Alentejo, Algarve, and Lisbon, and a dummy for the islands Azores and Madeira. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table B11: Labor market effects of repatriates on female natives - different region fixed effects

Outcomes for female natives	OLS		IV		
	(1)	(2)	(3)	(4)	(5)
Δ LFP	-0.512 (0.581)	-0.655 (0.979)	-2.319* (1.027)	-2.276* (1.028)	-2.203* (1.030)
Δ Unemployment rate	-1.426 (0.733)	-1.231 (0.924)	0.114 (0.536)	0.112 (0.527)	0.089 (0.530)
Δ Employment share	0.087 (0.596)	-0.113 (1.043)	-2.020* (0.970)	-1.982* (0.968)	-1.910* (0.972)
Δ Share Employee	-0.771 (0.558)	-0.399 (0.738)	-2.010*** (0.600)	-1.986** (0.606)	-1.910** (0.604)
Δ Share Entrepreneur	0.604 (0.500)	0.034 (0.444)	-0.193 (0.589)	-0.182 (0.589)	-0.186 (0.575)
Δ Share Employer	-0.014 (0.031)	-0.032 (0.031)	-0.072 (0.038)	-0.071 (0.038)	-0.069 (0.037)
Δ Share Self-employed	0.618 (0.514)	0.066 (0.426)	-0.121 (0.568)	-0.111 (0.568)	-0.117 (0.553)
Controls	NO	YES	YES	YES	YES
Instrument	-	-	IV1	IV2	IV3
First-stage coefficient	-	-	0.517	0.501	0.370
First-stage F-statistic	-	-	75.84	73.86	83.79
Observations	30	30	30	30	30

Notes: Robust standard error in parentheses. The independent variable is the sample of repatriates over the native population above 15. All outcomes refer to changes between 1960 and 1981. LFP, Employment share, Share Employee, Share Entrepreneurship, Share Employer and Share Self-employed are calculated as shares over the native working age population. The unemployment rate refers to the share of unemployed over the native labor force. Controls contain the following parameters in 1960: unemployed and entrepreneurs as share of working age population, inactive and population below 15 as share of total population, those with higher education as share of those above 15. IV1 is the instrument based on educational network effects, IV2 is the instrument based on network effects according to the colony lived in and IV3 is the basic Bartik instrument. All regressions contain three regional dummies: a dummy containing the NUTS 3 regions Centre and North, a dummy comprising Alentejo, Algarve, and Lisbon, and a dummy for the islands Azores and Madeira. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table B12: Labor market effects of repatriates on male natives - without NUTS 2 FE

Outcomes for male natives	OLS		IV		
	(1)	(2)	(3)	(4)	(5)
Δ LFP	0.228 (0.366)	0.266 (0.335)	-0.590 (0.394)	-0.583 (0.405)	-0.534 (0.380)
Δ Unemployment rate	-0.186 (0.137)	-0.176 (0.217)	0.309 (0.161)	0.311 (0.160)	0.299 (0.157)
Δ Employment share	0.397 (0.399)	0.401 (0.465)	-0.874* (0.437)	-0.870 (0.448)	-0.811 (0.422)
Δ Share Employee	-0.490 (0.936)	-0.891 (1.046)	-3.484*** (0.967)	-3.474*** (0.977)	-3.348*** (0.927)
Δ Share Entrepreneur	0.318 (0.573)	0.712 (0.755)	1.969* (0.871)	1.961* (0.871)	1.903* (0.842)
Δ Share Employer	-0.396 (0.204)	-0.561* (0.222)	-0.859*** (0.260)	-0.857*** (0.259)	-0.825*** (0.243)
Δ Share Self-employed	0.714 (0.566)	1.273 (0.846)	2.829** (0.905)	2.818** (0.906)	2.728** (0.867)
Controls	NO	YES	YES	YES	YES
Instrument	-	-	IV1	IV2	IV3
First-stage coefficient	-	-	0.371	0.355	0.274
First-stage F-statistic	-	-	13.00	12.29	16.19
Observations	30	30	30	30	30

Notes: Robust standard errors in parentheses. The independent variable is the sample of repatriates over the native population above 15. All outcomes refer to changes between 1960 and 1981. LFP, Employment share, Share Employee, Share Entrepreneurship, Share Employer and Share Self-employed are calculated as shares over the native working age population. The unemployment rate refers to the share of unemployed over the native labor force. Controls contain the following parameters in 1960: unemployed and entrepreneurs as share of working age population, inactive and population below 15 as share of total population, those with higher education as share of those above 15. IV1 is the instrument based on educational network effects, IV2 is the instrument based on network effects according to the colony lived in and IV3 is the basic Bartik instrument. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table B13: Labor market effects of repatriates on female natives - without NUTS 2 FE

Outcomes for female natives	OLS		IV		
	(1)	(2)	(3)	(4)	(5)
Δ LFP	-0.512 (0.581)	-0.535 (0.946)	-1.421 (1.475)	-1.393 (1.471)	-1.386 (1.445)
Δ Unemployment rate	-1.426 (0.733)	-1.322 (1.008)	0.462 (0.594)	0.473 (0.582)	0.419 (0.585)
Δ Employment share	0.0868 (0.596)	0.00810 (0.994)	-1.432 (1.275)	-1.411 (1.268)	-1.381 (1.250)
Δ Share Employee	-0.771 (0.558)	-0.372 (0.705)	-1.848* (0.779)	-1.836* (0.780)	-1.776* (0.758)
Δ Share Entrepreneur	0.604 (0.500)	0.140 (0.403)	0.218 (0.681)	0.222 (0.681)	0.194 (0.662)
Δ Share Employer	-0.014 (0.031)	-0.034 (0.033)	-0.091* (0.036)	-0.091* (0.036)	-0.087* (0.036)
Δ Share Self-employed	0.618 (0.514)	0.173 (0.387)	0.309 (0.667)	0.313 (0.668)	0.281 (0.647)
Controls	NO	YES	YES	YES	YES
Instrument	-	-	IV1	IV2	IV3
First-stage coefficient	-	-	0.371	0.355	0.274
First-stage F-statistic	-	-	13.00	12.29	16.19
Observations	30	30	30	30	30

Notes: Robust standard errors in parentheses. The independent variable is the sample of repatriates over the native population above 15. All outcomes refer to changes between 1960 and 1981. LFP, Employment share, Share Employee, Share Entrepreneurship, Share Employer and Share Self-employed are calculated as shares over the native working age population. The unemployment rate refers to the share of unemployed over the native labor force. Controls contain the following parameters in 1960: unemployed and entrepreneurs as share of working age population, inactive and population below 15 as share of total population, those with higher education as share of those above 15. IV1 is the instrument based on educational network effects, IV2 is the instrument based on network effects according to the colony lived in and IV3 is the basic Bartik instrument. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table B14: Labor market effects of repatriates on male natives - sample robustness

Outcomes for male natives	IV 1 - based on educational network effect				
	(1)	(2)	(3)	(4)	(5)
Δ LFP	-0.109 (0.298)	-0.097 (0.265)	-0.133 (0.364)	-0.201 (0.551)	-0.113 (0.309)
Δ Unemployment rate	0.217 (0.121)	0.194 (0.107)	0.229 (0.146)	0.401 (0.226)	0.226 (0.123)
Δ Employment share	-0.333 (0.323)	-0.296 (0.289)	-0.135 (0.417)	-0.615 (0.604)	-0.346 (0.333)
Δ Share Employee	-3.974*** (0.912)	-3.541*** (0.824)	-4.509*** (1.049)	-7.345*** (1.791)	-4.135*** (0.902)
Δ Share Entrepreneur	3.128*** (0.773)	2.787*** (0.696)	3.736*** (0.899)	5.781*** (1.506)	3.255*** (0.767)
Δ Share Employer	-0.687*** (0.160)	-0.612*** (0.142)	-0.854*** (0.189)	-1.270*** (0.304)	-0.715*** (0.167)
Δ Share Self-employed	3.815*** (0.858)	3.400*** (0.773)	4.590*** (0.979)	7.051*** (1.680)	3.970*** (0.854)
Controls	YES	YES	YES	YES	YES
N of repatriates in sample	310,199	351,427	258,148	219,117	245,619
First-stage coefficient	0.520	0.583	0.385	0.281	0.499
First-stage F-statistic	56.73	51.69	76.61	40.29	60.48
Observations	30	30	30	30	30

Notes: Robust standard error in parentheses. The independent variable is the sample of repatriates over the native population above 15. All outcomes refer to changes between 1960 and 1981. LFP, Employment share, Share Employee, Share Entrepreneurship, Share Employer and Share Self-employed are calculated as shares over the native working age population. The unemployment rate refers to the share of unemployed over the native labor force. All regression contain dummies for NUTS 2 regions. Controls contain the following parameters in 1960: unemployed and entrepreneurs as share of working age population, inactive and population below 15 as share of total population, those with higher education as share of those above 15. IV1 is the instrument based on educational network effects, IV2 is the instrument based on network effects according to the colony lived in and IV3 is the basic Bartik instrument. (1) excludes students from the sample of repatriates, (2) uses an age range of 15-64 years, (3) uses as age range 25-59 years (4) excludes all inactive repatriates, (5) includes only Portuguese-born repatriates. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table B15: Labor market effects of repatriates on female natives - sample robustness

Outcomes for male natives	IV 1 - based on educational network effect				
	(1)	(2)	(3)	(4)	(5)
Δ LFP	-1.900** (0.702)	-1.693** (0.625)	-2.326** (0.853)	-3.512** (1.350)	-1.977** (0.722)
Δ Unemployment rate	0.070 (0.527)	0.062 (0.470)	0.158 (0.624)	0.129 (0.975)	0.073 (0.548)
Δ Employment share	-1.612* (0.671)	-1.437* (0.598)	-1.949* (0.807)	-2.980* (1.287)	-1.678* (0.690)
Δ Share Employee	-1.950*** (0.584)	-1.737*** (0.519)	-2.303** (0.710)	3.603** (1.127)	-2.029*** (0.594)
Δ Share Entrepreneur	0.141 (0.466)	0.126 (0.415)	0.115 (0.552)	0.261 (0.859)	0.147 (0.485)
Δ Share Employer	-0.073* (0.035)	-0.065* (0.031)	-0.0832 (0.0425)	-0.135* (0.064)	-0.076* (0.036)
Δ Share Self-employed	0.214 (0.455)	0.191 (0.405)	0.198 (0.539)	0.396 (0.839)	0.223 (0.473)
Controls	YES	YES	YES	YES	YES
N of repatriates in sample	310,199	351,427	258,148	219,117	245,619
First-stage coefficient	0.520	0.583	0.385	0.281	0.499
First-stage F-statistic	56.73	51.69	76.61	40.29	60.48
Observations	30	30	30	30	30

Notes: Robust standard error in parentheses. The independent variable is the sample of repatriates over the native population above 15. All outcomes refer to changes between 1960 and 1981. LFP, Employment share, Share Employee, Share Entrepreneurship, Share Employer and Share Self-employed are calculated as shares over the native working age population. The unemployment rate refers to the share of unemployed over the native labor force. All regression contain dummies for NUTS 2 regions. Controls contain the following parameters in 1960: unemployed and entrepreneurs as share of working age population, inactive and population below 15 as share of total population, those with higher education as share of those above 15. IV1 is the instrument based on educational network effects, IV2 is the instrument based on network effects according to the colony lived in and IV3 is the basic Bartik instrument. (1) excludes students from the sample of repatriates, (2) uses an age range of 15-64 years, (3) uses as age range 25-59 years (4) excludes all inactive repatriates, (5) includes only Portuguese-born repatriates. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table B16: Labour market effects of repatriates on male natives - pre-shock denominator

Outcomes for male natives	OLS		IV		
	(1)	(2)	(3)	(4)	(5)
Δ LFP	0.297 (0.248)	0.839 (0.449)	0.169 (0.577)	0.163 (0.587)	0.197 (0.562)
Δ Unemployment rate	-0.150 (0.0766)	-0.215 (0.152)	0.350 (0.242)	0.354 (0.241)	0.334 (0.232)
Δ Employment share	0.424 (0.283)	0.994 (0.510)	-0.206 (0.652)	-0.216 (0.667)	-0.163 (0.635)
Δ Share Employee	0.144 (0.605)	-0.0663 (1.149)	-6.875** (2.501)	-6.835** (2.555)	-6.459** (2.329)
Δ Share Entrepreneur	-0.148 (0.332)	0.427 (0.877)	5.697** (2.085)	5.643** (2.103)	5.341** (1.924)
Δ Share Employer	-0.0709 (0.173)	-0.542* (0.214)	-1.302*** (0.367)	-1.313*** (0.371)	-1.280*** (0.341)
Δ Share Self-employed	-0.0770 (0.281)	0.969 (1.002)	6.999** (2.353)	6.956** (2.376)	6.622** (2.170)
Controls	NO	YES	YES	YES	YES
Instrument	-	-	IV1	IV2	IV3
First-stage coefficient	-	-	0.528	0.510	0.375
First-stage F-statistic	-	-	55.05	50.94	63.64
Observations	30	30	30	30	30

Notes: Robust standard errors in parentheses. The independent variable is the sample of repatriates over the native population above 15. All outcomes refer to changes between 1960 and 1981. LFP, Employment share, Share Employee, Share Entrepreneurship, Share Employer and Share Self-employed are calculated as shares over the native working age population. The unemployment rate refers to the share of unemployed over the native labour force. All regression contain dummies for NUTS 2 regions. Controls contain the following parameters in 1960: unemployed and entrepreneurs as share of working age population, inactive and population below 15 as share of total population, those with higher education as share of those above 15. IV1 is the instrument based on educational network effects, IV2 is the instrument based on network effects according to the colony lived in and IV3 is the basic Bartik instrument.

* $p < 0.05$,

** $p < 0.01$, *** $p < 0.001$.

Table B17: Labour market effects of repatriates on female natives - pre-shock denominator

Outcomes for female natives	OLS		IV		
	(1)	(2)	(3)	(4)	(5)
Δ LFP	-0.131 (0.301)	-0.354 (0.751)	-3.427* (1.661)	-3.373* (1.673)	-3.270* (1.597)
Δ Unemployment rate	-0.904 (0.585)	-0.772 (0.721)	0.240 (0.959)	0.248 (0.942)	0.256 (0.911)
Δ Employment share	0.145 (0.371)	-0.0607 (0.727)	-2.972 (1.524)	-2.932 (1.526)	-2.844 (1.455)
Δ Share Employee	-0.0270 (0.293)	-0.637 (0.674)	-3.511* (1.474)	-3.468* (1.497)	-3.353* (1.412)
Δ Share Entrepreneur	-0.055 (0.345)	0.329 (0.505)	0.176 (0.842)	0.168 (0.840)	0.153 (0.806)
Δ Share Employer	0.017 (0.019)	-0.026 (0.038)	-0.127 (0.070)	-0.126 (0.070)	-0.121 (0.068)
Δ Share Self-employed	-0.071 (0.358)	0.354 (0.498)	0.303 (0.824)	0.294 (0.824)	0.274 (0.789)
Controls	NO	YES	YES	YES	YES
Instrument	-	-	IV1	IV2	IV3
First-stage coefficient	-	-	0.528	0.510	0.375
First-stage F-statistic	-	-	55.05	50.94	63.64
Observations	30	30	30	30	30

Notes: See notes in table B16. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table B18: Labor market effects of male repatriates on male natives - baseline

Outcomes for male natives	OLS		IV		
	(1)	(2)	(3)	(4)	(5)
Δ LFP	0.248 (0.337)	0.472 (0.429)	-0.218 (0.291)	-0.223 (0.306)	-0.183 (0.287)
Δ Unemployment rate	-0.181 (0.122)	-0.057 (0.160)	0.230 (0.120)	0.233 (0.120)	0.224 (0.118)
Δ Employment share	0.416 (0.369)	0.490 (0.494)	-0.449 (0.323)	-0.457 (0.341)	-0.410 (0.318)
Δ Share Employee	-0.228 (0.857)	-1.552 (1.016)	-3.915*** (1.009)	-3.914*** (1.039)	-3.805*** (0.979)
Δ Share Entrepreneur	0.158 (0.522)	1.458 (0.761)	3.009*** (0.847)	2.995*** (0.859)	2.932*** (0.822)
Δ Share Employer	-0.361* (0.175)	-0.535** (0.153)	-0.629*** (0.179)	-0.637*** (0.182)	-0.642*** (0.175)
Δ Share Self-employed	0.519 (0.512)	1.993* (0.817)	3.638*** (0.967)	3.633*** (0.981)	3.574*** (0.940)
Controls	NO	YES	YES	YES	YES
Instrument	-	-	IV1	IV2	IV3
First-stage coefficient	-	-	0.745	0.716	0.300
First-stage F-statistic	-	-	9.99	9.54	12.34
Observations	30	30	30	30	30

Notes: Robust standard errors in parentheses. The independent variable is the sample of male repatriates over the native male population above 15. All outcomes refer to changes between 1960 and 1981. LFP, Employment share, Share Employee, Share Entrepreneurship, Share Employer and Share Self-employed are calculated as shares over the native working age population. The unemployment rate refers to the share of unemployed over the native labor force. All regression contain dummies for NUTS 2 regions. Controls contain the following parameters in 1960: unemployed and entrepreneurs as share of working age population, inactive and population below 15 as share of total population, those with higher education as share of those above 15. IV1 is the instrument based on educational network effects, IV2 is the instrument based on network effects according to the colony lived in and IV3 is the basic Bartik instrument.

* $p < 0.05$,

** $p < 0.01$, *** $p < 0.001$.

Table B19: Labor market effects of female repatriates on female natives - baseline

Outcomes for female natives	OLS		IV		
	(1)	(2)	(3)	(4)	(5)
Δ LFP	-0.618 (0.637)	-1.309 (0.944)	-1.972** (0.711)	-1.944** (0.720)	-1.916** (0.723)
Δ Unemployment rate	-1.420 (0.789)	-0.650 (1.032)	0.106 (0.551)	0.104 (0.546)	0.126 (0.532)
Δ Employment share	-0.006 (0.648)	-0.863 (0.974)	-1.680* (0.676)	-1.657* (0.680)	-1.642* (0.676)
Δ Share Employee	-0.848 (0.616)	-1.220 (0.766)	-1.912** (0.583)	-1.896** (0.596)	-1.880** (0.595)
Δ Share Entrepreneur	0.549 (0.547)	0.0809 (0.530)	0.0140 (0.444)	0.0169 (0.446)	0.0174 (0.443)
Δ Share Employer	-0.0122 (0.0352)	-0.0593 (0.0410)	-0.0746* (0.0352)	-0.0745* (0.0354)	-0.0733* (0.0356)
Δ Share Self-employed	0.561 (0.564)	0.140 (0.522)	0.0886 (0.434)	0.0914 (0.437)	0.0906 (0.433)
Controls	NO	YES	YES	YES	YES
Instrument	-	-	IV1	IV2	IV3
First-stage coefficient	-	-	1.01	0.720	0.253
First-stage F-statistic	-	-	92.34	17.94	23.56
Observations	30	30	30	30	30

Notes: Robust standard errors in parentheses. The independent variable is the sample of female repatriates over the native female population above 15. All outcomes refer to changes between 1960 and 1981. LFP, Employment share, Share Employee, Share Entrepreneurship, Share Employer and Share Self-employed are calculated as shares over the native working age population. The unemployment rate refers to the share of unemployed over the native labor force. All regression contain dummies for NUTS 2 regions. Controls contain the following parameters in 1960: unemployed and entrepreneurs as share of working age population, inactive and population below 15 as share of total population, those with higher education as share of those above 15. IV1 is the instrument based on educational network effects, IV2 is the instrument based on network effects according to the colony lived in and IV3 is the basic Bartik instrument.

* $p < 0.05$,

** $p < 0.01$, *** $p < 0.001$.

Appendix C

Consumers

Figure C1: Products in Sample: *All Product Groups*

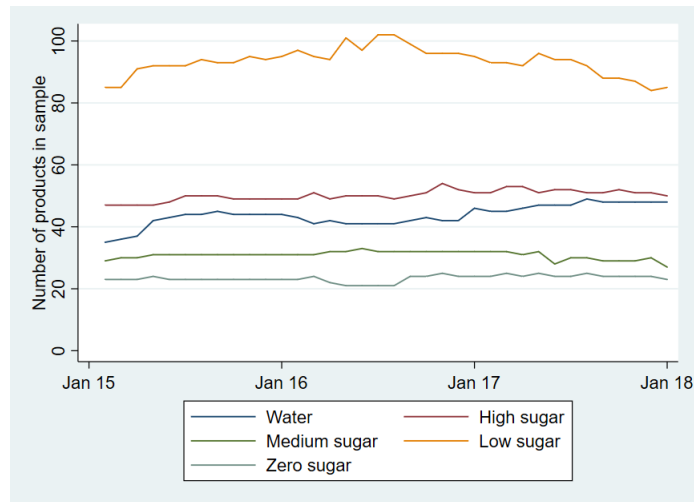
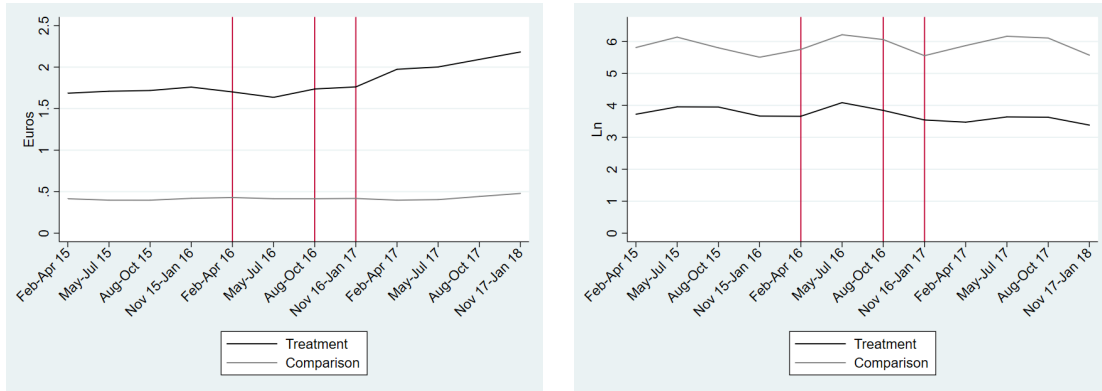
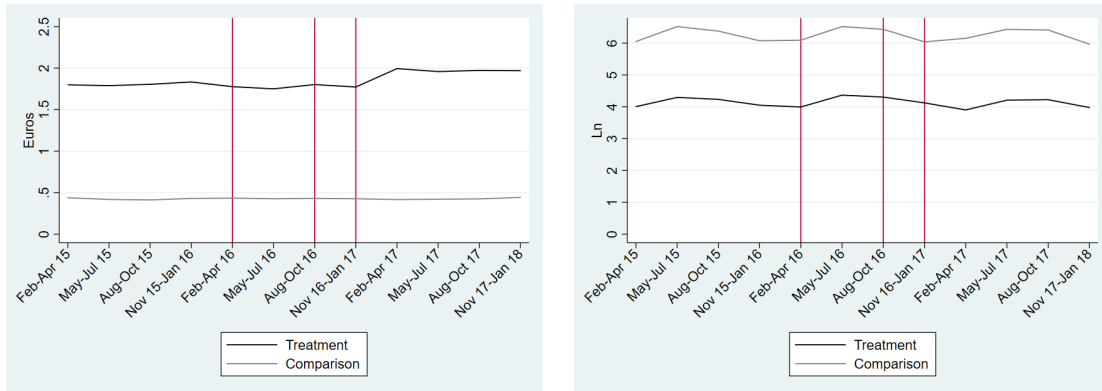


Figure C2: Trends in Prices and Quantities: *High Sugar*

A. Unbalanced Panel



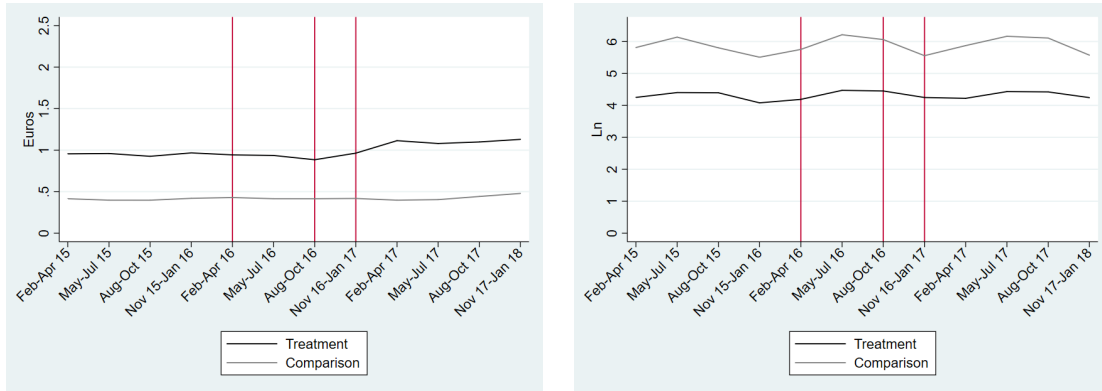
B. Balanced Panel



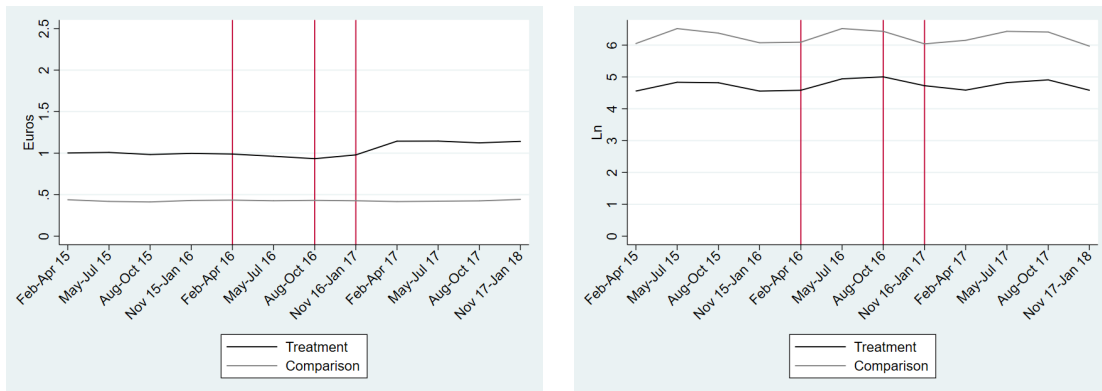
Notes: Average price (in euros) and ln(quantity of liters sold).

Figure C3: Trends in Prices and Quantities: *Medium Sugar*

A. Unbalanced Panel



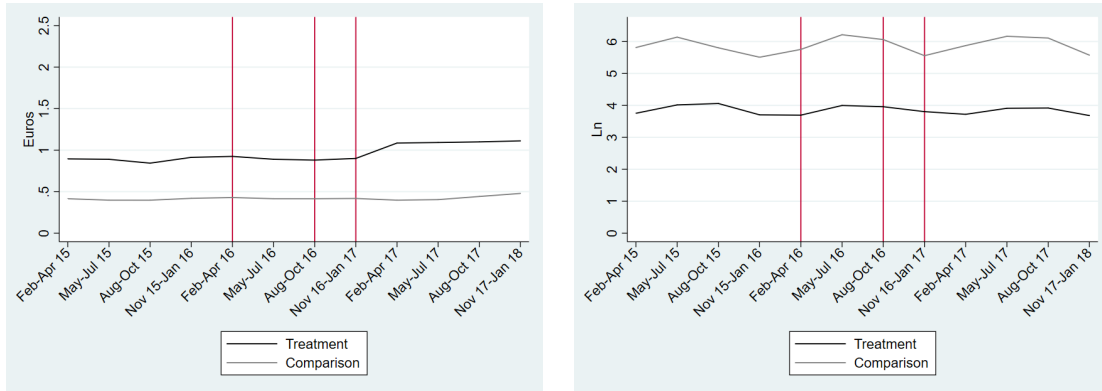
B. Balanced Panel



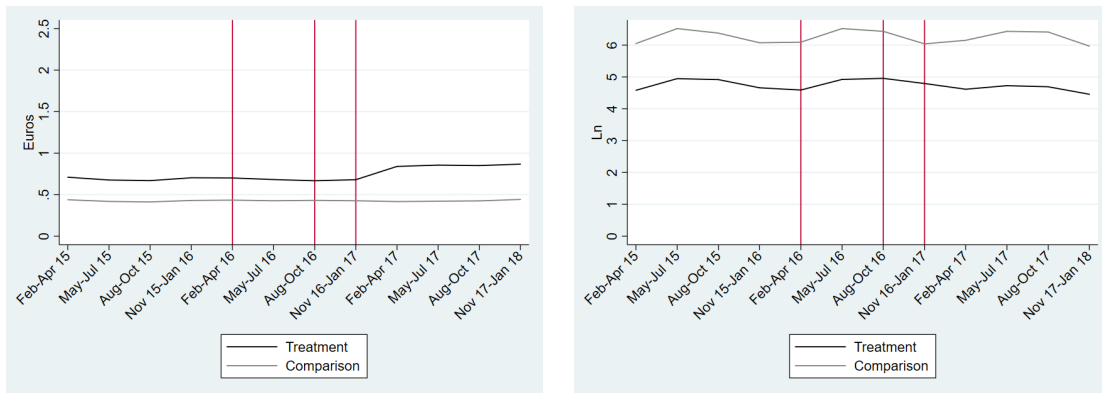
Notes: Average price (in euros) and ln(quantity of liters sold).

Figure C4: Trends in Prices and Quantities: *Low Sugar*

A. Unbalanced Panel



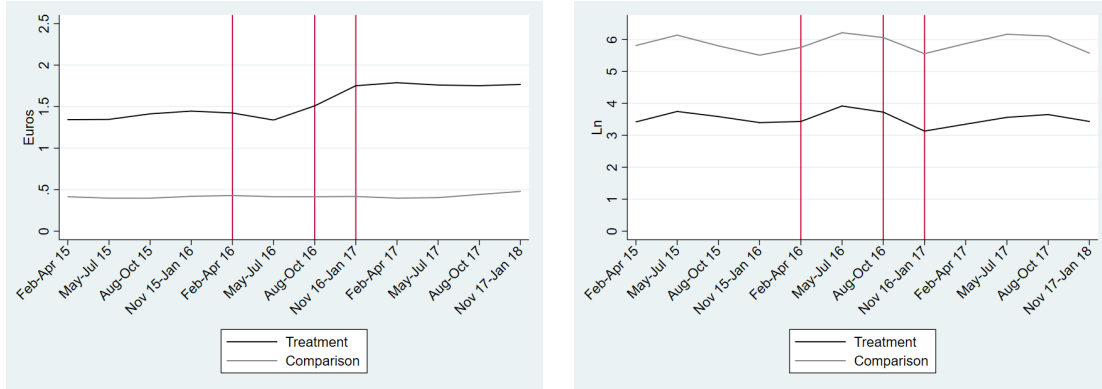
B. Balanced Panel



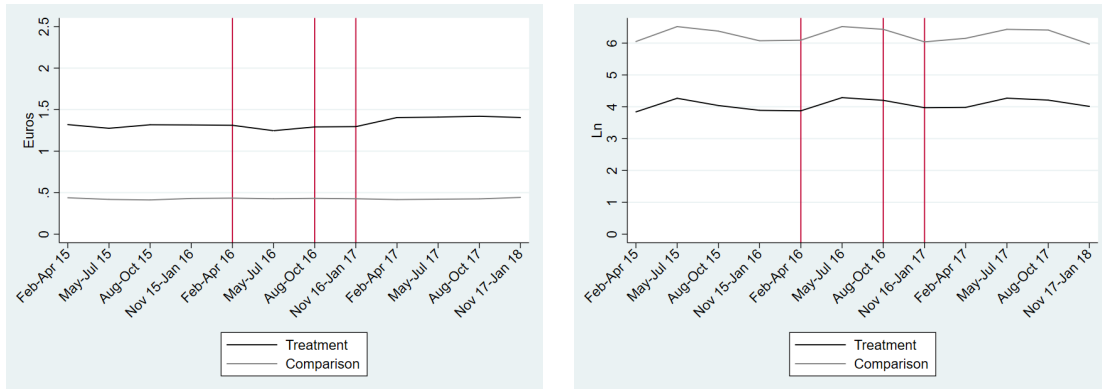
Notes: Average price (in euros) and ln(quantity of liters sold).

Figure C5: Trends in Prices and Quantities: *Zero Sugar*

A. Unbalanced Panel



B. Balanced Panel



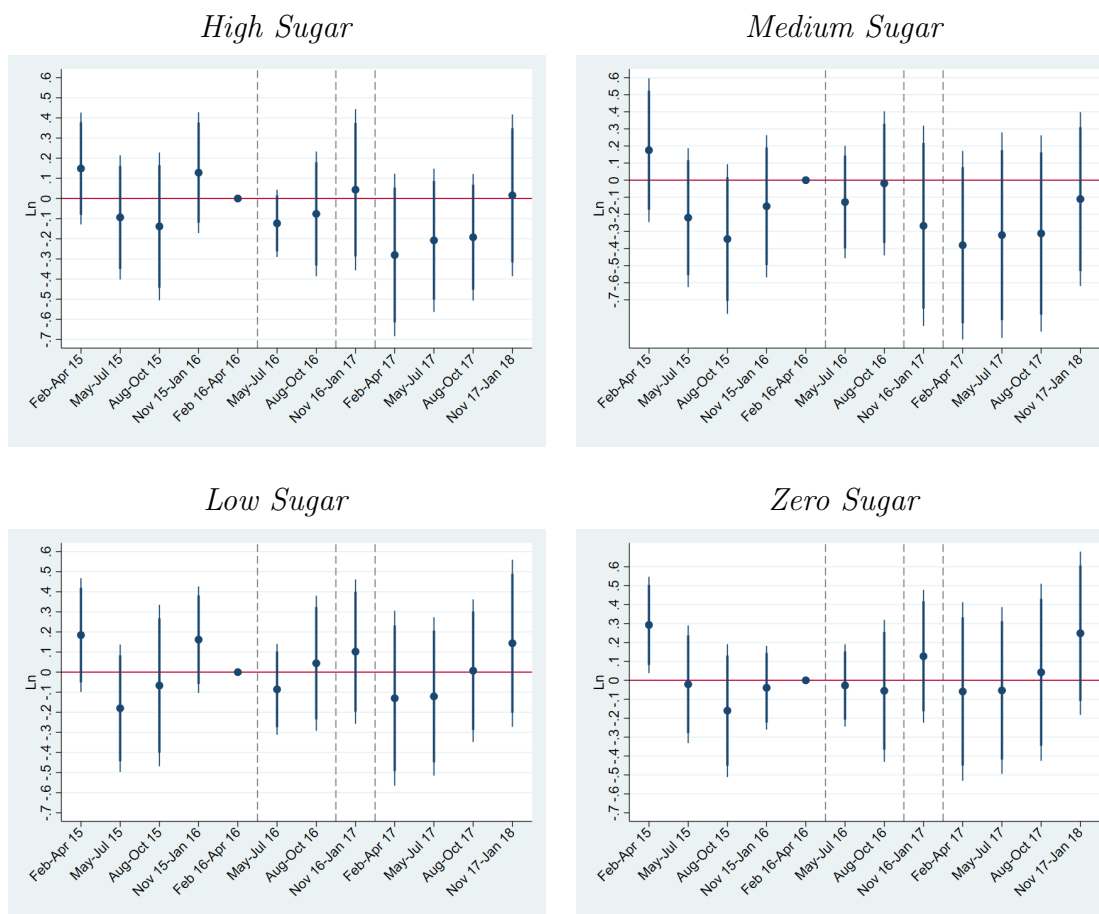
Notes: Average price (in euros) and ln(quantity of liters sold).

Table C1: **Difference-in-differences Results: Prices in ln**

	ln(Price)			
	<i>HS</i>	<i>MS</i>	<i>LS</i>	<i>ZS</i>
<u>A. Unbalanced Panel</u>				
<i>UnderDiscussion</i> × <i>PG</i>	-0.034*** (0.011)	-0.035* (0.017)	-0.019 (0.012)	-0.052*** (0.011)
<i>UnderApproval</i> × <i>PG</i>	0.008 (0.019)	0.009 (0.032)	-0.018 (0.014)	0.014 (0.015)
<i>Implementation</i> × <i>PG</i>	0.161*** (0.053)	0.162*** (0.032)	0.190*** (0.024)	0.082* (0.044)
<i>N</i>	647966	567784	1107415	499099
adj. <i>R</i> ²	0.985	0.973	0.966	0.987
<u>B. Balanced Panel</u>				
<i>UnderDiscussion</i> × <i>PG</i>	-0.032** (0.012)	-0.062*** (0.015)	-0.033* (0.017)	-0.050*** (0.007)
<i>UnderApproval</i> × <i>PG</i>	0.015 (0.021)	-0.015 (0.023)	-0.021 (0.017)	0.017 (0.018)
<i>Implementation</i> × <i>PG</i>	0.213*** (0.072)	0.149*** (0.033)	0.226*** (0.025)	0.133* (0.073)
<i>N</i>	301075	317611	510061	224960
adj. <i>R</i> ²	0.988	0.981	0.965	0.986
Quarter FE	✓	✓	✓	✓
Product-store FE	✓	✓	✓	✓
Month-region FE	✓	✓	✓	✓

Notes: Standard errors in parentheses are clustered by brand. Stars indicate significance levels of 10% (*), 5% (**), and 1%(***). *UnderDiscussion*: May-October 2016; *UnderApproval*: November 2016-January 2017; *Implementation*: February 2017-January 2018. *PG* stands for the following product groups: *HS*(*HighSugar*), *MS*(*MediumSugar*), *LS*(*LowSugar*), and *ZS*(*ZeroSugar*).

Figure C6: Event Study Results for Ln(Quantity of Liters Sold): Aggregation to the Brand Level



Note: Standard errors are clustered by brand.

Table C2: Difference-in-differences Placebo Results

	Price (in euros)				ln(Quantity of liters sold)			
	<i>HS</i>	<i>MS</i>	<i>LS</i>	<i>ZS</i>	<i>HS</i>	<i>MS</i>	<i>LS</i>	<i>ZS</i>
	<u>A. Unbalanced Panel</u>							
<i>Fev</i> – <i>Apr</i> 2016 × <i>PG</i>	-0.026 (0.023)	-0.026 (0.018)	0.025 (0.041)	-0.005 (0.014)	-0.004 (0.071)	0.135 (0.088)	-0.067 (0.111)	0.044 (0.065)
<i>N</i>	253013	222241	452699	198364	253013	222241	452699	198364
adj. <i>R</i> ²	0.982	0.958	0.956	0.986	0.907	0.861	0.878	0.913
	<u>B. Balanced Panel</u>							
<i>Fev</i> – <i>Apr</i> 2016 × <i>PG</i>	-0.030 (0.027)	-0.019 (0.018)	-0.004 (0.021)	-0.006 (0.014)	0.019 (0.053)	0.065 (0.080)	-0.020 (0.080)	0.007 (0.047)
<i>N</i>	177911	160980	282091	137346	177911	160980	282091	137346
adj. <i>R</i> ²	0.984	0.962	0.946	0.987	0.942	0.913	0.908	0.952
Quarter FE	✓	✓	✓	✓	✓	✓	✓	✓
Product-store FE	✓	✓	✓	✓	✓	✓	✓	✓
Month-region FE	✓	✓	✓	✓	✓	✓	✓	✓

Notes: Standard errors in parentheses are clustered by brand. Stars indicate significance levels of 10% (*), 5% (**), and 1%(***). *UnderDiscussion*: May-October 2016; *UnderApproval*: November 2016-January 2017; *Implementation*: February 2017-January 2018. *PG* stands for the following product groups: *HS*(*HighSugar*), *MS*(*MediumSugar*), *LS*(*LowSugar*), and *ZS*(*ZeroSugar*).

Table C3: Difference-in-differences Different Comparison Group

	Price (in euros)		ln(Quantity of liters sold)	
	Baseline	With Sparkling Water	Baseline	With Sparkling Water
A. Unbalanced Panel				
<i>UnderDiscussion</i> × <i>HS</i>	-0.040** (0.015)	-0.027 (0.016)	-0.084 (0.051)	-0.094* (0.051)
<i>UnderApproval</i> × <i>HS</i>	-0.016 (0.048)	-0.019 (0.048)	-0.005 (0.093)	-0.036 (0.084)
<i>Implementation</i> × <i>HS</i>	0.160*** (0.034)	0.148*** (0.035)	-0.065 (0.080)	-0.060 (0.068)
<i>N</i>	647966	770332	647966	770332
adj. <i>R</i> ²	0.980	0.977	0.904	0.898
<i>UnderDiscussion</i> × <i>MS</i>	-0.031** (0.014)	-0.019 (0.016)	-0.026 (0.107)	-0.035 (0.106)
<i>UnderApproval</i> × <i>MS</i>	0.003 (0.025)	0.000 (0.026)	0.041 (0.184)	0.010 (0.179)
<i>Implementation</i> × <i>MS</i>	0.152*** (0.021)	0.141*** (0.022)	0.003 (0.113)	0.008 (0.104)
<i>N</i>	567786	690152	567786	690152
adj. <i>R</i> ²	0.963	0.952	0.855	0.859
<i>UnderDiscussion</i> × <i>LS</i>	-0.016** (0.006)	-0.004 (0.010)	-0.150** (0.066)	-0.158** (0.066)
<i>UnderApproval</i> × <i>LS</i>	-0.011 (0.011)	-0.015 (0.014)	0.085 (0.089)	0.054 (0.080)
<i>Implementation</i> × <i>LS</i>	0.154*** (0.025)	0.142*** (0.026)	-0.185** (0.077)	-0.179*** (0.066)
<i>N</i>	1107429	1229795	1107429	1229795
adj. <i>R</i> ²	0.957	0.952	0.879	0.876
<i>UnderDiscussion</i> × <i>ZS</i>	-0.042*** (0.010)	-0.029** (0.012)	-0.041 (0.048)	-0.051 (0.048)
<i>UnderApproval</i> × <i>ZS</i>	-0.022 (0.055)	-0.025 (0.056)	-0.040 (0.127)	-0.071 (0.121)
<i>Implementation</i> × <i>ZS</i>	0.093*** (0.020)	0.082*** (0.021)	0.027 (0.091)	0.031 (0.082)
<i>N</i>	499099	621465	499099	621465
adj. <i>R</i> ²	0.984	0.977	0.906	0.899
B. Balanced Panel				
<i>UnderDiscussion</i> × <i>HS</i>	-0.027* (0.015)	-0.018 (0.017)	-0.034 (0.059)	-0.047 (0.059)
<i>UnderApproval</i> × <i>HS</i>	-0.029 (0.057)	-0.040 (0.058)	0.189*** (0.054)	0.154*** (0.052)
<i>Implementation</i> × <i>HS</i>	0.173*** (0.052)	0.158*** (0.052)	-0.056 (0.090)	-0.076 (0.084)
<i>N</i>	301075	376191	301075	376191
adj. <i>R</i> ²	0.983	0.980	0.940	0.931
<i>UnderDiscussion</i> × <i>MS</i>	-0.050*** (0.015)	-0.041** (0.017)	0.049 (0.098)	0.036 (0.097)
<i>UnderApproval</i> × <i>MS</i>	-0.017 (0.017)	-0.029 (0.019)	0.239*** (0.079)	0.203** (0.077)
<i>Implementation</i> × <i>MS</i>	0.143*** (0.018)	0.129*** (0.019)	0.038 (0.086)	0.017 (0.079)
<i>N</i>	317611	392727	317611	392727
adj. <i>R</i> ²	0.971	0.959	0.913	0.908
<i>UnderDiscussion</i> × <i>LS</i>	-0.019** (0.007)	-0.010 (0.011)	-0.052 (0.058)	-0.066 (0.058)
<i>UnderApproval</i> × <i>LS</i>	-0.011 (0.012)	-0.023 (0.015)	0.237* (0.119)	0.202* (0.118)
<i>Implementation</i> × <i>LS</i>	0.162*** (0.025)	0.147*** (0.026)	-0.134* (0.065)	-0.155** (0.057)
<i>N</i>	510064	585180	510064	585180
adj. <i>R</i> ²	0.932	0.936	0.908	0.904
<i>UnderDiscussion</i> × <i>ZS</i>	-0.041*** (0.010)	-0.032** (0.013)	0.009 (0.097)	-0.004 (0.097)
<i>UnderApproval</i> × <i>ZS</i>	-0.013 (0.046)	-0.024 (0.047)	0.171** (0.066)	0.135** (0.064)
<i>Implementation</i> × <i>ZS</i>	0.102** (0.042)	0.088* (0.043)	0.119 (0.155)	0.098 (0.151)
<i>N</i>	224960	300076	224960	300076
adj. <i>R</i> ²	0.985	0.974	0.939	0.930
Quarter FE	✓	✓	✓	✓
Product-store FE	✓	✓	✓	✓
Month-region FE	✓	✓	✓	✓

Notes: Standard errors in parentheses are clustered by brand. Stars indicate significance levels of 10% (*), 5% (**), and 1%***). *UnderDiscussion*: May-October 2016; *UnderApproval*: November 2016-January 2017; *Implementation*: February 2017-January 2018. *PG* stands for the following product groups: *HS*(HighSugar), *MS*(MediumSugar), *LS*(LowSugar), and *ZS*(ZeroSugar).

Table C4: Difference-in-differences Robustness Results

	Price (in euros)			ln(Quantity of liters sold)		
	Baseline	No Own Brand	No Border	Baseline	No Own Brand	No Border
A. Unbalanced Panel						
<i>UnderDiscussion</i> × <i>HS</i>	-0.040** (0.015)	-0.046** (0.017)	-0.040** (0.014)	-0.085 (0.051)	-0.080 (0.059)	-0.083 (0.051)
<i>UnderApproval</i> × <i>HS</i>	-0.016 (0.048)	-0.025 (0.059)	-0.016 (0.048)	-0.005 (0.093)	-0.035 (0.114)	-0.005 (0.093)
<i>Implementation</i> × <i>HS</i>	0.160*** (0.034)	0.146*** (0.041)	0.160*** (0.034)	-0.065 (0.080)	-0.060 (0.084)	-0.063 (0.080)
<i>N</i>	647412	532198	633089	647412	532198	633089
adj. <i>R</i> ²	0.980	0.978	0.980	0.904	0.879	0.904
<i>UnderDiscussion</i> × <i>MS</i>	-0.031** (0.014)	-0.033* (0.016)	-0.032** (0.014)	-0.026 (0.107)	-0.051 (0.118)	-0.024 (0.107)
<i>UnderApproval</i> × <i>MS</i>	0.003 (0.025)	0.000 (0.028)	0.003 (0.025)	0.041 (0.184)	0.034 (0.212)	0.042 (0.184)
<i>Implementation</i> × <i>MS</i>	0.152*** (0.021)	0.154*** (0.024)	0.152*** (0.021)	0.003 (0.113)	-0.043 (0.129)	0.004 (0.113)
<i>N</i>	567289	480691	554839	567289	480691	554839
adj. <i>R</i> ²	0.963	0.958	0.963	0.854	0.801	0.855
<i>UnderDiscussion</i> × <i>LS</i>	-0.016** (0.006)	-0.012 (0.007)	-0.016** (0.006)	-0.150** (0.066)	-0.128 (0.077)	-0.149** (0.065)
<i>UnderApproval</i> × <i>LS</i>	-0.011 (0.011)	-0.011 (0.015)	-0.011 (0.011)	0.085 (0.089)	0.116 (0.108)	0.084 (0.089)
<i>Implementation</i> × <i>LS</i>	0.154*** (0.025)	0.162*** (0.031)	0.154*** (0.025)	-0.185** (0.078)	-0.198** (0.097)	-0.185** (0.077)
<i>N</i>	1106727	838023	1082533	1106727	838023	1082533
adj. <i>R</i> ²	0.957	0.953	0.957	0.879	0.830	0.879
<i>UnderDiscussion</i> × <i>ZS</i>	-0.042*** (0.010)	-0.045*** (0.010)	-0.042*** (0.010)	-0.041 (0.048)	-0.030 (0.052)	-0.040 (0.048)
<i>UnderApproval</i> × <i>ZS</i>	-0.022 (0.055)	-0.034 (0.071)	-0.022 (0.056)	-0.041 (0.127)	-0.058 (0.158)	-0.039 (0.127)
<i>Implementation</i> × <i>ZS</i>	0.093*** (0.020)	0.078*** (0.019)	0.093*** (0.020)	0.027 (0.091)	0.029 (0.101)	0.029 (0.091)
<i>N</i>	498730	412539	487810	498730	412539	487810
adj. <i>R</i> ²	0.984	0.983	0.984	0.906	0.882	0.906
B. Balanced Panel						
<i>UnderDiscussion</i> × <i>HS</i>	-0.027* (0.015)	-0.033 (0.023)	-0.027* (0.015)	-0.033 (0.059)	0.016 (0.060)	-0.032 (0.059)
<i>UnderApproval</i> × <i>HS</i>	-0.029 (0.057)	-0.054 (0.086)	-0.029 (0.057)	0.190*** (0.054)	0.229*** (0.072)	0.189*** (0.054)
<i>Implementation</i> × <i>HS</i>	0.173*** (0.052)	0.146* (0.078)	0.172*** (0.052)	-0.056 (0.090)	0.004 (0.075)	-0.055 (0.090)
<i>N</i>	300967	210932	294813	300967	210932	294813
adj. <i>R</i> ²	0.983	0.981	0.983	0.940	0.915	0.940
<i>UnderDiscussion</i> × <i>MS</i>	-0.050*** (0.015)	-0.057*** (0.017)	-0.051*** (0.015)	0.049 (0.098)	0.029 (0.116)	0.051 (0.098)
<i>UnderApproval</i> × <i>MS</i>	-0.017 (0.017)	-0.024 (0.016)	-0.017 (0.017)	0.238*** (0.079)	0.280*** (0.075)	0.239*** (0.078)
<i>Implementation</i> × <i>MS</i>	0.143*** (0.018)	0.144*** (0.022)	0.143*** (0.018)	0.038 (0.086)	-0.010 (0.099)	0.039 (0.086)
<i>N</i>	317467	253532	310808	317467	253532	310808
adj. <i>R</i> ²	0.971	0.964	0.971	0.913	0.866	0.914
<i>UnderDiscussion</i> × <i>LS</i>	-0.019** (0.007)	-0.011 (0.007)	-0.019** (0.007)	-0.052 (0.058)	-0.011 (0.067)	-0.052 (0.057)
<i>UnderApproval</i> × <i>LS</i>	-0.011 (0.012)	-0.015 (0.016)	-0.011 (0.012)	0.237* (0.119)	0.398*** (0.076)	0.235* (0.119)
<i>Implementation</i> × <i>LS</i>	0.162*** (0.025)	0.184*** (0.029)	0.162*** (0.025)	-0.134* (0.065)	-0.157* (0.085)	-0.135** (0.065)
<i>N</i>	509992	335847	498978	509992	335847	498978
adj. <i>R</i> ²	0.932	0.902	0.932	0.908	0.851	0.908
<i>UnderDiscussion</i> × <i>ZS</i>	-0.041*** (0.010)	-0.050*** (0.014)	-0.042*** (0.010)	0.009 (0.097)	0.077 (0.123)	0.009 (0.098)
<i>UnderApproval</i> × <i>ZS</i>	-0.013 (0.046)	-0.035 (0.073)	-0.013 (0.046)	0.171** (0.066)	0.253*** (0.052)	0.171** (0.066)
<i>Implementation</i> × <i>ZS</i>	0.102** (0.042)	0.064 (0.053)	0.102** (0.042)	0.119 (0.155)	0.228 (0.150)	0.120 (0.155)
<i>N</i>	224888	164518	220281	224888	164518	220281
adj. <i>R</i> ²	0.985	0.984	0.985	0.939	0.915	0.940
Quarter FE	✓	✓	✓	✓	✓	✓
Product-store FE	✓	✓	✓	✓	✓	✓
Month-region FE	✓	✓	✓	✓	✓	✓

Notes: Standard errors in parentheses are clustered by brand. Stars indicate significance levels of 10% (*), 5% (**), and 1%***). *UnderDiscussion*: May-October 2016; *UnderApproval*: November 2016-January 2017; *Implementation*: February 2017-January 2018. *PG* stands for the following product groups: *HS*(HighSugar), *MS*(MediumSugar), *LS*(LowSugar), and *ZS*(ZeroSugar).

Table C5: Difference-in-differences Location Results

	Price (in euros)			ln(Quantity of liters sold)		
	Baseline	Lis/ Op	No Lis/ Op	Baseline	Lis/ Op	No Lis/ Op
A. Unbalanced Panel						
<i>UnderDiscussion</i> × <i>HS</i>	-0.040** (0.015)	-0.038*** (0.013)	-0.042** (0.016)	-0.085 (0.051)	-0.076 (0.048)	-0.091 (0.055)
<i>UnderApproval</i> × <i>HS</i>	-0.016 (0.048)	-0.018 (0.051)	-0.015 (0.046)	-0.005 (0.093)	0.004 (0.094)	-0.011 (0.093)
<i>Implementation</i> × <i>HS</i>	0.160*** (0.034)	0.158*** (0.033)	0.161*** (0.035)	-0.065 (0.080)	-0.057 (0.083)	-0.071 (0.079)
<i>N</i>	647412	268608	378804	647412	268608	378804
adj. <i>R</i> ²	0.980	0.980	0.980	0.904	0.900	0.906
<i>UnderDiscussion</i> × <i>MS</i>	-0.031** (0.014)	-0.030** (0.014)	-0.032** (0.015)	-0.026 (0.107)	-0.040 (0.106)	-0.016 (0.108)
<i>UnderApproval</i> × <i>MS</i>	0.003 (0.025)	0.001 (0.025)	0.005 (0.025)	0.041 (0.184)	0.056 (0.173)	0.030 (0.192)
<i>Implementation</i> × <i>MS</i>	0.152*** (0.021)	0.151*** (0.020)	0.154*** (0.022)	0.003 (0.113)	0.008 (0.106)	-0.001 (0.119)
<i>N</i>	567289	236691	330598	567289	236691	330598
adj. <i>R</i> ²	0.963	0.965	0.962	0.854	0.852	0.856
<i>UnderDiscussion</i> × <i>LS</i>	-0.016** (0.006)	-0.017** (0.007)	-0.016** (0.006)	-0.150** (0.066)	-0.139** (0.062)	-0.157** (0.069)
<i>UnderApproval</i> × <i>LS</i>	-0.011 (0.011)	-0.012 (0.010)	-0.010 (0.012)	0.085 (0.089)	0.083 (0.087)	0.086 (0.092)
<i>Implementation</i> × <i>LS</i>	0.154*** (0.025)	0.154*** (0.026)	0.155*** (0.025)	-0.185** (0.078)	-0.179** (0.076)	-0.189** (0.079)
<i>N</i>	1106727	458807	647920	1106727	458807	647920
adj. <i>R</i> ²	0.957	0.955	0.958	0.879	0.878	0.880
<i>UnderDiscussion</i> × <i>ZS</i>	-0.042*** (0.010)	-0.042*** (0.010)	-0.041*** (0.010)	-0.041 (0.048)	-0.065 (0.045)	-0.024 (0.050)
<i>UnderApproval</i> × <i>ZS</i>	-0.022 (0.055)	-0.024 (0.056)	-0.021 (0.055)	-0.041 (0.127)	-0.027 (0.139)	-0.049 (0.119)
<i>Implementation</i> × <i>ZS</i>	0.093*** (0.020)	0.096*** (0.020)	0.091*** (0.021)	0.027 (0.091)	0.007 (0.093)	0.042 (0.091)
<i>N</i>	498730	205082	293648	498730	205082	293648
adj. <i>R</i> ²	0.984	0.985	0.984	0.906	0.903	0.908
B. Balanced Panel						
<i>UnderDiscussion</i> × <i>HS</i>	-0.027* (0.015)	-0.027 (0.016)	-0.028* (0.014)	-0.033 (0.059)	-0.019 (0.054)	-0.043 (0.066)
<i>UnderApproval</i> × <i>HS</i>	-0.029 (0.057)	-0.039 (0.065)	-0.021 (0.052)	0.190*** (0.054)	0.231*** (0.063)	0.158*** (0.053)
<i>Implementation</i> × <i>HS</i>	0.173*** (0.052)	0.169*** (0.053)	0.175*** (0.051)	-0.056 (0.090)	-0.032 (0.096)	-0.074 (0.087)
<i>N</i>	300967	128501	172466	300967	128501	172466
adj. <i>R</i> ²	0.983	0.984	0.983	0.940	0.943	0.938
<i>UnderDiscussion</i> × <i>MS</i>	-0.050*** (0.015)	-0.046*** (0.015)	-0.053*** (0.015)	0.049 (0.098)	0.023 (0.094)	0.069 (0.102)
<i>UnderApproval</i> × <i>MS</i>	-0.017 (0.017)	-0.016 (0.016)	-0.018 (0.018)	0.238*** (0.079)	0.244*** (0.072)	0.234** (0.085)
<i>Implementation</i> × <i>MS</i>	0.143*** (0.018)	0.141*** (0.018)	0.145*** (0.019)	0.038 (0.086)	0.054 (0.077)	0.025 (0.093)
<i>N</i>	317467	135373	182094	317467	135373	182094
adj. <i>R</i> ²	0.971	0.974	0.969	0.913	0.918	0.911
<i>UnderDiscussion</i> × <i>LS</i>	-0.019** (0.007)	-0.017** (0.008)	-0.021*** (0.007)	-0.052 (0.058)	-0.042 (0.057)	-0.060 (0.058)
<i>UnderApproval</i> × <i>LS</i>	-0.011 (0.012)	-0.009 (0.011)	-0.013 (0.012)	0.237* (0.119)	0.235* (0.116)	0.239* (0.122)
<i>Implementation</i> × <i>LS</i>	0.162*** (0.025)	0.165*** (0.026)	0.160*** (0.024)	-0.134* (0.065)	-0.118* (0.060)	-0.147** (0.070)
<i>N</i>	509992	215434	294558	509992	215434	294558
adj. <i>R</i> ²	0.932	0.926	0.935	0.908	0.911	0.907
<i>UnderDiscussion</i> × <i>ZS</i>	-0.041*** (0.010)	-0.038*** (0.009)	-0.044*** (0.011)	0.009 (0.097)	-0.003 (0.099)	0.018 (0.097)
<i>UnderApproval</i> × <i>ZS</i>	-0.013 (0.046)	-0.015 (0.051)	-0.012 (0.043)	0.171** (0.066)	0.214** (0.081)	0.139** (0.060)
<i>Implementation</i> × <i>ZS</i>	0.102** (0.042)	0.104** (0.045)	0.101** (0.041)	0.119 (0.155)	0.118 (0.165)	0.119 (0.150)
<i>N</i>	224888	95061	129827	224888	95061	129827
adj. <i>R</i> ²	0.985	0.986	0.984	0.939	0.942	0.938
Quarter FE	✓	✓	✓	✓	✓	✓
Product-store FE	✓	✓	✓	✓	✓	✓
Month-region FE	✓	✓	✓	✓	✓	✓

Notes: Standard errors in parentheses are clustered by brand. Stars indicate significance levels of 10% (*), 5% (**), and 1%***). *UnderDiscussion*: May-October 2016; *UnderApproval*: November 2016-January 2017; *Implementation*: February 2017-January 2018. *PG* stands for the following product groups: *HS*(HighSugar), *MS*(MediumSugar), *LS*(LowSugar), and *ZS*(ZeroSugar).

Table C6: Difference-in-differences Different Sizes

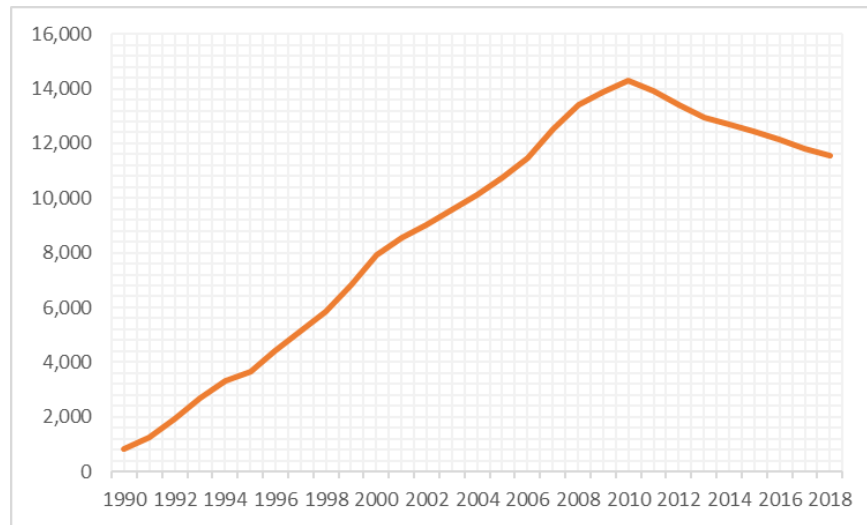
	Price (in euros)			ln(Quantity of liters sold)		
	Baseline	Less 1L	1L or More	Baseline	Less 1L	1L or More
A. Unbalanced Panel						
<i>UnderDiscussion</i> × <i>HS</i>	-0.040** (0.015)	-0.048** (0.021)	-0.034** (0.013)	-0.084 (0.051)	-0.012 (0.055)	-0.136** (0.051)
<i>UnderApproval</i> × <i>HS</i>	-0.016 (0.048)	-0.039 (0.085)	0.003 (0.016)	-0.005 (0.093)	0.030 (0.107)	-0.040 (0.117)
<i>Implementation</i> × <i>HS</i>	0.160*** (0.034)	0.153** (0.059)	0.167*** (0.021)	-0.065 (0.080)	0.003 (0.073)	-0.130 (0.102)
<i>N</i>	647966	455341	487984	647966	455341	487984
adj. <i>R</i> ²	0.980	0.981	0.965	0.904	0.919	0.878
<i>UnderDiscussion</i> × <i>MS</i>	-0.031** (0.014)	-0.048*** (0.014)	-0.025 (0.015)	-0.026 (0.107)	0.072 (0.052)	-0.071 (0.136)
<i>UnderApproval</i> × <i>MS</i>	0.003 (0.025)	-0.021 (0.018)	0.015 (0.030)	0.041 (0.184)	0.093 (0.113)	0.015 (0.222)
<i>Implementation</i> × <i>MS</i>	0.152*** (0.021)	0.179*** (0.023)	0.140*** (0.029)	0.003 (0.113)	0.084 (0.072)	-0.035 (0.145)
<i>N</i>	567786	381824	481321	567786	381824	481321
adj. <i>R</i> ²	0.963	0.983	0.921	0.855	0.898	0.838
<i>UnderDiscussion</i> × <i>LS</i>	-0.016** (0.006)	-0.049*** (0.008)	-0.005 (0.005)	-0.150** (0.066)	-0.079 (0.052)	-0.174** (0.084)
<i>UnderApproval</i> × <i>LS</i>	-0.011 (0.011)	-0.014 (0.015)	-0.010 (0.012)	0.085 (0.089)	-0.138** (0.066)	0.167 (0.102)
<i>Implementation</i> × <i>LS</i>	0.154*** (0.025)	0.145*** (0.038)	0.158*** (0.021)	-0.185** (0.077)	-0.122 (0.087)	-0.211** (0.083)
<i>N</i>	1107429	516459	886329	1107429	516459	886329
adj. <i>R</i> ²	0.957	0.972	0.893	0.879	0.920	0.864
<i>UnderDiscussion</i> × <i>ZS</i>	-0.042*** (0.010)	-0.028** (0.012)	-0.056*** (0.013)	-0.041 (0.048)	-0.030 (0.036)	-0.051 (0.076)
<i>UnderApproval</i> × <i>ZS</i>	-0.022 (0.055)	-0.053 (0.086)	0.036*** (0.012)	-0.040 (0.127)	0.086 (0.104)	-0.236 (0.190)
<i>Implementation</i> × <i>ZS</i>	0.093*** (0.020)	0.106*** (0.022)	0.076** (0.031)	0.027 (0.091)	0.079 (0.076)	-0.039 (0.170)
<i>N</i>	499099	406759	387699	499099	406759	387699
adj. <i>R</i> ²	0.984	0.986	0.959	0.906	0.918	0.871
B. Balanced Panel						
<i>UnderDiscussion</i> × <i>HS</i>	-0.027* (0.015)	-0.039* (0.019)	-0.015 (0.010)	-0.034 (0.059)	0.028 (0.056)	-0.098* (0.048)
<i>UnderApproval</i> × <i>HS</i>	-0.029 (0.057)	-0.078 (0.095)	0.023 (0.015)	0.189*** (0.054)	0.158* (0.082)	0.221*** (0.067)
<i>Implementation</i> × <i>HS</i>	0.173*** (0.052)	0.154 (0.092)	0.192*** (0.009)	-0.056 (0.090)	-0.005 (0.072)	-0.110 (0.105)
<i>N</i>	301075	234273	230683	301075	234273	230683
adj. <i>R</i> ²	0.983	0.983	0.960	0.940	0.945	0.934
<i>UnderDiscussion</i> × <i>MS</i>	-0.050*** (0.015)	-0.055*** (0.014)	-0.048** (0.017)	0.049 (0.098)	0.134** (0.051)	-0.001 (0.125)
<i>UnderApproval</i> × <i>MS</i>	-0.017 (0.017)	-0.020 (0.018)	-0.015 (0.020)	0.239*** (0.079)	0.190*** (0.037)	0.268** (0.110)
<i>Implementation</i> × <i>MS</i>	0.143*** (0.018)	0.186*** (0.028)	0.118*** (0.022)	0.038 (0.086)	0.122 (0.072)	-0.013 (0.108)
<i>N</i>	317611	221261	260231	317611	221261	260231
adj. <i>R</i> ²	0.971	0.984	0.934	0.913	0.944	0.899
<i>UnderDiscussion</i> × <i>LS</i>	-0.019** (0.007)	-0.045*** (0.003)	-0.013* (0.006)	-0.052 (0.058)	-0.078** (0.027)	-0.045 (0.066)
<i>UnderApproval</i> × <i>LS</i>	-0.011 (0.012)	0.009 (0.008)	-0.017 (0.012)	0.237* (0.119)	-0.069** (0.029)	0.319*** (0.099)
<i>Implementation</i> × <i>LS</i>	0.162*** (0.025)	0.128*** (0.008)	0.171*** (0.027)	-0.134* (0.065)	-0.138** (0.054)	-0.133* (0.072)
<i>N</i>	510064	237063	436882	510064	237063	436882
adj. <i>R</i> ²	0.932	0.984	0.904	0.908	0.946	0.904
<i>UnderDiscussion</i> × <i>ZS</i>	-0.041*** (0.010)	-0.044** (0.016)	-0.038*** (0.007)	0.009 (0.097)	0.02 (0.067)	0.018 (0.177)
<i>UnderApproval</i> × <i>ZS</i>	-0.013 (0.046)	-0.042 (0.084)	0.023** (0.010)	0.171** (0.066)	0.296*** (0.076)	0.017 (0.170)
<i>Implementation</i> × <i>ZS</i>	0.102** (0.042)	0.111** (0.043)	0.092 (0.058)	0.119 (0.155)	0.158* (0.074)	0.070 (0.346)
<i>N</i>	224960	197567	191274	224960	197567	191274
adj. <i>R</i> ²	0.985	0.986	0.965	0.939	0.945	0.932
Quarter FE	✓	✓	✓	✓	✓	✓
Product-store FE	✓	✓	✓	✓	✓	✓
Month-region FE	✓	✓	✓	✓	✓	✓

Notes: Standard errors in parentheses are clustered by brand. Stars indicate significance levels of 10% (*), 5% (**), and 1%***). *UnderDiscussion*: May-October 2016; *UnderApproval*: November 2016-January 2017; *Implementation*: February 2017-January 2018. *PG* stands for the following product groups: *HS*(HighSugar), *MS*(MediumSugar), *LS*(LowSugar), and *ZS*(ZeroSugar).

Appendix D

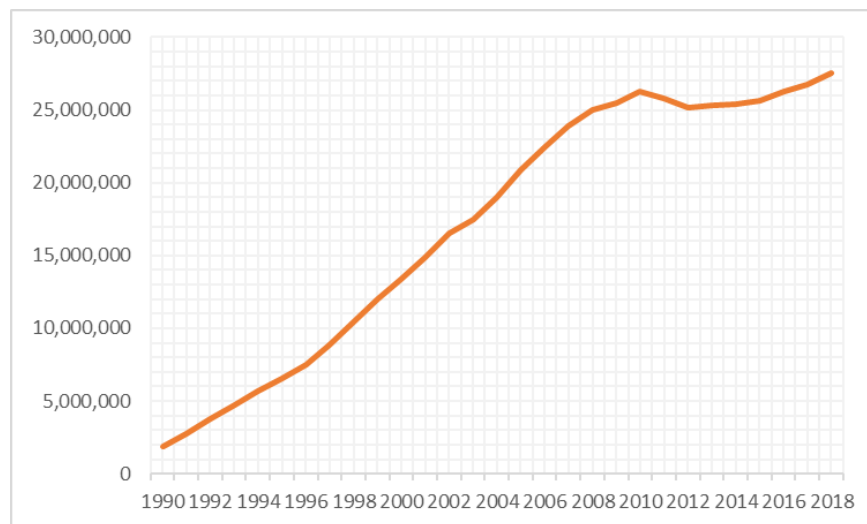
Voters

Figure D1: Number of ATM machines in Portugal



Source: INE and SIBS

Figure D2: Amount of withdrawals in ATMs Portugal (in constant euros)



Source: INE and SIBS

Figure D3: Turnout Rates (in %)
2017 Local Elections - Mainland Portugal

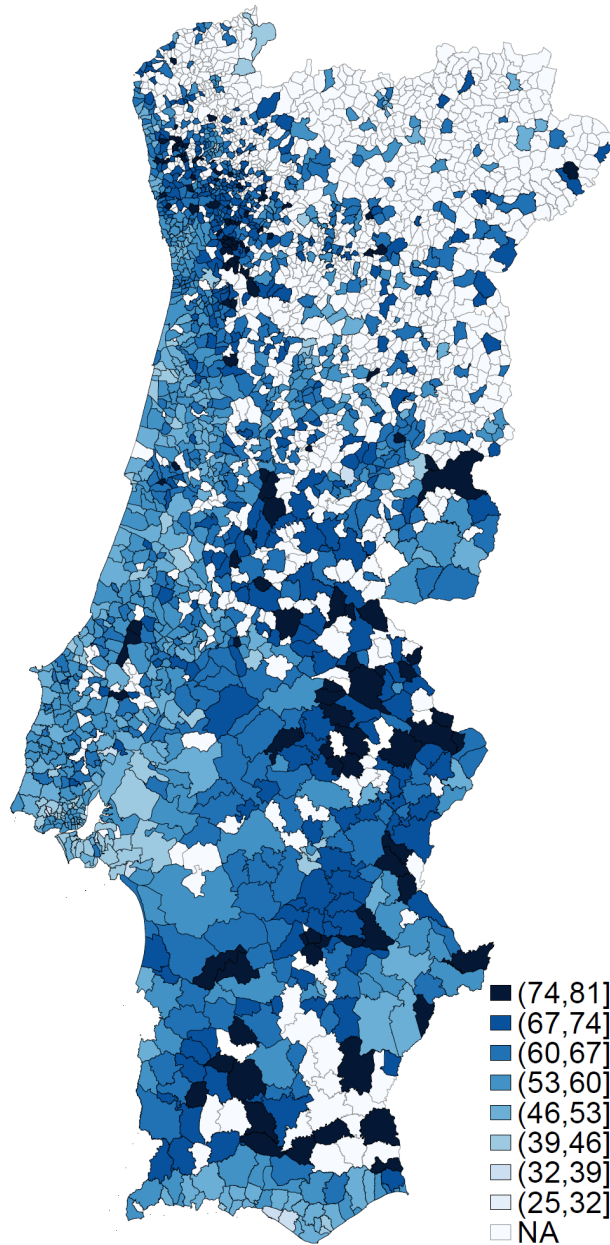


Figure D4: Campaign (treatment message)



Notes: “Votar é um dever” means “To vote is a duty”. “Este domingo/ amanhã/ hoje vote” means “This Sunday/ tomorrow/ today vote”.

Figure D5: Estimated Treatment Effects as in Figure 4.3

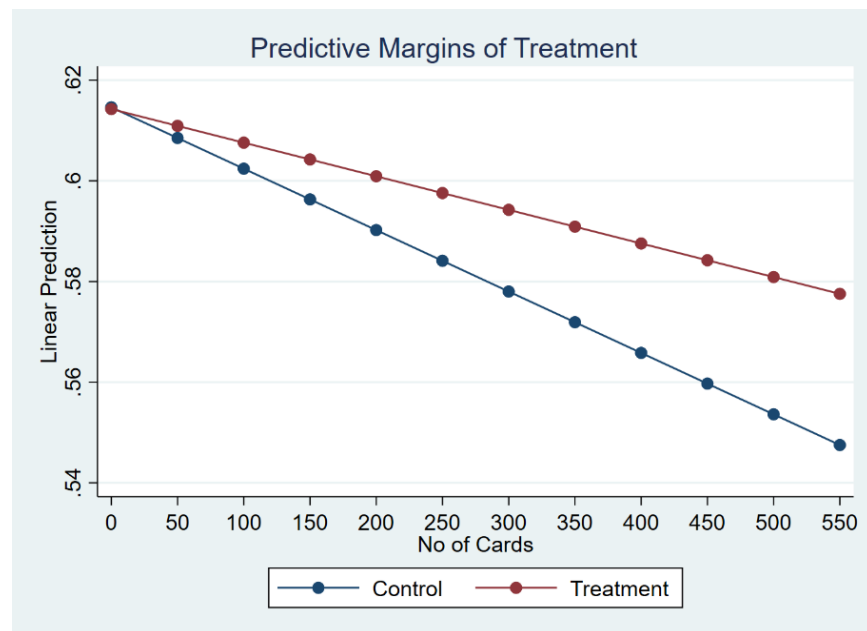


Table D1: **Balance Tests**

Variable	Treatment	Control	Difference (Std. Error)
Stratification Controls			
Voters Density	509.111	515.436	-6.324 (64.032)
Number of ATM Devices	7.029	6.991	0.038 (0.726)
Number of Commercial Banks	1.141	1.223	-0.082 (0.158)
Turnout 2013 (%)	0.595	0.595	0.000 (0.005)
Term Limit Dummy 2017	0.082	0.088	-0.006 (0.014)
Other Controls			
-Socio-demographic and Economic			
Population Below 15 Share 2011 (%)	24.637	24.672	-0.035 (0.247)
Population Above 70 Share 2011 (%)	16.635	16.463	0.172 (0.365)
New Citizens Share 2011 (%)	2.139	2.146	-0.007 (0.063)
Until Primary Education Share 2011 (%)	39.559	39.293	0.266 (0.395)
Tertiary Education Share 2011 (%)	7.644	7.842	-0.198 (0.256)
Mean Value September 2016	40380.310	40017.870	362.436 (4177.158)
Unemployment Rate 2011 (%)	12.328	12.439	-0.112 (0.194)
Service Workers Share 2011 (%)	61.589	61.988	0.399 (0.699)
-Fiscal and Political			
Transfer to Civil Parish per capita	0.032	0.031	0.001 (0.001)
Winning Margin 2013 (%)	22.081	21.463	0.619 (0.916)
Number of Candidates 2017	3.563	3.599	-0.036 (0.064)
Independent Mayor Dummy	0.101	0.096	0.005 (0.015)
Leftist Share 2013 (%)	48.773	49.026	-0.253 (1.124)
Central Government Alignment Dummy	0.449	0.447	0.002 (0.025)
ATM Usage (Entire Campaign)			
Number of Cards (/100)	22.686	22.498	0.188 (2.507)
Number of Operations (/100)	38.674	38.421	0.253 (4.281)
Number of Withdrawals (/100)	19.398	19.244	0.154 (2.110)
Past Elections			
Turnout Rate EU 2014 (%)	33.999	33.723	0.277 (0.359)
Turnout Rate Leg 2015 (%)	56.277	56.099	0.178 (0.371)
Turnout Rate Pres 2016 (%)	49.090	48.869	0.221 (0.393)

Notes: Robust standard errors are depicted in parenthesis. Stars indicate significance levels of 10% (*), 5% (**), and 1% (***)

Table D2: **Descriptive Statistics of the post-treatment recall survey**

Variable	Mean	Std. Dev.	Min	Max
Dependent Variable				
Recall GOTV Campaign	0.188	0.391	0	1
Recall Any Campaign	0.506	0.501	0	1
Experimental Variable				
Treatment	0.596	0.492	0	1
Control Variables				
Gender	0.500	0.501	0	1
Age	51.949	15.977	19	91
Primary Education	0.152	0.360	0	1
Secondary Education	0.264	0.442	0	1
Tertiary Education	0.539	0.500	0	1
Low Interest in Politics	0.514	0.501	0	1
Medium Interest in Politics	0.232	0.423	0	1

Notes: N= 175, corresponding to our preferred specification in Table 4.2. The omitted categories in the control variables are No Primary Education and High Interest in Politics.

Table D3: **Baseline Results per Tercile**

	Turnout Rate 2017		
	1st Tercile	2nd Tercile	3rd Tercile
Treatment	-0.113 (0.414)	-0.232 (0.397)	0.244 (0.265)
Municipal dummies	Yes	Yes	Yes
Stratification Controls	Yes	Yes	Yes
Other Controls	Yes	Yes	Yes
Number of observations	568	567	568
Adjusted R2	0.001	0.574	0.87

Notes: The dependent variable, Turnout Rate 2017, is measured in percentage terms. Terciles according to ATM Usage are measured using the number of unique cards. Treatment is a binary indicator that takes value one if all ATMs in the civil parish displayed the voting campaign. There are 308 municipal dummies. The vector of Stratification Controls includes registered voters' density, the number of ATM devices, the number of commercial banks in 2016, and turnout in 2013 municipal elections. The vector of Other Controls includes the share population below 15 years old in 2011, the share of population above 70 years old in 2011, the share of new citizens in 2011 (since 2007), the share of citizens with primary education and below in 2011, the share of citizens with tertiary education in 2011, the mean value withdrawn in ATMs in September 2016, the unemployment rate in 2011, transfers to civil parishes per capita, the winning margin share in 2013 Local Elections, the number of candidates in the 2017 local election, an independent mayor dummy for 2017, the share leftist votes in the 2013 local election, a central government alignment dummy. Robust standard errors are depicted in parenthesis. Stars indicate significance levels of 10% (*), 5% (**), and 1% (***).

Table D4: **Difference-in-Differences Results**

Turnout Rate Local Election	
Treatment*Year 2017	0.113 (0.305)
Municipal dummies	Yes
Year dummies	Yes
Number of observations	3 406
Adjusted R2	0.590

Notes: The dependent variable, Turnout Rate Local Election (2017 and 2013), is measured in percentage terms. Treatment is a binary indicator that takes value one if all ATMs in the civil parish displayed the voting campaign. There are 308 municipal dummies. Robust standard errors are depicted in parenthesis. Stars indicate significance levels of 10% (*), 5% (**), and 1% (***)

Table D5: **Intensity Effects: Weekend**

	Turnout Rate 2017					
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	-0.213 (0.351)	-0.034 (0.197)	-0.199 (0.351)	-0.030 (0.196)	-0.239 (0.355)	-0.041 (0.197)
Treatment * Number of Cards (*100)	0.027** (0.013)	0.010** (0.005)				
Number of Cards (*100)	-0.109*** (0.010)	-0.038*** (0.014)				
Treatment * Number of Operations (*100)			0.017** (0.009)	0.006* (0.003)		
Number of Operations (*100)			-0.070*** (0.007)	-0.023** (0.009)		
Treatment * Number of Withdrawals (*100)					0.036** (0.017)	0.012** (0.006)
Number of Withdrawals (*100)					-0.138*** (0.013)	-0.052*** (0.019)
Municipal Dummies	Yes	Yes	Yes	Yes	Yes	Yes
Stratification Controls	No	Yes	No	Yes	No	Yes
Other Controls	No	Yes	No	Yes	No	Yes
N	1 703	1 703	1 703	1 703	1 703	1 703
Adjusted R2	0.620	0.876	0.619	0.876	0.621	0.876

Notes: The dependent variable, Turnout Rate 20117, is measured in percentage terms. Treatment is a binary indicator that takes value one if all ATMs in the civil parish displayed the voting campaign. There are 308 municipal dummies. The vector of Stratification Controls includes registered voters' density, the number of ATM devices, the number of commercial banks in 2016, and turnout in 2013 municipal elections. The vector of Other Controls includes the share population below 15 years old in 2011, the share of population above 70 years old in 2011, the share of new citizens in 2011 (since 2007), the share of citizens with primary education and below in 2011, the share of citizens with tertiary education in 2011, the mean value withdrawn in ATMs in September 2016, the unemployment rate in 2011, transfers to civil parishes per capita, the winning margin share in 2013 Local Elections, the number of candidates in the 2017 local election, an independent mayor dummy for 2017, the share leftist votes in the 2013 local election, a central government alignment dummy. Robust standard errors are depicted in parenthesis. Stars indicate significance levels of 10% (*), 5% (**), and 1% (***)

Table D6: Intensity Effects: Robustness Check without Lisbon and Oporto

Excluding:	Turnout Rate 2017					
	Lisbon and Oporto Municipalities			Lisbon and Oporto Districts		
Treatment	-0.025 (0.199)	-0.022 (0.199)	-0.023 (0.199)	-0.190 (0.225)	-0.193 (0.224)	-0.181 (0.225)
Treatment * Number of Cards (*100)	0.006* (0.003)			0.014*** (0.005)		
Number of Cards (*100)	-0.011 (0.010)			-0.024 (0.016)		
Treatment * Number of Operations (*100)		0.003* (0.002)			0.008*** (0.003)	
Number of Operations (*100)		-0.007 (0.006)			-0.016* (0.010)	
Treatment * Number of Withdrawals (*100)			0.007* (0.004)			0.015*** (0.005)
Number of Withdrawals (*100)			-0.011 (0.016)			-0.013 (0.021)
Municipality Dummies	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
N	1 673	1 673	1 673	1 390	1 390	1 390
Adjusted R2	0.876	0.876	0.876	0.863	0.863	0.863

Notes: The dependent variable, Turnout Rate 2017, is measured in percentage terms. Treatment is a binary indicator that takes value one if all ATMs in the civil parish displayed the voting campaign. There are 308 municipal dummies. The vector of Stratification Controls includes registered voters' density, the number of ATM devices, the number of commercial banks in 2016, and turnout in 2013 municipal elections. The vector of Other Controls includes the share population below 15 years old in 2011, the share of population above 70 years old in 2011, the share of new citizens in 2011 (since 2007), the share of citizens with primary education and below in 2011, the share of citizens with tertiary education in 2011, the mean value withdrawn in ATMs in September 2016, the unemployment rate in 2011, transfers to civil parishes per capita, the winning margin share in 2013 Local Elections, the number of candidates in the 2017 local election, an independent mayor dummy for 2017, the share leftist votes in the 2013 local election, a central government alignment dummy. Robust standard errors are depicted in parenthesis. Stars indicate significance levels of 10% (*), 5% (**), and 1% (***)

Table D7: Intensity Effects: Robustness Check without Autonomous Regions

Excluding:	Turnout Rate 2017					
	Azores and Madeira Autonomous Regions					
Treatment	-0.071 (0.208)	-0.071 (0.208)	-0.078 (0.208)	-0.080 (0.209)	-0.080 (0.208)	-0.086 (0.208)
Treatment * Number of Cards (*100)	0.006** (0.003)			0.006** (0.003)		
Number of Cards (*100)	-0.010 (0.010)			-0.010 (0.010)		
Treatment * Number of Operations (*100)		0.004** (0.002)			0.004** (0.002)	
Number of Operations (*100)		-0.008 (0.006)			-0.008 (0.006)	
Treatment * Number of Withdrawals (*100)			0.007** (0.003)			0.008** (0.003)
Number of Withdrawals (*100)			-0.019 (0.013)			-0.018 (0.013)
Municipality Dummies	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Social Support Controls	No	No	No	Yes	Yes	Yes
N	1 546	1 546	1 546	1 546	1 546	1 546
Adjusted R2	0.873	0.873	0.873	0.873	0.873	0.873

Notes: The dependent variable, Turnout Rate 20117, is measured in percentage terms. Treatment is a binary indicator that takes value one if all ATMs in the civil parish displayed the voting campaign. There are 308 municipal dummies. The vector of Stratification Controls includes registered voters' density, the number of ATM devices, the number of commercial banks in 2016, and turnout in 2013 municipal elections. The vector of Other Controls includes the share population below 15 years old in 2011, the share of population above 70 years old in 2011, the share of new citizens in 2011 (since 2007), the share of citizens with primary education and below in 2011, the share of citizens with tertiary education in 2011, the mean value withdrawn in ATMs in September 2016, the unemployment rate in 2011, transfers to civil parishes per capita, the winning margin share in 2013 Local Elections, the number of candidates in the 2017 local election, an independent mayor dummy for 2017, the share leftist votes in the 2013 local election, a central government alignment dummy. Robust standard errors are depicted in parenthesis. Stars indicate significance levels of 10% (*), 5% (**), and 1% (***)

Table D8: Intensity Effects: Robustness Check Horse Race with (selected) Control Variables

	Turnout Rate 2017				
	(1)	(2)	(3)	(4)	(5)
Treatment	-0.022 (0.198)	-0.018 (0.197)	-1.000 (1.223)	0.133 (0.346)	0.496 (0.872)
Treatment * Number of Cards (*100)	0.007** (0.003)	0.07** (0.03)	0.007** (0.003)	0.007** (0.004)	0.007** (0.003)
Number of Cards (*100)	-0.013 (0.010)	-0.013 (0.016)	-0.013 (0.010)	-0.014 (0.010)	-0.013 (0.010)
Treatment * Voters' Density	-0.000 (0.000)				
Treatment * Number of Commercial Banks		-0.035 (0.061)			
Treatment * Until Primary Education Share 2011			0.023 (0.031)		
Treatment * Tertiary Education Share 2011				-0.027 (0.040)	
Treatment * Service Workers Share 2011					-0.009 (0.014)
Municipal Dummies	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes
N	1 703	1 703	1 703	1 703	1 703
Adjusted R2	0.876	0.876	0.876	0.876	0.876

Notes: The dependent variable, Turnout Rate 20117, is measured in percentage terms. Treatment is a binary indicator that takes value one if all ATMs in the civil parish displayed the voting campaign. There are 308 municipal dummies. The vector of Stratification Controls includes registered voters' density, the number of ATM devices, the number of commercial banks in 2016, and turnout in 2013 municipal elections. The vector of Other Controls includes the share population below 15 years old in 2011, the share of population above 70 years old in 2011, the share of new citizens in 2011 (since 2007), the share of citizens with primary education and below in 2011, the share of citizens with tertiary education in 2011, the mean value withdrawn in ATMs in September 2016, the unemployment rate in 2011, transfers to civil parishes per capita, the winning margin share in 2013 Local Elections, the number of candidates in the 2017 local election, an independent mayor dummy for 2017, the share leftist votes in the 2013 local election, a central government alignment dummy. Robust standard errors are depicted in parenthesis. Stars indicate significance levels of 10% (*), 5% (**), and 1% (***)

Table D9: Intensity Effects: Falsification Tests using Previous Elections Variables

	Turnout Rate EU 2014		Turnout Rate Leg 2015		Turnout Rate Pres 2016				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Treatment	0.067 (0.232)	0.066 (0.232)	0.069 (0.232)	0.198 (0.202)	0.200 (0.202)	0.196 (0.203)	0.011 (0.221)	0.015 (0.220)	0.011 (0.221)
Treatment * Number of Cards (*100)	0.004 (0.003)			0.002 (0.003)			0.005 (0.003)		
Number of Cards (*100)	0.001 (0.009)			0.006 (0.009)			-0.001 (0.011)		
Treatment * Number of Operations (*100)		0.002 (0.002)			0.001 (0.002)			0.003 (0.002)	
Number of Operations (*100)		0.003 (0.006)			0.007 (0.006)			0.003 (0.007)	
Treatment * Number of Withdrawals (*100)			0.004 (0.003)			0.003 (0.003)			0.005 (0.004)
Number of Withdrawals (*100)			0.001 (0.013)			0.006 (0.012)			-0.003 (0.014)
Municipality Dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	1 703	1 703	1 703	1 703	1 703	1 703	1 703	1 703	1 703
Adjusted R2	0.803	0.803	0.803	0.707	0.707	0.707	0.787	0.787	0.787

Notes: The dependent variable, Turnout Rate 20117, is measured in percentage terms. Treatment is a binary indicator that takes value one if all ATMs in the civil parish displayed the voting campaign. There are 308 municipal dummies. The vector of Stratification Controls includes registered voters' density, the number of ATM devices, the number of commercial banks in 2016, and turnout in 2013 municipal elections. The vector of Other Controls includes the share population below 15 years old in 2011, the share of population above 70 years old in 2011, the share of new citizens in 2011 (since 2007), the share of citizens with primary education and below in 2011, the share of citizens with tertiary education in 2011, the mean value withdrawn in ATMs in September 2016, the unemployment rate in 2011, transfers to civil parishes per capita, the winning margin share in 2013 Local Elections, the number of candidates in the 2017 local election, an independent mayor dummy for 2017, the share leftist votes in the 2013 local election, a central government alignment dummy. Robust standard errors are depicted in parenthesis. Stars indicate significance levels of 10% (*), 5% (**), and 1% (***).

Table D10: Cost-Effectiveness of GOTV Tactics (adapted from Green and Gerber (2019))

GOTV effort	Start-up and overhead costs	Ongoing management	Effectiveness per contact	Statistically significant?	Dollar cost per vote
Door-to-door	Recruit, prepare walk lists	Substantial training and supervision	One vote per 15 contacts plus spillover effects on housemates	Yes	At \$16/hour and 6 contacts/hour, one vote costs \$31
Leafleting	Recruit, prepare walk lists and leaflets	Monitor walkers, check work	One vote per 189 voters reached by leaflets	Not significantly greater than zero	-
Direct mail, nonpartisan	Design, print, distribute	Intensive during start-up, then postal service takes over	One vote per 273 recipients	Yes, large number of studies	At \$0.50/ piece, one vote costs \$91
Phone, volunteer	Recruit enthusiastic callers	Ongoing training and supervision	One vote per 35 contacts	Yes, large number of studies	At \$16/hour and 16 contacts/hour, one vote costs \$35
Commercial live calls	Obtain phone list	Requires monitoring to ensure quality	One vote per 125 contacts	Yes, large number of studies	At \$0.50/ contact, one vote costs \$63
E-mails	A mass e-mail list	Most of the work is in the start-up	No effects, except when sent by registrar	Large number of studies show that average effect cannot be large	-
Television	Produce and place ads	None	Raises turnout by 0.5 percentage points	Not significantly greater than zero	-
Radio	Produce and place ads	None	Raises turnout by 1 percentage points	Not significantly greater than zero	-

Notes: Dollar cost per vote excludes start-up and management costs and may vary due to local market conditions. For more information see Table 12.1 from Green and Gerber (2019).