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Essays in Public Economics

by

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Abstract

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This dissertation consists of three chapters and analyzes individuals' and firms' response to tax and government spending policies.

The first chapter focuses on the economic incidence of a large value added tax (VAT) cut in the restaurant industry in France. In particular it estimates the share of the tax cut falling on workers, firm owners and consumers by analyzing the VAT cut applied to French sit-down restaurants – a drop from 19.6 percent to 5.5 percent – in July 2009. Theoretically, we develop a standard partial equilibrium model of consumption tax incidence that includes consumption substitutability between the taxed good and an untaxed good, and markets for production inputs, which we allow to vary with the tax. Empirically, we use firm-level data and a difference-in-differences strategy to show that the reform increased restaurant profits and the cost of employees, and aggregate price data to estimate the decrease in prices produced by the reform. We compare sit-down restaurants to (a) non-restaurant market services and (b) non-restaurant small firms, and find that prices decreased by around 2 percent, the cost per employee increased by 3.9 percent and the return to capital increased by around 10 percent. Using these reduced-form estimates we conduct a welfare analysis and find that (1) the effect on consumers was limited, (2) employees shared around 20 percent of the benefit eighteen months after the reform, and 29 percent thirty months after the reform, and (3) the reform mostly benefited capital owners, who received around 50 percent of the tax cut, both in the short-run and in the long-run.

The second chapter derives a more general result on the price effect of VAT changes. It shows that prices respond asymmetrically to increases and decreases in VAT. We combine monthly commodity price data with information on value added tax (VAT) rates across several European countries for the period 1996-2015 and show that prices respond more to VAT increases than to VAT decreases. We explain this asymmetric pass-through to prices with a simple fairness argument developed in the behavioral economics literature. Our finding cautions against using incidence estimates derived in previous studies without accounting for the direction of the tax change and questions the effectiveness of reductions in VAT to achieve redistribution or stimulate economic growth.

The third chapter of the dissertation focuses on a major government spending program in the United States: unemployment insurance (UI). Specifically, I use data on geographic mobility from the Survey of Income and Program Participation (SIPP) and state-year variation in unemployment insurance (UI) laws to evaluate the link between unemployment benefit generosity and mobility decisions of unemployed workers in the United States in the period 2001-2012. My empirical strategy uses a proportional hazard model to study whether the probability of moving during the unemployment spell depends on UI benefit generosity. I find that (1) higher UI weekly benefit amounts increase unemployed workers' geographic mobility, (2) the effect is stronger for more liquidity constrained unemployed workers (3) UI weekly benefit amounts are a stronger determinant of unemployed workers' geographic mobility than UI duration. Theoretically, I develop a spatial equilibrium model with risk-averse and liquidity constrained unemployed workers, and explain the empirical findings with the presence of high monetary moving costs.

To my parents, without whom I would have not been able to achieve this.
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Chapter 1

Who Really Benefits from Consumption Tax Cuts? Evidence from a Large VAT Reform in France

with Youssef Benzarti

1.1 Introduction

While consumption tax cuts are generally designed to benefit consumers, their economic incidence can in practice be significantly different from their statutory incidence. Determining whether consumers are the main beneficiaries is critically important given that consumption taxes are such widely used fiscal policy tools. For instance, the value-added tax (VAT) accounts for 30 percent of total tax revenue in the European Community and amounts to 7.5 percent of GDP for the EU-27 countries in the period 2000-2011.[1] In the United States, 45 out of the 50 states charge sales taxes, which in 2015 accounted for around 35 percent of total state tax revenue.

By understanding who bears the incidence of consumption taxes we can also compare their effectiveness relative to other tax instruments that aim to achieve policy goals like redistribution and economic growth. For example, a lower tax rate on the consumption of food items, as is common in most European countries, is usually considered a way of redistributing revenue to lower income individuals, or as a policy tool to increase firm investment and labor demand in specific sectors. In practice, if VAT cuts do not change firm owners' pricing or production decisions, it is possible that neither of these objectives is achieved.

The existing theoretical literature on tax incidence is largely concerned with evaluating whether firms or consumers primarily bear the economic incidence of consumption tax changes. The standard partial equilibrium framework defined in Fullerton and Metcalf 2002 shows that the incidence on producers and consumers depends on how elastic the supply

[1] Averages for each EU country are reported in Barbone et al. 2013.

curve is relative to the demand curve. More complicated models (Harberger 1962) include the possibility that on the supply side employees and capital owners are affected differently by changes in consumption taxes.

Conversely, previous empirical studies, such as Clement Carbonnier 2007, have mainly focused on the price effects of the tax. Because of data limitations, they have missed the effect on employees and capital owners. Our paper helps fill this gap by providing a full incidence analysis considering a large reduction in the VAT – from 19.6 to 5.5 percent – for sit-down restaurants in France. To extend the empirical literature, we not only consider the incidence on firm owners and consumers, but on employees as well.

Furthermore, our paper offers evidence that is consistent with the general findings of Benzarti and Carloni 2015a. This complementary study combines price and VAT information for a large set of commodities across European countries in the period 1996-2015, and documents an asymmetry in how prices respond to VAT changes. It finds that VAT cuts produce a lower pass-through to prices than do VAT increases. This suggests that VAT cuts are on average not a very effective policy if the goal is to benefit consumers.

Our empirical analysis focuses on the effects of a large reduction in the VAT rate in the restaurant industry in France. In July 2009, the VAT rate for meals consumed in sit-down restaurants was reduced from 19.6 percent to 5.5 percent, while the VAT on no other commodity in the economy was affected. While the effect of the reform on after-tax prices (figure A.1) and total turnover (figure A.7) has been documented in previous studies (Houel 2010, Lafféter and Sillard 2014), no previous work has offered such a comprehensive analysis on the effect of the reform.

In this paper we combine information on national prices from the French National Institute of Statistics and Economic Studies (INSEE) and Eurostat with firm level data from AMADEUS (Bureau van Dijk). Our analysis is articulated in two steps.

First, we use a difference-in-differences strategy and compare sit-down restaurants (our treatment group) to two alternative control groups: non-restaurant market services (our preferred control group)[2], and small non-restaurant firms. We find that both the cost of employees and the return to capital increased in sit-down restaurants after the reform, while we also observe a small decline in prices for meals served in sit-down restaurants. Taken together, these effects suggest that while the VAT cut affected consumers, employees, and firm owners, sit-down restaurant owners were the main beneficiaries of the tax cut.

Secondly, we estimate the share of the VAT burden falling on consumers, employees and firm owners using the theoretical framework of Auerbach and Hines 2002. Our estimates suggest that firm owners bear around 50 percent of the incidence both in the short-run (six months after the reform) and in the long-run (30 months after the reform), that employees bear around 29 percent of the tax in the long-run, and that consumers benefit relatively little from the reform (around 22 percent in the long-run).

[2]Non-restaurant market services follows the definition of the INSEE and includes services that are comparable to sit-down restaurants given that they are traded on the market and are not subsidized by the government. See data appendix for a more detailed definition.

In addition, our analysis documents heterogeneous responses for firms with different characteristics. While we do not find statistically significant differences between small and larger firms, we show that new firms exhibit a larger profit increase following the VAT cut, partly because the cost of employees does not increase as it does for established firms. Furthermore, we do not find that firm concentration[3] matters for the incidence of the tax, given that the effect of the reform is homogenous across geographical areas.

In our theoretical analysis, which we use to explain the empirical evidence, we extend the standard partial equilibrium model of consumption tax incidence developed in Fullerton and Metcalf 2002 to incorporate markets for labor, capital and material goods, while also allowing for consumption substitutability between the taxed good and other goods in the economy. This last feature is important because it allows us to explicitly consider the possibility that, after a VAT reform, individuals replace the consumption of the affected good with the consumption of other goods in the economy.[4] We derive a formula linking the elasticity of the price with respect to the VAT to the elasticity of demand and supply, as done in Fullerton and Metcalf 2002, and develop a framework to show how the other variables of the model react to a change in the VAT.

Our study faces two main limitations. The first, related to our incidence analysis, is that we do not have information on average wage paid by the firm or on which workers get paid most within the firm,[5] but only on the average cost per employee, which includes wages, salaries and taxes on wages paid by the employer. What we consider an increase in wages could in principle be driven by (1) firm owners paying themselves with the additional sales revenue, (2) a reduction in the government subsidy received for each employee, and (3) employees working longer hours after the reform. However, we demonstrate in the paper that these identification threats are unlikely to hold, and that the observed increase in the cost per employee most likely reflects an increase in wages and salaries.

The second concern is that our incidence analysis combines information on national (rather than firm-level) prices with micro-level balance sheet information from a sample of sit-down restaurants. Given that the data we use excludes information on very small firms, which are less likely to react to the reform,[6] it is probable that our estimate of the incidence on consumers is biased upward. We hope to better address this issue in future studies using firm level rather than national aggregate price information.

[3]Firm concentration is defined as the ratio between the total number of sit-down restaurants and the total population in the year previous to the reform.

[4]While this is not explicitly modelled in the standard partial equilibrium model of tax incidence defined in Fullerton and Metcalf 2002, it is implicitly incorporated into the elasticity of demand ϵ^D.

[5]In future work we hope to examine this issue more closely by matching payroll information for each firm with firm balance sheet data.

[6]This is shown in Harju and Kosonen 2013, which focuses on a VAT cut for sit-down restaurants in Finland. The authors consider price cuts in small versus large firms, and show that small firms are less likely to cut their prices than larger firms.

1.2 Relation to Previous Literature

The primary contribution of the paper is to develop an incidence analysis of consumption taxes in a market where firms face close to perfect competition, like the restaurant industry in France. Theoretically, our framework builds on the standard partial equilibrium model of tax incidence described in Fullerton and Metcalf 2002, which assumes perfectly competitive markets and focuses on the price response to changes in consumption tax in a single market: the market for the taxed good. The main prediction in that model is that the burden of the consumption tax falling on consumers and producers depends on how elastic the demand and supply curves are relative to each other. The more elastic supply is relative to demand, the higher the share of the burden falling on consumers.

We extend the partial equilibrium model of Fullerton and Metcalf 2002 to incorporate markets for labor, capital and material goods, as well as an untaxed sector, that is exogenous to the tax but is relevant to model the demand for the taxed good. Our model differs from other two-sector models that have been used in the literature, which either extend consumption taxes to more than one sector, or depart from the perfect competition assumption. Our goal is not to develop an optimal taxation framework, as done in Ramsey 1927 and Myles 1989, but rather to focus on how the burden or benefit of a given tax is distributed. We also differentiate ourselves from most other papers on consumption tax incidence, Delipalla and Keen 1992, Anderson, De Palma, and Kreider 2001 and Schröder 2004, which analyze ad-valorem and unit taxation under different market structures, but do not explicitly model the incidence of the tax on specific inputs of production. The main difference with consumption tax incidence in competitive markets is that the effect on prices no longer depends only on the price elasticity of demand and supply, but also on the degree to which firm production decisions interact with each other as well as on the second derivative of demand to prices.

The empirical literature on the incidence of consumption taxes is limited, and mainly focuses on the effect of consumption tax changes on prices. Several studies, including Poterba 1996 and Besley and H. S. Rosen 1999, have established that in the United States prices do react to consumption taxes, with the magnitude depending on the source of variation used and the market structure considered. Poterba 1996, for instance, finds that retail prices in the clothing industry rose by two-thirds of the amount of sales tax during the Great Depression (1925-1939), while they rose approximately by the amount of the sales tax in the post-war period 1949-1977. On the other hand, Besley and H. S. Rosen 1999 focuses on the period 1982-1990, and analyzes the incidence of sales taxes across 12 commodities and 155 cities. They find heterogeneous price responses across commodities, with the pass-through to prices being higher than 100 percent in some cases. They rationalize their findings with a theoretical model that assumes monopolistic competition. More recently, studies focused on the United States have evaluated the incidence of gasoline taxation (Chouinard and Perloff 2004, Doyle and Samphantharak 2008 and Kopczuk et al. 2013), tobacco taxation (Gruber and Kőszegi 2004, Hanson and R. Sullivan 2009) and alcohol taxation (Kenkel 2005), and find that a large fraction of consumption taxes are borne by consumers, at times with over-shifting of the tax.

Other studies have focused on the effect of VAT changes in European countries. Copenhagen 2008 considers VAT changes in labor-intensive sectors across six different countries, showing that VAT cuts have a lower impact on prices than VAT increases. Labor intensive sectors are also studied in Kosonen 2015 and Harju and Kosonen 2013, which consider large VAT tax cuts in Finland in the salon and restaurant industries respectively. Comparing the prices charged by hairdressers and massage parlors, which they use as a control group, Kosonen 2015 find that prices decrease by 50 percent of the tax change. Perhaps the closest paper to ours, Harju and Kosonen 2013 considers a large VAT reform in the Finnish restaurant industry and find that a 9 percent VAT cut in the restaurant industry produced a 2.1 percent decrease in prices, implying a pass-through to consumers of around one fourth. While their study focuses mainly on the heterogeneous effects of the tax, which depends on a restaurant's location, type of products sold, and size, it finds similarly to us that the incidence on prices, output and employment are limited. Differently from their study, we also find a smaller effect on wages, and develop a full incidence analysis that assigns shares of incidence to consumers, employees and firm owners.

Recent studies on VAT changes in France have mainly focused on the price incidence of the tax. Two papers by Carbonnier have focused on heterogeneous responses in the short-run across markets with different degrees of competition. Clément Carbonnier 2008 finds that the 1995 VAT tax increase had larger effects in more competitive (labor intensive) markets, while the 2000 tax decrease had a more substantial impact in oligopolistic (capital intensive) markets. [7] They find price elasticities of 0.53 and 0.86 in 1995, and of 0.16 and 1.52 in 2000. Analogously Clement Carbonnier 2007 compares the 1987 VAT cut on new car sales with the 1999 tax decrease for housing repair services and estimates that the consumer share of the consumption tax burden was 57 percent and 77 percent, respectively. More importantly for our study, Lafféter and Sillard 2014 estimated the price effect of the 2009 VAT cut on restaurant prices. Using a difference-in-differences estimation strategy and different counterfactuals, they find that prices in the restaurant sector decreased by 2 percent, and that the cumulative incidence on prices over 18 months was around 20 percent, which is slightly lower than our estimate. Differently from us, their study did not develop a full incidence analysis and only focused on the price response to the VAT cut using national level price data.

[7] Using the framework developed in Delipalla and Keen 1992, they explain the empirical findings with a combination of adjustment costs and the curvature of the demand elasticity to prices. Firms in competitive markets are generally smaller and are therefore more likely to lack the financial resources to supply more output when consumption taxes decrease, therefore reacting less to tax decreases. Conversely, firms in oligopolistic markets are less reactive to the tax as the demand elasticity increases with prices, because changes in prices become more costly as prices increase, while price changes produce increasingly lower gains as prices decrease.

1.3 Institutional Background

The 2009 VAT Reform

Before the reform was implemented in 2009, the French restaurant industry had two different VAT regimes: a standard rate of 19.6 percent applied to sit-down meals, and a reduced rate of 5.5 percent for take-away meals. Unsurprisingly, this differentiated tax regime was seen as unfair by small restaurants, which were put at a disadvantage compared to large corporations offering take-away food. Discussion over cutting the VAT in the sit-down restaurant sector had already started in 2002, but a European Union Directive concerning the rules on VAT rates for labor-intensive services prevented the French government from implementing the VAT cut. A few months after the European Commission amended the Directive in July 2008, a pre-agreement was concluded between France and Germany to reduce the VAT in labor-intensive sectors such as restaurants. This pre-agreement, which was reached on January 20, 2009, was followed by active measures from the French finance ministers, who on March 10, 2009 included sit-down restaurants on the list of sectors that could benefit from a reduced VAT rate.

The reform's main goals were to (a) decrease the price of meals consumed in sit-down restaurants, (b) stimulate employment and investment in the sit-down restaurant industry, where total turnover had declined by around 10 percent in the period 1995-2009, and (c) equalize the VAT rate between sit-down meals and take-away meals. Importantly, the French government gathered the representatives of the business associations of the restaurant sector (*Etats généraux de la restauration*) and committed to the *Contrat d'Avenir* three months before the reform was implemented. This non-binding agreement, which was signed on April 28th 2009, offered precise directives on how the tax cut should be used to benefit consumers, and to increase both employment and investment.[8] While the agreement had specific directives on how prices should be cut and targeted the price of sit-down meals as well as take-away goods (the latter of which was not affected by the VAT cut), it only covered about one half of the restaurant population, given that the rest was unaffiliated with any business association. Restaurants who agreed to the reform displayed the change in prices on their menus, and there is anecdotal evidence of strong social pressure on restaurants to decrease their prices. However, the limited decrease in prices we observe in the 30 months following the reform signals that the *Contrat d'Avenir* played a limited role.

On July 1, 2009, the VAT rate on sit-down meals was reduced from 19.6 percent to 5.5 percent, while the tax on alcoholic beverages (19.6 percent), take-away meals and soft drinks (5.5 percent) was left unchanged. At the same time the French government removed a payroll subsidy to which all restaurants and hotels had been eligible since August 2004, and which had been introduced as a temporary measure to stimulate employment in restaurants and hotels. The monthly amount of the subsidy received for each employee hired depended on whether the worker was paid close to minimum wage, and on the tenure of the firm,

[8] See section A.1 in the appendix of the paper for details on the *Contrat d'Avenir*.

and reached a maximum of 1,368 euros per year.[9] While the timing of the subsidy removal overlapped with the VAT reform, previous studies[10] have shown that, while being a costly program for the government, the employment effects of the subsidy were limited.

Employment and Wages in the Restaurant Sector

Restaurants are an important part of the economy in France. According to the INSEE, the share of consumer spending on restaurants has increased from 5.1 percent to 5.9 percent between 1960 and 2007, while the share on food expenditures has decreased from 31.4 percent to 21.9 percent.[11] Around two thirds of consumer spending on food services outside the home goes to sit-down and fast-food restaurants, with the remaining third spent on *traiteurs*[12], canteens, bars and cafeterias.

While the industry has been growing over the years, the traditional structure of sit-down restaurants has not changed over time. Most of the establishments in the restaurant sector are small and employ less than 10 workers. According to FAFIH 2011, which reports employment characteristics for French restaurants and hotels, around 47 percent of workers were employed in establishments with less than 10 workers in 2010, while 14 percent were in restaurants or hotels with more than 50 workers. In addition, sit-down restaurants are highly labor-intensive: labor costs are a major cost in restaurants, and wage setting can be summarized as follows.

First, labor is not very flexible. Indeed, around 78 percent of individuals working in restaurants and hotels were hired with open-ended contracts (*Contrat à Durée Indeterminée* – CDI) in 2013, while around 16 percent had fixed-term contracts (*Contrat à Durée Determinée* – CDD). CDI are contracts that are very hard to revoke: if a worker is fired, employers can incur substantial fees. It is also costly to fire a worker under a CDD contract, but employers are not required to extend expired contracts.[13] The remaining share of workers is composed of apprentices, workers whose employment is subsidized by the government (*Contrats Aidés*), and owners, who account for 1.8 percent of the industry workforce.

Secondly, a considerable fraction of the labor cost consists of the wages of minimum wage employees. The French minimum wage (SMIC, *Salaire Minimum Interprofessionel de Croissance*) is set at the national level and applies to all employees and types of firms. It is indexed to both inflation and past wage growth and is raised every year in July. The wage varies depending on the employee's tenure and the job category, and in 2015 started at 9.61 euros per hour. Seguin 2011 estimated that in restaurants and hotels, around 40 percent of employees were paid the minimum wage in 2003, compared to the 12-15 percent national average.

[9] See the appendix of the paper for more information on the subsidy program.

[10] See for example Houel 2010 for a description of the effects of the subsidy program.

[11] See Consales, Fesseau, and Passeron 2009 for a more detailed analysis.

[12] Catering businesses devoted to take-out food and banquets services.

[13] Details on these contractual forms can be found in the appendix.

Lastly, wage setting is unaffected by collective bargaining, because the industry is dominated by very small firms and collective agreements are very scarce (Fougère, Gautier, and Le Bihan 2010). This feature differentiates the restaurant sector from other sectors of the economy, where annual wage negotiations between employer associations and union or employee representatives occurs at both the industry and company level.

1.4 A Model of Consumption Tax Incidence

In this section we provide a framework to help understand the effect of a consumption tax change on prices, wages and returns to capital in a sector (the taxed sector x) in which commodities are subject to a VAT. We use a partial equilibrium model in which prices, wages and returns to capital in the rest of the economy (the untaxed sector z) are exogenous and therefore not affected by the tax.[14] In our model the economy is populated by $\overline{N}_x + \overline{N}_z$ individuals, which can be employees, capital owners or suppliers of material goods (or a combination).

Individuals demand two types of goods: a taxed good x and an untaxed good z. Labor in each sector is homogeneous in skills and preferences, and there is no factor mobility between sectors. Firms are subject to an ad-valorem tax τ on their value-added, operate in a perfectly competitive market, are characterized by a constant returns to scale (CRS) technology, and use capital, labor and material goods as their inputs of production. Finally, tax revenues R are collected from the taxed good, and are redistributed in a lump-sum fashion to all the individuals in the economy. Figure 1.1 shows the main structure of the model, while figure 1.2 describes the mechanisms through which a change in the VAT rate affects the equilibrium price.

Consumers We assume that individuals' utility functions have a CES form and are therefore convex and separable with respect to consumption goods. In addition, each individual has iso-elastic disutility of labor, with the effect that labor supply does not depend on individual income. Preferences of each of the $\overline{N}_s$ individuals working in sector $s = \{x, z\}$ are modeled by the utility function:

$$U_i = (\alpha x_i^{\frac{\sigma-1}{\sigma}} + (1-\alpha) z_i^{\frac{\sigma-1}{\sigma}})^{\frac{\sigma}{\sigma-1}} - \frac{l_i^{1+\beta}}{1+\beta}$$

where x_i and z_i are individual demands for the taxed and untaxed good, α is the share parameter, σ is the elasticity of substitution between consumption goods and β is the disutility of labor parameter. Each individual in our economy maximizes utility subject to the budget constraint:

$$p_x x_i + p_z z_i = w_s l_i + r_s k_i + cm_i + \frac{R}{\overline{N}_x + \overline{N}_z}$$

[14] This is a reasonable assumption given that sit-down restaurants are a small fraction of the economy. Our setting can be applied to other cases in which the taxed sector is small relative to the economy.

where p_x is the price of the taxed good and p_z is the price of the untaxed good z, w_s are wage earnings if the individual is employed in sector $s = \{x, z\}$, $r_s k_i$ is capital income for an individual i supplying k_i units of capital in sector s, cm_i is income from material goods sold by individual i at a per-unit price c and R are government revenues collected from consumption of the taxed good. Given that we abstract from general equilibrium effects of the tax on the untaxed sector, we take z as the numeraire good and normalize $p_z = 1$, while also taking w_z and r_z as exogenous. In our model total capital supply is fixed in the short-run but upward sloping in the long-run, so that:[15]

$$K_s^S = \sum_{i=1}^{\overline{N}_s} k_i = r_s^{\frac{1}{\mu}} \overline{K}_s \quad \text{with} \quad s = \{x, z\}$$

where $\frac{1}{\mu}$ is the elasticity of the capital supplied to the rental rate of capital and $\mu \to \infty$ in the short-run. Aggregate labor supply in the taxed sector is a function of wages, the marginal disutility of labor, and the exogenous number of employable workers in sector s:

$$L_s^S = \sum_{i=1}^{\overline{N}_s} l_i = w_s^{\frac{1}{\beta}} \overline{N}_s \quad \text{with} \quad s = \{x, z\} \tag{1.1}$$

and the quantity of material goods sold is determined entirely by firms' demand for it. Solving the individual utility maximization problem, and aggregating over the $\overline{N}_x + \overline{N}_z$ individuals in the economy, it follows that the aggregate Marshallian demand for the taxed good is:

$$X^D = \sum_{i=1}^{(\overline{N}_x + \overline{N}_z)} x_i^D = \frac{(p_x/\alpha)^{-\sigma}}{\alpha^\sigma p_x^{1-\sigma} + (1-\alpha)^\sigma} I \tag{1.2}$$

where I is the aggregate income in the economy, and includes labor income from individuals working in both the taxed and the untaxed sector, aggregate capital income, income from material goods, and government revenue, which is redistributed to all individuals in a lump sum.[16]

The equation above shows that the demand for the taxed good depends on the relative price of the taxed and untaxed goods, on the shares parameters, on the elasticity of substitution and on aggregate income.

[15] For simplicity, our model does not incorporate dynamic considerations, adjustment costs or capital accumulation. In addition, we do not include capital in the individual utility maximization problem, assuming that the supply of capital is exogenous to the model. While allowing for an upward sloping capital supply makes the model more general, it is not crucial for our analysis, where the longest time horizon considered is 2 years.

[16] Similarly:

$$Z^D = \sum_{i=1}^{(\overline{N}_x + \overline{N}_z)} z_i^d = \frac{(1/(1-\alpha))^{-\sigma}}{\alpha^\sigma p_x^{1-\sigma} + (1-\alpha)^\sigma} I$$

Firms Market conditions are characterized by perfect competition in the production of both the taxed and the untaxed goods. Given that the equilibrium for the untaxed good is not affected by the ad-valorem tax τ, it is not discussed here. Each firm in the taxed sector uses labor, material goods and capital as its inputs of production, and produces according to a Cobb-Douglas constant return to scale technology. Firms share the same cost structure and are subject to a VAT τ, and per-unit cost of material goods purchased by the firm is equal to c.[17] Therefore each firm's $j = 1, .., J$ profit maximization problem is given by:

$$\max_{l_j, m_j, k_j} \pi_j = (1-\tau)[p_x A l_j^{\gamma} m_j^{\delta} k_j^{1-\gamma-\delta} - c m_j] - w_x l_j - r_x k_j$$

Given that firms are symmetric, $l_j^D = \frac{L_x^D}{J}$, $m_j^D = \frac{M_x^D}{J}$ and $k_j^D = \frac{K_x}{J}$, where L_x^D and M_x^D are the aggregate demand for labor and material goods. The following profit maximization conditions give the aggregate demand functions for labor, material goods and capital:

$$w_x = \gamma(1-\tau) A p_x \left(\frac{{M_x^D}^{\delta} {K_x}^{1-\gamma-\delta}}{{L_x^D}^{1-\gamma}}\right) \tag{1.3}$$

$$c = \delta A p_x \left(\frac{{L_x^D}^{\gamma} {K_x}^{1-\gamma-\delta}}{{M_x^D}^{1-\delta}}\right) \tag{1.4}$$

$$r_x = (1-\gamma-\delta)(1-\tau) A p_x \left(\frac{{L_x^D}^{\gamma} {M_x^D}^{\delta}}{{K_x}^{\gamma+\delta}}\right) \tag{1.5}$$

Government Government tax revenues are collected from a VAT on firms, which is equally redistributed to all individuals in the economy in a lump sum. Given the aggregate demand function for the taxed good X^D, total revenue collected by the government equals:

$$R = \tau p_x \left[\frac{(p_x/\alpha)^{-\sigma}}{\alpha^{\sigma} p_x^{1-\sigma} + (1-\alpha)^{\sigma}} I\right]$$

where aggregate income is an increasing function of government tax revenues:

$$I = ({w_x}^{\frac{\beta+1}{\beta}} \overline{N}_x + {w_z}^{\frac{\beta+1}{\beta}} \overline{N}_z + {r_x}^{\frac{\mu+1}{\mu}} \overline{K}_x + {r_z}^{\frac{\mu+1}{\mu}} \overline{K}_z + c(M_x^D + M_z^D) + R)$$

It follows that we can solve explicitly for government revenues to get:

$$R = \frac{I^n}{\alpha^{\sigma} p_x^{1-\sigma}(1-\tau) + (1-\alpha)^{\sigma}}$$

[17] This corresponds to assuming that the supply curve for material goods is perfectly elastic, so that the equilibrium quantity for material goods is determined entirely by the demand for material goods.

where $I^n = I - R$ denotes aggregate income net of government transfers. It follows that the aggregate demand for the taxed good can be expressed as:[18]

$$X^D = \frac{(p_x/\alpha)^{-\sigma}}{\alpha^\sigma p_x^{1-\sigma}(1-\tau) + (1-\alpha)^\sigma} I^n$$

Effect of the Tax on Prices While we are unable to solve for the equilibrium price, total differentiation of the demand and supply of the taxed good gives us a formula for the price elasticity to the tax change. In particular, in the short-run, when capital supplied is fixed, we have:[19]

$$\frac{d\log p_x}{d\log(1-\tau)} = \frac{[\frac{(1-\alpha)^\sigma}{\alpha^\sigma p_x^{1-\sigma}(1-\tau)+(1-\alpha)^\sigma} - \frac{(\beta+1)(1-\delta)}{F}\theta - \frac{cM_x^D}{I^n}]}{[\frac{(\sigma-1)(1-\alpha)^\sigma}{\alpha^\sigma p_x^{1-\sigma}(1-\tau)+(1-\alpha)^\sigma} + \frac{\beta+1}{F}\theta]} \tag{1.6}$$

where θ is the fraction of aggregate income coming from individuals outside the taxed sector (the untaxed sector, or rest of the economy). For high enough θ, the elasticity is negative, meaning that a higher net of tax rate (or a lower tax) leads to a lower price for the taxed good. In this case, though a lower tax rate increases the demand of the taxed good by increasing wages in the taxed sector (income effect), the lower tax rate also increases the supply of the taxed good, which reduces prices. If the income of individuals in the taxed sector is a small fraction of the economy's aggregate income (θ is high), then the income effect of the tax is smaller than the substitution effect, and therefore the price level decreases.

This elasticity can be rewritten as a function of the elasticities of demand and supply to the price of the taxed good:

$$\frac{d\log p_x}{d\log(1-\tau)} = [\frac{\sigma(1-\alpha)^\sigma}{\alpha^\sigma p_x^{1-\sigma}(1-\tau)+(1-\alpha)^\sigma} + \frac{\delta(\beta+1)}{F}\theta - \frac{cM_x^D}{I^n}]\frac{1}{(\epsilon^S - \epsilon^D)} - 1 \tag{1.7}$$

Therefore the incidence on consumers (in the form of higher prices) is higher when aggregate demand for the taxed good is relatively inelastic to changes in prices, the coefficient or substitution is high or the relative preference for the untaxed good is high, whereas it is lower when the aggregate supply of the taxed good is relatively inelastic to changes in prices. This result is similar to the standard partial equilibrium model incidence formula defined in

[18]Similarly the aggregate demand for the untaxed good can be rewritten as:

$$\begin{aligned} Z^D &= [\frac{(1-\alpha)p_x}{\alpha}]^\sigma \frac{(p_x/\alpha)^{-\sigma}}{\alpha^\sigma p_x^{1-\sigma}(1-\tau)+(1-\alpha)^\sigma} I^n \\ &= [\frac{(1-\alpha)p_x}{\alpha}]^\sigma X^D \end{aligned}$$

[19]In the model appendix of the paper we derive the elasticity for the long-run, when capital supply is upward sloping, and show that imposing $\mu \to \infty$ gives us the short-run equilibrium.

Fullerton and Metcalf 2002, in that it is a function of how elastic demand and supply are to changes in prices in the goods market.[20]

There are two main differences. First, and differently from Fullerton and Metcalf 2002, the possibility to decrease the consumption of the taxed good is modelled explicitly, and depends directly on the elasticity of substitution σ as well as the share parameter α.[21] Our model supports the theory that the higher the substitutability between goods, the lower the price elasticity to the tax. Intuitively, if consumers can easily substitute away from the consumption of the taxed good, then the incidence on them is lower. Secondly, it includes the indirect effect of the tax on material goods used in production, which are an input not directly distorted by the tax. The price elasticity formula (1.7) shows that the higher the share of material goods used in production δ, or the disutility from labor β, the higher the price elasticity. This is because in our model changes in the tax rate distort the labor and capital market equilibria, which has an indirect impact on the equilibrium demand for material goods. The magnitude of this indirect effect depends on how responsive labor supply is to the tax, as captured by $(\beta + 1)$. The larger $(\beta + 1)$, the more elastic labor, wages and material costs will be to the tax, and therefore the higher the negative effect on the supply of the good. This creates a leftward shift of the supply curve, and increasing prices for the taxed good.

1.5 Incidence of the Tax

In this section we develop a formula that allows us to empirically estimate the share of the consumption tax falling on consumers, employees, firm owners and sellers of material goods. Though we assume in the baseline empirical analysis that the reform has no significant effect on the per-unit price of material goods purchased, the theoretical analysis developed below is general enough to include the effect on the sellers of material goods.[22] From the model above firm value-added is divided between employees and firm owners:

$$(1 - \tau)(p_x X - c_x M_x) = w_x L_x + r_x K_x$$

Using the theoretical framework of Auerbach and Hines 2002 we can then separate the share of the tax burden falling on consumers, employees, firm owners, and sellers of material goods. The framework defined in Auerbach and Hines 2002 shows that while the marginal excess burden created by an increase in consumption taxes can be of first order significance if a tax is already in place, the first order welfare effect of the tax is given by the change in the

[20] This is perhaps easiest to see in a model with no material goods. In this case $\delta = 0$ and the incidence on prices paid by consumers depends on the demand and supply elasticities to price in the taxed good market as well as the elasticity of substitution and consumer preferences for the taxed versus the untaxed good.

[21] This is not new to the public finance literature. The seminal paper Harberger 1962 was the first to model substitutability across markets in a general equilibrium setting.

[22] In the discussion section of the paper we show how our results would change if the reform also affected the sellers of material goods.

revenue collected keeping quantities fixed and is a reasonable approximation for the total welfare effect of the tax.[23]

To further explore this, figure 1.3 shows the effect on consumers of the taxed good in the case of a VAT decrease. In order to build intuition, we consider the scenario in which prices faced by consumers decrease by exactly the amount of the tax change, that is $p_1 = p_0 + \Delta\tau$. The change in total revenue produced by the tax equals A-D, with A being the loss in revenue on the quantity sold before the reform, and D being the revenue collected on the additional units sold as the tax is lowered. Given that the deadweight loss decreases by C+D with the tax cut, the overall welfare effect of the tax equals A+C, where A is first-order and C is second-order. In our analysis, we are assigning shares of the first order welfare effect of the tax A to consumers, employees, firm owners, and sellers of material goods. If, as it is the case in 1.3, the price cut observed after the reform equals the pre-reform price plus the tax change ($p_1 = p_0 + \Delta\tau$), then consumers are the only beneficiaries of the tax cut, because the change in tax revenue (holding quantities fixed) corresponds to the first order effect of the tax on consumers. If instead $p_1 > p_0 + \Delta\tau$ ($p_1 < p_0 + \Delta\tau$), then the effect on consumers is smaller (larger) than the change in tax revenue, and some of the tax cut is distributed to (paid by) employees, sellers of material goods and firm owners through changes in wages (w_x), per-unit cost of material goods (c_x) and the return to capital (r_x).[24]

Using the envelope theorem we have that:

$$(1-\tau)p_x dX = w_x dL_x + (1-\tau)c_x dM_x + r_x dK_x$$

and therefore ignore changes in quantities produced by the tax cut. The first order welfare effect of the tax is thus given by the sum of the extra revenue collected on the pre-reform value-added and the extra revenue collected from the increase in value-added produced by the change in the price of output and material goods:

$$d\tau[Xp_x - c_x M_x] + \tau[X dp_x - M_x dc_x] = X dp_x + L_x(-dw_x) + K_x(-dr_x) + M_x(-dc_x)$$

It follows that the burden of the tax on consumers, employees, capital owners and sellers of

[23]While the framework defined in Auerbach and Hines 2002 is mostly suited to studying differential changes in taxes and prices, the change in revenue keeping quantities fixed is a better approximation of the total welfare effect of the tax than the change in the total revenue collected after the tax change, which would include the extra revenue raised on the units sold previous to the tax change and the revenue loss from the decrease in quantity sold.

[24]The changes for w_x, c_x and r_x would be shown in separate figures.

material goods can be decomposed as:

$$\underbrace{\frac{d\ln p_x}{d\tau(1-\delta)+\tau(d\ln p_x-\delta d\ln c_x)}}_{\text{Share on Consumers}}-\underbrace{\gamma\frac{d\ln w_x}{d\tau(1-\delta)+\tau(d\ln p_x-\delta d\ln c_x)}}_{\text{Share on Employees}}$$
$$-\underbrace{(1-\gamma-\delta)\frac{d\ln r_x}{d\tau(1-\delta)+\tau(d\ln p_x-\delta d\ln c_x)}}_{\text{Share on Capital Owners}}-\underbrace{\delta\frac{d\ln c_x}{d\tau(1-\delta)+\tau(d\ln p_x-\delta d\ln c_x)}}_{\text{Share on Sellers of Material Goods}}=1 \tag{1.8}$$

In this formula, each term is divided by the first order welfare effect of the tax on value-added, and the sum of the incidence shares is guaranteed to sum to one. While for example firm owners might also be consumers of the taxed good, this simple framework is useful to separate the incidence of the tax on the different categories of individuals, keeping everything else equal. If a firm owner is also a consumer, then the overall incidence for him/her takes into account both the incidence on consumers and the incidence on firm owners.

In our main incidence estimates below we assume that the reform did not have a direct effect on the per-unit cost of material goods purchased (so that $d\ln c_x = 0$), though in the appendix of the paper we also show results assuming that the observed change in the cost of materials was caused by the VAT reform.

1.6 Data

Annual data on French firms' financial statements and balance sheets come from the Bureau van Dijk (BvD) AMADEUS dataset,[25] which covers all private firms reporting to local tax authorities and/or data collection agencies for the period 2004-2012.[26] The data include standard income statement and balance sheet information such as total turnover, cost of employees, profits, material costs, fixed and current assets, and detailed assets and liabilities. The dataset also contains information on the name, zip code, sector and legal form of the firm. Industries are classified according to the NAF Rev.2 classification, the French national statistical classification of activities introduced in 2008. Each firm in the dataset is associated with a unique industry code, corresponding to its main activity.[27]

[25] Access to AMADEUS was obtained through the Wharton Research Data Services (WRDS) license.

[26] The dataset includes around 1/3 of French sit-down restaurants, and the universe comprises around 100,000 establishments per year.

[27] For instance, if a restaurant offers both sit-down and take-away services, it only receives one industry code. Though this could be a potential source of bias in our analysis, the data do not allow us to control for it. In theory, VAT rates do not specifically apply to restaurants but rather to goods therefore if a restaurant is classified as sit-down but offers take-out food, the VAT rate that should apply is the take-out rate. In practice, prices are VAT-inclusive and unless restaurants have two sets of prices, they are unlikely to charge a different price to customers.

We only consider unconsolidated balance sheets to avoid biasing our estimates with any reporting manipulation that could occur between a subsidiary and its parent company. Unconsolidated data constitute around 70 percent of observations in the AMADEUS data.[28]

In addition, our analysis focuses on specific balance sheet items, which are expressed in per-employee amounts. While only half of unconsolidated balance sheets contained in AMADEUS has information on the average number of employees, this information allows us to distinguish between the employment and wage effects of the reform. At the end of the paper we show that our evidence on aggregate outcomes is robust to including the full sample of firms.

As shown in table 2.1, which displays summary statistics for our treatment and control groups, this selection leaves us with 147,958 sit-down restaurants, 1,482,447 firms operating in non-restaurant market services, and 1,737,234 non-restaurant small firms in the period 2004-2012.

We are primarily interested in estimating the effect of the reform on the value-added per employee, the cost per employee (which includes wages, salaries and taxes on salaries), the number of employees per firm, and the return to capital per employee. While cost per employee and the number of employees are measured directly in AMADEUS, value-added considers income going to labor and capital and is computed as the sum of pre-tax profits, cost of employees, depreciation and interest payments.[29] It is the sum of the cost of employees and the return to capital, which we compute as a residual and includes firm profits, depreciation, interest payments and expenses that are neither labor nor material goods expenses.[30] Throughout the analysis return to capital represents gross income received by firm owners, either in the current period (in the form of profits), or in the future (through investment in the current period).[31]

Monthly price data for sit-down meals consumed in France are from the French National Institute of Statistics (INSEE) [32], while prices for our control groups are computed using

[28]Most of the remaining share of firm level observations is constituted by firms with limited financial information. Consolidated data account for only 0.2 percent of observations in the period 2004-2012.

[29]Effectively, value-added computed this way corresponds to subtracting the cost of employees, the cost of materials and other expenses from a firm's total sales.

[30]For example, operating expenses (purchase of capital goods and supplies, repair and improvements, advertising and promotion, etc.), occupancy and utilities expenses (rent, real estate taxes, utilities, property insurance), as well as general and administrative expenses.

[31]Similarly to what is done in Bartelsman, Haltiwanger, and Scarpetta 2013 and Asker, Collard-Wexler, and De Loecker 2014 we estimate the return to capital for firm j as the residual:

$$\widehat{\text{Return to Capital}}_j = \text{Sales}_j - \text{Material Costs}_j - \text{Cost of Employees}_j$$

Alternative measures of return to capital can be used. For example, one could exclude depreciation and interest payments from our measure of return to capital. In practice this does not substantially change our results. Figure A.16 and table A.6 in the appendix show how our results change when using alternative measures of return to capital.

[32]The INSEE surveys around 200,000 commodities across 27,000 firms every month, from which it constructs its price indexes by consumption category. See Lafféter and Sillard 2014 for a detailed description of

Eurostat monthly price data. Eurostat provides information on the price of goods by 4-digit Classification of Individual Consumption According to Purpose (COICOP) as well weights used to compute aggregate indexes. While we cannot distinguish between the price charged by small firms versus large firms, we use this information to compute the average price of goods sold in the economy in non-restaurant market services and non-restaurant firms.

Finally, we use information on hours and days worked per week from the labor force survey *Enquete Emploi en Continu* (EEC), which contains detailed information on employment (as well as unemployment and training activities) over the twelve months prior to the date of the interview. The survey samples around 400,000 individuals per year and interviews them for six trimesters. It contains self-reported information on the industry of employment, the total number of hours worked during a reference week, and the number of hours worked above the legal limit set by French law.

1.7 Empirical Strategy

Mean Impact Estimation

In order to effectively model the mean impact of the VAT change, we use a difference-in-differences (DD) framework, in which T denotes the treatment group affected by the tax reform, and C is the control group. In the mean impact estimation we consider the pre-reform period 2004-2008 and limit the post-reform period to 2009-2011, given that an additional VAT reform was implemented in the restaurant sector on January 1, 2012, when the VAT rate was raised from 5.5 to 7 percent. The mean effect of the VAT reform on the outcomes of interest is estimated with the following regression:

$$\log Y_{idt} = \eta \cdot \mathbb{1}\{i \in T\} \times After + \lambda_t + \omega_i + \epsilon_{idt} \tag{1.9}$$

where i indexes the individual firm, d indicates the *département* in which the firm is located,[33] t indexes the year in which the outcome is measured, and $After$ is a dummy variable equal to one in the post-reform period 2009-2011. In addition, the estimation model includes year fixed effects λ_t, which controls for differences across years shared by the treatment and control groups, and firm fixed effects ω_i, which controls for firm characteristics that do not change over time. As a result, our identification strategy uses within firm variation across time once aggregate differences over time are controlled for.[34]

the INSEE survey methodology.

[33]France is divided in 96 *départements*, which are administrative divisions whose land area covers around 2300 square miles on average, and median population around 500 thousand in 2001, which is around 21 times the median population of a U.S. county.

[34]In principle we could also include observable firm characteristics that change over time and across individual firms: legal status, the amount of fixed assets (tangible, intangible and other), the amount of current assets (stocks, debt and other), the amount of non-current assets (long-term debt and other) and current liabilities (loans, creditors and other) and the amount of shareholder funds (capital and other). In practice however they do not change the results substantially and can be mechanically correlated with the outcomes of interest.

The error term ϵ_{idt} is clustered by *département* to control for the possibility of within group correlation among firms located in the same geographical area. ϵ_{idt} captures unobserved individual × département × year shocks to the outcome of interest. It is also assumed to be uncorrelated with the regressor of interest, so that

$$E[\epsilon_{idt} \times (\mathbb{1}\{i \in T\} \times After)|\lambda_t, \omega_i] = 0$$

is satisfied. While this identifying assumption is not directly testable in the data, it would be violated only if there were omitted factors that are not controlled by firm characteristics and that affect the treatment and control groups differentially over time.

Our preferred control group includes all non-restaurant firms operating in market services. The definition of market services follows the INSEE definition and includes services that are comparable to the restaurant industry because of their similar nature, but not directly substitutable with restaurants.[35] It includes wholesale and retail trade; repair of motor vehicles and motorcycles; accommodation service activities; information and communication; financial and insurance activities; real estate activities; professional, scientific and technical activities; and administrative and support service activities. It excludes sectors that are not traded on the market such as transportation, public administration activities, education, human health and social work activities as well as entertainment and recreation activities.[36]

We then use an alternative control group which, though not as desirable as non-restaurant market services, serves as robustness checks to our findings. Given that most sit-down restaurants are small firms with less than 10 employees, we consider all non-restaurant small firms as a second control group. In our sample, around 90 percent of sit-down restaurants are small firms, which is close to the aggregate data reported by the INSEE, according to which 86 percent of restaurants employ less than 10 workers. This second control group is not as close to sit-down restaurants as market services in terms of the nature of the activity, but is more comparable in terms of firm size.[37]

[35] In principle one could restrict the set of services considered even further by only including services that are offered to consumers and not to corporations. This would for example exclude services like financial and insurance activities.

[36] See data appendix for a detailed description of services included in the analysis.

[37] While *other restaurants* (which includes both cafes and other self-service catering (56.10B) and take-away restaurants (56.10C)) would also seem an appealing control group because it has similar characteristics to the sit-down restaurants sector and was not affected by the VAT reform, we do not consider it in our analysis. First, from a consumer perspective, it is likely that sit-down restaurants and other restaurant services are highly substitutable. Therefore, when sales increase in one sector, they probably decrease in the other sector as consumers move from one to the other. Secondly, from a producer perspective, *other restaurants* might react to price changes in sit-down restaurants by adjusting their own prices in the same direction. This is consistent for example with the evidence in figure A.1 and is likely to bias our difference-in-differences estimation downward. Finally, there is a large pre-trend in the difference between prices of sit-down restaurant meals and goods consumed in *other restaurants*.

Estimates by Year

The mean impact estimation defined above does not provide evidence on the dynamics of the adjustment in the outcomes we are interested in. We extend the previous analysis by assessing how quickly the variables we are interested in react to the change in the VAT, and how the impact evolves over time. This allows us to assess how long changes following the reform last, and informs our incidence analysis, which distinguishes between short-run, medium-run and long-run effects of the reform. In order to explore the dynamics, we augment our main model with leads and lags indicator variables and consider the period 2004-2012:

$$\log Y_{idt} = \sum_{\nu=-k}^{q} \eta_\nu \cdot \mathbb{1}\{i \in T\} \times \mathbb{1}\{t = \nu\} + \lambda_t + \omega_i + \epsilon_{idt} \tag{1.10}$$

where λ_t capture shocks across years that are common to both the treatment and the control group. Given that we are controlling for time-invariant characteristics of the treatment group, not all the difference-in-difference coefficients are identified. For this reason, we normalize the coefficient on the event dummy in 2008 to be zero. The coefficients of interest η_ν deliver event-study like coefficients, and allow us to quantify the effect of the reform every year before and after it is implemented. It can be interpreted as the percent change in the outcome of interest in each given year relative to the pre-reform year assuming that, absent the tax change, the difference between treatment and control groups would have been the same as in 2008.

1.8 Results

In this section we show evidence on aggregate prices around the time of the reform and discuss the evidence on both the mean impact estimation and the estimated event-time coefficients for value-added per employee, the average cost per employee, the average number of employees and the average return to capital per employee. For comparison purposes, we also show estimates for the cost of materials per employee, a component of sales we discuss later in the paper.

Overall, our estimates suggest that the VAT reform caused a decrease in prices, as well as an increase in the pre-tax value-added per employee, the average cost of employees, and the average return to capital per employee. We show that the reform had a small effect on the quantity of goods sold and on employment, as indicated by the limited change in output displayed in panel b. of figure A.7. The reform also failed to increase employment given its limited effect on the number of employees hired by each firm and on the small increase in the number of sit-down restaurants.

Prices Our price analysis compares the price of sit-down meals to the price of non-restaurant market services. While sit-down restaurants can in theory sell other goods as

well, such as take-away meals, whose price have also been shown to decrease with the reform,[38] we focus on the price of sit-down meals because it is likely to be the most important component of sales in sit-down restaurants.

The drop in the price of sit-down meals observed in July 2009 was an unusual event in the restaurant industry, but figure 1.4 shows that it was limited relative to the VAT cut. Prices dropped by around 1.3 percent in the first month after the reform, while the VAT cut amounted to 14.1 percent, implying a pass-through of 9.7 percent.[39] The pass-through of the tax is especially small if one compares the 2009 reform to the 2012 and 2014 reforms, when the VAT rate on restaurants was raised from 5.5 to 7 percent and from 7 to 10 percent respectively.[40] The pass-through to prices amounted to around one half of the tax increase for the 2012 reform, and around 40 percent for the 2014 reform.

Figure 1.5 shows the log-difference between the seasonally adjusted price of sit-down meals and the price of market services relative to June 2009.[41] The figure shows that the price of sit-down meals dropped by around 2.1 percent in the month after the reform, that the log-difference increased until the beginning of 2010, and that it started to decrease from then on to reach around 1 percent in December 2011. This evidence confirms that while the VAT cut had an immediate effect on prices using market services as a comparison, it was both small and temporary. [42]

While the pass-through of the VAT cut was low, the true effect on prices could have been even lower if sit-down restaurants increased their prices in anticipation of the reform. This concern is shared by the study of Lafféter and Sillard 2014, which points out that prices increased at an unusually high rate in the months preceding the reform. Though this is a possibility, the increase in price observed for restaurants and hotels in the period January 2008-July 2009 was not specific to France and happened in many other European countries as well, as displayed in figure A.3. This increase could have been caused by the increase in the international price of food materials, as shown in figure A.4.

Furthermore, the reform was only announced in January 2009, while the increasing price trend started at the beginning of 2008. Therefore, though we cannot completely rule out the possibility that restaurant owners increased prices in anticipation of the reform, we don't

[38] Evidence of this is presented in figure A.1.

[39] This small drop in prices is unsurprising given the general findings of Benzarti and Carloni 2015a, which considers a large set of commodities across European countries over 20 years, and shows that the price drop following tax decreases is on average not statistically significant from zero.

[40] Evidence on the incidence for the 2012 and 2014 reforms is displayed in figure A.2.

[41] We seasonally adjust the price series estimating monthly fixed effects for the period January 2004 to December 2011, and subtracting them to the non-seasonally adjusted series provided by both the INSEE and EUROSTAT. In the price data, market services include COICOP commodities that EUROSTAT classifies as services and that are produced in sectors that the INSEE classifies as non-restaurant market services. See the data appendix for more details on the list of services considered.

[42] A direct implication is that the before-tax producer price charged by restaurants increased considerably after the reform. If prices dropped by 2.1 percent, and the cost of materials had remained unchanged, both sales and value-added would have increased by 12 percent. In practice value-added increased by less than that because the cost of materials purchased by restaurants also went up in 2009.

think this was likely.[43]

Balance Sheet Items Table 1.2 shows the mean effect of the reform on balance sheet variables, estimated by specification (1.9), which uses within-firm variation and controls for systematic differences in firm performance across years. Column (2) shows the effect of the reform on the value-added per employee: on average the value-added of sit-down restaurants increased by 6.9 percent relative to firms in market services in the post-reform period 2009-2011. In addition, figure 1.6 shows the dynamics of the adjustment in the post-reform period. It suggests that while value-added in sit-down restaurants had been slightly declining relative to market services in the period 2004-2008, it increased considerably in the post-reform years, and the effect was permanent. It shows that, relative to 2008, value-added per employee increased by around 5 percent in 2009, and by 10 percent in 2010.[44]

At the same time, our estimates indicate that the cost per employee increases by 3.9 percent after the reform, as reported in column (3) of table 1.2. While the increase could have been due to either an increase in wages and salaries or an increase in the taxes (paid by the employer) on wages, we show evidence supporting the conclusion that wages and salaries increased slightly after the reform. As with value-added, the effect on the average cost per employee is also long-lasting, as illustrated in figure 1.7. The figure, which plots event-time coefficients for the period 2004-2012, shows that in the pre-reform period 2004-2008 the difference in the average cost per employee between sit-down restaurants and market services is zero relative to 2008, while it is positive and strongly significant in the years following the reform.

While the reform had a positive effect on the cost per employee, column (1) of table 1.2 shows that it did not have an impact on firm employment. Our event-type estimates (displayed in figure 1.8) confirm that the number of workers employed by each sit-down restaurant did not change significantly after the reform. Though the average number of employees in each given firm also did not change, perhaps more firms came to the market, thereby increasing total employment. Figure A.6 shows that this was not the case either, and that although the 2009 increase in the number of firms was higher than for market services, the difference was small. Therefore we conclude that the reform did not have an effect on employment, which is unsurprising given that the output produced by sit-down restaurants also did not change significantly, as shown in panel b. of figure A.7.

Lastly, the reform had a positive effect on both profit per employee and the return to capital-per-employee, which we define as value-added minus the cost of employees and including profits. Profit-per-employee increased by 18 percent in the post-reform period,

[43]In the appendix we estimate a counterfactual distribution for the log-difference in prices using the January 2004 to December 2007 data to predict prices in the period January 2008 to June 2009, which we show in figure 1.5. See table A.8 for the incidence estimates using the counterfactual log-difference.

[44]The comparison with aggregate data is complicated by the fact that the sample of firms we consider misses very small firms, which might have experienced a higher value-added increase. In addition, our estimates compare sit-down restaurants to market services, which also increased value-added in the post-reform period, as shown in the unconditional series in figure in A.5.

while column (4) of table 1.2 indicates that capital-per-employee increased by around 10 percent.[45] The statistically significant effect of the reform on profits and return to capital is clear in figures 1.10 and 1.11, which plot event-type coefficients. Figure 1.11 shows that the return on capital per employee increased by around 10 percent from 2008 to 2009, that it kept increasing in 2010, and that it started decreasing in 2011, possibly because of increasing competitive pressures in the industry.

Figure 1.12 summarizes the effect of the reform on the outcomes of interest and shows that the return to capital increased the most after the reform, suggesting that firm owners benefited largely from the VAT cut. The next section provides a more precise estimate of the incidence.

1.9 Tax Incidence

In this section we compute the incidence of the VAT reform on consumers, employees and firm owners and show how it evolves over time in the 30 months following the reform. We show our incidence estimates in figure 1.13, and mainly focus on three time horizons, which correspond to the normal balance sheet closing dates (December of 2009, 2010 and 2011). These three time horizons help create an overlap between firm balance sheet information, which is recorded only once a year, and the monthly price data. We denote the short-run as 6 months after the reform (December 2009), the medium-run as 18 months after the reform (December 2010), and the long-run as 30 months after the reform (December 2011). December 2011 is a reasonable choice for our longest time horizon because a new VAT reform was implemented on January 1st 2012, when the VAT rate on all restaurants was raised from 5.5 to 7 percent.

In the incidence analysis, we use equation (1.8) to estimate the share of the tax burden borne by consumers, employees and firm owners and assume that the reform does not affect the per-unit price of material goods purchased ($d \ln c_x = 0$). To implement equation (1.8) empirically we need information on the percent changes in p_x, w_x and r_x, as well as the sales share going to consumers, employees and firm owners. Therefore we use the estimated η_ν from our event-type analysis to approximate $d \ln w_x$ and $d \ln r_x$, and the log-differences in prices shown in figure 1.5 as an estimate for $d \ln p_x$.[46]

We divide each firm's cost of employees, cost of material goods and return to capital by its total sales, and compute the average share of sales revenue going to workers ($\widehat{\gamma}$), sellers of material goods ($\widehat{\delta}$) and capital owners ($\widehat{(1-\gamma-\delta)}$ in the pre-reform year 2008. For instance,

[45] Given that the average profit per employee in 2008 was 2,500 euros, our findings suggest that total profits increased by about 2,250 euros for an average sit-down restaurants hiring 5 employees.

[46] In the appendix of the paper we also show incidence estimates under three alternative scenarios. First, table A.8 shows the incidence under the counterfactual price distribution shown in figure A.10. Second, table A.9 shows the incidence using the counterfactual change in the return to capital per-employee displayed in figure A.11 using a linear fit of the pre-reform years 2004-2008. Lastly, in table A.10, we consider the change in the cost of material goods.

the share of income going to employed workers is estimated as:

$$\widehat{\gamma} = \sum_{j=1}^{J} \gamma_j = \sum_{j=1}^{J} \frac{\text{Cost of Employees}_j}{\text{Sales}_j}$$

Given that return to capital is value-added minus the cost of employees, this guarantees that firm value-added is divided between firm owners and employees. Empirically, we find that $\widehat{\gamma} = 0.39$, $\widehat{\delta} = 0.32$ and $\widehat{(1 - \gamma - \delta)} = 0.29$.

Finally, we approximate the denominator of equation (1.8) with the sum of the numerators, so that the shares sum to one. While this is an approximation, we show in the appendix table A.7 that the approximation error is not very large. The approximation error reflects the fact that (1) we are combining national prices with firm-level balance-sheet information and (2) equation (1.8) is better suited for small changes in taxes, while in our scenario we have a large VAT cut. A direct implication is that the approximation is not very good if the reform produces large quantity changes.

Short-Run Incidence The short-run incidence of the reform is measured in December 2009, six months after the reform. Panel a. of table 1.3 shows that the VAT cut increased the return to capital per employee by 10.3 percent between June and December 2009. The average cost per employee also increased after the VAT reform, but the change was small compared to the increase in the return to capital. Panel a. of table 1.3 shows that it only increased by 1.5 percent, explaining the low share of the incidence borne by employees in the short-run, which is summarized by the blue area in figure 1.13.

Surprisingly our estimates also suggest that around 40 percent of the short-run incidence of the tax fell on consumers, as displayed by the green area in figure 1.13. There are two reasons behind this result. First, we combine national price data with balance sheet information from a subset of firms. If the price reaction of the firms not sampled in AMADEUS is smaller than that of the firms we consider, then the large estimated incidence on consumers reflects the fact that the pass-through to prices is larger in our sample than it is in the population.[47] Second, we are ignoring changes in the quantity of material goods purchased (and changes in quantities more generally), a point that we address later on in the discussion. While the VAT cut amount to 14.1 percent, aggregate data show that value-added only increased by only around 6.5 percent in the first month after the reform, suggesting that the concurrent increase in material goods (likely because of the rising price of commodities) decreased the benefit that both firm owners and employees received from the VAT cut. If one compares the 2.1 percent decrease in prices to the 6.5 increase in value-added, then the large estimated incidence on consumers is less surprising.

[47] While we would need firm-level price data to address this point fully, if very small firms (which are known to be under-represented in AMADEUS) cut their prices by less than large firms do, then it is reasonable to expect that the incidence on consumers is larger in our sample than it is for the population. This is for example the case in the study by Harju and Kosonen 2013.

Overall, our short-run estimates suggest that the effect of the VAT cut is largest on the return to capital: table 1.3 shows that sit-down restaurant owners share 50 percent of the benefit from the VAT cut 6 months after the reform.

Medium-Run Incidence The medium-run impact of the reform reflects the effect of the tax 18 months after the reform, in December 2010. Panel b. of table 1.3 shows that the cost per employee increased by 4.7 percent relative to 2008, which means an incremental change of 3.2 percent in 2010 relative to 2009. At the same time, the percent-change in the cost of materials in the medium-run is comparable to the one estimated in the short-run. Panel b. of table 1.3 also shows that, firm owners receive a high share of the benefit from the tax cut (52.8 percent) while the incidence on consumers (27 percent) is lower than it is in the short-run. The lower incidence on consumers relates to the medium-run increase in the price of sit-down meals, as shown in figure 1.5). Our estimated medium-run incidence is slightly higher than what Lafféter and Sillard 2014 find 18 months after the same reform.[48]

On the whole, the medium-run incidence is not very different from the short-run incidence, given that restaurant owners are the main beneficiaries of the VAT cut. This is displayed by the red area in figure 1.13, which also shows that the share of the incidence on employees increases relative to the short-run, from 10.3 to 20.2 percent.

Long-Run Incidence Panel c. of table 1.3 shows that in the long-run, which we define as December 2011, the share of the burden on employees increased to 29.4 percent, while it changed to 48.2 percent for firm owners and 22.4 percent for consumers. The larger share of the incidence on employees in the long-run is a result of the decrease (relative to the medium-run) estimated effect on the return to capital and prices. In fact, table 1.3 shows that in 2011 the average cost of employees increased by 6.4 percent relative to 2008, while the return to capital per employee increased by 14 percent, and the price of sit-down meals by 1.9 percent. The increasing share of the benefit going to employees is also displayed in figure 1.13, which shows an increasing blue area in the long-run.

1.10 Tax Elasticities

In this section, we present estimates of the tax elasticities by considering changes in the outcome variables from the pre-reform period 2004-2008 to the post-reform period 2009-2011. Specifically, we are interested in quantifying the effect of a one-percent change in the net-of-tax rate $(1-\tau)$ on the outcomes of interest, which are primarily prices, the return to capital per employee, and the cost per employee.

[48] In their study they compare the price of sit-down meals and other goods to the price of control groups that are different from ours. Their control groups are the price of goods in other sub-sectors of the restaurant industry, the price of sit-down restaurants in Italy, and a composite price index.

The price elasticity to the VAT cut can be computed directly by dividing the percent change in prices shown in figure 1.5 by the percent change in the net of tax rate $(1-\tau)$:[49]

$$\frac{d\log p}{d\log(1-\tau)} \cong \frac{\Delta p/p}{\Delta(1-\tau)/(1-\tau)} \cong \frac{\Delta p/p}{0.1754}$$

Table 1.4 shows the price elasticity of the tax at different time horizons. Given the immediate drop in prices following the tax cut, the elasticity is largest (though still small) one month after the reform, in the very short-run. Column (1) of the table shows that the elasticity is -0.1203 in the very short-run, meaning that prices decreased by 0.12 percent for a 1 percent increase in the net-of-tax rate. This suggests that the immediate effect on prices was limited considering the magnitude of the VAT cut. While figure 1.5 shows that the price of sit-down restaurants meals kept decreasing (relative to the price of market services) in the 18 months following the reform, the price elasticities for the short- (column(2) of table 1.4) and medium-run (column (3) of table 1.4) are not significantly larger, decreasing to -0.1306 and -0.1351 respectively. Column (4) of the same table shows that the price elasticity then decreased to -0.1106 in the long-run, when prices of sit-down restaurant meals started increasing relative to market services.

For the other outcomes of interest, which are measured in firms' balance sheets, we use a two-stage least squares procedure (as in Saez, Slemrod, and Giertz 2012) and estimate:

$$\log Y_{idt} = \mathrm{e} \cdot log(1-\tau) + \lambda_t + \omega_i + u_{idt}$$

where we instrument the percent change in the net-of-tax rate $log(1-\tau)$ with $\mathbb{1}\{i \in T\} \times After$, a dummy variable interacting treatment group and post-reform period. The outcome elasticity estimate e indicates the mean elasticity of the outcome variable Y to the change in the net-of-tax rate in the period 2009-2011 relative to the pre-reform period 2004-2008, assuming that the outcome variable would have evolved in the same way as in market services absent the reform. As in our main specification (1.9), standard errors are clustered by *département*, which are indexed by *d*.

Column(1) of table 1.5 confirms the finding that the reform did not have a statistically significant effect on the number of employees hired by each firm. In addition, columns (2)-(5) show that an increase by one percent in the net-of tax rate increased value-added per employee by 0.43 percent, increasing the average cost per employee by 0.24 percent and the return to capital by 0.61 percent.

1.11 Hours Worked

In this section we show that the observed change in the cost per employee reflects an increase in the hourly wage rather than an increase in the number of hours worked. While hours

[49]The net of VAT tax rate went from 0.804 to 0.945 between June and July 2009, which corresponds to a 17.54 percent increase in $(1-\tau)$.

worked per week are highly regulated in France, employees are in principle allowed to work supplementary hours in an amount that varies with firm size, the nature of the business and the period of the year. The national legal limit on hours worked is 39 hours per week for restaurant employees, which is higher than the 35 hours per week in most of the other sectors of the economy. Supplementary hours are allowed with the condition that total hours do not exceed 48 per week, or 60 hours per week under very specific circumstances. It is therefore feasible that employees adjusted their hours worked after the reform, and that the increase in the average cost per employee observed does not come from an increase in hourly wages.

In order to test for this, we use survey data from the *Enquete Emploi en Continu* (EEC) and estimate event-time coefficients for changes in hours worked using within region [50] and across time variation in hours worked:

$$\log h_{irt} = \gamma \cdot \mathbb{1}\{i \in T\} + \sum_{\nu=-k}^{q} \delta_\nu \cdot \mathbb{1}\{i \in T\} \times \mathbb{1}\{t = \nu\} + X_{irt} + \lambda_t + \omega_r + \epsilon_{irt} \qquad (1.11)$$

where h_{irt} is a measure of labor intensity of individual i living (and employed) in region r in year t, the treatment group T includes all employees of sit-down restaurants, X_{irt} includes individual characteristics [51], λ_t are year fixed effects, ω_r are region fixed effects, and standard errors are clustered by region.

In the analysis presented in this paper we focus on self-reported measures of the number of hours worked during a reference week, as well as the average number of days worked in a given week.[52] We compare both the base amount of hours worked per week (which by law has to be lower than 39) and the supplementary hours, which are included in our measure of total hours per week.

Figure 1.14 displays unconditional mean values. Panel a. shows that in 2008 base hours worked per week in sit-down restaurants averaged to about 35.5 hours per week, compared to 35 hours per week worked in market services. While workers in sit-down restaurants are more likely to be part-time workers[53] the higher average in sit-down restaurants due to the higher legal limit on hours worked per week.

Panel b. of the same figure 1.14 suggests that the VAT reform also produced a small increase in supplementary hours worked. While the amount of supplementary hours worked is low on average (89 percent of workers in the sample did not work supplementary hours), they increased in sit-down restaurants in the year of the reform, both in absolute terms and

[50] France is divided into 27 administrative regions, which are larger than *départements*, and are the most detailed geographic information contained in the EEC data.

[51] We include age, gender, education, tenure, occupation, marital status, number of employed workers, number of unmarried children living in the household, establishment size, firm size, birth region, and quarter in which worker was surveyed.

[52] Considering alternative definitions of hours worked such as the number of hours normally worked in a given week or the number of hours corresponding to the salary declared in the survey does not change the results.

[53] In our data, 24 percent of workers in sit-down restaurants were part-time workers in 2008, compared to 15 percent in market services.

relative to non-restaurant market services. Furthermore, the average number of days worked also seems to have increased slightly after the reform, as documented in panel d. of the same figure.

While unconditional means show small increases in some of the outcomes, the effects estimated in 1.14 are not statistically significant. Figure 1.15, which shows the event time coefficients estimated using specification (1.11) are not statistically significant from zero. While the small effects might be due to the small sample size, we fail to conclude that hours worked per employee changed substantially following the VAT reform.

1.12 Heterogeneous Effects

In this section we study the heterogeneous effects of the VAT reform by estimating the baseline model on firms which have different characteristics in 2008, the year preceding the reform. We use our main specification (1.9) on sub-samples of firms with given characteristics and compare the estimates by group.

First, we divide firms by size. Though the majority of restaurants (around 90 percent in our sample) are small firms, we study whether small firms were impacted differently from medium and large firms. Panel a. of figure 1.19 shows that the effect on most outcomes is comparable across small and large firms, except for the effect on employment per firm, which is positive for small firms and null for medium and large firms.

Second, we stratify the sample based on firm tenure. Panel b. of figure 1.19 shows that the reform decreased employment and wages for new sit-down restaurants relative to new firms in market services. However, this finding likely is due to the large increase in employment and wages in new firms in market services in the period 2009-2011. In our sample, while the average cost per employee increased from 26.5 thousand euros in 2008 to 34.9 in 2011, it increased from 38.8 to 50.1 thousand euros for new firms in market services. It is therefore plausible that the growth rate of young firms in non-restaurant market services is generally larger than that of firms in the restaurant industry. In addition, figure 1.19 shows that new firms experience a higher increase in profits after the reform, possibly because they are less likely than existing firms to raise income paid to the inputs of production.

Finally, we divide the sample in quartiles of market concentration, defined as the ratio between the total number of restaurants and the total *département* population in 2008. As we find that market concentration is positively correlated with both density of *département* population and net hourly wage, we might for example expect that areas with a higher concentration of restaurants have a stronger incentive to adjust their prices because of competitive pressures. Moreover, consumers in high density areas could be more informed about the tax change, so sit-down restaurants also experience a stronger social pressure to lower their prices.[54] Panel a. of figure 1.20 shows that the effect on employment per firm is larger

[54]Since restaurants that lowered their prices indicated so on their menus, there is reason to expect consumers frowned upon sit-down restaurants that failed to adjust their prices. There is some anecdotal evidence to support this.

in areas with more restaurant concentration. In addition, panels b. and c. of the same figure show that while the effect on the cost per employee does not change significantly with market concentration, the effect on the return to capital is slightly larger in areas with higher market concentration. Neither population nor net hourly wage explain the small differences we find across areas with varying market concentration.[55]

1.13 Firm Entry and Exit

It is possible that the reform induced sit-down restaurant creation and/or selection into treatment. This might occur if (1) previously existing sit-down restaurants are more likely to report to local tax authorities after the VAT cut, (2) sit-down restaurants creation (relative to the control group) increases as a result of the reform, or (3) existing firms change the nature of their main activity to become sit-down restaurants. If the reform affects the composition of the treatment and control groups, then the estimated effect reflects the treatment on the treated (TOT) rather than the intent-to-treat (ITT) effect.

Given that not all firms are required to report to local tax authorities, AMADEUS does not allow us to precisely establish how many firms enter/exit the market and how many firms start/stop reporting to local tax authorities. [56]

Despite the data limitations, we can show the effect of the reform on three different group of firms: (1) all firms between 2008 and 2012 (TOT group), (2) firms that are active in 2008 but might exit the sample in the period 2009-2012, (3) firms that are active in 2008 and do not exit the sample in the period 2008-2012 (ITT group). Comparing (1) and (3) tells us about the effect that reporting to local tax authorities (some of which is created by firm entry) has on our estimates, while comparing (2) and (3) gives us information on the effect on firms which stop reporting (part of which exit the market).

Figure 1.16 uses specification 1.10. Panel (a) and (b) show that the ITT effect on the number of employees and the cost per employee is larger than the TOT effect, and consequently that the return to capital per employee increases by less for firms that are active in 2008 and stay in the sample throughout the period 2008-2012. More generally, the evidence is suggestive of the fact that sample exit is a more important source of bias than sample entry. In fact, the difference between (1) and (2) is negligible, relative to the difference between (1)-(2) and (3).

[55] Evidence on this is not reported for brevity but is available on request.

[56] While we are able to establish that around 30 percent of firms entering the sample every year are new firms, firms exiting the sample could be either (a) firms which stop reporting to tax authorities or (b) firms who exit the market.

1.14 Discussion

Cost of Materials

It is plausible that the VAT reform had an effect on both the quantity and price of material goods used in the restaurant industry. In fact, it is not unreasonable that the demand for material goods would have increased following the VAT reform, either to accommodate the small increase in output or to follow the directives of the *Contrat d'Avenir* on the general improvement of restaurant services. At the same time, sit-down restaurant owners increased the quality of the inputs purchased to improve the quality of services. [57]

While our estimates show that the cost of materials did increase between 2008 and 2009, as shown in figure 1.9, we do not have the data to separate between quantity and quality effects. Because of this, our incidence analysis takes the conservative approach of not considering the effect of the VAT reform on the per-unit cost of materials purchased by firms. [58]

Still, even if we assume that the increase in the cost of materials is due mainly to an improvement in the quality of inputs purchased by sit-down restaurants, our results do not change significantly. This is shown in table A.10 and figure A.12, which show that the estimated incidence on the sellers of material goods is relatively small: between 11 and 17 percent depending on the time horizon. Therefore, as in our main analysis, firm owners are still the group that benefits the most from the reform.

Short-Run versus Long-Run Effect of the Tax

In a perfectly competitive market with no government regulation and no adjustment costs, our model predicts that a decrease in the VAT rate produces an immediate decrease in prices, while increasing equilibrium wages, labor and return to capital. In practice, we believe that in our setting the directives set by the *Contrat d'Avenir* played some (though a limited) role in the short-run.

As mentioned earlier the French government set precise directives on how prices should be cut, likely causing short-run prices to drop below their competitive market equilibrium. This is shown in figure 1.17, where the short-run output is sold at price p_{SR}, which is lower than the competitive market price p_1. The same figure also shows that the short-run change in output depends the elasticity of the supply and demand for the good: in practice, the output response to the tax cut was small for the French VAT reform.[59]

[57] In addition, an increase in the per-unit cost of inputs purchased might be related to reasons that are unrelated to the reform, such as an increase in the international price of the commodities used by restaurants. While figure A.4 shows that this is unlikely to be the case for food commodities, we cannot completely rule out this channel as we do not observe the exact composition of the inputs purchased.

[58] That is, we assume $d \ln c_x = 0$.

[59] As more labor is used in production (capital is fixed in the short-run), the supply for the taxed good also increases, as shown in panel a. of figure 1.17, where the supply curve shifts down from $S0$ to $S1$. At the same time, lump-sum transfers from the government decrease with the VAT cut, decreasing the demand for

Consistently with our empirical findings, wages and returns to capital also adjust upward following the reform.[60] This is shown in figure 1.18, where the increases in the marginal products of labor (MPL) and capital (MPK) produce an increase in the demand for both labor and capital (as reflected by the upward shifts $LD1$ and $KD1a$). In addition, the demand for capital increases further to $KD1$ as the equilibrium labor increases, making capital more productive.

In the long-run it is instead unlikely that the *Contrat d'Avenir* played a significant role. Consistently, a few additional changes occur in our model as the economy approaches its long-run competitive market equilibrium. Prices increase relative to the short-run, as shown in panel b. of figure 1.17. At the same time, both equilibrium labor and capital increase further, as shown in panels b. and d. of figure 1.18, increasing economic output. This is compatible with the empirical evidence of figure A.7. In addition, the supply for capital $KS2$ becomes more elastic (as shown in panel d. of figure 1.18), reducing the return to capital, as displayed in our empirical estimates of figure 1.11. Lastly, as shown in panels b. and d. of figure 1.18, the MPL increases further in the long-run, resulting in further increases in the equilibrium labor and wage. This is again consistent with the increasing cost per employee shown in our empirical analysis, as in figure 1.7.

Effect of Payroll Subsidies

In this section we show that the post-reform increase in the cost per employee, which we interpret as an increase in wages, is unlikely to be driven by the payroll subsidy cut that took place in July 2009, at the same time the VAT cut was implemented. All restaurants and hotels had in fact been eligible for a monthly payroll subsidy for each employee hired since 2004, and the amount varied based on the employee's wage and the tenure of the firm.[61] After the July 2009 payroll subsidy cut net payroll taxes increased, raising the cost per employee. As a result, the estimates of our analysis might be biased by including the effects of both the VAT cut and the payroll subsidy cuts. In order to address this issue, we consider non-restaurant firms affected by the subsidy cut but not the VAT cut.[62]

We focus on the three sub-sectors of the hotel industry: (a) hotels and similar accommodation, (b) holiday and other short-stay accommodation and (c) camping grounds and recreational vehicle parks. Though this treatment is the most reasonable, identification of

the taxed good (as well as the untaxed good). As shown in section 1.4, if the fraction of aggregate income coming from the untaxed sector θ is high enough, the negative income effect on aggregate demand is larger than the positive income effect from higher wages in the taxed sector. If this is the case, the demand curve for the taxed good shifts downward from $D0$ to $D1$, and prices in the taxed sector decrease unambiguously.

[60] For simplicity, we do not consider changes in the market for material goods.

[61] See appendix for details on the payroll subsidy program.

[62] Though only sit-down restaurants were affected by the VAT cut, we do not consider the rest of the restaurant industry because it also did react to the reform. This is shown in the appendix figure A.1, and is consistent with either (1) competitive price adjustments or (2) restaurants following the price directives set by the *Contrat d'Avenir*, which targeted not just sit-down restaurants but also take-away restaurants and beverage services.

the effect of the payroll subsidy cut is complicated by the fact that the aggregate value-added in the hotel industry also increased after July 2009, as shown in the appendix figure A.8. This is possibly because of (1) spillovers from the restaurant industry to the hotel industry and (2) the fact that hotels might also be offering restaurant services as secondary activity. We denote the treatment group by SC and compare it to firms in market services by estimating:

$$\log Y_{idt} = \alpha \cdot \mathbb{1}\{i \in SC\} \times After + \lambda_t + \omega_i + \epsilon_{idt} \tag{1.12}$$

where λ_t are year fixed effects and ω_i are firm fixed effects and α measures the change in the outcome variable Y in the period 2009-2011 when one compares the hotel industry to non-restaurant market services. Table 1.7 shows that removing the hiring subsidy had a negative effect on employment, while having a positive effect on the remaining variables. The table shows positive effects on value-added, consistent with the aggregate evidence. In addition, as one would expect, the payroll subsidy cut had a negative effect on the number of employees hired by each hotel.

The effect on the cost per employee is harder to interpret, given the post-reform increase in value-added observed in figure A.8. On the one hand the payroll subsidy cut should have had a positive effect on the cost per employee given that taxes on wages became larger without the subsidy. On the other hand wages could have also increased given the increase in value-added observed in the hotel industry. We therefore think that α gives an upper bound estimate for the effect of the subsidy removal. Table 1.7 shows that the estimated effect on the cost per employee is also not as large as the effect we find in our main analysis (where the cost per employee increased by 3.9 percent), and therefore does not represent a major source of bias for our results.[63]

In general, the payroll subsidy granted to hotels and restaurants was substantially smaller than the 2009 VAT cut. Payroll subsidies were at most 1,368 euros per employee per year, while the VAT cut was a 14.1 percent reduction in the tax on total value-added. Given that the average value-added in our sample is around 300 thousand euros, and that each sit-down restaurants hires around 5.5 employees, it follows that the VAT cut resulted in savings of around 7,500 euros per employee per year.

Wages or Profits

We argue that it is unlikely that the cost per employee increased after the reform because sit-down restaurant owners increased wages paid to themselves. This is unlikely because restaurant owners in France are primarily self-employed, and the tax law is such that it is to their benefit to declare income in the form of profits. This is because in France restaurants are considered part of the "artisants, commerants et industriels"[64] sector, subject to a specific

[63] If the estimated effect of the payroll subsidy cut was the true α, one would need to subtract the estimated coefficient from the coefficient in our main estimates, which includes both the effect of the VAT cut and the effect of the payroll subsidy cut. Deflated event-type coefficients are reported in the appendix figure A.13.

[64] Craftsmen and traders.

tax regime, under which the tax on firm owners is lower than the tax on employees. Profits in this sector, from which firm owners pay themselves, are called "revenue mixte" by the fiscal authority and are treated as a mix of wage and profit income. The "revenue mixte" is subject to a sum of income tax and an "artisants, commerants et industriels" specific payroll tax rate, which is lower than the regular payroll tax rate paid for employees because firm owners do not contribute to (and are therefore not eligible for) unemployment insurance. Hence reducing firm profits to increase wages and salaries would not be profitable from a tax perspective.

We therefore conclude that, even if small restaurants were managed by several individuals – some of whom could be family members hired as employees – it is unlikely that firm owners transfer value-added from profits to wages. Indeed, the AMADEUS dataset reports the total compensation of firm owners as profits, whereas the variable measuring the cost of employees excludes firm owners. Importantly, the reform did not change the incentives on how firm owners should be paying themselves.

1.15 Robustness Checks

Changes in Local Economic Conditions

In principle the effects we identify could be affected by changes in local economic conditions over time that affect the treatment and control groups differently. In order to address this issue we add interaction terms between the unemployment rate in the firm's *département* where the firm is located and the dummy variables defining whether the firm belongs to the treatment or the control group:

$$\log Y_{idt} = \eta \cdot \mathbb{1}\{i \in T\} \times After + \gamma_1(\text{URate}_{dt} \times \mathbb{1}\{i \in T\}) + \gamma_2(\text{URate}_{dt} \times \mathbb{1}\{i \in C\}) + \\ + \lambda_t + \omega_i + \epsilon_{idt}$$

For this purpose we use INSEE data on quarterly unemployment rates by *départements* and compute the average unemployment rate by *départements* in each given year. Table 1.8, which follows the main specification (1.9) and includes year and firm fixed effects, shows that controlling for the differential effect of the local unemployment rate on the treatment and control groups does not substantially affect our main estimates. While the coefficient for the effect of the reform on value-added per employee is slightly lower than what we showed in table 1.2 (0.63 versus 0.69), all of the other coefficients are unchanged, suggesting that our findings are robust.

Employment Weighted Estimates

Our main estimates are not weighted by the number of employees. Therefore in this section we consider the mean impact of the reform by weighting observations by the number of

employees in the firm and estimate:

$$\log Y_{idt} = \eta^W \cdot \mathbb{1}\{i \in T\} \times After + \lambda_t + \omega_i + \epsilon_{idt}$$

where the coefficient of interest is η^W. Table 1.9 shows that weighting establishments by employment level does not change the statistical significance of the results, though the magnitudes change slightly. When one weights by firm employment, larger firms are assigned a higher weight, and therefore the estimated effects reflect more closely the behavior of those firms. Column (2) of table 1.9 shows that value-added per employee increased by 4.3 percent, compared to 6.9 percent in our main analysis. Similarly, column (3) shows that the effect on the average cost of employees is smaller than in the main analysis, and is around 2.2 percent (versus 3.9 percent). The estimated effect on the number of employees per firm is now negative and statistically significant, suggesting that larger firms are more likely to decrease employment than smaller firms (this is shown in column (1)). Finally, while the effect on profit per employee is unchanged, the effect on capital per employee decreases slightly, from 0.099 to 0.074, indicating that in this case capital per employee increased by 7.4 percent after the reform.

While weighting does change the magnitude of the estimated coefficients, reducing the incidence of the tax on employees and firm owners, we believe that un-weighted estimates are preferable for our analysis. First they are more likely to reflect the average response of a firm in the economy. Given that 90 percent of sit-down restaurants have less than ten employees (and a within group average of 4 employees per firm) while the remaining 10 percent hires around 19 employees per firm, weighting by number of employees would increase the weight on the medium-sized and large firms. Second, very small firms are under-represented in AMADEUS, meaning that our estimates already assign higher weight to firms that are larger than the population average. Weighting by number of employees would exacerbate this.

Alternative Control Group

In this section we show that the results of the main analysis do not depend on the particular control group used. Table 1.10 considers non-restaurant small firms as an alternative control group and shows that the estimated mean impact of the reform is highly comparable to what we found in our main analysis.

Column (2) of table 1.10 shows the effect on value-added per employee, which increased by 6.9 percent, a very similar effect to what we found in our primary analysis. Column (3) of the same table displays the mean impact on the cost per employee, which increased by 3.8 percent. While this effect is similar to our main estimate, column (1) of the same table shows that the effect on the number of employees is surprisingly positive and statistically significant. However, the effect is unlikely to be driven by the reform given the pre-trend shown in panel c. of figure 1.22, and likely reflects the fact that the average size of sit-down restaurants has increased over time. Finally, and consistently with the results of our main

analysis, we find that both the return to capital and the cost of materials increased with the reform. Columns (4) and (5) of table 1.10 shows that profit per employee increased by 20 percent in the post-reform period, while return to capital per employee went up by 11 percent.

Figure 1.21 shows the price difference between sit-down restaurant meals and non restaurant prices in the economy,[65] while figure 1.22 shows event-time estimates using 1.10 when we use non-restaurant small firms as a control group. Taken together, the evidence we show is comparable to the findings of our main analysis, suggesting that our main estimates are robust.

Despite the pre-trend in the price series displayed in figure 1.21, we also report the incidence results using this alternative control group. The estimates are very comparable to what we find in our main analysis. Table 1.11 shows that in the long-run the share of the incidence on employees is around 28.4 percent, while sit-down restaurant owners benefit the most from the reform, with an incidence of 50 percent.

Full Sample

The main analysis of this paper focuses on per employee variables, which implies that we only focus on firms whose number of employees is reported in the AMADEUS data. The main advantage of restricting the sample this way is that from the employee's perspective we can separate the effect of the reform on wages from changes in employment. While this helps the interpretation of our results, it comes at the cost of reducing the sample size by half.

We show in the appendix of the paper that the effects we find are generalizable to sit-down restaurants with no employment information, for which we observe aggregate firm variables but not per employee variables. We report the estimates on the full sample in the appendix of the paper. Figure A.14 shows event-time figures estimated in the full sample, when non-restaurant market services are used as a control group. The figure shows that the reform had a positive and statistically significant effect on all firm value-added, return to capital and profits. It also shows that the cost of employees increased in the two years following the reform, though we cannot separate the employment from the wage effect. Figure A.15 shows analogous results when non-restaurant small firms are used as a control group. The findings are again robust to including the full-sample.

[65] Non-restaurant prices are computed from Eurostat data, taking the national price index excluding energy prices p_{all}, the restaurant prices p_{rest} and weights w_{rest}, and computing:

$$p_{non-rest} = \frac{p_{all}100 - p_{rest}w_{rest}}{100 - w_{rest}}$$

While this reflects the price charged by all rather than just small non-restaurant firms, we believe it is a reasonable approximation of the price series we are interested in.

1.16 Conclusion

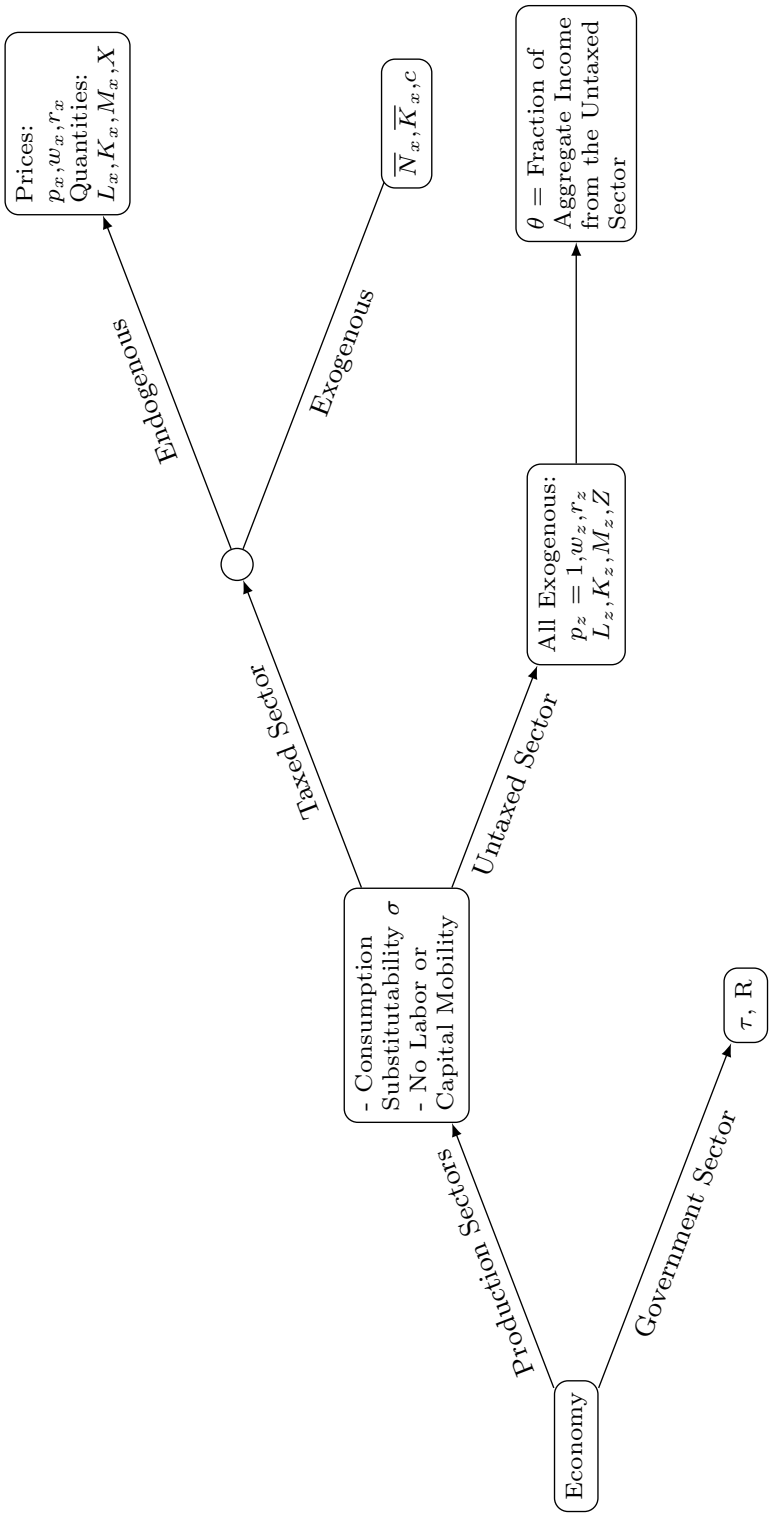
In this paper, we consider a large VAT cut in France to estimate the share of incidence that falls on consumers, firm owners and employees. We find that the reform mostly benefited sit-down restaurant owners, that consumers benefited relatively little and that employees shared around 20 percent of the benefit 18 months after the reform and around 29 percent 30 months after the reform.

To theoretically explain our findings, we extend the standard partial equilibrium model of consumption tax incidence developed in Fullerton and Metcalf 2002 to include consumption substitutability between the taxed good and other goods traded in the economy, as well as markets for the inputs of production, which can be affected by the tax. In our framework firms operate in a competitive market and use labor, capital and material goods as their inputs of production. Our model provides clear predictions on how prices, labor and capital income react to a change in VAT and instruct the empirical findings of the paper.

Using a difference-in-differences strategy, and comparing sit-down restaurants to non-restaurant market services and non-restaurant small firms, we show that value-added, the return to capital and the cost of employees increased with the reform. We also find small effects of the reform on the quantity sold by sit-down restaurants, and a limited effect on the price of sit-down meals, for which we estimate an immediate decrease of 2.1 percent and a pass-through of the VAT of around 15 percent.

Our results provide important insight on the distributional effects of consumption tax cuts, taking into account the effect on employees, an often overlooked group in the literature. In addition, they raise the question of whether cuts in consumption taxes are really beneficial to the group of individuals they target. This paper argues that this is not the case, as the consumption tax reform in question succeeded primarily in benefitting restaurant owners over its targeted beneficiaries: consumers.

Figure 1.1: Structure of the Model



Notes: The figure shows the structure of the partial equilibrium model defined in section 1.4.

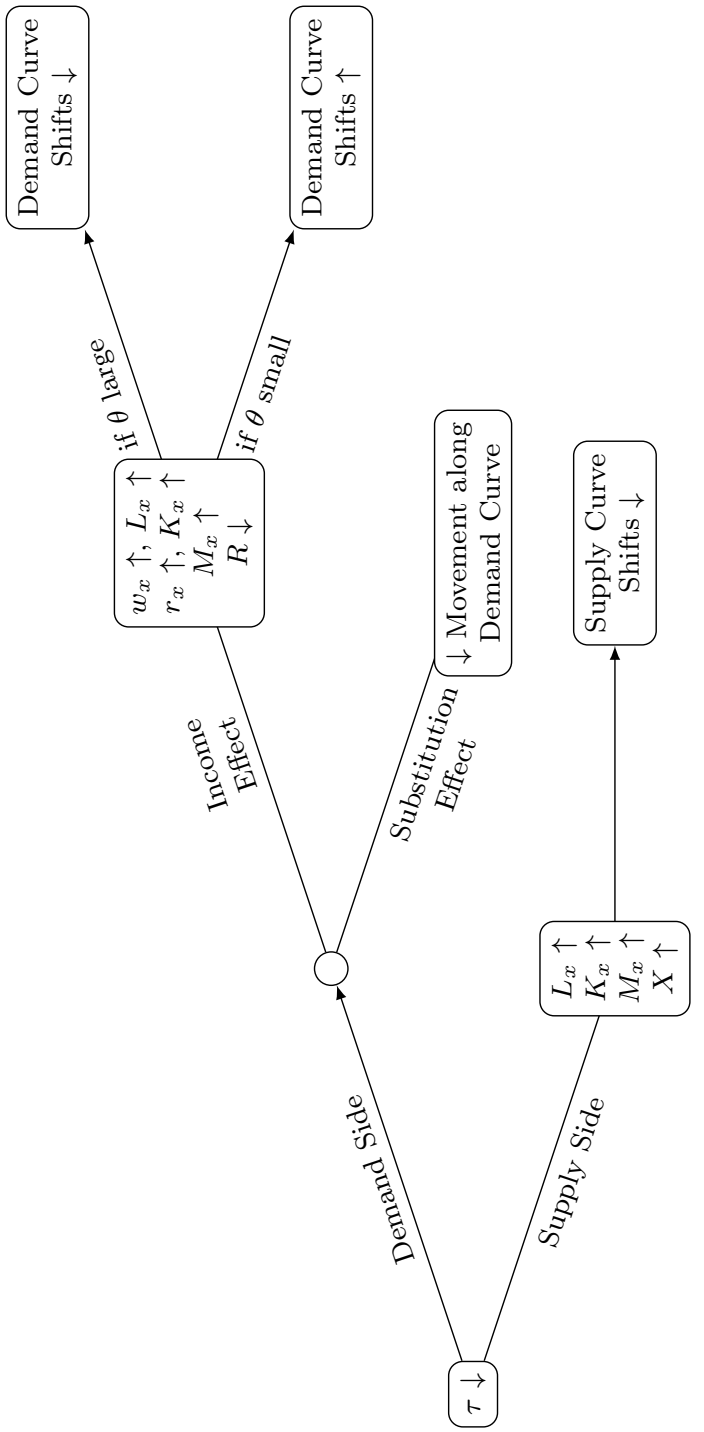
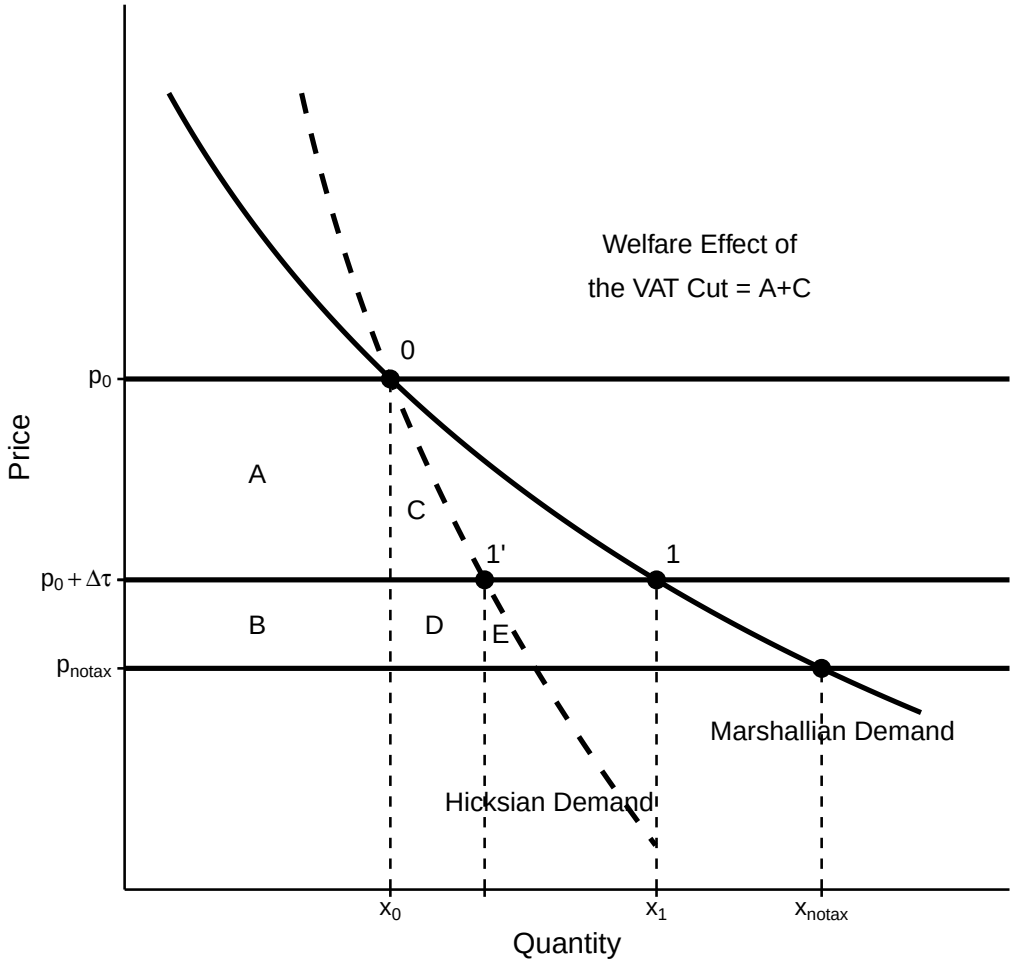


Figure 1.2: Effect of a Decrease in the VAT

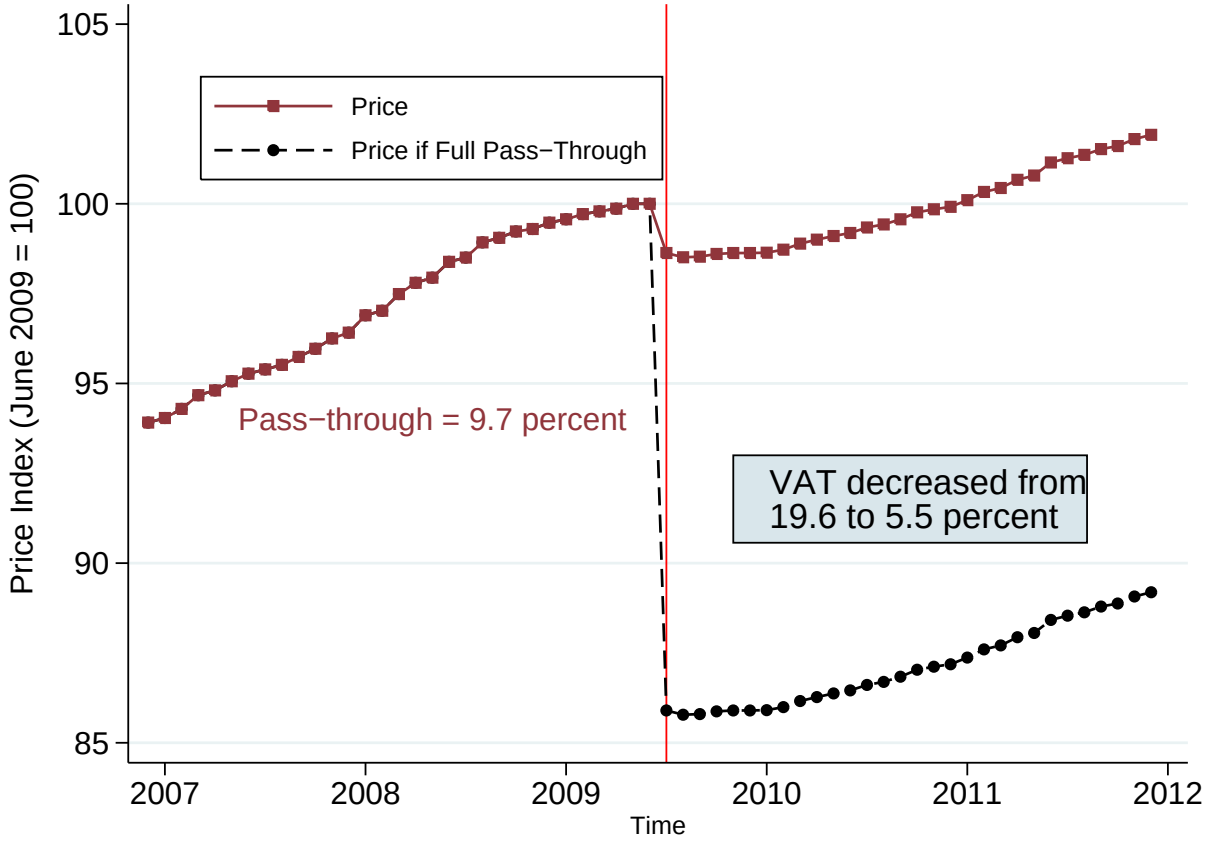
Notes: Given that the supply-side effect of the VAT cut is larger than the demand-side effect, the equilibrium price decreases after the tax change. In the short-run, the amount of capital K_x supplied is fixed and equal to $\overline{K}_x$.

Figure 1.3: Welfare Effect of VAT Decrease on Consumers if Full Pass-Through



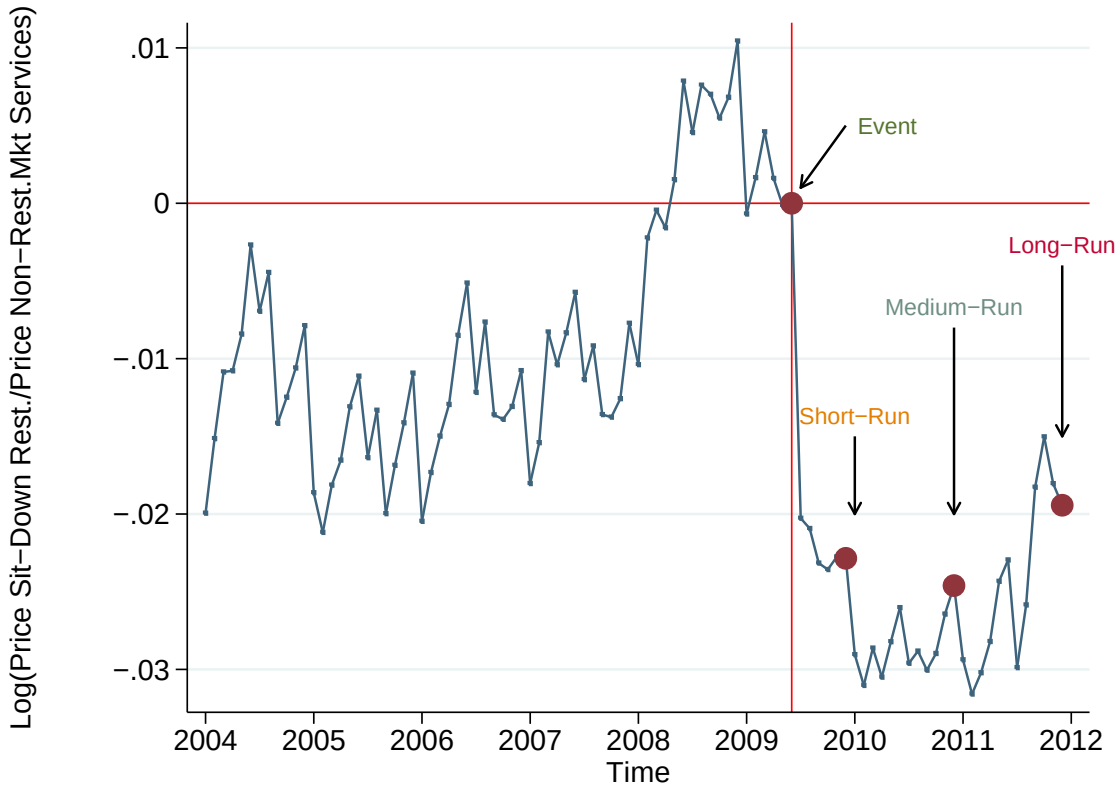
Notes: p_0 is the pre-reform price of the good, corresponding to a VAT tax rate of 19.6. $p_0 + \Delta\tau$ is the pre-reform price plus the change in the tax rate. If there is full pass-through to prices, then the welfare effect of the VAT decrease on consumers corresponds to the change in tax revenue (keeping quantities fixed), implying a 100 percent incidence on consumers.

Figure 1.4: Pass-Through of VAT on Prices



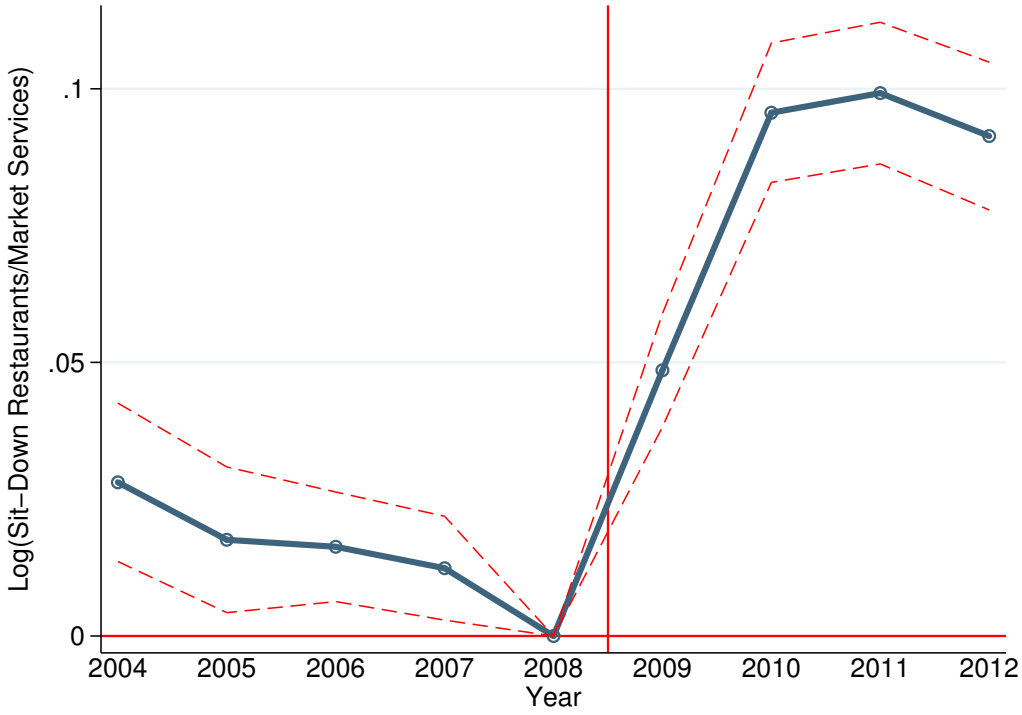
Notes: The price with full pass-through is computed assuming that its percent change is equal to the level difference in the VAT rate, which decreased from 19.6 to 5.5 percent.

Figure 1.5: Log-Difference in Prices: Sit-Down Restaurant Meals vs. Market Services



Source: INSEE and authors' computations on Eurostat data. The price of non-restaurant market services uses Eurostat price data and is computed as a weighted average (using EUROSTAT weights) of seasonally adjusted prices by four digits COICOP.

Figure 1.6: Value-Added per Employee: Sit-Down Restaurants vs. Market Services

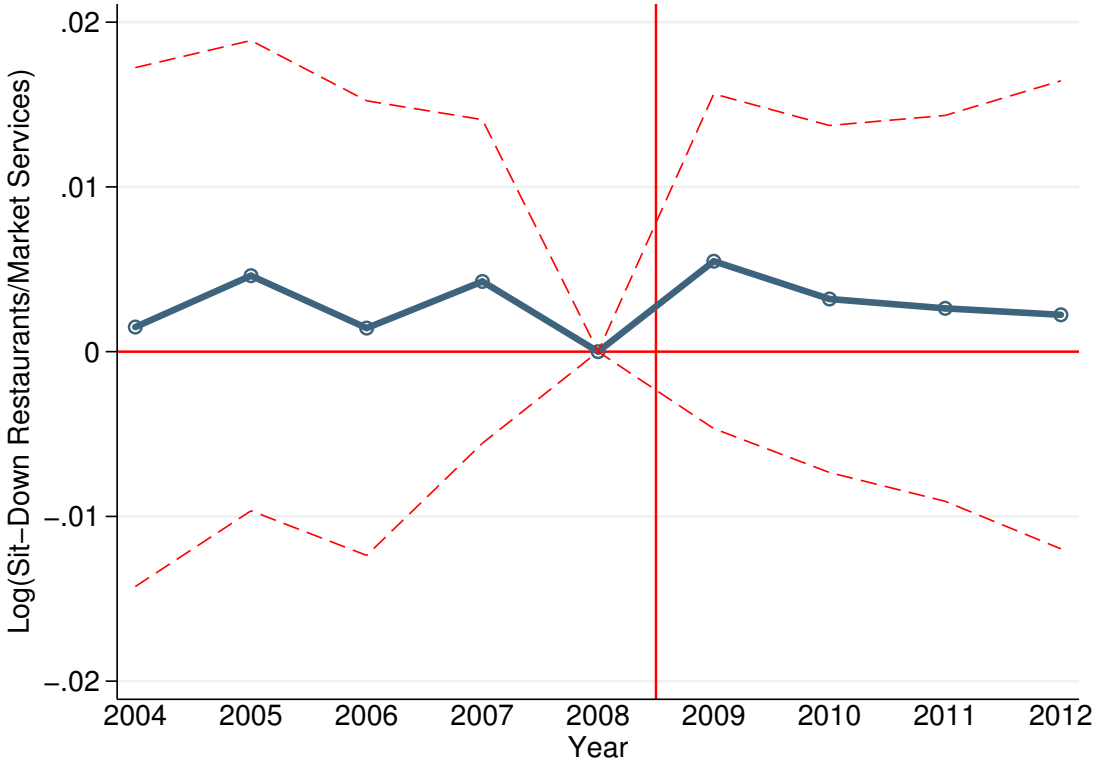


Notes: The figure shows event-time coefficients estimated using equation (1.10), which includes year and firm fixed effects. The treatment group includes all sit-down restaurants, while the control group includes all firms in non-restaurant market services. The dashed lines represent 95 percent confidence intervals.

Figure 1.7: Cost per Employee: Sit-Down Restaurants vs. Market Services

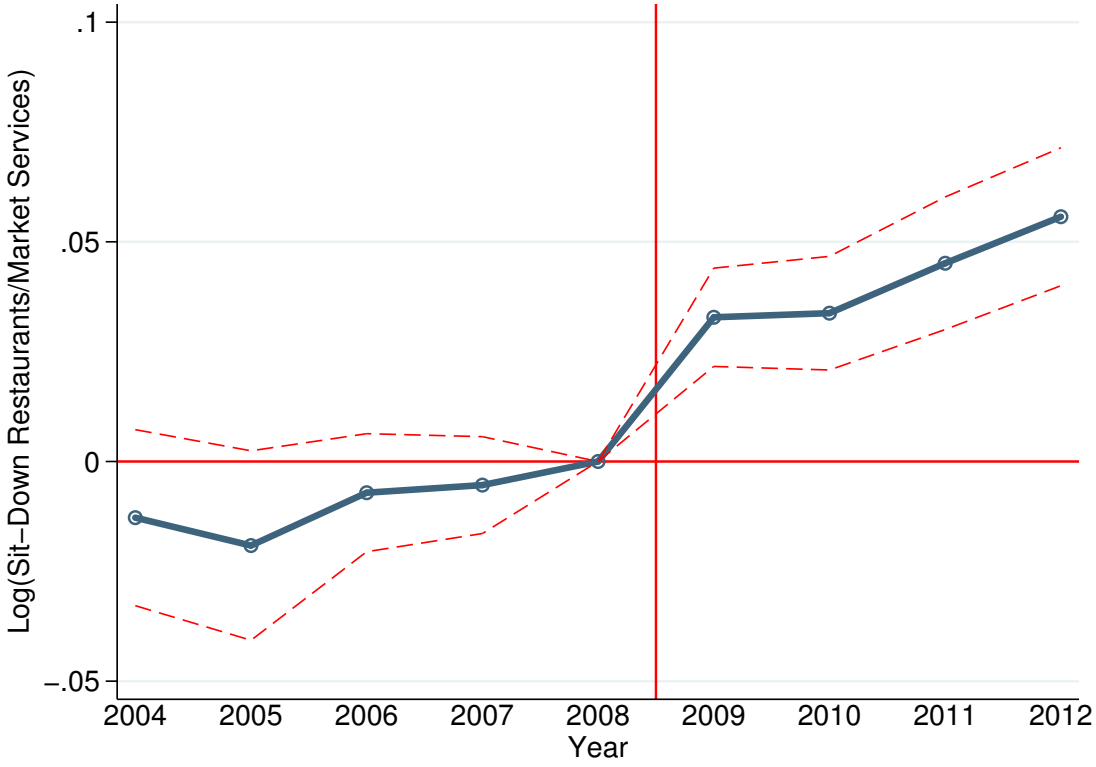
Notes: The figure shows event-time coefficients estimated using equation (1.10), which includes year and firm fixed effects. The treatment group includes all sit-down restaurants, while the control group includes all firms in non-restaurant market services. The dashed lines represent 95 percent confidence intervals.

Figure 1.8: Number of Employees per Firm: Sit-Down Restaurants vs. Market Services



Notes: The figure shows event-time coefficients estimated using equation (1.10), which includes year and firm fixed effects. The treatment group includes all sit-down restaurants, while the control group includes all firms in non-restaurant market services. The dashed lines represent 95 percent confidence intervals.

Figure 1.9: Cost of Materials per Employee: Sit-Down Restaurants vs. Market Services

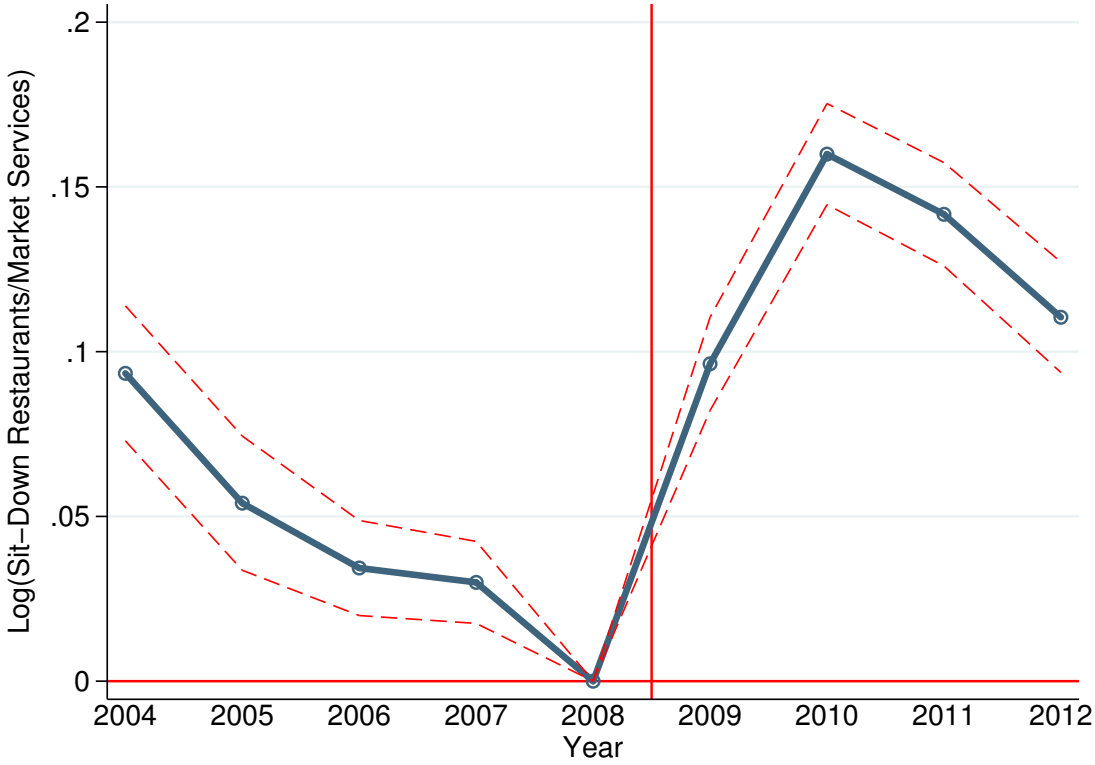


Notes: The figure shows event-time coefficients estimated using equation (1.10), which includes year and firm fixed effects. The treatment group includes all sit-down restaurants, while the control group includes all firms in non-restaurant market services. The dashed lines represent 95 percent confidence intervals.

Figure 1.10: Profit per Employee: Sit-Down Restaurants vs. Market Services

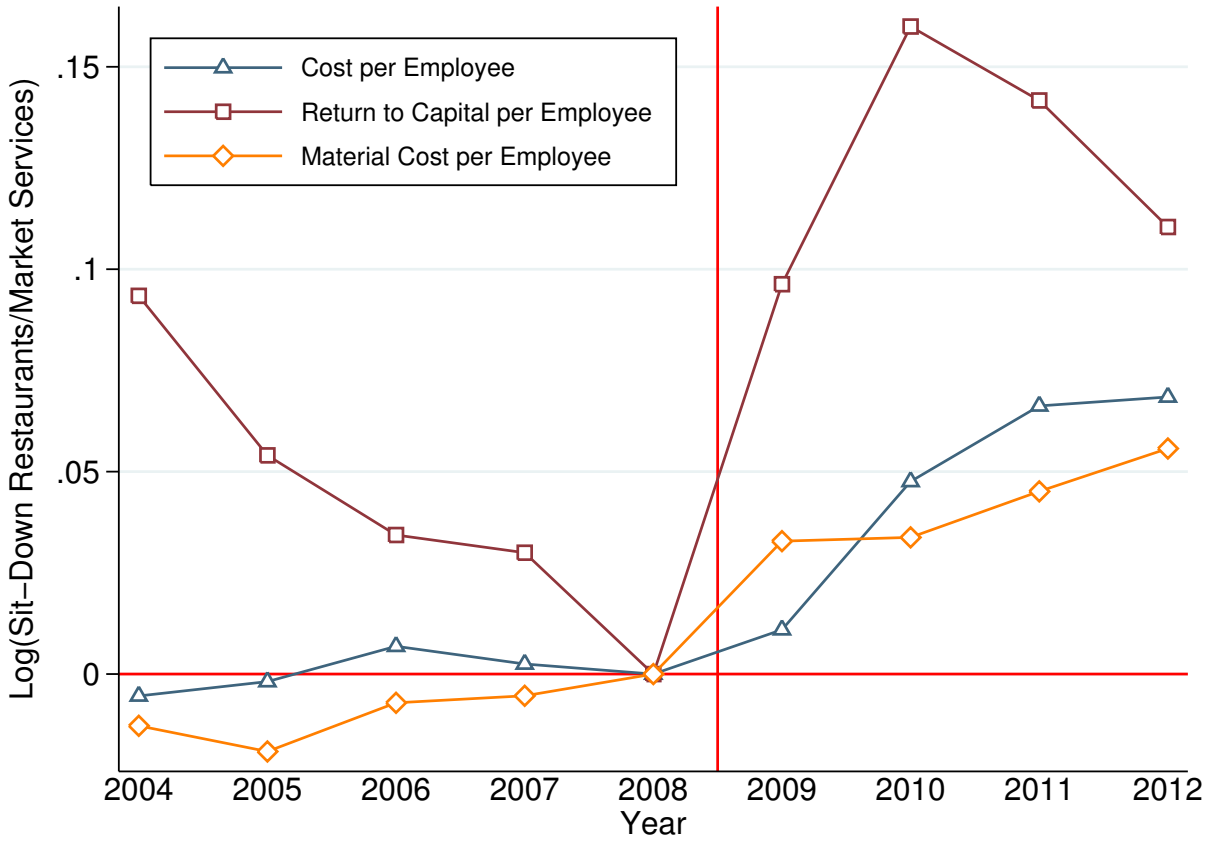
Notes: The figure shows event-time coefficients estimated using equation (1.10), which includes year and firm fixed effects. The treatment group includes all sit-down restaurants, while the control group includes all firms in non-restaurant market services. The dashed lines represent 95 percent confidence intervals.

Figure 1.11: Return to Capital per Employee: Sit-Down Restaurants vs. Market Services



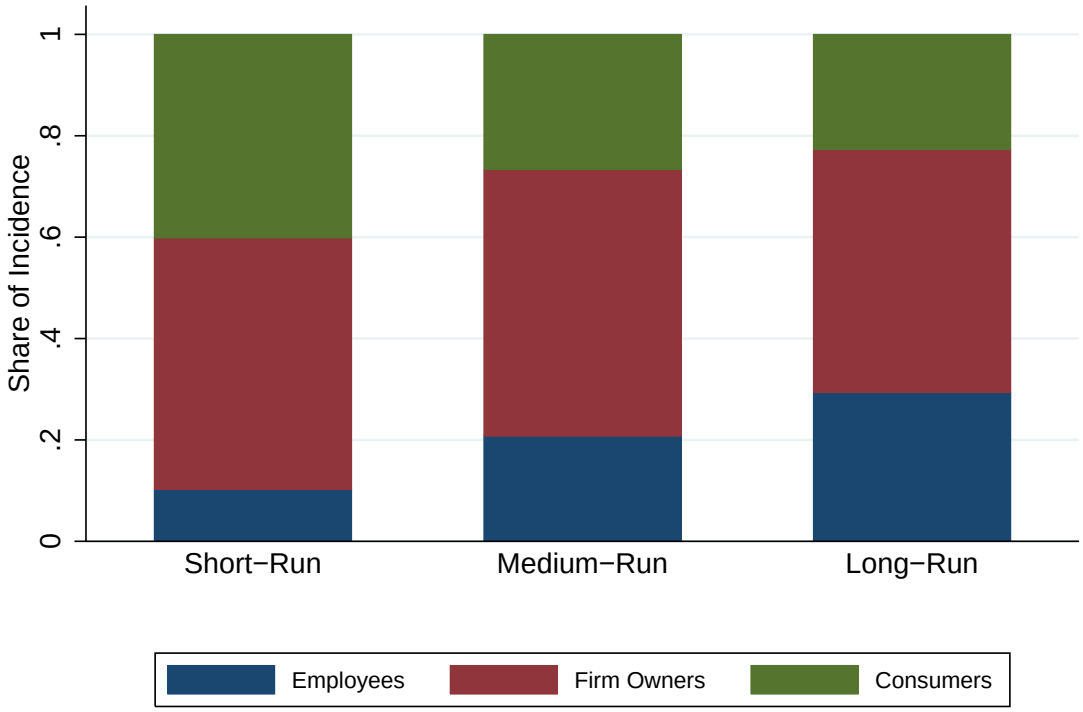
Notes: The figure shows event-time coefficients estimated using equation (1.10), which includes year and firm fixed effects. The treatment group includes all sit-down restaurants, while the control group includes all firms in non-restaurant market services. The dashed lines represent 95 percent confidence intervals.

Figure 1.12: Comparison of Estimated Effects



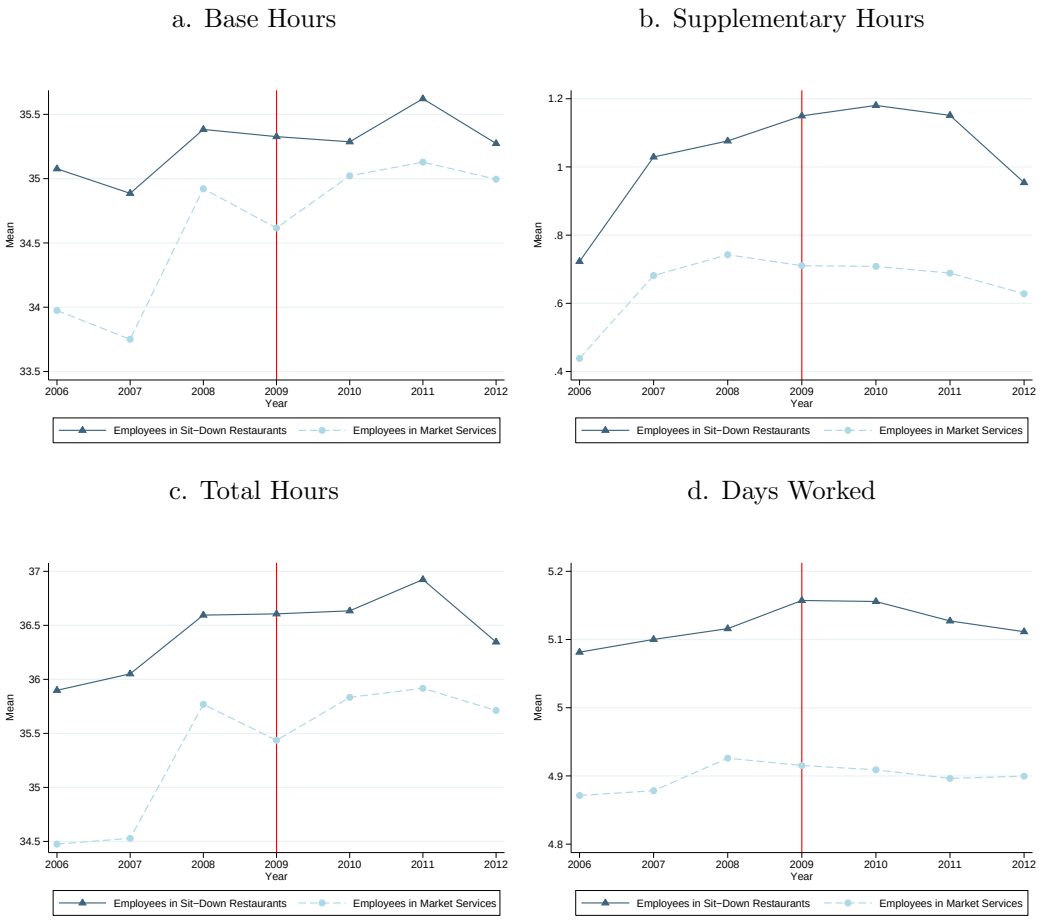
Notes: The figure shows event-time coefficients estimated using equation (1.10), which includes year and firm fixed effects. The treatment group includes all sit-down restaurants, while the control group includes all firms in non-restaurant market services.

Figure 1.13: Estimated Incidence of the VAT Reform



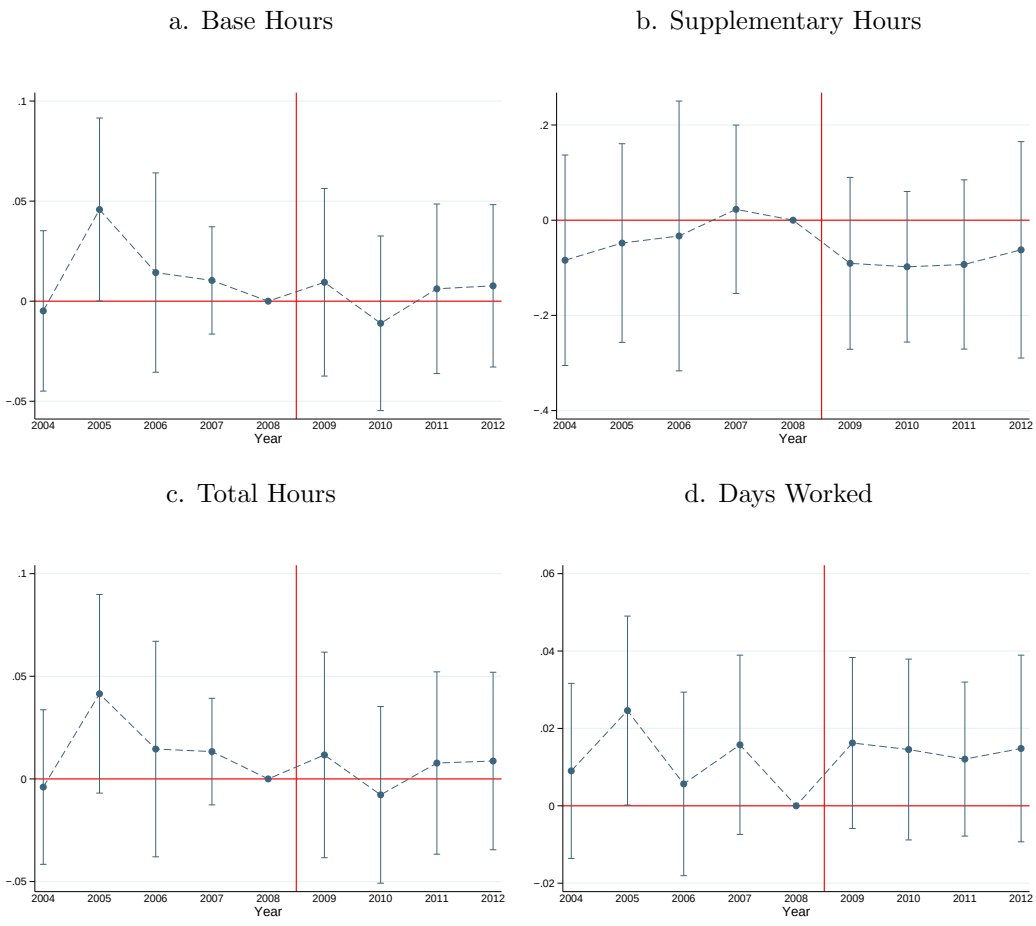
Notes: Incidence shares are computed using equation (1.8) with $d\ln c_x = 0$. The short-run denotes December 2009, the medium-run December 2010 and the long-run December 2011.

Figure 1.14: Hours Worked during Reference Week by Trimester: Unconditional Means



Notes: The treatment group includes employees of French sit-down restaurants, while the control group includes employees of non-restaurant market services. Averages are computed from the *Enquete Emploi en Continu* 2004-2012.

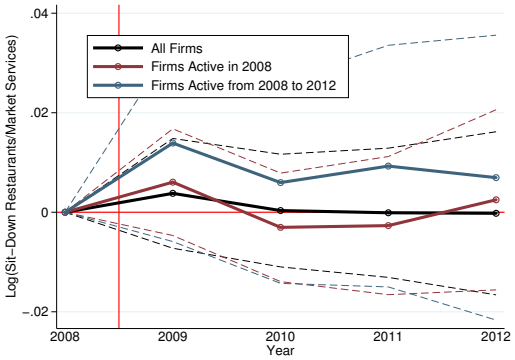
Figure 1.15: Hours Worked during Reference Week: Event-Time Estimates



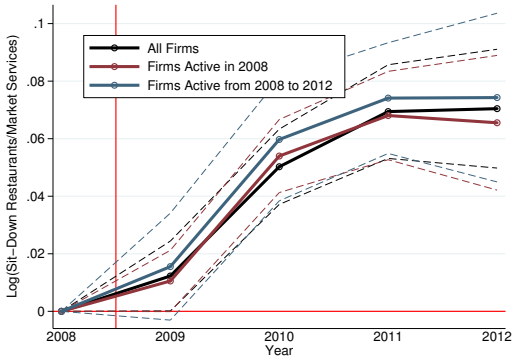
Notes: Event-style coefficients computed using equation (1.11). The treatment group includes employees of French sit-down restaurants, while the control group includes employees of non-restaurant market services. Averages are computed from the *Enquete Emploi en Continu* 2004-2012.

Figure 1.16: Firm Entry and Exit

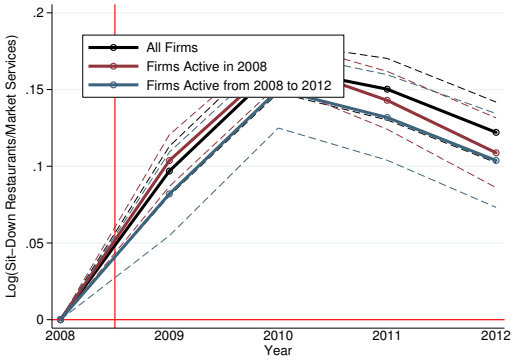
a. Number of Employees



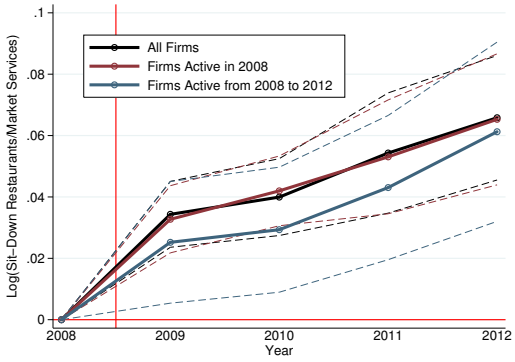
b. Cost per Employee



c. Return to Capital per Employee

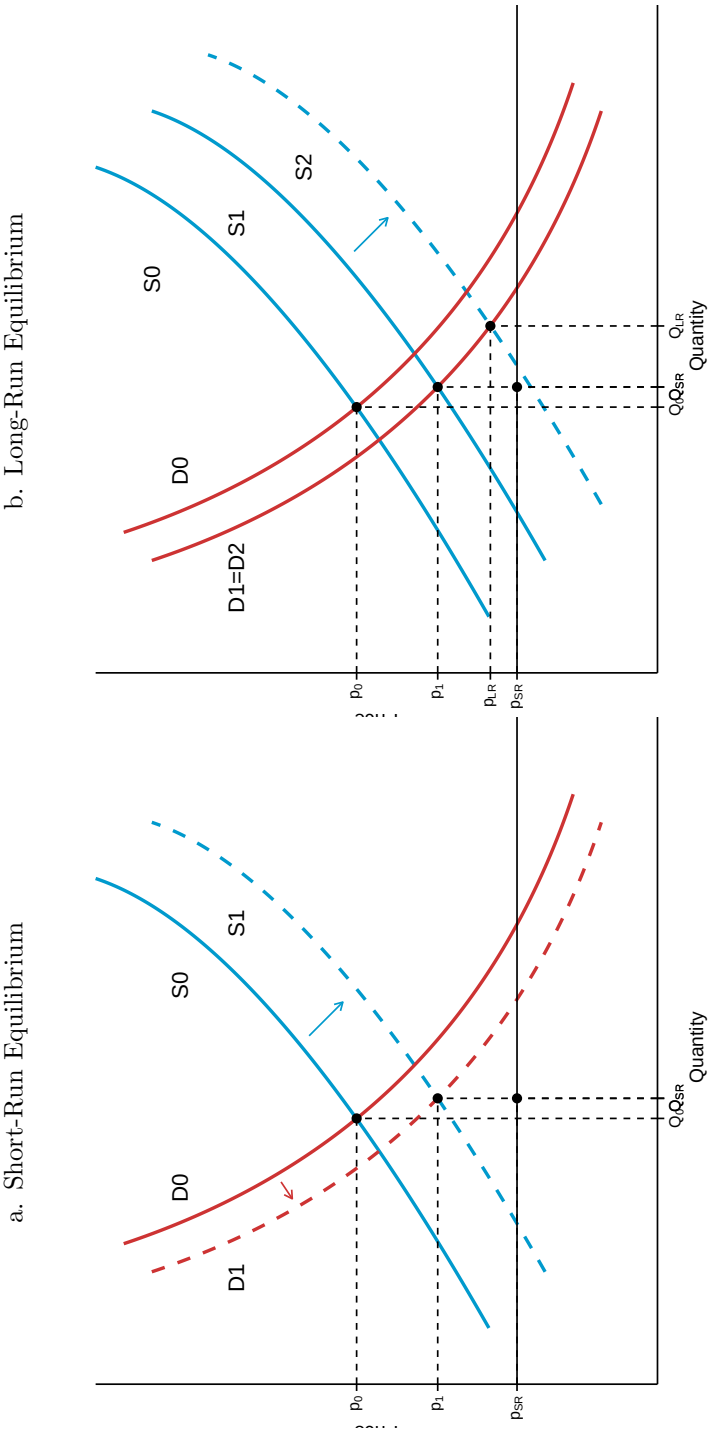


d. Cost of Materials per Employee



Notes: Figure shows coefficients estimated using equation (1.10) on the period 2008-2012. The treatment group includes sit-down restaurants, while the control group includes all firms in non-restaurant market services. The dashed lines represent 95 percent confidence intervals. *All firms* includes both firms which were active in 2008, and firms that entered the market later on. *Firms active in 2008* only includes firms which were active in 2008. Finally, *Firms active from 2008 to 2012* includes firms which were active in 2008 and did not exit the market in period 2008-2012.

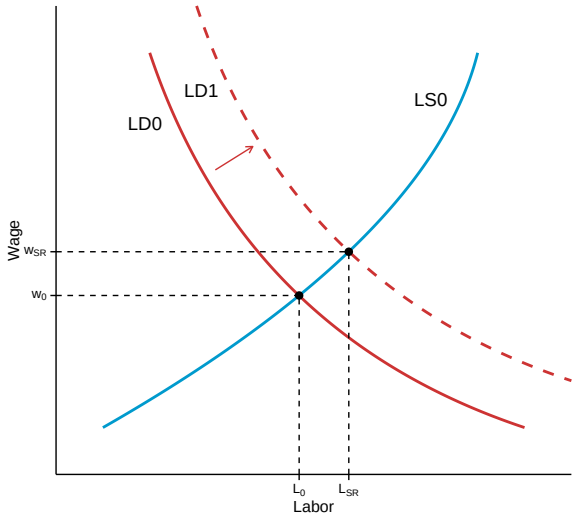
Figure 1.17: Goods Market Equilibrium after the Tax Change



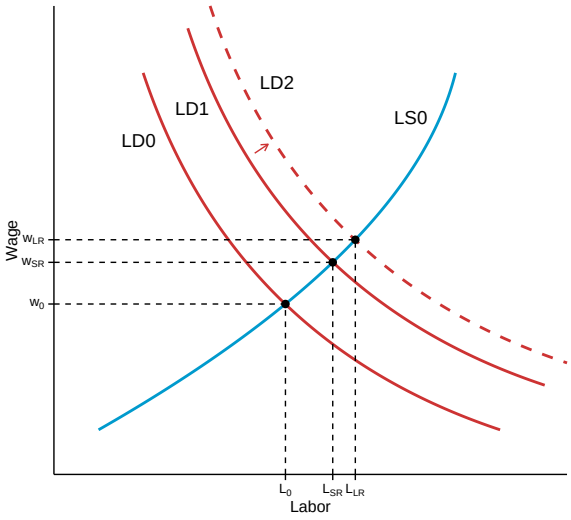
Notes: The figures show the short-run and long-run equilibrium in the market for the taxed good after a decrease in the VAT rate. In the short-run, the price charged by producers of the taxed good is set to p_{SR} by the government and is below the competitive market equilibrium price. In the long-run the market for the taxed good behaves as a perfectly competitive market.

Figure 1.18: Capital and Labor Markets in the Short-Run and Long-Run

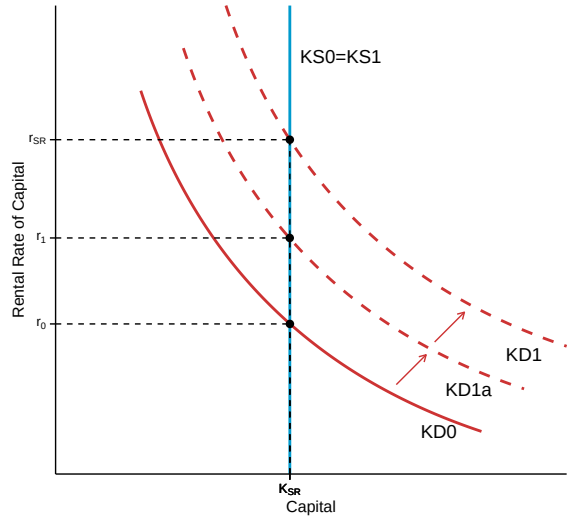
a. Labor Market: Short-Run



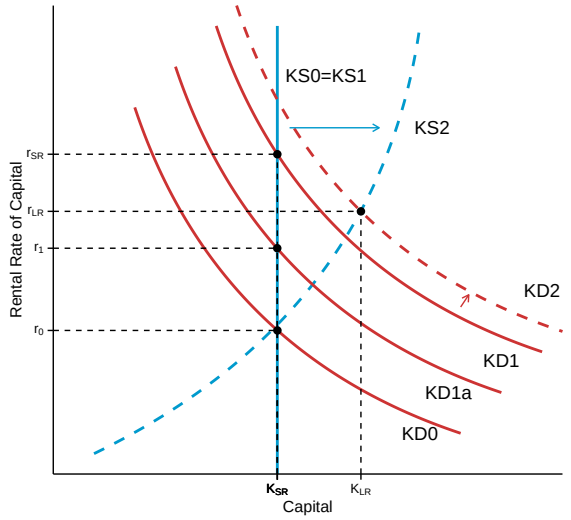
b. Labor Market: Long-Run



c. Capital Market: Short-Run



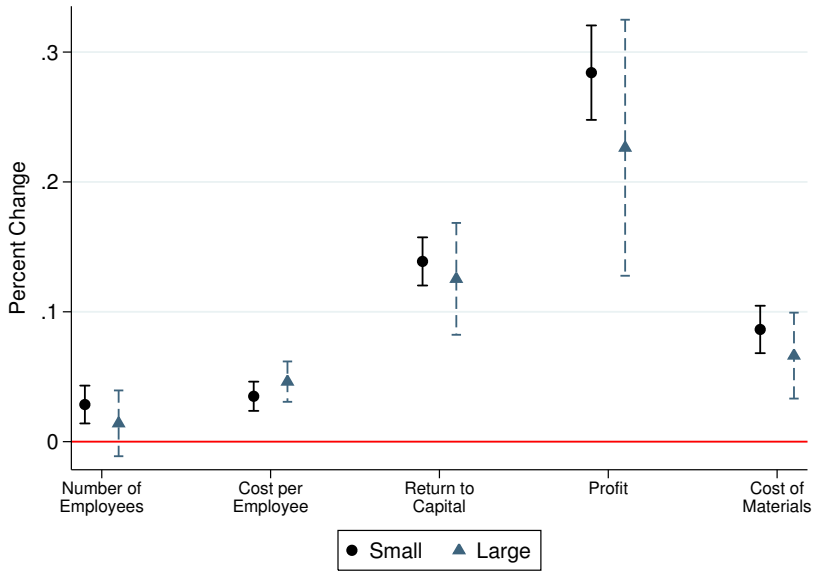
d. Capital Market: Long-Run



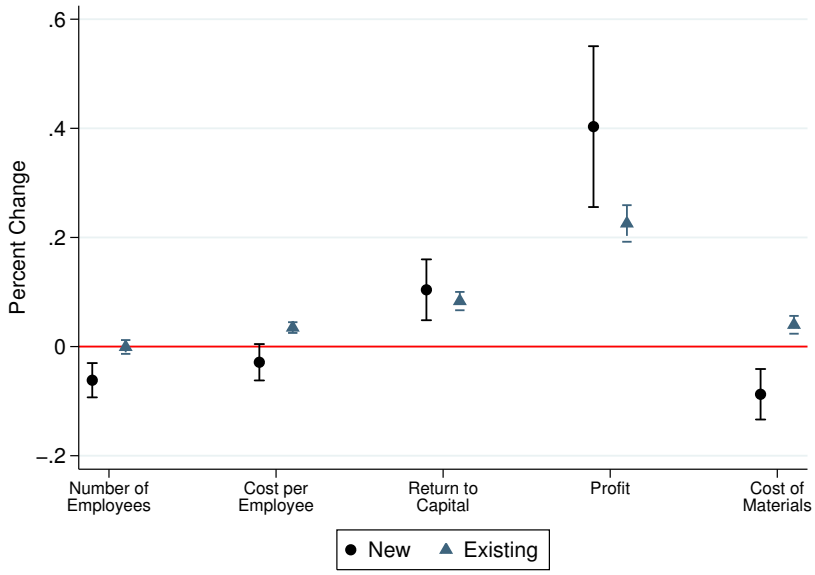
Notes: The figures show the short-run and long-run effect of a VAT cut on the labor and capital markets.

Figure 1.19: Effect of the Reform by Firm Size and Tenure

a. Small vs. Large Firms



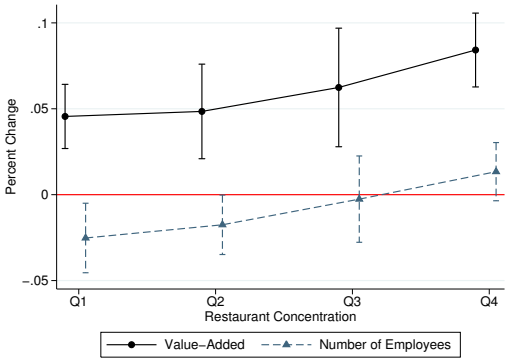
b. New vs. Existing Firms



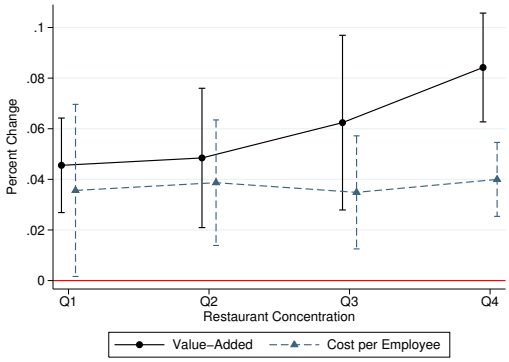
Notes: The figures report coefficients estimated using equation (1.9) on samples of firms with different sizes and tenures in the pre-reform period. Small firms are those with less than 10 employees, while new firms have existed for a year or less.

Figure 1.20: Effect by Market Concentration of Sit-Down Restaurants

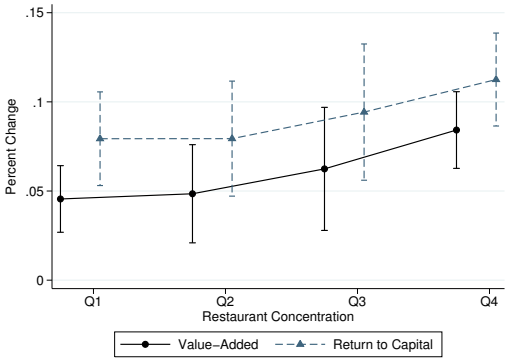
a. Number of Employees



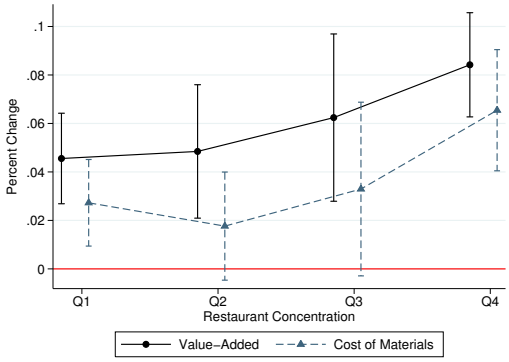
b. Cost per Employee



c. Return to Capital

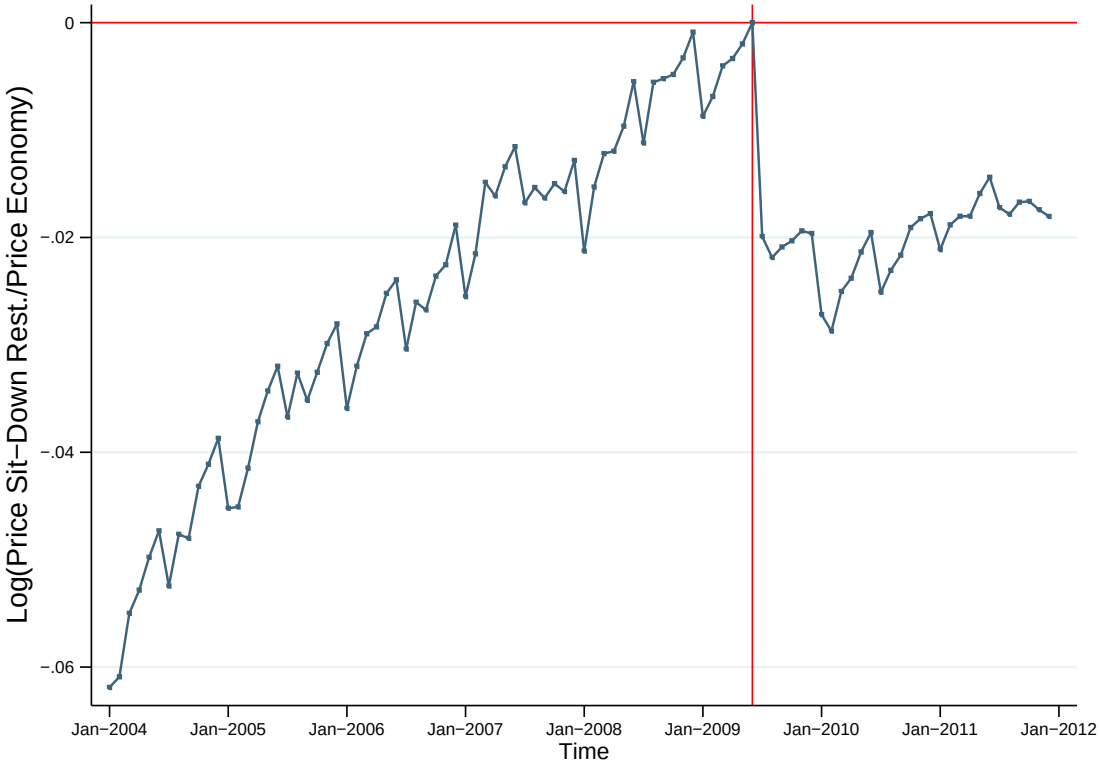


d. Cost of Materials



Notes: Market concentration is defined as number of restaurants/total population by *département* in 2008. The figures show coefficients estimated using equation (1.9), which includes year and firm fixed effects. The treatment group includes all sit-down restaurants, while the control group includes all firms in non-restaurant market services. 95 percent confidence interval are displayed for each data point.

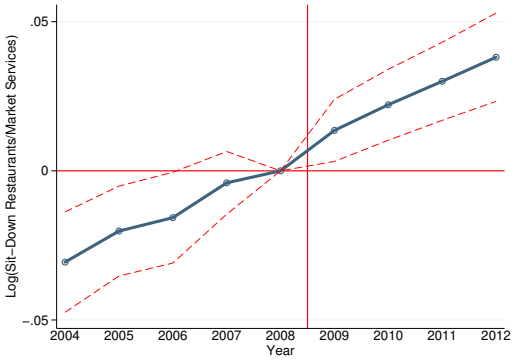
Figure 1.21: Prices Event-Time Estimates: Sit-Down Restaurants vs. Non-Restaurant Firms



Source: INSEE and authors' computations on Eurostat data.

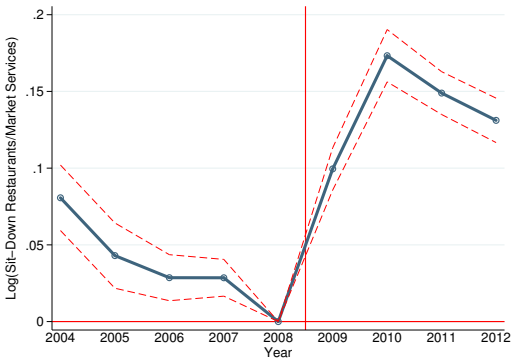
Figure 1.22: Event-Time Coefficients: Sit-Down Restaurants vs. Small Firms

a. Number of Employees per Firm

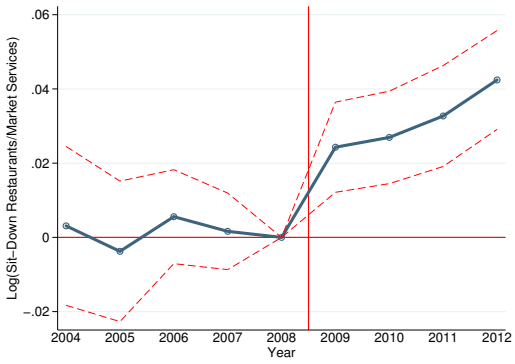


b. Cost per Employee

c. Return to Capital per Employee



d. Cost of Materials per Employee



Notes: The figures show event-time coefficients estimated using equation (1.10), which includes year and firm fixed effects. The treatment group includes all sit-down restaurants, while the control group includes all non-restaurant small firms. Value-added is computed as turnover minus material goods purchased. The dashed lines represent 95 percent confidence intervals.

Table 1.1: Outcome Variables: Sit-Down Restaurants vs. Control Groups for 2004-2012

	Treatment Group	Control Groups	
	Sit-Down Restaurants	Non-Restaurant Market Services	Non-Restaurant Small Firms
	(1)	(2)	(3)
Profit/Loss before tax	16,165	91,489	16,314
	(7,233)	(17,439)	(8,448)
Operating revenue (Turnover)	438,169	2,871,716	382,476
	(269,187)	(494,925)	(281,683)
Sales	426,683	2,798,209	374,341
	(261,938)	(482,643)	(275,886)
Number of employees	5.510	11.37	3.569
	(4)	(3)	(2)
Costs of employees	168,813	497,571	129,637
	(99,214)	(139,562)	(93,300)
Material costs	127,173	2,208,138	147,482
	(81,714)	(206,779)	(80,774)
Observations	147,958	1,482,447	1,737,234

Notes: All amounts are in real terms (2012 euros). Mean values with median values in parenthesis.

Table 1.2: Mean Impact Estimates

	Number of Employees	Value-Added per Employee	Cost per Employee
	(1)	(2)	(3)
After × Sit-Down Restaurant	0.0012	0.069***	0.039***
	(0.0064)	(0.0053)	(0.0051)
R^2	0.95	0.84	0.81
Observations	994,733	994,733	994,733
	Return to Capital per Employee	Profit/Loss per Employee	Cost of Material per Employee
	(4)	(5)	(6)
After × Sit-Down Restaurant	0.099***	0.18***	0.044***
	(0.0066)	(0.013)	(0.0053)
R^2	0.82	0.75	0.95
Observations	994,733	710,311	994,733
Year FE	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes

Notes: Coefficients on the interaction variable are average percent changes of the outcome variable estimated using (1.9) on the period 2004-2011. The control group used is firms operating in non-restaurant market services. Standard errors are clustered by *département* *** p<0.01.

Table 1.3: Incidence of the VAT Reform

	Panel A: Short-Run Incidence			
	Estimated Percent Change	Sales Share	Weighted Percent Change	Share of Incidence
Cost per Employee	0.015	39.3	0.006	**10.3**
Cost of Capital per Employee	0.098	29.4	0.029	**50.0**
Prices	-0.023			**39.7**
	Panel B: Medium-Run Incidence			
	Estimated Percent Change	Sales Share	Weighted Percent Change	Share of Incidence
Cost per Employee	0.047	39.3	0.018	**20.2**
Cost of Capital per Employee	0.16	29.4	0.047	**52.8**
Prices	-0.024			**27.0**
	Panel C: Long-Run Incidence			
	Estimated Percent Change	Sales Share	Weighted Percent Change	Share of Incidence
Cost per Employee	0.064	39.3	0.025	**29.4**
Cost of Capital per Employee	0.14	29.4	0.041	**48.2**
Prices	-0.019			**22.4**

Notes: Percent changes for the cost per employee and the return to capital per employee are estimated using the even-type coefficients computed from equation (1.10). The percent change in the return to capital per employee is based on the estimated event-type coefficients and the counterfactual log-difference shown in figure A.11. The change in prices is computed from the log-difference in prices between sit-down restaurants and non-restaurant market services. Sales shares reported in the table are firm averages in the pre-reform year 2008. Incidence estimates are computed using equation (1.8).

Table 1.4: Price Elasticities to the Tax

	Short-Run: 6 Months after Reform	Medium-Run: 18 Months after Reform	Long-Run: 30 Months after Reform
Price: $\frac{\Delta p}{p}$	-0.0229	-0.0237	-0.0194
Net-of-Tax Rate: $\frac{\Delta(1-\tau)}{1-\tau}$	0.1754	0.1754	0.1754
Price Elasticity: $\frac{\Delta p/p}{\Delta(1-\tau)/(1-\tau)}$	-0.1306	-0.1351	-0.1106

Notes: Price percent changes are computed by comparing the difference between the price of sit-down restaurant meals and the price of non-restaurant market services relative to one month prior to the reform. Price series are seasonally adjusted using monthly dummies.

Table 1.5: Mean Tax Elasticities for Selected Firm Balance Sheet Items

	Number of Employees (1)	Value-Added per Employee (2)	Cost per Employee (3)
Log(1-τ)	0.0076	0.43***	0.24***
	(0.034)	(0.028)	(0.027)
Observations	994,733	994,733	994,733
	Return to Capital per Employee (4)	Profit/Loss per Employee (5)	Cost of Material per Employee (6)
Log(1-τ)	0.61***	1.09***	0.27***
	(0.035)	(0.066)	(0.028)
Observations	994,733	710,311	994,733
Year FE	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes

Notes: The annual elasticities reported in the table are estimated using a two stage least squares procedure and using *After* $\times$ *Sit-Down Restaurants* as an instrument for $log(1-\tau)$. The pre-reform period covers 2004-2008 while the post-reform period includes 2009-2011. *** p<0.01.

Table 1.6: Hours Worked: Sit-Down Restaurants vs. Non-Restaurant Market Services

		Panel A: Base Hours		
After × Sit-Down Restaurants	-0.019	-0.016	-0.018	0.0024
	(0.015)	(0.016)	(0.016)	(0.015)
After	0.021***	0.039***	0.036***	-0.0078
	(0.0032)	(0.0059)	(0.0052)	(0.0066)
Sit-Down Restaurants	0.025***	0.022**	0.022**	0.043***
	(0.0078)	(0.0079)	(0.0080)	(0.0077)
R^2	0.00037	0.0030	0.012	0.13
Observations	284,475	284,475	284,475	214,949
		Panel B: Total Hours		
After × Sit-Down Restaurants	-0.017	-0.014	-0.016	0.0038
	(0.015)	(0.016)	(0.016)	(0.016)
After	0.025***	0.048***	0.045***	0.00087
	(0.0031)	(0.0055)	(0.0052)	(0.0058)
Sit-Down Restaurants	0.034***	0.031***	0.031***	0.051***
	(0.0092)	(0.0093)	(0.0094)	(0.0087)
R^2	0.00057	0.0039	0.011	0.13
Observations	284,602	284,602	284,602	215,052
		Panel C: Days Worked		
After × Sit-Down Restaurants	0.015	0.015	0.016	0.015
	(0.011)	(0.010)	(0.010)	(0.011)
After	-0.0037	-0.0034	0.0029***	0.0058***
	(0.0024)	(0.0024)	(0.00037)	(0.00044)
Sit-Down Restaurants	0.027**	0.027**	0.027**	0.032***
	(0.010)	(0.010)	(0.0099)	(0.010)
Observations	93,538	93,538	93,538	79,264
Region FE	No	Yes	Yes	Yes
Region × After	No	No	Yes	Yes
Individual Characteristics	No	No	No	Yes

Notes: Reported coefficients are percent changes in hours worked estimated using year and region fixed effects. Pre-treatment period is 2004-2008, while the post-treatment period includes 2009-2012. * p<0.10, ** p<0.05, *** p<0.01

Table 1.7: Effect of Hiring Subsidy Cut: Control Group = Market Services

	Number of Employees	Value-Added per Employee	Cost per Employee
	(1)	(2)	(3)
After × SC	-0.017***	0.030***	0.010
	(0.0060)	(0.0053)	(0.0064)
R^2	0.95	0.84	0.81
Observations	1,033,231	1,033,231	1,033,231
	Return to Capital per Employee	Profit/Loss per Employee	Cost of Materials per Employee
	(4)	(5)	(6)
After × SC	0.058***	0.081***	-0.014
	(0.0069)	(0.021)	(0.016)
R^2	0.82	0.74	0.95
Observations	1,033,231	734,290	1,033,231
Year FE	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes

Notes: Reported coefficients are percent changes in hotels (which are affected by the subsidy cut) relative to firms in non-restaurant market services. The pre-treatment period is 2004-2008, while the post-treatment period includes 2009-2011. * p<0.10, ** p<0.05, *** p<0.01

Table 1.8: Controlling for the Local Unemployment Rate

	Number of Employees (1)	Value-Added per Employee (2)	Cost per Employee (3)
After ×	0.0025	0.063***	0.040***
Sit-Down Restaurant	(0.0082)	(0.0070)	(0.0057)
URate ×	-0.016***	0.0081**	-0.0055
Sit-Down Restaurants	(0.0048)	(0.0040)	(0.0037)
URate ×	-0.014***	0.00068	-0.0035
Market Services	(0.0023)	(0.0056)	(0.0044)
R^2	0.95	0.84	0.81
Observations	994,733	994,733	994,733
	Return to Capital per Employee (4)	Profit/Loss per Employee (5)	Cost of Materials per Employee (6)
After ×	0.077***	0.18***	0.056***
Sit-Down Restaurant	(0.012)	(0.014)	(0.0082)
URate ×	0.024***	-0.017*	0.0077
Sit-Down Restaurants	(0.0040)	(0.0092)	(0.0056)
URate ×	-0.0023	-0.014**	0.022**
Market Services	(0.0054)	(0.0062)	(0.010)
R^2	0.82	0.75	0.95
Observations	994,733	710,311	994733
Year FE	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes

Notes: URate_{dt} is *departement* unemployment rate in year *t*. All specifications include year and firm fixed effects. Standard errors are clustered by *département*. * p<0.10, ** p<0.05, *** p<0.01

Table 1.9: Employment Weighted Mean Impact Estimates

	Value-Added per Employee (1)	Cost per Employee (2)	Return to Capital per Employee (3)	Profit/Loss per Employee (4)	Cost of Materials per Employee (5)
After × Sit-Down Restaurant	0.043***	0.022***	0.074***	0.18***	0.015
	(0.0072)	(0.0075)	(0.0079)	(0.018)	(0.025)
R^2	0.92	0.90	0.92	0.79	0.98
Observations	994,733	994,733	994,733	710,311	994,733
Year FE	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes

Notes: Coefficients on the interaction variable are weighted average percent changes of the outcome variable estimated using data from 2004 to 2011, using number of employees as a weight. The control group used is firms operating in non-restaurant market services. Standard errors are clustered by *département*. * p<0.10, ** p<0.05, *** p<0.01.

Table 1.10: Robustness Check: Sit-Down Restaurants vs. Small Firms

	Number of Employees	Value-Added per Employee	Cost per Employee
	(1)	(2)	(3)
After × Sit-Down Restaurant	0.031***	0.069***	0.038***
	(0.0078)	(0.0045)	(0.0047)
R^2	0.88	0.79	0.79
Observations	1,229,883	1,229,883	1,229,883
	Return to Capital per Employee	Profit/Loss per Employee	Cost of Materials per Employee
	(4)	(5)	(6)
After × Sit-Down Restaurant	0.11***	0.20***	0.026***
	(0.0058)	(0.014)	(0.0044)
R^2	0.77	0.70	0.93
Observations	1,229,883	837,528	1,229,883
Year FE	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes

Notes: Coefficients on the interaction variable are average percent changes of the outcome variable estimated using (1.9) on the period 2004-2011. The control group used is non-restaurant small firms. Standard errors are clustered by *département*. * p<0.10, ** p<0.05, *** p<0.01

Table 1.11: Incidence of the VAT Reform if Control Group = Small Firms

	Panel A: Short-Run Incidence			
	Estimated Percent Change	Sales Share	Weighted Percent Change	Share of Incidence
Cost per Employee	0.015	39.3	0.006	**10.3**
Cost of Capital per Employee	0.10	29.4	0.029	**50.0**
Prices	-0.023			**39.7**
	Panel B: Medium-Run Incidence			
	Estimated Percent Change	Sales Share	Weighted Percent Change	Share of Incidence
Cost per Employee	0.049	39.3	0.019	**20.4**
Cost of Capital per Employee	0.18	29.4	0.050	**53.8**
Prices	-0.024			**25.8**
	Panel C: Long-Run Incidence			
	Estimated Percent Change	Sales Share	Weighted Percent Change	Share of Incidence
Cost per Employee	0.064	39.3	0.025	**28.4**
Cost of Capital per Employee	0.15	29.4	0.044	**50.0**
Prices	-0.019			**21.6**

Notes: Percent changes for the cost per employee and the return to capital per employee are estimated using the even-type coefficients computed from equation (1.10). The change in prices is computed from the log-difference in prices between sit-down restaurants and the price in the economy excluding restaurants. Sales shares reported in the table are firm averages in the pre-reform year 2008. Incidence estimates are computed using equation (1.8).

Chapter 2

Do Prices Respond Differently to Increases and Decreases In Consumption Taxes?

with Youssef Benzarti

2.1 Introduction

Standard economic models predict that prices respond symmetrically to changes in consumption taxes. In this paper, we question this assumption by empirically showing that there is a higher pass-through to prices for tax increases than for tax decreases.

We document this asymmetry using variation in the value-added tax (VAT) and in prices across commodities and European countries in the period 1996-2015. Using monthly observations, we estimate how prices for a given set of commodities change in relation to the VAT applied to such commodities, while also accounting for country-specific inflation and for seasonal effects. We find a systematic asymmetry in the response of prices to VAT variations: prices increase following a VAT increase and decrease by less following a VAT decrease.[1] We explain this phenomenon with a fairness argument, as in Kahneman, Knetsch, and Thaler 1986. The main argument we propose can be summarized as follows: consumers find it fair for prices to increase when firms are protecting their profits from VAT increases, but do not expect firms to decrease profits following VAT decreases.

[1]Our work uses the same sources of data as in Benedek, De Mooij, and Wingender 2015, with a few differences. First, we consider a larger set of commodities, countries and years. Secondly, while the focus of Benedek, De Mooij, and Wingender 2015 is mainly on estimating the pass-through of VAT taxes, our interest is primarily on the asymmetric pass-through of VAT changes. Importantly, and though we use both a different empirical model and sample, we find an asymmetry in the pass-through of VAT taxes while Benedek, De Mooij, and Wingender 2015 does not.

This paper contributes to the large public finance literature on tax incidence.[2] Our study is the first to provide systematic evidence on the asymmetric pass-through of consumption taxes and shows that prices systematically respond more to increases in the VAT rate relative to decreases in the VAT rate. Clément Carbonnier 2005 is the most related paper to ours. The author documents asymmetric responses in prices that depend on the particular industries considered, and explains the findings with market structure.[3] The study finds that in some industries prices react more strongly to VAT increases, while in others the effect is stronger for VAT decreases. Our study goes beyond two limitations of Clément Carbonnier 2005. First, our work considers the entire set of commodities sold in a country, rather than a subset, while Clément Carbonnier 2005 only considers eleven commodities. Considering the full set of commodities turns out to be important because it shows systematically that prices respond significantly less to VAT decreases compared to VAT increases, which differs from Clément Carbonnier 2005 in that he finds that the direction of the asymmetry varies by industry. Second, we consider all VAT changes across countries over a period of 20 years, with tax changes being as large as 15 percentage points. In contrast, the evidence provided in Clément Carbonnier 2005 uses variation in VAT rates that is significantly smaller: 2 percentage point increases and 1 percentage point decreases. In addition, he considers VAT decreases that are smaller than VAT increases, which makes it more difficult to make a general statement about the asymmetric price response.

This paper also builds on a growing body of literature in industrial organization, which provides alternative explanations for the asymmetric pass-through of firm's costs to prices faced by consumers.[4] Ball and Mankiw 1994 for instance explains asymmetric price reactions with menu costs. Alternatively, Borenstein, Cameron, and Gilbert 1997 show that gasoline prices respond more to cost increases than to cost decreases, and argue that the asymmetry is partly due to the market power of retailers in the gasoline industry. Additional studies, which are described extensively in Politi and Mattos 2011, explain the asymmetry with the effect of consumer search costs (Benabou and Gertner 1993, Lewis 2011and Yang and Ye 2008) and inventory costs (A. S. Blinder 1982). Our study does not discard these alternative mechanisms, which do nevertheless seem sector-specific rather than applicable to all areas of the economy.

Though changes in VAT rates are in principle comparable to changes in firms' costs, they differ in many respects.[5] For example, tax variations are likely more salient than changing costs as they usually stem from largely discussed and well-publicized reforms. In

[2]See Fullerton and Metcalf 2002 for a survey of this literature.

[3]Politi and Mattos 2011 is another paper that considers asymmetric responses of prices to VAT reforms. It suffers from the same shortcomings as Clément Carbonnier 2005, that is a small sample of commodities and small consumption tax variations. In addition, the study uses a difference-in-differences strategy but does not show evidence on pre-reforms parallel trends.

[4]See J. Meyer and Cramon-Taubadel 2004 for a survey of the literature.

[5]The public finance literature rarely mentions costs asymmetries as a rationale for considering asymmetries in the pass-through of taxes. See for example the tax incidence chapter by Fullerton and Metcalf 2002 in the *Public Economics Handbook*.

addition, the statutory incidence of costs usually falls on firms, while the statutory incidence of consumption taxes falls on consumers.[6] Finally, taxes are subject to evasion but costs are not, which also affects incidence estimates as shown in Kopczuk et al. 2013.

The findings of this paper are relevant for two main reasons. First, our evidence warns that if the pass-through of taxes is asymmetric, then the estimated incidence relies heavily on whether one is considering tax increases or decreases. This needs to be considered in future studies on tax incidence. Secondly, our study shows that although tax increases may curb consumer behavior, consumption tax cuts are not fully passed through to consumers, particularly when when VAT rates are cut. This has significant policy implications, primarily that policy makers should not use consumption tax cuts to achieve redistribution to consumers. At the same time, it also raises the question of whether VAT cuts benefit economic growth, at least in the short-run.[7] As prices do not generally adjust downwards in the case of tax cuts, the reforms might simply increase firms' profits. Overall, while it is possible that government interventions to cut VAT rates increase aggregate demand by increasing aggregate income in the economy, the evidence presented in this work shows that consumption tax cuts do not usually benefit their targeted audience: consumers.

2.2 Institutional Background

The VAT is a consumption tax which applies to most goods and services produced in OECD countries.[8] Differently from sales taxes, which are collected at the time of the final sale to consumers, the VAT is imposed on each stage of the supply chain and is ultimately charged to consumers in the full amount.[9]

The VAT is applied to the value-added of goods and services sold: that is, the amount of value that is added to a given commodity produced at each stage of production. It is computed as the difference between the value of sales and the value-added generated in earlier stages of production, and is designed such that the tax paid on the value of intermediate goods purchased is deducted from the tax due on the value of goods sold.[10] Final consumers,

[6]There is a growing body of literature in public finance documenting the mismatch between the statutory incidence and the economic incidence of taxes. See for example Kopczuk et al. 2013 and Tazhitdinova 2015.

[7]There are many examples of differing opinion, for instance L. Kotlikoff and Leamer 2008, which argues that the government should have implemented a temporary reduction in sales taxes to stimulate demand during the Great Recession, believing that a decrease in sales taxes would result in a decrease in prices and therefore boost demand. This paper shows that such policy likely would not have been effective.

[8]The VAT is also called Goods and Services Tax (GST) in some non-European countries including Australia, Canada and New Zealand.

[9]Another main difference with sales taxes is that the VAT is generally included in the price of the goods and services purchased and are in many cases not visible to the consumer, while sales taxes are stated separately on the invoice.

[10]While businesses do not pay the VAT on inputs purchased, this is not true in the case of sales taxes. Firms have to remit sales taxes collected on the value of final goods sold and cannot deduct taxes on the purchase of intermediate goods.

which are the last component of the chain, cannot claim any tax credit and therefore pay the tax on the final value of goods purchased.

Over 150 countries implement the VAT, including all OECD countries with the exception of the United States.[11] Total revenue from VAT represents 18.7 percent of the total tax revenue in the OECD countries and account for 7.5 percent of GDP.[12] For the European Union as a whole, the VAT is the second largest source of tax revenue after labor income taxes, and amounts to around 30 percent of total tax revenue.

Member countries of the European Union generally have several VAT rates in place, including a standard rate that applies to the majority of commodities. Reduced VAT rates mainly apply to basic necessities such as food, heating and passenger transport, while tax exemptions can be applied to merit goods like health care and education. In general, such reduced rates are used to achieve distributional goals, and apply to goods that are expected to be consumed by lower income households. Reduced rates may also be used as an incentive for eco-friendly practices, or for financial services for which it is difficult to determine the VAT taxable base. Charitable donations are generally exempt from the VAT, in which case the VAT paid for intermediate commodities cannot be deducted.

2.3 Data

Price Data The price information used in this study comes from *Eurostat's Harmonised Indices of Consumer Prices* (HICP), which measures the price of goods and services purchased by households in the European Union. The dataset contains monthly non seasonally adjusted information on commodity prices across European countries and covers the period 1996-2015.[13]

The HICP provides monthly price data by Classification of Individual Consumption According to Purpose (COICOP), and is assembled according to a harmonized approach that makes cross-country information highly comparable.[14] Eurostat first collects the data from surveys conducted separately by each member country of the European Union. Next, Eurostat construct price series, which are harmonized to account for country specific sampling procedures. [15] This data is the single most reliable information on inflation across countries in the European Union and is also used by policy makers to measure inflation in Europe overall.

[11] See Owens, Battiau, and Charlet 2008 for a broad overview of VAT taxes in OECD countries.

[12] See the information collected in Barbone et al. 2013.

[13] Eurostat is an organization of the European Commission in charge of collecting data and harmonizing it in order to provide statistical information about member states of the European Union.

[14] Appendix tables B.1 and B.2 lists all the COICOP categories used in our analysis.

[15] In general, individual countries collect price data by sending field agents to different point of sales to record the posted prices of a given set of commodities. For example, France collects 160,000 prices every month in each of 27,000 points of sales to construct prices series for each commodity (see the website of the French Institute of Statistics and Economic Studies (INSEE) for a description of the price collection process: http://tinyurl.com/q6jm8gg).

Historical VAT Rates Information on VAT rates by commodity and country is provided directly by the *European Commission (EC)* in its annual report *VAT Rates Applied in the Member States of the European Community*. The report contains detailed information on the VAT rate applied to each commodity in each European country – including multiple VAT rates, if applicable –, as well as the exact date of the VAT reforms.[16] It covers commodities that are taxed at the standard VAT rate as well as those that are taxed at the reduced or super-reduced VAT rates.

Some clarifications are necessary on this source of data. First, the EC's report does not contain information on VAT rates between 1996 to 2003 for European countries that were not members of the European Union prior to May 1st 2004.[17] The EC reports are also missing information on labor-intensive commodities for some countries in the period 1996-1999.[18] We therefore exclude them from our analysis.

Overall, we consider 27 European countries: Austria, Belgium, Bulgaria, the Czech Republic, Cyprus, Denmark, Germany, Estonia, Greece, Spain, France, Finland, Ireland, Italy, Hungary, Latvia, Lithuania, Luxembourg, Malta, the Netherlands, Poland, Portugal, Romania, Slovakia, Slovenia, Sweden and the United Kingdom.

Sample Restrictions Matching the price data with the VAT data presents three main challenges. First, the EC does not directly provide COICOP codes for each commodity. We therefore assign each commodity in the EC dataset to a four-digits COICOP code based on the closest classification. The limitation is that although four-digit COICOP categories can include multiple commodities, each with a price series, they are all subject to the same VAT rates.[19] Similarly, it is possible that a given commodity group covers a set of different commodities taxed at different VAT rates, as it is for the *adult footwear* and *children footwear*. In both cases, the assigned VAT rate is an imprecise measure of the "actual" VAT rate applied to the good.

Secondly, even when VAT rate categories match price categories, some commodities (such as *transportation services*) face multiple VAT rates. As a result, it is difficult to identify which rate is the most widely used. For example, in France, *housing reparations* are subject to three different VAT rates depending on the age of the house being repaired and whether the

[16]The report does not include information on the date of commodities' re-classifications, which are changes in the tax regime that apply to a given commodity (for instance, reclassifying a commodity from the standard rate to the reduced rate). We intend to explore the effect of VAT reclassification in future analyses.

[17]These include the Czech Republic, Estonia, Cyprus, Latvia, Lithuania, Hungary, Malta, Slovenia and Slovakia. Similarly, the data for Bulgaria and Romania starts in 2007. The VAT was introduced in Slovenia in 1999. Because it only became a member of the European Union in 2013, we exclude Croatia from our analysis.

[18]Labor intensive commodities include: bicycles; shoes and leather goods; clothing and household linen; renovation and repairing of private dwellings; window cleaning and cleaning in private households; domestic care services; and hairdressing.

[19]See table B.3 for a sample of four-digit COICOP categories that fall in the Food three-digit COICOP category.

reparations are environmentally friendly. To address this, we show that the results do not differ when one uses either the maximum VAT rate and the average VAT rate.

Lastly, the EC documents are only published once a year. Therefore, as we observe the VAT rate at the time of each report, and know the national VAT reform dates, we assume that all changes in VAT rates observed from one year to the next occur at the time of a national VAT reform.[20] This assignment procedure is an approximation and it rules out the possibility that some VAT changes are in fact re-classifications that happen at a different time than when national VAT reform are implemented.

We perform our analysis on two different samples. First, we use what we call *full sample*, which is composed of all commodities that experience either an increase or a decrease in the VAT rate between 1996 and 2015. As shown in panel a. of table 2.1, this includes 3,389 VAT reforms and 48 commodities across 22 European countries. In addition, as shown in the same table, our analysis compares the price effects of 2,917 VAT increases and 472 VAT decreases.[21]

Secondly, we consider a *restricted sample* where we only focus on commodities that experience both a VAT increase and a VAT decrease between 1996 and 2015. The restricted sample includes 1,268 VAT changes, 816 of which are VAT increases. This sample is desirable for our analysis because it makes asymmetric VAT changes more comparable given that it considers commodities that have experienced at least one VAT increase and one VAT decrease.

2.4 Asymmetric Price Responses to VAT Changes

Graphical Evidence

In this section, we show evidence using cross-sectional and time variation in VAT rates for different commodities across European countries and compare prices responses to VAT increases and VAT decreases. We show unconditional means in the price index and the VAT rate in the three months before and after the reform, normalizing the series to 100 in the month previous to the reform.[22]

Graphical evidence using the *full sample* of commodities experiencing a VAT change in the period 1996-2015 is displayed in figure 2.1. The figure shows that while prices generally

[20] Similarly, if there is more than one reform between two EC report publications, we impute the changes in VAT at the time of such reforms.

[21] Given that, as noted above, some VAT reforms apply to more than one commodity, the number of VAT reforms considered is lower than the number of VAT changes used in the analysis. In our sample we consider 2,126 VAT reforms, among which 1,831 are VAT increases and 295 are VAT decreases.

[22] Alternative windows around the reform can be used. However, the larger the window the more likely it is that the price response reflects additional changes in the VAT rate and factors that are unrelated to the reform. In addition, as several VAT reforms occur within six months from each other, our strategy mitigates the concern that the pre-reform period of one reform overlaps significantly with the post-reform period of a previous reform.

increase following VAT increases, they do not decrease proportionately when the VAT decreases. While inflation is not taken into account in this figure, the figure suggests that the decrease in prices following a VAT decrease is very temporary. Figure 2.2 shows comparable evidence for our *restricted sample*, confirming that prices do respond asymmetrically to VAT changes.

The documented asymmetry does not seem to be specific to a limited number of consumption goods. Figures A.7 and A.8 show evidence by 2-digit COICOP groups, which are large aggregates of consumption goods. The asymmetry holds across most commodity categories: for instance figure A.8 shows that 7 out of 10 consumption categories exhibit an asymmetric price response to VAT taxes. Only two categories do not respond asymmetrically: *Furnishings and Maintenance of the House* and *Communication*. The first category shows no sharp responses to decreases or increases, while the second shows a symmetric response to changes in VAT rates. In addition, the price of *Clothing and Footwear* does not seem to react to VAT changes, mostly because their price is very seasonal; price drops are frequently observed in January, a month when a large number of clothing and footwear items goes on sale.

Figures 2.3 and 2.4, which consider the *full sample* and the *restricted sample* respectively, further illustrate the asymmetry. The two figures show the correlation between price changes and VAT changes in our data, and a linear fit in the case of tax increases and tax decreases. The figures show that: (1) in the case of VAT decreases, there is close to zero correlation between percent changes in the price and changes in the level of the VAT in the case of VAT decreases, and (2) the correlation is slightly positive in the case of tax increases.

Overall, the graphical evidence suggest that prices respond asymmetrically to increases and decreases in VAT rates, and that this evidence is robust across different commodities and sample restrictions. We investigate this in a more systematic way in the next section of the paper.

Estimation of the Asymmetry

Regression Analysis

In our empirical analysis we first estimate the average pass-through of the VAT for VAT increases and decreases. We use a difference-in-differences strategy to compare our treatment group to two alternative control groups: (1) the commodity price in the same month one year before and (2) the average commodity price in the same month in the three previous years.[23] Using these two control groups allow us to take into account country- and commodity-specific seasonal effects that might bias our results.

[23]We only consider VAT changes for which we have complete information on both the treatment and control group in the six months window around the reform. Our initial sample has 3,389 VAT changes, of which 2,917 are VAT increases. When using the commodity price 12 months before as a control group, we exclude 274 VAT changes, 192 of which are VAT increases. When using the average commodity price in the three previous years as a control group, we exclude 528 VAT changes, 375 of which are VAT increases.

We focus on the window [-3,3] months around each reform. In order to estimate the average pass-through of the VAT tax we instrument the level change in the VAT with Treat × After, where *Treat* is a dummy variable equal to one for the treatment group, and *After* is a dummy equal one in the reform month and the three months following the reform. In particular, we estimate:

$$\begin{aligned}\log p_{ict} = \beta_0 + \beta_1 \cdot \text{Treat} + \beta_2 \cdot \text{After} + \beta_3 \cdot \tau_{ict} + \\ + \beta_4 \log p^{all}_{ct,-i} + \beta_5 \log p^{EA19}_{it,-c} + \pi_t + \omega_c + \epsilon_{ict}\end{aligned} \tag{2.1}$$

where i is the commodity considered, c the country subscript and t is the month in which the price index is observed. Our specification controls for changes in economic conditions and international commodity prices. In particular, $\log p^{all}_{ct,-i}$ is the month t price of all commodities in country c excluding the 2-digit COICOP commodity category to which commodity i belongs, while $\log p^{EA19}_{it,-c}$ is the month t price of commodity i in the EA19 countries excluding country c to which commodity i belongs.[24] In addition, we also control for systematic differences in prices over time and European states as we include time fixed effects (π_t) and country fixed effects (ω_c). Standard errors are clustered by four digits COICOP category, to account for the serial correlation in commodity-specific errors.

β_3 is the estimated semi-elasticity of the VAT on prices and a measure of the pass-through of the tax. It corresponds to the ratio between the post-reform percent change in prices and the post-reform level change in the VAT rate (where T=Treat, Aft=After and Bef=Before):[25]

$$\beta_3 = \frac{[E(\log p^{Aft}_{ic}|T=1) - E(\log p^{Bef}_{ic}|T=1)] - [E(\log p^{Aft}_{ic}|T=0) - E(\log p^{Bef}_{ic}|T=0)]}{[E(\tau^{Aft}_{ic}|T=1) - E(\tau^{Bef}_{ic}|T=1)] - [E(\tau^{Aft}_{ic}|T=0) - E(\tau^{Bef}_{ic}|T=0)]}$$

We estimate the pass-through using an instrument variable (IV) approach rather than ordinary least squares (OLS) mainly because we are concerned about measurement error. Though it is unlikely that measurement error is systematically correlated with whether we focus on tax increases or tax decreases, it is a concern if we want to precisely estimate the pass-through of the tax for at least two reasons. First, measurement error is an issue if the measure of VAT rate we use (the maximum VAT rate) is not the rate generally applied to commodities to which more than one tax rate applies.[26] As mentioned previously, measurement error in the magnitude of the VAT change can also depend on the fact that the categories defined in the price and VAT data do not perfectly overlap. Secondly, measurement error can arise if the time of the VAT change is inaccurate. Because VAT rates are

[24]EA19 includes Belgium, Germany, Estonia, Ireland, Greece, Spain, France, Italy, Cyprus, Latvia, Lithuania, Luxembourg, Malta, the Netherlands, Austria, Portugal, Slovenia, Slovakia and Finland. Alternative aggregates of European countries can be used without affecting our results.

[25]The notation holds for a specification in which we exclude $\log p^{all}_{ct,-i}$, $\log p^{EA19}_{it,-c}$, π_t and ω_c. Including these variables makes the notation heavier without changing the interpretation of β_3.

[26]We could in principle test whether IV and OLS estimates give similar results for commodities that are subject to only one VAT rate. Note that this issue would still be present if we used the average rather than the maximum VAT rate.

only observed once a year, it is necessary for us to assign VAT rates to each month of data using only available information on national VAT reforms.[27]

Our next step is to examine more closely the dynamics of the adjustment, which we estimate by using a difference-in-differences framework where we consider the change in prices and VAT rates in each of the three months preceding and following the reform relative to the month prior to the reform, and use the same two control groups defined above. For instance, for the percent change in prices we use the following specification:

$$\log p_{ict} = \gamma_0 + \gamma_1 \cdot \text{Treat} + \sum_{\nu=-3}^{3} [D_{ict}^{\nu} \times (\eta_\nu + \beta_\nu \cdot \text{Treat})] + \tag{2.2}$$

$$+ \gamma_2 \log p_{ct,-i}^{all} + \gamma_3 \log p_{it,-c}^{EA19} + \pi_t + \omega_c + \epsilon_{ict} \tag{2.3}$$

where e_{ict} is the date of the VAT change for commodity i in country c and $D_{ict}^{\nu} = \mathbb{1}\{t = e_{ict} + \nu\}$ is a dummy equal one ν months from the VAT change. The coefficients of interest are the event-time coefficients β_ν, which measure the different between the treatment and control group ν months from the event.[28] Finally, we include time and country fixed effects as before, and control for changes in the price of other commodities in the same country, and for the price of the same commodity in other countries. As before, standard errors are clustered by four digits COICOP category. We report our findings in the next section.

Estimates

Table 2.2 compares 2,725 VAT increases to 390 tax decreases and uses specification (2.1) to estimate the pass-through of the tax in the case of VAT increases and decreases. Our preferred estimates are reported in column (4) for tax increases and column (8) for tax decreases. Column (4) of table 2.2 shows that around 50 percent of the tax is passed to prices in the case of tax increases. On the other hand, column (8) indicates that the pass-through of the tax is only 23 percent in the case of VAT decreases. If there were an symmetric response to the VAT tax, we would have observed the same positive coefficients for tax increases and decreases.

Similar results hold when we focus on the *restricted sample*, and compare the pass-through of 811 VAT increases to 373 VAT decreases. This is shown in table 2.3: in column (4) we estimate that around 38 percent of the tax is passed to prices in the case of VAT increases, while in column (8) we show that the pass-through is 25 percent for VAT decreases. Similarly to what we showed for the *full sample*, our event-type estimates point to the fact that there a substantial asymmetry in the price response to the VAT tax.

The asymmetry in price responses is also supported by the evidence displayed in figures 2.6 and 2.7, which show event-type coefficients for price and VAT changes around the time of the reform. Again, the evidence is consistent with the conclusion that price responses

[27] We perform a Hausman test comparing OLS and IV estimates using the *full-sample* and find that chi-square=0.0058, suggesting that measurement error is a concern in the OLS estimation.

[28] We standardize $\beta_{-1} = 0$.

to VAT changes are asymmetric: while prices react significantly to tax increases, with the strongest effect happening in the month of the reform, they do not react as much in the case of tax decreases.

To further substantiate our claim, we show in the appendix of the paper that similar results hold when using an alternative control group: the average price of the commodity in the same month in the three previous years. The results for the *full sample* are shown in table A.2 and figure A.2, while the results for the restricted sample are displayed in table A.3 and figure A.4.

2.5 What Explains Asymmetries In Pass Through?

Fairness Considerations

While standard economic theory doesn't offer a generalizable explanation for why prices react asymmetrically to VAT changes, we argue that an argument based on consumers' perception of fairness towards price changes following cost/tax increases and decreases can be used as an overarching explanation.

Kahneman, Knetsch, and Thaler 1986 provides evidence that fairness matters to consumers when firms set prices. They show that consumers perceive price increases as fair when costs increase, mainly because it is accepted that firms have to raise prices to protect their own profits. However, when costs are decreased, a substantial share of consumers find it acceptable for firms to lower their prices by only a limited amount for various reasons described below.

In their paper, Kahneman, Knetsch, and Thaler 1986 establish that consumers find price increases aimed at increasing profits unfair relative to price increases aimed at protecting profits using the following two scenarios:

1. *A hardware store has been selling snow shovels for $15. The morning after a large snowstorm, the store raises the price to $20.*

2. *Suppose that, due to a transportation mixup, their is a local shortage of lettuce and the wholesale price has increased. A local grocer has bought the usual quantity of lettuce at a price that is 30 cents per head higher than normal. The grocer raises the price of lettuce to customers by 30 cents per head.*

The authors find that 82 percent of individuals surveyed find situation 1 to be unfair, whereas only 21 percent find situation 2 to be unfair. This difference in opinions shows that the reasons behind price increases matter in determining whether they are perceived as fair or not.

The study of Kahneman, Knetsch, and Thaler 1986 also shows that a large share of individuals perceives the absence of price decreases to be fair in some cases. For instance, when costs are decreased:

1. *A small factory produces tables and sells all that it can make at $200 each. Because of changes in the price of materials, the cost of making each table has recently decreased by $40. The factory reduces its price for the tables by $20.*

2. *... the cost of making each table has recently decreased by $20. The factory does not change its price for the tables.*

79 percent of respondents find the first outcome acceptable and surprisingly, even in the case in which none of the reduction in costs is passed through to consumers, over half (53 percent) of the individuals find it acceptable. This stands in contrast to the previous scenarios where individuals find it unfair for firms to increase their profits when taking advantage of increased demand.

While Kahneman, Knetsch, and Thaler 1986 shows that consumers care about fairness in price setting by firms, it does not directly answer the question of whether firms are aware of this bias, which is necessary for us to explain the asymmetric changes in prices following VAT reforms. A. Blinder et al. 1998 addresses this question by presenting survey evidence, and shows that:

1. 64 percent of surveyed firms claim that "customers do not tolerate price increases after increases in demand" and

2. 71 percent of firms believe that "customers do tolerate price increases after increases in cost".

Therefore, the findings of A. Blinder et al. 1998 indicate that firms are likely to be aware of this bias and possibly act upon it.

Overall, this evidence suggests that individuals are more likely to perceive price increases that are aimed at maintaining profits as being fair and acceptable such as in the case of an increase in the VAT rate. On the other hand, they are less likely to perceive stable prices following VAT decreases as unfair. These two set of findings are consistent with the evidence on asymmetry we present in the main body of the paper, and support our explanation for why prices respond more to VAT increases than to VAT decreases.

Can Economic Conditions Explain the Asymmetry?

A first possible concern is that VAT increases are implemented when the economy is growing whereas VAT decreases occur during economic downturns. However, it is unclear how this would bias our results. On the one hand, if VAT decreases occur only when demand is sluggish, firms should be eager to reduce their prices to increase output sold, which would result in stronger price responses for VAT decreases and ameliorate the asymmetry. On the other hand, it is also possible that firms are less likely to cut prices during economic downturns to keep the tax cut as profits, supporting our findings of systematic asymmetry. In practice, it is not clear which argument should prevail.

This concern is unlikely to bias our results as our work considers a wide range of reforms, a substantial number of which are unrelated to variations in economic growth; rather they have the general goal of VAT harmonization across European countries. In fact, the EC sets strong rules on how VAT rates can vary in each member country. One such example, which we document in Benzarti and Carloni 2015b, is the VAT cut in the sit-down restaurant VAT rate in 2009 and the subsequent increases in 2012 and 2014 (see figure 2.5). Though the 2009 VAT cut happens following the Great Recession, this is merely a coincidence, given that a VAT decrease for sit-down restaurants had been discussed since 2001 and its implementation was delayed by the restrictions on the VAT applied to labor-intensive services imposed by the EC.

Moreover, our estimation framework controls for changes in the overall price level in the economy ($\log p^{all}_{ct,-i}$), for the changes in the price of the same commodity in other European countries ($\log p^{EA19}_{it,-c}$), and for systematic differences in economic conditions over time (π_t). We show in our main analysis that including these controls does not affect the asymmetric price responses to the VAT.

Can Market Structure Explain the Asymmetry?

Previous studies using market structure as an explanation for the asymmetry focus on a very narrow range of sectors, while our study focuses on a large set of commodities across several countries over 20 years. For example, Clément Carbonnier 2005 and Clement Carbonnier 2007 use market structure to explain the heterogeneous pass-through of VAT reforms. Clément Carbonnier 2005 finds that the 1995 VAT tax increase had larger effects in more competitive (labor intensive) markets, while the 2000 tax decrease had a more substantial impact in oligopolistic (capital intensive) markets.[29] They find price elasticities of 0.53 and 0.86 in 1995, and of 0.16 and 1.52 in 2000. Analogously Clement Carbonnier 2007 compares the 1987 VAT cut on new car sales with the 1999 tax decrease for housing repair services. He estimates that the consumer share of the consumption tax burden was 57 percent in the first reform and 77 percent in second.

While we are unable to control directly for market structure, our findings hold across a wide range of commodities (as shown in figures A.7 and A.8), evidence that is difficult to rationalize with standard economic theory. The previous literature has not made a convincing case for why an asymmetric reaction to the VAT might be by simply explained by considering demand and supply elasticities.

[29] The framework developed in Delipalla and Keen 1992 explains the empirical findings with a combination of adjustment costs and the curvature of the demand elasticity to prices. While firms in competitive markets are generally smaller and are therefore more likely to lack the financial resources to supply more output when consumption taxes decrease – therefore reacting less to tax decreases – firms in oligopolistic markets are less reactive to the tax when the demand elasticity increases with prices. This is because changes in prices become marginally more costly as prices increase, while producing increasingly lower gains as prices decrease.

Empirically, we believe our results are consistent with a behavioral story, though we are unable to test the market concentration theory directly. Controlling for market concentration comes with two main data challenges. First, there is no data to our knowledge that documents the market structure by industry across all European countries over a long period of time. Second, our data uses variation in the price of goods and services consumed by households rather than variation in industrial prices. Merging consumption prices with information on industry market structure is not straightforward, and is subject to approximations.

2.6 Heterogeneous Effects

Standard versus Reduced Rate

In this section we compare the asymmetric effect of VAT reforms on prices considering two subsets of commodities: those that are mainly taxed (1) at a standard rate and (2) at a reduced rate. Theoretically it is unclear whether we should expect the asymmetry to be stronger in one of the two subsets. One the one hand, the standard rate might be more salient in that is applied to a much larger number of goods. In practice though the VAT is in many cases not visible to consumers, and reforms of standard and reduced rates happen contemporaneously. On the other hand, the reduced rate applies to a subset of goods and services that are more likely to be necessity goods. The consumers of necessity goods might differ in how attentive they are to changes in the tax rate applied to the goods they purchase. For example, if lower income households consume more necessity goods, and are also more careful about their how they spend their income, they will also be more likely to notice changes in VAT rates.

Figures 2.10 and 2.11 consider the *full sample* and the *restricted sample* respectively, and show that the asymmetric response is slightly more noticeable for goods that are taxed at a standard rate. However, we are unable to provide a full explanation for this result. Part of the reason for this heterogeneity might depend on the sample size considered, which is significantly larger for changes in the standard VAT rate.

High versus Low GDP Growth

We then divide VAT reforms between those implemented after periods of high and low GDP per capita growth. Growth in GDP per capita is computed by comparing the quarterly GDP per capita in the month of the reform to GDP per capita in the same quarter one year before. VAT reform is defined as been preceded by a year of high growth in the GDP per capita are if the growth rate in GDP per capita is above the median for all the reforms included in the study.

Figure 2.12 compares the pass-through of VAT reforms to prices with high and low GDP per capita growth. Panels a. and c. use the commodity price and VAT rate one year before

as a control group and show that the asymmetry is significant for VAT reforms preceded by high GDP growth. In contrast, prices react only slightly in periods of low growth. Similar evidence holds in panels b. and d., which use averages in the previous three years as a control group.

2.7 Robustness Checks

Exclude January Reforms

We show here that the asymmetry does not depend on the month in which the reform is implemented. Figure A.13 shows in fact that a large fraction of the VAT reforms was implemented in January, raising the concern that the price reaction we identify depends on timing in a way that we do not control for in our main specification. Note that our regression analysis already controls for seasonal effects in prices. However, when we restrict the sample to reforms that did not happen in January, we find that our results are unaffected.

Figures A.11 and A.12 show that, consistent with our main findings, prices generally increase when the VAT increases, with an average pass-through of around 50 percent. At the same time, prices decrease following VAT decreases, but the decrease is neither statistically significant nor as a large as the price increase. Again, this suggests that prices respond asymmetrically to VAT changes.

Average VAT Rate

In our analysis we use the maximum VAT rate applied to each commodity as the reference VAT rate. While we think this is a reasonable measure to use, especially given that a unique rate is applied to a large share of the commodities we study, some commodities are subject to more than one VAT rate. In this section we show that our results are robust to using the *average* rather than the *maximum* VAT rate. Figure A.14 and A.15, which plot estimated event-type coefficients using specification (2.2), show that the evidence is comparable to the one displayed in our main analysis (figures 2.7 and 2.9).

Additional Controls

We show that our results are also robust to including country-time fixed effects, as well as controls for country-specific economic conditions. Columns (2) and (5) of table A.4 and A.5 compare the pass-through of VAT reforms in the case of VAT increases and decreases when one controls for country-time fixed effects. Columns (3) and (6) of the same table also include controls for country-specific monthly unemployment rate, quarterly GDP per capita and monthly interest rate. The table shows that while the magnitude of the results changes slightly, the asymmetric price response we identify in the paper is unchanged.

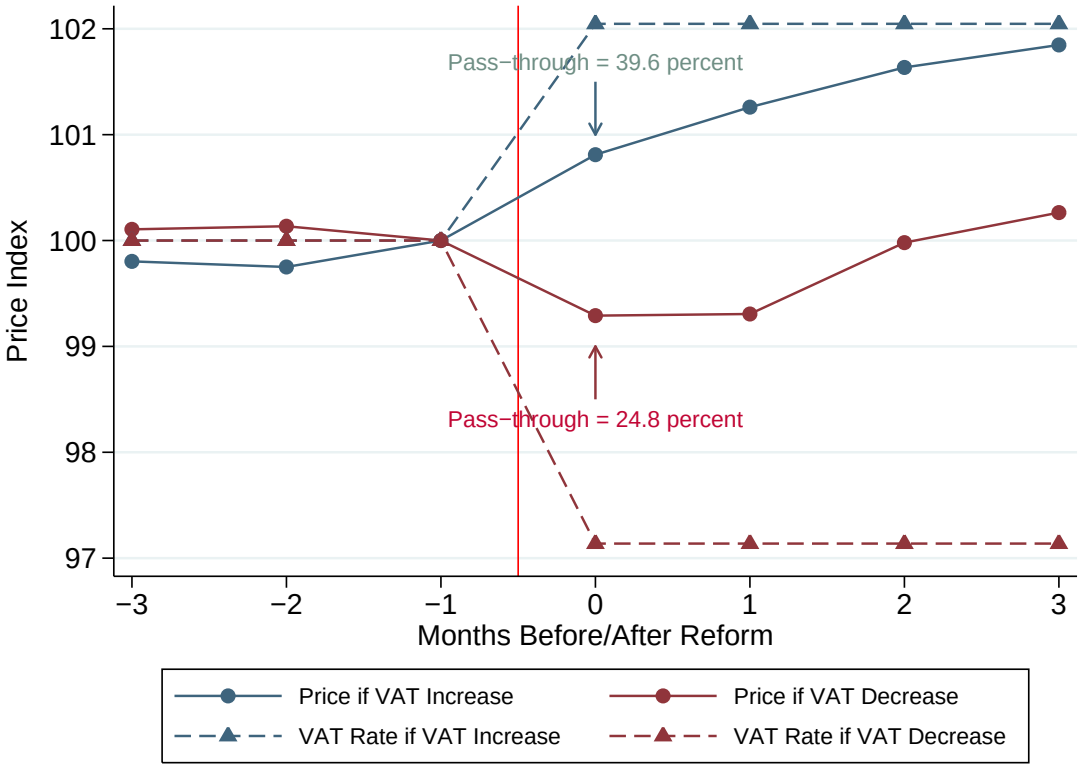
2.8 Conclusion

In this paper, we show that prices increase more following a VAT increase than they decrease following a VAT decrease. We use monthly price variation in the price of a wide range of commodities across European countries in the period 1996-2015 and find that the pass-through of VAT increases to prices is higher than it is for tax decreases.

Contrary to previous explanations, which have mainly focused on market structure, we consider an alternative interpretation for the observed asymmetry: we explain our results with a fairness argument based on the findings of Kahneman, Knetsch, and Thaler 1986. Consumers find it fair for prices to increase when firms are protecting their profits from VAT increases, but do not expect firms to decrease prices following VAT decreases.

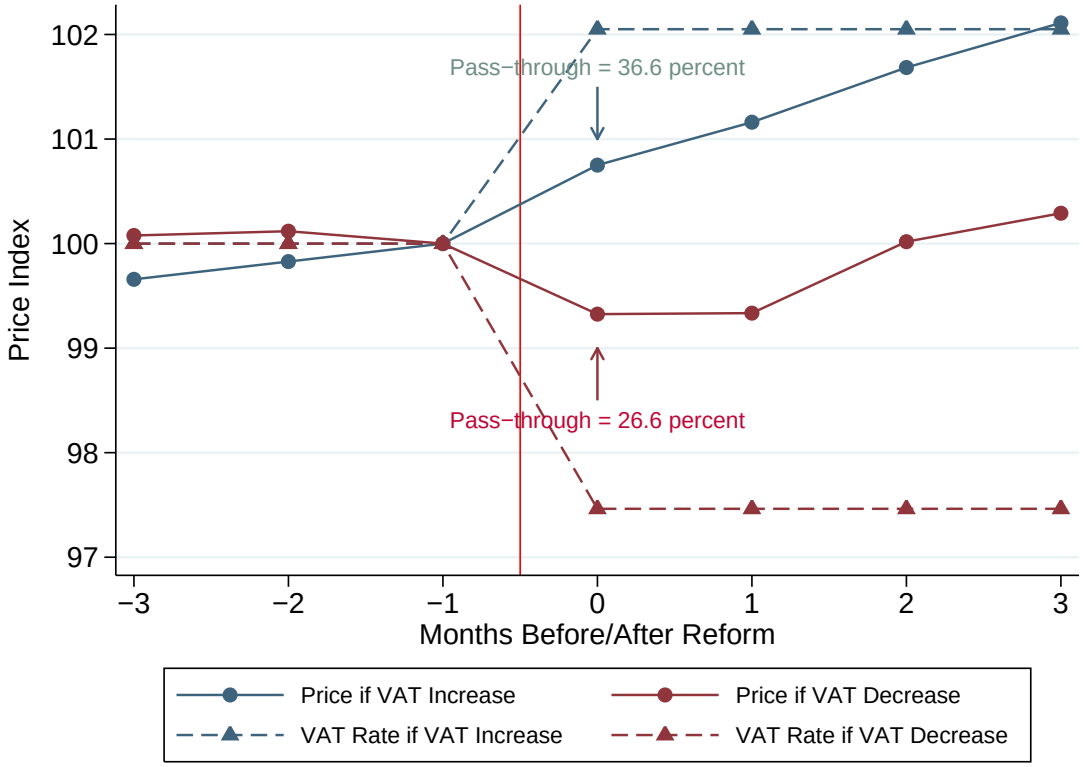
Our findings stand in contrast with the predictions of the theoretical literature on tax incidence, which assumes that the pass-through of taxes is symmetric. Our results are very relevant to policy makers today, as they caution against generalizing incidence estimates and urge that one should distinguish between tax increases and decreases in when considering the effects of fiscal policies. By extension, our paper questions the effectiveness of consumption tax cuts as a way to reduce prices and stimulate economic growth, at least in the sectors that are targeted by the tax cut.

Figure 2.1: *Full Sample*: Asymmetric Response of Prices to VAT Changes



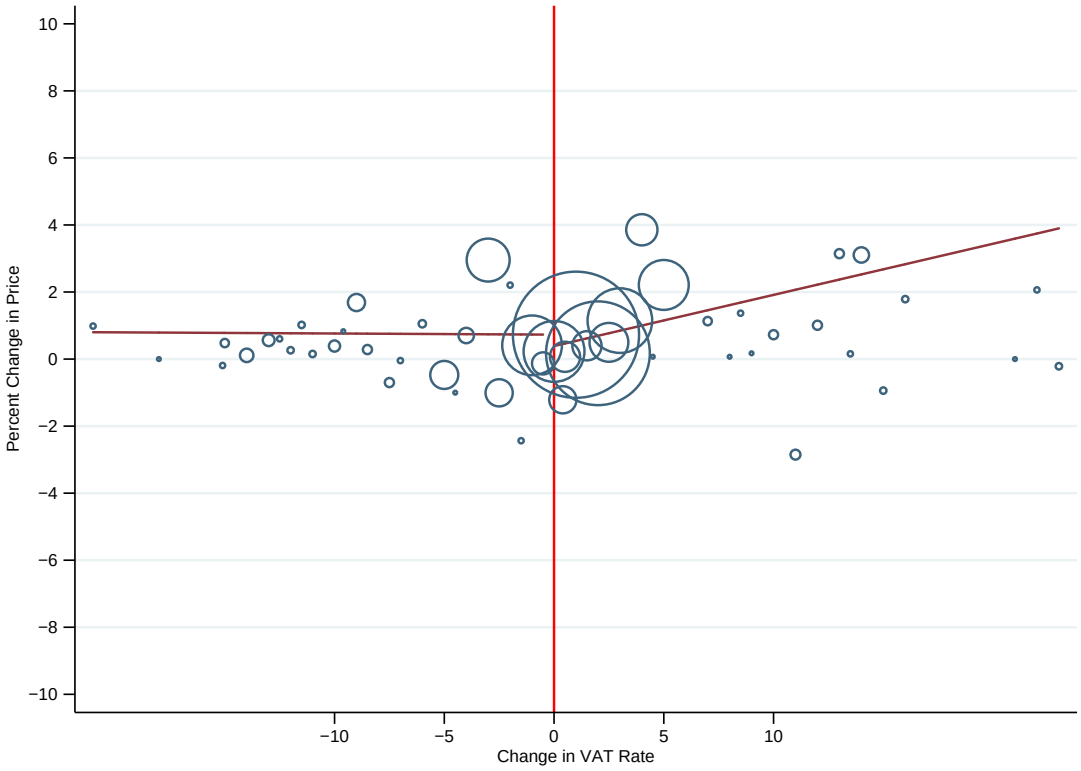
Notes: This figure shows the aggregate price levels before and after the VAT rate reforms. It is constructed using all VAT changes between 1996 and 2015. For each commodity the price index is normalized to 100 in the month prior to the VAT reform.

Figure 2.2: *Restricted Sample*: Asymmetric Response of Prices to VAT Changes



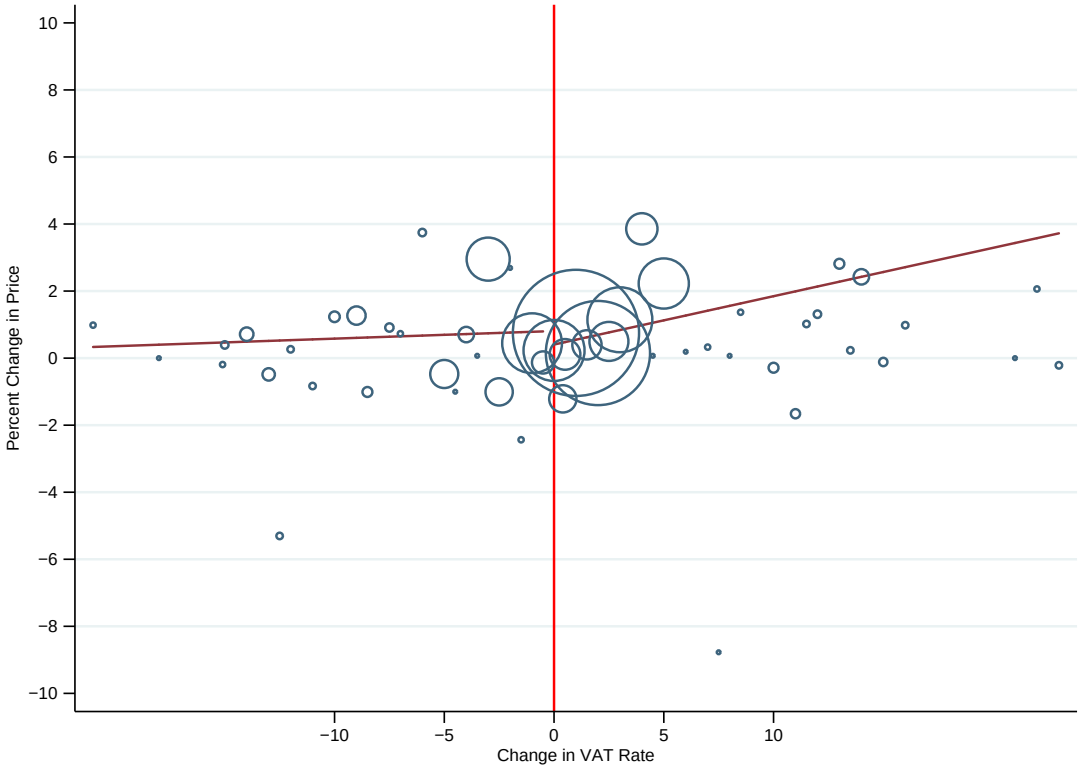
Notes: This figure shows the aggregate price levels before and after the VAT rate reforms. It is constructed using commodities that are subject to both a VAT increase and a VAT decrease between 1996 and 2015. For each commodity the price index is normalized to 100 in the month prior to the VAT reform.

Figure 2.3: *Full Sample*: Percent Change in Prices by Change in VAT rate



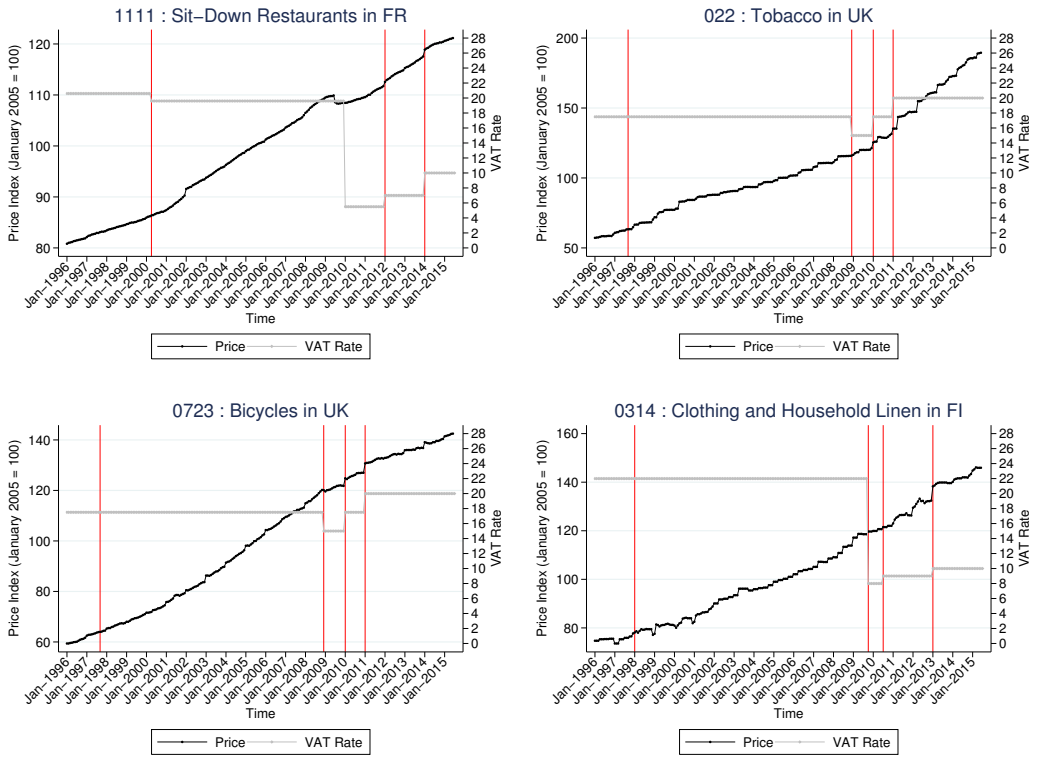
Notes: The figure is constructed using all VAT changes between 1996 and 2015. A linear fit of prices on negative and positive changes in VAT is estimated separately. Larger circles indicate a higher number of corresponding reforms for a given VAT change.

Figure 2.4: *Restricted Sample*: Percent Change in Prices by Change in VAT rate



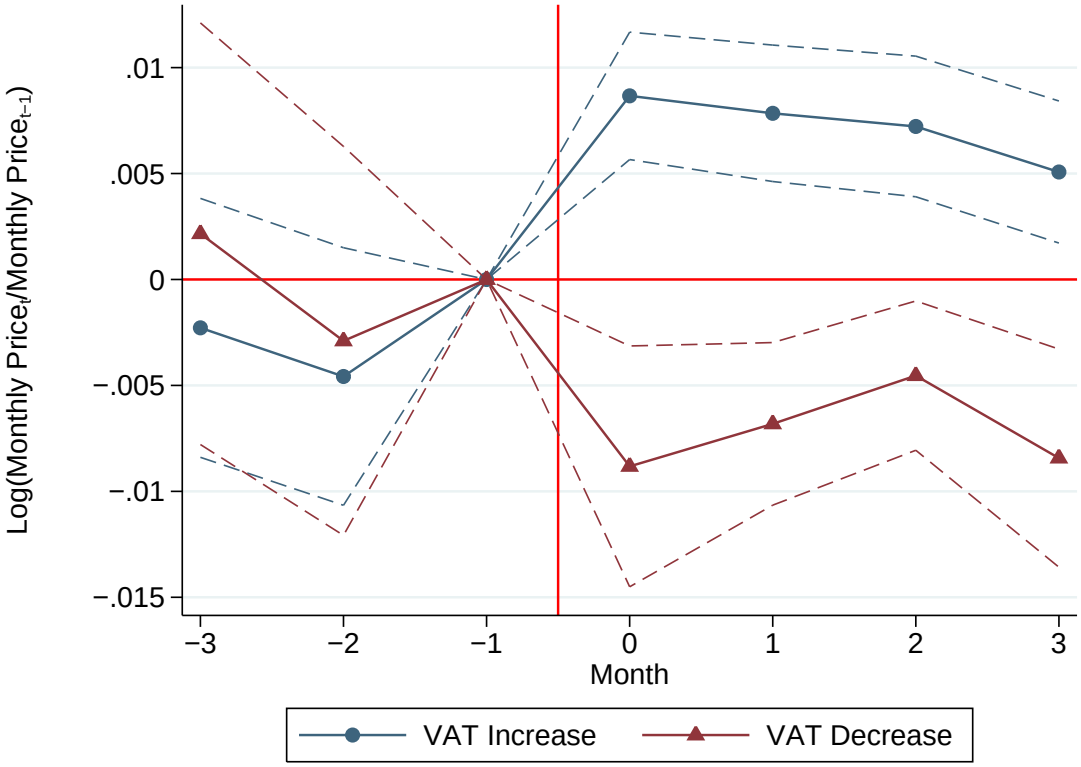
Notes: The figure is constructed using commodities that are subject to both a VAT increase and a VAT decrease between 1996 and 2015. A linear fit of prices on negative and positive changes in VAT is constructed separately. Larger circles indicate a higher number of corresponding reforms for a given VAT change.

Figure 2.5: Selected Individual Reforms



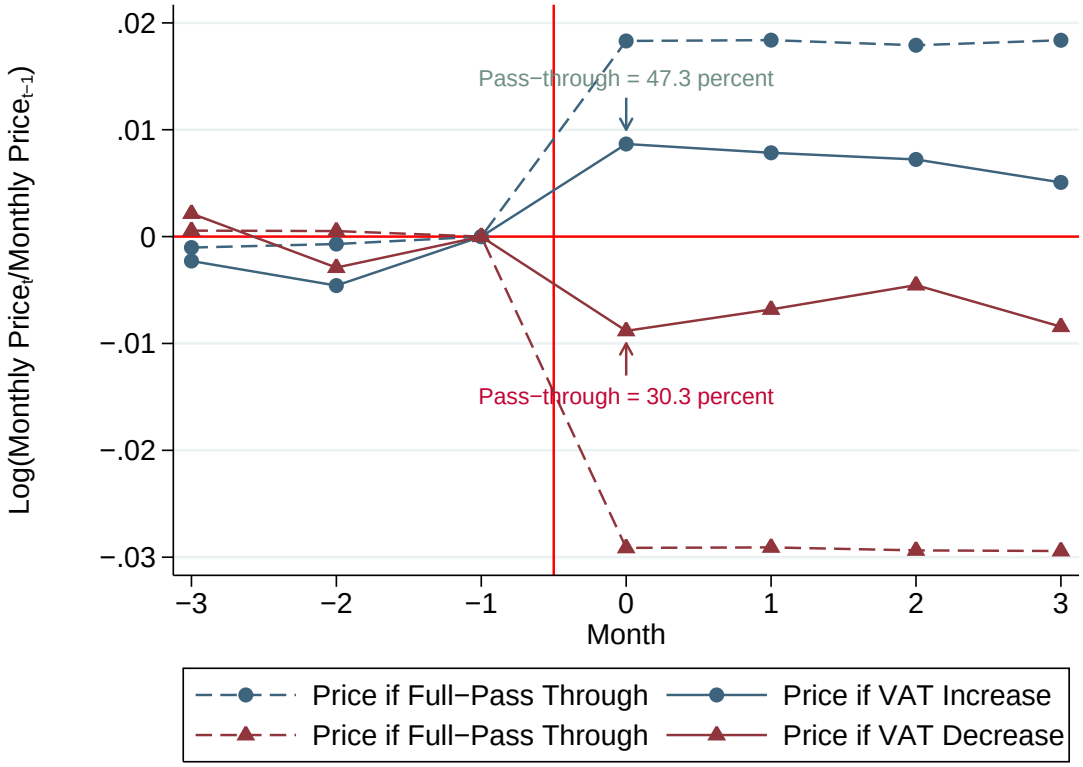
Notes: Computed using monthly price data by COICOP code from Eurostat, and VAT reforms for European Commission (EC) reports. Data are not seasonally adjusted. Red vertical lines indicate time of national standard, reduced or super-reduced VAT reforms. Numbers in the title of each sub-figure are the corresponding four-digit COICOP code.

Figure 2.6: *Full Sample*: Event-Study Estimates of Price Changes around VAT Reforms



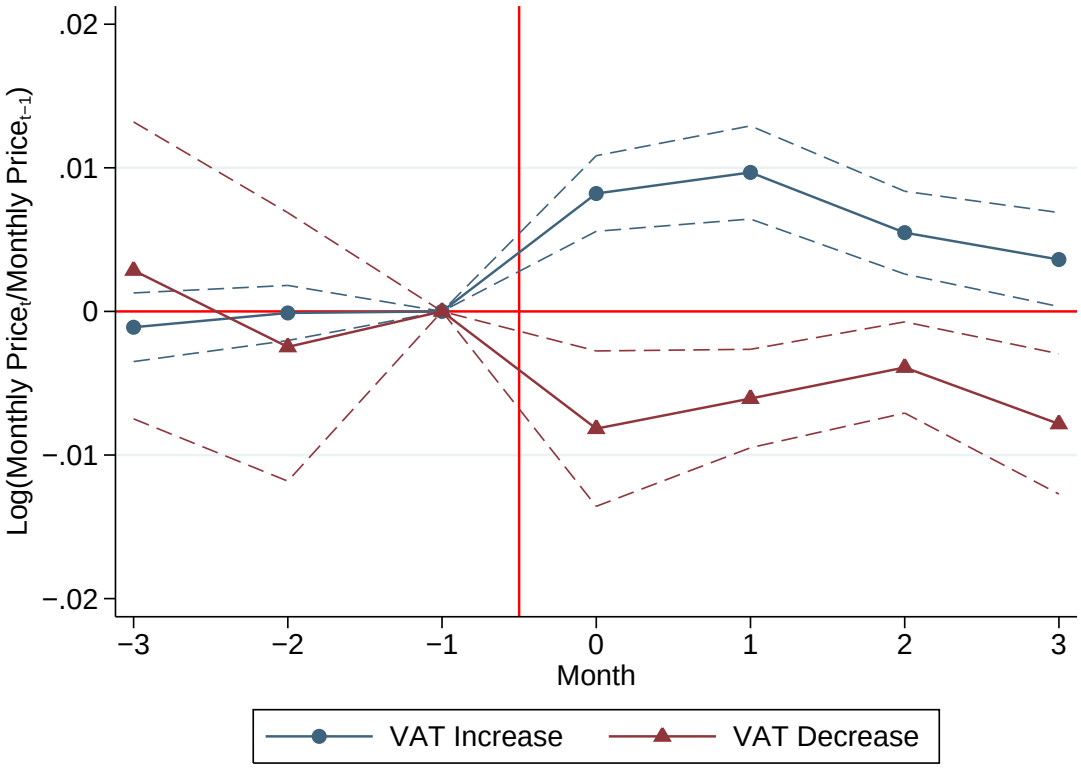
Notes: The figure is constructed using all VAT changes between 1996 and 2015. It shows event-type coefficients estimated using specification (2.2) on price changes around VAT increases and VAT decreases. Control group is the commodity price in the same month one year earlier. Dotted lines are 95 percent confidence intervals.

Figure 2.7: *Full Sample*: Event-Study Estimates of Price Changes and VAT Changes



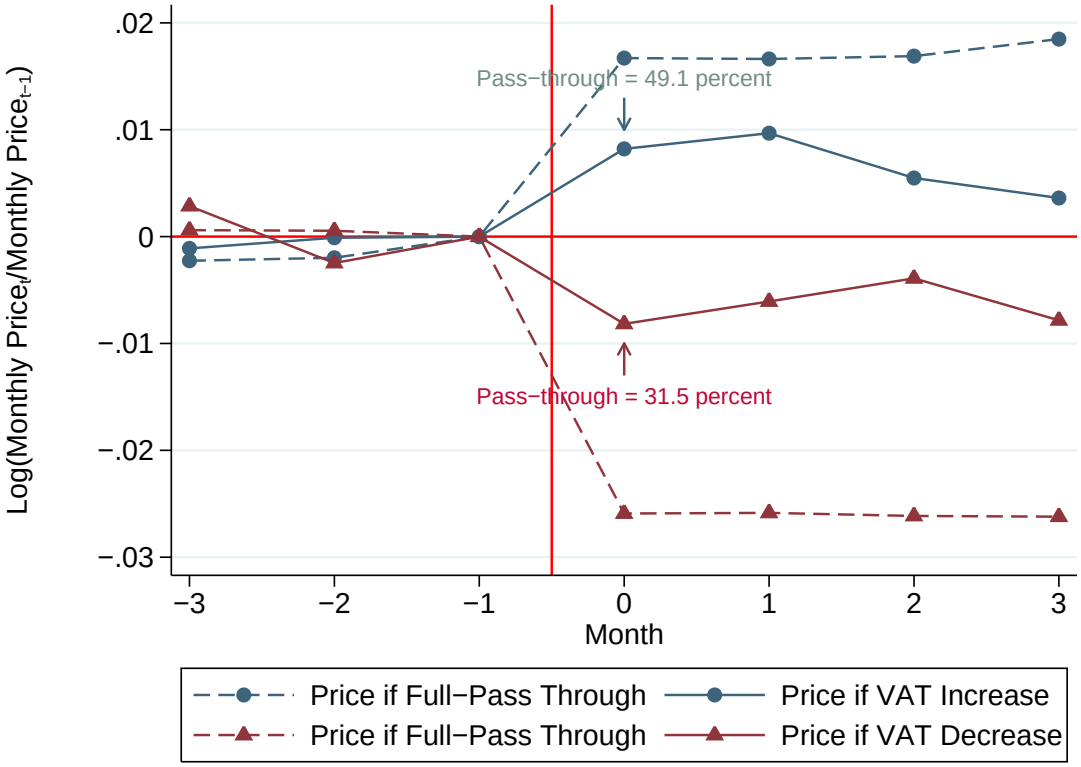
Notes: The figure is constructed using all VAT changes between 1996 and 2015. It shows event-type coefficients estimated using specification (2.2) on price changes around VAT increases and VAT decreases. Control group is the commodity price in the same month one year earlier.

Figure 2.8: *Restricted Sample*: Event-Study Estimates of Price Changes around VAT Reforms



Notes: The figure is constructed using commodities that experience both a VAT increase and a VAT decrease in the period 1996-2015. It shows event-type coefficients estimated using specification (2.2) on price changes around VAT increases and VAT decreases. Control group is the commodity price in the same month one year earlier. Dotted lines are 95 percent confidence intervals.

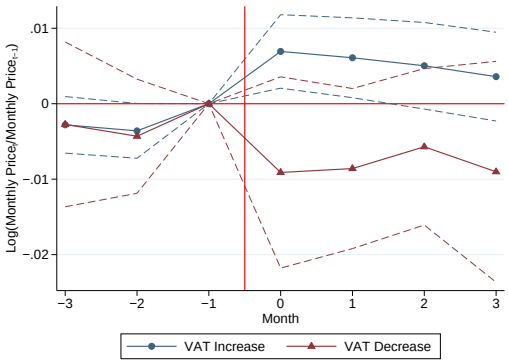
Figure 2.9: *Restricted Sample*: Event-Study Estimates of Price Changes and VAT Changes



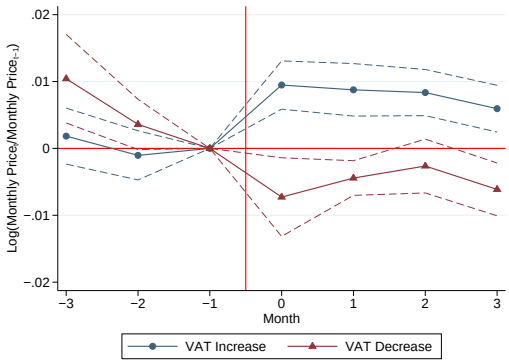
Notes: The figure is constructed using commodities that experience both a VAT increase and a VAT decrease in the period 1996-2015. It shows event-type coefficients estimated using specification (2.2) on price changes around VAT increases and VAT decreases. Control group is the commodity price in the same month one year earlier.

Figure 2.10: Heterogeneous Asymmetry: Event-Study Estimates of Price Changes and VAT Changes using Full Sample

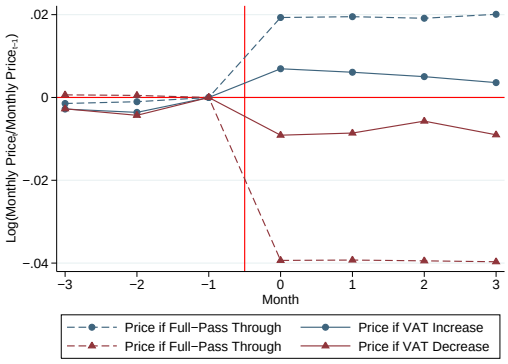
a. Price Effect if Reduced Rate



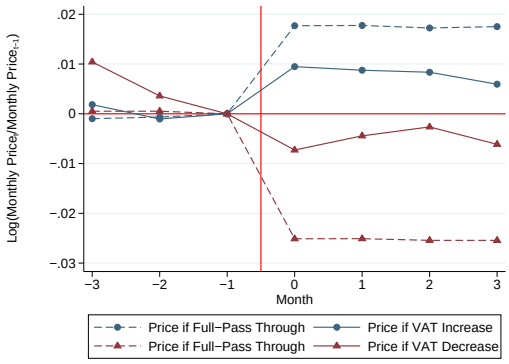
b. Price Effect if Standard Rate



c. Combined Effect if Reduced Rate

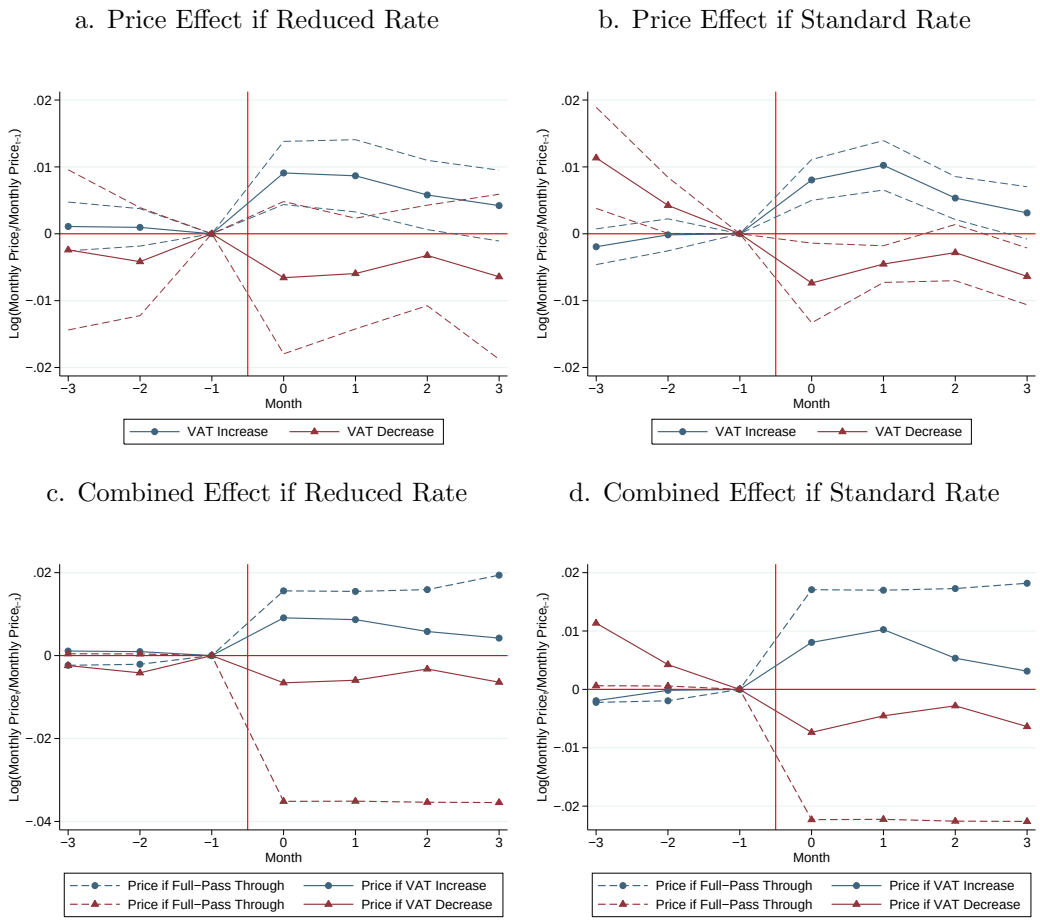


d. Combined Effect if Standard Rate



Notes: The figure is constructed using all VAT changes between 1996 and 2015. It shows event-type coefficients estimated using specification (2.2) on price changes around VAT increases and VAT decreases. Control group is the commodity price in the same month one year earlier. Dashed lines in panels a. and c. are 95 percent confidence intervals.

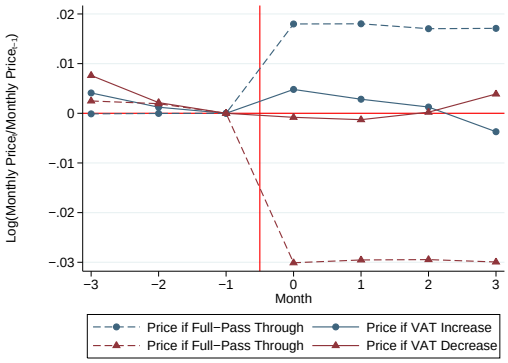
Figure 2.11: Heterogeneous Asymmetry: Event-Study Estimates of Price Changes and VAT Changes using Restricted Sample



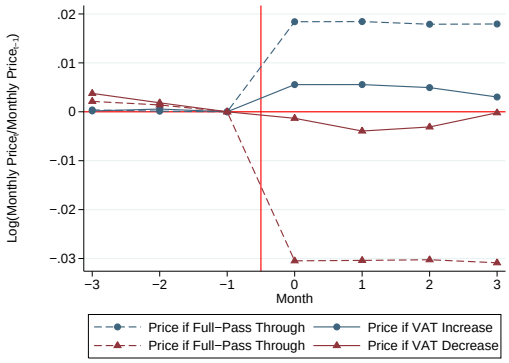
Notes: The figure is constructed using commodities that experience both a VAT increase and a VAT decrease in the period 1996-2015. It shows event-type coefficients estimated using specification (2.2) on price changes around VAT increases and VAT decreases. Control group is the commodity price in the same month one year earlier.

Figure 2.12: Event-Study Estimates by Growth in GDP-per-capita

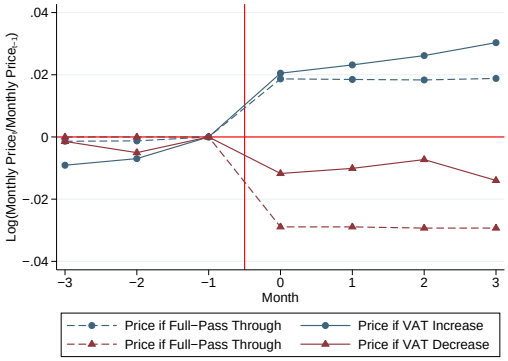
a. Low Growth, Control = One Year Lag



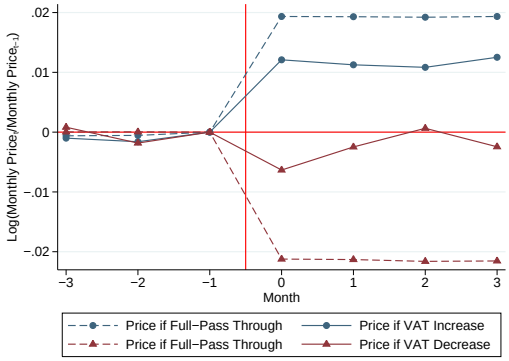
b. Low Growth, Control = Average of Three Year Lags



c. High Growth, Control = One Year Lag



d. High Growth, Control = Average of Three Year Lags



Notes: The figure is constructed using all VAT changes between 1996 and 2015. It shows event-type coefficients estimated using specification (2.2) on price changes around VAT increases and VAT decreases. High GDP-per-capita growth is defined as the growth in GDP-per-capita in the year preceding the VAT reform being above the median. Control group in panels a. and c. is the commodity price in the same month one year earlier, in panels b. and d. is the average commodity price in the same month in the 3 previous years.

Table 2.1: Summary Statistics on Maximum VAT Rate Applied

Panel A: All Commodities

	Number of VAT Reforms	Change in VAT Rate	Mean VAT After Reform	Standard Deviation	Minimum	Maximum
	(1)	(2)	(3)	(4)	(5)	(6)
VAT Changes	3,389	1.31	18.04	5.15	0	27
VAT Increases	2,917	2.01	18.14	5.20	4.5	27
VAT Decreases	472	-2.99	17.41	4.83	0	21

Panel B: Commodities to which Standard Rate is Generally Applied

	Number of VAT Reforms	Change in VAT Rate	Mean VAT After Reform	Standard Deviation	Minimum	Maximum
	(1)	(2)	(3)	(4)	(5)	(6)
VAT Changes	2,297	1.27	18.78	4.64	0	27
VAT Increases	1,968	1.91	18.88	4.72	5	27
VAT Decreases	329	-2.59	18.21	4.06	0	21

Panel C: Commodities to which Reduced VAT Rate Can be Applied

	Number of VAT Reforms	Change in VAT Rate	Mean VAT After Reform	Standard Deviation	Minimum	Maximum
	(1)	(2)	(3)	(4)	(5)	(6)
VAT Changes	1,092	1.41	16.47	5.80	0	27
VAT Increases	949	2.21	16.61	5.78	4.5	27
VAT Decreases	143	-3.91	15.58	5.87	0	21

Notes: Column (1) shows the number of VAT reforms considered; Column (2) shows the average change in the VAT rate in the month of the reform; Columns (3)-(6) display summary statistics for the VAT rate in the month of the reform. All summary statistics are constructed using the maximum VAT rate applied to each commodity.

Table 2.2: 2SLS Estimates of Pass-Through to Prices using Full Sample

	VAT Increase				VAT Decrease			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
VAT Rate	0.17**	0.36***	0.39***	0.50***	0.29***	-0.0014	0.047	0.23**
	(0.072)	(0.10)	(0.11)	(0.12)	(0.099)	(0.13)	(0.10)	(0.11)
Treat	0.027***	-0.0062***	-0.0099***	0.034***	0.025***	-0.013***	0.0024	0.048**
	(0.0036)	(0.0017)	(0.0036)	(0.012)	(0.0077)	(0.0044)	(0.015)	(0.024)
After	0.0099***	-0.0069***	0.012***	0.0028**	0.0034	-0.011***	0.024	-0.036*
	(0.0023)	(0.0022)	(0.0044)	(0.0014)	(0.0029)	(0.0036)	(0.017)	(0.018)
Log(Country Price)		0.53***	0.57***	-0.86**		0.43***	-0.23	-1.94**
		(0.13)	(0.093)	(0.37)		(0.14)	(0.50)	(0.92)
Log(EA19 Price)		0.61***	0.70***	0.66***		0.73***	0.79***	0.75***
		(0.14)	(0.17)	(0.16)		(0.16)	(0.22)	(0.21)
R^2	0.0090	0.46	0.49	0.53	0.011	0.57	0.61	0.64
Observations	38,150	38,150	38,150	38,150	5,460	5,460	5,460	5,460
First Stage F-Stat	1915.8	1770.5	1730	1677.1	148.7	149	147.8	145.6
Time FE	No	No	Yes	Yes	No	No	Yes	Yes
Country FE	No	No	No	Yes	No	No	No	Yes

Notes: The coefficient reported in the table indicate the pass-through of the VAT tax to prices estimated using specification (2.1). The control group is the commodity price in the same month one year earlier. Country price is computed as a weighted average of the prices in the 2-digit COICOP groups excluding the group to which the commodity belongs. EA19 price is computed by a weighted average of prices for the same commodity in other EA19 country. Standard errors are clustered by COICOP. * p<0.10, ** p<0.05, *** p<0.01

Table 2.3: 2SLS Estimates of Pass-Through to Prices using Restricted Sample

	VAT Increase				VAT Decrease			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
VAT Rate	0.29***	0.34***	0.35***	0.38***	0.30***	-0.031	0.12	0.25**
	(0.085)	(0.074)	(0.075)	(0.081)	(0.11)	(0.14)	(0.11)	(0.13)
Treat	0.029***	-0.0076**	-0.017**	0.050**	0.025***	-0.012***	0.024	0.051**
	(0.0049)	(0.0030)	(0.0070)	(0.021)	(0.0080)	(0.0047)	(0.017)	(0.025)
After	0.0064*	-0.011***	0.0011	0.030***	0.0031	-0.011***	0.052**	-0.067**
	(0.0037)	(0.0024)	(0.0085)	(0.0099)	(0.0028)	(0.0035)	(0.021)	(0.028)
Log(Country Price)		0.48***	0.64***	-1.15**		0.44***	-1.05*	-2.12**
		(0.12)	(0.14)	(0.54)		(0.14)	(0.63)	(0.98)
Log(EA19 Price)		0.89***	1.06***	0.99***		0.71***	0.76***	0.74***
		(0.17)	(0.23)	(0.19)		(0.17)	(0.22)	(0.21)
R^2	0.0049	0.49	0.55	0.60	0.0073	0.55	0.61	0.62
Observations	11354	11354	11354	11354	5222	5222	5222	5222
First Stage F-Stat	543.9	551.8	544.1	528	224.4	226.3	211.7	220.7
Time FE	No	No	Yes	Yes	No	No	Yes	Yes
Country FE	No	No	No	Yes	No	No	No	Yes

Notes: The coefficient reported in the table indicate the pass-through of the VAT tax to prices estimated using specification (2.1). The control group is the commodity price in the same month one year earlier. Country price is computed as a weighted average of the prices in the 2-digit COICOP groups excluding the group to which the commodity belongs. EA19 price is computed by a weighted average of prices for the same commodity in other EA19 country. Standard errors are clustered by COICOP. * p<0.10, ** p<0.05, *** p<0.01

Chapter 3

Geographic Mobility of Liquidity Constrained Unemployed Workers

3.1 Introduction

Local unemployment shocks during the Great Recession have varied significantly across states yet overall migration responses have been low, raising the concern among policy-makers that a mismatch between workers skills and skills required for available jobs may be slowing economic recovery. On the other hand many of the theories that have been offered to explain the recent decline in migration in the United States have stressed the financial constraints unemployed workers face at unemployment. A growing literature has in fact provided evidence that unemployed workers have little or no financial assets at unemployment (Browning and Crossley 2001, J. X. Sullivan 2008, Shimer 2008, Rothstein and Valletta 2014), which makes more unlikely to afford moving costs.

The main contribution of this paper is to use plausibly exogenous variation in unemployment benefits across time and U.S. states in the period 2001-2012 to provide empirical evidence on how unemployment insurance has affected mobility decisions of unemployed workers. Specifically, I estimate a Cox proportional hazard model of mobility during the unemployment spell, and find that a one percent increase in the unemployment insurance (UI) weekly benefit amount leads to a 0.33 increase in the hazard rate of moving over the unemployment spell. In addition, I find that the effect on mobility is largest for the most liquidity constrained unemployed workers, and provide evidence supporting the conclusion that higher unemployment benefit amounts act as a moving subsidy to households with low wealth.

The model I use to explain the empirical findings is closest to Moretti 2011, with some main differences. First of all, the model focuses specifically on unemployed workers and analyses migration decisions in a dynamic setting. Unemployed workers are assumed to live for two periods and all live in the same location in the first period. Each agent faces two main decisions: (1) how much to save/borrow across periods and (2) whether to move

in the second period. Secondly, monetary housing costs are relevant for the mobility decision. Unemployed workers choose optimal location based on housing costs and employment prospects, as well as monetary costs and idiosyncratic preferences for location. Thirdly, individual agents are risk-averse, making inter-temporal saving decisions desirable. Fourthly, the model distinguishes between liquidity constrained and unconstrained unemployed workers, with liquidity constrained workers assumed to be unable to save/borrow across periods.

The model offers three main predictions which match the data: (1) for large enough moving costs, unemployed workers are more likely to move if they are not liquidity constrained, (2) the higher the moving costs, the larger the difference in mobility between liquidity constrained and unconstrained workers and (3) the higher the moving costs, the more effective unemployment benefits are in reducing the mobility rates between constrained and unconstrained unemployed workers.

The results have strong implications for public policy. While the idea that unemployed workers would benefit from receiving moving subsidies is not new to the literature (Ransom 2014), this paper is the first to show that higher UI would mostly benefit mobility decisions of liquidity constrained households. The main policy prescription is that an asset-based unemployment insurance would help achieve a better match between workers and firms, with the benefit of increasing the lifetime tax payments of each individual. While higher UI benefits potentially raises welfare, previous work has also highlighted the negative effect of UI benefits on precautionary saving,[1]

The rest of the paper is organized as follows. Section 3.2 relates this work to the previous literature, section 3.3 describes the institutional setting, sections 3.4 and 3.5 define the theoretical framework and the testable predictions of the model. Section 3.6 presents the data sources used in this paper, while section 3.7 illustrates the empirical framework. Sections 3.8 and 3.9 present the main results of the paper, while sections 3.10 to 3.14 are devoted to robustness checks. Finally section 3.15 concludes.

3.2 Previous Literature

This work draws on findings from four different literatures: (1) determinants of workers' migration decisions, (2) migration decisions and local labor market shocks, (3) unemployment insurance and liquidity constraints, and (4) unemployment insurance and geographic mobility.

First, this study adds to the discussion on the determinants of workers' migration. Traditionally, economists have looked at migration decisions as human capital investment (Borjas 1987) and as part of a search and matching problem (Dahl 2002, Kennan and Walker 2011), and spatial equilibrium models have been based on the assumption that migration equalizes utility across space and that it is frictionless (Tiebout 1956,S. Rosen 1979,Roback 1982).

[1]See Engen and Gruber 2001 for a discussion of the crowd-out effect of UI benefits on households financial asset holdings. Other studies that have studied the interaction between UI and precautionary savings include Sheshinski and Weiss 1981, L. J. Kotlikoff 1987 and Hubbard and Judd 1987.

More recently, the attention has shifted to explaining more recent changes in mobility patterns and spatial equilibrium models have allowed for individual heterogeneity in tastes for location and mobility frictions (Kline 2010, Moretti 2011). This paper adds to the literature by proposing a partial equilibrium model of mobility for unemployed workers, and shows how unemployment benefits can help migration decisions.

Secondly, this study contributes to the growing literature on migration and local labor market shocks. Many recent studies have looked at migration as an insurance mechanism against local unemployment shocks (Blanchard et al. 1992, Glaeser and Gottlieb 2009, Kennan and Walker 2011, Moretti 2011), and have proposed different explanations for the recent decrease in geographic mobility. A large number of papers has put the emphasis on the importance of underwater mortgages (Ferreira, Gyourko, and Tracy 2010 , Donovan and Schnure 2011). Instead, Kaplan and Schulhofer-Wohl 2012 concluded that workers' mobility has recently decreased because of better information and less concentrated geographic specificity of skills. Most empirical evidence seems to agree with the findings of a recent paper by Yagan 2014 which, using panel data from the IRS, finds that migration insurance was low during the Great Recession. This study adds to the literature by offering an alternative an alternative (though not inconsistent) explanation of why geographic mobility might have declined in the last decade.

Thirdly, this work relates to the extensive literature on unemployment insurance (UI), which puts the emphasis on the insurance value of unemployment insurance programs. Higher consumption smoothing (Gruber 1994, Browning and Crossley 2001, Kroft and Matthew J. Notowidigdo 2011 and Schmieder, Wachter, and Bender 2012), lower liquidity constraints (Chetty 2008a) and higher reservation wages (Mortensen 1976) and Shimer and Werning 2005 are some among the most praised benefits of UI. While Chetty 2008a finds that the increase in unemployment duration caused by UI benefits is mainly due to liquidity constraints then moral hazard, Kroft and Matthew J. Notowidigdo 2011 and Schmieder, Wachter, and Bender 2012 investigate heterogeneity in consumption smoothing effects over the business cycle, and Studies on reservation wages effect of UI benefits include Mortensen 1976 and Shimer and Werning 2005. This work adds to that literature by showing that unemployment benefits is particularly beneficial to more liquidity constrained workers, to which it provides the means to use migration as an insurance mechanism.

Finally, this paper ties loosely with the literature on the relationship between unemployment benefits and geographic mobility. While standard theoretical results (Lippman and McCall 1979, Mortensen 1976) suggest that unemployment benefits reduce search effort and geographic mobility by increasing the reservation wage, not all the theoretical literature on the subject has agreed on this conclusion. For instance Barron and Mellow 1979, Tannery 1983 and Ben-Horim and Zuckerman 1987 have emphasized that increased expenditures linked to higher unemployment benefits have positive effects on search productivity. The empirical evidence is very limited. Hassler et al. 2005 argues that the difference in unemployment benefits between Europe and the United States explains differences in geographic mobility, with Europe having more generous unemployment benefits and lower mobility, while Tatsiramos 2004 focuses on European labour markets and argues that unemployment

benefits enhance mobility offsetting the negative effects of benefits on the incentive to move. For the U.S., a recent study by Hsu, Matsa, and Melzer 2013 shows that higher unemployment benefits reduce mortgage default and therefore homeowners' mobility. By using state-year variation in UI, this paper contributes to the literature by establishing a direct link between geographic mobility and unemployment insurance in the United States.

3.3 Institutional Setting

The unemployment insurance (UI) program in the United States is a federal program, but the level and duration of benefits is set by each state and can change over time. Unemployed individuals are only eligible to receive benefits if they lose their job through no fault of their own (typically a permanent or temporary layoff), and are required to be actively searching for jobs and to be available to work. A worker's benefit rights are established using wages and employment in base period, a 12 to 15 months period of time preceding the beginning of the unemployment spell. For most states, the base period spans the first four of the last five calendar quarters before the onset of unemployment.

In general, state UI laws vary greatly across states, with eligibility and the weekly benefit amount generally depending on either (1) the base period wage, (2) the base period's highest quarter wage, or (3) the number of hours worked in the base period. State laws also fix the maximum and minimum weekly UI benefit amounts made available to eligible unemployed workers. On top of the basic UI benefit some states allow unemployed workers to claim dependent allowances, with the amount of the allowance and the definition of dependent changing across states.

More importantly for the analysis of this paper, eligibility, weekly benefit amounts and duration of benefits are determined in the state in which wages are earned. When a worker moves to a different state, the Interstate Benefit Payment Plan allows UI benefits to still be paid by the state in which the worker has earned qualifying wages. Overall, and though maximum weekly benefit amounts have increased over time in nominal terms, figure 3.1 shows that the population weighted median real maximum weekly benefit amount has remained stable.

While UI duration is also determined from a worker's base period wage, potential UI duration in each state depends on the state's unemployment rate. Regular unemployment benefits are generally available for 26 weeks, while extended benefits and temporary benefits are made available in periods of high unemployment. The Extended Benefit (EB) program started in 1970 and allows unemployed individuals to receive up to an additional 20 weeks of unemployment compensation in states where the level and change in the state unemployment rate lies above a specified threshold, generally 6.5 percent for 13 weeks of extensions and 8.0 percent for 20 weeks. The cost of these extended benefits is evenly split between the federal government unemployment trust fund and the states that benefit from it.

Temporary unemployment relief programs have also been available in periods of particularly strong labor market distress, creating large variation in UI benefit potential duration

across time and states, as shown in figure 3.1. In particular, two major programs were introduced in the 2000s.

Firstly, the Temporary Extended Unemployment Compensation program of 2002 (TEUC02) lasted from 2002 until 2004 and provided extra weeks of unemployment benefits to unemployed workers who had received all regular unemployment benefits available to them. Funded entirely by the federal government, the TEUC program had a two-tiered program: (1) up to 13 weeks of TEUC were available to eligible unemployed workers in all states, (2) up to 13 additional weeks of benefits were available (TEUC-X) in states where an "Extended Benefit" period was in effect and an individual exhausted the first tier of TEUC.

Secondly, the Emergency Unemployment Compensation program of 2008 (EUC08), which initially provided 13 additional weeks of UI benefits in all states, and was modified several times. In particular, a second tier was implemented in late 2008 and provided up to 33 weeks of additional benefits, with benefits for weeks 21-33 made available if a state's total unemployment rate (TUR) was above 6 percent or if the insured unemployment rate (IUR) was higher than 4 percent. Tiers three and four were added in November 2009 and introduced two major changes: (1) tier two eligibility was expanded to 14 weeks, (2) tier two's unemployment rate threshold was eliminated and applied to tier three eligibility (an additional 13 weeks) (3) tier four eligibility (6 weeks) was based on a TUR of at least 8.5 percent or an IUR of at least 6 percent. [2]

3.4 Theoretical Framework

This section presents a two period model of saving and location decisions for unemployed workers, whose main goals are to illustrate the link between unemployment benefits and location decisions of unemployed workers and to explain the empirical findings. The model combines the public finance literature on the consumption smoothing benefits of unemployment benefits with the labor economics literature on migration choices.

While the public finance literature generally uses a CRRA utility function to model individual risk aversion,[3] I use a quadratic instantaneous utility function which simplifies algebra considerably, while not changing the main predictions of the model. In addition, initial assets are set equal to zero for all individuals, mostly because the difference between constrained and unconstrained workers is modelled by considering two scenarios: one in which borrowing is allows and one where it is not.

Differently from the previous literature on the determinants of mobility decisions, I assume individual risk aversion and use a two-period model.[4] At the same time, my model differs from the spatial equilibria models used in the literature (Roback 1982, Moretti 2011,

[2]Other unemployment insurance programs have become available due to specific contingencies, and are not considered in this study. These include the Disaster Unemployment Assistance, the Unemployment Compensation for Federal Employees, the Trade Readjustment Allowances, and the Self-Employment Assistance.

[3]See the seminar work of Baily 1978, as well as more recent work by Chetty 2004 and Chetty 2008b.

[4]Most of the previous literature assumes risk-neutrality and uses a static one-period model.

Diamond 2012, Serrato and Zidar 2014) in that it is a partial equilibrium rather than a general equilibrium model. This is for two main reasons. First and mainly, the general equilibrium effects on local rents, wages and amenities are likely not very substantial given that unemployed workers are a small share of the population. Secondly, a partial equilibrium model is more tractable. Studying general equilibrium effects would for example require adding employed workers to the model, complicating the model while also not changing its main predictions.

The main ingredients of the model can be summarized as follows: I assume that unemployed workers live for two periods, all reside in the same location in the first period, and decide whether to move in the second period, depending on the relative level of rents, job-finding rates, monetary moving costs [5] and individual idiosyncratic preferences for locations. [6] Individuals are risk averse, and are allowed to save across time periods to smooth their consumption.

Some important simplifications are made in the model which, though admittedly restrictive, would not change the main predictions of the model. First of all, labor is assumed to be homogeneous in skills and tastes, as in Roback 1982. All employed workers are paid the same wage and labor supply is inelastic, with each worker supplying one unit of labor. Secondly, neither the individual discount rate nor the real interest rate matter for individual saving decisions.

In my setup, unemployed workers' preferences are modelled with a quadratic instantaneous utility function. Each unemployed worker maximizes his lifetime utility, which is the sum of the first period's instantaneous utility and the utility expected in the second period. In period 1 all individuals are unemployed, they reside in city a and receive a fixed unemployment benefit b_i, while also paying rent r_a. Those individuals who decide to move to a different city also pay a monetary moving cost k in period 1. In period 2 individuals find a job with probably ϕ_c ($c = a, b$), in which case they receive a wage w (with $w > b$), and stay unemployed otherwise. General equilibrium effects on both local rents and job finding rates are ignored, so that both rent levels and job finding rates are exogenous to the model. As a result, the lifetime utility function of an unemployed worker's living in city $c = a, b$ in period 2 can be written as [7]

$$U_{ic} = u(b - r_a - s - k_c) + [\phi_c u(w - r_c + s) + (1 - \phi_c)u(b - r_c + s)] + e_{ic} \tag{3.1}$$

where s indicates individual saving.

Unconstrained Case In period 1, unemployed workers are faced with the decision of how much to save for period 2. If they plan on staying in city a in period 2, their optimal saving

[5] In this model monetary costs of moving are assumed to be constant across unemployed workers, though this assumption can be relaxed.

[6] Amenities are not explicitly considered in my model, mostly for analytical tractability.

[7] Given that explaining inter-temporal consumption patterns is not the main object of interest, and to simplify algebra, I impose the restriction $\beta = \frac{1}{1+r} = 1$. Furthermore, as discussed in the technical appendix: $k_a = 0$ and $k_b = k$.

is given by:

$$s_a^u = -\frac{\phi_a(w-b)}{2}$$

Intuitively, the higher their expected future gain from employment in period 2, the higher the amount borrowed in period 1. If instead they decide to move to city b, they have to pay a monetary cost of moving k, and their optimal saving takes the form:

$$s_b^u = -\frac{(r_a + k - r_b) + \phi_b(w-b)}{2}$$

which is again a function of the expected income gain from employment, but also of the rent difference between cities and the monetary moving cost. A direct implication is that the higher r_a, k or ϕ_b, the more unemployed worker will be borrowing in period 1. In order to decide whether to move or to stay, unemployed workers will compare the lifetime utility between staying in city a and moving to city b in period 2, given their optimal saving:

$$\begin{aligned} V_{ia}^u - V_{ib}^u &= (w-b)[\phi_a(1 - \frac{w+b}{2} + r_a) - \phi_b(1 - \frac{w+b}{2} + r_b)] + (r_b + k - r_a)(1-b) + \\ &+ \frac{1}{2}(r_a^2 + r_b^2 + k^2 + 2kr_a) + \frac{1}{4}\phi_a^2(w-b)^2 - \frac{1}{4}[(r_a + k - r_b) + \\ &+ \phi_b(w-b)]^2 + (e_{ia} - e_{ib}) \\ &= (v_a^u - v_b^u) + (e_{ia} - e_{ib}) \end{aligned}$$

where $(v_a^u - v_b^u)$ is the difference in average utility between staying in city a and moving to city b in the second period when the unemployed worker is unconstrained. If $V_{ia}^u > V_{ib}^u$ an unconstrained unemployed worker will prefer to stay in city a. Intuitively, the number of unemployed workers staying in city a depends on the difference in average utilities that an unemployed worker gets in the two places and on each individual's idiosyncratic preferences for location.

If I assume that idiosyncratic preferences for location $\frac{e_{ia}-e_{ib}}{q} \sim Logistic(0,1)$, and I normalize the total population of unemployed workers to one, I can also determine the fraction of unemployed workers staying in city a:

$$N_a^u = \Lambda(\frac{v_a^u - v_b^u}{q}) = \frac{exp(\frac{v_a^u - v_b^u}{q})}{1 + exp(\frac{v_a^u - v_b^u}{q})}$$

This is positively related to city a's job finding rate, to the difference between wages and unemployment benefits and to the monetary mobility cost. It is negatively related to the rent difference $(r_a - r_b)$ and unemployed workers' mobility (as described by the parameter q is).

Constrained Case In this case, the worker is not able to lend/borrow money across periods and therefore saving across periods is always equal to zero, so $s_a^c = s_b^c = 0$. The

difference in lifetime utility between staying in city a and moving to city b is given by:[8]

$$\begin{aligned} V_{ia}^c - V_{ib}^c &= (w-b)[\phi_a(1 - \frac{w+b}{2} + r_a) - \phi_b(1 - \frac{w+b}{2} + r_b)] + (r_b + k - r_a)(1-b) + \\ &+ \frac{1}{2}(r_a^2 + r_b^2 + k^2 + 2kr_a) + (e_{ia} - e_{ib}) \\ &= (v_a^c - v_b^c) + (e_{ia} - e_{ib}) \end{aligned} \tag{3.2}$$

As in the unconstrained case, if one assumes that idiosyncratic preferences for location are distributed $\frac{e_{ia}-e_{ib}}{q} \sim Logistic(0,1)$, and the total number of unemployed workers is normalized to one, then the number of unemployed workers living in city a is given by:

$$N_a^c = \Lambda(\frac{v_a^c - v_b^c}{q}) = \frac{exp(\frac{v_a^c - v_b^c}{q})}{1 + exp(\frac{v_a^c - v_b^c}{q})}$$

The main difference between the constrained and unconstrained cases is that individuals can smooth consumption whether the decide to stay in city a or not. Therefore, while the difference in the utility they get in each city depends on their ability to smooth consumption in the unconstrained case, this is no longer the case in the constrained case, where rents, job finding rates and unemployment benefits have a different effect, as illustrated in the next section.

3.5 Predictions of the Model

In this section I illustrate three main implications of the model, which help us build a link between the theory and the empirical evidence. In particular, I focus on monetary moving costs k which, as the model above shows, reduces the number of unemployed workers moving to city b in period 2.

Numerical simulations displayed in figure 3.4 provide graphical evidence on the importance of monetary moving costs.[9] Panel (a) compares the average utilities v_a and v_b in the constrained and unconstrained case, and under alternative values of k. It shows that, whether constrained or unconstrained, higher k reduces the utility of moving v_b, making it less likely for an unemployed worker to move. This is confirmed by panel (b) and (d) of the same figure, which show that the fraction of individuals moving to city b is a decreasing function of monetary moving costs.

In addition, k matters differently depending on the gain from moving in terms of employment opportunities (and rent differences). Comparing panels (b) and (d) of figure 3.4 shows that more unemployed workers move when the difference in job finding rates across cities

[8] See derivation in the appendix

[9] In the simulations I set the model parameters to: $q = 0.5$, $w = 1$, $r_a = 0.3$, $r_b = 0.3$. In addition, I compare two alternative scenarios: (1) high gain from moving $\phi_a = 0.5$, $\phi_b = 0.9$ (panels (a) and (b)), and (2) low gain from moving $\phi_a = 0.5$, $\phi_b = 0.6$ (panels (c) and (d)).

$\phi_a - \phi_b$ is higher (alternatively, when the rent difference $r_a - r_b$ is lower), also implying that monetary moving costs have less of an impact. The following three predictions illustrate further how moving costs are relevant for unemployed workers mobility decisions.[10]

Prediction 1: *For given differences in local rents $r_a - r_b$ and job finding rates $\phi_a - \phi_b$, and for a given employment premium $w - b$, unconstrained unemployed workers are more likely than constrained unemployed workers to move to city b if $k > \underline{k}$. Moreover, The higher the monetary moving costs k, the more constrained unemployed workers prefer to stay in city a relative to unconstrained workers, and therefore the fewer constrained workers move to city b.*

Intuitively, differences in location decisions between constrained and unconstrained workers depend on the utility gain of smoothing consumption across periods, which unconstrained workers benefit from while constrained workers don't. Panels (a) and (c) of figure 3.4 show that the consumption smoothing benefit of an individual who stays in city a in period 2 is independent of moving costs, while the consumption smoothing benefit of an individual who moves to city b increases with moving costs. The main reason for this is that the higher the monetary moving cost, the more unconstrained workers are likely to borrow to pay for the moving cost in the first period , an opportunity that constrained workers do not have. A direct implication is that with high enough moving costs, the consumption smoothing benefit of moving to city b is larger than the consumption smoothing benefit of staying in city a, implying that more unconstrained workers will move to city b in the second period, that is $N_a^u < N_a^c$.

The second sentence of prediction 1 is related to the first one, in that a higher cost of moving increases $v_b^u - v_b^c$, the value of consumption smoothing for individuals moving to city b, while at the same time leaving the consumption smoothing benefit of staying in city a $v_a^u - v_a^c$ unaffected. Simulations in both panel (b) and (d) of figure 3.4 show that the difference between the fraction of unconstrained and constrained unemployed workers moving to city b is increasing with the monetary moving cost k. Intuitively, while unconstrained workers can borrow against moving costs, while constrained workers cannot and therefore there are relatively fewer constrained unemployed workers who move as moving costs get larger.

[10] See appendix for proofs.

Prediction 2: *Higher unemployment benefits increase mobility if monetary moving costs $k > \underline{k}$, while they decrease mobility if $k < \underline{k}$.*

This is shown in figure 3.5, where I assume that moving to city b is desirable because the job finding rate is larger. [11]

The figure compares the effect of UI benefits on the fraction of workers moving to city b under different assumptions on the monetary moving costs. While UI benefits decrease geographic mobility if monetary moving costs are low, the difference between constrained and unconstrained workers decreases with higher unemployment benefits. With a low moving cost (10 percent of wage income) individuals staying in city a in equilibrium have strong idiosyncratic preferences for location and increasing UI benefits further reduces the value of moving given that, the employment premium $w-b$ is reduced. With a high moving costs (80 percent of wage income), more individuals are staying in city a not because they prefer to be there but because they cannot afford to move. In this case higher unemployment benefits help mobility.

Prediction 3: *Higher unemployment benefits reduce the difference in mobility rates between constrained and constrained workers. Moreover, the higher the monetary moving costs k, the more effective unemployment benefits are in reducing mobility differences between constrained and unconstrained unemployed workers and helping constrained unemployed workers move to city b, that is* $\frac{\partial[v_a^c - v_b^c] - [v_a^u - v_b^u]}{\partial b} < 0$.

For high enough k, higher unemployment benefits help both constrained and unconstrained workers by acting as a moving subsidy, though the marginal effect on constrained unemployed workers relative to unconstrained ones increases with the monetary moving cost k, so that the effect of UI benefits in reducing mobility differences between constrained and unconstrained unemployed workers is increasing with k.

3.6 Data

In my analysis I use data from the Survey of Income and Program Participation spanning the period 2001-2012. The SIPP is a nationally representative survey designed to provide accurate information about program participation in the United States. Individuals are followed for two to four years, and are interviewed every four months, when detailed information on individual, family and household characteristics is collected as part of the *core modules*. The SIPP is the most suitable publicly available data for the analysis performed in this study as it also contains monthly information on geographic mobility, as well as detailed information on household wealth, collected once a year as part as its *topical modules*. Specifically,

[11] Similarly, one could assume that the rent level is lower in city b and get a comparable prediction. In figure 3.5 I also assume that the rent level is the same across cities.

geographic mobility is assessed by asking individuals whether they have moved from the previous month, and the nature of the move.[12]

In the analysis, I restrict the sample to unemployed workers who (1) are between 25 and 65, (2) experience at least one job separation, (3) have at least three months of work history and have been included in the panel for at least three months, (4) are not on temporary layoffs, (5) are actively searching for a job and (6) report to have received UI benefits throughout their unemployment spell. [13] These restrictions leave 7,220 unemployment spells.

In other to assign weekly benefit amounts to each individual I use bi-annual information on state-level UI laws from the U.S. Department of Labor. Twice a year (January and July) the Employment and Training Administration publishes the *Significant Provisions of State Unemployment Insurance Laws*, which contains detailed information on maximum and minimum weekly benefit amounts, dependent allowances, as well as formulas that are used in each state to calculate weekly benefit amounts.

All variables used in the regression analysis are expressed in real variables using three alternative measures of cost of living. The first measure, which account for changes in the cost of living at the national level, is the 2000 CPI-U series. The other two measures, which allow the cost of living to vary across states, are constructed using information from the *American Community Survey* (ACS) and the *Bureau of Labor Statistics* (BLS) and follow closely the methodology developed in Moretti 2008. The first measure of local cost of living is analogous to Moretti's "Local CPI 1", in which housing costs are allowed to vary across states while non-housing costs are kept constant. The second measure is comparable to Moretti's "Local CPI 2", and allows both housing and non-housing costs to vary across states. Differently from Moretti, particularly because the SIPP only reports the state of residence, my local CPI measures vary across states rather than across metropolitan areas.[14]

Table 3.2 displays summary characteristics for the sample of unemployed workers used in the analysis. The median UI recipient is a white unmarried high-school graduate in his mid-thirties, with mean pre-unemployment wage of 28598 dollars, and average replacement rate around 48 percent. In the data, the average weekly benefit amount received is around 261 dollars per week, only slightly less than the average weekly benefit amount reported by the U.S. Department of Labor. In addition, table 3.2 shows that the nominal actual weekly benefit amounts have been increasing over time. This trend seems partly related to the fact that the maximum weekly benefit amounts have been increasing over time, at least in

[12]Though only the first move in each wave of data is reported in the survey, this feature is not crucial for the purpose of my analysis, where the the object of interest is the event of moving rather than the number of moves.

[13]These restrictions are comparable to what has been used in the previous studies focusing on unemployment insurance in SIPP (Chetty 2008a, Kroft and Matthew J. Notowidigdo 2011), with the exception of (1), which I impose mainly because wealth is measure at the household level, and as young individuals are more likely to live with other family members, household wealth is a poor measure of liquidity constraints for them.

[14]See appendix for details about how the local CPI indexes are constructed.

nominal terms, and partly to the increasing share of the population claiming unemployment insurance, as illustrated in figure 3.2. With more individuals claiming UI benefits, the income of the average claimant has also increased over time.

At the same time, data show that statutory unemployment durations have also increased over time, as illustrated in table 3.2, particularly because of the rising state unemployment rates, which have increased the number of available UI weeks. This is reflected in the data, where the actual duration of unemployment spells among unemployed workers has increased from 31 weeks in the 2001 panel to 45 weeks in the 2008 panel. In recent years, this increase has been driven by the availability of extended benefits as well as the Emergency Unemployment Compensation (EUC) program of 2008. As shown in panel A of figure 3.1, available weeks of unemployment duration during the Great Recession to reach a maximum of 99 weeks.

However, while state unemployment rates have affected UI duration, there is not a strong evidence that they have also driven changes in UI weekly benefit amounts. This is illustrated in Kroft and Matthew J. Notowidigdo 2011, who show that though changes in the national unemployment rate are highly correlated with changes in the average statutory maximum UI benefit level (averaged across states each year), state UI benefits are exogenous to local labor market conditions. This evidence is confirmed by our findings, as shown in figure 3.6, which plots the correlation between the state unemployment rate and the state maximum weekly benefit is -0.031.[15]

3.7 Empirical Strategy

In order to estimate the effect of UI benefits on the geographic mobility of unemployed workers, I use variation in the level of benefits caused by changes in UI laws. Specifically, I use a Cox proportional hazard model to estimate whether more generous unemployment benefits at the onset of the unemployment spell affect the hazard rate of moving during the unemployment spell:

$$\log h_{ihst} = \beta_0 + \beta_1 b_{is} + \beta_2 X_i + \beta_3 Z_h + \pi_s + \pi_{year} + u_{ihst}$$

where h_{ihst} is the hazard rate of moving for an unemployed worker i from household h and living in state s at t weeks from job separation. X_i is a vector of individual characteristics at the beginning of the unemployment spell, and includes age, gender, education, marital status, race, and real annual wage (in logs) while Z_h is a vector of household characteristics and includes number of kids, whether the spouse is working and housing tenure. In addition, the baseline specification controls for state and year fixed effects, the state unemployment rate at job separation, as well as a dummy variable for being on the "seam" between two waves.

[15]Therefore, though the empirical analysis defined below controls flexibly for the state unemployment rate, it seems unlikely that the variation in weekly benefit amounts is driven by changes in the state unemployment rates.

Adjusting for "seam bias" is necessary in the SIPP, where individuals are interviewed at four months intervals about the previous four months, and tend to report a disproportionally high number of changes on the seam between two waves (Moore 2008, Ham, Li, and Shore-Sheppard 2009).

In my analysis, I use use different measures of UI benefits. First, I construct individual weekly benefit amounts and replacement rates using state unemployment insurance (UI) laws at job separation. To do so, I take advantage of the panel structure of the data and construct pre-unemployment wages for each individual. This information allows me to compute relevant variables for UI benefit calculations such as the base period wage and the highest quarter wage. Though measurement error and inadequate information are a concern in measuring pre-unemployment wages accurately, this first measure has the benefit of assigning a weekly benefit measure to each individual, creating interesting variation for my analysis.[16] To check that the results are robust, I then use states statutory maximum weekly benefit amounts, as well actual average weekly benefit amounts using data from the Department of Labor, as done by the previous literature on unemployment insurance (Chetty 2008a,Kroft and Matthew J. Notowidigdo 2011).

In order to evaluate the heterogeneous effects of unemployment benefits on groups with different wealth, I use a stratified Cox hazard model, where j denotes a particular value of the characteristic of interest, each group is assumed to have a different baseline hazard of moving, and β_1^j measures the effect of unemployment benefit generosity on group j:

$$\log h_{ihjst} = \beta_{0,t}^j + \beta_1^j w_{ijs} b_{is} + \beta_2 X_{ij} + \beta_3 Z_{hj} \pi_j + \pi_s + \pi_{year} + u_{ihjst}$$

where w_{ijs} defines the wealth group j to which an individual i living in state s belongs.[17] In order to assign each individual to a wealth group, I use four alternative measures of household wealth.

The first measure is household net liquid wealth, which has been used in the previous literature on unemployment benefits and liquidity constraints (Chetty 2008a). Net liquid wealth equals total household wealth minus housing equity, business equity, vehicle equity and unsecured debt and provides a reliable measure of liquidity constraints. In fact, it assigns a value to assets that can be converted into cash quickly and at the same time without having a substantial impact on the price at which they are valued.

A second related measure is liquid wealth which, differently from the previous measure, also includes unsecured debt. This is a slightly more comprehensive measure than net liquid wealth as it takes into account all credit card debt, utility bills, medical bills, and other type of loan or credit extended without a collateral requirement. While there is evidence that high unsecured debt acts as a safety net for households with low assets (Gruber 2001, Babiarz, Widdows, and Yilmazer 2013) [18] and therefore allows them to smooth consumption

[16] Figure displays the average simulated weekly benefit amount for each state in the period 2001-2012.

[17] Throughout the analysis I divide the sample in two different ways: (1) household wealth is above or below median, (2) household wealth by quartile.

[18] However J. X. Sullivan 2008 shows that unsecured debt does not help households at the very low end of the asset distribution, which do not have sufficient access to unsecured credit.

over transitory unemployment spells (B. D. Meyer and J. X. Sullivan 2003, J. X. Sullivan 2008), it is also a feature of wealthier households, which have more access to credit. The data, as reported in table 3.2, show that unemployed workers in the fourth net liquid wealth quartile have more than 7 thousand dollars of unsecured debt, while those in the first wealth quartile have around 25 thousand dollars in unsecured debt, with the effect of increasing the dispersion in wealth across individuals compared to the previous measure of wealth.

My third and fourth measures of household wealth are total wealth and total net worth, which is defined as total wealth minus unsecured debt. While not focusing explicitly on unemployed workers, illiquid components of wealth have also been used in previous studies, particularly housing equity (Zeldes 1989, Hurst and Lusardi 2004, Fairlie and Krashinsky 2012) and total net worth (Evans and Jovanovic 1989). The main reason I add them is that while including assets and liabilities that are illiquid, total wealth and total net worth give a more comprehensive picture of household financial resources.

While the four household measures used in the analysis offer a comprehensive picture of liquidity constraints, two limitations of the wealth measures used in this study are worth being stressed. First, wealth variables are defined at the household rather than at the individual level. As this is not as not so much of a concern for individuals who have their own nuclear family, it is a source of bias when one includes individuals living with older family members.[19] More importantly, detailed household wealth variables are only available once a year in the SIPP. Therefore each individual is assigned the wealth observed most recently before job separation, instead of the current wealth at unemployment. A direct implication is also that workers who only have household wealth measured after job separation are excluded from the analysis [20]

3.8 Graphical Evidence

I begin by providing graphical evidence on the hazard rate of moving during the unemployment spell. Figure 3.7 shows that in my sample most of the moves occur in the first four months after job separation. In particular the blue bars show the fraction of unemployed workers still in the sample n months after job separation, while the black line displays the hazard rate of moving, conditional on being in the sample. It shows that while unemployed workers exit the unemployment spell over time, the fraction of them moving (conditional on still being unemployed) is largest in the few months of the unemployment spell.

I then plot nonparametric Kaplan-Meier survival curves for mobility during the unemployment spell and divide the sample into two groups, based on whether a worker has become unemployed in states and years in which the unemployment weekly benefit (WBA) was above or below the median. Panel (a) of figure 3.8 shows that the hazard rate of mobility is higher for unemployed workers who receive more generous weekly benefit amount, which amount

[19] This motivates the choice of excluding from the analysis individuals below 25.

[20] As pointed out in Chetty 2008a, including wealth measures observed after job loss would create an endogeneity issue.

amounts to 313.51 dollars per week on average. Panel (b) and (c) of the same figure show similar evidence using the state's statutory maximum WBA and the state's average WBA paid.

While more generous unemployment benefit seem to help geographic mobility, figure **??** investigates whether more generous UI benefits have different effects on individuals whose household wealth is different. Stratifying the sample by household net liquid wealth, one of the wealth measures used in the empirical analysis, it shows that individuals coming from less wealthy households benefit more from the UI benefits, particularly in the first few months of the unemployment spell, when mobility is largest.

Therefore, though these figures are non-parametric plots of the survival functions, they suggest that UI benefits do matter for geographic mobility. The next section offers a more systematic analysis by means of a parametric model.

3.9 Hazard Model Estimates

I start by showing that, irrespective of the unemployment benefit received, unemployed workers with lower household wealth move less. While this is a necessary condition for Prediction 1 to hold, it is not sufficient to argue that moving costs are driving differences across wealth groups. I illustrate the alternative channels that might be explaining the lower geographic mobility of less wealthy individuals.

I then present estimates of the effect of unemployment benefits on geographic mobility, both for the whole sample and for heterogeneous wealth groups. I show that higher UI weekly benefit amounts increase geographic mobility, particularly for individuals from less wealthy households. This evidence provides support to Predictions 2 and 3 of the model, while also highlighting the importance of moving costs relative to competing explanations.

Baseline Probability of Moving

Constrained Workers Move Less Table 3.4 compares hazard rates of moving for unemployed workers with different household wealth, using the liquidity constrained measures defined above. The baseline hazard considered in the estimation is the hazard rate of the lowest wealth group, and the coefficients reported in the table are the hazard rates of moving for each of the other wealth groups relative to the baseline.

The table shows that, while individuals with higher household wealth are generally more mobile (column 1), the difference across wealth groups is not statistically significant except for net liquid wealth. Columns 2 and 3 help decompose this finding. On the one hand wealthy renters are on average more mobile, on the other hand wealth owners are less mobile. Column 2 is certainly consistent with Prediction 1, while column 3 does not necessarily contradict it: as argued by Hsu, Matsa, and Melzer 2013, unemployed workers who own a house might be moving if they default on their home mortgage. Therefore if liquidity constrained homeowners are in general more likely to default, they are then also more likely to move.

That unconstrained unemployed workers are more mobile is also suggested by the effect of individual education level on the hazard rate of moving estimated in table 3.7, which shows that more educated workers are more likely to move.[21] While more educated might be more likely to move for reasons other than liquidity constraints, education is also positively correlated with household wealth in my sample,[22] and is measured more frequently than household wealth, making it a valid alternative to test mobility differences between constrained and unconstrained workers.

Is It Really Moving Costs? While moving costs could be an important reason why I observe unemployed workers' with lower household wealth to be less mobile, other channels might also be driving the results. Less information, more developed local social networks, and less experience with travelling are competing explanations that cannot be overlooked. To build a link with the theory, all of these channels could increase idiosyncratic preferences for location a among less wealthy individuals, therefore reducing their geographic mobility. Using information avaialble from the SIPP, I provide some evidence to show the relevance of information and local social networks, while I am unable to test for experience with travelling.

A first reason why unemployed workers with lower household wealth might move less is because they have more difficulty learning about job opportunities in other locations. To test for this mechanism, I use two alternative set of measures.

First, I focus on computer and internet use patterns, and whether individuals have used the internet to search for a job.[23] Individuals with low wealth might be more likely to rely on word-of-mouth rather than online resources, and their job search might therefore be more local. Table 3.5 estimates a linear probability model for the effect of wealth quartile on computer and internet use, and shows that unemployed workers with higher household wealth are generally more likely to (1) use a computer in the household (2) use internet and (3) use a computer at main job.

Secondly, I use education level as a general measure of resourcefulness. While education level might not capture the intensity and geographic location of individuals' job search, one could expect that lower educated individuals are not as able to take advantage of all the resources available while searching for a job.

A second reason why individuals with lower wealth might be less likely to move is that their social network might be more concentrated in their area of residence. Though I don't directly observe each individual's social network, I can use (1) the size of the company in which they were working before becoming unemployed and (2) whether the company was operating in more than one location as proxies. Having worked in a larger the company,

[21] This is a well-documented fact in the mobility literature. See for example Moretti 2008 and Matthew J Notowidigdo 2011.

[22] The correlation between education level and total real household wealth is around 20 percent in my sample.

[23] Unfortunately, the SIPP doesn't provide information on the location of jobs that workers search for. Other studies, including Marinescu 2014, have studied patterns of job search in a more systematic way.

or for a company located in more than one city, increases the chances of developing a less localized social network, therefore making it more likely that an unemployed worker moves to other places to look for job opportunities. Indeed, table 3.6 estimates a linear probability model and shows that individuals with higher household wealth are more likely to be working in firms employing a larger number of employees or operating in more locations.

Effect of UI Benefits on Geographic Mobility

Main Effect I now estimate the effect of more generous weekly benefit amounts on the hazard rate of moving using the methodology defined in section 3.7. This offers a way to empirically test Prediction 2 of my model. Indeed, finding that more generous unemployment benefits help geographic mobility for both constrained and unconstrained workers is consistent with a story in which mobility costs an important obstacle to geographic mobility. Finding the opposite effect would indicate instead that moving costs have of little relevance for unemployed workers' mobility decision.

The baseline estimates shown in table 3.7 report the elasticity of hazard rates with respect to unemployment benefit weekly benefit amounts, which is estimated to be positive and statistically significant. As shown in the same table, this result is robust to controlling flexibly for the pre-unemployment wage and the state unemployment rate.

Specifically, column 1 shows the estimate without controls for individual characteristics, while columns 2-4 include them. In addition, Column 2 controls for the monthly seasonally adjusted state-level unemployment rate, as well as the real (2000 dollars) individual wage (in logs), column 3 adds a fourth-order polynomial in the unemployment rate, column 4 adds a 10-piece log-linear wage spline. Column 4 of the table, which is my preferred specification, shows that increasing the real unemployment weekly benefit by one percent increases the hazard rate of moving during the unemployment spell by 0.33 percent.

Heterogeneous Effects I then proceed to the analysis on the hazard rate of moving for constrained versus unconstrained unemployed workers, and empirically test whether constrained workers benefit more from more generous unemployment benefits. The theoretical model assumes that this holds irrespectively of the monetary moving cost k, as postulated by Prediction 3.

In order to empirically test this hypothesis, I use the measures of household wealth previously defined, which offer a picture of how liquidity constrained each individual is. For each of the four measures of household wealth displayed in table 3.8, the first column stratifies individuals between those with household wealth above and below median, while the second column splits unemployed workers by wealth quartile. Table 3.8 shows that the most liquidity constrained workers are also the most likely to benefit from higher unemployment weekly benefit amounts. In particular Columns 1,3,5 and 7 illustrates that individuals with household wealth below the median benefit more from higher UI weekly benefit amounts, independently from the wealth measure used. In addition, columns 2,4,6 and 8, which divide

individuals by wealth quartile, show that in most cases unemployed workers in the lowest wealth quartile benefit the most from higher weekly benefit amounts.

Moving Costs Prediction 3 of the model also suggests that the higher the moving cost, the more effective unemployment benefits are in reducing mobility differences between constrained and unconstrained workers. While it is difficult to test for this empirically, particularly because my sample size is not very large, I use two alternative strategies.

The first strategy is based on the idea that for home-owners moving costs are an increasing function of the market value of the house. Selling a house is in fact associated with (1) a real estate fee based on the sales price of the house, (2) the state-level sales tax on the market value of the property, and (3) a capital gain tax on the difference between the market property value and the purchase cost of the house. Given that home mortgages add complications to the analysis [24], I focus on home-owners with no outstanding mortgage and compare mobility rates of individuals with different household liquid wealth and house market value.[25]

Table 3.9 shows that monetary moving costs are indeed a deterrent to geographic mobility. Column (1) interacts the market value of the house with wether the unemployed worker has household wealth above the median, while column (2) of the same table uses a dummy for whether the market value of the house is above or below median. Using either net liquid wealth or liquid wealth , the estimates indicate that as the market value of the house increases, the difference in mobility rates between liquidity constrained and unconstrained workers increases as well.

The second strategy uses the number of household members an individual is living with prior to unemployment as a proxy for monetary moving costs. The motivation is twofold. First, an individual separating from a larger household is more likely to increase his living and housing expenses. Secondly, an individual living with relatives is also more likely to benefit from informal family arrangements such as child care. This is shown in table 3.10, which illustrated how among individuals with kids, those living in less wealthy households are more likely to rely on family arrangements for child care. Moving away from the family safety net is likely to force an individual to buy the same services on the market.

Table 3.11 presents estimates on the effect of wealth and household size on the hazard rate of moving. It shows that while higher household size generally implies less mobility, this is more so for individuals with low wealth, given that the coefficient on the interaction variable is negative (though not statistically significant in the case of total net worth and total wealth).

[24] As illustrated in the literature review, several papers document how home mortgages are an obstacle to geographic mobility.

[25] I don't use total wealth or total net worth given that they are correlated with the market value of the house.

3.10 UI Duration

In this section I test whether UI benefit potential duration is a determinant of mobility decisions, comparably to UI weekly benefit amounts, and whether the effect is larger for more constrained individuals. I use potential total duration of UI benefit eligibility at job separation as determined by state-level UI laws. [26] The variation in UI duration mainly comes from variation in extended benefits and temporary unemployment insurance programs, which is constructed from weekly trigger notices published by the *Employment and Training Administration* from the Department of Labor provides weekly information.

Table 3.12 shows that eligible UI duration at job separation does not have a statistically significant effect on the hazard rate of moving during the unemployment spell. Panel A of table 3.12 considers the effect of the duration of regular UI benefits on the hazard rate of moving, while panel B shows the effect of total potential UI benefit duration (regular+extended+temporary programs). Column 1 of each panel displays the effect on the whole sample of unemployed workers, while columns 2-9 show estimated heterogeneous effects by wealth group. For each of the four measures of wealth used, I divide individuals in two alternative ways: (1) household wealth is above or below the median, (2) household wealth by quartile. Overall, table 3.12 shows that the link between potential UI benefit duration and geographic mobility is weak, and that more liquidity constrained unemployed workers do not benefit more from more weeks of UI benefits.

3.11 Alternative Measures of UI Weekly Benefit

In this section, I presents results to show how sensitive the results are to different measures of UI weekly benefit. As stressed previously, the weekly benefit amount measure used in the previous analysis are a function of base period wages reported prior to the unemployment spell, and are likely to suffer from measurement error as well as being endogenous to the mobility decision. Though I add a control for individual income in most of the analysis, and show that the main estimates of the paper are robust to controlling for individual income in a non-linear way (see table 3.7), it is possible that (1) pre-unemployment wages are measured imperfectly, in a way that is non-random across wealth groups, (2) mobility decisions and reported wages are correlated in a way that my model fails to account for, with the effect that the estimated effect of the UI weekly benefit amount on the hazard rate of moving is not well identified.

To address these issues, I use two additional measures of UI weekly benefit, which are more exogenous, but also have less variation:[27] (1) the state statutory maximum WBA and

[26] In theory, the number of unemployment weeks that each individual is eligible for can change during the unemployment spell, but in practice the variation in UI benefit duration is mainly driven by variation across unemployment spells. Therefore, I do not consider the effect that changing unemployment duration has on mobility decisions.

[27] Variation here is entirely driven by differences across states and time.

(2) the actual average WBA reported by the U.S. Department of Labor (DOL). These two measures are offer a good robustness check but have two main limitations: (1) they only vary across state-month and (2) they might not be as representative of each individual's actual weekly benefit amount as the predicted weekly benefit amount used in the previous analysis.

Table 3.13 show that, at least for the average weekly benefit amount, the estimates are consistent with those shown in the main analysis. While the coefficient in column 1, which shows the effect of higher UI WBA on mobility for the whole sample, is positive but not statistically significant, columns 2-5 confirm that unemployed workers with lowest household wealth benefit the most from higher UI benefits. On the other hand the estimated coefficients in Panel B, which uses the state statutory maximum WBA, are not statistically significant, even though their sign and magnitude are comparable to those of the other two measures of WBA used.

3.12 Local Cost of Living

In the previous analysis, real unemployment insurance variables were expressed in real terms using the national CPI-U index from the Bureau of Labor Statistics. Though I controlled for differences in the cost of living across years, I did not account for the fact that unemployed workers face different living and housing expenses depending on the states in which they reside.

In this section, I control for differences in the cost of living across states by using the same methodology as in Moretti 2008.[28] and develop two alternative price indexes. The *Local CPI 1* price index allows for the cost of housing to vary across states, while keeping non-housing costs the same. Similarly to what is done in Moretti 2008, differences in housing costs across states are computed using data from the American Community Survey 2001-2012 and compute the state average monthly rental cost of renting a 2 or 3 bedroom apartment.

The *Local CPI 2* price index accounts for state differences in the cost of both housing and non-housing consumption. In order to compute this index, I take advantage of the information released by the BLS on the local cost of living in 23 metropolitan areas (MSA). I use this information to estimate the component of non-housing costs that varies with housing costs across these MSA's and compute the state level *Local CPI 2* by weighting the cost of housing, the component of non-housing costs that vary with housing costs and the component of non-housing costs that does not vary with housing costs.

Panel A of table 3.14 shows that average real values for the predicted weekly benefit amount, the average weekly benefit amount reported by the DOL and the statutory maximum weekly benefit amount do not change significantly if I account for local differences in housing and non-housing prices. Consistently, Panel B of the same table shows that coefficients estimated using the baseline hazard model are robust to using alternative measures of cost of living. It shows that differences in the local cost of living do not matter significantly for unemployment weekly benefit amounts.

[28]See appendix for details.

Table 3.15 shows the estimated heterogeneous effect of real UI benefits on individuals with different household wealth deflating nominal amounts by the *Local CPI 1* and *Local CPI 2* price indexes. Consistently with what shown using the national price index, individuals from the lowest wealth quartile are generally more likely to benefit from higher UI weekly benefit amounts.

3.13 Placebo Tests

One plausible concern is that changes in unemployment benefits are correlated with changes in state level characteristics affecting the mobility pattern of all individuals living in each state. While state fixed effects account for permanent differences across states, it does not control for changes within a state that. If changes within a state (that are not common to all states) truly affect mobility behavior of individuals (including unemployed workers) in that state, then the effect of UI weekly benefits on mobility is not well identified. Given that including state trends would impose too much structure in my estimation, I perform two "placebo tests".

The first placebo test focuses on employed workers living in each state. Even though employed workers don't experience a job separation by definition (and therefore I cannot use the hazard model here), I use a linear probability model to test whether more generous UI benefits affect mobility decisions of employed workers. Panel A of table 3.16 shows the estimated coefficients using the average weekly benefit amount from the DOL, while panel B uses statutory maximum WBAs. If anything, higher UI benefit have a negative effect on the mobility employed workers with lowest household wealth.

A second placebo test focuses on individuals who experienced at least one unemployment spell in the period 2001-2012 but didn't receive unemployment benefits.[29] For these individuals we can construct a predicted weekly benefit amount using their pre-unemployment wage, as well as consider the other two measures of weekly benefit amount used in the rest of the analysis. All three panels of table 3.17 show that higher unemployment benefits have a statistically insignificant effect on mobility for this sample, and that heterogeneous effects are also not significant.

Therefore, both of this tests suggest that the effect of UI benefits on the hazard rate of moving that we are picking up in the main analysis can be given a causal interpretation.

3.14 Attrition

As with other longitudinal surveys, attrition is an issue in the Survey of Income and Program Participation. Attrition in the SIPP can occur for many reasons, which includes: (1) all household members were away the entire period, (2) the household members refused to be

[29]More precisely, these individuals reported being unemployed but also report not receiving any unemployment benefits while unemployed.

interviewed, (3) the interviewer was unable to locate the unit, (4) a serious illness or death had occurred in the household, (5) all household members had moved out of the country, or were living in armed forces barracks, or (6) all sample persons had moved and were living at an unknown address or were living more than 100 miles from a SIPP sampling unit with no available telephone number. In addition, previous studies report that in the SIPP, attrition is in the order of 30-35 percent over the life of a panel (Slud and Bailey 2006, Czajka, Mabli, and Cunnyngham 2007), showing there is reason to be concerned about non-random differences in the characteristics of attritors.

In my analysis I am mainly concerned that (1) migration outcomes are only observed for a non-random subset of the population, and depend on the ability of the survey's procedure to track individuals after they move, and (2) the generosity of unemployment benefits is correlated with the probability of attrition. Even though I use individual weights to compensate for longitudinal non-response in my main analysis [30] attrition needs to be addressed more closely.

The first concern is mitigated by the fact that, as documented in other studies (Kaplan and Schulhofer-Wohl 2012), the SIPP makes significant efforts to locate individuals who move, and attrition due to a move is relatively low.[31] Figure 3.9 constructed using my sample shows that attrition due to a move is relatively low compared to the overall number of moves.

The second concern is addressed by estimating a linear probability model in which the dependent variable equals one if the unemployed workers attrited during the unemployment spell.[32] Table 3.18 shows that, after controlling for individual and household characteristics, as well as differences across states and year, UI weekly benefits are not correlated with the probability of attriting the survey.

3.15 Conclusion

In this paper, I analyze how unemployment benefits affect migration decisions of unemployed workers in the United States between 2001 and 2012. To answer this question, I use state-year variation in unemployment insurance laws and estimate a proportional hazard model using data from the Survey of Income and Program Participation.

[30] As discussed by Bailey 2005 and Slud and Bailey 2010, individual weights assigned for each month compensate for non-random characteristics of attritors.

[31] The SIPP classifies attrition due to a move as either type C (moved out of scope) or type D (unable to follow) attrition. Moved out of scope are individuals who moved out of the country or exited the sample because of death. Unable to follow are individuals who moved more than 100 miles from a SIPP sampling area and could not be followed by the survey. the *Source and Accuracy Statement for the Survey of Income and Program Participation* for the SIPP 2004 and 2008, only 6.4 percent of the sample couldn't be followed after a move in the 2004 survey (which spans approximately 4 years), while only 6.1 percent couldn't be followed in the 2008 survey (the survey spans approximately 5 years).

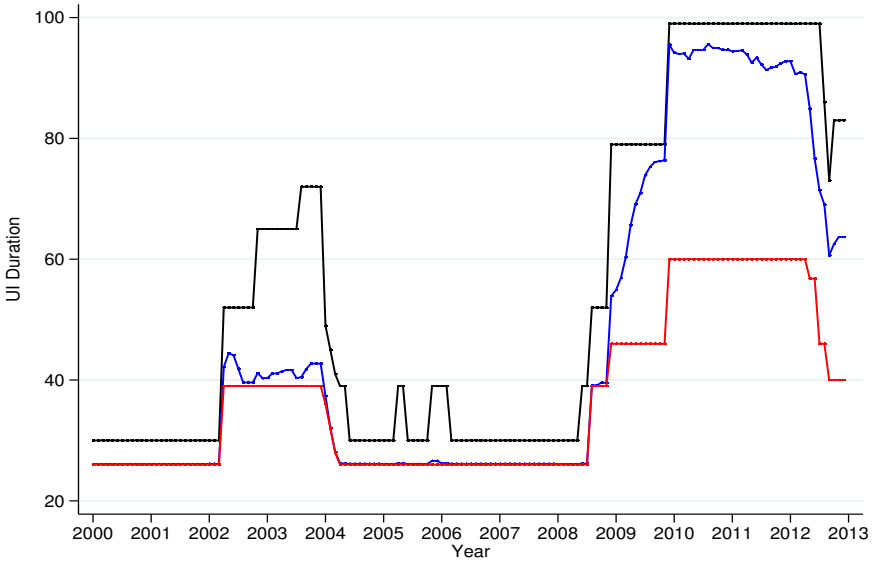
[32] Attrition includes both the case in which an individual leaves the survey and reenters later on and the case in which an individual leaves the sample permanently. In addition, individuals who reach the end of the survey and are still unemployed are also classified as attritors.

I find that higher weekly benefit amounts lead to higher mobility during the unemployment spells, particularly for unemployed workers with low household wealth. Consistently with the predictions of a simple spatial equilibrium model with risk-averse and liquidity constrained unemployed workers, I explain the empirical findings with high monetary moving costs, which affect liquidity constrained unemployed workers relatively more than other workers. While I also explore alternative explanations for why liquidity constrained workers are less likely to move geographically, none of them rules out the moving cost story.

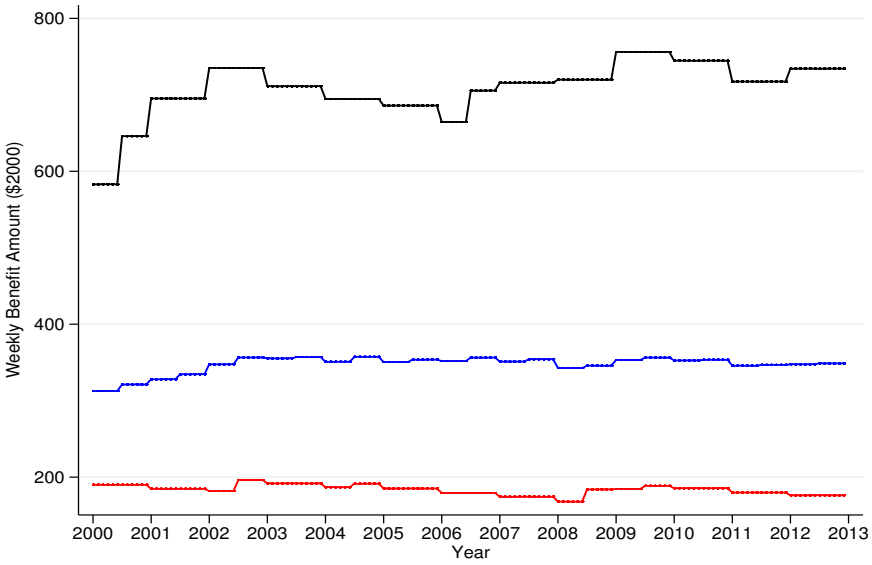
The main policy implication of my results is that household wealth can be used as a tag for unemployment insurance, given that higher unemployment benefits would help more liquidity constrained households as a moving subsidy. Though the previous literature has also stressed the moral hazard effects of more generous unemployment benefits, this paper offers one more argument in favor of the consumption smoothing benefits of unemployment insurance.

Figure 3.1: Unemployment Insurance Benefits in the Period 2000-2013

a. Potential UI Duration

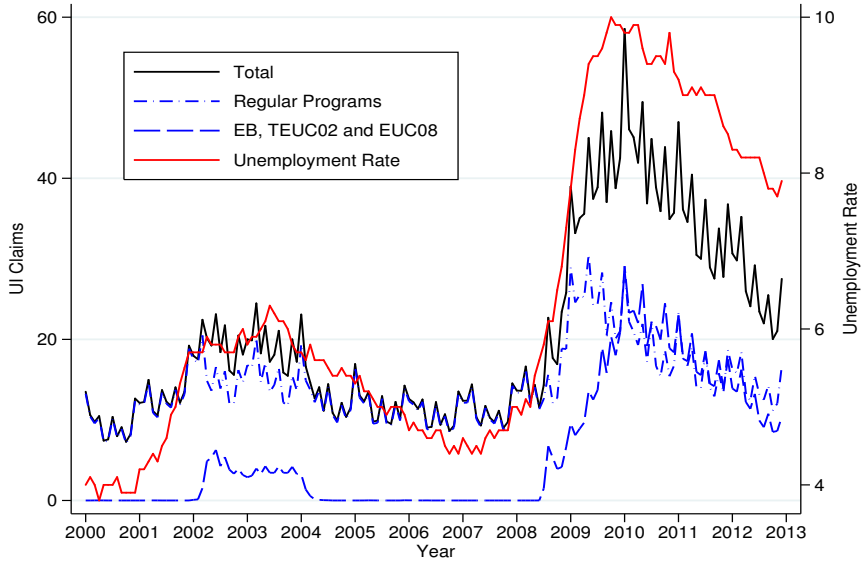


b. Maximum Weweekly Benefit Amount (WBA)



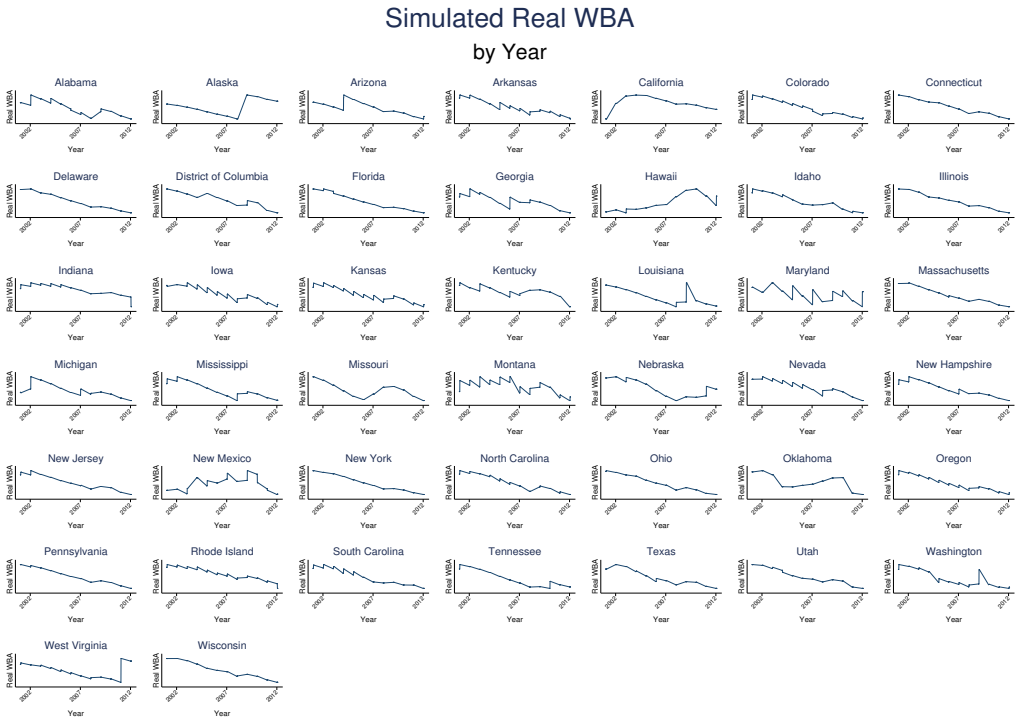
Notes: Black line shows maximum across states, blue line shows mean weighted by the state number of initial UI payments , and red line shows minimum across states. UI potential weeks refers to maximum available weeks of UI benefits, and includes regular UI benefits, extended benefits (EB) as well as temporary programs (TEUC02 and EUC08). Maximum weekly benefit amount includes dependents allowances.

Figure 3.2: Monthly UI Claims and the Unemployment Rate in the Period 2000-2013



Notes: Data on national monthly seasonally adjusted unemployment rate from the Bureau of Labor Statistics (BLS). Data on monthly UI claims from the U.S. Department of Labor (DOL) Employment and Training Administration.

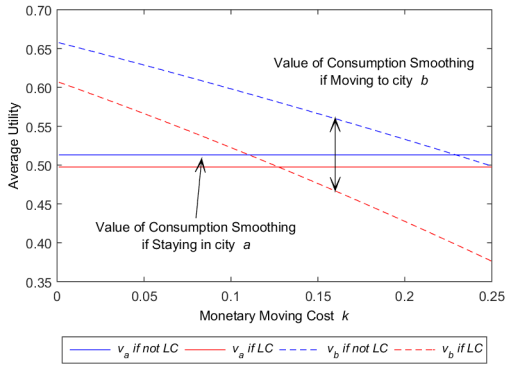
Figure 3.3: Simulated State UI Weekly Benefit Amounts, 2001-2012



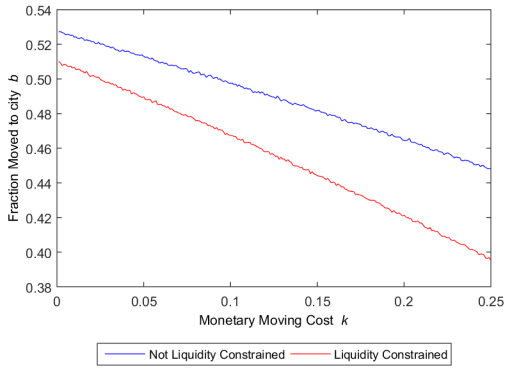
Notes: Computed by considering the wage distribution across all states in the first wave of the SIPP 2001 panel, and computing UI benefit weekly benefit amounts using state-level UI laws on benefit eligibility. Pre-unemployment wages used in simulations relate to unemployed spells for workers between 25 and 65, who experience at least one job separation, have at least three months of work history and have been included in the panel for at least three months, are not on temporary layoffs and are actively searching for a job.

Figure 3.4: Model Simulations: Mobility and Moving Costs

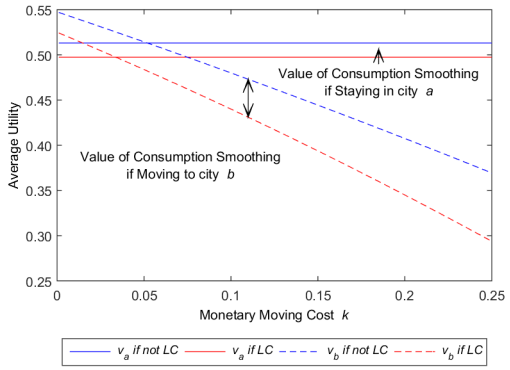
a. Large Gain from Moving: Utilities



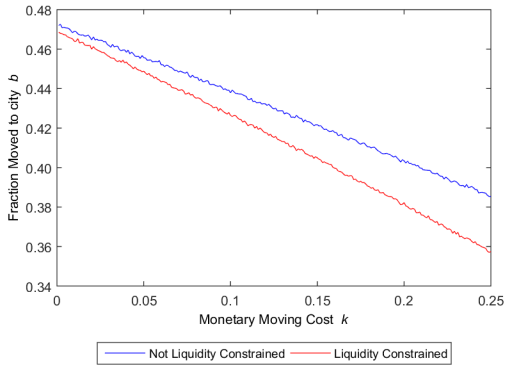
b. Large Gain from Moving: Fraction Moved



c. Small Gain from Moving: Utilities

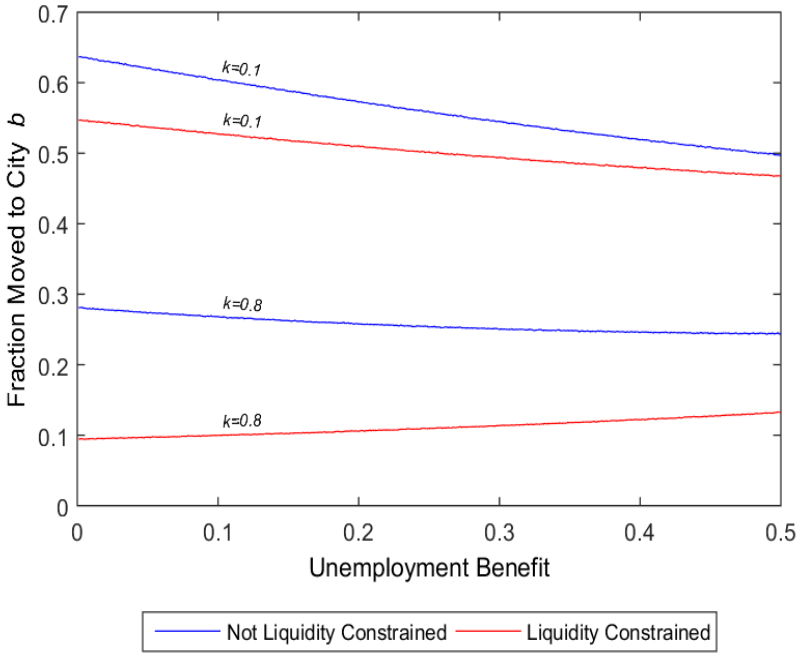


d. Small Gain from Moving: Fraction Moved



Notes: Figures show simulated fraction of individuals moving for each level of moving costs between 0 and 0.25. Figures based on 1 million simulations with $\frac{e_{ia}-e_{ib}}{q}$ drawn from a logistic distribution with mean zero and standard deviation one. Model parameters are set to: $q = 0.5$, $w = 1$, $r_a = 0.3$, $r_b = 0.3$. In addition, panel (a) and (b) have $\phi_a = 0.5$, $\phi_b = 0.9$, while panel (c) and (d) have $\phi_a = 0.5$, $\phi_b = 0.6$.

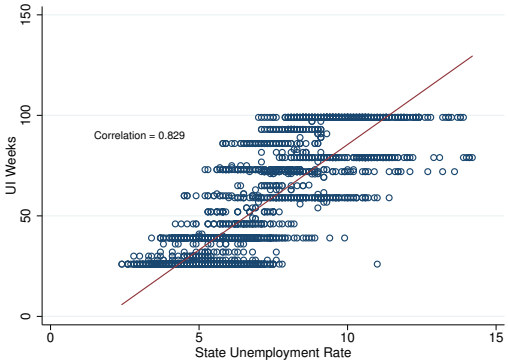
Figure 3.5: Model Simulations: UI Benefits and Moving Costs



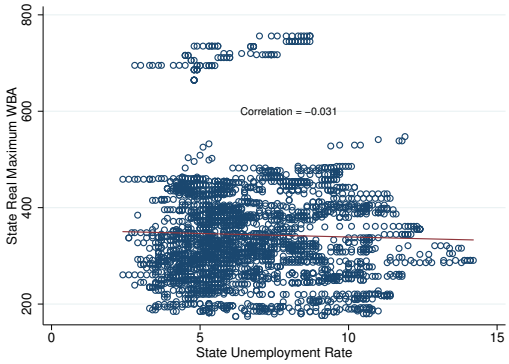
Notes: Figures show simulated fraction of individuals moving for each level of unemployment benefit between 0 and 0.5, under the assumption that there is a large gain from moving. Two scenarios are compared: (a) high moving costs ($k = 0.8$) and (b) low moving costs ($k = 0.1$). Figure based on 1 million simulations with $\frac{e_{ia}-e_{ib}}{q}$ drawn from a logistic distribution with mean zero and standard deviation one. Model parameters are set to: $q = 0.5$,$w = 1$,$\phi_a = 0.5$,$\phi_b = 0.9$,$r_a = 0.3$,$r_b = 0.3$.

Figure 3.6: UI Benefits Measures and State Unemployment Rates

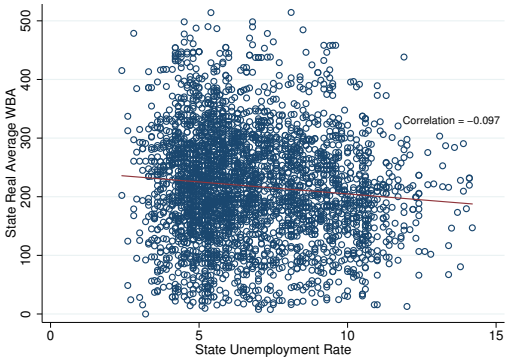
a. Potential UI Duration



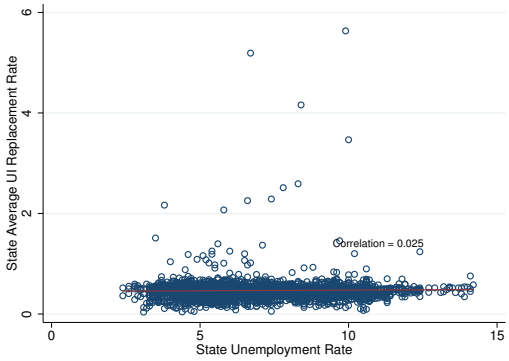
b. Maximum Real WBA



c. Actual Average Real WBA

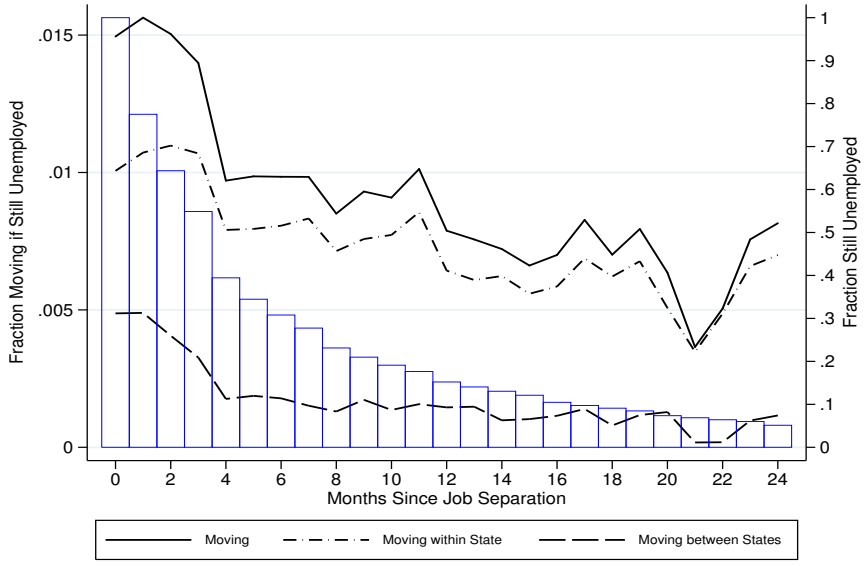


d. Actual Average Replacement Rate

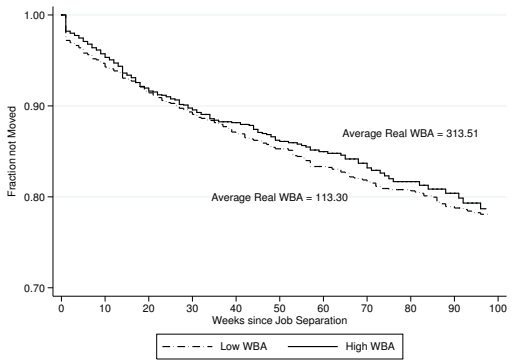


Notes: (a) Potential UI Duration indicates the maximum number of UI weeks for which an individual is eligible for in a given state, after considering regular benefits as well as extended benefits (EB) and benefits from temporary programs (TEUC02 and EUC08); (b) Maximum State weekly benefit amounts include dependent allowances; (c) and (d) are computed using pre-unemployment earnings and state-month variation in UI laws for benefit calculation and UI eligibility. Real amounts are expressed in 2000 dollars and are computed using the BLS CPI-U series. Monthly seasonally adjusted state unemployment rates from the Bureau of Labor Statistics (BLS).

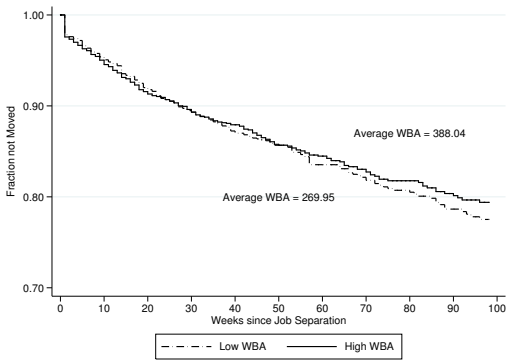
Figure 3.7: Monthly Mobility Hazard Rate after Job Separation



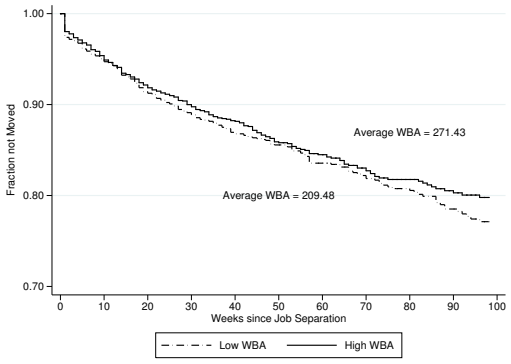
Notes: Computed using SIPP Panels 2001, 2004 and 2008. The sample of interest includes unemployed workers between 25 and 65, with at least one job separation, with at least three months of work history, are not on temporary layoffs, are actively searching for a job, and are eligible for UI benefits according to state UI laws.



(a) Actual Real WBA



(b) Statutory Maximum Real WBA



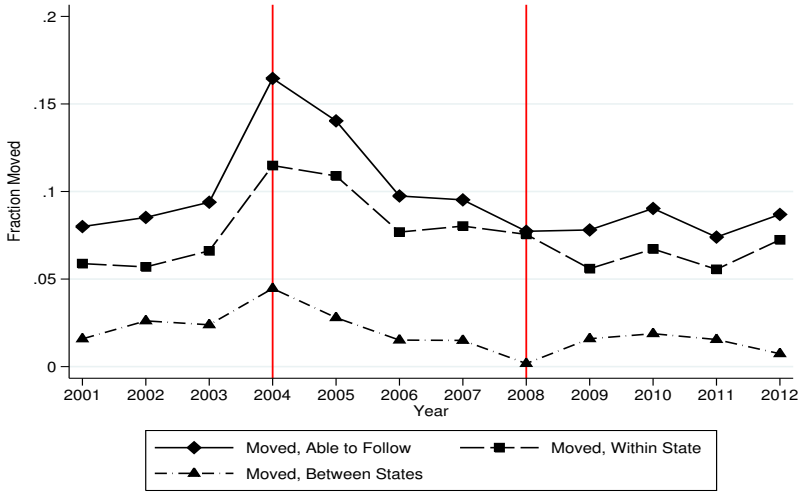
(c) Average State Real WBA (DOL)

Figure 3.8: Kaplan Meier Survival Curves, by Real UI WBA

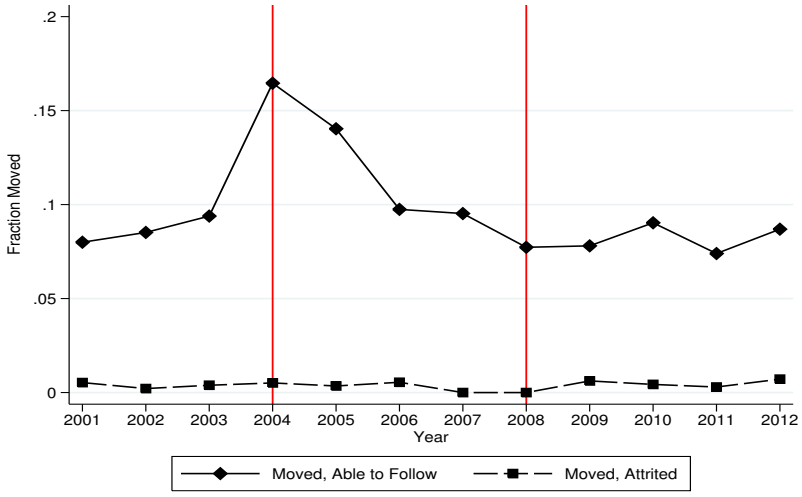
Notes: Computed using SIPP Panels 2001, 2004 and 2008. The sample of interest includes unemployed workers between 25 and 65, with at least one job separation, with at least three months of work history, are not on temporary layoffs, are actively searching for a job, and report receiving UI benefits at any point in the spell.

Figure 3.9: Type of Moves and Attrition from Moves

a. Type of Move



b. Attrition from Move



Notes: Computed using SIPP Panels 2001, 2004 and 2008. The red vertical lines indicate the beginning of a new panel. The sample of interest includes unemployed spells for workers between 25 and 65, who experience at least one job separation, have at least three months of work history and have been included in the panel for at least three months, are not on temporary layoffs, are actively searching for a job, and report receiving UI benefits at any point in the spell.

Table 3.1: Summary Statistics: Sample Characteristics, Part 1

	2001	2004	2008	All
	Demographic Characteristics			
Age	41.05	42.36	42.89	42.23
	(9.825)	(9.996)	(10.60)	(10.27)
Female	0.451	0.479	0.419	0.442
	(0.498)	(0.500)	(0.493)	(0.497)
Married	0.547	0.524	0.547	0.542
	(0.498)	(0.500)	(0.498)	(0.498)
Spouse is Working	0.397	0.379	0.372	0.381
	(0.489)	(0.485)	(0.483)	(0.486)
Have kids under 18	0.807	0.835	0.766	0.794
	(1.144)	(1.129)	(1.115)	(1.127)
Education 15+ years	0.364	0.423	0.482	0.434
	(0.481)	(0.494)	(0.500)	(0.496)
Education 12-15 years	0.510	0.458	0.405	0.448
	(0.500)	(0.498)	(0.491)	(0.497)
Education < HS	0.126	0.120	0.113	0.118
	(0.332)	(0.325)	(0.316)	(0.323)
Race White	0.815	0.792	0.800	0.802
	(0.389)	(0.406)	(0.400)	(0.398)
Race Black	0.130	0.137	0.129	0.131
	(0.336)	(0.344)	(0.335)	(0.338)
Other Race	0.0554	0.0706	0.0711	0.0664
	(0.229)	(0.256)	(0.257)	(0.249)

Notes: Computed using SIPP Panels 2001, 2004 and 2008. Means represent weighted averages of individual characteristics at job separation. UI amounts are in current dollars while duration values are in weeks. Mean of each variable with standard deviation in parentheses. See appendix A for sample selection criteria.

Table 3.2: Summary Statistics: Sample Characteristics, Part 2

	2001	2004	2008	All
	Unemployment Insurance Variables			
Base Period Wage	28329.2	29588.6	28283.9	28598.2
	(26627.5)	(28839.0)	(31011.1)	(29297.4)
Highest Quarter Wage	9075.4	8969.3	8923.4	8978.2
	(8317.2)	(9002.4)	(8876.3)	(8746.4)
Average Weekly Wage	698.1	689.9	686.4	690.6
	(639.8)	(692.5)	(682.8)	(672.8)
Weekly Benefit Amount	250.9	265.5	265.7	261.3
	(114.6)	(128.5)	(143.6)	(132.4)
Average Monthly State WBA	252.3	274.5	303.7	282.0
	(46.32)	(39.30)	(46.16)	(49.94)
Statutory Max. WBA	346.4	395.8	419.3	392.7
	(81.83)	(82.91)	(95.21)	(93.98)
Replacement Rate	0.440	0.463	0.468	0.458
	(0.158)	(0.153)	(0.252)	(0.206)
Statutory Max Reg.UB Duration	26.06	26.14	25.91	26.01
	(0.485)	(0.728)	(0.695)	(0.656)
Statutory Max Tot.UB Duration	33.97	26.35	78.29	53.43
	(8.864)	(1.433)	(19.68)	(28.00)
Unemployment Spell Duration	31.25	26.67	45.46	36.99
	(27.59)	(26.38)	(42.37)	(36.18)

Notes: Computed using SIPP Panels 2001, 2004 and 2008. Means represent weighted averages of individual characteristics at job separation. UI amounts are in current dollars while duration values are in weeks. Mean of each variable with standard deviation in parentheses. See appendix A for sample selection criteria.

Table 3.3: Summary Statistics: Real Assets and Liabilities by Net Liquid Wealth Group

	Net Liquid Wealth			
	First Quartile (Lower than -1314)	Second Quartile (-1314 to 1701)	Third Quartile (1701 to 39476)	Fourth Quartile (Higher than 39476)
Net Liquid Wealth	-20177.9 (-9109.9)	77.57 (0)	14713.5 (12161.5)	202688.1 (123073.3)
Liquid Wealth	4630.8 (789.7)	1067.7 (17.63)	19064.4 (16031.0)	210029.7 (129516.5)
Total Net Worth	11229.7 (-2728.0)	28582.0 (2864.0)	73440.6 (38626.0)	337104.0 (239071.0)
Total Wealth	36038.4 (9619.8)	29572.1 (3462.7)	77791.5 (43569.6)	344445.6 (246796.6)
Home Equity	48078.3 (31434.9)	51690.4 (32066.1)	69270.9 (39194.2)	121727.1 (87509.9)
Business Equity	2091.2 (0)	3319.5 (0)	3914.6 (0)	12083.5 (0)
Vehicle Equity	2084.7 (1826.6)	2360.5 (1481.3)	3927.1 (2989.0)	7834.5 (6414.8)
Total Debt	75032.8 (40275.0)	27333.0 (1196.5)	71694.6 (39671.1)	120740.8 (88292.5)
Unsecured Debt	24808.7 (12002.1)	990.1 (0)	4350.9 (881.7)	7341.6 (1531.1)

Notes: Computed using topical modules SIPP Panels 2001, 2004 and 2008. Mean of each variable at job separation with median in parentheses. Real variables are in 2000 dollars and computed using the CPI-U BLS price index. Net liquid wealth is computed as household total wealth minus home equity, business equity and vehicle equity, and unsecured debt. Wealth values at job separation are assigned using the most recent observation. See appendix A for sample selection criteria.

Table 3.4: Hazard Model Estimates, Constrained vs. Unconstrained Unemployed Workers

Hazard Rate of Moving	All		Renters		Owners	
	(1)	(2)	(3)	(4)	(5)	(6)
Net Liquid Wealth > Median	0.18*		0.35***		-0.37**	
	(0.11)		(0.12)		(0.15)	
Net Liquid Wealth Q2		-0.013		-0.075		0.18
		(0.12)		(0.16)		(0.28)
Net Liquid Wealth Q3		0.24**		0.24		-0.15
		(0.11)		(0.16)		(0.20)
Net Liquid Wealth Q4		0.018		0.52***		-0.52**
		(0.17)		(0.19)		(0.25)
Liquid Wealth > Median	0.087		0.31**		-0.43**	
	(0.12)		(0.12)		(0.19)	
Liquid Wealth Q2		0.24*		0.28*		-0.063
		(0.13)		(0.16)		(0.31)
Liquid Wealth Q3		0.29**		0.39***		-0.31
		(0.13)		(0.15)		(0.25)
Liquid Wealth Q4		0.095		0.67***		-0.75***
		(0.18)		(0.24)		(0.29)
Total Net Worth > Median	-0.022		0.39**		-0.62***	
	(0.16)		(0.17)		(0.19)	
Total Net Worth Q2		0.27**		0.26*		-0.47
		(0.13)		(0.15)		(0.32)
Total Net Worth Q3		0.24		0.36		-0.77***
		(0.16)		(0.27)		(0.27)
Total Net Worth Q4		-0.11		0.89***		-1.14***
		(0.24)		(0.30)		(0.33)
Total Wealth > Median	0.036		0.46**		-0.53***	
	(0.17)		(0.18)		(0.20)	
Total Wealth Q2		0.082		0.086		-1.01***
		(0.13)		(0.17)		(0.28)
Total Wealth Q3		0.21		0.36		-1.07***
		(0.18)		(0.25)		(0.28)
Total Wealth Q4		-0.16		0.79***		-1.46***
		(0.25)		(0.28)		(0.33)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	143225	143225	44844	44844	95471	95471

Notes: All specifications include these additional controls: age, gender, race, education level, marital status, number of kids, whether the spouse is working, housing tenure, monthly seasonally adjusted state-level unemployment rate, log of real (2000 dollars) individual wage and a dummy variable for being on the seam between two waves. All Estimates use individual weights at job separation. Standard errors are clustered by state and shown in parentheses. * p<0.10, ** p<0.05, *** p<0.01

Table 3.5: Wealth and Computer/Internet Use

	Computer in HH	Use internet	Use a computer at Home	Connect to Internet at Home
	(1)	(2)	(3)	(4)
Net Liquid Wealth Quartile	0.037***	0.032***	-0.0028	0.0071
	(0.0089)	(0.0091)	(0.0095)	(0.0054)
Liquid Wealth Quartile	0.088***	0.083***	0.0081	0.011
	(0.012)	(0.0090)	(0.0089)	(0.0095)
Total Net Worth Quartile	0.050***	0.034***	-0.015	-0.000099
	(0.0077)	(0.0082)	(0.010)	(0.0060)
Total Wealth Quartile	0.060***	0.038***	-0.016	-0.0032
	(0.0082)	(0.0089)	(0.013)	(0.0085)
Observations	3262	3262	2508	1919
	Use Computer at Main Job	Connect to Internet at Work	Use Internet to Search for Job	
	(5)	(6)	(7)	
Net Liquid Wealth Quartile	0.028***	0.0054	-0.019	
	(0.0093)	(0.0084)	(0.015)	
Liquid Wealth Quartile	0.072***	0.0031	0.013	
	(0.011)	(0.014)	(0.016)	
Total Net Worth Quartile	0.033***	0.0095	-0.035**	
	(0.010)	(0.010)	(0.017)	
Total Wealth Quartile	0.034***	0.0072	-0.035*	
	(0.011)	(0.012)	(0.018)	
Observations	3262	1306	2111	
Year FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes

Notes: The coefficients are estimated using a linear probability model on individuals who report having kids. All specifications include these additional controls: age, gender, race, education level, marital status, number of kids, whether the spouse is working, housing tenure, monthly seasonally adjusted state-level unemployment rate, log of real annual wage and a dummy variable for being on the seam between two waves. All Estimates use individual weights at job separation. Standard errors are clustered by state and shown in parentheses. * p<0.10, ** p<0.05, *** p<0.01

Table 3.6: Wealth and Last Employer's Characteristics

	Establishment Size	Employer Operations in More than One Location	Firm Size
	(1)	(2)	(3)
Net Liquid Wealth Quartile	0.035**	0.028***	0.020
	(0.016)	(0.0069)	(0.012)
Liquid Wealth Quartile	0.032**	0.040***	0.025*
	(0.014)	(0.0080)	(0.013)
Total Net Worth Quartile	0.033*	0.015*	0.014
	(0.017)	(0.0090)	(0.014)
Total Wealth Quartile	0.031	0.016*	0.015
	(0.019)	(0.0089)	(0.013)
Year FE	Yes	Yes	Yes
State FE	Yes	Yes	Yes
Observations	4867	4874	3013

Notes: Coefficients are estimated using a linear probability model on individuals who report employer's characteristics one month before job separation. All specifications include these additional controls: age, gender, race, education level, marital status, number of kids, whether the spouse is working, housing tenure, monthly seasonally adjusted state-level unemployment rate, log of real annual wage and a dummy variable for being on the seam between two waves. All Estimates use individual weights at job separation. Standard errors are clustered by state and shown in parentheses. * p<0.10, ** p<0.05, *** p<0.01

Table 3.7: Effect of Real UI Weekly Benefit Amount on Mobility

	(1)	(2)	(3)	(4)
Log(Real WBA)	0.088*	0.27*	0.27*	0.33**
	(0.050)	(0.15)	(0.15)	(0.15)
Age		-0.022***	-0.023***	-0.023***
		(0.0058)	(0.0054)	(0.0055)
Female		-0.032	-0.016	-0.014
		(0.095)	(0.10)	(0.10)
Education		0.15***	0.15***	0.16***
		(0.051)	(0.051)	(0.051)
Race of this person		-0.067	-0.064	-0.060
		(0.071)	(0.071)	(0.071)
Log(Real Annual Wage)		0.018	0.017	
		(0.10)	(0.10)	
Seam Dummy		1.31***	1.31***	1.31***
		(0.12)	(0.12)	(0.12)
Spouse is Working		-0.18	-0.28*	-0.29**
		(0.12)	(0.14)	(0.14)
Have Kids under 18		0.045	0.028	0.029
		(0.040)	(0.045)	(0.047)
Rent with Payment		1.38***	1.39***	1.40***
		(0.099)	(0.10)	(0.11)
Occupied Without Payment		2.00***	2.01***	2.00***
		(0.20)	(0.21)	(0.21)
State Monthly Unemployment Rate (SA)		-0.018	-2.29	-2.03
		(0.054)	(1.66)	(1.68)
Individual characteristics	No	Yes	Yes	Yes
Household characteristics	No	Yes	Yes	Yes
Urate Polynomial	No	No	Yes	Yes
Real Wage Spline	No	No	No	Yes
Observations	233627	199946	199946	199946

Notes: Coefficients reported are elasticities of mobility hazard rates with respect to the UI real weekly benefit amount (national 2000 dollars) estimated using a Cox proportional hazard model. All specifications include state and year fixed effects. Columns (2)-(4) include these additional controls: age, gender, race, education level, marital status, number of kids, whether the spouse is working, housing tenure and a dummy variable for being on the seam between two waves. Column (2) controls for the monthly seasonally adjusted state-level unemployment rate, as well as the real (2000 dollars) individual wage (in logs), column (3) adds a fourth-order polynomial in the unemployment rate, column (4) adds a 10-piece log-linear wage spline. All Estimates use individual weights at job separation. Standard errors are clustered by state and shown in parentheses. * $p<0.10$, ** $p<0.05$, *** $p<0.01$

Table 3.8: Effect of Real UI Weekly Benefit Amount on Mobility by Household Wealth

	Net Liquid Wealth		Liquid Wealth		Total Net Worth		Total Wealth	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Wealth < Median	0.33**		0.32**		0.35**		0.37**	
	(0.17)		(0.16)		(0.16)		(0.16)	
Wealth > Median	0.27		0.23		0.27		0.21	
	(0.24)		(0.26)		(0.23)		(0.22)	
Wealth Q1		0.18		0.44**		0.45**		0.47**
		(0.20)		(0.19)		(0.22)		(0.21)
Wealth Q2		0.46**		0.17		0.25		0.22
		(0.20)		(0.19)		(0.18)		(0.16)
Wealth Q3		0.38		0.34		0.49		0.32
		(0.25)		(0.28)		(0.33)		(0.29)
Wealth Q4		0.046		0.054		-0.0090		0.045
		(0.31)		(0.33)		(0.26)		(0.27)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	143225	143225	143225	143225	143225	143225	143225	143225

Notes: Coefficients reported are elasticities of mobility hazard rates with respect to the UI real weekly benefit amount (national 2000 dollars) estimated using a stratified Cox hazard model. All specifications include state and year fixed effects. All Estimates use individual weights at job separation. Standard errors are clustered by state and shown in parentheses. * p<0.10, ** p<0.05, *** p<0.01

Table 3.9: Property Value and Wealth for Homeowners with no Mortgage

	Pooled Sample		Net Liquid Wealth		Liquid Wealth	
	(1)	(2)	(3)	(4)	(5)	(6)
Log(Property Value)	-0.17		-0.17		-0.11	
	(0.13)		(0.13)		(0.16)	
Property Value > Median		-0.50		-1.61		-1.96
		(0.67)		(1.11)		(1.44)
Wealth > Median			-13.0*	-2.77***	-11.8	-2.62***
			(6.81)	(0.90)	(7.45)	(0.87)
Wealth > Median × Log(Property Value)			0.97*		0.88	
			(0.56)		(0.62)	
Wealth > Median × Property Value > Median				2.98**		3.26*
				(1.50)		(1.74)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	16210	16210	16210	16210	16210	16210

Notes: All specifications include these additional controls: age, gender, race, education level, marital status, number of kids, whether the spouse is working, housing tenure, monthly seasonally adjusted state-level unemployment rate, log of real (2000 dollars) individual wage and a dummy variable for being on the seam between two waves. All Estimates use individual weights at job separation. Standard errors are clustered by state and shown in parentheses. * p<0.10, ** p<0.05, *** p<0.01

Table 3.10: Wealth and Family Child Care Arrangements

	Grandparent	Adult Sibling	Other Relative	Any Relative
	(1)	(2)	(3)	(4)
Net Liquid Wealth Quartile	-0.0070	-0.0060	-0.0074*	-0.020**
	(0.0074)	(0.0046)	(0.0044)	(0.0098)
Liquid Wealth Quartile	0.0033	-0.0022	0.0027	0.00042
	(0.0093)	(0.0053)	(0.0052)	(0.012)
Total Net Worth Quartile	-0.019**	-0.0063	-0.011*	-0.028***
	(0.0082)	(0.0057)	(0.0059)	(0.0099)
Total Wealth Quartile	-0.019**	-0.0042	-0.014**	-0.030**
	(0.0092)	(0.0069)	(0.0055)	(0.011)
Year FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Observations	2210	2210	2210	2210

Notes: Coefficients are estimated using a linear probability model on individuals who report having kids. All specifications include these additional controls: age, gender, race, education level, marital status, number of kids, whether the spouse is working, housing tenure, monthly seasonally adjusted state-level unemployment rate, log of real annual wage and a dummy variable for being on the seam between two waves. All Estimates use individual weights at job separation. Standard errors are clustered by state and shown in parentheses. * p<0.10, ** p<0.05, *** p<0.01

Table 3.11: Mobility during Unemployment Spell, by Number of HH members

	Net Liquid Wealth		Liquid Wealth		Total Net Worth		Total Wealth	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
HH Size > Median	-0.076	-0.64*	-0.073	-0.64*	-0.068	-0.39	-0.071	-0.31
	(0.13)	(0.36)	(0.13)	(0.34)	(0.14)	(0.39)	(0.14)	(0.38)
Wealth > Median	0.18*	-0.32	0.089	-0.42	-0.020	-0.33	0.040	-0.19
	(0.11)	(0.34)	(0.12)	(0.28)	(0.16)	(0.34)	(0.17)	(0.35)
(HH Size > Median)		0.39*		0.39**		0.24		0.17
× (Wealth > Median)		(0.22)		(0.19)		(0.25)		(0.23)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	199946	143225	199946	143225	199946	143225	199946	143225

Notes: All specifications include these additional controls: age, gender, race, education level, marital status, number of kids, whether the spouse is working, housing tenure, monthly seasonally adjusted state-level unemployment rate, log of real (2000 dollars) individual wage and a dummy variable for being on the seam between two waves. All Estimates use individual weights at job separation. Standard errors are clustered by state and shown in parentheses. * p<0.10, ** p<0.05, *** p<0.01

Table 3.12: Effect of Eligible UI Duration at Unemployment on Mobility

Panel A: Regular UI Weeks									
	Pooled Sample	Net Liquid Wealth		Liquid Wealth		Total Net Worth		Total Wealth	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Regular UI Weeks	0.050								
	(0.12)								
Wealth > Median × Regular UI Weeks		0.10		0.11		0.12		0.12	
		(0.10)		(0.10)		(0.11)		(0.11)	
Wealth < Median × Regular UI Weeks		0.11		0.074		0.070		0.061	
		(0.13)		(0.13)		(0.12)		(0.13)	
Wealth Q1 × Regular UI Weeks			0.015		0.12		0.065		0.13
			(0.12)		(0.13)		(0.10)		(0.11)
Wealth Q2 × Regular UI Weeks			0.27**		0.10		0.22		0.12
			(0.13)		(0.11)		(0.16)		(0.12)
Wealth Q3 × Regular UI Weeks			0.14		0.082		0.18		0.16
			(0.15)		(0.14)		(0.13)		(0.13)
Wealth Q4 × Regular UI Weeks			0.086		0.061		-0.053		-0.073
			(0.12)		(0.12)		(0.13)		(0.14)
Panel B: Total UI Weeks									
Total UI Weeks	0.014*								
	(0.0074)								
Wealth > Median × Total UI Weeks		0.0072		0.0086		0.0079		0.0088	
		(0.0072)		(0.0079)		(0.0072)		(0.0076)	
Wealth < Median × Total UI Weeks		0.0060		0.0047		0.0033		0.0018	
		(0.0088)		(0.0085)		(0.0091)		(0.0097)	
Wealth Q1 × Total UI Weeks			0.0059		0.0078		0.0066		0.0047
			(0.0074)		(0.0080)		(0.0080)		(0.0079)
Wealth Q2 × Total UI Weeks			0.0071		0.0085		0.0088		0.013*
			(0.0078)		(0.0082)		(0.0073)		(0.0078)
Wealth Q3 × Total UI Weeks			0.0092		0.0072		0.0020		-0.00024
			(0.0087)		(0.0084)		(0.0090)		(0.0097)
Wealth Q4 × Total UI Weeks			-0.0021		-0.0011		0.0049		0.0037
			(0.0097)		(0.0099)		(0.011)		(0.011)
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	199946	143225	143225	143225	143225	143225	143225	143225	143225

Notes: Coefficients reported are semi-elasticities of mobility hazard rates with respect to the UI potential duration estimated using a stratified Cox hazard model. All columns include these additional controls: age, gender, race, education level, marital status, number of kids, whether the spouse is working, housing tenure, log of real annual wage and a dummy variable for being on the seam between two waves. All Estimates use individual weights at job separation. Standard errors are clustered by state and shown in parentheses. * p<0.10, ** p<0.05, *** p<0.01

Table 3.13: Robustness Check: Effect of Real UI Weekly Benefit Amount on Mobility

	Pooled Sample (1)	Net Liquid Wealth (2)	Liquid Wealth (3)	Total Net Worth (4)	Total Wealth (5)
Panel A: Average Weekly Benefit Amount (DOL)					
Log(Real WBA)	0.76				
	(0.87)				
Wealth Q1 × Log(Real WBA)		2.25**	1.43	2.02*	2.09*
		(1.14)	(1.23)	(1.22)	(1.21)
Wealth Q2 × Log(Real WBA)		0.98	1.71	1.07	0.95
		(1.34)	(1.28)	(1.15)	(1.27)
Wealth Q3 × Log(Real WBA)		1.09	0.95	1.75	1.41
		(1.42)	(1.49)	(1.52)	(1.56)
Wealth Q4 × Log(Real WBA)		0.87	0.88	-0.79	-0.39
		(1.33)	(1.42)	(1.36)	(1.56)
State FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Observations	143225	143225	143225	143225	143225
Panel B: Statutory Maximum Weekly Benefit Amount					
Log(Real WBA)	0.44				
	(0.50)				
Wealth Q1 × Log(Real WBA)		0.98	0.62	0.91	0.73
		(0.62)	(0.65)	(0.56)	(0.60)
Wealth Q2 × Log(Real WBA)		0.49	0.72	0.38	0.69
		(0.77)	(0.68)	(0.66)	(0.63)
Wealth Q3 × Log(Real WBA)		0.29	0.39	0.83	0.61
		(0.69)	(0.62)	(0.80)	(0.78)
Wealth Q4 × Log(Real WBA)		0.41	0.18	-0.65	-0.40
		(0.83)	(0.86)	(0.89)	(0.98)
State FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
UI Spells	199946	143225	143225	143225	143225

Notes: Coefficients reported are elasticities of mobility hazard rates with respect to the UI real weekly benefit amount (national 2000 dollars) estimated using a stratified Cox hazard model. All columns include these additional controls: age, gender, race, education level, marital status, number of kids, whether the spouse is working, housing tenure, log of real annual wage and a dummy variable for being on the seam between two waves. All Estimates use individual weights at job separation. Standard errors are clustered by state and shown in parentheses. * p<0.10, ** p<0.05, *** p<0.01

Table 3.14: Real Weekly Benefit Amount using Different Measures of CPI

	Summary Statistics on UI Weekly Benefit Amount		
	National CPI	Local CPI 1	Local CPI 2
Real WBA	223.2	225.8	222.9
	(222.2)	(224.4)	(222.6)
Real Monthly Average WBA (DOL)	239.9	243.5	240.5
	(241.4)	(248.3)	(247.5)
Real Maximum WBA	334.0	338.0	333.5
	(321.8)	(325.0)	(325.6)
	Hazard Model Estimates		
	National CPI	Local CPI 1	Local CPI 2
	(1)	(2)	(3)
Log(Real WBA)	0.33**	0.31**	0.31**
	(0.15)	(0.15)	(0.15)
Year FE	Yes	Yes	Yes
State FE	Yes	Yes	Yes
UI Spells	199946	199946	199946

Notes: Coefficients reported are elasticities of hazard rates with respect to the real UI weekly benefit amount estimated using a Cox proportional hazard model. See appendix for definition of local CPI measures. All specifications include these additional controls: age, gender, race, education level, marital status, number of kids, whether the spouse is working, housing tenure and a dummy variable for being on the seam between two waves, a fourth-order polynomial in the unemployment rate, and a 10-piece log-linear wage spline. All Estimates use individual weights at job separation. Standard errors are clustered by state and shown in parentheses. * p<0.10, ** p<0.05, *** p<0.01

Table 3.15: Hazard Model Estimates using Local CPI Measures

	Panel A: Local CPI 1								
	Pooled Sample	Net Liquid Wealth		Liquid Wealth		Total Net Worth		Total Wealth	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Log(Real WBA)	0.31**								
	(0.15)								
Wealth > Median × Log(Real WBA)		0.33**		0.31**		0.33**		0.35**	
		(0.17)		(0.16)		(0.16)		(0.16)	
Wealth < Median × Log(Real WBA)		0.24		0.21		0.27		0.22	
		(0.25)		(0.28)		(0.23)		(0.22)	
Wealth Q1 × Log(Real WBA)			0.17		0.45**		0.43*		0.46**
			(0.20)		(0.20)		(0.22)		(0.21)
Wealth Q2 × Log(Real WBA)			0.47**		0.16		0.24		0.19
			(0.19)		(0.19)		(0.17)		(0.17)
Wealth Q3 × Log(Real WBA)			0.36		0.31		0.50		0.35
			(0.26)		(0.32)		(0.33)		(0.29)
Wealth Q4 × Log(Real WBA)			0.0074		0.049		-0.0091		0.050
			(0.31)		(0.33)		(0.27)		(0.28)
	Panel B: Local CPI 2								
Log(Real WBA)	0.31**								
	(0.15)								
Wealth > Median × Log(Real WBA)		0.33**		0.31**		0.33**		0.35**	
		(0.17)		(0.16)		(0.16)		(0.16)	
Wealth < Median × Log(Real WBA)		0.23		0.21		0.28		0.23	
		(0.25)		(0.29)		(0.23)		(0.22)	
Wealth Q1 × Log(Real WBA)			0.17		0.45**		0.42*		0.45**
			(0.20)		(0.20)		(0.22)		(0.21)
Wealth Q2 × Log(Real WBA)			0.47**		0.15		0.24		0.19
			(0.19)		(0.19)		(0.16)		(0.17)
Wealth Q3 × Log(Real WBA)			0.35		0.31		0.49		0.35
			(0.27)		(0.33)		(0.33)		(0.29)
Wealth Q4 × Log(Real WBA)			0.0046		0.055		-0.0010		0.059
			(0.31)		(0.33)		(0.27)		(0.28)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	199946	143225	143225	143225	143225	143225	143225	143225	143225

Notes: Coefficients reported are elasticities of hazard rates with respect to the real UI weekly benefit amount estimated using a Cox proportional hazard model. See appendix for definition of local CPI measures. All specifications include these additional controls: age, gender, race, education level, marital status, number of kids, whether the spouse is working, housing tenure and a dummy variable for being on the seam between two waves, a fourth-order polynomial in the unemployment rate, and a 10-piece log-linear wage spline. All Estimates use individual weights at job separation. Standard errors are clustered by state and shown in parentheses. * p<0.10, ** p<0.05, *** p<0.01

Table 3.16: Placebo Test 1: Effect of UI Benefits on Employed Workers

	Panel A: Average Weekly Benefit Amount				
	Pooled Sample (1)	Net Liquid Wealth (2)	Liquid Wealth (3)	Total Net Worth (4)	Total Wealth (5)
Log(Real WBA)	-0.0067	-0.017*	-0.012	-0.021*	-0.021*
	(0.0054)	(0.0087)	(0.011)	(0.011)	(0.011)
Wealth Q2		-0.087*	-0.028	-0.084	-0.098
		(0.049)	(0.058)	(0.060)	(0.063)
Wealth Q3		-0.043	0.0068	-0.10*	-0.097
		(0.041)	(0.053)	(0.056)	(0.058)
Wealth Q4		-0.053	-0.035	-0.084	-0.082
		(0.044)	(0.054)	(0.061)	(0.062)
Wealth Q2 × Log(Real WBA)		0.015*	0.0055	0.015	0.017
		(0.0090)	(0.011)	(0.011)	(0.012)
Wealth Q3 × Log(Real WBA)		0.0075	-0.0011	0.018*	0.017
		(0.0076)	(0.0098)	(0.010)	(0.011)
Wealth Q4 × Log(Real WBA)		0.0092	0.0065	0.015	0.015
		(0.0082)	(0.0099)	(0.011)	(0.011)
Constant	0.060**	0.12**	0.088	0.14**	0.14**
	(0.029)	(0.046)	(0.057)	(0.058)	(0.060)
Year FE	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes
Observations	781260	578698	578698	578698	578698
	Panel B: Maximum Weekly Benefit Amount				
	Pooled Sample (1)	Net Liquid Wealth (2)	Liquid Wealth (3)	Total Net Worth (4)	Total Wealth (5)
Log(Real WBA)	-0.0059*	-0.017***	-0.011*	-0.020***	-0.017**
	(0.0033)	(0.0054)	(0.0062)	(0.0065)	(0.0070)
Wealth Q2		-0.071**	-0.014	-0.069	-0.042
		(0.033)	(0.045)	(0.046)	(0.045)
Wealth Q3		-0.055**	0.0010	-0.098**	-0.073*
		(0.026)	(0.037)	(0.041)	(0.040)
Wealth Q4		-0.051*	-0.025	-0.090**	-0.068*
		(0.027)	(0.040)	(0.042)	(0.041)
Wealth Q2 × Log(Real WBA)		0.012**	0.0028	0.012	0.0069
		(0.0058)	(0.0080)	(0.0079)	(0.0078)
Wealth Q3 × Log(Real WBA)		0.0091*	-0.000035	0.016**	0.012*
		(0.0046)	(0.0066)	(0.0070)	(0.0069)
Wealth Q4 × Log(Real WBA)		0.0084*	0.0045	0.015**	0.011
		(0.0047)	(0.0070)	(0.0072)	(0.0071)
Constant	0.057***	0.12***	0.085**	0.14***	0.12***
	(0.019)	(0.031)	(0.036)	(0.039)	(0.041)
Year FE	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes
Observations	781260	578698	578698	578698	578698

Notes: Placebo sample includes individuals who did not report being unemployed. Coefficients reported are semi-elasticities of mobility rate with respect to the UI real weekly benefit amount (national 2000 dollars) estimated using a linear probability model. All columns include these additional controls: age, gender, race, education level, marital status, log of real annual wage, number of kids, whether the spouse is working, housing tenure and a dummy variable for being on the seam between two waves. Standard errors are clustered by state and shown in parentheses. * p<0.10, ** p<0.05, *** p<0.01

Table 3.17: Placebo Test 2: Effect of UI on Unemployed Workers not Receiving UI Benefits

Panel A: Real Weekly Benefit Amount

	Pooled Sample	Net Liquid Wealth	Liquid Wealth	Total Wealth
	(1)	(2)	(3)	(4)
Log(Real WBA)	0.034			
	(0.073)			
Wealth Q1 × Log(Real WBA)		-0.069	-0.0015	-0.0095
		(0.090)	(0.069)	(0.073)
Wealth Q2 × Log(Real WBA)		-0.0098	-0.085	-0.080
		(0.075)	(0.092)	(0.11)
Wealth Q3 × Log(Real WBA)		-0.042	-0.11	-0.11
		(0.11)	(0.12)	(0.11)
Wealth Q4 × Log(Real WBA)		-0.16	-0.11	-0.070
		(0.13)	(0.12)	(0.12)
Year FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
UI Spells	699555	544700	544700	544700

Panel B: Average Weekly Benefit Amount

	Pooled Sample	Net Liquid Wealth	Liquid Wealth	Total Wealth
	(1)	(2)	(3)	(4)
Log(Real WBA)	0.28			
	(0.22)			
Wealth Q1 × Log(Real WBA)		0.46	0.41	0.28
		(0.32)	(0.33)	(0.32)
Wealth Q2 × Log(Real WBA)		0.49	0.55	0.27
		(0.38)	(0.35)	(0.30)
Wealth Q3 × Log(Real WBA)		-0.20	-0.17	0.12
		(0.31)	(0.31)	(0.39)
Wealth Q4 × Log(Real WBA)		-0.087	-0.17	0.13
		(0.38)	(0.35)	(0.37)
State FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	699555	544700	544700	544700

Panel B: Maximum Weekly Benefit Amount

	Pooled Sample	Net Liquid Wealth	Liquid Wealth	Total Wealth
	(1)	(2)	(3)	(4)
Log(Real WBA)	-0.12			
	(0.25)			
Wealth Q1 × Log(Real WBA)		-0.0053	0.0093	-0.16
		(0.29)	(0.31)	(0.28)
Wealth Q2 × Log(Real WBA)		0.059	-0.019	-0.18
		(0.32)	(0.28)	(0.35)
Wealth Q3 × Log(Real WBA)		-0.52	-0.42	-0.22
		(0.34)	(0.35)	(0.32)
Wealth Q4 × Log(Real WBA)		-0.49*	-0.53**	-0.18
		(0.29)	(0.27)	(0.28)
State FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	699555	544700	544700	544700

Notes: Placebo sample includes individuals who experienced an unemployment spell in the period 2001-2012 and did not report receiving UI benefits. Coefficients reported are elasticities of mobility hazard rates with respect to the UI real weekly benefit amount (national 2000 dollars) estimated using a stratified Cox hazard model. All columns include these additional controls: age, gender, race, education level, marital status, number of kids, whether the spouse is working, housing tenure, log of real annual wage and a dummy variable for being on the seam between two waves. All Estimates use individual weights at job separation. Standard errors are clustered by state and shown in parentheses. * p<0.10, ** p<0.05, *** p<0.01

Table 3.18: UI Weekly Benefits and Attrition

	Attrited (1)	Attrited (2)	Attrited (3)	Attrited (4)	Attrited (5)
Log(Real WBA)	0.0050				
	(0.011)				
Log(Real Max.WBA)		0.020			
		(0.015)			
Log(Real Avg.WBA)			0.030		
			(0.018)		
Log(Real Regular UI Amount)				0.0045	
				(0.011)	
Log(Real Total UI Amount)					0.0049
					(0.0093)
Individual Characteristics	Yes	Yes	Yes	Yes	Yes
Household Characteristics	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes	Yes
Observations	6675	6675	6675	6675	6675

Notes: Estimated using linear probability model where dependent variable is whether unemployed worker attrited during the unemployment spell. All columns include these additional controls: age, gender, race, education level, marital status, number of kids, whether the spouse is working, housing tenure, log of real annual wage and a dummy variable for being on the seam between two waves. All Estimates use individual weights at job separation. Standard errors are clustered by state and shown in parentheses. * p<0.10, ** p<0.05, *** p<0.01

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Appendix A

Who Really Benefits from Consumption Tax Cuts? Evidence from a Large VAT Reform in France

A.1 *Contrat D'Avenir* Details

The Sarkozy government signed the *Contrat D'Avenir* in April 2009, at the time of the restaurant VAT reform we are analyzing. The contract was not binding and only involved unionized restaurants, which represent approximately 50 percent of the restaurant industry.

The goal of the contract was to give directives on how to allocate the surplus created by the reduction in the VAT rate. These directives involved four broad categories: employment; prices; investments and modernization of the restaurant sector; and work conditions and social dialogue.

The price directives depended on the type of restaurant:

- Sit-down restaurants were encouraged to reduce prices by 11.8% for 7 out of the 10 following items: appetizer, entree (meat or fish), daily special, dessert, appetizer-entree menu, entree-dessert menu, kid's menu, soda or fruit juice, mineral water, coffee, tea or herbal tea. In case a restaurant did not sell at least seven of these products, it could also reduce prices by 11.8% for products that represent more than one third of total turnover, excluding alcoholic drinks.

- Although no tax reduction was enacted in take-out restaurants, the government instructed them to reduce price for their reference menus by 5%.

- Cafes and juice bars: a full incidence of the VAT reduction on the price.

The employment directives aimed to create 40,000 jobs over two years in addition to the 15,000 jobs that are created in the restaurant industry every year on average.

The work conditions and social dialogue directives aimed to broadly improve remuneration (for example, faster salary increases over the years), health coverage and training, and to reduce the use of illegal workers.

Finally, the modernization directive aimed at improving employee and customers' safety (including better hygiene), increasing customers' comfort (for example through the purchase of better tables and chairs), the acquisition of environmentally friendly equipment, the renewal of electronic equipment, and increasing the size of the restaurants.

A report by the Ministere de l'Artisanat, du Commerce et du Tourisme[1] attempts to analyze whether these directives were achieved. A significant issue they struggle with is that no clear measures were established ex-ante. For example, the price drops the directives suggest are not given a time frame making it hard to assess.

It is worth re-emphasizing that these measures were not binding and were not enforced by the government.

A.2 2004-2009 Payroll Tax Reductions

This government subsidy program, targeting the *Hotels Cafés et Réstaurants* (HCR) industry, was implemented in 2004 as a temporary measure to help restaurants before the introduction of the VAT cut in 2009. The 2004-804 Law established that firms operating in the HCR sector are eligible for an employment subsidy, initially available for one year and a half. In addition, the 2004-1239 Decree approved on November 22, 2004 defined the criteria for the implementation of the subsidy. The subsidy was subsequently extended to the period 2006-2008 by the 2005-1719 Law (December 30, 2005), the 2006-1666 Law (December 21, 2006) and the 2007-1822 Law (December 24, 2007), with small changes relative to the original measures. In July 2009, the 2009-888 Law abolished the subsidy, as the VAT cut from 19.6 to 5.5 percent became effective.

Subsidies under this program could not be claimed for the following categories of workers:

- All young workers below 18 years old, who already receive a subsidy of 10-20 percent.
- Employees hired under one of these contracts: (a) *contrat jeune en entreprise*, (b) *contrat initiative emploi*, (c) *contrat d'apprentissage*, (d) *contrat d'insertion RMA*, (e) *contrat de l'aide dégressive a l'employeur (ADE)*, (f) *contrat d'accès a l'emploi dans les DOM*.
- Employees for which the employer claims the minimum wage (SMIC) in the hotel industry.
- Employees working in the following sub-industries: *hébergements non touristiques* (NAF 55.2 F), *cantines et restaurants d'entreprise* (code NAF 55.5A) and *restauration collective sous contrat* (NAF 55.5 C)

[1] Ministry of Craft, Commerce and Tourism.

Table A.1: Maximum Monthly Subsidy per Full-Time Employee in 2008

	Employee Earning Reference Salary	Employee Earning More than Reference Salary	
		Existing Establishments	New Establishments
	(1)	(2)	(3)
Hotels and similar accommodation (55.10Z)[(a)]	114.4	114.4	28.6
Holiday and other short-stay accommodation (55.20Z)	114.4	114.4	57.2
Camping grounds and recreational vehicle parks (55.30Z)	114.4	114.4	28.6
Sit-down restaurants (56.10A)	180	180	180
Cafeterias and other self-service catering (56.10B)	180	180	180
Fast food restaurants (56.10C)	114.4	67.925	67.925
Event catering activities (56.21Z)	114.4	114.4	57.2
Beverage serving activities (56.30Z)[(b)]	114.4	90	90

Notes: Reference salary is defined as the minimum wage (SMIC) from 2004 to 2007 and the SMIC plus 3 percent after 2007. Existing establishments have been open for more than a year, while new establishments have operated for a year or less. All amounts are expressed in euros.
(a) Different amounts apply to the sub-category *Hôtels touristiques avec restaurant*. In this case a new firm hiring an employee earning more than the reference salary gets up to 90 euros.
(b) Both existing and new bowling alleys and casinos receive 28.6 euros for each worker earning more than the reference salary, while both existing and new discos receive 71.5 euros for each worker earning more than the reference salary.

Notice that though these subsidies were industry-specific, restaurants also received government subsidies that applied to all industries, and that were not abolished in 2009. These included subsidies on contributions paid (*allègements de cotisations sociales*) established by the January 2003 Fillon law, which could be received on top of the industry specific subsidies, and subsidies for firms operating in economically depressed geographic areas,[2] which a firm could not receive if it claimed the industry specific subsidies. Finally, subsidies received by each firm could not be larger than 200,000 euros over three years, as established by European rules on government subsidies.

[2]These include the *Zones Franches Urbaines (ZFU)*, the *Zones de Revitalisation Rurale (ZRR)* and the *Zones de Redynamisation Urbaine (ZRU)*

A.3 Employment Contract types

Indeterminate Length Contract Indeterminate Length Contracts (*Contrats a Duree Inderterminee*) do not have a specific expiry date. Workers are employed for an undetermined length of time. Termination occurs if workers decide to quit, if they are fired or if they retire. If workers are fired, employers are expected to pay them a severance pay. This type of contract usually starts with a 4-month trial period during which the contract can be terminated at no cost.

Determinate Length Contract Determinate Length Contracts (*Contrats a Duree Derterminee*) have a specific expiry date after which the contract is terminated unless it is renewed for an additional period of time or if the contract is transformed into an Indeterminate Length Contract. It is estimated that there were 2,250,002 such contracts in 2009.

A.4 Income and Payroll Tax Rates

Table A.2: Marginal Income Tax Rates and Payroll Tax Rates

Income	τ	p
up to 9,690 euros	0%	50.85%
9,690 to 12,344 euros	14%	41.95%
12,344 to 26,764 euros	14%	21.28%
26,764 to 71,754 euros	30%	21.28%
71,754 to 151,956 euros	41%	21.28%
above 151,956 euros	45%	21.28%

Table A.3: Payroll Taxes

			Employee Share of Payroll Taxes			
Monthly Salary	Sickness	Old Age	Family	Unemployment	Retirement	Total
0 to 1.3 min. wage	0.75%	6.75%	0%	2.40%	3.80%	13.70%
1.3 min. wage to 3,086	0.75%	6.75%	0%	2.40%	3.80%	13.70%
3,086 to 9,258	0.75%	0.10%	0%	2.40%	8.90%	12.15%
9,259 to 12,344	0.75%	0.10%	0%	2.40%	0%	3.25%
12,344 to 24,688	0.75%	0.10%	0%	0.00%	0.33%	1.18%
more than 24,988	0.75%	0.10%	0%	0.00%	0.33%	1.18%
			Employer Share of Payroll Taxes			
Monthly Salary	Sickness	Old Age	Family	Unemployment	Retirement	Total
0 to min. wage	13.10%	8.40%	5.40%	4.30%	6.22%	37.42%
min. wage to 1.3 min. wage	13.10%	8.40%	5.40%	4.30%	6.22%	37.42%
1.3 to 1.6 min. wage	13.10%	8.40%	5.40%	4.30%	6.22%	37.42%
1.6 min. wage to 3,086	13.10%	8.40%	5.40%	4.30%	6.22%	37.42%
3,086 to 9,258	13.10%	1.60%	5.40%	4.30%	14.30%	38.70%
9,259 to 12344	13.10%	1.60%	5.40%	4.30%	14.30%	38.70%
12,344 to 24,688	13.10%	1.60%	5.40%	0%	0%	20.10%
more than 24,988	13.10%	1.60%	5.40%	0%	0%	20.10%

Notes: This table reports the share of payroll taxes paid by employers and employees as well as the total payroll taxes paid.

A.5 Model

Short-Run Equilibrium: Vertical Capital Supply

Firms

Each symmetric firm's $j = 1, .., J$ profit maximization problem is given by:

$$\max_{l_j, m_j, k_j} \pi_j = (1-\tau)[p_x A l_j^{\gamma} m_j^{\delta} k_j^{1-\gamma-\delta} - c m_j] - w_x l_j - r_x k_j$$

It follows that the demand for labor, material goods, and capital are determined from the FOCs:

$$\frac{\partial \pi_j}{\partial l_j} = \gamma(1-\tau) A p_x (\frac{m_j^{\delta} k_j^{1-\gamma-\delta}}{l_j^{1-\gamma}}) - w_x = 0$$

$$\frac{\partial \pi_j}{\partial m_j} = \delta A p_x (\frac{l_j^{\gamma} k_j^{1-\gamma-\delta}}{m_j^{1-\delta}}) - c = 0$$

and:

$$\frac{\partial \pi_j}{\partial k_j} = (1-\gamma-\delta)(1-\tau) A p_x (\frac{l_j^{\gamma} m_j^{\delta}}{k_j^{\gamma+\delta}}) - r_x = 0$$

Given that firms are symmetric, $l_j^D = \frac{L_x^D}{J}$, $m_j^D = \frac{M_x^D}{J}$ and $k_j^D = \frac{\overline{K}_x}{J}$. Therefore:

$$\frac{\partial \pi_j}{\partial l_j} = \gamma(1-\tau) A p_x (\frac{M_x^{D\delta} \overline{K}_x^{1-\gamma-\delta}}{L_x^{D 1-\gamma}}) - w_x = 0 \tag{A.1}$$

$$\frac{\partial \pi_j}{\partial m_j} = \delta A p_x (\frac{L_x^{D\gamma} \overline{K}_x^{1-\gamma-\delta}}{M_x^{D 1-\delta}}) - c = 0 \tag{A.2}$$

$$\frac{\partial \pi_j}{\partial k_j} = (1-\gamma-\delta)(1-\tau) A p_x (\frac{L_x^{D\gamma} M_x^{D\delta}}{\overline{K}_x^{\gamma+\delta}}) - r_x = 0 \tag{A.3}$$

Combining the labor supply function, the material goods demand function (A.2) and the labor demand function (A.1), we get:

$$w_x = \{(\frac{\delta}{c})^{\delta} [\gamma(1-\tau)]^{1-\delta} A p_x (\frac{\overline{K}_x}{\overline{N}_x})^{1-\gamma-\delta}\}^{\frac{\beta}{F}} \tag{A.4}$$

where $F = (\beta+1)(1-\delta) - \gamma$. We can substitute this into the labor supply function to get the labor equilibrium level as a function of prices and taxes:

$$L_x = \overline{N}_x [\{(\frac{\delta}{c})^{\delta} [\gamma(1-\tau)]^{1-\delta} A p_x (\frac{\overline{K}_x}{\overline{N}_x})^{1-\gamma-\delta}\}]^{\frac{1}{F}} \tag{A.5}$$

Plugging this expression into the demand for material goods (A.2) gives:

$$M_x = \{(\frac{\delta}{c})^{\beta+(1-\gamma)} [\gamma(1-\tau)]^{\gamma}[Ap_x]^{\beta+1}\overline{K}_x{}^{(1-\gamma-\delta)(\beta+1)}\overline{N}_x^{\gamma\beta}\}^{\frac{1}{F}} \tag{A.6}$$

Combining the labor supply function, the material goods demand function (A.2) and the capital demand function (A.3) gives:

$$r_x = (1-\gamma-\delta)\{(\frac{\delta}{c})^{\delta(\beta+1)} \gamma^{\gamma}(1-\tau)^{(\beta+1)(1-\delta)}[Ap_x]^{\beta+1}(\frac{\overline{N}_x}{\overline{K}_x})^{\gamma\beta}\}^{\frac{1}{F}} \tag{A.7}$$

Finally, plugging in the equilibrium labor (A.5) and material goods (A.6) used by the firm into the production function gives us the equilibrium output supplied:

$$X^S = \{A^{\beta+1}(\frac{\delta}{c})^{\delta(\beta+1)} [\gamma(1-\tau)]^{\gamma}p_x^{\gamma+\delta(\beta+1)}\overline{K}_x{}^{(1-\gamma-\delta)(\beta+1)}\overline{N}_x^{\gamma\beta}\}^{\frac{1}{F}} \tag{A.8}$$

While we are unable to solve for the equilibrium price, total differentiation of the demand and supply of the taxed good gives us a formula for the price elasticity to the tax change:

$$\frac{d\log p_x}{d\log(1-\tau)} = \frac{[\frac{(1-\alpha)^{\sigma}}{\alpha^{\sigma}p_x^{1-\sigma}(1-\tau)+(1-\alpha)^{\sigma}} - \frac{(\beta+1)(1-\delta)}{F}\theta - \frac{cM_x^D}{I^n}]}{[\frac{(\sigma-1)(1-\alpha)^{\sigma}}{\alpha^{\sigma}p_x^{1-\sigma}(1-\tau)+(1-\alpha)^{\sigma}} + \frac{\beta+1}{F}\theta]} \tag{A.9}$$

Price Elasticity to the Tax Change

Given the aggregate demand function for the taxed good:

$$X^D = \frac{(p_x/\alpha)^{-\sigma}}{\alpha^{\sigma}p_x^{1-\sigma}(1-\tau)+(1-\alpha)^{\sigma}}(w_x^{\frac{\beta+1}{\beta}}\overline{N}_x + w_z^{\frac{\beta+1}{\beta}}\overline{N}_z + r_x\overline{K}_x + r_z\overline{K}_z + c(M_x^D + M_z^D))$$

Taking the log of each side and total differentiating yields:

$$\begin{aligned} d\log X^d = -\sigma d\log p_x - \frac{\alpha^{\sigma}p_x^{1-\sigma}(1-\tau)[(1-\sigma)d\log p_x + d\log(1-\tau)]}{\alpha^{\sigma}p_x^{1-\sigma}(1-\tau)+(1-\alpha)^{\sigma}} + \\ + (1-\theta)\frac{\beta+1}{F}[(1-\delta)d\log(1-\tau) + d\log p_x] - \frac{cM_x^D}{I^n}d\log(1-\tau) \end{aligned}$$

where:

$$\theta = \frac{w_z^{\frac{\beta+1}{\beta}}\overline{N}_z + r_z\overline{K}_z + cM_z^D}{(w_x^{\frac{\beta+1}{\beta}}\overline{N}_x + w_z^{\frac{\beta+1}{\beta}}\overline{N}_z + r_x\overline{K}_x + r_z\overline{K}_z + c(M_x^D + M_z^D))}$$

is the share of total aggregate income in the untaxed sector and:

$$I^n = (w_x^{\frac{\beta+1}{\beta}}\overline{N}_x + w_z^{\frac{\beta+1}{\beta}}\overline{N}_z + r_x\overline{K}_x + r_z\overline{K}_z + c(M_x^D + M_z^D))$$

is aggregate income net of government redistribution. Dividing each term by $d\log(1-\tau)$:

$$\frac{d\log X^d}{d\log(1-\tau)} = [-\sigma - \frac{\alpha^\sigma p_x^{1-\sigma}(1-\tau)(1-\sigma)}{\alpha^\sigma p_x^{1-\sigma}(1-\tau)+(1-\alpha)^\sigma}]\frac{d\log p_x}{d\log(1-\tau)} - \\ - \frac{\alpha^\sigma p_x^{1-\sigma}(1-\tau)}{\alpha^\sigma p_x^{1-\sigma}(1-\tau)+(1-\alpha)^\sigma} + (1-\theta)\frac{\beta+1}{F}[(1-\delta)+\frac{d\log p_x}{d\log(1-\tau)}] - \frac{cM_x^D}{I^n} \tag{A.10}$$

Similarly, given the aggregate supply of the taxed good:

$$X^S = \{A^{\beta+1}(\frac{\delta}{c})^{\delta(\beta+1)}[\gamma(1-\tau)]^\gamma p_x^{\gamma+\delta(\beta+1)}\overline{K}_x^{(1-\gamma-\delta)(\beta+1)}\overline{N}_x^{\gamma\beta}\}^{\frac{1}{F}}$$

Then log-differentiating and rearranging:

$$\frac{d\log X^S}{d\log(1-\tau)} = \frac{\gamma}{F} + \frac{\gamma+\delta(\beta+1)}{F}\frac{d\log p_x}{d\log(1-\tau)} \tag{A.11}$$

Given market clearing in the goods market, we can equate (A.10) to (A.11) to solve for the price elasticity:

$$\frac{d\log p_x}{d\log(1-\tau)} = \\ = \frac{[\frac{(1-\alpha)^\sigma}{\alpha^\sigma p_x^{1-\sigma}(1-\tau)+(1-\alpha)^\sigma} - \frac{(\beta+1)(1-\delta)}{F}\theta - \frac{cM_x^D}{I^n}]}{[\frac{(\sigma-1)(1-\alpha)^\sigma}{\alpha^\sigma p_x^{1-\sigma}(1-\tau)+(1-\alpha)^\sigma} + \frac{\beta+1}{F}\theta]} \tag{A.12}$$

Demand and Supply Elasticities to Price

Price elasticity of the demand for the taxed good:

$$\epsilon^D = \frac{d\log X^D}{d\log p_x} = -\sigma - \frac{(1-\sigma)\alpha^\sigma p_x^{1-\sigma}(1-\tau)}{\alpha^\sigma p_x^{1-\sigma}(1-\tau)+(1-\alpha)^\sigma} + (1-\theta)\frac{\beta+1}{F} \\ = \frac{(1-\sigma)(1-\alpha)^\sigma}{\alpha^\sigma p_x^{1-\sigma}(1-\tau)+(1-\alpha)^\sigma} - \frac{\beta+1}{F}\theta + \frac{\gamma+\delta(\beta+1)}{F}$$

Price elasticity of the supply for the taxed good:

$$\epsilon^S = \frac{d\log X^S}{d\log p_x} = \frac{\gamma+\delta(\beta+1)}{F}$$

Therefore the denominator of (A.12) is:

$$[\frac{(\sigma-1)(1-\alpha)^\sigma}{\alpha^\sigma p_x^{1-\sigma}(1-\tau)+(1-\alpha)^\sigma} + \frac{\beta+1}{F}\theta] = \epsilon^S - \epsilon^D$$

and the numerator of (A.12) is:

$$\epsilon^D - \epsilon^S + \frac{\sigma(1-\alpha)^\sigma}{\alpha^\sigma p_x^{1-\sigma}(1-\tau)+(1-\alpha)^\sigma} + \\ + \frac{\delta(\beta+1)}{F}\theta - \frac{cM_x^D}{I^n} = [\frac{(1-\alpha)^\sigma}{\alpha^\sigma p_x^{1-\sigma}(1-\tau)+(1-\alpha)^\sigma} - \frac{(\beta+1)(1-\delta)}{F}\theta - \frac{cM_x^D}{I^n}]$$

It follows that:

$$\frac{d\log p_x}{d\log(1-\tau)} = \\ = \frac{\epsilon^D - \epsilon^S + \frac{\sigma(1-\alpha)^\sigma}{\alpha^\sigma p_x^{1-\sigma}(1-\tau)+(1-\alpha)^\sigma} + \frac{\delta(\beta+1)}{F}\theta - \frac{cM_x^D}{I^n}}{\epsilon^S - \epsilon^D} \\ = [\frac{\sigma(1-\alpha)^\sigma}{\alpha^\sigma p_x^{1-\sigma}(1-\tau)+(1-\alpha)^\sigma} + \frac{\delta(\beta+1)}{F}\theta - \frac{cM_x^D}{I^n}]\frac{1}{(\epsilon^S - \epsilon^D)} - 1$$

where $F = (\beta+1)(1-\delta) - \gamma$

Elasticities of Outcome Variables to the Tax

Given the equilibrium wage (A.4), the wage elasticity to the consumption tax is:

$$\frac{d\log w_x}{d\log(1-\tau)} = \frac{\beta}{F}[(1-\delta) + \frac{d\log p_x}{d\log(1-\tau)}] \\ = \frac{\beta}{F}\{[\frac{\sigma(1-\alpha)^\sigma}{\alpha^\sigma p_x^{1-\sigma}(1-\tau)+(1-\alpha)^\sigma} + \frac{\delta(\beta+1)}{F}\theta - \frac{cM_x^D}{I^n}]\frac{1}{(\epsilon^S - \epsilon^D)} - \delta\}$$

Analogously, given (A.5), the equilibrium labor elasticity to the consumption tax is:

$$\frac{d\log L_x}{d\log(1-\tau)} = \frac{1}{F}[(1-\delta) + \frac{d\log p_x}{d\log(1-\tau)}] \\ = \frac{1}{F}\{[\frac{\sigma(1-\alpha)^\sigma}{\alpha^\sigma p_x^{1-\sigma}(1-\tau)+(1-\alpha)^\sigma} + \frac{\delta(\beta+1)}{F}\theta - \frac{cM_x^D}{I^n}]\frac{1}{(\epsilon^S - \epsilon^D)} - \delta\}$$

Given (A.6), the elasticity of material goods purchased to the consumption tax is:

$$\frac{d\log M_x}{d\log(1-\tau)} = \frac{1}{F}[\gamma + (\beta+1)\frac{d\log p_x}{d\log(1-\tau)}] \\ = \frac{1}{F}\{[\frac{\sigma(1-\alpha)^\sigma}{\alpha^\sigma p_x^{1-\sigma}(1-\tau)+(1-\alpha)^\sigma} + \frac{\delta(\beta+1)}{F}\theta - \frac{cM_x^D}{I^n}]\frac{1}{(\epsilon^S - \epsilon^D)} - (1-\gamma)\}$$

Given (A.7), the rental rate elasticity to the tax is:

$$\frac{d\log r_x}{d\log(1-\tau)} = \frac{\beta+1}{F}[(1-\delta) + \frac{d\log p_x}{d\log(1-\tau)}] \\ = \frac{\beta+1}{F}\{[\frac{\sigma(1-\alpha)^\sigma}{\alpha^\sigma p_x^{1-\sigma}(1-\tau)+(1-\alpha)^\sigma} + \frac{\delta(\beta+1)}{F}\theta - \frac{cM_x^D}{I^n}]\frac{1}{(\epsilon^S - \epsilon^D)} - \delta\}$$

Finally, the output elasticity to the tax using (A.8) is given by:

$$\begin{aligned}\frac{d\log X}{d\log(1-\tau)} &= \frac{1}{F}[\gamma+(\gamma+\delta(\beta+1))\frac{d\log p_x}{d\log(1-\tau)}] \\ &= \frac{(\gamma+\delta(\beta+1))}{F}\{[\frac{\sigma(1-\alpha)^\sigma}{\alpha^\sigma p_x^{1-\sigma}(1-\tau)+(1-\alpha)^\sigma}+\frac{\delta(\beta+1)}{F}\theta-\frac{cM_x^D}{I^n}]\frac{1}{(\epsilon^S-\epsilon^D)}- \\ &- \frac{\delta(\beta+1)}{\gamma+\delta(\beta+1)}\}\end{aligned}$$

Long-Run Equilibrium: Upward Sloping Capital Supply

The main difference in our long-run analysis is that the capital supply function is upward sloping:

$$K_x = \overline{K}_x {r_x}^{\frac{1}{\mu}}$$

where $\frac{1}{\mu}$ is the elasticity of the capital supplied to the rental rate of capital. In the short-run $\mu \to \infty$ and therefore $K_x = \overline{K}_x$. Compared to the short-run equilibrium, the long-run equilibrium takes into account the additional effect that taxes have on the equilibrium level of capital, which in turns affects the equilibrium levels of w, L, r, M and X.

Equilibrium

We can express the long-run equilibrium as a function of the short-run equilibrium and the equilibrium amount of capital used by firms. Specifically, in our long-run equilibrium:

$$K_x^{LR} = \overline{K}_x {r_x}^{\frac{1}{\mu}} = \overline{K}_x(1-\gamma-\delta)\{(\frac{\delta}{c})^{\delta(\beta+1)}\gamma^\gamma(1-\tau)^{(\beta+1)(1-\delta)}[Ap_x]^{\beta+1}(\frac{\overline{N}_x}{\overline{K}_x})^{\gamma\beta}\}^{\frac{1}{\psi}}$$

where $\psi = \mu(\beta+1)(1-\delta)+\gamma(\beta-\mu)$. The equilibrium for the other main outcomes of the model can be expressed as:

$$\begin{aligned}r_x^{LR} &= (1-\gamma-\delta)\{(\frac{\delta}{c})^{\delta(\beta+1)}\gamma^\gamma(1-\tau)^{(\beta+1)(1-\delta)}[Ap_x]^{\beta+1}(\frac{\overline{N}_x}{\overline{K}_x})^{\gamma\beta}\}^{\frac{1}{\psi}} \\ w_x^{LR} &= w_x^{SR} {r_x^{LR}}^{\frac{\beta(1-\gamma-\delta)}{\mu F}} = w_x^{SR}(\frac{K_x^{LR}}{\overline{K}_x})^{\frac{\beta(1-\gamma-\delta)}{F}} \\ L_x^{LR} &= L_x^{SR} {r_x^{LR}}^{\frac{(1-\gamma-\delta)}{\mu F}} = L_x^{SR}(\frac{K_x^{LR}}{\overline{K}_x})^{\frac{(1-\gamma-\delta)}{F}} \\ M_x^{LR} &= M_x^{SR} {r_x^{LR}}^{(1-\gamma-\delta)\frac{\beta+1}{\mu F}} = M_x^{SR}(\frac{K_x^{LR}}{\overline{K}_x})^{(1-\gamma-\delta)\frac{\beta+1}{F}} \\ X_x^{LR} &= X_x^{SR} {r_x^{LR}}^{(1-\gamma-\delta)\frac{\beta+1}{\mu F}} = X_x^{SR}(\frac{K_x^{LR}}{\overline{K}_x})^{(1-\gamma-\delta)\frac{\beta+1}{\mu F}}\end{aligned} \tag{A.13}$$

Price Elasticity to the Tax

In the long-run the elasticity of output demanded to the tax is:

$$\begin{aligned}\frac{d\log X^d}{d\log(1-\tau)} &= [-\sigma - \frac{\alpha^\sigma p_x^{1-\sigma}(1-\tau)(1-\sigma)}{\alpha^\sigma p_x^{1-\sigma}(1-\tau)+(1-\alpha)^\sigma}]\frac{d\log p_x}{d\log(1-\tau)} - \\ &- \frac{\alpha^\sigma p_x^{1-\sigma}(1-\tau)}{\alpha^\sigma p_x^{1-\sigma}(1-\tau)+(1-\alpha)^\sigma} + (1-\theta)\frac{\beta+1}{F}[(1-\delta)+\frac{d\log p_x}{d\log(1-\tau)}\frac{(\mu+1)F}{\psi}] - \\ &- \frac{cM_x^D}{I^n} + \frac{(\beta+1)^2(1-\delta)(1-\gamma-\delta)}{F\psi}(\frac{w_x^{\frac{\beta+1}{\beta}}\overline{N}_x + cM_x^D}{I_n}) \\ &= [\frac{d\log X^d}{d\log(1-\tau)}]^{SR} + \\ &+ \underbrace{\frac{(\beta+1)^2(1-\delta-\gamma)}{F\psi}[(1-\delta)(\frac{w_x^{\frac{\beta+1}{\beta}}\overline{N}_x + cM_x^D}{I_n}) + (1-\theta)\frac{d\log p_x}{d\log(1-\tau)}]}_{\text{Additional Effect in the Long-Run}}\end{aligned} \tag{A.14}$$

where the aggregate income net of government redistribution is:

$$I^n = (w_x^{\frac{\beta+1}{\beta}}\overline{N}_x + w_z^{\frac{\beta+1}{\beta}}\overline{N}_z + r_x^{\frac{\mu+1}{\mu}}\overline{K}_x + r_z^{\frac{\mu+1}{\mu}}\overline{K}_z + c(M_x^D + M_z^D))$$

The elasticity of output supplied to the tax is:

$$\frac{d\log X^S}{d\log(1-\tau)} = \underbrace{\frac{\gamma}{F} + \frac{\gamma+\delta(\beta+1)}{F}\frac{d\log p_x}{d\log(1-\tau)}}_{\text{Short-Run Elasticity}} + \frac{(\beta+1)^2(1-\gamma-\delta)}{F\psi} \tag{A.15}$$

Given market clearing in the goods market, we can equate (A.14) to (A.15) to solve for the price elasticity:

$$\begin{aligned}&\frac{d\log p_x}{d\log(1-\tau)} = \\ &= \frac{[\frac{(1-\alpha)^\sigma}{\alpha^\sigma p_x^{1-\sigma}(1-\tau)+(1-\alpha)^\sigma} - \frac{(\beta+1)(1-\delta)}{F}\overbrace{\frac{(\mu+1)F}{\psi}}^{\text{Long-Run}}\theta - \frac{cM_x^D}{I^n} - \overbrace{\frac{(\beta+1)^2(1-\gamma-\delta)(1-\delta)}{F\psi}\frac{r_x^{\frac{\mu+1}{\mu}}\overline{K}_x}{I_n}}^{\text{Long-Run}}]}{[\frac{(\sigma-1)(1-\alpha)^\sigma}{\alpha^\sigma p_x^{1-\sigma}(1-\tau)+(1-\alpha)^\sigma} + \frac{\beta+1}{F}\underbrace{\frac{(\mu+1)F}{\psi}}_{\text{Long-Run}}\theta]}\end{aligned} \tag{A.16}$$

where the fraction of aggregate income from the untaxed sector is:

$$\theta = \frac{w_z^{\frac{\beta+1}{\beta}}\overline{N}_z + r_z^{\frac{\mu+1}{\mu}}\overline{K}_z + cM_z^D}{(w_x^{\frac{\beta+1}{\beta}}\overline{N}_x + w_z^{\frac{\beta+1}{\beta}}\overline{N}_z + r_x^{\frac{\mu+1}{\mu}}\overline{K}_x + r_z^{\frac{\mu+1}{\mu}}\overline{K}_z + c(M_x^D + M_z^D))}$$

Therefore $\frac{(\mu+1)F}{\psi}$ and $\frac{(\beta+1)^2(1-\gamma-\delta)(1-\delta)}{F\psi}\frac{r_x^{\frac{\mu+1}{\mu}}\overline{K}_x}{I_n}$ are the extra terms relative to the short-run elasticity (A.12) and reflect the equilibrium capital adjusting in the long-run. Given that $\psi = \mu[(\beta+1)(1-\delta)-\gamma]+\gamma\beta$, and that $[(\beta+1)(1-\delta)-\gamma] > 0$, then:

$$\lim_{\mu\to\infty} \frac{(\beta+1)^2(1-\gamma-\delta)(1-\delta)}{F\psi}\frac{r_x^{\frac{\mu+1}{\mu}}\overline{K}_x}{I_n} = 0$$

and by l'Hopital's rule:

$$\lim_{\mu\to\infty} \frac{(\mu+1)F}{\psi} = 1$$

which gives us exactly (A.12), the formula derived for the short-run equilibrium.

Demand and Supply Elasticities to Price

Price elasticity of the demand for the taxed good:

$$\epsilon_{LR}^D = \epsilon_{SR}^D + \frac{(\beta+1)^2(1-\gamma-\delta)}{F\psi}(1-\theta)$$

Price elasticity of the supply for the taxed good:

$$\epsilon_{LR}^S = \epsilon_{SR}^S + \frac{(\beta+1)^2(1-\gamma-\delta)}{F\psi}$$

Therefore the denominator of (A.12) is:

$$\begin{aligned}[\frac{(\sigma-1)(1-\alpha)^\sigma}{\alpha^\sigma p_x^{1-\sigma}(1-\tau)+(1-\alpha)^\sigma} + \frac{\beta+1}{F}\frac{(\mu+1)}{F\psi}\theta] &= [\epsilon_{SR}^S - \epsilon_{SR}^D] + \frac{(\beta+1)^2(1-\gamma-\delta)}{F\psi}\theta \\ &= [\epsilon_{LR}^S - \epsilon_{LR}^D]\end{aligned}$$

and the numerator of (A.12) is:

$$\begin{aligned}&\epsilon^D - \epsilon^S + \frac{\sigma(1-\alpha)^\sigma}{\alpha^\sigma p_x^{1-\sigma}(1-\tau)+(1-\alpha)^\sigma} + \\ &+ \frac{\delta(\beta+1)}{F}\frac{(\mu+1)F}{\psi}\theta - \frac{cM_x^D}{I^n} - \frac{(\beta+1)^2(1-\gamma-\delta)(1-\delta)}{F\psi}\frac{r_x^{\frac{\mu+1}{\mu}}\overline{K}_x}{I_n} \\ &= [\frac{(1-\alpha)^\sigma}{\alpha^\sigma p_x^{1-\sigma}(1-\tau)+(1-\alpha)^\sigma} - \frac{(\beta+1)(1-\delta)}{F}\frac{(\mu+1)F}{\psi}\theta - \frac{cM_x^D}{I^n} - \\ &- \frac{(\beta+1)^2(1-\gamma-\delta)(1-\delta)}{F\psi}\frac{r_x^{\frac{\mu+1}{\mu}}\overline{K}_x}{I_n}]\end{aligned} \tag{A.17}$$

It follows that:

$$\frac{d\log p_x}{d\log(1-\tau)} =$$

$$= \frac{\epsilon^D_{LR} - \epsilon^S_{LR} + \frac{\sigma(1-\alpha)^\sigma}{\alpha^\sigma p_x^{1-\sigma}(1-\tau)+(1-\alpha)^\sigma} + \frac{\delta(\beta+1)}{F}\frac{(\mu+1)F}{\psi}\theta - \frac{cM_x^D}{I^n} - \frac{(\beta+1)^2(1-\gamma-\delta)(1-\delta)}{F\psi}\frac{r_x^{\frac{\mu+1}{\mu}}\overline{K}_x}{I_n}}{\epsilon^S_{LR} - \epsilon^D_{LR}}$$

$$= \frac{\left[\frac{\sigma(1-\alpha)^\sigma}{\alpha^\sigma p_x^{1-\sigma}(1-\tau)+(1-\alpha)^\sigma} + \frac{\delta(\beta+1)}{F}\frac{(\mu+1)F}{\psi}\theta - \frac{cM_x^D}{I^n} - \frac{(\beta+1)^2(1-\gamma-\delta)(1-\delta)}{F\psi}\frac{r_x^{\frac{\mu+1}{\mu}}\overline{K}_x}{I_n}\right]}{\epsilon^S - \epsilon^D} - 1$$

where $F = (\beta+1)(1-\delta) - \gamma$ and $\psi = \mu[(\beta+1)(1-\delta) - \gamma] + \gamma\beta = \mu F + \gamma\beta$.

Elasticities of Outcome Variables to the Tax

The long-run elasticities in the outcomes of interest are slightly different than the short-run ones. In particular

$$[\frac{d\log w_x}{d\log(1-\tau)}]^{LR} = \frac{\beta}{F}\{\overbrace{(1-\delta)+\frac{d\log p_x}{d\log(1-\tau)}}^{\text{Short-Run}}+ \underbrace{+\frac{(1-\gamma-\delta)}{\mu}[\frac{\mu(\beta+1)(1-\delta)}{\psi}+\frac{\mu(\beta+1)}{\psi}\frac{d\log p_x}{d\log(1-\tau)}]}_{\text{Long-Run}}\}$$

$$[\frac{d\log L_x}{d\log(1-\tau)}]^{LR} = \frac{1}{F}\{\overbrace{(1-\delta)+\frac{d\log p_x}{d\log(1-\tau)}}^{\text{Short-Run}}+ \underbrace{+\frac{(1-\gamma-\delta)}{\mu}[\frac{\mu(\beta+1)(1-\delta)}{\psi}+\frac{\mu(\beta+1)}{\psi}\frac{d\log p_x}{d\log(1-\tau)}]}_{\text{Long-Run}}\}$$

$$[\frac{d\log r_x}{d\log(1-\tau)}]^{LR} = \frac{\beta+1}{F}\overbrace{[(1-\delta)+\frac{d\log p_x}{d\log(1-\tau)}]}^{\text{Short-Run}}\ \underbrace{\frac{\mu F}{\psi}}_{\text{Long-Run}}$$

$$[\frac{d\log K_x}{d\log(1-\tau)}]^{LR} = \underbrace{\frac{\beta+1}{\psi}[(1-\delta)+\frac{d\log p_x}{d\log(1-\tau)}]}_{\text{Long-Run}}$$

$$[\frac{d\log M_x}{d\log(1-\tau)}]^{LR} = \frac{(\beta+1)}{F}\{\overbrace{\frac{\gamma}{\beta+1}+\frac{d\log p_x}{d\log(1-\tau)}}^{\text{Short-Run}}+ \underbrace{+\frac{(1-\gamma-\delta)}{\mu}[\frac{\mu(\beta+1)(1-\delta)}{\psi}+\frac{\mu(\beta+1)}{\psi}\frac{d\log p_x}{d\log(1-\tau)}]}_{\text{Long-Run}}\}$$

$$[\frac{d\log X}{d\log(1-\tau)}]^{LR} = \frac{1}{F}\{\overbrace{\gamma+(\gamma+\delta(\beta+1))\frac{d\log p_x}{d\log(1-\tau)}}^{\text{Short-Run}}+ \underbrace{+\frac{(\beta+1)^2(1-\gamma-\delta)}{F\psi}[(1-\delta)+\frac{d\log p_x}{d\log(1-\tau)}]}_{\text{Long-Run}}\}$$

Incidence of the Tax

In equilibrium, total after-tax firm revenue equals total income, that is:

$$(1-\tau)(p_x X - c_x M_x) = w_x L_x + r_x K_x$$

By the envelope theorem, we have:

$$(1-\tau)p_x dX = w_x dL_x + (1-\tau)c_x dM_x + r_x dK_x$$

In this case the first order effect of the tax is given by:

$$X d(\tau p_x) - c_x M_x d\tau - \tau M_x dc_x = X dp_x + L_x(-dw_x) + K_x(-dr_x) + M_x(-dc_x)$$

from which:

$$\frac{X dp_x}{X d(\tau p_x) - c_x M_x d\tau - \tau M_x dc_x} - \frac{L_x dw_x}{X d(\tau p_x) - c_x M_x d\tau - \tau M_x dc_x} - \\ - \frac{K_x dr_x}{X d(\tau p_x) - c_x M_x d\tau - \tau M_x dc_x} - \frac{M_x dc_x}{X d(\tau p_x) - c_x M_x d\tau - \tau M_x dc_x} = 1$$

Given the Cobb-Douglas production function, we have $\gamma = \frac{w_x L_x}{p_x X}$, $\delta = \frac{c_x M_x}{p_x X}$ and $(1-\gamma-\delta) = \frac{r_x K_x}{p_x X}$. It follows that:

$$\underbrace{\frac{d\ln p_x}{d\tau(1-\delta) + \tau(d\ln p_x - \delta d\ln c_x)}}_{\text{Share on Consumers}} - \underbrace{\gamma \frac{d\ln w_x}{d\tau(1-\delta) + \tau(d\ln p_x - \delta d\ln c_x)}}_{\text{Share on Employees}} \\ - \underbrace{(1-\gamma-\delta)\frac{d\ln r_x}{d\tau(1-\delta) + \tau(d\ln p_x - \delta d\ln c_x)}}_{\text{Share on Capital Owners}} - \underbrace{\delta \frac{d\ln c_x}{d\tau(1-\delta) + \tau(d\ln p_x - \delta d\ln c_x)}}_{\text{Share on Sellers of Material Goods}} = 1$$

Table A.4: Services Included in Price Index for Market Services

COICOP Code	Description
03.1.4	Cleaning, repair and hire of clothing
04.1.1/2	Actual rentals paid by tenants including other actual rentals
04.3.2	Services for the maintenance and repair of the dwelling
04.4.2	Refuse collection
04.4.3	Sewage collection
04.4.4	Other services relating to the dwelling n.e.c.
05.1.3	Repair of furniture, furnishings and floor coverings
05.3.3	Repair of household appliances
05.6.2	Domestic services and household services
08.1.0	Postal services
08.2/3.0	Telephone and telefax equipment and telephone and telefax services
09.1.5	Repair of audio-visual, photographic and information processing equipment
11.2.0	Accommodation services
12.5.2	Insurance connected with the dwelling
12.5.3	Insurance connected with health
12.5.4	Insurance connected with transport
12.5.5	Other insurance
12.6.2	Other financial services n.e.c.
12.7.0	Other services n.e.c.

Notes: This table reports COICOP codes used by Eurostat to describe price categories included in the service sector and categorized as market services by the INSEE.

A.6 Data

Definition of Market Services

Following level 1 of the French NAF Rev.2 industry classification and the official definition from the French National Institute of Statistics and Economic Studies (INSEE), this group includes: wholesale and retail trade, repair of motor vehicles and motorcycles (section G), accommodation service activities (division 55 in section I), information and communication (section J), financial and insurance activities (section K), real estate activities (section L), professional, scientific and technical activities (section M) and administrative and support service activities (section N).

Market services does not include services that are either non-marketable or subsidized by the government such as transportation and storage (section H), public administration and defense, compulsory social security (section O), education (section P), human health and social work activities (section Q), arts, entertainment and recreation (section R), and other services (section S).

Table A.5: Services Excluded from Price Index for Market Services

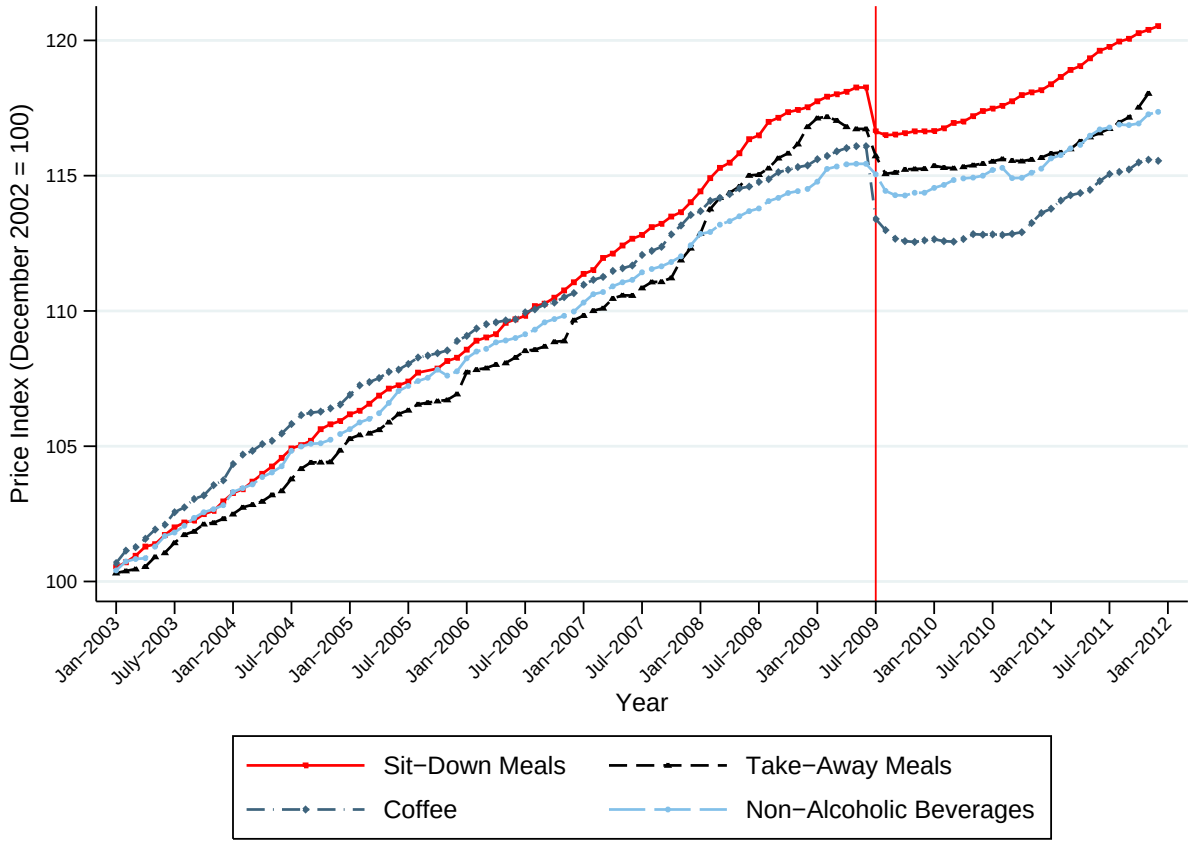
COICOP Code	Description
06.2.1/3	Medical and paramedical services
06.2.2	Dental services
06.3.0	Hospital services
07.2.3	Maintenance and repair of personal transport equipment
07.2.4	Other services in respect of personal transport equipment
07.3.1	Passenger transport by railway
07.3.2	Passenger transport by road
07.3.3	Passenger transport by air
07.3.4	Passenger transport by sea and inland waterway
07.3.5	Combined passenger transport
07.3.6	Other purchased transport services
09.2.3	Maintenance and repair of other major durables for recreation and culture
09.4.1	Recreational and sporting services
09.4.2	Cultural services
09.6.0	Package holidays
10.X.0	Pre-primary and primary, secondary, post-secondary non-tertiary, tertiary education, and education not definable by level
11.1.1	Restaurants, cafes and the like
11.1.2	Canteens
12.1.1	Hairdressing salons and personal grooming establishments
12.4.0	Social protection

Notes: This table reports COICOP codes used by Eurostat to describe price categories included in the service sector but excluded from the market services definition used by INSEE.

Goods Produced in Market Services

A.7 Figures

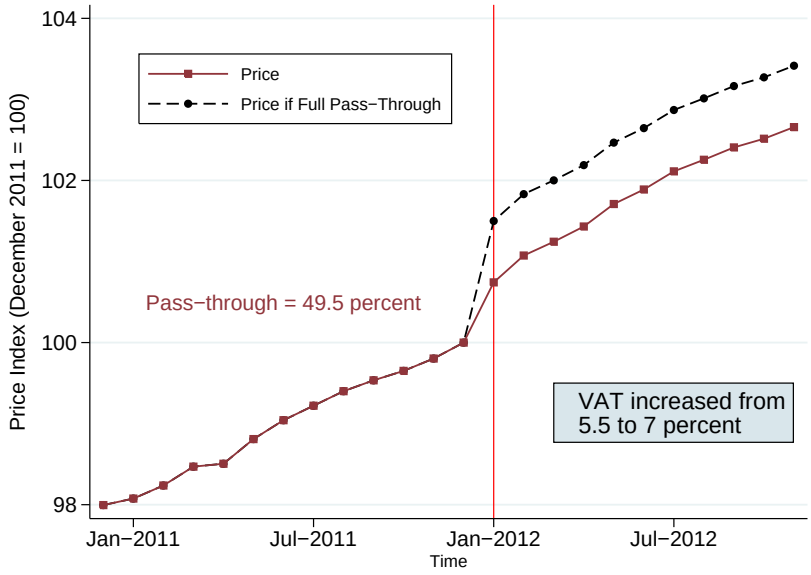
Figure A.1: Price Level in the *Restauration et Cafes* Sector



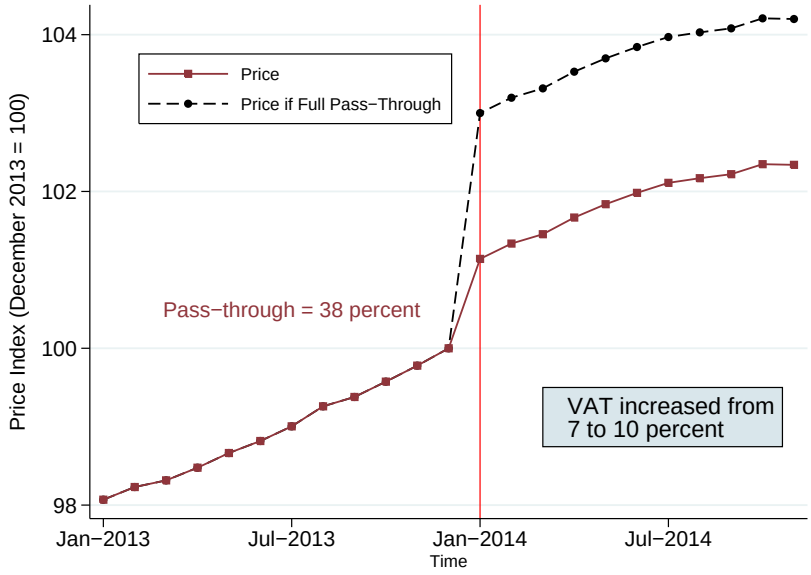
Notes: Computed using data on national price index from the INSEE.

Figure A.2: Pass-Through of VAT for 2012 and 2014 Reforms

a. 2012 Reform

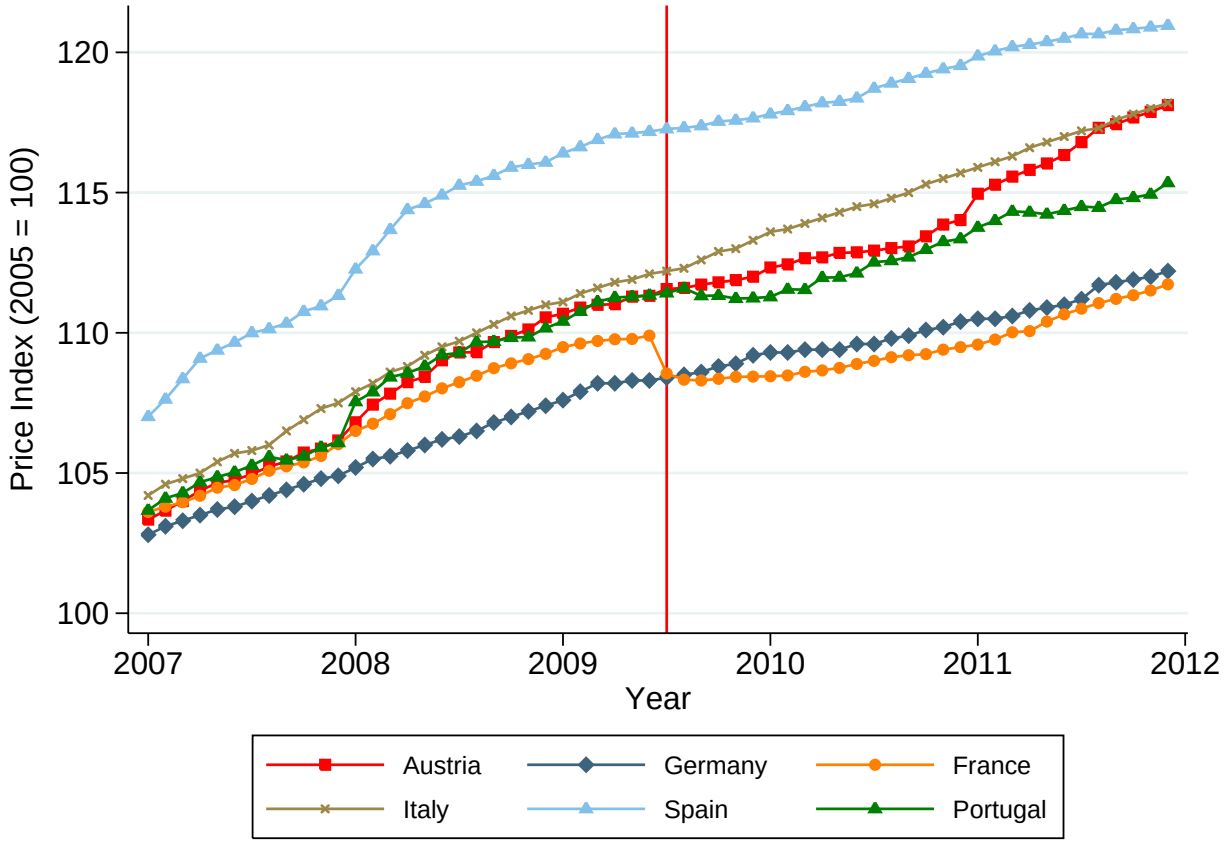


d. 2014 Reform



Source: Authors' computations using INSEE data.

Figure A.3: Price Level in the *Restauration et Cafes* Sector, Country Comparison



Source: Eurostat.

Figure A.4: International Price of Food Materials

Source: INSEE.

Figure A.5: Unconditional Means: Sit-Down Restaurants vs. Market Services

a. Value-Added per Employee

b. Cost per Employee

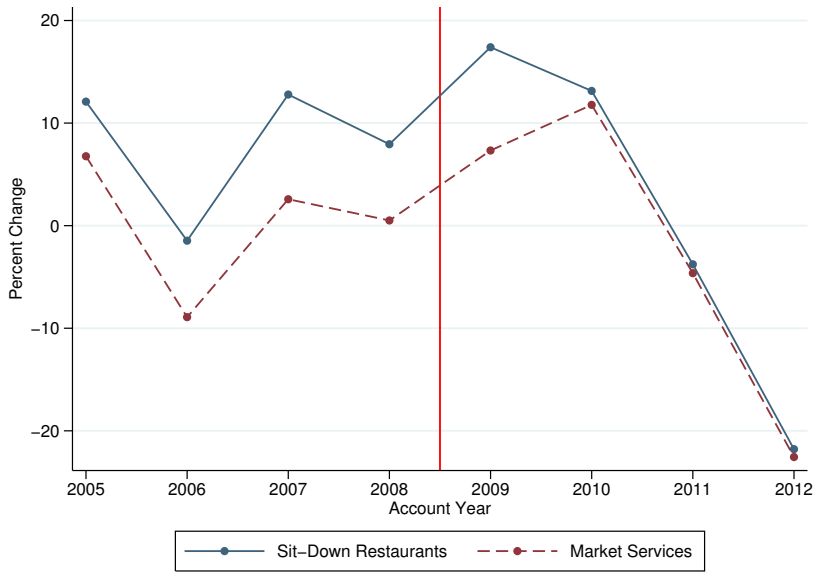
c. Number of Employees per Firm

d. Profit per Employee

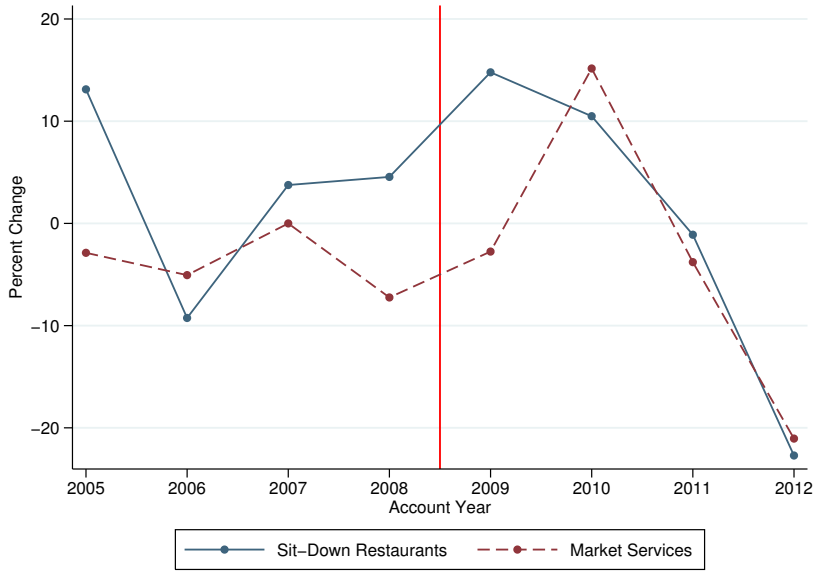
Notes: Computed using data on French sit-down restaurants from AMADEUS. All amounts are expressed in 2012 euros. The treatment group includes French sit-down restaurants, while the control group includes all firms in the French service sector (see the data appendix for details). Information is taken from restaurants' unconsolidated balance sheets. The top 1 percent and the bottom 1 percent of the profit-loss distribution are excluded from both the treatment and the control group.

Figure A.6: Yearly Percent Change in Total Employment

a. Total Number of Firms



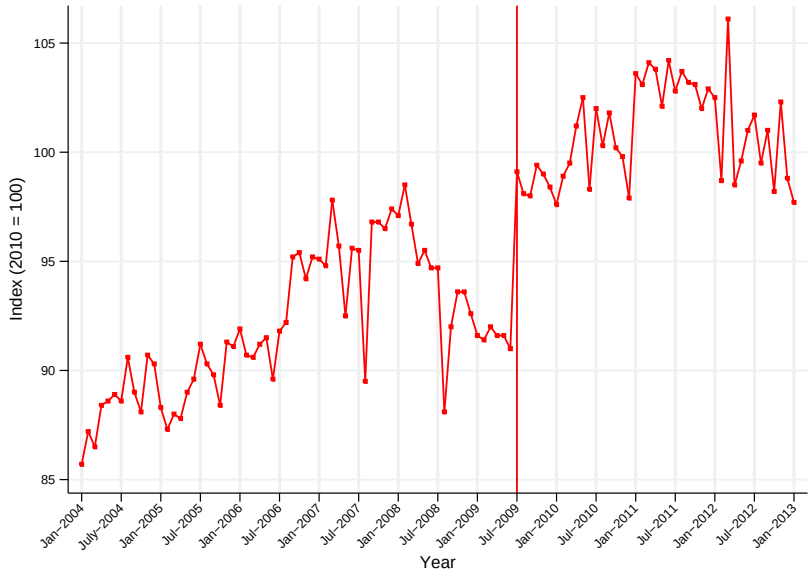
b. Total Number of Employees



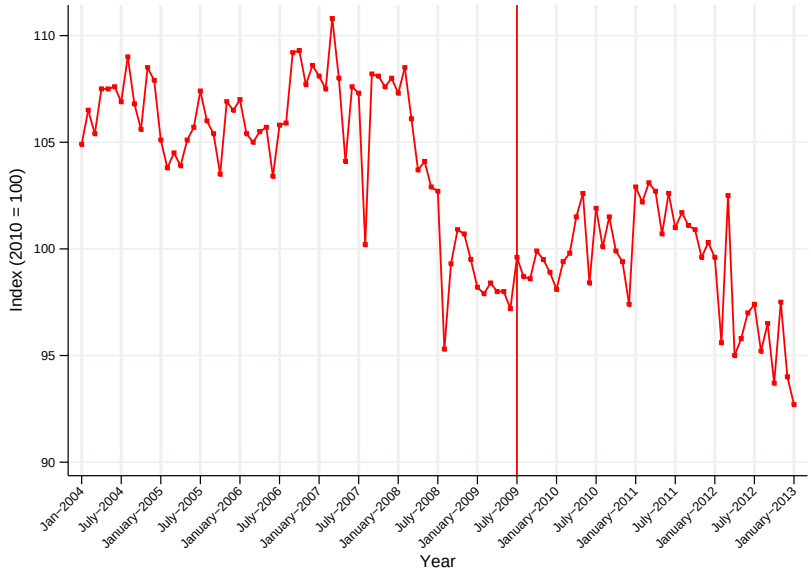
Notes: Computed using data on French sit-down restaurants from AMADEUS. The treatment group includes French sit-down restaurants, while the control group includes all firms in non-restaurant market services. Information is taken from restaurants' unconsolidated balance sheets. The top 1 percent and the bottom 1 percent of the profit-loss distribution are excluded from both the treatment and the control group.

Figure A.7: Total Turnover in Sit-Down Resturants

a. Sit-Down Restaurants, Value

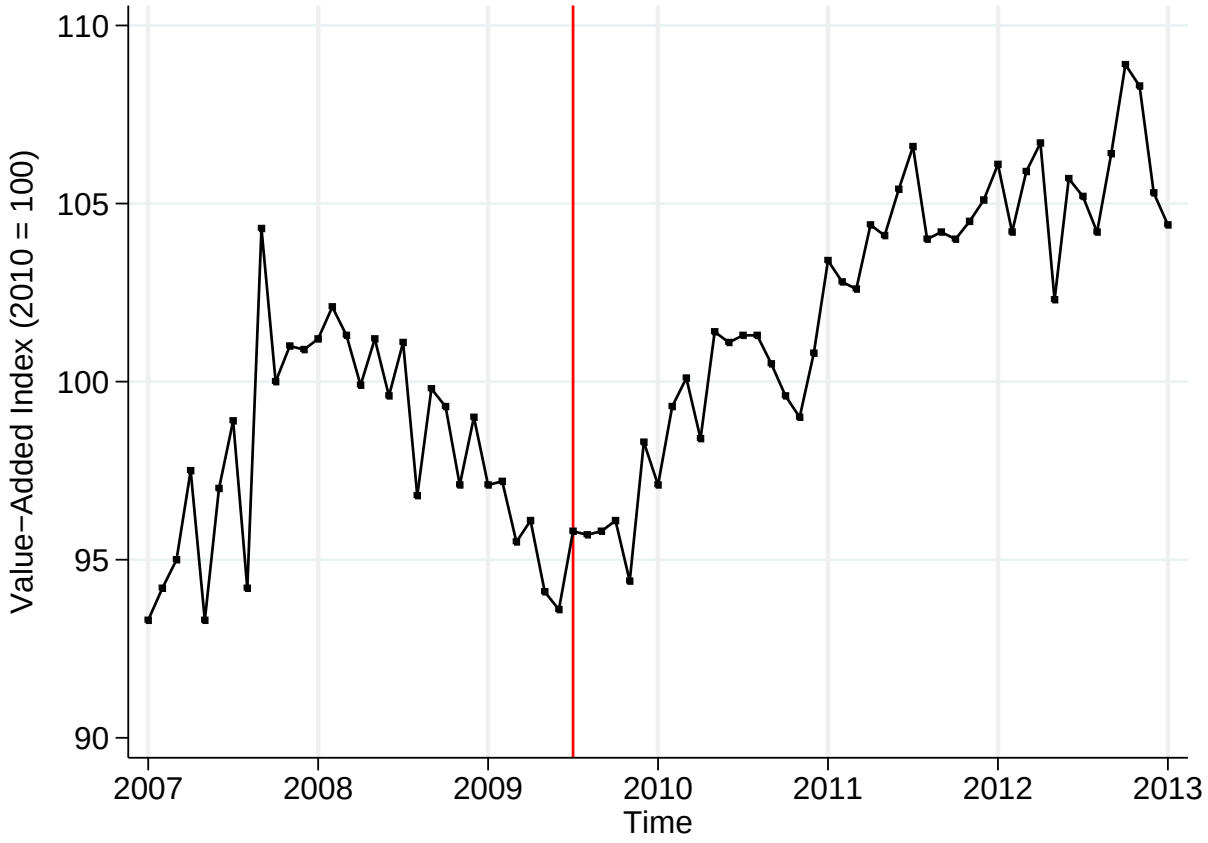


b. Sit-Down Restaurants, Volume



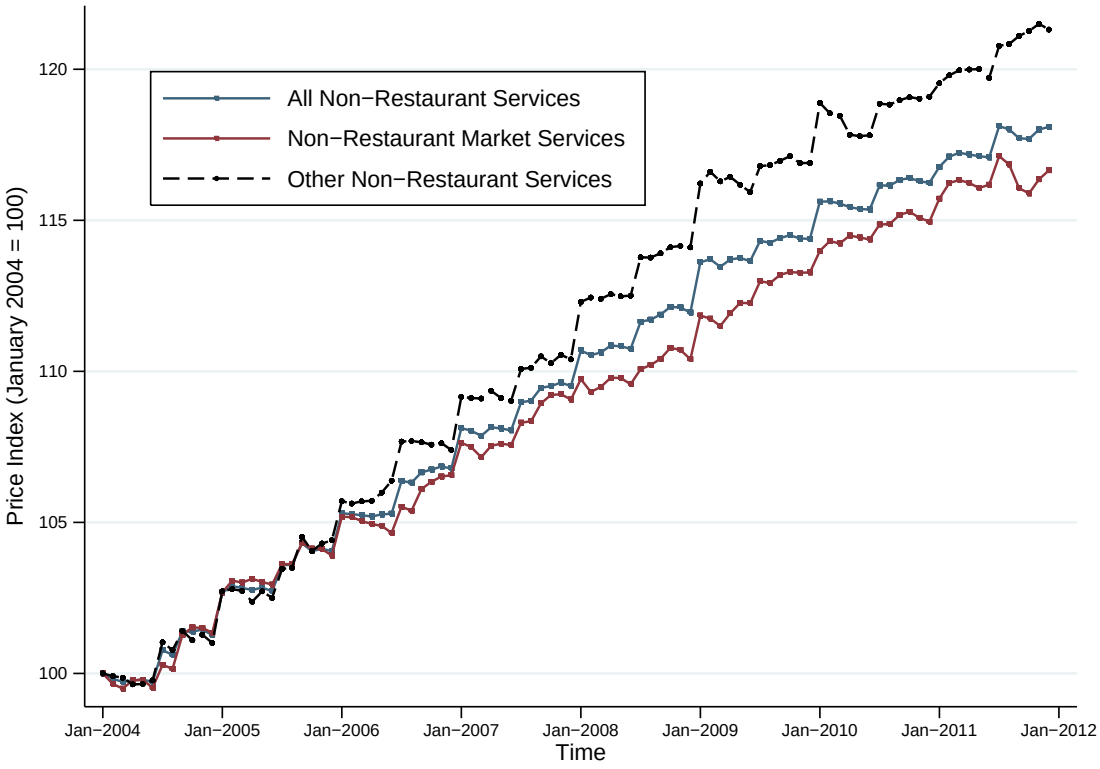
Source: INSEE.

Figure A.8: Total Turnover in *Hotels* sector



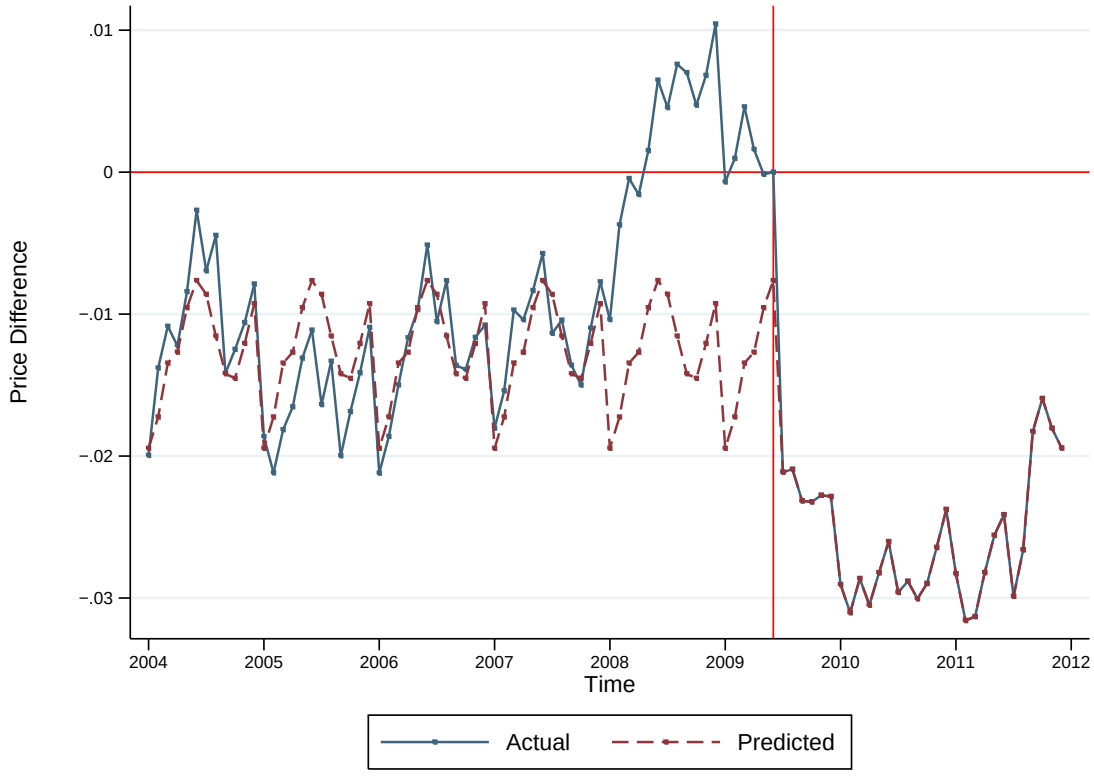
Source: INSEE.

Figure A.9: Price of Market Services vs. Other Services



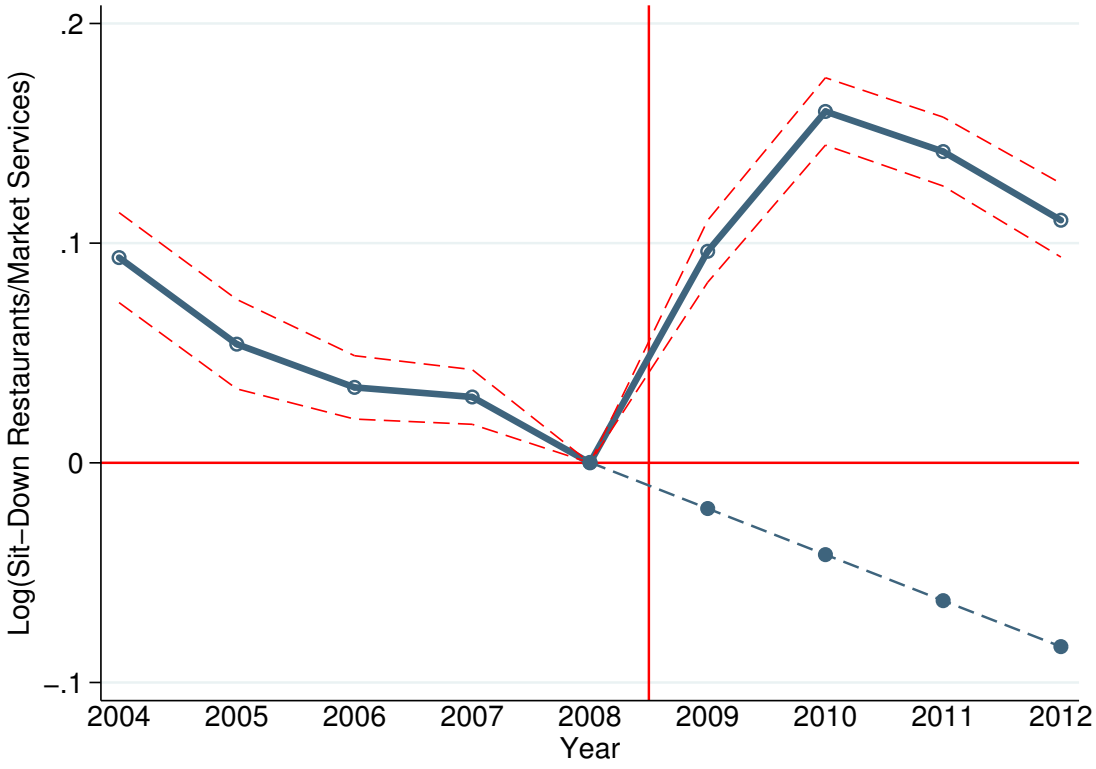
Notes: Price of market services versus other services using definitions above. Price data from EUROSTAT normalized to 100 in January 2004. Price series are seasonally adjusted using monthly dummy variables.

Figure A.10: Counterfactual Price Difference



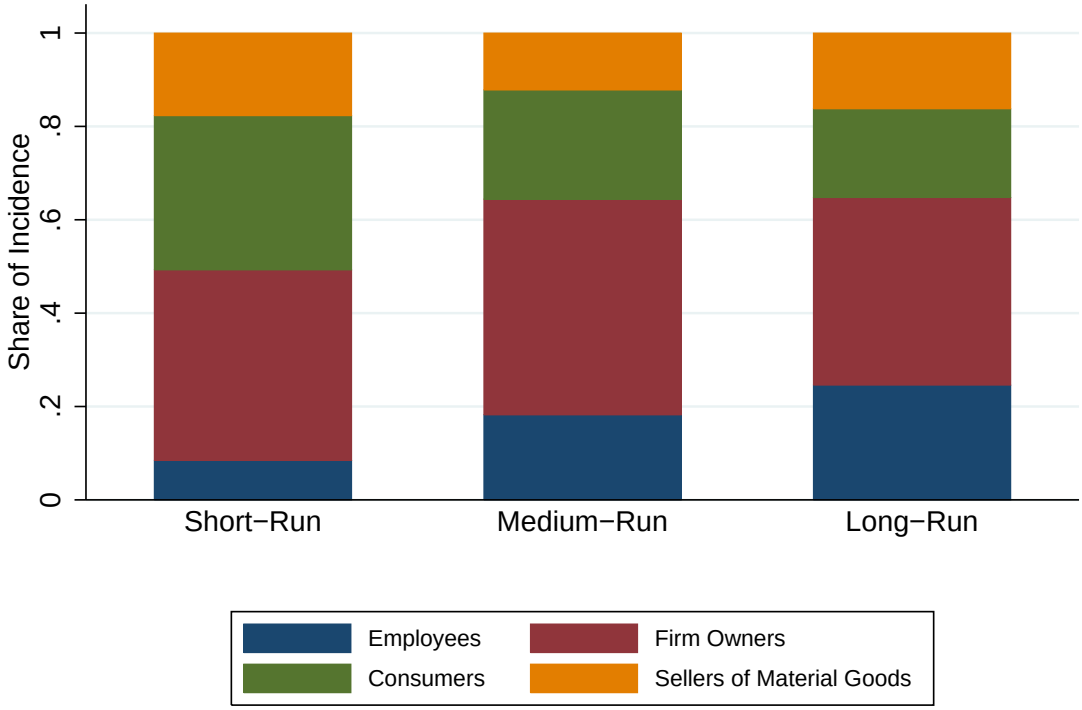
Source: INSEE and authors' computations on Eurostat data. Price series for market services is weighted average of seasonally adjusted price series computed using Eurostat price data by four digit COICOP and EUROSTAT weights. Price series are seasonally adjusted using monthly dummies. The counterfactual price difference is constructed from fitting the price change in the period January 2004-December 2007 on an eighth order polynomial of the month variable.

Figure A.11: Counterfactual Return to Capital per Employee: Sit-Down Restaurants vs. Market Services



Notes: The figure shows event-time coefficients estimated using equation (1.10), which includes year and firm fixed effects. The counterfactual log-difference is constructed using a linear fit on the pre-reform years 2004-2008. The treatment group includes all sit-down restaurants, while the control group includes all firms in market services. The dashed lines represent 95 percent confidence intervals.

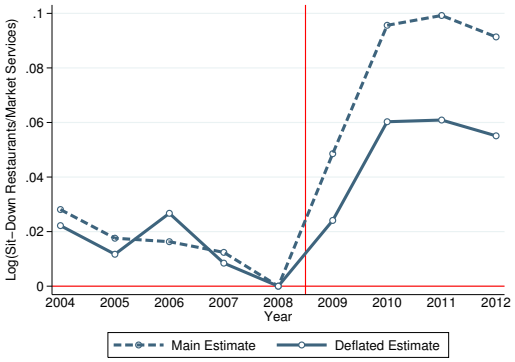
Figure A.12: Estimated Incidence of the VAT Reform, Including Sellers of Material Goods



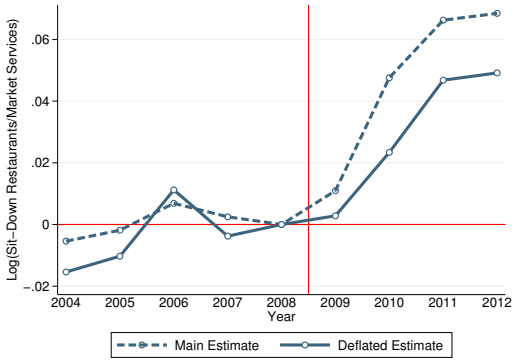
Notes: Incidence shares are computed using equation (1.8). The short-run denotes December 2009, the medium-run December 2010 and the long-run December 2011.

Figure A.13: Deflated Coefficients: Sit-Down Restaurants vs. Market Services

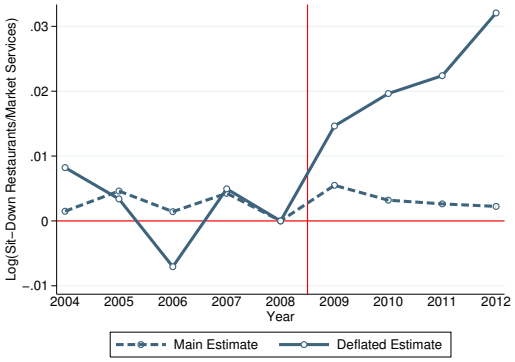
a. Value-Added per Employee



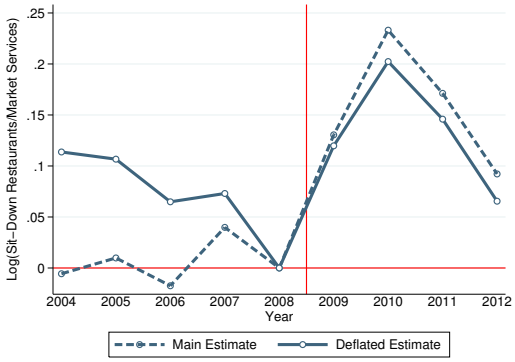
b. Cost per Employee



c. Number of Employees per Firm

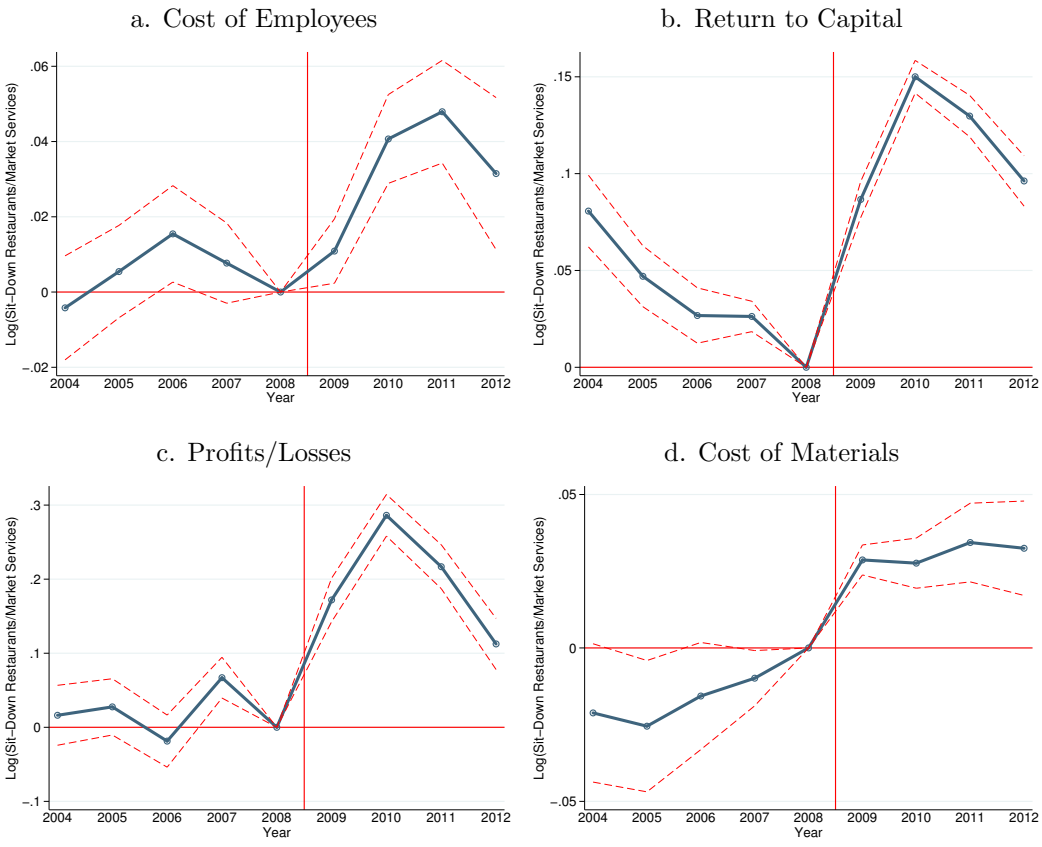


d. Profit per Employee



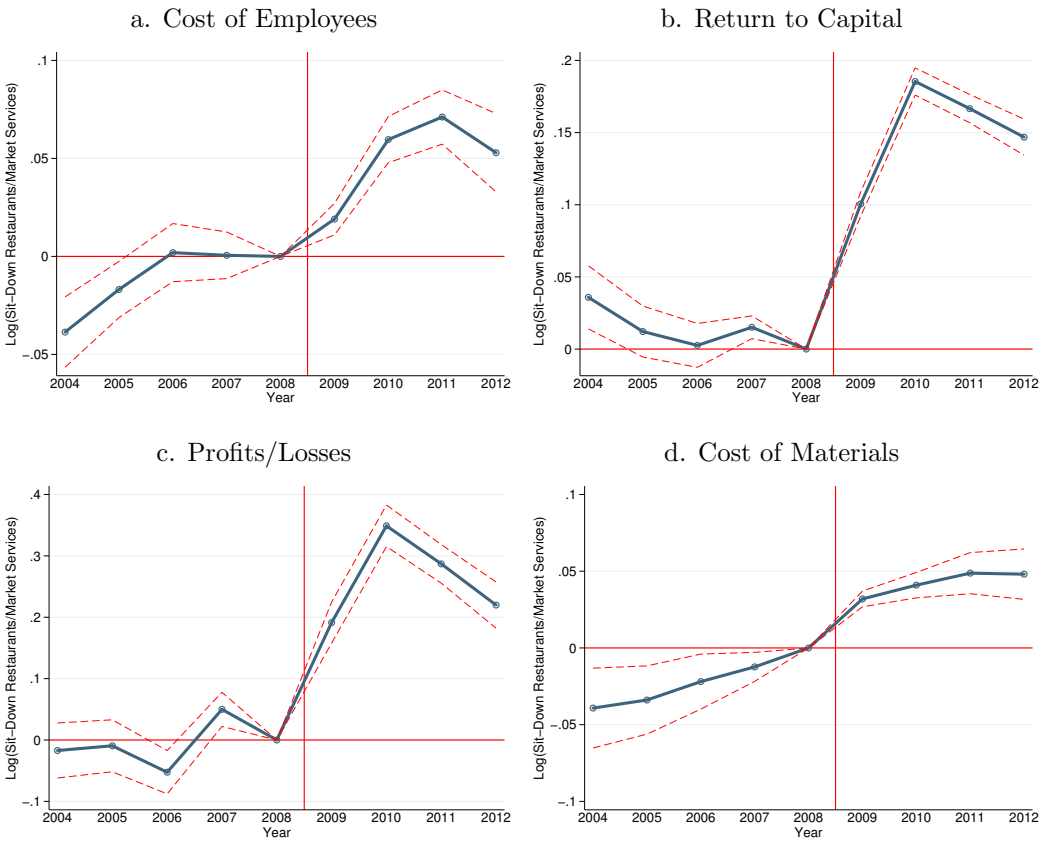
Notes: Deflated coefficients are a lower bound of the effect of the VAT. We find this by subtracting event-type coefficients computed for hotel sector vs. non-restaurant market services from the event-type coefficients found using equation (1.9) on sit-down restaurants vs. non-restaurant market services.

Figure A.14: Event-Time Coefficients on Full-Sample: Sit-Down Restaurants vs. Market Services



Notes: The figures show event-time coefficients estimated using equation (1.10), which includes year and firm fixed effects. The treatment group includes all sit-down restaurants, while the control group includes firms in non-restaurant market services. Value-added is computed as turnover minus material goods purchased. The figures consider the full sample of firms with unconsolidated balance sheets. The dashed lines represent 95 percent confidence intervals.

Figure A.15: Event-Time Coefficients on Full-Sample: Sit-Down Restaurants vs. Small Firms



Notes: The figures show event-time coefficients estimated using equation (1.10), which includes year and firm fixed effects. The treatment group includes all sit-down restaurants, while the control group includes non-restaurant small firms. Value-added is computed as turnover minus material goods purchased. The figures consider the full sample of firms with unconsolidated balance sheets. The dashed lines represent 95 percent confidence intervals.

Figure A.16: Alternative Measures of Return to Capital

Notes: The figure show event-time coefficients estimated using equation (1.10).

Table A.6: Mean Impact Estimates on Alternative Measures of Return to Capital

	Return to Capital	Profit	Net Return to Capital-1	Net Return to Capital-2
	(1)	(2)	(3)	(4)
After × Sit-Down Restaurant	0.10***	0.24***	0.01	0.02***
	(0.0088)	(0.017)	(0.0062)	(0.0061)
R^2	0.94	0.81	0.96	0.96
Observations	994,733	753,806	989,816	962,912
Year FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes

Notes: Coefficients on the interaction variable are estimated from equation (1.9) and indicate average percent changes of the outcome variable in the period 2009-2011 relative to the period 2004-2008. The net return to capital in column (3) is computed as return to capital minus profits, while the net return to capital in column (4) is computed as return to capital minus profits, interest payments and depreciation. The control group is firms operating in non-restaurant market services. See data appendix for definition of market services. Standard errors are clustered by *département* * p<0.10, ** p<0.05, *** p<0.01.

Table A.7: Approximation Error

	Short-Run: 6 Months after Reform	Medium-Run: 18 Months after Reform	Long-Run: 30 Months after Reform
$d\tau(1-\delta)+\tau dlnp_x$	0.1004	0.1006	0.099
Sum of Numerators	0.0580	0.0890	0.0850
Approximation Error	0.0424	0.0116	0.0140

A.8 Tables

Table A.8: Incidence of the VAT Reform using Counterfactual Price Distribution

	Panel A: Short-Run Incidence			
	Estimated Percent Change	Sales Share	Weighted Percent Change	Share of Incidence
Cost per Employee	0.015	39.3	0.006	**12.0**
Cost of Capital per Employee	0.098	29.4	0.029	**58.0**
Prices	-0.015			**30.0**
	Panel B: Medium-Run Incidence			
	Estimated Percent Change	Sales Share	Weighted Percent Change	Share of Incidence
Cost per Employee	0.047	39.3	0.018	**22.2**
Cost of Capital per Employee	0.16	29.4	0.047	**58.0**
Prices	-0.016			**19.8**
	Panel C: Long-Run Incidence			
	Estimated Percent Change	Sales Share	Weighted Percent Change	Share of Incidence
Cost per Employee	0.064	39.3	0.025	**32.5**
Cost of Capital per Employee	0.14	29.4	0.041	**53.2**
Prices	-0.011			**14.3**

Notes: Percent changes for the cost per employee, the return to capital per employee and the cost of material goods per employee are estimated using the even-type coefficients computed from equation (1.10). The change in prices is computed from the log-difference in prices between sit-down restaurants and non-restaurant market services. Sales shares reported in the table are firm averages in the pre-reform year 2008. Incidence estimates are computed using equation (1.8).

Table A.9: Incidence of the VAT Reform using Counterfactual for Return to Capital

	Panel A: Short-Run Incidence			
	Estimated Percent Change	Sales Share	Weighted Percent Change	Share of Incidence
Cost per Employee	0.015	39.3	0.006	**9.4**
Cost of Capital per Employee	0.119	29.4	0.035	**54.7**
Prices	-0.023			**35.9**
	Panel B: Medium-Run Incidence			
	Estimated Percent Change	Sales Share	Weighted Percent Change	Share of Incidence
Cost per Employee	0.047	39.3	0.018	**17.6**
Cost of Capital per Employee	0.204	29.4	0.060	**58.8**
Prices	-0.024			**23.6**
	Panel C: Long-Run Incidence			
	Estimated Percent Change	Sales Share	Weighted Percent Change	Share of Incidence
Cost per Employee	0.064	39.3	0.025	**24.0**
Cost of Capital per Employee	0.205	29.4	0.060	**57.7**
Prices	-0.019			**18.3**

Notes: Percent changes for the cost per employee are estimated using the even-type coefficients computed from equation (1.10). The percent change in the return to capital per employee is based on the estimated event-type coefficients and the counterfactual log-difference shown in figure A.11. The change in prices is computed from the log-difference in prices between sit-down restaurants and non-restaurant market services. Sales shares reported in the table are firm averages in the pre-reform year 2008. Incidence estimates are computed using equation (1.8).

Table A.10: Incidence of the VAT Reform including Effect on Cost of Materials

	Panel A: Short-Run Incidence			
	Estimated Percent Change	Sales Share	Weighted Percent Change	Share of Incidence
Cost per Employee	0.015	39.3	0.006	**8.6**
Cost of Capital per Employee	0.098	29.4	0.029	**41.4**
Cost of Materials per Employee	0.038	31.3	0.012	**17.1**
Prices	-0.023			**32.9**
	Panel B: Medium-Run Incidence			
	Estimated Percent Change	Sales Share	Weighted Percent Change	Share of Incidence
Cost per Employee	0.047	39.3	0.018	**17.7**
Cost of Capital per Employee	0.162	29.4	0.048	**47.0**
Cost of Materials per Employee	0.039	31.3	0.012	**11.8**
Prices	-0.024			**23.5**
	Panel C: Long-Run Incidence			
	Estimated Percent Change	Sales Share	Weighted Percent Change	Share of Incidence
Cost per Employee	0.064	39.3	0.025	**24.5**
Cost of Capital per Employee	0.142	29.4	0.042	**41.2**
Cost of Materials per Employee	0.052	31.3	0.016	**15.7**
Prices	-0.019			**18.6**

Notes: Percent changes for the cost per employee, the return to capital per employee and the cost of material goods per employee are estimated using the even-type coefficients computed from equation (1.10). The change in prices is computed from the log-difference in prices between sit-down restaurants and non-restaurant market services. Sales shares reported in the table are firm averages in the pre-reform year 2008. Incidence estimates are computed using equation (1.8).

Appendix B

Do Prices Respond Differently to Increases and Decreases In Consumption Taxes?

B.1 Data

Table B.1: COICOP Codes

COICOP Codes	Description
01	**Food and Non-Alcoholic Beverages**
01.1	Food
01.2	Non-Alcoholic Beverages
02	**Alcoholic Beverages, Tobacco and Narcotics**
02.1	Alcoholic Beverages
02.2	Tobacco
02.3	Narcotics
03	**Clothing and Footwear**
03.1	Clothing
03.2	Footwear
04	**Housing, Water, Electricity, Gas and Other Fuels**
04.1	Actual Rentals For Housing
04.2	Imputed Rentals For Housing
04.3	Maintenance and Repair of the Dwelling
04.4	Water Supply and Misc Services Relating to the Dwelling
04.5	Electricity, Gas and Other Fuels
05	**Furnishings, Household Equipment and Routine Household Maintenance**
05.1	Furniture and Furnishings, Carpets and Other Floor Coverings
05.2	Household Textiles
05.3	Household Appliances
05.4	Glassware, Tableware and Household Utensils
05.5	Tools and Equipment for House and Garden
05.6	Goods and Services for Routine Household Maintenance
06	**Health**
06.1	Medical Products, Appliances and Equipment
06.2	Outpatient Services
06.3	Hospital Services
07	**Transport**
07.1	Purchase of Vehicles
07.2	Operation of Personal Transport Equipment
07.3	Transport Services

Notes: This table reports the COICOP codes used to aggregate prices as well as their description.

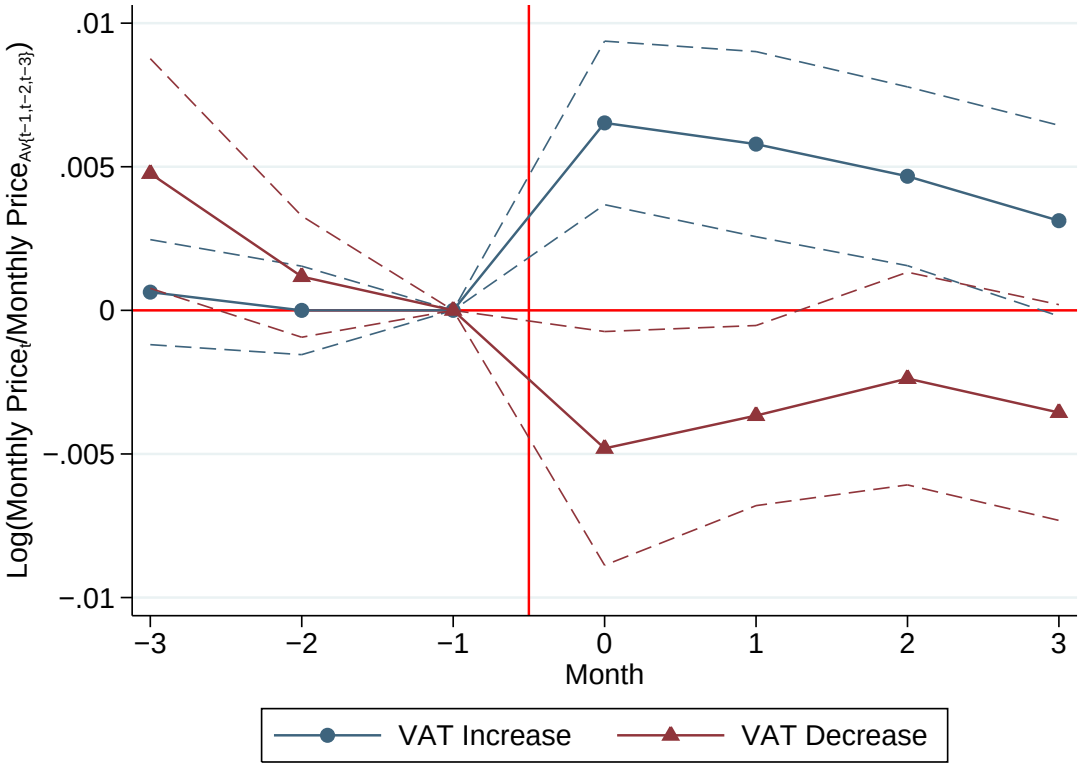
Table B.2: COICOP Codes (continued)

COICOP Code	Description
08	**Communication**
08.1	Postal Services
08.2	Telephone and Telefax Equipment
08.3	Telephone and Telefax Services
09	**Recreation and Culture**
09.1	Audio-Visual, Photographic and Information Processing Equipment
09.2	Other Major Durables For Recreation and Culture
09.3	Other Recreational Items and Equipment, Gardens and Pets
09.4	Recreational and Cultural Services
09.5	Newspapers, Books and Stationery
09.6	Package Holidays
10	**Education**
10.1	Pre-Primary and Primary Education
10.2	Secondary Education
10.3	Post-Secondary Non-Tertiary Education
10.4	Tertiary Education
10.5	Education Not Definable By Level
11	**Restaurants and Hotels**
11.1	Catering Services
11.2	Accommodation Services
12	**Misc. Goods and Services**
12.1	Personal Care
12.2	Prostitution
12.3	Personal Effects
12.4	Social Protection
12.5	Insurance
12.6	Financial Services
12.7	Other Services

Notes: This table reports the COICOP codes used by Eurostat to describe price categories.

B.2 Figures

Figure A.1: *Full Sample*: Event-Study Estimates of Price Changes around VAT Reforms using Alternative Control



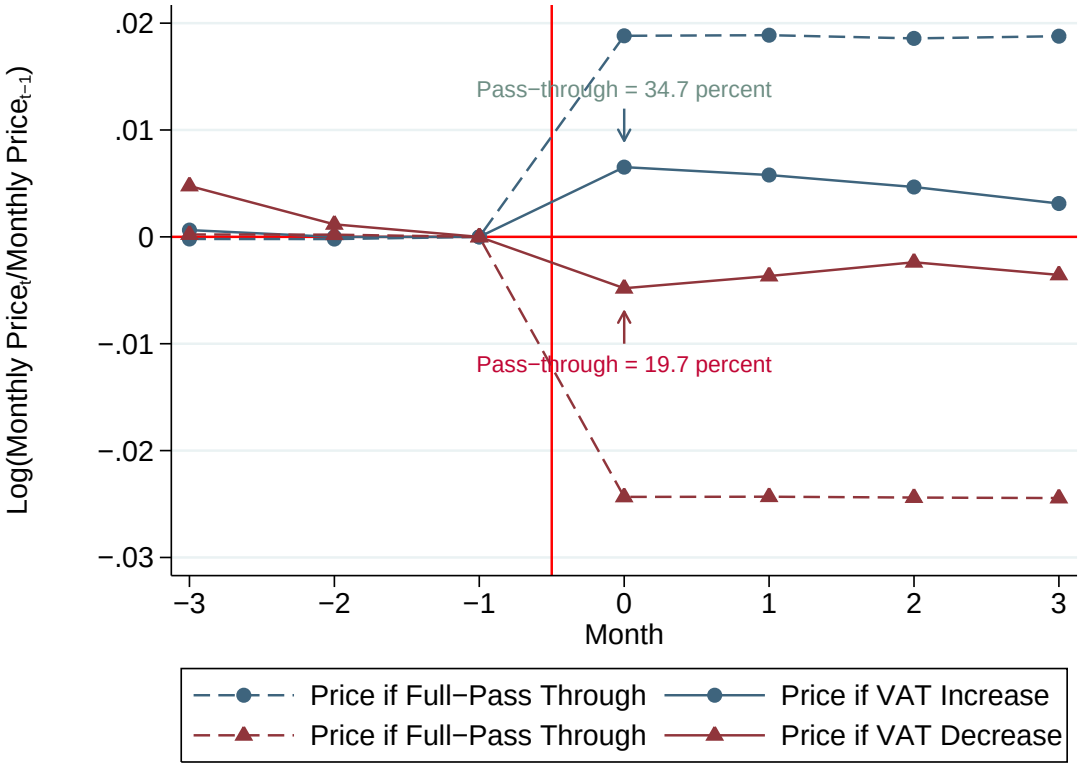
Notes: The figure is constructed using all VAT changes between 1996 and 2015. It shows event-type coefficients estimated using specification (2.2) on price changes around VAT increases and VAT decreases. Control group is the average commodity price in same month in the previous 3 years. Dotted lines are 95 percent confidence intervals.

Table B.3: Examples of 4 digit COICOP Codes

COICOP Code	Description
01.1.1	Bread and Cereals
01.1.2	Meat
01.1.3	Fish and Seafood
01.1.4	Milk, Cheese and Eggs
01.1.5	Oils and Fats
01.1.6	Fruit
01.1.7	Vegetables
01.1.8	Sugar, Jam, Honey, Chocolate and Confectionary
01.1.9	Food Products
01.1.10	Bread and Cereals

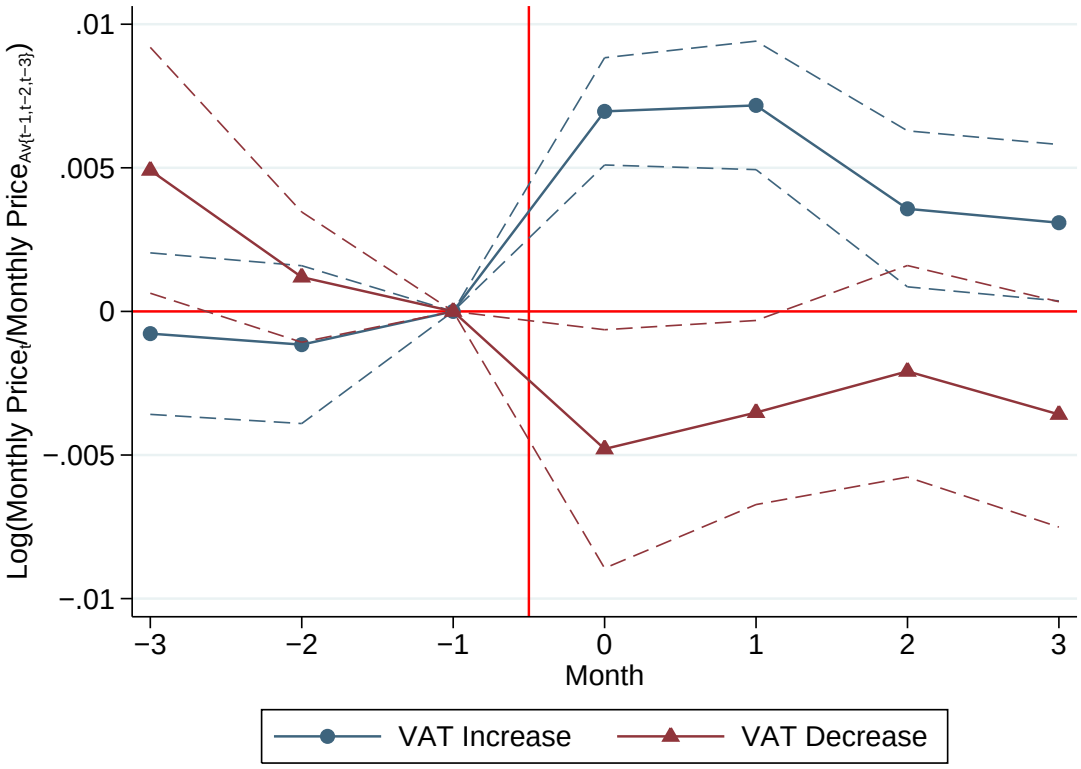
Notes: This table reports the detailed Food category for each 4 digit COICOP code.

Figure A.2: *Full Sample*: Event-Study Estimates of Price Changes and VAT Changes using Alternative Control



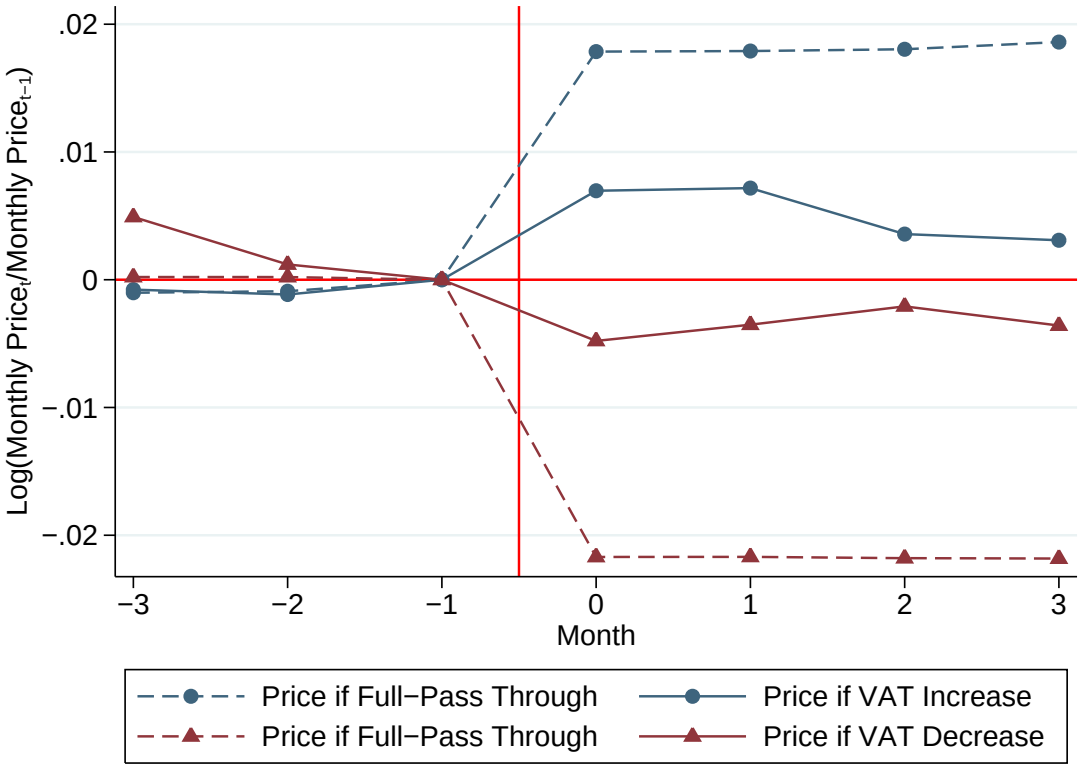
Notes: The figure is constructed using all VAT changes between 1996 and 2015. It shows event-type coefficients estimated using specification (2.2) on price changes around VAT increases and VAT decreases. Control group is the average commodity price in same month in the previous 3 years.

Figure A.3: *Restricted Sample*: Event-Study Estimates of Price Changes around VAT Reforms using Alternative Control



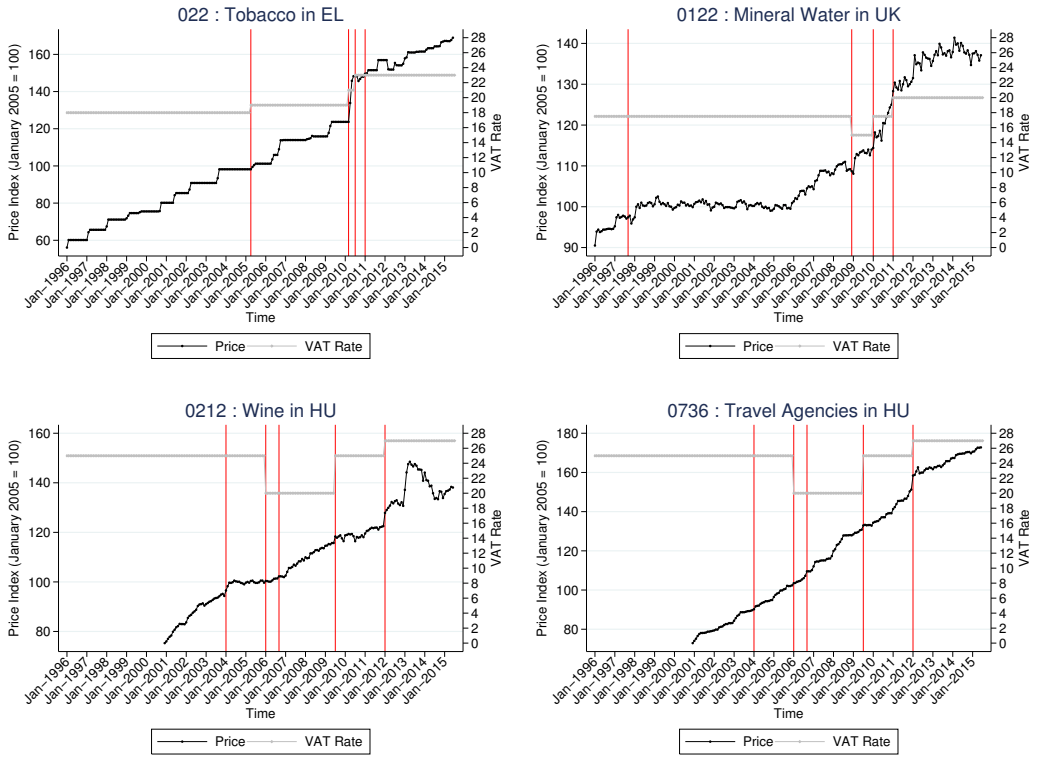
Notes: The figure is constructed using commodities that experience both a VAT increase and a VAT decrease in the period 1996-2015. It shows event-type coefficients estimated using specification (2.2) on price changes around VAT increases and VAT decreases. Control group is the average commodity price in same month in the previous 3 years. Dotted lines are 95 percent confidence intervals.

Figure A.4: *Restricted Sample*: Event-Study Estimates of Price Changes and VAT Changes using Alternative Control



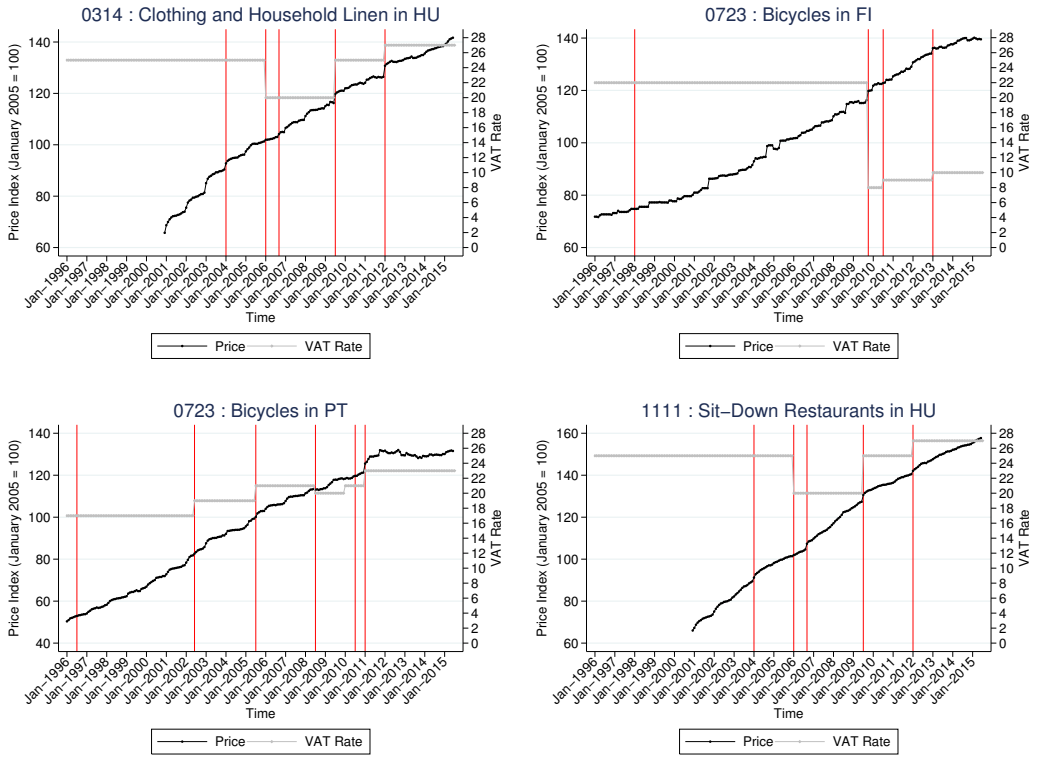
Notes: The figure is constructed using commodities that experience both a VAT increase and a VAT decrease in the period 1996-2015. It shows event-type coefficients estimated using specification (2.2) on price changes around VAT increases and VAT decreases. Control group is the average commodity price in same month in the previous 3 years.

Figure A.5: Individual Reforms



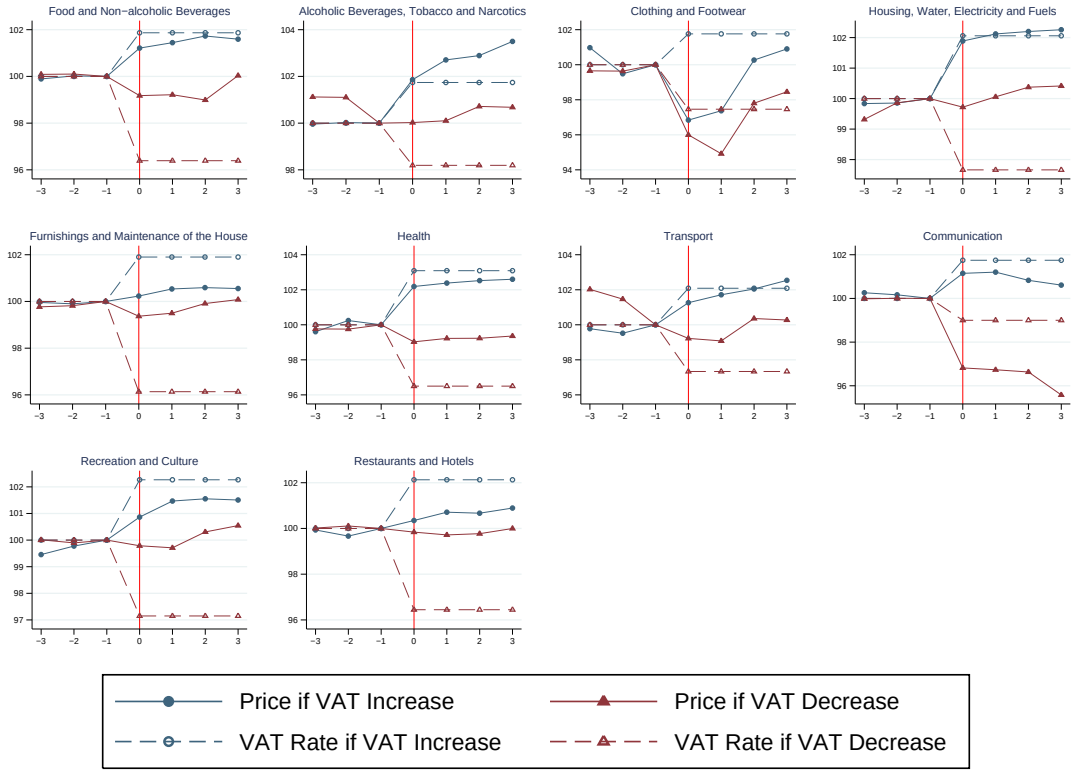
Notes: Computed using monthly price data by COICOP code from Eurostat, and VAT reforms for European Commission (EC) Reports. Data are not seasonally adjusted. Red vertical lines indicate time of reform of national standard, reduced or super-reduced VAT rate. Numbers in the title of each sub-figure are the corresponding four-digit COICOP code.

Figure A.6: Individual Reforms



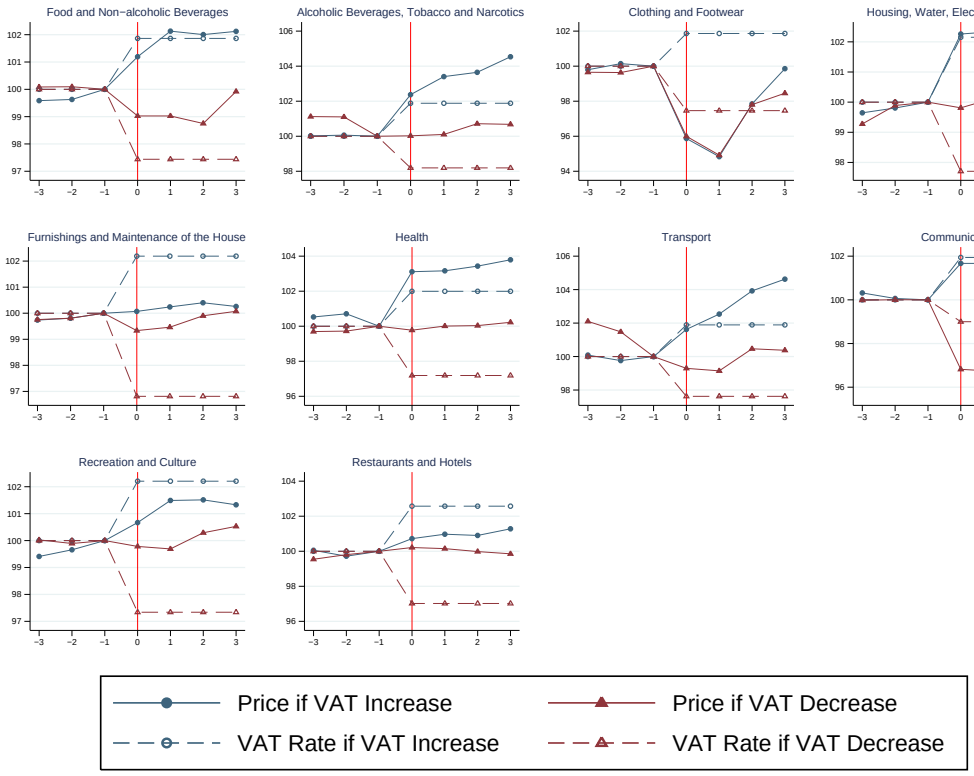
Notes: Computed using monthly price data by COICOP code from Eurostat, and VAT reforms for European Commission (EC) Reports. Data are not seasonally adjusted. Red vertical lines indicate time of reform of national standard, reduced or super-reduced VAT rate. Numbers in the title of each sub-figure are the corresponding four-digit COICOP code.

Figure A.7: Asymmetric Response of Prices to VAT Changes by 2-Digit COICOP Code in the Full Sample



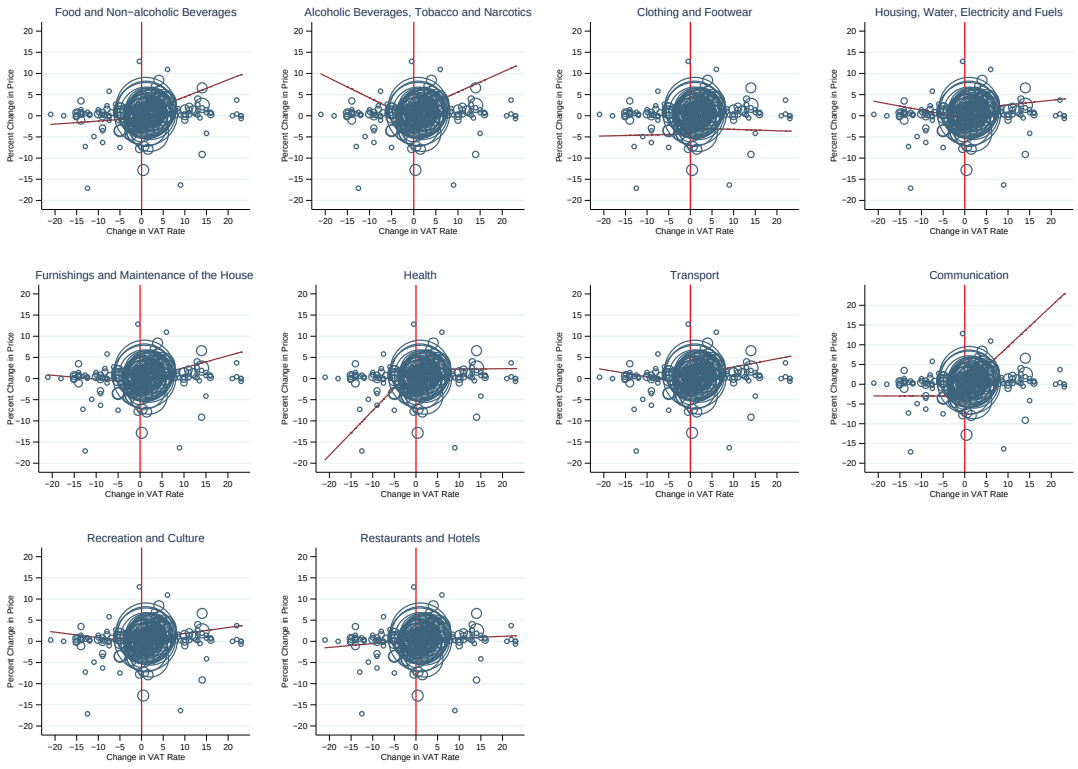
Notes: The figure shows unconditional means and is constructed using all VAT changes between 1996 and 2015. For each commodity the price index is normalized to 100 in 2005.

Figure A.8: Asymmetric Response of Prices to VAT Changes by 2-Digit COICOP Code in the Restricted Sample



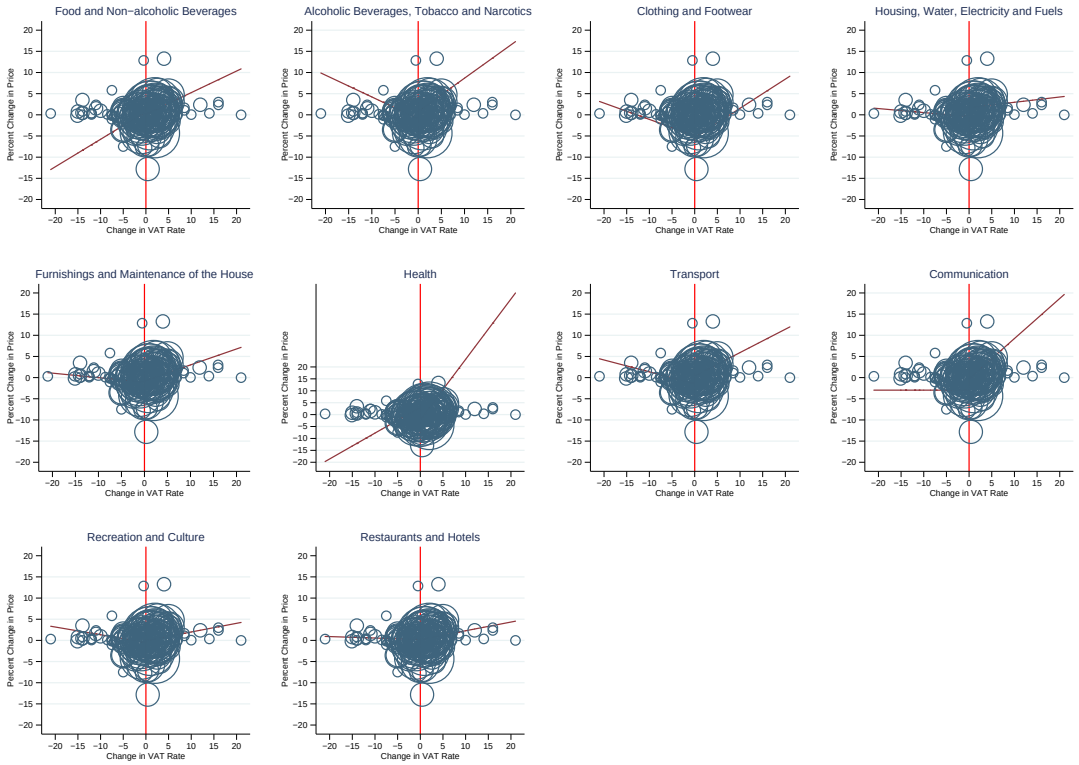
Notes: The figure shows unconditional means and is constructed using commodities which are subject to both a VAT increase and a VAT decrease between 1996 and 2015. For each commodity the price index is normalized to 100 in 2005.

Figure A.9: Percent Change in Prices by Change in VAT rate in Full Sample, by 2-Digit COICOP Code



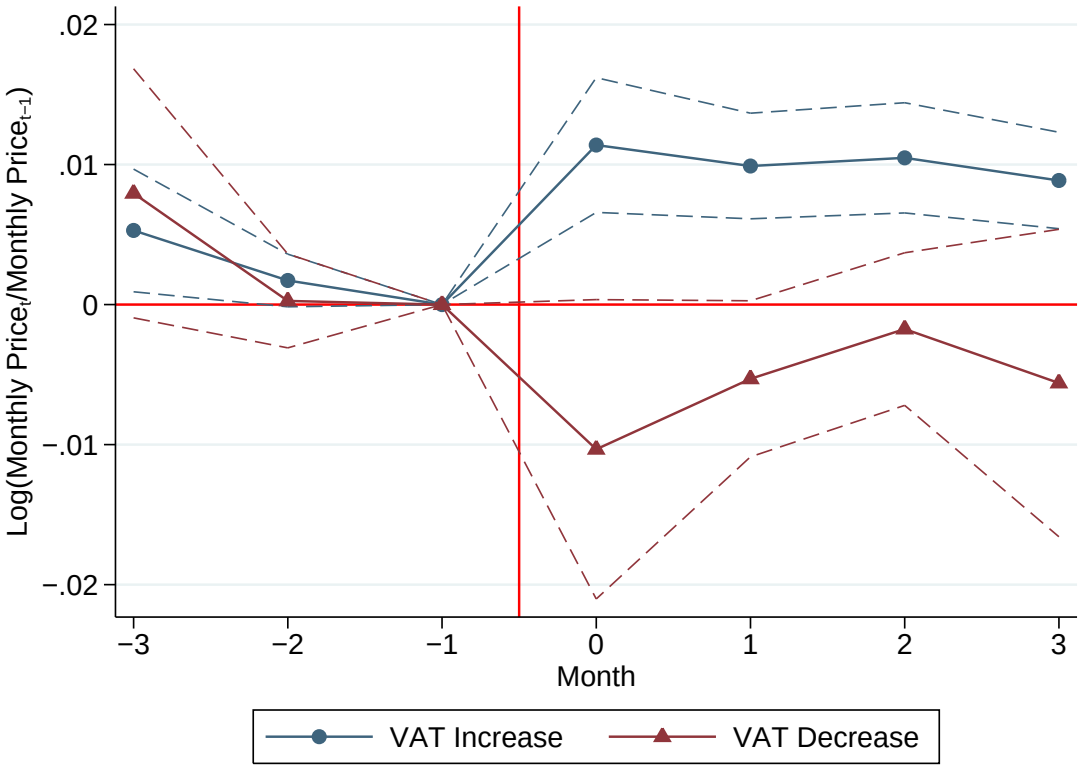
Notes: The figure is constructed using all VAT changes between 1996 and 2015. A linear fit of prices on negative and positive changes in VAT is estimated separately. Larger circles indicate a higher number of corresponding reforms for a given VAT change.

Figure A.10: Percent Change in Prices by Change in VAT rate in Restricted Sample, by 2-Digit COICOP Code



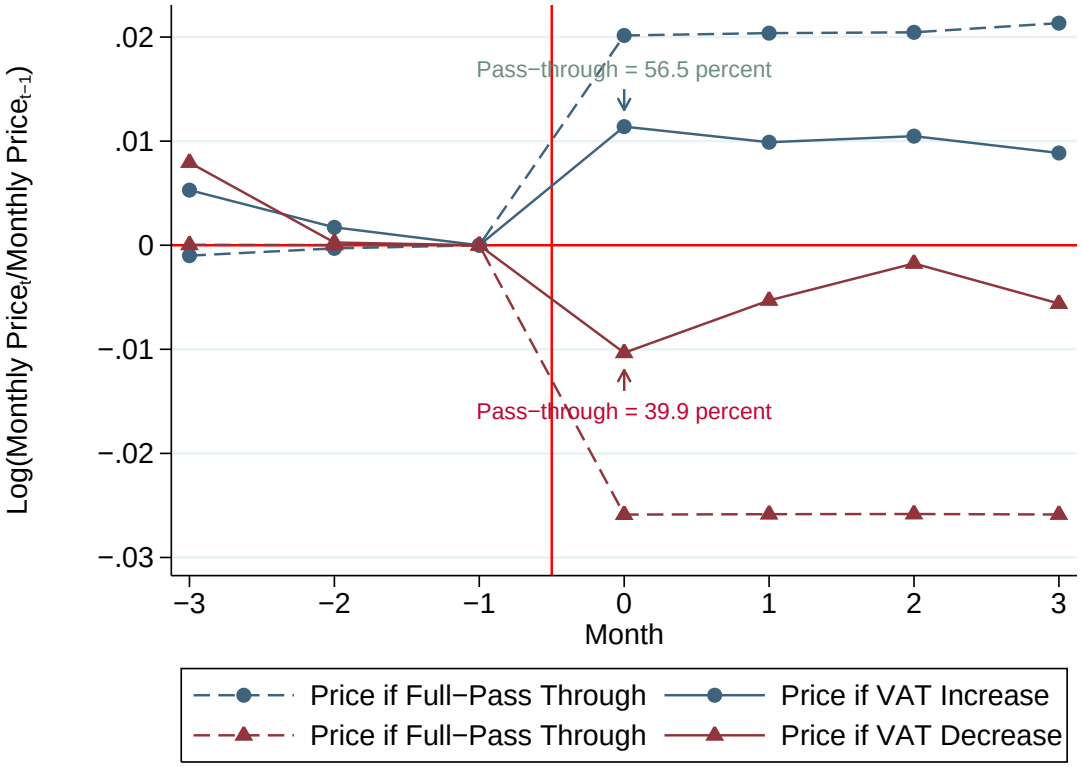
Notes: The figure is constructed using commodities which are subject to both a VAT increase and a VAT decrease between 1996 and 2015. A linear fit of prices on negative and positive changes in VAT is constructed separately. Larger circles indicate a higher number of corresponding reforms for a given VAT change.

Figure A.11: Event-Study Estimates of Price Changes: Sample excluding January Reforms



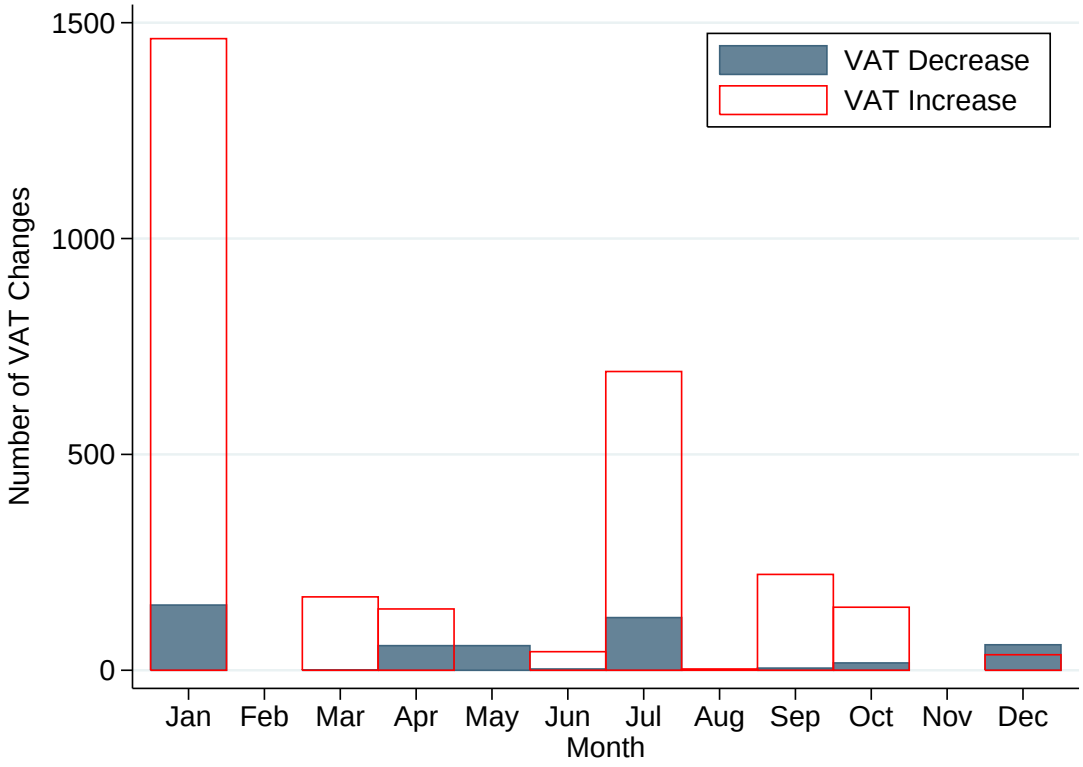
Notes: The figure is constructed using all VAT changes in the period 1996-2015 other than the January ones. It shows event-type coefficients estimated using specification (2.2) on price changes around VAT increases and VAT decreases. Control group is the commodity price in the same month one year before. Dashed lines are 95 percent confidence intervals.

Figure A.12: Event-Study Estimates of Price Changes and VAT Changes: Sample excluding January Reforms



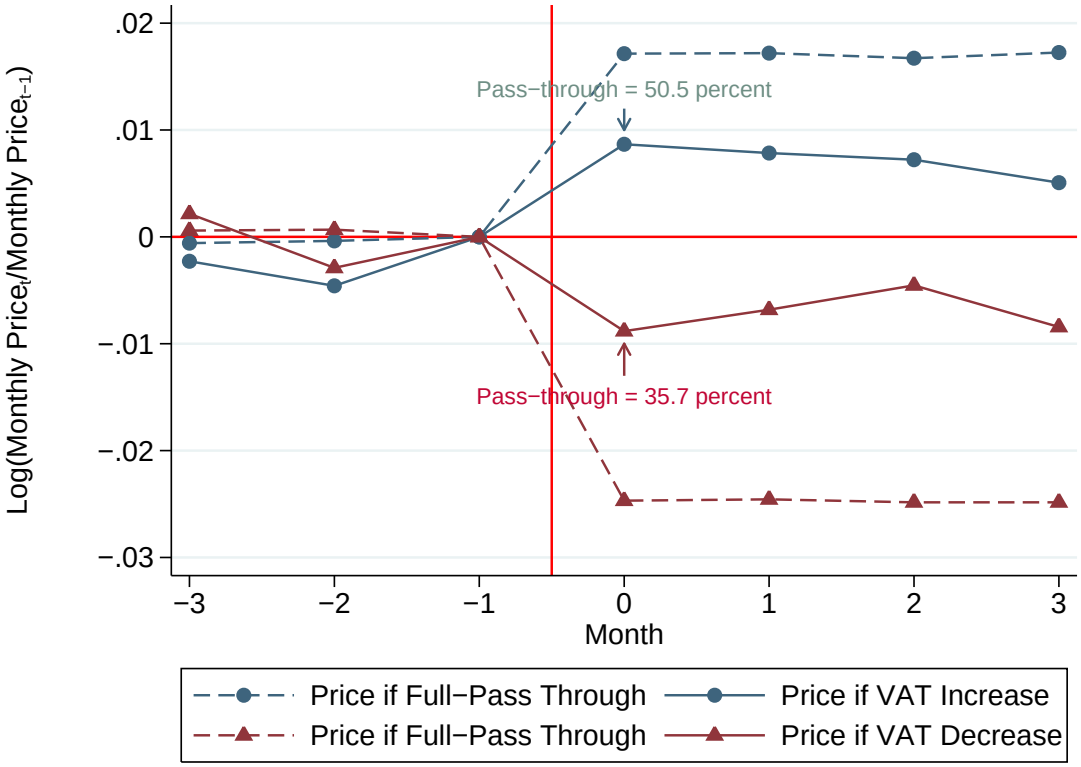
Notes: The figure is constructed using all VAT changes in the period 1996-2015 other than the January ones. It shows event-type coefficients estimated using specification (2.2) on price changes around VAT increases and VAT decreases. Control group is the commodity price in the same month one year earlier.

Figure A.13: VAT Reforms and Price Changes in Months with no VAT Reform



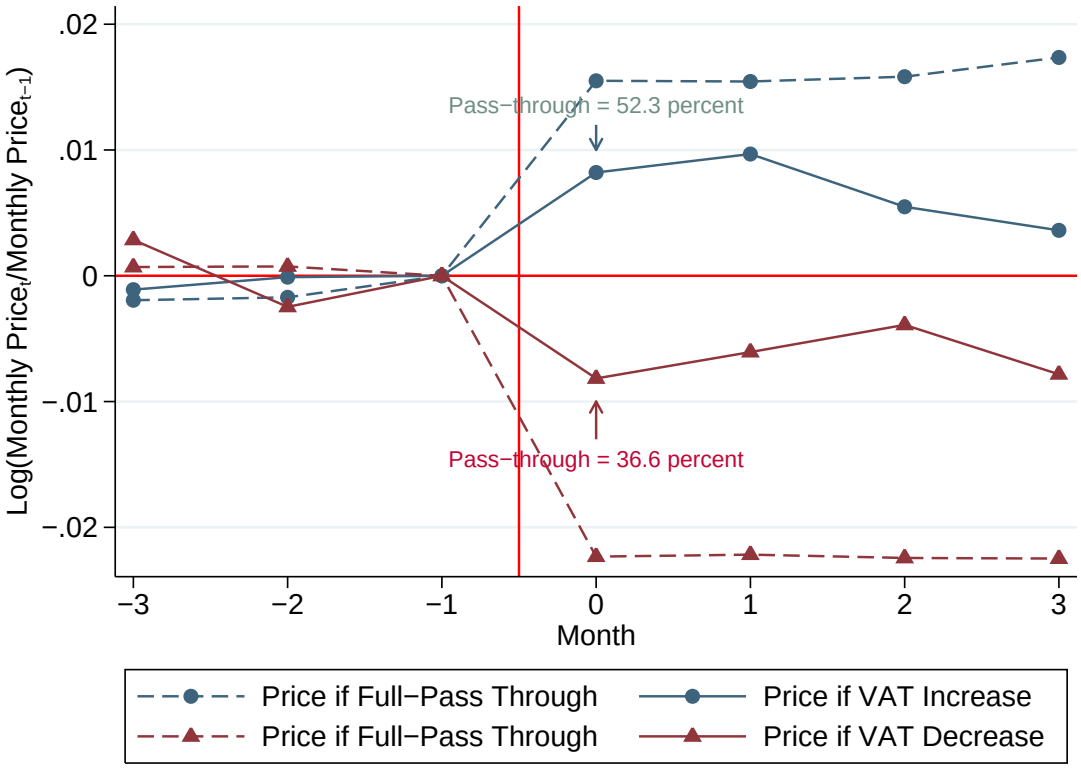
Notes: The figure shows the total number of VAT increases and decreases considered in our data by month of the year.

Figure A.14: *Full Sample*: Event-Study Estimates of Changes in Prices and Average VAT Rates



Notes: The figure is constructed using all VAT changes in the period 1996-2015. It shows event-type coefficients estimated using specification (2.2) on price changes around VAT increases and VAT decreases. Control group is the commodity price in the same month one year earlier. Average VAT Rate is mean of VAT rates applied.

Figure A.15: *Restricted Sample*: Event-Study Estimates of Changes in Prices and Average VAT Rates



Notes: The figure is constructed using commodities that experience both a VAT increase and a VAT decrease in the period 1996-2015. It shows event-type coefficients estimated using specification (2.2) on price changes around VAT increases and VAT decreases. Control group is the commodity price in the same month one year earlier. Average VAT Rate is mean of VAT rates applied.

Table A.1: Summary Statistics on Alternative Measures of VAT Rate

Panel A: Maximum VAT Rate

	Number of VAT Reforms	Change in VAT Rate	Mean VAT After Reform	Standard Deviation	Minimum	Maximum
	(1)	(2)	(3)	(4)	(5)	(6)
VAT Changes	3,389	1.31	18.04	5.15	0	27
VAT Increases	2,917	2.01	18.14	5.20	4.5	27
VAT Decreases	472	-2.99	17.41	4.83	0	21

Panel B: Average VAT Rate

	Number of VAT Reforms	Change in VAT Rate	Mean VAT After Reform	Standard Deviation	Minimum	Maximum
	(1)	(2)	(3)	(4)	(5)	(6)
VAT Changes	3,389	1.25	16.91	5.59	0	27
VAT Increases	2,917	1.88	17.02	5.62	3	27
VAT Decreases	472	-2.61	16.23	5.39	0	21

Panel C: Minimum VAT Rate

	Number of VAT Reforms	Change in VAT Rate	Mean VAT After Reform	Standard Deviation	Minimum	Maximum
	(1)	(2)	(3)	(4)	(5)	(6)
VAT Changes	3,389	1.20	15.80	7.23	0	27
VAT Increases	2,917	1.75	15.92	7.23	0	27
VAT Decreases	472	-2.23	15.05	7.18	0	21

Notes: Column (1) shows the number of VAT reforms considered; Column (2) shows the average change in the VAT rate in the month of the reform; Columns (3)-(6) display summary statistics for the VAT rate in the month of the reform. Panel A. considers the maximum VAT rate applied to each commodity, while panel B. considers the average VAT rate and panel C. considers the minimum VAT rate.

B.3 Tables

Table A.2: 2SLS Estimates of Pass-Through to Prices using Full Sample when Control Group = Average of 3 Year Lags

	VAT Increase				VAT Decrease			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
VAT Rate	0.15**	0.21**	0.25***	0.25***	0.30**	0.11	0.12	0.23***
	(0.074)	(0.082)	(0.080)	(0.081)	(0.13)	(0.082)	(0.078)	(0.084)
Treat	0.063***	-0.010***	-0.070***	0.0079	0.043***	-0.025***	-0.0092	0.050
	(0.0077)	(0.0027)	(0.0077)	(0.016)	(0.012)	(0.0080)	(0.028)	(0.037)
After	0.010***	-0.0050***	0.016***	0.0012	0.0011	-0.0086***	0.032**	-0.031**
	(0.0022)	(0.0011)	(0.0049)	(0.0019)	(0.0024)	(0.0019)	(0.016)	(0.015)
Log(Country Price)		0.44***	0.62***	0.061		0.37***	-0.14	-1.31*
		(0.099)	(0.11)	(0.22)		(0.13)	(0.48)	(0.71)
Log(EA19 Price)		0.89***	1.13***	1.04***		0.90***	1.07***	1.04***
		(0.099)	(0.14)	(0.12)		(0.15)	(0.20)	(0.18)
R^2	0.032	0.52	0.53	0.59	0.010	0.58	0.63	0.65
Observations	35588	35588	35588	35588	4466	4466	4466	4466
First Stage F-Stat	2091.3	2039.9	2092.9	1997.9	92.4	93.7	90.2	89.7
Time FE	No	No	Yes	Yes	No	No	Yes	Yes
Country FE	No	No	No	Yes	No	No	No	Yes

Notes: The coefficient reported in the table indicate the pass-through of the VAT tax to prices estimated using specification (2.1). The control group is the average commodity price in the same month in the 3 previous years. Country price is computed as a weighted average of the prices in the 2-digit COICOP groups excluding the group to which the commodity belongs. EA19 price is computed by a weighted average of prices for the same commodity in other EA19 country. Standard errors are clustered by COICOP. * p<0.10, ** p<0.05, *** p<0.01

Table A.3: 2SLS Estimates of Pass-Through to Prices using Restricted Sample when Control Group = Average of 3 Year Lags

	VAT Increase				VAT Decrease			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
VAT Rate	0.16*	0.28***	0.46***	0.31***	0.32**	0.10	0.18**	0.25**
	(0.091)	(0.072)	(0.083)	(0.068)	(0.15)	(0.094)	(0.090)	(0.098)
Treat	0.061***	-0.016***	-0.071***	0.018	0.043***	-0.026***	0.021	0.057
	(0.010)	(0.0054)	(0.013)	(0.024)	(0.012)	(0.0082)	(0.029)	(0.038)
After	0.0093**	-0.0089***	0.0047	0.015**	0.00096	-0.0087***	0.042**	-0.053**
	(0.0036)	(0.0022)	(0.0077)	(0.0075)	(0.0024)	(0.0018)	(0.017)	(0.023)
Log(Country Price)		0.47***	0.62***	-0.074		0.37***	-0.76	-1.49**
		(0.12)	(0.15)	(0.29)		(0.13)	(0.53)	(0.74)
Log(EA19 Price)		0.86***	1.10***	1.01***		0.90***	1.05***	1.03***
		(0.16)	(0.23)	(0.20)		(0.15)	(0.19)	(0.17)
R^2	0.022	0.51	0.51	0.59	0.011	0.58	0.64	0.65
Observations	10976	10976	10976	10976	4368	4368	4368	4368
First Stage F-Stat	770.4	814.5	774.3	780	145.2	148.9	135.5	140.2
Time FE	No	No	Yes	Yes	No	No	Yes	Yes
Country FE	No	No	No	Yes	No	No	No	Yes

Notes: The coefficient reported in the table indicate the pass-through of the VAT tax to prices estimated using specification (2.1). The control group is the average commodity price in the same month in the 3 previous years. Country price is computed as a weighted average of the prices in the 2-digit COICOP groups excluding the group to which the commodity belongs. EA19 price is computed by a weighted average of prices for the same commodity in other EA19 country. Standard errors are clustered by COICOP. * p<0.10, ** p<0.05, *** p<0.01

Table A.4: Robustness: 2SLS Estimates of Pass-Through to Prices using Restricted Sample when Control Group = One Year Lag

	VAT Increase			VAT Decrease		
	(1)	(2)	(3)	(4)	(5)	(6)
VAT Rate	0.48***	0.54***	0.83***	0.19*	0.24**	0.19
	(0.11)	(0.12)	(0.17)	(0.10)	(0.11)	(0.13)
Treat	0.025**	0.047***	0.041***	0.036*	0.050**	0.041*
	(0.0100)	(0.015)	(0.014)	(0.021)	(0.024)	(0.024)
After	0.0012	0.013***	0.0090***	-0.00067	0.0066	-0.00091
	(0.0018)	(0.0034)	(0.0026)	(0.0040)	(0.0052)	(0.0035)
Log(Country Price)	-0.56*	-1.28***	-1.65***	-1.50*	-2.05**	-2.19**
	(0.29)	(0.46)	(0.57)	(0.77)	(0.95)	(1.01)
Log(EA19 Price)	0.66***	0.64***	0.62***	0.75***	0.76***	0.75***
	(0.17)	(0.16)	(0.16)	(0.21)	(0.21)	(0.21)
Log(Monthly URate)			0.079***			0.035
			(0.022)			(0.036)
Log(Quarterly GDP Per Capita)			0.20***			0.24***
			(0.052)			(0.078)
Log(Monthly Interest Rate)			0.031***			-0.013
			(0.0076)			(0.016)
R^2	0.52	0.53	0.52	0.63	0.64	0.64
Observations	38,150	38,150	35,766	5,460	5,460	5,414
First Stage F-Stat	1,714.1	1,609.4	990.6	152.5	147.8	146.1
Time × Country FE	No	Yes	Yes	No	Yes	Yes

Notes: The coefficient reported in the first line of the table indicates the pass-through of the VAT tax to prices estimated using specification (2.1). The control group is the average commodity price in the same month one year before. Country price is computed as a weighted average of the prices in the 2-digit COICOP groups excluding the group to which the commodity belongs. EA19 price is computed by a weighted average of prices for the same commodity in other EA19 country. Data on monthly unemployment rates, quarterly GDP per capita and monthly interest rates are from Eurostat. Standard errors are clustered by COICOP. * p<0.10, ** p<0.05, *** p<0.01

Table A.5: Robustness: 2SLS Estimates of Pass-Through to Prices using Restricted Sample when Control Group = Average of 3 Year Lags

	VAT Increase			VAT Decrease		
	(1)	(2)	(3)	(4)	(5)	(6)
VAT Rate	0.25***	0.26***	0.57***	0.20**	0.23***	-0.036
	(0.081)	(0.081)	(0.13)	(0.082)	(0.084)	(0.20)
Treat	0.0052	0.013	0.010	0.036	0.053	0.030
	(0.015)	(0.017)	(0.018)	(0.033)	(0.037)	(0.036)
After	0.0031**	0.0052**	0.0052**	-0.0036	0.00092	-0.0098**
	(0.0014)	(0.0025)	(0.0022)	(0.0034)	(0.0037)	(0.0042)
Log(Country Price)	0.10	-0.016	-0.38	-1.01	-1.36*	-1.66*
	(0.20)	(0.24)	(0.34)	(0.62)	(0.71)	(0.89)
Log(EA19 Price)	1.04***	1.04***	1.02***	1.03***	1.04***	1.04***
	(0.12)	(0.12)	(0.12)	(0.18)	(0.18)	(0.18)
Log(Monthly URate)			0.051***			0.068
			(0.016)			(0.056)
Log(Quarterly GDP Per Capita)			0.28***			0.47**
			(0.066)			(0.23)
Log(Monthly Interest Rate)			0.032***			0.018
			(0.010)			(0.017)
R^2	0.59	0.59	0.59	0.64	0.65	0.66
Observations	35588	35588	33078	4466	4466	4254
First Stage F-Stat	2020.6	1961.6	1044.4	92.3	91.3	115.9
Time × Country FE	No	Yes	Yes	No	Yes	Yes

Notes: The coefficient reported in the first line of the table indicates the pass-through of the VAT tax to prices estimated using specification (2.1). The control group is the average commodity price in the same month in the three previous years. Country price is computed as a weighted average of the prices in the 2-digit COICOP groups excluding the group to which the commodity belongs. EA19 price is computed by a weighted average of prices for the same commodity in other EA19 country. Data on monthly unemployment rates, quarterly GDP per capita and monthly interest rates are from Eurostat. Standard errors are clustered by COICOP. * p<0.10, ** p<0.05, *** p<0.01

Appendix C

Geographic Mobility of Liquidity Constrained Unemployed Workers

C.1 SIPP Sample Selection

The sample used in this paper is from the 2001,2004 and 2008 panels of the Survey of Income and Program Participation. My sample includes unemployed workers between 25 and 65, with at least one job separation, with at least three months of work history, who are not on temporary layoffs, are actively searching for a job, and report receiving UI benefit during the unemployment spell. In my analysis, I also drop observations from Alaska, Idaho, Montana Maine, Vermont, North Dakota, South Dakota,Iowa, and Wyoming given that the SIPP does not provide a unique geographic identifier for these states. Pooling all job separations for individuals between 25 and 65 yields 51,859 unemployment spells. Dropping unemployed workers who are on temporary layoff leaves 39,257 unemployment spells. Keep only individuals who are actively searching for a job leaves 20,530 unemployment spells. Finally, keep only individuals who report receiving Ui benefits during their unemployment spell leaves 7,280 unemployment spells.

C.2 Local Cost of Living

The construction of "Local CPI1" and "Local CPI2" follows closely Moretti 2008, with the main difference that price indexes are constructed at the state level rather than the metropolitan area (MSA) level. Cost of housing is measured from the "gross monthly rental cost" on 2 or 3 bedroom apartments reported in the 2000 Census and the 2000-2013 American Community Survey (ACS). The gross monthly rent includes contract rents and the cost of utilities and fuels. The housing cost in state s is the average of the gross rent paid by all the individuals renting a 2 or 3 bedroom apartments in that state. Moreover, rents are imputed for top-coded observations by multiplying the value of the top code by 1.3.

Local CPI 1 This first measure allows for housing costs to vary across states, while keeping non-housing costs constant. The average housing and non-housing costs across cities are normalized to 1 in 2000, and non-housing costs vary over time only with changes in the national CPI-U index. I measure the cost of housing by focusing on rental costs, though my results are robust to using the price of owner occupied houses. Non housing costs are computed combining the information on the national CPI and national housing costs:

$$NHP_t = \frac{CPI_t}{1-w} - \frac{wHP_t}{1-w}$$

where $CPI - U$ is the change in the national CPI, HP_t is the average nationwide change in housing costs and w is the weight assigned to housing by the BLS.

Local CPI 2 This second measure allows for both housing and non-housing costs to vary across time, as well as across states. To construct this measure, I use the fact that the BLS releases a local CPI measure for 23 metropolitan areas (MSAs) every year. Following the methodology used in Moretti 2008, I first compute the correlation π between non-housing and housing costs using variation across MSAs and time for the period 2000-2012:[1]

$$NHP_{ct} = \pi HP_{ct} + u_{ct}$$

by estimating $\Delta CPI_{ct} = \alpha \Delta HP_{ct} + v_{ct}$ and computing $\widehat{\pi} = \frac{\widehat{\beta}-w}{1-w}$, where w is the weight that the BLS assigns to housing costs at the national level every year. Empirically, $\beta = 0.516$, and $\pi = .199$ in 2013, while w vary between 0.37 in 2000 and 0.40 in 2013. Using $\widehat{\pi}$ I can then predict the systematic component of non-housing costs in each state $E(NHP_{st}|HP_{st}) = \widehat{\pi} HP_{st}$. Finally, I compute "Local CPI 2" as a weighted sum of the cost of housing and both the systematic and non-systematic component of non-housing costs. Specifically, "local CPI 2" for state s is computed as:

$$\text{Local CPI 2} = [w + (1-w)\widehat{\pi}]HP_{st} + [1 - (w + (1-w)\widehat{\pi})]NHP_t$$

where HP_{st} is the change in housing prices in state s, NHP_t is the national non-housing cost index computed above, and w is the weight that the BLS assigns to housing costs.

C.3 Model

Unconstrained Case

Given unemployed worker's quadratic instantaneous utility function [2]:

$$u(x) = x - \frac{1}{2}x^2$$

[1] Notice that housing costs by MSA can only be computed from the Census 2000 and the ACS 2005-2013, given that the ACS 2001-2004 has only information on state of residence.

[2] This function has the property that $u'(x) > 0$ and $u''(x) < 0$ for each $x \in [0, 1]$. Implicitly we are also imposing the restriction that $b < w < 1$.

saving s, unemployment benefits b, job finding rates ϕ_c, wages w, rents r_c, monetary moving costs k_c (with $k_a = 0$ and $k_b = k$) and idiosyncratic preferences for location e_{ic}, unemployed workers living in city a in period 1 and locating in city $c = a, b$ in period 2 have lifetime utility:

$$U^u_{ic} = (b - r_a - s - k_c) - \frac{1}{2}(b - r_a - s - k_c)^2 + \{\phi_c[(w - r_c + s) - \frac{1}{2}(w - r_c + s)^2] + \quad \text{(C.1)}$$
$$+ (1 - \phi_c)[(b - r_c + s) - \frac{1}{2}(b - r_c + s)^2]\} + e_{ic}$$

With $c = a$, the worker is staying in city a, and the lifetime utility function is given by:

$$U^u_{ia} = (b - r_a - s) - \frac{1}{2}(b - r_a - s)^2 + \{\phi_a[(w - r_a + s) - \frac{1}{2}(w - r_a + s)^2] + \quad \text{(C.2)}$$
$$+ (1 - \phi_a)[(b - r_a + s) - \frac{1}{2}(b - r_a + s)^2]\} + e_{ia}$$

Optimal saving is determined by:

$$\frac{\partial U^u_{ia}}{\partial s} = -u'(b - r_a - s) + [\phi_a u'(w - r_c + s) + (1 - \phi_a)u'(b - r_a + s)] = 0$$
$$\Rightarrow -1 + (b - r_a - s) + \phi_a[1 - (w - r_a + s)] + (1 - \phi_a)[1 - (b - r_a + s)] = 0$$
$$\Rightarrow s^u_a = -\frac{\phi_a(w - b)}{2}$$

Therefore the higher the gain from employment in period 2, the higher expected future utility is and the higher the amount borrowed in period 1. Plugging optimal saving into (C.2), we get the lifetime utility function:

$$V^u_{ia} = b - r_a + \frac{\phi_a(w - b)}{2} - \frac{1}{2}(b - r_a + \frac{\phi_a(w - b)}{2})^2 +$$
$$+ \phi_a[w - r_a - \frac{\phi_a(w - b)}{2} - \frac{1}{2}(w - r_a - \frac{\phi_a(w - b)}{2})^2] +$$
$$+ (1 - \phi_a)[b - r_a - \frac{\phi_a(w - b)}{2} - \frac{1}{2}(b - r_a - \frac{\phi_a(w - b)}{2})^2] + e_{ia}$$

from which:

$$V^u_{ia} = b(2 - b) + \phi_a(w - b)[1 - \frac{w + b}{2} + r_a] - 2r_a(1 - b) - r_a^2 + \frac{1}{4}\phi_a^2(w - b)^2 + e_{ia} \quad \text{(C.3)}$$

With $c = b$, the worker is moving to city b, and the lifetime utility function is given by:

$$U^u_{ib} = (b - r_a - s - k) - \frac{1}{2}(b - r_a - s - k)^2 + \{\phi_b[(w - r_b + s) - \frac{1}{2}(w - r_b + s)^2] +$$
$$+ (1 - \phi_b)[(b - r_b + s) - \frac{1}{2}(b - r_b + s)^2]\} + e_{ib} \quad \text{(C.4)}$$

Optimal saving is determined by:

$$\begin{aligned}\frac{\partial U^u_{ib}}{\partial s} &= -u^{'}(b-r_a-s-k)+\phi_b[u^{'}(w-r_b+s)]+(1-\phi_b)[u^{'}(b-r_b+s)]=0\\ &\Rightarrow -1+(b-r_a-s-k)+\phi_b[1-(w-r_b+s)]+(1-\phi_b)[1-(b-r_b+s)]=0\\ &\Rightarrow s^u_b=-\frac{(r_a+k-r_b)+\phi_b(w-b)}{2}\end{aligned}$$

Therefore the higher the gain from future employment, the higher the expected future utility and the amount borrowed in the present. Moreover, the higher the rent difference or the mobility cost, the higher the amount borrowed in the present. Plugging optimal saving into (C.4), we get the lifetime utility function:

$$\begin{aligned}V^u_{ib} &= b-r_a+\frac{(r_a+k-r_b)+\phi_b(w-b)}{2}-k-\frac{1}{2}(b-r_a+\frac{(r_a+k-r_b)+\phi_b(w-b)}{2}-k)^2+\\ &+[b-r_b-\frac{(r_a+k-r_b)+\phi_b(w-b)}{2}-\frac{1}{2}(b-r_b-\frac{(r_a+k-r_b)+\phi_b(w-b)}{2})^2]+e_{ib}\end{aligned}$$

which can be rewritten as:

$$\begin{aligned}V^u_{ib} &= b(2-b)+\phi_b(w-b)[1-\frac{w+b}{2}+r_b]-(r_a+r_b+k)(1-b)-\\ &-\frac{1}{2}(r_a^2+r_b^2+k^2+2kr_a)+\frac{1}{4}[(r_a+k-r_b)+\phi_b(w-b)]^2+e_{ib}\end{aligned} \tag{C.5}$$

Combining (C.3) and (C.5), we can derive the equilibrium number of unconstrained workers living in each city in period 2. The difference in lifetime utility between staying in city a and moving to city b is given by:

$$\begin{aligned}V^u_{ia}-V^u_{ib} &= (w-b)[\phi_a(1-\frac{w+b}{2}+r_a)-\phi_b(1-\frac{w+b}{2}+r_b)]+(r_b+k-r_a)(1-b)+\\ &+\frac{1}{2}(r_a^2+r_b^2+k^2+2kr_a)+\frac{1}{4}\phi_a^2(w-b)^2-\frac{1}{4}[(r_a+k-r_b)+\phi_b(w-b)]^2+(e_{ia}-e_{ib})\end{aligned} \tag{C.6}$$

Assuming $\frac{e_{ia}-e_{ib}}{q}\sim Logistic(0,1)$, we have:

$$N^u_a=\Lambda(\frac{v^u_a-v^u_b}{q})=\frac{exp(\frac{v^u_a-v^u_b}{q})}{1+exp(\frac{v^u_a-v^u_b}{q})}$$

where

$$\begin{aligned}v^u_a-v^u_b &= (w-b)[\phi_a(1-\frac{w+b}{2}+r_a)-\phi_b(1-\frac{w+b}{2}+r_b)]+(r_b+k-r_a)(1-b)+\\ &+\frac{1}{2}(r_a^2+r_b^2+k^2+2kr_a)+\frac{1}{4}\phi_a^2(w-b)^2-\frac{1}{4}[(r_a+k-r_b)+\phi_b(w-b)]^2\end{aligned}$$

Therefore the higher city a's job finding rate, city b's rent, the monetary cost of moving, or the idiosyncratic preference for city a the higher the number of workers choosing to stay in city a. On the contrary the larger ϕ_b or r_a, the higher the number of workers moving to city b.

Constrained Case

Consider the lifetime utility function in (C.1) with the difference that lending or borrowing are not allowed, that is $s_a^c = s_b^c = 0$. With $c = a$ the worker is staying in city a, and the indirect lifetime utility function is given by:

$$V_{ia}^c = (b - r_a) - \frac{1}{2}(b - r_a)^2 + \phi_a[w - r_a - \frac{1}{2}(w - r_a)^2] + \\ + (1 - \phi_a)[b - r_a - \frac{1}{2}(b - r_a)^2] + e_{ia}$$

which can be rewritten as:

$$V_{ia}^c = b(2 - b) + \phi_a(w - b)[1 - \frac{w + b}{2} + r_a] - 2r_a(1 - b) - r_a^2 + e_{ia} \tag{C.7}$$

With $c = b$, the worker is moving to city b and the indirect lifetime utility function is given by:

$$V_{ib}^c = (b - r_a - k) - \frac{1}{2}(b - r_a - k)^2 + [b - r_b - \frac{1}{2}(b - r_b)^2] + e_{ib}$$

which can be rewritten as:

$$V_{ib}^c = b(2 - b) + \phi_b(w - b)[1 - \frac{w + b}{2} + r_b] - (r_a + r_b + k)(1 - b) - \\ - \frac{1}{2}(r_a^2 + r_b^2 + k^2 + 2kr_a) + e_{ib} \tag{C.8}$$

Combining (C.7) and (C.8), we can derive the equilibrium number of constrained workers living in each city in period 2. The difference in lifetime utility between staying in city a and moving to city b is given by:

$$V_{ia}^c - V_{ib}^c = (w - b)[\phi_a(1 - \frac{w + b}{2} + r_a) - \phi_b(1 - \frac{w + b}{2} + r_b)] + (r_b + k - r_a)(1 - b) + \\ + \frac{1}{2}(r_a^2 + r_b^2 + k^2 + 2kr_a) + (e_{ia} - e_{ib}) \tag{C.9}$$

Assuming $\frac{e_{ia} - e_{ib}}{q} \sim Logistic(0, 1)$, we have:

$$N_a^c = \Lambda(\frac{v_a^c - v_b^c}{q}) = \frac{exp(\frac{v_a^c - v_b^c}{q})}{1 + exp(\frac{v_a^c - v_b^c}{q})}$$

with

$$v_a^c - v_b^c = (w - b)[\phi_a(1 - \frac{w + b}{2} + r_a) - \phi_b(1 - \frac{w + b}{2} + r_b)] + (r_b + k - r_a)(1 - b) + \\ + \frac{1}{2}(r_a^2 + r_b^2 + k^2 + 2kr_a)$$

As before, the higher city a's job finding rate, city b's rent, the monetary cost of moving, or the idiosyncratic preference for city a the higher the number of workers choosing to stay in city a. On the contrary the larger ϕ_b or r_a, the higher the number of workers moving to city b.

Workers Living in Each City

We can now compare the number of unemployed workers living in each city in the "unconstrained" and "constrained" equilibria. Combining (C.3) and (C.7), and defining unconstrained with NC and constrained with C, we get that:

$$V_{ia}^u - V_{ia}^c = \frac{1}{4}\phi_a^2(w-b)^2$$

similarly, combining (C.5) and (C.8) gives us:

$$V_{ib}^u - V_{ib}^c = \frac{1}{4}[(r_a + k - r_b) + \phi_b(w-b)]^2$$

Therefore more workers choose to live in a when there are no borrowing constraints if the consumption smoothing benefits when staying in city a are larger than the consumption smoothing benefits of moving to city b:

$$\phi_a^2(w-b)^2 < [(r_a + k - r_b) + \phi_b(w-b)]^2 \Leftrightarrow (\phi_a - \phi_b)(w-b) < r_a + k - r_b$$

If the difference in employment probabilities less than compensates for the difference in rents, net of moving costs, workers are more likely to move to city b if they are not borrowing constrained.

Proof of Testable Predictions

Prediction 1:
Denoting unconstrained with NC and constrained with C, we can combine (**??**) and (**??**) to get the consumption smoothing value when the individual decides to stay in city a in the second period.

$$V_{ia}^u - V_{ia}^c = \frac{1}{4}\phi_a^2(w-b)^2$$

and:

$$V_{ib}^u - V_{ib}^c = \frac{1}{4}[(r_a + k - r_b) + \phi_b(w-b)]^2$$

Therefore:

$$V_{ib}^u - V_{ib}^c > V_{ia}^u - V_{ia}^c \Leftrightarrow r_a + k - r_b > (\phi_a - \phi_b)(w-b)$$

Prediction 2:
For unconstrained workers, we get:

$$\frac{\partial[V_{ib}^u - V_{ia}^c]}{\partial k} = -(1-b) - r_a - k + \frac{1}{2}[\phi_b(w-b) + (r_a + k - r_b)] < 0$$

while for liquidity constrained workers, we get:

$$\frac{\partial[V_{ib}^c - V_{ia}^c]}{\partial k} = -(1-b) - r_a - k < 0$$

Therefore the difference is equal to:

$$\frac{1}{2}[\phi_b(w-b) + (r_a + k - r_b)]$$

and increasing in k.

Prediction 3:

For unconstrained workers, we get:

$$\frac{\partial[V_{ib}^u - V_{ia}^c]}{\partial b} = -[\phi_b(1-b+r_b) - \phi_a(1-b+r_a)] - (r_a - r_b - k) - \\ - \frac{\phi_b}{2}(r_a + k - r_b) - \frac{1}{2}(w-b)(\phi_b^2 - \phi_a^2)$$

while for liquidity constrained workers, we get:

$$\frac{\partial[V_{ib}^c - V_{ia}^c]}{\partial b} = -[\phi_b(1-b+r_b) - \phi_a(1-b+r_a)] - (r_a - r_b - k)$$

Therefore the difference is equal to:

$$\frac{\phi_b}{2}(r_a + k - r_b) + \frac{1}{2}(w-b)(\phi_b^2 - \phi_a^2)$$

C.4 Welfare Analysis

Unconstrained Case

Unemployed Workers

- With $\frac{e_{ia}-e_{ib}}{s} \sim Logistic(0,1)$, and the total population normalized to one, the average (and total) worker utility given optimal location choice is (using the result of Arcidiacono and Miller (2011)) :

$$V = Emax\{V_{ia}, V_{ib}\} = s\gamma + slog(exp(\frac{v_a}{s}) + exp(\frac{v_b}{s}))$$

- The change in total utility as b is increased is given by:

$$\frac{\partial V}{\partial b} = \frac{exp(\frac{v_a}{s})}{exp(\frac{v_a}{s}) + exp(\frac{v_b}{s})}\frac{\partial v_a}{\partial b} + \frac{exp(\frac{v_b}{s})}{exp(\frac{v_a}{s}) + exp(\frac{v_b}{s})}\frac{\partial v_b}{\partial b} \\ = N_a\frac{\partial v_a}{\partial b} + N_b\frac{\partial v_b}{\partial b}$$

where, for $c = a, b$:

$$\frac{\partial v_c}{\partial b} = \frac{\partial V_{ic}}{\partial b}$$

- Define (with $c \neq c^{'}$, $c = a, b$)

$$V_c = [Emax\{V_{ia}, V_{ib}\}|V_{ic} > V_{ic'}]$$

Using the result from Arcidiacono and Miller (2011) that, with $e_{ic} \sim EV1$:

$$E[e_{ic}|V_{ic} > V_{ic'}] = \gamma - log[\frac{exp(v_c)}{exp(v_c) + exp(v_c')}]$$

we have that

$$\begin{aligned} V_c &= v_c + sE[\frac{e_{ic}}{s}|\frac{V_{ic}}{s} > \frac{V_{ic'}}{s}] \\ &= s\gamma + slog(exp(\frac{v_a}{s}) + exp(\frac{v_b}{s})) \\ &= V \end{aligned}$$

- Therefore the effect of unemployment benefits on total utility for unemployed workers in each city is given by:

$$\begin{aligned} \frac{\partial[V_a N_a]}{\partial b} &= V_a \frac{\partial N_a}{\partial b} + \frac{\partial V_a}{\partial b} N_a \\ &= [s\gamma + slog(exp(\frac{v_a}{s}) + exp(\frac{v_b}{s}))]\frac{1}{s}[(r_a - r_b) - \phi_a(1 - b + r_a) - \frac{1}{2}\phi_a^2(w - b)]\times \\ &\times [\Lambda(\frac{v_a - v_b}{s})(1 - \Lambda(\frac{v_a - v_b}{s}))] + [N_a\frac{\partial v_a}{\partial b} + N_b\frac{\partial v_b}{\partial b}]N_a \end{aligned}$$

and

$$\begin{aligned} \frac{\partial[V_b N_b]}{\partial b} &= V_b \frac{\partial N_b}{\partial b} + \frac{\partial V_b}{\partial b} N_b \\ &= -[s\gamma + slog(exp(\frac{v_a}{s}) + exp(\frac{v_b}{s}))]\frac{1}{s}[(r_a - r_b) - \phi_a(1 - b + r_a) - \frac{1}{2}\phi_a^2(w - b)]\times \\ &\times [\Lambda(\frac{v_a - v_b}{s})(1 - \Lambda(\frac{v_a - v_b}{s}))] + [N_a\frac{\partial v_a}{\partial b} + N_b\frac{\partial v_b}{\partial b}]N_b \end{aligned}$$

Total Welfare

- As the model above only considers unemployed workers, one needs to add to it a measure of employed workers' welfare. Total welfare is then:

$$W = V + \lambda[B^e - T^e]$$

where λ is the marginal value of consumption for employed workers, B^e is total value of unemployment benefits for employed workers, and T^e is the total tax burden on employed workers.

- Then it follows that:

$$\begin{aligned}\frac{\partial W}{\partial b} &= \frac{\partial V}{\partial b} + \lambda[\frac{\partial B^e}{\partial b} - \frac{\partial T^e}{\partial b}] \\ &= N_a\frac{\partial v_a}{\partial b} + N_b\frac{\partial v_b}{\partial b} + \lambda[\frac{\partial B^e}{\partial b} - \frac{\partial T^e}{\partial b}] \\ &= N_a[(2-\phi_a)(1-b+r_a) - \frac{1}{2}\phi_a^2(w-b)] + N_b[2(1-b)+(r_a+r_b)] + \lambda[\frac{\partial B^e}{\partial b} - \frac{\partial T^e}{\partial b}]\end{aligned}$$

Constrained Case

- With $\frac{e_{ia}-e_{ib}}{s} \sim Logistic(0,1)$, and the total population normalized to one, the average (and total) worker utility given optimal location choice is (using the result of Arcidiacono and Miller (2011)) :

$$V = Emax\{V_{ia}, V_{ib}\} = s\gamma + slog(exp(\frac{v_a}{s}) + exp(\frac{v_b}{s}))$$

- As shown already, the change in total utility when b is increased is given by:

$$\frac{\partial V}{\partial b} = N_a\frac{\partial v_a}{\partial b} + N_b\frac{\partial v_b}{\partial b}$$

and

$$V_c = V$$

- Therefore the effect of unemployment benefits on total utility for unemployed workers in each city is given by:

$$\begin{aligned}\frac{\partial[V_aN_a]}{\partial b} &= V_a\frac{\partial N_a}{\partial b} + \frac{\partial V_a}{\partial b}N_a \\ &= [s\gamma + slog(exp(\frac{v_a}{s}) + exp(\frac{v_b}{s}))]\frac{1}{s}[(r_a-r_b) - \phi_a(1-b+r_a)]\times \\ &\times [\Lambda(\frac{v_a-v_b}{s})(1-\Lambda(\frac{v_a-v_b}{s}))] + [N_a\frac{\partial v_a}{\partial b} + N_b\frac{\partial v_b}{\partial b}]N_a\end{aligned}$$

and

$$\begin{aligned}\frac{\partial[V_bN_b]}{\partial b} &= V_b\frac{\partial N_b}{\partial b} + \frac{\partial V_b}{\partial b}N_b \\ &= -[s\gamma + slog(exp(\frac{v_a}{s}) + exp(\frac{v_b}{s}))]\frac{1}{s}[(r_a-r_b) - \phi_a(1-b+r_a)]\times \\ &\times [\Lambda(\frac{v_a-v_b}{s})(1-\Lambda(\frac{v_a-v_b}{s}))] + [N_a\frac{\partial v_a}{\partial b} + N_b\frac{\partial v_b}{\partial b}]N_b\end{aligned}$$

Total Welfare

- Given that total welfare is:

$$W = V + \lambda[B^e - T^e]$$

it follows that:

$$\begin{aligned}\frac{\partial W}{\partial b} &= \frac{\partial V}{\partial b} + \lambda[\frac{\partial B^e}{\partial b} - \frac{\partial T^e}{\partial b}] \\ &= N_a\frac{\partial v_a}{\partial b} + N_b\frac{\partial v_b}{\partial b} + \lambda[\frac{\partial B^e}{\partial b} - \frac{\partial T^e}{\partial b}] \\ &= N_a[(2-\phi_a)(1-b+r_a)] + N_b[2(1-b)+(r_a+r_b)] + \lambda[\frac{\partial B^e}{\partial b} - \frac{\partial T^e}{\partial b}]\end{aligned}$$

Increasing Unemployment Benefits

Increasing unemployment benefits causes more workers to move to city a if there are borrowing constraints, given that (from above):

$$[\frac{\partial[V_{ia} - V_{ib}]}{\partial b}]^{NC} - [\frac{\partial[V_a - V_b]}{\partial b}]^{C} = -\frac{1}{2}\phi_a^2(w-b)$$

The effect of an increase in unemployment benefits on total welfare is larger when unemployed workers are borrowing constrained, given that:

$$\frac{\partial W}{\partial b} = N_a[(2-\phi_a)(1-b+r_a) - \frac{1}{2}\phi_a^2(w-b)] + N_b[2(1-b)+(r_a+r_b)] + \lambda[\frac{\partial B^e}{\partial b} - \frac{\partial T^e}{\partial b}]$$

with no borrowing constraints and:

$$\frac{\partial W}{\partial b} = N_a[(2-\phi_a)(1-b+r_a)] + N_b[2(1-b)+(r_a+r_b)] + \lambda[\frac{\partial B^e}{\partial b} - \frac{\partial T^e}{\partial b}]$$

with borrowing constraints. As a consequence, the more borrowing constrained unemployed workers are, the higher unemployment benefits should be.