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Abstract

We analyze price responses to large restaurant VAT rate reductions in two different European countries. Our results show that responses in the short and medium run were clustered around two focal points of zero pass-through and full pass-through. Differences between independent restaurants and chains is the key explanation for this pattern. While nearly all independent restaurants effectively ignored the tax reductions and left consumer prices unchanged, a substantial fraction of restaurants belonging to chains chose a rapid and complete pass-through. In the longer run, prices converged, but primarily through a price reversion among chain restaurants. The stark difference in price responses does not appear to arise because of different market characteristics such as location, initial price levels, meal types or restaurant segment.

Keywords: firm types, VAT incidence, price setting, restaurants
JEL-codes: [H22, H32, E31]

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1 Introduction

An increasingly active literature within public finance explores the price incidence of consumption taxes (Carbonnier 2007, Doyle and Samphantharak 2008, Kenkel 2005, Kosonen 2015, Benzarti and Carloni 2017, Benzarti et al. 2017, and Rozema 2017) finding varying rates of pass-through onto consumer prices. The typical explanation for the varying results rests on differences in elasticities of demand and supply or the degree of competition among firms (e.g. Myles 1989 and Fullerton and Metcalf 2002).[1] For our purposes, two aspects of these explanations are particularly noteworthy; first, they tend to imply that the distribution of price adjustments relative to pre-tax prices is smoothly centered around an average pass-through, and second, they do not explore the link between internal firm-level factors and price adjustments, as is often the case within public finance (see e.g. Slemrod and Gillitzer, 2014). In this paper we use uniquely detailed micro data on price adjustments around two VAT reforms showing that some firms react strongly and others not at all. This non-smooth bi-modal price-change distribution is intimately related to distinct types of price-setting firms, even when holding observed market conditions constant.

We analyze price responses to VAT-rate reductions for restaurants in Finland (9pp) and Sweden (13pp). To execute the analysis, we collected data on meal-level prices across time as well as firm and market characteristics that are matched to administrative tax-records on revenues and costs. The data allow us to follow the prices of the same meals over time, and to examine the full distribution of price changes for different types of firms. To assess the importance of time effects, we collected identical data from neighboring countries.

On average, we find a short-run price response of one quarter of full pass-through, defined as unchanged producer prices. This limited average response is in line with several recent studies of service-sector VAT responses (see e.g. Carbonnier 2007, Kosonen 2015 and Benzarti and Carloni 2017). However, the distributions of price responses reveal strikingly clean, but heterogeneous, price-change patterns. On one side, the major-

[1] For studies of tax evasion and generic cross-industry differences, see e.g. Kopczuk et al. (2016) and Marion and Muehlegger (2011). For rare studies of firm heterogeneity see Kopczuk and Slemrod (2006) and Best et al. (2015).

ity of prices were completely unchanged a few months after the reduced tax rates were implemented. On the other side, we instead find a *full* short-run pass-through for the majority of prices that did adjust. This non-smooth division into two distinct spikes ("all-or-nothing") is not part of standard public finance predictions (or standard explanations for a low pass-through). Furthermore, the all-or-nothing shape is not present in our control countries where the price-change distributions instead have a spike at zero, but otherwise display a continuous set of actual price changes.[2]

Our rich micro-level data are exceptionally well suited to analyze the underlying sources of the observed price-response heterogeneity. We collected detailed sub-market indicators which cover various aspects of meal- types and locations but we also documented if the restaurants are independent operations (referred to as Independents) or belong to a chain or franchise (Chains). This collection was inspired by recent research suggesting substantial heterogeneity across firms in management practices and strategies (see Bloom and Van Reenen 2010, Bloom et al. 2013, and Drexler et al. 2014) which we believed may expand into the price-setting domain. The basic idea is that the dichotomy between independents and chains should be related to price-setting practices, since the two types of restaurants are likely to use different managerial inputs in the pricing decision process. An obvious reason is that there may be fixed costs of setting or adjusting prices that can be shared across restaurants within larger operations (we show that chains do coordinate their price adjustments across restaurants).

Our results show that the distinction between independents and chains is a crucial determinant of the distribution of price adjustments: Only 4.8 percent of independent restaurants changed their prices *at all* due to the reform, whereas the same estimate is 38 percent for chain restaurants, and 25 percent of them chose an exact full pass-through. To quantify the importance of chains vs. independents relative to other aspects of the restaurants and meals, we run a set of regressions where we explain the short-run price responses by aspects of the product (the type of restaurant and meal), the initial price (level and if a round number), the location (local restaurant density, located in a mall) and other key indicators (belonging to employer

[2]The same is true for alcohol prices (which were exempted from the VAT reductions) within the treated restaurants.

organization, a dummy for changed items on the menu). Strikingly, we find that a single dummy separating independents and chains has a much larger explanatory power than the *combination* of all these other factors. We take this as strong support for the notion that the type distinction is fundamental, and quantitatively important.

We further show that most of the chain restaurants that initially chose a full pass-through abandoned this new reduced price within 6 months and instead increased their prices at a much higher rate than other restaurants. In contrast, the majority of independent restaurants kept their initial pre-reform prices intact until our final survey 15-18 months after the reform.[3] As a consequence of independents' inactivity and chains' reversions, average market pass-through was *reduced* over time even after accounting for inflation. This is very different from the standard text-book notion (building on Adam Smith) that price responses should increase over time due to capital adjustments and new market entrants.

On the robustness side, we show that the distinction between our two types of restaurants does not appear to be a proxy for other confounding factors. Chains and independents operate in similar market segments; both groups feature fast-food venues and finer restaurants, and the initial price distributions are surprisingly similar. Moreover, diverging price responses are substantial within each quartile of initial prices and remain if we focus on establishments located close to each other within the same restaurant-dense areas, and when we exclusively zoom in on restaurants located in malls. The main results do not appear to be due to tax evasion since our administrative tax data show that VAT payments fell by equal amounts for both types of restaurants. Similarly, our analysis of tax credited inputs and the number of traded meals suggests that differential changes in meal quality are unlikely to explain our findings.[4] The complete lack of response from almost all of the independent restaurants can only be explained by standard tax-incidence models if demand is infinitely elastic or

[3]Those that did change their prices displayed a smooth distribution centered around the initial price.

[4]Measurement errors are unlikely to explain the results since they also hold within the sub-sample of restaurants where some of the prices actually changed. The observed time variations in sales suggest that it is unlikely that a large set of firms chose not to reduce prices because they operated at full capacity.

supply inelastic, both of which seem to be *a priori* unlikely explanations. Furthermore, a joint analysis of our price data and administrative tax data suggests that the demand for restaurant services is quite inelastic for both restaurant groups.[5]

A set of further results suggests that the differences in tax responses we uncover do reflect fundamental underlying differences in pricing behavior. Most notably, chains are more likely to change their prices in times when VAT rates are fixed, independents are much more likely to use round-number prices, and chains (but not independents) had an abnormal frequency of price increases during the Estonian currency conversion.

Overall, we conclude that the average short- and medium-run price response to consumption taxes is unlikely to be fully understood without accounting for firm-level heterogeneity, thus supporting the Slemrod and Gillitzer (2014) argument that modeling firm-level decisions is an important area for future developments within public finance. This conclusion is supported by the fact that the distinction between independents and chains explains more of the variation in responses to the reform than extensively studied aspects such as within-market coordination or market density. We believe that building micro-foundations of firm behavior may help to explain the diverging results regarding responses to large tax changes found in previous studies (see e.g. Cabral et al. 2017, Carbonnier 2007, Gruber and Koszegi 2004, Kenkel 2005, Kosonen 2015, Benedek et al. 2015, Benzarti and Carloni 2017, Benzarti et al. 2017, and Rozema 2017).[6]

The paper is structured as follows: Section 2 presents institutions, data and methods. Section 3 shows results on the short- and long-run pass-through for independents and chains. Section 5 presents supporting evidence on coordination, outputs and inputs, round-number prices and currency conversions. Section 5 concludes the study.

[5]Although imprecisely estimated, this result suggests that the short-run gains from lowering prices was in fact modest.

[6]Benzarti et al. (2017) include a case study from Finnish hairdressers finding that pass-through for VAT reduction was significantly lower than pass-through for VAT increase. Our results are consistent with that study in the sense that we study VAT reduction in a service sector and find on average low pass-through. The hairdressing industry in Finland consists mainly of independent firms.

2 Reforms and data

2.1 The reforms

All countries within the EU use value added taxation (VAT) for consumption taxes with a restricted number of rates. Since 2009, an EU Directive has allowed member states to apply one of their reduced rates to restaurant services. France was the first to reduce restaurant VAT, from 19.6 to 5.5 percent in 2009. Sweden and Finland followed shortly after.

In Finland, the VAT rate for restaurant meals was cut from the standard rate of 22 percent to a reduced rate of 13 percent from July 1st, 2010. In Sweden the corresponding VAT rate was reduced from 25 to 12 percent from January 1st, 2012. In both countries, the VAT rate for alcohol remained at the original standard rate after the reform. The changes in VAT legislation were passed relatively close to the reform, which makes large pre-reform anticipatory effects unlikely.

2.2 Measuring the pass-through

Goods-specific changes in VAT affect firms symmetrically to goods-specific changes in sales taxes. The reason is that the tax formula calculates the tax on sales and the crediting of inputs separately, and they can change independently of each other.

We measure the impact of the VAT reforms on prices by adjusting consumer prices relative to a *full pass-through* (FP). Full pass-through is defined as the change in consumer prices (p) at unchanged producer prices (ϕ). Formally, denoting VAT after (before) the reform by τ^a (τ^b) and using that $p = \phi(1+\tau)$:

$$FP = \frac{\phi(1+\tau^a) - \phi(1+\tau^b)}{\phi(1+\tau^b)}$$

The impact of the VAT rate change on consumer prices relative to full pass-through is denoted by Δ and defined as:

$$\Delta = \frac{p^a - p^b}{p^b} * 100/FP \tag{1}$$

where p^a (p^b) is consumer price after (before) the reform. The full pass-through (FP) implies a drop in consumer prices of -7.4 percent in Finland and -10.4 percent in Sweden. Notably, and in contrast to sales taxes in the US, consumer prices within the EU are always displayed including VAT.

When interpreting our analysis of these tax changes, it is important to note that they are sizable, also if contrasted with normal price variations.[7]

2.3 Outline of the empirical approach

We study the price evolution within Swedish and Finnish restaurants using the evolution in neighboring countries to assess how prices would have evolved if the taxes had remained unchanged. Estonia serve as the contrast for the Finnish reform, whereas Finland is the contrast for the Swedish reform (based on the assumption that the Finnish price responses had leveled out at that time, at least at the relatively high frequency that we are analyzing the data). For robustness, we use restaurant alcohol prices within the treated restaurants as an alternative control. Since alcohol prices may be confounded by potential spillover-effects between the treated and control services, we mostly focus on the cross-country controls.[8] Regardless of the controls, our strategy relies on standard differences-in-differences (DD) assumptions, i.e. that the behavior of the control group (neighboring countries) properly reflects the (counterfactual) evolution of the treatment group in absence of treatment. However, when we shift our focus towards potential firm-type differences in price responses, deviations from this identifying assumption causes problems only if they are systematically related to the types of firms.

The rationale for using neighboring EU countries as controls mimics that of the vast number of state-level DD studies within the US since Card and Krueger (1994). Institutions, geography, culture, climate, seasonal holidays, vacation periods and seasonality in national food production are similar across the countries we study. This partly reflect the fact that Finland and Sweden were a single country when many important institutions

[7] In Appendix B, Figure B1, we show that a full pass-through would lie outside of the whole distribution of price changes during a fixed VAT rate.

[8] Since too few firms use tax-rate contingent prices, we are unable to use takeaway prices as an alternative control even though these were subject to an unchanged tax rate.

were formed.

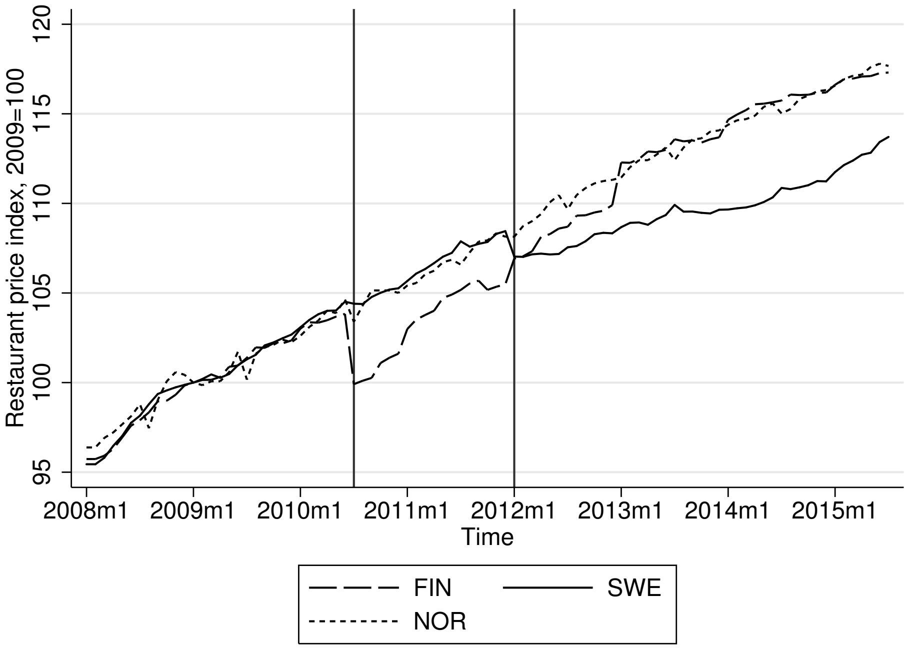


Figure 1: CPI component of restaurant-meal prices in Finland, Sweden and Norway

Note: Monthly data on consumer prices for restaurant meals collected by national statistical offices in Finland, Sweden and Norway. Vertical lines are for restaurant-meal VAT cuts in Finland (July 2010) and Sweden (January 2012). The Figure does not show the price evolution for Estonia due to the lack of separate data on restaurant meals from the Estonian CPI.

To check whether the basic idea of using neighboring countries to assess the importance of time effects is reasonable, we start by illustrating the evolution of the restaurant-meal component of the CPI (a small sample) in a set of neighboring countries. Here we use data on Sweden, Finland and Norway. We replace Estonia by Norway in the Figure because we, unfortunately, were unable to get access to CPI data on Estonian restaurant-meal prices. The evolution is shown in Figure 1. As is evident, the CPI meal prices have trends that are largely parallel in the different countries with only two exceptions: Finnish meal prices dropped in July 2010 as VAT for Finnish restaurant meals was reduced from 22 to 13 percent, and Swedish meal prices dropped in January 2012 when VAT was reduced from 25 to

12 percent.[9][10]

We are not able to cluster the standard errors by the country level as, in principle, would be recommended by the logic of Cameron et al. (2008). This is a feature following the case-study nature of the exercise here, we only have two country-level reforms to analyze.[11]

2.4 Data

We collected prices directly from the restaurants using a protocol outlined in Appendix A. We first drew random samples of restaurants from national tax registers, which list all firms liable to taxation by industry in each country. For the Finnish reform, we collected pre-reform data (from Finland and Estonia) in May-June 2010 and short-run incidence data in July-August 2010. For the Swedish reform, we collected pre-reform data (from Sweden and Finland) in October-November 2011 and the short-run incidence data in February-March 2012. We repeated these surveys half a year and a year and half later. Finally, we linked the survey data to firm-level register data on turnover, profits and the total wage bill in the treated countries.

Data were collected by teams of research assistants within each country. Most of the restaurants had a website that included prices for meals and we used these if possible. If no website was found, we contacted the restaurant by phone. This procedure allowed us to survey restaurants across a large geographic area based on a random sampling frame. We explore potential differences between web and phone prices in the robustness section.

For each restaurant, we collected prices and meal names for 7 to 11 meals in pre-set categories (meal types). The exact meal types described in Appendix A depend on the type of restaurant and include main courses, vegetarian meals starters, desserts and pre-set lunches. We also collected

[9] Figure B2 in Appendix B provides further evidence along the same lines, documenting parallel pre-reform trends in sales and wage bills in Finnish and Swedish restaurants.

[10] In the analysis below, we document the price-change distributions in treated and control settings (Finland and Estonia). To further document the robustness of the results, in Appendix B, Figure B3, we use price-change distribution of Norwegian restaurant meals and alcohol sold in restaurants as alternative control groups.

[11] Instead, we provide standard errors clustered by the firm-level and, as an alternative, (wild bootstrapped) errors that are clustered at the zip-code level.

information on alcohol prices (unchanged VAT) when applicable. The assistants chose the exact meals within each category and recorded the prices and names so that we could follow the exact meals across time. Additional information included restaurant type and name, location characteristics such as located in a mall or on a restaurant-dense street.

2.4.1 Independent restaurants and chains

A main element in our analysis is the role of price-setting firm types. Throughout, we define restaurants that (according to our survey) are not part of a chain or franchise, as *Independent,* and other restaurants as *Chains*. More precisely, we define all restaurants belonging to brand names with two or more restaurants as chains and add restaurants belonging to very large firms (belonging to the top quartile of total firm-level wage bills). In the results section below we discuss supporting evidence regarding this division and provide some insights into the heterogeneity within the groups.

2.5 Descriptive statistics

Table 1: Descriptive statistics

	Chain			Independent		
	Mean	Median	SD	Mean	Median	SD
Share of restaurants	0.371	0		0.629	1	
Meal price	10.134	8	7.262	8.985	7.304	7.715
Mall-dummy	0.188	0	0.391	0.089	0	0.285
Price quartile: 1 = smallest and 4 = highest						
1	0.223	0	0.416	0.275	0	0.447
2	0.177	0	0.382	0.228	0	0.420
3	0.258	0	0.438	0.249	0	0.433
4	0.342	0	0.474	0.248	0	0.432
Restaurant density: 1 = least dense and 5 = densest						
1	0.083	0	0.275	0.194	0	0.395
2	0.101	0	0.302	0.184	0	0.387
3	0.171	0	0.377	0.142	0	0.349
4	0.229	0	0.420	0.178	0	0.382
5	0.415	0	0.493	0.303	0	0.459
Restaurant classification						
Fast food	0.256	0	0.436	0.224	0	0.417
À la carte	0.544	1	0.498	0.555	1	0.497
Cafe	0.074	0	0.261	0.118	0	0.323
Lunch	0.126	0	0.332	0.103	0	0.303
N of restaurants		898			1,712	
N of meals		4,092			6,924	
Firm-level[a] wage bill	22,384,642	1,794,554	75,345,249	331,516	199,333	348,199
Firm-level[a] turnover	159,931,072	2,331,829	558,455,839	343,519	211,372	445,702

Note: Price is the price of meals in euros. Mall is for restaurants in malls or shopping-dense areas. Price quartiles are based on pre-reform (restaurant averaged) meal prices by country. Restaurant density is based on the number of restaurants by zip code (5d in Finland and Estonia, 3d in Sweden), where all restaurants with Mall=1 are in category 5. "Lunch" is for restaurants open mainly for lunch and breakfast. Annual turnover is tax-inclusive sales. Wage bill and turnover are from administrative registers, nominal amounts converted to euros.
[a]Measured at the *firm-level*; each firm within the chain category can (and will, except for franchises) involve multiple *restaurants*.

Table 1 displays descriptive statistics separately for independents and chains. Overall, about two-thirds of our restaurants are independent. Independents and chains are fairly similar in most dimensions. In particular, the two types contain very similar fractions of fast-food restaurants, à la carte restaurants, cafes and lunch restaurants, and the average meal prices are

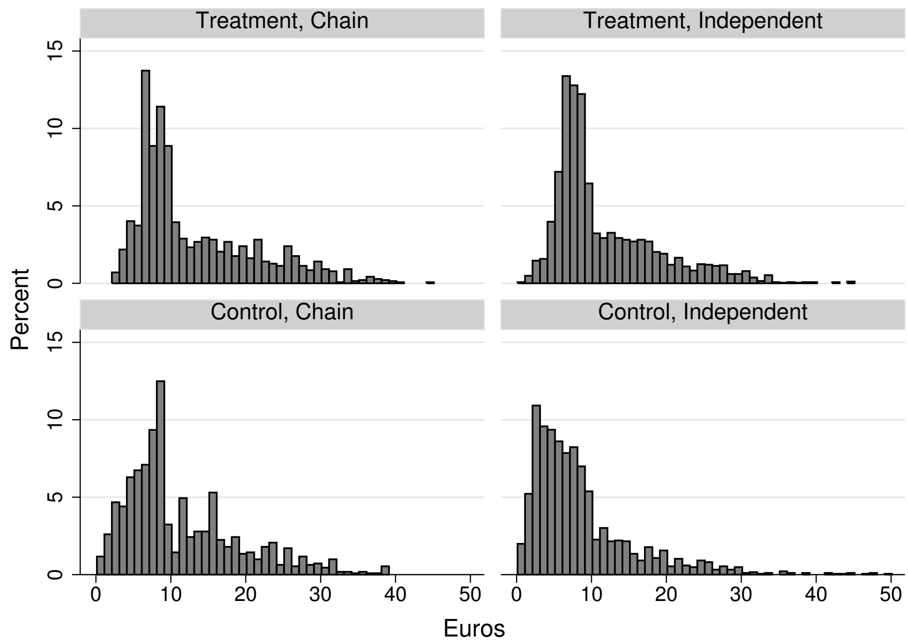


Figure 2: Pre-reform prices by treatment status and type
Note: Data from our own price collections. All prices are converted to euros.

only marginally higher in the chains.

The bottom two statistics show that the chain restaurants belong to larger firms. This is a natural consequence of the fact that chain firms tend to span across multiple restaurants.[12]

Figure 2 shows the price distributions separately for independents and chain restaurants, divided by treatment status. As is evident, the price distributions are overlapping with very similar shapes. Although not crucial for identification, we find it reassuring that the distributions in treatment and control countries are similar before the reforms, and that this holds for both restaurant types.

3 Main results

In this section, we start by showing the overall impact of the reforms on the short-run price-change distributions. We then turn to the analysis where

[12]It can further be noted that approximately 17 percent of all treated chain restaurants are franchises, whereof about 2/3 are owned by the franchise itself.

we distinguish between independent restaurants and chains. We end the section by discussing the medium-term impact of the reforms.

3.1 Overall pass-through

We first show estimates of the average short-run pass-through of the VAT reforms onto prices. This impact was already visible in the analysis of the restaurant-meal component of CPI depicted in Figure 1 above. Using our own micro data instead allows us to follow the same meals over time for a large set of data points and to study the anatomy of the price changes.

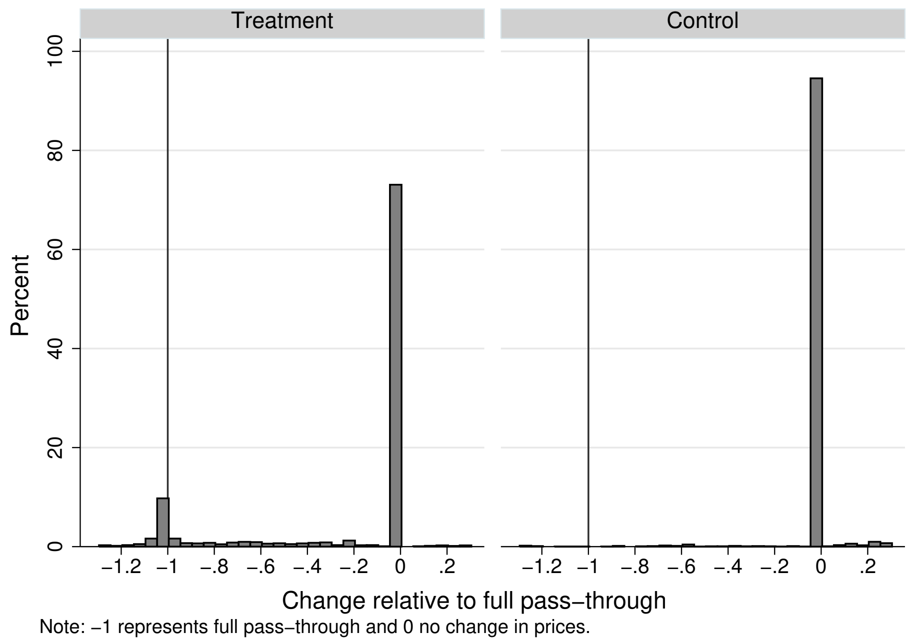


Figure 3: Distribution of price changes in the two reforms

Note: Meal-level price changes from 1-2 months before to 1-2 months after reforms. Normalized; -1 is full pass-through.

Figure 3 shows the price-change distributions relative to full pass-through. The differences between the treatment and control groups imply a noticeable, but relatively modest, average short-run price effect of the reforms.[13] The large spikes at zero in both groups indicate that many prices did not

[13] As our final interest lies in the behavior of firms, we do not re-weight our main analysis by firm sales as is done in the CPI calculations.

change at all. Although the spike at zero is clearly larger for the control group (indicating that the reforms had an impact on prices), it remains remarkably pronounced for the treatment group as well, despite the large reductions in VAT rates. The second visible spike for the treatment group is at full pass-through (i.e. at -1), indicating that when meal prices changed, they often changed by the full pass-through. Conclusions would be identical if we instead had constructed the counterfactual distributions from Norwegian restaurant meals or from alcohol sold in the treated restaurants (see Appendix B, Figure B3).

To test the robustness of the raw differences in Figure 3, we estimate statistical models relying on the Differences-in-Differences (DD) logic. This allows us to include a rich set of control variables. Throughout, we use the pass-through $\triangle$ as the dependent variable. Formally, we estimate:

$$\Delta_{ijr} = B_1 D_{jr}^{Treat} + B_2(X_{ijr}) + u_{ijr}, \tag{2}$$

using data on meal i at restaurant j and reform r, where D_{jr}^{Treat} is a dummy for restaurants in the treatment group. Note that the first-difference form for the outcome removes all unobserved meal-specific constant factors as in a "meal fixed effects" model. X contains a vector of covariates capturing (market) factors besides ownership structure, which could affect tax incidence. These include the (initial) price quartile and restaurant type dummies described in Table 1 as well as a set of meal type dummies described in Appendix A, zip-code fixed effects and a dummy for whether the collection was made by phone or from the web. All X variables are measured separately by reform (i.e. Swedish vs. Finnish).

Table 2 shows the results. Column (1) is without any controls for the Finnish reform and column (2) is for the Swedish reform. The estimates suggest a larger pass-through in the case of the Finnish reform compared to the Swedish reform. Column (3) shows the estimate for pooled reforms and suggests an impact of 27 percent of full pass-through. Reassuringly, including very detailed controls (column 4) capturing the significance of restaurant class, meal type and initial price quartile has only a marginal impact on the estimate of interest.

Table 2: Average short-run pass-through

	(1)	(2)	(3)	(4)
	Finnish reform	Swedish reform	Pooled reforms	Pooled reforms
	Pass-through	Pass-through	Pass-through	Pass-through
Treatment	-0.313***	-0.173***	-0.268***	-0.236***
	(0.101)	(0.060)	(0.087)	(0.076)
	[0.019]	[0.022]	[0.014]	[0.017]
N	5,287	5,048	10,335	10,335
R^2	0.047	0.013	0.032	0.057
Rest class * reform				x
Meal type * reform				x
Price Q * reform				x

Note: Dependent variable is Δ of equation (1). Wild bootstrapped standard errors with one digit zip-code-level clusters in parentheses and with firm-level clusters in square brackets. *** p<0.01, ** p<0.05, * p<0.1.

3.2 Pass-through at independent restaurants and chains

We now turn to the role of restaurant and meal characteristics in explaining the overall price-change patterns. Figure 4 shows the price-change distributions separately for chains and independents, by treatment status. As the Figure shows, the pass-through is visibly different between these. About 60 percent of chain restaurants reduced their prices after the reforms, whereas almost 90 percent of the independent restaurants kept their prices constant despite the large VAT reductions. Thus, the dichotomy between independents and chains is a key predictor for where the treated restaurants end up within the bi-modal price-change distribution shown in Figure 3.

Even if we focus on control-group restaurants, we note that chains are more likely to change their prices (mostly upwards, for natural reasons) than independents. This suggests that independents have less active pricing strategies even in normal (non-reform) times. Importantly, however, the price-change distribution in the control group is uni-modal, that is, there is no spike at all corresponding to the full pass-through spike we saw in the treated distributions.

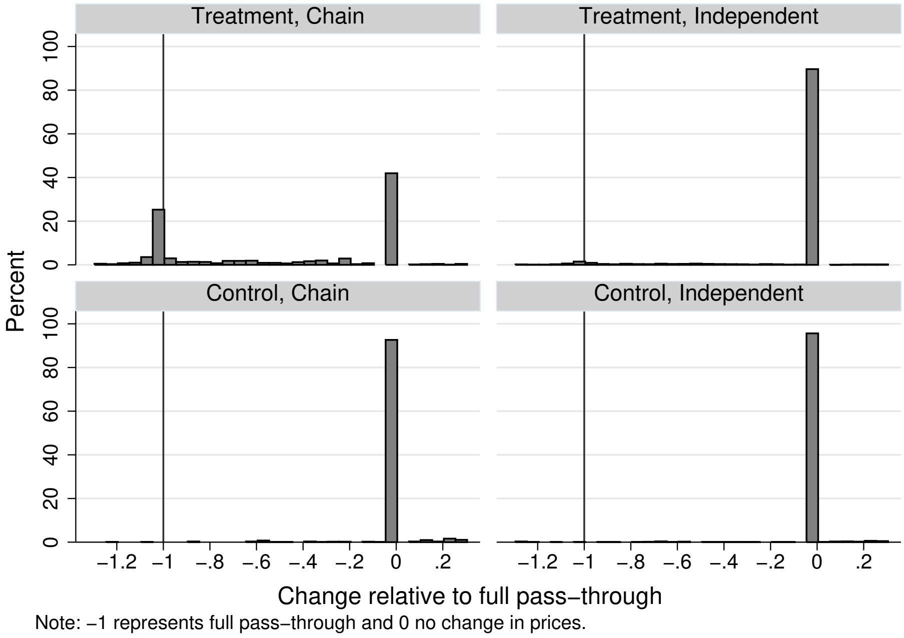


Figure 4: Short-run pass-through, by treatment status and type
Note: Meal-level price changes from 1-2 months before to 1-2 months after reforms. Normalized; -1 is full pass-through.

Comparing treated and controls, we find that the reforms increased the probability that independents changed their prices by a very modest 4.8 percent and virtually none of them used a full pass-through. In contrast, the probability of changing prices increased by 38 percent for the chains and the probability of a full pass-through was 0.25 (relative to a baseline of zero).[14]

The distinction between independents and chains thus appears to be a crucial determinant of the heterogeneous pass-through. To make this more precise, we run a regression where we predict the pass-though by the independent-dummy and the other variables in our data. Table 3 shows the results. The partial R2 for the independent dummy is larger than the partial R2 of a *combined* set of variables capturing the market segment of the restaurant.[15] We have verified that there are no differential responses

[14]The probabilities of changes are estimated using as the outcome whether or not a price changed, see Appendix B, Table B1. All conclusions are identical if we use alcohol prices in treated restaurants as an alternative control, see Appendix B, Figure B4.

[15]The variables are: four restaurant class dummies [fast-food, à la carte, café or lunch

in terms of meal-exit margin between independents and chains (detailed results are in Appendix B, Table B9).

Table 3: Partial coefficients and R-squared values for different variables

Only treated restaurants (N = 5762)			
Dependent: Pass-through			
Variable	Partial Coeff.	Partial R2	Partial R2
Independent	0.1868	0.0349	0.0349
Mall	0.0605	0.0037	
Rest class (ref. fast food)			
À la carte	0.0377	0.0014	
Cafe	-0.0227	0.0005	
Lunch	-0.0775	0.0060	
Price quartile: ref. smallest			
2	-0.0176	0.0003	
3	-0.0307	0.0009	
4	-0.0380	0.0014	
Density: no. rest. quartile, ref: smallest			
2	-0.0101	0.0001	
3	-0.0387	0.0015	
4	-0.0742	0.0055	
Meal exit	-0.0186	0.0003	
Confederation	-0.0624	0.0039	
Round before price	0.0391	0.0015	
All other variables (sum)			0.0270

Note: Table shows the partial coefficients and partial R-squared values for the Independent dummy and individual explanatory variables regressed on the pass-through using data solely on treated restaurants. Table also shows the sum of partial R-squared of other variables. These variables include the variables in Table 1, and dummy for restaurants that have changed one or more of their meals between the first and second collection rounds (meal exit), a dummy for restaurants belonging to a hospitality industry confederation (confederation), and a dummy for restaurants having at least one round meal price (round before price).

Next, we estimate a DD-model based on equation (2), but where the treatment dummy is interacted with the independent dummy:

restaurant] *and* four price-quartile dummies *and* four dummies for the restaurant density in the location), *and* other measures of rigidity (a dummy for some meal exiting the menu, a dummy for using round-number prices) *and* membership in a lobby organization (the relevant employer confederation.

$$\Delta_{ijr} = \beta_1 D_{jr}^{Treat} + \beta_2 D_{jr}^{Indep} + \beta_3 (D_{jr}^{Indep} * D_{jr}^{Treat}) + \beta_4 (X_{ijr}) + \varepsilon_{ijr}, \quad (3)$$

where D_{jr}^{Indep} is a dummy for independent restaurants. The coefficient β_1 identifies the effect of the VAT reform on the change in prices for chains, β_2 measures any additional price trend for independents within the control regions and β_3 reveals the process of interest, that is, differences in responsiveness to the reforms between independents and chains. X contains the same variables as above.

Table 4: Short-run pass-through by type

	(1)	(2)	(3)	(4)	(5)
	Finnish reform Pass-through	Swedish reform Pass-through	Pooled reforms Pass-through	Pooled reforms Pass-through	Pooled reforms Pass-through
Treatment	-0.631***	-0.273***	-0.553***	-0.450***	-0.483***
	(0.178)	(0.084)	(0.179)	(0.142)	(0.122)
	[0.051]	[0.039]	[0.026]	[0.033]	[0.067]
Independent	-0.028	-0.081	-0.089	-0.053	-0.060**
	(0.050)	(0.061)	(0.056)	(0.043)	(0.030)
	[0.050]	[0.029]	[0.023]	[0.025]	[0.027]
Independent	0.534***	0.167***	0.453***	0.380***	0.341***
*Treatment	(0.141)	(0.069)	(0.135)	(0.117)	(0.105)
	[0.055]	[0.047]	[0.031]	[0.033]	[0.037]
N	5,287	5,048	10,335	10,335	10,335
R^2	0.127	0.015	0.065	0.083	0.126
Treatment effect	-0.097	-0.106	-0.100	-0.069	-0.142
for independents	(0.204)	(0.112)	(0.179)	(0.145)	(0.146)
Rest class * reform				x	x
Meal type * reform				x	x
Col method * reform				x	x
Price Q * reform					x
ZIP fe					x

Note: Dependent variable is Δ of equation (1). Treatment effect for independents is calculated as sum of estimates for Treatment and Independents*Treatment. Wild bootstrapped standard errors with one digit zip-code-level clusters in parentheses and with firm-level clusters in square brackets. *** p<0.01, ** p<0.05, * p<0.1.

Table 4 presents the results. In columns (1) and (2), we show the results separately for the Finnish and Swedish reforms, and in columns (3) through (5) we pool the two reforms together. Coefficient β_1, the price incidence for chains, is estimated to be -0.63 for Finland, -0.27 for Sweden and -0.55 when pooling the reforms in column (3). Importantly, coefficient β_3, the difference between independents and chains, is large and statistically significant both for the two reforms separately; 0.53 for Finland, 0.17 for Sweden and 0.45 when pooling the reforms. The estimate for the impact of the reform on the independent restaurants, calculated as the combination of Treatment and the interaction of Independent and Treatment coefficients, shown in the bottom of the table, is negative but close to zero and statistically insignificant in all five columns.

A reasonable *a priori* hypothesis for the observed difference in meal price changes between independents and chains is that they operate in different types of markets. To investigate this concern, we use four indicators of the nature of the relevant market: (i) restaurant classification (fast food, à la carte, café, lunch restaurant), (ii) meal type (mostly 7 categories within each class, see Appendix A), (iii) the level of the original (pre-reform) prices in quartiles, and (iv) the zip code.[16] As a first test of the market hypothesis, we re-estimate the model controlling for restaurant classification and meal type dummies interacted with treatment status in column (4).[17] We then add (initial) price quartile dummies interacted with treatment status, and zip-code fixed effects in column (5). This means that the estimates are based only on comparisons between restaurants (of different types) that compete within the same price range and location, and that are selling similar types of products. Note that the interactions with treatment status soaks up the overall estimate of the pass-through. Although the point estimate of interest in column (5) is marginally reduced (from 0.45 to 0.34) when adding the very large set of covariates compared to no additional covariates in column (3), the main thrust of the difference also remains in these very tight specifications.[18]

[16]Also controlling for indicator variable of restaurant being located in a mall does not affect any of the results of interest.

[17]We also control for collection method (phone/internet) interacted with treatment status in columns (3) and (4).

[18]Table B1 in Appendix B also shows the estimates on the probability of short-run price changes by restaurant types using exactly the same method and set of covariates as in Table 4. Reassuringly, the interpretation of these results is very similar to the

It is important to note that Figure 4 shows a bi-modal distribution of price changes only in the treatment group but not in the control groups. It therefore seems unlikely that our results are driven by the particular choice of control group. Along these lines, we find identical results if using alcohol prices as a within-country control.[19]

Overall, these results suggest that neither location, restaurant category, nor price segments can explain why independent restaurants respond so differently from restaurants belonging to chains. Notably, it seems unlikely that otherwise similar (located close to each other and serving meals in the same price range) independents and chains should face demand elasticities that are different enough to explain the large remaining response-gap between the two.

3.3 Medium-run pass-through

We now turn to the longer-run effects using data from four separate collections; the first two are (as before) 1-2 months before the reforms and 1-2 months after the reforms, the third collection was 3-6 months after the reforms, and the fourth 15-18 months after the reforms. We still follow the same meal prices over time, provide precise measures of price changes and control for the unobserved meal size and quality, but here we only have data on the treated countries. Obviously, some of the meals have changed, reducing the sample size as time from the first collection elapses. The treated part of the sample decreases from 5,762 observations (price collection right after the reforms) to 4,262 observations in the last price collection 15-18 months after the reforms, but the frequency of exits does not differ between the treated chains and independents (see Table B9 in Appendix B for details).

Figure 5 shows the distribution of meal price changes between the first collection and the consecutive three collections for the treatment group. The upper panel of the figure is for chains and the lower panel for indepen-

pass-through estimates.

[19] See Appendix B, Table B2 for results with alcohol as control. We have also verified that the differences between independents and chains are similar across the distribution of initial prices (see Appendix B, Figure B5). The main deviation is that the graph indicates that the pass-through is highest for chains operating in the lowest price segment.

dent restaurants. The first two panels from the left show the immediate price change (the same as in Figure 4), the second set of panels shows the price changes until 3-6 months after the reform and the final set of panels shows corresponding numbers for 15-18 months after the reform. The initial spike at full pass-through in the chain restaurant distribution vanishes almost completely within 3-6 months from the reform. The Figure also shows that a non-trivial fraction of meal prices are at the pre-reform price level a full year and a half after the reform. This holds especially for independents and for those chains that did not initially change their prices.

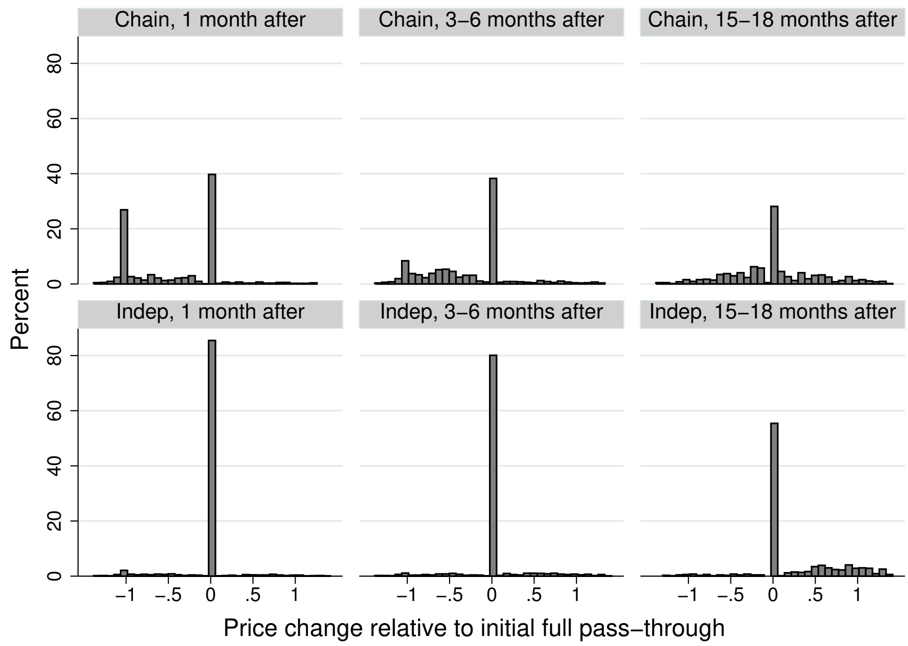


Figure 5: Medium-run pass-through, by treatment status and type
Note: Meal-level price changes from 1-2 months before to 1-2 months after, 3-6 months after and 15-18 months after reforms. Normalized; -1 is full pass-through.

As an additional exercise, it turns out to be illustrative to separate the longer run price responses depending on whether the initial price was changed or not, despite the obvious endogeneity. Figure 6 shows the results from this exercise. The figure indicates that many of the prices that were at the full pass-through straight after the reform reverted back to the exact pre-reform price after 15-18 months. In contrast, prices of the meals that were stable across the reform remained much more stable in the following periods also. Thus, the convergence of averages (between chains and independents) is to a large extent driven by the fact that the chains that initially reduced their prices later moved back towards their pre-reform starting point.

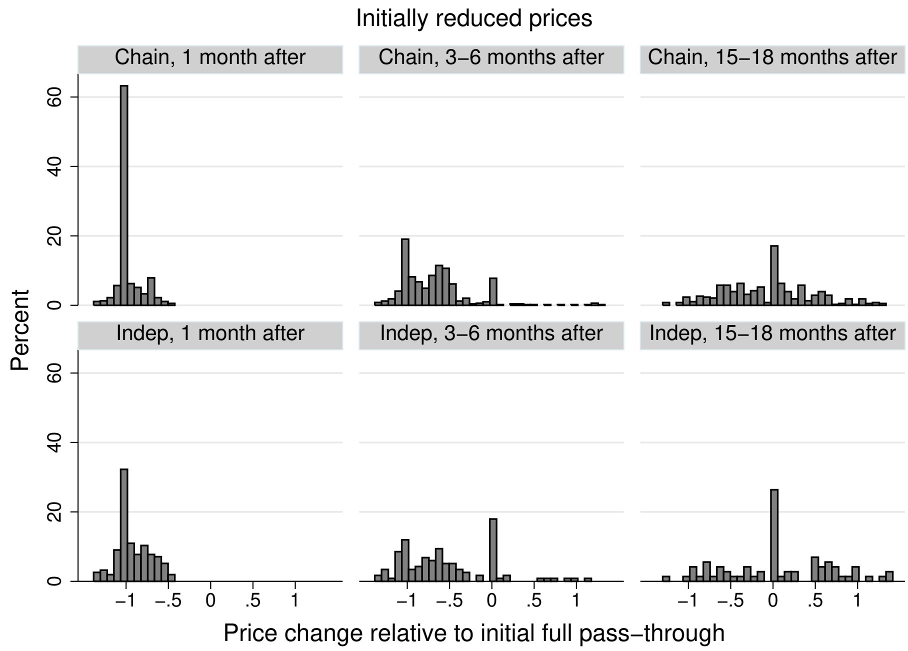
We cannot use the prices from other countries as controls when analyzing medium-run responses, since Estonia, which is the control country for the Finnish reform, had a currency conversion (kroon to euro) at the beginning of 2011. Instead, we utilize alcohol prices in the same restaurants to control for time effects. We use the price change relative to the initial price scaled by the full pass-through as the outcome throughout.

We display estimates in Table 5 in three different panels, each having a different time-distance to the reforms; panel A shows immediate pass-through, panel B 3-6 months after and panel C 15-18 months after.[20]

As expected, the short-run estimates mimic the results from Table 4. The immediate reduction in prices is about -0.49 for chains and 0.41 larger than that for independents. More importantly, the results indicate that the average difference in price levels between chains and independents started to decline already 3-6 months after the reform. The average price difference is further converging after 15-18 months, at which time the estimated difference is considerably smaller (0.1) and statistically insignificant. We have verified that we get similar results if we directly estimate the differences between independents and chains using data within the treated samples. The average pass-through for treated chains is larger after adding the very rich set of control variables in column (3), probably due to the fact that we are pushing the identification into a space with quite little variation left.[21]

[20]A caveat for the final panel is that there was a tiny increase in alcohol taxes in Finland (below 1 percent of retail price increase for beer and even less for wine), when using alcohol prices as a control for Sweden 15-18 months after the reform.

[21]See Table B3 in Appendix B for details. The change in alcohol taxes discussed above does not seem to drive the convergence between independents and chains in the last period, as expected from Figure 5.



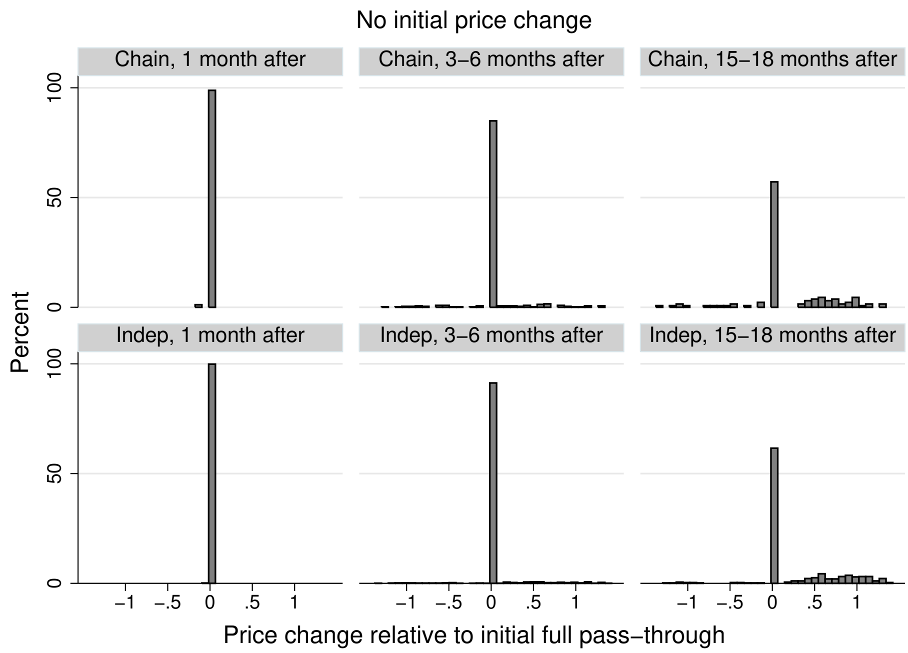


Figure 6: Medium-run pass-through divided by restaurant type, initial price change and collection rounds

Note: Meal-level price changes from 1-2 months before to 1-2 months after, 3-6 months after and 15-18 months after reforms by restaurant type and initial price change (left panel for those changing prices and right panel for those not changing prices). Normalized; -1 is full pass-through.

Table 5: Medium-run price responses (pass-through) by type using alcohol prices as a control group

	(1)	(2)	(3)
	Pass-through	Pass-through	Pass-through
Panel A: 1-2 months after			
Treated (food)	-0.488***	-0.321***	-0.502***
	(0.169)	(0.117)	(0.162)
Independent	-0.041	-0.057	-0.074
	(0.075)	(0.086)	(0.086)
Treated * Independent	0.405***	0.388***	0.396***
	(0.147)	(0.135)	(0.137)
N	6,326	6,326	6,255
R^2	0.057	0.089	0.100
Panel B: 3-6 months after			
Treated (food)	-0.372***	-0.523***	-0.530***
	(0.120)	(0.169)	(0.171)
Independent	-0.040	-0.075	-0.094
	(0.081)	(0.105)	(0.115)
Treated * Independent	0.329***	0.338***	0.353***
	(0.128)	(0.123)	(0.133)
N	5,425	5,425	5,369
R^2	0.027	0.044	0.056
Panel C: 15-18 months after			
Treated (food)	-0.018	-0.316**	-0.404***
	(0.127)	(0.136)	(0.152)
Independent	0.012	-0.050	-0.154
	(0.383)	(0.133)	(0.207)
Treated * Independent	0.099	0.205	0.253
	(0.158)	(0.175)	(0.202)
N	4,545	4,545	4,509
R^2	0.001	0.024	0.042
Rest class * reform		x	x
Meal type * reform		x	x
Price Q * reform			x
ZIP fe			x

Note: Dependent variable is Δ of equation (1). Wild bootstrapped standard errors with one digit zip-code-level clusters in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

4 Pricing strategies and mechanisms

This section presents additional results in several dimensions with the intent to shed additional light on various potential mechanisms that could explain our main results. We end the section by collecting the empirical results and discussing their joint relationship to potential mechanisms.

4.1 Restaurant density and price-change coordination

A possible explanation for differences in pass-through is the degree of market competition. To investigate this issue, we calculate the area-level density of restaurants and analyze the relationship between the density and the initial price response. We group the restaurants by density quantiles (at zip-code level) and add all restaurants located in malls to the densest group. The results are displayed in Figure 7. As is evident, the proxy for the degree of competition does indeed predict the pass-through in the expected direction (more competition, higher pass-through), but only for the chains. The independents ignore the reform, regardless of density.[22] The result for chains (more pass-though in areas with more competitors) resembles the findings of Cabral et al. (2017) for the insurance industry.

We have also analyzed various aspects of price coordination in the spirit of, e.g., Houde (2012) and Thomadsen (2005). Within 5 major cities,[23] we divided the zip codes into smaller areas consisting of a few blocks each. Within these areas, we measured the average pass-through among *other* restaurants and analyzed the association between the response of each restaurant and the average response of others in the same area. Column (1) of Table 6 shows the main DD estimate for this more limited sample. Column (2) presents the estimated price-response coordination across all restaurants within the same area. Column (3) adds to this an estimate of coordination of price changes within the same restaurant type and area. We find no significant evidence of coordination within neighborhoods. The point estimates for coordination within the area are negative

[22] In Appendix B, Figure B6, we show results for restaurants located in malls. Consistent with the results in Figure 7, chain restaurants in malls respond more heavily than other chains, but independent restaurants ignore the reforms regardless of location.

[23] Helsinki, Tampere, Turku, Stockholm, Gothenburg, and Malmö

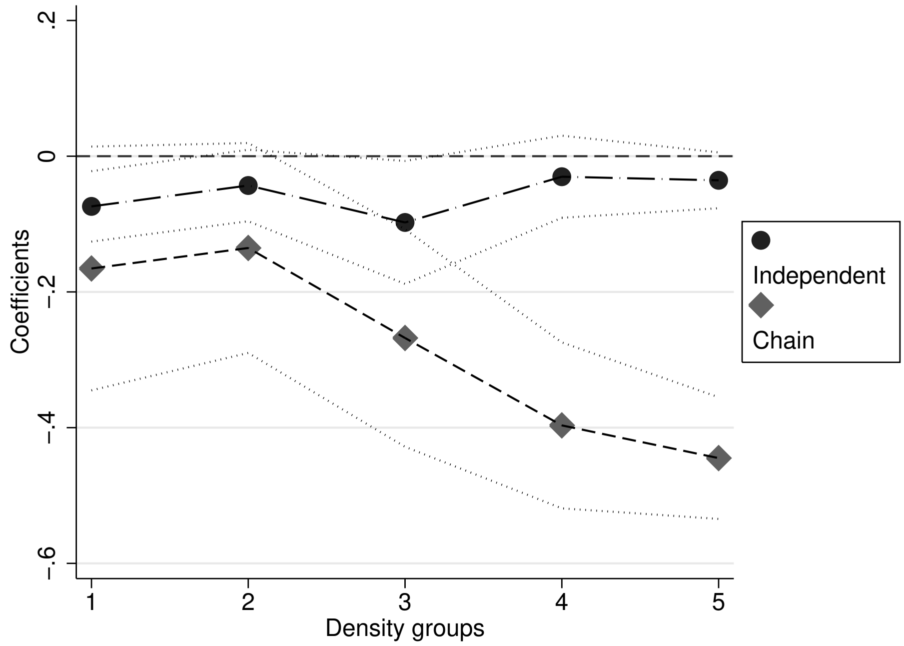


Figure 7: Pass-through according to restaurant density

Note: Dependent variable is Δ of equation (1). Density is measured by quantiles at the zip-code level. All restaurants in malls are placed in the densest category.

in both columns (2) and (3), and price coordination within the same type in the same area is estimated to be positive, but very close to zero. None of these estimates are statistically significant, and the evidence thus suggests that neighboring restaurants do not coordinate their price responses (within or across types). Together with Figure 7 this suggests that restaurants in denser areas react more, not because of the interactions with their close neighbors, but because restaurants that (for other reasons) are more responsive to tax cuts are selected into denser areas. Moreover, the insignificant association between price responses of neighboring restaurants suggests that the cross-price elasticity within the restaurant industry is low. The lack of direct price competition suggests that the market is best characterized as one with imperfect (e.g. monopolistic) competition between differentiated products. Although a standard imperfect competition model is insufficient to explain the differing pricing strategies among independents and chains, it appears a prerequisite for them to pursue separate pricing strategies.

Table 6: Coordination in price changes across restaurants and meals

	(1)	(2)	(3)	(4)	(5)
Independent	0.566***	0.675***	0.415**		0.171***
	(0.171)	(0.275)	(0.206)		(0.032)
Others in the same area		-0.197	-0.252		
		(0.153)	(0.213)		
Others in the same area and same type			0.089		
			(0.186)		
Others in the same group				0.700***	
				(0.232)	
Other prices in the same restaurant					0.494***
					(0.062)
N	1,035	1,035	1,035	2,085	5,564
R^2	0.149	0.157	0.158	0.136	0.191

Note: Dependent variable is Δ of equation (1). Columns (1) through (3) are for restaurants in restaurant-dense areas only. Column (2) adds the average price change of other restaurants in the same area. Columns (3) includes the average price pass-through of other restaurants in the same area and the average price pass-through of other restaurants in the same area in the same restaurant type (Independent/Chain). Column (4) is for chains only. The estimate is for the average price change of other restaurants in the same chain. Column (5) includes all treated restaurants. The estimate is for the average change in other prices within the same restaurant. Wild bootstrapped standard errors with one digit zip-code-level clusters in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

We turn to study price coordination within chains (column (4)) and within restaurants (column (5)), and the evidence suggests substantial coordination (0.7 and 0.5 respectively) in both of these dimensions. The fact that chains coordinate their price responses at least as much across different restaurants as the typical restaurant coordinates its prices within the restaurant suggests that chain-level pricing decisions are highly coordinated.[24] This last finding is consistent with those of Della-Vigna and Gentzkow (2017), who show that chains have very similar prices for the same products across wide range of different markets.

[24] The finding of substantial coordination within chains is well in line with Conlon and Rao (2015) and previous results from the IO literature, whereas the lack of local coordination is not (see e.g. Houde 2012 and Thomadsen 2005).

4.2 Chain types, collection method, and altered menus

We have explored the extent to which the price responses differ in many dimensions (see Appendix B, Tables B4 and B5 for details) and the pass-through for chains is larger in malls, and other restaurant-dense areas, in the lowest price quartile, among chains belonging to the national restaurant confederation, among lunch restaurants, followed by fast food and cafeterias, and dinner (à la carte) restaurants. Moreover, among franchising restaurants, franchise-owned have greater pass-through than company-owned. In none of the cases do we find any response for independents. The estimated pass-through does not differ by price collection method (phone or internet). We divided the sample according to whether or not the restaurant changed some of the meals in their menu at the time of the reform. Chains that altered some items on the menu had a slightly larger response but, again, there was no statistically significant change in prices for independents in any of the sub-samples.

4.3 Quantities and inputs: evidence from tax registers

Next, we investigate how inputs and outputs change with the reform for the two types of firms relying on administrative data originating from tax authorities in the treated countries. The administrative data are collected at the firm level, so that one firm observation sometimes combine information from many restaurants belonging to the same chain.[25] Figure 8 shows the development of quarterly log changes of inputs (credited against VAT) and the quarterly remitted VAT before and after the reforms separately for chains and independents.[26] For expositional reasons we normalize the series at zero four quarters before the reforms. As is evident, inputs for both types of firms remained stable across the reforms. This indicates that neither the (reported) quantities nor the qualities have responded to the reforms. As the inputs develop similarly for the two types of restaurants,

[25] We are able to analyze the average behavior of chains in these data, but the firm-level nature of the administrative records prevents us from fully exploring heterogeneity across different types of chain restaurants.

[26] Data only include surveyed restaurants since surveys identify chains. We further excluded observations with >100% change in annual sales. These data restrictions also apply to Figure 9 and Table B6, presented in Appendix B.

and raw food materials are a significant part of inputs in the restaurant industry, we conclude that meal-quality responses appear to be an unlikely explanation for the observed drastic differences in price pass-through.

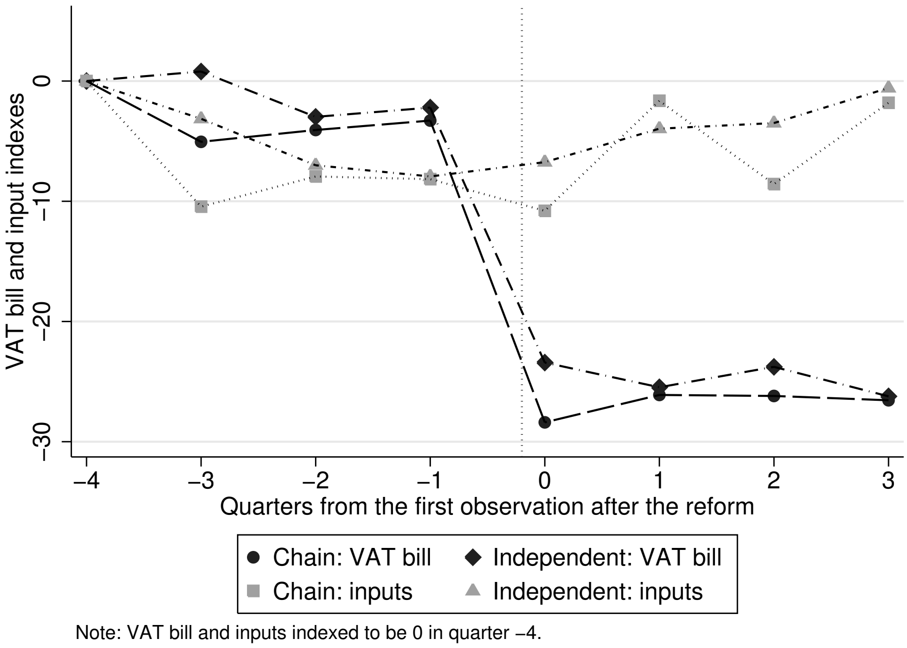


Figure 8: Inputs and VAT remittances, by quarter relative to the reform
Note: Coefficients of quarter indicators in a regression where the dependent variable is the log 4-quarter change in VAT bills and inputs credited against VAT by restaurant type. Based on administrative data for the surveyed firms. VAT bills and inputs are indexed to be zero at 4 quarters before the reforms. Dotted line marks the reform.

Furthermore, Figure 8 shows that both types of restaurants remitted VAT prior to the reforms, and the reduced VAT rates generated clear drops in remitted VAT for both groups. This is important since it speaks against tax evasion as an explanation for the main results. In the extreme, if all consumption taxes are evaded, changes in consumption taxes would for obvious reasons not affect prices but this does not seem to be the case for either of the restaurant types.[27] The fact that the independents in our data do remit VAT, and reduce their remittances to a similar degree as the chains, clearly speaks against the notion that tax evasion can explain the

[27]Changes in the tax rate could affect tax evasion (as well as real decisions) under a less extreme assumption of partial tax evasion, depending on the model (see, e.g., the discussion in Slemrod and Yitzhaki 2002).

diverging price responses.

Next we present a set of results building up towards an analysis of the impact on (a proxy for) the number of traded meals. The idea is that the tax-inclusive revenue each month equals the number of sales multiplied by the average firm-specific price. Since we observe the averages for both revenues and prices (prices from our own survey and revenue from the tax data), we can generate a proxy for the number of traded meals by dividing revenues by prices. The results are presented in graphical form in Figure 9, and in Table format in Appendix B, Table B6.

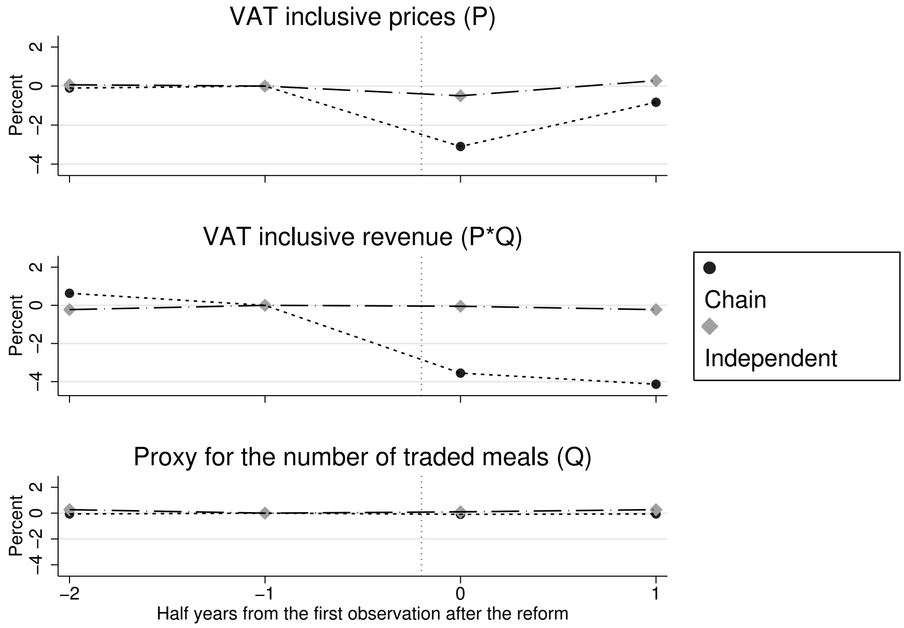


Figure 9: Changes in log consumer prices, VAT-inclusive revenue and quantity

Note: Coefficients of half-year indicators in a regression where the dependent variables are log half-year tax-inclusive prices (P), tax-inclusive revenues (P*Q) and a proxy for the quantity of traded meals (P*Q/P) by restaurant type. In order to take into account the huge variation in tax-inclusive revenue (P*Q) in the administrative data, the revenue is smoothed by controlling with the revenue of exactly one year before for each firm. Also, due to the high variation in quarterly sales and inputs, we have restricted the data by excluding observations with more than 100% annual changes in sales. Sample consists only of surveyed firms. Dotted vertical line marks the reform.

The first panel of Figure 9 repeats the consumer price analysis, displaying the falling prices among the chains (using the survey data). The second panel shows the evolution of total firm-level revenues (using administrative data for the same sample), which also falls for the chains relative to the independents.[28] The final panel shows the impact on the quantities, measured as revenues deflated by consumer prices. We find no differences between chains and independents in terms of quantities as measured by our proxy for the number of traded meals.[29] Here it should be acknowledged that the underlying estimates (as shown in Appendix B, Table B6) are imprecise since the firm-level revenue data are extremely volatile (as shown in Appendix B, Figure B7) and our sample sizes are not very large. But, taken at face value, the results indicate that the shift towards lower relative prices among the chains does not appear to have increased their market shares to any noticeable degree.[30] This suggests that their demand elasticity is low and that different demand elasticities between independents and chains are unlikely to explain the large differences in pass-through we observe. Furthermore, the inelastic demand together with less than full pass-through suggest that the profit for restaurants increased, and that the profits for chains increased (in the very short term) by less than the profits of independents.

4.4 Other evidence on pricing strategies

4.4.1 Round numbers

One factor which may contribute to our main results is that independent restaurants have less precise pricing strategies and rely on crude price targets instead. According to e.g. Levy et al. (2011), round-number prices can be interpreted as an indicator of less strategic price setting. In our data, this is much more prevalent among independents. We define a price as *round* if it takes an integer value in euros (in Finland) or 10 SEKs or

[28]It is important to note that we do not have an external control group for this analysis.

[29]We also did the analysis using register data by winsorizing the data 1 percent from both upper and lower tail of distribution instead of dropping excessively large variation in sales, and the results from that exercise are effectively the same, no statistically significant change in quantities for chains nor independents.

[30]An inelastic change in quantities due to VAT reduction is consistent with the findings in the analysis for hairdressers by Kosonen (2015).

10 EEKs (in Sweden and Estonia), which are roughly comparable numbers accounting for exchange rates.[31]

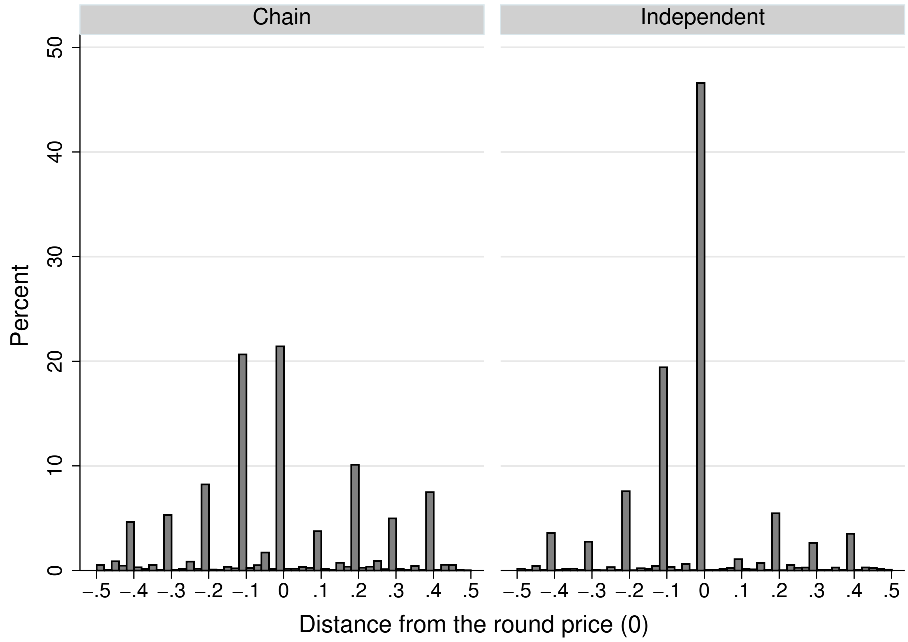


Figure 10: Round-number pricing by type

Note: Price distances to the closest round number. Round numbers are integer euros, or multiplicative of 10 SEKs or EEKs. Round prices are normalized to zero, bandwidth: 0.02 units.

Figure 10 shows the distributions of price distances to the closest round number separately for independent restaurants and chains. Clearly, chain restaurants (left-hand panel) rely much less on round numbers than the independents (right-hand panel). Almost 50 percent of the meal prices are round among the independent restaurants whereas the corresponding number for chains is just above 20 percent. Using multiples of 5 instead for Estonia does not alter the conclusions. The results are not driven by an excessive use of close-to-round-number prices (e.g. 9.99 or 9.95) since these events are extremely rare within our data. The results are robust to inclusions of a rich set of control variables in a regression framework (see Table B7 in Appendix B for details). Overall, the independents are 29 percentage points more likely to use round-number prices than chains.

[31]The exchange rates of 1 euro = 9.06 SEK = 15.65 EEK in December 2010.

To corroborate the interpretation of round prices as rigid, it can be shown that price responsiveness to the VAT-reforms are lower for (initially) round than non-round prices (see details in Appendix B, Figure B8).

4.4.2 Pricing during currency conversions

Next, we analyze the price responses to the currency conversion from the Estonian kroon (EEK) to the euro at the beginning of 2011. The reason is that currency conversions potentially creates an opportunity for firms to strategically increase their prices without negative customer reactions in a setting with an unchanged marginal product.

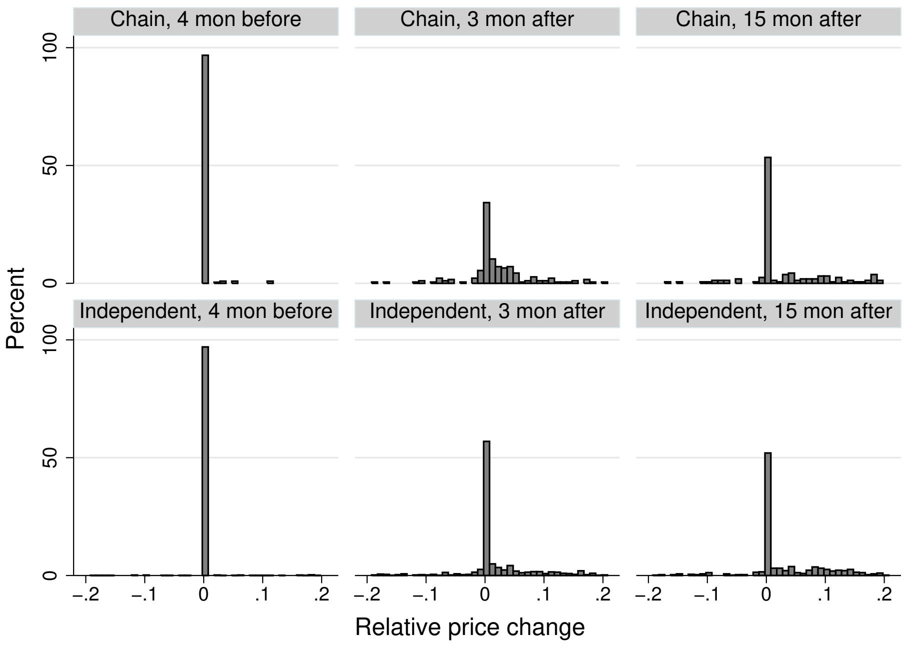


Figure 11: Relative price changes around Estonian currency conversion
Note: Meal price changes for Estonian restaurants before, during, and after Estonia joined the Eurozone.

The resulting relative price-change distributions are shown in Figure 11. Each panel shows the relative price changes across two collection moments at different time intervals. Restaurants belonging to chains (relative to independents) increased their prices more often just at the time of the currency conversion, shown in the middle panels, than in surrounding time periods shown in the other panels. As in the other dimensions, the differences are substantial (17 percentage points) and accounting for covariates do not change the impression (see Appendix B, Table B8).

4.5 Summary and discussion of mechanisms

Our main results are 1) the price-change distribution after the VAT reduction is bi-modal with large spikes at full pass-through and zero pass-through and 2) virtually all of the pass-through is due to price changes among chain restaurants, while independent restaurants are responsible for the spike at zero. In this subsection we discuss how various potential mechanisms relate to our main results and our auxiliary evidence.

Standard tax incidence theories. In the simplest, perfect competition and a single good case, the elasticities of demand and supply are the sole determinants of price incidence and the more inelastic side bears the burden of taxation. This implies that to explain a zero (full) pass-through, one needs to assume perfectly elastic (inelastic) demand or perfectly inelastic (elastic) supply.[32] If the distribution of supply and demand elasticities are smooth, then we would expect that the firm-level price responses should be smoothly distributed around average pass-through. Extensions with, e.g., imperfect competition can generate a larger or smaller pass-through, but the pass-through should only be exactly zero or full under extreme assumptions about market structures and/or supply curves (see Myles 1989, Weyl and Fabinger, 2013).

Due to the prominent role played by the demand elasticity in standard incidence theory, it is natural to consider the extent to which this elasticity differs between chains and independents. One reason for such difference could be that independents and chains differ in the loyalty of their customer base. We cannot explore this hypothesis directly with our price or administrative data.[33] However, we believe that our evidence suggest a difference

[32]Starting from non-zero tax levels, or assuming ad valorem taxes, complicates the formula slightly but does not change the main intuition for the role of the elasticities.

[33]One hypothesis would be that independents have longer customer relations than chains. However, our results show that the meal exit rates are indistinguishable between independents and chains (see Appendix Table B9) and unrelated to the reform. This results thus do not lend support to the notion that the relationships to the customer base are entirely different between independents and chains. A more direct exploration

in demand elasticities is unlikely to, on its own, be the main explanation for our results. First and foremost, to explain the bi-modal price-change distribution would require extreme assumptions about the differences in demand elasticities and our data do not lend support to such extreme assumptions. Independents and chains provide meals within similar price ranges as shown by the overlapping price distributions (Figure 2). Chains and independents located close to each other still respond very differently to the VAT reductions (Table 4). In fact, the rich set of obvious market indicators are (even jointly) much less related to the size of tax response than the chain-independent dichotomy (Table 3). And importantly, our estimates on administrative data (although statistically imprecise) suggest that demand is equally inelastic for both types of restaurants (Figure 8 and Figure 9).

We also show that restaurants located next to each other do not appear to react to each others' prices (Table 6). The last result suggest that cross-price elasticities between restaurants are low. This suggests that restaurants (irrespective of type) supply differentiated products, and thus are engaged in imperfect (perhaps monopolistic) competition. Although imperfect competition is insufficient to, on its own, explain the differences between chain and independent restaurants, it would help to explain why consumers do not react much to changes in relative prices.

Information / tax evasion. Firms could ignore VAT changes if they are unaware of these reforms, or if they evade all the taxes regardless. However, we find it highly unlikely that any firm could have missed any of the reforms as we (admittedly, anecdotally) perceived the VAT reductions as highly visible in the media at the time they happened. As more direct evidence, we observe that both restaurant groups actually did change the amount of remitted VAT (Figure 8). Obviously, this could not happen if firms did not know about the tax changes, or if they evaded VAT altogether.

Capacity constraints. Firms may be in a situation where they cannot serve more customers due to short-term capacity constraints, which would make price reductions meaningless. Such constraints could explain our results if they were binding for virtually all of the independents, but fewer

of this hypothesis would, however, require a detailed survey of restaurant customers. We believe this to be an interesting avenue for future research.

of the chains. However, if the initial response was thwarted by capacity constraints, we would expect them to grow stronger in the medium term as the constraints are eased. Thus, the pass-through would become greater over time for the independents. Instead, we observe the opposite (Figure 5). Chains start to revert back towards their pre-reform price-levels already 6 months after the reform. Independents are slower to increase their initially non-changing prices, but when they change them, the changes are in form of increases rather than decreases.

Menu costs. A standard theoretical rationale for the pattern of non-changing prices is the assumption of fixed costs for changing prices ("menu costs", as in e.g. Golosov and Lucas 2007).[34] Such costs, if larger than the benefits of the changing prices, can explain inaction in times of tax changes. Our analysis shows that only focusing on restaurants that made some changes in the composition of meals between surveys (Table B4 in Appendix B) does not change our main results, which points away from a very literal interpretation in terms of costs from changing menus. However, menu-costs, in a more general sense, may still play a role and our general interpretation is that menu costs may have contributed to some of the patterns we observe. Such costs are likely to be particularly large for independents, which potentially could explain why independents do not change their prices. However, to be able to explain the spike at full pass-through, a standard menu-cost model would need to be supplemented with additional elements.

Fixed choice sets for firms, experimentation and salience. The mass point at full pass-through can potentially be explained by another set of models where agents are assumed to reduce the complexity of their pricing problem by optimizing over a discrete set of predetermined pricing options while trying to improve their information set at the same time. Models in this vein include the multi-armed bandit models of Rothschild (1974), Bergemann and Välimäki (1996) or Keller and Rady (1999), and the rational inattention model by Matějka (2016). In the context of a tax reform, the logic could be applied to a discrete set of possible reactions

[34] Recent extensions include Nakamura and Steinson (2008), who nest the model with a standard Calvo model, and Midrigan (2011), who discusses the case where multi-product firms have to pay a fixed cost for changing any price.

where a full and zero pass-through are two natural focal points.[35]

It can, in addition, be rational for firms to either choose a very large pass-through or a zero pass-through if customers are inattentive to small price reductions, rendering such reductions useless for the price-setting firms. Gabaix (2014) proposes that customers choose to optimize over a reduced set of possible choice variables (a "sparse" matrix). This can force firms to use large price reductions when they reduce prices in order to catch the attention of consumers. In the context of tax reductions, it seems plausible that the increased salience of reductions that *exactly* match a full pass-through could be a useful selling point in marketing campaigns and thereby further elevate the probability that consumers will react (see e.g. Chetty et al. 2009, Finkelstein 2009, and Chetty et al. 2014 for empirical evidence on salience in other settings).

Internal characteristics of firms. The characteristics of firms, beyond factors discussed above, are usually assumed to be of limited importance in the consumption-tax literature. But a number of recent studies have highlighted that very diverse sets of management practices varying in quality coexist in the same markets, despite being important for firm performance (e.g. Bloom and Van Reenen 2010, Bloom et al. 2013, and Drexler et al. 2014). The fact that management practices in general appear to vary widely across firms suggests that pricing strategies can differ between independent establishments and those that belong to chains or franchises in ways that are relevant for the pass-through of VAT reductions. Most notably, chain establishments (in our case, restaurants) are more likely to have employees that are specialized in price setting, which may also be relevant for theories of discrete pricing options discussed above. In line with previous research, our results suggest that pricing decisions are coordinated across restaurants within chains (see Table 6). Furthermore, to the extent that chains are more resilient to variability in revenues due to, for example, better access to financial markets, they may also be more willing to experiment with elaborate but uncertain pricing strategies, as in

[35] During the VAT reductions, we observed anecdotally that chains advertised that they had lowered their prices with the exact full pass-through. At least in these cases, where the full pass-through was used as a marketing tool, it seems likely to assume that they needed to do exactly this, and that in this case full pass-through was a focal point.

the multi-armed bandit models.[36]

As indicated by results in Table 6, the price-change correlation appears to be low among neighboring restaurants. This suggests that also cross-demand elasticities are low in the restaurant industry, a feature which seems as a necessary prerequisite for different (types of) restaurants to display different types of pricing behavior (in contrast, intense competition with high cross-price elasticities would have forced restaurants to follow similar pricing strategies).

4.5.1 Our overall interpretation

Providing a comprehensive formal theory explaining all of our results simultaneously is beyond the scope of this paper. But our overall interpretation is that the relationship between our results and several of the theories discussed in this section provide insights into the likely nature of the mechanisms. Menu costs, sparsely optimizing consumers and/or reduced choice sets of firms are likely explanations of the apparent discrete nature of price-change distributions since the response distributions are obviously too extreme to be explained by standard (smooth) supply and demand models. Differing internal characteristics of the firms appear to provide a necessary element in order to explain the fundamental differences between independents and chains in pricing activity, reflected in VAT pass-through and in other key pricing dimensions (round prices and currency responses). These differences may arise because of managerial practices and/or resilience to uncertain outcomes.

A possible conjecture, although we cannot claim it to be uniquely consistent with the data, is that our results arise because firms act in an environment where consumers respond more forcefully to large salient price changes. Firms can potentially benefit from timing price changes relative to major events such as tax changes (and currency conversions) if these alter the salience of price changes. Using an exact full pass-through when taxes are reduced can be a possible strategy to generate salient price reduc-

[36] The basic idea that the decision problems of independent agents are different from those of employees is formalized in Lazear (2004, 2005), where entrepreneurial firms are run by agents who need to attend to multiple, sometimes complicated, tasks and thus need to be generalists ("jacks of all trades").

tions because the message is easily communicated in marketing campaigns and it may be possible to increase prices during currency conversions with limited consumer responses. But since major events are exceptionally rare, firms may be unable to foresee the extent of consumer responses. Hence, going down to the focal point of full pass-through and then slowly moving back up again (as chains do in our data) is a possibly rewarding, but also risky, strategy whereas remaining at the old price can be a safer option which may suit the independents better if these are more averse to price experimentation.

5 Conclusions

The literature on efficient consumption taxes has paid little attention to the role of internal characteristics of firms since Diamond and Mirrlees (1971), except in some rare cases, in particular when discussing tax compliance. Moreover, the previous literature on tax incidence has been heavily focused on cases where the incidence depends on elasticities of demand and supply, and the degree of competition, assuming that differences in price incidence across settings only arise because of heterogeneous consumers. In this paper we have documented that different types of firms respond very differently to consumption-tax reforms even if operating in very similar market segments.

Our results from two restaurant VAT rate reductions in Finland and Sweden show that the overall immediate pass-through pattern was bi-modal. Many meal prices remained constant in the short run and others were reduced by the exact amount corresponding to a full pass-through. In contrast, the price-change distributions in our control settings, restaurants in neighboring countries and alcohol prices within tax reform countries, are smooth around a spike at zero. Differences between the price setting of independent restaurants and restaurants belonging to chains is the key explanation for the bi-modal price-change distribution. Almost all of the independent restaurants kept their prices constant and thus effectively ignored the reforms whereas a substantial fraction of restaurants belonging to chains or franchises reduced their prices to a full pass-through during the reforms. We show that independents and chains operate in very similar market segments, as indicated by price levels and types of meals they serve, which suggests that they should face reasonably similar demand elasticities. Furthermore, accounting for very detailed indicators of market segments such as price, location and restaurant category does not explain the differences between independent and chains. Instead, the differences we observe in VAT pass-through appear to arise because independent restaurants are considerably less active in their pricing decisions in all dimensions.

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Appendix A: Data collection method

Our data are from a price collection method which was originally developed to analyze the effects of the VAT cut on restaurant-meal prices in Finland in 2010. The original idea was to use Estonian restaurant-meal prices as a comparison group for Finnish restaurant-meal prices. We then repeated the exercise for the Swedish reform from the beginning of 2012, but in this case used Finnish restaurants as the control group for Swedish restaurants.

We took random samples of restaurants (based on industrial classification) from tax registers of countries in the treatment and control groups before the reforms. In particular, we took random samples from Finnish and Estonian registers for the Finnish reform in April 2010, and from Swedish and Finnish registers for the Swedish reform in October 2011. We did not stratify the sample by any characteristics (such as geography) and thus, took a pure random sample from the tax registers in different countries. Using the stratified method would have been impossible for Estonia at the time of the Finnish reform due to the lack of reliable information based on which to stratify, and to have similarly constructed samples from different countries, we did not stratify any of the samples.

We collected prices from approximately 750 restaurants in Finland and 400 in Estonia around the Finnish reform and 700 from both Finland and Sweden around the Swedish reform. From each reform we collected meal prices 1-2 months before the reforms as well as 1-2 months, 3-6 months and 15-18 months after the reforms. In the collection, the sources of price observations were mainly from the web pages of restaurants. If web pages (with meal prices on them) were not available (approximately in 30% of restaurants), we collected the prices by calling the restaurant. For the prices that were collected from web pages, we saved the date of the last update to the website to make sure that websites were actively used and that the prices on them represented the current actual meal prices. To further make sure that web pages show valid prices for meals, after collecting prices from the internet the price collectors visited a small number of restaurants in the city centers of Helsinki and Stockholm to check that the prices in the menus are the same as those posted in the web pages.

In the initial collection, the exact name of the meals and the prices were recorded. In the consecutive collection rounds the prices of the same name-matching meals were collected, provided that these were still available on the menu. If the menu of the restaurant was changed between collection rounds and there was no meal with exactly the same name, we collected the price of the most similar meal in the new menu determined by the price collector. We also saved a dummy for each of changed meals to be able to examine if this affects the pass-through rates. In addition, we saved a separate dummy if the whole menu of a restaurant was changed between collection rounds. In approximately 15% of cases we could not reach a restaurant, and were not able to collect the prices. The most common reason for this was that the restaurant did not operate anymore. This is natural as the tax register information that we used to construct the sample was from the past, and not always perfectly up to date.

Restaurants were divided into four categories: à la carte, fast food, cafeteria (including pubs) and lunch restaurants. The price collection instructions were slightly altered depending on the category of restaurant. For example, from an à la carte restaurant we collected the prices of appetizers, main courses and desserts. Instead, we surveyed a smaller set of meals from cafes (e.g. soup of the day, a cake and coffee latte), because they usually offer more limited menus. We wanted to collect at least the most common meal served by each restaurant, and this was determined by the price collector (research assistant) based on the rules that were developed to ease the decision. For each restaurant category we developed a somewhat different rule for which types of prices to collect. For example, from a fish food restaurant that falls into à la carte category, we collected the first fish meal on the menu. In the case of Indian restaurant typical meals we collected are "Chicken Tikka Masala" or "Lamb Curry". For fast food restaurants or cafes, we typically collected the prices of hamburger, pizzas or kebabs. For lunch restaurants, the typical meal is "the lunch of the day" or the lunch buffet.

We attempted to collect a minimum of 7 and a maximum of 11 meals and drinks from each restaurant category, but we could not always find enough suitable items to collect. Thus the lowest number of items per restaurant we were able to collect is 3.

Importantly, while examining the restaurant from different sources, we also collected several restaurant characteristics for each restaurant; the geographical location of a restaurant, whether or not the restaurant belongs to a chain, whether or not located in a mall or shopping street, and in some cases the information about the chain the restaurant belongs to.

Furthermore, we linked tax register data to our price sample using unique firm identifiers. These data include the monthly amounts of VAT remittances, wage sums and organizational forms of restaurants.

Appendix B: Additional tables and figures

Table B1: Probability of short-run price changes by restaurant type

	(1)	(2)	(3)
	Finnish reform	Swedish reform	Pooled reforms
	Outcome: 1 if $\triangle$p>0.5%, 0 otherwise		
Treatment	0.647***	0.118	0.383***
	(0.180)	(0.087)	(0.139)
Independent	-0.023	-0.045	-0.087
	(0.032)	(0.068)	(0.077)
Independent	-0.522***	-0.162*	-0.335**
*Treatment	(0.169)	(0.088)	(0.130)
N	5,287	5,048	10,335
R^2	0.388	0.027	0.169
Treatment effect	0.125	-0.044	0.048
for independents	(0.169)	(0.124)	(0.191)

Note: Regression results for the probability of meal price changes after VAT reductions in Finland and Sweden by restaurant types. The outcome is 1 if a restaurant has changed a meal price by more than 0.5%, and zero otherwise. Treatment effect for independents is calculated as sum of estimates for Treatment and Independents*Treatment. Wild bootstrapped standard errors with one digit zip-code-level clusters in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table B2: Pass-through when using alcohol prices as an alternative control group

	(1)	(2)	(3)	(4)
VARIABLES	Country	Alcohol	Country	Alcohol
Treatment	-0.268***	-0.232***	-0.553***	-0.488***
	(0.098)	(0.076)	(0.192)	(0.158)
Independent			-0.089*	-0.041
			(0.049)	(0.086)
Independent			0.453***	0.405**
*Treatment			(0.138)	(0.171)
N	10,335	6,326	10,335	6,326
R^2	0.032	0.008	0.065	0.057

Note: Dependent variable is Δ of equation (1). Wild bootstrapped standard errors with one digit zip-code-level clusters in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table B3: Medium-run price responses by type (pass-through)

	(1)	(2)	(3)
	Pass-through	Pass-through	Pass-through
Panel A: 1-2 months after			
Treated Independent	0.364***	0.332***	0.321***
	(0.132)	(0.121)	(0.121)
N	5,762	5,762	5,762
R^2	0.055	0.088	0.101
Panel B: 3-6 months after			
Treated Independent	0.289***	0.265**	0.258**
	(0.094)	(0.106)	(0.100)
N	4,943	4,943	4,943
R^2	0.027	0.044	0.058
Panel C: 15-18 months after			
Treated Independent	0.111	0.157**	0.100
	(0.090)	(0.071)	(0.074)
N	4,196	4,196	4,196
R^2	0.001	0.026	0.046
Rest class * reform		x	x
Meal type * reform		x	x
Price Q * reform			x
ZIP fe			x

Note: Dependent variable is Δ of equation (1). Wild bootstrapped standard errors with one digit zip-code-level clusters in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table B4: Short-run pass-through by type in different samples

	Located in mall		By pre-reform price quantiles			
	Yes	No	1	2	3	4
Treatment	-0.754***	-0.497***	-0.775***	-0.553***	-0.438**	-0.508***
	(0.244)	(0.181)	(0.250)	(0.179)	(0.197)	(0.176)
Independent	-0.074	-0.088	-0.049	-0.121*	-0.059	-0.133*
	(0.071)	(0.059)	(0.064)	(0.065)	(0.045)	(0.068)
Independent *	0.634***	0.396***	0.682***	0.430***	0.368**	0.375***
Treat	(0.116)	(0.144)	(0.138)	(0.083)	(0.150)	(0.146)
N	1,198	9,137	2,566	2,182	2,681	2,906
R^2	0.154	0.049	0.157	0.070	0.035	0.052
	Confederation		By restaurant type			
	Yes	No	Fast food	À la carte	Cafe	Lunch
Treatment	-0.664***	-0.407**	-0.685**	-0.410**	-0.610***	-0.856***
	(0.215)	(0.192)	(0.330)	(0.170)	(0.197)	(0.276)
Independent	-0.113	-0.045	-0.059	-0.108**	0.044	-0.193
	(0.074)	(0.040)	(0.067)	(0.049)	(0.064)	(0.161)
Independent *	0.493***	0.323*	0.644**	0.332**	0.430***	0.550***
Treat	(0.089)	(0.193)	(0.326)	(0.140)	(0.149)	(0.177)
N	3,314	7,021	2,410	5,772	1,005	1,148
R^2	0.122	0.028	0.137	0.030	0.088	0.165
	Price collection method		Meal exits			
	Internet	Phone	0	> 0		
Treatment	-0.586***	-0.554***	-0.495***	-0.908**		
	(0.190)	(0.202)	(0.180)	(0.352)		
Independent	-0.111***	0.001	-0.064*	-0.314		
	(0.036)	(0.090)	(0.034)	(0.235)		
Independent *	0.473***	0.446**	0.412***	0.736***		
Treat	(0.084)	(0.181)	(0.150)	(0.286)		
N	7,306	3,029	8,619	1,716		
R^2	0.086	0.021	0.060	0.089		

Note: Dependent variable is Δ of equation (1). Wild bootstrapped standard errors with one digit zip-code-level clusters in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table B5: Short-run pass-through by restaurant density and franchise status

	Restaurant density quantile			
Sample: All	1	2	3	4
Treatment	-0.075*	-0.135***	-0.091**	-0.094*
	(0.043)	(0.038)	(0.039)	(0.053)
Independent	0.220	0.123***	0.097***	0.023
	(0.168)	(0.046)	(0.038)	(0.027)
Independent *	-0.394**	-0.352***	-0.517***	-0.762***
Treat	(0.180)	(0.065)	(0.066)	(0.083)
N	1,772	2,764	3,063	2,736
R^2	0.024	0.039	0.104	0.117
Sample: Only chains	Franchises			
Franchise	-0.173***			
	(0.064)			
Franchise: Franchise owned		-0.318***		
		(0.072)		
Franchise: Company owned		0.068		
		(0.115)		
Constant	-0.394***	-0.394***		
	(0.028)	(0.029)		
N	2,118	2,118		
R^2	0.007	0.016		

Note: Dependent variable is Δ of equation (1). Wild bootstrapped standard errors with firm-level clusters in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table B6: Results from administrative data comparing chains and independents

	(1)	(2)	(3)	(4)	(5)
	△Log Inputs	△Log VAT	△Log C. price	△Log P*Q	△Q proxy
After	0.006	-0.226***	-0.031***	-0.027	-0.018
	(0.023)	(0.024)	(0.008)	(0.019)	(0.022)
After*	-0.008	-0.006	0.020**	0.019	0.006
Independent	(0.025)	(0.020)	(0.009)	(0.018)	(0.017)
N	8,049	7,986	8,442	7,986	7,986
R^2	0.000	0.163	0.012	0.005	0.003
no. restaurants	1,204	1,191	1,244	1,191	1,191

Note: Regression results for treated restaurants (as in Figures (8) and (9)) using data one year before and after the reforms (after=1 if 1 year after the reforms and zero otherwise). In column (1) inputs refer to quarterly inputs that are credited against VAT, and in column (2) VAT refers to the quarterly remitted VAT. Column (3) shows the average percentage changes in consumer prices, and column (4) depicts the average percentage changes in VAT-inclusive revenue. In column (5), the quantity of traded meals is calculated by dividing the VAT-inclusive revenue by the VAT-inclusive meal price (consumer price) for each restaurant within the price sample. In order to take into account the huge variation in tax-inclusive revenue (P*Q) in the administrative data, the revenue is smoothed by controlling with the revenue of exactly one year before for each firm. Also, due to the high variation in quarterly sales and inputs, we have restricted the data by excluding observations with more than a 100% change in annual sales. In addition, data includes only those restaurants from which we were able to collect prices. Wild bootstrapped standard errors with one digit zip-code-level clusters in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table B7: Round-number pricing:

	(1)	(2)	(3)	(4)
	Round	Round	Round	Round
Independent	0.292***	0.293***	0.299***	0.266***
	(0.020)	(0.020)	(0.028)	(0.030)
Right after	-0.028	-0.028	-0.031	-0.028
	(0.025)	(0.022)	(0.020)	(0.018)
3-6 months after	-0.077**	-0.076**	-0.078**	-0.075**
	(0.038)	(0.036)	(0.036)	(0.035)
15-18 months after	-0.037**	-0.036**	-0.040**	-0.035*
	(0.015)	(0.015)	(0.016)	(0.021)
Rest class (ref. fast food)				
À la carte			-0.258**	-0.053
			(0.128)	(0.102)
Cafe			-0.185	-0.163
			(0.153)	(0.166)
Lunch			-0.064	0.029
			(0.072)	(0.062)
Price quartile: ref. smallest				
2				-0.217***
				(0.079)
3				-0.240***
				(0.072)
4				-0.269***
				(0.092)
Constant	0.248***	0.074	0.063	0.593***
	(0.000)	(0.067)	(0.119)	(0.000)
N	19,892	19,892	19,892	19,892
R^2	0.080	0.088	0.125	0.182
Price splines (10)		x	x	x
Rest class * reform			x	x
Meal type * reform			x	x
Price Q * reform				x
ZIP fe				x

Note: Regression results from the model where a dummy indicator of round-number price is the outcome. The main variable of interest is the independent variable measuring to what extent independent restaurants use round-number prices more often than chain restaurants. Subsequent columns introduce more covariates shown in the Table. Wild bootstrapped standard errors with one digit zip-code-level clusters in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table B8: Probability of price changes before, during, and after Estonian currency change by type: Estonian restaurants

	(1)	(2)	(3)	(4)	(5)
	Outcome: 1 if △p>0.5%, 0 otherwise				
4 months before	0.045***	0.045***	0.045***	0.045***	0.045***
	(0.017)	(0.016)	(0.016)	(0.016)	(0.016)
3 months after	0.665***	0.665***	0.665***	0.664***	0.665***
	(0.055)	(0.055)	(0.054)	(0.054)	(0.055)
15 months after	0.534***	0.534***	0.533***	0.533***	0.533***
	(0.063)	(0.063)	(0.062)	(0.061)	(0.062)
4 months before	-0.016	-0.016	-0.016	-0.015	-0.015
* Independent	(0.020)	(0.019)	(0.019)	(0.019)	(0.019)
3 months after	-0.167**	-0.167**	-0.167**	-0.167**	-0.167**
* Independent	(0.070)	(0.068)	(0.068)	(0.068)	(0.066)
15 months after	-0.015	-0.014	-0.015	-0.015	-0.014
* Independent	(0.039)	(0.036)	(0.037)	(0.035)	(0.035)
N	7,252	7,252	7,252	7,252	7,252
R^2	0.364	0.365	0.366	0.366	0.366
Meal type		x	x	x	x
Price Q			x	x	x
Rest class				x	x
Mall					x

Note: Regression results for the probability of price changes after Estonian currency change from kroon to euros from the beginning of 2011 by restaurant types. The outcome is 1 if a restaurant has changed a meal price by more than 0.5%, and otherwise zero. Results are from OLS models for different price collections, 4 months before, 3 months after and 15 months after the currency change. Wild bootstrapped standard errors with five digit zip-code clusters in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table B9: Results on meal exits by treatment and restaurant type

	(1)	(2)
VARIABLES	By second	By third
Treatment	0.103***	0.102***
	(0.040)	(0.033)
Independent	0.061	0.033
	(0.061)	(0.028)
Independent *Treatment	-0.004	0.034
	(0.050)	(0.031)
N	27,530	24,170
R^2	0.014	0.019

Note: Regression results for the probability of meal having exited the sample in the second or third collection round by treatment and restaurant types. The outcome is 1 if a meal price was not observed in the second or third collection round and zero otherwise. Results are from OLS models. Wild bootstrapped standard errors with one digit zip-code-clusters in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

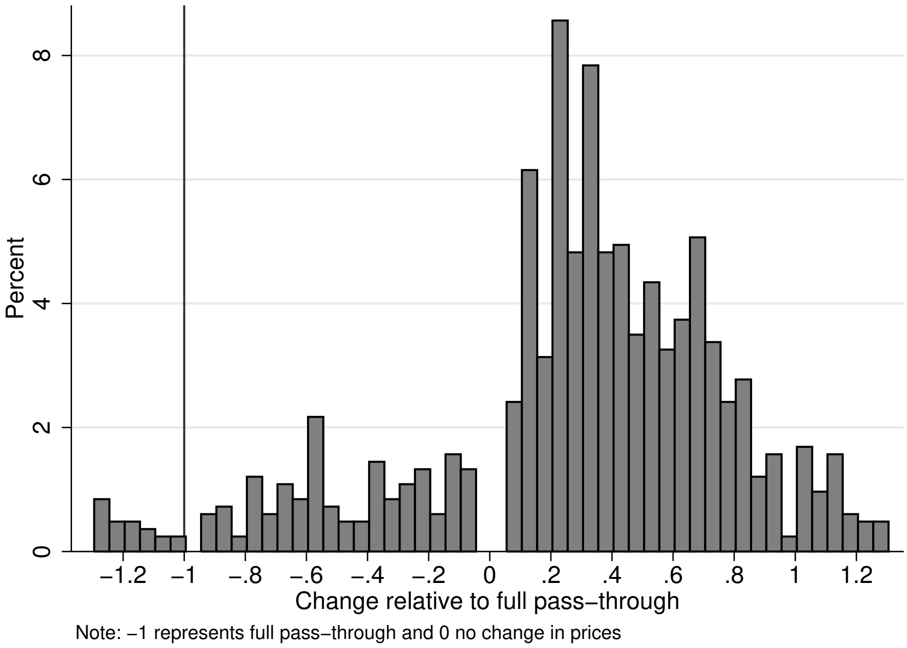

Figure B1: Meal price changes in control countries

Note: The Figure shows the distribution of changes in meal prices in control countries relative to the full pass through (-1 in the Figure). The Figure pools data from Estonia and Norway during the Finnish reform and from Finland and Sweden during the Swedish reform to get more observations and, hence, a smoother distribution. The Figure excludes unchanged prices that were changed by less than 0.5% that is 83.9 percent of the total price sample. See Figure B3, upper panel, for the full distribution for each control country. 2.7 percent of all (non-zero) price-changes were larger than the corresponding full price pass-through.

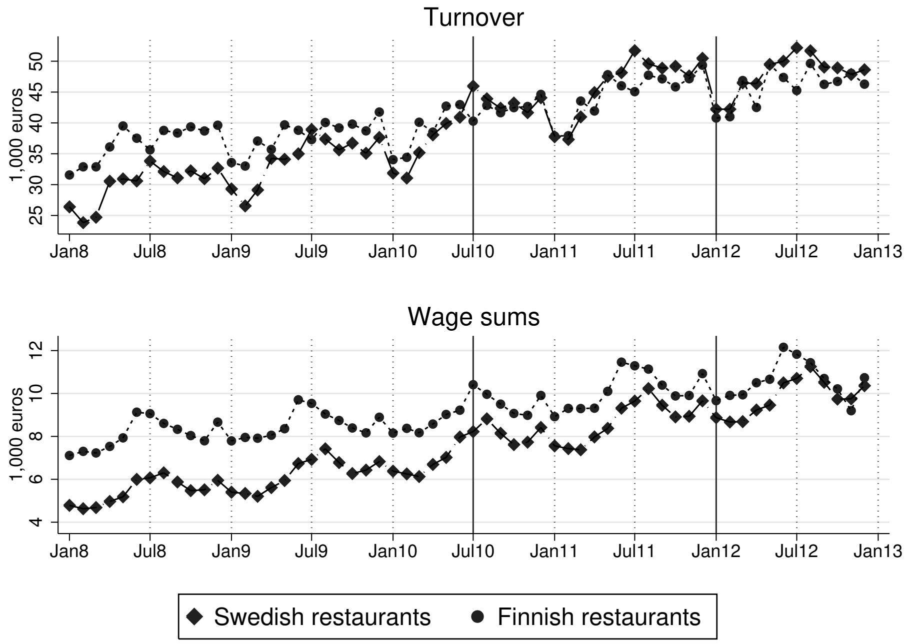

Figure B2: Longer-term development of average tax-inclusive turnover and wage sums of restaurants in Finland and Sweden

Note: Upper panel: Average monthly tax-inclusive turnover (sales). Lower panel: Wage sums paid to employees. All sums measured in thousands of euros. Vertical lines in the Figure refer to the VAT cuts for restaurants in Finland (July 2010) and in Sweden (January 2012).

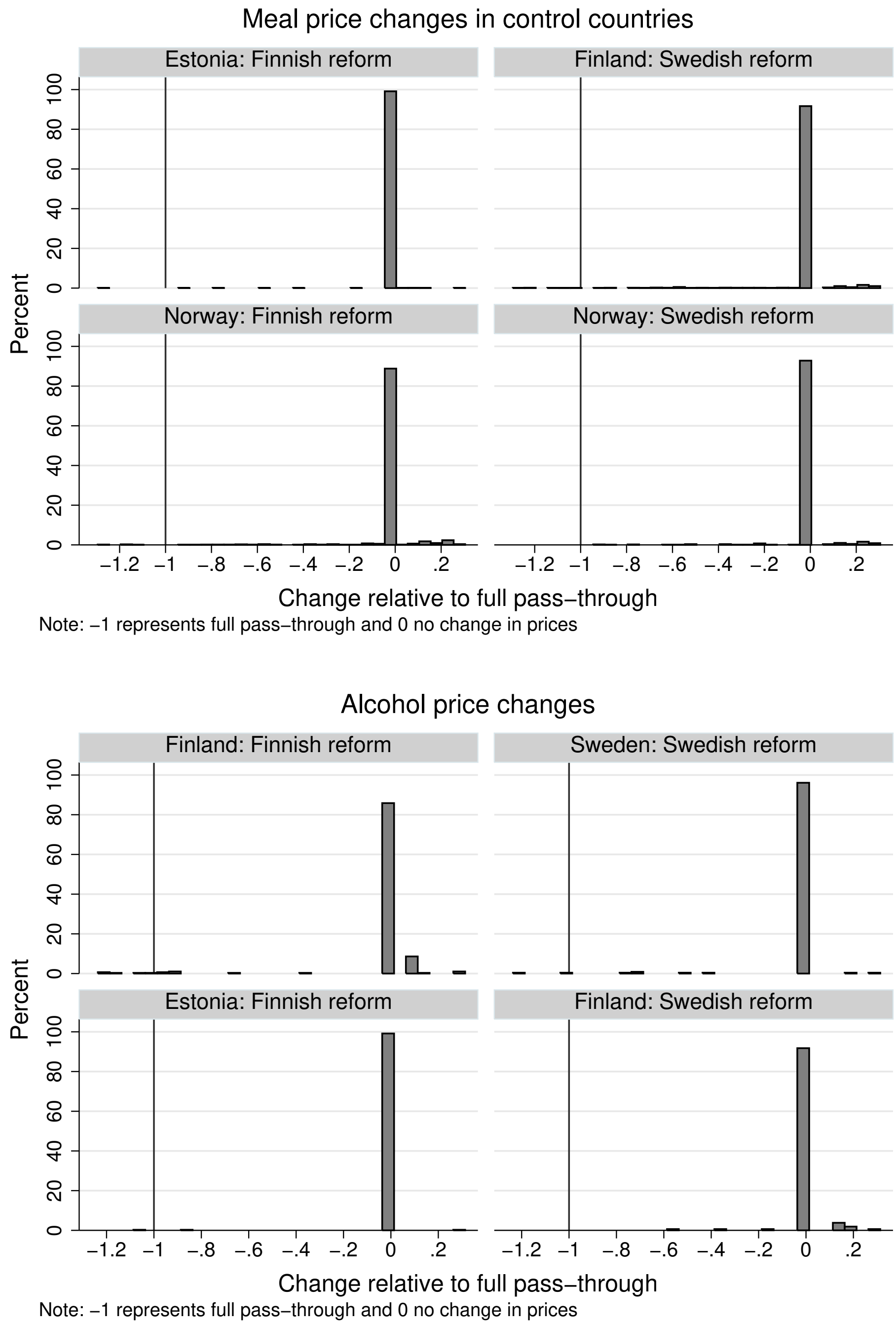

Figure B3: Alternative controls: Norway instead of Estonia/Finland, and alcohol prices within and across countries

Note: First row is the baseline. Second row is CPI data from Norway (not available separately for independents and chains), third row is alcohol prices within countries, final row is alcohol prices in the original control countries (Estonia/Finland).

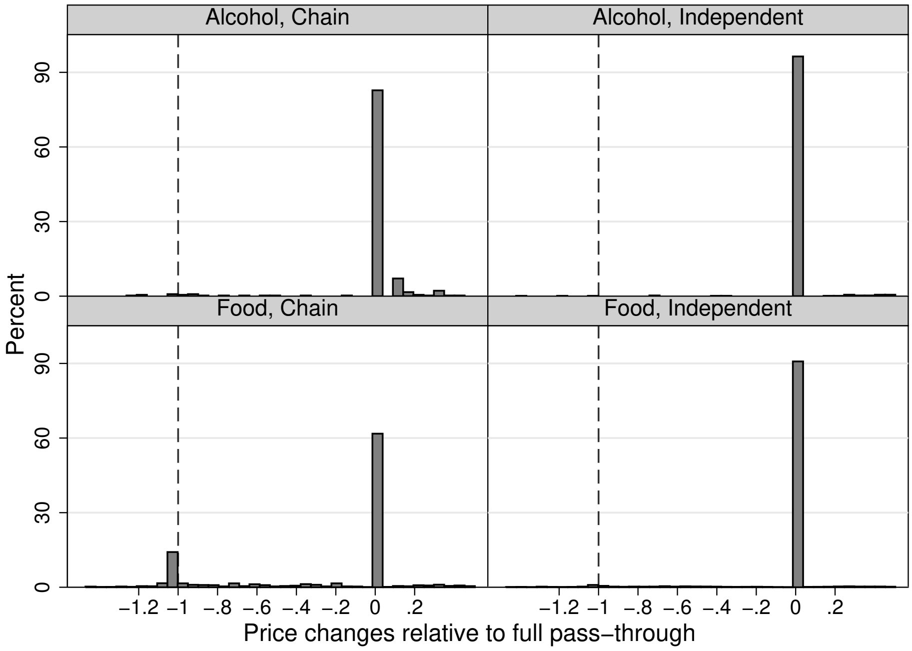

Figure B4: Short-run pass-through comparing meal to alcohol prices by type

Note: Alcohol and meal-level price changes from 1-2 months before to 1-2 months after reforms. Normalized; -1 is full pass-through.

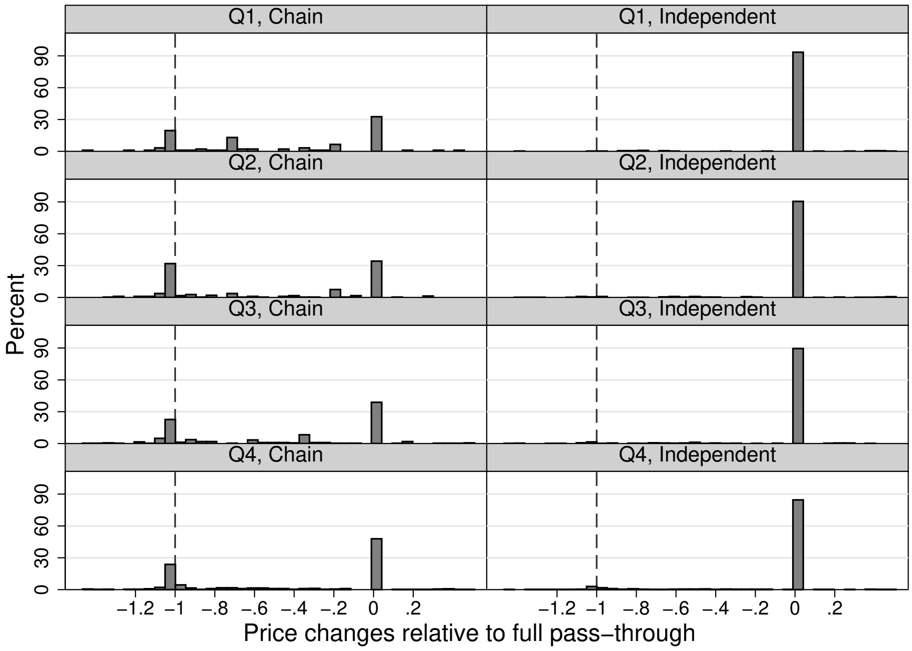

Figure B5: Short-run pass-through by price quartiles

Note: Price quartiles are calculated based on initial prices at the restaurant level.

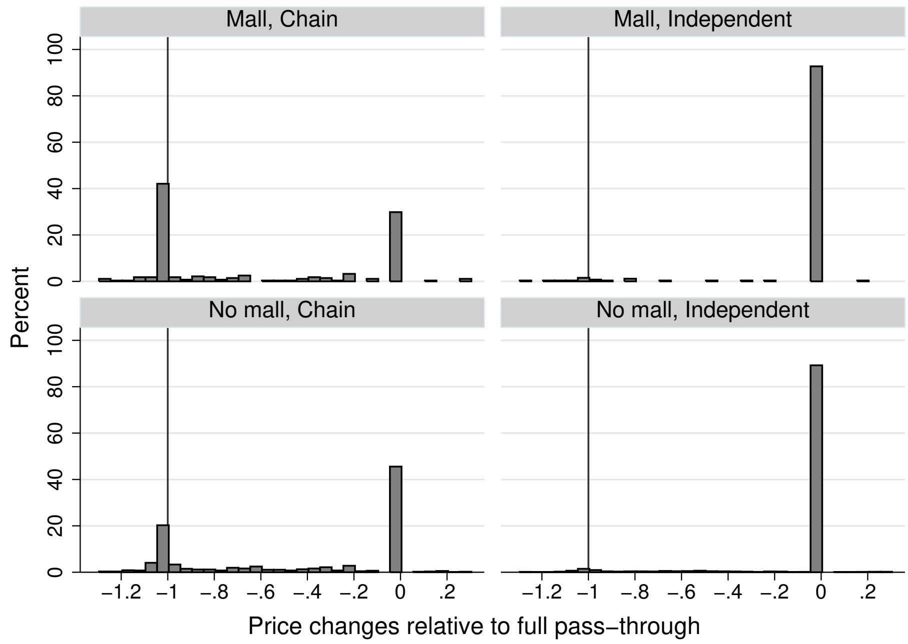

Figure B6: Short-run pass-through divided by restaurant type and restaurants located in malls

Note: Distributions of meal price changes by restaurant type and restaurants located in malls in the reforms relative to the full pass-through. Price changes are normalized so that -1 refers to the full pass-through in each reform and 0 refers to no change in prices.

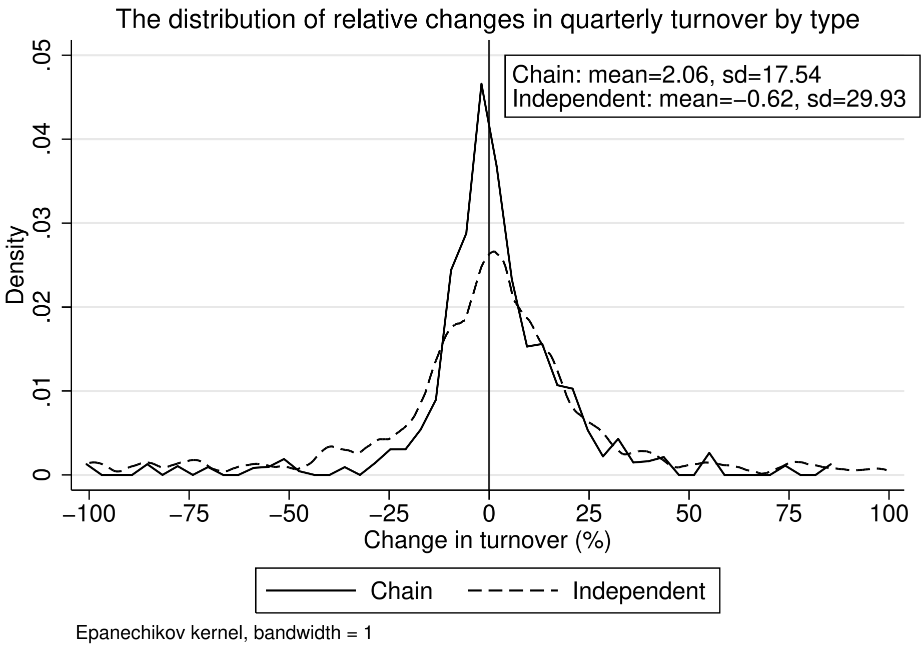

Figure B7: Changes in sales relative to own history

Note: Kernel densities of relative changes in quarterly sales for chain and independent restaurants. We calculate a relative change in sales for each firm from two quarters before and after the reforms. We restrict the changes to be between -100 and 100%. The bandwidth is 1%.

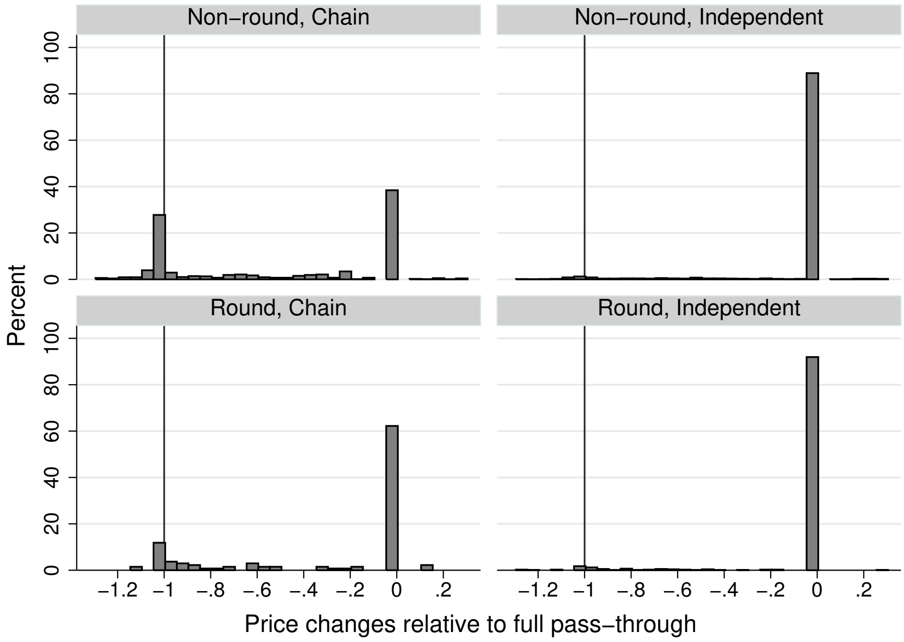

Figure B8: Short-run pass-through divided by restaurant type and round-number pricing

Note: Distributions of meal price changes by restaurant type and whether or not pre-reform price was round in the reforms relative to the full pass-through. Price changes are normalized so that -1 refers to the full pass-through in each reform and 0 refers to no change in prices.