Firm types, price-setting strategies, and consumption-tax incidence

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Abstract

We analyze price responses to large restaurant VAT rate reductions in two different European countries and show that price 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 cannot be explained by location, initial prices or other market-segment indicators such as meal or restaurant types. Further evidence on the use of round-number pricing, on price-change frequencies, and on pricing behavior during currency conversions suggests that the diverging price responses to consumption-tax reforms reflect fundamental differences in price-setting behavior between the two types of firms.

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 2016, and Rozema 2016) 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, Fullerton and Metcalf 2002 and Carbonnier 2014).[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).[2] 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. We further document that this non-smooth bimodal price-change distribution is intimately related to two distinct types of price-setting firms even when holding observed market conditions constant.

We analyze price responses to VAT-rate reductions in the restaurant industry in Finland (9pp reduction) and in 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 for a representative sample of restaurants. The price data allow us to follow the prices of the same meals over time, and thus we are able to examine the full distribution of price changes for different types of firms. To assess the importance of time effects, we use identical price data from neighboring countries.[3]

[1] Other aspects discussed in the literature include opportunities for 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).

[2] The limited role played by firm-level heterogeneity is a general phenomenon and a weakness within the public finance literature (Slemrod and Gillitzer, 2014).

[3] In much of the analysis, we focus on across-firm heterogeneity, and for this analysis our identification strategy only requires that the difference between restaurant types would have evolved similarly in the neighboring countries in the absence of the VAT

The results show strikingly clean price-change patterns. The average short-run price response is only a quarter of full pass-through, defined as unchanged producer prices. This small response is in line with recent studies on consumption taxes in the service sector (see e.g. Carbonnier 2007, Kosonen 2015 and Benzarti and Carloni 2016). The distributions of price responses uncover a pattern of fundamental heterogeneity. On one side, the majority of prices were completely unchanged a few months after the reduced tax rates were implemented. Such pricing inactivity is not part of standard public finance predictions (or standard explanations for a low pass-through), and even traditional menu cost models such as Golosov and Lucas's (2007) would struggle to explain why prices remain rigid when firms face such a large cost shock.[4] 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 present when we study price changes under a fixed VAT rate. Instead, the price-change distributions in the control countries have a spike at zero but otherwise display a continuous set of actual price changes. The same is true for alcohol prices (which were exempted from the VAT reductions) within the treated restaurants.

The fact that prices responded with a full pass-through or not at all suggests a fundamental heterogeneity across products, submarkets or firms. Our rich micro-level data is exceptionally well suited for an analysis of such heterogeneity. We collected detailed information on the types of restaurants, the types of meals and the locations of the restaurants. Some of these features are quite standard, but when collecting the data we also conjectured that internal aspects of the price-setting firms may be important. We therefore collected data on whether the restaurants are independent operations (referred to as *Independents*) or belong to a chain or franchise (*Chains*).[5] There is by now mounting evidence suggesting substantial het-

reforms. For robustness, we show that we get similar results when using within-country restaurants' alcohol prices (not subject to VAT changes) as the control.

[4]See Klenow and Malin (2010) for a review of studies documenting the frequency of price changes and Carlsson and Skans (2012) for a recent study. Nakamura and Steinson find evidence suggesting that the price-change frequency responds forcefully to cost changes when analyzing data from a large retail firm. Carlsson (2014) finds the opposite when using data from manufacturing plants.

[5]As we show, the restaurant sector is very well-suited for an analysis of firm-side het-

erogeneity across firms in management practices and strategies (see Bloom and Van Reenen 2010, Bloom et al. 2013, and Drexler et al. 2014), and we believed that this heterogeneity should expand into the price-setting decisions.[6] The basic idea is that the dichotomy between independents and chains should be a strong predictor of different price-setting practices since larger operations are able to put more resources into the pricing decisions if there are fixed costs of setting (or adjusting) prices. In addition, recent work on discrete pricing spaces (see e.g. Gabaix 2014 and Matějka 2016), as we observe here, are based on the notion that agents rationally choose to reduce the complexity of decision making. This may be particularly relevant for price setting within independent operations.

Our results show that the firm-type distinction is indeed crucial for understanding the distribution of price adjustments: The short-run impact was zero for virtually all restaurants in the group of independent restaurants, whereas a substantial fraction of chain restaurants instead chose a full pass-through. The explanatory power of the dummy separating independents and chains in regressions on the short-run reaction is much larger than the *combined* impact of a large set of variables capturing various 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 indicators of rigidities or seriousness of the firm (belonging to employer organization, a dummy for changed items on the menu). Notably, we find no evidence of non-zero spikes in the price-change distributions for any of the two types of restaurants within the control countries or for our alternative control, alcohol prices. Thus our results on firm types is insensitive to the choice of control group.

We further show that most 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.[7] In

erogeneity in this dimension since equally sized establishments that are located side-by-side can be either single-unit businesses or parts of company-owned chains or franchises.

[6]The only public finance result in this direction that we are aware of is Kosonen (2015), who found that firm size matters for tax responses among barbers.

[7]Those that did change their prices displayed a smooth distribution centered around

a low-inflation environment (less than 2 percent/year) this implies that the pass-through among independents remained very low for the full follow-up period. As a consequence of the continued inactivity of the independents and the relative-price increases among chains, the average pass-through was *reduced* over time. Notably, this is very different from the standard text-book argument (building on Adam Smith) that price responses should increase over time due to, for example, capital adjustments and new market entrants.

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 overlapping. Thus, the bulk of the difference in price responses survives when controlling for the location, initial price, the type of restaurant and the type of meal. 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.[8] 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 supply inelastic, both of which seem to be *a priori* unlikely explanations, and the joint analysis of our price data and the administrative tax data further suggests that the demand for restaurant services is quite inelastic.

Our favored interpretation is instead that the results emerge because the independents and chains have very different price-setting strategies and that these strategies are important for the price response to the VAT changes. As supporting evidence, we document that the price-setting

the initial price.

[8]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.

strategies of chains and independents differ in several related dimensions. Chains are more likely to change their prices in times when VAT rates are fixed. Moreover, we show that large chains coordinate their price responses between sites,[9] and that independents are much more likely to use round-number prices. We interpret this as fairly direct evidence of simplified pricing strategies following Levy et al. (2011).[10] Finally, we note that results in Cavallo et al. (2015) suggest that strategically price-setting firms can use currency conversions to raise prices more than otherwise in order to minimize customer responses. We show that chain restaurants, but not independents, had an abnormal frequency of price increases during the Estonian conversion to the euro, a result that suggests that chains are more strategic. An important reason for why these strategies may have a first-order impact on the pass-through, is that the average price response induced by the VAT reduction is very large compared to the range of price changes that restaurants make when tax rates are fixed. Thus, there is likely to be considerable uncertainty regarding the outcomes of strategies involving such large price reductions, in particular for those that rarely change their prices at all.

Overall, our results suggest that the (average) short- and medium-run price response to consumption taxes cannot be fully understood without accounting for firm-level heterogeneity, thus supporting the Slemrod and Gillitzer (2014) argument that this is an important area for future developments within the public finance literature.[11] Notably, 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.[12]

Thus a viable way forward for the public finance literature is to intro-

[9]In contrast, price responses are not coordinated between restaurants from different chains that share a location.

[10]The chains that do use round prices respond much less to the reforms than other chain restaurants, but still more than independents.

[11]This view is in sharp contrast to, for example, the traditional optimal taxation view of Diamond and Mirrlees (1971) that the literature should focus on the demand side and ignore firms entirely, because profits are taxed away and/or all supply curves should be treated as perfectly elastic.

[12]We contribute to this literature by showing that prices are correlated within local areas through similar firms self-selecting there, but that firms still do not react to each others' tax-induced price changes.

duce micro-foundations for price-setting decisions that allow for firm-level heterogeneity. Our results provide several pieces of evidence regarding elements that should be covered by such micro-foundations. The fact that the price-change distributions of chains have a "missing middle" suggests that price changes are either associated with substantial fixed costs, or that perceived benefits from price reductions are strongly increasing in the size of the adjustments.[13] This suggests that firms use discrete pricing strategies. The clear differences between chains and independents further suggest that firms of different types differ in the nature of this discreteness. The lower frequency of price changes (also when taxes are fixed), the lower price responsiveness to changes in costs (VAT), the lower probability of taking account of price-change opportunities (currency conversions) and the more frequent use of simplified prices (round numbers) all suggest that independents are more rigid in their behavior whereas chains use more responsive, although not necessarily smoother, pricing practices. This suggests that the more specialized price setters within chains may optimize over a larger set of possible action spaces and/or be more likely to exploit perceptions about non-linear consumer responses such as in the Gabaix (2014) model. Our results suggest that introducing micro-foundations with these features may be useful when trying to explain the variations in pass-through found in previous studies (see e.g. Cabral et al. 2015, Carbonnier 2007, Gruber and Koszegi 2004, Kenkel 2005, Kosonen 2015, Benedek et al. 2015, Benzarti and Carloni 2016, and Rozema 2016) and provide a better foundation for analyzing the welfare consequences of consumption taxes.

The paper is structured as follows: Section 2 briefly reviews the relevant theory. Section 3 presents institutions, data and methods. Section 4 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 6 concludes the study.

[13] Dharmapala et al. (2011) discuss the role of fixed costs for the "missing middle" in firm-size distributions. Gabaix (2014) provides a pricing model where consumers are inattentive to small adjustments.

2 Standard consumption-tax models, and some extensions

This section first gives some key elements of standard consumption-tax models. As alluded to in the introduction, the extreme cases of zero and full pass-through and the role of firm heterogeneity will be important components in our analysis. Hence, we present the conditions under which standard theories can explain these phenomena before presenting elements outside of the standard consumption-tax literature that we believe adds insights in these dimensions.

2.1 Standard consumption-tax models

A key result arising from very basic economic theory is that the pass-through of taxes depends on how markets work. In the simplest, perfect competition and a single good case, using p to denote consumer prices and introducing a specific tax t we get the standard pass-through formula:

$$\frac{dp}{dt} = \frac{\epsilon_S}{\epsilon_S + \epsilon_D} = \frac{1}{1 + \frac{\epsilon_D}{\epsilon_S}},$$

where $-\epsilon_D$ is the demand elasticity and ϵ_S the supply elasticity. Thus, 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.[14]

The literature naturally contains many extensions to this very simple tax-incidence model. The Harberger (1962) general equilibrium model allows for multiple goods and factor markets. Other extensions include various ways of accommodating imperfect competition among firms (Myles 1989, Weyl and Fabinger, 2013). Imperfect competition could lead to a pass-through that is greater than or smaller than it would be in perfect competition models. The direction depends on the form of interaction between firms and the consumers reactions captured, for example, by the

[14]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.

curvature of the demand function.

However, the standard tools used for analyzing consumption taxes have in common that they suggest that responses to VAT reductions should reach zero or full pass-through only under extreme assumptions about market structures and/or supply curves. Furthermore, the models do not allow for any explicit heterogeneity due to internal characteristics (beyond the supply curves) between firms operating in the same market. Jointly, this implies that the firm-level price responses in most cases should be smoothly distributed around an average pass-through

2.2 Price stickiness and discrete pricing strategies

Although the public finance literature provides few explanations for the existence of discrete pricing responses, there is other literature on price setting that can be of more use in this dimension. In the macro-inspired literature on micro-level price dynamics (see e.g. Klenow and Malin 2010, for a survey), it has been noted that price-change distributions tend to contain large spikes at zero. A standard theoretical rationale for this pattern is the assumption of fixed costs for changing prices ("menu costs", as in e.g. Golosov and Lucas 2007).[15] These costs should lead to sticky prices when costs are relatively stable but, importantly, firms should pay the menu cost and adjust their prices when changes in costs are large relative to the menu cost. Thus, menu costs need to be substantial in order to motivate inaction in times of large tax changes.

Although menu cost models can explain a zero pass-through in the short run if the costs are very large, they do not offer predictions of any other mass-points, such as a full pass-through. However, in another set of models, agents are instead assumed to reduce the complexity of their pricing problem by optimizing over a discrete set of predetermined pricing options while trying to improve the 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,

[15] 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.

the logic could be applied to a discrete set of possible reactions where a full and zero pass-through are two natural focal points.

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) formalizes this idea and proposes that customers choose to optimize over a reduced set of possible choice variables (a "sparse" matrix). This can lead to downward price rigidities and force firms to use large price reductions once they do 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 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).

2.3 Heterogeneity and the characteristics of price-setting agents

As should be evident from the discussion in section 2.1, internal characteristics of firms, beyond cost and productivity structures summarized in the supply curve, 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 of different qualities 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 firms' pricing strategies, and hence responses to tax reforms, can in fact depend on the characteristics of the price-setting agent.

In particular, it seems reasonable to assume that price-setting strategies differ between independent establishments and those that belong to chains or franchises. The reason is that chain establishments (in our case, restaurants) are more likely to have employees that are specialized in price setting, which may affect the pricing responses in several ways, in particular related to the theories of discrete pricing ranges discussed above. The larger scope

within chains may reduce the rigidities created by fixed menu costs. In addition, increased specialization may allow chains to pursue more elaborate pricing strategies in various dimensions: Price setters within chains may optimize over a larger set of possible action spaces and/or be more likely to exploit perceptions about non-linear consumer responses as in the Gabaix (2014) model discussed above. 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 pricing strategies if the outcome of such experimentation is uncertain as in the multi-armed bandit models.[16]

3 Reforms and data

3.1 The reforms

All countries within the EU use value added taxation (VAT) for consumption taxes. EU regulations stipulate the use of one standard VAT rate and, at most, two reduced 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, meals eaten off the restaurant premises ("takeaway") were already being taxed at the reduced rate before the reforms. The VAT rate for alcohol remained at the original standard rate after the reform. In both countries the changes in VAT legislation were passed relatively close to the reform, which makes large pre-reform anticipatory effects unlikely.

[16]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").

3.2 Measuring the pass-through

Although the tax is labeled a value added tax, the effects of goods-specific changes in the VAT on producing firms (here, restaurants) are symmetrical to effects due to changes in sales taxes. The reason is that the tax formula calculates the tax on sales and the crediting of inputs separately. Hence, the crediting of inputs remains unchanged when the VAT on produced goods is reduced.

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 byτ^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. Hence, Δ is the relative price change observed on the price tags, scaled as fractions of the full pass-through.

When interpreting our analysis of these tax changes, it is important to note that they are sizable. Changes in VAT rates are normally within a single percentage point. The kind of VAT reclassifications that we study here, and which result in substantial VAT rate variations, are quite rare. The reductions are also sizable if contrasted with normal price variations. In Appendix B, Figure 13, we show that a full pass-through would lie outside of the whole distribution of price changes that we observe when studying price changes during a fixed VAT rate. Thus, it is likely that firms face a lot of uncertainty regarding customer responses if they choose

a large pass-through.

3.3 Outline of the empirical approach

Our basic empirical approach is to 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. As will be shown below, however, the choice of control group is not crucial for our main conclusions (in particular, regarding all short-run results) since the price-change patterns of the control groups are stable (as expected) and the patterns of the treated groups are very distinct from those of the control group.

Our main strategy is to use Estonia as the contrast for the Finnish reform, and Finland as 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). In alternative robustness exercises, we use restaurant alcohol prices within the treated countries (and restaurants) as an alternative control. The obvious drawback of using alcohol prices is potential spillover effects between the treated and control services and we therefore focus on the cross-country controls.[17] An analysis of the average impact of the reform thus relies on the standard differences-in-differences (DD) assumption, that is, 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 countries as controls mimics that of the vast number of state-level DD studies conducted in a US setting since Card and Krueger (1994). As with neighboring states in the US, Finland and Sweden have very similar institutions, geography, and climate; they share a border and have similar cultures, seasonal holidays, vacation

[17]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.

periods and seasonality in national food production.[18] In fact, Finland and Sweden were a single country for several hundred years until 1809, a period when many important institutions were formed. During the sample period, all three countries were covered by the same EU regulations concerning, among other things, VAT legislation.

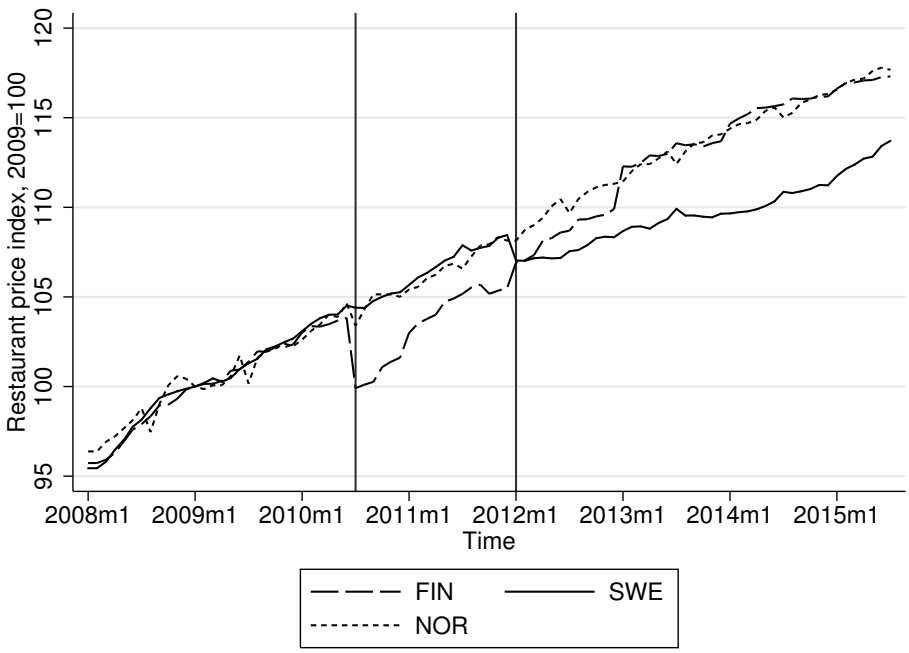

Figure 1: CPI component of restaurant-meal prices in Finland, Sweden and Norway

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.

In our main analysis, we rely on data we collected on our own, starting just before the reform. The reason is that standard CPI data in the treated countries cover very few (less than 60 in Sweden) restaurants per survey round. Hence, they cannot be used for an analysis of price-response

[18]In both countries (as in Estonia) Christmas and the New Year are celebrated in a similar manner and bank holidays are of similar length and on the same dates.

heterogeneity. Our own data contain a richness (and sample size) that is unavailable in standard CPI collections of prices, but they do not cover a very long pre-reform period since the reforms were announced shortly before implementation.

To check whether the basic idea of using neighboring countries to assess the importance of time effects is reasonable, however, we start by illustrating the evolution of the restaurant-meal component of the CPI in a set of neighboring countries.[19] 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. In the analysis, in Appendix B, Figure 12, we use price-change distribution of Norwegian restaurant meals and alcohol sold in restaurants as alternative control groups for the overall prices changes during the reforms. The purpose is to show that price-change distributions are indeed rather stable over short time horizons in the absence of large changes in VAT rates.

3.4 Data

We collected prices directly from the restaurants using our own price collection protocol (Appendix A). We first drew a random sample of restaurants in Sweden, Finland and Estonia from national tax registers. The registers contain all firms liable to taxation in these countries, listed by their primary industry. By using the national tax registers as the base for our random sample, we linked our survey data on turnover, profits, the number of employees and the total wage bill to firm-level registers at an annual frequency.

Prices were collected by separate teams of research assistants within

[19]Figure 11 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.

each country. Our first choice was to collect the prices from restaurant web pages. Most, but not all, of the restaurants had a website that included prices for meals. If no such website was found, we contacted the restaurant by phone. This procedure allowed us to collect prices and other information from a fairly large number of restaurants across a large geographic area based on a random sampling frame.[20]

For each round, we collected prices and meal information for 7 to 11 meals as well as information on alcohol prices (having unchanged VAT) at each restaurant from a defined protocol. Depending on the type of restaurant, we collected prices of starters, main courses, vegetarian meals, pre-set lunches and so forth (see Appendix A for details). The assistants chose the exact meals within each category with the intention of following them over time. Since we planned to follow the exact meals across time, it did not matter exactly which meals the assistants chose within each category. Along with the prices, we also recorded other information about the restaurant, including its type and surroundings, for exampe, whether it was located in a mall or on a restaurant-dense street.

In the case of the Finnish reform, we collected the pre-reform data in May and June 2010 and the short-run incidence data in July and August 2010. The counterfactual for Finland was chosen to be Estonia (at that stage we were, for obvious reasons, not aware that there would be a reform in Sweden a few years later). For the Swedish reform, our pre-reform survey was run in October and November 2011 and the short-run incidence survey in February and March 2012. In the Swedish case, we used Finland as the control country under the assumption that the Finnish responses had leveled out at that time, at least at the relatively high frequency that we are analyzing the data. Empirically this seems to be the case. Although our main analysis focuses on the short-run responses, we repeated the survey half a year and a year and half later, which enables us to examine medium-term price effects.

[20]This procedure bears some resemblance to the survey method presented in Kenkel (2005), where the aim was to study the pass-through of alcohol taxes before and after an excise tax increase in Alaska. One significant difference is that in the majority of cases we were able to rely on prices on the web pages, while Kenkel (2005) relied on a phone survey.

3.4.1 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.

3.5 Descriptive statistics

Table 1 gives descriptive statistics for our data. As the distinction between independents and chains is the main theme of our analysis, we show the statistics separately by restaurant type. Almost two-thirds of the data consist of independent restaurants. Most other characteristics are, perhaps surprisingly, similar. 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 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.

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*.

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 we rely on differences-in-differences (DD) and therefore do not require that the price levels be identical before the reforms, we find it reassuring that the distri-

butions in treatment and control countries are similar before the reforms. Importantly, the initial distributions are also very similar across our two restaurant types, suggesting that the types of restaurants are competing in roughly similar market segments. In the empirical analysis, we account for any remaining differences in pre-reform prices.

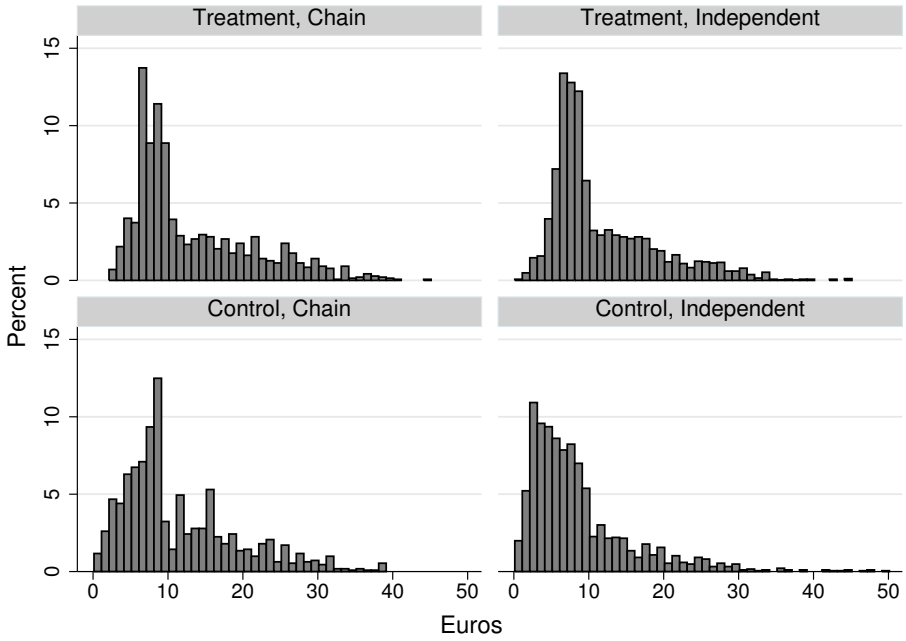

Figure 2: Pre-reform prices by treatment status and type
Note: Data from our own price collections. All prices are converted to euros.

3.6 Regression equations and inference

We will analyze the data in two ways. The first is to show the distributions of price changes. As it turns out, this approach, which explores the full richness of our data, will show all our main results very clearly. The second type of analysis relies on estimating statistical models relying on the differences-in-differences (DD) logic. These models allow us to provide standard errors and test the robustness of the raw differences to the inclusion of a rich set of control variables.

Throughout, we use the pass-through $\triangle$ (see equation 1) as the dependent variable to get the scales comparable across the reforms.

We first estimate a model capturing the overall pass-though. 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.[21] Note that the first-difference form for the outcome removes all unobserved meal-specific constant factors as in a "meal fixed effects" model.

In much of our analysis, we let the impact vary between independent restaurants and chains as defined above. When estimating how the impact varies across these two groups, we estimate:

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

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.

For both of these models 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.

A standard concern in DD settings is that the error term (u_{ijr} and ε_{ijr} respectively) may be correlated within groups (see e.g. Bertrand et al. 2004). To verify that such concerns are not distorting our inference, we apply the block-bootstrap method designed for settings with few clusters;

[21] In the majority of analyses the treatment and control groups are defined by the country in which a restaurant is located, but we also show results from a specification where the groups are defined by type of item (meal versus alcohol).

see Cameron et al. (2008) for a further discussion. We cluster at the level of identifying information, that is, reform times treatment status in equation (2) times independent-dummy level in equation (3).[22] By pooling the two reforms, we thus not only provide an analysis with greater external validity but also ensure that we have a large-enough number of clusters to provide meaningful inference. Results separately by reform are presented in Appendix B.

4 Main results

In this section we show our empirical results. We start by showing the overall impact of the reforms on the short-run price-change distributions. We then turn to the analysis where we distinguish between independent restaurants and chains. We end the section by discussing the medium-term impact of the reforms.

4.1 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 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.[23] The large spikes at zero in both groups indicate that many prices did not 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

[22] However, in parts of the analysis, we rely on models with very high-dimensional fixed effects, and this prevents us from using the block bootstrap method at this level of aggregation. In these cases, we instead use zip-code clusters. To facilitate comparison, we (also) report zip-code clustered standard errors in the cases where the block bootstrap works.

[23] 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.

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. To further build confidence that price-change distribution features a large spike at zero in the absence of large VAT changes, we show the price-change distributions at the time of the two reforms for two alternative control groups, Norwegian restaurant meals and alcohol sold in restaurants, in Appendix B, Figure 12. These price-change distributions are very similar to the price-change distribution for our main control group.

Table 2 quantifies the average short-run price responses using the DD-strategy of equation 2. Column (1) is without any controls, and the estimate suggests an impact of 27 percent of full pass-through. Reassuringly, including very detailed controls (column 2) capturing the significance of restaurant class, meal type and initial price quartile has only a marginal impact (increase) on the estimate of interest.[24]

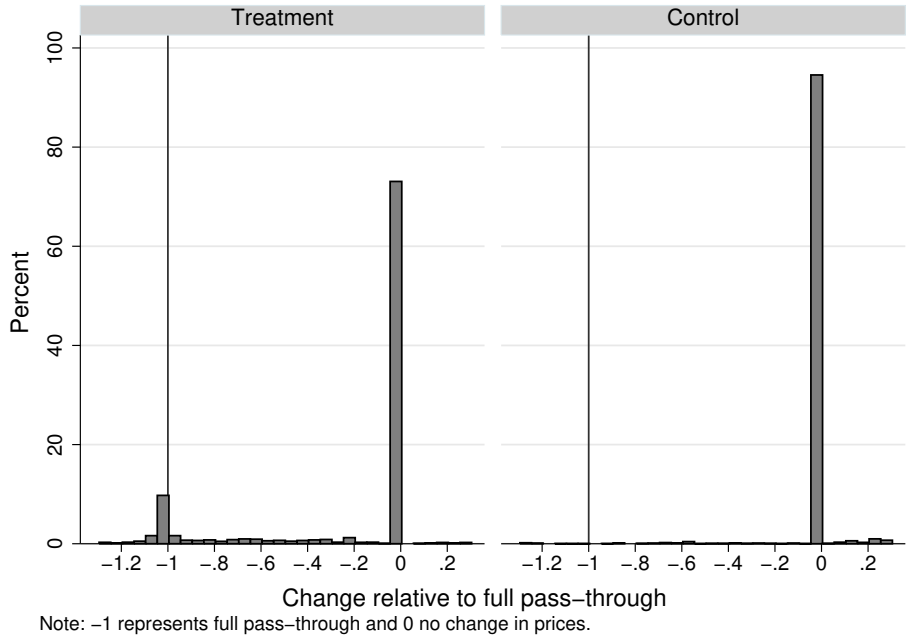

Figure 3: Distribution of price changes in the two reforms

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

[24]Table 9 in Appendix B shows the results separately for the two reforms.

Table 2: Average short-run pass-through

	(1)	(2)
	Pass-through	Pass-through
Treatment	-0.268***	-0.326***
	(0.035)	(0.112)
	[0.110]	-
N	10,335	10,335
R^2	0.032	0.047
Rest class * treat		x
Meal type * treat		x
Price Q * treat		x

Note: Dependent variable is Δ of equation (1). Block bootstrapped standard errors with zip-code-level clusters in parentheses and eight clusters (reform times country times type) in square brackets. The latter cannot be computed for the final column. *** p<0.01, ** p<0.05, * p<0.1.

4.2 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. We first study the predictive power of the restaurant type variable compared to other key variables in Table 3. The Table compares the statistical relevance of the Independent-dummy in comparison to other key variables. The results show that the partial R2 for the independent dummy in a price-change equation within the treated group is larger than the partial R2 of a *combined* set of variables capturing the market segment of the restaurant (four restaurant class dummies [fast-food, à la carte, café or lunch 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). We return to the importance of these measures below.

Table 3: Partial coefficients and R-squared values divided by explanatory 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 (column (3)). These variables include all variables presented in Table 1 that are also used in our baseline analysis. In addition, they include a dummy for restaurants that have changed some of their meals (one or more) in the menu between the first and second collection rounds (meal exit), a dummy for restaurants belonging to a confederation that represents the hospitality industry (confederation), and a dummy for restaurants using round meal prices (one or more) in their menu (round before price).

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 the restaurant types. 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 reductions in VAT rates. Thus, the dichotomy between independents and chains is a key predictor for where the treated restaurants end up within the bimodal price-change distribution shown in Figure 3.

Notably, the prices do not change at all for most restaurants within the control group (not covered by the reform), which is expected given previous research on short-run price dynamics (e.g. Klenow and Malin, 2010). However, chains are in fact more likely to change their prices (mostly upwards, for natural reasons) than independents when we only analyze control-group restaurants. This suggests that independents have a less adaptive pricing strategy even in normal (non-reform) times. Importantly, however, the price-change distribution in the control group is unimodal, that is, there is no spike at all corresponding to the full pass-through spike we saw in the treatment distributions.

To verify the robustness, we have replicated the graphs using alcohol prices (within the same restaurants), which were unaffected by the VAT change, as an alternative control group. This produces very similar results. In particular, changes in alcohol prices do not show any evidence of a two-spiked distribution (see Figure 14 in Appendix B). As a complementary exercise, we have also analyzed the probability of meal-level exits. The results (Appendix B, Table 14), show no signs of differential responses on the meal-exit margin between independents and chains.

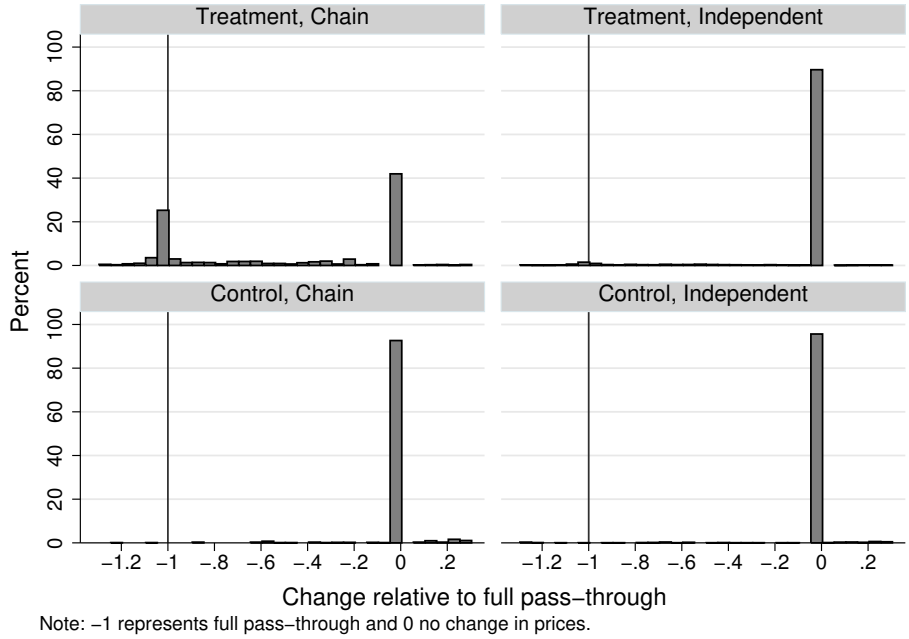

Figure 4: Short-run pass-through, by treatment status and type
Note: Meal-level price changes from 1-2 months before to 1-2 months after reforms. Normalized; -1 is full pass-through.

To quantify the short-run price changes due to the reforms, and to be able to investigate the potentially confounding role of market characteristics, we estimate the DD regressions implied by equation (3). Table 4 presents the results. In column (1), we show the estimated average impact of the reforms, while accounting for a dummy for independents, and the effects remain at around -0.27 as in Table 2 above. In column (2) we interact the treatment dummy with the dummy for independent restaurants. Now, the treatment variable captures the impact for chains, which is estimated to be -0.55. Importantly, the difference from independents is large (0.45) and statistically significant. The implied estimate for the impact of the reform on the independent restaurants is close to zero (-0.09) and statistically insignificant when block bootstrapping the standard errors with eight clusters.

A reasonable *a priori* hypothesis for the observed difference in meal price changes between independents and chains is that they operate in dif-

ferent 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.[25] 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 (3).[26] We then add (initial) price quartile dummies interacted with treatment status and zip-code fixed effects in column (4). 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 is marginally reduced (from 0.45 to 0.34) when adding the very large set of covariates, the main thrust of the difference also remains in these very tight specifications.

It is important to note that the results displayed in Figure 4 only show a bimodal distribution of price changes in the treatment group. Thus, since all the action lies in the reform part of the sample, it seems quite unlikely that our results (at least qualitatively) are driven by the particular choice of control group. However, to verify this interpretation in the regression framework, we have also estimated the regression models using alcohol prices as a within-country control (since alcohol VAT remained unchanged) and, unsurprisingly, the results are very similar to the main results (see Appendix B, Table 7).

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

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

Table 4: Short-run pass-through by type

	(1)	(2)	(3)	(4)
	Pass-through	Pass-through	Pass-through	Pass-through
Treatment	-0.272***	-0.553***	-0.551***	-0.552***
	(0.038)	(0.065)	(0.099)	(0.071)
	[0.172]	[0.206]	[0.208]	-
Independent	0.161***	-0.089***	-0.066**	-0.074**
	(0.046)	(0.031)	(0.033)	(0.029)
	[0.189]	[0.067]	[0.224]	-
Independent		0.453***	0.423***	0.352***
*Treatment		(0.066)	(0.061)	(0.045)
		[0.212]	[0.283]	-
N	10,335	10,335	10,335	10,335
R^2	0.043	0.065	0.074	0.129
Rest class * treat			x	x
Meal type * treat			x	x
Col method * treat			x	x
Price Q * treat				x
ZIP fe				x

Note: Dependent variable is Δ of equation (1). Zip-code areas are merged together whenever there are fewer than 60 observations in one area. Block bootstrapped standard errors with zip-code-level clusters in parentheses and eight clusters (reform times country times type) in square brackets. The latter cannot be computed for the final column.*** p<0.01, ** p<0.05, * p<0.1.

As an additional test, we have analyzed the responses separately by initial price quartile. The results are displayed in Appendix B, Figure 15. The difference between independents and chains remains remarkably similar across the distribution.[27]

Overall, we interpret these results as suggesting that neither location, restaurant category, nor price segments can explain why independent restaurants respond so differently from restaurants belonging to chains. In particular, it seems highly unlikely that similar restaurants that are located close to each other and serving meals with similar prices before the reforms,

[27]The main deviation is that the graph indicates that the pass-through is highest for chains operating in the lowest price segment.

should face completely different demand elasticities. Furthermore, to explain the zero pass-through for independent restaurants with conventional models, the demand for meals served by the independent restaurants needs to be perfectly elastic, which seems implausible.

4.3 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 as shown in Table 14 in Appendix B, the frequency of exits does not differ between the treated chains and independents.

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 independent 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.

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 16 in Appendix B

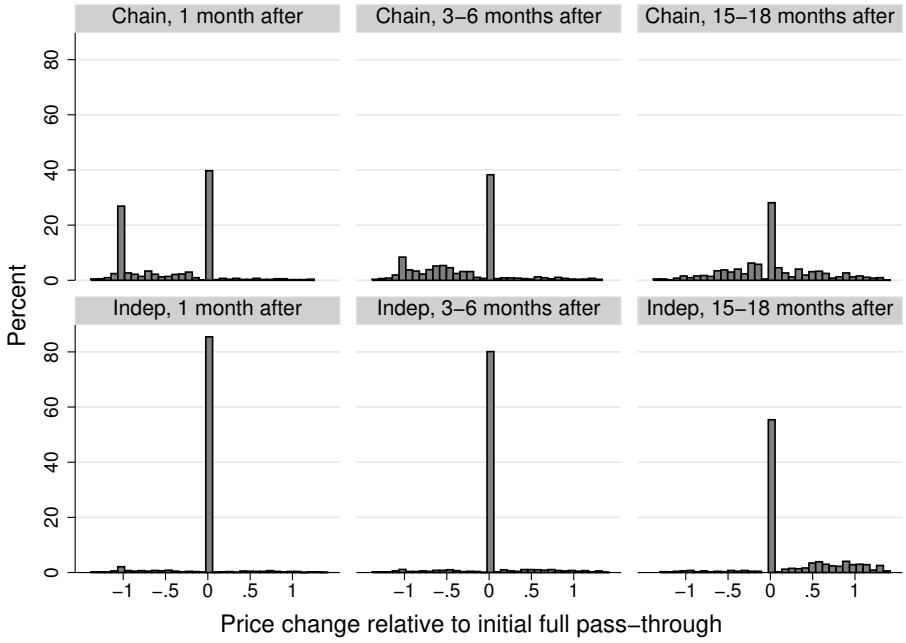

Figure 5: Medium-run pass-through, by treatment status and type
Note: Meal-level price changes from 1-2 months before to 1-2 months after, 3-6 months after and 15-18 months after reforms. Normalized; -1 is full pass-through.

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

As expected, the short-run estimates mimic the results presented above. 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 differences between chains and independents started to decline by the third collection 3-6 months after the reform. Their average price responses are converging after 15-18 months, at which time the estimated difference is considerably smaller (0.1) and statistically insignificant.

Table 8 in Appendix B shows a similar picture regarding differences between independents and chains based on models that only use data from the treated group. Thus, 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.

5 Mechanisms and pricing strategies

5.1 Restaurant density and price-change coordination

As we discuss in section 3, standard theory focuses on the degree of market competition as the key explanation for differences in pass-through. To further 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 6. 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.[28]

[28]In Appendix B, Figure 17, we show results for restaurants located in malls. Consistent with the results in Figure 6, chain restaurants in malls respond more heavily than other chains, but independent restaurants ignore the reforms regardless of location.

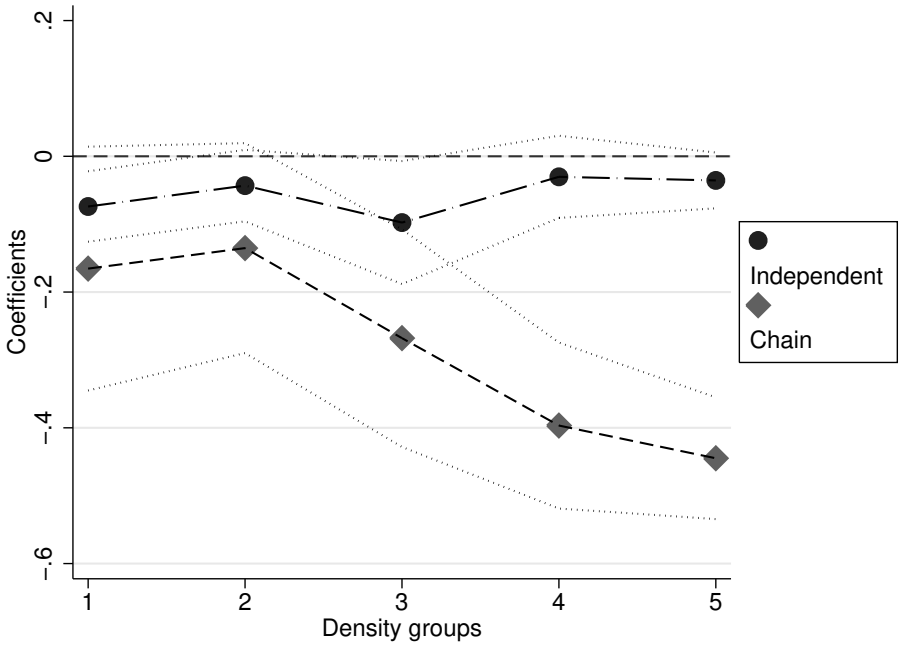

Figure 6: Pass-through according to restaurant density

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.

We have also analyzed restaurants located in specific restaurant-dense locations in the major cities within our data. Starting from zip codes in the cities of Helsinki, Tampere, Turku, Stockholm, Gothenburg, and Malmö, we divided these zip codes into smaller areas consisting of a few blocks each. Using this area code we created a variable indicating the average pass-through among *other* restaurants of the same type (independent or chain) in the same area. We then proceed in the spirit of price-coordination studies (see e.g. Houde 2012 and Thomadsen 2005) and analyze how correlated price changes are across restaurants within the same area to see how many cross-restaurant interactions there are in the responses to the VAT reforms.[29]

Column (1) of Table 6 shows the main DD estimate for this more limited sample. Column (2) presents the estimated price-response coordination across restaurants within the same area. Surprisingly, the point estimate

[29]We also calculated the average pass-through of other restaurants of the same type in some other randomly chosen area, to serve as a contrast. The randomly matched contrast comes in close to zero and is insignificant in the regressions.

for the coordination parameter is negative, but statistically insignificant (and, unfortunately, not very precisely estimated). The absence of a positive estimate implies that restaurants do not seem to change their prices as a response to the behavior of neighboring restaurants. Combining this result with the density result presented in Figure 6 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.

Further results in Table 6 show how price responses are coordinated within chains (column 3) and within restaurants (column 4). The evidence suggests substantial coordination (0.7 and 0.5 respectively) in both these dimensions. We interpret the fact that chains appear to coordinate their price responses at least as much across their different restaurants as the typical restaurant coordinates its prices within the restaurant as strongly supporting the notion of coordinated chain-level pricing strategies.[30]

5.2 Heterogeneity (within the chain category)

Next, we investigate which types of chains were the most responsive to the reforms in the short run. We do this by repeating the specification in equation (3) for different sub-samples. For completeness, we continue to include the independents in the models, but as we know that virtually none of them reacted, all differences between sub-samples will arise because of differences between sub-samples of chains.

We summarize the results briefly here; for details see Appendix B, Table 10. The pass-through is larger in malls, in the lowest price quartile, among chains belonging to the national restaurant confederation, among lunch restaurants, followed by fast food and cafeterias, and last, dinner (à la carte) restaurants. 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).

Finally, 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.

[30]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).

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 subsamples. Thus, the added response among chains that had more flexible menus cannot explain the difference between chains and independents. The result has two implications. First, models relying on fixed costs of changing anything on the menu, as argued by Midrigan (2011), would predict that firms that changed at least one meal could reset any price on the menu without friction, and that does seem to be the case to some extent. Second, if our data were compromised by outdated web pages, we should see larger differences when page updates were confirmed through meal changes. The results, if anything, point in the opposite direction.

5.3 Reported quantities and inputs: evidence from tax registers

Next, we complement the pass-through analysis by investigating how inputs and outputs change with the reform for the two types of firms. We utilize administrative data originating from tax authorities in the analysis.

Figure 7, 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. In order to take into account the huge variation in sales and inputs in the administrative data, we have restricted the data by excluding observations with more than a 100 percent change in annual sales.[31]

For expositional reasons we normalize the series at zero four quarters before the reforms in Figure 7. A key result from the Figure is that 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 raw food materials are a significant part of inputs in the restaurant industry, changes in meal quality are difficult to achieve without adjusting the input costs. Since the inputs develop similarly for the two

[31] In addition, the data include only the surveyed restaurants since we need the survey to identify the chains. These data restrictions also apply to Figure 8 and Table 11, presented in Appendix B.

types of restaurants, quality responses appear to be an unlikely explanation for the observed differences in price pass-through.

Although not included in standard consumption-tax models, *tax evasion* could potentially affect the incidence of consumption taxes. In the extreme, if all consumption taxes are evaded, changes in consumption taxes would for obvious reasons not affect prices. 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). An important factor is thus whether or not firms remit VAT prior to consumption-tax reforms, and whether these remittances change with the reforms. As long as covered firms do remit VAT, real costs for earning income imply that changes in the consumption tax rates will have an impact on firm-level decisions, including their prices (Slemrod and Yitzhaki 2002). As shown in Figure 7, both types of restaurants remitted VAT prior to the reforms, and the reduced VAT rates generated clear drops in remitted VAT for both groups. Thus, behavioral effects (increased sales or decreased tax evasion) are, as expected, too small to counter the negative mechanical effect of the reduced rates. 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 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 8, and in table format in Appendix B, Table 11. The first panel of Figure 8 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.[32] The

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

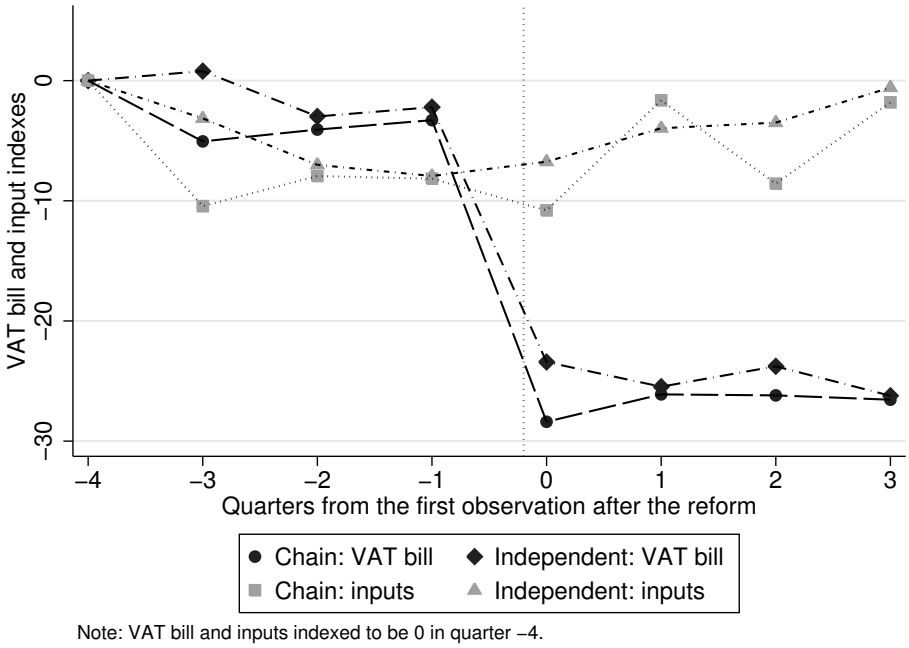

Figure 7: Inputs and VAT remittances, by quarter relative to the reform
Note: Coefficients of quarter indicators in a regression where the dependent variable is the log 4-quarter change in VAT bills and inputs credited against VAT by restaurant type. Based on administrative data for the surveyed firms. VAT bills and inputs are indexed to be zero at 4 quarters before the reforms. Dotted line marks the reform.

final panel shows the impact on the quantities, measured as revenues deflated by consumer prices. Clearly, we find no differences between chains and independents in terms of quantities as measured by our proxy for the number of traded meals. Here it should be acknowledged that the underlying estimates (as shown in Appendix B, Table 11) are imprecise since the firm-level revenue data are extremely volatile (as shown by Figure 18 in Appendix B) 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.[33] This suggests that their demand elasticity is low, and that the (chain) strategy of lowering prices was unsuccessful, at least if evaluated by the impact on short-term sales.

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

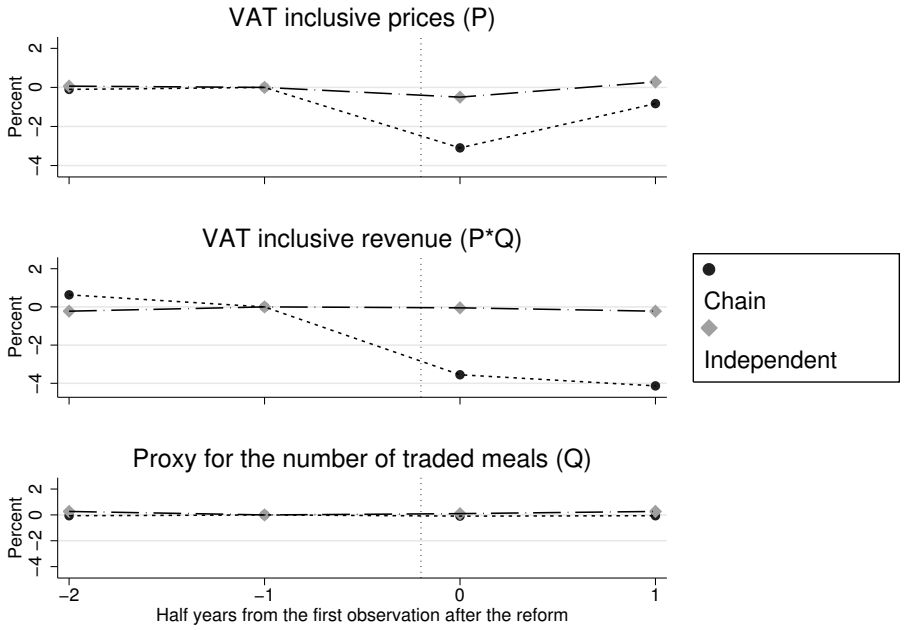

Figure 8: Changes in log consumer prices, VAT-inclusive revenue and quantity

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 line marks the reform.

5.4 Round-number pricing

One possible explanation for our main results is that independent restaurants have less precise pricing strategies and rely on crude price targets instead (see discussion in section 2.3). A first piece of suggestive evidence supporting this hypothesis is provided by the fact that independents appear to change prices less often in normal (non-reform) times as well; see results in section 4.2 above.

To provide more evidence on the hypothesis that the independent businesses respond less to the tax reforms because they use cruder pricing rules, we have analyzed the restaurants' use of round-number prices. A large lit-

erature analyzes the lack of round-number pricing as evidence of strategic price setting, see for example Levy et al. (2011) and references therein. Following this literature our hypothesis is that round-number prices are a reflection of a less detailed pricing strategy. We define a price as *round* if it takes an integer value in euros (in Finland) or 10 SEKs or 10 EEKs (in Sweden and Estonia), which are roughly comparable numbers accounting for exchange rates (all roughly comparable to integer values of USD).[34] Our main interest is in contrasting the incidence of round prices (e.g. a 9 euro lunch) to the frequency of close non-round prices (i.e. 8.90 or 9.10 euro lunches). Figure 9 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.

Additionally, we have quantified the difference in the probability of using round numbers in regressions in order to account for potential confounders such as the market segment or price range of the restaurant and the meal; detailed results are in Table 12 in Appendix B. Independent restaurants are 29 percentage points more likely to use round-number prices than chain restaurants, and the estimate remains stable and statistically significant when more covariates are added.[35]

We have also explored the price responsiveness to the VAT-reforms separately for (initially) round and non-round prices. The results (details in Appendix B, Figure 19) imply that the chains that do use round prices are substantially less likely to adjust their prices in response to the reforms than other chain restaurants, although chains with round prices are not quite as unresponsive as independent restaurants. Our interpretation of

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

[35]Restaurants with local competition have fewer round prices, and the same applies for restaurants belonging to the employer confederation and those that changed some of the content of their menu. Restaurants located in malls and fast-food restaurants appear to use more round prices for their meals.

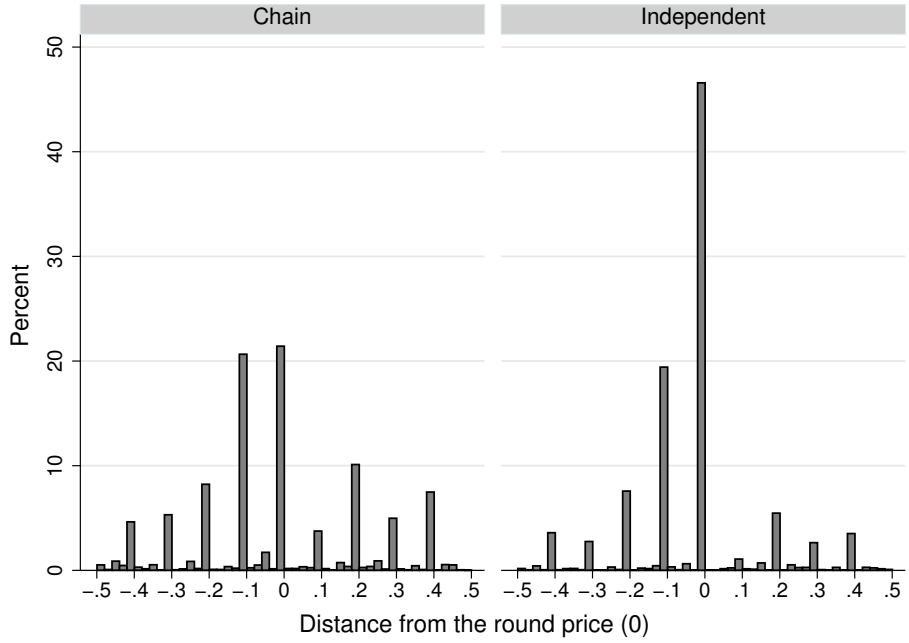

Figure 9: Round-number pricing by type

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.

these results is that they support the notion that round prices are indeed an indicator of inflexible pricing strategies, strategies which apparently are used more frequently among independents.

5.5 Price increases during currency conversions

Our main results show that a large share of the (mostly chain) restaurants that actually responded to the reforms did so by allowing for a full pass-through onto prices. This behavior is difficult to reconcile with standard tax-incidence theories, but it could be viewed as an outcome of strategic price-setting behavior if the chains believed that they would have received specific benefits by hitting the full pass-through mark. One such reason would be that they perceive their customers as being more responsive to large and visible price reductions than to small adjustments; see Gabaix

(2014) for a discussion on consumer attentiveness and price setting.[36]

In order to find external evidence on how strategic price-changing behavior may differ between independents and chains, we have analyzed the price responses to the currency conversion from the Estonian kroon (EEK) to the euro at the beginning of 2011. This is an interesting experiment since currency conversions are expected to leave marginal production costs unchanged, and only require a change of price tags. On the other hand, customers may find it difficult to keep track of the exact prices during the conversion. Thus, it potentially creates an opportunity for firms to strategically increase their prices without negative customer reactions. Chains should use this opportunity more than independent restaurants if the chains, as we believe, are (attempting to be) more strategic in their price-setting behavior.

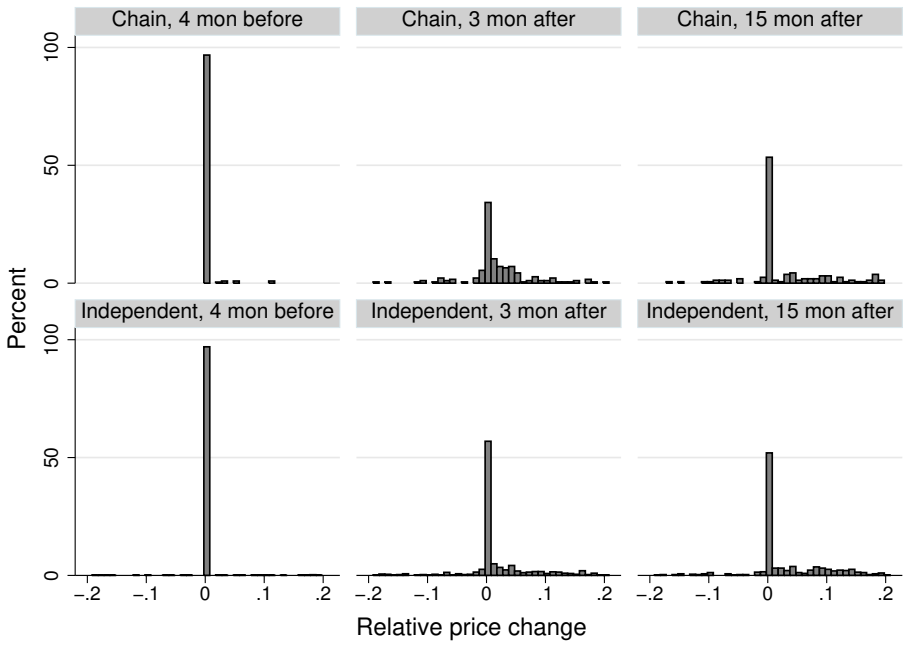

Figure 10: Relative price changes around Estonian currency conversion
Note: Meal price changes for Estonian restaurants before, during, and after Estonia joined the Eurozone.

The resulting relative price-change distributions are shown in Figure 10.

[36] Anecdotal evidence drawn from advertisements suggest that this may have been the case.

Each panel shows the relative price changes across two collection moments at different time intervals. The results show that restaurants belonging to chains (relative to independents) increased their prices more often just at the time of the currency conversion than in surrounding time periods. Regression analysis (details in Appendix B, Table 13) confirms the intuition of the Figure: independents were 17 percent less likely to change their prices during the currency change than chains. This result is robust to including additional control variables.

6 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. Instead, the previous literature on tax incidence has been heavily focused on cases where tax incidence depends on elasticities of demand and supply, and the degree of competition, assuming that differences in price incidence only matter because of heterogeneous consumers. In this paper we have documented that different types of firms respond very differently to consumption-tax reforms.

Our results from two restaurant VAT rate reductions in Sweden and Finland 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 our 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. Accounting for very detailed indicators of market segments such as price, location and restaurant category does not explain the differences between restaurant types.

Our results thus suggest that firms use discrete pricing strategies and that firms of different types differ in the nature of this discreteness. We further show that independents have a lower frequency of price changes when taxes are fixed, have a lower price responsiveness to changes in VAT, make more limited use of currency conversions for price increases and use round numbers more frequently. Jointly, these findings suggest that independents are more rigid in their pricing strategies than chains, although both sets of firms appear to use discrete pricing rules. The reason may be that specialized price setters within chains optimize over a larger set of possible action spaces as in multi-armed bandit models, or as in rational inattention models, such that of Matějka's (2016), or that they are more likely to exploit non-linear consumer responses as in Gabaix (2014). The results thus support the notion of Kopczuk and Slemrod (2006) and Slemrod and Gillitzer (2014) that introducing micro-foundations with fundamental firm-level heterogeneity is a promising route forward when trying to assess the welfare consequences of VAT reforms and when trying to understand how and why the VAT pass-through varies across settings and sectors.

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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. The idea was to use Estonian restaurant-meal prices as a comparison group for meal prices among Finnish 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 collected prices from approximately 750 restaurants in Finland and 400 in Estonia around the Finnish reform as well as 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 the web pages of restaurants. If web pages with meal prices on them were not available, we collected the prices by calling the restaurant. In the initial collection the exact name of the meal and the price was recorded, and then in consecutive collection rounds the price of the same meal was collected, provided it still was available on the menu.

Restaurants are 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 it is natural to collect main courses and desserts, but we needed to survey a smaller set of meals from cafes. We attempted to collect a minimum of 7 and a maximum of 11 meals and drinks from each restaurant category, but because we could not always find enough suitable items to collect, the minimum number per restaurant is 3 meals or drinks. We wanted to collect at least the most common meal served by each restaurant, and this was determined by the price collector (research assistant). We also collected prices for other meal types, such as a vegetarian dish, salad, appetizer and dessert, and soda and coffee prices.

Importantly, while examining the restaurant from different sources, we also collected several restaurant characteristics from each restaurant; the

specific location of a restaurant, whether or not the restaurant belongs to a chain, is located in a mall, and has a weekly changing lunch menu.

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

Appendix B: Additional tables and figures

Table 7: Pass-through when using alcohol prices as an alternative control group

VARIABLES	(1) Country	(2) Alcohol	(3) Country	(4) Alcohol
Treatment	-0.268**	-0.232**	-0.553***	-0.487***
	(0.132)	(0.108)	(0.210)	(0.103)
Independent			-0.089	-0.041
			(0.065)	(0.098)
Independent			0.453**	0.405***
*Treatment			(0.214)	(0.103)
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). Block bootstrapped standard errors with eight clusters (reform times country times type) *** p<0.01, ** p<0.05, * p<0.1.

Table 8: 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***	0.357***	0.278***
	(0.056)	(0.055)	(0.034)
N	5,762	5,762	5,762
R^2	0.055	0.067	0.151
Panel B: 3-6 months after			
Treated Independent	0.289***	0.276***	0.223***
	(0.042)	(0.041)	(0.039)
N	4,943	4,943	4,943
R^2	0.027	0.039	0.100
Panel C: 15-18 months after			
Treated Independent	0.111	0.137*	0.070
	(0.093)	(0.073)	(0.088)
N	4,196	4,196	4,196
R^2	0.001	0.009	0.059
Rest class * treat		x	x
Meal type * treat		x	x
Price Q * treat			x
ZIP fe			x

Note: Dependent variable is Δ of equation (1). Zip-code areas are merged together whenever there are fewer than 60 observations in one area. Block bootstrapped standard errors with zip-code-level clusters in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 9: Pass-through, separately by reform

	(1)	(2)	(3)	(4)
	Finnish reform	Finnish reform	Swedish reform	Swedish reform
	Pass-through	Pass-through	Pass-through	Pass-through
Treatment	-0.256***	-0.631***	-0.172***	-0.273***
	(0.032)	(0.080)	(0.027)	(0.046)
Independent	0.337***	-0.028	-0.006	-0.081**
	(0.074)	(0.055)	(0.026)	(0.039)
Independent		0.534***		0.167***
*Treatment		(0.080)		(0.056)
N	5,287	5,287	5,048	5,048
R^2	0.099	0.127	0.013	0.015

Note: Dependent variable is Δ of equation (1). Block bootstrapped standard errors with zip-code-level clusters in parentheses and 1000-2000 replications: *** p<0.01, ** p<0.05, * p<0.1.

Table 10: 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***
	(0.102)	(0.061)	(0.132)	(0.115)	(0.079)	(0.079)
Independent	-0.074	-0.088**	-0.049	-0.121**	-0.059	-0.133**
	(0.070)	(0.034)	(0.058)	(0.060)	(0.051)	(0.052)
Independent *	0.634***	0.396***	0.682***	0.430***	0.368***	0.375***
Treat	(0.119)	(0.061)	(0.139)	(0.120)	(0.087)	(0.080)
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***
	(0.067)	(0.120)	(0.153)	(0.061)	(0.113)	(0.135)
Independent	-0.113**	-0.045	-0.059	-0.108***	0.044	-0.193
	(0.048)	(0.039)	(0.057)	(0.034)	(0.062)	(0.119)
Independent *	0.493***	0.323***	0.644***	0.332***	0.430***	0.550***
Treat	(0.078)	(0.118)	(0.164)	(0.062)	(0.119)	(0.147)
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***		
	(0.070)	(0.113)	(0.065)	(0.186)		
Independent	-0.111***	0.001	-0.064**	-0.314**		
	(0.033)	(0.056)	(0.027)	(0.167)		
Independent *	0.473***	0.446***	0.412***	0.736***		
Treat	(0.073)	(0.125)	(0.067)	(0.184)		
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). Block bootstrapped standard errors with zip-code-level clusters in parentheses and 500 replications. Zip-code areas are merged together whenever there are fewer than 60 observations in one area. *** p<0.01, ** p<0.05, * p<0.1.

Table 11: 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
	(0.030)	(0.039)	(0.012)	(0.022)	(0.024)
After*	-0.008	-0.005	0.020*	0.019	0.006
Independent	(0.028)	(0.026)	(0.011)	(0.025)	(0.028)
N	8,043	7,981	8,434	7,981	7,981
R^2	0.000	0.178	0.012	0.008	0.007
no. restaurants	1,203	1,190	1,244	1,190	1,190

Note: Regression results for treated restaurants (as in Figures (7) and (8)) 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. Block bootstrapped standard errors with municipality-level clusters and 2000 replications: *** p<0.01, ** p<0.05, * p<0.1.

Table 12: Round-number pricing:

	(1)	(2)	(3)	(4)
	Round	Round	Round	Round
Independent	0.292***	0.293***	0.295**	0.266**
	(0.099)	(0.108)	(0.122)	(0.125)
Right after	-0.028	-0.028	-0.031	-0.028
	(0.021)	(0.021)	(0.022)	(0.021)
3-6 months after	-0.077	-0.076	-0.080	-0.079
	(0.087)	(0.086)	(0.087)	(0.088)
15-18 months after	-0.037	-0.036	-0.041	-0.039
	(0.039)	(0.041)	(0.043)	(0.045)
Rest class (ref. fast food)				
À la carte			0.058	0.002
			(0.080)	(0.049)
Cafe			-0.040	-0.031
			(0.062)	(0.076)
Lunch			-0.060	-0.068
			(0.210)	(0.157)
Price quartile: ref. smallest				
2				-0.022
				(0.023)
3				0.062
				(0.066)
4				0.161**
				(0.071)
Constant	0.248***	0.074	0.075	0.177**
	(0.079)	(0.127)	(0.106)	(0.089)
N	19,892	19,892	19,892	19,892
R^2	0.080	0.088	0.106	0.175
Price splines (10)		x	x	x
Rest class * treat			x	x
Meal type * treat			x	x
Price Q * treat				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. Block bootstrapped standard errors with country, reform, and treatment level clusters. *** p<0.01, ** p<0.05, * p<0.1.

Table 13: Probability of price changes before, during, and after Estonian currency change by type: Estonian restaurants

	(1)	(2)	(3)	(4)	(5)
	Outcome: 1 if $\triangle$p>0.5%, 0 otherwise				
4 months before	0.045***	0.045***	0.045***	0.045***	0.045***
	(0.015)	(0.015)	(0.015)	(0.016)	(0.016)
3 months after	0.665***	0.665***	0.665***	0.664***	0.665***
	(0.041)	(0.041)	(0.041)	(0.043)	(0.042)
15 months after	0.534***	0.534***	0.533***	0.533***	0.533***
	(0.053)	(0.053)	(0.053)	(0.053)	(0.052)
4 months before	-0.016	-0.016	-0.016	-0.015	-0.015
* Independent	(0.019)	(0.019)	(0.019)	(0.019)	(0.019)
3 months after	-0.167***	-0.167***	-0.167***	-0.167***	-0.167***
* Independent	(0.049)	(0.047)	(0.048)	(0.049)	(0.050)
15 months after	-0.015	-0.014	-0.015	-0.015	-0.014
* Independent	(0.057)	(0.058)	(0.057)	(0.057)	(0.055)
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. Block bootstrapped standard errors with 5-digit zip-code clusters. *** p<0.01, ** p<0.05, * p<0.1.

Table 14: Results on meal exits by treatment and restaurant type

VARIABLES	(1) By second	(2) By third
Treatment	0.103**	0.102***
	(0.043)	(0.035)
Independent	0.061	0.033
	(0.064)	(0.057)
Independent	-0.004	0.034
*Treatment	(0.071)	(0.070)
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. Block bootstrapped standard errors with 8 clusters; country, reform, and treatment level. *** p<0.01, ** p<0.05, * p<0.1.

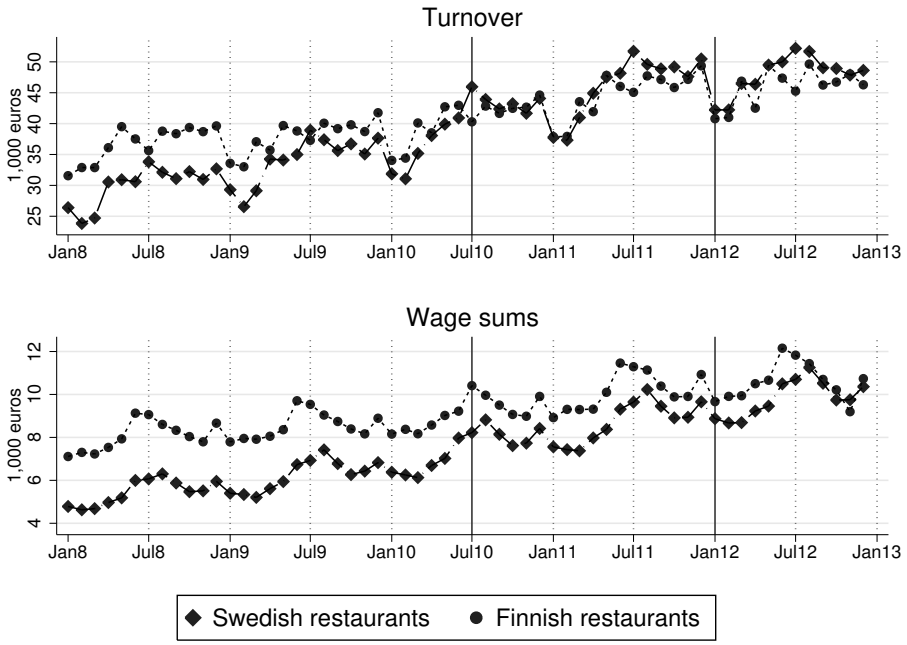
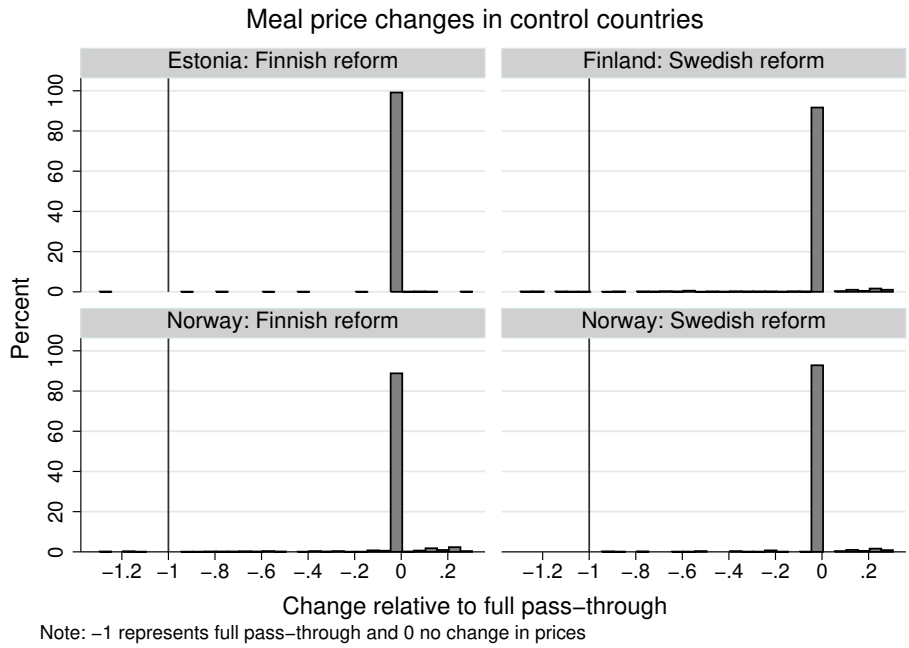


Figure 11: Longer-term development of average tax-inclusive turnover of restaurants in Finland and Sweden

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).



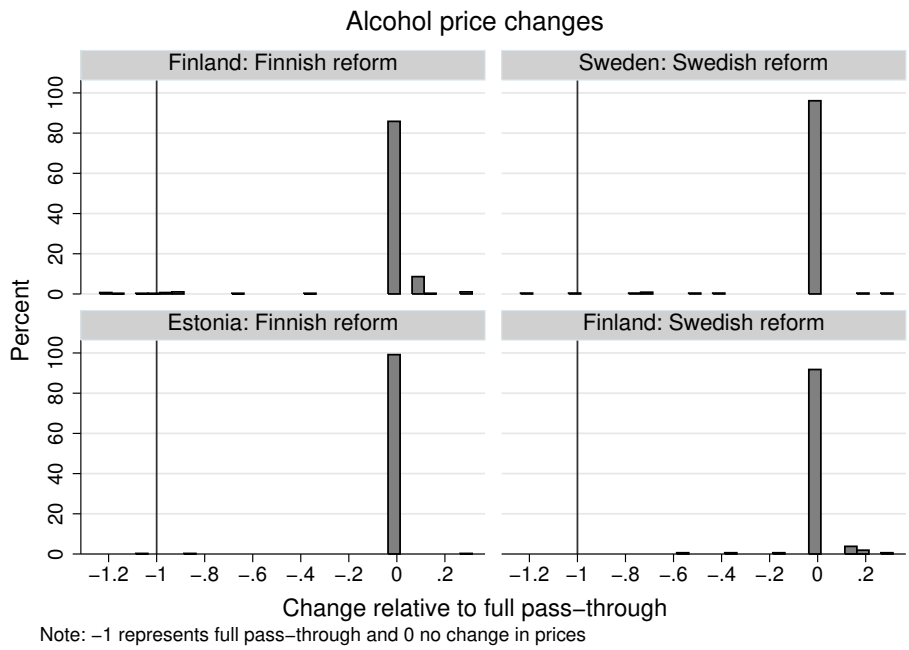


Figure 12: Meal price changes in control countries by reforms and alcohol price changes by countries and reforms

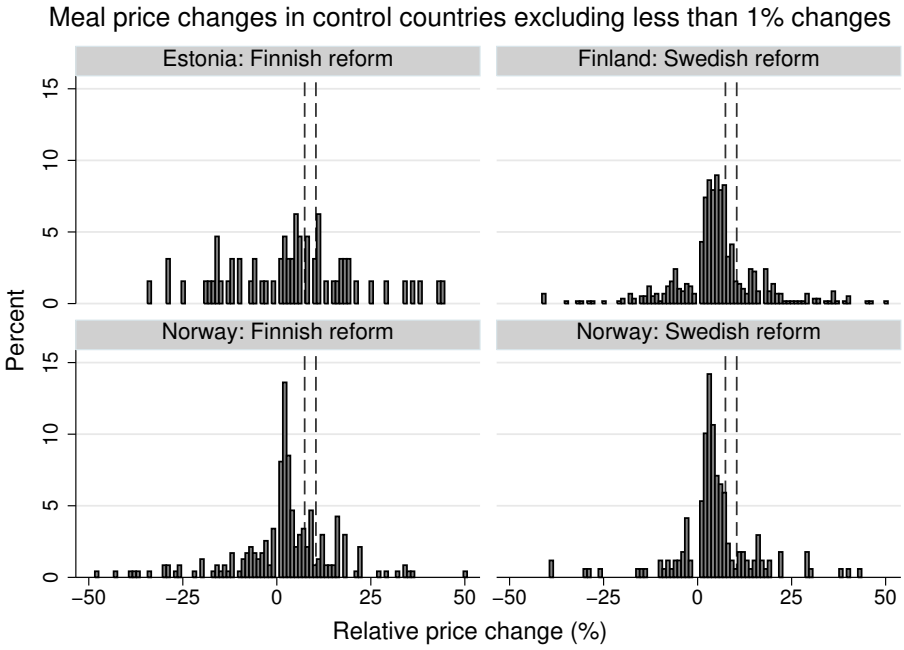


Figure 13: Meal price changes in control countries excluding less than 1% changes in meal prices

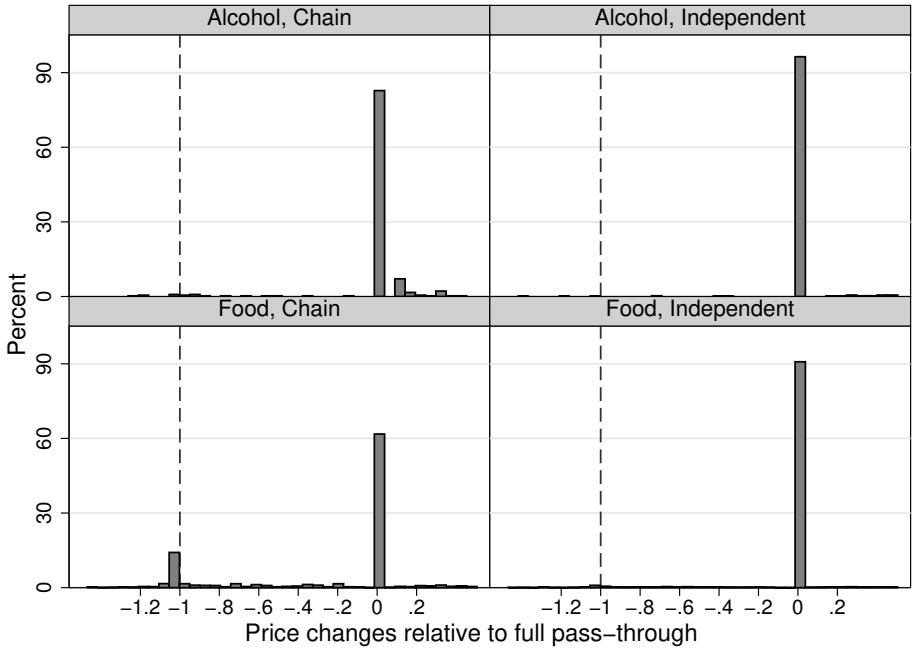


Figure 14: Short-run pass-through comparing meal to alcohol prices by type

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

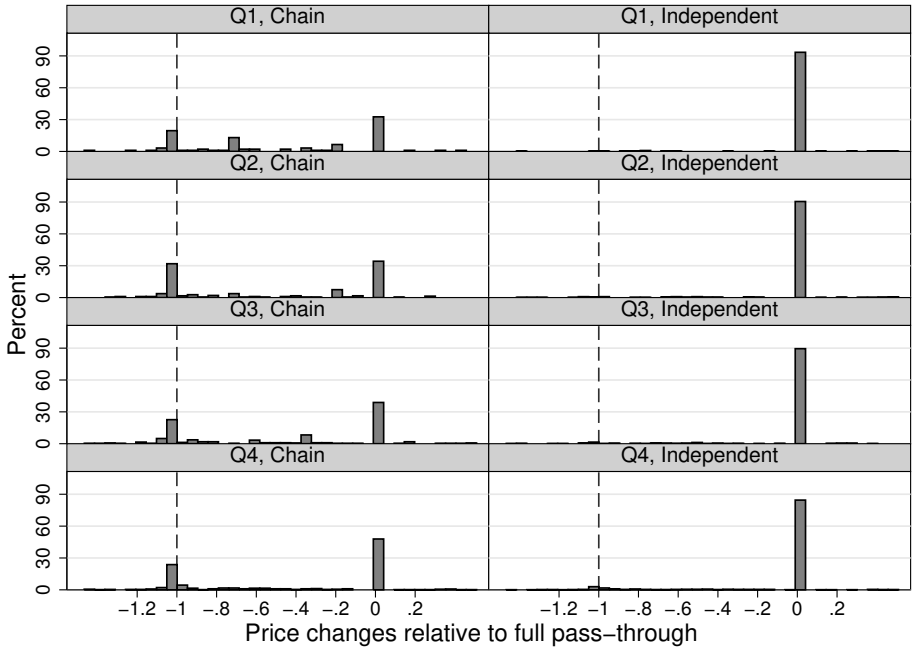


Figure 15: Short-run pass-through by price quartiles

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

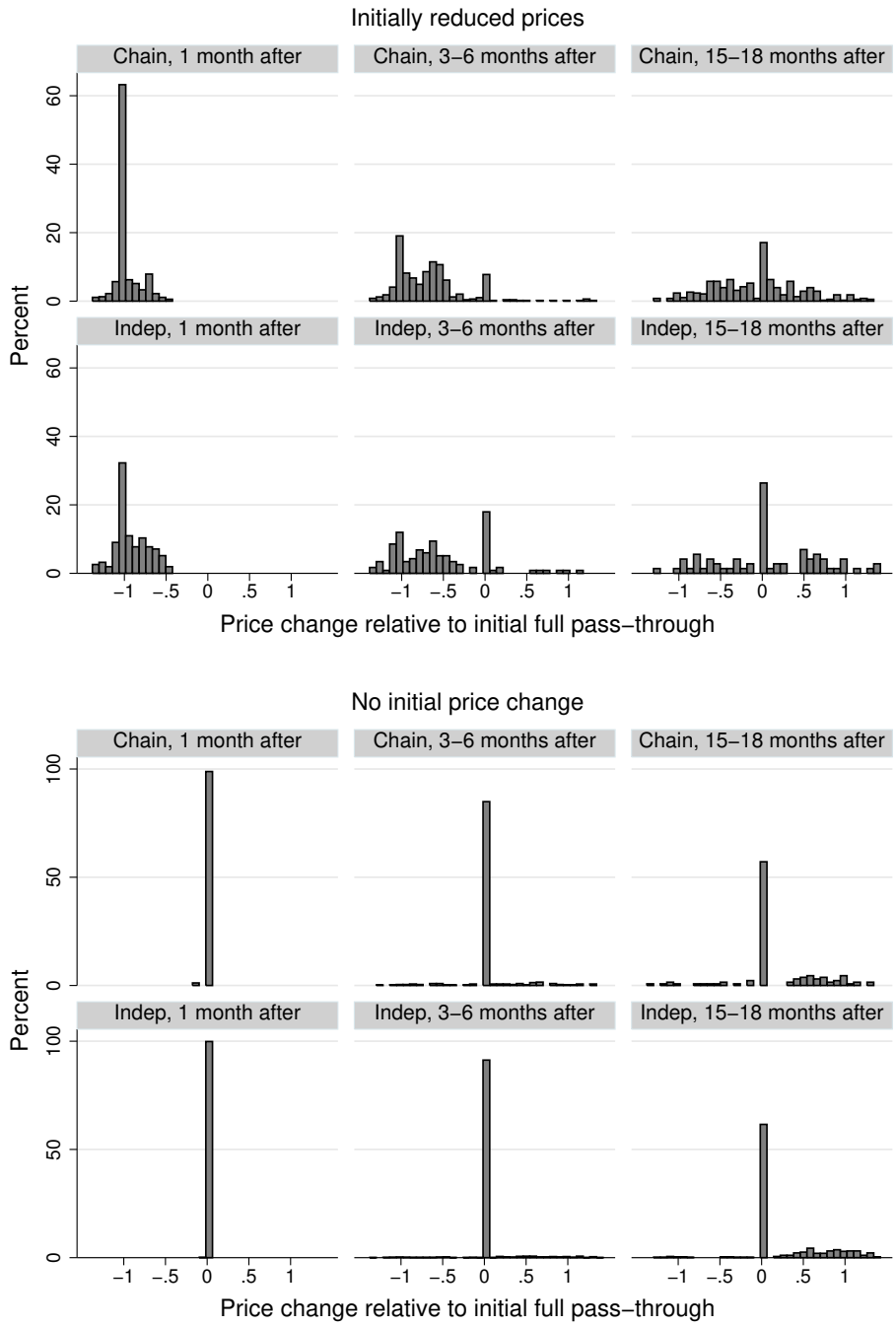


Figure 16: Medium-run pass-through divided by restaurant type, initial price change and collection rounds

Note: Distributions of meal price changes by restaurant type, initial price change (upper panel shows the distribution for those changing prices right after the reform and lower panel for those not changing prices) and collection rounds 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.

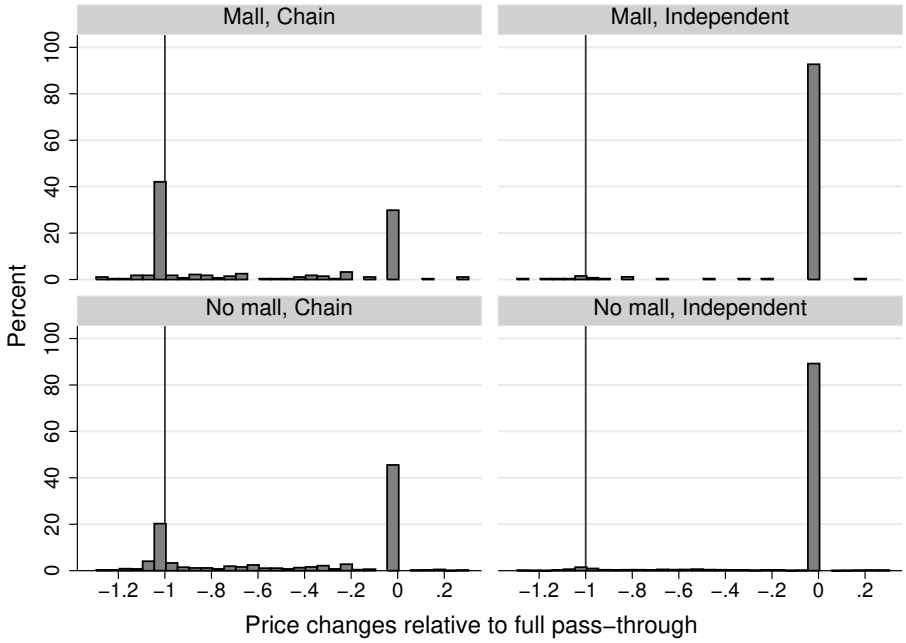


Figure 17: Short-run pass-through divided by restaurant type and restaurants located in malls

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.

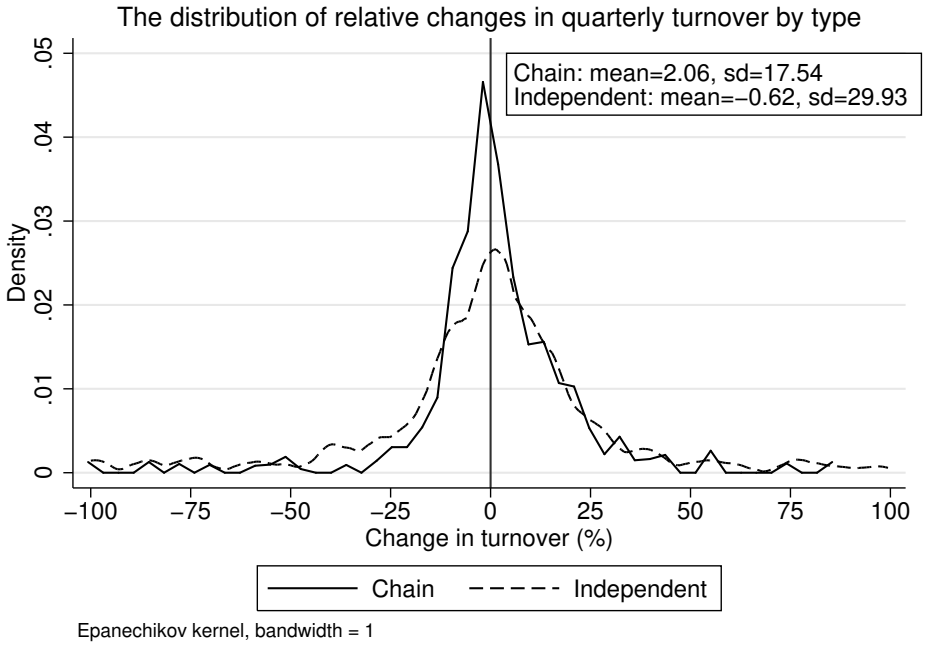


Figure 18: Changes in sales relative to own history

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%.

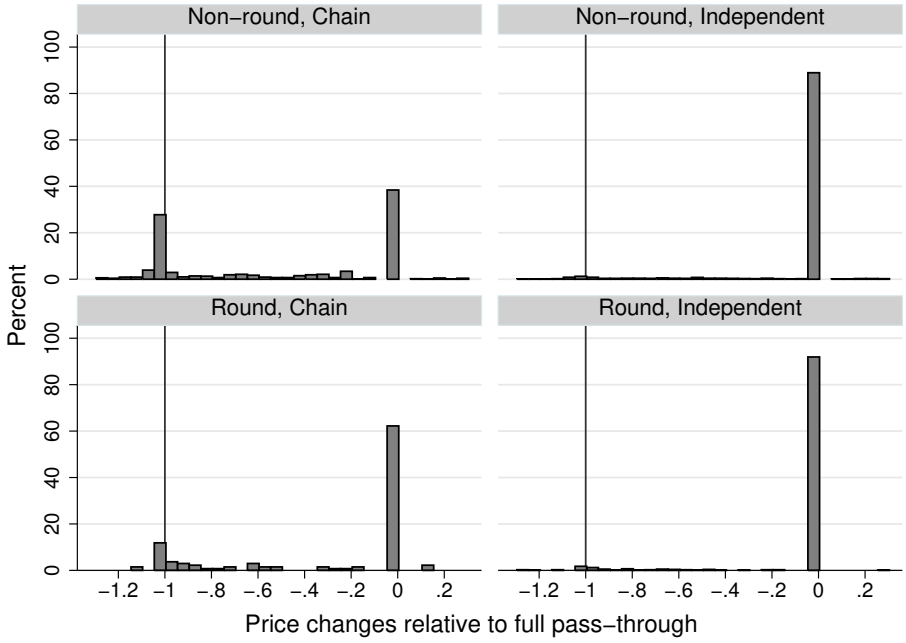


Figure 19: Short-run pass-through divided by restaurant type and round-number pricing

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.

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***	-0.462***	-0.428***
	(0.091)	(0.089)	(0.086)
Independent	-0.041	-0.041	-0.097
	(0.081)	(0.079)	(0.092)
Treated * Independent	0.405***	0.391***	0.369***
	(0.095)	(0.095)	(0.092)
N	6,326	6,326	6,255
R^2	0.057	0.067	0.140
Panel B: 3-6 months after			
Treated (food)	-0.372***	-0.356***	-0.344***
	(0.098)	(0.094)	(0.096)
Independent	-0.040	-0.075	-0.108
	(0.100)	(0.098)	(0.107)
Treated * Independent	0.329***	0.345***	0.335***
	(0.104)	(0.102)	(0.105)
N	5,425	5,425	5,425
R^2	0.027	0.036	0.089
Panel C: 15-18 months after			
Treated (food)	-0.018	-0.058	-0.117
	(0.093)	(0.082)	(0.073)
Independent	0.012	-0.021	-0.110
	(0.189)	(0.189)	(0.215)
Treated * Independent	0.099	0.144	0.220
	(0.208)	(0.200)	(0.202)
N	4,545	4,545	4,545
R^2	0.001	0.005	0.039
Rest class * treat		x	x
Meal type * treat		x	x
Price Q * treat			x
ZIP fe			x

Note: Dependent variable is Δ of equation (1). Zip code areas are merged together whenever there are fewer than 60 observations in one area. Block bootstrapped standard errors with zip-code-level clusters in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 6: Coordination in price changes across restaurants and meals

	(1)	(2)	(3)	(4)
	Pass-through	Pass-through	Pass-through	Pass-through
Independent	0.566***	0.675***		0.171***
	(0.064)	(0.107)		(0.032)
Others in the same area		-0.197		
		(0.140)		
Others in the same group			0.700***	
			(0.126)	
Other prices in the same restaurant				0.494***
				(0.062)
N	1,035	1,035	2,085	5,564
R^2	0.149	0.157	0.136	0.191

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