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Abstract

Studying very detailed micro data collected around two different VAT reforms in Europe, we show that tax incidence is heavily dependent on the characteristics of the price-setting firms. The reforms generated bimodal price-change distributions; nearly all independent restaurants left prices unchanged whereas a substantial fraction of restaurants belonging to chains chose a complete passthrough. These differences cannot be explained by location, initial prices or other market-segment indicators. Instead, differences appear to arise because independent restaurants aim for (very) crude price ranges rather than fine-tuned optimized prices, whereas chains use more elaborate, coordinated pricing strategies.

JEL-Codes: H220, H320, E310.

Keywords: firm heterogeneity, VAT, price incidence, price setting, restaurants.

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1 Introduction

An increasingly active literature within public finance explores the incidence of consumption taxes (Carbonnier 2007, Doyle and Samphantharak 2008, Kosonen 2015, Benzarti and Carloni 2015, and Rozema 2015). This literature documents substantial variations in tax incidence. The typical explanation for the varying results rests on differences in market level conditions such as elasticities of demand and supply and the degree of competition among firms (e.g. Myles 1989, Weyl and Fabinger 2013 and Carbonnier 2014).[1] Thus, a standard assumption in most of the literature is that firms only differ in these characteristics, and given them, tax incidence is deterministic. In this paper, we challenge this assumption and document substantial systematic heterogeneity in the way different businesses facing the same market level conditions react to VAT reforms.

Empirically, we analyze price responses to VAT-reductions in the restaurant industry in Finland and Sweden. We use very detailed micro data on the anatomy of price changes to study how the VAT reforms affect different types of firms. In addition to the uniquely detailed evidence of tax incidence we are able to produce, our data also allows us to present novel evidence regarding if and how different types of firms adjust (and coordinate) their prices in response to a changing cost structure, and to what extent their price setting behavior is in line with predictions from existing theories on firm-level pricing strategies.[2]

We divide the restaurants into two categories; we refer to restaurants that do not belong to chains or franchises as *Independents* and other restaurants as *Chains*. This dichotomy is based on the notion that conditions under which prices are set in the two types of restaurants differ. Pricing decisions in independent restaurants are likely to be made by owners or other on-site managers who are responsible for multiple decisions, including many of a very practical nature such as staffing and cooking, whereas pricing decision in chains are more likely to be made by specialists. Price

[1] Other aspects discussed in the literature include opportunities for tax evasion and generic cross-industry differences, see e.g. Kopczuk et al. (2013) and Marion and Muehlegger (2011).

[2] See Klenow and Malin (2010) for a review of the literature on the anatomy and dynamics of price changes, and Carlsson (2014) for a recent study. For price coordination, see e.g. Houde (2012) and Thomadsen (2005) and references therein.

setters at independent restaurants are thus more likely to set prices under constrained optimization.[3] Restaurants belonging to chains, on the other hand, have access to a larger infrastructure which could allow them to collect information on market characteristics. Their further-reaching scope may also allow them to coordinate price setting decisions if needed and potentially attempt (but not necessarily succeed) to increase profits through elaborated price setting strategies. Although we expect the dichotomy of firm-types to exist throughout the economy with variations both within and across industries, we believe that the restaurant sector is very well-suited for an analysis of firm-side heterogeneity since equally sized establishments that are competing side-by-side can be either single-unit businesses or parts of company-owned chains or franchises.[4] We show that independent restaurants and chains operate in similar market segments as both groups feature fast-food venues as well as finer restaurants.

We use data on VAT-reductions in Finland during July 2010 and in Sweden during January 2012, and rely on the price evolution in neighboring countries to control for time effects. We collected data on meal-level price changes across time for a representative sample of restaurants in the relevant countries, and matched these to administrative records held by the tax authorities in the two treated countries. These data allow us to dig deep into the anatomy of the price responses. In particular, we are able to follow the prices of the same meals over time, which allows us to examine the full distribution of price changes for different types of firms. We are also able to control for the economic environment faced by the firms. Due to the detailed nature of our data, we can provide a very precise account of the distribution of price changes across time, as well as document potential coordination of meal-level price responses within sites, chains, and locations.

The results show strikingly clean price change patterns. The over-

[3]For example, they should be more likely to optimally ignore certain aspects of price setting due to their multidimensional choice set, i.e. using a more "sparse" strategy for pricing in the Gabaix, (2014) sense. Independents could also follow different objective functions than chains with no differences in optimization constraints.

[4]It may be argued that the choice of industry may have geared the analysis towards finding results that are more in line with standard theory since the restaurant industry may be closer to perfect competition than many other industries. However, our results are quite far from the predictions of perfect competition models.

all pass-through is fairly low, a quarter of full pass-through in the short run. However, this average pass-through masks considerable heterogeneity. The short-run impact is virtually zero for the group of independent restaurants, whereas a substantial fraction of chain restaurants choose a full pass-through instead. We perform a number of robustness checks to see if this difference is due to the location, initial price (an indicator of market segment) or type of restaurant and find no support for these notions.[5] Most notably, we show that the difference remains throughout the initial price distribution, as well as when we focus on establishments located close to each other within the same restaurant-dense areas, and when we zoom in on restaurants located in malls. Using administrative tax data, we can rule out tax evasion as the main explanation. Our analysis of VAT payments, tax credited inputs and the number of traded meals also suggest that customer responses to price reductions were small and that changes in, for example, meal quality are unlikely to explain our findings.

Our most striking finding is the immediate pass-through of zero for independent businesses. This result can only be explained by standard tax incidence models if demand is infinitely elastic or supply inelastic, both of which seem as unlikely explanations. A possible interpretation of the results is that price setters at independent restaurants chose to ignore the reforms because they rely on simplified pricing strategies, i.e. they aim for cruder price targets and therefore do not re-optimize in response to the VAT reductions.[6] Additional results support this interpretation. In particular, we analyze the types of prices the restaurants set (in the absence of the reform) and show that independent restaurants are considerably more likely to use prices that are rounded to integer values on their meals even after accounting for market factors such as the price range.[7] Independent restaurants also change their prices less frequently even in absence of VAT

[5]The lack of responses due to the VAT cut is somewhat in line with findings in the Kosonen (2015) study of Finnish hairdressers.

[6]The crude strategies could be the results of different, perhaps better, beliefs about the responsiveness of the customers as in the model by Gabaix (2014) although our auxiliary analysis of price changes during an Estonian currency conversion indicates that independents may not be universally superior in their pricing strategies.

[7]In this context, it is also notable that the month-to-month and year-to-year variances in taxed turnover within firms is enormous. This suggests that firms face large idiosyncratic shocks and therefore they may find it difficult to learn about their demand by experimenting with small changes in prices.

reforms. It should be noted that the basic idea that some businesses may behave in ways that differ from standard theory, although largely absent in the literature on tax incidence, is well in line with findings in other related studies: Lazear (2004, 2005) asserted that smaller entrepreneurial firms tend to be run by generalists who need to attend to multiple, sometimes complicated, tasks. Bloom and Van Reenen (2010), Bloom et al. (2013) and Drexler et al. (2014) found that the quality of managerial practices vary widely across firms and that firms benefit from management training. In addition, it seems likely that owners or managers at independent businesses resemble regular consumers in their behavior and these have been shown to be affected by "non-standard" elements such as default savings decisions and tax salience (Chetty et al. 2009, Finkelstein 2009, and Chetty et al. 2014).

We also document that although the tax incidence pattern for chains is more in line with standard models *on average*, their distributions of pricing choices do not fit the standard models perfectly either. Somewhat simplified, our results suggest that chains either responded by fully shifting the reduced VAT to prices or by not changing their prices at all. The high frequency of full immediate pass-through is consistent with highly strategic pricing behavior, for example in order to induce a response from otherwise inattentive customers (similar to the logic of a sale),[8] to elicit goodwill from customers, or as a part of a coordinated effort to prevent the governments from resetting the VAT rates.

Furthermore, we show that although the average pass-through appears to converge between the two types of firms over time (being statistically insignificant after 12-15 months), the difference in pass-through distributions remain throughout. The convergence of averages is primarily achieved through additional price increases by those restaurants (belonging to chains) that initially responded with a full pass-through. Firms that did not initially reduce their price at all (effectively ignored the reform), were instead more likely to keep their prices constant throughout our 18 months follow-up period.[9]

[8] See e.g. Klenow and Malin (2010) or Gabaix (2014).

[9] The high frequency of reverting prices among those that initially chose a full pass-through speaks against the notion that the choice was driven by an extremely elastic supply.

To substantiate the hypothesis that price-setting strategies in chains are more elaborated, and that this may explain the differences in pass-through, we first show that large chains coordinate their price responses between sites, whereas price responses are not coordinated between restaurants that do not belong to the same chain but share location. We then proceed to use data from a currency conversion (Estonian Euro introduction). According to standard theory, currency conversions should not affect price levels, but empirically they appear to do so (Cavallo et al. 2015) and a possible reason is that firms choose to increase their prices in times when price changes are less salient. We show that chain restaurants, but not independents, used the introduction of the Euro as an opportunity to increase their prices more than otherwise, suggesting that chains have price setting strategies that are more elaborated than those of the independents.

Overall, we believe that our results provide clear evidence for the notion that independents and chains respond very differently to changes in consumption taxes for other reasons than market-level conditions. This stands in stark contrast to the standard assumption that market factors alone are important for predicting the price responses to changes in VAT rates. Our results strongly suggest that independent businesses in the restaurant industry rely on pricing rules that greatly reduces their price-change frequency and their responsiveness to consumption tax reforms, in stark contrast to pricing behavior of chains. The price setting strategies of chains instead generates an average response which is more in line with standard theory, but with considerable (non-standard) heterogeneity in the price dynamics. These results could help explain the varying tax-incidence and other price pass-through results found in previous literature (Cabral et al. 2015, Carbonnier 2007, Doyle and Samphantharak 2008, Kosonen 2015, Benzarti and Carloni 2015, and Rozema 2015).

The structure is 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. All appendices are available online.

2 Standard tax-incidence models, and some extensions

2.1 Standard tax-incidence models

In this short section we highlight a few features of standard tax incidence models that are useful as a background for our empirical analysis.

A key result arising from the economic theory is that tax incidence depends on how markets work. In the simplest, perfect competition and a single good case, markets clear and firms are price (p) takers so that demand (D) equals supply (S) in equilibrium. If we introduce a tax t we get $D(p) = S(p - t)$ and the standard tax incidence 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 tax incidence and the more inelastic side bears the burden of taxation. This implies, e.g., that to explain a zero pass-through, one needs to assume perfectly elastic demand or perfectly inelastic supply. To get a full pass through, the demand elasticity instead needs to be zero, or the supply elasticity infinite. 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. The elasticities are (typically) defined at the market and goods level and tax incidence should thus be the same for all firms who compete on the same market.

In models of imperfect competition the role of the supply elasticity is replaced by more advanced assumptions regarding firm behavior but the shape of demand curve continues to play an important role. Weyl and Fabinger (2013) compare the monopoly and perfect competition cases and show that tax incidence in other symmetric (but more elaborated) imperfect competition models falls between that of monopoly and perfect competition. They show that the tax incidence for a monopoly can be written in a form that resembles that of perfect competition:

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

where ϵ_{ms} measures the curvature of log demand. Thus, in the monopoly case the shape of the demand curve largely determines the tax incidence. In other symmetric imperfect competition cases, the form of interaction between the firms also plays an important role. However, the resulting tax incidence response falls between the perfect competition and monopoly models.

Weyl and Fabinger (2013) also analyze strategic interactions of firms in their context. It is natural to assume that firms would react to the actions of their competitors, but the possible action space is vast; a firm could change its prices, advertise, differentiate its products and so forth. In general, a price decrease in one firm could lead to either a price increase or decrease in a competing firm depending on whether firms are strategical complements or substitutes. However, these models do not yield clear predictions for tax incidence, since the outcomes depend on the nature of interaction between firms. In our empirical section, we provide some evidence on how firms acting in the the same (small) geographical areas react to other firms' price changes during the reforms.

2.2 Price stickiness

The standard analysis of tax incidence assumes that price setting is a continuous choice. However, in the broader literature on micro-level price dynamics (see e.g. Klenow and Malin, 2010), motivated by New Keynesian concerns about how to model nominal rigidities, it is observed that the price change distribution tends 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).[10] Recently, Gabaix (2014) has also proposed that price rigidities may arise because firms are expecting their customers to be inattentive to small price

[10]Recent extensions include Nakamura and Steinson (2008) who nests 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.

reductions. A key prediction from all of these models is that the firm-level pass-through of a change in consumption taxes may be discontinuous, in particular with a non-trivial fraction of firms leaving their prices fixed, at least in the short run.[11]

2.3 Tax evasion

Tax evasion is not included in standard tax-incidence models, but it 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, under a less extreme assumption of partial tax evasion, affect tax evasion as well as real decisions, 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 (which they do in our case), real costs for earning income implies that changes in the consumption tax rates will have an impact on firm-level decisions, including their prices (Slemrod and Yitzhaki, 2002).

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. From 2009, an EU Directive allows member states to apply one of its 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

[11] Further behavioral explanations for nominal rigidities include discontinuous updating of information (as in Mankiw and Reis, 2002) or firm-level inattention to macroeconomic shocks (as in Mackowiak and Wiederholt, 2009), but (as modelled) these rigidities do not explain zero price responses.

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 ("take away") were taxed at the reduced rate already before the reforms. Alcohol VAT remained at the original standard rate after the reform. In both countries the changes in VAT legislation was passed relatively close to the reform, which makes large pre-reform anticipatory effects unlikely.

We measure the impact on prices by means of the pass-through relative to full pass-through, 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) (i.e. stable producer prices) implies a drop of consumer prices of -7.4 percent in Finland and -10.4 percent in Sweden. Notably, and in contrast to in sales taxes in the US, consumer prices within the EU are always displayed including VAT. Hence, Δ is the price-change observed on the price tags, scaled as fractions of the full pass-through.

3.2 Outline of the empirical approach

Our basic empirical approach is to compare the price evolution within Swedish and Finnish restaurants with the evolution in neighboring countries. We use Estonia as the control for the Finnish reform, and Finland as the control for the Swedish reform. An analysis of the average impact of the reform thus relies on the standard differences-in-differences (DD) assumption, i.e. that the behavior of the control group (neighboring countries) properly reflects the (counterfactual) evolution of the treatment group in absence of treatment. However, as our focus is on potential firm-type differences in price responses, deviations from this identifying assumption only causes problems 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, geographic location (both share similar climate), share a border, have similar culture, seasonal holidays, vacation periods and seasonality in national food production.[12] They are also covered by the same EU regulations concerning VAT legislation.

Nevertheless, it is possible that the restaurant industries in neighboring countries develop in different ways over time. In our main analysis, we rely on data we collected on our own, starting just before the reform. These data contain a richness (and sample size) that is unavailable in standard CPI-collections of prices, but for obvious reasons they do not cover a very long pre-reform period.[13] To check whether the key assumptions are reasonable, we instead start by illustrating the evolution of the restaurant-meal component of the CPI in Sweden, Finland and Norway (unfortunately we do not have the CPI-data for Estonian restaurant meals). 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.

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

[13]Notably, the Swedish CPI-data only cover about 60 restaurants at each survey round.

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).

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.

3.3 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. These 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 a separate team of research assistants within 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 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.

For each round, we collected prices and meal information for 7 to 11 meals at each restaurant from a defined protocol. Depending on the type of restaurant, we collected elements such as starters, main courses, vegetarian meals, pre-set lunch prices and so forth, see Appendix A for details. The assistants chose the exact meals within each category with the intention that these should be possible to follow over time. Since we planned to follow the exact meals across time, it was not essential exactly which meals the assistants chose within each category. Along with the prices, we also recorded other information such as restaurant type and categorical information about the restaurant and the surroundings, such as indicators for being 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 to June 2010 and the short-run incidence data in July to 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 two years later). For the Swedish reform, our pre-reform survey was run in October to November 2011 and the short-run incidence survey in February to March 2012. In the Swedish case, we used Finland as the control country. Although our main analysis focuses on the short-run responses, we also repeated the survey half a year and a year and half later (for the treated countries), which enables us to also examine medium-term price effects. A disadvantage with the longer term responses is that a larger fraction of meals or restaurants have exited, and thus cannot be followed over time.

3.3.1 Independent restaurants and chains

A main element in our analysis for the tax incidence of the VAT reforms is the role of price-setting firm types. Throughout, we define restaurants that (according to our survey) are not part of a chain or franchised restaurants, 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).[14]

We use this split of the data since we conjecture that independent restaurants are less likely to have employees that are specialized on price setting. Naturally, the scale of the operation allows restaurants within the chain-category to be more specialized and to use their wider span to collect more detailed information about the relevant market structure. In contrast, pricing decisions within independent restaurants are more likely to be made by owners, entrepreneurs or other managers who need to perform a wide set of tasks (including staffing, and possibly, cooking) whereof pricing is just one. This difference should make independents more exposed to some of the concerns that have been assumed to cause price stickiness (referenced above) such as inattentiveness or lack of proper information. Independents may also differ from chains in their views regarding how customers update their perception of a "normal" price (in the Gabaix, 2014, sense) in the wake of a VAT reduction.[15] Chains may, on the other hand, be assumed to have more elaborated dynamic objectives, such as participating in coordinated price setting, and attempt to make use of their more comprehensive networks for collecting better market information. Thus, we believe that the dichotomy we use should be a reasonable proxy for operations that set prices under substantially different conditions.

3.4 Descriptive statistics

Table 1 gives the descriptive statistics divided by the restaurant type. Almost two thirds of the data consist of independent restaurants. The bottom two statistics show that the chain restaurants are larger in size. But most other characteristics are 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

[14] We study heterogeneity within the chain-group in Appendix B.

[15] Of course, we cannot rule out that they also differ in their objective functions, independent restaurants may be more likely to focus on satisfying their customers in ways that differ from pure profit maximizing behavior.

marginally higher in the chains.

Table 1: Descriptive statistics

	Chain			Independent		
	Mean	Median	SD	Mean	Median	SD
Share of restaurants	0.371	0		0.629	1	
Price	10.134	8	7.262	8.985	7.304	7.715
Mall	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
Ala 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 firms		898			1,712	
N of prices		4,092			6,924	
Annual wage bill	22,384,642	1,794,554	75,345,249	331,516	199,333	348,199
Annual 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" are for restaurants open mainly during lunch and breakfast. Annual turnover is tax inclusive sales. Wage bill and turnover are from administrative registers, nominal amounts converted to Euros.

Figure 2 shows the price distributions separately for independent and chain restaurants, divided by treatment status. As is evident, the price distributions are overlapping with very similar shapes. The comparability across treatment status matter, since restaurants in neighboring countries will be used to approximate the counterfactuals for restaurants in reform countries. Thus, similar distributions is a positive feature, although we will rely on differences-in-differences (DD) and therefore do not require that the price levels are identical before the reforms. Importantly, the initial distributions are also very similar for the two restaurant types, suggesting that the restaurants are competing in roughly similar market segments. In the empirical analysis, we account for remaining differences in pre-reform prices.

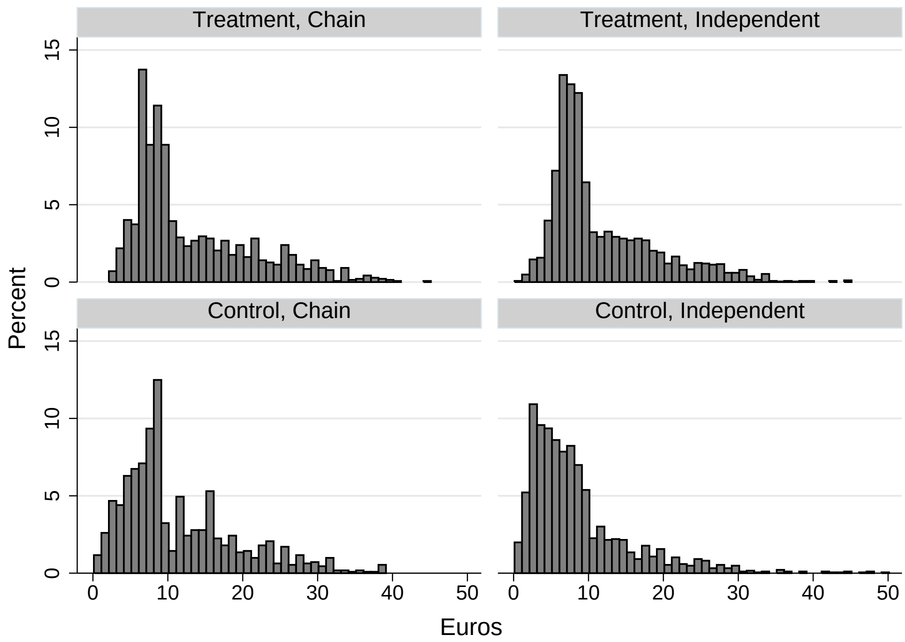

Figure 2: Pre-reform prices by treatment status and type

Note: Data from our own price collections. All prices are converted to Euros.

3.5 Methods

In the main analysis, we pool across the two reforms, but let the impact vary between independent restaurants and chains as defined above. Our very detailed micro data allow us to follow the price development for a given meal served in a specific restaurant over time. We are therefore able to analyze $\triangle$ of equation (1) separately by meal. As explained above, $\triangle = -1$ indicates a full pass-through of the reform in question (-7.4 percent in the Finnish reform and -11.4 percent in the Swedish reform).

When running DD-regressions for the average impact of the two groups, we let the outcome be Δ defined in equation (1), and estimate:

$$\Delta_{ijr} = \beta_1 D_{jr}^{Treat} + \beta_2 D_{jr}^{Independent} + \beta_3 (D_{jr}^{Independent} * D_{jr}^{Treat}) + \beta_4 (X_{ijr}) + \varepsilon_{ijr}, \quad (2)$$

using data on meal i at restaurant j and reform r, where D_{jr}^{Treat} is a dummy for restaurants in the treatment group and $D_{jr}^{Independent}$ is a dummy for independent restaurants. Notably, the difference form for the outcome takes care of all unobserved meal-specific constant factors. 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, i.e. differences in responsiveness to the reforms between independents and chains. X contains a vector of other covariates capturing other (market) factors besides ownership structure which could explain differences in tax incidence between the two groups. These variables are described in Table 1.

A standard concern in DD-settings is that the error term (ε_{ijr}) 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 with clusters at the level of our identifying information (i.e. reform times treatment-status times independent-dummy level), see Cameron et al. (2008) for a further discussion.[16]

4 Main results

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

[16] 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 bootstrap works.

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 controls imply a noticeable, but relatively modest, average short-run price effect of the reforms.[17] The large spikes at zero indicate that many prices did not change at all. Although this spike clearly is larger for the control group (indicating that the reforms had an impact on prices), it remains remarkably pronounced for the treatment group as well, despite the large reductions in VAT rates. The second visible spike for the treatment group is at full pass-through (i.e. at -1), indicating that when meal prices changed, they often changed by the full pass-through.

Table 2 quantifies the average short-run price responses using the DD-strategy of equation 2 (without the independent dummy, for now). Column (1) is without any controls, and the estimate suggests an impact of 27 percent of full pass-through. Reassuringly, including very detailed controls (col. 2) capturing restaurant class, meal type and initial price quartile has only a marginal impact (increase) on the estimate of interest.[18]

[17]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.

[18]Table 6 in Appendix B shows the results separately for the two reforms.

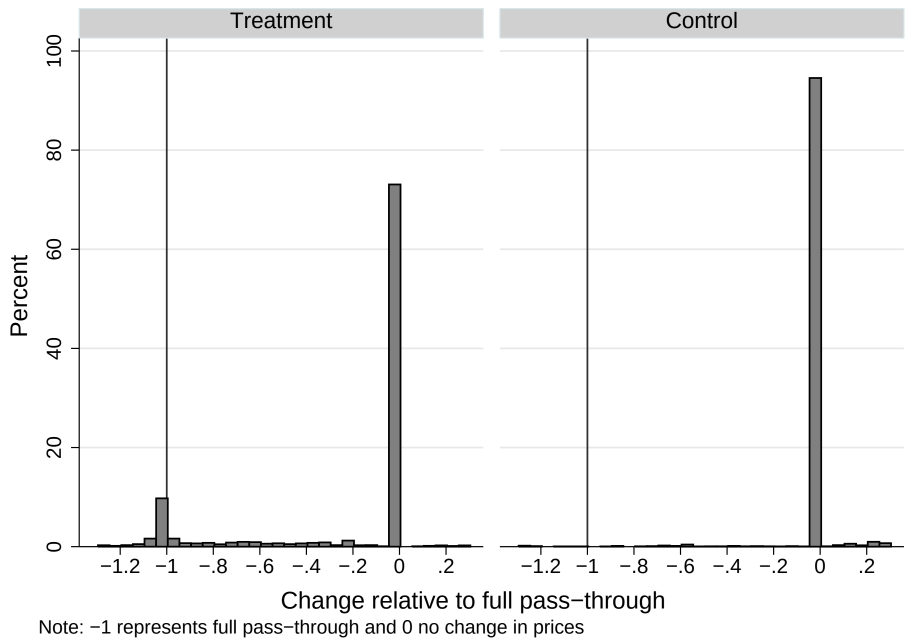

Figure 3: Distribution of price changes in the two reforms

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

	(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

Table 2: Average short-run pass-through

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 independent restaurants and chains in explaining the overall price change patterns. Figure 4 shows the price change distributions separately for chains and independents, by treatment status. As the figure shows, the pass-through is very 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.

The price change patterns of chains and independents appear similar within the control group not covered by the reform. There the prices do not change at all for most restaurants, which is expected given previous research on short-run price dynamics (e.g. Klenow and Malin, 2010). Control group chains are, however, slightly more likely to change their prices (mostly upwards, for natural reasons) than control group independents, suggesting that independents have a less adaptive pricing strategy also in normal times.

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 of equation 2. Table 3 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 to 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 hypothesis for the observed difference in meal price changes between independents and chains is that they operate in different types of

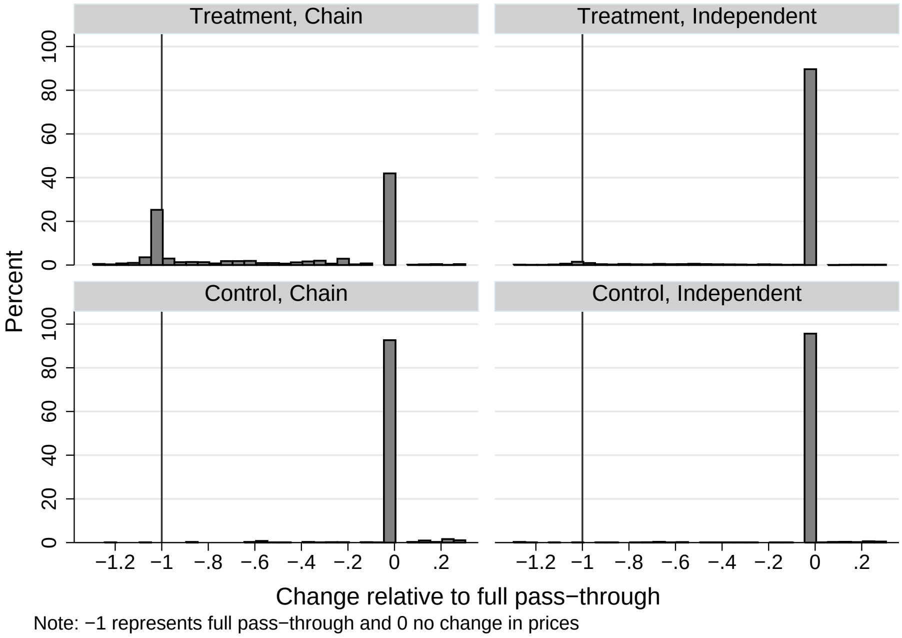

Figure 4: Short-run pass-through, by treatment status and type
Note: Meal-level price changes from 1-2 months before to 1-2 months after reforms. Normalized; -1 is full pass-through.

markets. To investigate these concerns, we use four indicators of the local market: (i) restaurant classification (fast food, à la carte, cafe, lunch restaurant), (ii) meal type (mostly 7 categories, see Appendix A), (iii) the level of the original (pre-reform) prices in quartiles, and (iv) the zip code.[19] 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). 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 only based 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 estimate of 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

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

covariates, the main thrust of the difference also remains in these very tight specifications.[20]

Table 3: Short-run pass-through by type

	(1)	(2)	(3)	(4)
	Pass-through	Pass-through	Pass-through	Pass-through
Treatment	-0.272***	-0.553***	-0.590***	-0.439***
	(0.037)	(0.063)	(0.098)	(0.051)
	[0.173]	[0.206]	[0.203]	-
Independent	0.161***	-0.089***	-0.086***	-0.066**
	(0.044)	(0.031)	(0.031)	(0.029)
	[0.183]	[0.066]	[0.061]	-
Independent		0.453***	0.440***	0.339***
*Treatment		(0.065)	(0.064)	(0.046)
		[0.210]	[0.207]	-
N	10,335	10,335	10,335	10,335
R^2	0.043	0.065	0.073	0.128
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 less 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 online Appendix B, Figure 12. The difference between independents and chains remains remarkably similar across the distribution.[21]

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 par-

[20] We also repeated the estimates in column (1) to (3) using alcohol price in the same restaurant as a control. Alcohol products were not affected by the VAT reduction. The results (not reported here) are very similar to those in Table 3 indicating very robust results.

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

ticular, it seems highly unlikely that similar restaurants that are located close to each other and serving meals with similar prices before the reforms, should face completely different demand elasticities. Furthermore, to explain the zero pass-through for independent restaurants with conventional models the demand needs to be perfectly elastic, which seems even more unlikely.

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 price over time, 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. On the other hand, following the same meals allows us to provide precise measures of price changes and control for the unobserved meal size and quality.

We have quantified the average price changes over time in regressions based on equation 2, but using a panel of price observations for each meal. The outcomes are log-prices at each point in time and we display estimated coefficients for time-since-reform dummies (with pre-reform prices as the omitted category). Table 4 displays the regression results. As expected, the short-run estimates mimic the results presented above. The immediate reduction in prices is about -0.033 for chains and 0.028 larger than this for independents implying a small and insignificant overall effect 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.

Notably, the convergence of average prices masks considerable remain-

Table 4: Medium-run price responses (log prices) by type

	(1)	(2)	(3)	(4)
	Log prices	Log prices	Log prices	Log prices
Right after	-0.015**	-0.033**	-0.033**	-0.033**
	(0.007)	(0.013)	(0.013)	(0.013)
3-6 months after	-0.008	-0.022***	-0.022***	-0.022***
	(0.005)	(0.008)	(0.008)	(0.007)
15-18 months after	0.026***	0.020***	0.019***	0.019***
	(0.005)	(0.006)	(0.006)	(0.006)
Right after		0.028**	0.028**	0.028**
* Independent		(0.014)	(0.014)	(0.013)
3-6 months after		0.022***	0.022***	0.023***
* Independent		(0.008)	(0.008)	(0.008)
15-18 months after		0.010	0.010	0.010
* Independent		(0.010)	(0.010)	(0.008)
N	21,107	21,107	21,107	21,107
R^2	0.040	0.044	0.048	0.068
Rest Class * treat			x	x
Meal type * treat			x	x
Price Q * treat				x
ZIP fe				x

Note: Data only include treated restaurants. The dependent variable is log prices. The left-out category is the initial prices. All models include meal fixed effects. Zip code areas are merged together whenever there are less than 60 observations in one area. Block bootstrapped standard errors with country times type clusters in parenthesis. *** p<0.01, ** p<0.05, * p<0.1.

ing differences in price change distributions between 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 panels from the left is the immediate price change (the same as in Figure 4) , the second set of panels is for the total price change 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 already 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 for both types of restaurants.

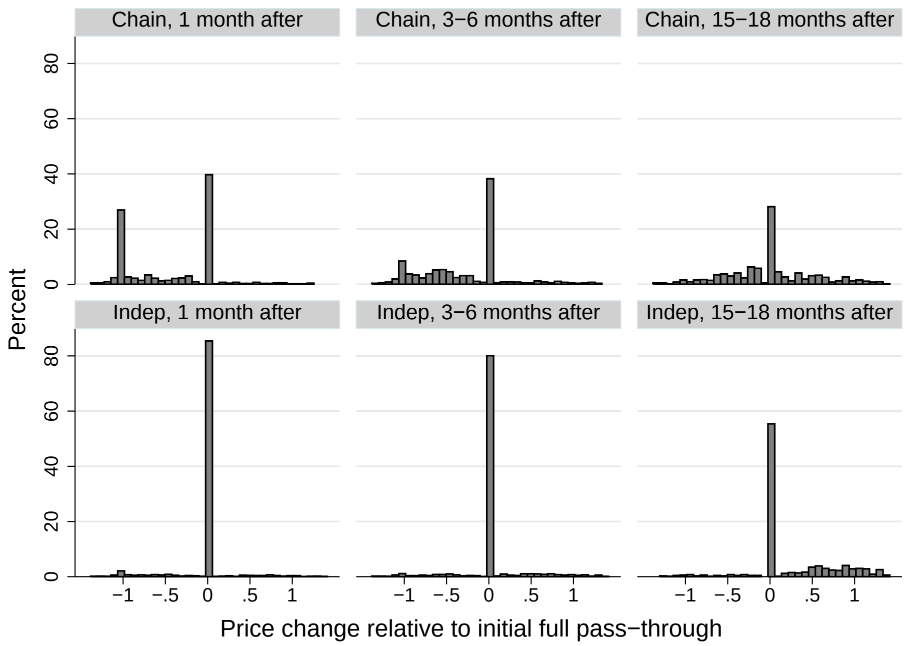

Figure 5: Medium-run pass-through, by treatment status and type
Note: Meal-level price changes from 1-2 months before to 1-2 months after, 3-6 months after and 15-18 months after reforms. Normalized; -1 is full pass-through.

As a final exercise on medium-run evidence, 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 13 in Appendix B shows the results from this exercise. It turns out that many of the prices that were at full pass-through straight after the reform reverted back to the exact pre-reform price after 15-18 months. As a contrast, the prices of the meals that were stable across the reform remained much more stable also in the following periods. 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.

5 Mechanisms and diverging pricing strategies

5.1 Restaurant density and price-change coordination

As discussed in section 3, standard theory predicts that the average price responses should vary with the degree of market competition (Weyl and Fabinger 2013). To tentatively 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 degree of pass-through, but only for the chains. The independents ignore the reform, regardless of density.[22]

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 much cross-restaurant interactions there are in the responses to the VAT reforms.[23]

Column (1) of Table 5 shows the 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 for the coordination parameter is negative, but statistically in-

[22] In Appendix B Figure 14 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 reform regardless of location.

[23] 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.

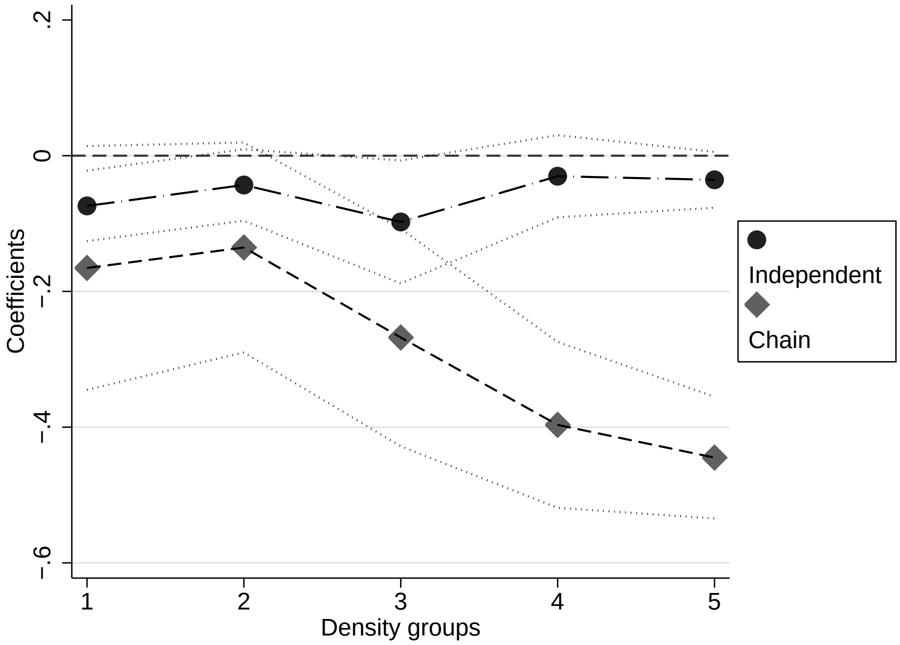

Figure 6: Pass-through according to restaurant density

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.

significant (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 the close neighbors but because restaurants which (for other reasons) are more responsive to tax cuts are selected into denser areas.

Further results in Table 5 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.[24]

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

Table 5: 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.

5.2 Heterogeneity within the chain category

In Appendix B, Table 7, we investigate which types of chains were the most responsive to the reforms. We do this by interacting the chain and treatment dummies with each other and with additional variables of interest. We focus on the chain restaurants here, since based on our main results in Table 3, we already know that most of price changes occur among them.

We first show that the pass-through for chain restaurants that changed 1-3 surveyed meals across the reform was similar to the pass-though for those that left all prices unchanged. The pass-through is somewhat smaller (for the remaining meals) among those that changed more than 3 meals. These results have two implications. First, models relying on fixed costs of changing anything on the menu, as Midrigan (2011), would predict that firms who changed at least one meal could reset any price on the menu without frictions, and that does not seem to be the case. Second, if our data were compromised by outdated web-pages, we should see larger price responses when page updates were confirmed through meal changes. The results, if anything, point in the opposite direction.

The appendix table separates the pass-through by pre-reform price quartiles (controlled for in Table 3), showing that the lowest quartile responded the most to the VAT rate cuts. This is consistent with fast-food and lunch chains being responsible for the highest pass-through. In column (3) we investigate the importance of being located in a mall. These chains have larger pass-through than other chains, confirming the result in Figure 14. The results also show that franchises have a larger pass-through than other chains. This result is interesting, since being a franchise is likely to be correlated with the size of the network that the chain forms. Finally, we interacted the chain and treatment variable with our restaurant density dummies; the results confirm the intuition of Figure 6.

5.3 Reported quantities and inputs: evidence from tax registers

The evidence so far points to drastically different pass-through for independents and chains. Here we complement this picture by utilizing administrative data originating from tax authorities in order to investigate how inputs and outputs change with the reform for the two types of firms.

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.[25]

For expositional reasons we normalize the series at zero four quarters before the reforms (inputs are measured on the right-hand scale) 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,

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

changes in meal quality are difficult to achieve without adjusting the input costs. Since the inputs develop similarly for the two types of restaurants, quality responses appear to be an unlikely explanation for the observed differences in price pass-through.

In contrast to the credited inputs, it is clear that the reduced VAT-rates generated clear drops in remitted VAT for both groups. Thus, behavioral effects (increased sales or decreases in 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.

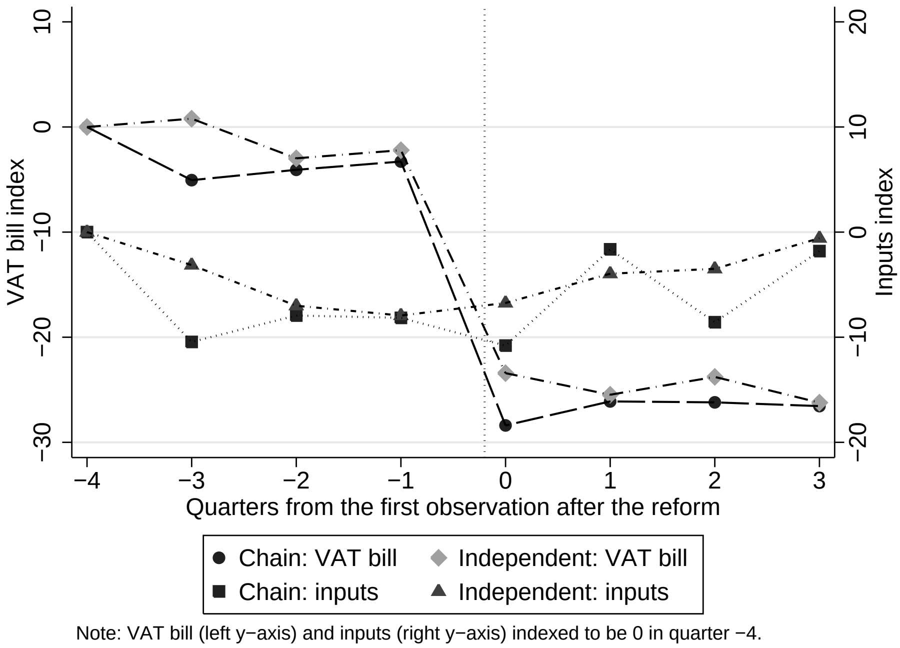

Figure 7: Inputs and VAT remittances, by quarter relative to the reform
Note: Coefficients of quarter indicators in a regression where the dependent variable is the log 4-quarter change in VAT bills and inputs credited against VAT by restaurant type. Based on administrative data for the surveyed firms.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

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 8. 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.[26] The final panel shows the 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 8) are imprecise since the firm-level revenue data are extremely volatile (as shown by Figure 15 in the 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.[27] 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.

[26] The result indicates a small decline in revenues also for both types of firms after the reform, but it is important to note that we do not have an external control group for this analysis.

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

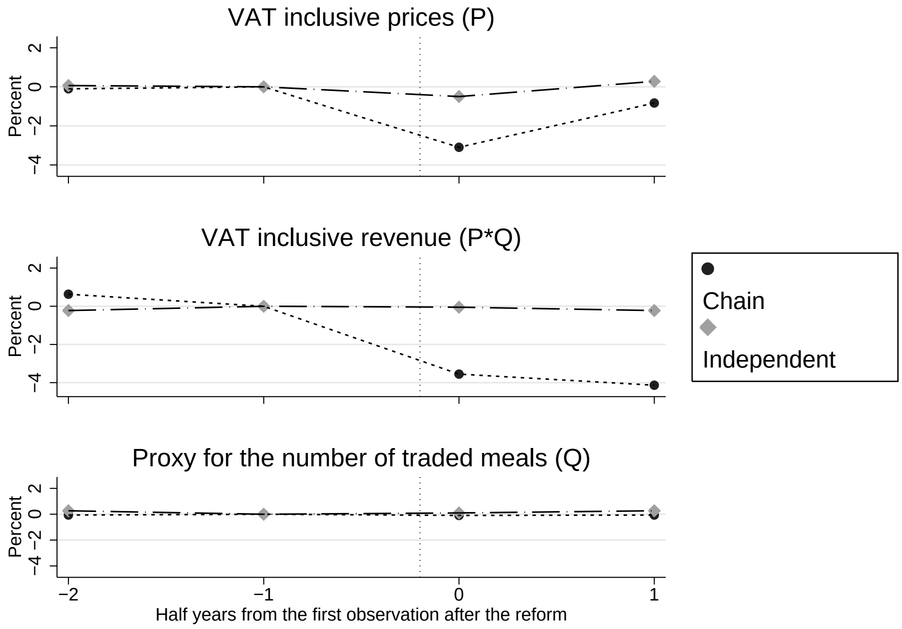

Figure 8: Changes in log consumer prices, VAT inclusive revenue and quantity

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 consist of only surveyed firms. Dotted line marks the reform.

5.4 Round number pricing

The results presented so far suggest that independent firms are less likely to respond to VAT reforms than restaurants that belong to chains or franchises, even when operating within what appears to be the same market segment. One possible explanation for this pattern is that independent firms have less precise pricing strategies and rely on crude price targets instead. This could be motivated by the complexity of figuring out the exact parameters of the demand function that these restaurants are facing (in particular since their economic environment appears to be very volatile), or because their objective functions are different. A first piece of suggestive evidence in this direction is provided by the fact that independents appear to change prices less often in normal (non-reform) times as well; see the discussion 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 literature analyzes the lack of round number pricing as evidence of strategic price setting, see e.g. 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).[28] Our main interest is in contrasting the incidence of round prices of (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 show 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 amongst the independent restaurants whereas the corresponding number for chains is just above 20 percent.

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 of the restaurant. Table 9 in Appendix B presents the results from regressions where the dependent variable is a dummy for round prices. This outcome is regressed against the independent dummy and an extensive set of controls (including the price range). Although several of the covariates help explain the round number pricing, the largest (and most statistically precise) estimate is for the independent dummy.[29] Independent restaurants are 29 percentage points more likely to use round number prices than chain restaurants and the differences remain stable and statistically significant when more covariates are added.

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

[29] Restaurants with local competition have less 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 meal prices.

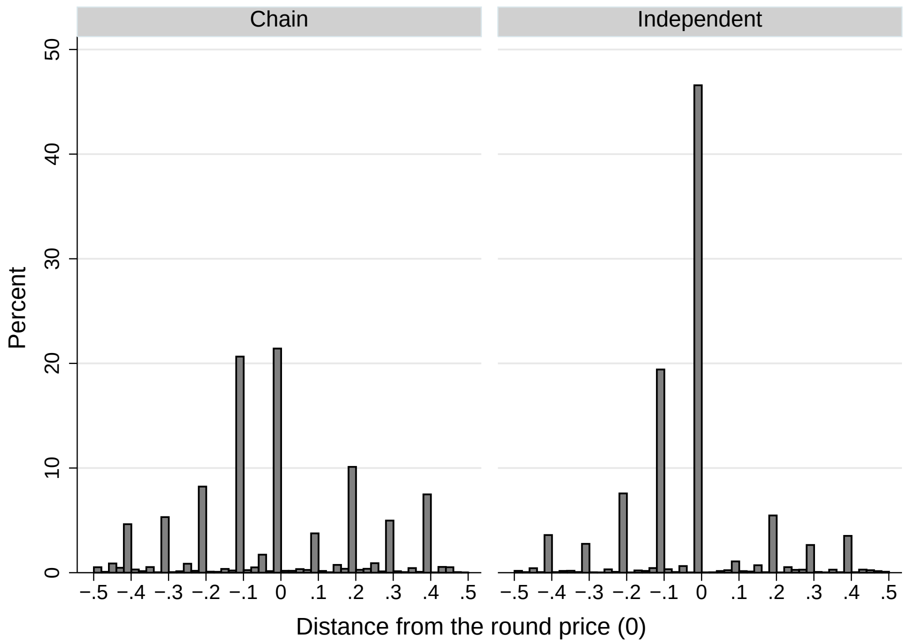

Figure 9: Round number pricing by type

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.

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.[30]

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 Estonian Krooni (EEK) to Euro. This is an interesting experiment since currency conversions are

[30] Anecdotes from advertisements suggest that this may have been the case.

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. Our conjecture is that 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.

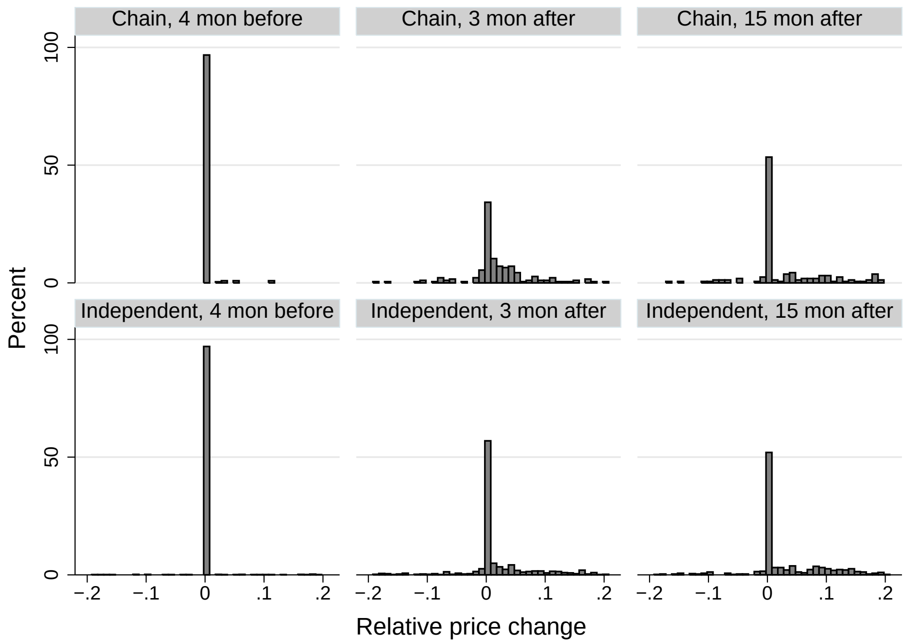

Figure 10: Relative price changes around Estonian currency conversion
Note: Meal price changes for Estonian restaurants before, during, and after Estonia joined the Euro-zone.

The resulting relative price change distributions are shown in Figure 10. 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 results in Appendix B, Table 10 confirm the intuition of the Figure. The outcome in the regressions is a dummy for whether or not the restaurant increased prices by 0.5 percent or more. The estimated interaction term shows that independents were 17 percent less likely to change their prices during the currency change than chains were. The result is robust to including additional control variables.

6 Conclusions

The previous literature on tax incidence has been heavily focused on cases where tax incidence only depends on market level conditions such as the elasticities of demand and supply, and the degree of competition (e.g.Weyl and Fabinger 2013). In this paper we have instead documented that different types of firms respond very differently to consumption tax reforms.

Our results from two restaurant-VAT 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. Restaurant ownership structure explains a significant part of this pattern. Almost all of the independent restaurants kept their prices constant and thus effectively ignored the reform.

Contrary to the finding for the independents, 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 difference between restaurant types.

Given that independent restaurants are likely to be run by entrepreneurs or managers who need to concentrate on many other tasks than just pricing strategies, one possible explanation is that these firms use much cruder pricing strategies. Several findings support the notion of widely different pricing strategies: Apart from the low impact of the reform, we also find that independents are less likely to change their prices and rely more on round number pricing. In contrast, chain restaurants coordinate their price responses across different sites and had a much higher probability to increase their prices during a currency conversion.

It is notable that the pass-through pattern for restaurants belonging to chains, on average, is more in line with expectations from standard models in the sense that they reduced their prices in response to the reform. What is less expected is that, within this restaurant class, some restaurants fully shifted the tax reduction to prices, while others did not react at all to the reforms. This leads to an expected average pass-through, but the dichotomous anatomy of these price changes does not follow directly from standard theory. One possible explanation is that restaurants that responded by a full pass-through did so for strategic reasons, either relative to customer responses or as part of a coordinated effort to ensure that policy makers kept the reduced VAT rates in the future. The fact that that many of the restaurants with a full short-run pass-through reverted their prices within 3 to 6 month after the reform is consistent with this explanation.

Overall our results signify that tax incidence depends on the types of firms populating the market. This result will be important to take into account in future studies of tax incidence. In particular, it highlights the usefulness of collecting firm-level data when analyzing consumption tax reforms. Moreover, the results suggest that policymakers should take the firm-type distributions into account when forecasting the impact of potential consumption tax reforms.

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For Online Publication

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 due to not always finding 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 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 6: Pass-through, separately by reform

	(1)	(2)	(3)	(4)
	Finnish reform Pass-through	Finnish reform Pass-through	Swedish reform Pass-through	Swedish reform 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 *Treatment		0.534***		0.167***
		(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 7: Average pass-through with additional interaction terms

	(1)	(2)	(3)	(4)	(5)	(6)
	Pass-thr	Pass-thr	Pass-thr	Pass-thr	Pass-thr	Pass-thr
Treatment	-0.099**	-0.104**	-0.102**	-0.100**	-0.112***	-0.119***
	(0.045)	(0.046)	(0.045)	(0.040)	(0.042)	(0.040)
Chain	0.096	0.090	0.088	0.089	0.087	0.089
	(0.067)	(0.070)	(0.067)	(0.067)	(0.063)	(0.065)
Chain*	-0.513***	-0.710***	-0.396**	-0.424*	-0.264	-0.416***
treat	(0.197)	(0.164)	(0.199)	(0.225)	(0.188)	(0.121)
Changed meal: 3 categ., ref: no meal changes						
1-3 changed	-0.039					-0.048
* Chain * treat	(0.113)					(0.121)
>3 changed	0.140**					0.048
* Chain * treat	(0.070)					(0.061)
Price quartile: ref. smallest						
2 * Chain * treat		0.294***				0.224***
		(0.025)				(0.051)
3 * Chain * treat		0.338***				0.228**
		(0.100)				(0.109)
4 * Chain * treat		0.350***				0.209***
		(0.072)				(0.065)
Mall			-0.255***			-0.178**
* Chain * treat			(0.063)			(0.091)
Franchising				-0.235***		-0.061**
* Chain * treat				(0.072)		(0.029)
Density: no. rest. quartile, ref: smallest						
2 * Chain * treat					-0.065	-0.057
					(0.109)	(0.101)
3 * Chain * treat					-0.231**	-0.229***
					(0.099)	(0.087)
4 * Chain * treat					-0.533***	-0.432**
					(0.140)	(0.179)
N	10,335	10,335	10,335	10,335	10,335	10,335
R^2	0.067	0.071	0.069	0.067	0.076	0.083

Note: Dependent variable is Δ of equation (1). Block bootstrapped standard errors with country, reform, and treatment level clusters. *** p<0.01, ** p<0.05, * p<0.1.

Table 8: Results from administrative data comparing chains and independents

	(1)	(2)	(3)	(4)	(5)
	$\triangle$Log Inputs	$\triangle$Log VAT	$\triangle$Log C. price	$\triangle$Log P*Q	$\triangle$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 (similarly as is presented in the 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 have succeeded to collect prices. Block bootstrapped standard errors with municipality level clusters and 2000 replications: *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 9: 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)				
Ala 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 10: 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 Krooni 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.

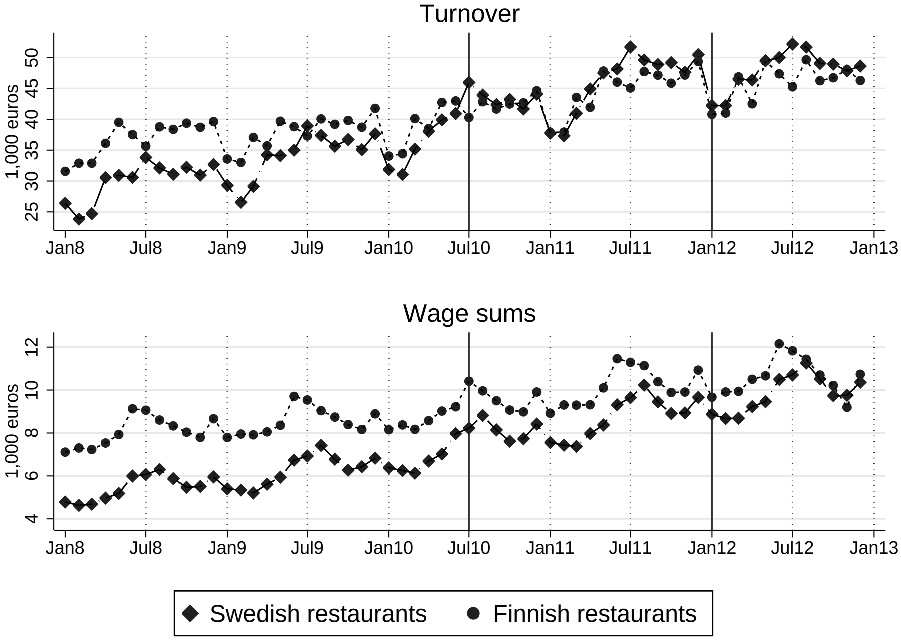


Figure 11: Longer-term development of average tax inclusive turnover of restaurants in Finland and Sweden

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).

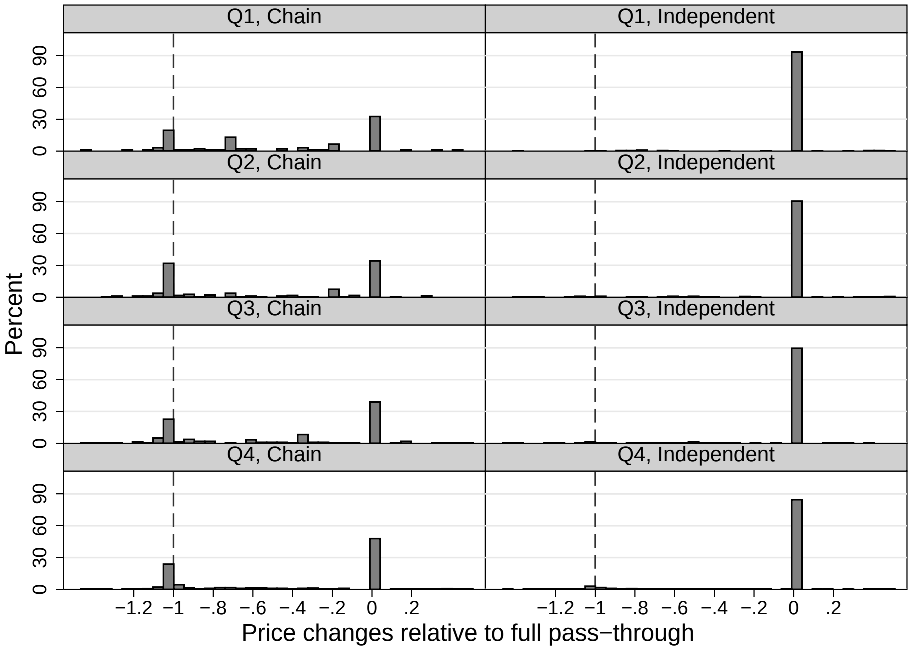
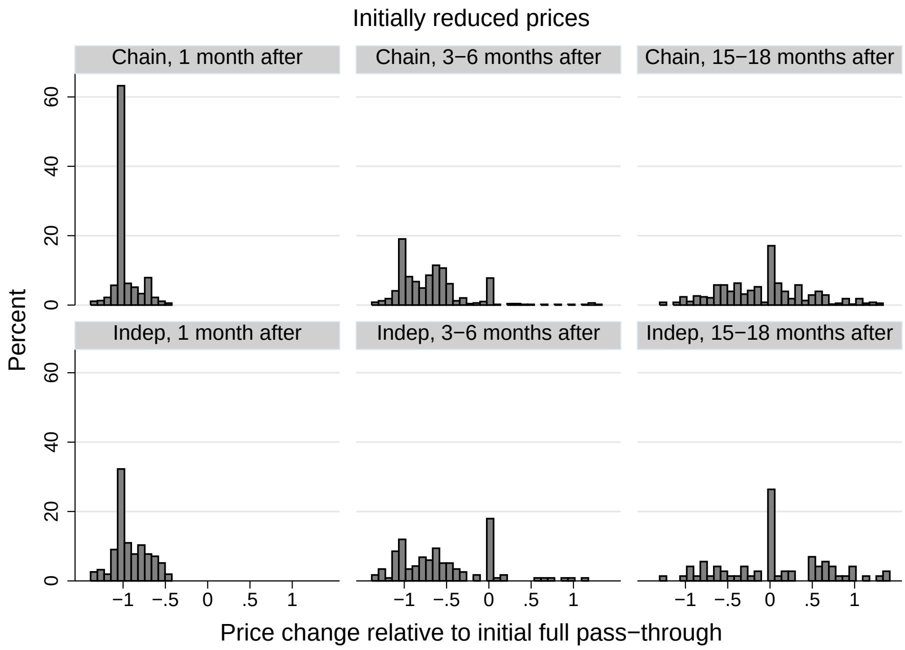


Figure 12: Short-run pass-through by price quartiles

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



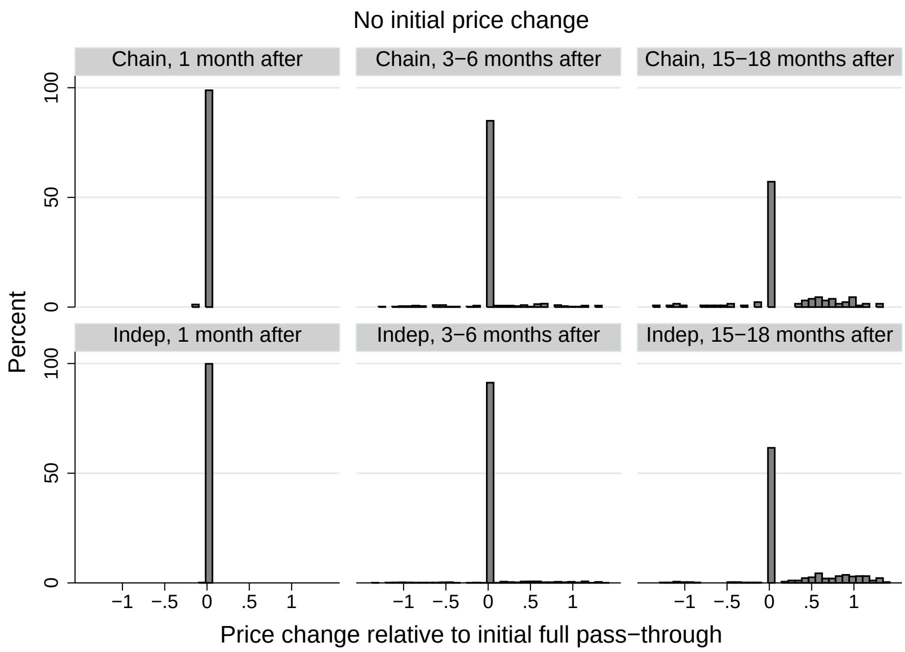


Figure 13: Medium-run pass-through divided by restaurant type, initial price change and collection rounds

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.

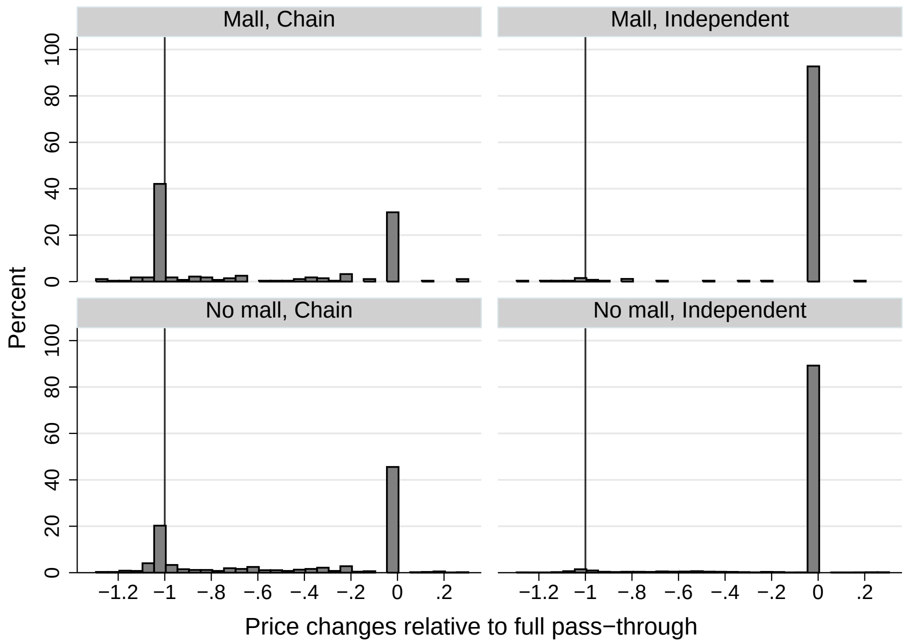


Figure 14: Short-run pass-through divided by restaurant type and restaurants located in malls

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.

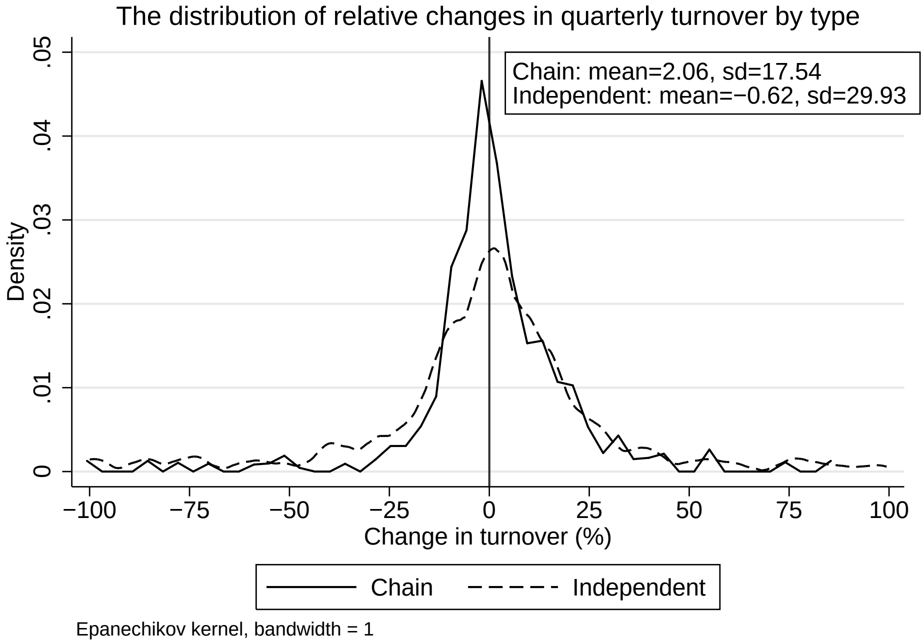


Figure 15: Changes in sales relative to own history

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 percent. The bandwidth is 1 percent.