How do national firms respond to local shocks? Evidence from excise taxes*

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Abstract

The pass-through of local shocks to prices is important for trade adjustment, inequality, and tax policy. Standard models assume that firms fully respond to these shocks, but recent research suggests that local prices may be insensitive to them, because national chains charge geographically uniform prices. Using data on beer, liquor, and cigarette sales in 31,000 retail stores, we examine the impact of 71 state excise tax changes passed between 2006 and 2016. Even though prices are largely uniform in the cross-section, we find clear responses of local prices to local excise tax changes. We find no spillovers to unaffected stores in affected chain, and pass-through rates in national chains are similar to pass-through rates in local chains, and the pass-through of a federal tax is similar to the pass-through of the state taxes. These facts are inconsistent with the predictions of simple models of uniform pricing. Our results suggest that much of the apparent uniformity in pricing is likely driven by retailers setting prices according to a uniform markup over wholesale costs.

JEL codes: D40, L11, L81

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Geographically dispersed firms are an important part of the economy. For example, retailers with multiple storefronts account for over 70 percent of sales and payroll (U.S. Bureau of the Census, 2012), while firms operating across multiple states make up over 68 percent of total employment (Giroud and Rauh, 2019). Many economic shocks and policies, however, occur locally. The Great Recession had heterogeneous labor market impacts across states (Yagan, 2019; Beraja et al., 2019); the housing boom of the early 2000s raised housing wealth more in some areas than others (Stroebel and Vavra, 2019); the fracking boom had large local impacts on income, wages, and amenities (Feyrer et al., 2017; Bartik et al., 2019); Chinese import competition had larger effects for some labor markets than others (Autor et al., 2016); minimum wages and other labor market policies are locally determined (e.g., Cengiz et al. (2019)). A final example is tax policy: states and municipalities set their own income, sales, and excise taxes, and these can vary substantially from state to state. For example, in 2018, state cigarette excise taxes varied from \$0.17 per pack in Missouri to \$4.35 in New York (Orzechowski and Walker, 2019).

How do national firms respond to these local shocks? The price response to changes in policy and other economic shocks is a critical determinant of their incidence and their ultimate welfare consequences. Textbook models assume that firms set local prices optimally, with different demand, costs, or policies implying different prices across markets. Yet in a recent, important paper, DellaVigna and Gentzkow (2019) document the uniform pricing puzzle that retail prices are uniform within a retail chain, in the sense that they vary much less within chain than across chain, and are uncorrelated with large cross-store differences in demand conditions. This pattern also exists for home improvement products (Adams and Williams, 2019), and is consistent with other evidence that chains account for much of the variation in prices, even among identical products and markets (Hitsch et al., 2017). A natural interpretation of this evidence is that firms are unwilling or unable to charge different prices in different markets. If true, this uniform pricing constraint has dramatic implications: a local cost shock would have only a small effect on local prices relative to a national shock of the same size, but it would have a spillover effect, as firms would raise prices both in stores exposed to the shock and in unexposed, far away stores.

We study how national grocery, food, and drug chains respond to local shocks in the form of excise taxes. Excise taxes, levied at the wholesale stage, are shocks to retailers' marginal costs, and are set by local municipalities. We estimate chain- and product-specific pass-through rates for each of 71 changes in state excise taxes for beer, liquor, and cigarettes. We find, first, that stores directly exposed to the excise tax change (because they are in the state with the change) have a clear response, with pass-through rates around 1, meaning a $1 tax increase typically results in a $1 price increase. We find no evidence that these local shocks spill over to unexposed, out-of-state stores in exposed chains, as the median pass-through rate among unexposed stores is 0.02. We benchmark our estimated pass-through rates of local taxes against the pass-through rate of a federal excise tax increase. The response to the federal increase is only slightly larger than the response to local taxes. Thus firms respond similarly to local and to national cost shocks. To reconcile this finding with the fact that local prices seem unresponsive to local demand conditions, we argue that firms may set prices as a uniform markup over marginal cost.

Excise taxes are important to study for several reasons. First, they are set locally, with substantial variation across states and even municipalities, making it possible to study local responses of national chains. Second, in 2018 excise taxes generated $200 billion in revenue for state and local governments and another $100 billion for the federal government,[1] making them an important source of revenue for many governments. How the burden of this tax is split between retailers and consumers will be determined by the pass-through of excise taxes to final prices. Third, recent excise tax increases—on alcohol, cigarettes, and especially sugar-sweetened beverages—are intended to change behavior rather than raise revenue per se. The welfare consequences of these taxes will depend on how retail prices respond (Allcott et al., 2019). Finally, as excise taxes are levied on producers, they represent a cost shock, while most of the existing research on uniform pricing has typically focused on demand shocks. Consequently, looking at excise tax pass-through helps complete the picture of how retail chains set prices.

To structure our analysis, in Section 1, we develop a model of price setting by a multi-market monopolist. We consider two cases: a flexible monopolist who can charge any

[1] See Table 3.3 in Bureau of Economic Analysis (2019).

price in any market, and a uniform-pricing monopolist constrained to charge the same price in all markets. For a flexible monopolist, the pass-through rate of a tax increase in one market depends only on the demand it faces in that market, and in general the pass-through could be above or below one. For a uniform-pricing monopolist, however, we develop two testable implications. First, a tax increase in one market raises prices in every market. We call this a spillover to unexposed stores in an exposed chain. Second, the pass-through rate in the affected market is greater for a monopolist more exposed to the tax increase, i.e., with a greater share of its sales in that market.

We test these predictions using the Nielsen Retail Scanner data, as described in Section 2. These data contain weekly revenue and quantity data at the store-product level. The data offer several advantages. They are national in scope so we can examine a wide set of policy changes. They are highly detailed, with product-specific information on both revenues and quantities, allowing us to identify pass-through effects free of aggregation bias. We limit our sample to widely available products. We focus on 32,000 stores in 77 chains with sales of beer, liquor, or cigarettes (because these product categories are the ones most commonly subject to excise taxes). Collectively, the stores in our sample are exposed to 71 excise tax changes. While these tax changes can be large, most chains are not highly exposed to any one tax increase. For example, Washington state increased its beer tax to $1.13 in June 2010. The average chain operating in Washington in our sample, however, sold beer across 19 states, and had only 23 percent of its beer revenue come from its stores in Washington. This suggests substantial scope for uniform pricing to attenuate the pass-through of the tax increase.

We begin our empirical analysis, in Section 3, with a detailed difference-in-differences analysis of this beer tax increase in Washington. This tax increase is a natural starting point for two reasons. First, it is a large tax increase, equivalent to roughly 5 percent of the baseline retail price, giving us power to detect effects in a simple framework. Second, and more importantly, the focus lets us avoid a potential identification challenge that our model highlights. The challenge is that, under uniform pricing, there are spillovers to *unexposed* stores in *exposed* chains. For example, Safeway has stores in Washington and

Idaho.[2] While the Idaho stores are not directly exposed to the Washington state excise tax increase, their prices may be affected because their chain has stores in Washington. Thus, the best controls for the difference-in-difference approach are stores in chains with no Washington presence. However, these stores may themselves respond to other states' tax changes, if other states change their taxes around the same time as Washington. Fortunately, there are no other state excise tax changes within a 21-month window of Washington's, so our control group is not contaminated by other state tax changes.

The Washington case study reveals four important findings. First, exposed stores in multi-state chains respond sharply and clearly to the tax, with an implied pass-through rate of 1.25. Second, unexposed stores in exposed chains—such as a Safeway in Idaho—do not respond to the Washington tax increase; their average pass-through rate is nearly zero. Third, we find no evidence that more exposed chains respond more strongly to the tax increase. Fourth, we find implicit evidence that pass-through rates to local shocks are not attenuated compared to national shocks. One chain in our data is essentially local to Washington; 93 percent of its sales are in Washington. For this chain the tax change is just like a national shock and the pass-through rate is 1.3, which is similar to that of the five national chains operating in Washington and elsewhere, 1.2.

Overall, therefore, the Washington case study provides evidence most consistent with the flexible pricing view. In Section 4, we investigate the generality of this finding by studying all state beer, liquor, and cigarette excise tax changes. We study an additional six beer tax changes, four liquor tax changes, and 60 cigarette tax changes in an event-study framework, matching the exposed chains to a control group of stores in states without any contemporaneous tax changes.

The analysis of all excise tax changes confirms the key findings from the Washington case study. Our framework lets us estimate pass-through rates that vary by event, product, and chain. Looking across all events, there is clear evidence of local price responses, even among national chains, with a median and mean pass-through of about 1. We find no spillovers to unexposed stores in exposed chains, and we find the pass-through rate is

[2] We do not know the identity of the chains in our data, so this example is merely illustrative, as are all cases where we mention retailers by name.

not higher for chains or products that are more exposed to the tax change. Finally, we see a remarkably similar pass-through rate among national and local chains. Defining "national" chains as ones with a local product share of less than 20 percent, and "local chains" as ones with a share above 90 percent, we find that the distribution of pass-through rates are extremely similar for both groups. This comparison is important because it lets us examine, indirectly, whether national chains respond similarly to local and national shocks.

As a final comparison, we look at the pass-through of a *national* tax change: a $0.62 increase in the federal excise tax on cigarettes in 2009. This national shock is large enough that we can credibly estimate its effect chain-by-chain, and compare chain specific pass-through of the national shock to chain-specific pass-through of local shocks, for the 35 chains for which we estimate state cigarette tax pass-through. A difficulty with estimating pass-through of this shock, however, is that *producer* prices increased just before the tax change. When we do not account for these changing input prices, we estimate a pass-through rate of about 1.4 for the federal tax, versus 1.1 for the state taxes. When we adjust for producer prices, we estimate a federal pass through of 0.95. Regardless of which approach we take, the pass-through of federal and state excise taxes are fairly similar, much closer than we would expect under simple models of uniform pricing.

Overall, therefore, our results are most consistent with the flexible pricing model, and provide clear evidence against the view that uniform pricing attenuates the local effects of local excise taxes. Although this might appear inconsistent with the uniform pricing results in DellaVigna and Gentzkow (2019), in Section 5, we offer a simple reconciliation: we hypothesize that retail chains set prices as a uniform percent markup over marginal cost. Because marginal costs typically vary little within chain (Stroebel and Vavra, 2019), this rule generates uniform pricing across markets, but also implies that only exposed stores respond to local cost shocks. We show that this hypothesis is consistent both with our data, as pass-through rats vary across chains but do not vary with demand or competition conditions across stores within a chain. The hypothesis is also consistent with many recent findings on retail prices (e.g. Eichenbaum et al. (2011); Gopinath et al. (2011); McShane et al. (2016); Hitsch et al. (2017); DellaVigna and Gentzkow (2019); Adams and Williams (2019); Arcidiacono et al. (2020)). The possibility of a uniform markup for private chains echoes the findings of Miravete et al. (2020) for Pennsylvania's state-run liquor store.

Uniform markups have at least two important implications. First, as national chains appear to respond fully to local excise tax changes, they suggest that uniform pricing does not attenuate the response to many sin taxes, an important finding for policy makers hoping these taxes change behavior. Second, they imply that the economic incidence of a tax may depend on its statutory incidence. A tax levied on the demand side of the market could affect prices through changes in demand, but uniform markups suggest that prices would not respond to such a tax under constant marginal costs.

These findings contribute to three literatures. First, we contribute to the large literature that examines how retail prices respond to policy, particularly excise taxes. Much of this literature has focused on a single or small number of tax changes and asked whether pass-through rates are above or below 1. For example Kenkel (2005) and Conlon and Rao (2019) find liquor taxes can have a pass-through rate well above 1. Cigarette pass-through rates are often lower, in part because of the possibility of purchasing in low-tax states, on Indian reservations, or on the internet (Keeler et al., 1996; Hanson and Sullivan, 2009; Goolsbee et al., 2010; Harding et al., 2012; Carpenter and Mathes, 2016).[3] Recent local taxes on sugar-sweetened beverages or sweets also produce pass-through rates around 1, albeit with mixed consumption effects (Cawley and Frisvold, 2017; Cawley et al., 2020, 2018; Kosonen and Savolainen, 2019). Also relevant is recent research examining how minimum wages affect grocery prices (Leung, 2018; Renkin et al., 2019), finding at most modest impacts. [4]

We contribute to this literature in three ways. First, we provide a comprehensive analysis of how retail chains responded to the most recent set of excise tax changes. Second, we investigate the heterogeneity in pass-through, finding no evidence that national chains respond less than local chains, and finding only slightly smaller responses to local than

[3]This literature typically focuses on retail prices. Rozema (2017) shows that whole sale and retail prices exhibit very similar pass-through rates of cigarette taxes.

[4]Renkin et al. (2019) argue that the observed response is consistent with full pass-through, while Leung (2018) argues that larger grocery chains' responses are attenuated because of uniform pricing frictions. These findings are not in conflict with our results because the minimum wage affects both demand and cost, whereas we study the response to a pure cost shock.

to national tax changes. Third, the literature until now has not considered the possibility that uniform pricing within chains might cause spillovers to untreated states. In principle, such spillovers could substantially bias estimated pass-through rates, but we show that such spillovers are not a concern in practice.

We contribute to a second literature examining tax incidence when economic actors may not be fully rational. Chetty et al. (2009) and Taubinsky and Rees-Jones (2018) relax the standard assumption that households are fully aware of taxes, exploring incidence when taxes are not fully salient. Kopczuk et al. (2016) show that levying a tax on people with a greater ability to evade can change the incidence of the tax. These papers, like most of the tax incidence literature, assume firms fully react to the tax. An important exception is Harju et al. (2018), who find that independent restaurants do not respond to VAT reductions in Sweden and Finland, but chain restaurants show near-complete pass-through. That result is consistent with our finding of high excise tax pass-through among retail chains. Harju et al. (2018) do not consider, however, how heterogeneous exposure affects pass-through. Our findings that national chains react roughly fully to local shocks provide support for the assumption that firms fully react to taxes.

Finally, we contribute to a literature on uniform pricing puzzles. Several papers document that prices vary too little despite large demand variation. For example, movie ticket prices do not vary across movies (Orbach and Einav, 2007), rental car prices do not vary with the age of the car (Cho and Rust, 2010), drywall prices do not vary within large geographic markets (Adams and Williams, 2019), retail prices of a given product vary little across stores within a chain (DellaVigna and Gentzkow, 2019; Hitsch et al., 2017) or between online and offline (Cavallo, 2017), and grocery stores responded little to large demand shocks (Gagnon and Lopez-Salido, 2019). The literature has largely focused on local demand shocks, and the evidence is sometimes interpreted to mean that firms do not respond locally to *any* local cost shocks. We show, however, that national firms respond to local cost shocks by raising local prices and not other prices.

1 Modelling national chains' response to local prices

We construct a model that shows how the pass-through of local shocks differs with the presence (or lack) of uniform pricing by the retailer. While simple, the model is analytically tractable and lays out a set of empirically testable implications that serve as the basis for the remainder of the paper. To provide a concise set of results, we focus on the pricing decision of a multi-market monopolist. Relaxing the assumption of the monopolistic market structure, however, to more general market structures would not affect the qualitative aspects of the results (Weyl and Fabinger, 2013).[5]

A monopolist faces demand for its good across N distinct markets, $x_m(p_m)$, where demand in market m only depends on the price charged in that location, p_m, and so at times we omit the market specific subscript. We assume these demand functions are twice continuously differentiable. We denote the price elasticity of demand as $\varepsilon(p) \equiv -(p/x)x'$, where x' is the first derivative of the demand function. We denote the convexity of demand as $\zeta(p) \equiv -p(x''/x')$, where x'' is the second derivative of demand. The firm faces constant marginal costs of supplying goods to each market given by c_m.

1.1 Flexible pricing benchmark

To provide a basis for comparison, we present the results of the flexible price benchmark. The monopolist sets prices in each local market to maximize profits in each of the separate markets that it serves according to the following objective function:

$$\max_{p_1,\ldots,p_N} \sum_m \left[p_m - c_m\right] x_m(p_m)$$

which leads to the usual characterization of optimal prices:

$$p_m^\star \;=\; \frac{\varepsilon_m}{\varepsilon_m - 1} c_m.$$

[5]For further discussion as well as a more explicit derivation of the intermediate steps of the results of this section, see Section A in the appendix.

The pass-through of any market n's change in marginal cost on market m's price is given by:

$$\rho_{mn} \equiv \frac{dp_m}{dc_n} = \begin{cases} \frac{1}{2-\zeta_n/\varepsilon_n} & \text{if } n = m \\ 0 & \text{otherwise.} \end{cases}$$

This expression reveals three important implications of flexible pricing. First, the pass-through of a local shock to local prices depends on the curvature of demand (a celebrated result; see Bulow and Pfleiderer (1983); Weyl and Fabinger (2013); Mrázová and Neary (2017)), not only on its level or elasticity. In general, the pass-through rate can be above or below 1, and it is not pinned down by the elasticity except under functional form restrictions. Second, a national cost-shock, which raises all costs by the same amount, has the same local pass-through rate as a local cost-shock. Third, there are no cross-market spillovers–a cost change in one market does not affect the prices in other markets.

1.2 Uniform pricing

Recent evidence suggests that prices may not be fully flexible across markets (DellaVigna and Gentzkow, 2019). We derive implications of uniform pricing for pass-through of "local" costs. Specifically, we examine a monopolist that is constrained to only choose one price for its product across the N markets. In this case, the firm chooses a single price ($\overline{p}$) to maximize its aggregate profits across all of the markets it serves according to the following objective function:

$$\max_{\overline{p}} \sum_m [\overline{p} - c_m]\, x_m(\overline{p})$$

The pass-through to prices in market m of a marginal cost change in market n (ρ_{mn}), which by uniform pricing is the same for all markets m ($\overline{\rho}_n$) is given by:

$$\rho_{mn} = \overline{\rho}_n \equiv \frac{d\overline{p}}{dc_n} = \frac{s_n \varepsilon_n}{2\sum_m s_m \varepsilon_m - \sum_m s_m \left[\frac{\overline{p}-c_m}{\overline{p}}\right] \varepsilon_m \zeta_m}$$

where $s_m \equiv (x_m / \sum_n x_n)$ denotes share of quantity demanded in market m across the N markets.[6] This pass-through rate condition under uniform pricing provides the basis for our first empirical testable condition.

Prediction 1. *Under uniform pricing, the pass-through rates of a "local" cost shock in all the markets served by the monopolist are greater than zero.*

The first empirically testable implication of uniform pricing allows for a variety of heterogeneity across establishments including marginal costs, demand elasticities, and/or curvature across markets. Broadly speaking, to the extent that firms adopt the optimal uniform pricing strategy across the establishments in different markets, the price response to a cost shock in one market is transmitted to other markets otherwise unaffected by the shock.

For the next set of results, we put additional structure on the form of demand and the sort of heterogeneity across markets the monopolist faces.

Assumption 1. *The demand in each market m, $x_m(p)$, is multiplicatively separable in a heterogeneous "market size," φ_m, and a common individual demand function $\tilde{x}(p)$. Thus, the demand in each market m is given by $x_m(p) = \varphi_m \tilde{x}(p)$.*

This assumption is consistent with commonly used demand forms including constant elasticity and linear demand. It is restrictive, however, in that it assumes the elasticity and curvature of demand to be constant within a firm across markets (although both can vary with the level of price); we denote the firm-wide elasticity $\tilde{\varepsilon}$ and the firm-wide curvature $\tilde{\zeta}$.[7]

Under this assumption, the pass-through rate of the monopolist's price from a "local" cost shock in market n across all the markets it serves takes a particularly simple form:

$$\overline{\rho}_n \quad = \quad \frac{s_n}{2 - \tilde{\zeta}/\tilde{\varepsilon}}$$

[6] Implicitly, this characterization assumes the monopolist's uniform price $\overline{p} > c_m$ for all m.

[7] Assumption 1 assumes homogenous price elasticities across markets. This assumption deviates from the environment of DellaVigna and Gentzkow (2019), as their framework allows for heterogeneity on this dimension. Theoretically, the environment of DellaVigna and Gentzkow (2019) is not sufficient to guarantee that uniform pricing leads to an attenuation effect of the pass-through rate of marginal cost shocks generally, as this depends critically on the origin of the cost shock and how that market's price elasticity relates to the rest of the markets served by the monopolist.

which leads to two additional empirically testable conditions of uniform pricing.

Prediction 2. *Under uniform pricing and Assumption 1, the pass-through rate of a "local" cost shock in that market is attenuated compared to fully flexible pricing. The pass-through is proportional to the monopolist's "exposure" to the cost-shock, i.e., the share of the monopolist's demand in the market with the cost shock.*

A flexibly pricing monopolist treats the pricing decision of each market independently and consequently has a pass-through rate of $1/(2 - \tilde{\zeta}/\tilde{\varepsilon})$. Relative to this benchmark, a uniform pricing monopolist does not fully pass on a "local" cost shock ($s_n < 1$). The pass-through rate for a uniform pricing monopolist is attenuated by $1 - s_n$, the inverse of the monopolist's exposure to the shock.

Indirect evidence suggests that the spillover and attenuation effects of uniform pricing may be substantial. For example, DellaVigna and Gentzkow (2019) find in the simulations that "the dampening effect of uniform pricing on local price responses is dramatic" (p. 2077). Specifically, they use their estimated demand systems to simulate how uniform pricing retail chains would respond to state- or county-specific demand shocks, relative to equivalent national shocks. They find that state-specific shocks are attenuated by about two-thirds. Demand shocks in large states like California generate substantial spillovers, for example they substantially affect Nevada prices. These simulations indicate potentially important price effects of uniform pricing. The empirical goal in our paper is to provide direct evidence on these magnitudes.

Before turning to the empirical sections, however, we note an important identification concern raised by the model. A natural way to estimate local pass-through rates would be to take two stores in the same chain, with only one of them exposed to the cost shock. Indeed, in an elegant design, Cawley et al. (2018) take exactly this approach, studying the impact of Philadelphia's sugar-sweetened beverage tax on soda prices in Philadelphia's airport. The airport straddles the city border, and so some stores are subject to the tax and some stores are not. Although comparing stores across the city line appears very clean, under uniform pricing it will substantially understate the pass-through rate, because stores in the same chain but on both sides of the dividing line will respond equally.

Thus to estimate pass-through rates under uniform pricing, our control group needs to consist of completely clean stores, not belonging to chains exposed to the cost shock.[8]

2 Data

2.1 The Nielsen Retail Panel

Our main dataset is derived from the Nielsen Retail Scanner Data. It contains store-week-product level revenue and units sold over the period of 2006–2016. There are more than 42,000 stores and most of them are food, drug, or mass merchandise stores. They operate in 49 contiguous states in the mainland U.S. We restrict the set of chains, stores, and products studied. Because we study excise taxes on beer, liquor, and cigarettes, we limit the sample to stores selling at least one of these products. Stores in control states where only state-owned or operated stores are allowed to sell liquor or beer are still included in the sample as long as they sell one of the products we study.

We begin by selecting stores and chains. Our sample selection approach follows DellaVigna and Gentzkow (2019) closely. Nielsen records two "chain" identifiers: a parent code reflecting the umbrella company (such as Albertson's LLC parent company) and a retailer code reflecting the brand (such as Albertson's and Shaw's, both owned by Albertson's LLC). We define a chain as a unique parent-retailer combination. First, we exclude stores in the data for less than 104 weeks, as well as stores that switch chains over time. Next we restrict the chains studied to those present in the sample for at least eight years. In some cases the retailer codes are associated with multiple parent codes (possibly because of mergers). In those cases, we keep only the modal parent-retailer combinations if the modal combination accounts for at least 80 percent of stores given retailer code and drop all combinations otherwise.

Our final sample consists of 30,911 stores in 84 chains selling beer, 60 chains selling liquor, and 78 chains selling cigarettes. Table 1 summarizes the final sample. A majority of chains selling beer, liquor, and cigarettes are multi-state, and the average multi-state

[8] Cawley et al. (2018) were certainly sensitive to this concern; they also estimated models that excluded the untaxed stores.

Table 1: Summary statistics

Category	Beer	Liquor	Cigarettes
Number of products	24	9	9
Unit size	288 oz	750 ml	1 pack (20 cigs)
# Stores	19666	6507	29113
# Chains	84	60	78
# Multi-state chains	57	27	55
Mean # states among multi-state chains	6.95	5.22	7.22
Price per unit:			
Mean	24.66	15.55	5.30
25th percentile	24.05	15.40	4.55
Median	24.39	15.41	5.26
75th percentile	24.95	15.57	5.98

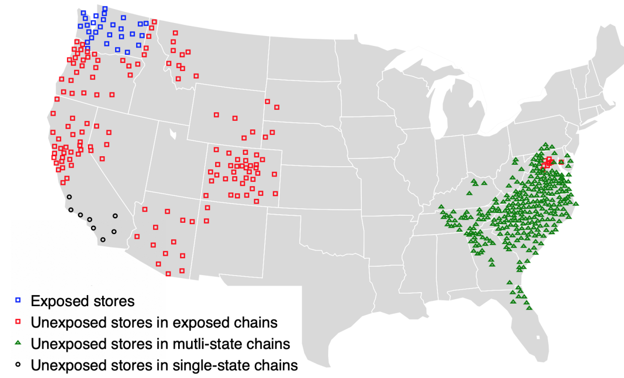
Note: Sample consists of grocery, drug, and mass merchandise selling the indicated product category. We limit the sample to widely available products as described in the text.

chain has stores in about 6 states. Thus for many chains, an excise tax change in one state will affect costs for only a minority of its stores.

A product is defined as a upc-version combination.[9] We select products which are widely available across geography and time because these products are less likely to have missing prices which occur in store-weeks with zero sales. We define availability of products by the share of store-weeks with positive sales among all store-weeks which have sold at least one product in the same module and are in the same store category (e.g., food stores) throughout 11 years. We pick the set of products with at least 80% availability in food stores or drug and mass merchandise stores. In the end, we have 24 beer, nine liquor, and nine cigarette products. We work with prices per standardized units, which are a 24 pack of beer, a 750-ml bottle of wine, and a pack of cigarettes. We show in Appendix B that pricing of these products appears uniform in the DellaVigna and Gentzkow (2019) sense.

[9]Some products have identical quantity per unit, pack size, size unit, UPC description and brand description. We aggregated these products and treated them as one UPC.

Figure 1: Example of exposed and unexposed stores for Washington's tax change



Note: This figure illustrates our definition of exposed stores, unexposed stores in exposed chains, unexposed stores in unexposed multi-state chains, and unexposed stores in single-state chains. We plot the counties in which stores operate, for one exposed chain, one unexposed multi-state chain, and one unexposed single-state chain. The size of each market is proportional to the number of stores in that county-chain.

3 Washington State Case Study

3.1 Background

We begin our empirical analysis with a detailed examination of Washington's state beer tax increase. On June 1, 2010, Washington state increased its beer tax by $0.50 per gallon, or $1.13 per 288 ounces. The tax increase was in part a response to a budget shortfall induced by the Great Recession.[10] The tax increase was temporary, with a planned (and implemented) expiration of July 1, 2013. Although many other states increased excise taxes in the wake of the Great Recession, no other states changed their beer tax in the period from nine months before the Washington increase until 12 months after. We use these 21 months as the sample period. This means that prices in other states should not be influenced by other taxes changing simultaneously.

To examine the effect of the tax increase, we divide stores in our data into one of four mutually exclusive groups. We define "exposed stores in exposed chains" as stores in Washington. In our data, there are eight exposed chains in Washington, and all of them are multi-state, so for simplicity we refer to exposed stores in exposed chains as "exposed

[10] See Washington Final Bill Report 6143-S.E2 SBR FBR 10 E1.

stores." We define "unexposed stores in exposed chains" as stores outside of Washington that belong to a chain with stores in Washington. To illustrate these definitions, we plot as blue and red squares all the markets in which a single chain in our data operates. The blue squares are in Washington and represent exposed stores. The red squares, outside of Washington, are unexposed stores in an exposed chain. We likewise define "unexposed stores in multi-state chains" as stores not in Washington that belong to a multi-state chain with no Washington stores. The green triangles illustrate one such set of stores. The final category is "unexposed stores in single-state chains;" the black circle on the map is an example. There is no single-state chain within Washington in the Nielsen data. We treat unexposed stores in unexposed chains—both multi- and single-state—as clean controls.

Table 2 provides some information about the exposed and unexposed stores and chains. There are 647 exposed stores in 8 chains; those chains also contain 9,614 unexposed stores. There are an additional 7,654 stores in unexposed chains, about 80 percent of which are in multi-state chains. The mean exposed chain has stores in 20 states, and less than a quarter of its beer sales come from Washington. The most exposed chain, however, has 92 percent of its sales from Washington.

3.2 Aggregate price response

We start by plotting in Figure 2 the differential price trends for exposed stores and unexposed stores in exposed chains, relative to unexposed stores in unexposed multi-state chains and in unexposed single-state chains. To construct the figure, we first residualized prices net of store-product and time-product fixed effects where time is measured in weeks during the 95 weeks of sample period. We then plot, for each month and each of the four groups, the average residualized price, averaging across stores and products. This figure shows trends because it is net of store-product means, and differential trends because it is net of time-product means.

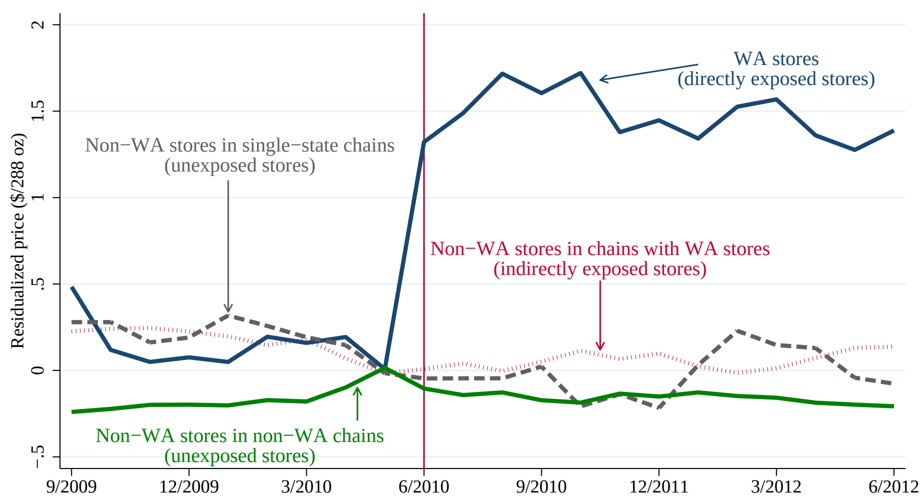
The figure shows several important facts. First, and most obviously, there is a clear jump in prices in the exposed stores. The jump is about $1.40. It happens within the month of the policy change and it is sustained for the 12 months after the policy change.

Table 2: Chains exposed and unexposed to the Washington tax increase

Group	# Stores	# Chains	# States Min.	# States Mean	# States Max.	Exposure (%) Min.	Exposure (%) Mean	Exposure (%) Max.
Exposed stores	647	8	2	20	38	0.00	0.23	0.92
Unexposed stores, exposed chains	9,614	8	2	20	38	0.00	0.23	0.92
Unexposed, multi-state chains	6,347	48	2	4	21	.	.	.
Unexposed, single-state chains	1,307	27	1	1	1	.	.	.
Exposed	646	8	2	19	36	0.00	0.23	0.93
In exposed chain	8,867	8	2	19	36	0.00	0.23	0.93
Unexposed, multi-state	5,229	43	2	4	19	0.00	0.00	0.00
Unexposed, single-state	1,366	30	1	1	1	0.00	0.00	0.00

Note: Exposed stores are stores in Washington; stores in exposed chains are not in Washington but belong to chains which own Washington stores. "Unexposed, multi-state" refers to multi-state chains with no Washington presence, and "Unexposed, single-state" refers to single-state chains (all of which are outside Washington). We report the minimum, median, and maximum number of states in which chains in the indicated group operate, and for exposed chains we report the minimum, median, and maximum exposure, defined as the share of the chain's beer revenue that comes from Washington, in the year prior to the tax increase.

Figure 2: Exposed stores responded to Washington's excise tax increase, but unexposed stores in exposed chains did not



Note: This figure plots the residual price, net of store-product and time-product fixed effects, averaging over stores and products in the indicated group and month, for 9 months before to 12 months after Washington's excise tax increase.

Such a rapid response is consistent with the fact that retailers regularly change their prices, alternating between promotional and base prices at weekly frequency and even changing their base prices multiple times per year (Nakamura and Steinsson, 2008). Second, there is essentially no trend in the clean controls (i.e., unexposed stores in unexposed chains) before or after the excise tax increase. This indicates that prices were moving roughly in parallel for the two groups prior to the tax increase, and it validates a difference-in-differences design. Interpreting the \$1.40 price increase in the exposed stores as the effect of the tax, the figure implies a pass-through rate of about 1.25 (=1.4/1.1). Third, the unexposed single-state stores move roughly in parallel with the clean controls, suggesting they may be a valid control for the multi-state chains. Fourth, the unexposed stores in exposed chains do not appear to respond to the tax at all, providing evidence against spill-over effects to other states. If anything, their prices fall slightly.

For exposed chains, we observe that the prices in WA stores converge to those in other states if the pre-price in WA was lower than in other states. On the other hand, price difference between WA and other states diverge if the pre-price in WA was higher than or equal to the price in other states. Figure D.1 in Appendix draws the price difference (and tax difference) pre and post-period for each multi-state chain.

3.3 Heterogeneous price responses

Figure 2 suggests clear pass-through for exposed stores and no pass-through for unexposed stores. However, the figure averages over all chains and products. Our model of uniform pricing implies heterogeneous responses across chains and products that are more exposed. We estimate heterogenous responses to the tax increase with the following regression for the price of product i in store s (belonging to chain c) and week t:

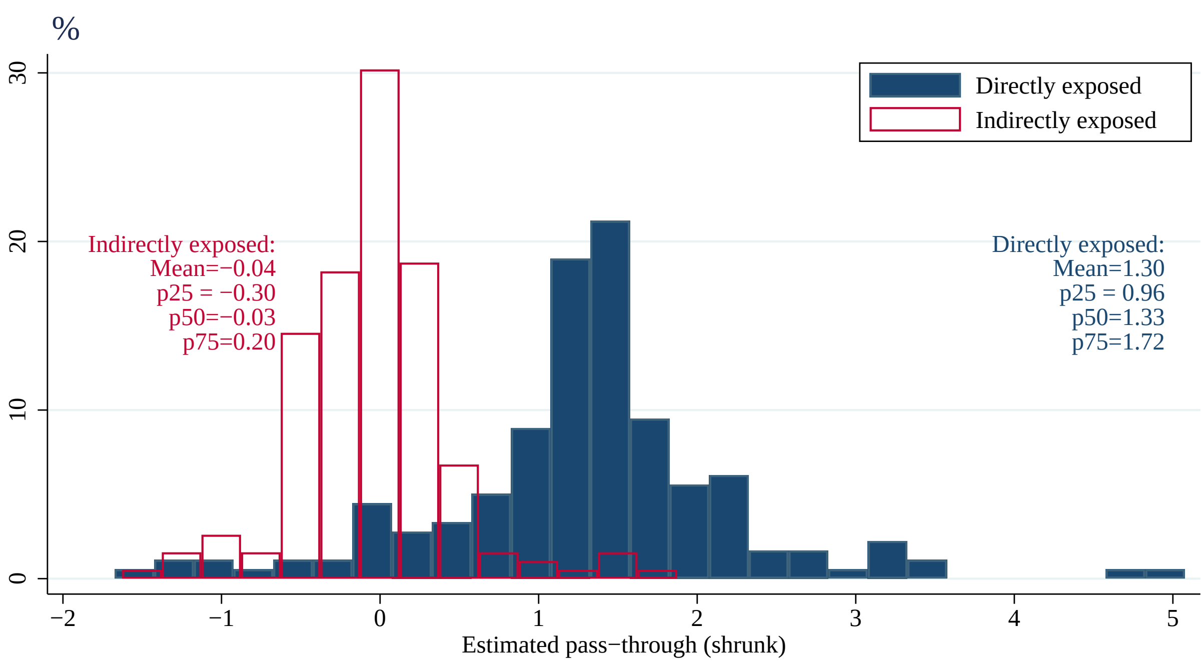
$$p_{ist} = \sum_{c \in \mathcal{C}} \left(\rho^1_{ic} \Delta_\tau \times Post_t \times Exposed_s + \rho^0_{ic} \Delta_\tau \times Post_t \times Unexposed_s \right) + \theta_{is} + \mu_{it} + \varepsilon_{ist}, \quad (1)$$

where c indexes chains, $\mathcal{C}$ is the set of exposed chains, $\Delta_\tau \equiv 1.13$ is the size of the tax increase, $post_t$ is a dummy variable indicating weeks after the tax change, $Exposed_s$ is an indicator for exposed stores, $Unexposed_s$ is an indicator for unexposed stores in exposed chains (i.e., clean control), and θ and μ are product-store and product-time fixed effects. The coefficients of interest, ρ^e_{jc} and ρ^u_{jc}, are heterogeneous pass-through rates, allowed to vary separately by chain c, by exposed and unexposed stores (e vs. u) and by product. We estimate this specification product-by-product, and for each product we obtain up to 16 pass-through parameters, two for each of the eight exposed chains, although not all chains sell all products. These pass-through rates are precisely estimated (the average standard errors is 0.09) although there are some that are not. Following DellaVigna and Gentzkow (2019), when we report the distribution of estimates we shrink the individual pass-through rate towards the mean estimate (separately for exposed and unexposed stores). See Appendix C for more details.

We plot the distribution of pass-through rates for exposed and unexposed stores in exposed chains in Figure 3. Although the estimates are heterogeneous, there is clear separation in the distributions. For unexposed stores, the distribution of pass-through rates is centered on zero, while for exposed stores, it is centered around 1.25. Despite the heterogeneity in pass-through, the histogram therefore confirms the implication of Figure 2 that pass-through rates are large and positive for exposed stores but small and centered on zero for unexposed stores.

Nonetheless, the figure does point to substantial heterogeneity in pass-through, with some exposed stores having low or even negative pass-through rates. Such heterogeneity is in principle consistent with our model of uniform pricing, because it implies that chains with near-zero exposure would have near-zero pass-through. We test this implication more explicitly in Figure 4. In panel A of the figure, we plot each product-chain's pass-through (among exposed stores) against that product-chain's exposure (i.e., the share of that product-chain's pre-period beer revenue in Washington). We see only a weak association between exposure and pass-through. We can see this point most clearly by considering the most exposed chain, which has an exposure of 0.93. This chain is very nearly a Washington-only chain, so it is expected to react to the local tax increase as if it were a national tax increase, according to uniform pricing. However, we see that this chain's pass-through is about 1.2, very close to the overall average pass-through rate, and lower than the pass-through rate for several chains with an exposure of 25 percent or less.

Figure 3: Distribution of pass-through for exposed and unexposed stores, WA tax increase



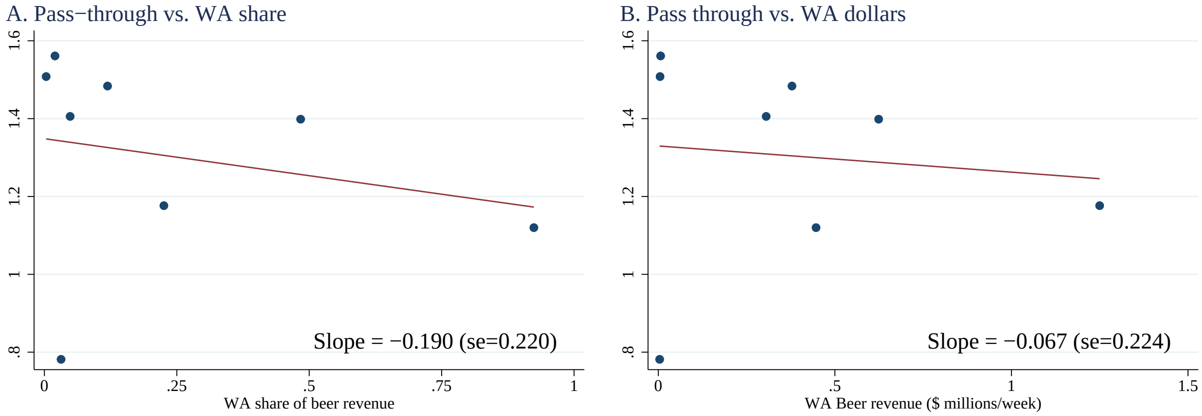
Note: This figure plots the distribution of (shrunk) estimated pass-through rates for exposed and unexposed stores, estimated via Equation 1. Figure reports mean and 25th, 50th, and 75th percentiles of estimated pass-through rates, for each group.

Among those five chains, the average pass-through is 1.2.

Although this share-based exposure measure is motivated by our model of uniform prices, a natural alternative measure is each chain's pre-period Washington revenue. Such a measure could be motivated by a model with fixed attention/noticing costs or price adjustment costs (i.e., menu costs). We show in Panel B of Figure 4 that product and chain-level pass-through rates are also uncorrelated with a dollar-based measure of exposure.[11]

[11]These figures plot chain-average pass-through against chain-level exposure measures. In Appendix Figure 4, we plot chain-product level pass-through against chain-product level exposure. As most of the variation in exposure is across chains, the product-level figures are similar but noisier.

Figure 4: Chain-level pass-through uncorrelated with exposure to the WA tax increase



Note: This figure plots the average chain-level pass-through (obtained as the simple average of product-specific pass-through estimates among exposed stores) against each chain's exposure to WA's tax increase, defined as its prior year share of beer revenue in Washington (Panel A) or prior year weekly average beer revenue in Washington (Panel B).

4 Analyzing all state-level excise tax changes

The Washington case study yields four results on the price response of national chains to a local cost shock. First, we see a clear response among exposed stores. Second, unexposed stores in exposed chains do not respond. Third, more exposed chains do not respond more sharply to the tax increase, and fourth, the local chain appears to respond similarly to the national chain. In this section we ask to what extent these conclusions generalize to a broader set of excise tax changes. It turns out that each of these results is general to examining a broad set of local tax changes.

4.1 The excise tax changes

We attempted to identify all state excise tax changes affecting alcohol and tobacco products over the period July 2006 to July 2016. We focused on this time period so that we would have at least six months of data before and after each tax change. We focused on state rather than local policies because localities as observed in the Nielsen do not

Table 3: Summary statistics on excise tax changes

Category	Beer	Liquor	Cigarettes
# Events	7	4	60
A. Distribution of absolute value tax change			
Mean	0.42	0.33	0.58
10th percentile	0.09	0.13	0.10
90th percentile	1.13	0.63	1.00
B. Characteristics of exposed chains			
# Chains	10	8	7
# Exposed stores	521	133	329
# Unexposed stores	6,487	1,279	10,643
Mean # states in which chain operates	14	5	17
C. Characteristics of unexposed chains			
# Multi-state chains	43	19	37
# Stores in multi-state chains	7,628	2,760	6,338
# Single-state chains	28	26	25
# Stores in single-state chains	1,249	826	1,069

Note: This table reports summary statistics for the tax events we study. Each event is a state excise tax change for a given category of products. The tax change is per 288 ounces of beer, per 750 ml of liquor, and per pack of cigarettes. We report the average number of exposed chains and exposed stores, as well as the average number of unexposed chains and unexposed stores (averaging across events within a category). Finally, the Mean # states in which a chain operates is mean across chain-events within a category.

always line up with localities that set excise taxes.[12] We focused on alcohol (beer, liquor, and wine) and tobacco because these products are the most common groceries subject to excise tax; alcohol and tobacco excise tax revenue account for 15 percent of state and local excise taxes.[13] Ultimately we did not include any wine tax changes because only a single wine product met our availability criteria, as wine is highly differentiated.

To identify cigarette tax changes, we used the CDC STATE System (Centers for Disease Control and Prevention, 2019), which provides a database of cigarette tax changes, with information on the size of the tax change as well as the date of enactment and implemen-

[12] For example, we considered studying the recently implemented soda tax in Berkeley, CA, but the "Berkeley" market in Nielsen includes non-Berkeley counties.

[13] 2014 Annual Surveys of State and Local Government Finances. Gasoline is also subject to an excise tax, but gasoline stations do not appear to engage in uniform pricing (e.g., Houde (2012)).

tation. To identify beer and liquor excise taxes, we started with data from the Brewer's Almanac (the source used by Chetty et al. (2009) and Ruhm (1996)), which provides a snapshot of current beer excise tax rates. We then used the Wayback Machine to obtain historical excise tax rates going back to 2005, as well as changes in these rates. We cross-checked the rate changes with annual tax rate data from Tax Policy Center.[14] We obtained the exact date of each alcohol tax change, as well as additional information, by finding the legislation authorizing that change, as well as news coverage describing the change. Additional information is important because alcohol excise tax changes are sometimes accompanies by other regulatory changes such as alcohol-specific sales tax changes or changes in Sunday sales.

We only estimate pass-through rates for events which involve simple excise tax changes. We exclude any tax change which is not a pure excise tax change, i.e., accompanied by other policy changes directly affecting alcohol sales. For example, Tennessee implemented a standard volume-based beer excise tax in July 2013, but it phased out a price-based tax simultaneously. Further, we do not estimate pass-through rates for three events with negligible tax changes, less than $0.05 per unit, because it would be difficult to precisely measure pass-through of such small changes. Finally, we do not study tax changes when we lack the data to do so. In particular, we exclude tax changes in Alaska and Hawaii because Nielsen has no coverage in those states, we exclude liquor in Rhode Island because Rhode Island sells liquor in government-owned stores, and we exclude events earlier than July 1, 2006, or later than June 30, 2016, because the pre- or post-period would be less than 6 months long.

The final set of all beer, liquor, and excise taxes that we study is in Appendix Table D.1. We refer to each row of the table as an event, i.e., a specific excise tax change. We estimate pass-through rates for 71 total events: seven beer tax changes, four liquor tax changes, and 60 cigarette tax changes. We provide summary statistics on these events in Table 3. The typical event involves a change of $0.33 to $0.59 per unit (which varies from 2 to 10 percent of average prices). The typical event affects 7-10 chains, and these chains are typically multi-state chains. The average exposed chain has stores in 16 states, and

[14]`https://www.taxpolicycenter.org/statistics/state-alcohol-excise-taxes`

exposed stores represent less than 8 percent of exposed chains total stores (e.g., 521/6,487 for beer).

4.2 Empirical approach

Overview: Our empirical approach for studying all excise tax changes closely parallels our approach for studying the Washington beer tax increase. We estimate pass-through rates for each event in isolation, looking at prices in a one-year window around the event (the "event window"). First, for each event, we drop chains which entered or exited in the event state during the event window. The majority of events do not have such chains.[15] Next, for each event, we identify a set of clean control stores, and then estimate chain-product specific pass-through. To define the clean control stores, we first define control states for each event as the states that did not themselves have an excise tax change for the given category during the event window. For example, New York increased its beer tax on May 1, 2009, and North Carolina and Illinois both increased their beer taxes on September 1, 2009, so North Carolina and Illinois would not be clean controls for New York (or for each other) and all stores in these two states are dropped for the New York event analysis.[16]

Stores in the control states are not necessarily clean controls, however, because they may belong to a chain facing an excise tax increase in a different state. Our Washington case study avoided this problem by construction, because no other states changed their beer tax within our event window. For our general analysis, we take two approaches to this issue. The first approach, motivated by the Washington evidence of no spill-overs to unexposed stores in exposed chains, simply assumes that all unexposed stores in exposed chains do not respond to any tax increase. In this approach all stores in control states are clean controls.

The assumption of no spillovers, however, is strong and limiting, in the sense that

[15]One chain for beer in NC in 2009, one chain for each of six cigarettes events.

[16]In defining clean control states, we exclude all states with an excise tax change in the event window, rather than just the studied states. So for example we do not estimate pass-through rates for Tennessee's 2013 tax change, and we do not treat Tennessee as a clean control state for any beer excise tax changes between July 2012 and July 2014.

it does not let us test the generality of the Washington no-spillover result. In our second approach, we therefore relax this assumption. To do so, we limit our sample—both treatment and control—to "clean chains," meaning chains which were exposed only to a single tax change within the event window. For example, suppose Wegmans has stores in both North Carolina and New York, and Food Lion has stores in both New Jersey and North Carolina. When studying New York's beer tax increase, our first approach would include both these chains (but exclude the North Carolina stores), whereas the second approach would exclude both chains entirely. As this example indicates, our second approach ends up excluding many chains, including many exposed chains, particularly the most national chains. In the first approach, we estimate pass-through for 71 events and 518 chain-events, but in the second approach we estimate pass-through only for 58 events and 197 chain-events. Because the second approach ends up with less precise estimates and a more selected sample, we view it as a robustness check rather than a primary specification.

Estimating equation: Both approaches lead to the following estimating equation for the produce of product i in store s (belonging to chain c), for event e:

$$p_{ist} = \rho^{e}_{ice} tax_e Post_t Exposed_s + \rho^{u}_{ice} tax_e Post_t (1 - Exposed_s) Exposed_{c(s)} + \beta_{is} + \gamma_{it} + \nu_{ist}. \quad (2)$$

The objects of interest are ρ^1_{ice} and ρ^0_{ice}, the price response per unit of tax increase, among exposed and unexposed stores. This equation includes fixed effects for store-product (β_{is}) and for week-product (γ_{it}). tax_e is the size of the tax change for event e, $Post_t$ is an indicator for weeks after the tax change, $Exposed_i$ is an indicator for whether the store is in the state with the tax change, and $Exposed_{c(i)}$ is an indicator for a chain owning any exposed stores. In our main approach, the sample includes all stores in the state of the tax change and all stores in clean control states. In our robustness approach allowing for more spillovers, we exclude all stores in chains with exposure to other tax changes during the event window. Note that, even under the first approach, we can estimate spill-overs to unexposed stores (measured by ρ^0_{ice}), although we assume that tax changes other than event e's do not generate spillovers. In either approach, the identifying assumption is that the price trend of the clean controls is what the price trend of the exposed stores and unexposed stores in exposed chains would have been, had there not been an excise tax change. We estimate heteroscedasticity robust standard errors, clustered on chain.

We estimate Equation 2 event-product by event-product.[17] Following DellaVigna and Gentzkow (2019), we require that valid estimates have standard errors between 0.01 and 1.25. As in the Washington case study, to adjust for sampling error, in some cases we report Empirical-Bayes shrunk pass-through estimates, $\tilde{\rho}^{j}_{ice},\ j = 0, 1$; and we winsorize both $\hat{\rho}^{j}_{ice}$ and $\tilde{\rho}^{j}_{ice}$ at -3 and 5.[18]

Comparison with alternative approach: A straightforward approach would be to estimate a simple two-way fixed-effect model that pools all tax changes. The model could allow for heterogeneous response by category or type of store. We do not pursue this alternative approach for two reasons. First, our model of uniform pricing and of flexible pricing implies that pass-throughs are inherently heterogeneous, varying across chains and markets. It would be difficult to accommodate that heterogeneity in the pooled approach. Second, we are interested in examining heterogeneous responses that differ by pre-period exposure to the tax change. This would be difficult to define in a pooled regression, but is straightforward in our event-by-event approach.

4.3 Preliminary graphical evidence

Before presenting our full set of estimates, we begin by generalizing the graphical approach in Figure 2 to accommodate all events. This graphical evidence adds value because it shows transparently the data underlying our estimates, and provides evidence supporting the "parallel trends" assumption that our empirical strategy requires.

To these ends, our goal is to make analogous composites of residualized prices that we made for the Washington (beer tax) event case study but for all the excise tax change events in our universe of events, grouping events with similar sized tax changes. More

[17] Estimating event-by-event entails some efficiency loss because the same store-product and week-product appears in multiple events. This approach produces tremendous computational savings, however.

[18] We did not winsorize the Washington estimates because all of them were between -3 and 5; more generally, the winsorizing is more important for small tax changes, which produce larger standard errors and more extreme (positive and negative) pass-through estimates.

Figure 5: Exposed stores responded to excise tax changes, but unexposed stores in exposed chains did not

Note: This figure plots the residual price, net of store-product, and time-product fixed effects in each type of stores (unexposed stores in unexposed chains, exposed stores in exposed chains, and unexposed stores in unexposed chains), and each group of events (tercels of positive tax changes, and two events with negative tax changes). The series for unexposed chains is often indistinguishable from the series for unexposed stores in exposed chains because they are both often almost exactly zero.

precisely, for each of our 71 events, we calculate store-week level residual prices, defined as price net of store-product and week-product fixed effects (estimated in each event window). We divide the events into four groups: the two events with negative tax changes, and then the three terciles of positive tax changes. For each group of event, we then average the residualized prices over observations in a particular event, set of store types (e.g., exposed stores, unexposed stores in exposed chains, and clean controls), and week in event time (i.e., weeks relative to the tax change).

The results are presented in Figure 5. The general patterns presented in this figure echo the Washington case study figure closely. For the events with large (positive) tax changes, there is a clear and immediate price increase that sharply diverges from the price responses of each set of control stores. For the smaller (postive) tax changes, the price change is less clear and slower to materialize, but still a divergence from the pricing response of the other groups of stores. For the events that represent a tax decrease, the price response in the exposed stores is a subsequent decrease.[19] Furthermore, across all of the panels there are no indications of violations of the "parallel pre-trends" assumption, which supports the underlying empirical strategy of the main results.

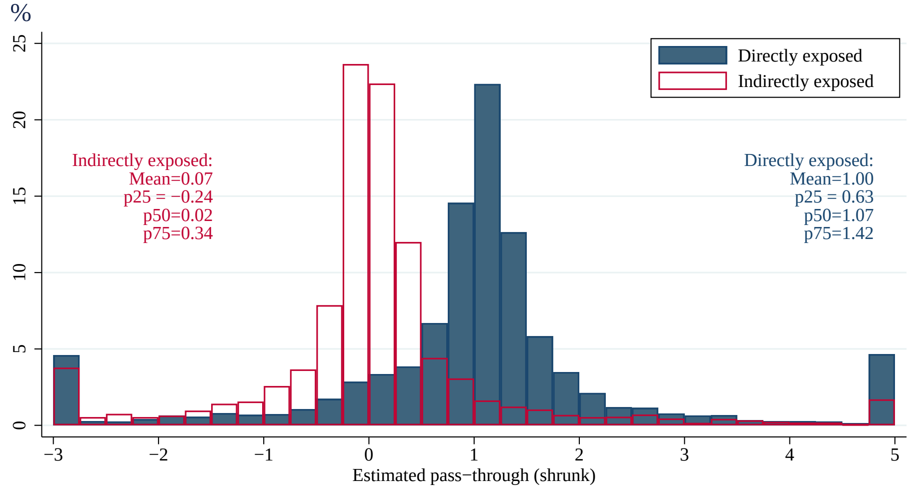
4.4 Results

We begin by plotting in Figure 6 the distribution of $\tilde{\rho}^1_{ice}$, the shrunk pass-through estimates for exposed stores, as well as the distribution of $\tilde{\rho}^0_{ice}$, the shrunk pass-through estimates for unexposed stores in exposed chains. The figure shows a clear price response among exposed stores: the mean pass-through rate for exposed stores is 1.00, with a median of 1.07 and an interquartile range of 0.63 to 1.42. Thus most chains respond to most excise tax changes by adjusting their prices in exposed stores, and the typical price adjustment is about the same size as the typical excise tax, i.e., the mean and median pass-through rate for exposed stores are both about 1.

By contrast, we estimate essentially no pass-through among unexposed stores in exposed chains. The mean pass-through rate is 0.07 and the median is 0.02. Although the

[19] This panel's trend for the treated stores is noisier because it averages across only two events.

Figure 6: Distribution of pass-through for exposed and unexposed stores, all tax changes



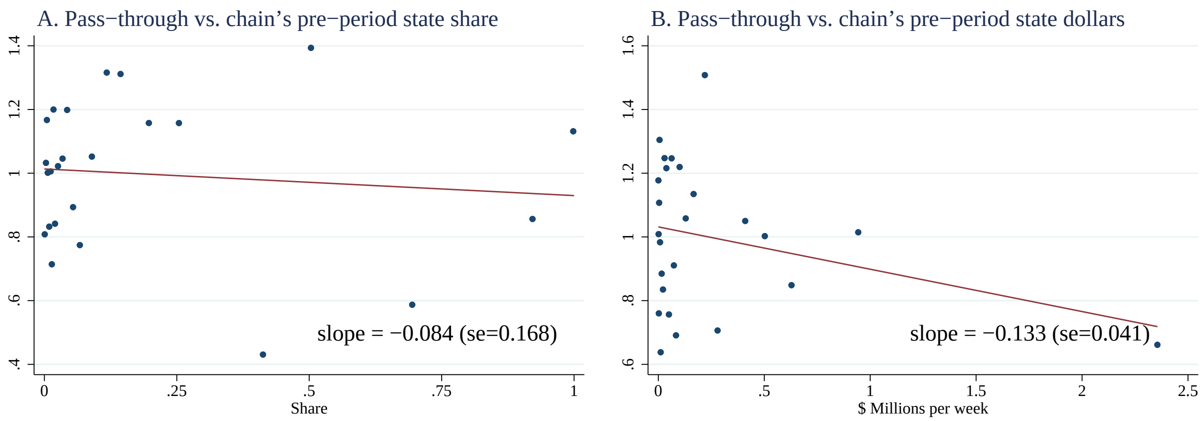
Note: This figure plots the distribution of (shrunk) estimated pass-through rates for exposed and unexposed stores, estimated via Equation 2. Figure reports mean and 25th, 50th, and 75th percentiles of estimated pass-through rates, for each group.

unexposed and exposed distributions do have some overlap, there is very little. For example, the 75th percentile of pass-through, 0.34, is the 19th percentile for unexposed stores. Overall, most chains do not appear to respond to one state's tax change by adjusting out-of-state prices.

Figure 6 also reveals considerable heterogeneity in the pass-through estimates. In principle this heterogeneous response could reflect chain- or product-level heterogeneity in exposure to the taxes; our uniform pricing model implies larger responses among more exposed chains and products. We investigate this possibility in Figure 7. The figure plots binned pass-through estimates against binned chain-level exposure to the tax change. We define exposure in market share (in panel A) or in dollars (in panel B), using pre-event data. To construct the figure, we match each $\hat{\rho}^1_{ice}$ to the exposure of chain c to event e. We then aggregate the data to 25 bins of exposure, and plot bin averages.

The figure shows essentially no correlation between pass-through estimates and chain-level exposure. The most and least exposed chains respond similarly. The overall association is slightly negative. We see no evidence that most exposed chains have a larger pass-through rate. In Appendix Figure D.3, we show the correlation between pass-through

Figure 7: Chain-level pass-through uncorrelated with exposure, all tax changes



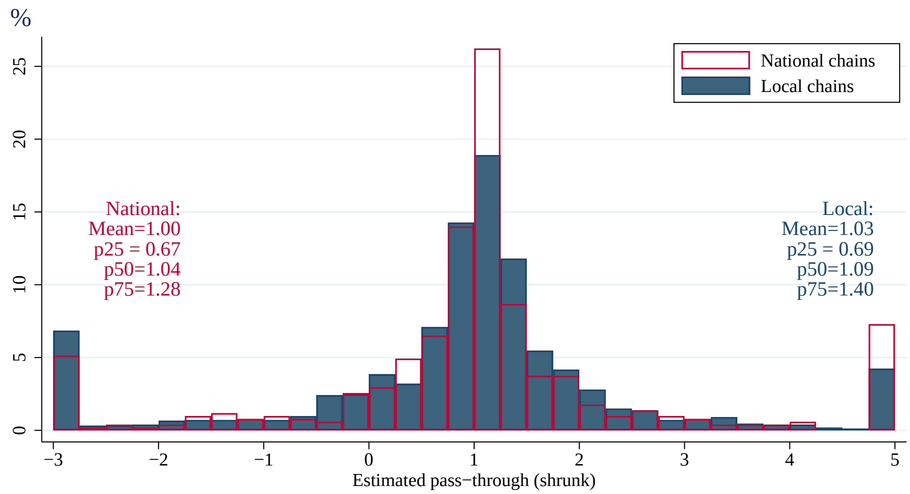
Note: This figure plots the average chain-level pass-through (obtained as the simple average of product-specific pass-through estimates among exposed stores) against each chain’s exposure to the event state’s tax change. Exposure is defined in panel A as the chain’s prior year share of revenue in the event state (for the event’s category), and in panel B as the chain’s total revenue in the prior year (for the event’s category, per week, in millions of dollars).

and product-level (rather than chain-level) exposure. Again we see no association between exposure and pass-through.

Robustness We report the results of a series of robustness tests in Appendix Table D.2. We represent our baseline results in column 1. One concern with our main estimates is that they assume no simultaneous spillovers of other tax changes. We therefore consider an alternative sample that avoids this possibility by excluding any chains with exposure to other states’ tax changes (for a given event). With this alternative sample, we estimate slightly lower pass-through among exposed stores, but very similar overall results, as column 2 of the table shows.

We also considered robustness in two other dimensions. Because most exposed chains are multi-state chains, we considered an alternative control group that consists of multi-state chains only (columns 3 and 5). And because in principle prices could be set at the parent company level rather than the chain level (e.g., Albertson’s rather than Jewel-Osco or Safeway), we also considered specifications (column 4 and 5) that excluded contaminated *parents* which is a superset of contaminated control chains. Our main finding—of pass-through near 1 for exposed stores, and near 0 for unexposed stores—is robust to

Figure 8: Distribution of pass-through for national and local chains



Note: This figure plots the distribution of (shrunk) estimated pass-through rates for exposed, estimated via Equation 2, separately for national and local chains. National chains are ones with less than 20% of their pre-period sales in the event state. Local chains are ones with at least 90% of their pre-period sales in the event state. Figure reports mean and 25th, 50th, and 75th percentiles of estimated pass-through rates, for each group.

these alternative sample selection approaches; the mean and median of the estimates remain quite close to 1 for exposed stores and 0 for unexposed stores. The main difference is that the distribution of estimates is somewhat wider in our robustness tests, likely because of greater sampling error resulting from the smaller control group.

In column 6 we consider robustness to a separate concern: that stores in our control group could be affected because of demand spillovers from cross-border shopping. This concern is prevalent in the literature on cigarette taxation (e.g., Harding et al. (2012)). Such spillovers might induce stores in Oregon, for example, to raise their prices in response to Washington's beer tax increase. In column 6 we exclude all chains (exposed or not) with stores in states that border the event state because some distribution of pass-through rates changes only trivially.

4.5 Benchmarking the response of national chains to local shocks

So far we have shown that local cost shocks generate a local response among national chains, but no non-local response, and no differential response by more exposed chains. Although these findings are inconsistent with the predictions of our simple uniform pricing model, they still leave open the possibility that national chains underreact to local shocks, relative to national shocks. In this section, we therefore compare the response of national chains to local shocks to two benchmarks: their response to a national excise tax increase, and the response of *local* chains to local shocks, which we argue is a proxy for the response of national chains to national shocks.[20]

Comparing the response of local and national chains We compare estimated pass-through among local chains to estimated pass-through among national chains. The idea behind this comparison is that, from the perspective of the uniform pricing model, a local shock and a national shock are equivalent for a local chain, but not for a national chain. Thus if uniform pricing attenuates pass-through, we should see national chains react less than local chains to a given excise tax change. Operationalizing this approach requires that we define local and national chains. We define local chains as ones with a pre-event revenue share of at least 90 percent (i.e., at least 90 percent of their category-specific revenue in the year prior to the tax-change was from the state with the tax change), and we define local chains as ones with at most a 20 percent pre-event revenue share.[21]

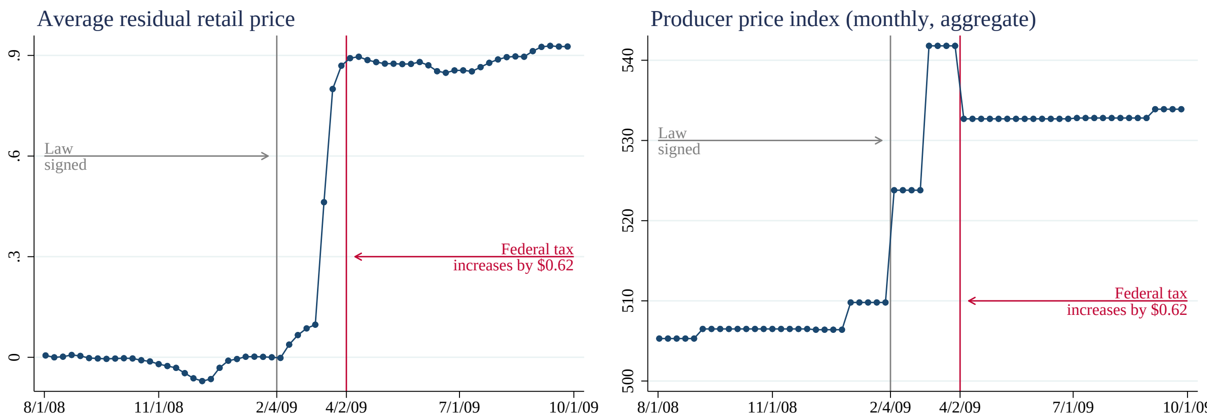
We plot the distribution of $\tilde{\rho}^1_{ice}$ in Figure 8. The figure shows that the distributions nearly overlap. The mean and median pass-through rates are slightly higher for local chains than for national chains, but the difference is slight: local chains have a pass-through rate that is about 3 percent higher than national chains, despite being 4.5 times as exposed.

Comparing the response to a national excise tax change: The local chain response to local shocks is an imperfect proxy for the national chain response to a national shock. We provide direct evidence on this question by examining the pass-through of national excise

[20]Under most forms of imperfect competition, an industry wide cost shock results in higher pass-through rates than firm specific idiosyncratic cost shocks. Given the local nature of retail competition (e.g., Smith (2006)), we interpret a state-wide excise tax–which applies to all retailers–as an industry wide cost shock.

[21]Although these cutoffs are arbitrary, the results are not sensitive to them.

Figure 9: Residual cigarette prices around the CHIP Reauthorization Act of 2009



Note: The left panel plots the weekly average residual price cigarette price, averaging across all stores and products in our data, around the CHIP Reauthorization Act of 2009, which raised the federal cigarette excise tax by $0.62 per pack. The residual price is the store-product-week price net of a store-product fixed effect, normalized to zero in the week before announcement. The rate panel plots the producer price index for tobacco, which is measured monthly.

tax increases. This approach faces two challenges. First, estimating the pass-through of national excise tax changes is inherently difficult, however, because there is no control group. Second, federal excise tax changes are relatively rare, at least for our products. During our sample period there is only one excise tax change: the Children's Health Insurance (CHIP) Reauthorization ACT of 2009, signed February 4, 2009, which raised federal excise tax on cigarettes grew from $0.39 per pack to $1.01, effective April 2, 2009.

We examine the impact of this tax change using simple pre-post comparisons of prices. For each product j, store s, and week t, we define $\tilde{p}_{jst}$ as the residual price for that store-product-week, net of the store-product mean price over our sample period. In the left panel of Figure 9, we plot the simple average, across stores and products, of this residual price, for August 2008 until October 2009, i.e., six months before announcement to six months after enactment. In constructing the figure, we exclude states which had a tax change during this time period. The figure shows no trend except an increase in price right after announcement, a sharp increase a few weeks later, and a very sharp jump in the week of enactment and the week before. Because our residualized price nets out store-product heterogeneity, this jump reflects changes in the average posted price, rather than changes in stores visited or products purchased. The sharp jump—and relatively little trend in the weeks immediately before signing or after implementation—suggests that simple pre-post comparisons may be adequate for capturing the effect of the tax increase on prices. However, the right panel of the figure shows a complicating detail. While our interest is in how *retail* prices respond to input costs, the right panel plots a measure of *wholesaler* input costs, the tobacco producer price index (PPI), obtained from U.S. Bureau of Labor Statistics (2020). The PPI increases sharply in both February and March of 2009 before falling in April.

This sharp increase in producer prices presents a challenge to estimating retailer pass-through of federal excise taxes. We estimate this pass-through using data from August 2008 to October 2009, excluding states with tax changes, with the following model:

$$\tilde{p}_{sct} = \beta_{0c} + \rho_c^F FedTax_t + \beta_{1c} PPI_t + e_{ct}. \tag{3}$$

The dependent variable is the average residual price (net of store-product fixed effects) in state s, chain c, and week t. Our pass-through measure is ρ_c^F. We estimate the model chain-by-chain.

The model adjusts linearly for the tobacco PPI. This choice of control is important and merits discussion. We acknowledge, first, that it would be inappropriate to control for the PPI if our goal were to estimate the overall effect of the excise tax increase and we thought that the excise tax increase caused the PPI increase. We believe nonetheless that our control is appropriate for three reasons. First, our goal is not to estimate the overall effect of the federal tax increase, but to estimate retail pass-through of the federal change. If the federal tax increase did affect the PPI, that would raise wholesale prices (net of retail taxes) by more than the amount of the excise tax increase, and so failing to control for PPI would cause us to overestimate pass-through. Second, we think it is unlikely that there were anticipatory responses to the federal changes. Such responses would typically imply a decrease in the PPI (because the PPI is a pre-tax price), and we see no evidence of anticipatory price changes in Figure 5. Third, even if producer prices did respond to tax changes, these responses would be absorbed by our time fixed effects when we estimate pass-through of local changes, so for comparing the national and federal pass-through, it is appropriate to adjust for PPI. Thus our preferred specification controls for PPI. However, we also report results that do not control for PPI.

Table 4: Distribution of pass-through of federal and state excise tax changes

Tax change	Federal		State
	No PPI	PPI	
	(1)	(2)	(3)
Mean	1.43	0.95	1.07
25th percentile	1.29	0.80	0.92
50th percentile	1.40	0.88	1.06
75th percentile	1.49	1.00	1.21
Mean exposure	1	1	0.58
# Chains	35	35	35

Note: This table reports the distribution of estimated chain-specific pass-through rates of federal excise tax changes (in columns 1 and 2) and state tax changes (in column 3). We estimate the federal pass-through using Equation 3. The first column does not control for the tobacco PPI and the second column does. We estimate the state pass-through using Equation 2. We report the distribution of local pass-through rates among chains in the federal sample. "Exposure" is the average share of pre-tax-change revenue for a given chain, averaging over the tax changes to which it is exposed.

We obtain an estimated federal pass-through rate for each of 35 chains.[22] We report the distribution of estimated federal pass-through rates in Table 4, estimated without the PPI control in column 1, or with it in column 2. In column 3, for comparison, we report the distribution of pass-through rates of state excise tax changes, for these 35 chains. (These are obtained as the average within-chain estimate of ρ^{e}_{ice} from Equation 2, averaging over all events to which the chain is exposed.)

When we do not adjust for PPI, the pass-through rate of the federal tax is about 33 percent higher than the pass-through rate of state taxes. This higher pass-through rate is entirely explained by PPI, however: when we control for PPI, the national pass-through rate is slightly lower than the local pass-through rate. Our interpretation of these results

[22]This is fewer than the 69 chains in our data that sell cigarettes, because we exclude states with an excise tax change during our estimation window.

is that the pass-through rates of local and national tax changes are similar. An alternative interpretation is that the response to national changes is larger because producer prices respond as well as local prices.[23] In either case, however, uniform pricing does not appear to attenuate responses to local excise tax changes.

5 Reconciliation

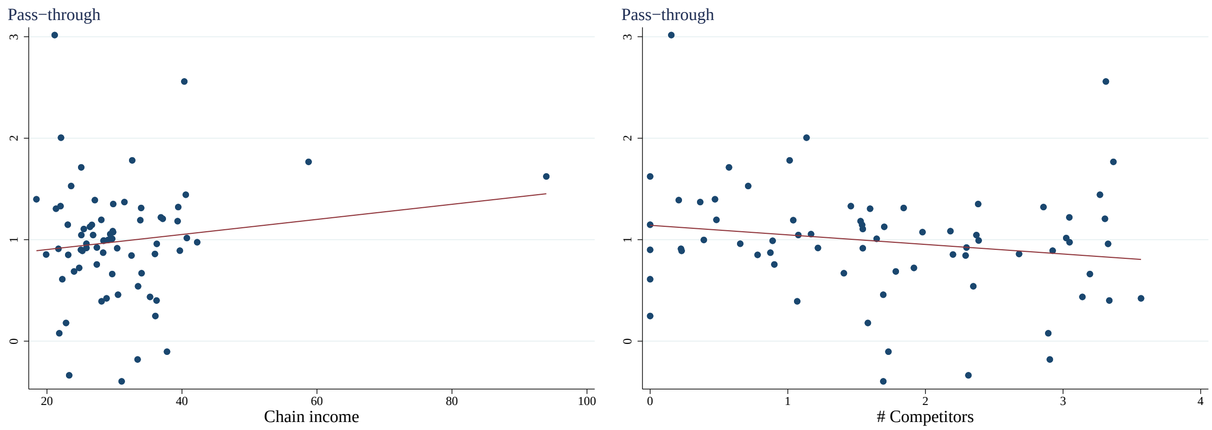
So far we have shown that national chains adjust local prices to local cost changes. These price adjustments happen rapidly, do not vary with a chain's exposure to the local cost change, and are similar in magnitude to the price response to a national cost change. These findings may seem inconsistent with recent research on "uniform" pricing, which shows that prices are uncorrelated with income across stores within chain, and that prices are much more similar within chains than across chains (DellaVigna and Gentzkow, 2019). In this section we offer a simple reconciliation: retail chains charge uniform *markups* rather than uniform prices. This explanation can account for the key facts on both pass-through and uniform pricing. To see why, suppose that chain c follows a pricing rule for product i in store j of the form

$$p_{ijc} = (1 + \mu_c)cost_{ijc}.$$

The (percent) markup μ_c could depend on chain-level factors which could be product or demand specific, but not store specific. Such factors could include the degree of competition the chain typically or the average income of its customers. This pricing rule implies that local cost shocks are passed through at a rate $1 + \mu_c$ for all stores in a chain, regardless of their local demand or competition. It also implies that prices are uncorrelated with local income or other demand characteristics, as long as local demand are uncorrelated with wholesale prices - a plausible assumption as most wholesale prices are nationally determined (Stroebel and Vavra, 2017; DellaVigna and Gentzkow, 2019). We now provide two kinds of evidence for uniform markups: first, we in our data that pass-through varies

[23]Muehlegger and Sweeney (2017) provide evidence that industry wide shocks have larger pass-through rates than firm specific–or, more local–cost shocks in the oil industry during the U.S. fracking boom. Andrade and Zachariadis (2016) investigate the distinction between local and global shocks on prices across a diverse set of products in the international context.

Figure 10: Correlates of chain-level pass-through



Notes: Figure plots chain average pass-through against chain-average income (in $1000s) in the left panel and chain-average competition (i.e. average number of rival chains in each stores' market).

across chains but, within a chain, it is not correlated with differences in demand across markets. Second, we show that uniform markup reconciles a wide set of facts about retail pricing documented in the literature

5.1 Documenting heterogeneity in pass-through

We begin by showing that pass-through rates vary across chains. We do so in two ways. First, we take our full set of (winsorized) pass-through estimates, which vary at the chain-product-even level. We then regress these estimates on a set of fixed effects for product, event, and chain. We estimate these regressions category-by-category and pooling all categories. We find that, relative to just event and product fixed effects, chain fixed effects explain an additional 10-14 percentage points of the variation in pass-through. We then test the hypothesis that the chain fixed effects are jointly equal to zero, i.e. that chain factors explain none of the variation in pass-through rates. As we report in Appendix Table D.3, we reject this hypothesis for each category and overall.[24] This provides non-parametric evidence that chains differ in their pass-through rates.

To provide more evidence on cross-chain variation in pass-through rates, we construct

[24]These are estimated with considerable error, so we should not expect a very high R^2.

Table 5: Correlates of chain-level pass-through

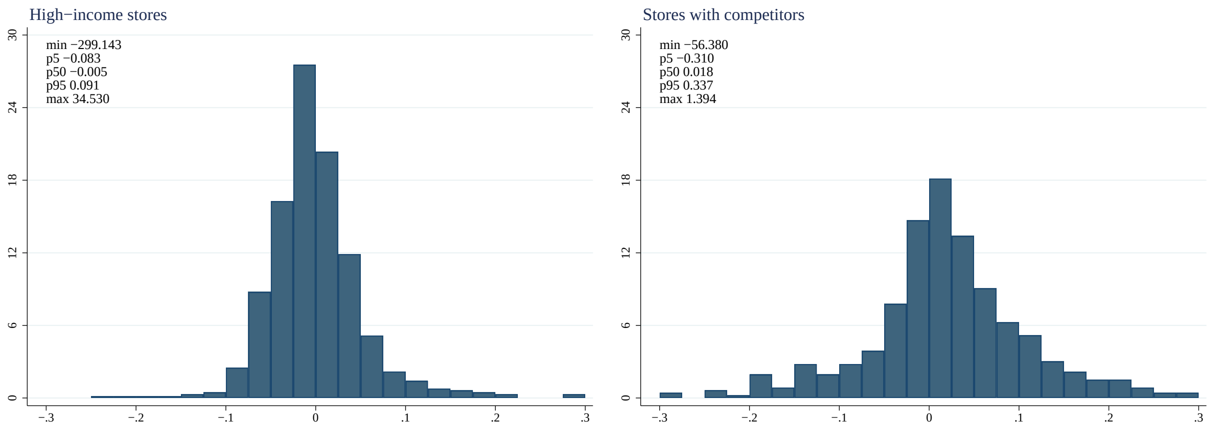
	(1)	(2)	(3)
Chain income ($1000s)	0.007		0.010
	(0.005)		(0.005)
# Competitors		-0.094	-0.117
		(0.073)	(0.068)
R^2	0.018	0.031	0.063
# Chains	70	70	70
Mean income	29.8	29.8	29.8
SD income	6.6	6.6	6.6
Mean # competitors	1.5	1.5	1.5

Notes: Table reports coefficients from a regression of chain-level pass-through on chain-level income, chain-level competition, or both. Robust standard errors in parentheses..

chain-specific estimates of pass-through by taking the simple average of each chain-event-product pass-through estimate. This yields a single pass-through per chain, ρ_c. We would expect ρ_c to vary across chains because different chains vary in the demand they face or the competitive conditions in which they operate. We therefore regress ρ_c on chain average income and chain average competition (defined as the simple average income of stores in a chain or the simple average number of competitors that a chain's stores face). We show the relationship between ρ_c, income, and competition in Figure 10. We see that chain-level pass-through increases with income and decreases with competition, although in neither case does the relationship appear very strong. We quantify the relationship by regressing chain-level pass-through on income and competition. We report the results in Table 5. The associations are not very precise (and only significant in the joint regression in column (3)). However the magnitudes are economically meaningful: going from the 10th percentile of income to the 90th implies a change in pass-through of 0.18 (on a base of about 1), and going from the 10th to the 90th percentile of competition implies a change of -0.34.

The cross-chain association between pass-through rates, income, and competition is not matched by a within-chain association. To show this, we re-estimate our main pass-

Figure 11: High-income and high-competition stores do not show differential pass-through relative to other stores in the same chain



Notes: The figures plot the distribution $\tilde{\rho}_{\Delta}$, the estimated within-chain pass-through differential for high-income stores (on the left) or stores for facing competition (on the right). The individual estimates are shrunk via Empirical Bayes. We truncate the distributions at +/- 0.3, which is approximately the inner 98%.

through equation (Equation 2) but allowing for heterogeneous pass-through by store-level characteristics. Specifically we estimate

$$\begin{aligned} p_{ist} =& \rho_{\Delta} tax_e Post_t Exposed_s \Delta_s + \rho^e_{ice} tax_e Post_t Exposed_s + \\ & \rho^u_{ice} tax_e Post_t (1 - Exposed_s) Exposed_{c(s)} + \beta_{is} + \gamma_{it} + \nu_{ist}, \end{aligned} \tag{4}$$

where Δ_s is an indicator for either "store has above average income (relative to chain mean)" or "store has no competition." (That is, we estimate twice per event, once for each interaction.) Because Equation 4 allows for chain-specific pass-through estimates, ρ_{Δ} measures store specific deviations from chain-average pass-through (among high income or high competition stores). The coefficient therefore answer the question of whether, within a chain, pass-through rates differ for high-income or low-competition stores.

We plot the distribution of the ρ_{Δ} estimates in Figure 11. The left panel shows the distribution of differential pass-through in high-income stores (relative to low-income stores in the same chain), and the right panel the distribution of differential pass-through in stores facing competition (relative to stores in the same chain not facing competition).[25]

[25]We shrink these estimates like we do our main estimates.

In both cases the distributions are centered around zero. Although the estimates are sufficiently imprecise there are fairly large left and right tails, we see no indication of heterogeneous pass-through rates for high-income stores or for high competition stores. We conclude from this that although pass-through rates vary across chains, they do not vary within chain, across stores (or at least not in clearly predictable ways).

5.2 Uniform markups in the literature

Several examples of other results in the retailer pricing literature provide complementary evidence of uniform markups. Arcidiacono et al. (2020) examine local price responses to a large demand shock: the entry of a Walmart Supercenter. They find that even though incumbent retailers' revenue falls sharply upon Walmart's entry, incumbent prices do not change, either preemptively or after. To explain the pricing behavior, Arcidiacono et al. (2020) argue because Walmart's entry does not affect costs, it does not trigger a price reaction from incumbents.[26]

Eichenbaum et al. (2011) examine the retail and wholesale prices of a large retailer with hundreds of stores across the country. Eichenbaum et al. (2011) show retail prices change rarely and typically in conjunction with changes in costs, so as to maintain a consistent markup over costs. McShane et al. (2016) finds similar evidence for a different retailer. Examining the retail and wholesale prices of a retailer with stores in both the U.S. and Canada, Gopinath et al. (2011) show that markups are very similar even across national borders. Additionally, Hitsch et al. (2017) argue that differences in demand across stores within a retailer are likely very difficult to measure. To the extent that retailers are unable to discern differences in the price elasticity of demand across their stores, a natural pricing strategy might involve a uniform markup.

[26] In effect, the results Arcidiacono et al. (2020) provide another example of a surprising "uniform" pricing result albeit in the time series dimension alongside sharp changes in local competition, as opposed to the cross-sectional facts provided by DellaVigna and Gentzkow (2019); Adams and Williams (2019).

6 Conclusion

We investigated the pass-through of local excise tax increases among national firms. We found four key results. Firms passed these shocks through completely, with pass-through rates around one or larger among their directly exposed stores. Second, we found no spillovers to unexposed stores. Third, more and less exposed firms—those with a greater or smaller share of their revenue in the area with the tax increase—had roughly equal pass-through rates. Fourth, the pass-through rate to local prices of a local tax increase is only slightly smaller than the pass-through rate of a federal tax increase.

Each of these findings is inconsistent with simple models of uniform pricing. At first pass, these results therefore appear at odds with prior literature documenting uniform prices within chains (overall or across broad prizing zones) and showing that chains account for a great deal of price variation, over and above market-specific factors (DellaVigna and Gentzkow, 2019; Hitsch et al., 2017; Adams and Williams, 2019). However, these prior papers have often shown that prices respond little to demand shocks, whereas our evidence points to pass-through of marginal cost shocks. We argue that a natural way to reconcile these findings is to hypothesize that national chains set a uniform *markup* for all markets. Such a hypothesis rationalizes a wide set of findings on retail pricing.

If true, the uniform markup hypothesis has at least two important implications. First, as national chains appear to respond fully to local excise tax changes, they suggest that uniform pricing does not attenuate the response to many sin taxes, an important finding for policy makers hoping these taxes change behavior. Second, the economic incidence of a tax may depend on its statutory incidence. A tax levied on the demand side of the market could affect prices through changes in demand, but uniform markups suggest that prices would not respond to such a tax under constant marginal costs.

We acknowledge, however, that there are of course other explanations for why local prices respond to local cost shocks but not demand shocks. For example, cost shocks may be more salient than demand shocks, or the cost shocks we study may be more costly to ignore than the typical demand shock. We believe that investigating these mechanisms is a fruitful avenue for future research, as is exploring the implications of uniform markups.

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Online Appendix

A Proofs for the Theoretical Framework

Our theoretical framework outlined two empirical predictions that we investigate in the rest of the paper. For transparency, we provide the omitted intermediate steps that lead to these predictions of which many were omitted in the main body of the paper.

Given the flexible pricing monopolist's objective function:

$$\max_{p_1,\ldots,p_N} \sum_m \left[p_m - c_m\right] x_m(p_m)$$

the (necessary) first order condition for the optimal price in each market is given by:

$$x_m(p) + p x_m'(p) - c_m x_m'(p) \quad = \quad 0$$

which leads to the usual characterization of the optimal price over marginal cost:

$$p^{\star} - c_m \quad = \quad -\frac{x_m(p)}{x'(p)}$$

Or, dividing both sides by $p^{\star}$ the celebrated Lerner elasticity rule for optimal pricing:

$$\frac{p^{\star} - c_m}{p^{\star}} \quad = \quad -\frac{x_m(p)}{p^{\star} x'(p)} = \frac{1}{\varepsilon_m}.$$

To characterize the pass-through of the flexible pricing monopolist, we implicitly differentiate the first order condition (i.e., $f(\cdot) = 0$) given above for both c and p, which yield:

$$\begin{aligned} f_c &= -x_m'(p) \\ f_p &= 2x_m'(p) + \left[p - c_m\right] x_m''(p). \end{aligned}$$

By implicit differentiation the pass-through rate of a change in marginal cost in market m is given by:

$$
\begin{aligned}
\rho &= -\frac{f_c}{f_p} \\
&= \frac{x'_m(p)}{2x'_m(p) + [p - c_m]\, x''_m(p)} \\
&= \frac{1}{2 + [p - c]\, \frac{x''_m(p)}{x'_m(p)}} \\
&= \frac{1}{2 - \left[\frac{p - c_m}{p}\right] \frac{-p x''_m(p)}{x'_m(p)}} \\
&= \frac{1}{2 - \frac{\zeta_m}{\varepsilon_m}}
\end{aligned}
$$

where the final equality follows from the characterization of the optimal price, and the definitions of the price elasticity of demand and curvature of demand. This completes the results for the flexible monopolist.

Next, we present the intermediate steps for the uniform pricing monopolist. The uniform pricing monopolist's objective function:

$$
\max_{\overline{p}} \sum_m [\overline{p} - c_m]\, x_m(\overline{p})
$$

the (necessary) first order condition for the optimal (uniform) price in each market is given by:

$$
\sum_m x_m(\overline{p}) + \sum_m [\overline{p} - c_m]\, x'_m(\overline{p}) \quad = \quad 0.
$$

To characterize the pass-through of the uniform pricing monopolist, we implicitly differentiate the first order condition (i.e., $f(\cdot) = 0$) given above for a particular c_n and $\overline{p}$, which yield:

$$
\begin{aligned}
f_{c_n} &= -x'_n(p) \\
f_{\overline{p}} &= 2 \sum_m x'_m(p) + \sum_m [p - c_m]\, x''_m(p).
\end{aligned}
$$

By implicit differentiation the pass-through rate of a change in marginal cost in market n is given by:

$$
\begin{aligned}
\overline{\rho}_n \equiv \frac{d\overline{p}}{dc_n} &= -\frac{f_{c_n}}{f_{\overline{p}}} \\
&= \frac{x_n'(p)}{2\sum_m x_m'(p) + \sum_m \left[p - c_m\right] x_m''(p)} \\
&= \frac{(-p)x_n'(p)}{2\sum_m (-p)x_m'(p) + \sum_m \left[\frac{p-c_m}{(p)}\right](p)x_m'(p)(-p)\frac{x_m''(p)}{x_m'}} \\
&= \frac{x_n(p)(-p)\frac{x_n'(p)}{x_n(p)}}{2\sum_m x_m(p)(-p)\frac{x_m'(p)}{x_m(p)} + \sum_m \left[\frac{p-c_m}{(p)}\right] x_m(p)(p)\frac{x_m'(p)}{x_m(p)}(-p)\frac{x_m''(p)}{x_m'}} \\
&= \frac{s_n\varepsilon_n}{2\sum_m s_m\varepsilon_m - \sum_m \left[\frac{p-c_m}{p}\right] s_m\varepsilon_m\zeta_m}
\end{aligned}
$$

where the last equality follows from multiplying the numerator and denominator by $X \equiv \sum_m x_m(p)$.

This completes the first characterization of the pass-through under uniform pricing which justifies the first prediction that we investigate in the rest of the paper.

The second prediction we investigate was made under a more restricted set of demand conditions the monopolist faces across markets. For clarity we re-state that assumption here:

The demand in each market m, $x_m(p)$, is multiplicatively separable in a heterogeneous "market size", φ_m, and a common individual demand function $\tilde{x}(p)$. Thus, the demand in each market m is given by $x_m(p) = \varphi_m \tilde{x}(p)$.

While this assumption encapsulates many forms of demand used in applied work, it does restrict the elasticity and curvature of demand to be the same across markets. Under this assumption, which results in a constant price elasticity of demand ($\tilde{\varepsilon}_n$) and curvature ($\tilde{\zeta}_n$) across markets, and noting that the first order condition for the optimality of the uniform price guarantees that $\sum_m \left[\frac{p-c_m}{p}\right] s_m\varepsilon_m = 1$, the pass-through rate for the uniform pricing monopolist takes the form:

$$
\overline{\rho}_n = \frac{s_n}{2 - \tilde{\zeta}/\tilde{\varepsilon}}
$$

where this formulation is simply a consequence of applying the implications mentioned above to the more general formulation of the pass-through rate of the uniform pricing monopolist.

B Documenting uniform pricing among our products

We present two pieces of evidence on uniform pricing for our products. First, we show that for our products, prices for a given product appear highly similar across stores within a chain, but not across stores in different chains. Second, we show that within a chain, prices are uncorrelated with store income, but across chains, prices are highly correlated with income. In both cases we follow DellaVigna and Gentzkow (2019) exactly, and we replicate their general facts for our specific categories. Because we find that our categories exhibit the same signs of uniform pricing as do the categories studied by DellaVigna and Gentzkow (2019), this evidence shows that our choice of product categories does not account for our finding that national chains respond to local cost shocks.

B.1 Measuring similarity

We construct measures of uniform pricing. We define three measures of similarity at the store-product level. To construct the measures, we begin by sampling, for each chain, up to 200 pairs of stores within the chain, as well as 200 pairs consisting of a store in that chain and a store in another chain. For each sampled pair we obtain the complete time series of prices for all products in our categories satisfying our availability criteria.

Our first similarity measure is the quarterly absolute log price difference. For each product, quarter, and store in the pair, we calculate the quarterly average log price. We then calculate the absolute value of the difference in log price, between the two pairs in the store, averaging over all the quarters in which we have data for both stores. This measure captures quarterly similarity in prices across the stores; it is a measure of similarity in price levels. We winsorize the average absolute difference at 0.3.

Our second measure is the weekly log price correlation. For each product-year-store, we calculate the average log price, and find the residual price as the deviation from this average. We then calculate the correlation between stores in the pair in this residual price. This measure captures the similarity in price deviations from the mean, so it measures similarity in price changes.

Our third measure is the share of store-week pairs with nearly identical prices. These are defined as weeks in which the absolute difference in log prices is less than 0.01.

B.2 Documenting similarity in prices

We plot the distribution of our similarity measures for stores within the same chain and in different chains in Appendix Figures B.1, B.2 and B.3. We plot the distribution separately for each product category, to show that each of our products exhibits signs of uniform pricing. Overall the figures indicate a high degree of similarity within a chain but much less similarity across chain, and they closely resemble the figures in DellaVigna and Gentzkow (2019). We report the mean and standard deviation of each of these measures, by category, in Appendix Table B.1. The averages indicate slightly less similarity for our products than for DellaVigna and Gentzkow's sample of all grocery products. For example the mean absolute difference in quarterly log prices ranges is 0.036 for beer, 0.031 for liquor, and 0.066 for cigarettes, versus 0.03 for DellaVigna and Gentzkow. Similarly the average correlation in weekly prices is 0.6-0.7 for our products and 0.8 for Della Vigna and Gentzkow, and the weekly identical share is about 0.5 for our products and 0.6 for DellaVigna and Gentzkow. These comparisons show that our products exhibit highly similar prices within chains, although the similarity is not as extreme as we see for the typical grocery product.

B.3 The price income correlation

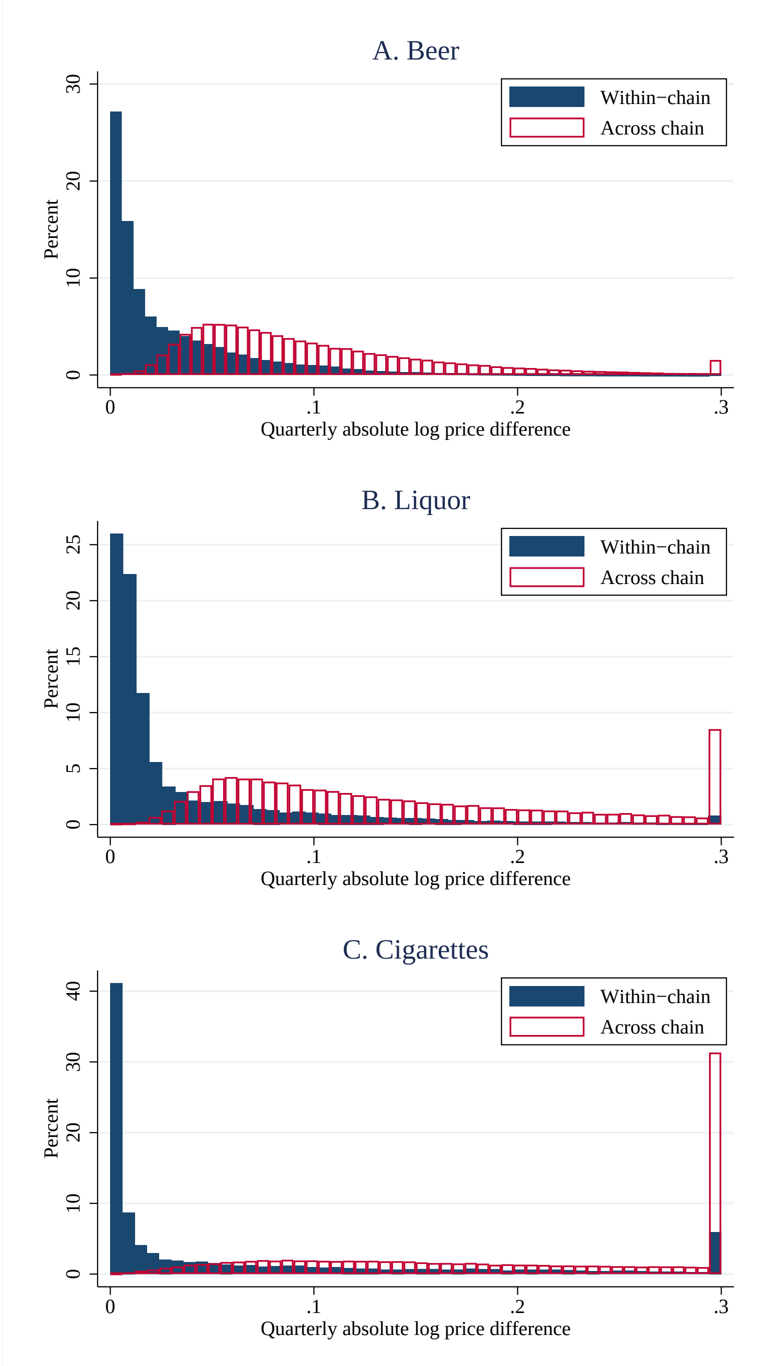
The evidence in Appendix Figures B.1, B.2, and B.3 shows that prices are more similar within chains than across chains, but this could simply reflect the fact that demand and cost conditions are more uniform within chains than across chains. As DellaVigna and Gentzkow (2019) argue, a key piece of evidence of uniform pricing (relative to the standard model of price setting with market power) is that prices across stores within a chain are uncorrelated with income, even though income predicts demand elasticities.

We replicate this fact for our categories. Specifically, letting p denote log price and $j-s-y-t-$ denote product-store-year-time, we define residual log prices here as $\tilde{p}_{jsty} \equiv p_{jsty} - \bar{p}_{jy}$, i.e., the product's price net of its annual average. We define the store's average price as the average of $\tilde{p}_{jsty}$ within the store, and we define the chain's average price as the average of $\tilde{p}_{jsty}$ within the chain. To define income for store s, we use Nielsen HomeScan data to identify the zip codes of all shoppers visiting s. We define the income of s as the average household income from 2008-12 ACS among those zipcodes weighted by number of visits. We define chain income as the simple average income of the stores in the chain.

We plot the within-chain price-income correlation in panels A, C, and E of Appendix Figure B.4, and the between-chain correlation in panels B, C, and D. The within-chain correlation plots store prices relative to chain average price against store income relative to the chain average income.[27] For all of our products we observe a weak within-chain relationship between prices and income; it is even slightly negative for beer and liquor. Looking across chains, however, we observe a much stronger relationship between price and income.

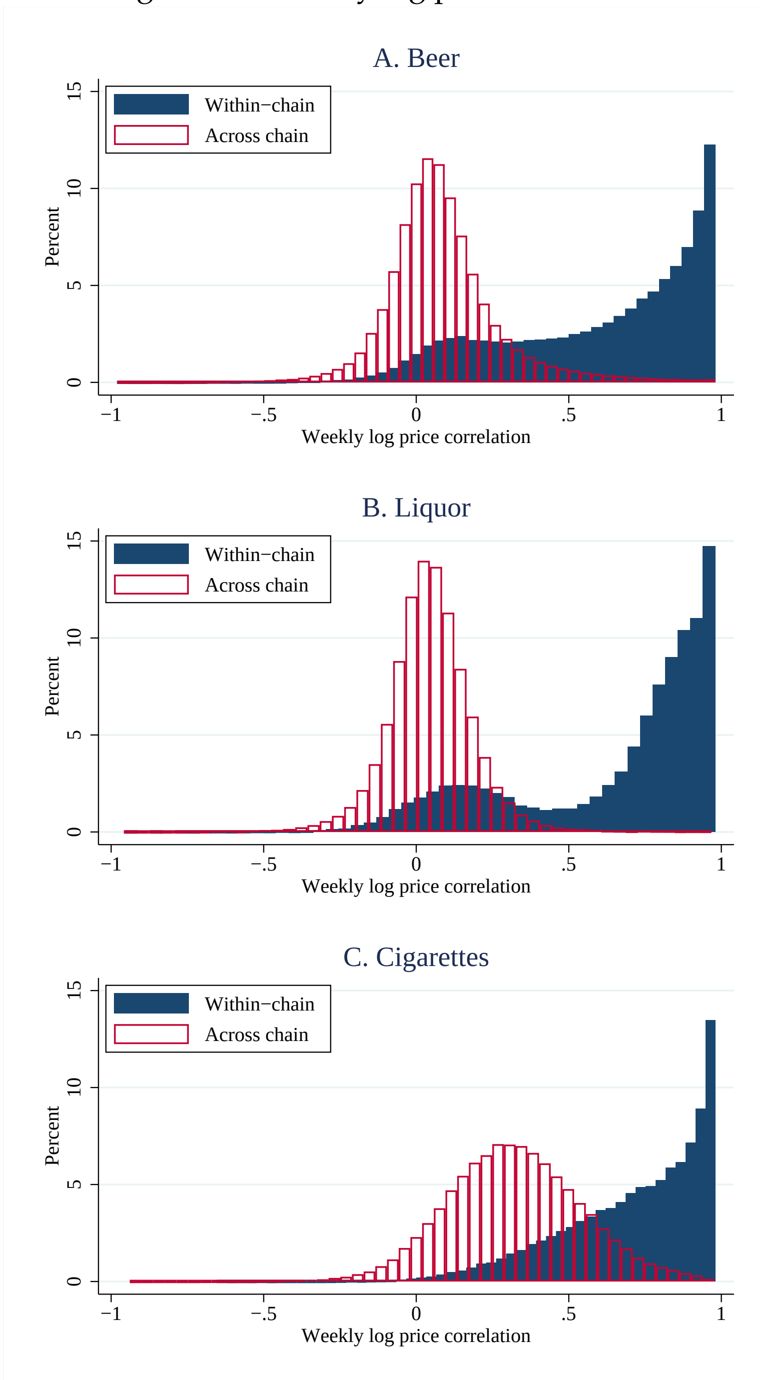
[27]We bin the data to 10 bins of income, whereas DellaVigna and Gentzkow use 25. We use fewer bins because our sample size is much smaller, as we show category-specific relationships. This is the only departure from the DellaVigna-Gentzkow procedure.

Figure B.1: Average quarterly price difference



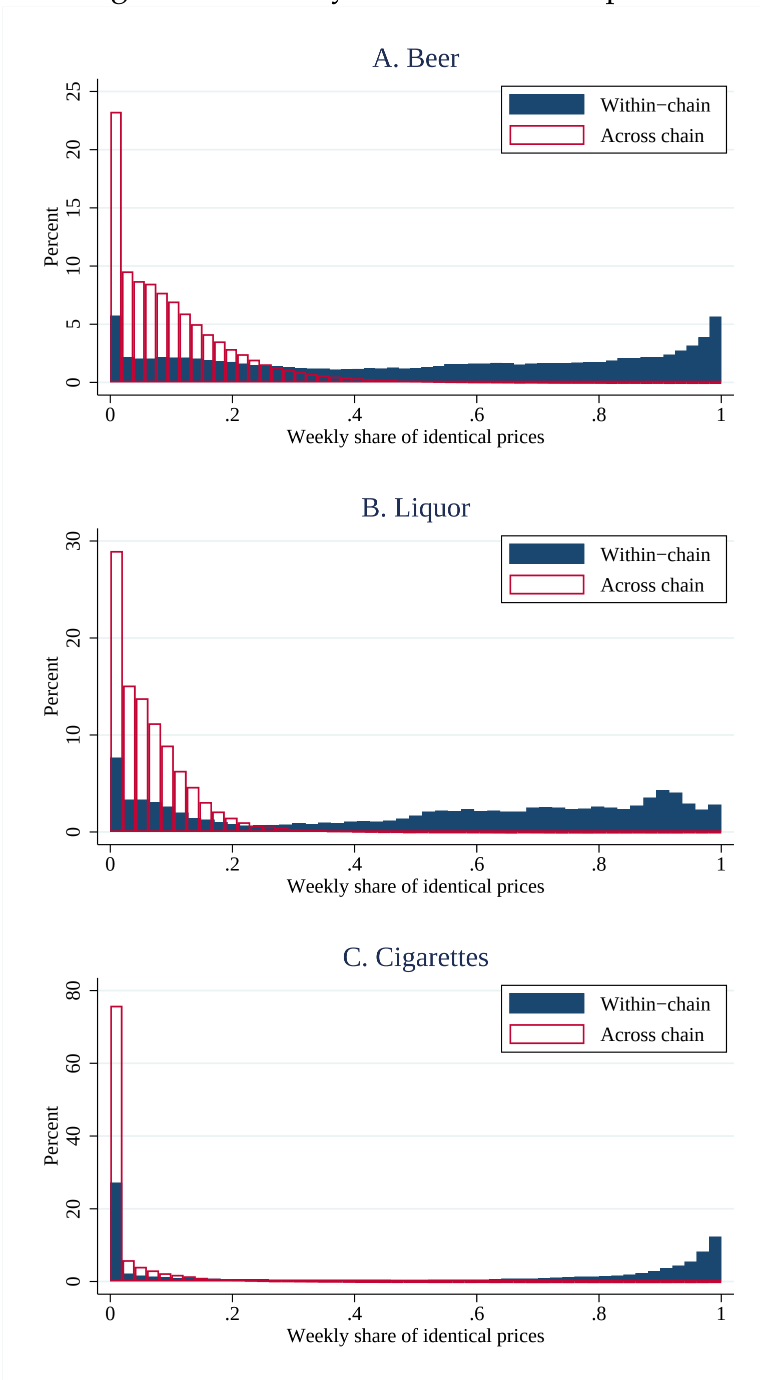
Note: This figure plots the distribution of the quarterly difference in log prices between pairs of stores in the same chain (in solid bars) or different chains (in red bars), for 200 pairs of stores per chain and for all available products in each category.

Figure B.2: Weekly log price correlation



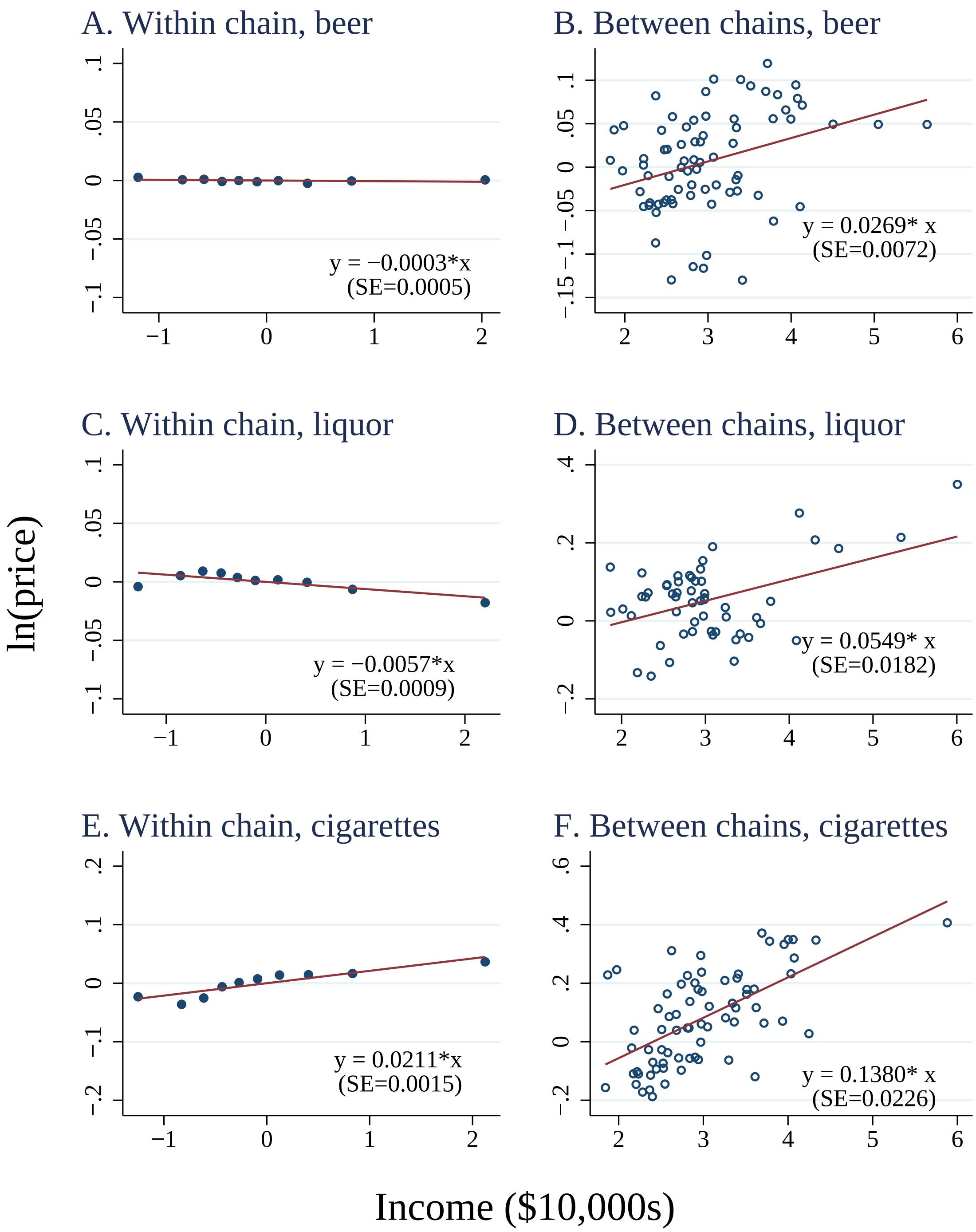
Note: This figure plots the distribution of the weekly log price correlation in log prices between pairs of stores in the same chain (in solid bars) or different chains (in red bars), for 200 pairs of stores per chain and for all available products in each category.

Figure B.3: Weekly share of identical prices



Note: This figure plots the distribution of the share of weeks within a product-store-pair in which the difference in log prices is less than 0.01, between pairs of stores in the same chain (in solid bars) or different chains (in red bars), for 200 pairs of stores per chain and for all available products in each category.

Figure B.4: Prices are correlated with income across chains but not within-chain



Note: Panels A, C, and E, plot store average log price (net of product-year fixed effects and relative to chain mean) against store income (relative to chain main), for each of our three categories. Panels B., D., and F. plot chain average price against chain average income. We report OLS regression estimates. For the within-chain regressions, the unit of observation is a store and the standard errors are clustered on chain. For the between-chain regressions, the unit of observation is a chain and the standard errors are heteroskedasticity-robust.

Table B.1: Measures of pricing similarity, by category

Measure	Absolute difference in quarterly log prices		Correlation in (demeaned weekly) log prices		Share of weekly log prices within one log point	
	Same chain	Different chain	Same chain	Different chain	Same chain	Different chain
	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A: Beer					
Mean	.032	.101	.607	.086	.524	.104
SD	.04	.062	.324	.195	.334	.111
# Chain-products	1830	1928	1809	1923	1829	1928
	Panel B: Liquor					
Mean	.04	.138	.649	.048	.534	.069
SD	.058	.082	.33	.146	.331	.077
# Chain-products	462	522	459	522	462	522
	Panel C: Cigarettes					
Mean	.066	.194	.71	.322	.524	.028
SD	.093	.094	.23	.222	.421	.072
# Chain-products	677	692	677	692	677	692

Note: This table reports the mean and standard deviation of the measures of pricing similarity for pairs of stores within the same chain or in different chains. The sample consists of up to 200 pairs of stores per chain.

C Shrinkage procedure

Our product-event pass-through estimates are estimated with sampling error. When we discuss the distribution of pass-through estimates, we use Empirical Bayes Shrinkage to adjust for this sampling error. The procedure we use goes back to the economics of education literature (where it is used to adjust differences in teacher- or school-specific value-added measures, see e.g., Kane and Staiger (2008); Jacob and Lefregren (2008); Angrist et al. (2017)). This approach is also used by DellaVigna and Gentzkow (2019) for their store-level elasticity estimates; we follow their implementation and description closely.

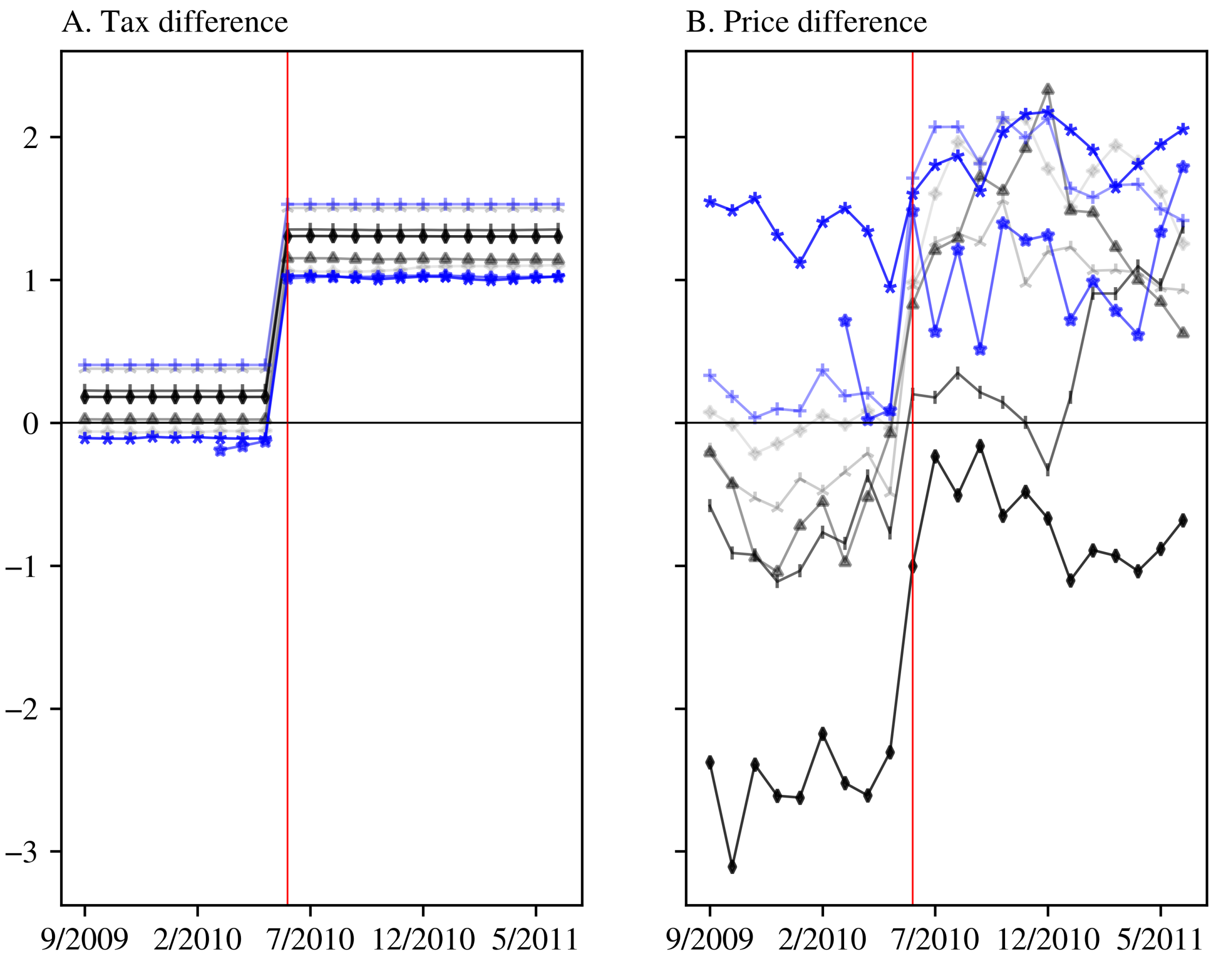
We define the shrunk pass-through estimate for product j in chain c and event e

$$\tilde{\rho}_{jce} = \left(\frac{\sigma_j^2}{\sigma_j^2 + Var(e_{jce})} \right) \hat{\rho}_{jce} + \left(\frac{Var(e_{jce})}{\sigma_j^2 + Var(e_{jce})} \right) \bar{\rho}_j,$$

where $\bar{\rho}_j$ and σ_j^2 are the prior mean and variance (at the category level), and e_{jce} is the estimation error in $\hat{\rho}_{jce}$. We define $Var(e_{jce})$ as the estimate of the asymptotic variance of the estimated pass-through, from Equation 1 or 2. We measure $\bar{\rho}_j$ as the average pass-through rate for product j, averaging across all chains and events, and we measure σ_j^2 as $Var(\hat{\rho}_{jce})$ minus the average estimation variance for that product. We use these shrunk pass-throughs when reporting or plotting the distribution of pass-through estimates, in Figures 3, 6, 8, and 8, as well as Table D.2.

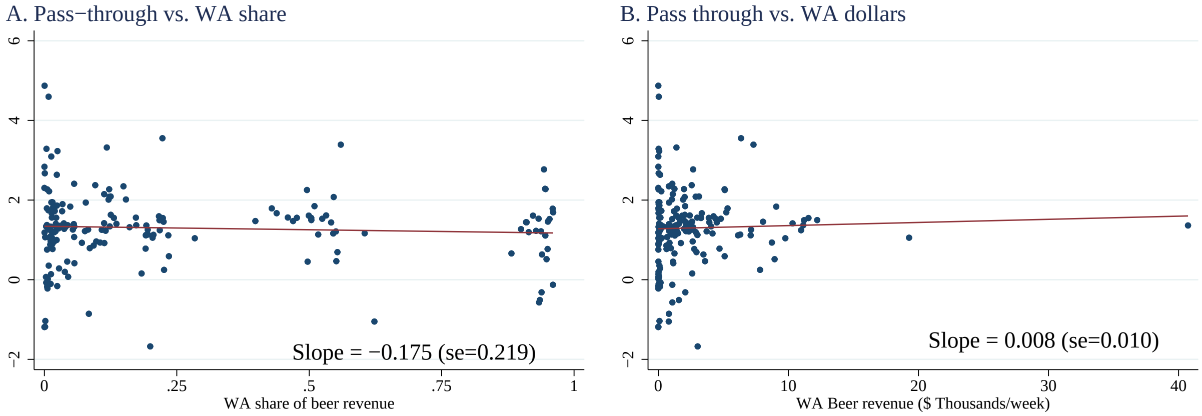
D Additional figures and tables

Figure D.1: Within-chain average difference between WA and non-WA stores



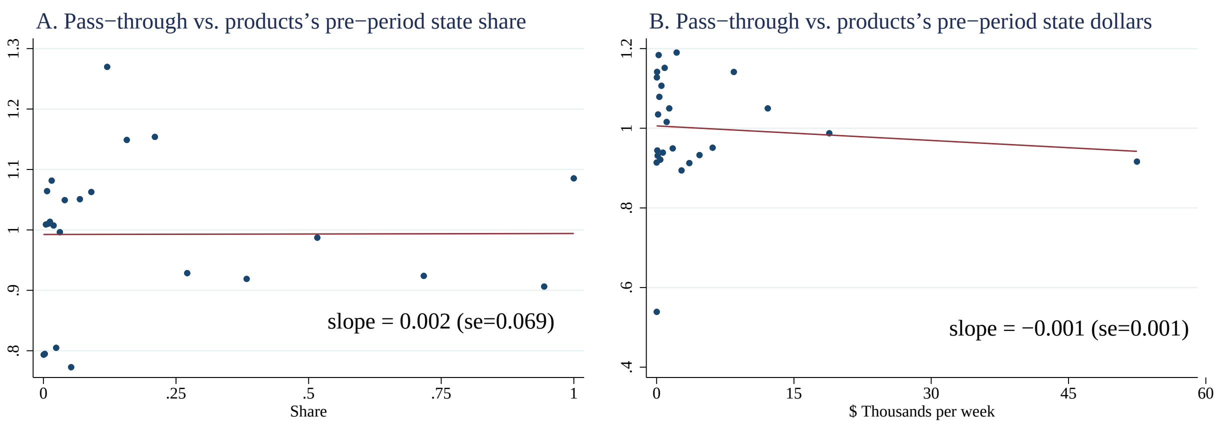
Note: This figure compares stores in WA and non-WA for each of eight exposed chain. Panel A plots the average tax difference between WA and non-WA stores for each chain in monthly frequency. Panel B shows the average price of sample beer UPCs between WA and non-WA stores for each chain. Chains with gray color had lower average price in WA in the pre-period, and the difference was larger as the color is darker. Their prices diverge after the tax increase in WA. Blue color indicates chains which had higher average price in WA in the pre-period, and the difference was larger as the color is darker. Their prices converge after.

Figure D.2: Product-level pass-through and product-level exposure, WA tax increase



Note: This figure plots the product-chain specific pass-through against each product-chain's exposure to WA's tax increase, defined as its pre-tax-increase share of beer revenue in Washington (Panel A, among all revenue for that product-chain) or pre-tax-increase weekly average beer revenue in Washington (Panel B).

Figure D.3: Product-level pass-through and product-level exposure, all tax changes



Note: This figure plots the product-chain specific pass-through against each product-chain's exposure to each tax event. Exposure is defined as the product-chain's prior year share of revenue in the event state (Panel A, among all revenue for that product-chain) or prior year weekly average revenue (Panel B). Data are aggregated to 25 equal-sized bins.

Table D.1: List of excise tax changes

	State	Date	Change	# Exposed chains	# Unexposed chains	Why not analyzed
Beer	NY	2009-05-01	.07	9	42	
	IL	2009-09-01	.1	13	39	
	NC	2009-09-01	.19	12	39	
	WA	2010-06-01	1.13	8	43	
	CT	2011-07-01	.11	4	52	
	TN	2013-07-01	2.28			Other taxes changed at the same time
	WA	2013-07-01	-1.13	8	47	
	RI	2014-07-01	.02			Negligible change
	RI	2015-01-01	.004			Negligible change
	TN	2016-01-01	.32			Other taxes changed at the same time
	LA	2016-04-01	.19	10	38	
Liquor	NJ	2009-08-01	.22	3	21	
	IL	2009-09-01	.8	10	16	
	CT	2011-07-01	.18	3	24	
	RI	2013-07-01	.33			Control state
	LA	2016-04-01	.1	8	14	
Cigarettes	AK	2006-07-01	.2			Nielsen does not have data in AK
	NC	2006-07-01	.05	8	37	
	NJ	2006-07-01	.18	9	36	
	VT	2006-07-01	.7	5	41	
	HI	2006-09-30	.2			Nielsen does not have data in HI
	AZ	2006-12-07	.82	6	34	
	SD	2007-01-01	1	4	32	
	TX	2007-01-01	1	6	32	
	IA	2007-03-01	1	5	32	
	AK	2007-07-01	.2			Nielsen does not have data in AK
	CT	2007-07-01	.49	3	34	
	IN	2007-07-01	.44	7	30	
	NH	2007-07-01	.28	4	33	
	TN	2007-07-01	.42	9	28	
	DE	2007-08-01	.6	7	32	
	HI	2007-09-30	.2			Nielsen does not have data in HI
	MD	2008-01-01	1	9	33	
	WI	2008-01-01	1	4	39	
	NY	2008-06-03	1.25	8	40	
	MA	2008-07-01	1	3	36	
	VT	2008-07-01	.2	4	35	
	HI	2008-09-30	.2			Nielsen does not have data in HI
	NH	2008-10-15	.25	4	34	
	AR	2009-03-01	.56	8	29	
	RI	2009-04-10	1	3	35	
	MS	2009-05-15	.5	6	36	
	FL	2009-07-01	1	6	35	
	HI	2009-07-01	.6			Nielsen does not have data in HI
	KY	2009-07-01	.3	6	35	
	NH	2009-07-01	.7	4	37	
	NJ	2009-07-01	.13	5	36	
	VT	2009-07-01	.45	4	37	
	WI	2009-07-01	.75	5	35	
	DE	2009-08-01	.45	9	31	
	NC	2009-09-01	.1	10	32	
	CT	2009-10-01	1	4	39	
	DC	2009-10-01	1.5	6	37	
	PA	2009-11-01	.25	9	34	
	WA	2010-05-01	1	7	37	
	HI	2010-07-01	.4			Nielsen does not have data in HI
	NM	2010-07-01	.75	6	38	
	NY	2010-07-01	1.6	4	40	
	SC	2010-07-01	.5	8	36	
	UT	2010-07-01	1.01	6	38	
	CT	2011-07-01	.4	6	44	
	HI	2011-07-01	.2			Nielsen does not have data in HI
	NH	2011-07-01	-.1	5	45	
	VT	2011-07-01	.38	5	44	
	IL	2012-07-01	1	11	42	
	RI	2012-07-01	.03	4	49	
	MN	2013-07-01	1.6	6	45	
	MA	2013-07-31	1	5	46	
	NH	2013-08-01	.1	6	46	
	OR	2014-01-01	.13	7	44	
	VT	2014-07-01	.13	7	39	
	MN	2015-01-01	.03	6	39	
	KS	2015-07-01	.5	5	41	
	LA	2015-07-01	.5	9	37	
	NV	2015-07-01	1	9	37	
	OH	2015-07-01	.35	7	39	
	VT	2015-07-01	.33	7	39	
	RI	2015-08-01	.25	4	40	
	AL	2015-10-01	.25	6	38	
	CT	2015-10-01	.25	7	37	
	MN	2016-01-01	.1	6	41	
	OR	2016-01-01	.01			Negligible change
	LA	2016-04-01	.22	9	37	
	CT	2016-07-01	.25	7	40	
	WV	2016-07-01	.65	8	39	
	PA	2016-08-01	1			Not enough sample period post event

Note: This table reports all state excise tax changes for beer, liquor, and cigarettes that we identified as occurring in the period 2006–2016. The change is the legislated tax increase per unit (288 oz beer, 750 ml liquor, 20 cigarettes). We do not analyze some of the tax changes for reasons explained in the table. For the analyzed changes we report the number of exposed and unexposed chains.

Table D.2: Robustness of pass-through estimates to alternative samples

Sample	(1) Main	(2) Clean chains	(3) Clean chains; only multi-state	(4) Clean parents	(5) Clean parents; only multi-state	(6) No border states
A. Pass-through among exposed stores						
Mean	1.00	1.07	0.99	1.14	1.06	0.98
25th percentile	0.63	0.54	0.42	0.57	0.49	0.56
50th percentile	1.07	1.05	1.04	1.09	1.09	1.08
75th percentile	1.43	1.52	1.47	1.61	1.53	1.44
B. Pass-through among unexposed stores in exposed chains						
Mean	0.07	0.02	-0.03	0.03	-0.03	0.06
25th percentile	-0.24	-0.38	-0.41	-0.35	-0.39	-0.24
50th percentile	0.02	-0.01	-0.01	-0.01	-0.01	0.01
75th percentile	0.36	0.38	0.36	0.40	0.37	0.35
Contaminated stores?	No	No	No	No	No	No
Contaminated control chains?	Yes	No	No	No	No	Yes
Contaminated parents?	Yes	Yes	Yes	No	No	Yes
Single-state control chains?	Yes	Yes	No	Yes	No	Yes
Border states	Yes	Yes	Yes	Yes	Yes	Yes
# Events	71	58	59	56	57	58
# Treated chain-events	518	197	198	176	177	372

Note: This table reports the distribution of shrunk estimated pass-through rates among exposed stores in exposed chains (panel A) and unexposed stores in exposed chains (panel B). In column 1, we present our baseline estimates, where the sample consists of all stores in the event state or in clean control states. In column 2, we limit the sample to clean chains, i.e., those without any stores exposed to a tax change in a different state in the event window. In column 3, we further limit the sample to exclude control stores that belong to single-state chains. In column 4, we limit the sample (relative to baseline) to include only clean parent companies, i.e., excluding any chain belonging to a parent company that is exposed to other tax changes during the event window. In column 5, we further limit the sample (relative to column 4) to exclude control stores that belong to multi-state chains. In column 6, we modify the main sample to exclude all stores in border states, to avoid contamination from demand spill overs.

Table D.3: Chain fixed effects are significant, implying cross-chain variability in pass-through

Category	Beer (1)	Liquor (2)	Cigarettes (3)	All (4)
R^2 (no chain FE)	0.134	0.123	0.237	0.177
R^2 (+ chain FE)	0.276	0.236	0.348	0.237
F-stat	7.2	3.7	10.5	7.5
p-value	0.000	0.000	0.000	0.000
N estimates	1,043	238	3,332	4,613
N chains	38	26	68	72

Notes: The first row reports the R^2 from a regression of pass-through estimates (at the chain-event-product level) on event and product fixed effects for the indicated category. The second row reports the R^2 from a regression that adds chain fixed effects. The third and fourth rows report the F-statistic and p-value for the null hypothesis that the chain fixed effects are jointly equal to zero. The fifth row reports the number of observations in the regression (i.e. the number of pass-through estimates) and the final row reports the number of chains.