How Do National Firms Respond to Local Cost Shocks?*

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Abstract

Recent research shows prices are insensitive to local demand conditions because national chains charge geographically uniform prices. We examine the price response to local cost shocks, including 68 excise tax changes, 76 sales tax changes, and other geographically-based cost differences, using data on 35,151 retail stores in 143 multi-state chains. We find local cost shocks are passed-through to local prices, with no spillovers to unaffected stores in otherwise affected chains, and at similar rates for national and local chains. Firms adjust local prices according to local cost changes, suggesting retailers respond asymmetrically to local cost and demand shocks.

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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 (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 distributional effects as well as their ultimate welfare consequences. Textbook models, abstract from the idea of national chains, assuming that firms set local prices optimally, with different demand, costs, or policies implying different prices across markets. This approach dominates applied work (e.g., Nevo (2001); Atkin and Donaldson (2015); Suárez Serrato and Zidar (2016); Pakes (2017)). Yet in a recent, important paper, DellaVigna and Gentzkow (2019) challenge the textbook model, documenting the uniform pricing puzzle that retail prices are uniform within a retail chain, in the sense that prices are nearly uniform within chain; what little price variation exists is 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). Uniform pricing has dramatic implication: a local shock has only a small effect on local prices relative to a national shock of the same size, but it has a spillover effect, as firms would raise prices both in their stores directly exposed to the shock and in their far away, unexposed stores.

We study how national grocery, mass merchandise, and drug chains respond to local cost shocks. We focus primarily on excise taxes, which are levied by local municipalities at the wholesale stage, and represent shocks to retailers' marginal costs. Examining 68 local tax changes affecting beer, liquor, cigarettes, and soda, we find little evidence that uniform pricing constrains local responses. We find that national firms only change local prices in the affected areas, and local pass-through to local and national cost shocks are similar. Stores directly exposed to the excise tax change (because they are in the state or county with the change) have a clear response, with pass-through rates around 1. We find no evidence for spillovers. Following the tax change, we see no price response in unexposed, out-of-state stores belonging to chains with some exposure to the tax increase. Extending the breadth of our findings, we also examine the pass-through of local costs induced by sales tax changes, distance-to-manufacturer, wholesale price regulations, and wholesale prices. In each case we find local price responses. Overall our results show that uniform pricing does not attenuate the response to local cost shocks. Taken with existing evidence that local demand shocks generate little price response, our results suggest firms respond to local cost and demand shocks asymmetrically.

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 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 three testable implications. First, a tax increase in one market raises prices in every market. We call this a spillover to indirectly exposed stores. 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. Third, local pass-through to a local cost-shock would be smaller than to a national cost-shock of the same amount.

We test these predictions using the Nielsen Retail Scanner data, as described in Section 2. These data contain weekly revenue and quantity data for many large, national chains.

The data are product specific, allowing us to identify pass-through effects free of aggregation bias. We limit our sample to widely available products sold in 35,151 stores and 143 chains with sales of beer, liquor, cigarettes, or soda–the product categories commonly subject to excise taxes. Collectively, the stores in our sample are exposed to 68 state- or county-specific excise tax changes. Most chains are not highly exposed to a given tax change. For example, Washington state increased its beer tax substantially 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 case study of Washington's beer tax increase, a large tax increase coming in the middle of a 21-month window when no other state changed its beer tax. The case study shows how we avoid a potential identification challenge that our model highlights. The challenge is that, under uniform pricing, there are spillovers to indirectly exposed stores. For example, a Safeway in California is indirectly exposed to the Washington tax increase because Safeway has Washington stores.[1] Indirectly exposed stores are invalid controls under uniform pricing, so we form a control group from stores in chains with no Washington presence.

The Washington case study reveals four important findings. First, directly exposed stores respond sharply and clearly to the tax, with an implied pass-through rate of 1.05. Second, indirectly exposed stores—such as a Safeway in California—do not respond to the Washington tax increase. 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; in the year prior to the tax increase, 93 percent of its beer sales are in Washington. For this chain the tax change is just like a national shock and the pass-through rate is 0.94, which is identical to that of the four national

[1] 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.

chains operating in Washington and elsewhere, 0.94.

To show the generality of these findings, in Section 4 we extend our analysis to all 68 beer, liquor, cigarette, and soda excise tax changes between mid-2006 and mid-2018 subject to analysis with our data. We find clear local price responses with no spillovers. Among directly exposed stores, prices rise $1.01 following a $1 tax increase. For indirectly exposed stores, prices change little, falling by a statistically insignificant $0.02. Pass-through rates vary little with chain-level exposure to the shock, nor do they differ for small or large cost shocks, although we do find that cost decreases—excise tax cuts—generate smaller pass-through rates.

These local responses are inconsistent with the strongest form of uniform pricing, but it is possible that uniform pricing frictions attenuate local responses to local shocks, relative to national shocks. We rule out this possibility by estimating the pass-through of a *national* tax change: a $0.62 increase in the federal excise tax on cigarettes in 2009. Across multiple identification strategies, we estimate that the national shock is passed-through at a rate of 108-116 percent, close to the pass-through rate of state tax increases. Thus, national retail chains respond locally to local excise tax changes, with no apparent out-of-jurisdiction spillovers to indirectly exposed stores.

Excise taxes represent cost shocks that are salient, public, locally uniform, and persistent. We show in Section 5, however, that our finding of local pass-through of local cost shocks extends to several other types of cost shocks: sales tax changes, wholesale prices, local price regulations, and shipping costs associated with distance. Examining all local sales tax changes, we find pass-through elasticities of near one for directly exposed stores—a 1 percent increase in the sales tax rate induces a 1 percent increase in the after-tax price that customers face, with no spillover to indirectly exposed stores. Cross-state differences in the wholesale price are passed through one-for-one. Federal regulations place county-specific floors on the price of milk received by farmers, and chain-specific prices move one-for-one with these price floors. And beer prices in a given market and for a given chain-product are higher, the greater the distance to the brewery. Thus, we find that national firms adjust local prices to a wide range of local cost shocks, with no spillovers to their indirectly exposed stores.

Our findings contribute to multiple literatures in industrial organization, public finance, and behavioral economics. Primarily we contribute to an emerging literature documenting how prices in national firms vary with heterogeneous market conditions, which we discuss in greater detail in the next section. A clear conclusion from this literature, discussed in more detail in Section 1.1, is that prices vary little within chain, relative to cross-chain differences, even within a market (Hitsch et al., 2017; DellaVigna and Gentzkow, 2019; Adams and Williams, 2019). Large local demand shocks generate little small or zero effects on local prices, (Arcidiacono et al., 2020; Gagnon and Lopez-Salido, 2019), although they may spillover to indirectly exposed stores (Handbury and Moshary, 2021; García-Lembergman, 2020). Our contribution relative to this literature is to examine the effect of local cost shocks. In contrast to existing evidence, we find clear and substantial local prices responses, and no evidence of spillovers. Our evidence, combined with prior literature, suggests that firms' respond asymmetrically to local demand and cost shocks. These findings rule out some models of retail pricing—such as perfect competition, literal uniform pricing, or fully flexible Nash-Bertrand competition—but are consistent with multiple other models, including fairness concerns, tacit collusion, and especially managerial inattention, provided that cost shocks are more salient than demand shocks.

These findings also contribute to the large literature that examines how retail prices respond to policy, particularly excise taxes. Much of excise tax literature has focused on a single or small number of tax changes and asked whether pass-through rates are above or below 1 (Kenkel, 2005; Conlon and Rao, 2019; Keeler et al., 1996; Hanson and Sullivan, 2009; Goolsbee et al., 2010; Harding et al., 2012; Carpenter and Mathes, 2016; Cawley and Frisvold, 2017; Cawley et al., 2020, 2018; Kosonen and Savolainen, 2019)).[2] This research has not investigated whether local tax changes generate attenuated responses among national chains, nor whether they spillover across jurisdictions. We contribute to this literature by showing that national chains are not attenuated in their responses to local tax changes, and these tax changes generate no spillovers to indirectly exposed stores. These

[2]This literature typically focuses on retail prices. Rozema (2018) shows that wholesale and retail prices exhibit very similar pass-through rates of cigarette taxes.

results imply that incidence of local tax changes is born locally.

Finally, we contribute to a 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. While the literature on uniform pricing calls this assumption into question, our results support it.[3]

1 Pricing at National Chains and Local Shocks

1.1 Evidence of Uniform Pricing

The price of a given product is much more uniform within a given chain than across chains. Studying grocery, drug, and mass merchandise chains, DellaVigna and Gentzkow (2019) compare measures of price similarity (at the product level) for pairs of stores in the same chain and in different chains. For same-chain store pairs, the correlation in weekly prices is 0.81, versus 0.09 for different-chain pairs. 62 percent of weekly prices are identical (within 1 percent) for within-chain pairs, versus 10 percent for different-chain pairs. Similarly, Hitsch et al. (2017) document that chain fixed effects explain a considerable share of within product-market price variation. Studying home improvement stores, Adams and Williams (2019) show that prices are often nearly identical for a given chain-product, within geographically large pricing regions.[4]

Moreover, several papers have documented small price responses to geographically local demand shocks. DellaVigna and Gentzkow (2019) show that customer income varies

[3]Two recent papers also explore whether firms react fully to taxes. Harju et al. (2018) find that independent restaurants do not respond to VAT reductions in Sweden and Finland, but chain restaurants show near-complete pass-through. They do not consider, however, how differential exposure affects pass-through. Benzarti et al. (2020) show an asymmetric response to VAT increases and decreases, with attenuated pass-through of tax decreases persisting for several years. This attenuation is distinct from the possible attenuation due to uniform pricing.

[4]While these papers document uniform prices across locations for a given physical product, other work documents uniform prices across distinct products, likely with profit-reducing consequences, for movies (Orbach and Einav, 2007), rental cars (Cho and Rust, 2010), and music (Shiller and Waldfogel, 2011).

substantially across stores within a chain, but store-level prices (within a chain) are uncorrelated with this variation in income. Gagnon and Lopez-Salido (2019) show that, when natural disasters displace demand (moving it to a given store, from competitors forced to close), prices respond little—even for permanent demand shifts such as from Hurricane Katrina. Similarly, when Walmart opens near a given store, demand falls substantially—revenue declines by 16 percent—but prices do not change (Arcidiacono et al., 2020). When school districts expand eligibility for the free school lunch program, households' demand for groceries falls sharply, but local retail prices do not adjust Handbury and Moshary (2021).

Most strikingly, local demand shocks appear to spill over to prices in otherwise unaffected markets. Handbury and Moshary (2021) show that the prices a chain charges in a given county respond to the expansion of free school lunches in the *average* county in which the chain operates, so that collectively out-of-market changes in demand spill over to prices in the local market. Similarly, García-Lembergman (2020) finds that as home prices fell during the Great Recession—generating substantial negative demand shocks—chains responded more to the average change in home prices across all counties in which they operated than to local house price changes. Echoing these findings, DellaVigna and Gentzkow (2019) find that the prices between chains are highly correlated with chain level customer income.[5]

1.2 A Model of Local Cost Shocks Under Uniform Pricing

Given the evidence summarized above, we construct a model that shows how the pass-through of local cost shocks differs with the presence (or lack) of uniform pricing by the retailer. The model lays out a set of empirically testable implications without taking a stance on the convexity of demand (and how it relates to the price elasticity of demand)

[5] Further evidence for spillovers comes from Leung (2021) and Renkin et al. (2020), who examine how minimum wage increases affect grocery prices. This evidence is difficult to interpret because minimum wages affect both demand and cost. Renkin et al. (2020) find the pass-through elasticity of state minimum wage changes to retail prices is between 0.04 to 0.08 on average, consistent with full pass-through (as in our cost results). However, both Leung (2021) and Renkin et al. (2020) find that chains with national network of stores have a smaller response to minimum wage increases than chains completely exposed to shock, consistent with demand spillovers.

nor how the local (marginal) cost conditions of the retailer vary across markets.[6]

A multi-market 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.[7] 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 retailer faces constant marginal costs of supplying goods to each market given by c_m. This set-up allows for a great deal of heterogeneity: demand and cost functions can differ essentially arbitrarily across markets or firms.

Flexible Pricing Benchmark

In the flexible pricing benchmark, the monopolist sets prices to maximize profits separately for each market 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 ($p_m^\star$):

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

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

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

[6]In these ways, the model is a generalization of the uniform pricing model presented in DellaVigna and Gentzkow (2019).

[7]The monopolist assumption is for simplicity only; we show in Appendix A that our results carry through in any market structure, with arbitrary cross-firm heterogeneity.

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

Uniform Pricing

In the case of uniform pricing, we assume the monopolist is constrained to only choose one price for its product across the N markets. In this case, the retailer chooses a single price ($\overline{p}$) to maximize its aggregate profits across all of the markets according to the following objective function:

$$\max_{\overline{p}} \sum_m \left[\overline{p} - c_m\right] x_m(\overline{p}).$$

Under uniform pricing, the pass-through to prices in market m of a marginal cost change in market n, is the same for all markets m and is given by:

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

where $s_n \equiv (x_n / \sum_m x_m)$ denotes the share of quantity demanded in market n across the N markets.[8] Thus we have our first testable implication:

Prediction 1. *(Spillovers) Under uniform pricing, a "local" cost shock in one market raises prices in all markets served by the monopolist.*

Intuitively, uniform pricing means that if a firm wants to raise prices in the market with the cost shock, it must raise prices in all other markets as well.

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

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

Assumption 1. *The demand in each market m, $x_m(p)$, takes a form such that the convexity of demand ($\zeta(p)$) is the same across each of the markets, and remains greater than zero.*

This assumption is consistent with commonly used forms of demand including constant elasticity and linear demand, and does not place any restrictions on demand being either subconvex or superconvex, or having sub- or super-pass-through (Mrázová and Neary, 2017). Additionally, this assumption does not remove the possibility that each market might have different price elasticities of demand at a certain price, or remove the possibility that the price elasticities (convexity) vary. We denote the retailer-wide convexity of demand $\zeta > 0$, dropping the market specific subscript.

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 the form:

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

where $\overline{\varepsilon}$ denotes the quantity weighted average price elasticity of demand across the markets. This result leads to two additional empirically testable conditions of uniform pricing.

Prediction 2. *(Attenuated Pass-through) Under uniform pricing and Assumption 1, the (quantity weighted) average pass-through rate across "local" cost shocks is attenuated compared to fully flexible pricing.*

Prediction 3. *(Exposure) Under uniform pricing and Assumption 1, the pass-through is proportional to the monopolist's "exposure" to the cost-shock, i.e., the share of the monopolist's total demand that comes from the store in the market with the cost shock, holding the price elasticity of demand constant.*

A flexibly pricing monopolist treats the pricing decision of each market independently and consequently has a pass-through rate of $1/(2 - \zeta/\varepsilon_n)$. Relative to this benchmark, the average pass-through rate of the uniform pricing monopolist is attenuated, or "local" cost shocks are not fully passed through. For a market with an average price elasticity of demand, the pass-through rate of a "local" cost shock for a uniform pricing monopolist is attenuated by $1 - s_n$, which is decreasing in the retailer's exposure to the shock. In addition, under uniform pricing retailers' local responses to a local cost-shock would be smaller than to a national cost-shock of the same amount. In the next sections, we test the predictions of the flexible and uniform pricing models.

Before turning to the tests, 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 retailer, with only one of them exposed to the cost shock. However, under uniform pricing, prices will rise by the same amount for a given retailer, so this approach would severely understate the effect of the shock. 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.

2 The Nielsen Retail Panel

Our main dataset is derived from the Nielsen Retail Scanner Data. The database contains quantity and revenue data at the store-week-product level, with products defined as UPCs (barcodes). Over 42,000 stores in 48 states and D.C. contribute data, with most stores in the food, drug, and mass merchandise segments. The Retail Scanner dataset of Nielsen captures over 50 percent of the revenue from all food and drug stores, and a third of revenue coming from mass merchandise (Nielsen, 2018). Our data cover the period 2006-2018. We restrict the set of chains, stores, and products studied. Because we study excise taxes on beer, liquor, cigarettes, and soda, 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 35,151 stores in 83 chains selling beer, 41 chains selling liquor, 77 chains selling cigarettes, and 74 chains selling sugar-sweetened beverages.[9] Table 1 provides summary statistics on the final sample. A majority of chains are multi-state, and the average multi-state chain has stores in about 8 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.[10] 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 13 years. We pick the set of products with at least 80% availability in food stores or drug and mass merchandise stores. For the sugar-sweetened beverage taxes, we look only at carbonated soft drinks because other categories—such as fruit juices, tea, or low-calorie soft drinks (including sparkling water)—are inconsistently taxed. In the end, we have 23 beer, nine liquor, nine cigarette, and 46 sugar-sweetened carbonated beverages products. These set of products constitute an average of 15 percent of their categories revenue across all the stores in Nielsen (e.g., see Table G.5).

We work with prices at the individual product level that are in standardized units,

[9]Our analysis sample selection of chains and stores, encompasses over 80 percent of revenue covered by Nielsen for the Beer and Soda product categories, and over 67 percent for all of our primary product categories (Beer, Cigarettes, Liquor, and Soda).

[10]Under this definition a six pack of 12 ounce cans of Bud Light and would be a different product from a six pack of 12 ounce bottles of Bud Light. 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.

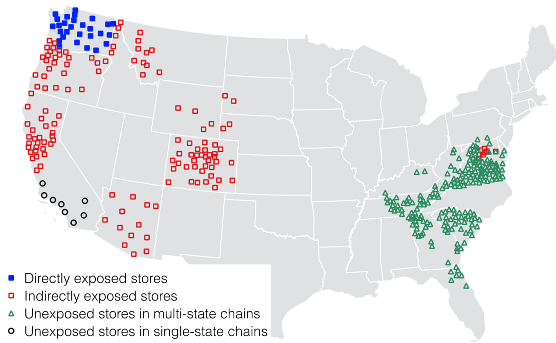
Table 1: Summary statistics

Category	Beer	Liquor	Cigarettes	Soda
Number of products	23	9	9	46
Unit size	288 oz	750 ml	20 cigs	1 oz
# Stores	18,646	1,905	29,805	28,703
# Chains	83	41	77	74
# Multi-state chains	50	20	53	44
Mean # states among multi-state chains	7.48	4.15	7.43	9.48
Price per unit:				
Mean	24.95	15.70	5.85	6.66
25th percentile	20.00	9.99	4.85	2.38
Median	23.98	15.70	5.64	3.20
75th percentile	28.00	19.98	6.58	8.75

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. Soda price is measured as cents per ounce.

which are a 24 pack of beer, a 750-ml bottle of wine, a pack of cigarettes, and one ounce of soda. Prices are not directly reported by Nielsen, but revenue is. We define price as revenue per standardized unit sold at the store-week-UPC level.[11] This price measure is net of excise taxes (which are remitted by wholesalers and therefore incorporated into the on-the-shelf price by retailers) but gross of sales taxes (which are remitted by the retailer and collected at the register). When we study sales taxes in Section 5, we work with the after-sales-tax price. We show in Appendix B that pricing of these products appears uniform in the DellaVigna and Gentzkow (2019) sense: prices are highly correlated across pairs of stores in the same chain, much less so across pairs of stores in different chains, and within a given chain prices are not correlated with store income.

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



Note: This figure illustrates our definition of directly exposed stores, indirectly exposed stores, 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 solid squares are directly exposed. The hollow squares belong to the same chain but are out of Washington, so they are indirectly exposed. 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 state's beer tax increase. On June 1, 2010, Washington state increased its beer tax by $0.50 per gallon, $1.13 per 288 ounces, about 5 percent of the average price of beer. The tax increase was in part a response to a budget shortfall induced by the Great Recession.[12] The tax increase was temporary, expiring July 1, 2013. Although many other states increased excise taxes in the wake of the Great Recession, no other states changed their beer tax or sales tax in the period from nine months before the Washington increase until 12 months after. We use these 21 months as the sample period, so prices in other states are not influenced by other simultaneous tax changes.

To examine the effect of the tax increase, we divide stores in our data into one of four

[11] Average per unit revenue is likely close but not exactly equal to posted prices. There are two sources of discrepancy (Einav et al., 2010). First, some shoppers use loyalty cards or coupons to get a discount. Second, the Nielsen week is Sunday to Saturday, but some stores change prices midweek, so average per-unit revenue is a quantity-weighted average of the two posted prices occurring during that seven-day period.

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

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

Group	# Stores	# Chains	# States			Exposure(%)		
			Min.	Mean	Max.	Min.	Mean	Max.
Directly exposed stores	647	8	2	19	35	0.00	0.23	0.92
Indirectly exposed chains	9,239	8	2	19	35	0.00	0.23	0.92
Unexposed multi-state chains	5,054	44	2	4	19	.	.	.
Unexposed single-state chains	1,559	29	1	1	1	.	.	.

Note: "Directly exposed" stores are in Washington; "Indirectly exposed" stores 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. Exposure is defined as the share of the chain's beer revenue that comes from Washington, in the year prior to the tax increase.

mutually exclusive groups. "Directly exposed stores" are those in Washington. There are eight such chains in our data, all multi-state. "Indirectly exposed stores" are stores outside of Washington that belong to one of these eight chains. To illustrate these definitions, we plot as solid and hollow squares all the markets in which a single chain in our data operates. The solid squares are in Washington and represent directly exposed stores. The hollow squares, outside of Washington, belong to the same chain as the blue squares, and hence are indirectly exposed. We likewise define "unexposed multi-state chains" as chains with no Washington presence. The triangles illustrate one such set of stores. The final category is "unexposed single-state chains;" the circles illustrate one such example. We refer to 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 directly exposed stores and 9,239 indirectly exposed stores. There are an additional 6,613 stores in unexposed chains, about 80 percent of which are in multi-state chains. The mean exposed chain has stores in 19 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

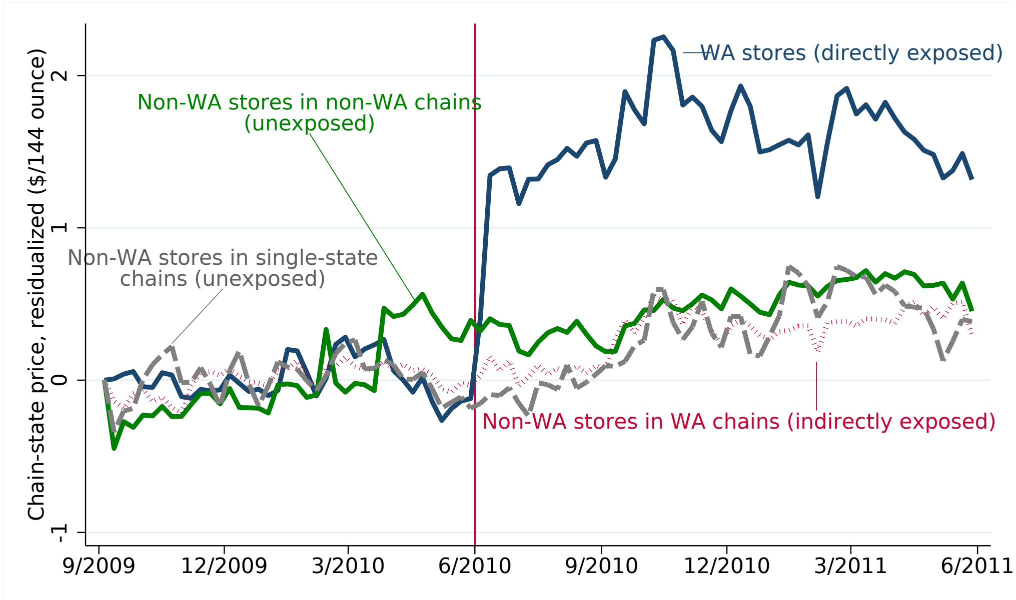
For our analysis we first construct a residual price for store i product j and week t, equal to actual price p_{ijt}, minus the store-product mean $\bar{p}_{ij}$. For computational ease, we next aggregate the residual prices to the chain-state-week level, aggregating across stores and products. We obtain the chain-state-week average residual price, $\tilde{p}_{cst}$, as the simple average of residual prices across stores and products in a given chain-state-week. This aggregation has several advantages. It is computationally convenient. Because it is based on residuals net of store-product fixed effects, it is not affected by changing composition of products or stores. Because the taxes we study apply to all products in a given state, and indirect exposure is also determined at the chain-state level, our aggregation does not lose any relevant information for the effects of excise tax changes. We refer to $\tilde{p}_{cst}$ as the chain-state price.

In Figure 2, we show the average chain-state level price around Washington's beer tax increase, averaging among chain-states in each of our four groups of chains and normalizing each series so that its average in the pre-period is zero. The figure shows several clear patterns. First, most strikingly, for directly exposed stores there is a large increase in prices coinciding with the tax increase. The increase occurs within the month of the policy change, it is sustained for the 12 months after the policy change, and it appears to grow somewhat over the first few months. Second, the other three groups of stores show a general steady increase in prices but no sharp increase. Third, indirectly exposed stores—stores not in Washington but belonging to a chain with a Washington presence—show no differential price trend, relative to the unexposed stores. Fourth, all prices move roughly in parallel prior to the excise tax increase, suggesting that the unexposed, multi-state and unexposed, single-state chains may be valid controls in a difference-in-differences design.

The excise tax increase could have increased or decreased the uniformity of pricing. For exposed chains, the prices in Washington stores converge to those in other states if the pre-price in WA was lower than in other states. On the other hand, the price differences between Washington and other states diverge if the pre-price in Washington was higher than or equal to the price in other states. Appendix Figure G.1 draws the price difference (and tax difference) pre- and post-period for each multi-state chain.

We quantify the pass-through rate with the following regression for the average resid-

Figure 2: Only directly exposed stores responded to Washington's excise tax increase



Note: This figure plots the average chain-state price around Washington's beer tax increase in June 2010, separately by chain exposure to the store. The pre-period average price has been normalized to zero for each group of chains.

ual price in chain c, state s, and week t:

$$\tilde{p}_{cst} = \rho^{\mathrm{d}} \Delta\tau \cdot \mathrm{post}_t \cdot \mathrm{d}_{cs} + \rho^{\mathrm{i}} \Delta\tau \cdot \mathrm{post}_t \cdot \mathrm{i}_{cs} + \mu_{cs} + \theta_t + \epsilon_{cst}. \tag{1}$$

Here $\Delta\tau_e$ is the tax change, \$1.13, post_t is an indicator for the period after the tax change, and d_{cs} and i_{cs} indicate directly exposed and indirectly exposed stores, μ_{cs} are chain-state fixed effects, and θ_t are time fixed effects.

The parameters of interest in Equation 3 are ρ^{d}, and ρ^{i}, the tax pass-through rate, among directly and indirectly exposed stores. This two-way fixed effect model identifies these pass-through rates as the change in prices among exposed stores (direct or indirect), relative to unexposed stores, and scaled by the tax change. The identifying assumption is that, absent the tax change, exposed and unexposed stores would have identical price changes. Support for this assumption comes from Figure 2, which shows roughly parallel trends in prices prior to the tax increase.

We report the estimated pass-through rates in Table 3, as well as the difference be-

Table 3: Only directly exposed stores passed through Washington's excise tax increase

	Directly Exposed (1)	Indirectly Exposed (2)	Difference (3)
Pass-through	1.05	-0.12	1.16
	(0.18)	(0.15)	(0.09)
Observations	29,999	29,999	29,999
Chains	80	80	80

Note: This table reports the pass-through rate for Washington's beer tax increase, among directly and indirectly exposed stores, as well as the difference between them, based on Equation 1. The sample consists of 80 chains and 29,999 chain-state-week observations. See notes to Table 2 for sample definitions. Robust standard errors, clustered on chain, in parentheses.

tween them. We estimate a pass-through rate of 100 percent among directly exposed stores. Directly exposed stores fully pass through the tax increase, but this does not necessarily indicate flexible pricing, because unattenuated pass-through might be in excess of 100 percent. However we also estimate a pass-through rate of -12 percent among indirectly exposed stores. This estimate is statistically insignificantly different from zero, but significantly different from the pass-through rate amount directly exposed stores. Thus, inconsistent with the uniform pricing model, we find greater pass-through among directly exposed stores, and no spillovers to indirectly exposed stores.

3.3 Heterogeneous price responses

Our results so far average over all chains and products. Our model of uniform pricing implies heterogeneous responses across chains and products that are more exposed. To allow for such heterogeneity, we expand Equation 1 to include a set of chain-specific pass-through rates among directly and indirectly exposed stores:

$$\begin{aligned}\tilde{p}_{jst} = &\sum_{k \in \mathcal{C}} \left(\rho_k^{\mathrm{d}} \Delta\tau \cdot \mathrm{post}_t \cdot \mathrm{direct}_{cs} + \rho_k^{\mathrm{u}} \Delta\tau \cdot \mathrm{post}_t \cdot \mathrm{u}_{cs} \right) \cdot 1\left\{\mathrm{chain}_c = k\right\} \\ &+ \mu_{cs} + \theta_t + \varepsilon_{cst}.\end{aligned} \tag{2}$$

Here $\rho_k^{\rm d}$ and $\rho_k^{\rm i}$ are the pass-through rates among directly and indirectly exposed stores in chain k. As we have eight exposed chains, we obtain 16 pass-through estimates. To test the prediction that more exposed chains have higher pass-through, we plot in Figure 3 the estimated ρ_k against each chain's exposure to the tax increase, defined as the share of their beer revenue coming from Washington in the year prior to the tax increase.

Figure 3 shows, first, that for each chain pass-through rate among directly exposed stores is positive and significantly different from zero, and among indirectly exposed stores the pass-through rate is smaller and insignificantly different from zero. For seven of the eight chains, the 95 percent confidence intervals do not overlap for directly indirectly exposed stores. Second, although exposure ranges from near zero to above 90 percent, the most and least exposed stores are similar in their pass-through rate. The chain with 93 percent exposure has a pass-through rate of 94 percent, which turns out to exactly equal the average pass-through rate of the four "national" chains with less than 10 percent exposure. Overall the association between exposure and pass-through rates is near zero. Regressing the chain-specific pass-through rate on exposure, we estimate a coefficient of 0.07 (with a standard error of 0.26), meaning a 100 percent increase in exposure is associated with only a 7 percentage point increase in the pass-through rate. Finally, we see that even among a highly exposed chain, with 90 percent of its beer revenue from Washington, there is no spillover to indirectly exposed stores.

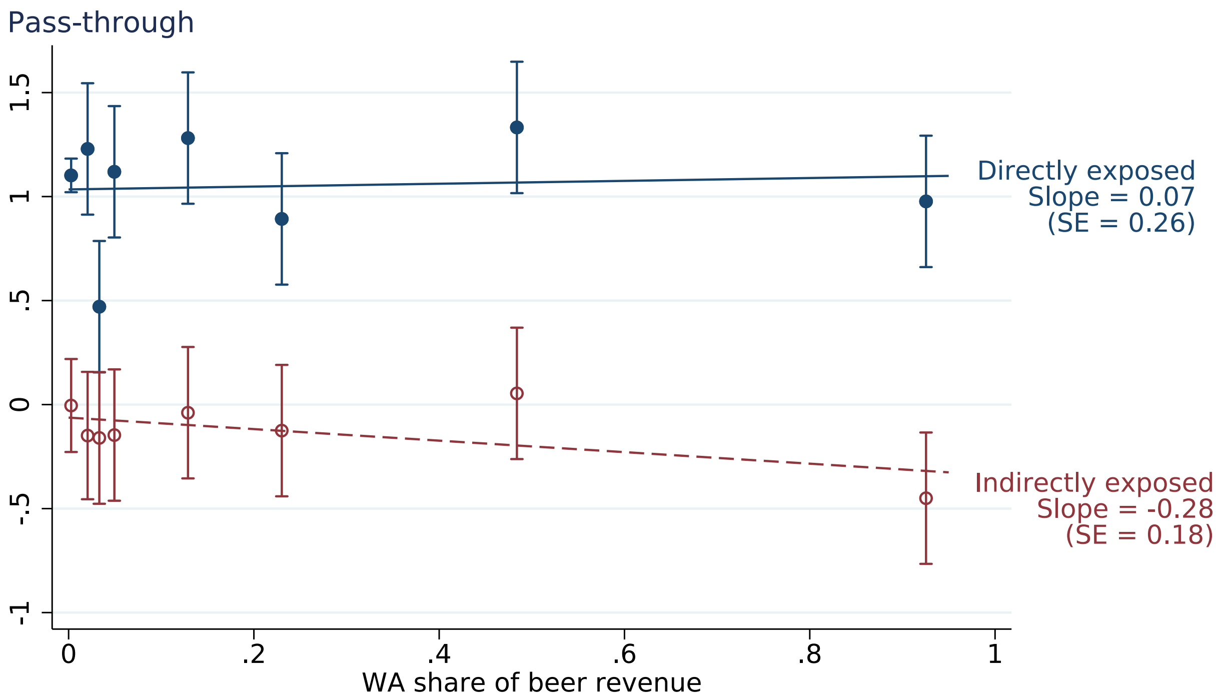
4 Analyzing All State-level Excise Tax Changes

In this section, we show that the pass-through results of the Washington case study generalize by examining a broad set of local excise tax changes affecting several product categories.

4.1 The excise tax changes

We attempt to identify all state excise tax changes affecting alcohol and tobacco products over the period July 2006 to July 2018. We focus on this set of taxes for several reasons.

Figure 3: Pass-through of Washington's beer tax increase does not vary with exposure



Note: This figure plots chain-specific pass-through rates, among directly exposed stores (solid circles) and indirectly exposed stores (hollow circles) against each chain's exposure to the Washington tax increase. The pass-through estimates are from Equation 2. Exposure is defined as the chain's share of beer revenue from Washington, in the year prior to the tax increase. The vertical lines are 95 percent confidence intervals, based on heteroskedasticity-robust standard errors clustered on chain. The figure also reports the estimated slope from an OLS regression of pass-through against exposure, along with heteroskedasticity-robust standard errors.

Table 4: Summary statistics on excise tax changes

Category	All	Beer	Liquor	Cigarettes	Soda
A. Distribution of absolute value tax change					
# Events	68	4	2	60	2
Mean	0.49	0.61	0.51	0.54	1.25
10th percentile	0.01	0.08	0.28	0.04	1.05
90th percentile	1.00	1.13	0.74	1.00	1.45
B. Characteristics of exposed chains, in mean					
# Chains	7.0	10.2	9.5	6.8	5.5
# Directly exposed stores	305	617	134	296	120
# Indirectly exposed stores, in-state	1,052	.	.	.	1,052
# Indirectly exposed stores, out-of-state	9,644	5,775	494	10,101	12,804
# States in which chain operates	15.1	12.2	3.5	15.5	29.2
% Exposure	0.14	0.22	0.25	0.13	0.02
C. Characteristics of unexposed chains, in mean					
# Multi-state chains	31.7	36.0	12.5	31.9	35.5
# Stores in multi-state chains	5,491	5,409	795	5,465	11,117
# Single-state chains	23.9	25.5	15.5	23.9	28.0
# Stores in single-state chains	820	884	284	833	830

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 the mean across chain-events within a category.

The time range guarantees we have at least six months of data before and after each tax change. We focus on state tax changes because states are well-measured in the Nielsen data; finer jurisdictions such as cities are not. These products are themselves important; alcohol and tobacco are the most common groceries subject to state-level excise taxes, and taxes on these products account for 15 percent of state and local excise taxes.[13] In addition to the state taxes, we also study two recently enacted taxes on sugar-sweetened beverages (SSB or "soda" taxes), in San Francisco and Philadelphia. These county-level taxes can be studied in Nielsen, and looking at them lets us study a broader set of products, as well as examine highly local tax changes.

[13]2014 Annual Surveys of State and Local Government Finances. Note that gasoline is also subject to an excise tax, but gasoline stations do not appear to engage in uniform pricing (e.g., Houde (2012)). Note also that we do not study wine, even though it is subject to these excise taxes, because only a single wine product meets our availability criteria, as wine is highly differentiated.

To identify cigarette tax changes, we use 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 implementation. To identify beer and liquor excise taxes, we start 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-check the rate changes with annual tax rate data from Tax Policy Center.[14] We obtain 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. This additional information is important because alcohol excise tax changes are sometimes accompanied by other regulatory changes such as alcohol-specific sales tax changes or changes in Sunday sales.

We only study 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 retail pricing. We therefore exclude excise tax changes which are passed at the same time as general or category-specific sales tax changes. For example, Tennessee implemented a standard volume-based beer excise tax in July 2013, but it phased out a price-based tax simultaneously. We exclude a handful of tax changes which are difficult or impossible to study: Rhode Island's beer taxes in 2014 and 2015, because the two tax changes were less than a year apart; all taxes in Alaska and Hawaii, which are not in the Nielsen data; and liquor taxes in Rhode Island, which is a liquor control state.

The final set of all excise taxes that we study is listed in Appendix Tables G.1-G.4, along with notable exclusions. We refer to each row of the table as an event, i.e., a specific excise tax change affecting a specific product category. We estimate pass-through rates for 68 total events: four beer tax changes, two liquor tax changes, 60 cigarette tax changes, and two soda tax changes. We provide summary statistics on these events in Table 4. The typical event involves a change of \$0.49. As in our Washington case study, for each event we define exposed chains (which have a presence in the event state), directly exposed

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

stores (located in the event state), and indirectly exposed stores (not themselves subject to the tax change, but belonging to an exposed chain). For state-level changes (for beer, liquor, and cigarettes), indirectly exposed stores are always out of state. For the county-level soda taxes, indirectly exposed stores can be in-state. The typical event affects 7 chains, and these chains are typically multi-state chains. The average exposed chain has stores in 15 states. On average chains are not very exposed to tax changes. Directly exposed stores represent 2.8 percent of all stores among exposed chains, and their revenue represents 14 percent of (category-specific) revenue in the pre-period.

4.2 Empirical approach

Overview: We begin by constructing event-specific data sets, looking at prices in a one-year "event window" around the event date. First, for each event, we drop chains which entered or exited in the event state during the event window. 55 of our 62 events have no chains. Next, for each event, we identify directly exposed stores as ones in the jurisdiction raising the tax, and indirectly exposed stores as unexposed ones in a chain with exposed stores. For the sugar-sweetened beverage taxes, which are county-specific, we differentiate between directly exposed, in-state stores (stores in California or Pennsylvania but not in San Francisco or Philadelphia), and indirectly exposed, out-of-state stores.

For each event, we next define a set of control states and clean control stores. Control states did not themselves have an excise or sales 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 control states for New York and all stores in these two states are dropped for the New York event dataset. All stores in control states are not necessarily clean controls, however, because they may belong to a chain facing another 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 deal with this issue. The first approach, motivated by the Washington evidence of no spillovers to indirectly exposed stores, simply assumes that all unexposed stores in exposed chains do not respond to any tax increase (except possibly the focal event). In this approach, all stores in control states are clean controls, and we limit the sample to clean control stores, directly exposed stores, and indirectly exposed stores in control states.

The assumption of no spillovers, however, is strong. 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 belonging to a parent company that was 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. For 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—particularly directly exposed and national chains—resulting in a sample size about two-thirds smaller. We therefore prefer the first approach.

We create a set of chain-state-event level datasets, in parallel to our analysis of the Washington beer tax increase, consisting of weekly average residual prices. We create store-product-weekly residual prices, defined as the actual price less the store-product mean. We then define the chain-state price as the simple average of the residualized price (averaging over stores and products, within a state-week). For county-level tax changes, we aggregate to the relevant tax jurisdiction, for example splitting up Pennsylvania into Philadelphia County and the rest of the state. We denote the chain-state-week-event average residual price as $\tilde{p}_{cste}$ and for simplicity we refer to it as the price.[15] By aggregating to the chain-state level, we vastly reduce the computational burden but retain the advantages of the store-product level data: we have residualized out any price effects coming from changing compositions of purchased goods, and we can measure prices separately among directly exposed, indirectly exposed, and unexposed stores (since we never aggregate across these categories).

[15] This price is event specific because the set of weeks used to residualized the raw prices is event specific.

Estimating equation: To maximize power, we stack the event-specific datasets into a single dataset and estimate pass-through in a single pooled model for the price of chain c, state s, in week t and event e:

$$\tilde{p}_{cste} = \rho^{\mathrm{d}} \Delta\tau_e \cdot \mathrm{post}_{te} \cdot \mathrm{direct}_{cse} + \rho^{\mathrm{i}} \Delta\tau_e \cdot \mathrm{post}_{te} \cdot \mathrm{indirect}_{cse} \rho^{\mathrm{ii}} \Delta\tau_e \cdot \mathrm{post}_{te} \cdot \text{indirect-in}_{cse} + \mu_{cse} + \theta_{te} + \epsilon_{cste}. \tag{3}$$

Here $\Delta\tau_e$ is the event-specific tax change, post_{te} is an indicator for the period after the tax change, and direct_{cse}, $\mathrm{indirect}_{cse}$, and indirect-in_{cse} indicate directly exposed stores, indirectly exposed out-of-state stores, and indirectly exposed in-state stores, respectively. μ_{cse} is event-specific chain-by-state set of fixed effects,[16] and θ_{te} is event-specific time fixed effects. Because we include event-specific state and time fixed effects, this specification allows for essentially unrestricted heterogeneity across events. The only restriction is that the pass-through rates are the same, although in specifications below we relax this restriction. We calculate heteroskedasticity-robust standard errors, clustered on chain, which account for the usual concern of autocorrelated residuals, as well as the fact that the same chain can serve as a control in multiple events.

The parameters of interest in Equation 3 are ρ^{d}, ρ^{i}, and ρ^{ii} the excise tax pass-through rate, among directly exposed stores, indirectly exposed stores, and indirectly exposed in-state stores. Under our simple model of uniform pricing, we expect $\rho^{\mathrm{d}} = \rho^{\mathrm{i}} = \rho^{\mathrm{ii}}$. However, under flexible pricing, we expect $\rho^{\mathrm{d}} > 0$, and $\rho^{\mathrm{i}} = \rho^{\mathrm{ii}} = 0$.

Equation 3 identifies pass-through rates as, essentially, the differential change in prices from pre-period to post-period, for exposed stores relative to unexposed stores, scaled by the change in the tax rates, and averaged across events. Our identification assumption is that prices would not have changed differentially in the exposed stores, absent the tax change. To test for such differential changes, and to examine the dynamics of price

[16] In fact we include slightly richer fixed effects, chain-state-event-tax jurisdiction, where tax jurisdiction refers to the geography with the tax change. For state-level changes this is collinear with state. For our two county-level changes, we end up with separate fixed effects for the county changing its tax and the rest of the state.

adjustments, we estimate event-study versions of Equation 3:

$$
\begin{aligned}
\tilde{p}_{cste} = & \sum_{m \neq -1} \rho_m^{\text{d}} \Delta_{\tau e} \cdot \text{direct}_{cse} \cdot 1\{m_{te} = m\} \\
& + \sum_{m \neq -1} \rho_m^{\text{i}} \Delta_{\tau e} \cdot \text{indirect}_{cse} \cdot 1\{m_{te} = m\} \\
& + \sum_{m \neq -1} \rho_m^{\text{ii}} \Delta_{\tau e} \cdot \text{indirect-in}_{cse} \cdot 1\{m_{te} = m\} \\
& + \mu_{cse} + \theta_{te} + \epsilon_{cste}.
\end{aligned}
\tag{4}
$$

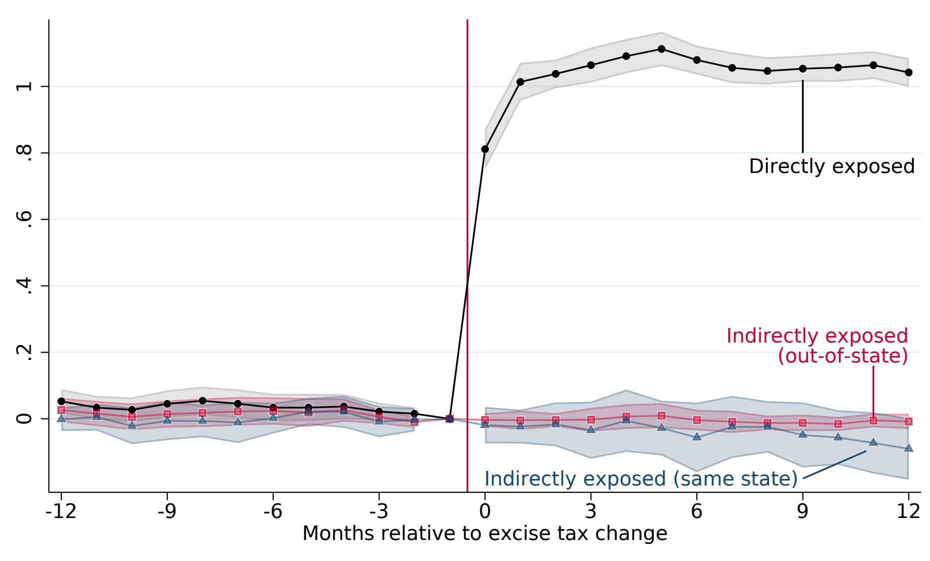
Here m_{te} is the month of week t, relative to the date of event e, so month 0 is the month of the tax change. This specification allows for month-specific pass-through rates, ρ_m. If our identification assumption is valid, we expect $\rho_m^{\text{d}}, \rho_m^{\text{i}}$ and ρ^{ii} to be zero when $m < 0$.

While our baseline specifications assumes equal pass-through rates across chains and events, the model of Section 1 implies that these rates are heterogeneous, varying with chains' exposure to the tax change. We therefore also estimate models of the following form:

$$
\begin{aligned}
\tilde{p}_{cste} = & \rho^{\text{d}} \Delta\tau_e \cdot \text{post}_{te} \cdot \text{direct}_{cs} + \rho_X^{\text{d}} \Delta\tau_e \cdot \text{post}_{te} \cdot \text{direct}_{cs} \cdot X_{ce} \\
& + \rho^{\text{i}} \Delta\tau_e \cdot \text{post}_{te} \cdot \text{indirect}_{cs} + \rho_X^{\text{i}} \Delta\tau_e \cdot \text{post}_{te} \cdot \text{indirect}_{cs} \cdot X_{ce} \\
& + \rho^{\text{ii}} \Delta\tau_e \cdot \text{post}_{te} \cdot \text{indirect-in}_{cs} \rho_X^{\text{ii}} \Delta\tau_e \cdot \text{post}_{te} \cdot \text{indirect-in}_{cs} \cdot X_{ce} \\
& + \mu_{cse} + \theta_{te} + \epsilon_{cste}.
\end{aligned}
\tag{5}
$$

Here X_{ce} is a measure of chain c's exposure to event e. Our primary measure is the share of c's pre-period revenue (for the given product category) in the event state. In robustness tests we also measure exposure with an indicator for being a "local chain," which we define as a chain having at least 90% of its revenue in the event state. About one in eight exposed chains are local in this sense. ρ_X measures how pass-through varies with exposure. Because larger chains may be more geographically dispersed, and hence less exposed, we risk conflating heterogeneous pass-through by exposure with heterogeneous pass-through by size. In some specifications we therefore also control for interactions between post, directly/indirectly exposed, and chain size, as proxied by chain-category

Figure 4: Only directly exposed stores respond to excise tax changes



Note: Figure plots the coefficients from Equation 4, along with their 95% confidence intervals, based on chain-clustered standard errors. The coefficients can be interpreted as the trend in prices among directly exposed and indirectly exposed stores, relative to unexposed stores, following a $1 excise tax change.

total revenue in the pre-period.

4.3 Results

Event study: We begin by presenting the event study estimates, from Equation 4, graphically in Figure 4. The figure plots the estimated monthly pass-through rates, as well as their 95% confidence intervals. The estimates are flat around 0 in each month prior to the tax change month, for directly exposed and indirectly exposed stores. Thus, we see no anticipatory effects nor reason to doubt the parallel trends assumption. Following the tax increase, there is a sharp increase in prices among the directly exposed stores. The increase in the first month is about 80 percent of the tax increase (i.e. $\rho_1^{\mathrm{d}} \approx .8$), and it rises to over 100 percent in the second month, where it remains for at least 10 more months. Thus among directly exposed stores, we see immediate, sharp pass-through that grows to near 100 percent after two months. We see no spillovers to indirectly exposed stores. For both in-state and out-of-state indirectly exposed stores, estimated price responses remain zero after the tax change (in month 12 we estimate a marginally significant but negative response among indirectly exposed, in-state stores).

Pass-through estimates: We report our main pass-through estimates in Table 5. In column (1) we pool all categories, and in columns (2)-(4) we report pass-through rates obtained from estimating Equation 3 category-by-category. We estimate a precise 101 percent pass-through rate for directly exposed stores. Thus, our first conclusion from the Washington case study—that local cost shocks are passed through to local prices—generalizes to a wide set of excise tax changes. Indeed for each category we estimate a substantial and significant pass-through rate, although the magnitude varies across categories, with lower pass-through rates for beer and sugar-sweetened beverages and higher pass-through rates for cigarettes and liquor.

In contrast, for indirectly exposed stores we estimate statistically insignificant pass-through rates of near-zero. Our point estimate is -2 percent for indirectly exposed stores (out-of-state) and -4 percent for indirectly exposed in-state stores (which is identified by excise tax changes on sugar sweetened beverages only). In no category do we see statistically significant or economically meaningful pass-through for indirectly exposed stores. We always reject the hypothesis that the pass-through rates are equal for directly exposed stores and indirectly exposed stores (p-value $<$0.001 in all cases). These results extend the generality of our second finding from Washington: indirectly exposed stores in exposed chains do not respond to the tax change.

Robustness: Our findings are robust to a number of alternative specification choices. We show this robustness in Appendix Table G.6, which includes our baseline estimates in column (1) for comparison. In column (2), we limit the sample to chains with no simultaneous exposure to multiple excise or sales tax changes.[17] In column (3), we address the concern that our pass-through rates may be attenuated by people shopping out-of-state (Harding et al., 2012; Baker et al., 2017) by dropping stores in border counties. In column (4), we address the concern instead by dropping border *states*, i.e., states bordering the event state, which may be contaminated controls. Finally, in column (5) we omit the three months immediately before and after the tax change, to capture potential longer-run re-

[17] Our baseline sample excludes states with tax changes within a year of the event state. Here, we exclude chains whose parent company has any presence in the excluded states.

Table 5: Pass-through rates estimated from all excise tax changes

Category	All (1)	Beer (2)	Liquor (3)	Cigarettes (4)	SSB (5)
Directly exposed	1.01	0.72	1.42	1.05	0.79
	(0.02)	(0.12)	(0.14)	(0.01)	(0.07)
Indirectly exposed	-0.02	-0.09	0.03	-0.01	0.01
	(0.01)	(0.10)	(0.18)	(0.01)	(0.03)
Indirectly exposed, same state	-0.04				-0.03
	(0.03)				(0.04)
# Observations	1,775,913	120,601	19,283	1,555,735	80,294
# Chains	143	83	41	77	74

Note: This table reports the pass-through rate ρ for directly exposed stores (ρ^{d}), indirectly exposed stores (ρ^{i}), and indirectly exposed stores in the same state (ρ^{ii}) as the tax change, from Equation 3. Indirectly exposed stores are not themselves exposed to the tax increase but belong to chains with exposure to the tax increase. Robust standard errors, clustered on chain, in parentheses. Beer, liquor, and cigarette taxes are state-level and so there are no in-state, indirectly exposed stores in those categories. SSB refers to sugar-sweetened beverages.

sponses. In all cases our point estimates do not change meaningfully; we continue to find substantial pass-through among directly exposed stores but not indirectly exposed ones.

Heterogeneity: The uniform pricing model's final prediction is that pass-through rates vary with exposure, with near-zero pass-through among chains with near-zero exposure. We test this prediction by including interactions between the tax change and exposure. We report these interaction effects in Table 6. The coefficients in column (1) indicate that a directly exposed store in a chain with zero exposure has a pass-through rate of 99 percent, but for a store in a chain with 100% exposure, the pass-through rate would be 106 percent, an insignificant difference. We estimate statistically insignificant pass-through among indirectly exposed stores, with an insignificant and small coefficient on the interaction with exposure. These estimates change little after controlling for chain revenue (interacted with direct/indirect and with the tax change), as we show in column (2), although we do find a significantly greater pass-through rate for completely exposed stores here (a 9 percentage point increase in pass-through for a 100 percentage point increase in exposure). This finding is robust to measuring exposure with an indicator for "local chain"—by this measure, directly exposed stores in highly local chains have insignificantly higher pass-through rates, and indirectly exposed stores in such chains have significantly lower pass-through rates. Thus, our final conclusion from Washington largely generalizes: more exposed chains do not show substantially higher pass-through.

As an alternative dimension of heterogeneity, we estimate pass-through separately for "large" tax changes (above the median positive amount), "small" tax changes (below median, but positive), and negative tax changes. We plot event studies for these different tax changes in Appendix Figures G.2, G.3 and G.4, and we report pass-through rates in Table G.7. We see rapid and near complete pass-through for both large and small tax increases, but slower and incomplete pass-through of tax decreases. However, in no case do we see spillovers to indirectly exposed stores.[18]

4.4 Benchmarking against the response to a national shock

The results thus far do not indicate if the price responses to our local cost shocks are attenuated relative to the response to a national shock. To investigate this possibility, we examine the pass-through of a substantial increase in the federal cigarette tax, from $0.39 to $1.01, the only federal excise tax change to any of our products during our sample period. This increase was legislated by the Children's Health Insurance Program (CHIP) Reauthorization ACT, which was signed into law on February 4, 2009, and became effective April 2, 2009.

Examining the pass-through of this tax change is difficult because it affects all tobacco products in all states. We consider three identification strategies. First, we use simple pre-post comparisons, adjusting for observables. These comparisons could be biased because of nonlinear trends generated, for example, by changing income or unemployment around the Great Recession. To address this possibility, our second identification strategy uses the simple average price of other groceries as a counterfactual price trend. To more systematically match the price trend, our third identification strategy forms a syn-

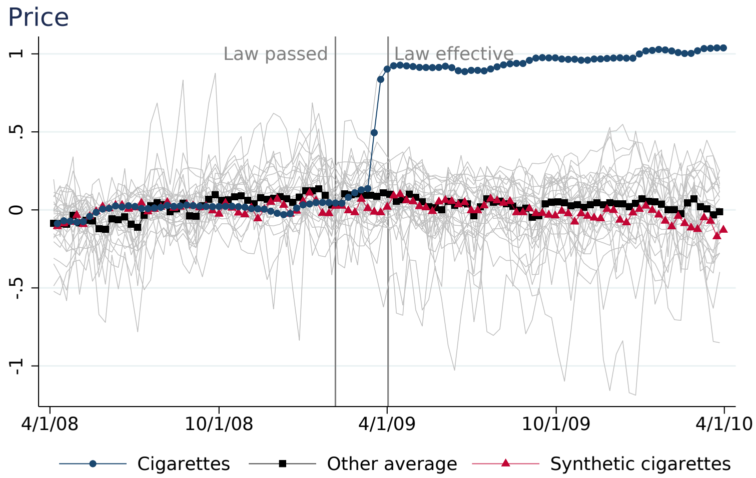
[18]Our finding of asymmetric responses to tax increases and decreases is consistent with Benzarti et al. (2020), who find smaller responses to VAT decreases than increases, as well as asymmetric responses to wholesale gasoline prices (Borenstein et al., 1997; Peltzman, 2000). While Benzarti et al. find the asymmetry lasts for years, we cannot reject complete pass-through by 11 months after the tax change.

Table 6: Pass-through rates by exposure

Exposure measure	Revenue share		Local indicator	
	(1)	(2)	(3)	(4)
Directly exposed	0.99	0.98	1.00	1.00
	(0.02)	(0.03)	(0.02)	(0.03)
× Exposure	0.07	0.09	0.03	0.04
	(0.04)	(0.04)	(0.03)	(0.04)
Indirectly exposed	-0.02	-0.02	-0.02	-0.02
	(0.01)	(0.02)	(0.01)	(0.02)
× Exposure	0.01	0.02	-0.27	-0.26
	(0.08)	(0.08)	(0.12)	(0.12)
Indirectly exposed, in-state	-0.02	-0.10	-0.04	-0.10
	(0.02)	(0.06)	(0.03)	(0.05)
× Exposure	-0.58	0.09		
	(0.50)	(0.45)		
# Observations	1,775,913	1,775,913	1,775,913	1,775,913
# Chains	143	143	143	143
Control for chain size?	No	Yes	No	Yes

Note: This table reports the main effect of an excise tax change on prices, for directly and indirectly exposed stores, as well as coefficient on the interaction between the tax change and the chain's exposure (the rows labelled "× Exposure"). Exposure in columns (1) and (2) is measured as the chain's category-specific pre-period revenue share in the tax changing state. In columns (3) and (4) it is measured with an indicator for at least 90 percent revenue share. (There are no chains that are local to the county-specific taxes and so the interaction between local and in-state is not identified.) Robust standard errors, clustered on chain, in parentheses.

Figure 5: Residual prices around the CHIP Reauthorization Act of 2009



Note: This figure plots the weekly average residual price in each module, 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 so that its 2008 average is zero. The thick blue line is cigarette prices. The thin lines are the 30 other product categories. The solid line is the average of the other categories' prices, and the dashed line is the synthetic cigarette price series.

thetic control for cigarette prices using a weighted average of the prices in other categories (Abadie et al., 2010).

To analyze the federal taxes, we focus on prices from April 2, 2008 through April 2, 2010. We extend our sample to include not only cigarettes but all widely available products in the 31 product categories studied by DellaVigna and Gentzkow (2019). We limit the sample to states that did not have their own state-level tobacco excise tax change during the time period. As in our main analysis, we residualize all prices net of store-product fixed effects. For each category, we calculate the average weekly price (taking the simple average of residualized prices across stores and products). We scale prices so that they equal the average cigarette price in 2008 (to adjust for differences in unit size across categories), and we shift the series so that they each average zero in 2008.

Figure 5 plots the trends in residualized prices around the federal excise tax increase. The circles show the average price of cigarettes. The thin lines show the average price of each other category. Cigarette prices show a slight trend prior to the excise tax increase

Table 7: Pass-through of the federal cigarette tax increase

Control group	None		Other groceries	
	(1)	(2)	(3)	(4)
Pass-through	1.54	1.08	1.56	1.16
	(0.01)	(0.09)	(0.05)	(0.08)
Interim pass-through	0.54	0.18	0.43	0.11
	(0.19)	(0.14)	(0.04)	(0.06)
# Modules	1	1	31	31
# Observations	104	104	3224	3224
Control for PPI	No	Yes	No	Yes
Time fixed effects	No	No	Yes	Yes

Note: This table reports the estimated pass-through rate of the federal cigarette tax increase. Columns (1) and (2) include only cigarette prices and are based on Equation 6 using data from April 2, 2008, through April 2, 2010, in states without tobacco tax increases. Columns (3) and (4) use other groceries as a control group and are based on Equation 7. In columns (1) and (2) we report heteroscedasticity-robust standard errors in parentheses. In columns (3) and (4) we further cluster the standard errors on product category.

announcement. They rise about 30 cents in the weeks immediately after the announcement, then rise an additional 60 cents in the weeks just before the law becomes effective, remaining elevated in the following year. No other product category shows a comparable rise in prices. There is essentially no trend in prices for the average category, nor for the synthetic control.[19]

To quantify the implied pass-through rate, we estimate a series of regressions, beginning with a simple model that regresses cigarette taxes on the tax change interacted with the interim period (between the date the law passed and the date it became effective) and the post period:

$$\text{price}_t = \alpha + \rho^{\text{interim}} \Delta\tau \cdot \text{interim}_t + \rho \Delta\tau \cdot \text{post}_t + \epsilon_t \tag{6}$$

This is a simple pre-post comparison, scaling the price change by the tax change. We report the estimates in column (1) of Table 7. We estimate a pass-through rate of 54 percent in the pre-period and 154 percent in the post period. This is substantially higher than

[19] For more details on the construction of the synthetic control, see Appendix D.

the 101 percent pass-through rate of state-level excise changes. However the pre-post comparison is potentially confounded by an increase in the tobacco producer price index around the time of the federal tax increase; see Appendix Figure G.5. This increase was likely not driven by the federal tax increase, because the tax's statutory incidence was on wholesalers, and the PPI measures producer prices. However the coincident PPI increase likely drove prices up beyond the effect of the tax itself. A further confounding factor is that the tax increase was implemented during the Great Recession, when falling incomes may have led to lower grocery prices.

To address the confounding influence of PPI changes, we include tobacco PPI as a regressor. The results, in column (2) of Table 7, show a pass-through rate of 108 percent, or slightly higher than the rate for state-level taxes. Column (2) also shows that the apparent anticipatory price increase in the interim period was likely due to the simultaneous increase in tobacco PPI rather than the tax increase, as the interim pass-through rate is now small and insignificant.

To address the possibility of confounding income effects, or other general trends driven by the Great Recession and its aftermath, we use general grocery prices to control for any general trend in prices. We expand our sample to include 30 additional grocery prices, and we estimate the following model for prices in category c and week t:

$$\text{price}_{ct} = \alpha + \rho^{\text{interim}} \Delta\tau \cdot \text{interim}_{ct} \cdot \text{cig}_c + \rho\Delta\tau \cdot \text{post}_{ct} \cdot \text{cig}_c + \beta \text{PPI}_t \cdot \text{cig}_c + \mu_c + \theta_t + \epsilon_{ct} \quad (7)$$

This model adjusts for general trends in grocery prices with a set of time fixed effects, θ_t. It controls for category differences in prices with category fixed effects μ_c. The pass-through rates are the coefficients on the interactions between a cigarette category dummy, post period, and the tax change. We estimate versions of Equation 7 that do and do not control for tobacco PPI interacted with the cigarette dummy.

We report the results from this controlled specification in columns (3) and (4) of Table 7. Controlling for a general trend makes very little difference to the estimated pass-through rate, consistent with the flat general trend in grocery prices evident in Figure 5. Again once we control for PPI, we estimate that federal and state excise taxes are passed through at a similar rate.

Equation 7 identifies the counterfactual trend in cigarette prices (i.e., the θ_t) from, essentially, the trend in average grocery prices. However the simple average of all categories may be a poor counterfactual for cigarettes. In Appendix D we construct a synthetic control group for cigarettes, and we find pass-through rates very similar to those in columns (1) and (3) of Table 7. Thus, a more principled approach to choosing the control group yields similar estimates.

Overall we find that, after adjusting for PPI, the federal tax is passed through similarly but at a slightly higher rate than state excise taxes, at a near equal rate, about 108 percent versus 101 percent. The similarity of the federal and state pass-through rates is most consistent with the flexible pricing model. However, the smaller pass-through rate for state tax changes may indicate some attenuation because of uniform pricing. Although it is also consistent with other explanations, such as cross-border shopping attenuating pass-through of state-specific changes.

5 Other Local Cost Shocks

Excise taxes are potentially unusual in that they are salient, public, locally uniform, and persistent. Thus, our findings so far are potentially not representative of how local cost shocks are passed-through in general. In this section, we present evidence of how prices respond to several additional types of local cost shocks: sales tax changes, wholesale prices, local price regulations, and shipping costs associated with distance. In each case, we find that these cost shocks are passed through to local retail prices. In the case of sales taxes, where we can measure spillovers to indirectly exposed stores, we find none.

5.1 Sales Taxes

Approach: We begin with an analysis of sales tax changes. This analysis sales taxes parallels the analysis of excise taxes closely and keeps our focus on beer, liquor, and

Table 8: A wide variety of local cost shocks are passed through locally within-chain

Shock	(1) Sales taxes	(2) Wholesale prices	(3) Price regulations	(4) Shipping distance
Effect on Directly exposed	0.97	0.75	1.14	1.17
	(0.14)	(0.15)	(0.19)	(0.26)
Effect on Indirectly exposed	-0.06			
	(0.15)			
# Observations	1,633,971	1,116	4,233,373	204,502
# Chains	83	3	81	77
Categories	Beer, Liquor, Cigarettes	Cigarettes	Milk	Beer

Notes: Column (1) reports the pass-through elasticity (i.e., a log-log specification), while columns (2)-(4) report pass-through (from a level-level regression). The exact specification differs from column to column; see text for details. For shocks on other than sales taxes, we cannot estimate effects on indirectly exposed stores because there is no variation in costs for them (conditional on the set of fixed effects). In all cases we report heteroscedasticity-robust standard errors, clustered on chain, in parentheses.

cigarettes.[20] Here, we briefly summarize how the procedure differs, leaving the remaining details (including details on the selected events) to Appendix F. First, for our measure of prices we use the after-sales-tax price. This requires us adding the relevant sales tax rate to the price we observe, as sales taxes are not included in the price reported by Nielsen. Second, we use the log-log functional form, because sales taxes are an ad valorem tax. More specifically, for each event e our measure of price for product i in store s in week t is $\ln(\text{Price}_{iste}(1+\tau_{iste}))$ where τ is the ad valorem sales tax rate. Analogously to our excise tax analysis, we get a set of residualized prices net of a set of store-product fixed effects, and then get an average (residualized, log) price at the chain-state-week level, $\ln p_{cste}$, by taking a simple average across products and stores.

We estimate the pass-through rate of the sales tax rate to retail prices for each of our

[20] We do not expand the product categories for the sales tax analysis, because for a majority of states food stables including most groceries are exempt from sales taxes (e.g., see Tax Foundation (2019)). Alternatively, beer, liquor, and cigarettes are almost never exempt from sales taxes.

product categories with the following regression:

$$\ln p_{cste} = \rho^{\mathrm{d}} \Delta \ln(1+\tau_e) \cdot \text{post}_{te} \cdot \text{direct}_{cse} + \rho^{\mathrm{i}} \Delta \ln(1+\tau_e) \cdot \text{post}_{te} \cdot \text{indirect}_{cse} \\ + \mu_{cse} + \theta_{te} + \epsilon_{cste}. \tag{8}$$

$\Delta \ln(1+\tau_e)$ measures the change in the net-of-sales tax rate for event e in log points, while post_{te}, direct_{cse}, indirect_{cse} have the same interpretation as our excise tax analysis, and the inclusion of chain-state-event (μ_{cse}) and time-event (θ_{te}) fixed effects are also the same. The coefficients ρ^{d} and ρ^{i} therefore measure the sales tax pass-through as elasticities. $\rho^{\mathrm{d}} = 1$, for example, would mean complete pass-through.

Results: The first column of Table 8 reports our estimates for the pass-through elasticity for after-tax prices among directly exposed stores, ρ^{d}; and indirectly exposed stores, ρ^{i}. We estimate a pass-through elasticity of 0.97 among directly exposed stores, and an insignificant -0.06 for indirectly exposed stores. These coefficients are statistically significantly different from each other. Thus, sales tax changes are fully passed through among directly exposed stores, with no spillovers to indirectly exposed stores.[21] We present additional results, including an event study analysis, in Appendix F.

5.2 Cigarette Wholesale Prices

Approach: Using data from Rozema (2018), we analyze wholesale cigarette prices for Minnesota, New Jersey, and Rhode Island, covering the period July 31, 2009–January 31, 2014. We focus on the three chains with a presence in at least two of these states for two years or more of our sample. We match wholesale prices and retail prices at the brand level, yielding three brands (averaging over products in some cases). Finally, we conduct the analysis at chain-brand-state-month level (averaging prices across stores and weeks).

We estimate the pass-through rate of wholesale prices to the retail prices of cigarettes with the following regression:

$$\text{Price}_{icst} = \rho \text{Wholesale Price}_{ist} + \beta \text{Excise Tax}_{st} + \mu_{ict} + \varepsilon_{icst}, \tag{9}$$

[21] This "full" pass-through requires no active adjustment on the retailers' part; if they do not adjust the shelf price, the incidence of the tax change will be entirely on demand.

where Price_{icst} is the average retail price of cigarette brand i, in chain c, in state s, in month t; Wholesale Price_{ist} is the reported wholesale price; Excise Tax_{st} is the state-level excise tax; and μ_{ict} is a set of brand-chain-month fixed effects. Because we condition on brand-chain-month fixed effects, ρ measures pass-through of local wholesale prices to local retail prices within a chain. Because we condition on the state excise tax, ρ captures wholesale price variation not driven by variation in excise taxes. Note that we do not include state fixed effects, so ρ is identified by cross-state variation in wholesale prices.

Results: The second column of Table 8 reports our estimate of the pass-through of cigarette wholesale prices to retailer prices, ρ. We estimate a pass-through rate of 0.75 (standard error of 0.15), which is insignificantly different from one. Thus, cross-state differences in wholesale prices translate into cross-state differences in retail prices, even for a given retailer and even when these differences are not driven by excise taxes. Here, we caution that our results only cover a small number of chains and states because of the availability of our wholesale price data, and so may not generalize.

5.3 Regulated Milk Prices

Approach: The federal government regulates the price of a drinking milk sold by farms by setting a national minimum price, which varies monthly, and a county-specific "geographic price differential," which remains fixed over time.[22] To estimate how these milk costs are passed through to retail prices even within chains, we use the five products in the milk category that satisfy our availability criteria over the period 2010-2016, excluding lactose-free products.[23] Because the federally regulated price of milk varies at the monthly frequency, we aggregate to the product-store-month level taking the simple average of prices across weeks.

[22] This price minimum applies to prices at the farm, rather than at the retailer, but should be a factor in determining retailers' input costs. We plot the quartiles of the geographic price differentials in Appendix Figure G.6. The differentials are generally lower in the dairy producing parts of the country (Midwest and Pacific Northwest) and higher in the North- and Southeast.

[23] We start in 2010 because this is the first year we observe the monthly minimum price. We focus on drinking milk and not other dairy products because geographic differential applies only to drinking milk. A difficulty with this product category is that three of the five products are private label, meaning the retailer's generic brand. Nielsen uses a single UPC code for all private label products, so we cannot distinguish between the private brands of different retailers. We address this difficulty by including a set of product-by-chain fixed effects, which is equivalent to separate fixed effects for each private label product.

We estimate the pass-through rate of the regulated (farm) price of milk to the retail price of milk with the following regression:

$$\text{Price}_{ijt} = \rho \text{Regulated Price}_{jt} + \mu_{ict} + \epsilon_{ijt}, \tag{10}$$

where Price_{ijt} is the average price for product i, in store j, and month t; $\text{Regulated Price}_{jt}$ is the regulated price (at the county level) relevant for store j in month t constructed by combining the national minimum monthly price (Agricultural Marketing Service, USDA, 2021) and the geographic differential (Electronic Code of Federal Regulations, 2021); and μ_{ict} is a set of product-chain-month fixed effects. Note that we can only measure how chains respond to local regulated prices, not to their average regulated price (a measure of spillover) because there is no variation in the chain-average regulated price, conditional on a chain-product-month fixed effect.

Results: The third column of Table 8 reports our estimate of the pass-through of the regulated minimum milk price to retail prices, ρ. We estimate a pass-through rate of 1.14 (standard error of 0.19), which is insignificantly different from one. Thus, local milk prices respond to local price regulations administered at the manufacturer (i.e., farm) level, even for a given retailer and product.

5.4 Shipping Costs and Distance to Brewery

Approach: Last, we investigate how costs associated with shipping affect local retail prices. Here, we focus on beer because brewery locations have been previously reported, and prior work has documented that distance to brewery affects retail prices (Ashenfelter et al., 2015; Miller and Weinberg, 2017), although the within-chain association between price and distance has not been established. For this analysis, we use our sample of widely available products in the Beer and Light Beer product categories, over the period of 2006-2007. We focus on these years to avoid the confounding influence of the 2008 merger between SABMiller and Molson-Coors, which had spatially heterogeneous price effects and a change in competition (Miller and Weinberg, 2017). We match each product to its nearest brewery, and, for each product-market, calculate the distance between market centroid and brewery. Because distance-to-brewery varies at the store-product level but not over time, we aggregate to the store-product level, taking the simple average of the weekly prices for each store-product.

We estimate the pass-through of costs associated with shipping distance to the retail price of beer price with the following regression:

$$\text{Price}_{ij} \quad = \quad \rho\{\tau \times \text{DIST}_{ij}\} + \gamma_{ic} + \mu_{cm} + \varepsilon_{ij}, \tag{11}$$

where Price_{ij} is the average price for product i in store j, DIST_{ij} is the distance in thousands of miles to the nearest brewery, the scalar parameter τ scales the measure of miles between store and brewery into dollars per 24-pack (our tax unit), and γ_{ci} and μ_{cm} are a set of chain-product and chain-market fixed effects, respectively. We set $\tau = \$0.62$ per 24-pack per 1,000 miles, based on an estimate of the marginal trucking cost per mile (Williams and Murray, 2020).[24] With chain-by-product fixed effects, we identify the pass-through of distance-related costs by comparing products in a given chain that differ in their distance to the nearest brewery.

Results: The fourth column of Table 8 reports our estimate of the pass-through of distance-related costs to retail prices, ρ. We estimate a pass-through rate of 1.17 (standard error of 0.26), which is insignificantly different from one. Thus, costs associated with shipping distance are passed through locally for a given chain-product. Caution should be exercised in generalizing our pass-through rate, as our primary measure of shipping costs is likely to have at least two forms of measurement error. First, we measure distance using the crow-fly distance instead of the by-road shipping distance. Second, we rely on a rough estimate of trucking costs per mile to scale our measure of shipping costs into dollars per case. Nevertheless, we can reject the hypothesis that for the typical retail chain, shipping costs are unrelated to the retail price, because under that hypothesis we would expect $\rho = 0$, even with measurement error in distance or trucking costs.

[24] Williams and Murray (2020) report trucking costs of $1.548 per mile in 2010, the earliest available. To obtain a cost of $0.62 per 1000 miles/case beer, we assume an 18-wheeler can carry 45,000 pounds of cargo, or 2500 cases of beer each weighing 288 ounces. Federal regulations set gross vehicle weight at 80,000 pounds, and empty trailers weight 35,000 pounds (Federal Highway Administartion, 2015).

5.5 Summary

Several forms of local costs faced by retailers are passed on locally, roughly one-for-one, without apparent spillovers to unexposed stores in exposed chains. These results complement our analysis of excise taxes as they involve local cost shocks that have very different features than excise tax changes which tend to be salient, public, locally uniform, and persistent. For example, sales taxes have been shown not to be salient to consumers, because they are not included in the posted price of the product at the shelf and instead only applied at the register (Chetty et al., 2009). Additionally, the local cost differences that arise from differences in wholesale prices or the costs associated with shipping are unlikely to be public, nor necessarily locally uniform, as distance-to-brewery varies across products in a given market. And, while the cost differences associated with shipping distance and the federally regulated milk prices resemble permanent differences, cost differences arising from wholesale prices are less permanent. More generally we think it is unlikely that our focus on long-lasting shocks explains the pass-through patterns we estimate. Existing literature finds clear evidence that retails are responsive to wholesale prices, and these prices change multiple times per year (Eichenbaum et al., 2011). For instance, Nakamura and Zerom (2010) (Table 2) and Goldberg and Hellerstein (2012) (Table 2) show using linked retail-wholesale price information that that retailers pass-through wholesale price changes immediately and at a one-to-one rate, and that any incomplete pass-through of commodity cost shocks occurs at the wholesale level. Thus, our results using local excise tax changes are unlikely to be the result of the particular features of excise taxes—or that they are taxes, but instead the consequence of the more general pattern that retailers fully and immediately respond to local cost changes, and do so only locally.

6 Discussion

Our results–in conjunction with the existing literature–show a clear asymmetry exists between the price responses to local demand and local cost shocks.[25] This asymmetry

[25] We emphasize that the asymmetry we document is likely to hold only for local shocks; a great deal of evidence shows that prices respond fully to nation-wide demand shocks (e.g., Wollmann (2018); Pakes

has important implications for tax policy, because it indicates that the statutory incidence of taxes may affect their economic incidence–which would affect how effective different tax policies would be in shifting consumer behavior (e.g., “sin” taxes). Beyond these immediate implications, our results are also informative more broadly for the validity of a wide set of pricing models–an issue we briefly discuss in this section. While our results do not point conclusively to a single model, we argue below that they are consistent with some models—managerial inattention, tacit collusion, and fairness constraints—but not others, such as perfect competition or standard imperfect competition.

Models consistent with asymmetry

Managerial Inattention/Attention costs In models of inattention, managers are inattentive to some decision-relevant factors because attention is costly and there is a large number of potentially decision relevant factors. Moreover, managers may put more weight on factors which are observed more precisely (Caplin, 2016; Gabaix, 2019). These models can potentially rationalize the asymmetry that we have documented, because wholesale costs are likely much more salient than store-specific demand elasticities. Indeed, we suspect that retailers can measure their costs quite well. On the other hand, demand shifters are likely quite difficult to measure. For instance, Hitsch et al. (2017) show that store-product level elasticity differences would be statistically difficult to detect, at least within a given market, even for a sophisticated retailer.

Tacit Collusion Another possible explanation is that firms are engaged in a form of tacit collusion. Tacit collusion is especially plausible for retailers who compete across many geographic markets. This repeated multi-market contact makes it easier to collude on prices (Bernheim and Whinston, 1990). Firms that are tacit colluding on prices might fail to respond to local demand shocks, while fully responding to cost shocks, especially those that are common across firms (e.g., excise taxes). The adoption of uniform pricing might itself facilitate tacit collusion. Consistent with this idea, Adams and Williams (2019) show in the case of drywall sold in home improvement stores that uniform pricing softens price competition.

(2017)).

Fairness Considerations/Brand Reputation Finally, the asymmetry could also be explained by fairness and brand reputation considerations. In an influential paper, Kahneman et al. (1986) report survey evidence that consumers viewed price increases as unfair if they stemmed from demand increases, but not if they stemmed from cost increases. Surveys of retailer managers often cite fairness and the impact of unfair pricing on their brand's reputation as central to their pricing decisions (Blinder et al., 1998; Fabiani et al., 2006; DellaVigna and Gentzkow, 2019)).

Models inconsistent with asymmetry

The asymmetric price response to local demand and cost shocks rules out some common models of pricing. Nash-Bertrand pricing cannot account for the fact that prices do not vary with store-level elasticities, and literal uniform pricing cannot account for the local cost pass-through absent any attenuation or spillovers.[26] While our results are consistent with perfect competition, perfect competition is inconsistent with the substantial evidence that retailers exhibit some degree of market power.[27]

7 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.

[26] Models of the vertical contracts between manufacturers and retailers (e.g., Berto Villas-Boas (2007)) also typically treat the factors of local demand and cost symmetrically in determining prices.

[27] Across a large set of studies, estimated own-price elasticities of demand vary from roughly -5 to -2, implying markups on the order of 25-100 percent, far too large for perfect competition (e.g., see DellaVigna and Gentzkow (2019); Hitsch et al. (2017) on a wide set of products; Asker (2016); Miller and Weinberg (2017) for beer; Conlon and Rao (2015); Miravete et al. (2018) for liquor; Hendel and Nevo (2013); Allcott et al. (2019) for soda; and Gallet and List (2003) for cigarettes).

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 have argued that while several explanations can reconcile these findings, we view explanations that center on managerial inattention as likely to be more successful.

Regardless of the appropriate behavioral model, the asymmetry between retailers' price response to demand and cost shocks 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 if managers are less responsive to these changes prices might 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 forms of managerial inattention.

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