How do national chains respond to local cost shocks?
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Online Appendix

Appendices

A Theoretical Framework Derivations and Further Discussion

A.1 Pass-through Derivations

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

Given the flexible pricing monopolist's objective function:

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

where the last equality follows from multiplying the numerator and denominator by $(1/X)$, where $X \equiv \sum_m x_m(p)$.

This completes the first characterization of the pass-through under uniform pricing which justifies the first prediction that we investigated in the primary empirical analysis of the paper.

The second prediction was made under a more restricted set of demand conditions the monopolist faces across markets. For clarity, we restate that assumption.

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

This assumption encapsulates many forms of demand used in applied work, and is consistent with retailers facing different price elasticities of demand across the markets they face. Under this assumption, denoting the constant convexity of demand to be ζ across each market, and noting that the first order condition for the optimality of the uniform price guarantees that $\sum_m \left[\frac{\overline{p}-c_m}{\overline{p}}\right] s_m\varepsilon_m = 1$, the pass-through rate for the uniform pricing monopolist takes the form:

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

where $\overline{\varepsilon}$ is simply the quantity weighted average of the price elasticities of demand in each market.

To show that the (quantity) weighted average pass-through rate of the uniform pricing monopolist will be less than the perfectly flexible benchmark, it is enough to show that

$$
\begin{aligned}
\sum_j s_j \overline{\rho}_j &= \sum_j s_j \left[\frac{s_j \varepsilon_j}{2\overline{\varepsilon} - \zeta} \right] \\
&\leq \sum_j \frac{s_j \varepsilon_j}{2\overline{\varepsilon} - \zeta} = \frac{\overline{\varepsilon}}{2\overline{\varepsilon} - \zeta} \\
&\equiv g(\mathbb{E}_s[\epsilon_j]) \leq \mathbb{E}_s[g(\epsilon_j)] \\
&= \sum_j s_j \left[\frac{1}{2 - \zeta/\varepsilon_j} \right] = \sum_j s_j \rho_j
\end{aligned}
$$

where the function $g(\cdot)$ which takes as its argument ε is given by $g(\cdot) = \frac{1}{2-\zeta/\varepsilon}$, the expectation operator ($\mathbb{E}$) is taken over the quantity shares s, and where the first inequality holds by $s_j \in [0, 1]$, and the second inequality holds by the Jensen's inequality and $g(\cdot)$ being convex.

A.2 Alternative Market Structure Discussion

In footnote 6 of Section 2, we note that the choice of modeling the pricing decision of a monopolist as opposed to alternative market structures does not affect the qualitative aspects of the pass-through predictions. This subsection describes the required modifications to the model to accommodate other forms of competition.

To allow for a wide variety of different behavioral models of competitive behavior and for this to potentially vary across markets, we take the approach of Bresnahan (1989) and Weyl and Fabinger (2013) and assume that firms equate marginal costs with "perceived" marginal revenue which is parameterized for each market by, θ_m, which controls the degree of competitiveness in market m.[30] In the case that a firm has no market power (i.e., exhibits price-taking behavior, or perfect competition), then $\theta_m = 0$. In the case that a firm is a monopolist, then $\theta_m = 1$; where for symmetric Cournot quantity competition θ_m takes the intermediate value of the reciprocal of the number of firms. Moreover, Weyl and Fabinger (2013) show that this formulation nest several forms of imperfect competition.

Flexible Pricing Benchmark

Under this extended version, the flexible pricing benchmark model's optimal price can be shown to be as follows:

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

[30]While we allow for the strength of competition to vary arbitrarily across markets, we assume that θ_m is otherwise constant in that it does not vary with *changes* in prices or costs in any particular market.

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

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

It is easy to see from these formulations, that under no market power (i.e., $\theta = 0$), prices equal marginal cost, and pass-through is one. And, that substituting $\theta = 1$ for the monopoly case yields the flexible pricing equations reported in the main paper. More importantly, these equations also make clear that the critical elements of the flexible pricing benchmark are unchanged under a more agnostic stance of the degree of competition and how that competition varies across markets.

Uniform Pricing

Alternatively, under this extended formulation the uniform pricing 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:

$$\overline{\rho}_n = \frac{s_n \varepsilon_n}{\overline{\varepsilon} + \sum_m s_m \theta_m \varepsilon_m - \sum_m \left[\frac{\overline{p} - c_m}{\overline{p}}\right] s_m \varepsilon_m \zeta_m}$$

where $\overline{\varepsilon} = \sum_m s_m \varepsilon_m$ as in the main paper. And, under Assumption 1, the pass-through rate reduces to the following:

$$\overline{\rho}_n = \frac{s_n \varepsilon_n}{\overline{\varepsilon} + \sum_m s_m \theta_m \varepsilon_m - \overline{\theta}\zeta}$$

where $\overline{\theta} = \sum_m s_m \theta_m$. Define the (quantity share weighted) covariance between the competitiveness and the price elasticity of demand across markets $\text{Cov}\left[\theta_m, \varepsilon_m\right] \equiv \sigma_{\theta\varepsilon}$. Then, the pass-through rate can be further simplified to the following:

$$\overline{\rho}_n = \frac{s_n \varepsilon_n}{\overline{\varepsilon} + \sigma_{\theta\varepsilon} + \overline{\varepsilon}\overline{\theta} - \overline{\theta}\zeta}.$$

These equations make clear that the key comparative statics between the uniform pricing and flexible pricing benchmark are unchanged. Specifically, under uniform pricing local cost shocks will spill over to otherwise unaffected markets, and that local price responses to local cost shocks will be attenuated, where the extent of the attenuation will be decreasing in the exposure of the retailer to the cost shock–holding the price elasticity of demand *and* the competitiveness of the local market constant.

B Documenting Uniform Pricing among Our Products

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

B.1 Measuring similarity

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

In our baseline approach we sample all stores uniformly. However, one reason prices may be more correlated within chains than across chains is that stores within a chain tend to be geographically clustered. If pricing determinants such as costs, demand, or competition are also geographically clustered, then we will see more price similarity within chain than across chain, even under fully flexible pricing. To address this possibility we therefore consider a second approach which always samples pairs of stores in the same state. This approach holds fixed state-level factors (including, importantly, excise taxes) within the pair.

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

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

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

B.2 Documenting similarity in prices

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

Looking only at pairs of stores in the same state yields higher similarity overall, but does not change the conclusion that prices are highly similar within chain but differ substantially across chains. We report measures of pricing similarity for within-state pairs in Appendix Table B.2, and their distribution in the righthand side of Appendix Figures B.1, B.2 and B.3. Looking within state especially increases within-chain pricing similarity for beer, liquor, and cigarettes—and not soda. As soda is not commonly subject to excise taxes, this result is consistent with the fact that excise taxes are a source of non-uniform pricing.

B.3 The price income correlation

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

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

We plot the within-chain price-income correlation in panels A, C, and E of Appendix Figure B.4, and the between-chain correlation in panels B, C, and D. The within-chain correlation plots store prices relative to chain average price against store income relative to the chain average income.[31] For all of our products we observe a weak within-chain

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

relationship between prices and income; it is even slightly negative for beer and liquor. Looking across chains, however, we observe a much stronger relationship between price and income.

parture from the DellaVigna-Gentzkow procedure.

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

Measure	Absolute difference in quarterly log prices		Correlation in (demeaned weekly) log prices		Share of weekly log prices within one log point	
	Same chain	Different chain	Same chain	Different chain	Same chain	Different chain
	(1)	(2)	(3)	(4)	(5)	(6)
			Panel A: Beer			
Mean	.033	.102	.603	.086	.527	.106
SD	.041	.061	.325	.196	.333	.11
# Chain-products	1740	1830	1721	1825	1739	1830
			Panel B: Liquor			
Mean	.041	.141	.643	.046	.536	.069
SD	.061	.082	.333	.145	.328	.078
# Chain-products	448	519	444	519	448	519
			Panel C: Cigarettes			
Mean	.068	.194	.704	.311	.514	.027
SD	.094	.094	.232	.213	.42	.068
# Chain-products	672	684	672	684	672	684
			Panel D: Soda			
Mean	.039	.119	.69	.058	.465	.073
SD	.05	.075	.28	.159	.323	.107
# Chain-products	3137	3289	3132	3285	3137	3289

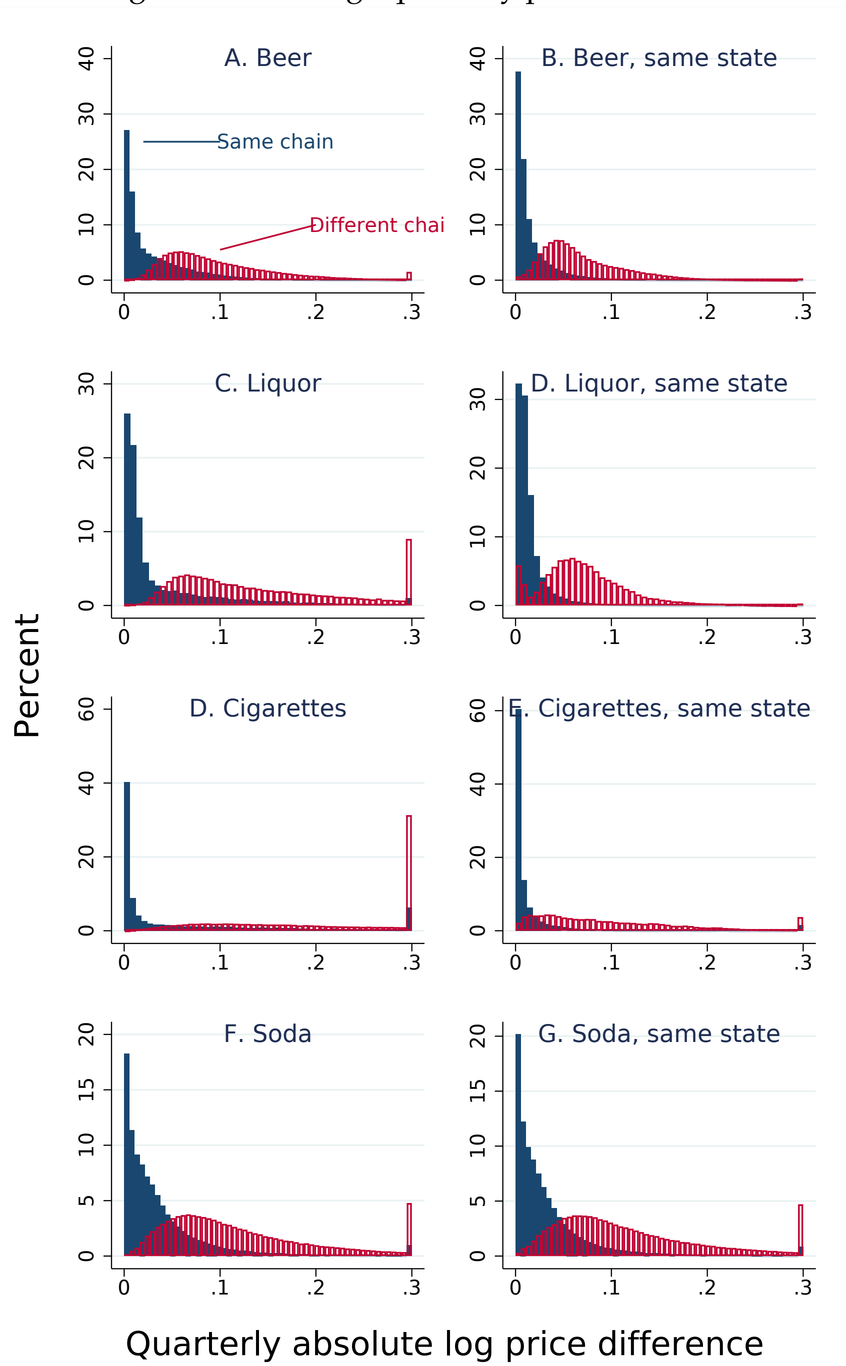
Note: This table reports the mean and standard deviation of the measures of pricing similarity for pairs of stores within the same chain or in different chains. The sample consists of up to 200 pairs of stores per chain.

Table B.2: Measures of pricing similarity, by category, within-state pairings

Measure	Absolute difference in quarterly log prices		Correlation in (demeaned weekly) log prices		Share of weekly log prices within one log point	
	Same chain	Different chain	Same chain	Different chain	Same chain	Different chain
	(1)	(2)	(3)	(4)	(5)	(6)
			Panel A: Beer			
Mean	.017	.074	.73	.171	.665	.166
SD	.024	.049	.239	.234	.278	.164
# Chain-products	1714	1826	1700	1814	1712	1826
			Panel B: Liquor			
Mean	.015	.073	.798	.184	.674	.191
SD	.018	.048	.165	.28	.226	.241
# Chain-products	442	519	439	519	442	519
			Panel C: Cigarettes			
Mean	.018	.1	.783	.377	.76	.137
SD	.046	.077	.185	.232	.289	.208
# Chain-products	671	685	671	685	671	685
			Panel D: Soda			
Mean	.035	.118	.718	.069	.486	.078
SD	.047	.075	.261	.176	.321	.117
# Chain-products	3134	3285	3130	3281	3134	3285

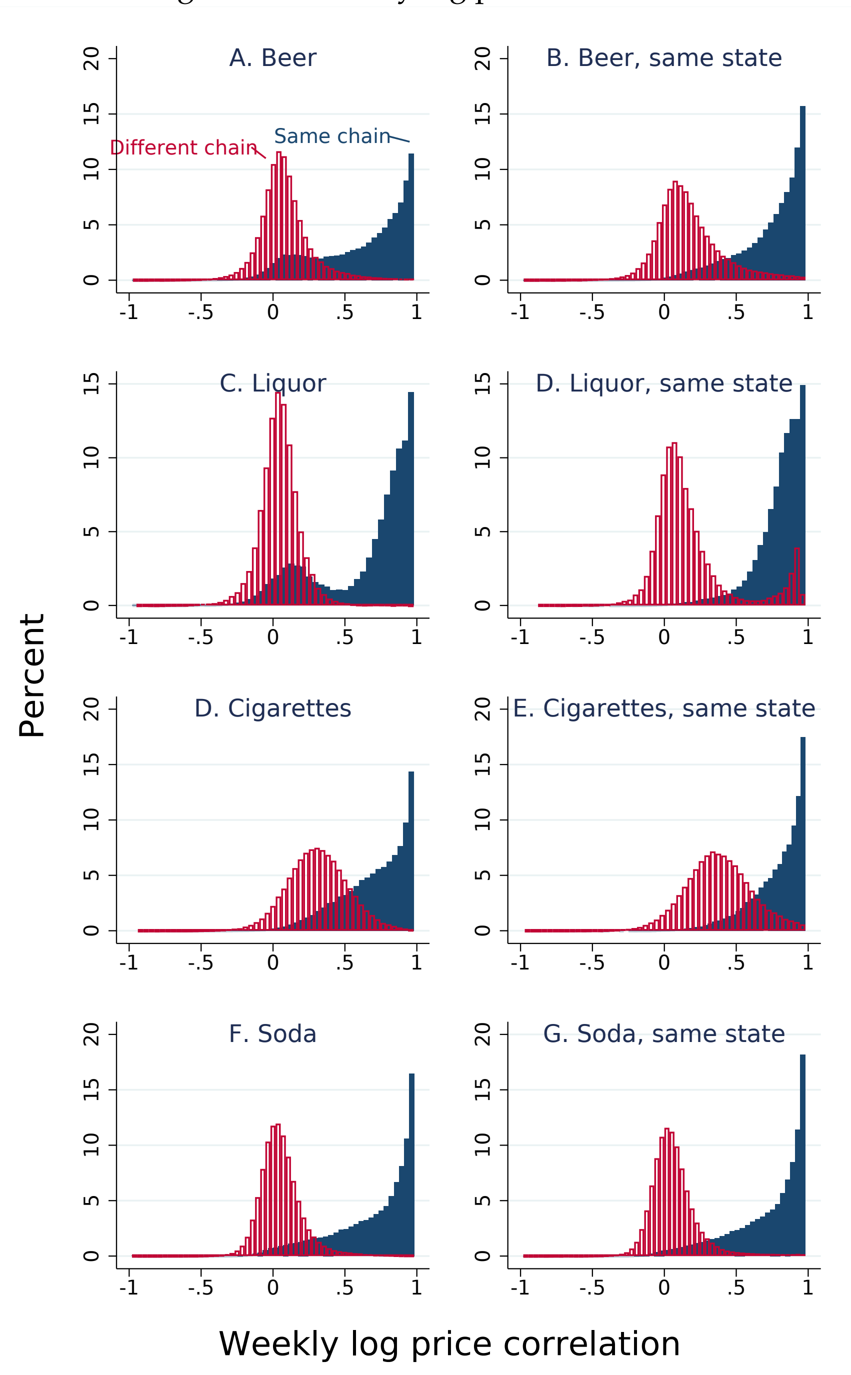
Note: This table reports the mean and standard deviation of the measures of pricing similarity for pairs of stores within the same state and either within the same chain or in different chains. The sample consists of up to 200 pairs of stores per chain.

Figure B.1: Average quarterly price difference



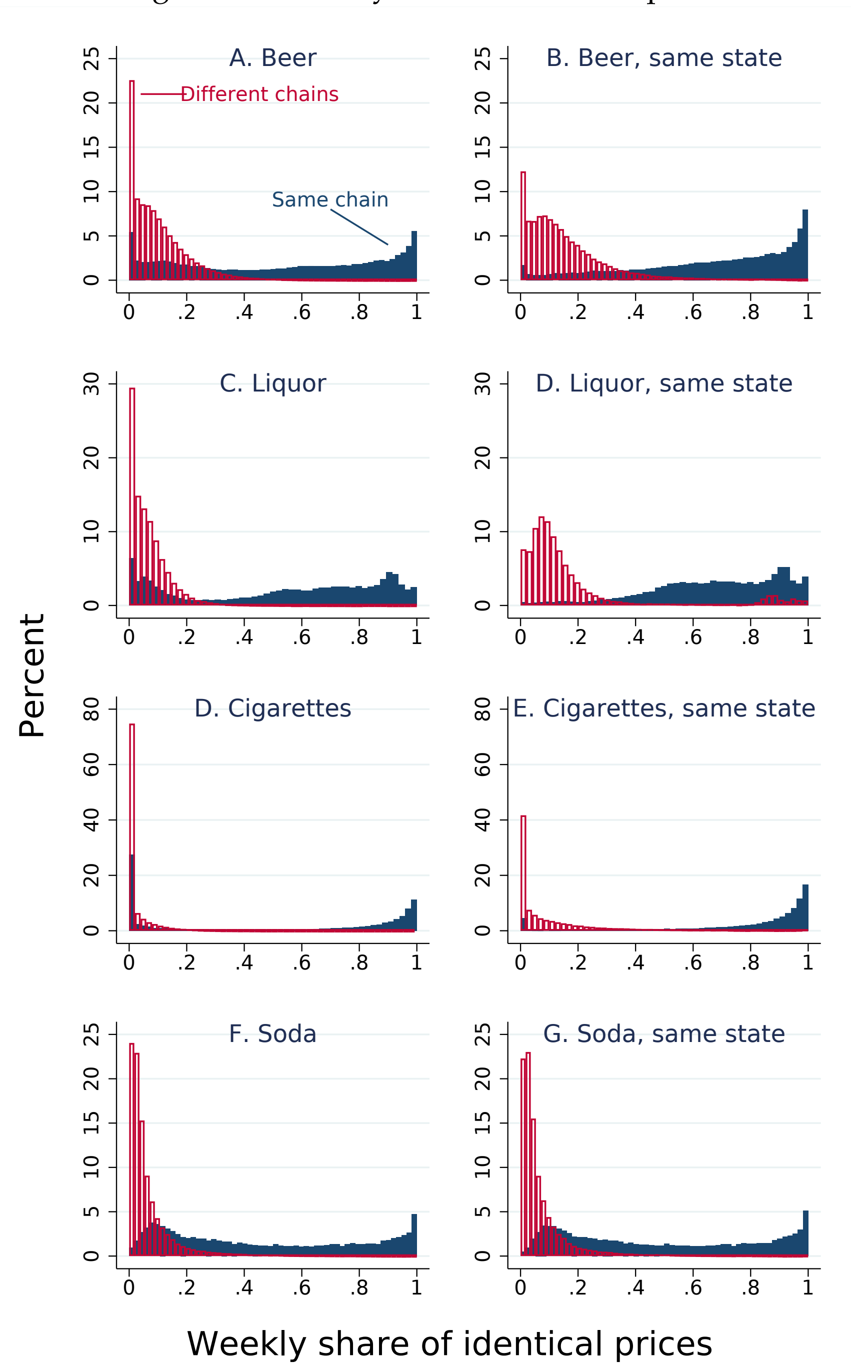
Note: This figure plots the distribution of the quarterly difference in log prices between pairs of stores in the same chain (in solid bars) or different chains (in red bars), for 200 pairs of stores per chain and for all available products in each category.

Figure B.2: Weekly log price correlation



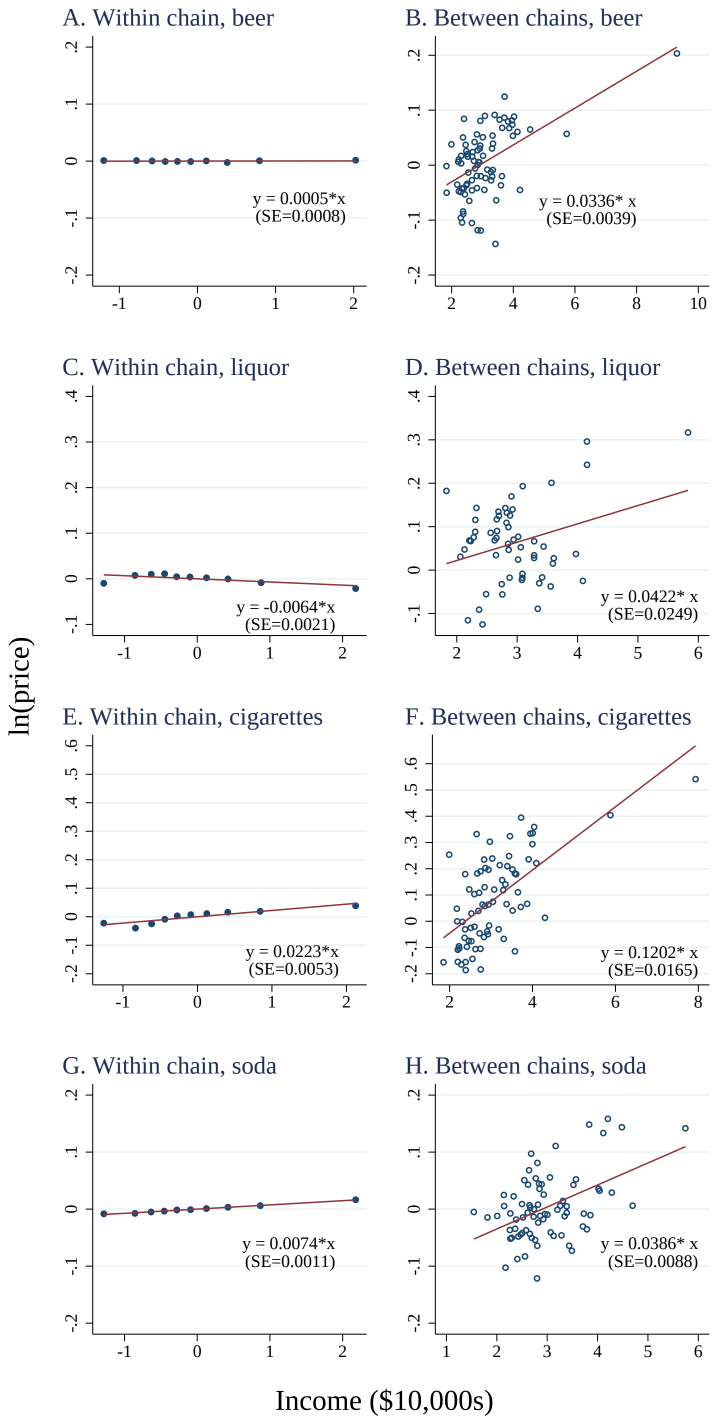
Note: This figure plots the distribution of the weekly log price correlation in log prices between pairs of stores in the same chain (in solid bars) or different chains (in red bars), for 200 pairs of stores per chain and for all available products in each category.

Figure B.3: Weekly share of identical prices



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

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



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

Table C.1: Revenue and exposure, by terciles of revenue

Revenue tercile	Observations	Revenue Range	% Local	% National
1	157	0.0-0.3	25	46
2	163	0.3-4.0	8	59
3	158	4.0-40.7	0	90

Note: The unit of observation is a chain-event. This table sorts observations by revenue in the event's category in the year prior to the event and reports the indicated statistics in each tercile. Revenue is in millions of dollars per week. Local chains have at least 90 percent of their pre-event revenue from the event states, and national chains have at most 10 percent.

C Additional Evidence on Heterogeneous Responses by Exposure

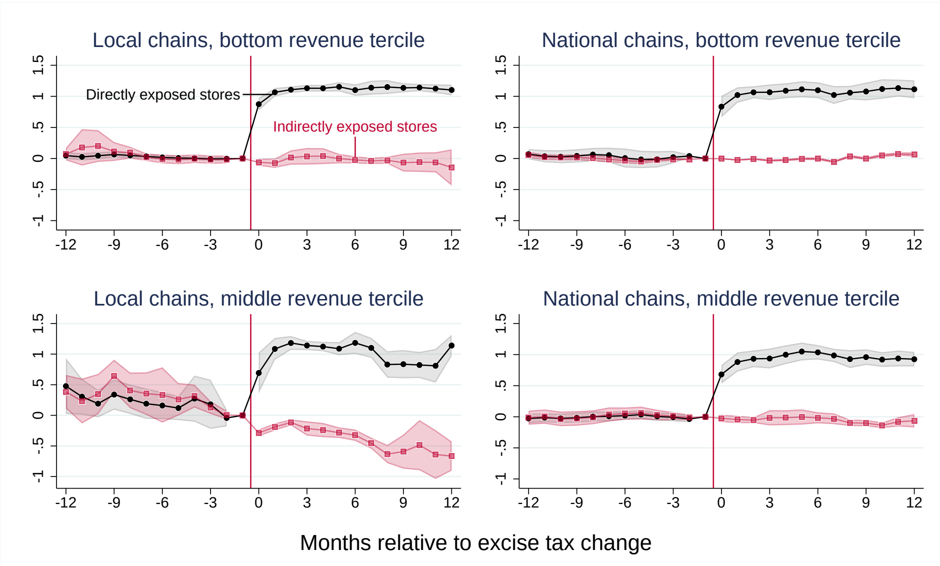
In the main analysis we investigate heterogeneous pass-through rates by chains' exposure to a given tax event. We consider two exposure measures: the chain's share of revenue in the event state in the year prior to the tax, and an indicator for "local" chains, with at least 90 percent of their revenue in the event state. A concern with these measures is that exposure is correlated with chain size: larger chains are typically less exposed. We address that concern in the main analysis by conditioning on size (as proxied by pre-period revenue). Specifically we include interactions between size, direct or indirectly exposed, and the tax change, allowing for heterogeneous responses by both size and exposure. A limitation of that approach is that it assumes linearity in size.

Here we consider an alternative approach that relaxes the linearity assumption by fully conditioning on a coarsened measure of size. To begin, we bin the 478 chain-events into terciles of pre-period revenue. Appendix Table C.1 reports the range of pre-period revenue in each tercile, as well as the share of chain-events in each tercile that are "local" or "national" (at most 10 percent of their pre-period revenue in the event state). The table shows that chain revenue is both dispersed and skewed. Even within tercile there is a wide range in size, and the largest chain-event is 10 times larger than the 67th percentile. The table also confirms that the size is closely related to exposure: the largest chains are very unlikely to be local and very likely to be national. However, the table also shows that within the bottom two terciles there is a good deal of variation in exposure, with at least 8 percent of chains in each tercile local and at least 46 percent national.

We fully condition on size in two ways. First, we re-estimate our stacked event study analysis, Equation 4, but separately by both tercile of chain size and for local and national chains, among the bottom two terciles of size.[32] These specifications let us estimate separate pass-through rates for chains of a given size and exposure. We plot the results in

[32] We do not estimate pass-through for in-state, indirectly exposed stores, because there are no chains that are local to the county-level tax changes.

Figure C.1: Event studies by size and exposure



Note: This 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. Each panel is limited to the indicated set of chains. "Local" chains have at least 90% of their revenue from the event state in the year prior to the event, and "national" chains have at most 10% of their pre-event revenue from the event state.

Figure C.1. We see pass-through rates among directly-exposed stores of near 100 percent in both terciles, for local and national chains.

Second, we re-estimate the stacked difference-in-difference models allowing for heterogeneous pass-through rates for local chains, Equation 5, but limiting the sample to local or national chains (i.e., excluding the intermediate category) in a given tercile of size. We report the results in Table C.2. We find pass-through rates of 106 percent for directly exposed stores among chains in the bottom tercile of revenue, and 91 percent among the middle tercile. Because the sample is limited to national and local chains, this is the pass-through rate for national chains. We estimate a insignificant difference in pass-through rates for local chains in both cases. We also find, for both terciles, that the pass-through rate is small for indirectly exposed stores. We find a negative differential pass-through rate for indirectly exposed stores in local chains (relative to national chains); the difference is significant for the middle tercile (consistent with the evidence in Figure C.1 and Table 6.

Overall we find that local and national chains pass-through a local cost increase at approximately the same rate among directly exposed stores. We find no evidence for positive spillovers to indirectly exposed stores. In some cases, however, indirectly exposed stores appear to reduce their prices following the excise tax increase. Thus our main conclusion—that more exposed chains do not respond more to the tax increase, nor do they show greater spillovers—is robust to alternative ways of handling heterogeneous

Table C.2: Pass-through for national and local chains, by size

Size	Bottom tercile	Middle tercile
	(0.040)	(0.045)
Differential pass-through direct, local chain	0.013	-0.189
	(0.046)	(0.118)
Pass-through indirect	0.010	-0.043
	(0.016)	(0.024)
Differential pass-through indirect, local chain	-0.108	-0.605
	(0.068)	(0.068)
# Observations	1,193,197	1,179,067
# Chains	95	96

Notes: This table reports the pass-through rate for directly and indirectly exposed chains, as well as the differential pass-through among local chains. The sample is limited to national and local chains, belonging to the indicated tercile of size. We do not look at the top tercile because it contains no local chains.

firm size.

D Synthetic Control Analysis of the Federal Tax Change

To analyze the federal excise tax increase, we construct a synthetic control group for cigarettes, using the procedure of Abadie et al. (2010). As described in the text, to prepare the data, we first rescale prices in each category so that their 2008 average equals the average cigarette price. This rescaling adjusts for the fact that some products are more expensive per unit (e.g., laundry detergent). Next we residualize the store-product (scaled) weekly price, taking out a store-product mean. Finally we construct a category-specfic time series equal to the average (across stores and products) of the weekly residualized price. In this section we let p_{ct} refer to the category-week average residualized price, and we let category 0 refer to cigarettes, so p_{0t} is weekly cigarette prices.

We are interested in the pass-through rate of the federal excise tax increase. To motivate our synthetic control approach, we introduce a potential outcomes framework. Let $p_{0t}(0)$ be the period t price that would be charged in the absence of the tax increase, and $p_{0t}(1)$ the price with the tax increase. Let $d\tau$ be the amount of the tax increase and T the period of the increase. The period t effect is $p_{0t}(1) - p_{0t}(0)$, and the period t pass-through rate is $(p_{0t}(1) - p_{0t}(0))/d\tau$. We do not observe both potential outcomes, of course. For $t < T$ we observe only $p_{0t} = p_{0t}(0)$ and for $t \geq T$ we observe $p_{0t} = p_{0t}(1)$.

We use the synthetic control method to impute the missing potential outcome, following Abadie et al. (2010). Our candidate synthetic controls are the average residualized prices of the other grocery categories. The synthetic control approach chooses a set of weights for the other price series so that they match pre-treatment values (and, in general, pre-treatment predictors) as closely as possible. The synthetic control is then defined as the weighted average of the controls, $\hat{p}_{0t}(0) = \sum_c w_c p_{ct}$, and the synthetic control treatment effect is $p_{0t} - \hat{p}_{0t}(0)$. We scale this treatmetn effect by $d\tau$ (i.e. \$0.62) to obtain a pass-through rate.

We choose the weights to match the average monthly prices in each of the first six months of our data (January, 2008 through June, 2008). We refer to these months as the matching period. We chose this matching period for two reasons. First our goal with the synthetic control is to adjust for confounding due to the price changes from the Great Recession. The matching period is the first six months of the Great Recession (according to NBER dating), so by matching prices in these months we select products whose prices trended similarly during the recession. Second, we retain a long period between the end of the matching period and the announcement of the tax increase, so we can assess the quality of the match in a hold-out period.

We plot the estimated pass-through rate in Figure D.1 as the solid blue line. (We plot the level of the synthetic control series in the main text, Figure 5.) During the matching period the estimates are almost exactly zero, unsurprisingly.[33] In the next several months, before policy announcement, we continue to estimate a pass-through rate of zero, which lends some validity to the synthetic control approach. A few weeks after the law's passage, the estimated pass-through rate rises sharply to about 150 percent. It remains elevated, rising further to almost 200 percent at times.

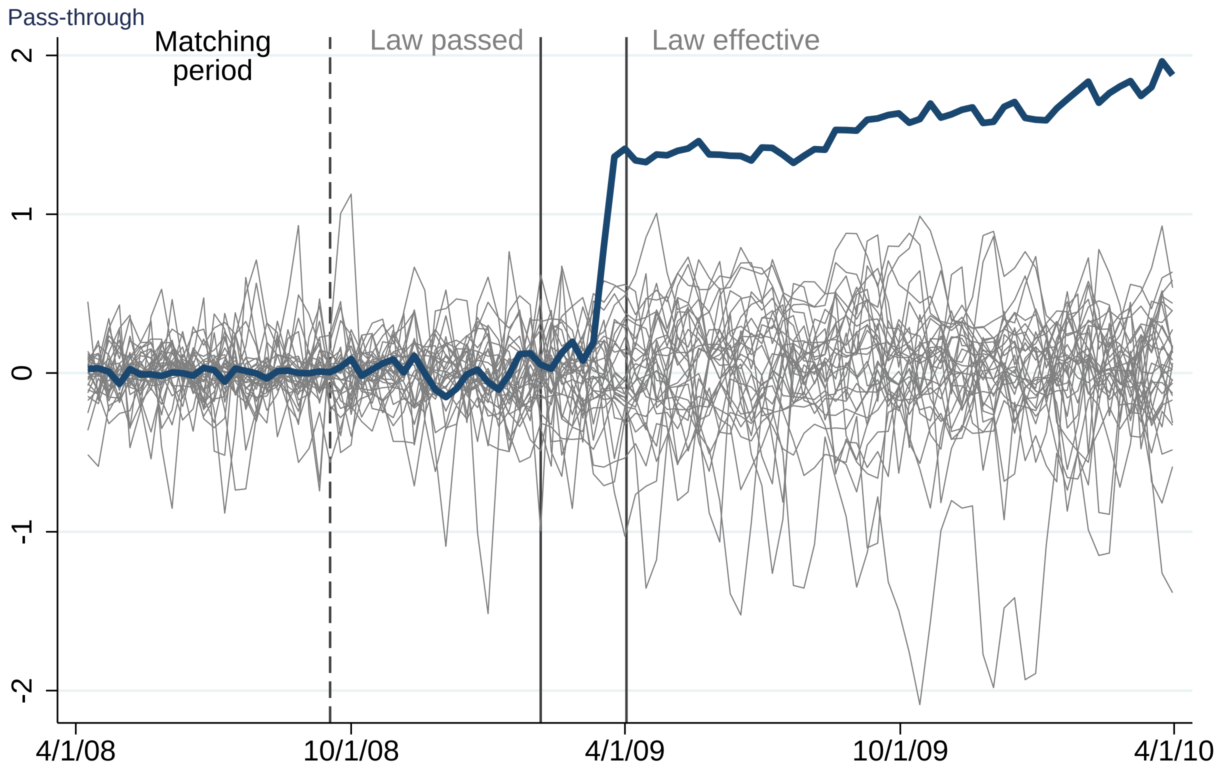
To assess the statistical significance of our findings, we conduct standard randomiza-

[33] They are not exactly zero because we match monthly average prices, not each week's price.

tion inference. Specifically, we reproduce our synthetic control analysis in placebo tests treating each of the 30 non-cigarette categories as if they were subject to the tax. For each category we find a synthetic control from among the other non-cigarette categories, and we plot as a thin gray line that category's (placebo) treatment effect and pass-through rate. During the matching period and pre-period we see that the treatment effect for cigarettes lies in the middle of the distribution of placebo estimates. After the law's effective date, however, the pass-through rate for cigarettes is greater than for every placebo category (except for a small number of weeks where there is one category with a higher pass-through rate). Thus random inference yields a p-value for the pass-through rate of 1/30 or less for all weeks.

Taking a synthetic control approach, we estimate a pass-through rate of 150-200 percent. This rate is quite similar to the pass-through rate of 175 percent we obtain when we use other groceries as a control group and do not control for the tobacco PPI. This similarly implies that our estimated pass-through rate is not terribly sensitive to the choice of control group and, more generally, that trends in grocery prices from the Great Recession likely do not substantially bias our estimates.

Figure D.1: Synthetic control estimates of pass-through rates, cigarettes, and placebo categories



Note: The thick line in the figure is the synthetic control estimate of the pass-through rate of the federal tax, equal to the difference between actual cigarette prices and synthetic prices, scaled by the tax change (0.62). The synthetic control group is selected to match each monthly price in the indicated matching period. The thin gray lines show placebo estimates. To construct these estimates, we treat each category as the treated one, and estimate a synthetic control group, excluding cigarettes.

E Cigarette Wholesale Prices

In Section 5, we estimated the pass-through of the federal excise tax change on cigarettes associated with the CHIP Reauthorization Act of 2009. This law raised the federal excise tax from $0.39 to $1.10 per pack, and became effective on April 2, 2009. A primary challenge with estimating the pass-through rate for this tax change event is the lack of a compelling control group of stores/chains to best control for the counterfactual path of retail prices absent the federal tax change. This challenge relates to how much of the variation in retail prices leading up to the enactment of the excise tax change to control for (see Figure 5).

In the main analysis, we use the producer price index (PPI) for producers of cigarettes as a control. The PPI measures how the selling prices of different producers, in our case– producers of cigarettes, changes over time. Thus, we view this price series as being related to prices faced by wholesalers and distributors of cigarettes. Given its monthly frequency and cross-sectional aggregation it might attenuate the movements of these prices preceding and following the federal excise tax change. For that reason, we show that using daily state-level wholesale prices at the brand level show similar price dynamics.

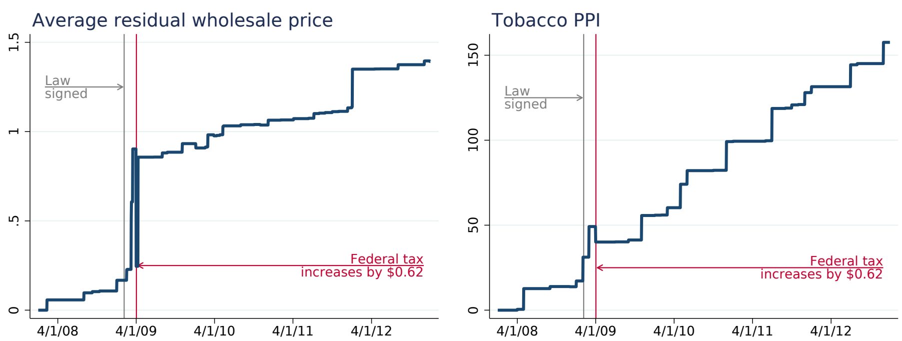
To do this, we use the data collected and used by Rozema (2018).[34] These data represent the manufacturer list prices reported to states' regulatory agencies (mandated by law), to aid in the construction of retail price minimums. These prices are reported for each day, and are brand specific. While the original data set of Rozema (2018) encompasses six states, we use the wholesale prices reported by the states of Minnesota and New Jersey as these two states are in the Nielsen data and have clean data available around the 2009 federal tax increase. Figure E.1 plots the time series of the wholesale prices for the five years around the enactment of the federal excise tax change in 2009. More specifically, we plot the average residual wholesale price (solid blue line) together with the PPI (dashed black line; normalized to have an identical value as the wholesale prices on 1/1/2008). The residual wholesale price is the brand-state-day wholesale price net of a brand-state fixed effect.

This figure shows three points relevant to our main analysis. First, the PPI largely tracks the movements of the average wholesale price of cigarettes. Second, the small increase in the PPI that occurred after the passage of the law (February 4th, 2009), but before the enactment of the law (April 2nd, 2009) also appears in the wholesale price series. Third, this small PPI increase is comparable to the small increases that occurred frequently throughout the five years displayed here.[35] In summary, we view these patterns as supportive of the approach and interpretation of our primary analysis surrounding the federal excise tax change for cigarettes that was aided by the use of the PPI as a control.

[34]We are grateful to Kyle Rozema for sharing his data with us. For more details on this data including broad summary statistics, see Rozema (2018) (e.g., Table 1).

[35]Among the four states we have wholesale prices for, only New Jersey had its own change in its state excise tax for cigarettes–which occurred on July 1, 2009.

Figure E.1: Residual cigarette wholesale prices around the CHIP Reauthorization Act of 2009



Note: The left panel plots the daily average residual cigarette wholesale price for the states of Minnesota and Rhode Island, over the five-year period of 2008-2012, including the period surrounding the federal excise tax change associated with CHIP Reauthorization Act of 2009 which raised the federal cigarette excise tax by $0.62 per pack. The residual wholesale price is the average brand-state-day price net of a brand-state fixed effect. The right panel plots the tobacco PPI (reported at monthly frequencies). Both are normalized to zero for January 1, 2008.

F Sales Taxes

F.1 The sales tax events

For our sales tax analysis, we gather all state-level sales tax changes reported in the annual summaries of the Tax Foundation between 2006 and 2018.[36] We then collect information on the tax rates prior to and after the tax changes, the effective date and the announcement date, and the scope of the law (whether it covers tobacco, liquor and/or cigarettes). We use a collection of state government websites, state legislature archives, and news media reports. We also confirmed that our set of state-level sales tax changes match the sales tax changes described by Baker et al. (2020) during the 2006-2015 period, when our series overlap with theirs.

We define a sales tax event as a sales tax change affecting a given category (beer, liquor, or cigarettes). Our 40 sales tax changes encompass 104 events; not every category-by-change is represented because some states exempt some categories from sales tax, and because some events are category-specific. We then limit the set of sales tax events studied, consistent with our analysis of excise taxes. Specifically we exclude events: in Hawaii and Alaska, for which Nielsen lacks data; for liquor in alcohol control states; with simultaneous changes in excise taxes; and events with less than 6 months of pre- or post-period data. We report in Appendix Table F.1 the final set of sales tax changes and the categorise to which they apply. For our main estimates, reported in the Table 8 text, we further drop the 8 cigarette sales tax that occurred within one year of the federal cigarette tax increase on April 2, 2009. We explain below the reasoning behind dropping these events, and we show that including them does not alter our conclusion of complete pass-through of sales tax changes among directly exposed stores.

F.2 Additional empirical analyses

Event study: In Table 8 we report the pass-through elasticity of sales tax events among directly and indirectly exposed stores. Like our main pass-through estimates for excise taxes, these elasticities are identified under the assumption that, absent the sales tax change, there would be no differential change in prices among directly or indirectly exposed stores, relative to unexposed stores. We assess that identifying assumption by estimating event study models, analogous to Equation 4, except the dependent variable is the residualized log of the after-sales-tax price, and we do not examine pass-through of in-state, indirect exposed stores, because there are no such stores.

We plot the results in Figure F.1. Upon enactment of a 1 percent increase in sales tax, prices among directly exposed stores rise immediately by a bit less than 1 percent, thus pass-through is near-complete among directly-exposed stores. The figure shows no anticipatory behavior, just a sharp jump in prices among directly exposed stores at the time of the tax increase.

The federal change: Our main analysis excludes cigarette sales tax events overlapping with the federal change. In Table F.2 we report our main result with this exclusion, as well

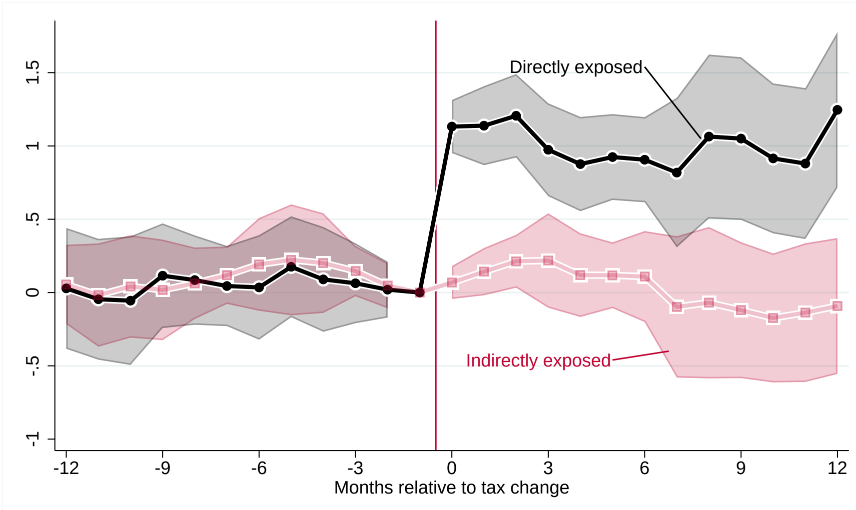
[36]See, e.g., Tax Foundation (2016). We thank Jannelle Cammenga for providing us historical sales tax rates compiled by the Tax Foundation.

Table F.1: List of state sales tax changes

State	Effective date	Change	Prior level	Categories affected
NJ	2006-07-08	.01	.06	Beer, Liquor
ID	2006-10-01	.01	.05	Beer, Cigarettes
NC	2006-12-01	-.0025	.045	Beer
SC	2007-06-01	.01	.05	Beer, Liquor, Cigarettes
MD	2008-01-01	.01	.05	Beer, Liquor
UT	2008-01-01	-.001	.0475	Beer, Cigarettes
IN	2008-04-01	.01	.06	Beer, Liquor
IA	2008-07-01	.01	.05	Beer, Liquor, Cigarettes
UT	2009-01-01	.0005	.0465	Beer, Cigarettes
CA	2009-04-01	.01	.0625	Beer, Liquor, Cigarettes
MN	2009-07-01	.00375	.065	Beer, Liquor
NV	2009-07-01	.0035	.065	Beer, Liquor, Cigarettes
MA	2009-08-01	.0125	.05	Beer, Liquor, Cigarettes
AZ	2010-06-01	.01	.056	Beer, Liquor, Cigarettes
KS	2010-07-01	.01	.053	Cigarettes
NM	2010-07-01	.00125	.05	Beer, Liquor
MA	2011-01-01	-.0625	.0625	Liquor
CA	2011-07-01	-.01	.0725	Beer, Liquor, Cigarettes
MD	2011-07-01	.03	.06	Beer, Liquor
NC	2011-07-01	-.01	.0575	Beer, Cigarettes
CA	2013-01-01	.0025	.0625	Beer, Liquor, Cigarettes
AZ	2013-06-01	-.01	.066	Beer, Liquor, Cigarettes
AR	2013-07-01	.005	.06	Beer, Liquor, Cigarettes
KS	2013-07-01	-.0015	.063	Cigarettes
OH	2013-07-01	.0025	.055	Beer, Liquor, Cigarettes
VA	2013-07-01	.003	.05	Beer, Cigarettes
ME	2013-10-01	.005	.05	Beer, Liquor, Cigarettes
SD	2016-07-01	.005	.04	Beer, Liquor, Cigarettes
CA	2017-01-01	-.005	.065	Beer, Liquor
NJ	2017-01-01	-.00125	.07	Beer, Liquor, Cigarettes
NJ	2018-01-01	-.00625	.06875	Beer, Liquor, Cigarettes
LA	2018-07-01	-.0055	.05	Beer, Liquor, Cigarettes

Note: This table lists the sales tax changes we study (with a change of 0.1 a 10 percent change), the level of the tax prior to the change, and the affected product categories.

Figure F.1: Log after-tax prices around sales-tax changes



Note: This figure plots the coefficients from Equation 4, along with their 95% confidence intervals, based on chain-clustered standard errors. The dependent variable is the residualized average log price, net of sales taxes, at the chain-state-week level. The coefficients can be interpreted as the trend in log prices prices among directly exposed and indirectly exposed stores, relative to unexposed stores, following a 1% excise tax change.

as estimates that include these events (in column 2). Including them changes the estimate slightly, reducing the pass-through elasticity among directly exposed stores from 0.97 to 0.86.

We exclude these events because the large federal tax increase interacts poorly with our log specification. The federal tax creates a large *level* change in prices for all stores, treatment and control. However the *percent* change in prices induced by the federal tax varies across stores; it is of course larger for stores with lower baseline prices. Thus pre-existing level differences between treatment and control end up creating trend differences in log prices.

To illustrate the issue, we plot average log cigarette prices (residualized net of store-product fixed effects) around Nevada's July, 2009 sales tax, for directly exposed stores, indirectly exposed stores, and clean controls, in Figure F.2. The figure shows an obvious increase in April, 2009, when the federal tax increased. More subtly, the increase in log prices is evidently larger for Nevada stores (directly exposed) than for the clean controls, as the clean controls start off below the Nevada stores but end up above them. The reason for this greater percent increase is that, prior to the tax increase, Nevada's prices (i.e. among directly exposed stores) averaged \$4.35 per pack, whereas the clean control prices averaged \$4.09. Thus in percent terms, Nevada's prices should increase by less than the control group, and we see that after the federal increase, Nevada's prices fall below the control group. Thus relative to trend, the simple difference-in-difference estimator implies a large fall in prices, and hence a very negative pass-through rate. The

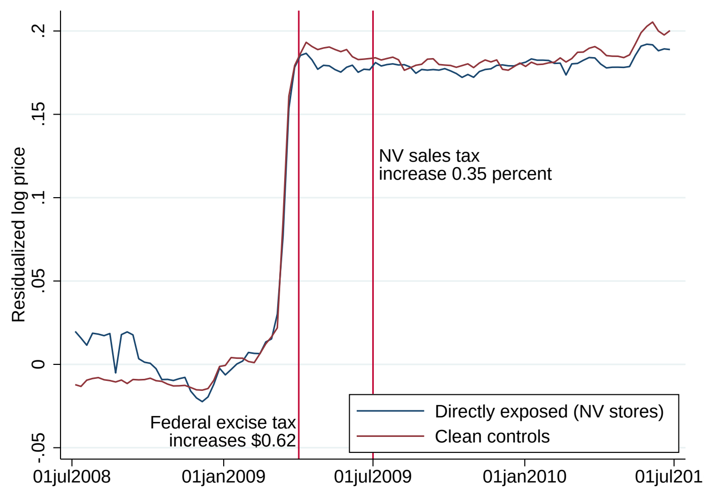
Table F.2: Sales tax pass-through elasticity among directly and indirectly exposed stores

	(1)	(2)
Elasticity, directly exposed stores	0.97	0.86
	(0.14)	(0.13)
Elasticity, indirectly exposed stores	-0.06	-0.07
	(0.15)	(0.16)
p-value, test of equality	0.00	0.00
# Observations	1,633,971	1,742,141
# Chains	83	83

Notes: The sample consists of stacked sales tax event samples. Each event-specific sample is limited to states with a sales tax increase, plus clean control states without an increase, both limited to a one-year window around the event. To obtain the "direct elasticity" and "indirect elasticity" elasticities, we regress the indicated outcome on the change in log(1+tax), interacted with post $\times$ directly exposed and post $\times$ indirectly exposed, as well as fixed effects for chain-state-event and time-event. Robust standard errors, clustered on chain, in parentheses.

large federal tax change in levels, combined with our log specification, makes it difficult to find valid control groups for sales tax changes occurring around the time of the federal tax change. Because the sales tax changes are small relative to the federal change, this specification problem produces very large pass-through estimates; here we estimate that Nevada's sales tax increase of 0.35 percent is passed through at a rate of -10 (i.e., prices fall by 3.5 percent, relative to the control group).

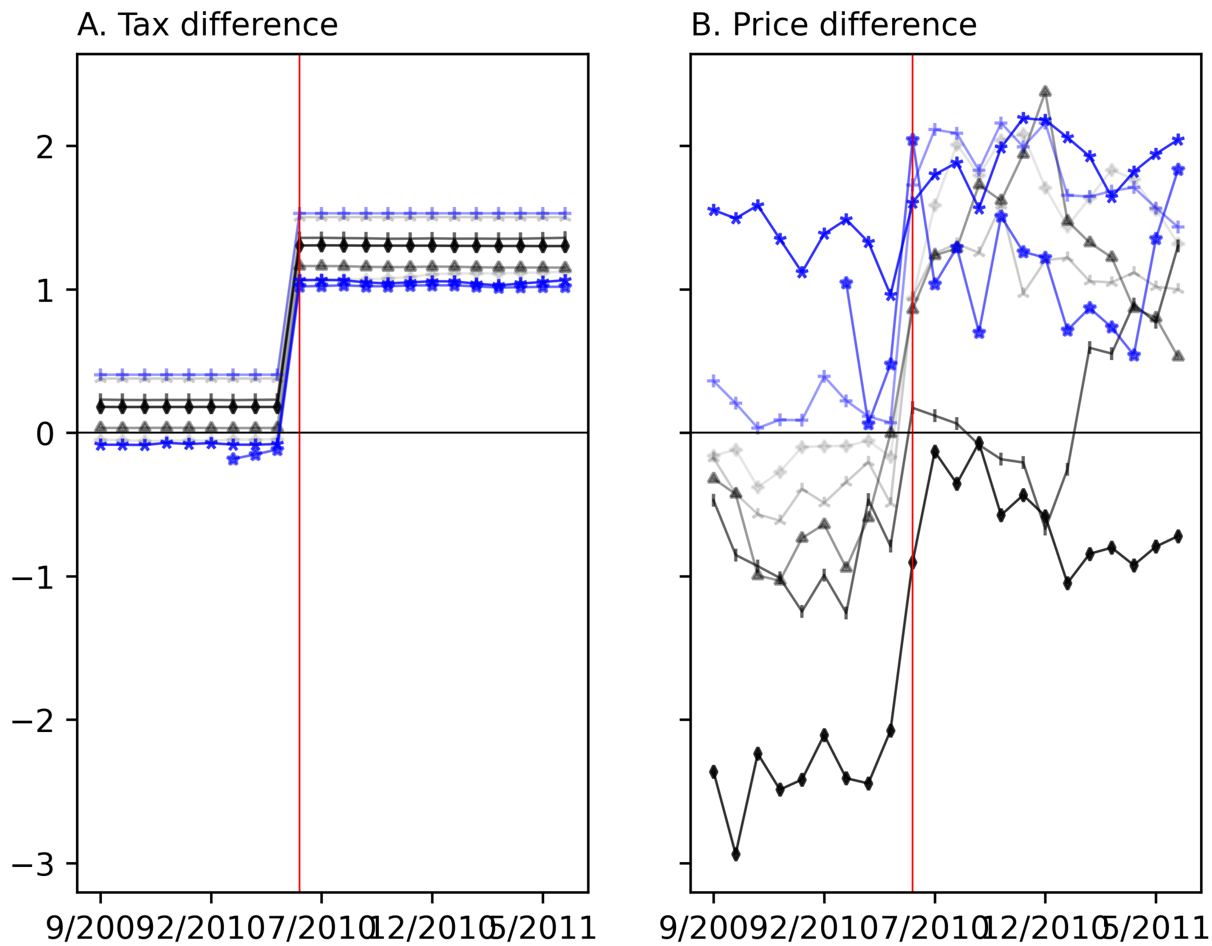
Figure F.2: Log cigarette prices around Nevada's sales tax increase and the federal excise tax increase



Note: Figure plots the average residualized (net of store-product fixed effects) log cigarette price for Nevada stores and for clean control stores in the one-year window around Nevada's sales tax increase of 0.35 percent on July 1, 2009.

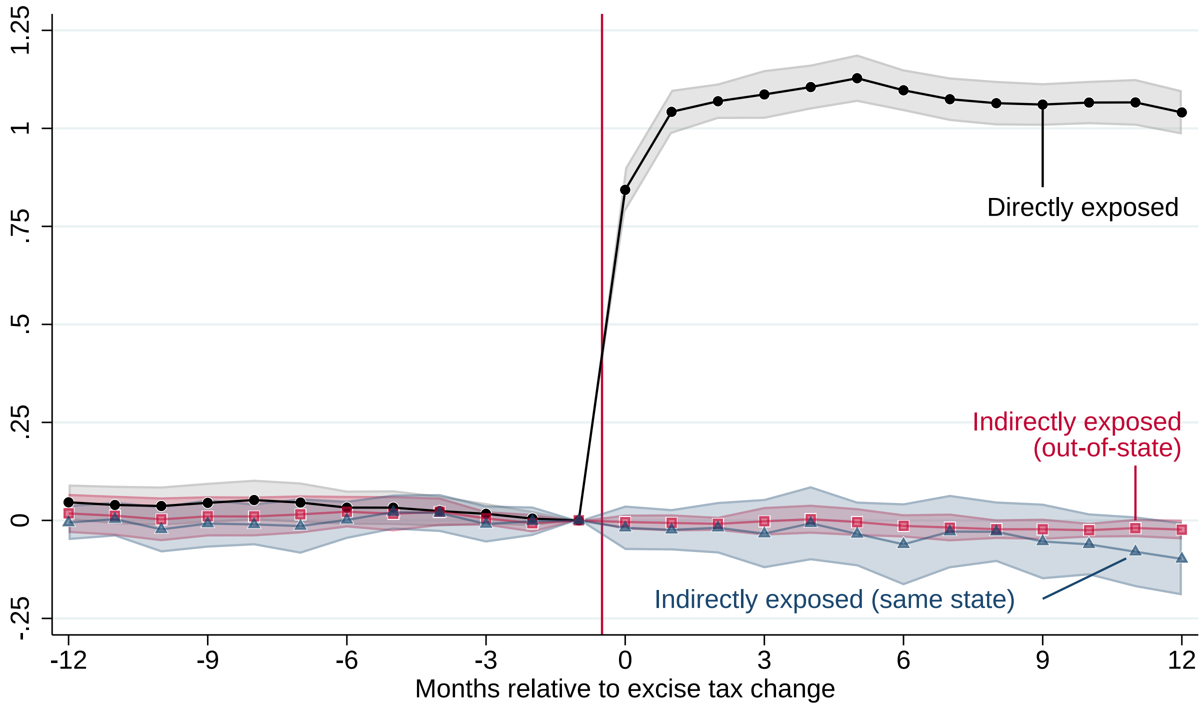
G Additional Figures and Tables

Figure G.1: Within-chain average difference between WA and outside of WA stores



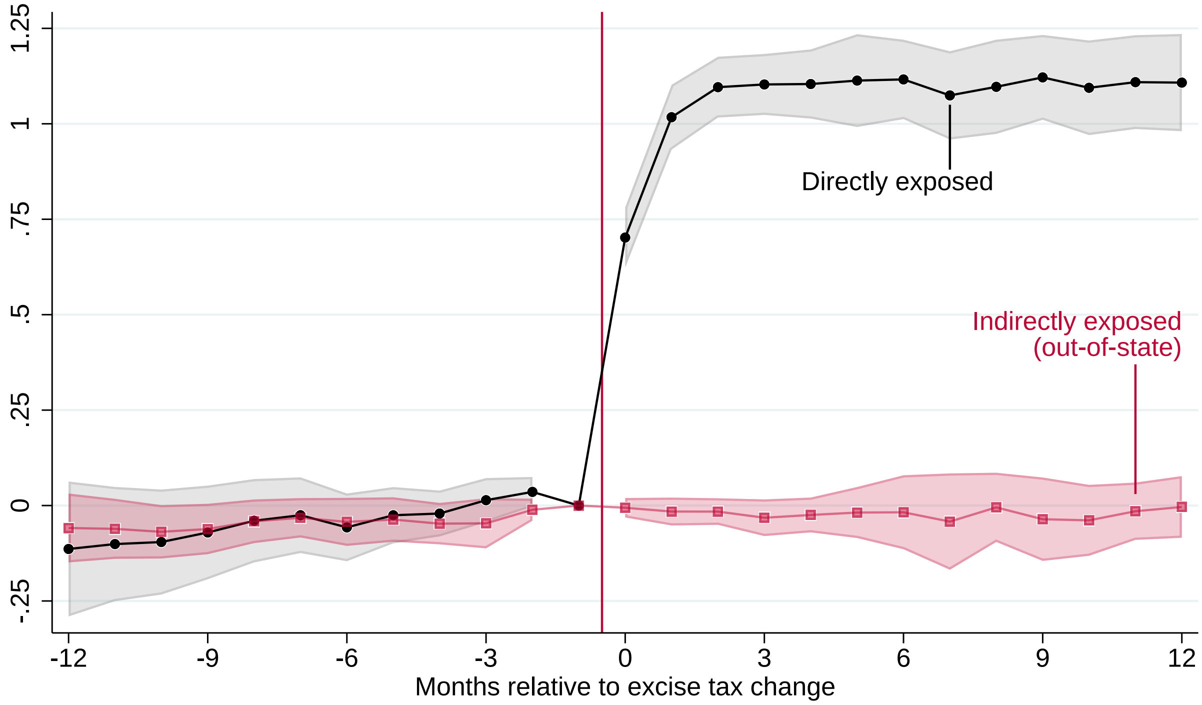
Note: This figure compares stores in WA and outside of WA for each of eight exposed chain. Panel A plots the average tax difference between WA and non-WA stores for each chain in monthly frequency. Panel B shows the average price difference of sample beer UPCs between WA and non-WA stores for each chain.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

Figure G.2: Trends in prices around large excise tax increases



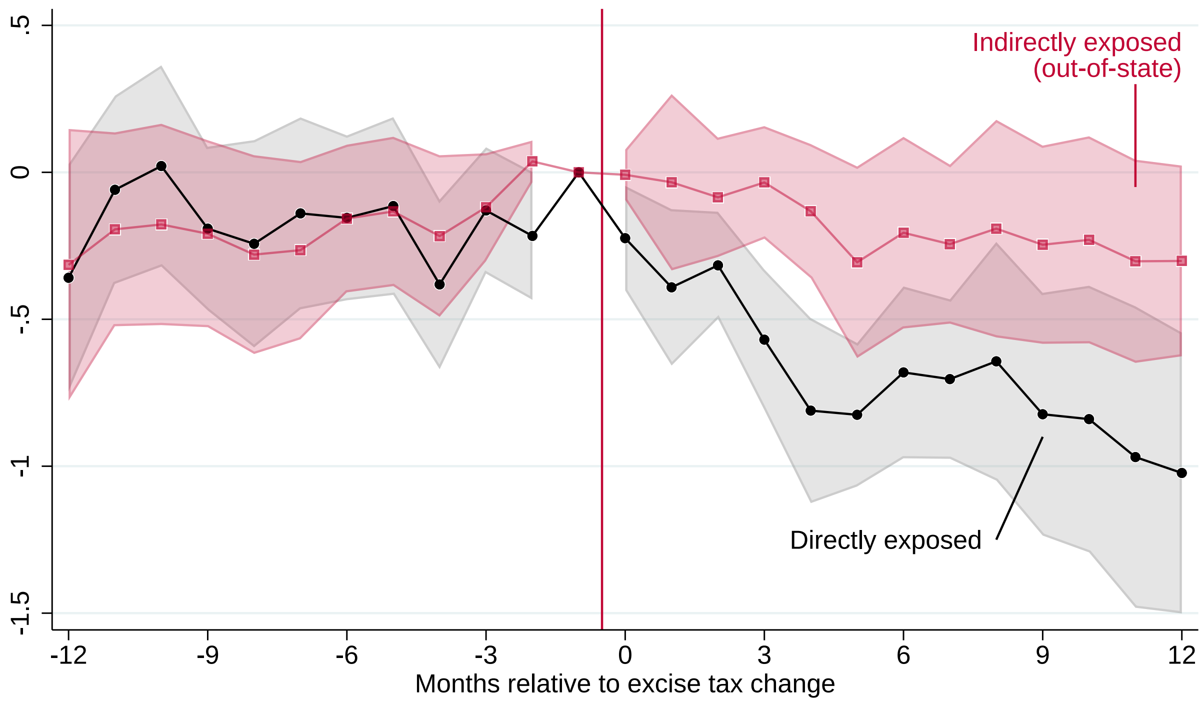
Note: The sample is limited to tax changes of above-median size (among positive tax changes). This figure plots the coefficients from Equation 4, along with their 95% confidence intervals, based on chain-clustered standard errors. The coefficients can be interpreted as the trend in prices among directly exposed and indirectly exposed stores, relative to unexposed stores, following a $1 excise tax change.

Figure G.3: Trends in prices around small excise tax increases



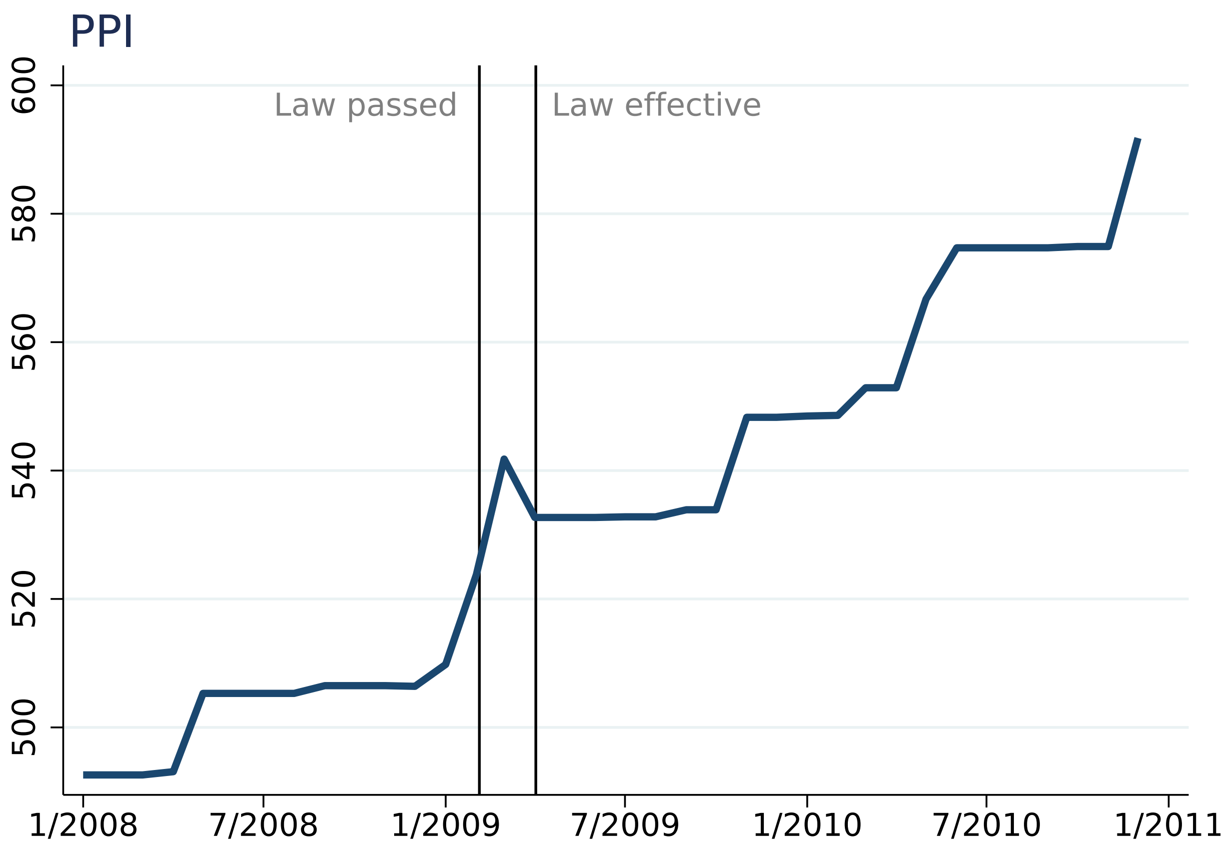
Note: The sample is limited to tax changes of small-median size (among positive tax changes). This figure plots the coefficients from Equation 4, along with their 95% confidence intervals, based on chain-clustered standard errors. The coefficients can be interpreted as the trend in prices among directly exposed and indirectly exposed stores, relative to unexposed stores, following a $1 excise tax change.

Figure G.4: Trends in prices around excise tax decreases



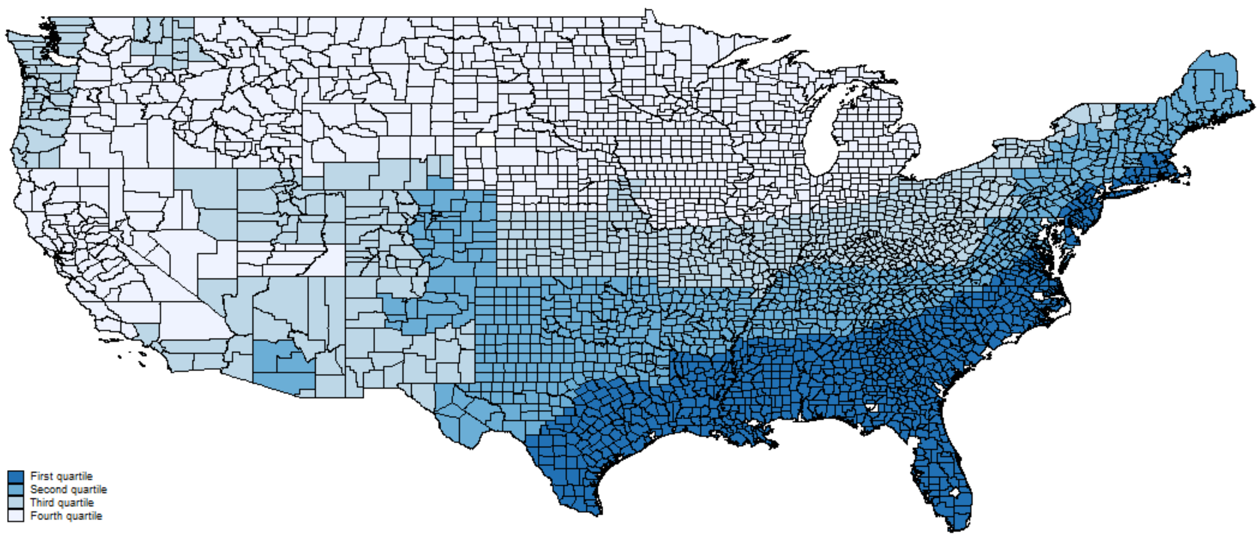
Note: The sample is limited to the two excise tax decreases. This figure plots the coefficients from Equation 4, along with their 95% confidence intervals, based on chain-clustered standard errors. The coefficients can be interpreted as the trend in prices among directly exposed and indirectly exposed stores, relative to unexposed stores, following a $1 excise tax decrease, so a coefficient of -1 implies complete pass-through.

Figure G.5: Tobacco producer price index around the time of CHIP reauthorization



Note: This figure plots the monthly tobacco PPI around the time of the CHIP Reauthorization Act of 2009, which raised federal cigarette excise taxes by $0.62.

Figure G.6: Class I milk county-level price differentials



Note: This figure reports the quartiles of the milk price geographical differentials by county (stipulated in $ per hundredweight of milk), using the user-contributed STATA function"spmap" (Pisati, 2022). The first quartile includes counties that have a price differential over $3.00 per hundredweight. The second quartile includes counties that have a price differential between $2.30 and $3.00 per hundredweight. The third quartile includes counties that have a price differential between $1.80 and $2.30 per hundredweight. The fourth quartile includes counties with a price differential less than $1.80 per hundredweight. Source: Electronic Code of Federal Regulations (2021).

Table G.1: List of beer excise tax changes

State	Date	Change	# Exposed chains	# Unexposed chains	Why not analyzed
NY	2009-05-01	.07	8	35	
IL	2009-09-01	.1	13	31	
NC	2009-09-01	.19			Sales tax change within +-1 year within state.
WA	2010-06-01	1.13	8	37	
CT	2011-07-01	.11			Sales tax change within +-1 year within state.
TN	2013-07-01	2.28			Other taxes changed at the same time.
WA	2013-07-01	-1.13	8	41	
RI	2014-07-01	.02			Not enough time between events within state.
RI	2015-01-01	.004			Not enough time between events within state.
TN	2016-01-01	.32			Other taxes changed at the same time.
LA	2016-04-01	.19			Sales tax change within +-1 year within state.

Note: This table reports all state excise tax changes for beer that we identified as occurring in the period 2006–2018. The change is the legislated tax increase per 288 ounces. We do not analyze some of the tax changes for reasons explained in the table. For the analyzed changes we report the number of exposed and unexposed chains.

Table G.2: List of cigarette excise tax changes

State	Date	Change	# Exposed chains	# Unexposed chains	Why not analyzed
AK	2006-07-01	.2			Nielsen does not have data in AK.
NC	2006-07-01	.05			Sales tax change within +-1 year within state.
VT	2006-07-01	.6	5	39	
NJ	2006-07-15	.18			Sales tax change within +-1 year within state.
HI	2006-09-30	.2			Nielsen does not have data in HI.
AZ	2006-11-07	.82	6	33	
SD	2007-01-01	1	4	32	
TX	2007-01-01	1	6	31	
IA	2007-03-15	1	5	31	
AK	2007-07-01	.2			Nielsen does not have data in AK.
CT	2007-07-01	.49	3	32	
IN	2007-07-01	.44			Sales tax change within +-1 year within state.
NH	2007-07-01	.28	5	31	
TN	2007-07-01	.42	9	27	
DE	2007-07-31	.6	7	29	
HI	2007-09-30	.2			Nielsen does not have data in HI.
MD	2008-01-01	1			Sales tax change within +-1 year within state.
WI	2008-01-01	1	4	35	
NY	2008-06-03	1.25	7	33	
MA	2008-07-01	1	3	39	
VT	2008-07-01	.2	4	38	
HI	2008-09-30	.2			Nielsen does not have data in HI.
DC	2008-10-01	1	6	28	
NH	2008-10-15	.25			Not enough time between events within state.
AR	2009-03-01	.56	8	24	
KY	2009-04-01	.3	6	28	
RI	2009-04-10	1	3	32	
MS	2009-05-15	.5	6	27	
FL	2009-07-01	1	6	28	
HI	2009-07-01	.6			Nielsen does not have data in HI.
NH	2009-07-01	.45			Not enough time between events within state.
NJ	2009-07-01	.12	8	30	
VT	2009-07-01	.25	5	30	
DE	2009-08-01	.45	9	25	
NC	2009-09-01	.1			Sales tax change within +-1 year within state.
WI	2009-09-01	.75	5	26	
CT	2009-10-01	1	4	28	
DC	2009-10-01	.5	6	26	
PA	2009-11-01	.25	9	24	
WA	2010-05-01	1	7	25	
HI	2010-07-01	.4			Nielsen does not have data in HI.
NM	2010-07-01	.75			Sales tax change within +-1 year within state.
NY	2010-07-01	1.6	8	32	
SC	2010-07-01	.5	8	29	
UT	2010-07-01	1	6	30	
MN	2011-01-01	.004	5	34	
CT	2011-07-01	.4			Sales tax change within +-1 year within state.
HI	2011-07-01	.2			Nielsen does not have data in HI.
NH	2011-07-01	-.1	5	38	
VT	2011-07-01	.38	5	37	
DC	2011-09-14	.004	6	37	
IL	2012-06-24	1	11	34	
RI	2012-07-01	.04	4	42	
MN	2013-07-01	1.6	6	39	
MA	2013-08-01	1	5	37	
NH	2013-08-01	.1	5	37	
OR	2014-01-01	.13	7	34	
VT	2014-07-01	.13	7	37	
DC	2014-10-01	.4	6	36	
MN	2015-01-01	.07	6	38	
KS	2015-07-01	.5			Sales tax change within +-1 year within state.
LA	2015-07-01	.5			Sales tax change within +-1 year within state.
NV	2015-07-01	1	9	33	
OH	2015-07-01	.35	7	34	
RI	2015-07-01	.25	4	37	
VT	2015-07-01	.33	7	35	
AL	2015-10-01	.25	6	33	
CT	2015-10-01	.25			Not enough time between events within state.
DC	2015-10-01	.01	6	33	
MN	2016-01-01	.1	6	34	
OR	2016-01-01	.01	7	32	
LA	2016-04-01	.22			Sales tax change within +-1 year within state.
CT	2016-07-01	.25			Not enough time between events within state.
WV	2016-07-01	.65	8	30	
PA	2016-08-01	1	10	28	
DC	2016-10-01	.01	6	29	
MN	2017-01-01	.04	6	31	
CA	2017-04-01	2			Sales tax change within +-1 year within state.
RI	2017-07-01	.5	4	31	
DE	2017-09-01	.5	9	28	
CT	2017-12-01	.45	6	31	
OR	2018-01-01	.01	5	28	
KY	2018-07-01	.5	7	27	
OK	2018-08-23	1			Not enough sample period post event.
DC	2018-10-01	2.02			Not enough sample period post event.

Note: See notes to Table G.1. The units of the change are dollars per 20 cigarettes.

Table G.3: List of liquor excise tax changes

State	Date	Change	# Exposed chains	# Unexposed chains	Why not analyzed
NJ	2009-08-01	.22	3	14	
IL	2009-09-01	.8	8	11	
CT	2011-07-01	.18			Sales tax change within +-1 year within state.
RI	2013-07-01	.33			Control state.
LA	2016-04-01	.1			Sales tax change within +-1 year within state.

Note: See notes to Table G.1. The units of the change are dollars per 750 ml.

Table G.4: List of soda excise tax changes

Location	Date	Change	# Exposed chains	# Unexposed chains	Why not analyzed
Philadelphia, PA	2017-01-01	1.5	7	34	
Cook county, IL	2017-08-02	1			Not enough time between events within state.
Cook county, IL	2017-12-01	-1			Not enough time between events within state.
San francisco, CA	2018-01-01	1	3	37	

Note: See notes to Table G.1. The units of the change are cents per ounce.

Table G.5: Additional summary statistics on selected stores and products

	Beer	Cigarettes	Liquor	Soda	
Yearly revenue	All	\$5.8bn	\$5.9bn	\$3.2bn	\$4.9bn
	Sample stores	\$4.7bn	\$4.0bn	\$2.4bn	\$3.9bn
Revenue share	Sample stores	0.82	0.67	0.75	0.80
	Sample products	0.13	0.05	0.06	0.33
	Sample stores, products	0.11	0.03	0.05	0.27
Availability share	Sample food stores	0.88	0.84	0.84	0.91
	Sample drug and mass merch. stores	0.47	0.82	0.65	0.60

Note: This table reports the average yearly revenue and share of revenue for our selection of stores and products relative to all the products and stores in the Nielsen sample.

Table G.6: Robustness of pass-through estimates to alternative specification

Specification	Main	Clean parents	No border counties	No border states	Long-term
	(1)	(2)	(3)	(4)	(5)
A. Pass-through rates					
Directly exposed	1.01	1.05	0.99	1.01	1.03
	(0.02)	(0.04)	(0.03)	(0.02)	(0.02)
Indirectly exposed	-0.02	0.00	-0.02	-0.02	-0.02
	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)
Indirectly exposed, same-state	-0.04	-0.05	-0.04	-0.04	-0.04
	(0.03)	(0.06)	(0.03)	(0.03)	(0.03)
B. Tests of equality (p-values)					
Direct = indirect	0.00	0.00	0.00	0.00	0.00
Direct = indirect, out-of-state	0.00	0.00	0.00	0.00	0.00
# Observations	1,775,913	478,427	1,730,059	1,624,137	1,472,473
# Chains	96	95	96	96	96
Contaminated stores?	No	No	No	No	N
Contaminated parents?	Yes	No	Yes	Yes	Yes
Border counties	Yes	Yes	Yes	No	Yes
Months	[-12, 12]	[-12, 12]	[-12, 12]	[-12, 12]	[-12,-9]∪[3,12]

Note: 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. We also report p-values for tests of the null hypothesis that $\rho^{\mathrm{d}} = \rho^{\mathrm{i}}$ and $\rho^{\mathrm{d}} = \rho^{\mathrm{i}}$. Robust standard errors, clustered on chain, in parentheses. In column (2) we exclude chains whose parent companies are subject to multiple, simultaneous tax changes. In column (3) we exclude stores in border counties in the event state. In column (4) we exclude state bordering the event state. In column (5) we exclude the 3 months immediately before and after the tax change.

Table G.7: Pass-through by size of tax change

Size of tax change	Above median (positive) (1)	Below median (positive) (2)	Negative (3)
A. Pass-through rates			
Directly exposed	1.03	1.11	0.51
	(0.02)	(0.07)	(0.07)
Indirectly exposed	-0.02	0.02	0.01
	(0.01)	(0.04)	(0.05)
Indirectly exposed, same state	-0.04		
	(0.03)		
B. Tests of equality (p-values)			
Direct = indirect	0.000	0.000	0.000
Direct = indirect, in-state	0.000		
# Observations	827,826	921,598	61,530
# Chains	95	81	78

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. Column (1) is limited to events with positive, above-median tax changes, column (2) to positive, below-median tax changes, and column (3) to tax decreases. We also report p-values for tests of the null hypothesis that $\rho^{\text{d}} = \rho^{\text{i}}$ and $\rho^{\text{d}} = \rho^{\text{i}}$. All of the tax changes with within-state variation are large so we do not report in-state spillovers in columns (2) and (3). Robust standard errors, clustered on chain, in parentheses.