Getting into the Weeds of Tax Invariance

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Abstract

We provide the first general empirical test of tax invariance (TIV). When a 25 percent tax remitted by manufacturers was eliminated in Washington state and the retail cannabis excise tax was simultaneously increased from 25 to 37 percent—a shift intended to be revenue-neutral—TIV did not hold. Manufacturers kept two-thirds of their tax savings instead of passing all their savings through to retail firms via lower prices as predicted by TIV. One-third of the retail tax increase was passed on to consumers via higher retail prices – TIV would have predicted constant or even declining tax-inclusive retail prices.

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1 Introduction

Tax invariance (TIV)—the principle that who remits taxes does not influence incidence—is a bedrock principle of tax design. TIV allows policymakers to focus on minimizing administrative and evasion costs without worrying about the welfare effects of alternative tax collection strategies. TIV is routinely taught in "Principles of Economics" courses (McConnell et al., 2018; Mankiw, 2020). While recent empirical work suggests that TIV can fail under specific circumstances—when tax evasion opportunities vary along the supply chain (Slemrod, 2008; Kopczuk et al., 2016; Brockmeyer and Hernandez, 2016), when there are price rigidities (Muysken et al., 1999; Saez et al., 2012; Lehmann et al., 2013), or if tax salience is different for consumers and firms (Chetty et al., 2009; Finkelstein, 2009)—it is unclear whether TIV simply does not hold, or just that it cannot be applied in particular settings.

We provide a more general test of TIV than has previously been possible by studying the cannabis market in Washington state.[1] The frequently-audited comprehensive regulatory reporting system makes tax evasion difficult. Prices both increase and decrease often, which means rigidities are unlikely. Tax salience is likely high for manufacturers, retailers, and consumers. Regulatory requirements ensure that owners are highly-skilled and well-capitalized. The posted retail prices include all taxes, so tax-inclusive prices are likely salient to consumers. Finally, tax leakage and competition are not relevant as the market is closed. Each gram of cannabis purchased in Washington was grown in Washington, and vice versa, and neighboring states did not have legal cannabis markets at the time.

We study an ideal reform for testing TIV. Prior to July 1, 2015, a 25% gross receipts

[1] We describe the market in Section 2.

tax applied to each transfer of cannabis. Cultivators remitted the tax when they sold to manufacturers, manufacturers remitted the tax when they sold to retailers, and retailers remitted the tax when they sold to consumers. The retail tax was required to be included in the posted price making it functionally equivalent to other excise and sales taxes. After the reform, the retail tax was increased to 37% and all other taxes were eliminated. Crucially, this change was unexpected by market participants; the reform was passed on June 27, 2015, and signed by the Governor on June 30 (La Corte, 2015).

We measure the effects of this reform using an interrupted time series regression in first differences; that is, we ask how prices change in the week of the reform relative to weeks surrounding the reform. Identification rests on the assumption that, after controlling for product characteristics, prices would not have changed in the week of the reform (relative to a baseline trend) in the reform's absence. We conduct event study and placebo analyses which provide no evidence to reject this assumption. We employ this approach rather than a difference-in-differences design as the only potential comparison state is Colorado, which had a significantly different regulatory and industry structure—the assumption that prices in the two states co-move in the period of the reform is likely much stronger than the assumptions we impose.

Our setting features imperfect competition—retailers have substantial market power (Hollenbeck and Uetake, 2019; Mace et al., 2020) and manufacturers, while more competitive, retain market power too (see Table 1). Given the the emphasis on imperfect competition in tax incidence analysis (Weyl and Fabinger, 2013), we examine how TIV predictions vary for a percent-based tax—the relevant tax in our setting—depending on the level of competition. We compare two extremes: perfect competition and a monopolist retailer and monopolist

manufacturer. We show that manufacturers pass along their entire savings in response to the elimination of their tax in both situations. Under perfect competition, retailers leave their tax-inclusive prices unchanged. Under monopoly, retailers cut their prices to maximize profits under the new system. Our setting lies between these extremes.

We then examine how manufacturer prices change post-reform. Our framework predicts that manufacturers' prices should decrease 28.7% from pre-reform levels. Given that per-gram tax revenue would fall slightly in that scenario,[2] we also consider a second benchmark, the amount manufacturers needed to pass-through to leave retailers' per-gram profits and consumer-facing tax-inclusive prices constant post-reform (17.7%). We find that manufacturers reduce their prices by only 7.2%; we reject the null hypothesis of TIV based on either benchmark at the 0.1 percent level.

Finally, we examine retail behavior. Our framework predicts that retailers should either leave their tax-inclusive prices constant or decrease them. Instead, we find tax-inclusive retail prices *increased* by an average of 2.5%. Retailers pass through one-third of the tax increase to consumers. Another roughly one-third is borne by manufacturers, leaving retailers to bear about one-third of the increase. We find evidence that retailers maintained constant tax-exclusive markups, consistent with our model's pricing rule.

In summary, we find that TIV fails. A reform that should have left the welfare of manufacturers, retailers, and consumers unchanged or improved instead increased the profits of manufacturers at the expense of retailers and consumers. We conclude by discussing potential mechanisms for this result and implications for policymakers and future research.

[2]Revenue would have remained roughly constant if tax-exclusive prices remained constant—i.e. retailers had passed along their entire tax increase to consumers.

2 Background

Our analysis focuses on the adult-use cannabis market in Washington state, which opened in July 2014 after a successful ballot initiative in 2012. We have written elsewhere about the history of this market (Miller and Seo, 2018; Hansen et al., 2020)—here we focus on features of the market and the reform that underlie our analysis.

Washington's cannabis market consists of three types of firms: cultivators, who grow cannabis plants, manufacturers, who transform plant material into marijuana products, and retailers, who sell products they obtain from manufacturers to consumers. Potential entrants have to pass background checks and undergo a lengthy regulatory process requiring substantial capital investment before entry. Cultivators face capacity constraints—the largest firms may cultivate 30,000 sq. ft. of plant canopy and may not merge to increase capacity. While retailers must be financially independent from other firms, a cultivator and a manufacturer may vertically integrate, though the capacity constraint remains. When the reform was implemented, approximately 94% (by weight) of usable marijuana—dried and cured cannabis flowers—was produced through a vertically-integrated process (Hansen et al., 2020). Thus, we focus our analyses on two types of firms, "manufacturers" and "retailers".[3]

The market features a closed supply: all cannabis sold by retailers is grown in the state, and every ounce grown legally within the state is sold at a Washington retailer. These rules are enforced through the state's "seed-to-sale" traceability system, which tracks each plant from cultivation through processing and retail. This system was implemented to respond to the informal federal regulations created by the "Cole Memo" (Cole, 2013). The system

[3]State law calls cultivators "producers" and manufacturers "processors"—we choose nomenclature to represent functional equivalents in other markets.

provides information that can be used to check for tax evasion: retailers cannot sell cannabis without manufacturing records, which forces manufacturers to report accurately.[4] Reporting is enforced through frequent audits—firms typically face one or more visits per year—backed by significant penalties for non-compliance.

Washington's initial tax regime consisted of a 25% tax collected at every transfer of cannabis. Vertically-integrated manufacturers owed no tax on intra-firm transfers. The reform we analyze eliminated the 25% excise taxes within the supply chain and increased the retail excise tax rate to 37%. The excise tax applied to the sales-tax-inclusive price pre-reform and the sales-tax-exclusive price post-reform. Accounting for changes to the base and rate of the retail tax, the reform changed the retail tax rate by 6.93%.[5] This change was designed to be revenue neutral under the assumption that tax-*exclusive* prices remained constant (whereas TIV predicts constant tax-*inclusive* prices). We account for both the change in the rate and the base of the retail excise tax in our analyses. We provide calculations of revenue pre- and post-reform in Section 4. Other regulations concerning cannabis production, distribution, and sales were unaffected.

Our identification assumes that the policy change was unanticipated. The bill originated and was passed in the Washington House during the 2015 Regular Session, but stalled in the Senate. The bill was reintroduced in the First Special Session, but again stalled. Finally, on June 27, the last day of the Second Special Session, the bill passed both chambers. The Governor signed it on June 30 and the law went into effect the next day. Contemporaneous

[4]Retailers can under-report their sales, but such behavior is detectable as retail sales can be compared to purchases from manufacturers. Our estimates are unaffected by dropping the few retailers that engage in significant under-reporting.

[5]The average sales tax rate during this period was 8.9%, thus $\log\left(\frac{1.25(1+.089)}{1.37+.089}\right) = -0.0693$

reporting portrayed the industry as unprepared. According to one retail manager, "[we] have a few hours to change an entire market's pricing structure. It is an exceptionally short window for such a tremendous change" (La Corte, 2015).

3 A Framework for Tax Invariance

To motivate our empirical analyses, we introduce stylized models of manufacturers, retailers, and consumers. We assume a constant manufacturing marginal cost of mc. Given tax-inclusive retail price p, we assume demand has constant price elasticity, that is $q(p) = kp^{\epsilon}$ with $\epsilon < -1$. We evaluate two extremes: (1) perfectly competitive manufacturers and perfectly competitive retailers and (2) a monopolist manufacturer and monopolist retailer. We expect our empirical setting lies between the two (see Section 4).

Let p_m^i be the price charged by manufacturers to retailers including all taxes. Let p_m^e be the price charged by manufacturers to retailers exclusive of taxes. Given a manufacturing tax rate of τ_m, $p_m^e = p_m^i(1 - \tau_m)$. Similarly, let p_r^i be the tax-inclusive retail price and let p_r^e be the tax-exclusive retail price. Given a retail tax rate of τ_r, $p_r^i = p_r^e(1 + \tau_r)$. While these definitions are not parallel, they match our empirical setting.

Perfect Competition-Perfect Competition In perfect competition, the tax-exclusive price earned by manufacturers is equal to their marginal cost, and so the tax-inclusive price is $p_m^i = \frac{mc}{1-\tau_m}$. Perfectly competitive retailers face this price as their marginal cost, and so the tax-inclusive retail price is $p_r^i = mc\frac{1+\tau_r}{1-\tau_m}$. The total tax revenue collected is $TR = k\left(\frac{mc(1+\tau_r)}{1-\tau_m}\right)^{\epsilon}\frac{mc}{1-\tau_m}(\tau_r + \tau_m)$. To see TIV holds, define $\tau = \frac{1+\tau_r}{1-\tau_m}$. Then $p_r^i = mc \cdot \tau$ and

$TR = k(mc)^{\epsilon+1}\tau^{\epsilon}(\tau - 1)$. Given some τ, a policy maker can freely move one of τ_r or τ_m, solve for the other, and hold p_r^i and TR constant.

Monopolist-Monopolist Under monopoly, the retailer's profit maximization problem is $\max_{p_m^i} \left(\frac{p_r^i}{1+\tau_r} - p_m^i\right) k(p_r^i)^{\epsilon}$ which implies $p_r^i = \frac{\epsilon}{1+\epsilon} p_m^i(1+\tau_r)$. Note that the retailer's tax-exclusive price is a constant markup over their marginal cost p_m^i. The quantity is $q = b(p_m^i)^{\epsilon}$ where $b \equiv k\left[\frac{\epsilon}{1+\epsilon}(1+\tau_r)\right]^{\epsilon}$. The wholesaler's problem is $\max_{p_m^i}[p_m^i(1-\tau_m) - mc]b(p_m^i)^{\epsilon}$ which implies $p_m^i = \frac{\epsilon}{1+\epsilon}\frac{mc}{1-\tau_m}$. Thus, the tax-inclusive price charged by the manufacturer is independent of the retail tax and $p_i^r = \left(\frac{\epsilon}{\epsilon+1}\right)^2 \frac{1+\tau_r}{1-\tau_m} mc$ and $TR = k\left(\frac{\epsilon}{\epsilon+1}\right)^{2\epsilon+2}\left(mc\frac{1+\tau_r}{1-\tau_m}\right)^{\epsilon+1}\left[\frac{\tau_r}{1+\tau_r} + \frac{\tau_m}{1+\tau_r}\frac{\epsilon+1}{\epsilon}\right]$. Mechanically, if τ is defined as above, the term in brackets cannot be simplified to be a function of τ alone. Given some p_r, if a policy maker shifts τ_r and τ_m to hold p_r constant, TR must change. Thus, TIV fails. Intuitively, the percentage taxes act as demand shifters as in Weyl and Fabinger (2013), but the wholesaler does not internalize the retailer's response to retail percentage taxes because its effective demand elasticity is unchanged.

Given TIV generally does not hold in this monopolist-monopolist case, we want to understand the effect of a movement from a manufacturing tax to a retail tax. First, suppose that the policy $\omega_1 = \{\tau_r = 0, \tau_m = \tau\}$ is replaced with $\omega_2 = \{\frac{\tau}{1-\tau}, 0\}$. From the equations above, it is clear that p_r^i (and thus quantities) remains constant. The manufacturer passes through all of its tax savings, and earns identical profits. However, the retailer's profits decrease because the τ savings on the manufacturer's price is more than offset by the $\frac{\tau}{1-\tau}$ tax on their price. By the same logic, TR increases as $\frac{\varepsilon+1}{\varepsilon} < 1$. Now consider the policy $\omega_3 = \{\tau', 0\}$ where $\tau' = \frac{p_m^i(\omega_1)\tau}{p_r^i(\omega_1) - p_m^i(\omega_1)\tau}$ is "naive-revenue-neutral": it would raise the same total revenue *if* the tax-inclusive retail price was the same after the reform. In this case,

since $\tau' < \frac{\tau}{1-\tau}$, $p_r^i(\omega_3) < p_r^i(\omega_1)$ and $q(\omega_3) > q(\omega_1)$. Since $\varepsilon < -1$, profits for both firms and total tax revenues increase. Finally, suppose ω_1 is replaced with $\omega_4 = \{\tau'', 0\}$ where τ'' is chosen to be "true-revenue-neutral": $TR(\omega_1) = TR(\omega_4)$. Since $TR(\omega_3) > TR(\omega_1)$, $\tau'' < \tau'$ and thus ω_4 increases profits for both the retailer and the manufacturer beyond ω_3. This is consistent with the notion that, under monopoly, ad valorem taxes improve welfare over unit taxes (Suits and Musgrave, 1953; Skeath and Trandel, 1994; Keen, 1998).

In summary, the combination of market power and percent taxes leads traditional TIV to fail. However, revenue-neutral policies (whether "naive" or "true") that shift taxes from manufacture to retail lead to full pass-through from the manufacturer to the retailer and a decrease in tax-inclusive retail prices. Firms and consumers benefit from the change.

4 Data and Methods

Our data consist of administrative records from the "traceability" (or seed-to-sale) system maintained by the Washington State Liquor and Cannabis Board (WSLCB). We obtain data on all plants, products, and sales. We restrict our analysis to "usable marijuana" products—74.5% of the total transactions observed in our data. Within this category, products are differentiated by "strain" (analogous to fruit cultivars), potency, and whether the marijuana is loose or pre-rolled into a joint. These characteristics are captured by our fixed effects.

Harvested flowers and other plant material are converted into an "inventory lot" that is assigned a unique identifier (ID). Products or material within a single inventory lot are assumed to be homogeneous. Large inventory lots of finished product are split into smaller "retail" lots for sale to retailers. Each retail lot consists of multiple sealed packages of a

specific weight of cannabis (e.g. 1 gram, 3.5 grams, etc) which are considered identical. When lots are sold to retailers, the system records the date, quantity, and price, and assigns a new lot ID. Thus, retail lot IDs uniquely identify the retailer, manufacturer, cultivator, product, and package size.[6] We observe each retail sale and link the price, quantity, and transaction time to the relevant inventory lots.

We aggregate by inventory-lot-week. We exclude firms with less than two months of pre- and post-reform data. The reform also changed technical reporting rules which affect the price data. We clean the price data for each firm to reflect the prices faced by consumers using an algorithm based on rounding behavior verified by spot checks of historical menus.[7] See Appendix B for details.

Table 1 reports summary statistics for retail inventory lots for the six weeks pre-reform (the basis for our analyses in Section 5). The average tax-inclusive retail price was $13.03 per gram and the tax-exclusive price was $9.57 per gram. Retailer tax-exclusive prices are more than double manufacturer tax-inclusive prices. Both manufacturer and retail prices change week-over-week by more than one percent almost 40 percent of the time, split fairly evenly between price increases and price decreases, suggesting prices are not rigid.

The average market share of retailers in the 10-mile radius around their location was 31%, suggesting that there is substantial market power at retail, consistent with Hollenbeck and Uetake (2019) and Mace et al. (2020). The manufacturer market is effectively state-wide and the average market share is 1.4%. No manufacturer has more than 7% of the total market share. However, manufacturers may exert market power on individual retailers—on average,

[6] A small number of lots have multiple package sizes, which we identify and correct for.

[7] Cannabis retailers have limited access to financial services and so choose to set tax-inclusive prices that are round numbers (e.g. $8 or $10.25) to lower cash-handling costs. While this represents a potential friction, the effective minimum price change is smaller than the effects we estimate.

about seven manufacturers supply 75% of a retailer's inventory.

Across competitive environments, our framework predicts that manufacturers should pass through their savings to retailers via a $\log(1 - 0.25) = -28.7\%$ decrease in manufacturer tax-inclusive prices. If we estimate a different price response to the reform, we can reject the framework. Since the reform was not quite revenue-neutral, it may be possible to construct alternative models which both rationalize any price responses we observe and which feature a TIV result. To rule out this concern, we construct a second benchmark for manufacturer price changes: given pre-reform prices, to maintain both a constant tax-inclusive retail price and constant per-gram retail profits (and therefore to satisfy TIV), manufacturers would have to decrease their prices by an average of 64 cents, or 17.7%.[8]

Under a revenue-neutral reform, TIV predicts that retailers would reduce their tax-*exclusive* prices by 6.93% (the amount of the change in the retail tax rate) and maintain constant tax-*inclusive* prices. Under monopoly, we predict tax-inclusive prices will decline. As we calculate the reform is slightly revenue-decreasing,[9] our framework suggests retailers should reduce tax-inclusive prices further.

To summarize, if we estimate a decrease in average manufacturer tax-inclusive prices of less than 28.7% (in a statistically significant sense), we reject our model and reject TIV indirectly. If we estimate a decrease in average manufacturer tax-inclusive prices of less than 17.7%, we reject TIV directly. If we estimate any *increase* in retailer tax-inclusive prices, we reject TIV directly.

Figure 1 plots the panel of retail tax-exclusive prices normalized to the week before

[8] 13.03/(1.37+0.089)-13.03/(1.25*(1+0.089)) = 64 cents.

[9] If prices had remained constant, the reform would have decreased the average total tax revenue per gram from \$4.49 to \$4.10.

reform. For each week, we take inventory lots in their first week of sale and match them with the price paid to the manufacturer, restricting observations to those where the first retail sale and manufacturer sale both happened pre- or post-reform; thus, this illustrates the relation between retailer per-gram revenue and variable costs. The two series move in a highly correlated way through the entire pre- and post-reform period (including the period around April 20, an industry promotional event). This implies a constant markup of the retail tax-exclusive price over variable costs (the manufacturer price) that appears to be preserved in response to the tax reform. This figure depicts a set of products that is changing over time. To disentangle the effects of the reform from long-run trends and control for potential compositional changes, we employ regression and (in Section 5.3) event study analyses.

We model responses to the tax reform as an interrupted time-series in first differences:

$$\Delta log(p_{it}) = \alpha_0 + \alpha_1 \Delta TaxReform_t + \alpha_2 FE_i + u_{it}, \tag{1}$$

where i is described below, and t indicates the week. p is the wholesale or retail price per gram, $TaxReform$ is an indicator variable that is one after July 1, 2015 and zero before, and FE are fixed effects. α_1 is the parameter of interest.[10] Our analysis window spans six weeks before and after the reform—we examine the robustness of our estimates to this bandwidth. We two-way cluster standard errors on manufacturer and retail location (Cameron et al., 2011).[11] Our identifying assumption is that within a given product, there are no shocks in

[10]Without fixed effects, this regression is equivalent to an interrupted time series regression in levels with fixed effects at the level of our first differences and a control for time to the reform.

[11]Firm clusters or two-way clusters on firm and week yield similar standard errors.

the week of the reform that would have a significant and systematic impact on prices besides the direct effect of the tax reform. Given the short interval between observations (i.e a week, not a year), this assumption is plausible and we will provide placebo regression evidence that this assumption is reasonable.

For the manufacturer analysis, we aggregate to the manufacturer-retailer-strain-week level, so that i is a manufacturer-retailer-strain tuple, and then take first differences.[12] Each manufacturer-retailer-strain tuple does not sell every week. We thus calculate the minimal-length difference and include difference-length fixed effects.[13] The maximum difference-length allowed is 4 weeks. We are thus estimating the magnitude of price changes in response to the reform within a specific firm-product pairing. When we add retailer-manufacturer-strain fixed effects, we allow each retailer-manufacturer-strain to have a separate time trend.

For our main retail analysis, we aggregate to the inventory-lot-week level so that i is an inventory lot.[14] Retail sales from an inventory lot are frequent, so we construct one-week differences. We are thus estimating the change in the retail price of an inventory lot in response to the tax reform holding all possible product and firm variation constant. Sales of retail inventory lots typically last multiple weeks, so we include fixed effects for the week since the first week a particular inventory lot sold. When we add inventory lot fixed effects, we allow prices in each inventory lot to have a separate time trend.

We separately examine the first week of retail sales for each inventory lot and include only those that were purchased from manufacturers in the same week. Similar to our man-

[12] Aggregation beyond the inventory lot is required because each lot is sold only once. Other possible aggregations produce similar estimates with lower power (though statistical significance remains).

[13] These fixed effects are not significant. Our estimates are similar when restricted to one-week differences, but with less power.

[14] We are able to work at this level because retailers repeatedly sell out of a single inventory lot.

ufacturer analysis, we aggregate by retailer-manufacturer-strain and take varied differences. We include difference-length fixed effects. In these regressions, we ask how prices for *new inventory lots purchased post-reform* change relative to *pre-reform lots of the same strain from the same manufacturer*. This allows us to examine whether prices change more or less if the inventory was purchased post-reform relative to inventory that had already been purchased and was selling pre-reform.

5 Results

5.1 Manufacturer Price Response

Table 2 reports estimates of Equation (1) for manufacturers. The estimate in Column (1), which includes no fixed effects, implies that prices changed by -6.5% in response to the tax reform (statistically significant at the 0.1% level). When we include manufacturer-retailer-strain fixed effects in Column (2)—our baseline specification—the point estimate becomes -7.2% (significant at the 0.1% level). This is roughly one-third of the 17.7% price decrease needed to preserve retailer per-gram profits (and therefore to minimially satisfy TIV), and one-quarter of the 28.7% decrease predicted by our framework. We can reject the null hypothesis that our estimate is consistent with TIV at the 0.1% level. Column (3) repeats Column (2) for the price in levels instead of logs—we find that the reform decreased manufacturer prices by 23 cents, about one-third of the 64 cent bound. Across specifications, the observed price adjustment was greater than 1% for more than 75% of our observations—suggesting firms were aware of this reform and prices are not rigid. Even if we rescaled our

estimate assuming that any observation with minimal adjustment was caused by rigidities or lack of awareness, the data would still reject the null hypothesis of TIV.

The bottom panel of Table 2 repeats the specification of each column for a placebo reform dated one year later. The estimates are near zero across all four columns, providing support that our regression specifications are valid. The top panel of Figure A.1 considers bandwidths from 2 to 8 weeks and confirms that our estimates are not sensitive to the bandwidth chosen. We provide an event study of these results in Section 5.3.

5.2 Retail Price Response

Table 3 reports estimates of Equation (1) for retailers. The estimate in Column (1), which includes no fixed effects, implies that the reform decreased the tax-exclusive price by 4.5% (significant at the 0.1% level). We include inventory lot fixed effects in Column (2)—our baseline specification. The estimates are very similar; the coefficient in Column (2) implies that the reform reduced tax-exclusive retail prices by 4.4% (significant at the 0.1% level). Combined with the rate change, this implies that tax-*inclusive* prices increased by 2.5%; retailers passed through roughly one-third of the tax to consumers. We find that we can reject the null hypothesis of TIV-consistent pricing behavior at the 0.1 percent level.

As firms might have taken time to adjust (and the Independence Day holiday may have generated temporary price adjustments), Column (3) repeats Column (2) for two week differences and drops the first week after the reform, so that the effect of the reform is identified from the difference between the week before and the week after the reform. The estimates are approximately the same, indicating that neither of these concerns play a large role. We

will return to a broader discussion of timing in Section 5.3.

Table 3 Column (4) repeats Column (2) with the dependent variable in levels—we estimate that average retail tax-exclusive prices fell by 41 cents per gram. This implies that retailers are an average of 41 cents per gram worse off on existing inventory as a result of the reform. On fresh inventory, firms were roughly 18 cents per gram worse off (41 less the estimated 23 cent decrease in manufacturer prices estimated in Table 2). In other words, under TIV this reform should have caused manufacturer and retail tax-exclusive prices to fall by 64 cents leaving profit and consumers unaffected. Instead, it caused smaller manufacturer price cuts leaving both retailers and consumers worse off.

Table 3 Columns (5) and (6) take an alternative approach to identification examining inventory lots only in their first week and only if retailers purchased the inventory lot from the manufacturer in that week. For this, we create a panel of retail-processor-strain-weight group-weeks. The estimates are quite similar—a 4.9% decrease in Column (5) versus a 4.4% decrease in Column (2), suggesting that retailers' price responses are largely unaffected by whether they are still selling inventory lots purchased pre-reform or selling new inventory lots purchased post-reform. Column (6) adds the first-differenced log manufacturer price. When included, the coefficient on the wholesale price is not statistically different from one and the coefficient on $\Delta TaxReform$ is now approximately zero. This suggests that retailers largely maintained a constant tax-exclusive markup. This is consistent with the pricing rule derived in Section 3. In other words, while retail behavior *as a whole* is inconsistent with TIV, after conditioning on the pass-through from manufacturers, retailers behaved, on average, in a way consistent with marginal-cost pricing (and therefore potentially consistent with TIV).

The bottom panel of Table 3 repeats each column in the top panel for a one-year-later

placebo reform. If our estimates are valid, the coefficient on $\Delta Placebo$ should be roughly zero—exactly what we find. Even in Column (1) where the estimate is marginally significant, the coefficient is very close to zero. The bottom panel of Figure A.1 considers bandwidths from 2 to 8 weeks and confirms that our estimates are not sensitive to the bandwidth chosen.

5.3 Event Studies

The analyses above indicate that prices changed at the time of the reform—yet it is possible that these changes were part of the ongoing evolution of the market, something that the placebo tests one year later cannot rule out. Moreover, the estimates above do not indicate whether there is additional adjustment towards TIV beyond the first week. To address these issues, we conduct event studies for both the manufacturer and retailer responses using our baseline specifications from Tables 2 and 3. For manufacturers, we do not drop the $t-1$ tax reform coefficient due to our varied difference lengths.[15] Figure 2 plots the relevant coefficients and confidence intervals.

In both event studies in Figure 2, there is no clear trend in prices pre-reform. The entire response happens in period t, the reform date. Given the varied difference lengths for manufacturers, this implies that manufacturers adjust their prices the first time they sell a particular retail-strain pair post-reform. This is compelling evidence that our estimates are unlikely to be driven by the ongoing market evolution and are instead a true response to the reform. The immediate nature of the response suggests that prices in this market follow a unit root, further supporting our first-difference specification. Moreover, this suggests that our results are not driven by learning in the short run and there is no evidence in Figure 1 to

[15] E.g., for a two week difference that spans t-1 to t+1, both the t and t+1 coefficients are relevant.

suggest substantial adjustments based on long-run learning either (Doraszelski et al., 2018; Huang et al., 2018).

Appendix Figure A.2 replicates the event study plots one year later, further emphasizing the placebo findings in previous sections—our identification strategy is effective in this setting when tested in other periods with similar cyclicality and holiday patterns. If one wanted a difference-in-differences design, one could subtract the placebo estimates from the main estimates in Tables 2 and 3 and take the same approach here for the event studies. The estimates would be very similar.

6 Discussion and Conclusion

TIV is a key component of tax policy design and analysis—it states that taxes may be collected at any point in the supply chain without concern as to the ultimate incidence. While the literature has documented cases in which TIV fails, these results have come with caveats driven by specific frictions or asymmetries present in the markets studied. We study a reform in a market with none of the these issues and show that TIV fails. A reform intended to be welfare-neutral or even welfare-enhancing had negative consequences for both retailers and consumers. This result is driven by manufacturers, who on average reduced prices significantly less than TIV would predict. Conditional on manufacturer prices, we find evidence that retailers applied a constant markup over marginal costs, consistent with our model.

These results are not likely driven by market power; the wholesale market is more competitive than the retail market and thus we would expect violations of TIV to be more likely

for retailers than for manufacturers. Similarly, if manufacturers employed average-cost pricing mechanisms (Hall and Hitch, 1939; Altomonte et al., 2015), we would expect the reform to cause similar or larger price drops than under marginal-cost pricing. While the reform eliminated incentives for inefficient vertical integration and, in the long run, production increased (Hansen et al., 2020), increased production efficiency should similarly drive down prices. The frequency of price changes—and the prevalence of at least some drop in manufacturer prices in response to the reform—suggest that managerial inattention is not relevant (Gabaix, 2019). Our event studies suggest the response is immediate, which decreases the likelihood that learning can explain our findings.

Others have found asymmetric firm behaviors in related settings. Benzarti et al. (2020) is particularly relevant—they find increases in value-added taxes are passed-through to consumers at twice the rate of decreases. In our setting, retailers, which experienced a tax increase, passed-through taxes in a way that is consistent with standard models of profit maximization, while manufacturers, which experienced a tax decrease, failed to pass-through their savings as predicted. Unlike the VAT context, however, our setting features a simultaneous change and a marketplace where firms and consumers are highly aware of relevant prices; furthermore both retailers and manufacturers engage in repeated transactions with each other over a long period of time. More broadly, the industrial organization literature has identified potential asymmetries in firm responses to changes in demand and costs (Butters et al., 2019; DellaVigna and Gentzkow, 2019).

We view our results as consistent with models that generate asymmetric responses to changes in market conditions due to behavioral phenomena, as opposed to information, transaction, or competitive frictions. In particular, anchoring and loss aversion may explain

the outcomes we observe (Kahneman et al., 1982, 1991; Bernheim and Rangel, 2009). While the modal response by manufacturers in the week of the reform was to adjust their prices, the default option of "doing nothing" by maintaining constant tax-inclusive manufacturer prices (and thus realizing a significant increase in variable per-unit profits) may have anchored their negotiations with retail firms. The relatively common and small changes in manufacturer prices we do observe may be a result of competition—manufacturers may "do something" if they incorporate quantity or reputation effects into their analysis of post-tax outcomes (Rotemberg, 2011) and competitors may be compelled to act as a consequence. In contrast, in aggregate, retailers may have overcame their default "do nothing" option (constant tax-inclusive prices) because this option represented a loss in variable per-unit profit. Once the default was overcome, they made decisions consistent with profit maximization.

Our findings have wide-ranging implications for tax policy. First, designers of new taxes may face welfare tradeoffs when choosing where in a supply chain to locate a tax. Both efficiency and equity considerations arise. When considering efficiency, variation in elasticities or competitive structures across the market may affect optimal tax placement. In terms of equity, if a policy goal is to ensure all market participants bear portions of the tax, it may be necessary to impose taxes on these different groups directly.

Second, policymakers considering changes to existing tax policy face greater consequences for doing so. While it may be possible to implement revenue-neutral reforms, restructuring will create clear winners and losers. In this case, manufacturers benefited—despite being in an arguably more competitive market—while retailers and consumers were harmed.

Taken together, these concerns point to broader political economy issues surrounding tax policy (Winer and Hettich, 2006; von Hagen, 2008). Political systems may be designed

to limit the ability of policymakers to enact tax reforms and thus rational actors may unknowingly design systems which have additional inefficiencies as described here. Indeed, in Washington state, the legislature may not reform measures passed by ballot initiative for two years after passage. Though local government officials knew from the moment of passage that the gross receipts tax was likely to have negative consequences on the market, their hands were tied. Flexibility in political and policy systems may help avoid these concerns—though at the cost of volatility and asymmetric responses.

Finally, these results demonstrate a need for further experimental and modelling work. Modern models of competition, growth, trade, inflation, and the business cycle generally make assumptions about taxes which are appealing from a tractability standpoint. These assumptions generally imply TIV (e.g. Judd, 2002; Melitz, 2003; Galí, 2015). Instead of failures of TIV being the exception, our work provides evidence that TIV simply may not hold in practice because of the ubiquitous nature of default options. In the absence of TIV, it may be necessary to conduct experiments which examine the way in which firms and consumers respond to tax policy and construct models which more accurately capture these responses.

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Tables

Table 1: Pre-Reform Retail Summary Statistics

Variable	Obs.	Mean	Std. Dev.
Prices and Taxes			
Tax-Inclusive Retail Price ($/g)	63,668	13.033	3.798
Tax-Exclusive Retail Price ($/g)	63,668	9.570	2.783
Probability of > 1% Retail Price Increase	63,668	0.17	0.375
Probability of > 1% Retail Price Decrease	63,668	0.204	0.403
Manufacturer Price ($/g)	63,668	4.103	1.309
Probability of > 1% Manufacturer Price Increase†	7,954	0.177	0.382
Probability of > 1% Manufacturer Price Decrease†	7,954	0.196	0.397
Retail State + Local Sales Tax Rate	63,668	1.089	0.006
Tax Revenue Pre-Reform ($/g)	63,668	4.489	1.246
Competition			
Market Share of Retailer in 10 Mile Radius	63,668	0.313	0.282
Market-level Manufacturer Market Share	63,668	0.014	0.016
Retail-Level Manufacturer Concentration Index	63,668	6.997	2.691
Benchmarks Assuming TIV			
Expected Tax Revenue Post-Reform ($/g)	63,668	4.104	1.200
Manufacturer Pass-Through Cents	63,668	−0.640	0.185
Manufacturer Pass-Through Percent Change	63,668	−0.177	0.058

An observation is an inventory-lot-week pre-reform. The data come from our retail analysis set and cover the six weeks prior to the tax reform. Tax revenue is calculated using both excise and state and local sales taxes. The retail-level manufacturer concentration index is calculated as follows: for a given retailer, we sort their suppliers by the weight of inventory sold, and count the number needed to comprise at least 75% of total sales. The "benchmarks assuming TIV" account for changes in the base and rate of the retail excise tax. The "manufacturer pass-through" statistics assume constant tax-inclusive retail prices and indicate the post-reform changes to manufacturer prices that would have left retailer variable-profit-per-gram constant.
† These probabilities are calculated for the subset of retail-processor-strain-weight group-weeks when the inventory lot changes (and thus a new purchase from a manufacturer has occurred).

Table 2: Manufacturer Price Response

	(1) Δlog(Price)	(2) Δlog(Price)	(3) ΔPrice
		Tax Reform	
ΔTax Reform	−0.065***	−0.072***	−0.228***
	(0.015)	(0.018)	(0.068)
Observations	12,087	12,087	12,087
Manufacturer Firms	199	199	199
P-Value for Test of TIV-Predicted Pass-Through	0.000	0.000	0.000
		Placebo	
ΔPlacebo	0.001	0.000	0.014
	(0.012)	(0.014)	(0.040)
Observations	21,288	21,288	21,288
Manufacturer Firms	180	180	180
Bandwidth	6 weeks	6 weeks	6 weeks
MRS FE?	No	Yes	Yes

This table reports estimates of Equation (1) – other variables in that equation are included, but not reported. An observation is a manufacturer-retailer-strain-week. The outcome is the change in the log of the price per gram charged by the manufacturer to the retailer (except for in column (3) which is the same outcome, but not logged). MRS stands for manufacturer-retailer-strain fixed effects. The estimates are weighted by the total grams sold across the two weeks of the difference. The P-value tests the null hypothesis that the estimated pass-through is equal to that predicted by TIV. For the placebo regressions, we repeat the analysis one year later. These regressions are estimated with `reghdfe` in Stata. Standard errors twoway-clustered by manufacturer and retailer are in parentheses (Cameron et al., 2011). *5% significance level. **1% significance level. ***0.1% significance level.

Table 3: Retail Tax-Exclusive Price Response

	(1) Δlog(Price)	(2) Δlog(Price)	(3) Δlog(Price)	(4) ΔPrice	(5) Δlog(Price)	(6) Δlog(Price)
			Tax Reform			
ΔTax Reform	−0.045***	−0.044***	−0.046***	−0.413***	−0.049**	0.011
	(0.006)	(0.007)	(0.006)	(0.065)	(0.018)	(0.017)
Δlog(Manufacturer Price)						0.887***
						(0.084)
Observations	145,357	145,357	145,357	145,357	11,265	11,265
Retail Firms	110	110	110	110	110	110
Implied Tax-Inclusive Price Change	0.024	0.025	0.023	0.230	0.020	0.080
P-Value for Test of Constant Tax-Inclusive Price	0.000	0.000	0.000	0.000	0.270	0.000
			Placebo			
ΔPlacebo	−0.006*	−0.004	0.001	−0.029	−0.016	−0.004
	(0.003)	(0.003)	(0.002)	(0.017)	(0.012)	(0.009)
Δlog(Manufacturer Price)						0.642***
						(0.053)
Observations	253,123	253,123	253,123	253,123	11,534	11,534
Retail Firms	106	106	106	106	105	105
Bandwidth	6 weeks	6 weeks	6 weeks	6 weeks	6 weeks	6 weeks
MRS FE?	No	No	No	No	Yes	Yes
Inventory Lot FE?	No	Yes	Yes	Yes	No	No
Difference Length	1 week	1 week	2 weeks	1 week	1-4 weeks	1-4 weeks
Restricted to First Week Sales?	No	No	No	No	Yes	Yes

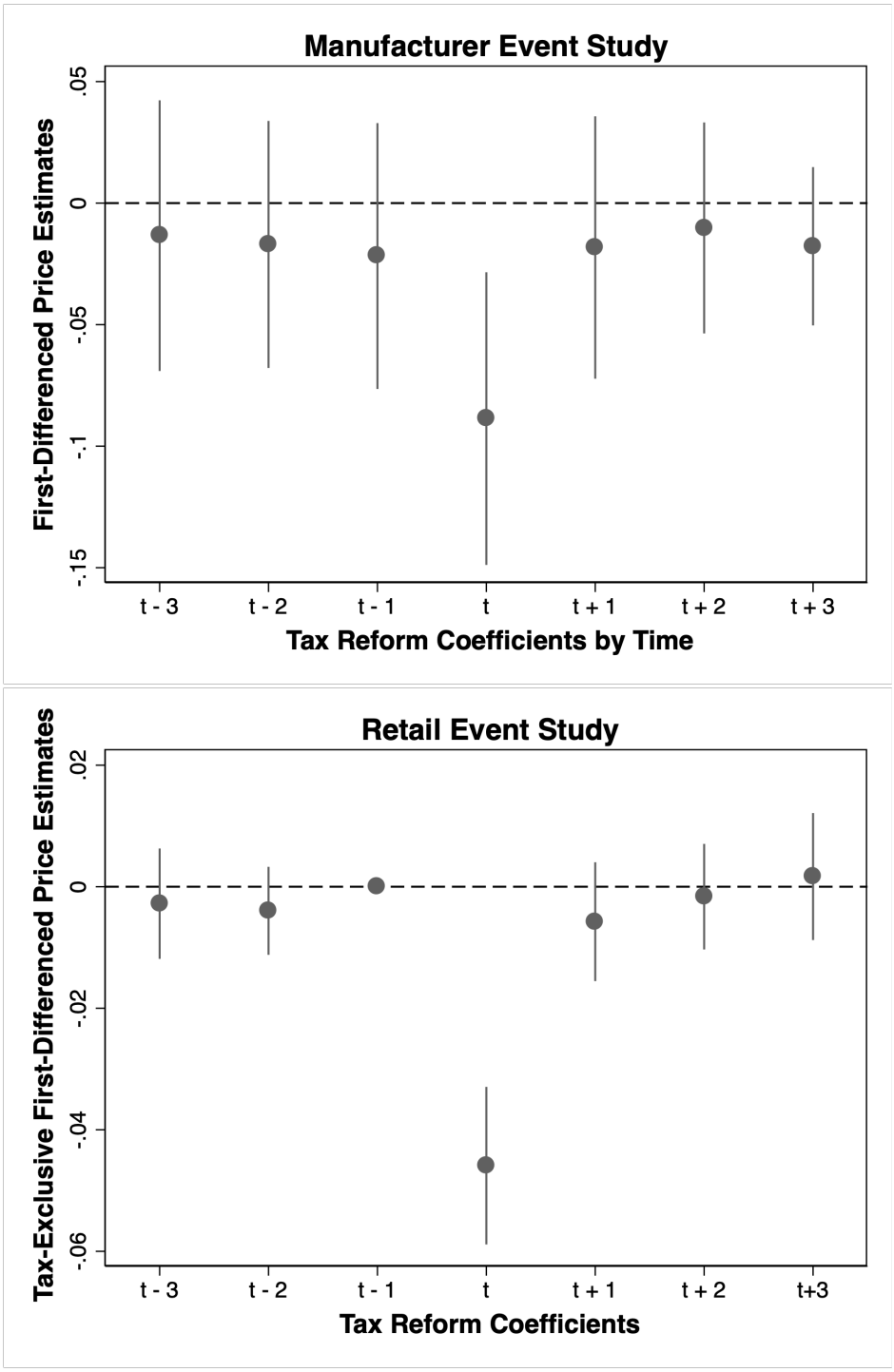
This table reports estimates of Equation (1) – other variables in that equation are included but not reported. An observation is an inventory-lot-week. The outcome is the log of the tax-exclusive price per gram charged by the retailer to consumers (except for in column (4) which is the same outcome, but not logged). MRS stands for manufacturer-retailer-strain fixed effects. The estimates are weighted by the total grams sold in the first week of the difference. The P-value tests the null hypothesis that the tax-inclusive price remained constant as predicted by TIV. For the placebo regressions, we repeat the analysis one year later. These regressions are estimated with reghdfe in Stata. In the last two columns we only include observations in their first week of being sold at retail and only if the cannabis was also purchased from the processor in that same week. Standard errors twoway-clustered by manufacturer and retailer are in parentheses (Cameron et al., 2011). *5% significance level. **1% significance level. ***0.1% significance level.

Figures

Figure 1: Retail and Manufacturer Prices

This figure plots average prices in Washington's cannabis industry for four months before and after the tax reform, normalized to 100 in the week before the reform. For each week, we take inventory lots in their first week of sale and match them with the price paid to the manufacturer, restricting observations to those for which the first retail sale and manufacturer sale both happened pre- or post-reform (before any applicable taxes are paid from the manufacturer to the government). This therefore illustrates the relation between retailer per-gram revenue and variable costs. The left dashed line in the figure marks 4/20 (an industry promotional period) and the right dashed line marks the week before the tax reform.

Figure 2: Manufacturer and Retail Price Event Study

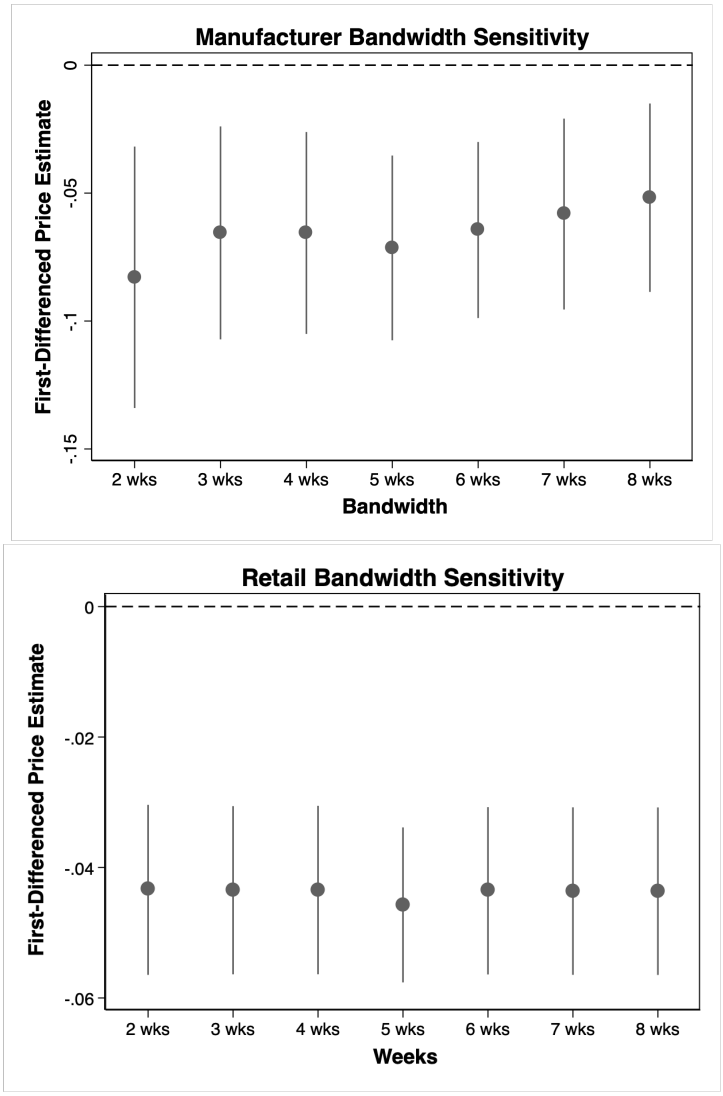


This figure plots estimates of Table 2 Column (2) (top panel) and Table 3 Column (2) (bottom panel) with additional leads and lags of $\Delta TaxReform$. The plotted coefficients are leads and lags of $\Delta TaxReform$. We include in the regression (but do not plot) leads and lags are for periods $t-4$ and before and $t+4$ and after as is standard in event study designs. The dots indicate the point estimates and the lines indicate 95% confidence intervals. See the notes for Tables 2 and 3 for regression details.

Appendices

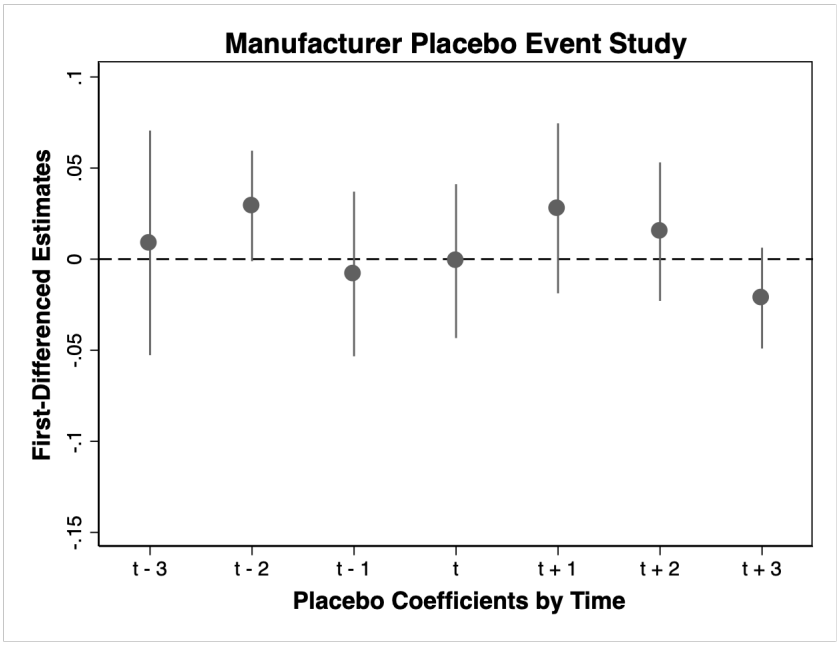
A Appendix Figures

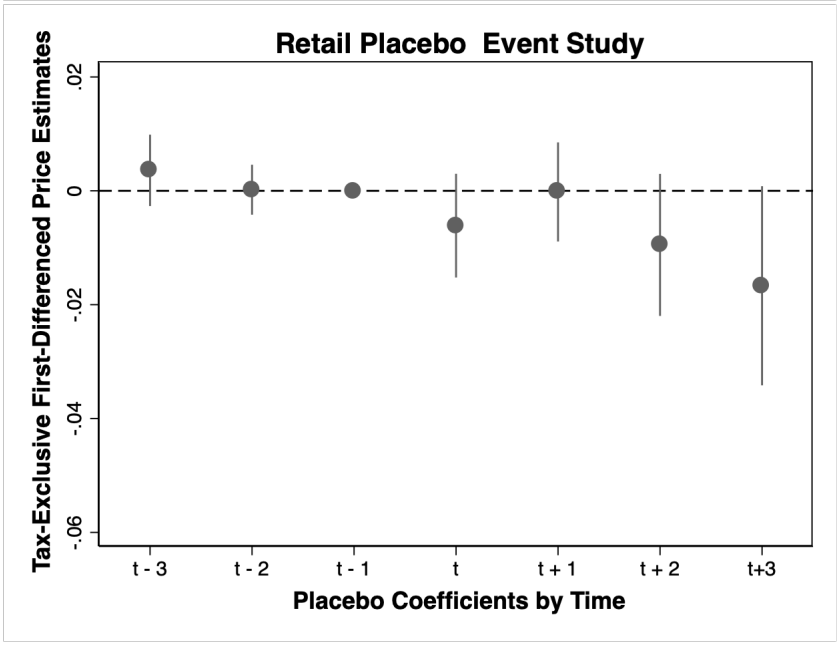
Figure A.1: Manufacturer and Retail Price Bandwidth Choices



This figure plots estimates of Table 2 Column (2) in the top panel and Table 3 Column (2) in the bottom panel, varying the bandwidth. The bandwidth in our baseline specifications is 6 weeks. The estimates plotted are for the coefficient on $TaxReform$. The dots indicate the point estimates and the lines indicate 95% confidence intervals. See the notes for Tables 2 and 3 for regression details.

Figure A.2: Manufacturer and Retail Price Event Study





This figure plots placebo estimates of Table 2 Column (2) (top panel) and Table 3 Column (2) (bottom panel) with additional leads and lags of $\Delta Placebo$. The plotted coefficients are leads and lags of $\Delta Placebo$. We include in the regression (but do not plot) leads and lags are for periods $t-4$ and before and $t+4$ and after as is standard in event study designs. The dots indicate the point estimates and the lines indicate 95% confidence intervals. See the notes for Tables 2 and 3 for regression details.

B Data Cleaning Appendix

In this appendix we detail our data cleaning procedure. We begin by detailing our methods for cleaning prices in the face of changing reporting requirements and tax rates. We then discuss other cleaning steps to transform the raw data into the set used in our analyses.

The retail sales prices reported by firms in the "seed-to-sale" traceability system were supposed to be all-tax-inclusive pre-reform and tax-exclusive post-reform. However, compliance varied from firm to firm and changed over time. For example, some firms reported prices with the sales tax included and some reported prices without the sales tax.

This reporting confusion means that we must infer, for each firm, how they reported their prices and therefore the true tax-inclusive and tax-exclusive prices they charged. For each firm-week, we assign a "multiplier" that reveals the relationship between the reported price and the price faced by consumers. This chosen multiplier is selected from a set of multipliers based on possible tax rates for the firm. We merge in the state and local sales tax rates for each firm in order to construct this choice set.[16] To understand the relationship between the multiplier, reported prices, and faced prices, consider the following equation:

$$Price_{Consumer} = Price_{Reported} \times Multiplier$$

We algorithmically determine which tax-based multiplier makes the prices faces by consumer's ($Price_{Consumer}$) most round for each week, where roundness is the closeness of the price to a 25 cent increment of a dollar. For each product type, $Price_{Reported}$ is the modal observed price for the week, where idiosyncratic discounts have been removed.[17]

We consider two orthogonal methods of determine the proper set of multipliers. Our results are robust to the method used. Ultimately, we find the modal firm never included

[16]For five firms, the state and local tax rates do not match the rates they are using, so we adjust these. And a few firms do not ever change their local tax rate for reporting purposes—we make that adjustment as well. This transforms these firms from very unround to very round, but otherwise has approximately no effect on the data as the difference between the statutory and reported local tax rates is very small.

[17]We determine that a price is a one-off discount if the price for that transaction is 5% to 95% (in increments of 5 percentage points) or 33%/66.67% less than the previously reported price.

the sales tax, included the excise tax pre-reform, and excluded the excise tax post-reform.

Cash Market Identification In order to determine how each firm reports their prices in the traceability system, we take advantage of two characteristics of retail prices. First, publicly advertised prices (or 'list' prices) are nearly universally all tax-inclusive. Second, retailers nearly always choose to set prices in whole-dollar or (rarely) quarter-dollar increments.[18] We use these two facts to determine the difference between the list prices faced by consumers and the prices reported in the traceability system.

We assign each firm a multiplier before and after the tax change. We begin by assigning the modal firm's multiplier choices to all firms—all firms' prices were adjusted by the state and local sales tax pre-reform and all firm's prices were adjusted by the excise tax plus state and local sales tax post-reform. We then make the following adjustments based on the results from our algorithm:

1. We leave prices unadjusted (i.e. a multiplier of 1) where are algorithm finds that this choice maximizes roundedness and at least 85 percent of weekly sales are round with this multiplier choice.[19] This applies to 16% of firms.

2. We adjust the multiplier post reform to account for only the excise tax when the algorithm finds that this choice maximizes roundness and at least 85 percent of weekly sales are round with this multiplier choice. This applies to one firm (out of 110).

   There are three additional firms for whom an only excise tax adjustment makes them most round, but their roundness in the immediate post period is less than 85 percent. We leave two of the firms alone because they were also left alone in the pre-reform period because of unroundness and we could either adjust them both before and after

[18] We verified this through conversations with retailers as well as using historical menus available through The Internet Archive and a full set of menus for almost all firms we took screen shots of on 7/18/2017.

[19] For the 4.5% of firms that suggest the multiplier could be 1 but are quite unround, there is too much uncertainty to confidently make an adjustment. Leaving these firms' multipliers unchanged, if wrong, will bias our estimates towards our main null hypothesis in the retail section of the paper—that firms did not adjust their prices in response to the reform.

the reform or leave them both alone with similar effects to the log price change. The third firm becomes more round a few weeks after the reform and keeps this multiplier through the end of our data (and we have confirmed the multiplier in the menu screen shots), so we make this multiplier adjustment.

3. There are two firms for whom the multiplier that makes them round post-reform is the excise tax + state and local sales taxes divided by the state and local sales tax rate. In both cases, we have clear evidence that this is because they adjusted their prices post-reform by making their prices sales-tax exclusive post-reform. One firm keeps this choice permanently and we see this in the menu screen shots at the end of our data. The other firm eventually adjusts to the modal firms' multiplier. Our assumption keeps prices roughly constant through this reporting change.

Product Batch Price Stability To provide additional evidence that our multiplier decisions are not systematically biasing our estimates, we consider a completely different mechanism for determining multipliers—we pick the set of multipliers that makes the tax-inclusive prices for the most number of inventory lots for a given firm the same pre- and post-reform.

There are a couple of reasons why this is a reasonable alternative to consider. A number of inventory lots did leave prices constant in response to the tax reform and the main null hypothesis in our retail analysis is that firms did not change their tax-inclusive prices—this is what we would expect if the tax reform was indeed tax invariant.

We consider two variants of this. One is to begin with the modal firms' multipliers and adjust it to another multiplier if it decreases the number of price changes by any margin. The second variant is to begin with our estimates based on roundedness and then make adjustments for firms that under the best set of multipliers leaves at least 25% of their inventory lots constant in response to the reform. The latter changes the multipliers for only four firms and three of those four leave the percent price changes quite similar. The former method decreases our baseline estimate by 0.4 percentage points and the latter decreases

our baseline estimate by 0.2 percentage points. This evidence strongly supports our price cleaning methods and suggests that any remaining bias is extremely small.

Additional Cleaning

In addition to adjusting retail prices, we also drop some extreme outliers in the data. In particular, we drop all wholesale transactions with a usable weight above 2,500 grams[20] and all retail transactions if the usable weight was above 28.5 grams.[21] We also drop all wholesale or retail price per grams above $42.[22] We censor the THC content data if it is zero or above 40 in both the manufacturer and retailer data.[23] We also drop wholesale prices less than $1. This effectively drops samples from our data, which are sold well below market value. We typically see these as the first recorded sale from a parentlot.

Lastly, we drop some firms or firm-days in our data set. In particular, we require for each firm that the first sales transaction occurs two months before the tax reform and continues to have transactions through the two months after the reform (either because they had not yet opened, had closed, or because they took a long hiatus from selling any cannabis). A few retailers conduct a "soft opening" by opening briefly, closing for more than a month, and then re-open permanently. In these cases, we drop the first brief selling period and consider their first activity date the first date upon re-opening in our data. We also drop 20 retail firms for whom at some point in the 8 weeks before or after the reform report their data only once per day—this is a clear indicator of poor overall data quality and, because of this, determining the appropriate multipliers for these firms is difficult.

[20]This is about 0.025% of wholesale transactions.
[21]This is because the maximum legal sale was one ounce. This step drops 0.15% of retail transactions.
[22]This is less than 0.03% of wholesale transactions and less than 0.04% of retail transactions.
[23]This affects 0.2% of wholesale transactions and 5% of retail transactions.