Shifting Tax Incidence: Evidence from the Washington State Cannabis Market

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Abstract

We study how prices respond when a 25 percent tax remitted by cannabis manufacturers was eliminated in Washington state and the retail excise tax was simultaneously increased from 25 to 37 percent—a shift intended to be revenue-neutral. We construct a simulated model of our setting and are able to accurately predict other firm adjustments, but fail by a wide margin to predict the observed price responses. Instead of passing along most of their savings to retail firms, manufacturers kept two-thirds of their tax savings. Retailers passed one-third of their tax increase on to consumers instead of cutting their prices as predicted by the model. These changes shifted tax incidence away from manufacturers, towards retailers and consumers.

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1 Introduction

When tax rates or systems change, there is increasing evidence that prices respond in surprising ways that are not predicted by classical models. Firms don't capture Prius tax subsidies even though supply is inelastic (Sallee, 2011) and increases in value-added taxes are passed-through to consumers at twice the rate of decreases (Benzarti et al., 2020). Prices don't respond to tax regime changes as predicted when evasion opportunities vary along the supply chain (Slemrod, 2008; Kopczuk et al., 2016; Brockmeyer and Hernandez, 2016), when prices are rigid (Muysken et al., 1999; Saez et al., 2012; Lehmann et al., 2013), or if tax salience differs between consumers and firms (Chetty et al., 2009; Finkelstein, 2009).

Our paper adds to this literature by examining a tax reform that occurred in the cannabis market in Washington state. Prior to July 1, 2015, a 25% gross receipts tax applied to each transfer in the market. Producers remitted the tax when they sold to manufacturers, manufacturers were taxed on sales to retailers, and retailers were taxed on sales to consumers. After July 1, the retail tax was 37% and all other taxes were eliminated. This change was unexpected by market participants – the reform was passed on June 27 and signed by the Governor on June 30 (La Corte, 2015).

We measure the effects of this reform empirically 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) if the reform had not occurred. We conduct event studies and placebo permutation tests which provide no evidence to reject this assumption.[1]

We compare the results of this exercise to a model of the industry that extends the model of Hansen et al. (2022) (which was previously used to successfully predict the effects of this reform on vertical integration decisions within the manufacturing supply chain) to incorporate a retail sector. Our model incorporates key features of the setting including market power (Mace et al., 2020; Hollenbeck and Uetake, 2021) and percentage taxes. Following Hansen et al. (2022), we calibrate the model using data from the pre-reform period, and then simulate the effects of the reform. The model predicts that manufacturers should decrease their tax-inclusive price by 25.3% from pre-reform levels. We find that manufacturers reduce their prices by only 7.2%.

While the incidence of the tax shifts substantially more away from manufacturers than predicted by the model, this is not universally true – about 8 percent of manufacturer-retail-strain pairs are within 10 percent of the model predictions. Splitting the manufacturer tax break 50-50 is not predicted by the model, but is more common in practice – almost 20 percent of manufacturer-retail-strain pairs are within 10 percent of this choice. Previous papers have examined explanations for shifts in incidence not predicted by models (i.e. a failure of tax invariance). Some of those explanations do not apply in our setting. Evasion opportunities don't vary along the supply chain in our setting because frequently-audited comprehensive regulatory reporting system makes tax evasion difficult everywhere in our setting (Slemrod, 2008; Kopczuk et al., 2016; Brockmeyer and Hernandez, 2016). When tax salience differs between consumers and firms this can play an important role (Chetty et al.,

[1] 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 study period is likely much stronger than the assumptions we impose.

2009; Finkelstein, 2009), but in our setting both sides of the market are firms who are both experiencing tax changes simultaneously. Prices change often (and typically in increments smaller than the predicted response), implying rigidities are unlikely (Muysken et al., 1999; Saez et al., 2012; Lehmann et al., 2013) and quarter or dollar adjustments do not have statistically different effects on the average response.

One explanation that might be particularly important in our setting is the broader price adjustment scene – could be using this as a good time to adjust, could be realizing they just made a large adjustment a couple of weeks before, etc. We do find evidence that this matters. DESCRIBE.

We then examine retail behavior. Our model predicts that retailers should respond to the reform by decreasing their tax-inclusive retail prices by 14.0%. We find that, instead, tax-inclusive prices *increased* by an average of 2.5%. In other words, retailers pass-through one-third of the tax increase to consumers. Another roughly one-third is borne by manufacturers, leaving retailers with about one-third of the increase. We find evidence that retailers maintained constant tax-exclusive markups, which is consistent with our model's pricing rule conditional on manufacturers' behavior.

In summary, our results are inconsistent with our model. The change in the tax regime should have increased welfare for both manufacturers (largely through production efficiencies as discussed by **?**), retailers (through incompletely passed-through cost savings), and consumers (through lower retail costs). Instead, manufacturers captured a larger increase in welfare, at the expense of retailers and particularly consumers, who paid higher tax-inclusive prices post-reform. We conclude by discussing potential mechanisms for this result, implications for policymakers, and future research.

2 Background

We analyze the adult-use cannabis market in Washington state, which opened in July 2014. We have written elsewhere about the history of this market (Hansen et al., 2020; Miller and Seo, 2021)—here we focus on features of the market and the reform underpinning our analysis.

The 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. 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., 2020b). Thus, we focus our analyses on two types of firms, "manufacturers" and "retailers".[2]

The market is closed: 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. The collected data can be used to check for tax evasion: retailers cannot sell cannabis without manufacturing records, which forces manufacturers to

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

report accurately.[3] Reporting is enforced through frequent audits—firms typically face one or more visits per year—backed by significant penalties.[4]

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 at retail 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 average 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 during 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 reporting portrayed the industry as unprepared. According to one retail manager, "[we] have a few

[3]Retailers can under-report sales, but such behavior is detectable by comparing retail sales and wholesale purchases. Our estimates are unaffected by dropping the few retailers that under-report substantially.

[4]We observe audit violations and our estimates remain approximately the same if we drop firms with under-reporting violations. Moreover, our results are not consistent with asymmetric tax evasion. Manufacturers would pass along less tax savings if retailers are more effective tax evaders, but in that case, retailers would still not pass along any of the tax to consumers.

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

hours to change an entire market's pricing structure. It is an exceptionally short window for such a tremendous change" (La Corte, 2015).

3 Model

In this section, we develop benchmark responses to the tax reform against which we compare our empirical findings. We do so by extending the model, calibration, and prediction exercise of Hansen et al. (2022), which we refer to as HMW, to include retail firms. In the interests of brevity and as the rest of the model is identical, we proceed by summarizing that work and then detailing the additional equilibrium conditions generated by the retail sector.

3.1 A summary of HMW

HMW's model is a stylized model of a supply chain in the style of Weyl and Fabinger (2013). In the model, firms may engage in either or both of two activities: producing intermediate goods from raw materials (which in our context refers to the growth and harvesting of plant material) and processing intermediate goods into final goods (which in our context refers to the processing plant material into usable cannabis products). There are two markets: a market for intermediate goods and a market for final goods — in the style of a standard double-marginalization problem, the demand for final goods is taken to be exogenous while demand for intermediate goods is endogenous. Within each market, goods are assumed to be identical, though firm behavior in each market is characterized by an exogenous firm-specific *conduct parameter*: under the *conduct condition*, firms set prices to equate their elasticity-and-tax-adjusted Lerner index equal to their conduct parameter.[6]

HMW then focus on *interior equilibria* in which each firm participates in both markets as firms in this setting generally do engage in both activities. Several equilibrium conditions come from the demand equation, market clearing, and the conduct condition for firms selling final goods. The remaining conditions come from the intermediate goods market: the opportunity cost for firms selling intermediate goods in that market consists of the lost revenue that could have been earned by conducting the processing step within the firm and selling additional final goods instead. In equilibrium, these marginal revenues must be equal. Similarly, firms buying intermediate goods must be indifferent between paying the market price and producing the goods internally.

Next, HMW calibrate an instantiation of the model with two firms and quadratic costs. This implies that one firm will sell goods in the intermediate goods market and the other will buy those goods. After assuming that the conduct parameters in the final goods market are homogenous across firms and the conduct parameter in the intermediate goods market is zero (i.e. firms are price takers in that market), the model has seven parameters: two demand parameters, four cost parameters, and a single conduct parameter. They calibrate the model using moments derived from the pre-reform data. Despite the parsimonious nature of the model, it performs well: at the calibrated parameters, model moments are within roughly 1% of the data moments.

With the calibrated moments in hand, HMW generate hypotheses about outcomes in the post-tax-reform market by simulating the effects of the reform. They then compare

[6]Weyl and Fabinger (2013) show that when products are weak substitutes, the conduct parameter in models of profit maximization ranges from 0 (perfect competition) to 1 (monopoly).

these predictions to empirical estimates of the outcomes. To summarize: the model predicts that the level of vertical integration (measured by the log fraction of cannabis sold that is produced in a vertically integrated process) would decrease by 2.2%; the empirical estimate is 2.3%. The model predicts that the firms engaged in selling intermediate goods would increase their production of those goods by 30.6%; the estimate is 31.2%. Finally, the model predicts that the firms engaged in buying intermediate goods would increase their production of intermediate goods by 1.8%; the estimate is 4.1% (though the confidence interval overlaps). They conclude that this simple model of the supply chain is is able to capture outcomes in the market "remarkably close[ly]" (Hansen et al., 2022, pg. 19).

3.2 Incorporating a supply side

The results of HMW indicate that their model can serve as a baseline from which to build predictions of retail outcomes. Doing so simply requires adding retail firms to the model. This is made more straightforward by the feature of Washington's cannabis market which requires that retail firms be owned and operated entirely independently from other firms; in other words, we can introduce retail firms in the model without affecting the equilibrium conditions in the market for intermediate goods. For brevity, we adopt the notation of HMW wholesale and make reference to certain equations contained therein.

Suppose there are n_r retailers, denoted by u, engaged in imperfect competition as specified below. Each produces a single retail product and faces a constant identical marginal cost of p_f, the tax-inclusive price charged by wholesalers. Each also faces a tax τ_r implemented as an excise tax: the unit net-of-tax revenue earned by the retailer is $p_r/(1+\tau_r)$. Market demand is smooth and is given by the demand function $p_r = D_r(q_r)$, where p_r is the price of the final good at retail and $q_r \equiv \sum_u q_{ru}$ is the total quantity of final goods sold at retail. Let ϵ_{D_r} be the price elasticity of demand, which is assumed to be greater than unity. Following Weyl and Fabinger (2013), we model imperfect competition by assuming the elasticity-and-tax adjusted Lerner index for each firm is set equal to a conduct parameter θ_r. That is,

$$\frac{p_r - (1+\tau_r)p_f}{p_r}\epsilon_{D_r} = \theta_r. \tag{1}$$

To integrate this retail market into HMW's model, we must first show that the implicit demand for final goods has a well-defined price elasticity. Suppose the retailers purchase their products from n upstream firms, denoted by i. Note that Equation (1) implies that for any p_f, there is some p_r and therefore some demand from consumers q_r. Thus, the price elasticity of demand faced by the upstream firms is $\epsilon_D \equiv \frac{dq}{dp_f}\frac{p_f}{q} = \frac{dq}{dp_r}\frac{dp_r}{dp_f}\frac{p_f}{q}$. Note that $\frac{dq}{dp_r} = \epsilon_{D_r}\frac{q}{dp_r}$. Rewrite Equation (1) to obtain $p_r = \frac{\epsilon_{D_r}}{\epsilon_{D_r}-\theta_r}p_f(1+\tau_r)$ so $\frac{dp_r}{dp_f} = \frac{\epsilon_{D_r}}{\epsilon_{D_r}-\theta_r}(1+\tau_r)$. Therefore,

$$\epsilon_D = (1+\tau_r)\frac{\epsilon_{D_r}}{\epsilon_{D_r}-\theta_r} \cdot p_f\frac{\epsilon_{D_r}}{p_r}. \tag{2}$$

The addition of a supply side adds one element to the equilibrium vector described in HMW, the retail price p_r, and one element to the parameter vector: θ_r. Equation (1) provides an additional equilibrium condition so there are as many conditions as equilibrium objects, with ϵ_D defined as above.

We proceed by calibrating the model following HMW: the calibration environment features two upstream firms who face quadratic costs and one retailer. We use the moments of HMW with the addition of the average tax-exclusive price-per-gram charged by retailers to consumers in the pre-reform period. The results of our calibration are reported in Table 1, which is analogous to Table 3 of HMW. Panel (a) reports the calibrated values of the parameters, while Panel (b) reports the moments in the data and in the calibrated model. Just as in HMW, the model is able to closely match the moments: each model moment is within approximately 1% of the data.

4 Data and Methods

Our data consist of records from the "traceability" 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 form-factor (loose or pre-rolled). These characteristics are captured by our fixed effects.

We aggregate retail sales by inventory-lot-week, where an inventory-lot is a batch of identical product. 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 data to reflect the prices faced by consumers using an algorithm based on retail rounding behavior verified by spot checks of historical menus.[7] See Appendix B for details.

Table 2 reports summary statistics for retail 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

[7]Cannabis retailers set tax-inclusive prices that are round numbers (e.g. $8 or $10.25). While this represents a potential friction, the effective minimum price change is smaller than the effects we estimate.

and the tax-exclusive price was $9.57 per gram. Retailer tax-exclusive prices are more than double manufacturer tax-inclusive prices.

The average market share of retailers in the 10-mile radius around their location was 31%, suggesting that there is substantial retail market power (Mace et al., 2020; Hollenbeck and Uetake, 2021). The manufacturer market is effectively state-wide; the average market share is 1.4% and none exceeds 7%. However, manufacturers may exert market power on individual retailers. We construct a retail-level manufacturer concentration index by sorting each retailer's suppliers by the weight of inventory sold and counting the number of manufacturers that comprise at least 75% of sales. On average, seven manufacturers supply 75% of a retailer's inventory.

4.1 Empirical Specification

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

$$\Delta log(p_{it}) = \alpha_0 + \alpha_1 \Delta TaxReform_t + \alpha_2 FE_i + u_{it}, \tag{3}$$

where i is the unit of observation as described for the manufacturer and retail analyses 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.[8] Because all firms are treated and generally included in our analysis, this parameter is an ATT. Our analysis window spans six weeks before and

[8]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.

after the reform.[9] We two-way cluster standard errors on manufacturer and retail location (Cameron et al., 2011).[10] Our identifying assumption is that within a given product, there are no shocks in 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.

Our identifying assumption is much more likely to hold in our setting than the assumption made in a classic difference-in-differences design – that, for a lengthy period (e.g. a year), two states would experience the same systematic price shocks in the cannabis market. In general, it's not clear which of these two would be more likely to hold *a priori*, but in this setting, it is very unlikely that the latter would hold because each state is a closed market and there are substantial differences in market regulatory structures, processes and outcomes.Moreover, we will provide evidence through placebo analyses and event studies that we have no reason to reject the null hypothesis that our identifying assumption is valid. When we use the same data to examine another natural experiment that allows for the addition of a comparison group to our RDIT design[11] (i.e. a difference-in-discontinuities regression) (Hansen et al., 2020a), we found the estimates remained quantitatively and qualitatively similar.

Our implementation of interrupted time series, also known as regression discontinuity in time (RDIT), addresses critiques previously raised against this method (Hausman and Rapson, 2018). We select a narrow bandwidth (measured in weeks, not years) and we estimate the regression in first-differences rather than levels; this, along with our event study figures, allows us to precisely pin down the response in the week of the reform, rather

[9] We demonstrate our estimates are robust to this choice in Figure A.5.
[10] Firm clusters or two-way clusters on firm and week yield similar standard errors.
[11] Washington firms near the Oregon border were "treated" while those away from the border were not.

than allowing the regression to obtain part of its identification from shifts that may happen many weeks away from the reform. In addition, we address autocorrelation over time with firm-level standard error clustering, we aggregate at the weekly level to avoid challenges associated with estimating day-of-week fixed effects, and we include fine-grained fixed effects to address any compositional shifts over time.

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. Retailer-manufacturer-strain fixed effects 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 a retail inventory lot. Retail sales from a given 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 weeks from the first sale out of that inventory lot. Inventory-lot fixed effects 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 manufacturer

[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.

analysis, we aggregate by retailer-manufacturer-strain and take varied length 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 3 reports estimates of Equation (3) for manufacturers. Column (1) estimates a 6.5% price decline in response to the tax reform. When we add manufacturer-retailer-strain fixed effects in Column (2) – our baseline specification – the point estimate becomes -7.2% (p-value: 0.000). This is roughly one-third of the 17.7% price decrease needed to preserve retailer per-gram profits (and therefore to minimally 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 (p-value: 0.000). Column (3) repeats Column (2) for the price in levels instead of logs – the reform decreased manufacturer prices by 23 cents, about one-third of the 64 cent benchmark. Table 3 Column (4) aggregates the data by months instead of weeks and we find the estimates are very similar with smaller standard errors. This aggregation is an alternative way to address manufacturers not selling every strain they produce to every retailer every week, and also allows us to examine a longer-term response.

The bottom panel of Table 3 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.[14] One could subtract the placebo estimates from the main estimates to create a differences-in-RD design; the estimates would be very similar.

It is possible that our rejection of TIV is driven by price stickiness or that the dampened response we observe (relative to the TIV benchmark) is driven by the sheer magnitude of the TIV-consistent response relative to typical price shifts in this market. Figure 1 provides evidence of price mobility by plotting the entire distribution of weekly price changes for each retail-manufacturer-strain pair in the baseline estimate as a histogram in the top panel (and monthly price changes in the bottom panel). The period of the tax reform is marked by the hollow green histogram and surrounding periods are marked by the gray histogram. The width of each bin is 0.04, so all price changes within 2% of zero are included in the bin centered around zero. The dashed lines from right to left indicate: full pass-through predicted by models of firm behavior (-28.7%), our alternative benchmark of pass-through (-17.7%), and zero (i.e no price change).

These histograms illustrate several things. First, we see the shift in the distribution in the period of the reform. Note that there is a substantial increase in pass-through all the way down to the full pass-through benchmark. While TIV fails in aggregate, there are retail-manufacturer-strain tuples for which TIV appears to hold in isolation. Second, the histograms highlight that large price shifts do occur with reasonable frequency in the absence of the reform – about 16 percent of retail-manufacturer-strain tuple weekly price changes

[14]In addition, we estimate many placebo regressions and construct a placebo permutation test for manufacturers in the top panel of Figure A.4. There are no placebo estimates as extreme as our estimate and the implied p-value is 0.012.

were at least as large as our alternative benchmark. This statistic rises to almost 20 percent at the monthly level. Moreover, less than 10 percent of retail-manufacturer-strain tuples do not adjust their prices in the period of the reform. Even if we rescaled our estimate assuming that any observation with minimal adjustment in this period was caused by rigidities or lack of awareness, the data would still reject the null hypothesis of TIV.[15]

Figure 1 is consistent with our identification assumptions and therefore provides additional evidence of the validity of our regression estimates – the price shifts are concentrated in the region one would expect. That is, there are substantial increases in the distribution of price decreases in the period of the reform in each of the bins up to our full pass-through benchmark, and much less beyond that benchmark. As expected, this can be seen most cleanly in the top panel for weekly price changes. If the response we estimate were partially attributable to some more generic market shift in that same week, there is no reason to think that the price shifts would have this particular pattern.

5.2 Retail Price Response

Table 4 reports estimates of Equation (3) for retailers. The coefficients are very similar in Columns (1) and (2). The coefficient in Column (2) – our baseline specification – implies that the reform reduced tax-exclusive retail prices by 4.4% (p-value: 0.000). 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 can reject the null hypothesis of TIV-consistent pricing behavior (p-value: 0.000).

[15] However, this does not rule out additional adjustments by many firms over a longer horizon. To fully assess whether there is evidence of a longer-run adjustment towards TIV, we consider event studies in Section 5.3.

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 4 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. Thus, retailers are an average of 41 cents per gram worse off on existing inventory as a result of the reform. On fresh inventory, retailers were roughly 18 cents per gram worse off (41 less the estimated 23 cent decrease in manufacturer prices estimated in Table 3). 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 4 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 suggesting retailers' price responses are largely unaffected by whether they are selling inventory lots purchased pre-reform or selling new inventory lots purchased post-reform. Column (6) adds the first-differenced log manufacturer price; the coefficient on the wholesale price is not statistically different from one and the coefficient on $\Delta TaxReform$ is now approximately zero. This suggests retailers largely maintained a constant tax-exclusive markup.[16] This is consistent with the adjusted Lerner pricing rule of Equation (1). Hence, while retail behavior *as a whole* is inconsistent with TIV, after conditioning on the pass-through from manufacturers, retailer behavioris consistent with marginal-cost pricing (and therefore potentially consistent with TIV).

In the bottom panel of Table 4 and in the bottom panel of Figure A.4, we repeat the placebo analyses we perform for our manufacturer sample for the retailer sample. These analyses provide no evidence of bias in our estimates.

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. 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 3 and 4. For manufacturers, we do not drop the $t-1$ tax reform coefficient due to our varied difference lengths.[17] Figure 2 plots the relevant coefficients and confidence intervals.[18]

Figure 2 demonstrates no discernible trend in prices pre-reform. This implies that once we control for the compositional shifts in Figure A.1 with appropriate fixed effects, we no longer observe any significant trends in prices prior to the reform. The entire response happens in period t, the reform week. Given the varied difference lengths for manufacturers, this implies

[16] We could also estimate the response of retail margins to the tax reform directly on this sample to reach the same conclusion.

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

[18] 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.

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 any 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 process, further supporting our first-difference specification. Moreover, this suggests that our results are not driven by learning or other short-run adjustments. Figure A.3 provides longer-run evidence by repeating the manufacturer event study aggregated at the monthly level. We find no evidence of long-run learning either (Doraszelski et al., 2018; Huang et al., 2018).[19]

6 Discussion and Conclusion

Tax invariance (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 failed to hold, 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 did not hold: 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 broad classes of models consistent with TIV would predict. Conditional on manufacturer prices, we find evidence that retailers applied a constant markup over marginal costs, consistent with standard models of firm behavior.

[19] Additionally, Figure A.1 plots matched retail and manufacturer prices in their first week of retail sale pre- and post-reform. If there were a slow-moving adjustment towards TIV, the gap between matched wholesale and retail prices (which will both experience the same market level shocks) would slowly diverge as wholesale prices continued to adjust downwards relative to retail prices towards the TIV equilibrium. We see no evidence of this behavior.

Our empirical results reject the pricing predictions of our model, which in previous work successfully predicted other supply-side decisions (Hansen et al., 2022). We expect these results to be robust to a great deal of model uncertainty. In particular, 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., 2020b); 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.

We thus turn to the literature establishing asymmetric firm responses to changes in market conditions for possible explanations, as others have found asymmetric firm behaviors in related settings. The work of 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 standard models.

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 may face greater consequences for doing so. While it may be possible to implement revenue-neutral reforms, restructuring could create 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). Our work provides more evidence that policymakers may not be able to rely on TIV because of the ubiquitous nature of default options in reform contexts. 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: Calibration summary

(a) Parameters

Variable	Description	Value
ϵ_D	Demand elasticity	1.1689
k	Demand level	449.477
β_{g1}	Cost to firm 1 of cultivating cannabis	0.0771
β_{g2}	Cost to firm 2 of cultivating cannabis	0.0429
β_{p1}	Cost to firm 1 of processing cannabis	0.0693
β_{p2}	Cost to firm 2 of processing cannabis	0.0254
θ_p	Conduct parameter in wholesale market	0.1447
θ_r	Conduct parameter in retail market	0.6087

ϵ_D and k are calibrated from Hansen et al. (2020). The other parameters are calibrated using the below moments.

(b) Pre-Reform Moments

Moment	Data	Model
Fraction vertically integrated	0.955	0.947
Manufacturer to retailer price	3.60	3.60
Cultivator to manufacturer price	1.53	1.53
Firm 1 / Firm 2 cultivation ratio	0.413	0.418
Firm 1 / Firm 2 processing ratio	0.537	0.534
Retail price	9.39	9.39

See text for detailed description of moments.

(c) Post-reform predicted outcomes

Outcome	Prediction
Change in log-manufacturer price	
Tax-inclusive	-0.253
Tax-exclusive	-0.004
Change in log-retail price	
Tax-inclusive	-0.140
Tax-exclusive	-0.253

We model the reform by removing the gross receipts tax, increasing the final goods tax to 37%, and moving the base of the excise tax from the sales-tax-inclusive price to the sales-tax-exclusive price.

Table 2: 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
Manufacturer Price ($/g)	63,668	4.103	1.309
Retail State + Local Sales Tax Rate	63,668	1.089	0.006
Tax Revenue Pre-Reform ($/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 3: Manufacturer Price Response

	(1) Δlog(Price)	(2) Δlog(Price)	(3) ΔPrice	(4) Δlog(Price)
		Tax Reform		
ΔTax Reform	−0.065***	−0.072***	−0.228***	−0.059***
	(0.015)	(0.018)	(0.068)	(0.014)
Observations	12,087	12,087	12,087	20,902
Manufacturer Firms	199	199	199	210
P-Value for Test of TIV-Predicted Pass-Through	0.000	0.000	0.000	0.000
		Placebo		
ΔPlacebo	0.001	0.000	0.014	−0.002
	(0.012)	(0.014)	(0.040)	(0.010)
Observations	21,288	21,288	21,288	42,354
Manufacturer Firms	180	180	180	208
Bandwidth	6 weeks	6 weeks	6 weeks	6 months
MRS FE?	No	Yes	Yes	Yes
Aggregation	Weekly	Weekly	Weekly	Monthly

This table reports estimates of Equation (3) – 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.

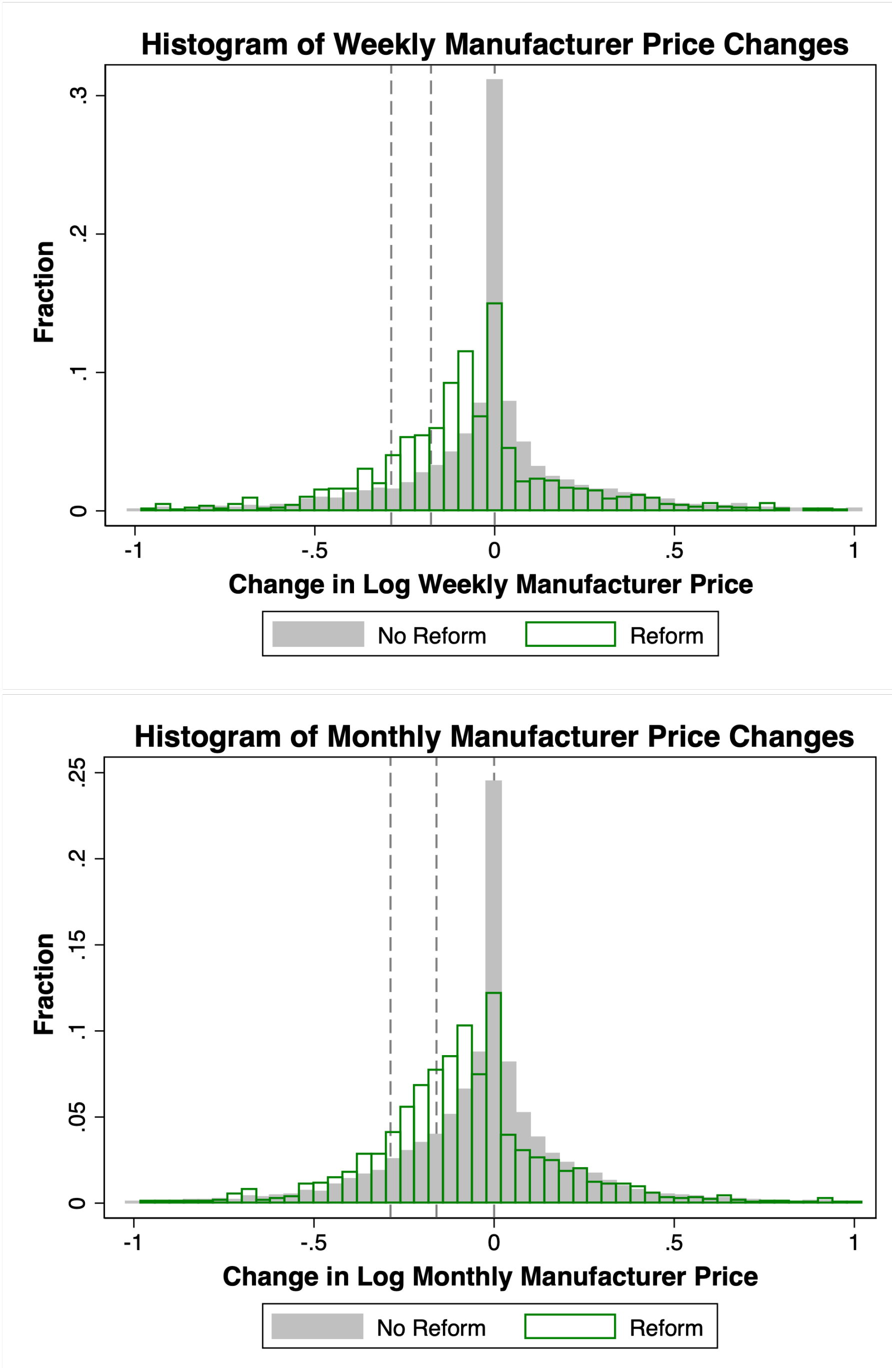
Table 4: Retail Tax-Exclusive Price Response

	(1)	(2)	(3)	(4)	(5)	(6)
	Δlog(Price)	Δlog(Price)	Δlog(Price)	ΔPrice	Δlog(Price)	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 (3) – 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.

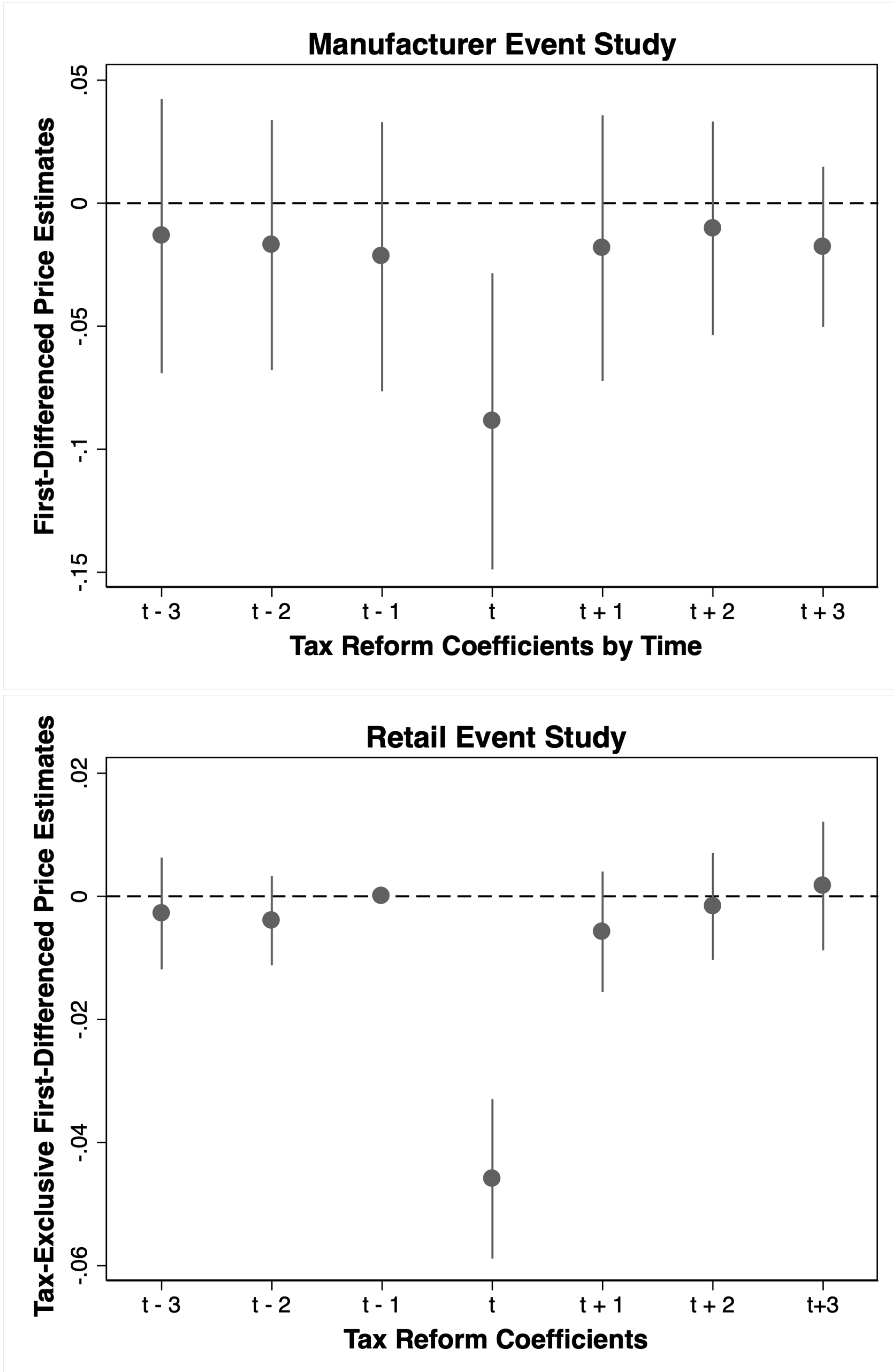
Figures

Figure 1: Histogram of Manufacturer Price Changes



This figure plots the dependent variable, $\Delta log(Price)$, for each retail-manufacturer-strain tuple in the baseline estimate sample. Weekly log manufacturer price changes are in the top panel and monthly log manufacturer price changes are in the bottom panel. The period of the tax reform is marked by the hollow green histogram and the other surrounding periods – six weeks or months pre- and post-reform – are marked by the gray histogram (the months affected by 4/20 are excluded from that histogram). The width of each bin is 0.04, so that all price changes within 2% of zero are included in the bin centered around zero. The dashed lines from right to left indicate: full pass-through predicted by models of firm behavior (-28.7%), our alternative benchmark of pass-through (-17.7%), and zero (i.e no price change). Outliers outside the interval [-1,1] are excluded.

Figure 2: Manufacturer and Retail Price Event Studies

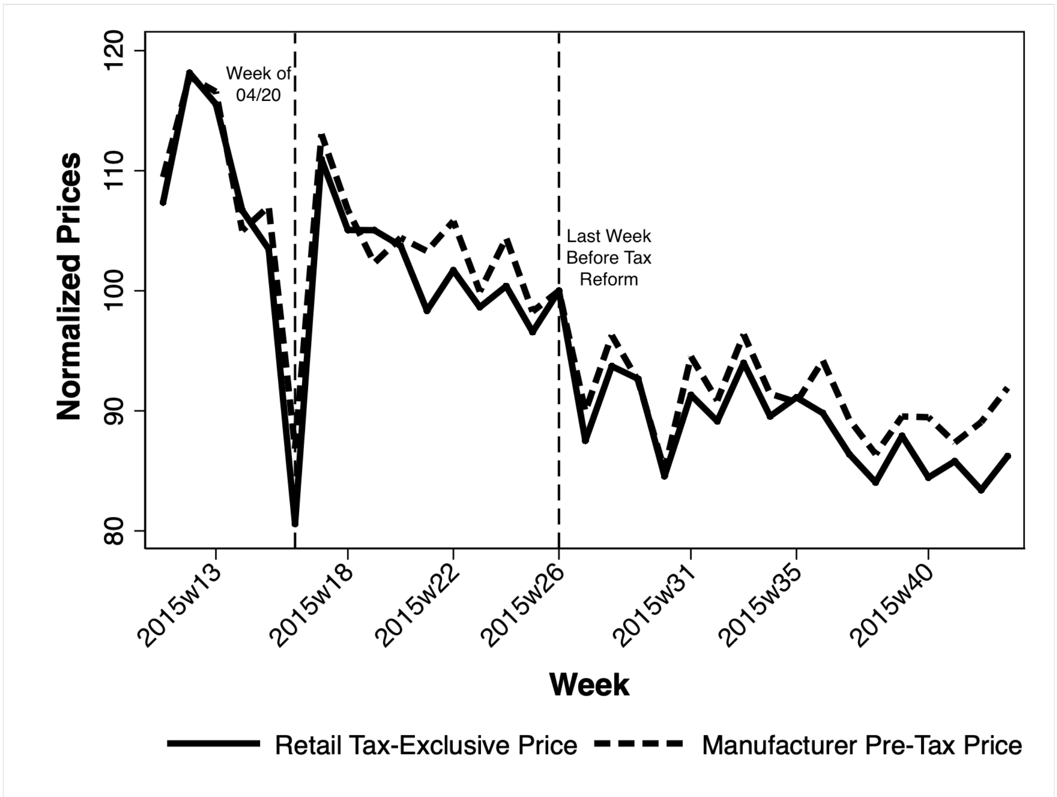


This figure plots estimates of Table 3 Column (2) (top panel) and Table 4 Column (2) (bottom panel) with additional leads and lags of $\Delta TaxReform$. The plotted coefficients are leads and lags of $\Delta TaxReform$. We include in the regression (but do not plot) leads and lags are for periods $t-4$ 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 3 and 4 for regression details.

Appendices

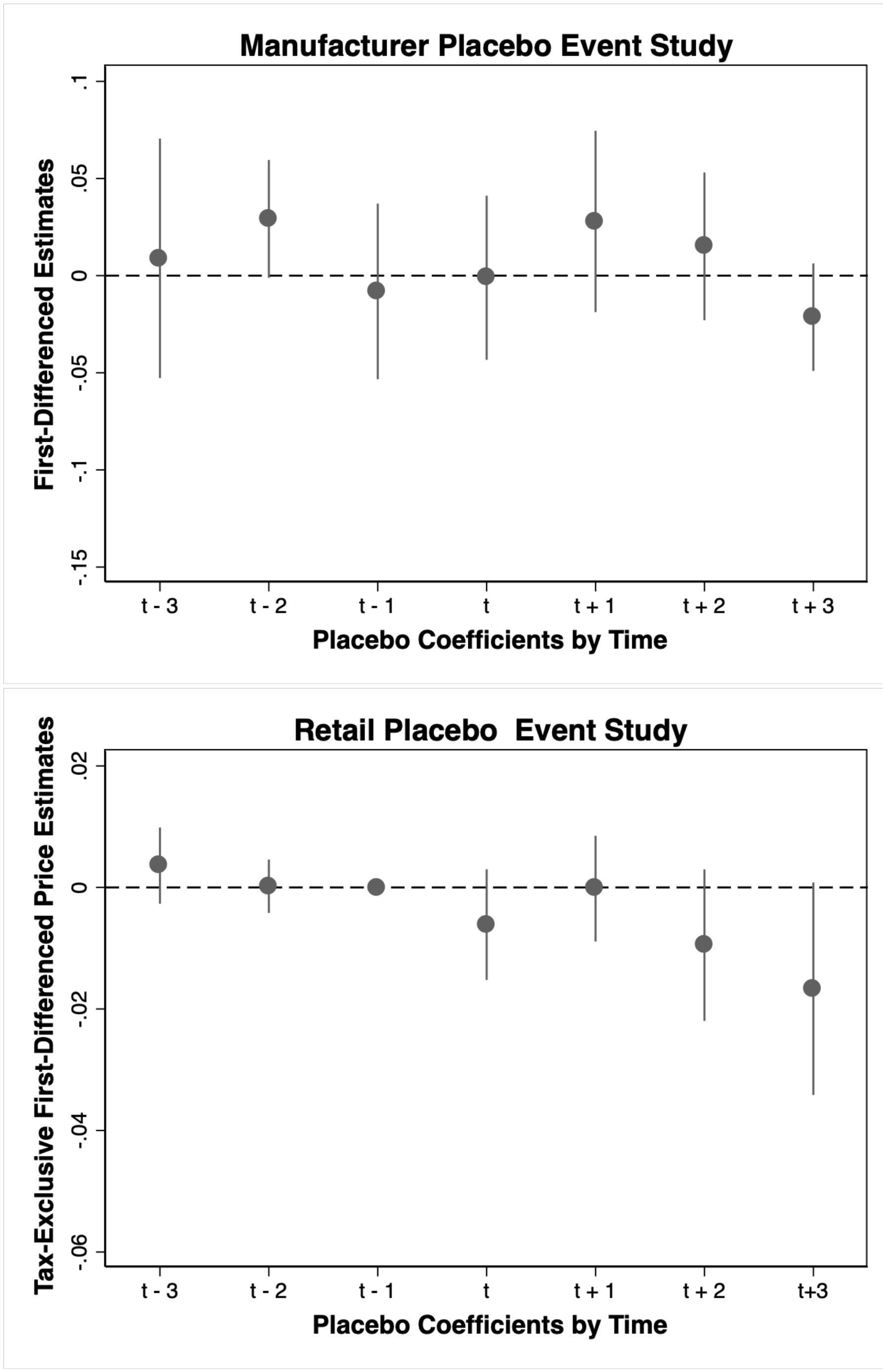
A Additional Figures

Figure A.1: Matched Retail and Manufacturer Prices



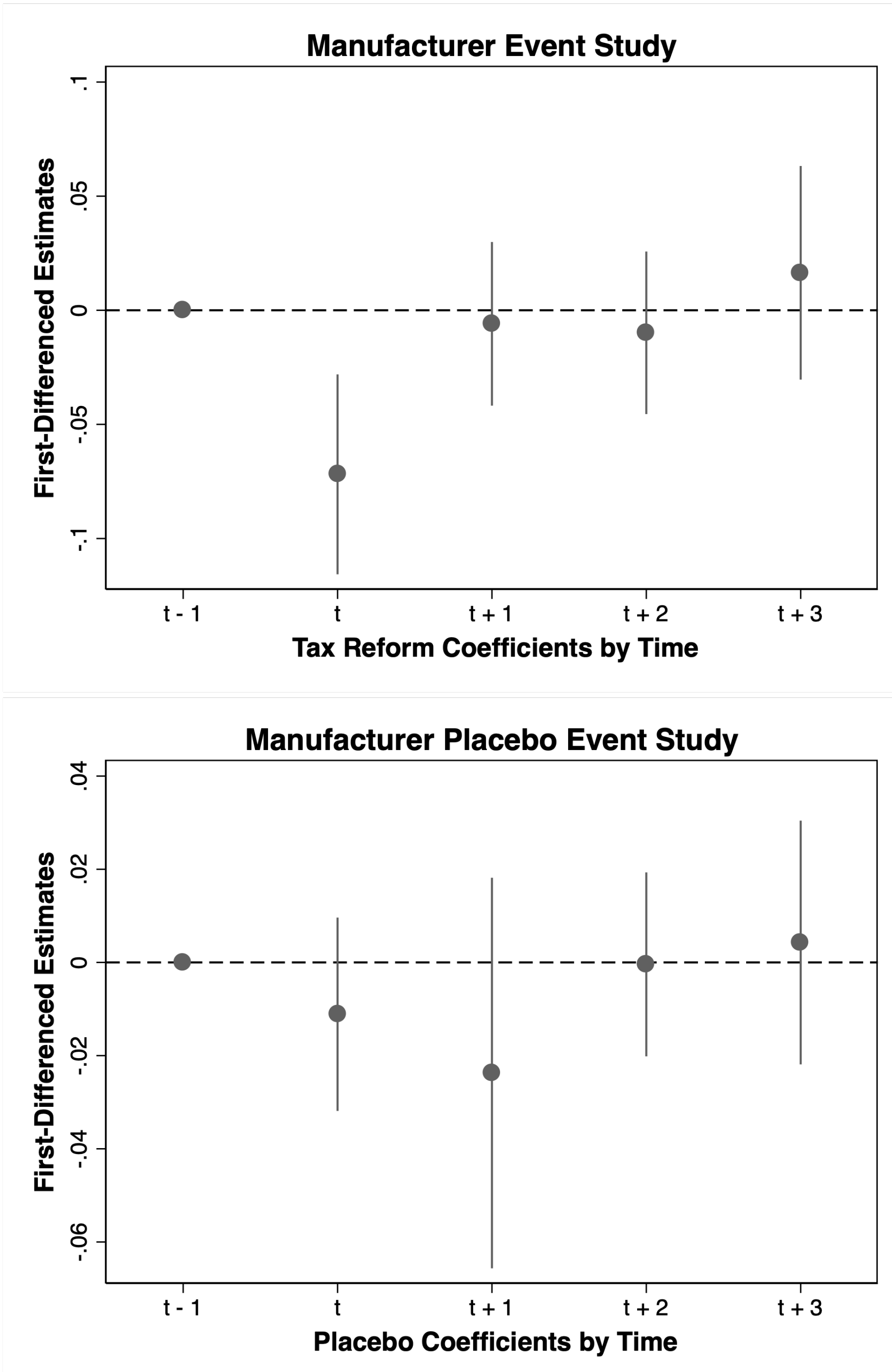
This figure plots raw average prices in Washington's cannabis industry for four months before and after the tax reform, normalized to 100 in the week before the reform. For each week, we take inventory lots in their first week of sale and match them with the price paid to the manufacturer, restricting 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 A.2: Placebo Manufacturer and Retail Price Event Study



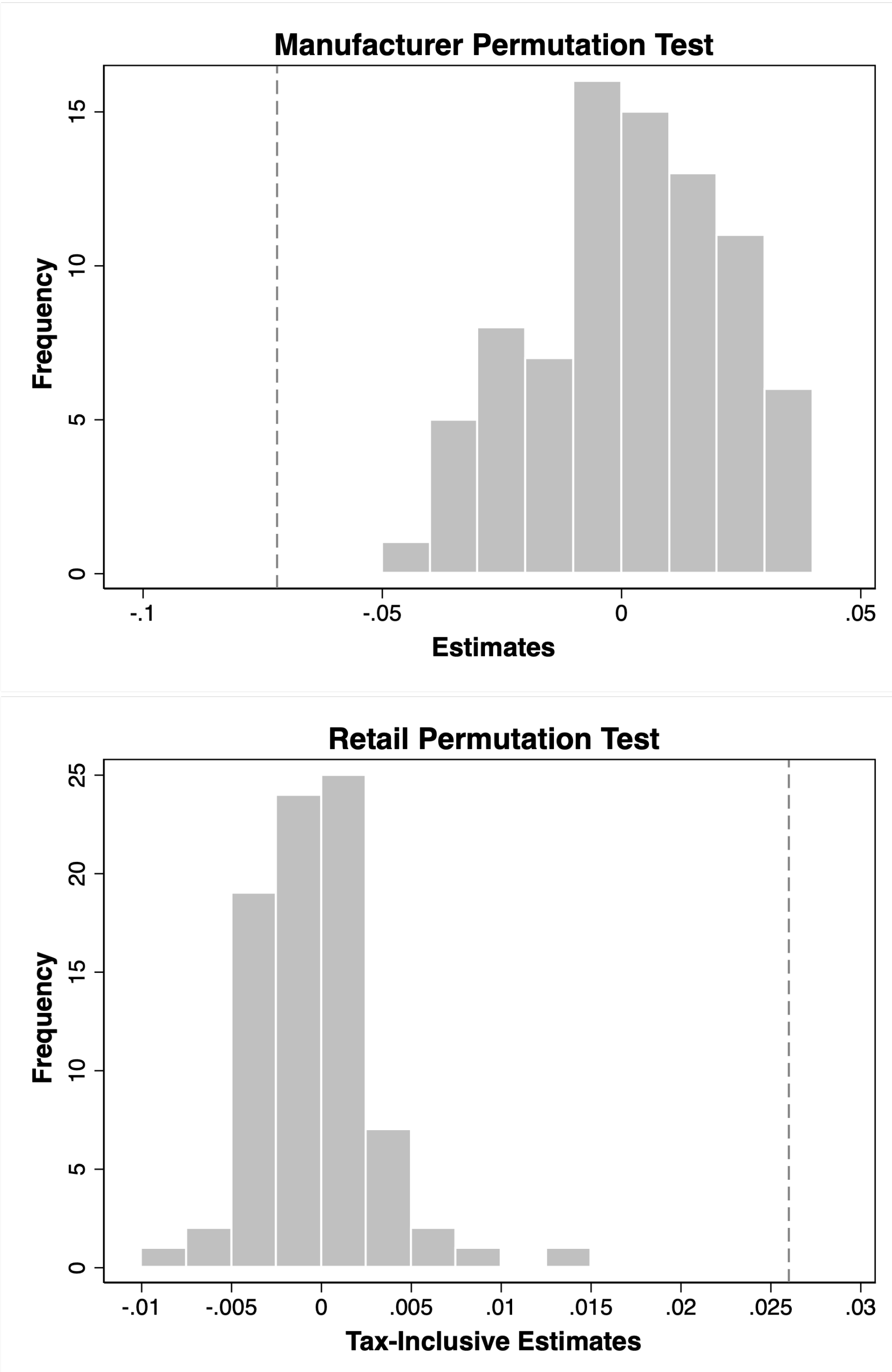
This figure repeats 2 one year later as a placebo robustness check. The plotted coefficients are leads and lags of $\Delta Placebo$. We include in the regression (but do not plot) leads and lags are for periods $t-4$ and before and $t+4$ and after as is standard in event study designs. The dots indicate the point estimates and the lines indicate 95% confidence intervals. See the notes for Tables 3 and 4 for regression details.

Figure A.3: Manufacturer Monthly Event Studies



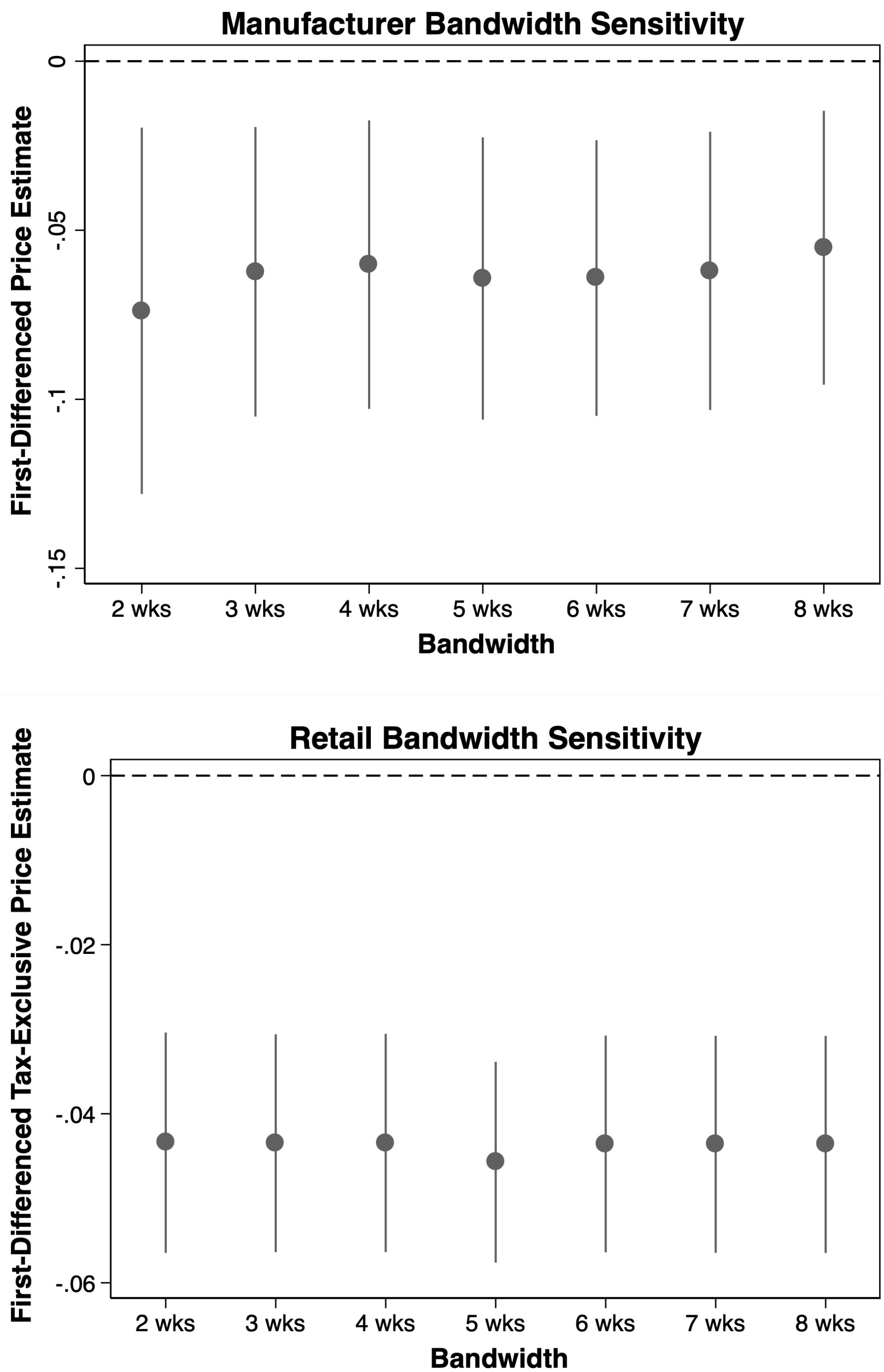
This figure repeats the top panels of Figure 2 and Figure A.2 for monthly, rather than weekly, differences. The plotted coefficients are leads and lags of $\Delta TaxReform$. The dots indicate the point estimates and the lines indicate 95% confidence intervals. We include in the regression (but do not plot) leads and lags are for periods $t-4$ and before, and $t+4$ and after as is standard in event study designs. We additionally exclude periods $t-2$, $t-3$ from our plot (even though they are included in the regressions) because first differences for both of those time periods include effects of the holiday 4/20 and are thus contaminated – we expect there to be a non-zero response in those months.

Figure A.4: Permutation Test Histograms



This figure conducts a placebo permutation test. The top panel is for our manufacturing price estimates and the bottom panel is for our retail tax-inclusive price estimates. The histogram plots placebo estimates using our baseline specification, but reassigning treatment to each week in our data, except those within two weeks of the tax reform, Black Friday, and 4/20. We also do not begin these estimates until there are at least 5,000 observations in our manufacturing regressions (which occurs in mid-March 2015). The gray dashed line marks our estimated effect in each figure. These lines are well outside the next most extreme estimates. There are 83 observations in each of our permutation tests, so the implied p-value is 0.012 (=1/83).

Figure A.5: Manufacturer and Retail Price Bandwidth Choices



This figure plots estimates of Table 3 Column (2) in the top panel and Table 4 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 3 and 4 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.[20] 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.[21]

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

[20]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.

[21]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.[22] 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.[23] 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

[22]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.

[23]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[24] and all retail transactions if the usable weight was above 28.5 grams.[25] We also drop all wholesale or retail price per grams above $42.[26] We censor the THC content data if it is zero or above 40 in both the manufacturer and retailer data.[27] 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.

[24] This is about 0.025% of wholesale transactions.
[25] This is because the maximum legal sale was one ounce. This step drops 0.15% of retail transactions.
[26] This is less than 0.03% of wholesale transactions and less than 0.04% of retail transactions.
[27] This affects 0.2% of wholesale transactions and 5% of retail transactions.