Vertical shocks and cost pass-through: evidence from matched scanner data

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Abstract

Many taxes and cost shocks affect more than one stage of the supply chain. This paper demonstrates that the vertical scope of a tax or cost shock has important implications for estimating and interpreting cost pass-through. Beyond the direct cost shock from the policy itself, firms can face an indirect cost shock from higher intermediate goods prices. The latter gives rise to divergent pass-through rates across firms with otherwise similar exposure to the tax or cost shock. Exploiting the vertically disintegrated market structure and rich scanner data of the Washington state cannabis industry, I demonstrate these points in the context of large labor cost shocks from minimum wage hikes. This paper illustrates how properly accounting for the vertical scope of cost shocks is crucial for evaluating 'who pays' for a variety of taxes and policy shocks.

JEL Classification: H22, L11, L13, L66, L81, Q12

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1 Introduction

Cost pass-through—the rate at which firms pass taxes or costs through to prices—is a key parameter for understanding the degree to which cost shocks are borne by consumers and firms. Economists use pass-through to determine the incidence of cost shocks like exchange rate fluctuations, commodity price shocks, taxes, and a broad range of economic policies like tariffs and minimum wages. A high pass-through rate implies that firms are able to shift the burden of cost shocks on to consumers, while a low pass-through rate means that the incidence of a cost shock is primarily borne by firms.

One aspect that is often overlooked when analyzing pass-through is the fact that cost shocks often affect multiple points of the supply chain. For example, cost shocks such as minimum wage increases, energy price spikes, and changes in business taxes can apply to upstream and downstream firms alike. This simple observation raises several important questions. Does the vertical scope of a tax or policy—i.e. whether the shock affects one or multiple points of the supply chain—have implications for 'who pays' for the cost shock? How do upstream and downstream market characteristics affect pass-through, and what role do heterogeneities across the supply chain play in the ability of firms to pass the burden of the shock on to consumers? Answering these questions is key to understanding the transmission of shocks through the price mechanism and is crucial for evaluating the incidence of cost shocks. Yet, vertical cost shocks are understudied and their implications are not well-understood.[1]

In this paper, I demonstrate that the vertical scope of a policy or cost shock has important implications for pass-through analysis. First, a cost shock affecting multiple points of the supply chain can be compounded for downstream firms. In addition to the direct shock from the policy change itself, these firms can face an indirect shock from higher intermediate goods prices, where the latter reflects upstream pass-through. Pass-through estimates that fail to account for the indirect cost shock may not capture the full impact of an industry-wide policy on prices and may underestimate the burden borne by consumers. Second, a vertical cost shock can lead to biased estimates of pass-through in empirical designs based on a treatment and a control group. This reflects that the indirect cost shock can elicit a price response at downstream firms that have no direct exposure to the policy itself, thus invalidating them as clean controls. Third, a vertical shock can lead to heterogeneous pass-through downstream. Downstream firms source their intermediate goods from different upstream firms and at different intensities. This generates variation in the indirect cost shock and implies that downstream firms with otherwise identical direct exposure to the policy can have vastly different price responses. Finally, the direct and indirect shocks can be passed through at different rates *even within the same firm*. To the extent that firms in the same local market face similar direct exposure to the tax or policy shock, the direct cost shock is an aggregate shock in local markets. In contrast, the indirect shock is idiosyncratic to specific firms. This

[1] By *vertical shock* I mean a policy or cost shock that directly affects both upstream and downstream firms.

limits a firm's ability to pass the indirect shock through to prices and drives a wedge between the pass through rates for the direct and indirect shocks. The divergence implies that the burden of the shock falls more heavily on consumers for the direct shock compared to the indirect shock.

I demonstrate these points empirically by studying the impact of statewide minimum wage hikes on prices in the Washington state cannabis industry. This setting allows me to overcome obstacles that have limited the analysis of vertical cost shocks in previous studies: (1) the need for complete input and output prices for the universe of firms, (2) the ability to clearly define vertical relationships between upstream and downstream firms, and (3) an exogenous cost shock that differentially affects all firms across the supply chain. The cannabis industry contains two types of firms: upstream manufacturers (i.e. cultivators) and downstream retailers. Crucially, vertical integration between manufacturers and retailers is not allowed. I use rich scanner-level data from the universe of establishments across the cannabis industry in Washington state. For each manufacturer, I observe monthly prices and quantities for all products sold to each retailer. For each retail store, I observe monthly prices and quantities for all products purchased from manufacturers, and the subsequent prices and quantities for those very same products sold to end consumers. Such rich establishment-level data on input and output prices are rarely available for an entire industry. The scanner data, combined with the vertical separation between manufacturers and retailers, reveals clearly defined vertical relationships and allows me to disentangle retailers' price responses to direct and indirect cost shocks. Finally, my data spans three predetermined statewide minimum wage hikes. Wages in the cannabis industry are low and minimum wage hikes represent a sizable labor cost shock for manufacturers and retailers alike. Cross-border sales to other U.S. states are prohibited such that manufacturers and retailers are subject to the same statewide minimum wage hikes. This creates an unusually clean set of labor cost shocks along the entire supply chain and narrows the set of possible confounders (e.g. labor and product market shocks in other regions).

To identify the response of prices to minimum wage hikes, I exploit geographic variation in minimum wage exposure for cannabis establishments using a difference-in-differences with continuous treatment estimator. My identification strategy is based on the idea that minimum wage hikes affect establishments with a high share of minimum wage workers more than those with a low share. As a first step, I abstract from the indirect cost shock and estimate the price response to minimum wage hikes under the assumption that the minimum wage only induces a labor cost shock. I find that a 10% increase in the minimum wage translates into a 0.6% increase in retail prices and a 1.7% increase in wholesale prices. By wholesale prices, I mean the prices charged by manufacturers to retailers. Next, I allow for minimum wage hikes to induce both a labor (i.e. direct) and intermediate goods (i.e. indirect) cost shock at cannabis retail stores. I exploit the fact that cannabis manufacturers produce a tradable good and sell to retailers statewide, whereas retailers operate brick-and-mortar stores in well-defined local markets. This mismatch in tradability creates variation in the indirect shock across retail stores with otherwise similar direct shocks. The richness of my data allows me to construct a shift-share instrument measuring each retailer's differential exposure to upstream pass-through. I augment my main specification with the instrument to separately identify the effects of the direct and indirect shocks on retail prices. I find that properly accounting for the indirect cost shock increases the overall effect of minimum wage hikes on retail cannabis prices from 0.6% to 2% (from a 10% hike).

I document important heterogeneities in the upstream price response to minimum wage hikes and show the implications for downstream pass-through. First, I show analytically that the wholesale price response increases with minimum wage exposure for cannabis manufacturers. Second, I estimate a large price response to minimum wage hikes for small manufacturers, but no effect for large manufacturers. This suggests that minimum wage pass-through decreases with the scale of production for cannabis manufacturers, consistent with the high fixed costs associated with cultivation. These upstream heterogeneities imply that the indirect cost shock can differ across retailers according to where they source their intermediate goods. Retailers that source from small manufacturers with a high share of minimum wage workers may face a large indirect cost shock, whereas retailers that source from large manufacturers with low minimum wage exposure may face little or no indirect cost shock. As a result, retailers with otherwise similar direct cost shocks can have vastly different price responses to minimum wage hikes.

Next, I investigate the role of strategic complementarities in the price response to minimum wage hikes. In oligopolistic markets, the price a firm sets is not only a function of its own costs, but also those of its competitors. A major advantage of my setting is that I observe the spatial distribution of cannabis retail stores and do not need to make assumptions about which establishments are direct competitors. I augment my main empirical equation with a measure of the minimum wage-induced cost shock at each retailer's direct competitors. I find that retailers are sensitive to their competitors' direct cost shocks but insensitive to their indirect cost shocks. I show that this contrast arises from a fundamental difference in the scope of the direct and indirect cost shocks. Under the assumption that retailers located in the same local market have similar wage structures, the direct cost shock is an aggregate shock to local markets. In contrast, retailers source their intermediate inputs from manufacturers in different locations and with different minimum wage exposures, meaning the indirect cost shock is idiosyncratic to specific retailers. Thus, the finding that retailers are sensitive to their competitors' direct cost shocks but insensitive to their indirect cost shocks is consistent with the theoretical prediction that strategic price effects are larger for aggregate cost shocks compared to idiosyncratic cost shocks (Muehlegger and Sweeney, 2022).

Finally, I employ a general theoretical model of firm production and derive the cost pass-through rates implied by my reduced form estimates. I build on the framework from Renkin et al. (2022) and derive analytically the relationship between the minimum wage elasticity of prices and the elasticity of marginal cost at constant output. I calibrate the model using reduced form estimates and cost shares for cannabis retailers. When ignoring strategic price effects, I find that retailers fully pass both the direct and the indirect cost shocks through to prices. When properly accounting for strategic price effects, the indirect pass-through rate remains unchanged but the direct cost shock is more than fully passed through to prices. This illustrates how, even within a given firm, strategic complementarity in prices can drive a wedge between the pass-through rates for the direct and indirect shocks.

The implications of my findings extend well beyond the cannabis industry. Differences in tradability upstream versus downstream are a common feature in many sectors. Data from the U.S. Commodity Flow Survey demonstrates that in many manufacturing and wholesale trade sectors, the majority of goods produced are shipped to distributors or retailers located in other regions. At the same time, 80% of U.S. retail sales are in brick-and-mortar stores, which aligns with the idea that retailers sell non-tradable goods in local markets. This mismatch in tradability implies that upstream cost shocks can proliferate downstream in many sectors and across a wide geographic area, and highlights the importance of accounting for the vertical scope of a policy or cost shock when analyzing its effect on prices. The similarities between the retail cannabis industry and other retail settings in terms of cost structures and demand elasticities, as well as the absence of a prevalent black market competing with legal cannabis sales, further supports the generalizability of my findings (Hollenbeck and Giroldo, 2022).

This paper contributes to two strands of literature. First, my study contributes to the extensive literature measuring the pass-through of cost shocks, the underlying drivers, and their implications. Previous studies have investigated the pass-through of costs to prices of oil (Borenstein et al., 1997), coffee beans (Nakamura and Zerom, 2010), cement (Miller et al., 2017); taxes (Marion and Muehlegger, 2011; Genakos and Pagliero, 2022); energy (Ganapati et al., 2020), exchange rates (Goldberg and Knetter, 1997); minimum wages (Renkin et al., 2022; Harasztosi and Lindner, 2019); and many other settings. Much of the literature focuses on cost shocks that directly affect a single stage of the production process. To the best of my knowledge, this is the first paper to estimate the pass-through of a cost shock that directly affects multiple stages of the production process and demonstrate the implications for pass-through analysis.

A few studies allow for a cost shock to have an indirect effect on prices through intermediate goods (Renkin et al., 2022; MaCurdy, 2015). These studies assume full pass-through upstream in order to infer the downstream price response at the aggregated industry level.[2] The focus of my paper is on pass-through at the firm level. I demonstrate how a vertical cost shock gives rise to heterogeneous pass-through rates downstream. This heterogeneity arises because downstream firms, even within the same local market, source their intermediate inputs from different upstream firms with differential exposure to the cost shock. Compared to the direct cost shock, the idiosyncratic nature of the indirect cost shock limits downstream

[2] An exception is Nakamura and Zerom (2010) who estimate the effect of an upstream commodity price shock (imported coffee beans) on upstream and downstream coffee prices.

firms' ability to pass the shock through to consumers.

Pass-through rates greater than one have been found in previous studies analyzing specific industries. Leung (2021) finds that grocery stores more than fully pass the cost of minimum wage hikes through to prices and attributes this to demand-induced feedback. Conlon and Rao (2020) examine excise taxes and retail prices in the distilled spirits market and show that nominal rigidities like price points can generate more than full pass-through. In contrast, this paper is the first to show empirically that strategic complementarity in prices can result in the overshifting of costs to prices.

The extent to which separate components of marginal cost are passed through to prices has not been studied extensively. An exception is Westphal (2024), who studies borrowing costs in the U.S. residential mortgage market. His paper shows that pass-through is increasing in consumers' awareness of the underlying cost shock. Since some components of marginal cost are more salient than others, the different components can be passed through at different rates. By estimating pass-through of the direct (i.e. labor) and indirect (i.e. COGS) cost shocks, I also document different pass-through rates for separate components of marginal cost. However, in this paper, divergent pass-through rates arise because the direct shock is aggregate while the indirect shock is idiosyncratic to specific firms.

Second, this paper contributes to a small but growing literature that documents strategic complementarity in prices. Using data from the Belgian manufacturing sector, Amiti et al. (2019) estimates firms' responsiveness to their own costs as well as competitors' prices. Muehlegger and Sweeney (2022) examine the pass-through of own and rival firms' costs in the U.S. oil refining industry. Both studies find that strategic complementarities account for half of firms' price response to cost shocks. My analysis expands on these studies by investigating strategic complementarities in the context of a vertical cost shock. In this paper, downstream firms are subject to both a direct and an indirect cost shock. Since the former is an aggregate shock but the latter is idiosyncratic to specific firms, strategic complementarities give rise to different pass-through rates for the two shocks within the same firm.

This paper proceeds as follows. Section 2 illustrates analytically how the vertical scope of a cost shock relates to pass-through. Section 3 describes the institutional context for my empirical analysis. Section 4 describes the data and the empirical strategy. Section 5 presents the main empirical findings. Section 6 develops a theoretical model of firm production and derives implied cost pass-through rates. Section 7 concludes.

2 Pass-through and vertical cost shocks

In this section, I illustrate analytically how the vertical scope of a cost shock relates to pass-through. This highlights the importance of accounting for upstream pass-through when estimating downstream pass-through. I consider the pass-through of a tax τ that is levied on all firms (the pass-through of an input cost shock, such as the minimum wage-induced labor cost shock I study, is analogous). Firm j sets profit-maximizing price p_j and faces tax-inclusive marginal costs α_j. Firm j purchases intermediate inputs from a set of upstream firms $s \in S$, where upstream firm s sets profit-maximizing price p_s and faces tax-inclusive marginal costs α_s. Each firm, whether upstream or downstream, can have a different exposure to the tax, with $\frac{d\alpha_j}{d\tau}$ capturing the marginal per unit tax rate for firm j.[3] The pass-through of the tax onto firm j's price can be decomposed as

$$\frac{\partial p_j}{\partial \tau} = \frac{\partial p_j}{\partial \alpha_j}\frac{d\alpha_j}{d\tau} + \frac{\partial p_j}{\partial \alpha_j}\sum_{s \in S}\frac{\partial \alpha_j}{\partial p_s}\frac{\partial p_s}{\partial \alpha_s}\frac{d\alpha_s}{d\tau} \tag{1}$$

where $\frac{d\alpha_s}{d\tau}$ reflects the effect of the tax on supplier s's marginal cost.

Equation 1 illustrates that the tax affects firm j's price both directly, through the firm's own tax costs, and indirectly through higher intermediate goods prices, where the latter reflects upstream pass-through. Thus, a tax levied on firm j alone will be passed through at a different rate than a tax levied on firm j and its suppliers. Beyond its own tax exposure, firm j's price response to the tax depends on (1) the degree of tax exposure at each supplier, $\frac{d\alpha_j}{d\tau}$, and (2) the sensitivity of firm j's marginal cost to the price set by each supplier, $\frac{\partial \alpha_j}{\partial p_s}$. The first point implies that a firm purchasing from suppliers that are more heavily exposed to the tax can have a larger price response to the tax. The second point implies that even if firms source their intermediate goods from the same suppliers, they can differ in their sensitivity to price changes from those suppliers ($\frac{\partial \alpha_j}{\partial p_s} \neq \frac{\partial \alpha_i}{\partial p_s}$), reflecting differences in the relative importance of a supplier's intermediate inputs for each firm.

These results have three implications for estimating and interpreting pass-through. First, omitting shocks to upstream firms will bias estimates of pass-through to firm j's prices. To obtain the full price response to a cost shock or policy change, it is necessary to account for both the direct and indirect effects. However, empirical models that use local exposure to a shock as their identifying variation may fail to pick up the indirect effect. This is particularly true for industries where upstream firms produce a tradable good that is sold to downstream firms across many local markets. This limits the researcher to estimating the pass-through rate of the direct shock, which may or may not be of primary policy interest. Second, downstream firms that are not directly affected by a shock may still adjust their prices if their suppliers are affected. This invalidates such firms as clean controls in empirical designs that use price changes at firms with no direct exposure to the cost shock as a counterfactual for price changes at directly exposed firms.[4] Third, downstream firms with the same direct exposure to the cost shock can have vastly different pass-through due to differences in indirect exposure. I demonstrate each of these points in the empirical analysis in the following

[3]In my empirical setting, differences in exposure come from geographic variation in firms' share of minimum wage workers.

[4]Muehlegger and Sweeney (2022) illustrate a similar result in the context of a horizontal cost shock and strategic complementarity in prices. They show that firm-specific pass-through depends on a firm's own exposure to the cost shock as well as their rivals' exposure.

sections.

3 Institutional context

3.1 The cannabis industry in Washington state

Approximately 50% of U.S. states have legal recreational cannabis markets. Washington state's cannabis market opened in July 2014 for adults 21 years and older. Cannabis has since become one of the largest agricultural industries in the state, contributing $1.85 billion to gross state product (Nadreau et al., 2020). Cannabis consumption is widespread, with approximately 30% of Washington adults consuming cannabis on a monthly basis (Washington State Department of Health, 2024). Consumption is relatively equal across race/ethnicity, education, and gender, but decreases monotonically with income and age. I detail the demographic characteristics of cannabis consumers in Appendix A.

The industry is regulated by the Washington State Liquor and Cannabis Board (LCB) which offers separate business licenses for upstream and downstream establishments (Washington State Legislature, Title 314, Chapter 55). Manufacturers (i.e. upstream establishments) can cultivate, harvest, and process cannabis but cannot sell to end consumers.[5] Retailers (i.e., downstream establishments) can purchase fully packaged and labeled products from manufacturers and sell them in retail stores. Manufacturers cannot own retail licenses and vice versa, creating complete vertical separation along the supply chain. Retailers can only buy from manufacturers located in Washington state and manufacturers can only sell to retailers in the state. This seals off the core of the supply chain from other U.S. states with legal recreational markets. Retailers cannot sell online, meaning retailers operate brick-and-mortar stores. Appendix A contains additional information on the cannabis supply chain.

Cannabis business licenses are capped by the LCB at 556 retailers and 1,426 manufacturers (Washington State Liquor and Cannabis Board, 2020). Licenses are granted at the establishment level so that a single firm can own several licenses. However, a firm can only own licenses of the same type. Approximately 65% of retail stores belong to one- or two-store firms; 25% of stores belong to 3-5 store chains; less than 11% of stores belong to chains with more than 5 stores.[6] Not all licenses are actively in business, meaning that some license holders have not opened an establishment and have no reported sales activity, especially at the manufacturer level. During the sample period, there were 511 active retailers and 692 active manufacturers.

[5] Within the cannabis industry these upstream firms are known as 'manufacturers' since they engage in both production (i.e. cultivation) and processing raw cannabis into derivative subproducts such as edibles and concentrates. In this paper, I prefer the term 'manufacturer' to describe these firms, as this captures both elements of the production process.

[6] When the market was created in 2014, the LCB allocated licenses according to a lottery. Since a single firm could apply for more than one license, the lottery created exogenous variation in firm size (see e.g. Hollenbeck and Giroldo, 2022).

The LCB distributes retail licenses to counties according to population density but there are no restrictions on where manufacturers can be located. Retailers are located in 37 of the 39 counties in Washington state while manufacturers are located in 35 counties.

Retail sales are subject to a 37% sales tax but there is no tax on upstream sales. Per month, retailers sell approximately 15,800 units and earn about $304,000 in (tax-inclusive) revenue, while manufacturers sell approximately 16,735 units and earn $107,000 in revenue (see Table 1).[7] For more information on establishment characteristics, see Appendix A.2.

Table 1 provides an overview of the cannabis product market. Retail stores sell a variety of cannabis products—around 470 distinct products per month on average. The LCB classifies products according 12 categories (Washington State Legislature, 2015). Usable marijuana (i.e. unprocessed dried flower) and concentrate for inhalation (e.g. for use in vape pens) account for 83% of all retail sales. Another 16% of retail sales comes from solid edibles (chocolate bars, gummies, etc), liquid edibles (soda and other infused drinks), and infused mix (e.g. pre-roll joints infused with concentrates). The remaining categories make up less than 2% of total revenue; these are topical products (e.g. creams and ointments), packaged marijuana mix (e.g. pre-roll joints), capsules, tinctures, transdermal patches, sample jar, and suppository.

Before moving on, it is worth noting several points. First, I refer to the price charged by retail stores to consumers as the *retail* price, and the price charged by manufacturers to retailers as the *wholesale* price. The latter reflects the dual role played by upstream establishments since, besides being manufacturers, they also act as wholesalers when viewed from the perspective of retailers. Second, since manufacturers occupy the upstream portion of the supply chain, I assume that the minimum wage only induces a labor cost shock for these firms (i.e. the minimum wage does not affect material input prices for these firms). This reflects that manufacturing inputs like hydroponic systems, grow lights, and raw materials (e.g. soil or fertilizer) can be purchased from suppliers outside of Washington state, meaning minimum wage pass-through to manufacturers' input prices is likely small. Therefore, for wholesale prices I only estimate direct pass-through of minimum wage hikes, whereas for retail prices I estimate both direct and indirect pass-through.

Cannabis labor

Cannabis is an important source of employment in Washington state, as the sector supports approximately 18,700 full-time equivalent (FTE) jobs (Nadreau et al., 2020). Several features of cannabis labor make the industry particularly well-suited for investigating the effects of minimum wage hikes. First, cannabis is primarily grown in small indoor facilities in a setting that is averse to mechanization and more labor intensive than outdoor cultivation (Caulkins and Stever, 2010). Most harvesting, drying, trimming, and packaging is done by

[7]For context, the average cannabis retailer generates about twice the revenue of an average convenience store or one-fifth of an average supermarket in the United States (Statista, 2022, 2024b).

Table 1: Product market descriptive statistics

(A) Establishment characteristics

	Retailers		Manufacturers	
	Monthly avg. per estab.	Sample total	Monthly avg. per estab.	Sample total
Establishments		511		692
Units sold	15,844	232.13 m	16,735^{+}	228.42 m^{+}
Distinct products	471	172,688	55^{+}	147,273
Sales	$304,032	$4.47 bn	$106,634	$1.46 bn

Notes: The table reports monthly averages at the establishment level during the sample period, and totals across all stores and months in the sample period. Retail sales are tax-inclusive; manufacturer sales are not taxed. Standard deviations are in parentheses. $^{+}$ For manufacturers, the LCB reports the unit weight for some product types (e.g. flower lots) in 1g units regardless of how the product is actually bundled. For such items, the number of units is the weight of the product in grams. As a result, the number of distinct products visible in the wholesale data is artificially low (since different unit weights are treated as a single product), and the number of units sold is artificially high.

(B) Product categories

	Retailers		Manufacturers	
Product category	Monthly sales (millions of $)	Market share	Monthly sales (millions of $)	Market share
Usable marijuana	$58.77	0.52	$22.95	0.61
Concentrate	$34.70	0.31	$10.62	0.28
Solid edible	$8.45	0.08	$1.21	0.03
Infused mix	$5.40	0.05	$1.72	0.05
Liquid edible	$2.96	0.03	$0.79	0.02
Other	$2.16	0.02	$0.63	0.02

Notes: The table reports the average monthly cannabis sales (in millions of dollars) across Washington state for the major product categories during the sample period, along with the corresponding market shares. Retail sales are tax-inclusive; manufacturer sales are not taxed. Manufacturer sales include sales to 'producer-only' licenses.

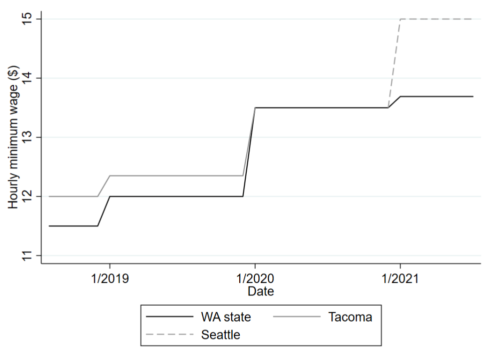
hand, as this allows growers to produce higher quality buds that sell at a higher price point (Jiang and Miller, 2022). Second, wages in cannabis are very low—less than 1/3 to 1/2 of the statewide average wage—reflecting the low-skill nature of cannabis labor. Cannabis manufacturers typically employ 1-2 'master growers', who manage cultivation systems and oversee harvesting, along with a much larger number of low-skill workers who harvest, trim, and package cannabis. At the retail level employees are known as 'budtenders'. Budtending requires no formal training and the job resembles low-skilled employment in other retail sectors. The low-skill nature of cannabis labor implies that both upstream and downstream establishments have a high degree of minimum wage exposure. Appendix A describes labor and wages in cannabis industry in further detail.

3.2 The minimum wage in Washington state

Figure 1 summarizes the minimum wage hikes used in my main analysis. In November 2016, Washington voters approved a ballot measure to scale up the state minimum wage from \$9.47 to \$13.50 by the year 2020. The measure spelled out predetermined, stepwise increases for January 1st of each year, with an initial increase to \$11.00 in 2017, then \$11.50 in 2018, \$12.00 in 2019, followed by the final increase to \$13.50 in 2020. Then, starting January 1st, 2021, the minimum wage was to adjust with the federal Consumer Price Index for Urban Wage Earners and Clerical Workers (CPI-W) on an annual basis. Besides the state minimum wage, there are two cities in Washington state with a binding citywide minimum wage. The city of Tacoma's minimum wage took effect in early 2016 with a predetermined schedule of annual increases designed such that the city and state minimum wages converged in 2020, with the latter binding for all subsequent years. Seattle's minimum wage went into effect in April 2015 and contained two sets of hikes depending on whether an employer paid towards an individual employee's medical benefits. For employees earning \$2.19 per hour in benefits (on top of their hourly wage), the minimum wage was identical to the state minimum wage except for a larger (predetermined) jump to \$15 in 2021. In my main analysis, I assume that this is the schedule of hikes applicable to cannabis establishments in Seattle. In Appendix D.2 I consider the alternative schedule for employees earning less than \$2.19 in benefits.[8] For both Seattle and Tacoma, the citywide hikes occurred on the same day of the year as the statewide hikes (January 1st).

[8]In that schedule, the minimum wage increased more steeply and reached \$15.75 in 2020, while in 2021 it adjusted according to a local CPI (this feature was written into the law in 2015). Due to the potential for reverse causality in that scenario, in a robustness check I drop Seattle establishments from the sample for the 2021 hike and find that results are unaffected (see Appendix D.2).

Figure 1: Minimum wage hikes in Washington state, August 2018-July 2021



Notes: The figure depicts the minimum wage hikes for the sample period in my analysis (August 2018 through July 2021). The state minimum wage applies to all cities except Seattle and Tacoma. Tacoma's minimum wage converged with the state minimum wage on January 1, 2020. Seattle's minimum wage is depicted under the assumption that employers paid at least $2.19/hour in benefits (the alternative schedule is depicted in appendix Figure D2).

4 Data

4.1 Price data

Cannabis establishments are required by law to record all sales and regularly upload data feeds to the LCB. Compliance with data reporting is strictly enforced by the LCB. When a business is issued a violation, it can receive a fine, a temporary license suspension, or both. In cases of repeated violations, a license can be revoked by the LCB board. Given such strict enforcement, violations are uncommon. In 2022 for example, the LCB issued 63 violations among over 1,100 active licensees (Washington State Liquor and Cannabis Board, 2024).

The data, which is usually reported weekly, contains detailed information on the price and quantity of each product sold by a manufacturer to a retailer, and the subsequent price and quantity of that very same product sold at the retail level. The data captures granular product differentiation. For example, a 1.0-gram package and a 2.0-gram package of the same usable marijuana strain produced by the same manufacturer are treated as different products in the data.

The LCB switched providers for its traceability system in October 2017 and again in December 2021, creating two structural breaks in the price data. My sample period lies between these breaks and spans August 2018 through July 2021, a period that covers three statewide and three citywide minimum wage hikes (see Figure 1). I obtained the data from Top Shelf Data, a data analytic firm that ingests the raw tracking data from the LCB and matches it with additional product information. The data covers the universe of sales from all 511 active retail establishments and 692 active manufacturing establishments during the sample period.

To estimate pass-through elasticities I follow previous studies (e.g., Renkin et al., 2022; Leung, 2021; Harasztosi and Lindner, 2019) and use as the dependent variable the natural logarithm of the monthly establishment-level price index. The log price index measures the price inflation for establishment j in month t, and is denoted as $\pi_{j,t}$:

$$\pi_{j,t} = \ln I_{j,t}, \text{ with } I_{j,t} = \prod_{c} I_{c,j,t}^{\omega_{c,j,y(t)}} \tag{2}$$

$I_{j,t}$ is an establishment-level Young price index that aggregates price changes across product subcategories c, where each subcategory is a unique category-unit weight combination. The index weight $\omega_{c,j,y(t)}$ is the revenue share of subcategory c in establishment j during the calendar year of month t.[9] The dependent variable is equivalent to the first difference of the log of the weighted store price level between month t and $t-1$. Appendix Figure B1 shows the distribution of the log price indexes for retail and wholesale prices. Both indexes are centered around zero but the wholesale price index exhibits larger variation in prices compared to the retail index. To limit the potential impact of outliers, I trim the wholesale price index above the 99.5th and below the 0.5th percentile of the monthly distribution in my main specification (results are robust to keeping outliers).

Establishment-level indices are common in the literature on establishment price movements and carry several advantages over more disaggregated product-level price data (Renkin et al., 2022; Leung, 2021; Harasztosi and Lindner, 2019). First, wages are paid at the establishment level, making the establishment a natural unit of analysis. Second, an establishment-level index allows the researcher to weight products by their importance for each establishment. Finally, entry and exit occurs at a much higher frequency for products compared to establishments, and a product-level time series would contain frequent gaps. Since the vast majority of cannabis establishments have succeeded at staying in business, the establishment-level panel is much more balanced. I describe the establishment-level price index in more detail in Appendix B.

4.2 Wage data

My identification strategy is based on the idea that minimum wage hikes affect establishments with a high share of minimum wage workers more than those with a low share. Since wages are not observable at the establishment level, I follow previous studies and use geographic variation in the minimum wage bite as a proxy (see e.g. Card (1992); Bossler and Schank (2022); Leung (2021); Renkin et al. (2022); Dustmann et al. (2022)). I define bite as the share of FTE workers in an industry-county earning below the new minimum wage two quarters prior to the hike. The industries are based on the North American Industrial Classification

[9] Price indexes are often constructed using lagged quantity weights (Renkin et al., 2022). Since product turnover is high in cannabis retail, lagged weights would limit the number of products used in constructing the price indexes. Thus, contemporaneous weights are used.

System (NAICS) which explicitly spells out classification for cannabis establishments of various types. NAICS 453 (”Miscellaneous store retailers”) captures all cannabis retailers since NAICS 453998 includes ”All Other Miscellaneous Store Retailers (except Tobacco Stores), including Marijuana Stores, Medicinal and Recreational” (US Census Bureau, 2017b). NAICS 111 (”Crop production”) captures cannabis manufacturers, since NAICS 111998 includes ”All Other Miscellaneous Crop Farming, including Marijuana Grown in an Open Field” and NAICS 111419 includes ”Other Food Crops Grown Under Cover, including Marijuana Grown Under Cover” (US Census Bureau, 2017b). Since cannabis retailers and manufacturers belong to different NAICS industries, a retailer and manufacturer located in the same county can each have a different bite. Figure 2 depicts the average industry-by-county bite in the sample period for the NAICS industries containing cannabis establishments.

By defining bite at the level of the three-digit industry, I assume that variation in wages at cannabis establishments resembles variation in the corresponding NAICS industries. I document several facts in support of this in Appendix A.

I obtained the bite data from the Washington Employment Security Department (ESD) which collects data on employment and wages in industries covered by unemployment insurance (about 95% of U.S. jobs).[10] A similar dataset has been used in the recent minimum wage literature (see e.g. Dube et al. (2016); Renkin et al. (2022); Leung (2021)).

Table 2 compares the pre-treatment characteristics of establishments above and below the median bite for each establishment type. Columns 1 and 2 show that the average retail unit price and unit price growth are similar at low- and high-bite stores prior to minimum wage hikes. Low bite stores sell slightly less products per month, have lower monthly revenue, and have less product variety compared to high bite stores, but the differences are small and never statistically significant. Similarly, columns 3 and 4 show that low- and high-bite manufacturers have a similar unit price, monthly revenue, and product variety prior to minimum wage hikes. An exception is that low-bite manufacturers sell a higher quantity than high-bite manufacturers; however, this is due to the fact that for some manufacturers, the LCB reports the number of units as the weight of the product in grams. As a result, the number of distinct products visible in the wholesale data is artificially low (since different unit weights are treated as a single product), and the number of units sold is artificially high. Overall, the similarities between low- and high-bite establishments prior to minimum wage hikes support the assertion that price changes at low-bite establishments serve as a valid counterfactual for price changes at high-bite establishments.

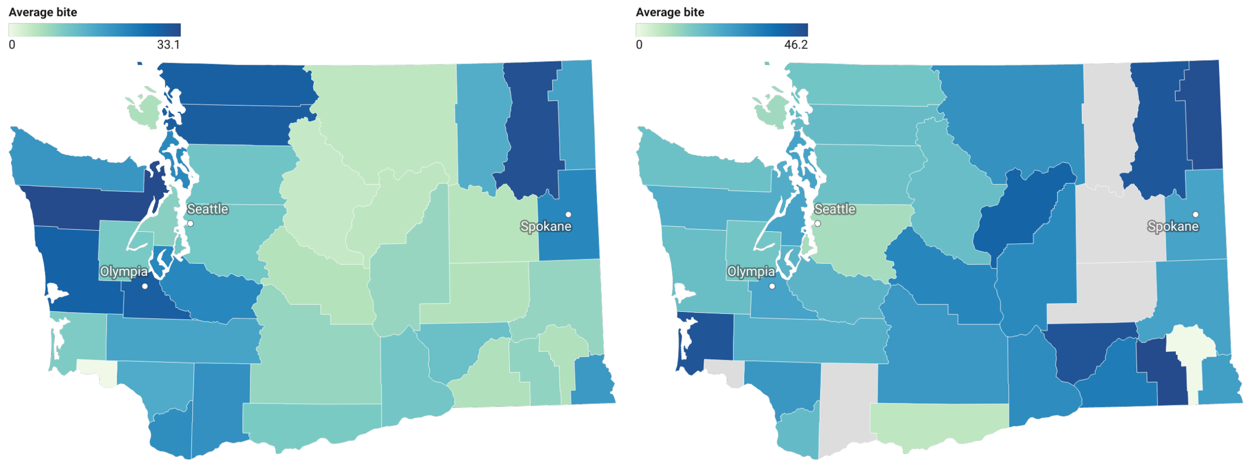
[10] The ESD data feeds into the better-known Quarterly Census of Employment and Wages (QCEW), a federal/state cooperative program that measures employment and wages in industries covered by unemployment insurance at the detailed-industry-by-county level.

Table 2: Pre-treatment summary statistics

	Retail		Wholesale	
	(1) Low bite	(2) High bite	(3) Low bite	(4) High bite
Unit price (in dollars)	26.85	26.59	11.41	11.68
	(4.83)	(5.13)	(11.04)	(5.90)
Unit price growth (percent)	0.2	0.1	0.2	0.2
	(3.5)	(3.0)	(6.3)	(6.6)
Units sold per month	11,436	13,385	65,998	12,328
	(12,779)	(12,544)	(570,332)	(42,921)
Monthly revenue (in dollars)	223,571	254,589	76,305	81,795
	(258,136)	(245,064)	(215,746)	(238,092)
Unique products per month	381	410	62	45
	(316)	(345)	(170)	(127)

Notes: The table summarizes establishment-level variables over all pre-treatment periods. Column 1 contains stores below the median bite for retailers in the sample, while Column 2 contains stores above the median bite. Columns 3 and 4 are analogous for producer-processors. The reported variables include unit price, average quantity sold per month, average revenue per month, and average number of distinct products sold per month. For producer-processors, units sold and unique products per month are affected by the LCB data collection practices as described in the main text. Standard deviations are in parentheses.

Figure 2: Average minimum wage bite, 2018-2021



(A) Crop production (B) Misc. retail

Notes: The figure shows average minimum wage bite for counties in Washington state over three statewide minimum wage hikes spanning 2019-2021. Bite is computed as the share of FTE earning below the new minimum wage two quarters prior to the hike. The panel on the left shows bite for crop production (NAICS 111), the industry that includes cannabis manufacturers. The panel on the right shows bite for miscellaneous store retailers (NAICS 453), the industry that includes cannabis retailers. Counties in grey indicate the data do not meet ESD confidentiality standards—these counties are not included in my analysis. Data source: Washington ESD.

5 Empirical strategy

5.1 Effect on wholesale prices

I estimate the effect of minimum wage hikes on wholesale cannabis prices under the assumption that the minimum wage only induces a labor cost shock at cannabis manufacturers. I employ a difference-in-differences (DiD) estimator with a continuous treatment. DiD with continuous treatment has been applied in a variety of settings, including investigating the effects of minimum wage hikes (Card, 1992; Bossler and Schank, 2022), the link between student loan credit expansion and college tuition (Lucca et al., 2019), and the effect of abortion clinic closures on abortion rates (Lindo et al., 2020).

I assign establishments a treatment intensity that is a function of their industry-by-county minimum wage bite. DiD with continuous treatment identifies a causal treatment effect under the assumption that the treatment intensity is independent of the outcome (Callaway et al., 2024).[11] This implies that, conditional on time and establishment fixed effects, price changes at establishments in counties with lower minimum wage bite provide a valid counterfactual for price changes at establishments in counties with higher bite. An additional assumption is that treatment *timing* is independent of the outcome, i.e. price changes do not drive minimum wage hikes. I validate these identifying assumptions in Section 6.2.

[11] Callaway et al. (2024) call this "strong" parallel trends.

Establishments can be treated up to three times since there are three minimum wage events during the sample period, each spaced 12 months apart. However, for a given event the treatment timing does not vary across establishments (i.e. no staggered treatment). Since firms may be forward-looking in their price setting, it is important to consider anticipatory effects that may cause price increases in the months leading up to the hike. Alternatively, firms may smooth price changes across several periods before and after a hike. The high frequency of the price data allows me to capture such dynamics, and I specify a distributed lag model with leads and lags before and after each hike. This is approach is used by previous studies on minimum wage pass-through (see e.g. Renkin et al., 2022; Leung, 2021). Since the establishment-level price index and the minimum wage bite are in first-differences, I estimate the following equation in first-differences:

$$\pi_{j,t} = \sum_{l=-5}^{6} \beta_l Direct_{j,t-l} + \gamma_t + \epsilon_{j,t}. \tag{3}$$

Equation 3 relates the monthly (log) price index at establishment j, $\pi_{j,t}$, to the direct treatment intensity in industry-county k, which is defined as the interaction between the percent change in the minimum wage applicable to establishment j and the minimum wage bite for event e in the industry-county k that establishment j belongs to:

$$Direct_{j,t} = \Delta \log MW_{j,e(t)} \times Bite_{k(j),e(t)} \tag{4}$$

Note that $\Delta \log MW_{j,e(t)}$ does not contribute to the identifying variation and simply scales the bite variable (the main identifying variation) such that the estimated coefficients are interpretable as pass-through elasticities at a given $Bite_{k(j),e(t)}$ (I show in Appendix D that results are similar when $Bite_{k(j),e(t)}$ is not scaled by $\Delta MW_{j,e(t)}$). Month-year fixed effects γ_t account for time-varying factors affecting cannabis prices that equally apply to all establishments, such as seasonality or COVID-19 effects. Since the identifying variation is at the county level, standard errors are clustered by county to allow for autocorrelation in unobservables within counties, as in Bertrand et al. (2004).

For a given minimum wage bite, the parameter β_l measures the percent change in establishment j's prices resulting from a one percent increase in the minimum wage l months after the minimum wage hike (or l months before when l is negative). While inflation is the dependent variable, I follow previous studies and present the estimates as the effect of the minimum wage on the price level (see e.g. Renkin et al. (2022); Leung (2021)). I thus normalize the effect to zero in a baseline period m months before each hike and report the cumulative treatment effect as the sum of β_l at various lags: $E_L = \sum_{l=-m}^{L} \beta_l$. The pre-treatment coefficients are reported in a similar manner with $P_L = -\sum_{l=m}^{-L-1} \beta_{-l}$.[12]

[12] Summed distributed lag coefficients are numerically equivalent to the parameter estimates from an event study design with binned endpoints. Since distributed lag coefficients measure treatment effect changes, one fewer lead has to be estimated compared to an event study specification (Schmidheiny and Siegloch, 2023).

An important consideration is the number of leads and lags to include in equation 3. One limitation is that minimum wage hikes occur in exact 12 month intervals, meaning event indicators get highly collinear when l is large. Another issue is that the establishment panel is not balanced, meaning that changes in the underlying sample may affect estimates when l is large (Renkin et al., 2022). Therefore, in my baseline estimation I opt for a non-overlapping 12-month event window. I show in appendix D that treatment effects remain stable over a longer event window.

One limitation is that my research design cannot distinguish between the effects of minimum wage legislation and implementation. If firms are forward-looking in their price setting, prices may adjust when a minimum wage hike is announced rather than when the hike actually takes effect.[13] The first two hikes in my sample period were announced in 2016, two and three years prior to implementation, respectively. Because my sample runs from August 2018 through July 2021, any price effects from that announcement fall outside of the sample window and cannot be estimated. For the third event, the magnitude of the hike was announced three months prior to implementation, meaning price effects at announcement can be directly observed using my event study framework. As detailed in Appendix D.2, I find no evidence of price effects at announcement but large effects at implementation for both wholesale and retail prices. This indicates that cannabis establishments wait until the cost shock hits before adjusting prices even if they have full prior knowledge about the magnitude of the shock.

5.2 Effect on retail prices

In contrast to manufacturers, cannabis retailers can face an indirect cost shock in addition to the direct cost shock from minimum wage hikes. Therefore, estimating equation 3 for retailers may not capture the full impact of minimum wage hikes on retail cannabis prices. To capture both direct and indirect effects, I augment equation 3 as follows,

$$\pi_{j,t} = \sum_{l=-5}^{6} \beta_l Direct_{j,t-l} + \sum_{l=-5}^{6} \psi_l Indirect_{j,t-l} + \gamma_t + \epsilon_{j,t}. \tag{5}$$

where $Indirect_{j,t}$ is a scaled shift-share instrument that measures the weighted average minimum wage exposure of the manufacturers that retailer j purchases from. $Indirect_{j,t-l}$ is calculated as follows:

$$Indirect_{j,t} = \sum_{m=s}^{S} \alpha_{j,m} \Delta \log MW_{m,e(t)} \times \sum_{m=s}^{S} \alpha_{j,m} Bite_{k(m),e(t)} \tag{6}$$

[13] Renkin et al. (2022), for example, find that price effects occur primarily in the three months following the passage of minimum wage legislation rather than after the hike itself.

Here, $\Delta MW_{m,e(t)}$ is the percent increase in the minimum wage applicable to manufacturer m for event e; $\alpha_{j,m}$ is the average share of retailer j's wholesale expenditures at manufacturer m from $t-4$ through $t-2$, i.e. in the months leading up to the hike; and $Bite_{k(m),e(t)}$ is the minimum wage bite for the industry-county k that manufacturer m is located in.[14] The first term in equation 6 measures the average minimum wage hike for the set of manufacturers that retailer j purchases from. As before, this term does not contribute to the identifying variation and serves as a scale factor such that the estimates of ψ_l are interpretable as pass-through elasticities.[15] The second term in equation 6 is a shift-share instrument that measures the weighted average minimum wage exposure of the manufacturers that retailer j purchases from.[16]

In equation 5, the indirect minimum wage pass-through elasticity flows from the parameter ψ_l. For a given level of exposure to upstream minimum wage bite, ψ_l measures the percent change in store j's prices that is solely attributable to the effect of the hike on store j's wholesale costs.

A key assumption is that equation 5 separately identifies the direct and indirect effects. Two conditions would need to be jointly met for the direct pass-through estimates to be contaminated by indirect pass-through. First, retailers would need to purchase predominantly from manufacturers located in the retailer's own county. Second, the bite variable for retailers would need to correlate with bite for manufacturers within each county.[17] In Appendix E, I show that the neither of these conditions holds: over 85% of retailers' wholesale purchases are from manufacturers located in other counties, and the (conditional) within-county correlation between manufacturer and retail bite is -0.03.

To further underscore the validity of my results, I examine whether the direct pass-through estimates for β_l change if I exclude $Indirect_{j,t}$ from equation 5. If estimates for direct pass-through were to change, this would cast doubt on the main identification strategy. I show in Section 6.1 that direct pass-through estimates are unaffected by the exclusion of $Indirect_{j,t}$.

Finally, I verify the indirect effect estimate using an alternative approach in Appendix G. Specifically, I leverage the richness of the scanner data and estimate a canonical cost pass-through regression relating the retail unit price to the wholesale unit price at the store-product-month level. This delivers an estimate of the wholesale cost pass-through elasticity, i.e. the effect on retail prices from a 1% increase in wholesale unit cost. This elasticity,

[14] Note that a retailer and a manufacturer located in the same county will have different bites since they belong to different three-digit industries.

[15] One could instead directly interact manufacturer expenditure share, hike size, and manufacturer bite as follows: $Indirect_{j,t} = \sum_{m=s}^{S} \alpha_{j,m} \Delta MW_{m,e(t)} Bite_{k(m),e(t)}$. Results are virtually identical under this definition of indirect treatment. However, the advantage of averaging before interacting (as in equation 6) is that the coefficient ψ_l can be interpreted as a pass-through elasticity.

[16] I consider only a reduced-form shift-share design.

[17] If the first condition is met but the second condition doesn't hold, then the minimum wage effect on wholesale prices is part of the error term but it is orthogonal to retail bite and hence does not bias direct pass-through estimates. If the second condition holds but not the first, then manufacturer bite and retailer bite are not independent but the minimum wage effect on wholesale prices in a given county has no impact on retail prices in that county since retailers don't purchase from local manufacturers.

combined with the minimum wage elasticity of wholesale prices (from equation 3), allows me to compute the implied indirect pass-through elasticity. The resulting indirect effect estimate is very similar to that obtained from equation 5.

6 Empirical results

6.1 Main results

Wholesale prices

Figure 3, shows the estimated effect of minimum wage hikes on wholesale cannabis prices. Panel A shows the distributed lag estimates while Panel B depicts cumulative effects on the price level relative to the month before a minimum wage hike. One question regarding the wholesale price estimates is whether to control for a treatment-specific pre-trend, since the baseline specification in Panel B reveals a slight downward trend in the pre-treatment period. The trend is interrupted by a large and highly statistically significant treatment effect in the period that the minimum wage hike occurs, but the contemporaneous treatment effect is slightly undone in subsequent periods as the pre-trend continues into the post-treatment period. Thus, while the trend does not mask the effect in period t, failure to account for the trend changes the interpretation of the results over a longer time horizon.[18] I apply two common strategies to control for the pre-trend, both of which yield similar results. First, I include region-by-time FE (i.e. interactions between month-of-sample and region dummy variables) to account for regional economic trends that may covary with bite and inflation. The regions are based on the three major socioeconomic regions in Washington state (west, central, east), where each region includes a subset of counties. To the extent that unobserved time-variant heterogeneity is common within regions, region-time FE will control for the treatment-specific trend (Neumark et al., 2014). Second, I apply the two-step procedure from Goodman-Bacon (2021) and re-estimate equation 3 using a trend-adjusted dependent variable. Specifically, I calculate the average of the distributed lag estimates (from equation 3) in the pre-baseline period and then extrapolate this pre-trend through the 12-month event window to obtain the treatment-specific linear trend $\hat{\pi}_{j,t}$. I then subtract the linear trend from the original dependent variable to get the trend-adjusted variable $\tilde{\pi}_{j,t(e)} = \pi_{j,t} - \hat{\pi}_{j,t}$. This procedure has been applied elsewhere in the minimum wage literature (see e.g. Bossler and Schank, 2022). As argued by Rambachan and Roth (2023), this assumes that the observable linear pre-trend is a valid counterfactual for the unobservable post-trend. I view this as a valid assumption since the mean observable post-treatment trend is -0.00074 (95% CI: -0.00218, 0.00070) which is nearly identical to and not statistically significantly different from the pre-treatment trend of -0.00077, (95% CI: -0.00213, 0.00059).

[18] The sample period coincides with a wholesale supply glut and falling wholesale prices (Schaneman, 2023). The trend could reflect covariation between the supply glut and minimum wage bite.

Figure 3 Panel B illustrates that the period t treatment effects are large and statistically significant at the 1-5% level for all three specifications. At the average bite (17.20%), a 10% increase in the minimum wage corresponds to a 1.07% increase in wholesale prices with the unadjusted dependent variable; 1.21% for the trend-adjusted specification; and 1.40% with region-time FE. In the latter two specifications, the pre-treatment period shows no significant trend and the large contemporaneous inflationary effect is no longer undone by the continuation of the pre-trend into the post-treatment period.[19] Thus, it matters little how one controls for the trend, as the linear trend adjustment and region-time FE specifications both lead to a permanently higher wholesale price level effect.

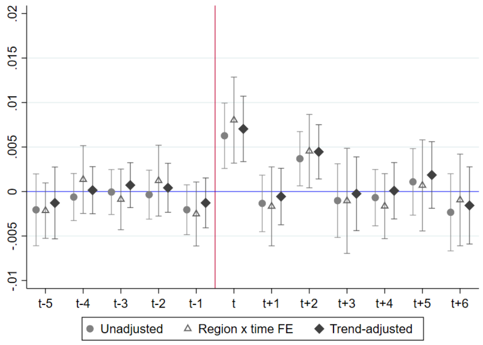
Retail prices

Figure 3 Panels C and D report estimates of the direct effect on retail prices. The figures show that the direct effect is similar whether or not one includes indirect bite as a control variable in equation 5. This supports the idea that the bite variable in equation 5 uniquely identifies the direct effect and avoids picking up the indirect effect of minimum wage hikes on prices. The figure also illustrates that the direct effect for retailers differs from that of manufacturers in several respects. First, effects for retailers show no pre-trend. Second, the treatment effect appears in $t-2$, i.e. one period prior to that for manufacturers, suggesting that retailers may be more forward-looking in their pricing than manufacturers. This is consistent with the findings of Hollenbeck and Uetake (2021), who find that Washington's cannabis retailers in have substantial market power and behave like local monopolists. Given the earlier treatment effect, I normalize the baseline period in $t-2$ when calculating cumulative effects on retail prices. For retailers at the average bite (19.37%), a 10% increase in the minimum wage corresponds to a 0.64% jump in prices in period t solely via the direct cost shock.

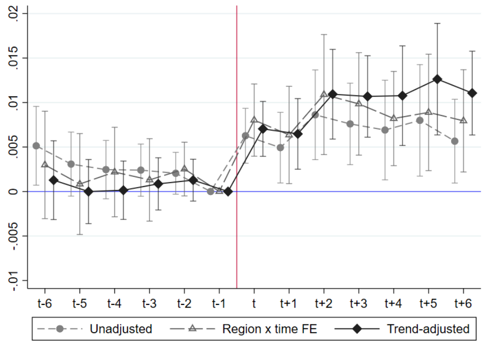
Panels E and F report estimates of the indirect effect on retail prices. The figures reveal a downward-sloping pre-trend interrupted by an inflationary shock in the treatment period (significant at the 10% level, see Panel E) followed by a continuation of the pre-trend into the post-treatment period. This trend mirrors the trend from the wholesale price regression in Panel B and is indicative of wholesale cost pass-through on the part of retailers. Unlike with the wholesale price regression, however, region-time FE cannot be used to control for the pre-trend since the dependent variable and indirect bite stem from different sets of establishments and retailers source a sizable share of their products from manufacturers located in other regions of the state (see Appendix A). Therefore, I apply the Goodman-Bacon (2021) procedure and re-estimate equation 5 with the dependent variable adjusted for the indirect bite-specific trend. Panel E illustrates that adjusting for the pre-trend does not meaningfully change the indirect effect in period t but results in pre-and post-treatment distributed lag coefficients that are closer to zero. The corresponding cumulative effects in Panel F

[19] At higher lags, the price level effects from the specification with region-time FE are slightly lower than the trend-adjusted regression, but the difference is not statistically significant.

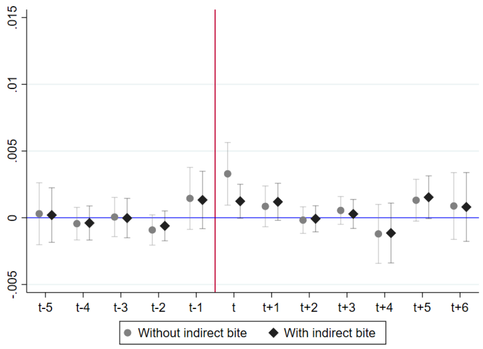
Figure 3: Effect of minimum wage hikes on cannabis prices



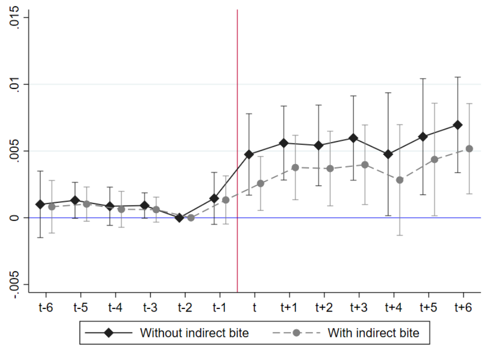
(A) Wholesale inflation, direct effect



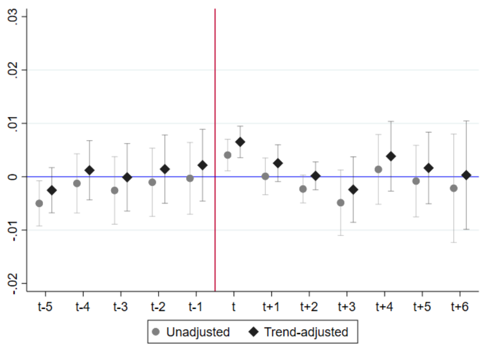
(B) Wholesale price level, direct effect



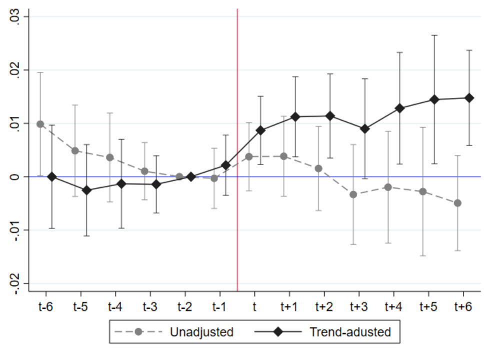
(C) Retail inflation, direct effect



(D) Retail price level, direct effect



(E) Retail inflation, indirect effect



(F) Retail price level, indirect effect

Notes: the figure shows estimated treatment effects from minimum wage hikes on cannabis prices. Coefficients in panels (A), (C), and (E) are interpretable as the percentage point effect on establishment-level price changes. Coefficients in panels (B), (D), and (F) are interpretable as cumulative price level effects (E_L) relative to the baseline period. The figure shows 90% confidence intervals based on SE clustered at the county level. The dependent variables are the establishment-level log price index. Data source: Top Shelf Data and Washington ESD, July 2018 to August 2021.

illustrate that the pre-treatment period shows no significant trend and the large contemporaneous inflationary effect is no longer undone by the continuation of the pre-trend into the post-treatment period. At the average indirect bite (18.14%), a 10% minimum wage hike corresponds to a 1.5% increase in retail prices through the indirect effect, i.e. solely through the effect of minimum wages on wholesale prices.

When combining the results from Panels D and F, I find a cumulative (direct + indirect) effect on retail prices of 0.64% + 1.5% = 2.14%. Thus, the indirect effect accounts for approximately 70% of the retail price response to minimum wage hikes, which roughly corresponds to the COGS share in cannabis retailers' variable costs (see Appendix A).

6.2 Threats to identification and robustness checks

Endogenous treatment and parallel trends

My empirical strategy for the direct effect relies on two main identifying assumptions. The first assumption is that treatment timing is independent of the outcome. This clearly holds in my setting since Washington's minimum wage schedule was announced in 2016, i.e. two years before the first treatment. The second assumption relates to parallel trends and requires that price changes at establishments with a given bite reflect what would have happened to all other establishments had they had that bite.[20] This assumption is violated if price changes drive treatment intensity. Since the treatment intensity is the product of two variables, $\Delta MW_{j,t-l} \times Bite_{k(j),t-l}$, such reverse causality must be addressed for each of these variables in turn. $\Delta MW_{j,t-l}$ would suffer from reverse causality if policymakers set the size of minimum wage hikes in proportion to local price changes (e.g. in an effort to keep real wages constant). This is not the case with the statewide hikes in my sample, since their sizes are either predetermined or linked to the CPI-W, a national—not local—price index.[21] $Bite_{k(j),t-l}$ would suffer from reverse causality if county-level price changes drove wages. Testing for this amounts to checking for differential pre-trends when estimating equation 3. Figure 3 Panel B shows a slight downward trend for the wholesale regression but the trend goes in the opposite direction of the treatment effect and disappears when region-time FE are included (see the discussion in Section 6.1). Figure 3 Panel D shows a flat pre-trend for the retail regression. Thus, the results from Figure 3 speak against reverse causality driving the observed price effects.

Despite the lack of pre-trends, it is still possible that prices at establishments with high bite would have evolved differently than establishments with low bite had the former had low bite. Callaway et al. (2024) show that such "selection bias" breaks the causal interpretation of the DiD estimate. I account for all time-invariant factors that could lead to selection bias,

[20] Callaway et al. (2024) refer to this as "strong" parallel trends.

[21] The city of Seattle has a citywide minimum wage that could be endogenous for some businesses for event 3 (January 1st, 2021). I address this possibility in Appendix D.2 and show that the main results are robust to dropping Seattle establishments for event 3.

such as establishment size and average revenues, through first-differencing which sweeps out establishment fixed effects. Further evidence against selection bias is provided in Table 2, which compares the pre-treatment characteristics of establishments above and below the median bite for each establishment type. The table shows that prior to minimum wage hikes, the unit price, unit price growth, quantity sold, monthly revenue, and product variety are similar at establishments with high bite compared to establishments with low bite. In Appendix Table D1, I show that this holds even when comparing establishments below the 25th percentile and above the 75th percentile of bite. The similar pre-treatment characteristics among establishments with high versus low bite speaks against selection bias in my setting.

Shift-share instrument validity

In equation 5, the main identifying variation in the indirect bite variable stems from retailers' differential exposure to manufacturers' minimum wage bite. Goldsmith-Pinkham et al. (2020) show that two conditions must hold for the shift-share instrument to be consistent. First, the instrument must have predictive power for changes in retailers' wholesale cost, i.e. first-stage relevance. To demonstrate this, I construct a wholesale cost index for each retailer and estimate equation 5 with this index as the dependent variable. Appendix Figure E8 confirms that the shift-share instrument induces a sharp and highly significant increase in retailers' wholesale costs in $t-1$. Second, retailers' wholesale expenditure shares only affect retail price growth through indirect pass-through and not through other potential confounding channels (i.e. the exclusion restriction). To test the plausibility of this assumption, Goldsmith-Pinkham et al. (2020) propose inspecting the correlation between pre-treatment characteristics of retail stores and the expenditure shares for the main manufacturer-counties that drive identification. If, for example, retailers' own bite variable covaries with their wholesale expenditure shares, then wholesale expenditure shares may be endogenous and the instrument may not be consistent. I report the correlates in Appendix Table E6. The table shows that retailers' bite and other store and location characteristics do not explain variation in wholesale expenditure shares, which provides suggestive evidence that retailers' expenditure shares are exogenous.

Further robustness checks

I conduct a number of additional robustness checks to rule out other factors potentially driving my findings. I report results from these additional robustness checks in Appendix D. First, I extend the event window to 19 months and show that the pre-treatment effects remain small and insignificant while the main treatment effect estimates remain significant nine months after minimum wage hikes (Figure D1).

To ensure that seasonal labor fluctuations do not cause endogeneity in the bite variable, I check whether results change if the bite variable is based on Q4 wages rather than Q3 wages. As Column 1 in Appendix Tables D2 and D3 illustrates, results are robust to using this alternative bite variable.

To confirm that my results are not driven by market entry or exit, I restrict the sample to establishments that are present at least 10 months for a given 12-month event. Appendix Tables D2 and D3 show that results are robust to using this more balanced sample.

Since the establishment-level price indexes are constructed using annual product and subcategory weights, the weights change at the same time as the minimum wage hike. To ensure that effect sizes are not an artifact of this weighting scheme, I use alternate weights based on a fiscal year starting in July and ending in June each year (i.e. six months offset from the weights in the baseline model).[22] Results are unaffected by this alternate weighting scheme (see Tables D2 and D3).

I also consider the possibility that firms may not fully comply with the new minimum wage. If that were the case, the bite variable would not accurately measure minimum wage exposure since higher bite would not translate into a larger cost increase for firms. To net out non-compliance, I redefine the bite variable as the difference between bite two quarters before and one quarter after the hike. In Appendix D.2 I show that results are robust to this alternative bite variable.

In Appendix D.2, I consider the possibility that the third minimum wage hike is endogenous in Seattle and I show that results are robust to dropping Seattle establishments for that event.

Next, I set the treatment intensity equal to the minimum wage bite itself to show that results do not rely on interacting bite with the size of the minimum wage hikes (Appendix D.2).

Finally, I test whether the results are sensitive to changing the level of industry classification used to measure minimum wage bite. I construct an alternative bite variable based on the more granular five-digit NAICS codes to show that the main results do not depend on the level of industrial classification.

Alternative specifications

In Table 3, I present several variants of my empirical strategy for manufacturers. I use the linear trend-adjustment as my preferred specification, as the treatment effects are not statistically significantly different from the unadjusted or region-time FE specifications but the pre-trend is removed. I normalize the baseline period in $t-2$ so that cumulative effects on wholesale and retail prices line up temporally. Changing the baseline period has no bearing on the distributed lag estimates and simply amounts to a downward shift in cumulative wholesale price level effects. All specifications include month-year FE. Column 1 shows the baseline specification. Column 2 shows that effects are similar when region-time FE are included. Treatment effects are unchanged when the dependent variable is no longer trimmed by 0.5%

[22] For the weights to cause endogeneity, the change in product and subcategory revenue shares within an establishment would need to covary with bite.

(Column 3).

DiD with continuous treatment produces a weighted average of all possible 2×2 comparisons of changes in outcomes for higher bite establishments relative to lower bite establishments. Callaway et al. (2024) show that the weights are all positive and integrate to one, but that comparisons between establishments with large differences in bite receive the most weight. To ensure that my parameter estimates are not driven by extreme comparisons, I trim the bite variable by by 0.5%. Column 4 shows that results are unchanged, indicating that the main results are not driven by treatment intensity outliers.

Columns 5-7 show results when the dependent variable is not adjusted for a linear pre-trend. Column 5 shows that with no linear trend adjustment, effects for the baseline specification are statistically significant, though smaller, through $t + 2$, but effects are undone by $t+4$ due to the continuation of the pre-trend. Column 6 shows an upward shift in effect sizes when the baseline period is set to one month before effects appear rather than two months before. At the same time, the pre-treament effects are larger and significant at the 10% level, relfecting that the final period with the negative trend has been shifted from post-treatment to pre-treatment. In Column 7, region-time FE are used to control for the pre-trend.

Table 4 shows that retail price effects are similarly stable across specifications. Compared to the baseline specification, the direct effect on retail prices is similar with region-time FE (Column 2); when the dependent variable is winsorized by 0.5% (Column 3); and when the bite variable is trimmed by 0.5% (Column 4). In Column 5, the dependent variable is adjusted for the indirect bite-specific trend visible in Figure 3 Panel E. The resulting estimates for the direct effect remain similar to the baseline estimates. This indicates that the indirect bite-specific trend is independent of the direct effect, and provides further evidence that the direct and indirect bite variables in equation 5 are independent of each other.

Columns 6-8 show estimates for the indirect effect on retail prices when the dependent variable is adjusted for the indirect bite-specific trend. Compared to the baseline specification, effects are similar when including region-time FE (Column 7) and winsorizing the dependent variable by 0.5% (Column 8). In Column 9, the dependent variable is not adjusted for the indirect bite-specific trend. The pre-treatment effect is large and positive, and pre- and post-treatment effects are monotonically decreasing in event-time. However, the trend is interrupted by a period t treatment effect that is the same magnitude as in the trend-adjusted regression in Column 1. This illustrates that trend-adjustment does not drive the primary finding that minimum wage hikes induce an indirect effect on retail cannabis prices.

Finally, Column 10 reports the combined direct and indirect effects when the dependent variable is adjusted for the indirect bite-specific trend. When compared with Column 1, the large effect size in Column 10 illustrates how the overall impact of minimum wage hikes on retail cannabis prices is driven primarily by the indirect effect.

Table 3: Direct effect on wholesale prices

	Trend-adjusted				Unadjusted		
	(1) Baseline	(2) Reg.-time FE	(3) Outliers	(4) Trimmed bite	(5) Baseline	(6) t-1 base	(7) Reg.-time FE
E_0	0.006***	0.007***	0.005**	0.006***	0.004**	0.006***	0.006**
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
E_2	0.010***	0.01***	0.010***	0.010***	0.007**	0.009***	0.008*
	(0.003)	(0.004)	(0.004)	(0.003)	(0.003)	(0.003)	(0.004)
E_4	0.009***	0.01***	0.01***	0.010***	0.005	0.007**	0.006
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.003)	(0.004)
$\sum$ Pre-event	1.0e-07	-0.003	2.0e-07	2.0e-07	0.003	0.005*	0.001
	(0.003)	(0.003)	(0.004)	(0.003)	(0.003)	(0.003)	(0.003)
N	14,777	14,777	14,932	14,735	14,777	14,777	14,777

Notes: Dependent variable: establishment-level inflation rate (1% trim). Listed coefficients are sums of the distributed lag coefficients E_L, L months after minimum wage hikes, relative to the normalized baseline period in $t-2$. Standard errors are clustered at the county level and are shown in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Data from Washington ESD and Top Shelf Data, July 2018-August 2021.

Table 4: Effect on retail prices

	Direct					Indirect				Combined
	(1) Baseline	(2) Reg.-time FE	(3) Winsorized	(4) Trimmed bite	(5) Detrend	(6) Baseline	(7) Reg.-time FE	(8) Winsorized	(9) Unadjusted	(10) Baseline
E_0	0.005**	0.002	0.003**	0.005**	0.003**	0.009**	0.010***	0.008**	0.005	0.011***
	(0.002)	(0.003)	(0.001)	(0.002)	(0.001)	(0.004)	(0.004)	(0.003)	(0.004)	(0.004)
E_2	0.005***	0.003	0.003**	0.005***	0.004**	0.011**	0.012**	0.010**	0.002	0.015***
	(0.002)	(0.003)	(0.002)	(0.002)	(0.002)	(0.005)	(0.005)	(0.004)	(0.005)	(0.005)
E_4	0.005*	0.004	0.003	0.004	0.003	0.013**	0.013**	0.011**	-0.002	0.016***
	(0.003)	(0.004)	(0.002)	(0.003)	(0.003)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
$\sum$ Pre	0.001	0.002	0.001	0.001	0.001	1.24e-07	-0.001	-0.001	0.010	0.001
	(0.002)	(0.001)	(0.001)	(0.002)	(0.001)	(0.006)	(0.006)	(0.005)	(0.006)	(0.006)
N	14,189	14,189	14,189	14,095	13,689	13,689	13,689	13,689	13,689	13,689

Notes: Dependent variable: establishment-level inflation rate. Listed coefficients are sums of the distributed lag coefficients E_L, L months after minimum wage hikes, relative to the normalized baseline period in $t-2$. Standard errors are clustered at the county level and are shown in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Data from Washington ESD and Top Shelf Data, July 2018-August 2021.

6.3 The role of market structure and firm characteristics

To gain more insight into the role of market structure in minimum wage pass-through, I conduct two main heterogeneity analyses. First, I examine whether direct pass-through differs by the scale of production. Second, I investigate whether direct pass-through varies by market concentration.

Scale of production

Wholesale prices. I exploit LCB rules that separate manufacturers into tiers governing their production capacity.[23] I define small manufacturers as those permitted to grow up to 10,000 square feet of plant canopy and large manufacturers as those that can grown up to 30,000 square feet of canopy. I split my sample into two subgroups comprising small and large manufacturers, and estimate my main specification (equation 3) separately for each of these subgroups.

In Table 5, I report results for both the trend-adjusted dependent variable (columns 1-2) and the unadjusted dependent variable (columns 3-4). Column 1 reveals a large and statistically significant effect of minimum wage hikes on wholesale prices at small manufacturers. At the average bite (17.2%), a 10% increase in the minimum wage corresponds to a 2% increase in wholesale prices four months after a hike. In contrast, the effect is close to zero and statistically insignificant for large manufacturers (Column 2). Columns 3 and 4 confirm that these results carry over when the dependent variable is not adjusted for the bite-specific trend.

Two features of cannabis production help explain why minimum wage pass-through decreases with the scale of production. First, the high fixed costs associated with indoor cannabis cultivation (Washington State Liquor and Cannabis Board, 2019; Caulkins and Stever, 2010; Caulkins et al., 2018), along with the capacity constraints imposed by the LCB, suggest that large manufacturers may benefit from economies of scale. Second, manufacturers manufacture a tradable good within the state and sell to retailers across the entire state (see Appendix Table E4). This suggests that the wholesale cannabis industry may be highly competitive. To test this, I use market concentration as a proxy for the degree of competition and calculate the average monthly HHI under different geographic definitions of the wholesale market.[24] I find a mean monthly HHI ranging from 0.10 (regional market) to 0.006 (statewide market), indicating that the wholesale cannabis industry has a low market concentration and is highly competitive. Taken together, low market concentration and

[23]Manufacturer licenses are based on a three-tier system governing the square footage of plant canopy that an establishment is legally permitted to operate. Tier 1 manufacturers can grow up to 2,000 square feet of plant canopy, tier 2 can grow up to 10,000 square feet, while tier 3 can operate up to 30,000 square feet. Tiers were assigned to establishments before the market fist opened in 2014 and once assigned an establishment cannot switch tiers.

[24]I first define markets at the relatively granular three-digit zip code level, then at the region, and finally at the aggregated state level.

economies of scale are consistent with minimum wage pass-through that decreases with the scale of production. Since the wholesale market is highly competitive, large manufacturers (i.e. those with more market power) may adjust to the minimum wage cost shock along margins other than price, whereas small manufacturers (i.e. those with little or no market power) may have no choice but to pass the costs through to wholesale prices.

These results have interesting implications for the effect of minimum wage hikes downstream. Retailers located in the same local market—and hence with roughly the same direct minimum wage cost shock—may have very different indirect cost shocks depending on their wholesale purchasing patterns. Retailers that purchase predominantly from small manufacturers may be exposed to a large indirect cost shock, while retailers that purchase from large manufacturers may have little (or no) indirect cost shock.

Retail prices. At the retail level, I use chain size as a proxy for the scale of production. I define chains as stores belonging to firms with three or more establishments, and consider one- and two-store firms as independent. Of the 511 retail stores in my sample, 330 are independent and 181 belong to a chain. I sort stores into two groups depending on whether they are independent or belong to a chain, and I estimate my main specification separately for each of these groups.[25]

Columns 5-6 show that the direct effect is twice as high at chain stores than at independent stores. This suggests that chain stores are better able to pass the cost shock through to consumers compared to independent stores. Two features of the retail cannabis market can explain this pattern. First, since online sales are prohibited, retailers sell a non-tradable good in highly localized markets, and retailers have been shown to behave like local monopolists (Hollenbeck and Uetake, 2021). Second, Hollenbeck and Giroldo (2022) find that market power increases with chain size for individual cannabis stores due to increasing returns to scale. Taken together, increasing returns to scale and local monopoly power are both consistent with minimum wage pass-through that increases with chain size.

To summarize, direct minimum wage pass-through in the cannabis industry *decreases* with the scale of production upstream but *increases* with the scale of production downstream. This contrast reflects differences in tradability upstream versus downstream, and this finding has implications for other retail sectors beyond cannabis. In industries where such a mismatch in tradability exists, market power can have different implications for cost pass-through at different points of the supply chain. More generally, this highlights the role of tradability in determining the extent to which firms with market power can pass costs through to prices.

[25]Results are similar when defining independent as single-store firms and chains as firms with two or more stores.

Market concentration

Next, I investigate whether direct pass-through varies by market concentration. Since manufacturers sell to retailers across the state, there is no obvious criteria for defining market concentration upstream. In contrast, the fact that cannabis retailers operate brick-and-mortar stores makes geographic concentration a natural criterion for characterizing retail markets. Therefore, I limit the analysis to retail stores. I consider each store in my sample as the focal point of its own market comprising the set of stores (including the focal store) within a 5-mile radius.[26] I calculate the Herfindahl–Hirschman Index (HHI) for each market in the sample, and divide the sample into two subgroups for stores above and below the sample median HHI.[27] I estimate my main specification on each subsample to test whether direct minimum wage pass-through differs between stores above and below the median HHI. Column 7 in Table 5 shows large and highly significant direct pass-through effects at stores in markets with low concentration, while column 8 shows effects that are small and not statistically significantly different from zero at stores in highly concentrated markets. This is a somewhat surprising result since one might expect stores in highly concentrated markets to exercise local monopoly power, and hence be better able to pass costs through to relatively captive consumers. One possible reason that pass-through is higher in less concentrated markets could be strategic complementarity in prices. Direct minimum wage exposure is expected to be similar across stores in a given local market, meaning the minimum wage hike represents a market-wide cost shock. In imperfectly competitive markets, pass-through of a market-wide cost shock increases with the number of firms (Muehlegger and Sweeney, 2022). I investigate strategic complementarity in cannabis prices in Section 6.4.

6.4 Accounting for strategic complementarity in prices

In oligopolistic markets, the price a firm sets is a function of not just its own costs, but also those of its rivals (i.e. strategic complementarity in prices). To the extent that establishments compete within their county, the estimates from equation 3 will capture strategic complementarities, since the bite variable is specified at the county level. However, if establishments compete over a larger geographic area, then the estimates from equation 3 fail to capture the full scope of strategic complementarity in prices and thus provide an incomplete picture of the effects of minimum wage hikes on prices. To account for potential strategic complementarities, I augment my main empirical equation with a measure of nearby rivals' minimum wage-induced cost shocks. I estimate the following equation separately for manufacturers

[26] Concentration is endogeneous if profitability affects market entry. Since the LCB caps the number of retail licenses and distributes them according to population density, profitability does not directly affect concentration in my setting.

[27] In defining markets as cannabis-only, I assume that cannabis products do not compete with products from other industries like alcohol and tobacco. This assumption may not hold perfectly, as Miller and Seo (2021) find that cannabis legalization reduced demand for alcohol by 15% and cigarettes by 5%, suggesting that cannabis, alcohol, and tobacco are substitutes to a certain extent.

Table 5: Price effects by market structure and scale of production

	Wholesale				Retail			
	Trend-adjusted		Unadjusted					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Small	Large	Small	Large	Indep.	Chains	Low con-centr.	High con-centr.
E_0	0.007*	0.003	0.009*	-0.001	0.003	0.004**	0.005***	0.002
	(0.003)	(0.003)	(0.004)	(0.004)	(0.001)	(0.002)	(0.001)	(0.002)
E_2	0.013**	0.0002	0.013*	-0.002	0.002	0.005***	0.004***	0.002
	(0.004)	(0.004)	(0.006)	(0.005)	(0.002)	(0.001)	(0.001)	(0.002)
E_4	0.012*	-0.0006	0.011	-0.006	0.003	0.006***	0.007***	9.24e-6
	(0.006)	(0.007)	(0.007)	(0.008)	(0.002)	(0.002)	(0.002)	(0.003)
$\sum$ Pre-event	2.05e-08	-1.28e-08	0.005	-0.010	-0.0004	0.001	-0.0002	-6.53e-5
	(0.003)	(0.005)	(0.004)	(0.006)	(0.001)	(0.003)	(0.0008)	(0.002)
N	9,641	5,136	9,641	5,136	9,289	4,755	7,288	6,756
Region-time FE	NO	NO	YES	YES	NO	NO	NO	NO

Notes: Dependent variable: establishment-level inflation rate (1% trim). Listed coefficients are sums of the distributed lag coefficients E_L, L months after minimum wage hikes, relative to the normalized baseline period in $t-2$. In columns 1 and 2 the dependent variable is adjusted for a bite-specific trend as detailed in section 6.1. See main text for description of specifications and columns. Standard errors are clustered at the county level and are shown in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Data from Washington ESD and Top Shelf Data, July 2018-August 2021.

and retailers:

$$\pi_{j,t} = \sum_{l=-5}^{6} \beta_l Direct^o_{j,t-l} + \sum_{l=-5}^{6} \psi_l Indirect^o_{j,t-l} + \sum_{l=-5}^{6} \lambda_l Direct^r_{-j,t-l} + \gamma_t + \epsilon_{j,t} \tag{7}$$

The first two treatment variables are the same as in equation 5, with the superscript o highlighting that they capture an establishment's own direct and indirect treatment variables. $Direct^r_{-j,t}$ is the average bite at establishments that are in neighboring counties and within 30 miles of establishment j, weighted by the inverse distance to establishment j.[28] This specification is similar to the one used in Muehlegger and Sweeney (2022), who estimate own and rival firms' cost pass-through in the U.S. oil refining industry. λ_l captures the effect of the minimum wage-induced cost shock at rival establishments on an establishment's own prices.

I report results in Table 6. Column 1 reproduces the baseline specification (with no rivals) for wholesale prices from Section 6.1. Column 2 shows that including rivals' bite does not meaningfully change the effect of minimum wage hikes on wholesale cannabis prices. This is consistent with the fact that manufacturers sell a substantial portion of their products to retailers located in other regions in Washington state (see Appendix Table E4). In other words, manufacturers manufacture a tradable good (within Washington state) and do not compete in local markets.

When estimating equation 7 for retailers, two facts stand out. First, comparing the first three rows of Columns 3 and 4 illustrates that rivals' bite does not meaningfully change the own-cost pass-through estimates. Second, the combined effect of own and rivals' bite substantially increases the effect minimum wage hikes on retail prices (see the bottom panel of Column 4). At the average bite (19.37), a 10% minimum wage hike increases retail prices 1.9% with rival bite, compared to 0.97% without rival bite, two months after the hike. This sensitivity to rivals' costs reflects that cannabis retailers compete in local markets, consistent with brick-and-mortar sales and transportation costs on the part of consumers.

Next, I investigate whether strategic complementarity in prices also applies to the indirect cost shock for retailers:

$$\pi_{j,t} = \sum_{l=-5}^{6} \beta_l Direct^o_{j,t-l} + \sum_{l=-5}^{6} \psi_l Indirect^o_{j,t-l} + \sum_{l=-5}^{6} \theta_l Indirect^r_{-j,t-l} + \gamma_t + \epsilon_{j,t} \tag{8}$$

Here, $Indirect^r_{-j,t}$ is the average indirect treatment for stores within 30 miles of store r, weighted by inverse distance. Column 5 reproduces that baseline results for indirect pass-through to retail prices from Section 6.1. Column 6 shows that rivals' indirect cost shocks have a slightly negative and insignificant effect on retail prices. As a result, the overall

[28] Results are similar with weights based on each rival's average monthly revenue in the months prior to a minimum wage hike. Results are also similar when $RivalBite_{-k(j)}$ is defined as the average bite for the counties that border establishment j's county, weighted by the number of establishments in each bordering county. See Appendix X.

(own + rivals') effect of the indirect cost shock becomes attenuated compared to the baseline specification.

Taken together, the results in Table 6 indicate that retailers are sensitive to their rivals' direct cost shocks but insensitive to their indirect cost shocks. This is consistent with the theoretical prediction that strategic price effects will be larger for aggregate cost shocks compared to idiosyncratic cost shocks. Under the assumption that cannabis retailers located in the same local market have similar wage structures, the direct cost shock is an aggregate cost shock in local markets. In contrast, since stores typically purchase from manufacturers in different locations and with different minimum wage exposures, the indirect cost shock is idiosyncratic to specific retailers in local markets. Retailers that purchase from manufacturers with high minimum wage exposure can face a large indirect cost shock, while stores that purchase from manufacturers with low minimum wage exposure can face a small indirect shock.

7 Cost pass-through rates

In Section 6.1, I obtained reduced form estimates of the effect of minimum wage hikes on retail cannabis prices. In this section, I derive the cost pass-through rates implied by those reduced form elasticities. In Section 7.1, I employ a general theoretical model of firm production to derive the minimum wage elasticity of marginal cost at constant output. In Section 7.2, I conduct an empirical calibration and estimate minimum wage elasticities of marginal cost. In Section 7.3, I combine the estimates from the empirical calibration with the pass-through elasticities from Section 6.1 to derive the implied pass-through rates for the direct and indirect cost shocks.

7.1 Theoretical framework

To quantify the degree of cost pass-through, I first need an estimate of the impact of minimum wage hikes on cannabis retailers' marginal cost. In this subsection, I describe the general theoretical framework and the assumptions necessary to estimate the effect of minimum wages on marginal cost.

I build on the model from Renkin et al. (2022), which I describe in more detail in Appendix K. I assume that cannabis retailers have a production technology $Q = F(X; L)$, where F is homogeneous to some degree. X is a composite input defined by a linear homogeneous aggregator over M different cannabis products, $X_1, X_2, ..., X_M$ with wholesale prices $P_1^x, P_2^x, ...P_M^x$. Similarly, L is a composite input over N different types of labor inputs $L_1, L_2, ..., L_N$ with wages $W_1, W_2, ...W_N$.

This production technology yields two expressions for the minimum wage elasticity of marginal cost. The first corresponds to the effect that runs through the labor component of

Table 6: The price response to own and rivals' cost shocks

	Wholesale		Retail			
			Direct		Indirect	
	(1)	(2)	(3)	(4)	(5)	(6)
	No rivals	30 miles, distance weights	No rivals	30 miles, distance weights	No rivals (indirect)	30 miles, distance weights (indirect)
E_0^o	0.0058***	0.0047**	0.0047**	0.0055**	0.0087**	0.0097**
	(0.0018)	(0.0022)	(0.0019)	(0.0023)	(0.0039)	(0.0044)
E_2^o	0.0097***	0.0074**	0.0054***	0.0064***	0.011**	0.011*
	(0.0032)	(0.0035)	(0.0018)	(0.0022)	(0.0048)	(0.0054)
E_4^o	0.0095***	0.0086**	0.0048*	0.0053	0.013**	0.013*
	(0.0036)	(0.0041)	(0.0028)	(0.0032)	(0.0064)	(0.0069)
E_0^r		0.0023		0.0039		-0.0025
		(0.0028)		(0.0032)		(0.0016)
E_2^r		0.0031		0.0044		-0.0032
		(0.0036)		(0.0030)		(0.0025)
E_4^r		-0.00045		0.0035		-0.0059**
		(0.0041)		(0.0032)		(0.0029)
Estimation summary						
$\sum$ Pre-event	0.00000010	-0.000024	0.0010	-0.000021	0.00000012	0.00011
	(0.0028)	(0.0031)	(0.0015)	(0.0024)	(0.0059)	(0.0071)
$E_0^o + E_0^r$	0.0058***	0.0070**	0.0047**	0.0094**	0.0087**	0.0072
	(0.0018)	(0.0029)	(0.0019)	(0.0047)	(0.0039)	(0.0048)
$E_2^o + E_2^r$	0.0097***	0.011***	0.0054***	0.011**	0.011**	0.0074
	(0.0032)	(0.0040)	(0.0018)	(0.0043)	(0.0048)	(0.0064)
$E_4^o + E_4^r$	0.0095***	0.0081*	0.0048*	0.0087*	0.013**	0.0074
	(0.0036)	(0.0045)	(0.0028)	(0.0052)	(0.0064)	(0.0080)
N	14,777	13,621	14,189	13,632	13,689	13,152
Detrended	YES	YES	NO	YES	YES	YES

Notes: Dependent variable: establishment-level inflation rate. Listed coefficients are sums of the distributed lag coefficients E_L, L months after minimum wage hikes, relative to the normalized baseline period in $t-2$. In columns 1 and 2 the dependent variable is adjusted for a bite-specific trend as detailed in section 6.1. See main text for description of specifications and columns. Standard errors are clustered at the county level and are shown in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Data from Washington ESD and Top Shelf Data, July 2018-August 2021.

marginal cost:

$$\eta_{mc}^{L} = \frac{\partial MC_Q^L}{\partial MW}\frac{MW}{MC_Q^L} = \underbrace{\frac{\overline{W}L}{C}}_{(i)} \cdot \underbrace{\frac{\partial \overline{W}}{\partial MW}\frac{MW}{\overline{W}}}_{(ii)} \tag{9}$$

where ∂MC_Q^L is the change in overall marginal cost that stems from a change in labor cost, C denotes the variable cost of cannabis retailers, and $\overline{W}$ is the average wage the store pays. The elasticity η_L^{mc} is thus the product of two factors: (i) the labor share in costs, and (ii) the minimum wage elasticity of the average wage.

The second expression corresponds to the effect that runs through the COGS component of marginal cost:

$$\eta_{mc}^{cogs} = \frac{\partial MC_Q^X}{\partial MW}\frac{MW}{MC_Q^X} = \underbrace{\frac{\overline{P^x}X}{C}}_{(iii)} \cdot \underbrace{\frac{\partial \overline{P^x}}{\partial MW}\frac{MW}{\overline{P^x}}}_{(iv)} \tag{10}$$

where $\overline{P^x}$ is the average wholesale price the store pays. The elasticity η_{cogs}^{mc} is the product of (iii) the COGS share of costs, and (iv) the minimum wage elasticity of wholesale prices. In the following subsection, I estimate (i)-(iv) and empirically calibrate η_{mc}^{L} and η_{mc}^{cogs}.

7.2 Empirical calibration

Next, I estimate the different components in equations 9 and 10.

Cost shares. I obtained data on cannabis retailers' labor costs from the Washington state ESD and the consulting firm High Peak Strategy. The data covers the years 2018-2020 and contains annual industry-wide information on average labor expenditures for cannabis retailers in Washington state. For COGS, I use the wholesale scanner data described in Section 4 to calculate the average annual wholesale expenditure for cannabis retailers. Taken together, the average annual labor and wholesale costs provide a comprehensive overview of the variable cost structure of cannabis retailers.[29] I estimate that the labor cost share at cannabis retailers is 0.25 and the COGS share is 0.75 (see Table A2). These estimates are similar to those found in other retail settings (Renkin et al., 2022; Leung, 2021).

Minimum wage elasticity of the average wage. I estimate the minimum wage elasticity of average earnings for retail cannabis workers using quarterly industry-by-county-level data from the Quarterly Census of Employment and Wages (QCEW). The QCEW publishes a quarterly count of employment and wages reported by employers covering more than 95 percent of U.S. jobs. For each industry-county, I calculate the average quarterly wage as the total quarterly wages paid divided by the average quarterly employment.[30] I then estimate

[29]In most retail settings, labor costs and COGS account for more than 99% of variable cost (Renkin et al., 2022).

[30]I assume that the elasticity of average earnings equals the elasticity of the average wage. This holds if there are no negative effects of minimum wage hikes on employment. In Appendix **??**, I find no evidence of negative employment effects.

the following two-way fixed effects regression in first-differences

$$\Delta log\overline{W}_{k,q} = \sum_{q=1}^{4} \beta_q \Delta MW_{k,e} \times Bite_{k,e} \times \delta_q + \delta_q + \epsilon_{c,q} \quad (11)$$

$\overline{W}_{k,q}$ is the average wage in industry-county k and quarter q, $\Delta MW_{k,e} \times Bite_{k,e}$ is the minimum wage bite in industry-county k corresponding to minimum wage event e, and δ_q is a quarter indicator that serves as a time FE. Table 7 shows that I find a significant and positive effect of minimum wage hikes on average wages in the quarter immediately following a hike. At the average bite, the minimum wage elasticity of average wages is 0.27 (P-value: 0.001) for cannabis retailers and 0.29 (P-value: 0.020) for manufacturers.

Table 7: Minimum wage elasticity of average wages

	Misc. retail		Crop production	
	Baseline	Controls	Baseline	Controls
Q1 (MW hike)	0.2712***	0.2692***	0.2855**	0.2872**
	(0.0827)	(0.0817)	(-0.1228)	(-0.1194)
Q2	-0.0039	0.0575	0.2494**	0.2012*
	(0.0922)	(0.0850)	(-0.1242)	(-0.1058)
Q3	-0.0633	-0.2228***	-0.1858**	-0.2253***
	(0.0641)	(0.0709)	(-0.0798)	(-0.0760)
Q4	-0.1207	-0.1422*	-0.0858	-0.0810
	(0.0710)	(0.0041)	(-0.0648)	(-0.0643)
N	88	88	106	106

Notes: The table reports minimum wage elasticities of average wages. The elasticities are estimates from equation 11 scaled by the average bite. Misc. Retail corresponds to NAICS 453. Crop production corresponds to NAICS 111. Standard errors are clustered at the county level and are shown in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Data from QCEW and Washington ESD (2018-2021).

Minimum wage elasticity of wholesale prices. In Section 6.1, I estimated a minimum wage elasticity of wholesale cannabis prices of 0.17. Assuming that the elasticity of wholesale prices is equal to the elasticity of wholesale unit cost, the former provides an estimate of the latter in equation 10.[31]

Minimum wage elasticities of marginal cost. Having estimated cannabis retailers' cost shares, the minimum wage elasticity of average wages, and the minimum wage elasticity

[31] This implies that cannabis retailers have low cross-price elasticities for wholesale demand.

of wholesale cost, I can now estimate the minimum wage elasticities of marginal cost from equations 9 and 10. I report the estimates in Table 8. For my baseline specification, the elasticity for the labor cost portion of marginal cost, η_{mc}^L, is $0.27 \times 0.25 = .07$. The elasticity for the COGS portion is $0.17 \times 0.75 = 0.12$.

7.3 Implied cost pass-through rates

Let η_p^L denote the minimum wage elasticity of retail price stemming from the labor cost shock, and η_p^{cogs} denote minimum wage elasticity of retail price stemming from the COGS shock. The cost pass-through rates for the direct and indirect cost shocks, ρ_D and ρ_{IND}, can be obtained by dividing these price elasticities by the respective cost elasticities

$$\rho_D = \frac{\eta_p^L}{\eta_{mc}^L}, \quad \rho_{IND} = \frac{\eta_p^{cogs}}{\eta_{mc}^{cogs}} \tag{12}$$

I report the implied pass-through rates in Panel C of Table 8. The baseline estimate for the indirect cost pass-through rate is 0.99. The direct cost pass-through rate differs depending on whether one accounts for strategic complementarity in prices, since strategic complementarities influence η_p^L, as shown in Section 6.4. Without strategic complementarity in prices, I obtain a direct cost pass-through rate of 1.54. With strategic complementarity in prices, however, the direct cost pass-through rate increases to 2.35. This demonstrates that strategic complementarity in prices drives a wedge between the direct and indirect cost pass-through rates.

8 Discussion and conclusion

In this paper, I show that the vertical scope of a tax or cost shock has important implications for pass-through analysis. Using a simple analytical framework, I show that a cost shock affecting multiple points of the supply chain can induce both a direct and an indirect effect on prices, and common empirical strategies may fail to pick up the latter. The indirect effect can further bias pass-through estimates by generating spillovers that contaminate the control group. Moreover, the indirect effect can give rise to a heterogeneous price response among firms with otherwise identical direct exposure to the policy or cost shock. Finally, a mismatch in tradability upstream versus downstream implies that the direct shock is an aggregate shock in local markets whereas the indirect shock is idiosyncratic to specific firms. This limits the ability of a firm to pass the indirect shock through to prices and drives a wedge between the pass-through rates of the different components of marginal cost.

I demonstrate these points empirically in the context of the vertically disintegrated market structure and rich scanner data of the Washington state cannabis industry. I exploit a set of large labor cost shocks from minimum wage hikes that differentially affect firms across the supply chain. I find that properly accounting for the indirect cost shock dramatically

Table 8: Cost pass-through rates from minimum wage hikes

A. Minimum wage elasticity of retail prices			
	Labor		COGS
	No rivals	Rivals	
Elasticity	0.11***	0.16***	0.12***
	(0.04)	(0.06)	(0.04)$^{+}$

B. Minimum wage elasticity of marginal cost		
	Labor	COGS
Elasticity	0.07***	0.12***
	(0.02)	(0.04)

C. Marginal cost pass-through rates^{+}			
	Labor		COGS
	No rivals	Rivals	
Implied cost pass-through	1.55**	2.35**	0.99**
	(0.71)	(1.13)	(0.46)
P-value ($H_0 : \rho = 1$)	0.44	0.23	0.99

Notes: $^{+}$: SEs and P-values computed using the delta method under the assumption of independent errors.

increases the estimated effect of minimum wage hikes on retail cannabis prices. I document important heterogeneities upstream and show how these give rise to divergent pass-through rates downstream. Finally, I show that cannabis retailers are sensitive to their competitors' direct cost shocks but are insensitive to their indirect cost shocks. As a result, stores pass the direct cost shock through to prices at a higher rate than the indirect shock. This divergence implies that the burden of the shock falls more heavily on consumers for the direct shock compared to the indirect shock.

While the cannabis industry is distinct, my findings have significant implications beyond this context. The retail cannabis industry shares several important characteristics with other retail sectors. These include a comparable variable cost structure (see Appendix A) and similar demand elasticities (Hollenbeck and Uetake, 2021), both of which are important determinants of cost pass-through. The lack of a competing black market (Hollenbeck and Uetake, 2021) underscores the generalizability of my results.

Further support for the broad applicability of my findings comes from the fact that differences in tradability upstream versus downstream are a common feature in many sectors. Data from the Commodity Flow Survey (CFS) illustrates that interstate commodity flows dominate within-state flows in most manufacturing and wholesale trade sectors (see Appendix H), indicating that upstream firms sell a tradable good in those sectors. At the same time, 80% of retail sales are in brick-and-mortar stores (Statista, 2024a), which indicates that retailers sell non-tradable goods in local markets.

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A Cannabis industry background

A.1 Cannabis consumption and supply in Washington state

Cannabis consumers Cannabis use is widespread in Washington state. Approximately 30% of Washington adults consume cannabis on a monthly basis (Washington State Department of Health, 2024). For context, about 10% of adults in Washington consume cigarettes. Figure A1 shows cannabis consumption in Washington state along various demographic lines. The data come from the Behavioral Risk Factor Surveillance System (BRFSS) survey, an annual survey conducted by the Department of Health in partnership with the Centers for Disease Control and Prevention (CDC). The purpose of the survey is to measure changes in the health behaviors of people in Washington state (Washington State Department of Health, 2024).

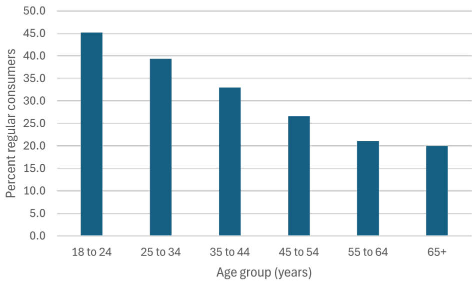
The figures show the percent of each demographic group that consumes cannabis at least once a month. Panel A illustrates that over 40% of adults age 18 to 24 use cannabis regularly; the same holds for adults age 25 to 34. 33% of adults age 35 to 44 consume regularly, while only one in five adults aged 55+ consume regularly. Panel B shows that regular cannabis consumption decreases monotonically with household income. There are no major differences between levels of education (Panel C). Panel D shows that consumption is highest among American indian, black, multiracial, and other (approx 40%), while consumption is lowest for asian and hispanic adults (approx. 25%). Panel E shows that males consume more than females (35% vs 25%).

The cannabis supply chain Figure A2 shows the stages of the cannabis supply chain. Most cannabis in Washington is grown in indoor facilities ranging in size from 2,000 to 30,000 square feet of plant canopy. When plants reach a mature stage, their buds are harvested, dried, and cured. The majority of cannabis is consumed in this unprocessed form (called "Usable marijuana") while the rest is processed into derivative subproducts like edibles and concentrates.

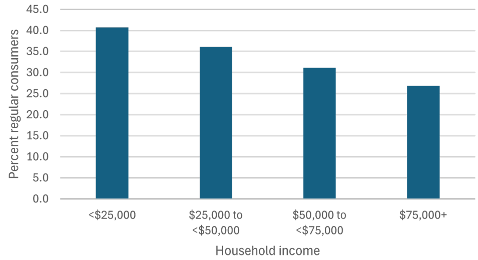
Wages in cannabis Wages in cannabis are significantly lower than in other industries in Washington state. This should come as no surprise: at the retail level, budtending is a low-skill job that requires no formal education, while the same holds for most jobs at the producer level.[32] Table A1 shows the average annual wage for cannabis establishments for the years 2018-2020 and compares it to the statewide average for the corresponding NAICS industry and all industries combined. For manufacturers, the annual gross wage gap to NAICS 111 is less than 3%; for retailers, the gap to NAICS 453 ranges from 8% to 11%. When converted to hourly wages (assuming 2,080 hours per year), the wage gap between cannabis manufacturers

[32]manufacturers typically employ a small number of "master growers" who are trained in cultivation, along with a much larger number of low-wage employees engaged in garden labor (e.g. harvesting, drying, trimming), filling pre-rolls, packaging, delivery and other manual labor tasks.

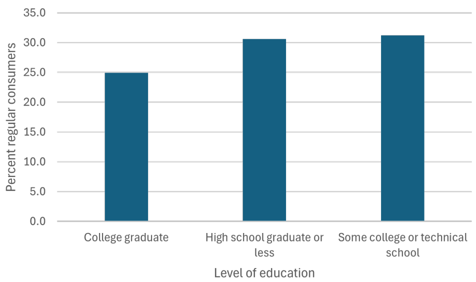
Figure A1: Demographic characteristics of regular cannabis consumers



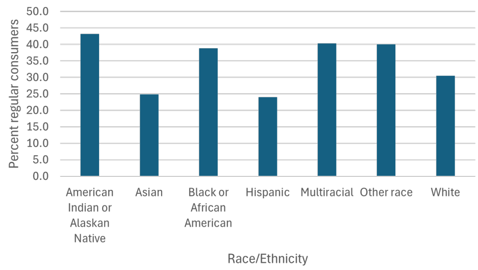
(A) Age



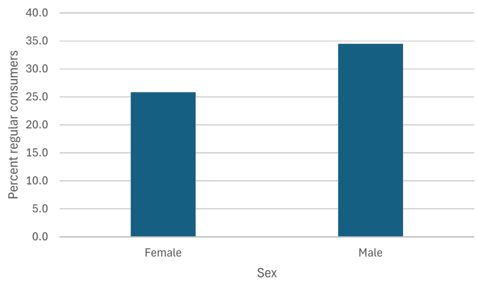
(B) Household income



(C) Education



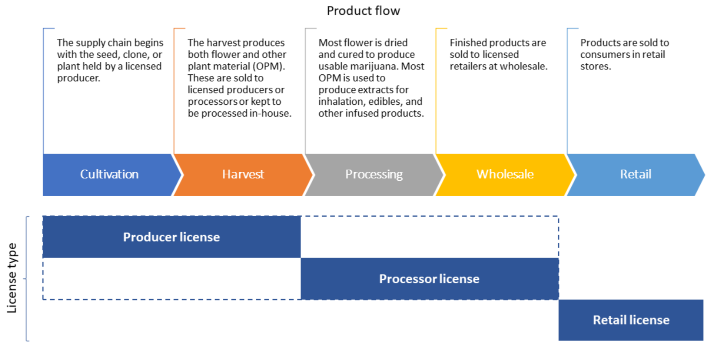
(D) Race/Ethnicity



(E) Sex

Notes: This figure presents the distribution of regular cannabis consumers in Washington state, broken down by various demographic characteristics: (A) age, (B) household income, (C) education level, (D) race/ethnicity, and (E) gender. Each bar represents the proportion of regular cannabis consumers within the respective subgroup. The data is from the 2021 Behavioral Risk Factor Surveillance System by the Washington State Department of Health, Center for Health Statistics.

Figure A2: The cannabis supply chain



Notes: This figure depicts the flow of cannabis products, from left to right, as they move through the supply chain. Only licensed manufacturers are permitted to cultivate and harvest cannabis plants; manufacturers can only sell to licensed processors, who in turn are permitted to process products; only processors can sell finished products at wholesale to retailers; licensed retailers can sell finished products to end consumers. An establishment can jointly hold producer and processor licenses, so the overwhelming majority of upstream establishments hold both licenses (i.e. manufacturers). Retailers may not hold a producer or a processor license and vice versa. As a result, production and retail activities are legally separated.

and NAICS 111 ranges from \$0.22 to \$0.37 per hour. For cannabis retailers, the gap is slightly larger: on average, cannabis employees earned between \$0.95 and \$1.58 less per hour than than NAICS 453, which amounts to a difference of 8% to 11%.

For both the cannabis industry and the NAICS industries, average wages are remarkably close to the wage floor imposed by the minimum wage. For manufacturers, the gross average wage is 5%-10% above the minimum wage for the years 2018 to 2020, while for retailers it ranges from 15%-19% above the minimum wage. Thus, to the extent that the wage distributions differ between cannabis establishments and their NAICS industries, these differences should come from the upper part of the wage distributions rather than the lower part (since outliers are bounded from below by the minimum wage but unbounded from above).

Furthermore, my regressions control for month-year and establishment fixed effects, which implies that any remaining measurement error is likely to be random and will lead to conservative treatment effect estimates. Finally, the dynamic difference-in-differences framework allows me to closely examine treatment effect timing, meaning that for estimates to be biased, non-random measurement error would have to induce bias in the exact period that the minimum wage hike occurs, a scenario which I consider unlikely. In Appendix D, I construct an alternative bite variable at the five-digit NAICS level and show that my results do not depend on the chosen level of industrial classification.

Table A1: Annual gross wages in the Washington state cannabis industry

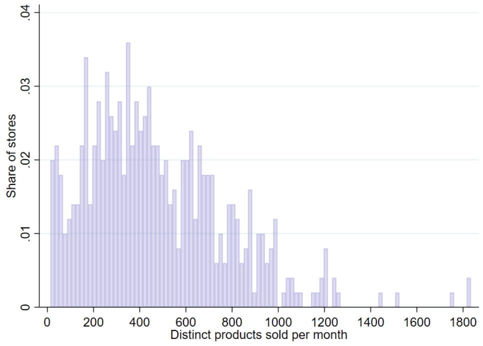
	Wholesale			Retail				
Year	Cannabis whole-sale	NAICS 111	NAICS 111419	Cannabis retail	NAICS 453	NAICS 453998	All pri-vate inds.	Min. wage
2018	\$27,906	\$28,804	\$28,371	\$26,126	\$28,116	\$31,848	\$66,156	\$23,920
2019	\$29,713	\$30,499	\$30,417	\$27,468	\$29,798	\$32,922	\$57,185	\$24,960
2020	\$32,315	\$33,026	\$33,459	\$29,534	\$32,847	\$34,847	\$76,801	\$28,080

Notes: This table compares average annual gross wage for workers at cannabis establishments for the years 2018-2020. Average annual gross wage is obtained by dividing total wages by average covered employment. Minimum wage is based on 2,080 hours per year. Data for 2021 is not available. Data from Washington state ESD and High Peak Strategy.

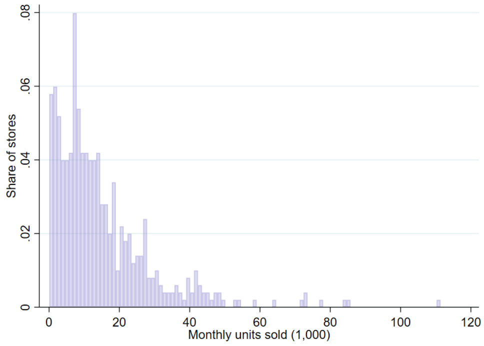
A.2 Cannabis retail stores

Store characteristics Figure A3 shows the distribution of store-level monthly averages for various store characteristics. Panel A shows the average number of distinct products sold per month across stores. A 1.0 gram package and a 2.0 gram package of Sunset Sherbert usable marijuana (i.e. unprocessed dried flower) produced by Northwest Harvesting Co are examples of distinct products in our data. The average store in our sample sells 473 distinct products per month (median: 419). However, Panel A reveals substantial variation across stores in our sample, with values ranging from as low as 13 to a maximum of 1,833 products per month. Panel B reports the average units sold per month across stores (in thousands). The average store in our sample sells 14,836 units per month (median: 10,905). As is the case with product variety, there is large variation in units sold across stores. Stores at the 1st percentile sell 287 units per month, while those at the 99th percentile sell 72,826 units per month. Panel C shows the distribution of tax-inclusive monthly revenue across stores. The average store generates \$285,320 revenue per month (median: \$205,377). Again, revenue varies across stores: stores at the 1st percentile generate \$3,765 while those at the 99th percentile generate \$1,447,000 per month.

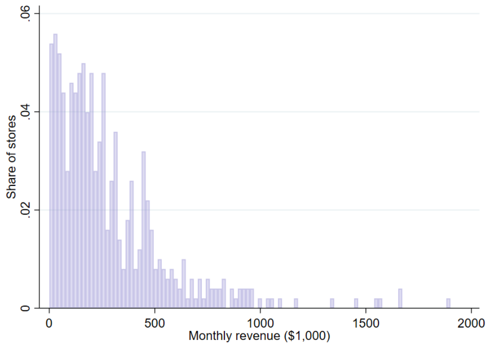
Figure A3: Distribution of monthly averages across stores



(A) Product variety



(B) Units sold



(C) Revenue (tax-inclusive)

Notes: The figures show the distribution of store-level average statistics across all stores in our sample. Panel A presents the distribution of the average monthly number of unique products sold. Panel B displays the distribution of the monthly average units sold. Panel C illustrates the distribution of average monthly sales revenue across stores.

Variable cost structure for cannabis retailers To ascertain the variable cost structure for cannabis retailers, we use aggregate payroll data on cannabis retailers from the Washington State Employment Security Department (ESD) and High Peak Strategy. The ESD collects data on employment and wages in industries covered by unemployment insurance (95% of U.S. jobs). The data spans the years 2018-2020. Table **??** illustrates that cannabis retailers have a similar variable cost structure as other retail industries studied in the literature. Renkin et al. (2022), for example, find that for U.S. grocery stores, COGS accounts for 83% of variable costs. Note that in most retail settings, cost of goods sold (COGS) and labor cost together account for 99% of variable cost while other expenditures like packaging and transport typically make up less than 1% of variable cost (Renkin et al., 2022).

Table A2: COGS and the labor share of costs for cannabis retailers

	Average expenditure		Variable cost share	
Year	Labor	COGS	Labor	COGS
2018	\$324,582	\$702,358	0.32	0.68
2019	\$370,897	\$1,187,462	0.24	0.76
2020	\$407,273	\$1,584,301	0.20	0.80

Notes: This table compares average annual labor expenditure and COGS expenditure for cannabis retail establishments in Washington state for the years 2018-2020. Aggregate payroll data on cannabis retailers is from the Washington state ESD and High Peak Strategy (2018-2020). Labor expenditure equals total wages divided by the number of active retail establishments. Establishments with missing UI data are excluded from total wages and establishment counts. COGS is the average annual wholesale expenditure for cannabis retailers in the estimation sample. Wholesale purchases from processor-only licenses are included. Wholesale expenditure data from Top Shelf Data (2018-2020).

B Establishment-level indexes

My empirical analysis uses traceability data provided by the data analytic firm Top Shelf Data (TSD), which ingests the raw tracking data from the Liquor and Cannabis Board (LCB) and matches it with additional product information. Note that the raw tracking data from the LCB includes each product's Stock Keeping Unit (SKU), but TSD does not report this. Instead, each product is identified by a unique combination of five elements: retailer, manufacturer, product category, unit weight, and product name. For products with no unit weight (such as liquid edibles), the first four elements identify the product. TSD then calculates the average price of product i at retail establishment j in month t as

$$P_{i,j,t} = \frac{TR_{i,j,t}}{TQ_{i,j,t}}. \tag{13}$$

where $TR_{i,j,t}$ is the revenue from product i at retailer j in month t, and $TQ_{i,j,t}$ is total quantity.

To construct establishment-level price indexes, I employ a two step process similar to that used by Renkin et al. (2022). In the first step, I use $P_{i,j,t}$ to construct a geometric mean of month-over-month changes for product subcategory c at establishment j:

$$I_{c,j,t} = \prod_i \left(\frac{P_{i,j,t}}{P_{i,j,t-1}} \right)^{\omega_{i,c,y(t)}} \tag{14}$$

where each subcategory is a unique category-unit weight combination.[33] For example, 1.0g usable marijuana and 2.0 gram usable marijuana are separate subcategories. Following Renkin et al. (2022), the weight $\omega_{i,c,y(t)}$ is the share of product i in total revenue of subcategory c in establishment j during the calendar year of month t.[34]

In the second step, I aggregate across subcategories to get the price index for establishment j in month t:

$$I_{j,t} = \prod_c I_{c,j,t}^{\omega_{c,j,y(t)}}. \tag{15}$$

Similar to the last step, the weight $\omega_{c,j,y(t)}$ is the share of subcategory c in total revenue in establishment j during the calendar year of month t.

Establishment-level price indexes for manufacturers are constructed in a very similar manner as with retailers, but for two exceptions. First, at the manufacturer level a product is identified by a unique combination of four elements (not five as with retailers): producer, product category, unit weight, and product name. Second, the wholesale price data exhibits

[33]Since unit weight is a major component of cannabis product differentiation (akin to volume in beverage sales), the majority of sales contain information on unit weight. Therefore, in the first step of the establishment index, I choose to aggregate at category-unit weight level rather than the category level.

[34]As pointed out byRenkin et al. (2022), price indexes are often constructed using lagged quantity weights. Since product turnover is high in cannabis retail, lagged weights would limit the number of products used in constructing the price indexes. Thus, contemporaneous weights are used.

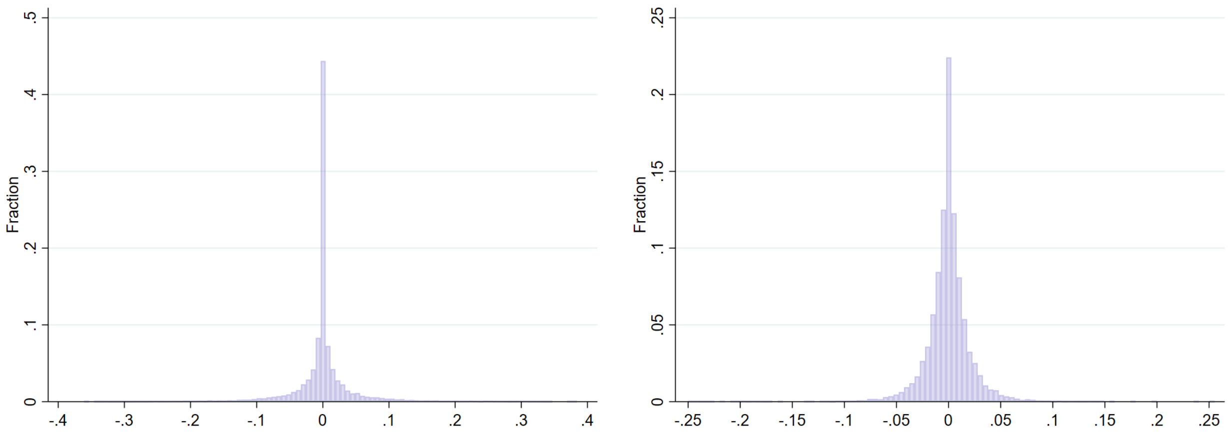
much larger variation in prices compared to the retail data. As a result, the first stage of the index $\frac{P_{i,j,t}}{P_{i,j,t-1}}$ in eq. 14 leads to a few inconceivable outliers such as a 562-factor increase in prices from one month to the next. To prevent outliers from driving results in my estimation, I trim the top and bottom 0.1% of the product indexes before calculating the subcategory index in equation 14. As Table B1 illustrates, trimming does not meaningfully change the location or shape of the distribution.

Table B1: First-stage price indexes

	Wholesale		Retail
	No trim	0.2% trim	No trim
Mean	1.004333	1.000440	1.000028
St. dev.	0.816940	0.026360	0.015641
Min	0.000667	0.652272	0.009345
1%	0.940171	0.946112	0.985232
25%	0.999989	0.999989	0.999848
Median	1.000000	1.000000	1.000000
75%	1.000000	1.000000	1.000139
99%	1.067935	1.060525	1.014273
Max	562.785120	1.646053	15.273730
N	1,658,554	1,657,326	7,590,876

Notes: This table shows descriptive statistics for product-level price indexes, $\frac{P_{i,j,t}}{P_{i,j,t-1}}$. The price index forms the basis for the subcategory index (i.e. the first step of the establishment index). Product-level price indexes are not trimmed for retailers because they exhibit much less variation than for wholesalers. Data source: Top Shelf Data, August 2018-July 2021.

Figure B1: Establishment-level inflation rates for cannabis, August 2018-July 2021



(A) Monthly wholesale inflation rates

(B) Monthly retail inflation rates

Notes: The figures show the distribution of monthly establishment-level inflation rates for cannabis manufacturers (Figure a) and retailers (Figure b) in the estimation sample. Data: Top Shelf Data, August 2018-July 2021.

C Wage data

NAICS classification for cannabis establishments

Defining the minimum wage bite variable at the industry-by-county level requires careful consideration of which industry codes to use since establishments in the cannabis industry may fall under more than one North American Industrial Classification System (NAICS) industry code. The underlying principle of the NAICS system—that establishments with similar production processes be grouped together—greatly facilitates this, since the NAICS codes align well with the vertically disintegrated structure of the cannabis industry. For example, NAICS 453 captures all cannabis retailers, since NAICS 453998 (a component of NAICS 453) includes "All Other Miscellaneous Store Retailers (except Tobacco Stores), including Marijuana Stores, Medicinal and Recreational" (US Census Bureau, 2017b). At the producer level, NAICS 111 captures all cannabis growers, since NAICS 111998 includes "All Other Miscellaneous Crop Farming, including Marijuana Grown in an Open Field" and NAICS 111419 includes "Other Food Crops Grown Under Cover, including Marijuana Grown Under Cover" (US Census Bureau, 2017b). Slightly complicating things is the fact that in addition to growing cannabis, most manufacturers are also processors (i.e. manufacturers). Processing falls under NAICS 424 which includes as a subcomponent "Other Farm Product Raw Material

Merchant Wholesalers, including Marijuana Merchant wholesalers" (NAICS 424590).[35] Importantly, though, NAICS classifies an establishment based on its primary activity, meaning that a producer-processor only belongs to NAICS 424 if the sales and revenue from processing activities exceed those of its own crop production (US Census Bureau, 2017b). I view it as more likely that a producer-processor belongs to NAICS 111 for two reasons. First, while it is not possible to directly compare the revenue share of crop production versus processing activities at the establishment level, at the industry level unprocessed "Usable Marijuana" accounts for over 61% of manufacturers' revenue in my sample period. Second, Jiang and Miller (2022) show that when cannabis was first legalized, the establishment count for NAICS 1114 in Washington increased by a similar count as the number of producer cannabis licenses. Moreover, the state saw a proportional increase in the number of workers and the total wages paid in NAICS 1114 (Jiang and Miller, 2022). Therefore, I classify all establishments with a joint producer-processor license as NAICS 111 under the assumption that crop production activities exceed processing activities for these establishments. Establishments with only a processor license (i.e. those allowed to process—but not grow—cannabis) would then be assigned NAICS 424, which is their proper classification. However, the very small number of processor licenses makes it difficult to identify treatment effects, so I drop processor-only licenses from my sample altogether.

Table C1 provides an overview of the representativeness of cannabis employment in the respective 3-digit NAICS industries. The employment share for cannabis retailers is larger than that for manufacturers, but the shares remain relatively constant over time for both manufacturers and retailers. The fact that NAICS 111 is less representative does not imply that measurement error for the producer regressions is greater than that for the retail regressions, since it could be the case that the industries contained in NAICS 453 are more homogeneous than those in NAICS 111. A better indication of measurement error is the relation between cannabis wages and wages in the corresponding 3-digit NAICS industry. Table **??** in appendix **??** shows that mean annual wages for cannabis establishments are remarkably similar to their corresponding NAICS industries and very close to the wage floor imposed by the minimum wage.

A final consideration is the granularity of industrial classification to use for the bite variable. Measuring bite at the three-digit industry level carries several advantages that make it preferable for the main analysis. First, cannabis manufacturers belong to different four-digit NAICS industries depending on whether they grow indoors or outdoors. Since I do not observe whether a given producer-processor grows indoors or outdoors, I would have to assume that all establishments are either indoor or outdoor growers, which increases

[35] A third industry, NAICS 115, may also apply to manufacturers, as it includes support activities for agriculture involving soil preparation, planting, and cultivating. However, to be in NAICS 115 an establishment must primarily perform these activities independent of the agriculture producing establishment, e.g. on a contractual basis. It is very unlikely that an establishment with a coveted producer-processor license would solely operate on a contractual basis without engaging in any production of its own. Therefore, I do not consider NAICS 115 in my analysis.

Table C1: Employment in cannabis relative to 3-digit NAICS industry

	Wholesale			Retail		
Year	Cannabis Wholesale	NAICS 111	Emp. share	Cannabis Retail	NAICS 453	Emp. share
2018	4,634	68,443	.07	3,988	25,411	.16
2019	4,727	64,112	.07	4,618	25,908	.18
2020	5,265	61,408	.09	5,047	22,517	.22

Notes: This table compares annual average employment at cannabis establishments and the respective NAICS subsectors for the years 2018-2020. Only UI covered employment is included (95% of US jobs). NAICS 111 and 453 correspond to crop production and miscellaneous store retailers, respectively. Data for 2021 is not available. Data from Washington state ESD.

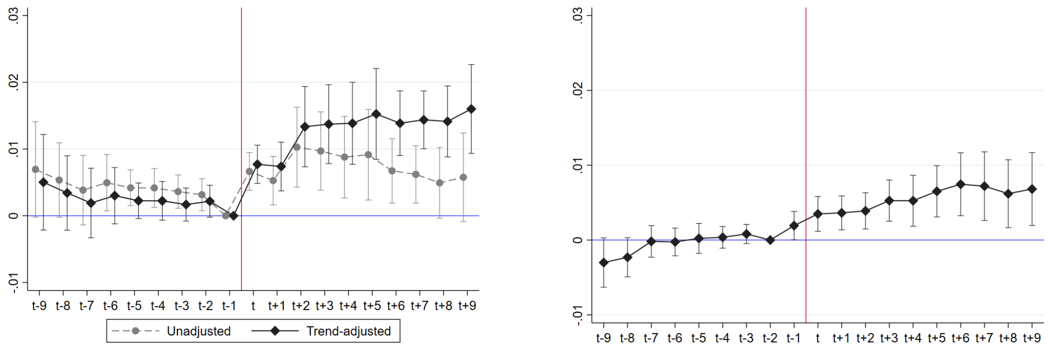
measurement error. In contrast, the 3-digit NAICS code captures both indoor and outdoor manufacturers and thereby avoids such measurement error. Second, with more detailed NAICS codes, the bite variable does not clear the Census Bureau's data privacy filters for several counties, resulting in a reduced sample size.

D Additional robustness checks

D.1 Longer event window

In this subsection, I show that the results from the main section based on a 12-month event window are unaffected by increasing the size of the event window. As Figures D1 and **??** illustrate, treatment effects materialize by $t+2$ and remain quite stable thereafter. Therefore, the 12-month event window used in the baseline estimation should adequately capture the short run effects of minimum wage hikes on retail cannabis prices.

Figure D1: Direct pass-through, 18-month event window



(A) Wholesale prices

(B) Retail prices

Notes: The figures show direct pass-through to prices over an 18-month event window. Both panels display cumulative price level effects (E_L) relative to the normalized baseline period in $t-1$ (wholesale prices) and $t-2$ (retail prices). Cumulative effects E_L are obtained by summing the distributed lag coefficients to lead or lag L as detailed in the main text. Both panels show 90% confidence intervals of the sums based on SE clustered at the county level. Data source: Top Shelf Data and Washington ESD, March 2018-December 2021.

Table D1: Pre-treatment summary statistics

(a) Retail

	(1) Below 50th pctile	(2) Above 50th pctile	(3) Below 25th pctile	(4) Above 75th pctile
Unit price (in dollars)	26.85	26.59	27.65	26.29
	(4.83)	(5.13)	(5.04)	(5.38)
Unit price growth (percent)	0.2	0.1	0.2	0.1
	(3.5)	(3.0)	(3.4)	(3.1)
Units sold per month	11,436	13,385	13,056	11,995
	(12,779)	(12,544)	(10,670)	(12,989)
Monthly revenue (in dollars)	223,571	254,589	259,780	225,179
	(258,136)	(245,064)	(217,614)	(249,569)
Unique products per month	381	410	448	354
	(316)	(345)	(336)	(313)

(b) Wholesale

	(1) Below 50th pctile	(2) Above 50th pctile	(3) Below 25th pctile	(4) Above 75th pctile
Unit price (in dollars)	11.41	11.68	11.80	11.04
	(11.04)	(5.90)	(11.55)	(5.23)
Unit price growth (percent)	0.2	0.3	0.2	0.2
	(6.6)	(6.2)	(6.3)	(6.6)
Units sold per month	65,998	12,328	88,074	10,377
	(570,332)	(42,921)	(667,509)	(24,719)
Monthly revenue (in dollars)	76,305	81,795	95,126	65,397
	(215,746)	(238,092)	(352,091)	(124,818)
Unique products per month	62	45	47	32
	(170)	(127)	(83)	(52)

Notes: The table summarizes establishment-level variables over all pre-treatment periods. Column 1 contains stores below the median bite for retailers in the sample, while Column 2 contains stores above the median bite. Columns 3 and 4 are analogous for producer-processors. The reported variables include unit price, average quantity sold per month, average revenue per month, and average number of distinct products sold per month. For producer-processors, units sold and unique products per month are affected by the LCB data collection practices as described in the main text. Standard deviations are in parentheses.

D.2 Alternative specifications and additional robustness checks

This subsection reports results from the alternative specifications discussed in Section **??** as well as additional robustness checks.

Table D2: Robustness checks for direct pass-through to wholesale prices

	Alternate bite variable		Reverse causality		Other	
	(1)	(2)	(3)	(4)	(5)	(6)
	Q4 bite	Compliance	No Seattle	No King county	Balanced panel	Alt. weights
E_0	0.006**	0.006***	0.006***	0.006***	0.006***	0.008**
	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.004)
E_2	0.011**	0.011***	0.010***	0.010***	0.008***	0.013***
	(0.004)	(0.004)	(0.003)	(0.003)	(0.003)	(0.004)
E_4	0.009**	0.011**	0.010***	0.010***	0.008**	0.018***
	(0.004)	(0.005)	(0.004)	(0.004)	(0.004)	(0.006)
$\sum$ Pre-event	-1.4e-07	2.1e-08	-1.2e-07	-1.7e-07	1.7e-07	-1.0e-07
	(0.003)	(0.004)	(0.003)	(0.003)	(0.003)	(0.006)
N	14,777	14,699	14,622	14,506	12,900	14,819
Time FE	YES	YES	YES	YES	YES	YES
Controls	NO	NO	NO	NO	NO	NO
Trend-adjusted	YES	YES	YES	YES	YES	YES

Notes: The listed coefficients are sums of the distributed lag coefficients E_L, L months after the minimum wage hikes, relative to the baseline period in $t-2$. The distributed lag coefficients are estimated from equation **??** with establishment-level inflation rate as the dependent variable. (1) uses Q4 bite in the treatment interaction term $\Delta MW_{j,t-l} \times bite_{k(j),t-l}$, while (2) uses the difference between bite two quarters before and one quarter after the hike. (3)-(4) account for possible endogeneity of Seattle hikes: (3) omits Seattle establishments for event 3 while (4) omits King county establishments for event 3. (5) restricts the panel to establishments that are present at least 10 months for a given event. For (6) the price indexes are constructed with expenditure weights based on the fiscal year starting in July and ending in June of each year. Estimates are unaffected by the inclusion of controls, winsorizing instead of trimming, and not trimming at all (results available on request). Standard errors of the sums E_L are clustered at the county level and are shown in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Data from Washington ESD and Top Shelf Data, March 2018-December 2021.

Minimum wage compliance and exempt workers When investigating minimum wage effects, it is important to consider the possibility that not all firms or workers comply with minimum wage hikes. If that were the case, then the share of FTE earning below the minimum wage would overestimate the impact of the minimum wage on firm costs, resulting in potentially non-random measurement error in the treatment variable. Luckily, bite lends itself well to measuring minimum wage compliance since bite can also be measured one quarter after the minimum wage hikes. Figure D2 shows the average bite one quarter after

Table D3: Robustness checks for direct pass-through to retail prices

	Alternate bite variable		Reverse causality		Other	
	(1)	(2)	(3)	(4)	(5)	(6)
	Q4 bite	Compliance	No Seattle	No King county	Balanced panel	Alt. weights
E_0	0.003**	0.004	0.003***	0.003***	0.003**	0.004***
	(0.001)	(0.003)	(0.001)	(0.001)	(0.001)	(0.001)
E_2	0.003**	0.006**	0.003**	0.003***	0.004**	0.004**
	(0.002)	(0.003)	(0.001)	(0.001)	(0.002)	(0.002)
E_4	0.004*	0.008**	0.004**	0.005***	0.005**	0.005**
	(0.002)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)
$\sum$ Pre -event	-7.6e-04	0.002	-6.0e-05	-7.7e-04	-0.001	2.8e-04
	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
N	14,044	13,859	13,422	12,995	13,390	14,042
Time FE	YES	YES	YES	YES	YES	YES
Controls	YES	YES	YES	YES	YES	YES

Notes: The listed coefficients are the sum of the distributed lag coefficients E_L, L months after the minimum wage hikes, relative to the baseline period in $t-2$. The distributed lag coefficients are estimated from equation 5 with establishment-level inflation rate as the dependent variable. All specifications include time fixed effects and county level controls (monthly unemployment rate and average monthly wage). (1) uses Q4 bite in the treatment interaction term $\Delta MW_{j,t-l} \times bite_{k(j),t-l}$, while (2) uses the difference between bite two quarters before and one quarter after the hike. (3)-(4) account for possible endogeneity of Seattle hikes: (3) omits Seattle establishments for event 3 while (4) omits King county establishments for event 3. (5) restricts the panel to establishments that are present at least 10 months for a given event. For (6) the price indexes are constructed with expenditure weights based on the fiscal year starting in July and ending in June of each year. Estimates are unaffected by the inclusion of controls, winsorizing instead of trimming, and not trimming at all (results available on request). Standard errors are clustered at the county level and are shown in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Data from Washington ESD and Top Shelf Data, March 2018-December 2021.

the minimum wage hikes between 2018-2021. While bite is low for most counties in the crop production industry (Panel A), several counties have relatively high bite for miscellaneous store retailers (Panel B). At my request, the ESD examined employee-level payroll data at the establishments responsible for these high bite counties and confirmed that the relatively high post-hike bite is a result of minimum wage exemptions rather than non-compliance or data reporting issues.[36] Under certain circumstances, employers can apply for permission to pay eligible employees less than the state minimum wage.[37] With the exception of workers with disabilities, however, exempt employees must still be paid 75% of the state minimum wage (85% for on-the-job training).[38] Thus, for exempt employees at the 75% threshold, the minimum wage hike still corresponds to a wage increase. Moreover, wages slightly above the minimum wage have been shown to be responsive to minimum wage hikes, meaning that the minimum wage hike likely increases wages for exempt employees above the 75% threshold too.[39]

To summarize, high post-hike bite values in some counties reflect sub-minimum wages paid to exempt employees. Since these employees likely experience a wage increase due to the minimum wage hike, the bite variable in the main analysis (computed two quarters prior to the hike) likely captures true minimum wage exposure. Nevertheless, I test whether removing exempt employees changes the results from the main part of the paper. To do this, I create a new bite variable that is equal to the difference between bite two quarters prior and one quarter after the hike:

$$\Delta Bite_{k(j)} = Bite_{k(j),Q3,y} - Bite_{k(j),Q1,y+1} \tag{16}$$

This effectively nets out non-compliance and exempt employees at the county level. Tables D2 and D3 show that results are robust to this alternative bite variable.

Seasonal labor in NAICS 111 A second issue is that Washington's crop production is highly seasonal and the major crop types are primarily harvested in Q3. Since the minimum wage hikes in my sample occur on January 1st of each year, the bite variable—calculated two quarters prior to the hike—is based on Q3 wages. As a result, the bite variable may overestimate true minimum wage exposure due to seasonal fluctuations in agricultural labor. If counties with higher observed bite employ more low-wage seasonal labor (e.g. low wage rural counties), then the measurement error is non-random and OLS is biased. Unlike in the previous subsection, this is not classical errors-in-variables. Nevertheless, an easy way

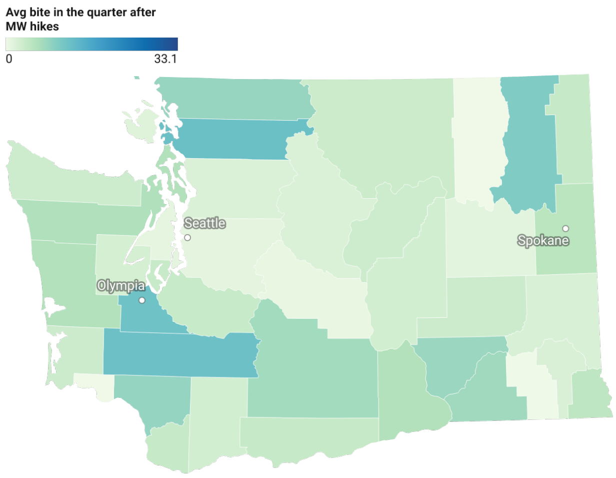
[36] The ESD has safeguards in place to flag sub-minimum wages at the employee and firm level. Implausibly low wages are either excluded from the bite variable or the wages are substituted with a previous valid quarter for that employer, adjusting for payroll and inflation.

[37] Eligibility applies to workers with a disability, employees in job training, student workers in vocational training, student workers employed at an academic institution, and apprentices. Permission must be granted by both the Washington state Department of Labor and Industries and the U.S. Department of Labor.

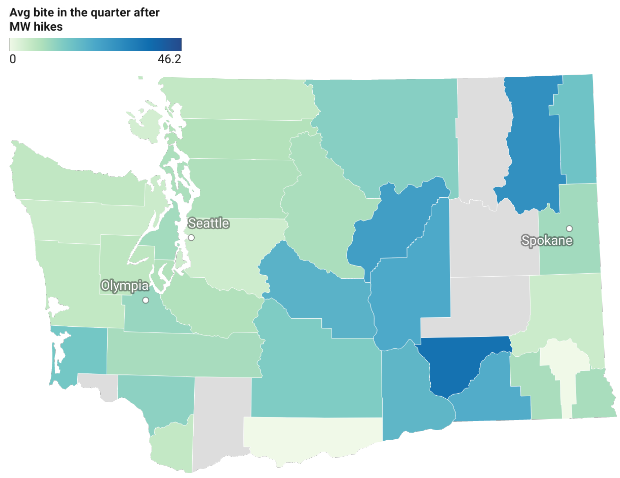
[38] See Washington State Legislature (1960).

[39] For example, Gopalan et al. (2021) find that wage increases extend up to $2.50 above the minimum wage.

Figure D2: Average bite one quarter after the minimum wage hike, 2018-2021



(A) NAICS 111 (Crop production)



(B) NAICS 453 (Misc. store retailers)

Notes: The figures show the average bite in the quarter after a minimum wage hike. Cannabis manufacturers belong to NAICS 111 (crop production) while cannabis retailers belong to NAICS 453 (miscellaneous store retailers). Data: Washington ESD, 2019-2021.

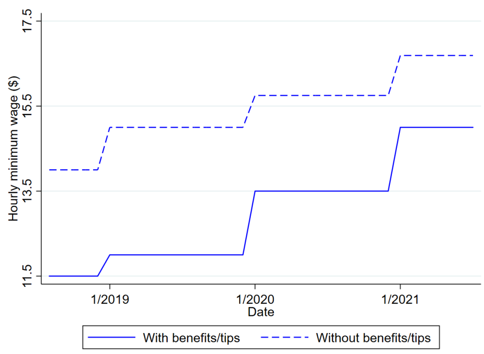
to overcome this would be to use Q4 bite instead, since Q4 does not coincide with any major harvest activity and hence should be free of seasonal wage fluctuations. As shown in TableD2, estimates are robust to using Q4 bite, suggesting the main results are not affected by measurement error from seasonal wage fluctuations.

Undocumented workers in NAICS 111 If a significant amount of labor in NAICS 111 is performed by low-wage, undocumented migrants who are not eligible for unemployment insurance (and hence do not factor into the bite variable), then the bite variable may overestimate minimum wage exposure. Counties with more undocumented workers will have a smaller true (unobserved) bite, which amounts to classical errors-in-variables. Several facts speak against this being problematic. First, the prevalence of undocumented agricultural labor likely correlates over time within a county. As such, county fixed effects should sweep away cross-county differences in this measurement error. Second, to the extent that measurement error remains after demeaning, the bias leads to conservative treatment effects by attenuating the OLS estimates.

Measurement error and treatment effect timing Finally, setting aside the reasoning laid out in the previous two subsections, the fact remains that any bias from non-random time-varying measurement error would need to coincide with the timing of the minimum wage hike. In other words, for the main results to be driven by measurement error, the bias would have to cause a sharp inflationary shock at precisely the same time as the hike—not before and not after. I view such a scenario as unlikely.

Reverse causality While the overwhelming majority of cities and counties in the sample are subject to exogenous statewide minimum wage hikes, there is one exception: the city of Seattle, located in King county, has a citywide minimum wage that may, under certain circumstances, result in an endogenous bite variable. This section lists the assumptions under which Seattle's minimum wage may be endogenous and reports results that take this potential endogeneity into account.

Figure D3: Seattle citywide minimum wage schedule, 2018-2022



Notes: The figure shows the schedule for the citywide minimum wage in Seattle. The solid blue line is the minimum wage applicable to employees who receive health benefits or tips, while the dashed line is the minimum wage for employees without benefits or tips. Data source: Washington ESD.

Employment at Seattle establishments is subject to one of two minimum wages, depending on employer contributions to employee medical benefits and whether an employee earns tips.[40] Employees who receive health benefits or tips are subject to a lower minimum wage than those who do not (Figure D3). For the former, the minimum wage schedule was pre-determined over the sample period, making the hikes contemporaneously exogenous.[41] For the latter group of employees, the hikes for events 1 and 2 (January 1, 2019 and January 1, 2020) were predetermined, while the hike for event 3 was linked to a local CPI. Thus, event 3 may be endogenous for some Seattle establishments, and potentially also for the county Seattle is located in (King county).

Since the treatment variable $\Delta MW_{j,t-l} \times Bite_{k(j),t-l}$ is the product of two parts, it is important to consider how Seattle's minimum wage affects each part in turn. The following assumptions delineate circumstances under which one or both of these parts could be endogenous.

Assumption 1 (Exogeneity)

[40] Technically, this only applies to small employers (500 or fewer employees), as large firms (over 500 employees) are subject to a separate minimum wage. However, no cannabis firm has more than 500 employees and the average firm size in King county is 10 employees for NAICS 111 and 11.5 employees for NAICS 453 during the sample period. I therefore omit the large firm minimum wage from my analysis.

[41] The schedule was determined in 2015.

1.A: All Seattle firms in NAICS 111 (NAICS 453) pay benefits or tips

Under Assumption 1.A, $\Delta MW_{j,t-l} \times Bite_{k(j),t-l}$ is contemporaneously exogenous because minimum wage hikes are predetermined for the entire sample period. The results in sections 6.1 are based on assumption 1.

Assumption 2 (No spillovers to King county)

2.A: No Seattle firms in NAICS 111 (NAICS 453) pay benefits or tips

2.B: There are no spillovers from the Seattle minimum wage hike to wages at establishments located outside of Seattle but in King County (applies to event 3 only).

Under assumption 2.A, $\Delta MW_{j,t-l}$ is predetermined (and hence exogenous) for Seattle establishments in events 1 and 2, but it is endogenous for event 3. Thus, Seattle establishments must be dropped from the sample for event 3. Under assumption 2.B, Seattle's endogenous hike at event 3 does not affect $Bite_{k(j),t-l(e=3)}$, meaning non-Seattle establishments located in King County can be kept in the sample for that event. $Bite_{k(j),t-l(e=3)}$ will be mismeasured for King County at event 3 which may attenuate estimates.

Assumption 3 (Spillovers to King county)

3.A: No Seattle firms in NAICS 111 (NAICS 453) pay benefits or tips

3.B: There are spillovers from the Seattle minimum wage hike to wages at non-Seattle establishments in King County (applies to event 3 only).

Assumption 3.A carries over from 2.A, meaning $\Delta MW_{j,t-l}$ is exogenous for Seattle establishments in events 1 and 2 but it is endogenous for event 3. Now however, assumption 3.B implies that $Bite_{k(j),t-l(e=3)}$ is also endogenous for event 3, since Seattle's endogenous minimum wage hike spills over to surrounding King county establishments, possibly lowering the King county bite. This means that all King county establishments must be dropped from the sample for event 3.

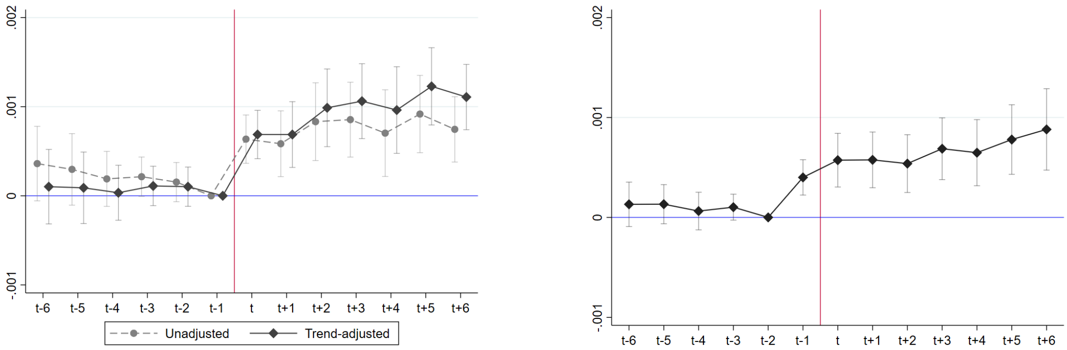
Table D2 reports results from estimating equation **??** under assumptions 2 and 3 for manufacturers. As columns 3 and 4 illustrate, wholesale price effects are very similar to those obtained in the main paper. Table D3 (columns 3 and 4) shows that the same holds for retail price effects. Taken together, these results suggest that reverse causality from Seattle's minimum wage does not drive my main results.

Bite as treatment intensity The main results do not rely on interacting bite with the size of the minimum wage hike. To verify this, I estimate a variation of equation **??**:

$$\pi_{j,t} = \sum_{l=-5}^{6} \beta_l Bite_{k(j),t-l} + X_{k(j),t} + \theta_k + \gamma_t + \epsilon_{j,t}. \tag{17}$$

Here, the treatment intensity is the minimum wage bite and it is not multiplied with $\Delta MW_{j,t-l}$. Figure D4 shows that retail and wholesale price effects follow a very similar time path to those in the main section. Since the treatment intensity variable is defined differently, the coefficients are not directly comparable to those in the main section. However, the relative magnitude of direct pass-through to wholesale and retail prices is the same as in the main part of the paper. Two months after a minimum wage hike, direct pass-through to wholesale prices is approximately twice the size of direct pass-through to retail prices.

Figure D4: Direct pass-through with bite-only treatment intensity



(A) Wholesale prices

(B) Retail prices

Notes: The figures show cumulative price level effects when the treatment intensity does not include an interaction term for the size of the minimum wage hike. Effects are cumulative relative to the normalized baseline period ($t-1$ for manufacturers, $t-2$ for retailers). Cumulative effects E_L are obtained by summing the distributed lag coefficients to lead or lag L as detailed in the main part of the paper. The figures show 90% confidence intervals of the sums based on SE clustered at the county level. The dependent variable is the establishment-level inflation rate. Estimates are from equation 3 with time and county fixed effects, estimated separately for manufacturers and retailers. In panel (a), the dependent variable is adjusted for a bite-specific trend as described in Section 6.1. Data source: Top Shelf Data and Washington ESD, August 2018-July 2021.

Bite at the detailed industry level (5-digit NAICS) In this section, I use an alternate bite variable based on more detailed NAICS codes and wage data from the QCEW. In particular, I define bite as the difference between the FTE weekly minimum wage salary and the actual average weekly wage, where the latter is reported by the QCEW on a quarterly basis. This bite variable is similar to that used in other papers on minimum wage effects (see e.g. Renkin et al. (2022); Leung (2021)). I estimate equation 3 with this alternative bite variable in place of the original bite variable in the treatment intensity interaction term $\sum_{l=-5}^{6} \beta_l \Delta MW_{j,t-l} \times Bite_{k(j),t-l}$. However, despite the more granular level of industrial classification, the alternative bite variable carries several disadvantages. First, due to the wage floor imposed by the minimum wage, outliers will pull the mean wage upwards. Thus, a bite variable proportional to the mean wage will likely underestimate true exposure to the minimum wage.[42] Second, while cannabis manufacturers belong to a single three-digit NAICS code (111), they fall under two different four- and five-digit NAICS codes depending on whether they are indoor or outdoor growers (indoor growers belong to NAICS 11141 while outdoor growers belong to NAICS 11199). Producer-processor licenses are based on a three-tier system governing the square footage of plant canopy a producer is permitted to operate. Tiers 1 and 2 permit 2,000 and 10,000 square feet of plant canopy, respectively, and thus largely comprise indoor grow operations (Washington State Liquor and Cannabis Board, 2021). Tier 3 manufacturers can operate up to 30,000 square feet of plant canopy, meaning tier 3 comprises more balanced mix of indoor and outdoor grow operations compared to tiers 1 and 2.[43] Thus, it is not possible to determine which five-digit NAICS code applies to the majority of tier 3 manufacturers, meaning substantial measurement error will result for tier 3 manufacturers in either case. Therefore, I drop tier 3 manufacturers from the sample and restrict the analysis to tiers 1 and 2 (i.e. indoor growers) and use NAICS 11141 for the bite variable.[44]

A final disadvantage to the more detailed industry classification is that the QCEW data does not distinguish between full-time and part-time workers, meaning the wage data are not based on FTE. This contrasts to the bite variable in the main specification, which is based on FTE.

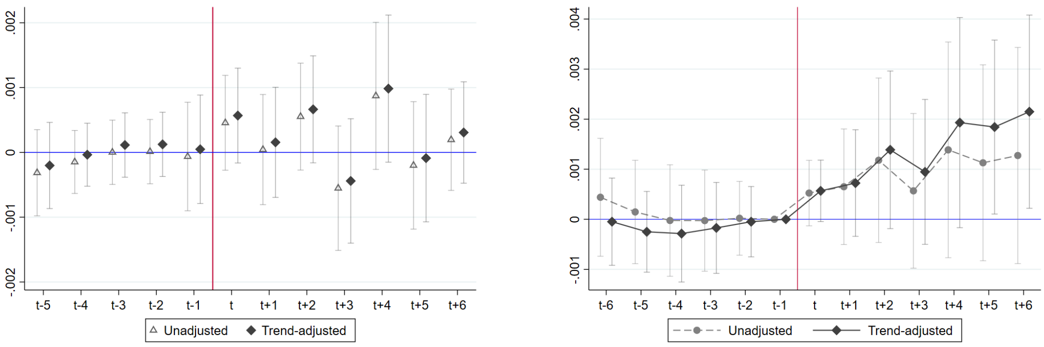
Figures D5 and D6 show sharp inflationary treatment effects at the period of the minimum wage hike for both wholesale and retail cannabis prices, and the effect is statistically significant at the 10% and 5% level, respectively.

[42] An alternative would be to use the median wage. Unfortunately, the QCEW does not publish median wages at the detailed industry-by-county level.

[43] For example, only 10% of Tier 1 manufacturers grow outdoors (Washington State Liquor and Cannabis Board, 2021).

[44] NAICS 11141 corresponds to "Food Crops Grown Under Cover" and includes as a subcategory "Other Food Crops Grown Under Cover, including Marijuana Grown Under Cover" (US Census Bureau, 2017b).

Figure D5: Direct pass-through to wholesale prices using 5-digit NAICS bite

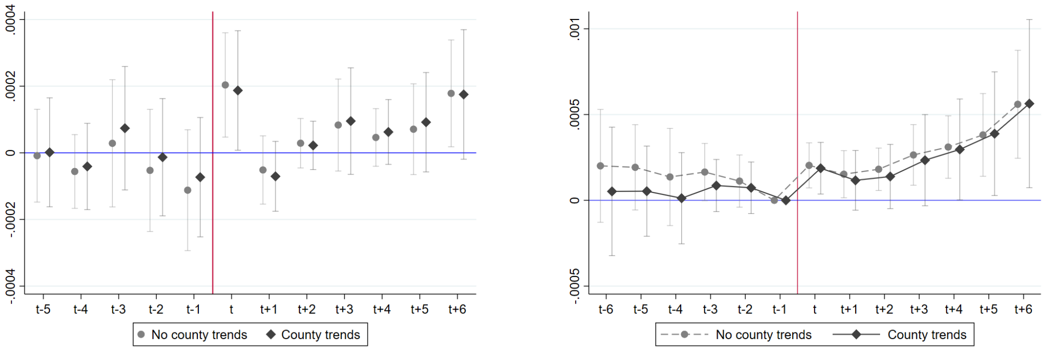


(A) Effect on the inflation rate

(B) Effect on the price level

Notes: The figures show estimates from equation 3 with the bite variable based on NAICS 11141. Tier 3 manufacturers and manufacturers are omitted from the estimation sample. Equation 3 is estimated with time fixed effects. The dependent variable is the establishment-level inflation rate, adjusted for a bite-specific trend as described in Section 6.1. The dependent variable is not trimmed. Panel (a) shows the distributed lag coefficients, β_l, with 90% confidence intervals based on SE clustered at the county level. Panel (b) depicts cumulative price level effects (E_L) relative to the baseline period in $t-1$. Cumulative effects E_L are obtained by summing the distributed lag coefficients to lead or lag L as detailed in section 5. Panel (b) shows 90% confidence intervals of the sums based on SE clustered at the county level. Data source: Top Shelf Data and Washington ESD, August 2018-July 2021.

Figure D6: Direct pass-through to retail prices using 5-digit NAICS bite



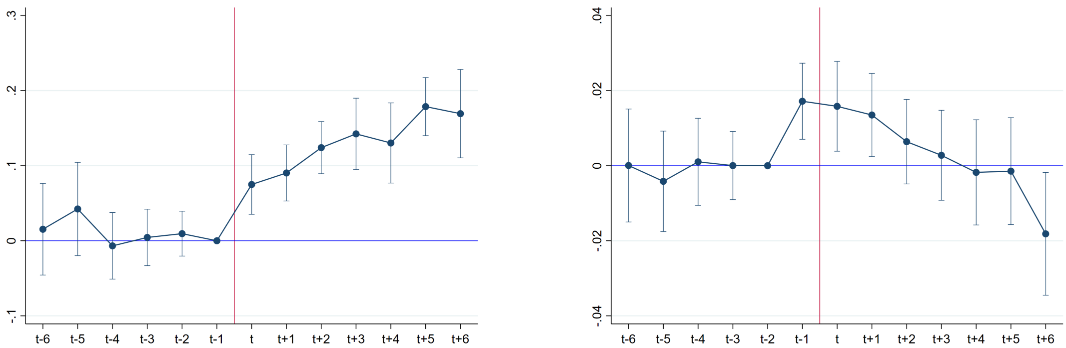
(A) Effect on the inflation rate

(B) Effect on the price level

Notes: The figures show estimates from equation 3 with the bite variable based on NAICS 45399. Equation 3 is estimated with time fixed effects. The dependent variable is the establishment-level inflation rate, which is not trimmed and not adjusted for a bite-specific trend. Panel (a) shows the distributed lag coefficients, β_l, with 90% confidence intervals based on SE clustered at the county level. Panel (b) depicts cumulative price level effects (E_L) relative to the baseline period in $t-1$. Cumulative effects E_L are obtained by summing the distributed lag coefficients to lead or lag L as detailed in section 5. Panel (b) shows 90% confidence intervals of the sums based on SE clustered at the county level. Data source: Top Shelf Data and Washington ESD, August 2018-July 2021.

Legislation vs. implementation for event 3 For event 3, the magnitude of the new minimum wage hike was announced in September 2020, three months before implementation on January 1st, 2021. In this section, I test whether price effects emerge at the time that the hike size was made public $(t-4)$ versus when it was implemented (t). To do this, I estimate equation 3 for event 3 only. Figure D7 shows no evidence of price level effects in $t-4$ for wholesale and retail prices. Instead, treatment effects appear in period $t-1$ for wholesale prices and $t-2$ for retail prices, which is identical to the results in the main part of the paper. Note that, in contrast to the main results, retail price effects for event 3 are undone in later periods and return to zero by $t+4$.

Figure D7: Direct pass-through for event 3



(A) Wholesale price level effects

(B) Retail price level effects

Notes: The figures show cumulative price level effects for event 3 only. Effects are cumulative relative to the normalized baseline period ($t-1$ for manufacturers, $t-2$ for retailers). Cumulative effects E_L are obtained by summing the distributed lag coefficients to lead or lag L as detailed in section 6.4. The figures show 90% confidence intervals of the sums based on SE clustered at the county level. The dependent variable is the establishment-level inflation rate. For the retail price level regression (panel b), the dependent variable is adjusted for a bite-specific trend as described in section 6.1. Data source: Top Shelf Data and Washington ESD, August 2020-July 2021.

E Shift-share instrument validity

E.1 Separate identification of the direct and indirect pass-through

In 5, a key assumption is that equation 5 separately identifies the direct and indirect effects of a minimum wage hike on retail cannabis prices. Two conditions would need to be jointly met for the direct pass-through estimates to be contaminated by indirect pass-through. First, retailers would need to purchase predominantly from manufacturers located in the retailer's own county. Second, the bite variable for retailers would need to correlate with bite for manufacturers within each county.[45] In Appendix **??**, I show that the neither of these conditions holds: over 85% of retailers' wholesale purchases are from manufacturers located in other counties, and the (conditional) within-county correlation between manufacturer and retail bite is -0.03. Finally, I examine whether the direct pass-through estimates $\hat{\beta}_l$ change if I exclude $Indirect_{j,t}$ from equation 5. If estimates for direct pass-through were to change, this would cast doubt on the main identification strategy. I show in Section 6.1 that direct pass-through estimates are unaffected by the exclusion of $\Delta Indirect_{j,t}$.

The geography of wholesale costs Table E4 shows the percentage of retailers' wholesale costs in relation to a manufacturer's geographic location. Column 1 shows that only 5.22% of retailers' wholesale expenditures go to manufacturers located in the same city as the retailer. Column 2 shows that less than 15% goes to manufacturers in the same county as the retailer. For Column 3, we sort counties into their respective 3-digit zip codes (retailers are located in 14 3-digit zip codes compared to 37 counties). Column 3 shows that less than 16% of wholesale cost goes to manufacturers located in the same 3-digit zip code. Next, we sort counties into three regions (west, central, east), defined by well-established topographic and economic boundaries. Column 4 shows that 62% of wholesale sales go to retailers in a different region than the manufacturer. Column 5 looks at the subset of establishments located in the west and east regions of the state, thus dropping manufacturers in the central region. The east and west regions are non-contiguous and are located on opposite sides of the state. For establishments located in these two regions, 23.9% of wholesale sales go to retailers located in the other region, that is to say, retailers on the opposite side of the state. Because the majority of retail establishments are located in the west and east regions, this share amounts to 21.4% of all of wholesale expenditures in the industry. Taken together, the results from Table E4 illustrate that there is no home bias in wholesale cannabis purchases.

[45] If the first condition is met but the second condition doesn't hold, then the minimum wage effect on wholesale prices is part of the error term but it is orthogonal to retail bite and hence does not bias direct pass-through estimates. If the second condition holds but not the first, then manufacturer bite and retailer bite are not independent but the minimum wage effect on wholesale prices in a given county has no impact on retail prices in that county since retailers don't purchase from local manufacturers.

Table E4: Share of retailers' wholesale costs by geographic proximity

	(1) Same city	(2) Same county	(3) Same 3-digit zip code	(4) Same region	(5) Non-contiguous region	(6) Same state
Percent of wholesale expenditure	5.22%	14.67%	15.59%	62.08%	23.90%	100%

Notes: This table shows the share of retailers' wholesale expenditure according to wholesalers' geographic proximity. The shares are based on 5.92 million unique wholesaler-retailer-product-month observations from August 2018 through July 2021. Retailers are located in 14 3-digit zip codes and 35 counties. Region groups counties into three categories: west, central, or east. Data from Top Shelf Data.

The relationship between manufacturer and retail bite Next, I illustrate that the relationship between manufacturer and retail bite within a county is weak. The raw correlation between bite for NAICS 111 and NAICS 453 is 0.18, but when controlling for county controls and FE, the relationship shrinks to -0.03. This indicates a very weak relationship between bite within counties.

Table E5: Within-county correlation between producer and retail bite

	(1) Raw correlation	(2) FE	(3) No FE
	0.18	-0.03	-0.05
		(0.09)	(0.09)
N	96	96	96

Notes: Column 1 shows the unconditional within-county correlation for bite. Columns 2 and 3 show OLS estimates from a county-level regression of bite for NAICS 111 on bite for NAICS 453, with county controls (log average wage and the unemployment rate, both for Q3). SE are in parentheses. Data from WA ESD, 2018-2020.

E.2 Assumptions for shift-share identification

In equation 5, the identifying variation in the indirect bite variable stems from retailers' differential exposure (i.e. wholesale expenditure shares) to a common set of shocks (i.e.

producer-processor origin county bite). Under this 'shares' interpretation of the shift-share instrument, (Goldsmith-Pinkham et al., 2020) show that instrument validity relies on two assumptions. In this subsection, I discuss these assumptions and provide supportive evidence in their favor.

First-stage relevance. First, the instrument must have predictive power for changes in retailers' wholesale cost, i.e. first-stage relevance. To demonstrate this, I construct a wholesale cost index for each retailer and estimate equation 5 with the wholesale cost index as the dependent variable. Figure E8 illustrates that the shift-share instrument induces a sharp and significant increase in retailers' wholesale costs. A 10% minimum wage increases retailers' wholesale costs by 2.7% (unadjusted, P-Value: 0.15) and 4.0% (trend-adjusted, P-value: 0.04) in the month of a minimum wage hike.

Share exogeneity. Second, retailers' wholesale expenditure shares can only affect retail price growth through indirect pass-through and not through any other potential confounding channel. Therefore, an important test is whether retail store and location characteristics systematically covary with retailers' wholesale expenditure shares (Goldsmith-Pinkham et al., 2020). Of particular importance is whether retailers' own minimum wage bite variable covaries with wholesale expenditure shares. If retailers with a high bite variable (and hence a large minimum wage-induced direct cost shock) systematically differ in their wholesale purchasing patterns compared to retailers with a low bite, then retailers' wholesale expenditure shares are not exogenous and the shift-share instrument is not consistent (Goldsmith-Pinkham et al., 2020). Besides bite, other characteristics include retailers' county-level unemployment rate and county-level log home value (demand shifters), as well as store-level characteristics like local market HHI, whether a store is independent or part of a chain, and product variety.[46]

Before proceeding, note that the shift-share estimator is equivalent to a weighted combination of estimates where each expenditure share is an instrument Goldsmith-Pinkham et al. (2020). These weights— called Rotemberg weights—provide insight into which of the shares get more weight in the overall design. Producer-processor origin counties with larger weights play a bigger role in the identifying variation. I estimate Rotemberg weights using the bartik.weight r package created by (Goldsmith-Pinkham et al., 2020). In Panel A of Table E6, I report the (event-specific) producer-processor origin counties with the five largest Rotemberg weights. By summing the Rotemberg weights for these five origin county-events, one sees that these counties account for 60% of the identifying variation driving the indirect pass-through estimates.

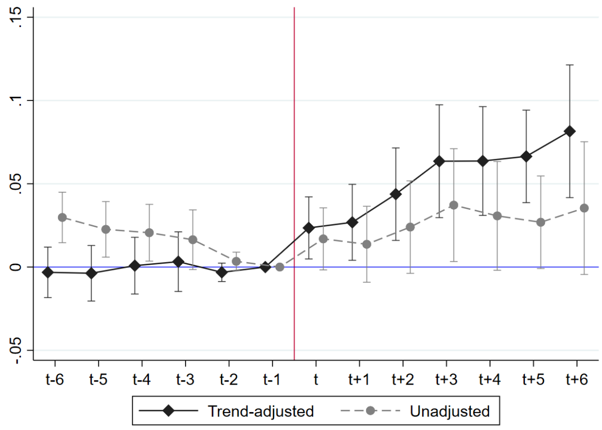
Next, I inspect the relationship between retail store characteristics and retailers' expen-

[46] I consider each retail cannabis store as the focal point of its own market comprising the set of cannabis stores (including the focal store) within a 5-mile radius. I calculate the Herfindahl–Hirschman Index (HHI) for each market.

diture shares for producer-processor origin counties. I focus on the producer-processor origin counties with the largest Rotemberg weights as these play an outsize role in the shift-share identification. For each of the five origin counties, I regress retailers' expenditure shares for those origin counties on retail store and location characteristics and region fixed effects.

I report the results from these regressions in Table E6, Panel B. Several facts stand out. First, the R-squared for the regressions ranges from 0.07 to 0.21, indicating that location- and store-level characteristics do not explain much of the variation in expenditure shares for the five most important producer-processor origin counties. Second, retailers' bite explains very little of the variation in wholesale expenditure shares for origin counties. This indicates that the magnitude of retailers' direct labor cost shock does not explain where retailers purchase their wholesale goods from. Third, while some coefficients are statistically significant, the magnitudes are low. For example, in Column 5 the coefficient on retailers' county-level unemployment rate is significant at the 5% level, yet a one percentage point increase in unemployment only increases a retailer's share of expenditures going to Pierce County by a meager 1.6%. Taken together, the results from Panel B provide suggestive evidence that wholesale expenditure shares are as good as random and that share exogeneity holds.

Figure E8: Effect of indirect bite on retailers' wholesale cost



Notes: The figure shows estimates from equation 5 where the dependent variable is the establishment-level wholesale cost index for cannabis retailers (in logs). The figure depicts cumulative wholesale cost effects (E_L) relative to the baseline period in $t-1$. Cumulative effects E_L are obtained by summing the distributed lag coefficients to lag L as detailed in the main text. The figure shows 90% confidence intervals of the sums based on SE clustered at the county level. Data source: Top Shelf Data and Washington ESD, July 2018 to August 2021.

Table E6: Manufacturer counties with the top 5 Rotemberg weights

(A) Summary of top 5 Rotemberg weights

Manufacturer county	Event	α_k	Bite$_k$	β_k
Thurston	2	0.158	37.7	0.0001
Whitman	2	0.129	12.1	-0.0043
Jefferson	3	0.112	24.4	0.0022
Yakima	2	0.109	16.5	-0.0011
Pierce	2	0.098	28.2	-0.0007

(B) Relationship between manufacturer county shares and retail store characteristics

	(1)	(2)	(3)	(4)	(5)
	Thurston county (event 2)	Whitman county (event 2)	Jefferson county (event 3)	Yakima county (event 2)	Pierce county (event 2)
Bite	0.0019**	-0.0009*	-0.0013	-0.0010	0.0005
	(0.0009)	(0.0005)	(0.0019)	(0.0009)	(0.0009)
Unemp. rate	-0.0109	0.0073	0.0092	0.0203*	0.0164**
	(0.0102)	(0.0055)	(0.0107)	(0.0118)	(0.0083)
Log home value	-0.0525	0.0183	-0.0302	0.0443	0.0551
	(0.0487)	(0.0257)	(0.0381)	(0.0314)	(0.0428)
Independent	0.0034	-0.0063	-0.0072	0.0022	0.0163*
	(0.0095)	(0.0054)	(0.0060)	(0.0065)	(0.0088)
Focal market HHI	-0.0226	0.0250**	-0.0118	0.0195	-0.0114
	(0.0193)	(0.0125)	(0.0161)	(0.0194)	(0.0222)
Product variety	0.000003	-0.000009	-0.00002***	-0.00004***	-0.00002***
	(0.000009)	(0.000008)	(0.000008)	(0.00001)	(0.000007)
R^2	0.068	0.210	0.169	0.149	0.116
N	369	80	113	251	309

Notes: Panel A reports statistics about the producer-processor county-events with the five largest Rotemberg weights. α_k are the Rotemberg weights for county-event k, $Bite_k$ is the minimum wage bite for crop production in county k, and β_k are the just-identified estimates based on each instrument. In Panel B, each column reports results of a single regression of producer-processor-county expenditure shares on retail store characteristics. Standard errors of the sums are clustered at the store level and shown in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

F Additional results for market structure and firm characteristics

G Wholesale cost pass-through regression

In this section, I examine how retailers adjust their prices in response to changes in wholesale unit cost. I employ methods from the extensive literature on cost pass-through (e.g. Hollenbeck and Uetake, 2021; Muehlegger and Sweeney, 2022; Conlon and Rao, 2020; Miller et al., 2017). My objective in estimating the unit cost pass-through rate is twofold. First, the unit cost pass-through elasticity can be combined with the wholesale price estimates from Section 6.1 to infer indirect the indirect effect of minimum wage hikes on retail cannabis prices. This provides a useful robustness check for the estimates obtained using the shift–share instrument in Section 6.1. Second, by augmenting the canonical pass-through regression with the wholesale costs of a store's competitors, I can estimate the sensitivity of retail prices to changes in competitors' costs. This allows me to determine the geographic scope of strategic complementarity in prices, which I use to define the boundary for the rivals specification in Section 6.4.

I proceed as follows: In Section G.1, I estimate the wholesale cost pass-through regression to determine the geographic scope of strategic complementarity in prices. In Section G, I use the regression results to calculate the implied indirect effect on retail prices from a minimum wage hike.

G.1 The scope of strategic complementarity in pricing

Theoretical framework

I follow the framework of Muehlegger and Sweeney (2022) and consider the pass-through of a tax (or input cost shock) τ onto the price of firm j. Firm j sets the profit-maximizing price p_j and faces tax-inclusive marginal costs α_j. Each firm in the market can have a different exposure to the tax, with $\frac{\partial \alpha_j}{\partial \tau}$ capturing the marginal unit tax rate faced by firm j. In oligopolistic markets, the price a firm sets is a function of not just its own costs, but also those of its rivals. The pass-through of the tax onto firm j's price can thus be decomposed as a direct (own-cost) and an indirect (competitors' cost) effect:

$$\frac{\partial p_j}{\partial \tau} = \frac{\partial p_j}{\partial \alpha_j}\frac{\partial \alpha_j}{\partial \tau} + \sum_{i \neq j} \frac{\partial p_j}{\partial p_i}\frac{\partial p_i}{\partial \alpha_i}\frac{\partial \alpha_i}{\partial \tau} \tag{18}$$

where $\frac{\partial p_j}{\partial p_i}$ is firm j's best response to a change in firm i's price.[47] Consequently, in the presence of imperfect competition, the strategic response of (untreated) competitors may disqualify them as a valid control group. It is therefore important to quantify the size and geographic scope of strategic complementarity in prices.

[47]For ease of exposition I consider competition in prices. Muehlegger and Sweeney (2022) show that this framework extends to a broad class of oligopolistic settings.

Empirical framework

To identify the scope of strategic complementary in prices, I follow the industrial organization literature that measures the pass-through of cost shocks and taxes. In particular, I build on the approach of Hollenbeck and Uetake (2021), who use similar data to evaluate the optimal cannabis sales tax. A major advantage of this approach is that, because I observe wholesale unit prices, I can directly measure how changes in unit cost are passed through to prices.[48] In addition to stores' own wholesale unit costs, I also observe the wholesale unit costs of their competitors. By relating stores' prices to competitors' cost changes, I can measure the effect of competitors' cost-induced price changes, i.e. strategic complementarity in prices. Moreover, I can test whether this effect is a function of the geographic distance between stores. I use the results of this analysis to guide my definition of rival stores in Section 6.4.

To investigate the geographic scope of strategic complementarity of prices, I sort competitors into 5-mile bins and calculate average wholesale unit price for each store-product-month-bin. I specify a model at the store-product-month level that relates a store-product's retail price to (i) the wholesale unit price and (ii) the average wholesale unit price paid by stores in each distance bin. By including both own costs and competitors' costs, I capture the total effect (i.e. own-cost and strategic price response) of an aggregate unit cost shock on stores' prices. Since cannabis transaction data is publicly available, stores have full information on competitors' unit costs and prices updated on an almost weekly basis. Therefore, I focus on contemporaneous changes in costs and prices. This is in line with the pass-through literature from other industries (see e.g. Hollenbeck and Uetake, 2021; Muehlegger and Sweeney, 2022; Conlon and Rao, 2020; Miller et al., 2017). I estimate the following model in first-differences:

$$\Delta p_{i,j,t} = \rho \Delta w_{i,j,t} + \sum_{r=1}^{R} \beta_r \Delta w_{i,r(j),t} + \Delta \gamma_t + \Delta \varepsilon_{i,j,t}, \tag{19}$$

where $p_{i,j,t}$ is the average price (in dollars) of product i sold at store j in month t, $w_{i,j,t}$ is the average wholesale price that retailer j pays for product i in month t, $w_{i,r(j),t}$ is the average wholesale price that competitors pay for product i in month t, and γ_t is the year-month FE. In my baseline specification, I set $R = 9$ ($R > 9$ does not meaningfully affect estimates but changes the sample size and standard errors). I cluster standard errors at the store level to allow for autocorrelation in unobservables within stores.

The effect of an aggregate change in unit costs on store j's prices comprises two parts. The first is the own cost pass-through rate, ρ, i.e. the increase in retail unit price at store j from the increase in store j's own wholesale unit cost. The second part is β_r which measures the pass-through of wholesale unit costs at competing stores in bin r to unit prices at store

[48]Wholesale costs are typically estimated from supply-side first order conditions. For similar approaches, see, for instance, Muehlegger and Sweeney (2022); Ganapati et al. (2020) who use variation in energy input costs to estimate the price pass-through of a hypothetical carbon tax or Miller et al. (2017) who estimate the pass-through of carbon pricing in the portland cement industry.

j. This is equivalent to the strategic price response between store j and competing stores in bin r.

In addition to my main specification, I report several variants of my pass-through regression in Table G7. In Column 2, I specify equation 19 using the first-difference of logs. This minimizes the influence of outliers and delivers pass-through elasticities instead of pass-through rates. In Column 3, I revert to dollars but specify the equation in levels with store-product FE.

Empirical results

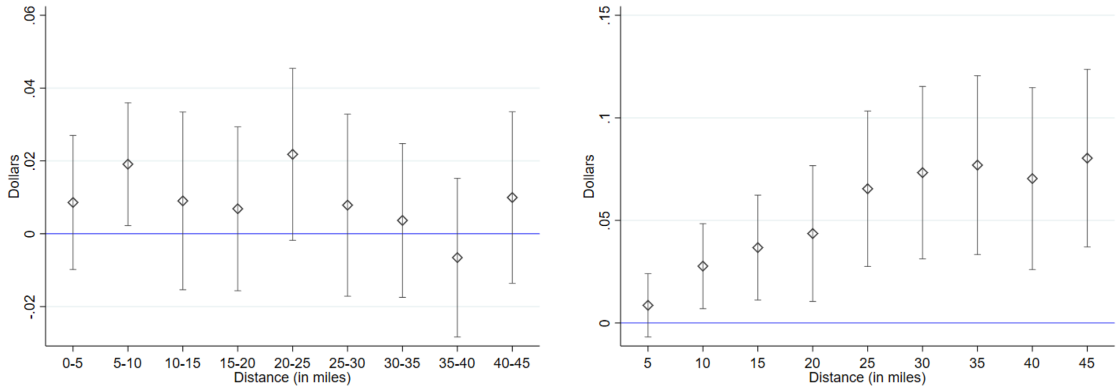
I find that a \$1 increase in a store's own unit cost leads to a retail price increase of \$1.65, implying an over-shifting of costs onto consumers. This over-shifting is consistent with the substantial market power of retailers. Moreover, the estimate is in line with Hollenbeck and Uetake (2021), but also a common finding for empirical studies estimating cost pass-through in other industries (Pless and van Benthem, 2019).

In Figure G9 Panel A, I report estimated pass-through rates of competitors' unit costs, β_r, from my baseline specification. The estimates differ across bins with the largest effects in the 5-10 mile and 20-25 mile bins. The fact that the effect fluctuates with distance could reflect commuting patterns, with the average daily distance travelled in Washington state ranging from less than 20 miles in some counties to more than 70 in others (Axios, 2024). Nevertheless, at the 25-30 mile bin, effects shrink and remain close to zero for three consecutive bins.

The β_r estimates in Panel A can be interpreted as marginal effects in that they measure the additional effect on store j's prices of increasing the geographic scope of an aggregate change in costs by another 5 miles. While this is informative about the geographic scope of strategic complementarity in cannabis prices, quantifying the actual effect on prices of an aggregate change in costs requires summing the marginal effects $\sum_{r=1}^{R} \beta_r$ up to a given distance bin R. The sum can be interpreted as the effect on store j's prices of an aggregate change in costs that affects all stores up to a given distance (while holding store j's costs constant). I report these sums at increasing distances in Panel B of Figure G9. Panel B further highlights that sensitivity to competitors' costs increases up to the 30-mile mark before plateauing thereafter. This aligns with a growing literature showing that the scope of cost shocks matters and that aggregate (i.e market-wide) cost shocks elicit a larger strategic price response than idiosyncratic or highly localized shocks (Muehlegger and Sweeney, 2022).

Table G7 illustrates that stores' sensitivity to changes in the costs of their competitors plateaus at the 30-mile mark across across all specifications. Moreover, the results indicate that an aggregate cost shock with sufficient geographic scope has non-negligible strategic price effects. When estimated in first-differences (Column 1), a \$1 increase in wholesale unit costs at all stores within a 30-mile radius corresponds to a \$0.09 increase in retail prices solely due to strategic complementarities. When estimated in levels (Column 3), prices increase \$0.45

Figure G9: The pass-through of competitors' wholesale unit costs to own unit prices



(A) Marginal effects

(B) Cumulative effects

Notes: Panel A shows estimated coefficients β_r for $r \in [1, 9]$ obtained from the pass-through regression (equation 19). Coefficients in Panel A are interpretable as the effect (in dollars) on store j's retail unit price from a \$1 increase in wholesale unit costs affecting all stores in distance bin r. Panel B shows cumulative sums of coefficients $\sum_{r=1}^{R} \beta_r$ for $R \in [1, 9]$. Coefficients in Panel B are interpretable as the effect on store j's retail unit price of a \$1 increase in wholesale unit cost affecting all stores up to r miles away. The figure shows 90% confidence intervals of the sums based on SE clustered at the store level. Data: Top Shelf Data, March 2018 through December 2021.

from a \$1 increase in wholesale cost.

Overall, the results from Figure G9 and Table G7 provide suggestive evidence of strategic complementarity in prices for cannabis stores within 30 miles of each other. Increasing the geographic scope of an aggregate cost shock appears to have little additional effect on store prices beyond the 30-mile mark. This suggests that stores located more than 30 miles from each other will not have a strategic price response to each others' minimum wage-induced labor cost shock. This empirically validates my choice of 30 miles as the cutoff for defining rival stores in Section 6.1.

G.2 The implied indirect effect of minimum wages on retail prices

In Section 6.1, I use a shift-share instrument to estimate the indirect effect of minimum wage hikes on retail cannabis prices. In this section, I use an alternative approach to estimate the indirect effect. Formally, the indirect minimum wage pass-through elasticity can be expressed as:

$$\frac{\partial P}{\partial MW}\frac{MW}{P} = \frac{\partial P}{\partial P^X}\frac{P^X}{P} \cdot \frac{\partial P^X}{\partial MW}\frac{MW}{P^X} \tag{20}$$

where P denotes the retail price level (in dollars) at a store, P^X is the wholesale price level that the store pays, and ∂MW is minimum wage hike (in dollars). I can estimate this elasticity as the product of two factors: (i) the wholesale cost pass-through elasticity, i.e. the percent change in unit retail prices resulting from a one percent increase in unit wholesale cost, and (ii) the minimum wage elasticity of wholesale prices. I obtained an estimate of (i) in the previous subsection, while I estimated (ii) in Section 3.

Table G7: Cumulative pass-through of competitors' unit costs

	(1) Dollars (FD)	(2) Logs (FD)	(3) Dollars (levels)
Own wholesale cost	1.654***	0.712***	1.294***
	(0.035)	(0.008)	(0.375)
Competitors' wholesale cost			
< 5 miles	0.004	0.001	0.068*
	(0.011)	(0.003)	(0.037)
< 10 miles	0.021	0.005	0.174***
	(0.014)	(0.003)	(0.058)
< 15 miles	0.031*	0.003	0.193**
	(0.018)	(0.004)	(0.081)
< 20 miles	0.056**	0.005	0.300***
	(0.023)	(0.005)	(0.100)
< 25 miles	0.087***	0.010**	0.319***
	(0.025)	(0.005)	(0.111)
< 30 miles	0.088***	0.015***	0.449***
	(0.027)	(0.005)	(0.139)
< 35 miles	0.097***	0.015***	0.474***
	(0.030)	(0.005)	(0.158)
< 40 miles	0.077**	0.014***	0.470***
	(0.031)	(0.005)	(0.165)
< 45 miles	0.087***	0.011**	0.422***
	(0.028)	(0.005)	(0.159)
N	2,012,861	2,012,861	3,239,632

Notes: The table reports estimates of pass-through rates of wholesale unit cost to retail unit price at the store-product-month level, according to equation 19. We report estimates for own wholesale cost changes and for average changes in wholesale costs at competing stores according to the distance-bins described in the main text. Competing store effects are cumulative sums $\sum_{r=1}^{R} \beta_r$. Coefficients are interpretable as the dollar increase in prices resulting the strategic price response from a $1 dollar wholesale cost shock affecting all stores within a given distance. Standard errors are clustered at the store level and shown in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

The combined estimates of the unit cost pass-through elasticity (from Table G7, Column 2) and the minimum wage elasticity of wholesale prices (from section 6.1) allow me to compute indirect pass-through to retail prices using equation 20. The baseline point estimate for the indirect pass-through elasticity is $0.71 \times 0.17 = 0.12$ (95% CI: 0.047, 0.219).[49] In other words, a 10% increase in the minimum wage leads to a 1.2% increase in retail cannabis prices from indirect pass-through, i.e. solely due to the effect of minimum wage hikes on wholesale prices. This is remarkably close to the indirect pass-through elasticity obtained using the shift-share instrument in Section 6.1. This provides further evidence in support of my indirect pass-through estimates from the main part of the paper.

[49] I calculate confidence intervals using the delta method under the assumption that the errors for the unit cost pass-through and the direct pass-through to wholesale prices are independent.

H Tradability in other sectors

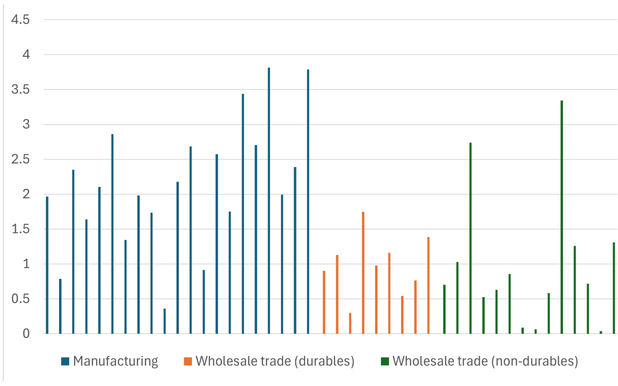
In this section, I use data from the Commodity Flow Survey to demonstrate that tradability is a common feature in many upstream sectors. The Commodity Flow Survey contains national and state-level data on domestic freight shipments by establishments in mining, manufacturing, wholesale, and other industries. The survey includes information on the industry, origin and destination, value, weight, modes of transportation, distance shipped, and ton-miles of commodities shipped in the U.S. Using this data, I construct a measure of tradability for each three-digit NAICS industry in the manufacturing, wholesale durables, and wholesale non-durables sectors. Specifically, I calculate the tradability ratio for a given industry n as

$$TR_n = \frac{\sum_s TV_{s,s'}}{\sum_s TV_{s,s}} \tag{21}$$

For a given industry n, and $s \neq s'$, $TV_{s,s'}$ is the total value of shipments with origin in state s' and destination in state s.[50] The numerator of equation 21 is therefore the total value of imports for industry n across states. $TV_{s,s}$ is the total value of shipments with both origin and destination in state s, so that the denominator measures the total value of within-state shipments in industry n. The ratio TR_n thus measures the home bias of a given industry, with a low value of TR_n corresponding to less home bias. In Figure H10, I depict TR_n for each three-digit NAICS industry in the manufacturing, wholesale durables, and wholesale non-durables sectors. The figure illustrates that the vast majority of manufacturing industries have a TR_n value that is greater than one, and several have values greater than 2. This indicates that manufacturing firms in the U.S. tend to sell tradable goods. In contrast, industries in the durable and non-durable wholesale trade sectors exhibit lower TR_n values. However, numerous industries have a TR_n close to one (i.e. approximately half the value of all shipments comes from out-of-state), which nevertheless indicates a substantial degree of tradability. Figure H10 therefore provides suggestive evidence that upstream industries (i.e. those in manufacturing and wholesale sectors) sell what can be considered tradable goods.

[50] Total value equals the shipment value multiplied by the CFS weighting factor (see US Census Bureau, 2017a).

Figure H10: The ratio of out-of-state to within-state commodity flows in the U.S.



Notes: The figure summarizes inter- and intra-state trade flows for all manufacturing and wholesale trade sectors contained in the U.S. Commodity Flow Survey. Each bar depicts the value of inter-state trade divided by the value of intra-state trade for a given industry. Data is from the 2017 Commodity Flow Survey.

I Other margins of firm adjustment

Table I1 presents the results of equation 3 estimated for other margins of adjustment to the minimum wage. Columns 1-2 use the first difference of log county employment as the dependent variable. Employment in column 1 is based on NAICS 11141 (”food crops grown under cover”), which contains cannabis manufacturers that grow indoors.[51] Column 2 uses NAICS 45399 (”all other miscellaneous store retailers”), which contains cannabis retailers. Column 3 uses the log of the establishment-level quantity index as the dependent variable. This measures the month over month percent change in quantity sold by a given establishment (see Appendix Section **??** for details). Note that cannabis consumption grows steadily throughout the sample period, and this growth may correlate with bite. Therefore, I estimate the regression with and without store FE, where the latter account for secular trends in quantity sold (since the dependent variable is in first differences). For completeness, the columns 5-6 reports the same specifications with the price index as the dependent variable. This is included to illustrate that including store trends does not change the price effects found in the main part of the paper.

The results in column 1 indicate that there is no negative employment effect for employment in the indoor crop industry (which contains cannabis manufacturers). For misc. store retailers (which contains cannabis retailers), employment increases by 2.6% in the month of the hike (significant at the 10% level), but the effect becomes insignificant by two months after the hike. Overall, columns 1-2 do not provide strong evidence of lasting employment effects. Column 3 shows evidence of a small, positive trend in quantity sold throughout the entire event window, with no sharp effects at any point. Column 4 shows that this time trend

[51] I do not estimate employment effects for the NAICS industry containing outdoor cannabis growers because the majority of manufacturers in Washington state grow indoors. See Section **??** for details.

largely disappears when store trends are included. In both specifications, there is no sharp change in quantity sold. This provides suggestive evidence that there is no effect of minimum wages on quantities sold at cannabis retailers. Column 5 reports the price effects from section 6.1, while column 6 includes store price trends. The results in columns 5-6 are very similar in magnitude, though standard errors are larger with store trends included. Taken together, columns 5-6 show that the price level effects from section 6.1 do not depend on the exclusion of store trends.

Table I1: Minimum wage effects on employment and quantity sold

	Employment		Quantity sold		Price	
	(1)	(2)	(3)	(4)	(5)	(6)
	Indoor crops	Misc. store retailers	No trends	Store trends	No trends	Store trends
E_0	-0.011	0.026*	-0.0007	-0.002***	0.003**	0.003**
	(0.027)	(0.014)	(0.001)	(0.001)	(0.001)	(0.002)
E_2	0.007	0.015	0.0005	-0.002***	0.003**	0.003*
	(0.029)	(0.014)	(0.001)	(0.001)	(0.001)	(0.002)
E_4	-0.034	0.033	0.002**	-0.001*	0.004**	0.004
	(0.032)	(0.024)	(0.001)	(0.001)	(0.002)	(0.003)
$\sum$ Pre-event	-0.024	0.020	-0.003***	-0.001*	-0.0002	-0.0001
	(0.028)	(0.024)	(0.001)	(0.001)	(0.001)	(0.001)
N	603	851	13,196	13,196	14,044	14,044

The table reports cumulative effects E_L relative to the normalized baseline period in $t-2$, as described in section 5. The dependent variables are: first-difference of log monthly county employment for NAICS 11141 (column 1), and NAICS 45399 (column 2); log establishment quantity index (columns 3-4); log establishment price index (columns 5-6). Columns 3-6 are trimmed by 1% as described in Section 5. The treatment intensity is defined as in previous Section 5. All specifications are in first differences and include month-year FE. Standard errors are clustered by county. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Data from the QCEW and Washington ESD, 2018-2021.

J Other margins of adjustment to minimum wage hikes

While the primary focus of this paper is the price level effects of minimum wage hikes, firms may adjust to the cost shock along other margins as well. In this section, I examine several other possible channels for firm adjustment.

J.1 Employment effects

I first examine the employment effects of minimum wage hikes. Since employment information is not available for cannabis establishments, I use monthly employment data from the QCEW at the 5-digit NAICS industry level.[52] My dependent variable is the first difference of (log) county employment. I construct separate panels of my dependent variable for the NAICS industries containing cannabis retailers and manufacturers. I then estimate my main regression model (equation 3) separately for each of these panels.

Appendix Table **??** shows that employment effects are mostly insignificant, suggesting that minimum wage hikes have no effect on employment in the industries containing cannabis establishments. However, I caution against over-interpreting these results. Since cannabis workers are a subset of employees at the 5-digit NAICS level, one cannot definitively rule out employment effects at cannabis establishments.

J.2 Demand feedback

This paper treats minimum wage hikes as a cost shock to cannabis establishments. However, it is conceivable that by raising the incomes of low-wage workers, minimum wage hikes affect demand for cannabis products which in turn could contribute to the retail price effects found in the main part of the paper.[53] In this subsection, I test for such demand effects. If the retail price elasticity of demand for cannabis products is non-zero, then a regression of quantity sold on treatment intensity will suffer from simultaneity. Nevertheless, the resulting bias will lead to conservative estimates and hence provide a lower bound—and useful test—of possible demand effects.[54] To test for demand effects

To investigate the effect of minimum wage hikes on demand, I construct an establishment-level quantity index as a proxy for demand. The quantity index, which is constructed the same way as the price index, measures the monthly percent change in quantity sold at the establishment level. I estimate my main regression equation (equation 3) for retailers with the quantity index as the dependent variable. I report the results in Appendix Table I1. The treatment effects are very close to zero and not statistically significant. This provides suggestive evidence that minimum wage hikes do not affect store-level demand for cannabis retailers.

[52] Cannabis retailers belong to NAICS 45399 ("all other miscellaneous store retailers"). Cannabis manufacturers that grow indoors belong to NAICS 11141 ("food crops grown under cover"). I do not estimate employment effects for the NAICS industry containing outdoor growers because the majority of manufacturers in Washington state grow indoors.

[53] Leung (2021) finds more than full minimum wage pass-through to grocery store prices in the U.S. and attributes part of the price effect to demand feedback.

[54] Treatment intensity is endogenous because it simultaneously affects prices and quantity demanded. Since $cov(p_{r,t}, \Delta MW_{r,t-l} \times Bite_{k(r),t-l}) > 0$, $cov(q_{r,t}, \Delta MW_{r,t-l} \times Bite_{k(r),t-l} > 0)$, and $cov(q_{r,t}, p_{j,t}) < 0$, OLS estimates are negatively biased.

J.3 Productivity: a discussion

Productivity is a further channel through which the minimum wage may affect firms and workers. Several mechanisms have been proposed in the literature. First, since the minimum wage reduces the price of physical capital relative to labor, firms may substitute machinery for workers. Mayneris et al. (2018) find evidence of this in the context of the 2004 minimum wage reform in China. I view this scenario as unlikely in the cannabis context, since cannabis production leaves little scope for technological adjustments. Most manufacturers grow cannabis indoors in a setting averse to mechanization, and most cannabis is harvested, dried, and trimmed by hand to produce aesthetically pleasing, higher-priced buds (see **??** for details).

Ku (2022) proposes a different mechanism through which the minimum wage might affect productivity: if the minimum wage causes workers to anticipate potential layoffs, workers may increase their individual effort to avoid being laid off. Alternatively, firms may substitute out of low-skilled labor and into high-skilled labor. Unfortunately, I do not observe employment at the establishment level so I cannot test either of these mechanisms.

The cost shock could also induce firms to adopt better management or organizational practices, which has been shown to increase productivity without the need for physical capital investment (Bloom et al., 2013; Atkin et al., 2017). To the extent that productivity-enhancing changes to business processes are implemented, they are likely long-term adjustments that carry a considerable lag before any productivity gains are realized. In that case, pass-through effects would decrease with successive minimum wage hikes. Yet I find the opposite to be true: of the three minimum wage events in my sample period, event 1 has the smallest pass-through effects while event 3 has the largest. Ultimately, however, I do not observe worker or firm productivity and therefore cannot rule out this channel.

K The minimum wage elasticities of marginal cost

In this section, I present a general theoretical model that illustrates the relationship between the minimum wage elasticity of prices and the minimum wage elasticity of marginal cost at constant output. I build on the model from Renkin et al. (2022) by adding a COGS component to the minimum wage elasticity of marginal cost.

I assume that cannabis retailers have a production technology $Q = F(X; L)$, where F is homogeneous to some degree. X is a composite intermediate input defined by a linear homogeneous aggregator G over M different cannabis products, $X = G(X_1, X_2, ..., X_M)$, with wholesale prices $P^x_1, P^x_2, ...P^x_M$. Similarly, L is a composite input defined by a linear homogeneous aggregator H over N different types of labor inputs $L = H(L_1, L_2, ..., L_N)$ with wages $W_1, W_2, ...W_N$. I assume competitive labor and intermediate input markets. The overall cost function can be expressed as $C_Q(\overline{W}, \overline{P^x}, Q)$ where $\overline{W}$ and $\overline{P^x}$ are the wage and price indexes at optimality. The effect of minimum wages on overall marginal cost can decomposed as:

$$MC_Q(\overline{W}, \overline{P^x}, Q) = \frac{\partial MC_Q^L}{\partial MW} + \frac{\partial MC_Q^X}{\partial MW}$$

where the first term is the effect that operates through the labor cost channel and the second term is the effect that operates through the COGS channel. I assume that the firm considers the labor and COGS components independently in its cost minimization problem. In the following two subsections, I derive the elasticity of each component of marginal cost to minimum wages keeping output constant.

K.1 The labor component of the elasticity of marginal cost

First, I derive the elasticity of the labor component of marginal cost. The derivations in this subsection closely follow those from Renkin et al. (2022).

Step 1: Deriving the labor cost index. I am interested in the factor price index $\overline{W}$ that measures the marginal cost of increasing L. Under the assumption that the firm minimizes each component of cost independently, the firm minimizes labor cost C_L as follows:

$$\begin{aligned} C_L(L, W_1, W_2, ...W_N) = & \min_{L_1, L_2, ...L_N} W_1L_1 + W_2L_2 + ... + W_NL_N \\ & \text{s.t.} \quad L = H(L_1, L_2, ..., L_N) \end{aligned}$$

The FOC for any L_i is $\lambda_i H_i' = W_i$. λ_i is the Lagrange multiplier and equals MC_{L_i}, the marginal cost of labor input L_i. To obtain the marginal cost of the composite input $L = H(L_1, L_2, ...L_N)$, one can leverage the fact that H is homogeneous of degree one:

$$C_L(L, W_1, W_2, ...W_N) = \arg\min_{L_i} \sum_{i=1}^{N} \lambda_i H_i' L_i = \lambda L$$

As λ equals the marginal cost of increasing labor inputs, one can plug in $\lambda = MC_L$ and solve the differential equation $C_L = MC_L L$. This yields $C_L = \overline{W}L$ for some $\overline{W}$ that is constant in L. This implies that marginal cost equals average cost, both are independent of the overall level of L, and $\overline{W} = C_L/L$:

$$\overline{W}(W_1, W_2, ..., W_N) = \sum_{i=1}^{N} \frac{W_i L_i^*}{L}$$

Step 2: Deriving the minimum wage elasticity of marginal cost (labor component) The overall cost function can be expressed as $C_Q(\overline{W}, \overline{P^x}, Q)$ and the overall marginal cost function can be written as $MC_Q(\overline{W}, \overline{P^x}, Q)$ ($\overline{P^x}$ is the wholesale cost index, see Section K.2 below). I am interested in the minimum wage-induced change in marginal cost that operates through the labor cost channel (as opposed to COGS). The derivative of marginal cost w.r.t.

minimum wages (via the labor cost channel) is:

$$\frac{\partial MC_Q^L}{\partial MW} = \frac{\partial \frac{\partial C_Q}{\partial Q}}{\partial \overline{W}} \frac{\partial \overline{W}}{\partial MW} = \frac{\partial L}{\partial Q} \frac{\partial \overline{W}}{\partial MW}$$

where the last step uses Shepard's Lemma. Next, converting the derivative to an elasticity:

$$\frac{\partial MC_Q^L}{\partial MW} \frac{MW}{MC_Q^L} = \underbrace{\frac{\overline{W} L}{C}}_{(i)} \underbrace{\frac{\partial \overline{W}}{\partial MW} \frac{MW}{\overline{W}}}_{(ii)} \underbrace{\frac{AC_Q}{MC_Q}}_{iii} \underbrace{\frac{\partial L}{\partial Q} \frac{Q}{L}}_{iv} \tag{22}$$

The labor component of the minimum wage elasticity of marginal cost is the product of (i) the share of labor cost in total variable cost; (ii) the minimum wage elasticity of average wages; (iii) the ratio of average cost to marginal cost; (iv) the output elasticity of labor demand.

K.2 The COGS component of the elasticity of marginal cost

Next, I derive the COGS component of the minimum wage elasticity of marginal cost.

Step 1: deriving the COGS index I am interested in the factor price index $\overline{P^x}$ that represents the marginal cost of increasing the composite intermediate input X. The firm minimizes intermediate input cost C_X as follows:

$$\begin{aligned} C_X(X, P_1^x, P_2^x, ...P_M^x) = \min_{X_1, X_2, ...X_M} \; & P_1^x X_1 + P_2^x X_2 + ... + P_N^x X_M \\ \text{s.t.} \quad & X = G(X_1, X_2, ..., X_M) \end{aligned}$$

Using the same logic as above, one can obtain the marginal cost of the composite input $X = G(X_1, X_2, ...X_M)$ by leveraging the fact that G is homogeneous of degree one:

$$C_X(X, P_1^x, P_2^x, ...P_M^x) = \arg\min_{X_i} \sum_{i=1}^{M} \gamma_i G_i' X_i = \gamma X$$

Since γ equals the marginal cost of increasing intermediate inputs, one can plug in $\gamma = MC_X$ and solve the differential equation $C_X = MC_X X$. This yields $C_X = \overline{P^x} X$ for some $\overline{P^x}$ that is constant in X. This implies that marginal cost equals average cost, both are independent of the overall level of X, and $\overline{P^x} = C_X / X$:

$$\overline{P^x}(X_1, X_2, ..., X_M) = \sum_{i=1}^{M} \frac{P_i^x X_i^*}{X}$$

Step 2: Deriving the minimum wage elasticity of marginal cost (COGS component)

$$\frac{\partial MC_Q^X}{\partial MW} = \frac{\partial \frac{\partial C_Q}{\partial Q}}{\partial \overline{P^x}} \frac{\partial \overline{P^x}}{\partial MW} = \frac{\partial X}{\partial Q} \frac{\partial \overline{P^x}}{\partial MW}$$

where the last step uses Shepard's Lemma. Converting the derivative to an elasticity:

$$\frac{\partial MC_Q^X}{\partial MW} \frac{MW}{MC_Q^X} = \underbrace{\frac{\overline{P^x} X}{C}}_{(i)} \underbrace{\frac{\partial \overline{P^x}}{\partial MW} \frac{MW}{\overline{P^x}}}_{(ii)} \underbrace{\frac{AC_Q}{MC_Q}}_{(iii)} \underbrace{\frac{\partial X}{\partial Q} \frac{Q}{X}}_{(iv)} \tag{23}$$

The COGS component of the minimum wage elasticity of marginal cost is the product of (i) the COGS share of total variable cost; (ii) the minimum wage elasticity of average wholesale price; (iii) the ratio of average cost to marginal cost; (iv) the output elasticity of intermediate inputs.

K.3 Putting it all together

(a) When F is homogeneous of degree h, the cost function can be written as $C(Q) = Q^{\frac{1}{h}}\omega$, where ω is constant in Q and typically depends on factor prices. This implies that

$$\frac{AC}{MC} = \frac{Q^{\frac{1}{h}-1}\omega}{\frac{1}{h}Q^{\frac{1}{h}-1}\omega} = h$$

(b) At the optimum, $L(\overline{W}, Q) = \frac{\partial C(\overline{W}, \overline{P^x}, Q)}{\partial \overline{W}}$ and $X(\overline{P^x}, Q) = \frac{\partial C(\overline{W}, \overline{P^x}, Q)}{\partial \overline{P^x}}$. Applying Shepard's Lemma,

$$\frac{\partial L}{\partial Q} \frac{Q}{L} = \frac{\partial \frac{\partial C}{\partial \overline{W}}}{\partial Q} \frac{Q}{\frac{\partial C}{\partial \overline{W}}} = \frac{\partial (Q^{\frac{1}{h}} \frac{\partial \omega}{\partial \overline{W}})}{\partial Q} \frac{Q}{Q^{\frac{1}{h}} \frac{\partial \omega}{\partial \overline{W}}} = \frac{1}{h} Q^{\frac{1}{h}-1} Q^{1-\frac{1}{h}} = \frac{1}{h}$$

and

$$\frac{\partial X}{\partial Q} \frac{Q}{X} = \frac{\partial \frac{\partial C}{\partial \overline{P^x}}}{\partial Q} \frac{Q}{\frac{\partial C}{\partial \overline{P^x}}} = \frac{\partial (Q^{\frac{1}{h}} \frac{\partial \omega}{\partial \overline{P^x}})}{\partial Q} \frac{Q}{Q^{\frac{1}{h}} \frac{\partial \omega}{\partial \overline{P^x}}} = \frac{1}{h} Q^{\frac{1}{h}-1} Q^{1-\frac{1}{h}} = \frac{1}{h}$$

Combining *(a)* and *(b)*, $\frac{AC}{MC} \frac{\partial L}{\partial Q} \frac{Q}{L} = \frac{AC}{MC} \frac{\partial X}{\partial Q} \frac{Q}{X} = 1$. As a result, equation 22 simplifies to

$$\frac{\partial MC_Q^L}{\partial MW} \frac{MW}{MC_Q^L} = \frac{\overline{W} L}{C} \frac{\partial \overline{W}}{\partial MW} \frac{MW}{\overline{W}}$$

and equation 23 simplifies to

$$\frac{\partial MC_Q^X}{\partial MW} \frac{MW}{MC_Q^X} = \frac{\overline{P^x} X}{C} \frac{\partial \overline{P^x}}{\partial MW} \frac{MW}{\overline{P^x}}$$

Thus, the labor cost component of the minimum wage elasticity of marginal cost equals the minimum wage elasticity of the average wage times that labor share of variable cost. The COGS component of the minimum wage elasticity of marginal cost equals the minimum wage elasticity of the average wholesale price times that COGS share of variable cost.

Assumptions

The derivations in this section rely on three assumptions. First, I must assume that different labor and intermediate inputs can be aggregated in a linearly homogeneous way. This holds if the shares of different worker and product types are independent of the store size. Second, I must assume that cannabis stores' production technology is homogeneous to some degree. This assumption is not very restrictive and it is fulfilled by all commonly used production functions. Third, I assume that output is constant. If this assumption is violated, then any change in output affects marginal cost in a manner that is not accounted for here. I investigate the effects of minimum wage hikes on cannabis store output in Appendix **??**. I find little evidence of a change in stores' output, which provides suggestive evidence of the constant output assumption.