Minimum Wage Pass-through to Wholesale and Retail Prices: Evidence from Matched Scanner Data

Carl Hase[1]

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Abstract

A growing empirical literature finds that firms pass the cost of minimum wage hikes onto consumers via higher retail prices. Yet, little is known about minimum wage effects on upstream prices and whether upstream pass-through interacts with downstream pass-through. I exploit the vertically disintegrated market structure of Washington state's legal recreational cannabis industry to investigate minimum wage pass-through to wholesale and retail prices. I utilize scanner data on $6 billion of transactions across the supply chain and leverage geographic variation in firms' minimum wage exposure across six minimum wage hikes between 2018 and 2021. When ignoring wholesale cost effects, I find retail pass-through elasticities consistent with existing literature—yet retail pass-through elasticities more than double once wholesale cost effects are accounted for. Retail markups do not adjust to the wholesale cost shock, indicating a full pass-through of the wholesale cost shock to retail prices. I document vertical heterogeneities and show that pass-through depends on the scale of production and market power in often unpredictable ways. The results highlight the importance of analyzing the entire supply chain when evaluating the product market effects of minimum wage hikes. Policymakers should look beyond horizontal market structures and account for vertical interactions when evaluating the impact of minimum wages on consumers.

Keywords: Minimum wages, inflation, wholesale prices, retail prices, price dynamics, price pass-through.

JEL Classification: E31, J23, J38, L11, L81

[1]Goethe University Frankfurt and JGU Mainz. Email: carlhase@stud.uni-frankfurt.de

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1 Introduction

Minimum wage laws are a popular tool for combating poverty and reducing economic inequality. Despite their pervasiveness, the question of 'who pays' for the minimum wage—i.e. firms, workers, or consumers—remains hotly debated. The answer to this question depends in part on how firms react to the labor cost shock induced by the minimum wage.[1] Firms may reduce employment (i.e. workers pay), adjust profits (i.e. firms pay), or raise prices (i.e. consumers pay).[2] A small but growing literature finds that retail price adjustment plays a key role, i.e. retailers pass the cost shock through to prices such that nominal wage gains from minimum wage hikes are partly offset by price increases (Leung, 2021; Renkin, Montialoux, & Siegenthaler, 2022). What is less understood is the effect of upstream minimum wage hikes on downstream prices.

If upstream labor costs are passed on to retailers via wholesale prices, then retailers will face not one, but two cost shocks from a minimum wage hike. The first is the higher labor cost of the retailer's own minimum wage employees—a direct effect. The second is higher wholesale prices—an indirect effect. To the extent that retailers pass both cost shocks on to consumers, retail price adjustment will reflect both *direct* and *indirect pass-through*. The latter may even eclipse the former since the cost of goods sold (COGS) accounts for over 80% of retailers' variable costs in many retail settings, and retail prices have been shown to be sensitive to even small changes in COGS (Eichenbaum, Jaimovich, & Rebelo, 2011; Nakamura & Zerom, 2010; Renkin et al., 2022). To assess the true impact of minimum wage hikes on real wages, it is therefore crucial to examine both direct and indirect pass-through to retail prices.

In this paper, I investigate the impact of minimum wage increases on wholesale and retail prices in the legal recreational cannabis industry. Approximately half of U.S. states have legal cannabis markets and the industry generates over $25 billion in annual revenue. I focus on Washington state's cannabis market for several reasons. First, cannabis is one of the largest agricultural industries in the state and a major source of low-wage employment. Second, clearly defined vertical relationships between producers and retailers allow me to distinguish between minimum wage pass-through at different points of the supply chain.[3] Third, the cannabis industry operates under statewide autarky, meaning producers and retailers are subject to the very same 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). Finally, rich scanner data provides a close-up of price dynamics for the universe of products at both the wholesale and retail levels. This enables straightforward estimation of direct and indirect pass-through using a reduced-form approach.[4]

To estimate minimum wage pass-through elasticities, I construct establishment-level price

[1]Recent evidence suggests that in some settings, workers' reactions to the minimum wage may also be important (see e.g. Ku (2022)).

[2]Previous studies find small employment and profit effects. Neumark (2019) provides an overview on the literature on the employment effects of minimum wages. For evidence on profit effects, see Draca, Machin, and Reenen (2011); Harasztosi and Lindner (2019).

[3]In other industries, the distinction between upstream and downstream firms is often blurred by vertical integration, making it difficult to distinguish between pass-through at different points of the supply chain.

[4]The data also bypasses reliability issues associated with internal firm prices. For example, Hong and Li (2017) argue that intrafirm prices may be vulnerable to accounting fictions for tax avoidance or record-keeping purposes.

indexes using monthly scanner-level data on \$6 billion of wholesale and retail transactions. My identification strategy exploits geographic variation in minimum wage exposure for 1,192 cannabis producers and retailers over a set of predetermined minimum wage hikes between 2018 and 2021. Using separate producer and retailer panels, I first estimate direct pass-through elasticities for wholesale and retail prices 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, consistent with existing literature (see e.g. Leung (2021)). Importantly, I also find that a 10% increase in the minimum wage corresponds to a 1.7% increase in wholesale prices, which confirms that retailers face a wholesale cost shock in addition to a labor cost shock.

I then investigate the extent to which the wholesale cost shock is passed through to retail prices. I am aided by unusually rich data on prices and quantities for the universe of retailers' wholesale purchases. This allows me to measure the sensitivity of retailers' unit prices to changes in wholesale unit cost by employing a variant of the canonical pass-through regression found in the large pass-through literature (Hollenbeck & Uetake, 2021; Muehlegger & Sweeney, 2022). The resulting estimates imply an indirect cost pass-through elasticity of 0.12. Thus, once wholesale cost shock is accounted for, minimum wage pass-through to retail prices more than doubles from 0.6% to 1.8% (from a 10% hike). This increase reflects a dominance of indirect over direct pass-through, with elasticities that are proportional to retailers' wholesale and labor cost shares. The finding that, at least in the cannabis industry, the majority of retail pass-through stems from changes in retailers' wholesale costs (i.e. indirect pass-through) rather than labor costs (i.e. direct pass-through) underscores the importance of examining the entire supply chain when investigating the effects of minimum wage hikes on retail prices.

Next, I study the role of markup adjustment in pass-through to retail prices. In particular, I show that retailers do not adjust markups to the increase in wholesale prices, indicating a full pass-through of the wholesale cost shock to retail prices.

One concern in the cannabis industry is that rules governing production capacity for producers have created an uneven playing field in which small producers operate on slim margins while large establishments enjoy a higher degree of market power (Washington State Liquor and Cannabis Board, 2021). This suggests that large producers may be able to absorb cost shocks along other margins (e.g. by adjusting profits) and thereby exhibit less price pass-through compared to smaller producers. I test this directly and find that pass-through elasticities monotonically decrease with the scale of production—and are zero for the largest producers—consistent with an increased ability of larger firms to adjust to the cost shock along other margins. This implies that retailers with the same direct minimum wage exposure can have vastly different indirect exposure, and hence different total pass through. This has implications that extend well beyond the cannabis industry and highlights the importance of upstream heterogeneity in understanding the impact of minimum wages on downstream prices.

Finally, I examine other potential margins of adjustment to minimum wage hikes. I find no evidence of employment effects for retailers or producers, and I document no effect on cannabis consumption. The latter finding precludes demand-induced feedback as a mechanism through which minimum wage hikes affect retail cannabis prices.[5]

[5]There is some debate about the potential for demand-induced feedback from minimum wage hikes to retail

Taken together, my results highlight the importance of examining the entire supply chain—beyond the final point of sale—when investigating the price level effects of minimum wage hikes. Since minimum wage pass-through to retail prices attenuates the increase in real wages desired by policymakers, it is important to consider both direct and indirect pass-through when evaluating the efficacy of minimum wages as a policy tool.

I make three main contributions in this paper. First, I provide evidence that minimum wages affect retail *and* wholesale prices. While previous studies have estimated minimum wage pass-through to retail prices, to the best of my knowledge, pass-through to wholesale prices has not been causally identified. Importantly, my results imply that retailers face a direct and an indirect cost shock from minimum wage hikes. Since wholesale costs typically dominate labor costs in retailers' variable cost structure, measuring the indirect cost shock is crucial to understanding the total effect of minimum wages on retailers' costs.

Second, I explicitly quantify direct and indirect pass-through effects. This allows me to compare their magnitudes and relate them to retailers' wholesale and labor cost shares. Since data on wholesale costs are rarely available the literature does not distinguish between these two forms of pass-through, meaning it is generally not possible to determine whether previous estimates capture combined (i.e. direct and indirect) effects or direct pass-through only. To see this, note that it is common to estimate pass-through by relating store-level prices to changes in state-level minimum wages. Renkin et al. (2022) show that such a model delivers different pass-through measures depending on implicit assumptions regarding the nature of retailers' wholesale purchases. If retailers predominantly purchase from local producers (e.g. as is the case with grocery stores), then reduced-form regressions from statewide hikes will capture both direct and indirect pass-through to retail prices. However, in industries with highly tradeable goods (e.g. drugstores and general merchandise stores), the wholesale cost shock is common across all states and indirect pass-through is absorbed by time fixed effects. In that case, reduced form estimates only capture direct pass-through and fail to reflect the full impact of minimum wages on retail prices. I overcome this issue by exploiting variation in the minimum wage bite at the industry-by-county level. Since cannabis retailers and producers belong to different three-digit industries, estimates of direct pass-through to retail prices are independent of wholesale cost effects. Moreover, full information on retailers' wholesale transactions allows me to quantify retailers' pass-through of wholesale cost shocks, which in turn enables me to estimate indirect pass-through.

Third, the literature on minimum wage pass-through focuses on restaurants, grocery, drug, and merchandise stores. By investigating pass-through to cannabis prices, I provide novel insight on firms' margins of adjustment in a large agricultural market of growing importance.

This paper primarily relates to three strands of literature. The first is the small but growing literature on the product market effects of minimum wages, which until recently has centered on the restaurant industry (see e.g. Aaronson (2001); Allegretto and Reich (2018); Fougere, Gautier, and Bihan (2010)). Most closely related are two papers by Renkin et al. (2022) and Leung (2021), who use high frequency scanner data to study the impact of a large number of state-level minimum wage hikes on consumer prices in the U.S. Both studies employ difference-in-

prices. Using similar data and time periods, Leung (2021) finds evidence in favor of such an effect while Renkin et al. (2022) do not.

differences with continuous treatment and find full and more than full pass-through to grocery prices, respectively, but no effect on prices at merchandise stores. I deviate from these studies by adopting an identification strategy that exploits geographic variation in the minimum wage bite at the industry level. Since cannabis retailers and producers belong to different industries, this allows me to separately identify direct pass-through to retail and wholesale prices. My paper is a natural extension of Renkin et al. (2022), as they consider the possibility that minimum wage hikes induce a wholesale cost shock, but they cannot test for it because their data does not include information on wholesale cost. Instead, they calculate an upper bound for indirect pass-through using input-output tables under the assumption of full pass-through to wholesale prices. In contrast, I directly observe wholesale cost because my data contains prices and quantities for the universe of retailers' wholesale transactions. I estimate wholesale cost pass-through using a linear panel regression at the store-product-month level. This allows me to obtain the implied indirect pass-through elasticity from minimum wage hikes.

By leveraging full information on retailers' wholesale costs, I also deviate from Renkin et al. (2022) in how I quantify the degree of cost pass-through. Renkin et al. (2022) divide the minimum wage pass-through elasticity by an estimate of the minimum wage elasticity of marginal cost, where the latter requires estimating the minimum wage elasticity of average wages. In contrast, since I observe the wholesale prices paid by each retailer, I can directly estimate the marginal cost pass-through rate from a linear panel regression at the store-product-month level Hollenbeck and Uetake (2021). I find that stores more than fully pass-through changes in marginal cost. Such overshifting is consistent with the findings of (Hollenbeck & Uetake, 2021), and indicates that cannabis retailers have substantial market power and demand is highly log convex.

Second, the paper contributes to the literature on the transmission of cost shocks to firm pricing, much of which concerns exchange rate pass-through in specific industries (see Burstein and Gopinath (2014) for an overview). These papers typically combine separate wholesale and retail data sets and use structural models to infer pass-through of wholesale cost shocks to retail prices (see e.g. (Bonnet, Dubois, Boas, & Klapper, 2013; Nakamura & Zerom, 2010).[6] In contrast, my data uniquely identify both parties to each wholesale transaction and allow me to trace each product as it moves across the supply chain. As a result, I can estimate indirect pass-through directly from the data using a reduced form approach. More generally, I add to the literature on the transmission of upstream cost shocks by extending it to the minimum wage context.

Third, the paper contributes to the small but growing literature that uses the cannabis industry to investigate topics in industrial organization. Most closely related are two papers that study the role of the market structure on cannabis firm pricing. Hollenbeck and Uetake (2021) consider the impact of cannabis license restrictions on retail market power while Hansen, Miller, and Weber (2022) examine how a change in Washington's cannabis tax affected vertical integration among cannabis producers. I build on this literature by investigating the effects of minimum wages on cannabis pricing. In addition, I use scanner data from a newer admin-

[6] An exception is Eichenbaum et al. (2011) who use data on prices and costs from a single U.S. retailer and find that retail price changes largely reflect changes in wholesale cost. Hong and Li (2017) uses similar data to investigate the role of market structure on retail pass-through.

istrative data software system that was introduced in early 2018. The newer data identifies products at the level of the stock keeping unit (SKU), which allows me to construct price indexes at a more granular level than was previously possible.

One issue with this type of analysis is the degree to which results from one industry can be used to infer pricing dynamics in other industries. I show that retail cannabis is remarkably similar to other industries studied in the literature. The variable cost structure for cannabis retailers is typical of grocery stores and other conventional retail settings, making the relative magnitudes of direct and indirect retail pass-through elasticities broadly applicable. Moreover, the price elasticity of demand—a key determinant of cost pass-through—has been shown to be similar for cannabis as for other industries studied in the literature (Hollenbeck & Uetake, 2021). Nevertheless, it is important to note that cannabis production is potentially more labor intensive than other agricultural industries due to the prevalence of small-scale indoor cultivation. Accordingly, the labor share of variable cost for cannabis producers is expected to be higher, and hence, the upstream labor cost shock induced by the minimum wage may be larger compared to other agricultural industries. Indeed, I chose to analyze the cannabis industry partly due to the labor intensive nature of cannabis cultivation, since identifying an upstream cost shock is a prerequisite for tracing pass-through of wholesale costs to retail prices.

This paper proceeds as follows. Section 2 describes the institutional context for the study. Section 3 details the data and the main empirical strategy. Section 4 presents direct pass-through estimates to wholesale and retail prices and discusses robustness checks. Section 5 investigates indirect pass-through to retail prices and compares them to the direct pass-through estimates from section 4. Section 6 shows how differences in tradability and market power have implications for the transmission of cost shocks to prices. Section 7 investigates other possible margins of adjustment to minimum wage hikes including employment, productivity, and demand effects. Section 8 concludes.

2 Institutional context

2.1 The cannabis industry in Washington state

In November 2012, voters in Washington state approved the creation of a legal recreational cannabis market for adults 21 years and older.[7] Cannabis has since become a major agricultural industry in the state. In 2020, retail sales topped $1.4 billion and the industry contributed $1.85 billion to gross state product, making it one of the largest agricultural crops in the state (Nadreau, Fortenbery, & Mick, 2020).

The cannabis industry is regulated by the Washington State Liquor and Cannabis Board (LCB) which offers separate business licenses for upstream and downstream establishments. Producer-processors (i.e. upstream establishments) can cultivate, harvest, and process cannabis but cannot sell to end consumers. Retailers (i.e. downstream establishments) can purchase fully packaged and labelled products from producer-processors and sell them to end consumers in retail stores (see Appendix Figure A1). Producer-processors cannot own retail licenses and vice

[7] Cannabis production and consumption remains prohibited at the federal level. However, in August 2013, the United States Department of Justice announced that it would not interfere with state-level legalization as long as distribution and sales were strictly regulated by states. This effectively green-lit legalization for U.S. states.

versa, creating complete vertical separation along the supply chain. Licenses are capped by the LCB at 556 retailers and 1,426 producer-processors, though not all licenses are actively in business, especially at the producer-processor level. Licenses are granted at the establishment level so that a firm can own several licenses. However, the vast majority are single-establishment firms. Retailers are located in 37 counties while producers are located in 35 counties in Washington state (the state has 39 counties in total). Retail sales are subject to a 37% sales tax but there is no tax on upstream sales.[8]

The cannabis industry operates under a statewide autarky. That is, retailers can only buy from producer-processors located in Washington state, and producer-processors can only sell to retailers in the state. This seals off the core of the supply chain from other states with legal recreational markets.[9] As a result, producer-processors and retailers are subject to the same minimum wage hikes, and hence, the same set of labor cost shocks.

Before moving on, it is worth noting several points. First, I use the term 'producer', 'producer-processor' and 'wholesaler' interchangeably throughout the paper when referring to upstream establishments. This reflects the dual role played by these establishments since, besides being producers, they also act as wholesalers when viewed from the perspective of retailers. Second, since producers occupy the upstream portion of the supply chain, I assume that the minimum wage only induces a labor cost shock for producer-processors, i.e. the minimum wage does not affect material input prices for these firms. This reflects that producer-processor 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 producers' input prices is likely small. Therefore, for wholesale prices I only estimate direct pass-through, whereas for retail prices I estimate both direct and indirect pass-through.

Cannabis labor

Cannabis is an important source of employment in Washington and the sector supports approximately 18,700 full-time equivalent (FTE) jobs (Nadreau et al., 2020). This mirrors the growing importance of cannabis employment in the U.S. more generally, where, according to one industry report, cannabis employs more than 428,000 workers (Barcott, With, Levenson, & Kudialis, 2022). 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 & Stever, 2010). Most harvesting, drying, trimming, and packaging is done by hand, as this allows growers to produce higher quality buds that sell at a higher price point (Jiang & 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 producer-processors 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'. Budtend-

[8] For comparison, liquor sales in Washington are subject to a 20.5% sales tax plus a unit tax of $3.7708 per liter. This amounts to a 61.8% tax on a 1.75 liter bottle with a listed price of $15.99 Hollenbeck and Uetake (2021).

[9] The supply chain is not 100 percent sealed off, since consumers from other states can travel to Washington to purchase cannabis at retail stores, and producer-processors can purchase certain inputs such as grow lights, soil, and fertilizers from businesses in other states.

ing 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.

2.2 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.[10] 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 F.5 I consider the alternative schedule for employees earning less than $2.19 in benefits. 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 F.5). For both Seattle and Tacoma, the citywide hikes occurred on the same day of the year as the statewide hikes (January 1st).

3 Data

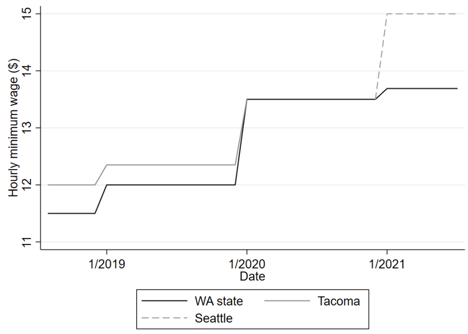
3.1 Price data

To monitor developments in the cannabis market, legalization came with stringent data reporting and sharing requirements for all licensed cannabis businesses. Establishments must record all sales and regularly upload data feeds to the LCB. The data, which is usually reported weekly, contains detailed information on the price and quantity of each product sold by a producer-processor to a retailer, and the subsequent price and quantity of that very same product sold at the retail level.[11] The LCB switched providers for its traceability system in October 2017

[10]Firms with over 501 employees are subject to a higher minimum wage than small employers. Since no cannabis business in Seattle has more than 500 employees, the large employer minimum wage does not apply.

[11]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

Fig. 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 F2).

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. Table 1 reports descriptive statistics for my estimation sample. The sample covers sales from 1,192 distinct retail and producer-processor establishments and contains an industry-wide average of 31,800 unique retail products and 18,268 unique wholesale products per month. To give an example of a cannabis product, a 1.0 gram package and a 2.0 gram package of Sunset Sherbert usable marijuana (dried flower) produced by Northwest Harvesting Co are treated as different products in the data.[12] The LCB classifies products into 12 categories. As Table 1 illustrates, usable marijuana and concentrate for inhalation account for more than 80% of all retail sales. Another 14% of retail sales comes from solid edibles (chocolate bars, cookies, 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. Over the entire sample period, the data contain \$4.47 billion and \$1.46 billion in retail and wholesale sales, respectively. Note that all retail cannabis sales are subject to a 37% excise tax, and retail prices and revenues reported in this paper are tax-inclusive. There is no tax on wholesale transactions.

To estimate pass-through elasticities I follow previous studies (e.g. Leung (2021); Renkin et al. (2022)) and define the dependent variable as the natural logarithm of the monthly establishment-

the LCB board. Given such strict enforcement, violations are uncommon. In 2021 for example, the LCB issued 66 violations among approximately 1,192 licensees. See: https://lcb.wa.gov/enforcement/violations-and-due-process

[12]Similar to how wines can be distinguished by the grape (e.g. Riesling, Chardonnay, etc), cannabis comes in many strains, which is 'Sunset Sherbert' in the given example.

Table 1: Descriptive statistics for cannabis establishments

(a) Sample totals

	Retail	Wholesale
Establishments	500	692
Units sold	232,133,427	228,423,415^{+}
Distinct products	172,688	147,273
Total revenue	$4.47 billion	$1.46 billion

(b) Establishment monthly averages

	Retail	Wholesale
Distinct products	471	55*
Revenue	$304,032	$106,634
Units sold	15,844	16,735*

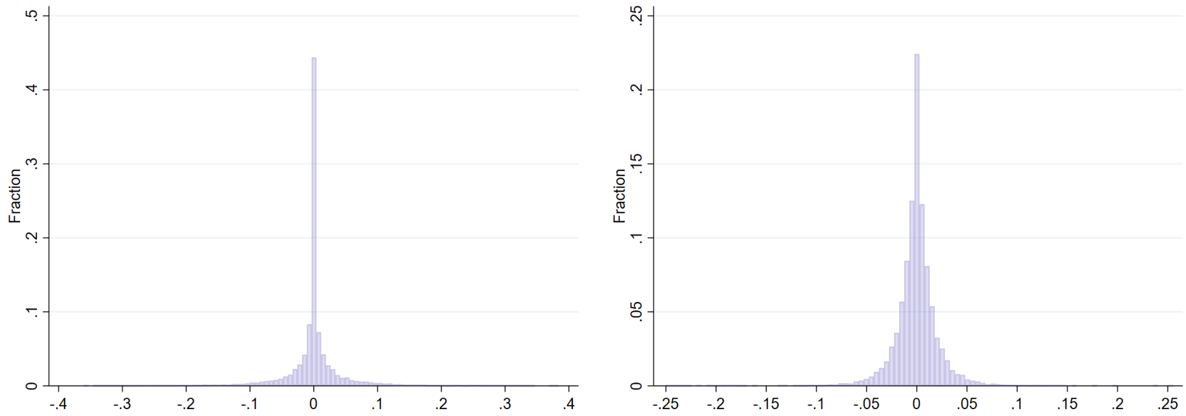
(c) Market share by product category

	Retail	Wholesale
Usable marijuana	0.53	0.61
Concentrate for inhalation	0.31	0.28
Solid edible	0.07	0.03
Liquid edible	0.03	0.02
Infused mix	0.04	0.04
Other	0.02	0.02

Notes: This table displays summary statistics for the estimation sample. The sample period is August 2018 through July 2021. Panel (a) reports totals across all establishments and months in the sample. Panel (b) reports monthly averages at the establishment level. Sales from processor-only establishments are excluded. Sales between producer-processor establishments are included. Retail revenue is based on tax-inclusive prices. Panel (c) shows market shares for the product categories defined by the LCB. "Other" includes any category with less than 1 percent market share. These are: topical, packaged marijuana mix, capsules, tinctures, transdermal patches, sample jar, and suppository. Sales from processor-only establishments are excluded. Sales between producer-processor establishments are included. Data source: Top Shelf Data.Data source: Top Shelf Data.

$^{+}$ For producers, 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.

Fig. 2. 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 producerrs (Figure a) and retailers (Figure b) in the estimation sample. Data: Top Shelf Data, August 2018-July 2021.

level price index:

$$\pi_{j,t} = \ln I_{j,t}, \text{ with } I_{j,t} = \prod_c I_{c,j,t}^{\omega_{c,j,y(t)}} \tag{1}$$

$\pi_{j,t}$ is the inflation rate for establishment j in month t; $I_{j,t}$ is an establishment-level Lowe price index that aggregates price changes across product subcategories c; the weight $\omega_{c,j,y(t)}$ is the revenue share of subcategory c in establishment j during the calendar year of month t.[13] To limit the potential impact of outliers, I trim inflation rates above the 99.5th and below the 0.5th percentile of the monthly distribution in my main specification (results are robust to keeping outliers).

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

3.2 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

[13] 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.

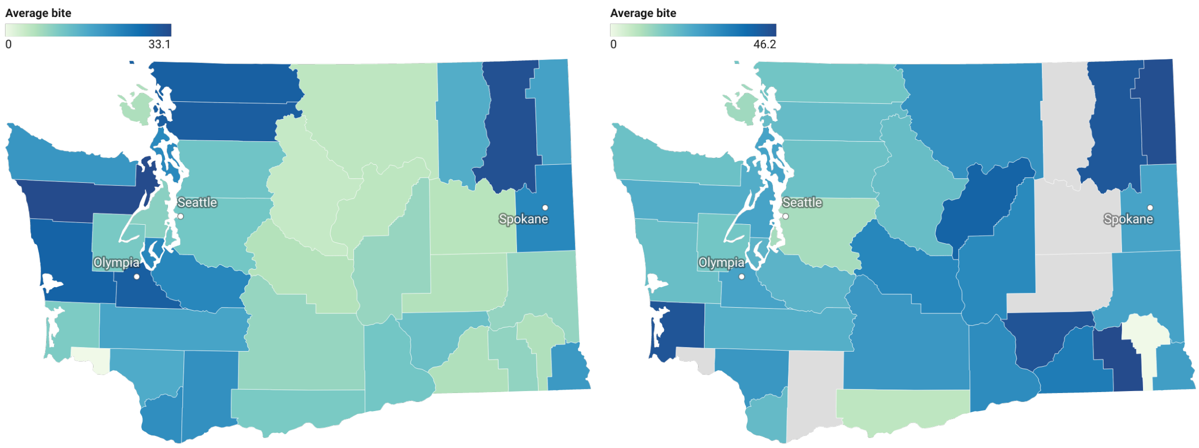
are not observable at the establishment level, I follow previous studies and use geographic variation in the minimum wage bite as a proxy (see e.g. Bossler and Schank (2022); Card (1992); Dustmann, Lindner, Schönberg, Umkehrer, and Berge (2022); Leung (2021); Renkin et al. (2022)). I define bite as the 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 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 producers, 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 producer-processors belong to different NAICS industries, a retailer and producer-processor located in the same county can each have a different bite. Figure 4 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. First, in Appendix A.2 I show that average wages for cannabis retailers and producers are very similar to those in the corresponding NAICS industries. Moreover, for both the cannabis industry and the NAICS industries, average wages are remarkably close to the wage floor imposed by the minimum wage. For producers, 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 (see Appendix A.2). 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 F, 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.

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).[14] A similar dataset has been used in the recent minimum wage literature (see e.g. Dube, Lester, and Reich (2016); Leung (2021); Renkin et al. (2022)).

[14]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.

Fig. 3. 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 producers. 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.

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 producer-processors have a similar unit price, monthly revenue, and product variety prior to minimum wage hikes. An exception is that low-bite producer-processors sell a higher quantity than high-bite producer-processors; however, this is due to the fact that for some producer-processors, 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.

4 Direct pass-through

I am interested in the effect of minimum wage hikes on wholesale and retail cannabis prices. In this section, I abstract from indirect pass-through and focus on the pass-through of the labor cost shock only (i.e. direct pass-through). I estimate direct pass-through separately for

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.

producer-processors (i.e. wholesalers) and retailers.

I outline my main empirical strategy in section 4.1. Section 4.2 reports my main results. In section 4.3, I show that my results stand up to an extensive set of robustness checks. In section 4.4, I investigate the role of market structure in minimum wage pass-through, and test for heterogeneous pass-through effects by establishment characteristics.

4.1 Empirical strategy

To estimate direct minimum wage pass-through elasticities, 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 (Bossler & Schank, 2022; Card, 1992), the link between student loan credit expansion and college tuition (Lucca, Nadauld, & Shen, 2019), and the effect of abortion clinic closures on abortion rates (Lindo, Myers, Schlosser, & Cunningham, 2020).

I assign establishments a treatment intensity that is a function of their industry-by-county minimum wage bite. DiD with continuous treatment estimates a summary parameter of all possible 2×2 comparisons of changes in outcomes for higher bite establishments relative to lower bite establishments, with each comparison weighted by the difference in bite and weights that integrate to one (Callaway, Goodman-Bacon, & Sant'Anna, 2024). 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).[15] 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 4.3.

There are three minimum wage events during my sample period, each spaced 12 months apart. Establishments can be treated up to three times, but 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. Leung, 2021; Renkin et al., 2022). Since the establishment-level price index and the minimum wage bite are first-differenced by construction, I specify the model in first differences. I estimate the following equation separately for retailers and producer-processors:

$$\pi_{j,t} = \sum_{l=-5}^{6} \beta_l \Delta \log MW_{j,t-l} \times Bite_{k(j),t-l} + \gamma_t + \epsilon_{j,t}. \tag{2}$$

Equation 2 relates the monthly inflation rate at establishment j, $\pi_{j,t}$, to the treatment intensity in industry-county k, which is defined as the interaction between the percent change in the min-

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

imum wage applicable to establishment j, $\Delta \log MW_{j,t-l}$, and the minimum wage bite in the industry-county k that establishment j belongs to, $Bite_{k(j),t-l}$. Note that $\Delta \log MW_{j,t-l}$ 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 $Bite_{k(j),t-l}$. I show in appendix F that results are similar when $Bite_{k(j),t-l}$ is not scaled by $\Delta MW_{j,t-l}$. 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, Duflo, and Mullainathan (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. Leung (2021); Renkin et al. (2022)). 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 $P_L = -\sum_{l=m}^{-L-1} \beta_{-l}$.[16]

An important consideration is the number of leads and lags to include in equation 2. 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 F that treatment effects remain stable over a longer event window.

When estimated for retailers, equation 2 uniquely identifies direct pass-through to retail prices and avoids picking up indirect pass-through effects. To see this, note that 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 producer-processors located in the retailer's own county. Second, the bite variable for retailers would need to correlate with bite for producer-processors within each county.[17] In Appendix A, I show that the neither of these conditions hold: over 85% of retailers' wholesale purchases are from producers located in other counties, and the (conditional) within-county correlation between producer and retail bite is -0.03.

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 actu-

[16]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 & Siegloch, 2023).

[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 producer-processor bite and retail 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 producers.

ally takes effect.[18] 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 F.9, 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.

4.2 Main results

Wholesale prices

I begin by estimating the effect of minimum wage hikes on wholesale prices. Figure 4a shows the estimated wholesale price level effects. One question regarding the wholesale estimates is whether to control for a treatment-specific pre-trend since the baseline specification reveals a slight downward trend in the pre-treatment period. Though the trend is interrupted by a large and highly statistically significant treatment effect in the period that the minimum wage hike occurs, 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.

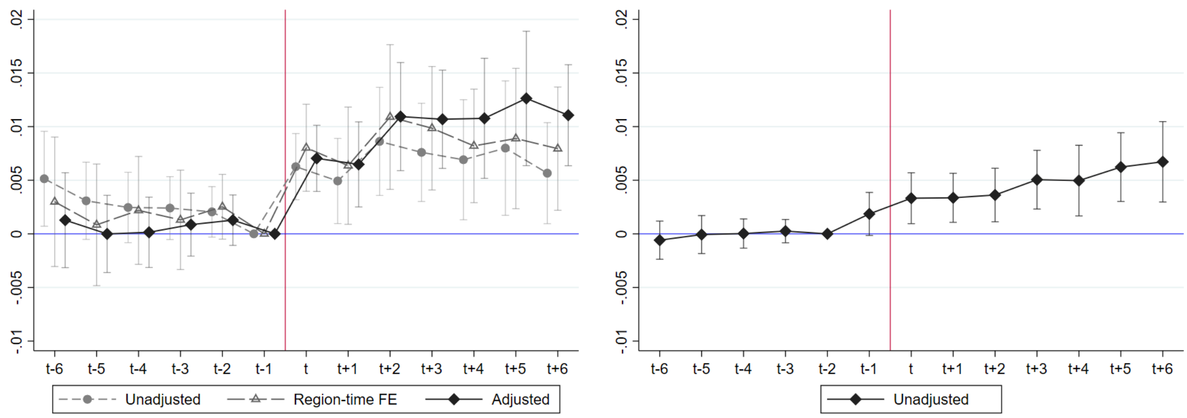
I apply two common strategies to control for the pre-trend, both of which yield similar results. First, I include region-time FE (i.e. interactions between time and region dummy variables) to account for regional economic trends that may covary with bite and inflation. The regions are based on the three major socioeconomic regions in Washington state, where each region includes a subset of counties (see Appendix G.3). To the extent that unobserved time-variant heterogeneity is common within regions, region-time FE will control for the treatment-specific trend (Neumark, Salas, & Wascher, 2014). Second, I apply the two-step procedure from Goodman-Bacon (2021) and re-estimate equation 2 using a trend-adjusted dependent variable. Specifically, I calculate the average of the distributed lag estimates (from equation 2) 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 & 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).

Figure 4a illustrates that the period t treatment effects are large and statistically significant

[18]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.

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.

Fig. 4. Direct minimum wage pass-through to prices



(a) Wholesale prices **(b)** Retail prices

Notes: the figure shows estimates from equation **??** under three different specifications: unadjusted, trend-adjusted, and region-time FE. The dependent variable is the establishment-level inflation rate for cannabis producers. The figure 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 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.

Retail prices

Figure 4b illustrates that the effects for retailers differ from those of producers 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 producers, suggesting that retailers may be more forward-looking in their pricing than producers. 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. Thus, the treatment effect in period t—while still large—is about half the size of that for producers. I analyze the relative magnitudes of producer and retail pass-through in more

[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.

detail in section 5.

4.3 Threats to identification, robustness checks and alternative specifications

In this section, I address potential endogeneity concerns in my setting. I present various alternative specifications and robustness checks to corroborate the validity of my results.

Endogenous treatment and parallel trends

My empirical strategy 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 2. Figure 4a 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 4.2). Figure 4b shows a flat pre-trend for the retail regression. Thus, the results from Figures 4b and 4b 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, such as establishment size and average revenues, through the inclusion of 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 F1, 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.

[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 **??** and show that the main results are robust to dropping Seattle establishments for event 3.

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 F. 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 F1).

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 F2 and F3 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 F2 and F3 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 (Tables F2 and F3).

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 account for such non-compliance, I redefine the bite variable as the difference between bite two quarters before and one quarter after the hike. This effectively nets out non-compliance at the county level. In Appendix F.4 I show that results are robust to this alternative bite variable.

In Appendix F.5, 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 F.6).

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 producer-processors. 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 wholesale and retail results line up temporally. Changing the baseline period has no bearing on the dis-

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

tributed 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 decrease slightly when establishment indicators are included to capture establishment-level price trends (since equation 2 is in first differences). Effect sizes are similar when the dependent variable is winsorized (Column 3) or includes outliers (Column 4).

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 5 shows that results are unchanged, indicating that the main results are not driven by treatment intensity outliers.

Columns 6-8 show results when the dependent variable is not adjusted for a linear pre-trend. In Column 6, region-time FE are used to control for the pre-trend. Column 7 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 8 shows an upward shift in effect sizes when the baseline period is set to one month before effects appear rather than two months before.

Table 4 shows that retail price effects are similarly stable across specifications. Columns 2 and 3 show similar effect sizes with store FE and region-time FE, respectively. Retail price effects are also not affected by winsorizing (Column 4), including outliers (Column 5), or trimming the bite variable (Column 6).

4.4 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

I begin with producer-processors. I exploit LCB rules that separate producer-processors into tiers governing their production capacity.[23] I define small producer-processors as those permitted to grow up to 10,000 square feet of plant canopy, and large producer-processors as those that can grown up to 30,000 square feet of canopy. I split my sample into two subgroups comprising small and large producer-processors, and estimate my main specification (equation 2) separately for each of these subgroups.

[23] Producer-processors 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 producer-processors 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.

Table 3: Direct effect on wholesale prices

	Trend-adjusted					Unadjusted		
	(1) Baseline	(2) Estab. trends	(3) Wins-orized	(4) Outliers	(5) Trim-med bite	(6) Reg.-time FE	(7) Baseline	(8) t-1 base
E_0	0.006***	0.004*	0.006***	0.005**	0.006***	0.006**	0.004**	0.006***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
E_2	0.010***	0.006*	0.010***	0.010***	0.010***	0.008*	0.007**	0.009***
	(0.003)	(0.003)	(0.003)	(0.004)	(0.003)	(0.004)	(0.003)	(0.003)
E_4	0.009***	0.004	0.010***	0.010***	0.010***	0.006	0.005	0.007**
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.003)
$\sum$ Pre-event	1.0e-07	0.005	1.0e-07	2.0e-07	2.0e-07	5.0e-04	0.003	0.005*
	(0.003)	(0.004)	(0.004)	(0.004)	(0.003)	(0.003)	(0.003)	(0.003)
N	14,777	14,777	14,932	14,932	14,735	14,777	14,777	14,777

Notes: Dependent variable: establishment-level inflation rate (1% trim) adjusted for a bite-specific trend as detailed in section 4.1. Listed coefficients are sums of the distributed lag coefficients E_L, L months after minimum wage hikes, relative to the normalized baseline period in $t-2$. (2) controls for establishment price trends. (3) uses a winsorized dependent variable (99% windsorization). (4) does not trim or winsorize the dependent variable. In (5) the bite variable is trimmed by 1%. For columns (6)-(8) the dependent variable is not trend-adjusted. (6) includes region-time FE, (7) does not. In (8) the normalized baseline period is in t-1 instead of 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: Direct effect on retail prices

	(1) Baseline	(2) Estab. trends	(3) Reg.-time FE	(4) Winsor-ized	(5) Outliers	(6) Trim-med bite
E_0	0.005**	0.005**	0.004**	0.003**	0.003**	0.005**
	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)	(0.002)
E_2	0.005***	0.006**	0.005**	0.003**	0.003**	0.005***
	(0.002)	(0.003)	(0.002)	(0.001)	(0.002)	(0.002)
E_4	0.005*	0.005	0.005	0.004**	0.003	0.004
	(0.003)	(0.004)	(0.003)	(0.002)	(0.002)	(0.003)
$\sum$ Pre-event	0.001	0.001	0.001	-0.0002	0.0008	0.001
	(0.002)	(0.002)	(0.001)	(0.0009)	(0.001)	(0.002)
N	14,189	14,189	14,189	14,044	14,189	14,095

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.

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 producer-processors. 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 producer-processors (column 2). Columns 3 and 4 confirm that these results carry over when the dependent variable is not adjusted for the bite-specific trend. Taken together, the results from columns 1-4 suggest that large producer-processors are able to adjust to the labor cost shock along other margins (e.g. employment or profits) compared to small producer-processors. I revisit this result in Section 7 and show that it is consistent with economies of scale and the fact that producer-processors manufacture a tradable (within state) good.[24]

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 producer-processors may be exposed to a large indirect cost shock, while retailers that purchase from large producer-processors may have little (or no) indirect cost shock.

At the retail level, I use chain size as a proxy for the scale of production. I split my sample into two subgroups comprising chains versus independent stores. I define chains as stores belonging to firms with three or more establishments, and consider one- and two-store firms as independent.[25] Of the 500 retail stores in my sample, 325 are independent and 175 belong to a chain. I estimate my main specification separately for each of these subgroups.

Columns 5-6 show that direct pass-through at chains is twice as large as at independent stores. This indicates that chain stores are better able to pass-through the cost shock to consumers compared to independent stores. I return to this result in section 7, and show that it is consistent with increasing returns to scale at cannabis stores (Hollenbeck & Giroldo, 2022) and local monopoly power.

Market concentration

Next, I investigate whether direct pass-through varies by market concentration. Since producer-processors sell to retailers across the state, there is no obvious criteria for defining market concentration upstream. In contrast, the local nature of cannabis retail makes geographic concentration a natural criteria 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 choose the 5-mile radius because the average cannabis consumer in Washington is located approximately five miles from the nearest retailer (Ambrose, Cowan, & Rosenman, 2021). I calculate the Herfindahl–Hirschman Index (HHI) for each market in the sample, and divide the sample into two subgroups for stores

[24] As shown in Appendix Table A3, producer-processors sell to retailers statewide. See section 7 for more details.

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

[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.

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 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. I discuss this result in more detail in section 7.

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 rates of a market-wide cost shock increase with the number of firms (Muehlegger & Sweeney, 2022). I investigate strategic complementarity in cannabis prices in Section 6.1.

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 **??**. 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.

[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 K. 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.

5 Indirect pass-through

In the previous section, I estimated the direct pass-through of minimum wage hikes to wholesale and retail cannabis prices. The results show that minimum wage hikes affect both wholesale and retail prices via the labor (i.e. direct) cost shock. The wholesale price effects imply that retailers face an indirect cost shock in addition to the direct cost shock. In this section, I derive indirect pass-through empirically and use it to quantify the total (i.e. direct and indirect) effect of minimum wage hikes on retail cannabis prices.

I describe my empirical approach in Section 5.1. In Section 5.2 I examine how retailers adjust their prices in response to changes in unit cost, employing methods from the extensive literature on cost pass-through (e.g. Conlon & Rao, 2020; Hollenbeck & Uetake, 2021; N. H. Miller, Osborne, & Sheu, 2017a; Muehlegger & Sweeney, 2022). My objective in estimating the unit cost pass-through rate is twofold. First, the unit cost pass-through rate is directly informative about the degree of cost pass-through. Second, the unit cost pass-through elasticity can be combined with the wholesale price estimates from Section 4 to infer indirect pass-through to retail prices. I derive the implied indirect pass-through elasticity in section 5.3.

5.1 Empirical framework

Formally, the indirect pass-through elasticity can be expressed as:

$$\frac{\partial P^r}{\partial MW}\frac{MW}{P^r} = \frac{\partial P^r}{\partial P^w}\frac{P^w}{P^r} \cdot \frac{\partial P^w}{\partial MW}\frac{MW}{P^w} \tag{3}$$

P^r denotes the retail price level (in dollars) at a store, P^w is the wholesale price level that the store pays, and 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 the latter in section 4.2. I estimate the former in the following subsection.

5.2 Unit cost pass-through

To estimate the wholesale cost pass-through elasticity, 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 wholesale prices are passed through to retail prices.[28]

In my main specification, I estimate the wholesale unit cost pass-through elasticity, i.e. the percent increase in retail unit price stemming from a one percent increase in wholesale unit price. I specify a model at the store-product-month level that relates a store-product's average monthly retail price to (i) that store-product's average monthly wholesale price and (ii) the

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

average monthly wholesale price paid by competing stores within a certain distance from the respective store. I include competitors' cost changes to capture potential strategic complementarity in prices (i.e. the effect of competitors' price changes stemming from their cost changes). I estimate the following model in first-differences:

$$\Delta p_{i,j,t} = \phi \Delta w_{i,j,t} + \sum_{r=1}^{R} \beta_r \Delta w_{i,r(j),t} + \gamma_t + \Delta \varepsilon_{i,j,t}, \tag{4}$$

where $p_{i,j,t}$ is the log average price of product i sold at store j in month t, $w_{i,j,t}$ is the log average wholesale price that retailer j pays for product i in month t, $w_{i,r(j),t}$ is the log average wholesale price that competitors pay for product i in month t, and γ_t is the year-month FE. Competitors are sorted into 5-mile bins $r(j)$ according to their geographic distance from store j. In the baseline specification, I set $R = 3$ (setting $R \geq 3$ does not affect estimates but changes the sample size and standard errors). Since the model is in first differences, product and retailer FE are swept out. As a robustness check I also estimate equation 4 in levels with store-product FE. The pass-through elasticity, ϕ, is the percent increase in retail prices at store j from a one percent increase in store j's wholesale prices. β_r measures the pass-through elasticity of wholesale unit costs at competing stores located in bin r to unit prices at store j. 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 pass-through literature from other industries (see e.g. Conlon & Rao, 2020; Hollenbeck & Uetake, 2021; N. H. Miller et al., 2017a; Muehlegger & Sweeney, 2022).

In a second specification, I estimate the unit cost pass-through *rate*, that is, the increase in retail unit prices (in dollars) stemming from a $1 increase in wholesale unit prices. Since there is no factor substitution at cannabis retail stores, a $1 increase in wholesale unit price amounts to a $1 increase in marginal cost, and the unit pass-through rate is equivalent to the marginal cost pass-through rate. The marginal cost pass-through rate is informative about the degree of cost pass-through, which I discuss further in section **??**.

It is worth noting that equation 4 is at the store-product-month level of aggregation. There are two reasons for using the disaggregated level in the baseline specification. First, the target parameter is the pass-through of *unit* (i.e. per product) cost to *unit* price. The disaggregation allows me to estimate this parameter by directly relating these two values. In contrast, a regression based on store-level indexes first aggregates products within their respective subcategory and then aggregates across subcategories within a store. While this is preferable when estimating treatment effects at the store level, the two-step aggregation necessarily breaks the direct vertical link between unit cost and unit price. Second, and more importantly, this link is further cleaved by the fact that competitors' cost indexes contain cost changes for products and subcategories that store j may not actually sell. In the extreme case, one might relate costs and prices for adjacent stores that sell non-overlapping baskets of goods and hence do not compete in prices at the product level. As a result, the estimated coefficients for competitors' cost changes—and by extension the own-cost pass-through elasticity—may be biased when using store-level indexes. Nevertheless, I estimate the pass-through regression using store-level price and cost indexes as a robustness check. In an additional test, I specify an index-based pass-through regression using only products belonging to the usable marijuana category and with prices and costs converted to price per gram. This is expected to increase the overlap with competitors' indexes since usable marijuana is a comparatively homogeneous product category with a higher degree of substitutability across stores.

Table 6 reports the results from the unit cost pass-through regressions. When estimating the unit cost pass-through elasticity (Column 1), I find that a 1% increase in wholesale unit cost leads to a 0.71% increase in retail unit price. This estimate is in line with findings from other industries (see e.g. Amiti, Itskhoki, & Konings, 2019; Ganapati et al., 2020).

Columns 3 and 4 report estimates for the pass-through rate in dollars. I find that a $1 increase in 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 (see Section 7). 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 & van Benthem, 2019).

Table 6: Unit cost pass-through

	(1) FD logs	(2) Logs	(3) (4) Dollars FD	Dollars
Own wholesale cost	0.712***	0.745***	1.023***	
	(0.008)	(0.008)	(0.159)	
Competitors' wholesale cost (0-30 miles)	0.003	0.010**	0.029	
	(0.002)	(0.004)	(0.045)	
N	3,580,835	5,695,425	11,840	

Notes: The table reports the pass-through rates of wholesale unit cost to retail unit price, at the store-product-month level. All specifications include month-year FE. Dependent variable is: the first-difference of price (column 1); the first-difference of the log price (column 2); price (column 3); log store-level monthly price index (columns 4), log store-level monthly price-per-gram index for Usable Marijuana products (column 5). SE are clustered at the store level and are shown in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Data: Top Shelf Data (March 2018 through December 2021).

5.3 Quantifying indirect pass-through to retail prices

The combined estimates of the unit cost pass-through elasticity (estimated above) and the minimum wage elasticity of wholesale prices (from section 4.2) allow me to compute indirect pass-through to retail prices using equation 3. Table **??** shows the results. Columns 1-3 correspond to different specifications for the minimum wage elasticity of wholesale prices from section 4.2. The baseline point estimate for the indirect pass-through elasticity is $0.71 \times 0.17 = 0.12$ (95% CI: 0.047, 0.219).[29] 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. The implied indirect pass-through falls slightly when based on the

[29] 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.

wholesale elasticities estimated with region-time FE (0.10, CI:) and no trend adjustment (0.09, CI:) (see section 4.2 for details).

I verify my indirect pass-through estimates using an alternative approach in Appendix section J. Specifically, I estimate a variant of my main empirical equation from section 4.1 for retailers, augmented with a shift-share instrument capturing the weighted average minimum wage exposure of the producer-processors that each retailer purchases from. I find an indirect pass-through elasticity of 0.12 which is identical to that from my main strategy. I describe the shift-share instrument in further detail in Appendix J.

Table 7: Minimum wage pass-through elasticities

	(1)	(2)	(3)
	A. Wholesale prices		
	Trend-adjusted	Reg.-time FE	Unadjusted
	0.17	0.14	0.12
	B. Retail prices		
Indirect	0.12	0.10	0.09
P-value			
Direct	0.06	0.06	0.06
Total (Direct + indirect)	0.18	0.16	0.15

Notes: This table reports the minimum wage elasticity of prices, two periods after a hike, computed at the corresponding average bite: $E_2 \times \overline{Bite} \times 0.01$. E_2 are taken from Tables 3 and 4. Columns 1-3 differ in the specification used for estimating the minimum wage pass-through elasticity to wholesale prices (See 4.2 for details). For all three columns, the direct pass-through elasticity to retail prices is the baseline estimate from column 1 in table 4. Data: Top Shelf Data and Washington ESD, August 2018 - July 2021.

Having obtained estimates of direct and indirect pass-through to retail prices, I now combine them to infer the total pass-through of minimum wage hikes to retail prices. I report the total pass-through elasticities in the bottom row of Table 7. Several facts stand out. First, the direct pass-through elasticity for retail prices of 0.06 is in line with existing findings from other retail sectors. Leung (2021), for example, finds a minimum wage pass-through elasticity of 0.06-0.08 for grocery store prices in the U.S.

Second, the total minimum wage elasticity of the retail price level, which is obtained by summing the direct and indirect elasticities, is 0.18—much larger than that from direct pass-through alone. Failing to account for indirect pass-through therefore dramatically under represents the effect of minimum wage hikes on retail cannabis prices.

Third, indirect pass-through accounts for two-thirds of the total retail pass-through elasticity, whereas direct pass-through accounts for only one third. The fact that the majority of the effect of minimum wages on retail cannabis prices stems from indirect pass-through is not surprising, given that COGS make up 60-80% of variable costs at cannabis stores (see Appendix Table A2).[30]

[30]This is a similar variable cost structure as other retail sectors. For example, Renkin et al. (2022) find that COGS

6 Strategic complementarity and cost pass-through rates

The results from the previous sections indicate that minimum wage hikes induce two cost shocks for cannabis retailers: a direct (i.e. labor) cost shock and an indirect (i.e. COGS) cost shock. The mismatch in tradability upstream versus downstream implies that these two cost shocks diverge in scope. Under the assumption that cannabis retailers in a given local market have similar wage structures, the direct cost shock is an aggregate cost shock. In contrast, the indirect cost shock is idiosyncratic since retailers in a given local market can purchase from different producer-processors. The divergence in scope can give rise to differences in the cost pass-through rates for each cost shock since standard theory indicates that in the presence of strategic complementarities, aggregate cost shocks will be amplified more than idiosyncratic ones. In this section, I investigate the impact of strategic complementarities on direct pass-through and quantify the implications for cost pass-through. In Section 6.1, I augment my main empirical strategy to incorporate strategic complementarities. In Section 6.2, I describe a general theoretical framework for deriving cost pass-through rates. In Section 6.3 I calibrate the model, and in Section6.4, I derive the implied cost pass-through rates for the different components of marginal cost.

6.1 Accounting for strategic complementarities

sentences about how main specification may capture some strategic complementarity, but not all. Introduce augmented model. report results.

6.2 Theoretical framework

To quantify the degree of cost pass-through one must first obtain an estimate of the impact of minimum wage hikes on cannabis retailers' 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 X. The derivations therefore closely follow Renkin et al. (2022).

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 N different cannabis products, $X_1, X_2, ..., X_N$ with wholesale prices $P_1, P_2, ...P_N$. 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 labor portion of marginal cost:

$$\eta_L^{mc} = \frac{\partial MC}{\partial MW}\frac{MW}{MC} = \underbrace{\frac{\overline{W}L}{C}}_{(i)} \cdot \underbrace{\frac{\partial \overline{W}}{\partial MW}\frac{MW}{\overline{W}}}_{(ii)} \tag{5}$$

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

account for more than 80% of grocery stores' variable costs.

Table 8: Direct minimum wage pass-through with strategic complementarity in prices

	Wholesale				Retail			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	No rivals	30 miles, distance weights	30 miles, revenue weights	Border counties	No rivals	30 miles, distance weights	30 miles, revenue weights	Border counties
E_0^o	0.0058***	0.00533	0.0086***	0.005**	0.003	0.004*		
	(0.0018)	(0.0036)	(0.0029)	(0.002)	(0.004)	(0.002)		
E_2^o	0.0097***	0.0078*	0.0139***	0.005***	0.003	0.004*		
	(0.0031)	(0.0044)	(0.0053)	(0.002)	(0.005)	(0.002)		
E_4^o	0.0095***	0.0091	0.0111***	0.005*	0.004	0.003		
	(0.0036)	(0.0057)	(0.0042)	(0.003)	(0.007)	(0.003)		
E_0^r		-5.25e-06	-0.0058*		0.004	0.003		
		(0.0038)	(0.0033)		(0.005)	(0.003)		
E_2^r		0.0019	-0.0087		0.004	0.004		
		(0.0045)	(0.0065)		(0.007)	(0.003)		
E_4^r		-0.0002	-0.0034		0.002	0.006		
		(0.0071)	(0.0059)		(0.009)	(0.005)		
	Estimation summary							
$\sum$ Pre-event	1.02e-07	0.0044	0.0018	0.001	0.0002	0.0006		
	(0.0028)	(0.0034)	(0.0043)	(0.002)	(0.002)	(0.002)		
$E_0^o + E_0^r$	0.0058***	0.0053***	0.0028	0.005**	0.007**	0.007**		
	(0.0018)	(0.0019)	(0.0021)	(0.002)	(0.003)	(0.003)		
$E_2^o + E_2^r$	0.00966***	0.0097***	0.0052	0.005***	0.007**	0.008***		
	(0.0032)	(0.0034)	(0.004)	(0.002)	(0.003)	(0.003)		
$E_4^o + E_4^r$	0.0095***	0.0089*	0.0078	0.005*	0.006	0.009*		
	(0.0036)	(0.0047)	(0.0053)	(0.003)	(0.004)	(0.004)		
N	14,777	14,388	14,741	14,189	14,105	13,997		
Trimmed	YES	YES	YES	NO	NO	NO		
Detrended	YES	YES	YES	NO	NO	NO		
Time FE	YES	YES	YES	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 **??**. 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.

minimum wage elasticity of the average wage.

The second expression corresponds to the COGS portion of marginal cost:

$$\eta_{cogs}^{mc} = \frac{\partial MC}{\partial MW}\frac{MW}{MC} = \underbrace{\frac{COGS}{C}}_{(iii)} \cdot \underbrace{\frac{\partial P_w}{\partial MW}\frac{MW}{P_w}}_{(iv)} \tag{6}$$

$COGS$ denotes the cost of goods sold and P_w 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_L^{mc} and η_{cogs}^{mc}.

6.3 Empirical calibration

(i) and (iii): 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 3 to calculate the average annual wholesale expenditure for cannabis retailers. Taken together, the average annual labor and wholesale costs provide a comprehensive overview of cannabis retailers' variable cost structure.[31] 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 (Leung, 2021; Renkin et al., 2022).

(ii): 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.[32] I then estimate the following two-way fixed effects regression in first-differences

$$\Delta log\overline{W}_{k,q} = \sum_{q=1}^{4} \beta_q \Delta MW_{k,e} \times Bite_{k,e} \times \delta_q + \delta_q + \epsilon_{c,q} \tag{7}$$

$\overline{W}_{k,q}$ 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 9 shows that I find a significant and positive effect of minimum wage hikes on average wages in the quarter immediately following a hike. At

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

[32] 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 X, I find no evidence of negative employment effects.

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 producer-processors.

Table 9: 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 7 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).

(iv): Minimum wage elasticity of wholesale prices

In Section 4.2, 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 6.[33]

Minimum wage elasticities of marginal cost

Having estimated cannabis retailers' cost shares, the minimum wage elasticity of average wages, and the minimum wage elasticity of wholesale cost, I can now estimate the minimum wage elasticities of marginal cost from equations 5 and 6. I report the estimates in Table 10. For my baseline specification, the elasticity for the labor cost portion of marginal cost, η_L^{mc}, is $0.27 \times 0.25 = .07$. The elasticity for the COGS portion is $0.17 \times 0.75 = 0.12$.

6.4 Implied cost pass-through rates

Let η_L^p denote the minimum wage elasticity of retail price stemming from the labor cost shock, and η_{cogs}^p denote minimum wage elasticity of retail price stemming from the COGS shock. The cost pass-through rates ρ_L and ρ_{cogs} can be obtained by dividing these price elasticities by the

[33]This implies that cannabis retailers have low cross-price elasticities for wholesale demand. [1 sentence pointing to evidence of this].

respective cost elasticities

$$\rho_L = \frac{\eta_L^p}{\eta_L^{mc}}, \quad \rho_{cogs} = \frac{\eta_{cogs}^p}{\eta_{cogs}^{mc}} \tag{8}$$

I report the implied pass-through rates in Panel C of Table 10. Without strategic complementarity in prices, I estimate a pass-through rate for the labor portion of marginal cost of 1.54. The estimated pass-through rate for the COGS portion of cost is 0.99. Both of these pass-through rates are not statistically significantly different from one. With strategic complementarity in prices, however, the pass-through rate for the labor cost portion increases to 2.35, reflecting the higher minimum wage elasticity of retail prices η_L^p. This exercise highlights how the mismatch in tradability upstream and downstream gives rise to a divergence in the pass-through rates of the different components of marginal cost. The divergence arises because the non-tradable good is subject to strategic complementarity in prices while the tradable good is not.

Table 10: 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.

7 Tradability, market power, and pass-through

This section examines several facts about the effect of minimum wages on cannabis prices that have interesting implications for the literature on the transmission of cost shocks.

Idiosyncratic local cost shocks

Since cannabis stores in local markets pay similar wages, the direct cost shock from minimum wage hikes is an aggregate cost shock for local markets. However, since stores typically purchase from producer-processors in different locations and with different minimum wage exposure, the indirect cost shock is idiosyncratic to specific retailers in local markets. Retailers that purchase from producer-processors with high minimum wage exposure will have large indirect pass-through effects, while stores that purchase from producer-processors with low minimum wage exposure will have little indirect pass-through.

This has two major implications for cost pass-through in other retail sectors more generally. First, when there is a mismatch in the tradability of goods upstream versus downstream, local stores' cost pass-through can differ despite stores being subject to the *same* aggregate cost shock. This divergence is expected to be strongest in retail sectors with low home bias (e.g. drugstores and general merchandise stores), since those sectors sell a high share of goods produced outside of local markets.[34] Second, the total pass-through of minimum wage hikes cannot be inferred from local minimum wage exposure alone. To understand the full impact of minimum wage hikes on retail prices, one must know stores' idiosyncratic exposure to upstream pass-through as well.

Scale of production, market power, and pass-through

An interesting finding from Section 4.2 is that direct minimum wage pass-through *decreases* with the scale of production upstream but *increases* with the scale of production downstream. In this subsection, I show that these contrasting findings are consistent with different levels of tradability upstream versus downstream.

I begin with the upstream market (i.e. producer-processors). Two features of cannabis production help explain why minimum wage pass-through decreases with the scale of production. First, capacity constraints imposed by the LCB (described in section 4.4) and the high fixed costs associated with indoor cannabis cultivation (Caulkins et al., 2018; Caulkins & Stever, 2010; Washington State Liquor and Cannabis Board, 2019) point to economies of scale for large producer-processors.[35] This suggests that large producer-processors may have more market power than small producer-processors.

Second, producer-processors manufacture a tradable (within state) good and sell to retailers across the entire state (see Appendix Table A3). This suggests that the wholesale cannabis industry is highly competitive. Using market concentration as a proxy for the degree of competition, I calculate the average monthly HHI under different geographic definitions of the wholesale market (see Appendix Table A3).[36] I find a mean monthly HHI ranging from 0.10 (regional market) to 0.006 (statewide market), indicating that the wholesale cannabis industry

[34] According to the Commodity Flow Survey, the average wholesale-to-retail transportation distance is comparatively large for general merchandise and drugstore products (1,083 miles for textiles; 1,063 miles for misc. merchandise, 783 miles for electronics; 623 miles for pharmaceutical products) and short for grocery products (183 miles for meats; 187 for cereals and grains; 373 miles for prepared foods) (US Census Bureau, 2017a).

[35] Production costs for cannabis cultivation are significantly higher than other indoor crops (Caulkins et al., 2018).

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

has a low market concentration and is highly competitive.[37]

Taken together, low market concentration and 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 producer-processors (i.e. those with more market power) may adjust to the minimum wage cost shock along margins other than price, whereas small producer-processors (i.e. those with little or no market power) may have no choice but to pass the costs through to wholesale prices.

Having discussed the wholesale market, I now turn to the retail market. Two features of the retail cannabis market can explain why minimum wage pass-through increases with the scale of production (i.e. chain size) for retail stores. First, unlike producer-processors, retailers sell a non-tradable good in highly localized markets. When estimating the unit cost pass-through regression (equation4), I find that retailers' sensitivity to competitors' unit costs decays with geographical distance (see Appendix Figure **??**). Strategic complementarity in prices thus appears to be highly localized among cannabis retail stores. This is in line with Hollenbeck and Uetake (2021), who find evidence of high travel or search costs on the part of cannabis consumers, and Ambrose et al. (2021), who find that cannabis consumption decreases with consumers' distance to the nearest store.

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, for producer-processors and retailers alike, the scale of production and market power appear to be positively correlated. Yet, differences in tradability lead to a different relationship between market power and cost pass-through. This has broad implications for retail sectors beyond cannabis. In industries where there is a mismatch in the tradability of goods upstream versus downstream, market power may 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.

8 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.

8.1 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

[37] This is corroborated by several industry surveys and news media reports Washington State Liquor and Cannabis Board (2021).

the 5-digit NAICS industry level.[38] 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 producer-processors. I then estimate my main regression model (equation 2) 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.

8.2 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.[39] 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.[40] 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 2) 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.

8.3 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, Poncet, and Zhang (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 producers 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).

[38]Cannabis retailers belong to NAICS 45399 ("all other miscellaneous store retailers"). Cannabis producer-processors 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 producer-processors in Washington state grow indoors.

[39]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.

[40]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.

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 (Atkin, Chaudhry, Chaudry, Khandelwal, & Verhoogen, 2017; Bloom, Eifert, Mahajan, McKenzie, & Roberts, 2013). 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.

9 Discussion and conclusion

9.1 Discussion

One issue with this type of analysis is the degree to which results from one industry can be used to infer price dynamics in other industries. Some of the characteristics that make Washington's cannabis industry an ideal laboratory for studying minimum wage pass-through also set the industry apart. The prevalence of small-scale indoor cultivation, along with rules governing the scale of cultivation, mean that cannabis production is likely to be more labor intensive than other agricultural industries (I discuss this topic in appendix **??**). Accordingly, the labor share of variable cost for cannabis producers is expected to be higher, and hence, the cost shock imposed by the minimum wage may be larger compared to other agricultural industries. At the retail level, however, the variable cost structure for cannabis establishments is remarkably similar to conventional retail settings (see appendix A.3), meaning the relative importance of the wholesale and labor cost shocks should be similar.

Another consideration when comparing industries is the price elasticity of demand, a key determinant of cost pass-through. Conditional on the degree of competition, a higher price elasticity reduces the ability of firms to pass cost shocks onto consumers (Weyl & Fabinger, 2013). This is an important consideration in the context of cannabis given its potentially addictive nature.[41] Using a similar cannabis scanner dataset as this paper, Hollenbeck and Uetake (2021) estimate an overall retail price elasticity of 1.04 (relative to the outside good), which is higher than elasticities for cigarettes and beer but lower than that for spirits.[42] Interestingly, this

[41] Epidemiological evidence on cannabis addiction is mixed. Recent evidence suggests that 9% of regular cannabis users become dependent, compared to 67.5% for nicotine, 22.7% for alcohol, and 20.9% for cocaine (Lopez-Quintero et al., 2011).

[42] Estimates of aggregate price elasticities range from .16 (Gordon & Sun, 2015) to 0.8 (Becker, Grossman, & Murphy, 1994) for cigarettes; 0.69 to 0.72 for beer (N. H. Miller & Weinberg, 2017); and 2.8 for spirits (Miravete, Seim, & Thurk, 2018).

elasticity is not substantially different from those found in non-addictive product markets.[43] This suggests that based on the price elasticity alone, retail cost pass-through in cannabis is not expected to differ from other industries studied in the literature.

A final distinction is that the cannabis market operates under statewide autarky. This implies that cannabis retailers may be constrained in their response to the wholesale cost shock since the set of substitutable wholesale products is partly determined by geography. In contrast, retailers in other industries can leverage interstate trade networks to substitute out of products with high pass-through to wholesale prices. Therefore, I view my indirect retail pass-through estimates as more applicable for industries with home bias (e.g. grocery stores), but potentially less applicable for industries with a high degree of geographic substitutability along the supply chain (e.g. drugstores and general merchandise stores).[44]

9.2 Conclusion

In this paper, I study the effects of minimum wage increases on wholesale and retail prices in Washington state's legal recreational cannabis industry. I use scanner-level data to estimate pass-through elasticities across a set of predetermined minimum wage hikes from 2018 to 2021. When ignoring pass-through to wholesale prices, I find that a 10% increase in the minimum wage raises retail prices by 0.77%. Yet, I also find substantial pass-through to wholesale prices: a 10% increase in the minimum wage raises wholesale prices by 1.72%. The existence of pass-through to wholesale prices implies that retailers face a wholesale cost shock in addition to the labor cost shock. When the empirical model is augmented to account for the wholesale cost shock, retail pass-through more than doubles to 2.04%. I find that retailers do not adjust markups to wholesale pass-through, indicating a full pass-through of the wholesale cost shock to retail prices. Moreover, pass-through to wholesale prices decreases with the scale of production, which suggests that large producers may adjust to the labor cost shock along other margins. The findings in this paper highlight the importance of examining the entire supply chain—beyond the final point of sale—when investigating the product market effects of minimum wage hikes.

[43] N. H. Miller, Osborne, and Sheu (2017b), for example, estimate an elasticity of 0.92 for the Portland cement industry.

[44] Using the 2007 Commodity Flow Survey, Renkin et al. (2022) provide evidence of substantial home bias in US grocery consumption.

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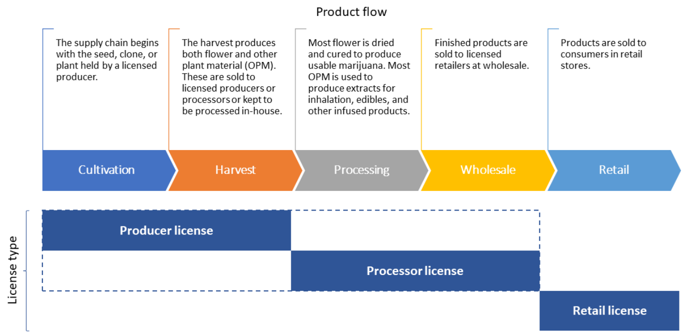
A Firm costs in the cannabis industry

A.1 The cannabis supply chain

Fig. A1. The stages of cannabis production



Notes: This figure depicts the flow of cannabis products, from left to right, as they move through the supply chain. Only licensed producers are permitted to cultivate and harvest cannabis plants; producers 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. producer-processors). Retailers may not hold a producer or a processor license and vice versa. As a result, production and retail activities are legally separated.

A.2 Cannabis labor

The labor intensive nature of cannabis production

Most cannabis plants are dioecious, meaning there are separate male and female plants, and buds with high concentrations of psychoactive compounds are exclusively produced by unpollinated female plants. Since pollination leads to seed production and inferior buds, producers are typically forced to discard the entire crop if it becomes cross-pollinated. Therefore, unlike other dioecious crops like fruits or nuts, where males and females must co-mingle, cannabis producers must carefully identify and remove any male cannabis plants from the growing area, since even a single male plant can pollinate—and thereby ruin—an entire crop. This laborious process is compounded by a heavy reliance on indoor cultivation, which is generally considered to be more labor intensive than outdoor production.[45] When cannabis plant buds have matured, they are harvested and trimmed by hand, a process which takes up to six hours per pound (Cervantes, 2006). Trimming is particularly labor intensive, as workers use hand trimmers to manually shape the harvested buds.[46] Other tasks like filling pre-roll shakers are also largely done by hand.

[45]Indoor cultivation offers stable growing conditions, year-round harvests, and enables more potent buds Aizpurua-Olaizola et al. (2016).

[46]As noted by (K. Miller & Seo, 2021), growers have shied away from mechanized trimmers since hand-trimming allows producers to extract higher quality buds and fetch higher prices from consumers.

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.[47] 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 producers, 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 producers 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%.

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

	Wholesale			Retail				
Year	Cannabis wholesale	NAICS 111	NAICS 111419	Cannabis retail	NAICS 453	NAICS 453998	All private 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.

[47] Producers 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.

A.3 Variable cost structure for cannabis retailers

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 (Renkin et al., 2022). Other expenditures like packaging and transport costs typically make up less than 1% of variable cost.

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).

A.4 The geography of wholesale costs

Table A3 shows the percentage of retailers' wholesale costs in relation to a producer's geographic location. Column 1 shows that only 5.22% of retailers' wholesale expenditures go to producers located in the same city as the retailer. Column 2 shows that less than 15% goes to producers in the same county as the retailer. For column 3, I 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 producers located in the same 3-digit zip code. Next, I 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 producer. Column 5 looks at the subset of establishments located in the west and east regions of the state, thus dropping producers 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 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 **??** illustrate that there is no home bias in wholesale cannabis purchases.

Table A3: Share of retailers' wholesale costs by geographic proximity

	(1)	(2)	(3)	(4)	(5)	(6)
	Same city	Same county	Same 3-digit zip code	Same region	Non-contiguous region	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.

A.5 The relationship between producer and retail bite

This section illustrates that the relationship between producer 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 A4: Within-county correlation between producer and retail bite

	(1)	(2)	(3)
	Raw correlation	FE	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.

B Details on prices and price indexes

B.1 Price descriptive statistics

Producer 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 producers 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.[48] A common complaint among cannabis producers is that small producers operate on slim margins while large producers enjoy a higher degree of market power (Washington State Liquor and Cannabis Board, 2021). If large producers have more market power, then one would expect higher markups (all else equal). While producer markups are not observable in the data, wholesale prices serve as a proxy for markups under the assumption of homogeneous marginal input costs across producers and product subcategories. Table B1 shows a U-shaped relationship between wholesale prices and the scale of production for nearly all product subcategories. For 3.5g and 7g of usable marijuana, average wholesale prices differ by less than 1% between small and large producers. The price gap grows to 5% for 14g products and 21% for 28g products, reflecting a flatter quantity discount for large producers compared to small producers.[49] However, note that 3.5g and 7g products generate over 2.5 times as much revenue as the higher unit weights, which indicates that for the majority of their sales, large producers are not able to charge higher prices compared to small producers.[50] This implies that the heterogeneous pass-through across the scale of production shown in Table **??** in the main part of the paper is not the result of different initial price levels between smaller and larger producers.

Table B2 reports average retail prices across chain size and product subcategories. Unlike the case with wholesale prices, retail prices exhibit an inverse U-shaped relationship between price and chain size, as independent retailers and large chains have higher average prices compared to mid-sized chains (2-3 stores). Importantly, however, for a given product subcategory, average prices are quite homogeneous across chain size. This again indicates that the heterogeneous pass-through shown in Table **??** in the main part of the paper does not reflect differences in initial prices between independent and chain stores.

[48]Tiers were assigned to establishments before the market fist opened in 2014 and once assigned an establishment cannot switch tiers. While producers can produce below the threshold for their tier, actual canopy usage has been found to be proportional to those thresholds (Washington State Liquor and Cannabis Board, 2019).

[49]For producers, 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. As a result, the average wholesale price listed under 1g is partly based on per-gram prices of larger package sizes. Therefore, caution is warranted when interpreting differences in wholesale prices for 1g products across the production tiers.

[50]One cannot rule out unobserved quality differences in large packages between small and large producers. If small producers sell lower quality buds in large package sizes (compared to large producers), then this may explain the steeper quantity discounts for small producers.

Table B1: Average wholesale prices for major product subcategories

Unit weight (grams)	(1) Small producers	(2) Medium-sized producers	(3) Large producers	Sales (millions of $)
	Concentrate for inhalation			
0.5g	9.60	12.85	11.09	32.03
1g	11.22	9.25	11.57	300.32
	Usable marijuana			
1g^{+}	4.00	3.25	2.97	99.83
3.5g	11.70	10.25	11.85	257.83
7g	22.33	20.33	22.66	74.03
14g	37.53	32.63	39.47	49.81
28g	55.75	54.56	67.58	75.65

Notes: This table reports average wholesale prices for the main cannabis product subcategories. Column 1 shows average prices for tier 1 producers (less than 2,000 sq. ft. of plant canopy); Column 2 corresponds to tier 2 producers (2,000 to 10,000 sq. ft. of plant canopy); Column 3 corresponds to tier 3 producers (10,000 to 30,000 sq. ft. of plant canopy). Data: Top Shelf Data, (August 2018 - July 2021).
$^{+}$ For producers, 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. As a result, for usable marijuana the average wholesale price listed under 1g is partly based on per-gram prices of larger package sizes.

Table B2: Average retail prices for major product subcategories

Unit weight (grams)	(1) Independent stores	(2) 2-3 stores	(3) 4+ stores	Sales (millions of $)
	Concentrate for inhalation			
0.5g	28.57	29.41	28.82	116.27
1g	29.51	29.55	29.45	969.54
	Usable marijuana			
1g	8.47	8.55	8.49	276.14
3.5g	28.29	29.06	28.17	677.17
7g	51.37	53.49	51.95	193.59
14g	82.62	83.38	80.18	137.49
28g	125.41	130.42	128.77	215.89

Notes: This table reports average retail prices for the main cannabis product subcategories. Column 1 shows average prices for establishments that do not belong to a retail chain. Column 2 corresponds to establishments belonging to a retail chain comprising 2-3 stores. Column 3 corresponds to establishments belonging to a chain with 4-5 stores. Data: Top Shelf Data, (August 2018 - July 2021).

B.2 Establishment-level price 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 SKU, but TSD does not report this. Instead, each product is identified by a unique combination of five elements: retailer-producer-category-unit weight-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

$$P_{i,j,t} = \frac{TR_{i,j,t}}{TQ_{i,j,t}}. \tag{9}$$

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:

$$I_{c,j,t} = \prod_i \left(\frac{P_{i,j,t}}{P_{i,j,t-1}} \right)^{\omega_{i,c,y(t)}} \tag{10}$$

where each subcategory is a unique category-unit weight combination.[51] 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.[52]

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

$$I_{j,t} = \prod_c I_{c,j,t}^{\omega_{c,j,y(t)}}. \tag{11}$$

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 producers are constructed in a very similar manner as with retailers, but for two exceptions. First, at the wholesale level a product is identified by a unique combination of four elements (not five as with retailers): producer-category-unit weight-product name. While a retailer may sell similar products produced by different producers, a producer creates the product and sells it to many retailers, which makes it unnecessary to identify a product at the five-element level. Note that this still allows for wholesale price discrimination, since the wholesale price of a single product may differ among retailers. Second, the wholesale price data exhibits much larger variation in prices compared to the retail data. As a result, the product-level index $\frac{P_{i,j,t}}{P_{i,j,t-1}}$ in eq. 10 leads to a few inconceivable outliers such as a 562-factor increase in prices from one month to the next. To prevent outliers from

[51]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.

[52]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.

driving results and to reduce standard errors in my estimation, I trim the top and bottom 0.1% of the product indexes before calculating the subcategory index in equation 10. As Table B3 illustrates, trimming does not meaningfully change the location or shape of the distribution but lowers the standard deviation considerably.

Table B3: Product-level 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.

B.3 Establishment-level markup index

This section describes the construction of the establishment-level markup indexes used in section **??**. For each individual product, the markup over marginal input cost (MIC) is defined as:

$$\mu_{i,r,t} = \frac{P_{i,r,t}}{MC_{i,r,t}} \tag{12}$$

where $P_{i,r,t}$ is the price of product i sold by retail establishment r in month t, and $MC_{i,r,t}$ is the wholesale price that retailer r pays for that very same product in period t. Note that this formulation implicitly assumes that the markup of interest is contemporaneous (period t retail price and period t wholesale price) rather than lagged (period t retail price and period $t-1$ wholesale price).

To construct establishment-level markup indexes, I employ a two step procedure similar to that used for the price indexes throughout the paper. In the first step, I use $\mu_{i,r,t}$ to construct a geometric mean of month-over-month changes in markups for product subcategory c at establishment r:

$$I^{\mu}_{c,r,t} = \prod_i \left(\frac{\mu_{i,r,t}}{\mu_{i,r,t-1}}\right)^{\omega_{i,c,y(t)}} \tag{13}$$

where each subcategory is a unique category-unit weight combination. The weight $\omega_{i,c,y(t)}$ is the share of product i in total revenue of subcategory c in establishment r during the calendar year of month t.

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

$$I^{\mu}_{r,t} = \prod_{c} I^{\omega_{c,r,y(t)}}_{c,r,t}. \tag{14}$$

Here, the weight $\omega_{c,r,y(t)}$ is the share of subcategory c in total revenue in establishment r during the calendar year of month t. The dependent variable is then obtained by taking the natural logarithm of markup index: $\Delta\mu_{r,t} = \ln I^{\mu}_{r,t}$.

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 producers are also processors (i.e. producer-processors). Processing falls under NAICS 424 which includes as a subcomponent "Other Farm Product Raw Material Merchant Wholesalers, including Marijuana Merchant wholesalers" (NAICS 424590).[53] 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 producer-processors' 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 & 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 producers, but the shares remain relatively constant over time for both producers

[53] A third industry, NAICS 115, may also apply to producer-processors, 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.

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 A1 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.

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.

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 producer-processors 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 measurement error. In contrast, the 3-digit NAICS code captures both indoor and outdoor producer-processors 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.

Measurement error in NAICS 111

The nature of agricultural labor in the United States means that one must consider whether the bite variable for NAICS 111 is subject to non-random measurement error. Non-random measurement error could arise for several reasons, and each is discussed in the following subsections.

Undocumented workers in NAICS 111

First, 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.

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 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 appendix F, estimates are robust to using Q4 bite, suggesting the main results are not affected by measurement error from seasonal wage fluctuations.

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.

D Conditions for valid identification

D.1 Direct pass-through: Identifying the Average Causal Response (ACR)

Difference-in-differences with continuous treatment is widely applied in empirical research, yet it is only recently that identification and interpretation of such DiD designs have been given formal treatment. Callaway, Goodman-Bacon, and Sant'anna (2021) show that a causal interpretation with continuous treatment requires different assumptions than with a binary treatment. This section highlights key insights from Callaway et al. (2021) and relates them to my research setting.

First, recall that the treatment intensity in my main specification (equation **??**) equals the percent increase in the minimum wage times the industry-by-county bite, $\Delta MW_j \times Bite_{k(j)}$ (I omit leads and lags here for ease of exposition). The key identifying variation comes from $Bite_{k(j)}$ while the ΔMW_j term acts as a scale parameter that enables interpreting the estimated coefficients as elasticities. In other words, omitting ΔMW_j does not change the results but simply alters the scale and interpretation of the estimated parameters (see appendix Figure F4). Therefore, in the rest of this subsection I refer to $Bite_{k(j)}$ as the relevant treatment intensity when discussing the causal parameter of interest and use the following simplified variant of my main empirical equation

$$\pi_{j,t} = \beta Bite_{k(j),t} + \gamma_t + \epsilon_{j,t}$$

where leads and lags have been removed for ease of exposition.

Callaway et al. (2021) show that the assumptions required for causal interpretation of β depend on the causal parameter of interest and the particular research setting. In settings where all groups are assigned some positive treatment intensity, comparisons between groups with different treatment intensities do not allow inference about the Average Treatment effect on the Treated (ATT) since no group can be used to estimate an untreated counterfactual. This applies to my research setting since there are very few untreated units (i.e. establishments in counties with bite equal to zero) which precludes identifying ATT-type parameters. Instead, the parameter of interest is the Average Causal Response (ACR) which captures the overall causal response of a small change in treatment intensity. The ACR is a weighted average of local comparisons of paths of outcomes known as the Average Causal Response on the Treated (ACRT). Specifically, the ACRT is the average difference between potential outcomes under treatment intensity d compared to potential outcomes under a marginal change in the treatment intensity for the group of units that actually experience treatment intensity d. Using the same notation as Callaway et al. (2021),[54]

$$\mathbb{E}[\Delta Y_t | D = d_j] - \mathbb{E}[\Delta Y_t | D = d_{j-1}] = ACRT(d_j | d_j) + \underbrace{ATT(d_{j-1} | d_j) - ATT(d_{j-1} | d_{j-1})}_{\text{"selection bias"}}. \quad (15)$$

The left side of equation 15 compares the change in the outcome Y between higher- and lower-treatment intensity units, where d_j and d_{j-1} are "neighboring" treatment intensities, with $d_j > d_{j-1}$. The right side illustrates that these comparisons are a combination of (i) the av-

[54]Note that for ease of exposition I use the multi-valued treatment (rather than continuous treatment) notation from Callaway et al. (2021).

erage causal response on the treated of treatment intensity d for units that receive treatment intensity d, $ACRT(d|d)$, and (ii) differences in the treatment effects between the two groups at the lower treatment intensity (which Callaway et al. (2021) call "selection bias"). Equation 15 implies that $ACRT(d|d)$ is identified if the selection bias equals zero among units that received *similar* treatment intensities. Intuitively, zero "local" selection bias implies that treatment effects would be similar across counties with similar bites, had those counties been assigned the same (nearby) bite d. While this is an inherently untestable assumption (similar to how parallel trends cannot be tested), I view zero local selection bias as valid in my setting for several reasons. First, "selection on gains" (i.e. treatment effect heterogeneity under a counterfactual treatment) would reflect local differences in the price elasticity of demand, which partly depends on local macroeconomic conditions such as county unemployment and average wages, which I explicitly control for in section **??**. Second, to the extent that county controls may not capture all of the determinants of cross-county differences in the price elasticity of demand, in section **??** I also include region-time FE. This involves a much more narrow set of comparisons between culturally, politically, and economically homogeneous counties (i.e. counties where one would expect similar selection on gains). Importantly, pass-through effects remain stable across these specifications, which indicates a lack of selection on gains.

D.2 Indirect pass-through: Identification with the shift-share instrument

A growing number of empirical studies use shift-share ("Bartik") instruments, defined as the inner product of growth rates (the "shift" or "shock") and exposure shares. Borusyak, Hull, and Jaravel (2022) develop an econometric framework in which shift-share (SSIV) identification stems from the (conditional) quasi-random assignment of shocks, thereby allowing for endogeneity in the exposure shares. Central to this framework is "shock orthogonality", a necessary and sufficient condition for instrument validity. This section summarizes the main points from Borusyak et al. (2022) regarding shock orthogonality and relates them to my research setting.

SSIV numerical equivalence

In what follows, I use the same notation as Borusyak et al. (2022) and abstract from time indexes for ease of exposition. Consider the case where one wants to estimate the causal effect in a reduced form linear model relating outcomes to the instrument and controls

$$y_l = \beta z_l + \gamma w_l + \varepsilon_l \tag{16}$$

In my research setting, y_l equals the monthly inflation rate for retail establishment l, and z_l captures retailer l's exposure to the minimum wage-induced wholesale cost shock (note that since the SSIV concerns indirect pass-through and not direct pass-through, I abstract from the latter by including it in the control vector w_l). Specifically, z_l is a shift-share instrument from a set of shocks, g_n, for $n = 1, ...N$, and shares $s_{l,n} > 0$ which define the relative exposure of each observation l to each shock n. In my setting, the shocks g_n are the minimum wage bite for cannabis producers located in county n, and $s_{l,n}$ is the share of retailer l's wholesale expenditure that goes to producers from county n. The SSIV is then the exposure-weighted average of the shocks

$$z_l = \sum_{n=1}^{N} s_{l,n} g_n. \tag{17}$$

By the Frisch-Waugh-Lovell Theorem the SSIV estimator $\hat{\beta}$ is equivalent to a bivariate IV regression of outcome and treatment residuals

$$\hat{\beta} = \frac{\sum_{l=1}^{L} z_l y_l^{\perp}}{\sum_{l=1}^{L} z_l z_l^{\perp}} \tag{18}$$

where $y_l^{\perp}$ ($z_l^{\perp}$) is the residual from the projection of y_l (z_l) on the control vector w_l. The key insight by Borusyak et al. (2022) is that the SSIV estimator $\hat{\beta}$ is numerically equivalent to a shock-level IV regression that uses the shocks g_n as the instrument in estimating the reduced form equation

$$\bar{y}_n^{\perp} = \alpha + \beta \bar{z}_n^{\perp} + \bar{\varepsilon}_n^{\perp} \tag{19}$$

where $\bar{v}_n = \frac{\sum_{l=1}^{L} s_{ln} v_l}{\sum_{l=1}^{L} s_{ln}}$ is the exposure-weighted average of variable v_l and the IV estimation is weighted by average shock exposure $s_n = \sum_{l=1}^{L} s_{ln}$. Specifically, Borusyak et al. (2022) show that

$$\hat{\beta} = \frac{\sum_{n=1}^{N} s_n g_n \bar{y}_n^{\perp}}{\sum_{n=1}^{N} s_n g_n \bar{z}_n^{\perp}}. \tag{20}$$

Note that the IV regression in equation 19 contains transformed shock-level aggregates of the variables rather than the original observation-level variables. In my research setting, shock-level refers to the county that one (or more) producers are located in. Thus, $\bar{y}_n^{\perp}$ is the average residualized inflation of the retailers most exposed to producers in county n (i.e. the nth shock), while $\bar{z}_n^{\perp}$ is the average residualized indirect bite for the retailers most exposed to producers from county n. Each shock g_n in this regression is weighted by s_n, which is the average share of retailers' wholesale expenditures (across all retailers in Washington state) that goes to producers from county n.

Shock orthogonality

The equivalence in equation 20 highlights that SSIV estimates can be seen as stemming from variation across shocks rather than observation-level exposure shares. Borusyak et al. (2022) build on this equivalence to show that the standard exclusion restriction translates into a "shock orthogonality" condition. In particular, they show that shock orthogonality is satisfied if shocks g_n are (conditionally) quasi-randomly assigned across n regardless of the average exposure s_n or the unobservable average (shock-level) error $\bar{\varepsilon}_n$. In the context of my research setting, this implies that the industry-by-county minimum wage bite for cannabis producers has the same expected value across counties n, 1) regardless of the average retail exposure to that county s_n, and 2) regardless of the aggregated structural error term. Condition 1) implies that producer counties with high average expenditure shares across retailers do not have systematically different bite than producer counties with low average expenditure shares. As an example, this is violated if the producers in a given county n are particularly popular among retailers statewide (hence, that county has a high s_n), and after controlling for county-level observables, the producers in that county have systematically higher (or lower) bite than producers in other counties. The raw correlation between bite and expenditure shares is 0.18, and when conditioning on county observables the relationship shrinks to 0.06 (95% CI: -.0457195, .1662991). This suggests that there is no systematic relationship between bite and average expenditure shares. Condition 2) implies that there is no relationship between producer bite for county n and the average unexplained variation in retail inflation for the retailers that purchase from producers in county n. This cannot be tested because the latter is unobserved. However, I view it as unlikely that after conditioning on county controls and retailer FE, retailers with large unexplained shocks to inflation purchase more from producer-counties with higher (or lower) bite.

Instrument relevance

Recall that shock orthogonality is the shock-level equivalent to the exclusion restriction. As in other IV settings, the exclusion restriction is not sufficient for shift-share consistency—instrument relevance is also required. I test for first-stage relevance by estimating a variant of equation **??**

$$w_{r,t} = \sum_{l=-5}^{6} \beta_l \Delta MW_{r,t-l} \times Bite_{k(r),t-l} + \sum_{l=-5}^{6} \psi_l IB_{r,P,t-l} + X_{k(r),q(t)} + \gamma_t + \epsilon_{r,t}. \tag{21}$$

where $w_{r,t}$ is the (log) wholesale cost index for retail establishment r in month t. First-stage relevance is then characterized by the cumulative sum of the distributed lag coefficients ψ_l relative to a normalized baseline period. Figure D1 shows that at the average indirect bite (18.14%), a 10% increase in the minimum wage corresponds to a 6.13% (unadjusted) to 12.10% (trend-adjusted) increase in retailers' wholesale costs relative to the normalized baseline period in $t-2$ (significant at the 10 percent and 1 percent level, respectively).[55] Note that the first stage estimates are much larger than the reduced form estimates in section **??**. This is to be expected since—despite the full pass-through of wholesale costs to retail prices documented in section **??**—wholesale prices are lower than retail prices, which means that a dollar increase in wholesale prices amounts to a larger percent change than a dollar increase in retail prices.

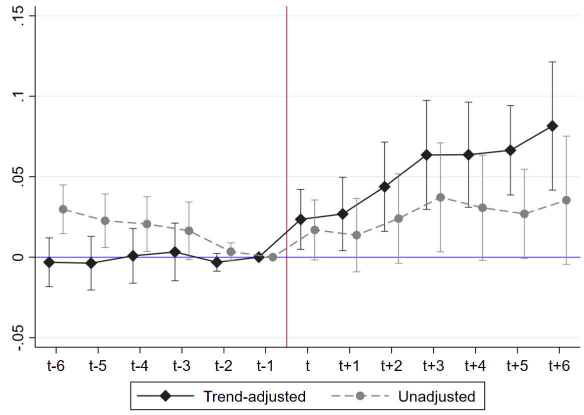
E Bite-specific trend

E.1 Direct pass-through to wholesale prices

Figure E1 illustrates that for all three specifications used in section 4 (unadjusted, trend-adjusted, region-time FE), the distributed lag coefficients are not statistically significantly different from zero for $t-5$ through $t-2$, and the period t treatment effects are large and not statistically significantly different from each other. While trend-adjusting the dependent variable does not change the contemporaneous treatment effect, it does affect pass-through estimates over a longer time horizon. Adjusting for the trend results in a permanently higher price level, whereas not adjusting results in the positive effect in period t being undone in subsequent periods. Therefore, it is important to ensure that the trend is robust to a variety of specifications and assumptions. This section provides a detailed exposition of the bite-specific trend and illustrates the empir-

[55]The respective F-statistics for the cumulative effects are 3.28 and 11.24.

Fig. D1. Effect of indirect bite on retailers' wholesale cost



Notes: The figure shows estimates from from equation 21 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.

ical validity of the adjustment using two different methods. The first method uses a single, pooled trend for to the entire sample period. This is the method that I use in the main part of the paper (see section 4). The second method estimates a separate trend for each of the three events and only adjusts the dependent variable for event 2, as that is the only event with a significant trend. As I show below, how one adjusts for the trend matters little, as both methods lead to similar results.

Pooled trend

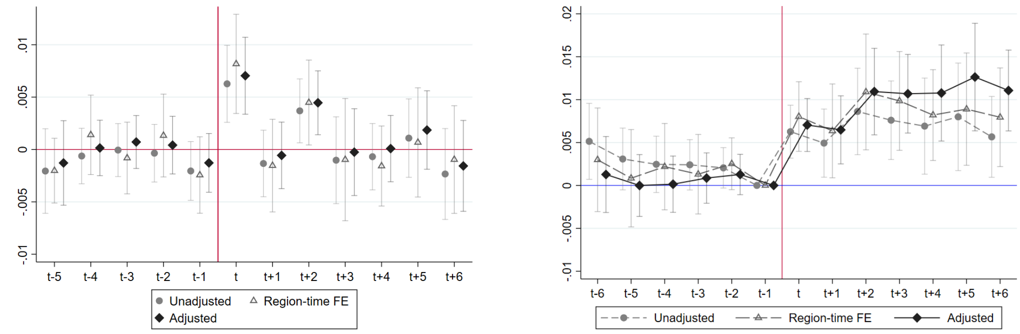
Since my research design pools three minimum wage events, the most obvious way to adjust for the bite-specific trend is to fit a single trend onto the pooled events. This is the strategy I adopt in the main part of the paper. A bite-specific trend in the price *level* occurs if $\bar{\hat{\beta}}_{pre}$, the average of the distributed lag coefficients in the pre-treatment period (i.e. the average *change* in the price level effect), is statistically significantly different from zero. As Table E1 illustrates, this is the case when the trend is estimated using all 5 pre-treatment leads. Though the trend extends through 5 leads (i.e. through $t-1$, see Figure **??**), I use the estimate from 4 leads (i.e. through $t-2$) when detrending since the base period is set to $t-2$ in my main analysis. Trend-adjustment proceeds as follows:

1. Compute the average of the distributed lag coefficients for the pre-baseline periods:

$$\bar{\hat{\beta}}_{pre} = 1/4 \sum_{l=2}^{5} \hat{\beta}_{-l}$$

where $\bar{\hat{\beta}}_{pre}$ is the average *change* in the pre-treatment price level effect.

Fig. E1. Direct pass-through of minimum wage hikes to wholesale prices



(a) Effect on the wholesale inflation rate (i.e. distributed lag coefficients)

(b) Effect on the wholesale price level (i.e. cumulative sums of distributed lag coefficients)

Notes: The figures show direct minimum wage pass-through to wholesale prices. Estimates are from equation **??**. The dependent variable is the establishment-level inflation rate for cannabis producers. Panel (a) shows the estimated distributed lag coefficients, $\hat{\beta}_l$, with 90% confidence intervals based on SE clustered at the county level. Panel (b) replicates figure **??** in the main paper and depicts cumulative price level effects (E_L) relative to the baseline period in $t-2$, with 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.

2. Use $\bar{\hat{\beta}}_{pre}$ to obtain predicted values for the bite-specific trend over the entire event window:
$$\hat{\pi}_{j,t} = \bar{\hat{\beta}}_{pre} \times \Delta MW_{j,t-l} \times Bite_{k(j),t-l}$$

3. Estimate equation **??** using adjusted inflation $\tilde{\pi}_{j,t}$ as the dependent variable:
$$\tilde{\pi}_{j,t} = \pi_{j,t} - \hat{\pi}_{j,t}$$

Separate trends

A concern with the trend adjustment presented above is that if one or more events exhibit a different trend—or no trend at all—then fitting a single trend onto all events may be misleading. Therefore, in this subsection, I examine each event separately and test for event-specific trends. Event-specific trends are estimated as follows: First, I estimate equation **??** separately for each event using the original (unadjusted) dependent variable. Next, for each event, I compute the average of the distributed lag coefficients for the pre-baseline periods, $\bar{\hat{\beta}}_{pre,e} = 1/4 \sum_{l=2}^{5} \hat{\beta}_{-l}$, where $\bar{\hat{\beta}}_{pre}$ is the average pre-treatment *change* in the price level effect for event e (since equation **??** is in first differences). As Table E1 illustrates, $\bar{\hat{\beta}}_{pre,e}$ is only statistically significantly different from zero for event 2, meaning there is no bite-specific trend for events 1 and 3. I therefore adjust $\pi_{j,t}$ for event 2 only and leave the other events unadjusted. Specifically, I use $\bar{\hat{\beta}}_{pre,e=2}$ to obtain predicted values for the bite-specific trend for the entire event window: $\hat{\pi}_{j,t(e=2)} = \bar{\hat{\beta}}_{pre(e=2)} \times \Delta MW_{j,t-l(e=2)} \times Bite_{k(j),t-l(e=2)}$. Finally, I estimate equation **??** using

Table E1: Bite-specific trend estimates for producers

	Pooled trend	Separate trends		
	(1)	(2) Event 1	(3) Event 2	(4) Event 3
4 leads	-0.00077	0.00179	-0.00090	-0.00370
	(0.00069)	(0.00261)	(0.00067)	(0.00848)
5 leads	-0.00103*	0.00056	-.00109**	-0.00485
	(0.00054)	(0.00209)	(0.00055)	(0.00788)
N	13,033	3,996	4,646	4,391
Time FE	YES	YES	YES	YES

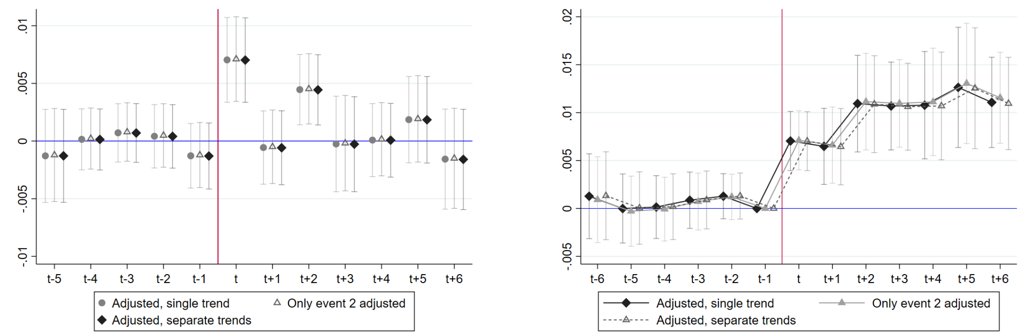
This table reports the average of the distributed lag coefficients obtained from estimating equation **??**, i.e. the average change in the estimated treatment effect during the pre-treatment period. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Data from Washington ESD and Top Shelf Data, August 2018-July 2021.

adjusted inflation $\tilde{\pi}_{j,t}$ as the dependent variable

$$\tilde{\pi}_{j,t(e)} = \begin{cases} \pi_{j,t(e)} & \text{if } e = 1,3 \\ \pi_{j,t(e)} - \hat{\pi}_{j,t(e)} & \text{if } e = 2 \end{cases} \tag{22}$$

Table E1 shows that events 1 and 3 exhibit no significant pre-trend while event 2 contains a pre-trend that is similar in magnitude to the single trend found in figure **??**. Therefore, I adjust the dependent variable for event 2 while leaving events 1 and 3 unadjusted. As Figure E2 illustrates, this approach leads to very similar results as the pooled adjustment. As a robustness check, I adjust all 3 events for an event-specific trend irregardless of statistical significance of the trend. Figure E2 shows that estimated effects are virtually identical.

Fig. E2. Comparing trend adjustment for direct pass-through to wholesale prices



(a) Effect on the inflation rate

(b) Effect on the price level

Notes: The figure compares direct pass-through to wholesale prices when the dependent variable is trend-adjusted in different ways. In both panels, estimates are from equation **??** with time fixed effects but no county fixed effects (since the event 2 trend cannot be estimated with county fixed effects). Results are robust to including county fixed effects. Panel (a) shows the estimated distributed lag coefficients, $\hat{\beta}_l$, with 90% confidence intervals based on SE clustered at the county level. Panel (b) displays 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 the main text. Panel (b) shows the 90% confidence intervals of the sums based on SE clustered at the county level. In panel b, the normalized base period is set to $t-1$. Data source: Top Shelf Data and Washington ESD, August 2018-July 2021.

E.2 Indirect pass-through to retail prices

To quantify the pre-treatment trend for indirect pass-through, I again take the average of the distributed lag coefficients for the pre-baseline period, $\bar{\hat{\psi}}_{pre} = 1/4\sum_{l=2}^{5}\hat{\psi}_{-l}$, where $\bar{\hat{\psi}}_{pre}$ is the average change in the pre-treatment effect. I find that $\bar{\hat{\psi}}_{pre} = -.0019$ (90% confidence interval: -0.0040 to 0.0002) which overlaps with the pre-trend for pass-through to wholesale prices, $\bar{\hat{\beta}}_{pre} = -0.0008$ (90% confidence interval: -0.0019 to -0.0004).[56]

[56] $\bar{\hat{\psi}}_{pre}$ is based on equation **??** with time fixed effects and county-level controls. $\bar{\hat{\beta}}_{pre}$ is based on equation **??** with time fixed effects. The pre-trend reported here is from pooled events. For both equations, the pre-trend is stable across a variety of specifications.

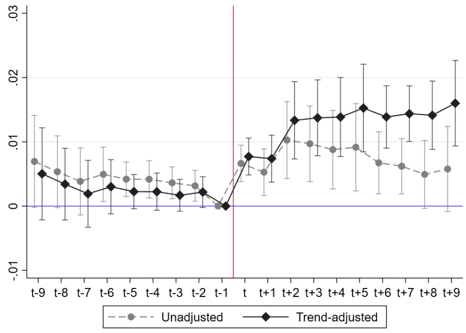
F Robustness checks

F.1 Comparing high and low bite establishments prior to treatment

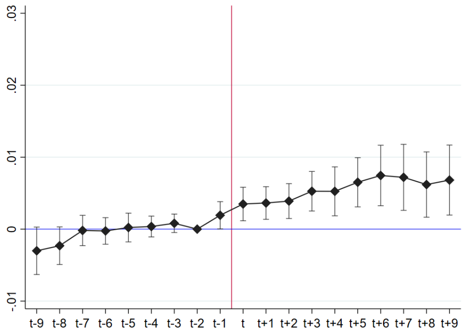
F.2 Longer event window

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 F1 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.

Fig. F1. 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 F1: 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.

F.3 Alternative specifications for direct pass-through

This subsection reports results from the robustness checks discussed in section **??**.

Table F2: 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.

Table F3: 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 **??** 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.

F.4 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 F2 shows the average bite one quarter after 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.[57] Under certain circumstances, employers can apply for permission to pay eligible employees less than the state minimum wage.[58] 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).[59] 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.[60]

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:

$$\Delta Bite_{k(j)} = Bite_{k(j),Q3,y} - Bite_{k(j),Q1,y+1} \tag{23}$$

This effectively nets out non-compliance and exempt employees at the county level. Tables F2 and F3 show that results are robust to this alternative bite variable.

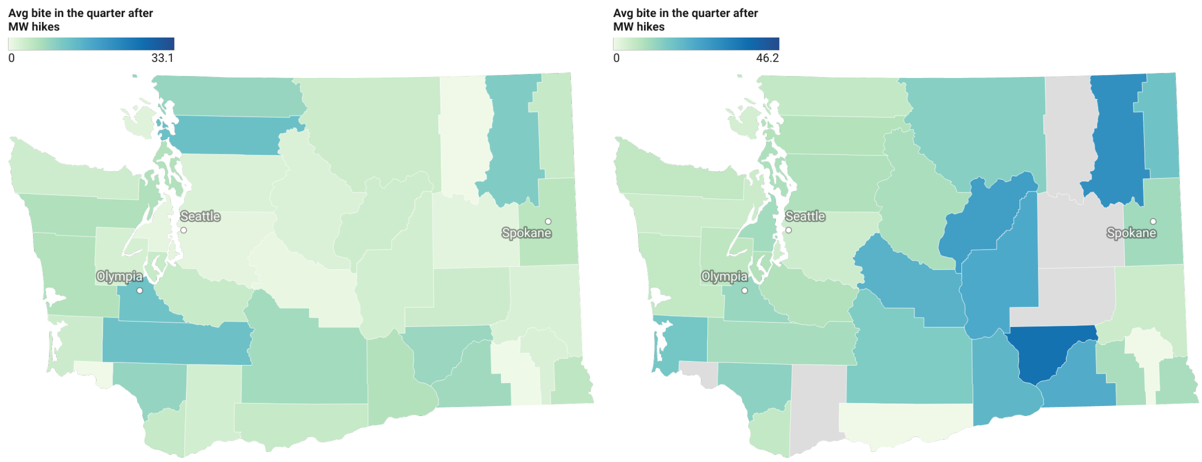
[57]The ESD has safeguards in place to flag sub-minimum wages at the employee and firm level. Implausibly low wages are either excluded from the bite variable or the wages are substituted with a previous valid quarter for that employer, adjusting for payroll and inflation.

[58]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.

[59]See Washington State Legislature (1960).

[60]For example, Gopalan, Hamilton, Kalda, and Sovich (2021) find that wage increases extend up to $2.50 above the minimum wage.

Fig. F2. Average bite one quarter after the minimum wage hike, 2018-2021



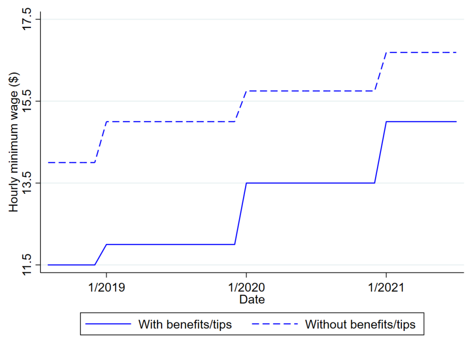
(a) NAICS 111 (Crop production) **(b)** NAICS 453 (Misc. store retailers)

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

F.5 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.

Fig. F3. 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.[61]

[61] Technically, this only applies to small employers (500 or fewer employees), as large firms (over 500 employees)

Employees who receive health benefits or tips are subject to a lower minimum wage than those who do not (Figure F3). For the former, the minimum wage schedule was pre-determined over the sample period, making the hikes contemporaneously exogenous.[62] 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)

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 4 and **??** 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

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.

[62] The schedule was determined in 2015.

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 F2 reports results from estimating equation **??** under assumptions 2 and 3 for producers. As columns 3 and 4 illustrate, wholesale price effects are very similar to those obtained in the main paper. Table F3 (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.

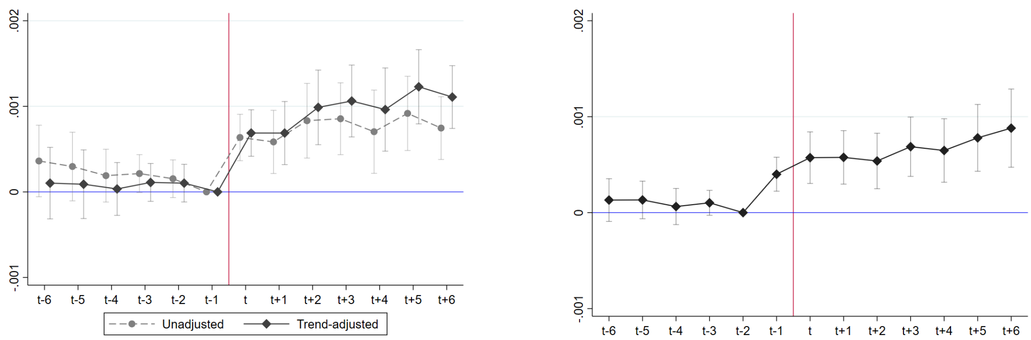
F.6 Bite as treatment intensity

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 **??**:

$$\pi_{j,t} = \sum_{l=-5}^{6} \beta_l Bite_{k(j),t-l} + X_{k(j),t} + \theta_k + \gamma_t + \epsilon_{j,t}. \tag{24}$$

Here, the treatment intensity is the minimum wage bite and it is not multiplied with $\Delta MW_{j,t-l}$. Figure F4 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.

Fig. F4. 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 producers, $t-2$ for retailers). Cumulative effects E_L are obtained by summing the distributed lag coefficients to lead or lag L as detailed in section 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. Estimates are from equation 24 with time and county fixed effects, estimated separately for producers and retailers. In panel (a), the dependent variable is adjusted for a bite-specific trend as described in Appendix E. Data source: Top Shelf Data and Washington ESD, August 2018-July 2021.

F.7 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. Leung (2021); Renkin et al. (2022)). I estimate equation **??** 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.[63] Second, while cannabis producer-processors 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 producers 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.[64] Thus, it is not possible to determine which five-digit NAICS code applies to the majority of tier 3 producers, meaning substantial measurement error will result for tier 3 producers in either case. Therefore, I drop tier 3 producers from the sample and restrict the analysis to tiers 1 and 2 (i.e. indoor growers) and use NAICS 11141 for the bite variable.[65]

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.

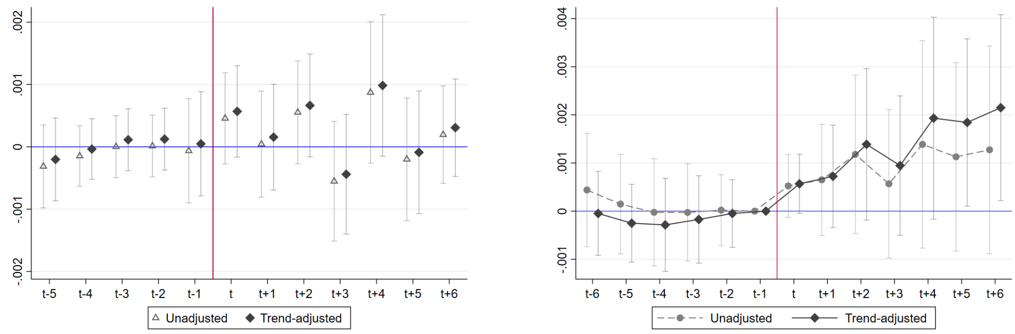
Figures F5 and F6 show sharp inflationary treatment effects at the period of the minimum wage hike for both wholesale and retail cannabis prices, and the effect is statistically significant at the 10% and 5% level, respectively.

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

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

[65] 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).

Fig. F5. Direct pass-through to wholesale prices using 5-digit NAICS bite



(a) Effect on the inflation rate **(b)** Effect on the price level

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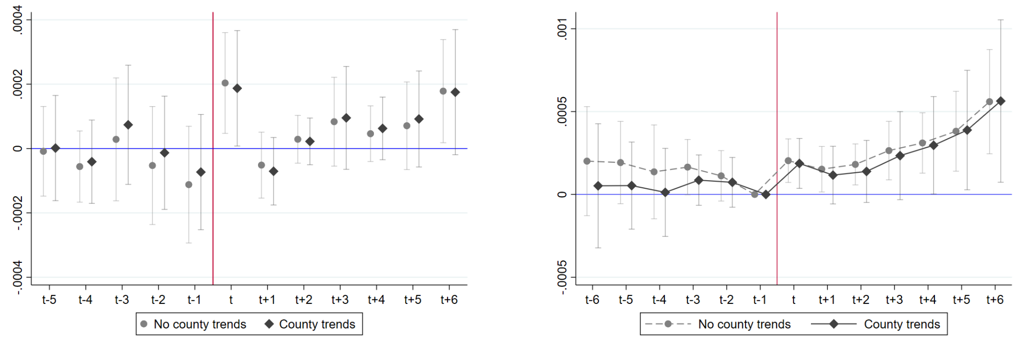
Notes: The figures show estimates from equation **??** with the bite variable based on NAICS 11141 as described in appendix F.7. Tier 3 producers and producer-processors are omitted from the estimation sample. Equation **??** is estimated with time and county fixed effects. The dependent variable is the establishment-level inflation rate, adjusted for a bite-specific trend as described in section **??**. 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 4. 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.

Fig. F6. Direct pass-through to retail prices using 5-digit NAICS bite



(a) Effect on the inflation rate **(b)** Effect on the price level

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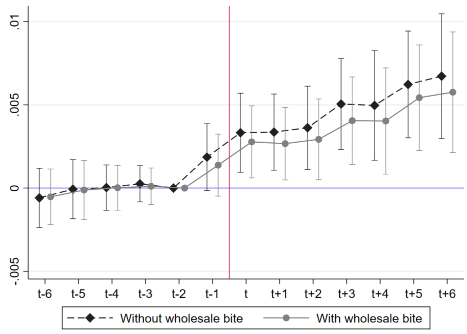
Notes: The figures show estimates from equation **??** with the bite variable based on NAICS 45399 as described in appendix F.7. Equation **??** is estimated with time and county fixed effects. The dependent variable is the establishment-level inflation rate, which is not trimmed and not adjusted for 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 4. 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.

F.8 Direct retail pass-through with indirect bite

When estimating indirect retail pass-through, one concern is whether the estimates for β_l—the direct pass-through rate—are affected by the inclusion of indirect bite as an additional variable. If estimates for direct pass-through were to change, this would cast doubt on the main identification strategy and, by extension, the results from section 4. Figure F7 compares cumulative direct pass-through from equation **??** with and without indirect bite included. Reassuringly, the figure shows that direct pass-through estimates are unaffected by the inclusion of indirect bite. The pre-treatment period is identical and treatment effects appear in $t-2$ for both specifications. Including indirect bite slightly attenuates the estimates, but the difference is not statistically significant (as evidenced by the overlapping confidence intervals).

Fig. F7. Direct pass-through with and without indirect bite as a control

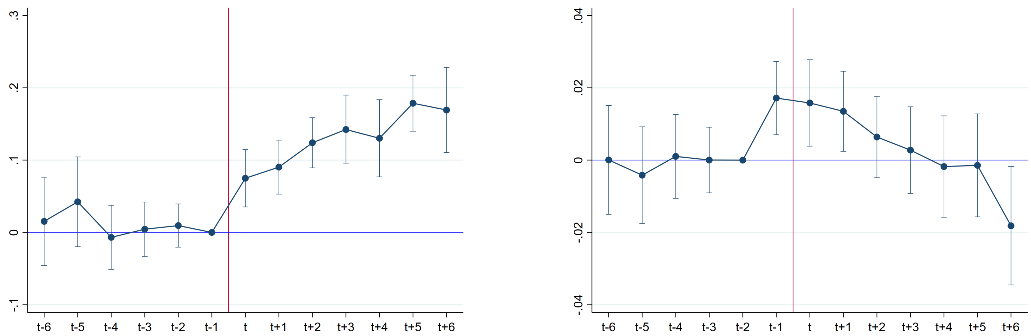


Notes: The figure compares cumulative direct price level effects for retailers when indirect bite is included versus omitted from equation **??**. Both specifications include time fixed effects and county-level controls. The estimated coefficients β_l are summed up to cumulative effects E_L relative to the baseline period in $t-2$. The figures show 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.

F.9 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 **??** for event 3 only. Figure F8 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 $t+4$.

Fig. F8. 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 producers, $t-2$ for retailers). Cumulative effects E_L are obtained by summing the distributed lag coefficients to lead or lag L as detailed in section 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 an event-specific bite-specific trend as described in section **??**. Data source: Top Shelf Data and Washington ESD, August 2020-July 2021.

G Strategic complementarity in pricing

In this appendix section, we discuss the implications and potential issues arising from strategic complementarity in pricing in our setting. Furthermore, we present estimation results measuring the extent of strategic pricing in the Washington state cannabis industry.

G.1 Theoretical framework

We 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:

$$\frac{\partial p_j}{\partial \tau} = \frac{\partial p_j}{\partial \alpha_j}\frac{\partial \alpha_j}{\partial \tau} + \sum_{i \neq j} \frac{\partial p_j}{\partial p_i}\frac{\partial p_i}{\partial \alpha_i}\frac{\partial \alpha_i}{\partial \tau} \tag{25}$$

where $\frac{\partial p_j}{\partial p_i}$ is firm j's best response to a change in firm i's price.[66] Consequently, in the presence of imperfect competition, the strategic response of (untreated) competitors may disqualify them as a valid control group.

G.2 Quantifying strategic complementarity in prices

To identify the scope of strategic complementary in prices, we follow the industrial organization literature that measures the pass-through of cost shocks and taxes. In particular, we 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 we observe wholesale unit prices, we can directly measure how changes in unit cost are passed through to prices.[67] In addition to stores' own wholesale unit costs, we also observe the wholesale unit costs of their competitors. By relating stores' prices to competitors' cost changes, we can measure the effect of competitors' cost-induced price changes, i.e. strategic complementarity in prices. Moreover, we can test whether this effect is a function of the geographic distance between stores. We use the results of this analysis to define unaffected local markets (a clean control inclusion criterion in Section 4.1).

To investigate the geographic scope of strategic complementarity of prices, we sort competitors into 5-mile bins and calculate average wholesale prices for each store-product-month-bin. We 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, we 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, we focus on contemporaneous changes in costs and prices. This is in line with the pass-through literature from other industries (see e.g. Conlon & Rao, 2020; Hollenbeck & Uetake, 2021; N. H. Miller et al., 2017a; Muehlegger & Sweeney, 2022). We estimate the following model in first-differences:

$$\Delta p_{i,j,t} = \rho \Delta w_{i,j,t} + \sum_{r=1}^{R} \beta_r \Delta w_{i,r(j),t} + \Delta \gamma_t + \Delta \varepsilon_{i,j,t}, \tag{26}$$

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 our baseline specification, we set $R = 12$ (setting $R \neq 12$ does not strongly affect estimates but

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

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

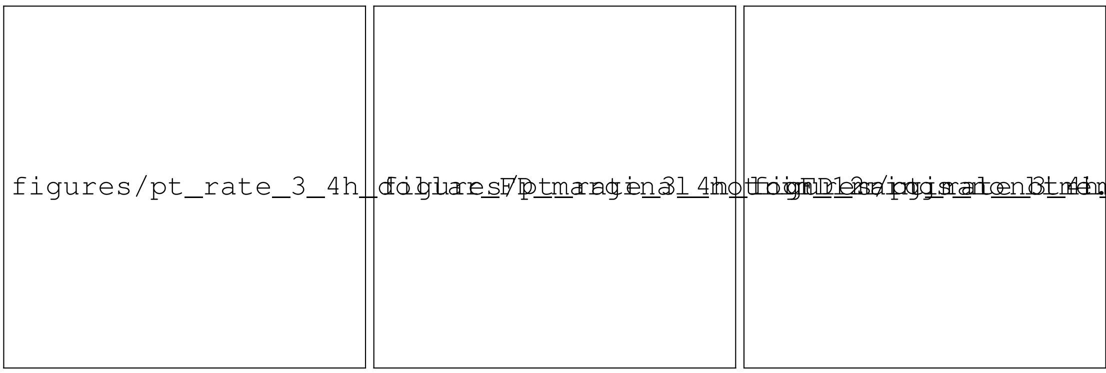
changes the sample size and standard errors). We cluster standard errors at the store level to allow for autocorrelation in unobservables within stores.

The overall effect of the aggregate cost shock on store j's prices comprises two parts: (i) the own cost pass-through rate, and (ii) the competitors' cost pass-through rate (i.e. the strategic price response). The own cost pass-through rate, ρ, is the increase in unit price at store j from the increase in store j's own unit cost. The coefficient β_r measures the pass-through of wholesale unit costs at competing stores in bin r to unit prices at store j. This is equivalent to the strategic price response between store j and competing stores in bin r. Thus, β_1 is the strategic price response for stores within 0-5 miles of store j, β_2 is the *additional* strategic price effect attributable to stores 5-10 miles away, and so on.

In addition to our main specification, we estimate several variants of our pass-through regression. First, we estimate equation 26 in levels rather than first-differences, with store-product FE. Second, we specify equation 26 using the first-difference of logs. This minimizes the influence of outliers and delivers pass-through elasticities instead of pass-through rates.

In Figure G9, we report estimated pass-through rates of competitors' unit costs for each distance bin $r \in [1, 12]$. Panels (a) and (c) show increasing marginal strategic complementary in prices up to the 30-mile mark. The fact that the effect increases with distance may 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 (**?**). Beyond 30 miles, the incremental effect of increasing the scope of the cost shock drops to zero for three consecutive bins. Interestingly, in panel (c) the 45-50 mile bin is positive and significant at the 10% level, though approximately half the size of the largest coefficients within 30-miles. Panel (b) shows some evidence of strategic complementarity in prices up to the 30-mile mark, again with effects that drop to zero for three consecutive bins thereafter.

Fig. G9. The pass-through of competitors' unit costs to own unit prices



(a) Dollars, first-differenced **(b)** First difference of logs **(c)** Dollars

Notes: the figures shows cumulative sums of coefficients $\sum_{r=1}^{R} \beta_r$ for $R \in [1, 12]$, obtained from the pass-through regression (equation 26). Panel (a) is estimated in dollars; Panel (b) is first-differences of dollars. Panel (c) is specified in logs; Panel (d) is first-differences of logs. 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.

The β_r estimates in Figure G9 measure the additional effect on store j's prices of increasing the geographic scope of an aggregate cost shock by another 5 miles. This is informative about the geographic scope of strategic complementarity in cannabis prices. However, to quantify the effect on prices of an aggregate cost shock, we report cumulative effects $\sum_{r=1}^{R} \beta_r$ at increasing distance bins in Table G4. Since effects change little beyond the 30-mile mark (see Figure G9), we only report cumulative effects up to 45 miles. The results in Table G4 indicate that an aggregate cost with sufficient geographic scope shock has non-negligible strategic price effects. A $1 increase in wholesale unit costs at all stores within a 30-mile radius corresponds to a $0.09 (Column 1), $0.10 (Column 2), and $0.45 (Column 3) increase in a store's retail price solely due to strategic complementarities. These effects are much larger than those from a cost shock affecting only rival stores (i.e. 0-5 miles) found in Table 6. 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 & Sweeney, 2022).

Overall, the results from Figure G9 and Table G4 provide suggestive evidence of strategic complementarity in prices for 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 a victimized store will not have a strategic price response to the crime-induced cost shock at victimized and rival stores. We therefore view Figure G9 and Table G4 as providing supportive evidence for our definition of unaffected local markets from Section 4.1.

Table G4: Cumulative pass-through of competitors' unit costs

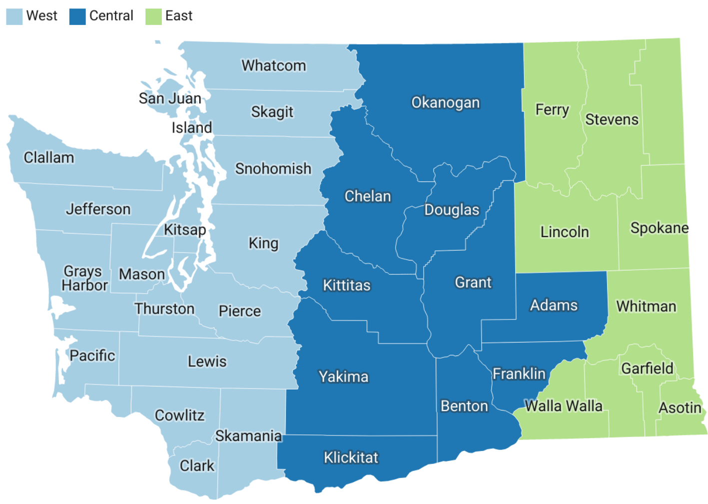
	(1) Dollars (FD)	(2) Logs (FD)	(3) Dollars (levels)
Own wholesale cost	1.654***	0.712***	1.294***
	(0.035)	(0.008)	(0.375)
Competitors' wholesale cost			
< 5 miles	0.004	0.001	0.068*
	(0.011)	(0.003)	(0.037)
< 10 miles	0.021	0.005	0.174***
	(0.014)	(0.003)	(0.058)
< 15 miles	0.031*	0.003	0.193**
	(0.018)	(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 26. 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. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

G.3 Region-time FE

The Cascade mountain range and a semiarid shrub-steppe create three distinct socioeconomic, political, and topographic regions in Washington state (West, Central, and East), depicted in Figure G10. To account for time-variant unobserved heterogeneity across these regions I estimate equation **??** with region-time FE (i.e. region $\times$ time interaction terms). To ensure the robustness of the region $\times$ time specification, I define regions in two different ways. The first is the specification presented in section **??**, which defines three regions (West, Central, East) based on well-established topographic and economic boundaries in Washington state. For the second version, I collapse the latter two regions (Central and East) into a single region (East), creating two distinct regions (West and East). This corresponds to the boundary specified by the Washington state legislature in repeated attempts to create two separate states Hallenberg (2017). Note that this boundary is clearly visible in the average bite depicted in Figure 3a in the main part of the paper. Results for the two-region version are not shown here; they are virtually identical to the three-region version and available upon request.

Fig. G10. Socioeconomic regions of Washington state



Notes: This figure shows the three major socioeconomic regions in Washington state and the counties within each region.

H Market concentration across the supply chain

I Other margins of firm adjustment

Table I1 presents the results of equation 2 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 producer-processors that grow indoors.[68] 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 producer-processors). 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 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 4.2, 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 4.2 do not depend on the exclusion of store trends.

J Estimating indirect pass-through with a shift-share instrument

This section provides more detail on the shift-share instrument discussed in section 5.

Since producers occupy the upstream portion of the supply chain, the pass-through estimates from equation **??** provide a complete measure of wholesale price adjustment in response to minimum wage hikes.[69] For retail prices, however, equation 2 only estimates direct pass-through and therefore fails to capture indirect pass-through. Thus, an analysis based solely on equation 2 may not reflect the full impact of the minimum wage on retail prices. To capture

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

[69] In practice, producers may also be subject to minimum wage pass-through from their input suppliers. However, producer inputs like hydroponic systems, grow lights, and raw materials can be purchased from suppliers outside of Washington state. Therefore, minimum wage pass-through to producer input prices is likely small. To simplify the analysis, I do not consider indirect pass-through for producers.

Table I1: Minimum wage effects on employment and quantity sold

	Employment		Quantity sold		Price	
	(1) Indoor crops	(2) Misc. store retailers	(3) No trends	(4) Store trends	(5) No trends	(6) 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 4.1. 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 4.1. The treatment intensity is defined as in previous Section 4.1. 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.

both direct and indirect effects, I estimate the following equation for retailers only:

$$\pi_{r,t} = \sum_{l=-5}^{6} \beta_l \Delta MW_{r,t-l} \times Bite_{k(r),t-l} + \sum_{l=-5}^{6} \psi_l IB_{r,P,t-l} + \gamma_t + \epsilon_{r,t}. \tag{27}$$

In contrast to equation 2, equation 27 contains not one, but two treatment variables. The first, $\Delta MW_{r,t-l} \times Bite_{k(r),t-l}$, is identical to that from equation 2 except that the index r replaces j to emphasize that the bite corresponds to retailer r. The second treatment variable is indirect bite, $IB_{r,P,t-l}$, a shift-share instrument that measures the weighted average minimum wage exposure of the producers that retailer r purchases from.[70] $IB_{r,P,t-l}$ is calculated as follows:

$$IB_{r,P,t-l} = \sum_{p=s}^{S} \alpha_{r,p} \Delta MW_{p,t-l} \times \sum_{p=s}^{S} \alpha_{r,p} Bite_{k(p),t-l} \tag{28}$$

Here, $\Delta MW_{p,t-l}$ is the size of the minimum wage hike for producer p; $\alpha_{r,p}$ is the average share of retailer r's wholesale expenditures going to producer p from $t-4$ through $t-2$, i.e. in the months leading up to the hike; and $Bite_{k(p),t-l}$ is the minimum wage bite for the industry-county k that producer p is located in.[71] Thus, the first term in equation 28 measures the average minimum wage hike for the set of producers that retailer r purchases from, while the second term measures the average bite for that same set of producers.[72] It is worth emphasizing that $\alpha_{r,p}$ contains no time index and is therefore fixed for each retailer-producer-event. In practice, retailers may react to the increase in wholesale prices by recalibrating their wholesale bundles (e.g. by substituting out of high pass-through products), in which case $\alpha_{r,p}$ would change from month to month. However, allowing $\alpha_{r,p}$ to vary within an event could result in reverse causality since a retailer's wholesale substitution patterns may reflect its own inflation. Defining $\alpha_{r,p}$ as the average expenditure share from $t-4$ through $t-2$ avoids this endogeneity, particularly since the results from the previous section indicate that wholesale price effects do not emerge until $t-1$. In other words, the expenditure shares $\alpha_{r,p}$ are based on a time frame prior to the emergence of the wholesale cost shock.

As an alternative to the indirect bite variable in equation **??**, one could use producers' geographic proximity as an instrument for retailers' exposure to pass-through to wholesale prices. This assumes that retailers purchase more from producers located nearby than those further away. However, I find little evidence supporting this assumption. Instead, a large share of retailers' wholesale purchases are from producers located in other parts of the state (see appendix **??**), which suggests that a distance-based instrument is unlikely to capture retailers' exposure to pass-through to wholesale prices.

A key assumption is that equation 27 separately identifies direct and indirect pass-through.

[70] Shift-share (or "Bartik") instruments are common in the empirical literature. For recent examples, see e.g. Hummels, Jørgensen, Munch, and Xiang (2014); Jaravel (2019); Xu (2022). I discuss and provide supportive evidence for instrument validity in appendix D.

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

[72] One could instead directly interact producer expenditure share, hike size, and producer bite as follows: $IB_{r,P,t-l} = \sum_{p=s}^{S} \alpha_{r,p} \Delta MW_{p,t-l} Bite_{k(p),t-l}$. Results are virtually identical under this definition of indirect bite. However, the advantage of averaging before interacting (as in equation 28) is that the coefficient ψ_l can be interpreted as a pass-through elasticity.

One way to test this is to examine whether the direct pass-through estimates $\hat{\beta}_l$ are affected by the inclusion of indirect bite as an additional variable. If estimates for direct pass-through were to change, this would cast doubt on the main identification strategy and, by extension, the results from the previous section. I show in appendix F that direct pass-through estimates are unaffected by the inclusion of indirect bite.

In equation 28, the indirect pass-through rate flows from the parameter ψ_l. For a given minimum wage hike, ψ_l measures the percent change in retailer r's prices resulting from a percentage point increase in indirect minimum wage exposure l months before the minimum wage hike. As with direct pass-through, indirect pass-through is best illustrated in terms of cumulative price level effects. Therefore, I again normalize the effect to zero in a baseline period m months before each hike and report the cumulative treatment effect as the sum of ψ_l at various lags: $E_L = \sum_{l=-m}^{L} \psi_l$. I report the pre-treatment coefficients in a similar manner, with $P_L = -\sum_{l=m}^{-L-1} \psi_{-l}$.

Figure J1a illustrates that the time path of indirect pass-through to retail prices is remarkably similar to the estimates of direct pass-through to wholesale prices from section 4. The figure reveals a downward-sloping pre-trend interrupted by an inflationary shock in the treatment period, followed by a continuation of the pre-trend into the post-treatment period. As in section 4.2, to quantify the pre-treatment trend I take the average of the distributed lag coefficients for the pre-baseline period, $\bar{\hat{\psi}}_{pre} = 1/4 \sum_{l=2}^{5} \hat{\psi}_{-l}$. I find no statistically significant difference between $\bar{\hat{\psi}}_{pre}$ and the bite-specific trend for pass-through to wholesale prices.[73] Accordingly, I apply the Goodman-Bacon (2021) procedure and re-estimate equation 28 with the dependent variable adjusted for the indirect bite-specific trend.[74]

Note that while it is informative to compare the time paths of indirect pass-through to retail prices and direct pass-through to wholesale prices, the implied pass-through elasticities are not directly comparable. The reason is that retail prices are higher than wholesale prices, meaning a given pass-through elasticity corresponds to a much larger absolute pass-through (in dollar terms) to retail prices than to wholesale prices. In contrast, direct and indirect pass-through to retail prices are directly comparable because they correspond to a single price level (i.e. the retail price level).

In Table **??**, I report cumulative effects of indirect pass-through to retail prices relative to the normalized baseline period two months prior to the hike. For the baseline specification, at the average indirect bite (18.14%), a 10% minimum wage hike corresponds to a 1.22% increase in retail prices two months after the hike.

[73] $\bar{\hat{\psi}}_{pre} = -.00102$ (90% confidence interval: -0.00329 to 0.00126), which overlaps with the pre-trend for pass-through to wholesale prices, $\bar{\hat{\beta}}_{pre} = -0.00197$ (90% confidence interval: -0.00367 to -0.00026). See appendix E for details.

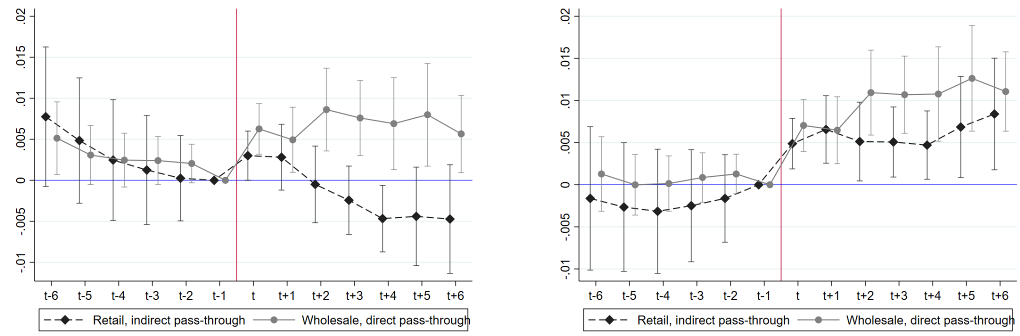
[74] Unlike in section 4.2, region-time FE cannot be used to control for the pre-trend since the dependent variable and indirect bite stem from different sets of establishments (retailers and producers, respectively). Moreover, as shown in appendix **??**, retailers purchase a large share of products from producers located in other regions of the state, meaning region-time FE based on a retailer's region will not capture the indirect bite-specific trend.

Table J2: Indirect pass-through of minimum wage hikes to the retail price level

	(1) Baseline	(2) No controls	(3) County FE	(4) Reg.-time FE	(5) Winsorized	(6) Outliers
E_0	0.00650**	0.00658**	0.00708***	0.00686***	0.00787**	0.00905**
	(0.00256)	(0.00256)	(0.00270)	(0.00263)	(0.00330)	(0.00383)
E_2	0.00675	0.00685	0.00798	0.00694	0.00912**	0.0122***
	(0.00454)	(0.00452)	(0.00505)	(0.00457)	(0.00438)	(0.00465)
E_4	0.00633*	0.00689*	0.00770*	0.00663*	0.00922*	0.0134**
	(0.00376)	(0.00378)	(0.00425)	(0.00382)	(0.00526)	(0.00621)
$\sum$ Pre-event	-1.94e-07	-0.000618	-0.000782	-0.000469	3.81e-08	-2.90e-08
	(0.00514)	(0.00514)	(0.00504)	(0.00521)	(0.00540)	(0.00582)
N	13,559	13,559	13,559	13,559	13,689	13,689
Time FE	YES	YES	YES	YES	YES	YES
Controls	YES	NO	YES	YES	YES	YES
County FE	NO	NO	YES	NO	NO	NO
Region-date FE	NO	NO	NO	YES	NO	NO
Trimmed	YES	YES	YES	YES	NO	NO
Winsorized	NO	NO	NO	NO	YES	NO
Trend-adjusted	YES	YES	YES	YES	YES	YES

Notes: The dependent variable is the establishment-level inflation rate for cannabis retailers, adjusted for a bite-specific trend as detailed in section **??**. The listed coefficients are the sum of the distributed lag coefficients E_L, L months after the minimum wage hikes, relative to the normalized baseline period in $t-2$. The distributed lag coefficients are estimated from equation **??**. The baseline specification in (1) includes as controls the monthly unemployment rate and monthly average wage, both at the county level. (2) excludes controls. (3) controls for county-level price trends. (4) includes region-time FE but not county FE. (5) uses a winsorized outcome (99% winsorization). (6) does not trim or winsorize the outcome. 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.

Fig. J1. Indirect pass-through to retail prices and direct pass-through to wholesale prices



(a) Unadjusted

(b) Trend-adjusted

Notes: The figures compare direct pass-through to the wholesale price level and indirect pass-through to the retail price level. Wholesale price effects are estimated from equation **??** with time fixed effects. Indirect retail price effects are estimated from equation **??** with time fixed effects and county-level controls. The estimated coefficients β_l and ψ_l, respectively, are summed up to cumulative effects E_L, relative to the normalized baseline period in $t-1$. Panel (a) shows estimated effects when the dependent variable is not adjusted for a pre-trend. Panel (b) shows estimated effects when the dependent variable is trend-adjusted following the Goodman-Bacon (2021) procedure described in section **??**. Both figures show 90% confidence intervals of the sums E_L based on SE clustered at the county level. Data source: Top Shelf Data and Washington ESD, July 2018-August 2021.