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Minimum Wage Pass-through to Wholesale and Retail Prices: Evidence from the Washington State Cannabis Industry

Carl Hase¹

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Abstract

A growing empirical literature finds that firms pass the cost of minimum wage hikes onto consumers via retail prices. Yet, little is known about minimum wage effects on wholesale prices and whether retailers face a wholesale cost shock in addition to the labor cost shock. I exploit the unique market structure of Washington state's legal recreational cannabis industry to investigate minimum wage pass-through to wholesale and retail prices. In a dynamic difference-in-differences framework, 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 rates consistent with existing literature—yet retail pass-through rates more than double once wholesale cost effects are accounted for. Retail markups do not adjust to wholesale pass-through, indicating a full transmission of wholesale cost shocks to retail prices. The results suggest that previous research may underestimate the impact of minimum wage increases on retail prices. This paper highlights the importance of analyzing the entire supply chain when evaluating the product market effects of minimum wage hikes.

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

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1 Introduction

Minimum wage laws are a hugely popular tool for combating poverty and reducing economic inequality.¹ Yet, 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 largely depends on how firms react to the labor cost shock induced by the minimum wage.² If firms reduce employment or non-wage compensation (e.g. vacation days or health benefits) for low-wage workers, then low-wage workers bear the brunt of the policy. If firms absorb the cost shock by reducing profits, then firms bear the cost of adjustment. Finally, firms may pass the labor cost shock on to consumers in the form of higher retail prices, in which case consumers pay for the minimum wage increase. Of these three margins of adjustment, the first has received the lion’s share of attention, and evidence on employment effects is conflicted.³ The second channel has received less attention, but existing findings point to small profit effects (Draca, Machin, & Reenen, 2011; Harasztosi & Lindner, 2019). Instead, the third channel seems to play a key role. With the aid of high-frequency price scanner data, a small but growing empirical literature finds that firms fully pass the cost shock through to retail prices, implying that nominal wage increases from minimum wage hikes are partly offset by increases in the prices of goods and services (Leung, 2021; Renkin, Montialoux, & Siegenthaler, 2022). While price scanner data exhibits unparalleled richness, however, it is largely confined to retail outlets like grocery, merchandise, and drug stores.⁴ As a result, less is known about minimum wage pass-through to prices in other sectors. Yet, scanner data carries an additional shortcoming in that it only conveys information on prices at the final point of the supply chain. In principle, minimum wage hikes may affect not only retail outlets, but firms higher up the supply chain as well. If suppliers’ increased 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 the higher wholesale prices from suppliers—an indirect effect. To the extent that retailers pass both cost shocks on to consumers, retail price adjustment reflects both *direct pass-through* and *indirect pass-through*. The latter may even eclipse the former since, in many retail settings, the cost of goods sold (COGS) accounts for over 80% of retailers’ costs, 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). Crucially, retail scanner data cannot distinguish between these two forms of pass-through because the data only captures point of sale prices. Moreover, reduced form

¹Approximately 90 percent of countries worldwide have instituted some form of minimum wage (International Labour Organization, 2021).

²Recent evidence suggests that in some settings, workers’ reactions to the minimum wage may also be important (see e.g. Ku (2022)).

³See Neumark (2019) for a recent overview.

⁴One way to overcome this limitation is to use internet-based pricing. Allegretto and Reich (2018), for example, exploit internet-based restaurant menus to study the effects of a 25% minimum wage increase in San Jose, California in 2013. They find an average price increase of 1.45%, indicating that most of the cost increase was passed on to consumers.

regressions using retail scanner data only reveal the full (i.e. direct and indirect) pass-through to retail prices in the special case that retailers purchase predominantly from local wholesalers. However, if wholesale goods are highly tradeable (as with e.g. drugstores and general merchandise stores), then indirect pass-through to retail prices is absorbed by time fixed effects (Renkin et al., 2022). In that case, estimates from retail scanner data only pick up direct pass-through effects, and hence, fail to capture the full effect of minimum wage hikes on retail prices. To assess the 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 prices in Washington state’s legal recreational cannabis industry. Washington’s cannabis market is an ideal laboratory for studying minimum wage pass-through for several reasons. Cannabis is one of the largest agricultural industries in the state and a major source of employment. The industry is labor-intensive and low-wage at all points of the supply chain, meaning that minimum wage hikes likely induce a sizeable cost shock for wholesalers and retailers alike.⁵ In other markets, the distinction between wholesaler and retailer is often blurred by vertical integration, making it difficult to distinguish between pass-through at different points of the supply chain. In contrast, vertical integration is strictly prohibited for cannabis businesses, creating clearly defined vertical relationships between wholesalers and retailers. Importantly, the cannabis industry operates under autarky, meaning wholesale and retail establishments are subject to the very same minimum wage hikes. This makes it possible to distinguish between direct and indirect pass-through to retail prices, but it also narrows the set of possible confounders by eliminating the influence of labor and product market shocks in other regions. Finally, unusually 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.⁶

To estimate minimum wage pass-through elasticities, I use a dynamic difference-in-differences framework and exploit geographic variation in minimum wage exposure for 1,192 wholesale and retail establishments over six minimum wage hikes between 2018 and 2021. I use scanner-level data on \$6 billion of wholesale and retail transactions to estimate pass-through to both wholesale and retail prices. Unlike previous studies that use retail scanner data, I also observe prices and quantities for the universe of retailers’ wholesale purchases, which enables me to separately identify direct and indirect pass-through to retail prices. When ignoring wholesale cost effects, I find that a 10% increase in the minimum wage translates into a 0.7% increase in retail prices, consistent with existing literature (see e.g. Leung (2021)). Yet, I also find that a 10% increase in the minimum wage corresponds to a 1.79% increase in wholesale prices. Crucially, once wholesale pass-through is accounted for, the estimated retail pass-through elasticity more than doubles to

⁵In this paper, I use the term “wholesaler”, “producer”, and “producer-processor” interchangeably. This reflects the legal structure of the cannabis market and is described in more detail in section 2.

⁶The data also bypasses reliability issues associated with internal firm prices. For example, Hong and Li (2017) argues that intrafirm prices may be vulnerable to accounting fictions for tax avoidance or record-keeping purposes.

1.92%. The substantial increase in retail pass-through reflects large indirect pass-through relative to direct pass-through effects (elasticities of 1.22% versus 0.7%), and the elasticities 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) indicates that studies that focus solely on retail prices may be incomplete in a significant way.

Two further findings indicate a full pass-through of the wholesale cost shock to retail prices. First, there is no statistically significant difference between indirect retail pass-through estimates and wholesale pass-through estimates. Second, retailers do not adjust markups to wholesale pass-through (instead, changes in retail markups entirely reflect direct pass-through). I also document substantial heterogeneity in pass-through to wholesale prices: wholesale pass-through elasticities decrease with the scale of production—and are zero for the largest wholesalers—which suggests that large wholesalers may adjust to the labor cost shock along other margins. I find no evidence of employment effects for retailers or wholesalers.

Taken together, these 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 poverty-reduction tool.

I make three main contributions in this paper. First, I provide evidence that minimum wages affect retail *and* wholesale prices. To the best of my knowledge, this has not been previously studied in the literature. Importantly, this implies that retailers face a direct and an indirect cost shock from minimum wage hikes. Second, I investigate whether wholesale pass-through is itself passed on to retail prices (i.e. indirect pass-through). Studies that fail to distinguish between direct and indirect pass-through will only capture the full effect of minimum wage hikes on retail prices in the special case that retailers predominantly purchase from local wholesalers. Renkin et al. (2022), for example, show that U.S. grocery stores source mostly from wholesalers located in the same state, and hence, difference-in-difference estimates of the effect of state minimum wage hikes on grocery prices likely capture both direct and indirect pass-through. However, in industries with highly tradeable goods (e.g. drugstores and general merchandise stores), reduced form regressions only capture direct pass-through and thus underestimate the true impact of minimum wages on retail prices, and hence, real wages. Third, the literature on minimum wage pass-through is underdeveloped and heavily centered 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); Harasztosi and Lindner (2019)). Most closely related is the work 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 a difference-in-differences framework 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 subsector level. Since cannabis retailers and wholesalers belong to different industry subsectors, 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 of wholesale pass-through but cannot test for it because their data does not include information on wholesale cost. Instead, they calculate an upper bound for wholesale pass-through using input-output tables under the assumption of full pass-through. In contrast, I directly observe wholesale cost because my data contains prices and quantities for the universe of retailers' wholesale transactions. I leverage this information to construct a measure of each retailer's exposure to wholesale pass-through, and I estimate indirect pass-through to retail prices using a reduced-form approach. By estimating the effects of wholesale pass-through on retail markups, I provide empirical support for Renkin et al.'s (2022) assumption of full wholesale cost pass through. Nevertheless, while Renkin et al. (2022) conclude that indirect pass-through may be similar in magnitude to direct pass-through to retail prices, I find indirect pass-through elasticities that are twice as large as direct pass-through elasticities, which reflects comparatively large wholesale pass-through effects in the cannabis market.

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).⁷ 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 Washington state's 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 Washing-

⁷ 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.

ton’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 administrative 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.

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 and discusses robustness checks. Section 5 investigates indirect pass-through to retail prices. Section 6 further dissects pass-through by examining markups over marginal input cost and price effect heterogeneity. Section 7 concludes.

2 Institutional context

This section gives an overview of the labor market characteristics and market structure of Washington’s cannabis industry, as well as institutional details on the minimum wage in Washington state.

2.1 The cannabis industry in Washington state

In November 2012, voters in Washington state approved the creation of a legal recreational marijuana market for adults 21 years and older.⁸ 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 the fourth most valuable agricultural crop in the state behind apples, wheat, and potatoes but ahead of timber, cherries, and hay (Nadreau, Fortenbery, & Mick, 2020).

Cannabis labor

Several features of cannabis labor make the industry particularly well-suited for investigating the effects of minimum wage hikes. First, cannabis is an important source of employment, and the sector supports approximately 18,700 full-time equivalent (FTE) jobs in Washington (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).⁹ Second, cannabis is particularly labor intensive. 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

⁸Cannabis 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 the state.

⁹To add perspective, there are more cannabis workers than hair stylists, barbers, and cosmetologists combined (Barcott et al., 2022).

a higher price point (Jiang & Miller, 2022). Cannabis is labor intensive at the retail level as well. Inside each store, a service counter forms a physical barrier between customers and the products, and customers can only make a purchase with the help of a sales representative known as a ‘budtender’. Budtenders service one customer at a time, and since a consultation can take several minutes depending on a customer’s needs, most retailers employ several budtenders per shift to boost sales volume. Third, 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 producers 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. Retail budtending requires no formal training and the job resembles low-skilled retail employment in other industries. As a result, a high degree of minimum wage exposure is likely at cannabis establishments at all points of the supply chain. Appendix G describes labor and wages in cannabis in further detail.

The cannabis market structure

A defining feature of Washington state’s cannabis industry is its unique market structure. The industry is regulated by the Washington State Liquor and Cannabis Board (LCB) which offers three separate licenses for cannabis businesses, each representing a different stage of the supply chain. The first license is for producers and it allows an establishment to cultivate, harvest, trim, dry, cure, and package cannabis to be sold at wholesale to other licensed producers and processors. The second license is for processors and it permits an establishment to process, dry, cure, and package cannabis to be sold at wholesale to other licensed processors or retailers. While there is some overlap between the producer and the processor licenses, the key distinction is that processors cannot cultivate plants and producers cannot sell to retailers. The third license is for retailers; they are permitted to sell usable cannabis products in retail stores. A key stipulation is that producer and processor licenses can be held simultaneously but retailers cannot obtain either producer or processor licenses.¹⁰ As a result, the vast majority of upstream establishments own both producer and processor licenses and are commonly referred to as ‘producer-processors’. Importantly, producer-processors may only sell cannabis to licensed retailers—they cannot sell directly to consumers. Retailers, moreover, can only sell to consumers. This creates a complete vertical separation between producer-processors on the one hand, and retailers on the other. Figure 2 illustrates the different stages of cannabis production and their relation to cannabis licensing.

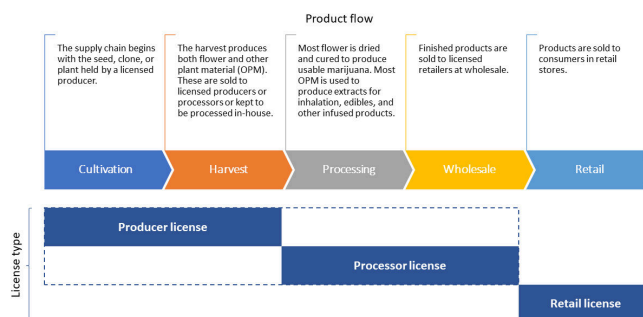
Another feature of the cannabis market is that it operates under autarky. That is, retailers can only buy from producer-processors located in Washington state, and producer-processors can only sell to retailers in the state. This ‘seals off’ the core of the supply chain

¹⁰Such ‘tied-house’ rules are a remnant of the early days of U.S. alcohol regulation. They were imposed by states to limit the market power of brewers and distillers and prevent monopolies from preying on consumers’ “worst habits” (Wallach, 2014). Washington state lawmakers adopted similar rules out of an abundance of caution and to increase the likelihood of legalization passing the state legislature (Wallach, 2014).

from nearby states with legal recreational markets such as Oregon or California.¹¹ As a result, the entire supply chain is subject to the same minimum wage hikes, and hence, the same labor cost shocks.

Before moving on, it is worth noting several points. First, since the number of establishments with only a processor license (as opposed to a producer-processor license) is very small, I drop these from my analysis. In contrast, I keep establishments with only a producer license since these belong to the same industrial classification as producer-processors (see section 3). Second, I use the term ‘producer-processor’ and ‘wholesaler’ interchangeably throughout the paper to refer to upstream establishments. This reflects the dual role played by these establishments in the cannabis market: besides being producers, they also act as wholesalers when viewed from the perspective of retailers. Third, since wholesalers occupy the upstream portion of the supply chain, I assume that the minimum wage only induces a labor cost shock for wholesalers—that is, the minimum wage does not affect material input prices for these firms. In principle, this assumption may not hold entirely and wholesalers may be subject to minimum wage pass-through from their input suppliers. However, wholesale 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 wholesale 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.

Fig. 1. The supply chain in the Washington state cannabis market



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, wholesale (i.e. producer-processors) and retail activities are legally separated.

¹¹The supply chain is not 100 percent sealed off, since consumers from other states can travel to Washington to purchase cannabis at retail stores, and wholesalers can purchase certain inputs such as grow lights, soil, and fertilizers from businesses in other states.

2.2 The minimum wage in Washington state

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 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.¹² 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. However, in a series of robustness checks, I also 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, I drop Seattle establishments from the sample for the 2021 hike and find that results are unaffected (see appendix D for details).¹³ For both Seattle and Tacoma, the citywide hikes occurred on the same day of the year as the statewide hikes (January 1st). Figure 2 summarizes the minimum wage hikes used in my main analysis.

3 Data and empirical strategy

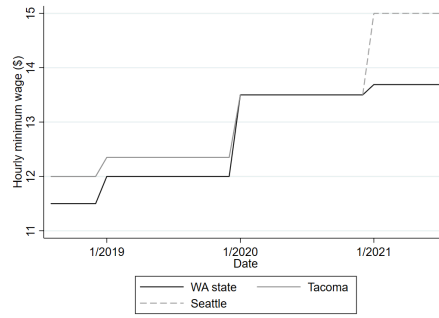
3.1 Price data

To monitor developments in the cannabis market, legalization came with stringent data reporting and sharing requirements for all licensed cannabis businesses. Producer-processors (i.e. wholesalers) and retailers are required to track every step of production from 'seed to sale' and they must regularly upload data feeds about plants, harvests, processing, transfers between businesses, and retail sales 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

¹²Firms 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.

¹³I also consider potential wage spillovers from Seattle to surrounding areas, in which case estimates for establishments in neighboring cities would also suffer from reverse causality in 2021. See appendix D for details.

Fig. 2. 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 figure 14).

of that very same product sold at the retail level.¹⁴ The LCB switched providers for its traceability system in October 2017 and again in December 2021, creating two structural breaks in the price data. My sample period lies between these breaks and spans August 2018 through July 2021, a period that covers three statewide and three citywide minimum wage hikes. I obtained the data from Top Shelf Data, a data analytic firm that ingests the raw tracking data from the LCB and matches it with additional product information. The estimation sample covers sales from 1,192 distinct retailers and wholesalers and contains an industry-wide average of 31,800 unique retail products and 18,268 unique wholesale products per month (see table 1). To give an example, a 1.0 gram package and a 2.0 gram package of Sunset Sherbert usable marijuana (flower) produced by Northwest Harvesting Co are treated as different products in the data.¹⁵ The LCB classifies products as belonging to one of 12 categories. As table 2 illustrates, usable marijuana (dried flower) and concentrate for inhalation account for more than 80 % of all retail sales.¹⁶ 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. Retailers are located in 37 counties while wholesalers are located in 35 counties in Washington state. Due to restrictions on the num-

¹⁴Compliance with seed-to-sale traceability is strictly enforced by the LCB. When a business is issued a violation, it can receive a fine, a temporary license suspension, or both. In cases of repeated violations, a license can be revoked by the LCB board. Given such strict enforcement, violations are uncommon. In 2021 for example, the LCB issued 66 violations among approximately 2000 licensees. See: <https://lcb.wa.gov/enforcement/violations-and-due-process>

¹⁵Similar 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.

¹⁶Concentrate for inhalation includes both solid cannabinoid concentrates like “wax” or “crumble” as well as liquid vape cartridges.

ber of licenses a firm can hold, the vast majority of retail and wholesale establishments are standalone firms. Over the entire sample period, the data contain \$4.47 and \$1.46 billion in retail and wholesale sales, respectively.

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*

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. The wholesale sample excludes establishments with a processor-only license. Sales between producer-processors are included. Data source: Top Shelf Data.

⁺ For wholesalers, 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.

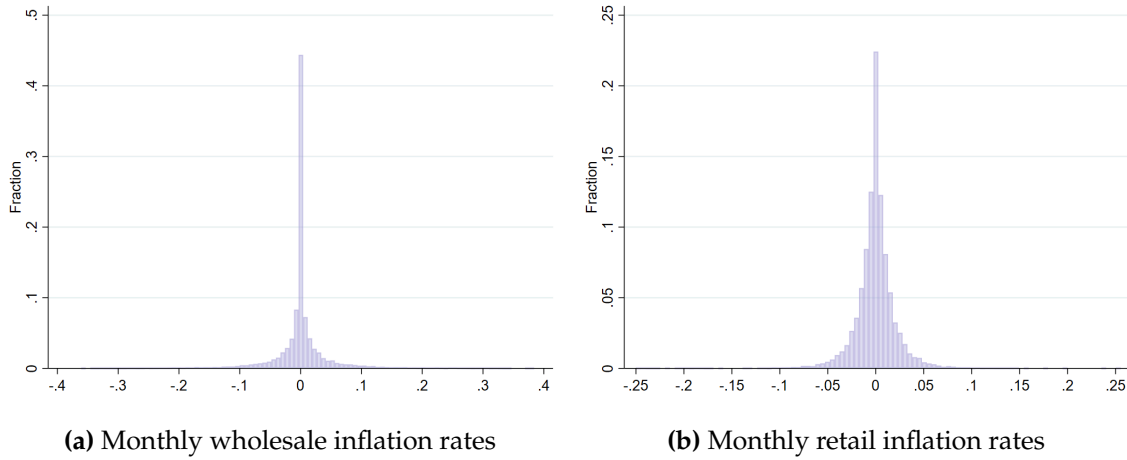
My empirical approach closely follows previous research on minimum wage pass-through to prices (e.g. Renkin et al. (2022), Leung (2021)). The dependent variable is the natural logarithm of the monthly establishment-level price index:

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

$\pi_{j,t}$ 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 .¹⁷ 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

¹⁷As pointed out by Renkin 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.

Fig. 3. Establishment-level inflation rates for cannabis, August 2018-July 2021



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

Table 2: 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 shows market shares for the product categories defined by the LCB. Market shares are calculated using the estimation sample which runs from August 2018 through July 2021. "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.

robust to keeping outliers). I describe the establishment-level price index in more detail in Appendix A.

An establishment-level price index is common in the minimum wage pass-through literature as it carries several advantages over a product-level index. As argued by Renkin et al. (2022), the establishment is a natural unit of analysis since wages are paid at the establishment level. In addition, an establishment-level price index allows the researcher to weight products by their importance for each establishment. Finally, entry and exit occurs at a much higher frequency for products compared to establishments, particularly in the cannabis industry where a product-level time series would contain frequent gaps. Since the vast majority of cannabis businesses have succeeded at staying in business, the establishment-level panel is much more balanced.

3.2 Wage data

My identification strategy rests on the idea that minimum wage hikes affect establishments with a high share of minimum wage workers more than those with a low share. Since wages are not observable at the establishment level, I follow previous studies and use geographic variation in the minimum wage bite as a proxy (see e.g. Card (1992)). I define bite as the share of FTE workers in a county-subsector earning below the new minimum wage two quarters prior to the hike. The subsectors are based on the North American Industrial Classification System (NAICS) which explicitly spells out classification for cannabis establishments of various types. NAICS subsector 453 ("Miscellaneous store retailers") captures all cannabis retailers since NAICS 453998, a component of that subsector, includes "All Other Miscellaneous Store Retailers (except Tobacco Stores), including Marijuana Stores, Medicinal and Recreational" (US Census Bureau, 2017). At the wholesale level, 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, 2017). 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). However, NAICS classifies an establishment based on its primary activity, meaning that a wholesaler only belongs to NAICS 424 if its revenue from processing activities exceeds that of its own crop production (US Census Bureau, 2017). Table 2 shows that unprocessed "Usable Marijuana" accounts for the majority of revenue for producer-processors, indicating that producer-processors belong to NAICS 111.¹⁸ Note that since very few establishments have only a processor license, I drop these from my sample.

¹⁸Jiang 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 wholesale cannabis licenses. Moreover, the state saw a proportional increase in the number of workers in the sector and the total wages paid (Jiang & Miller, 2022).

In Appendix D, I construct an alternative bite variable at the five-digit NAICS level and show that my results do not depend on the chosen level of industrial classification. Still, bite at the three-digit subsector 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 cannot observe whether a given producer-processor grows indoors or outdoors, I must assume that all establishments are either indoor or outdoor growers, which induces measurement error. In contrast, the three-digit NAICS subsector 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. I discuss these issues in more detail in appendix B.

By defining bite at the level of the three-digit industry subsector, I assume that variation in wages at cannabis establishments resembles variation in the corresponding industry subsectors. I provide several facts to support this assumption. First, in Appendix G I show that average wages for cannabis retailers and wholesalers are very similar to those in the corresponding NAICS subsectors.¹⁹ Moreover, for both the cannabis industry and the NAICS subsectors, average wages are remarkably close to the wage floor imposed by the minimum wage.²⁰ Thus, to the extent that the wage distributions differ between cannabis establishments and their NAICS subsectors, 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 local labor market conditions (county-level average wage and unemployment) as well as county or establishment fixed effects. This implies that any remaining measurement error is likely to be random, and hence 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. I view such a scenario as unlikely.

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).²¹ A similar dataset has been used in the recent literature on the labor market effects of minimum wages (see e.g. Dube, Lester, and Reich (2016); Leung (2021); Renkin et al. (2022)). While the treatment intensity varies across time and space in my sample, the timing of the treatment does not vary (i.e. no staggered treatment). Thus, the six minimum wage hikes (three citywide and three statewide hikes) in the sam-

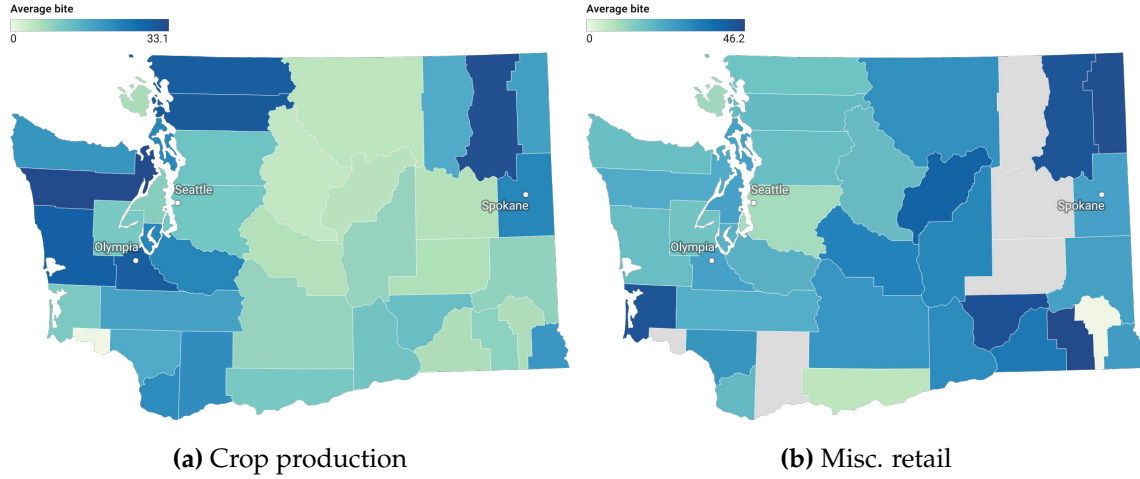
¹⁹I also find that average wages are homogenous among the industries contained in the relevant NAICS subsectors.

²⁰For wholesalers, the gross wage is between 5%-10% of the minimum wage, while for retailers it ranges from 15%-19% for the years 2018 to 2020. See appendix G.

²¹The 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.

ple period amount to three evenly spaced minimum wage events (each 12 months apart). Figure 4 depicts the average bite in the sample period for the NAICS subsectors containing cannabis establishments, by county. The figure shows substantial heterogeneity in bite across counties but also across the two subsectors.

Fig. 4. Average minimum wage bite, 2018-2021



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 subsector that includes cannabis wholesalers. The panel on the right shows bite for miscellaneous store retailers (NAICS 453), the industry subsector 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.

4 Direct minimum wage pass-through

4.1 Main identification strategy

My identification strategy is based on the idea that, conditional on a set of controls and fixed effects, inflation in establishments with less minimum wage exposure is a useful counterfactual for inflation in establishments with more exposure.²² 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. Since the high frequency of the price data allows me to capture such dynamics, I specify a distributed lag model with leads and lags before and after each hike. I estimate the following equation

²²Callaway, Goodman-Bacon, and Sant’anna (2021) show that with continuous treatment intensities like the minimum wage bite, the DiD estimator equals a weighted average of the 2×2 DiD comparisons between pairs of treatment intensities, scaled by the differences in the intensities.

separately for retailers and wholesalers:

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

Equation 2 relates the monthly establishment-level inflation rate, $\pi_{j,t}$, to the treatment intensity in county k , which is defined as the interaction between the percent change in the minimum wage applicable to establishment j , $\Delta MW_{j,t-l}$, and the minimum wage bite in the county k that establishment j is located in, $Bite_{k(j),t-l}$.²³ The vector of control variables, $X_{k(j),q(t)}$, contains the average wage and unemployment rate for county k in the quarter q of month t . I include these to absorb variation in cannabis prices related to macroeconomic factors that may covary with the minimum wage bite. County fixed effects θ_k absorb county trends in cannabis prices, while time fixed effects account for industry-wide changes in cannabis prices. 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 hike, the parameter β_l measures the percent change in establishment j 's prices resulting from a percentage point increase in minimum wage exposure l months before the minimum wage hike. Though inflation is the dependent variable, I follow Renkin et al. (2022) and present the estimates as the effect of the minimum wage on the price level. 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 $P_L = -\sum_{l=m}^{L-1} \beta_{-l}$.²⁴

By interacting the minimum wage hike with county-level bite, my approach resembles strategies that attempt to identify effects of aggregate shocks through cross-sectional variation in the fraction affected (see e.g. Bartik (1991); Goldsmith-Pinkham, Sorkin, and Swift (2020); Lucca, Nadauld, and Shen (2019)). This strategy is useful in the context of Washington's cannabis market as it enables me to estimate pass-through despite the relatively small number of minimum wage hikes. Another advantage is that the estimated price level effects E_L can be reformulated as pass-through elasticities at the average bite, allowing direct comparison to elasticities found in the literature on minimum wage pass-through (e.g. Leung (2021); Renkin et al. (2022)).

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 in the sample period, meaning event dummies get highly collinear when l is large. Moreover, to the extent that bite may correlate within a county across time, disentangling the effects of adjacent hikes becomes difficult when events overlap. Another issue is that the establishment

²³For establishments subject to a citywide minimum wage, $\Delta MW_{j,t-l}$ corresponds to the citywide hike.

²⁴Schmidheiny and Siegloch (2023) show that cumulative 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. Thus, a 12 month event window requires estimating 11 distributed lag coefficients.

panel is not balanced, meaning that changes in the underlying sample may affect estimates when l is large (Renkin et al., 2022). In light of these issues, I opt for a non-overlapping event window beginning six months prior and ending six months after each hike. This implicitly assumes that treatment effects do not change more than six months before or after an event. While this may seem restrictive, there is little reason to expect firms to respond to hikes more than six months before or after a minimum wage hike. Indeed, several studies find that firms adjust prices at most three months prior to an event and that effects plateau within 1-2 months after the hike (Leung, 2021; Renkin et al., 2022). Thus, restricting the effect window to a 12 month period around the hike should adequately capture the short-run impact of the minimum wage on prices.

A central concern with this research design is possible reverse causality. Since the treatment intensity is the product of two variables, $\Delta MW_{j,t-l} \times Bite_{k(j),t-l}$, the potential for reverse causality must be addressed for each of these variables in turn. $\Delta MW_{j,t-l}$ suffers from reverse causality if policymakers increase the minimum wage in response to local inflation (e.g. in an effort to keep real wages constant). This is clearly not the case with the statewide hikes in my sample, since they are either predetermined or linked to the CPI-W, a national—not local—price index.²⁵ $Bite_{k(j),t-l}$ suffers from reverse causality if county-level inflation drives wages. To account for this possibility, I include county fixed effects to absorb county-level differences in trend inflation. Moreover, the distributed lag specification allows me to closely examine effect timing. Thus, to the extent that differences in inflation trends remain, these can be easily distinguished from treatment effects.

It is important to highlight that, 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 must be met for the direct pass-through estimates to be contaminated by indirect pass-through. First, retailers must purchase predominantly from wholesalers located in the retailer’s own county. Second, the bite variable for retailers must correlate with bite for wholesalers within each county.²⁶ In appendix H, I show that the first condition does not hold since over 85% of retailers’ wholesale purchases are from wholesalers located in other counties. The second condition also does not hold since the within-county correlation coefficient for retail and wholesale bite is 0.33, which suggests a weak relationship between wholesale and retail bites.

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

²⁵The 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 E and show that the main results are robust to accounting for such endogeneity.

²⁶Importantly, both conditions must hold for the direct pass-through estimates to be contaminated by indirect pass-through effects. 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 wholesale 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 wholesalers.

hike actually takes effect.²⁷ 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 H, for both wholesale and retail prices I find no evidence of price effects at announcement but large effects at implementation. This indicates that cannabis establishments wait until the cost shock hits before adjusting prices even if they have full prior information on the magnitude of the shock.

4.2 Pass-through to wholesale prices

I begin by estimating the effect of minimum wage hikes on wholesale prices with time FE and county FE but no county controls. A central question regarding the wholesale estimates is whether to control for a treatment-specific pre-trend, since the baseline specification reveals a clear negative trend in the pre-treatment period. Nevertheless, 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 then undone in subsequent periods as the pre-trend continues into the post-treatment period. Thus, while the trend does not mask the impact factor itself, failure to account for the trend changes the interpretation of the results over a longer time horizon. In appendix C I show that the pre-trend is entirely driven by event 2, a period corresponding to a wholesale supply glut and falling wholesale prices across the industry. It is therefore plausible that for event 2 unobserved confounders covary with treatment intensity and wholesale cannabis deflation. The trend persists despite the inclusion of county FE because county means are based on all three events and the trend is only present for a single event.

Given the statistical significance of the pre-trend, coupled with the sharp inflationary treatment effect in period t , I apply two common strategies to control for the pre-trend, both of which yield similar results. First, I distinguish between the three major socioeconomic regions in Washington state, where each region includes a subset of counties.²⁸ To the extent that unobserved time-variant heterogeneity is common within these regions, region-time FE (i.e. interactions between time and region dummy variables) will control for the treatment-specific trend (Neumark, Salas, & Wascher, 2014).²⁹ This assumes that the proper counterfactual for inflation in counties with high treatment intensity is inflation in counties with low treatment intensity *located in the same region*; that is, the identifying information comes from within-region variation in the treatment intensity. I view this as

²⁷Renkin 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.

²⁸The cascade mountain range and an arid shrub-steppe create three distinct socioeconomic regions in Washington state (West, Central, and East). See Appendix I for details.

²⁹Unlike the baseline specification, I include county controls to further capture time-varying heterogeneity, and I exclude county FE since county means are calculated using all three events rather than just the event with the pre-trend (event 2). Results are robust to including both county FE and controls or neither.

a valid assumption given the stark economic, social, and political differences across the regions.³⁰ Moreover, the three regions are visible in terms of average bite in figure 4 (panel a). Second, I apply the two-step procedure from ? 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 remove the linear trend from the original dependent variable to get the trend-adjusted variable $\tilde{\pi}_{j,t(e)} = \pi_{j,t} - \hat{\pi}_{j,t}$. As argued by Rambachan and Roth (2023), this form of trend-adjustment 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 (-0.00150, p-value: 0.128) is nearly identical to—and not statistically significantly different from—the pre-treatment trend (-0.00156, p-value: 0.074).

Figure 5a illustrates that for all three specifications (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. Figure 5b shows the corresponding cumulative price level effects from the three specifications. With the trend-adjusted dependent variable, the pre-treatment period shows no significant trend, while the large contemporaneous inflationary effect carries over from the unadjusted regression. At the average bite (18.63%), a 10% increase in the minimum wage corresponds to a 1.03% increase in wholesale prices in period t . Moreover, with the trend-adjusted regression the treatment effect is no longer undone by the continuation of the pre-trend into the post-treatment period. In appendix C, I show that this holds even if I only adjust the dependent variable for event 2 (the only event with a treatment-specific trend) and leave events 1 and 3 unadjusted. The region-time FE specification yields nearly identical results, with a flat pre-trend, a period t pass-through elasticity of 1.06%, and a permanently higher price level.³¹ 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.

4.3 Pass-through to retail prices

Having obtained estimates for wholesalers, I next estimate equation 2 for retail establishments. Figure 6 illustrates that the effects for retailers differ from those of wholesalers in several respects. First, effects for retailers show no significant pre-trend.³² Second, the treatment effect appears in $t - 2$, i.e. one period prior to that for wholesalers, suggesting that retailers may be more forward-looking in their pricing than wholesalers.³³ Given the

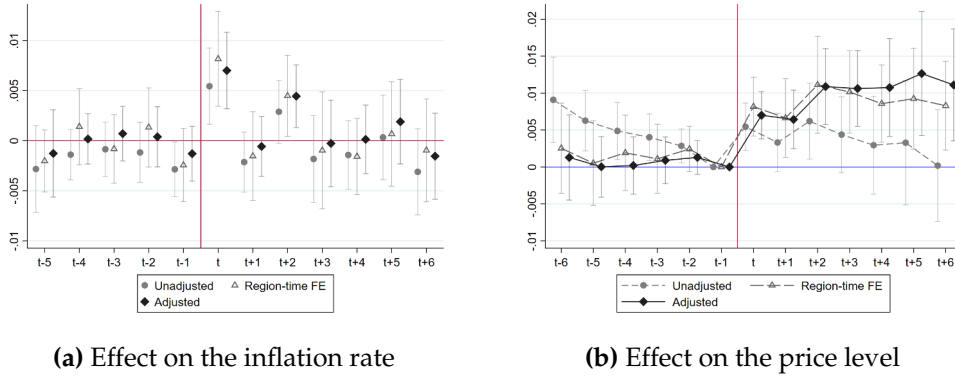
³⁰There is a long history of attempts by the state legislature to split Washington into several states more representative of local socioeconomic conditions, including in 1915, 1985, 1991, 2005, 2015, and most recently in 2017 (Hallenberg, 2017).

³¹At 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.

³²A slight positive pre-trend is visible in figure 6 but it is insignificant. I show in Appendix C that results are robust to adjusting for this pre-trend.

³³This 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. Though wholesalers’ market

Fig. 5. The effect of minimum wage hikes on wholesale prices



Notes: The figures show estimates from equation 2 under three different specifications: unadjusted, trend-adjusted, and region-time FE. The dependent variable is the establishment-level inflation rate. 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) 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 the main text. 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, July 2018 to August 2021.

earlier treatment effect, I normalize the baseline period in $t - 2$ when calculating cumulative effects on retail prices. For retailers at the average bite (17.52%), a 10% increase in the minimum wage corresponds to a 0.54% jump in prices in period t . Thus, the treatment effect in period t —while still large—is about half the size of that for wholesalers. I analyze the relative magnitudes of wholesale and retail pass-through in more detail in section 5.

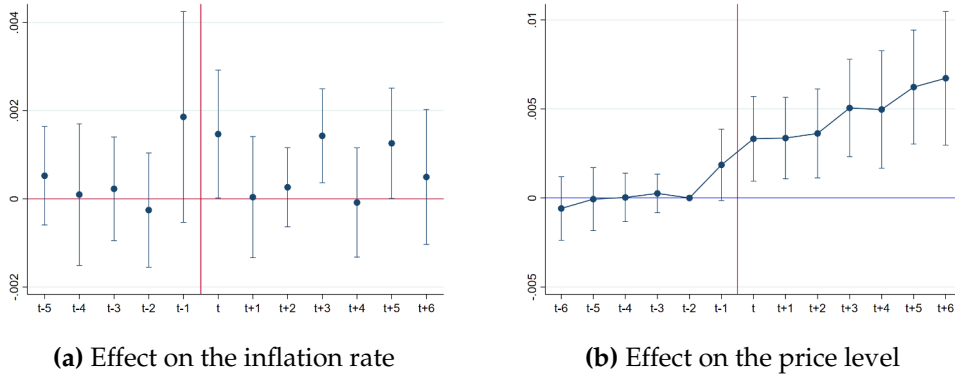
4.4 Robustness checks

Alternative specifications

The results from the previous subsection stand up to a multitude of robustness checks. In table 3, I present several variants of my empirical strategy for wholesalers. I use the linear trend-adjustment as my preferred specification as this enables direct comparison to the indirect pass-through estimates in section 6. Moreover, I normalize the baseline period in $t - 2$ so that cumulative wholesale and retail results line up temporally. Note that changing the baseline period has no bearing on the estimated coefficients from equation 2 and simply amounts to a (downward) level shift in cumulative wholesale price level effects. For the baseline specification (column 1), I estimate equation 2 with time and county fixed effects but no controls. Column 2 shows that the estimated effects are virtually identical when including county-level controls. Column 3 shows that effects increase when county FE are omitted. Effect sizes are not affected by winsorizing (column 4) or including outliers (column 5), but standard errors tend to be larger in both cases compared to the baseline

power has not been formally investigated in the literature, a common complaint among wholesalers is their lack of market power compared to retailers (Barbagallo, 2021; Schaneman, 2021; Washington State Liquor and Cannabis Board, 2021).

Fig. 6. The effect of minimum wage hikes on retail prices



Notes: The figures show direct minimum wage pass-through to retail prices. Estimates are from equation 2 with time fixed effects and county-level controls. The dependent variable is the establishment-level inflation rate. 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) 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.

specification.³⁴ Column 6 shows price level effects when region-time FE are used to control for the pre-trend rather than the linear trend adjustment. I omit county FE from column 6 since they are based on all three events and the pre-trend is only present for a single event (event 2). Instead, I include time-variant county controls as these should capture additional confounding differences in price trends during event 2.³⁵

As table 4 illustrates, retail price level effects are similarly stable across specifications. Column 2 shows that effect sizes do not depend on the inclusion of county controls; columns 3 and 4 show similar effect sizes with county FE and region-time FE, respectively. As with the wholesale regressions, retail price effects are not affected by winsorizing (column 5) or including outliers (column 6), though standard errors tend to be larger.

Further robustness checks

Besides testing different specifications, it is also important to consider the institutional assumptions underlying identification in my research design. In this section, I discuss these assumptions and the implications of them being violated. I report results from these additional robustness checks in appendix D.

First, since Washington's primary crop harvesting season is in Q3—the same quarter that the bite variable is calculated—it is important to ensure that seasonal labor fluctuations do not cause endogeneity in the bite variable. Therefore, I check whether results change if the bite variable is based on Q4 wages, i.e. outside of the main harvesting season. As tables 15 and 16 illustrate, results are robust to using this alternative bite variable.

³⁴Recall that I trim the top and bottom 0.5% of inflation per month in the baseline specification.

³⁵With the region-time FE specification, results are robust to including county FE, omitting controls, and a variety of other specifications. See Appendix D.

Table 3: Wholesale price level effects of minimum wage hikes

	(1) Baseline	(2) Controls	(3) No county FE	(4) Winsor- ized	(5) Outliers	(6) Region- time FE
E_0	0.00571*** (0.00192)	0.00568*** (0.00188)	0.00734*** (0.00182)	0.00555** (0.00230)	0.00523** (0.00250)	0.00574** (0.00245)
E_2	0.00959*** (0.00318)	0.00957*** (0.00312)	0.0128*** (0.00317)	0.00990*** (0.00349)	0.00990*** (0.00375)	0.00868** (0.00427)
E_4	0.00946** (0.00426)	0.00955** (0.00430)	0.0142*** (0.00364)	0.0127*** (0.00458)	0.0133*** (0.00490)	0.00614 (0.00400)
\sum Pre-event	-1.39e-07 (0.00349)	-0.000027 (0.00355)	-0.00315 (0.00277)	1.69e-07 (0.00485)	-1.49e-07 (0.00516)	0.00001 (0.00338)
N	14,777	14,777	14,777	14,932	14,932	14,777
Time FE	YES	YES	YES	YES	YES	YES
Controls	NO	YES	NO	NO	NO	YES
County FE	YES	YES	NO	YES	YES	NO
Trimmed	YES	YES	YES	NO	NO	YES
Winsorized	NO	NO	NO	YES	NO	NO
Trend-adjusted	YES	YES	YES	YES	YES	NO

Notes: The dependent variable is the establishment-level inflation rate adjusted for a bite-specific trend as detailed in section 4.2. 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 2. The control variables in (2) are the monthly unemployment rate and monthly average wage, both at the county level. (3) does not control for price trends at the county level. (4) uses a winsorized outcome (99% windsorization). (5) does not trim or winsorize the outcome. In (6) the dependent variable is not trend-adjusted but region-time FE are included; county FE are omitted and county controls are included to account for time-variant confounders in event 2 (see main text for details). 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 retail price level effects of minimum wage hikes

	(1) Baseline	(2) No con- trols	(3) County FE	(4) Reg.- time FE	(5) Winsor- ized	(6) Outliers
E_0	0.00332** (0.00144)	0.00330** (0.00134)	0.00328* (0.00176)	0.00306* (0.00175)	0.00284** (0.00132)	0.00448** (0.00210)
E_2	0.00362** (0.00145)	0.00330** (0.00162)	0.00331 (0.00192)	0.00426** (0.00182)	0.00367** (0.00175)	0.00527** (0.00175)
E_4	0.00497** (0.00200)	0.00384** (0.00173)	0.00528* (0.00289)	0.00596** (0.00246)	0.00446* (0.00246)	0.00545 (0.00340)
\sum Pre-event	-0.00059 (0.00108)	-0.00020 (0.00090)	-0.00136 (0.00139)	0.00013 (0.00118)	0.00050 (0.00118)	0.00103 (0.00141)
N	14,044	14,044	14,044	14,044	14,189	14,189
Time FE	YES	YES	YES	YES	YES	YES
Controls	YES	NO	YES	YES	YES	YES
County FE	NO	NO	YES	NO	NO	NO
Region-time FE	NO	NO	NO	YES	NO	NO
Trimmed	YES	YES	YES	YES	NO	NO
Winsorized	NO	NO	NO	NO	YES	NO

Notes: The dependent variable is the establishment-level inflation rate. 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 2. The baseline specification in (1) includes as controls the monthly unemployment rate and monthly average wage, both at the county level. (2) excludes county controls. (3) controls for county-level price trends. (4) includes region-time FE but not county FE. (5) uses a winsorized outcome (99% windsorization). (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.

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,

$$\Delta \text{Bite}_{k(j)} = \text{Bite}_{k(j),Q3,y} - \text{Bite}_{k(j),Q1,y+1} \quad (3)$$

This effectively nets out non-compliance at the county level. Tables 15 and 16 show that results are robust to this alternative bite variable.

An obvious concern is that policymakers may set minimum wage policy according to local price trends. If that were the case, then the treatment intensity would be endogenous due to reverse causality. Luckily, this concern does not apply to the statewide hikes in the sample since they are either predetermined (events 1 and 2) or linked to a national price index (event 3). However, the city of Seattle has a citywide minimum wage that could, under certain circumstances, be endogenous for some businesses in event 3. In Appendix D, I consider the scenarios under which Seattle's minimum wage could be endogenous and show that the main results are unchanged when accounting for such endogeneity.

Next, to ensure 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. 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 the fiscal year starting in July and ending in June each year (i.e. six months offset from the weights in the baseline model).³⁶ Results are unaffected by this alternate weighting scheme.

Next, I show that results do not rely on interacting bite with the size of the minimum wage hike (i.e. I set treatment intensity equal to minimum wage bite itself).

It is also important to test whether the results are impacted by the level of industry classification used to define the bite variable. Therefore, I construct an alternative bite variable based on 5-digit NAICS codes to show that the main results do not depend on the level of industrial classification used.³⁷

5 Indirect pass-through to retail prices

Since wholesalers occupy the upstream portion of the supply chain, the pass-through rates from equation 2 provide a complete measure of wholesale price adjustment in re-

³⁶For the weights to cause endogeneity, the change in product and subcategory revenue shares within an establishment would need to covary with bite. I view this as unlikely.

³⁷Though based on a more detailed level of industry classification, the alternate bite variable contains measurement error of a different sort, meaning it is not apparent that it is a better measure of minimum wage exposure. See appendix D for details.

sponse to the minimum wage.³⁸ 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 risks underestimating the true impact of the minimum wage on retail prices. To capture both direct and indirect effects, I estimate the following reduced form equation for retailers only:

$$\pi_{r,t} = \sum_{l=-6}^5 \beta_l \Delta MW_{r,t-l} \times Bite_{k(r),t-l} + \sum_{l=-6}^5 \psi_l JB_{r,P,t-l} + X_{k(r),q(t)} + \theta_k(r) + \gamma_t + \epsilon_{r,t}. \quad (4)$$

In contrast to equation 2, equation 4 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, $JB_{r,P,t-l}$, measures the weighted average minimum wage exposure of the wholesalers that retailer r purchases from. $JB_{r,P,t-l}$ therefore captures the intention-to-treat and it is calculated as follows:

$$JB_{r,P,t-l} = \sum_{p=s}^S \alpha_{r,p} \Delta MW_{p,t-l} \times \sum_{p=s}^S \alpha_{r,p} Bite_{k(p),t-l} \quad (5)$$

Here, $\Delta MW_{p,t-l}$ is the size of the minimum wage hike for wholesaler p ; $\alpha_{r,p}$ is the average share of retailer r 's wholesale expenditures going to wholesaler 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 county k that wholesaler p is located in.³⁹ Thus, the first term in equation 5 measures the average minimum wage hike for the set of wholesalers that retailer r purchases from, while the second term measures the average bite for that same set of wholesalers.⁴⁰ It is worth emphasizing that $\alpha_{r,p}$ contains no time index and is therefore fixed for each retailer-event. In practice, retailers may react to wholesale pass-through 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 pass-through does 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 wholesale pass-through.

As an alternative to the joint bite variable in equation 5, one could use wholesalers'

³⁸In practice, wholesalers may also be subject to minimum wage pass-through from their input suppliers. However, wholesale inputs like hydroponic systems, grow lights, and raw materials can be purchased from suppliers outside of Washington state. Therefore, minimum wage pass-through to wholesale input prices is likely small. To simplify the analysis, I do not consider indirect pass-through for wholesalers.

³⁹Note that a retailer and a wholesaler located in the same county will have different bites since bite is calculated at the industry subsector level.

⁴⁰One could instead directly interact wholesale expenditure share, hike size, and wholesaler bite as follows: $JB_{r,P,t-l} = \sum_{p=s}^S \alpha_{r,p} \Delta MW_{p,t-l} Bite_{k(p),t-l}$. Results are virtually identical under this definition of joint bite. However, the advantage of averaging before interacting (as in equation 5) is that the coefficient ψ_l can be interpreted as a pass-through elasticity.

geographic proximity as an instrument for retailers' exposure to wholesale pass-through. This assumes that retailers purchase more from wholesalers located nearby than those further away. However, I find little evidence supporting this assumption. Instead, a large share of retailers' wholesale purchases are from wholesalers located in other parts of the state (see appendix H). Thus, a distance-based instrument is unlikely to capture retailers' exposure to wholesale pass-through.

A key assumption is that equation 4 separately identifies direct and indirect pass-through. One way to test this is to examine whether the estimates for β_l (the direct pass-through estimates) are affected by the inclusion of joint 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 H that direct pass-through estimates are unaffected by the inclusion of joint wholesale bite.

In equation 4, 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 $P_L = -\sum_{l=m}^{L-1} \psi_{-l}$.

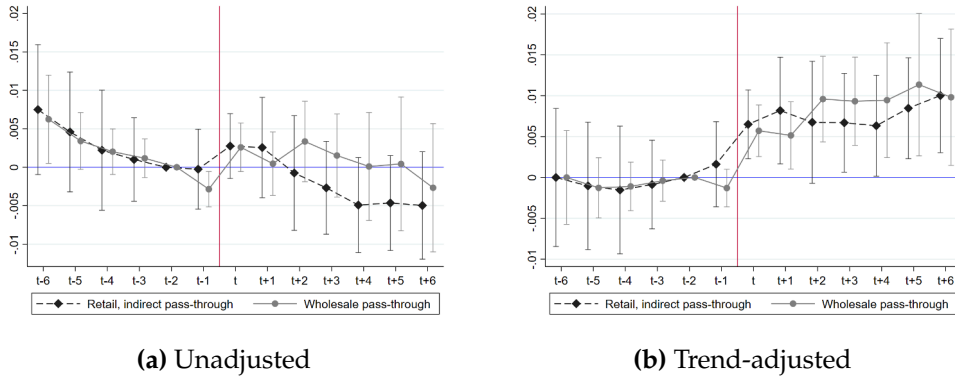
Figure 7a illustrates that the time path of indirect pass-through to retail prices is remarkably similar to the wholesale pass-through estimates from the previous section. The figure reveals a downward-sloping pre-trend interrupted by an inflationary shock in the treatment period, followed by a continuation 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, $\hat{\psi}_{pre} = 1/4 \sum_{l=2}^5 \hat{\psi}_{-l}$. I find no statistically significant difference between $\hat{\psi}_{pre}$ and the bite-specific trend for wholesale pass-through.⁴¹ Accordingly, I apply the ? procedure and re-estimate equation 4 with the dependent variable adjusted for the joint-bite-specific trend.⁴² Figure 7b illustrates that the treatment effect for indirect pass-through to retail prices is similar in magnitude and not statistically significantly different from direct pass-through to wholesale prices. This provides a first indication that cannabis retailers fully pass the wholesale cost shock on to retail prices.

In table 5, 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 joint bite (18.14%), a 10% minimum wage hike corresponds to a 1.22%

⁴¹ $\hat{\psi}_{pre} = -.00102$ (90% confidence interval: -0.00329 to 0.00126), which overlaps with the pre-trend for wholesale pass-through, $\hat{\beta}_{pre} = -0.00197$ (90% confidence interval: -0.00367 to -0.00026). See appendix C for details.

⁴²Unlike in section 4, region-time FE cannot be used to control for the pre-trend since the dependent variable and joint bite stem from different sets of establishments (retailers and wholesalers, respectively). Moreover, as shown in Appendix H, retailers purchase a large share of products from wholesalers located in other regions of the state, meaning region-time FE based on a retailer's region will not capture the joint bite-specific trend.

Fig. 7. Comparing wholesale pass-through and indirect retail pass-through



Notes: The figures compare wholesale price level effects and indirect retail price level effects of minimum wage hikes. Wholesale price effects are estimated from equation 2 with time fixed effects and county fixed effects. Indirect retail price effects are estimated from equation 4 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 $t - 2$. 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 procedure described in section 4.2. 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.

increase in retail price level two months after the hike.

Comparing indirect and direct pass-through to retail prices: a discussion

To facilitate comparison, table 6 summarizes the main pass-through effects for wholesale and retail prices. Several facts stand out. First, the magnitude of direct pass-through to retail prices (0.7%) is in line with existing studies. Leung (2021), for example, finds a minimum wage pass-through elasticity of 0.6-0.8% for grocery store prices in the U.S.⁴³

Second, for retail prices, the indirect pass-through elasticity exceeds the direct pass-through elasticity by a factor of 1.8. For retail stores in other sectors (e.g. grocery stores, general merchandise stores, etc) the cost of goods sold (COGS) typically makes up a larger share of retail variable costs compared to labor costs. If this applies to cannabis retailers as well, then it is reasonable for indirect pass-through to exceed direct pass-through to retail prices.⁴⁴ To quantify the relative importance of labor and COGS for cannabis retailers, I use aggregate payroll data on cannabis retailers from the Washington state ESD for the years 2018-2020.⁴⁵ I find that cannabis retailers' annual COGS expenditure exceeds labor expenditure by a factor of 2.1 (see appendix H). In other words, indirect and direct pass-through rates to retail prices are in line with the relative importance of wholesale and labor costs in cannabis retailers' variable costs.⁴⁶

⁴³Using different scanner data from the U.S., Renkin et al. (2022) find a minimum wage pass-through elasticity of 0.36% for grocery store prices.

⁴⁴For example, Renkin et al. (2022) find that COGS account for more than 80% of retailers' variable costs.

⁴⁵Data for 2021 is not available. I compute COGS directly from the cannabis traceability data.

⁴⁶Note that labor costs and COGS comprise the two largest parts of retailers' variable costs (Renkin et al.,

Table 5: Indirect retail price level effects of minimum wage hikes

	(1) Baseline	(2) No con- trols	(3) County FE	(4) Reg.- time FE	(5) Winsor- ized	(6) Outliers
E_0	0.00650** (0.00256)	0.00658** (0.00256)	0.00708*** (0.00270)	0.00686*** (0.00263)	0.00787** (0.00330)	0.00905** (0.00383)
E_2	0.00675 (0.00454)	0.00685 (0.00452)	0.00798 (0.00505)	0.00694 (0.00457)	0.00912** (0.00438)	0.0122*** (0.00465)
E_4	0.00633* (0.00376)	0.00689* (0.00378)	0.00770* (0.00425)	0.00663* (0.00382)	0.00922* (0.00526)	0.0134** (0.00621)
\sum Pre-event	-1.94e-07 (0.00514)	-0.000618 (0.00514)	-0.000782 (0.00504)	-0.000469 (0.00521)	3.81e-08 (0.00540)	-2.90e-08 (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 adjusted for a bite-specific trend as detailed in section 4.2. 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 2. 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.

Table 6: Minimum wage elasticity of the price level

	Wholesale	Retail	
		Direct	Indirect
E_2	0.00959/0.00837 ⁺	0.00362	0.00675
Avg. bite	18.63	19.43	18.14
MW elasticity of the price level (10% hike)	1.79/1.56	0.7	1.22

Notes: This table reports the minimum wage elasticity of prices from a 10% increase in the minimum wage, two periods after the hike, computed at the bite average bite: $E_2 \times \overline{Bite} \times .10$. E_2 are taken from tables 3, 4, and 5. Data: Top Shelf Data and Washington ESD, (August 2018 - July 2021).

⁺ 0.00946 is the estimated wholesale price level effect when the dependent variable is trend-adjusted. 0.00568 corresponds to the unadjusted regression with region-time FE. These two price level effects correspond to elasticities of 1.79 and 1.56, respectively.

Third, the total minimum wage elasticity of the retail price level, which is obtained by summing the direct and indirect elasticities, is 1.92%—much larger than that from direct pass-through alone.⁴⁷ Thus, failing to account for indirect pass-through dramatically underestimates the minimum wage effect on retail cannabis prices.

6 Dissecting the price effects

6.1 Effects on markups over marginal input cost

In figure 7, there is no statistically significant difference between indirect pass-through to retail prices and (direct) pass-through to wholesale prices. This suggests a full pass-through of the wholesale cost shock to retail prices. To further confirm this, I estimate the impact of minimum wage hikes on retail markups over marginal input cost (MIC), with the aim of determining the degree of wholesale cost pass-through to retail prices. I estimate the following equation for retail establishments:

$$\Delta\mu_{r,t} = \sum_{l=-6}^5 \beta_l \Delta MW_{r,t-l} \times Bite_{k(r),t-l} + \sum_{l=-6}^5 \psi_l JB_{r,P,t-l} + X_{k(r),q(t)} + \theta_k(r) + \gamma_t + \epsilon_{r,t}. \quad (6)$$

The dependent variable, $\Delta\mu_{r,t}$, is the monthly percent change in MIC markup for establishment r (see appendix F for details on the markup index). Table 7 displays the estimated markup effects and compares them to the direct and indirect retail price level effects found in sections 4-5. Comparing E_4 in columns 1 and 2 reveals that the direct effect on the markup is very similar to the direct effect on the price level. This is not surprising, since direct pass-through to prices entails a price increase that is—by definition—-independent of wholesale costs. In contrast, columns 3 and 4 reveal a large indirect effect on the price level in E_4 (significant at the 10% level) but a small and statistically insignificant indirect markup effect. This indicates no markup adjustment to the wholesale cost shock on the part of retailers and implies a full pass-through of the wholesale cost shock to retail prices.

6.2 Price effects by scale of production

Next, I test for heterogeneous pass-through across different establishment sizes. Producer-processor (i.e. wholesale) licenses are based on a three-tier system governing the square footage of plant canopy 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. Retail cannabis licenses have no such restrictions, so I sort establishments into tertiles for each event based on

2022)

⁴⁷Note that the direct and indirect pass-through elasticities are computed with different average bites. One could instead compute both elasticities from a single average bite value, but this would be an out of sample prediction for at least one of the elasticities. Since the average bite for indirect pass-through (18.14%) is similar to that for direct pass-through (19.43%), I compute the elasticities using their own average bite values. The total elasticity is then the sum of the direct and indirect elasticities.

Table 7: Minimum wage effects on retail prices and markups over marginal input cost

	Direct effects		Indirect effects	
	(1) Price level	(2) Markup	(3) Price level	(4) Markup
E_0	0.00332** (0.00144)	0.00277** (0.00137)	0.00650** (0.00256)	0.00338 (0.00293)
E_2	0.00362** (0.00145)	0.00293** (0.00145)	0.00675 (0.00454)	0.00210 (0.00475)
E_4	0.00497** (0.00200)	0.00506*** (0.00176)	0.00633* (0.00376)	-0.000940 (0.00432)
\sum Pre-event	-0.00059 (0.00108)	0.000461 (0.00149)	-1.94e-07 (0.00514)	-0.000760 (0.00633)
N	14,044	14,040	13,559	13,552
Time FE	YES	YES	YES	YES
Controls	YES	YES	YES	YES
Trimmed	YES	YES	YES	YES
Trend-adjusted	NO	NO	YES	YES

Notes: This table reports the effects of minimum wage hikes on retail markups over marginal input cost. The dependent variable is the establishment-level percent change in markup over marginal input cost. For ease of comparison, column (1) is copied from column 1 in table 4 and shows the direct pass-through effect of minimum wage hikes on the retail price level. Column (2) reports the direct effect on markups over MIC. Column (3) is copied from column 1 in table 5 and shows the indirect pass-through effect of minimum wage hikes on the retail price level. Column (4) reports the indirect effect on markups over MIC. The dependent variable in column (4) is adjusted for a bite-specific pre-trend as detailed in section 4.2. All specifications include time fixed effects and county controls (unemployment rate and average wage). 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 $t - 2$. Standard errors of the sums 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, August 2018-July 2021.

total revenue during the event window. I estimate equation 4 separately for each subsample and report the results in table 8. Columns 1-3 show that wholesale pass-through is monotonically decreasing with establishment size: small producer-processors exhibit much larger price level effects than medium-sized producer-processors, while effects for large producer-processors are close to zero and statistically insignificant.⁴⁸ This suggests that small producer-processors may be less able to absorb the minimum wage cost shock via other margins of adjustment (e.g. by adjusting profits). This could be due to low market power among small establishments, a common complaint among tier 1 producer-processors (see e.g. Washington State Liquor and Cannabis Board (2021)). Unfortunately, the data do not contain information on firm-level profits or employment, meaning it is not possible to answer this definitively.

For retailers, direct price level effects in E_0 are also monotonically decreasing with establishment size, but at higher lags medium-sized establishments have the largest effects (columns 4-6). Nevertheless, small retailers have consistently greater price level effects than large retailers (though the difference is not statistically significant). For indirect pass-through, medium establishments again have the largest effects (columns 7-9), but there is no systematic pattern when comparing only small and large establishments.

⁴⁸The difference between effect sizes for small and large producer-processors is statistically significant for E_2 but not for E_0 or E_4 .

Table 8: Price effects by establishment size

	Wholesale			Retail					
				Direct			Indirect		
	(1) Small	(2) Medium	(3) Large	(4) Small	(5) Medium	(6) Large	(7) Small	(8) Medium	(9) Large
E_0	0.01360* (0.00654)	0.00571 (0.00343)	0.00278 (0.00326)	0.00550* (0.00240)	0.00384* (0.00188)	0.00282* (0.00139)	0.00178 (0.00414)	0.01750*** (0.00494)	0.00041 (0.00753)
E_2	0.02490** (0.00964)	0.00959* (0.00468)	0.00026 (0.00452)	0.00444 (0.00263)	0.00637* (0.00267)	0.00163 (0.00168)	0.00545 (0.00692)	0.0178** (0.00671)	-0.00402 (0.00923)
E_4	0.02300* (0.00897)	0.00957 (0.00731)	-0.00031 (0.00930)	0.00403 (0.00300)	0.0109*** (0.00313)	0.00108 (0.00251)	0.00003 (0.00672)	0.0173* (0.00761)	0.00966 (0.00609)
\sum Pre-event	6.62e-08 (0.00849)	1.50e-07 (0.00467)	1.03e-07 (0.00540)	-0.00180 (0.00378)	-0.00198 (0.00162)	0.00123 (0.00262)	-9.54e-09 (0.00744)	-5.54e-08 (0.00839)	1.54e-07 (0.00740)
N	2,673	6,968	5,136	3,775	4,987	5,258	3,327	4,987	5,245
Time FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Controls	NO	NO	NO	YES	YES	YES	YES	YES	YES
County FE	YES	YES	YES	NO	NO	NO	NO	NO	NO
Trend-adjusted	YES	YES	YES	NO	NO	NO	NO	NO	NO

Notes: This table shows price level effects when estimating equation 4 for sub-samples based on establishment size. The listed coefficients are sums of the distributed lag coefficients E_L , L months after the minimum wage hikes, relative to the normalized baseline period in $t - 2$. For wholesalers, small corresponds to tier 1 producer-processors, medium to tier 2, and large to tier 3. For retailers, establishments are sorted into tertiles based on total establishment revenue for each event. 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, August 2018-July 2021.

6.3 Employment effects

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 the employment effects of minimum wage hikes during the sample period. Since employment information is not available for cannabis establishments, I use monthly employment data from the QCEW at the 5-digit NAICS industry level. I estimate the following distributed lag equation:

$$\Delta \ln Emp_{k,t} = \sum_{l=-6}^5 \beta_l \Delta MW_{t-l} \times Bite_{k,t-l} + X_{k,q(t)} + \theta_k + \gamma_t + \epsilon_{k,t}. \quad (7)$$

The dependent variable is the first difference of (log) county employment at the 5-digit industry level.⁴⁹ The treatment intensity $\sum_{l=-6}^5 \beta_l \Delta MW_{t-l} \times Bite_{k,t-l}$ is the same as in the previous sections but for one difference: Since equation 7 is at the county level, ΔMW does not include citywide minimum wage hikes. Table 9 shows that employment effects are mostly insignificant.⁵⁰ Overall, these results suggest that the minimum wage has no effect on employment for 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.

7 Discussion and 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 rates across six minimum wage hikes from 2018 to 2021. When ignoring wholesale pass-through, I find that a 10% increase in the minimum wage raises retail prices by 0.7%. Yet, I also find large wholesale pass-through effects: a 10% increase in the minimum wage raises wholesale prices by 1.79%. The existence of wholesale pass-through implies that retailers face a wholesale cost shock in addition to the labor cost shock. When wholesale pass-through effects are considered, retail pass-through rates more double to 1.92%. I find that retailers do not adjust markups to wholesale pass-through, indicating a full pass-through of wholesale cost shocks to retail prices. Moreover, wholesale pass-through decreases with production scale, which suggests that large wholesalers may adjust to the labor cost shock along other margins.

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

⁴⁹Cannabis 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.

⁵⁰Column 1 shows a positive effect that is significant at the 10% level. However, this effect disappears when including region-time fixed effects, which is an important robustness check in the minimum wage-employment context (see (Allegretto & Reich, 2018)).

Table 9: Minimum wage effects on employment, by industry

	Indoor crops			Retailers		
	(1)	(2)	(3)	(4)	(5)	(6)
E_0	0.0659* (0.0361)	-0.0108 (0.0256)	-0.00927 (0.0290)	0.00659 (0.0160)	0.0264* (0.0139)	0.0192 (0.0160)
E_2	0.0853* (0.0483)	0.00679 (0.0294)	0.00635 (0.0402)	0.000232 (0.0187)	0.0145 (0.0136)	0.000603 (0.0182)
E_4	0.0739 (0.0708)	-0.0341 (0.0320)	-0.0364 (0.0435)	0.0254 (0.0283)	0.0326 (0.0240)	0.0116 (0.0274)
\sum Pre-event	-0.0113 (0.0344)	-0.0239 (0.0276)	-0.0269 (0.0411)	0.0255 (0.0196)	0.0196 (0.0239)	0.0342 (0.0228)
N	603	603	603	851	851	851
Time FE	YES	YES	YES	YES	YES	YES
County FE	YES	NO	YES	YES	NO	YES
Region-time FE	NO	YES	YES	NO	YES	YES

The table reports cumulative employment effects E_l relative to the normalized baseline period in $t - 2$, as described in equation 7. The dependent variable is classified at the 5-digit NAICS level. Indoor crops corresponds to NAICS 11141, and retailers corresponds to NAICS 45399. The treatment intensity is defined as in the previous sections. Standard errors are clustered by county.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Data from the QCEW and Washington ESD, 2018-2021.

Washington’s cannabis industry an ideal laboratory for studying minimum wage pass-through also set the industry apart. The dominance 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 G). Similarly, since consumers must individually consult with a budtender to purchase products suggests that cannabis retail stores are more labor intensive than other types of retail outlets. Accordingly, the labor share of variable cost is expected to be higher, and hence, the cost shock imposed by the minimum wage may be larger compared to other industries. Therefore, I view my direct pass-through estimates as an upper-bound compared to less labor intensive industries.

Another difference to other industries is that the cannabis market operates under autarky. This implies that cannabis retailers may be constrained in their response to wholesale pass-through 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 wholesale pass-through. 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).⁵¹

Nevertheless, 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. In particular, studies that only look at retail scanner data may underestimate the true effect of minimum wages on retail prices.

⁵¹Using the 2007 Commodity Flow Survey, Renkin et al. (2022) provide evidence of substantial home bias in US grocery consumption.

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A Construction of 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 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-wholesaler-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:

$$P_{i,j,t} = \frac{TR_{i,j,t}}{TQ_{i,j,t}}. \quad (8)$$

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 :

$$I_{c,j,t} = \prod_i \left(\frac{P_{i,j,t}}{P_{i,j,t-1}} \right)^{\omega_{i,c,y(t)}} \quad (9)$$

where each subcategory is a unique category-unit weight combination.⁵² 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 .⁵³

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

$$I_{j,t} = \prod_c I_{c,j,t}^{\omega_{c,j,y(t)}}. \quad (10)$$

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 wholesalers 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): wholesaler-category-unit weight-product name. While a retailer may sell similar products produced by different wholesalers, a wholesaler 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

⁵²Since 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.

⁵³As pointed out by Renkin 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.

eq. 9 leads to a few inconceivable outliers such as a 562-factor increase in prices from one month to the next. To prevent outliers from driving results 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 9. As table 10 illustrates, trimming does not meaningfully change the location or shape of the distribution but lowers the standard deviation considerably.

Table 10: 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
<i>N</i>	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.

Table 11: Establishment inflation rates

	Retail		Wholesale	
	No trim	1% trim	No trim	1% trim
Mean	-.0001361	-.0003084	.0015973	.00139
Median	0	0	0	0
St. Dev	.0289969	.0234164	.0642829	.0538198
<i>N</i>	14,048	13,919	16,576	16,422

Notes: This table summarizes the dependent variable in the estimation sample, the establishment-level inflation rate. Inflation rates are obtained by taking the natural logarithm of the establishment-level price index derived in appendix A. The estimation sample runs from August 2018 through July 2021. Data source: Top Shelf Data.

B 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) subsector. The underlying principle of the NAICS system—that establishments with similar production processes be grouped together—greatly facilitates this, since the NAICS subsectors align well with the vertically disintegrated structure of the cannabis industry. For example, NAICS subsector 453 captures all cannabis retailers, since NAICS 453998, a component of that subsector, includes “All Other Miscellaneous Store Retailers (except Tobacco Stores), including Marijuana Stores, Medicinal and Recreational” (US Census Bureau, 2017). At the wholesale 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, 2017). 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).⁵⁴ Importantly, though, NAICS classifies an establishment based on its primary activity, meaning that a wholesaler only belongs to NAICS 424 if the receipts/sales and revenue from processing activities exceed those of its own crop production (US Census Bureau, 2017). 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 firm level, at the industry level unprocessed “Usable Marijuana” accounts for over 61% of producer-processors’ revenue in my sample period. Therefore, I classify all establishments with a joint producer-processor license as NAICS 111, which effectively assumes 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 12 provides an overview of the representativeness of cannabis employment in the respective 3-digit NAICS subsectors. The employment share for cannabis retailers is larger than that for wholesalers, but the shares remain relatively constant over time for both wholesalers and retailers. The fact that NAICS 111 is less representative does not

⁵⁴A third subsector, NAICS 115, may also apply to producer-processors. This subsector captures support activities for agriculture involving soil preparation, planting, and cultivating. However, to be in this subsector 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 main analysis.

imply that measurement error for the wholesale 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 at the subsector level. Table 19 in appendix G shows that mean annual wages for cannabis establishments are remarkably similar to their corresponding NAICS subsectors and very close to the wage floor imposed by the minimum wage.

Table 12: Employment in cannabis relative to 3-digit NAICS subsector

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

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

Measurement error in NAICS 111

The nature of agricultural labor in the United States means that one must consider whether the bite variable for NAICS 111 is not 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 underestimate minimum wage exposure. Counties with more undocumented workers will have a larger 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 periods 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 D, 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 measurement error would need to coincide with the timing of the minimum wage hike. In other words, 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.

C Bite-specific trends

C.1 Wholesalers

As shown in figure 5 in section 4, trend-adjusting the dependent variable for the wholesalers does not change the contemporaneous treatment effect, but it does affect pass-through rates 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. Given this discrepancy, 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 empirical 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 events 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 $\hat{\beta}_{pre}$, 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 13 illustrates, this is indeed the case: regardless of whether one includes 5 or 4 leads in the pre-treatment period, bite has a systematic, negative effect on inflation over the pre-treatment period. Though the trend extends through 5 leads (i.e. through $t - 1$, see figure 5), 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.

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 2 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, though the pooled events exhibit a significant pre-trend, the estimated trend may fit some events better than others. 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 show that events 1 and 3 exhibit no significant pre-trend while event 2 contains a strong pre-trend that largely mirrors the single trend found in figure 5.

Event-specific trends are estimated as follows: First, I estimate equation 2 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 *change* in the pre-treatment effect for event e (since equation 2 is in first differences). As table 13 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.⁵⁵ Specifically, I use $\bar{\hat{\beta}}_{pre,e=2}$ to obtain predicted values for the bite-specific trend for the entire

⁵⁵Results are similar when adjusting all events for a single trend or adjusting all events with an event-specific trend. See Appendix C for details.

Table 13: Bite-specific trend estimates for wholesalers

	(a) Pooled trend		(b) Separate trends		
	(1) No County FE	(2) County FE	(1) Event 1	(2) Event 2	(3) Event 3
4 leads	-0.00077 (0.00069)	-0.00156* (0.00087)	0.00179 (0.00261)	-0.00090 (0.00067)	-0.00370 (0.00848)
5 leads	-0.00103* (0.00054)	-0.00182*** (0.00070)	0.00056 (0.00209)	-.00109** (0.00055)	-0.00485 (0.00788)
<i>N</i>	13,033	13,033	3,996	4,646	4,391
Time FE	YES	YES	YES	YES	YES
County FE	NO	YES	NO	NO	NO

This table reports the average of the distributed lag coefficients obtained from estimating equation 2, i.e. the average change in the treatment effect in the pre-treatment period. Note that when estimating effects for each event separately (panel b), it is not possible to include county fixed effects since for the vast majority of counties there is no variation in bite within counties for a given event. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Data from Washington ESD and Top Shelf Data, August 2018-July 2021.

event window: $\hat{\pi}_{j,t(e=2)} = \hat{\beta}_{pre(e=2)} \times \Delta MW_{j,t-l(e=2)} \times Bite_{k(j),t-l(e=2)}$. Finally, I estimate equation 2 using adjusted inflation $\tilde{\pi}_{j,t}$ as the dependent variable

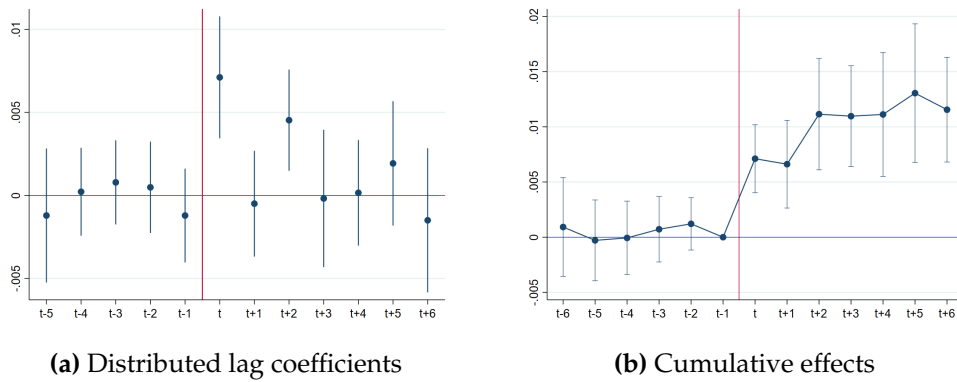
$$\tilde{\pi}_{j,t(e)} = \begin{cases} \pi_{j,t(e)} & \text{if } e = 1, 3 \\ \pi_{j,t(e)} - \hat{\pi}_{j,t(e)} & \text{if } e = 2 \end{cases} \quad (11)$$

Given these findings, I adjust the dependent variable for event 2 while leaving events 1 and 3 unadjusted. As figure 9 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 10a shows that estimated effects are very similar.

C.2 Direct pass-through to retail prices

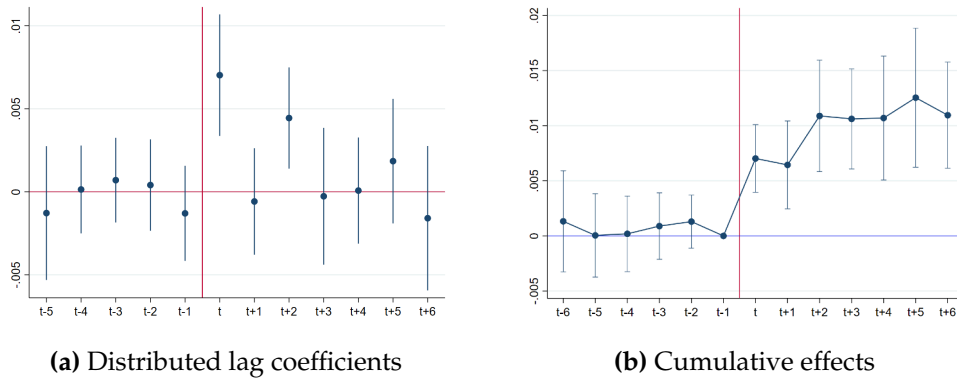
In section 4 of the main part of the paper, a slight upward slope to the price effects is visible in the pre-treatment period for retailers. Therefore, in this section, I check whether results for retailers change when adjusting for this trend. As in the previous subsection, I first fit a single trend onto the pooled events and then compare this to separate trends fit to each event.

Fig. 8. Wholesale price effects of minimum wage hikes, events 1 and 3 unadjusted, event 2 adjusted



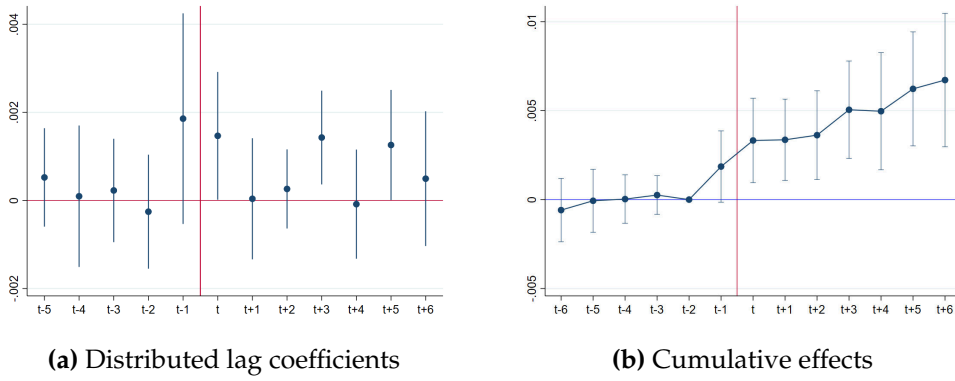
Notes: The figure depicts treatment effects when the dependent variable is trend-adjusted for event 2 but unadjusted for events 1 and 3. In both panels, estimates are from equation 2 with time fixed effects but no county fixed effects (since the event 2 trend cannot be estimated with county fixed effects). Results are robust to including county fixed effects. Panel a shows the estimated distributed lag coefficients with 90% confidence intervals based on SE clustered at the county level. Panel b displays cumulative effects E_L (as detailed in section 4) with 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.

Fig. 9. Wholesale price effects of minimum wage hikes, all events adjusted for event-specific trend



Notes: The figure depicts treatment effects when the dependent variable is trend-adjusted for event 2 but unadjusted for events 1 and 3. In both panels, estimates are from equation 2 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 with 90% confidence intervals based on SE clustered at the county level. Panel b displays cumulative effects E_L (as detailed in section 4) with 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.

Fig. 10. Retail price effects of minimum wage hikes, unadjusted



Notes: The figure depicts treatment effects when the dependent variable is not adjusted for a bite-specific trend. In both panels, estimates are from equation 2 with time fixed effects and county fixed effects. Panel a shows the estimated distributed lag coefficients with 90% confidence intervals based on SE clustered at the county level. Panel b displays cumulative effects E_L (as detailed in section 4) with 90% confidence intervals of the sums based on SE clustered at the county level. In panel b, the normalized base period is set to $t - 2$. Data source: Top Shelf Data and Washington ESD.

Pooled trend

As table 14 panel a illustrates, the pooled trend is small and not statistically significant. Figure 11 depicts the distributed lag coefficients and resulting cumulative price effects when adjusting the dependent variable for the pooled trend. Nevertheless, when adjusting the dependent variable for the trend in column 2, the distributed lag coefficients in the pre-treatment period go from slightly positive (figure 10 panel a) to zero (figure 11 panel a). As figure 11 panel b illustrates, adjusting for the bite-specific trend slightly attenuates the treatment effect so that the cumulative effects are no longer statistically significant. However, the main story does not change: there is a distinct inflationary effect beginning in $t - 2$ and ending in t , and the price effect plateaus in subsequent periods.

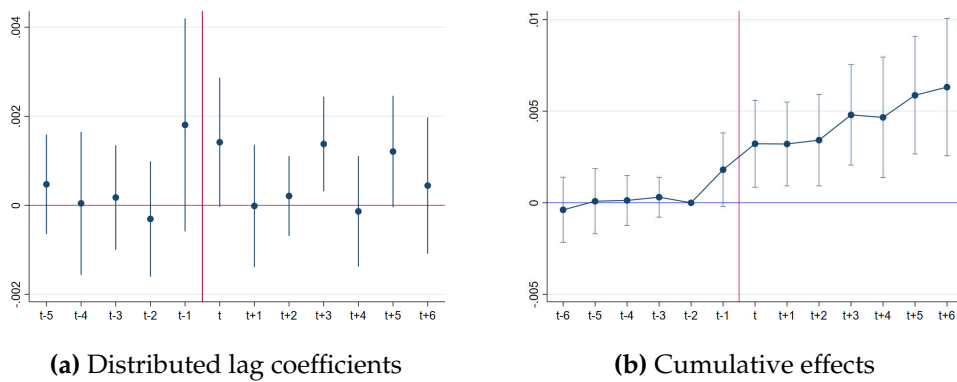
Separate trends

In panel b of table 14, separate trends are estimated for each event. The trends are equal and opposite for events 1 and 3, while event 2 has a small but statistically insignificant positive trend. Since only events 1 and 3 have statistically significant trends, I first adjust these events while leaving event 2 unadjusted. Figure 12 shows that results are virtually unchanged and the trend is not eliminated following the trend-adjustment. This is not surprising: the equal and opposite trends for events 1 and 3 imply equal and opposite trend adjustments, so that the net effect amounts to zero. Therefore, as a next step I check whether results change when adjusting all three events. Figure 13 illustrates that when adjusting all three events, the pre-treatment trend disappears and the main results become slightly attenuated. As with the pooled trend-adjustment, however, the main story does not change: a strong inflationary effect begins in $t - 2$ and extends to t , at which point the

Table 14: Bite-specific trend estimates for retailers

	(a) Pooled trend		(b) Separate trends		
	(1) No County FE	(2) County FE	(1) Event 1	(2) Event 2	(3) Event 3
4 leads	0.00015 (0.00027)	0.00034 (0.00035)	-0.00374* (0.00204)	0.00000 (0.00026)	0.00623*** (0.00229)
<i>N</i>	14,044	14,044	4,165	4,921	4,958
Time FE	YES	YES	YES	YES	YES
County FE	NO	YES	NO	NO	NO
Controls	YES	YES	YES	YES	YES

Notes: This table reports the average of the pre-treatment distributed lag coefficients obtained from estimating equation 2. The sample period is August 2018 through July 2021. The pre-treatment period includes four leads through period $t - 2$ since that is when treatment effects appear for retailers. Note that when estimating effects for each event separately (panel b), it is not possible to include county fixed effects since there is no variation in bite within counties for a given event (for the vast majority of counties). County level controls are the monthly unemployment rate and average wage. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Data from Washington ESD and Top Shelf Data, August 2018-July 2021.

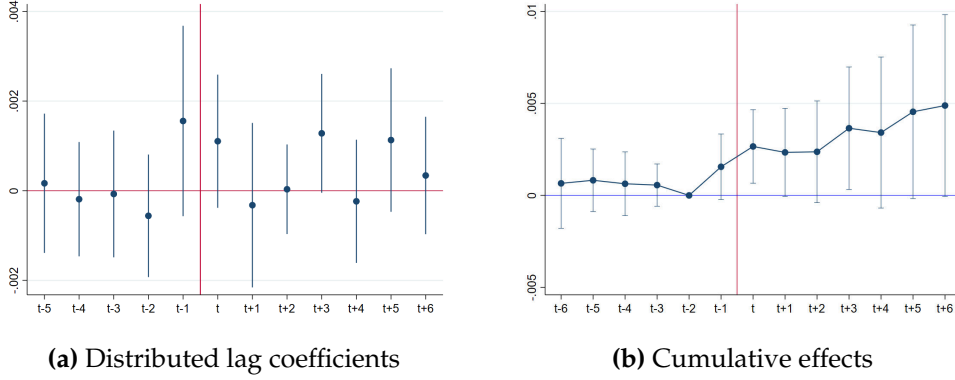
Fig. 11. Retail price effects of minimum wage hikes, adjusted with a pooled trend

Notes: The figure depicts treatment effects when the dependent variable is adjusted for a single bite-specific trend. The bite-specific trend is estimated with time and county fixed effects. In both panels, estimates are from equation 2 with time fixed effects and county fixed effects. Panel a shows the estimated distributed lag coefficients with 90% confidence intervals based on SE clustered at the county level. Panel b displays cumulative effects E_L (as detailed in section 4) with 90% confidence intervals of the sums based on SE clustered at the county level. In panel b, the normalized base period is set to $t - 2$. Data source: Top Shelf Data and Washington ESD, August 2018-July 2021.

effect plateaus.

Though the various trend adjustments in this subsection lead to slightly smaller treatment effects for retailer prices, this should not be over interpreted since the pooled trend in the main specification is not statistically significant to begin with. Thus, trend-adjustment for retailers can be seen as a conservative robustness check, and as figures 11-13 illustrate, the main results stand up to this check.

Fig. 12. Retail price effects of minimum wage hikes, events 1 and 3 adjusted, event 2 unadjusted



Notes: The figure depicts treatment effects when the dependent variable is trend-adjusted for events 1 and 3 but unadjusted for event 2. In both panels, estimates are from equation 2 with time fixed effects but no county fixed effects (since event-specific trends cannot be estimated with county fixed effects). Results are robust to including county fixed effects. Panel a shows the estimated distributed lag coefficients with 90% confidence intervals based on SE clustered at the county level. Panel b displays cumulative effects E_L (as detailed in section 4) with 90% confidence intervals of the sums based on SE clustered at the county level. In panel b, the normalized base period is set to $t - 2$. Data source: Top Shelf Data and Washington ESD.

C.3 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, $\hat{\psi}_{pre} = 1/4 \sum_{l=2}^5 \hat{\psi}_{-l}$, where $\hat{\psi}_{pre}$ is the average *change* in the pre-treatment effect. I find that $\hat{\psi}_{pre} = -0.00102$ (90% confidence interval: -0.00329 to 0.00126) which overlaps with the pre-trend for pass-through to wholesale prices, $\hat{\beta}_{pre} = -0.00197$ (90% confidence interval: -0.00367 to -0.00026).⁵⁶

D Robustness checks

This section reports results from the robustness checks discussed in section 5.

⁵⁶ $\hat{\psi}_{pre}$ is based on equation 4 with time fixed effects and county-level controls. $\hat{\beta}_{pre}$ is based on equation 2 with time fixed effects and county fixed effects. The pre-trend reported here is from pooled events. For both equations, the pre-trend is stable across a variety of specifications.

Table 15: Robustness checks for wholesale price effects

	Alternate bite variable		Reverse causality		Other		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Q4 bite	Compliance	No Seattle	No King county	Reg.-time and Estab. FE	Balanced panel	Alt. weights
E_0	0.00625** (0.00250)	0.00634*** (0.00227)	0.00576*** (0.00187)	0.00572*** (0.00187)	0.00589** (0.00273)	0.00622*** (0.00220)	0.00775** (0.00377)
E_2	0.0106** (0.00418)	0.0115*** (0.00421)	0.0100*** (0.00302)	0.00996*** (0.00306)	0.00986** (0.00482)	0.00807** (0.00331)	0.01348*** (0.00491)
E_4	0.00885 (0.00541)	0.0109* (0.00599)	0.00950** (0.00424)	0.00946** (0.00428)	0.00681 (0.00616)	0.00793* (0.00447)	0.01866*** (0.00700)
\sum Pre-event	-2.07e-08 (0.00395)	4.94e-08 (0.00513)	-1.64e-07 (0.00326)	1.15e-07 (0.00331)	1.57e-07 (0.00408)	-8.92e-08 (0.00348)	-1.54e-07 (0.00641)
N	14,777	14,699	14,622	14,506	14,777	12,900	14,819
Time FE	YES	YES	YES	YES	YES	YES	YES
County FE	YES	YES	YES	YES	NO	YES	YES
Controls	NO	NO	NO	NO	NO	NO	NO
Trend-adjusted	YES	YES	YES	YES	YES	YES	YES
Region-time FE	NO	NO	NO	NO	YES	NO	NO
Establishment FE	NO	NO	NO	NO	YES	NO	NO

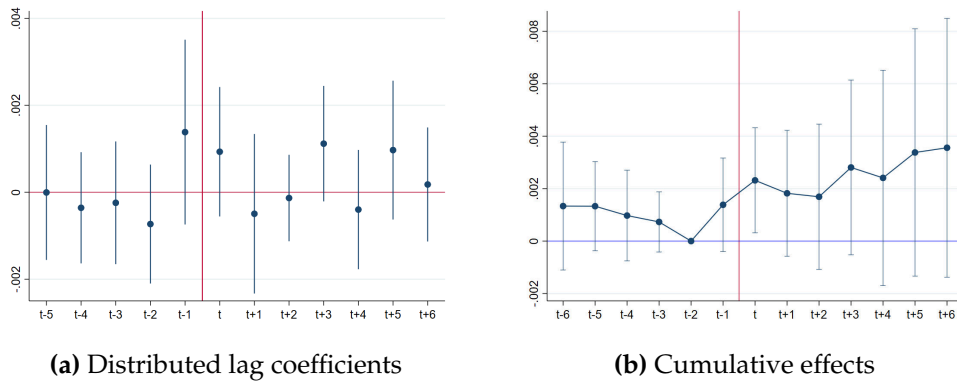
Notes: The listed coefficients are the sum of the distributed lag coefficients E_L , L months after the minimum wage hikes, relative to the baseline period in $t - 2$. The distributed lag coefficients are estimated from equation 2 with establishment-level inflation rate as the dependent variable. All specifications include time fixed effects and county fixed effects. (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) includes region-time FE and establishment FE. (6) restricts the panel to establishments that are present at least 10 months for a given event. For (7) 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, August 2018-July 2021.

Table 16: Robustness checks for direct retail price effects

	Alternate bite variable		Reverse causality		Other		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Q4 bite	Compliance	No Seattle	No King county	Reg.-time and estab. FE	Balanced panel	Alt. weights
E_0	0.00339** (0.00146)	0.00417 (0.00254)	0.00265*** (0.000958)	0.00284*** (0.00104)	0.00319 (0.00199)	0.00323** (0.00145)	.00353*** (.00114)
E_2	0.00322** (0.00154)	0.00556** (0.00252)	0.00282** (0.00125)	0.00343*** (0.00132)	0.00411* (0.00221)	0.00404** (0.00162)	.00390** (.00162)
E_4	0.00422* (0.00216)	0.00811** (0.00333)	0.00384** (0.00173)	0.00497*** (0.00188)	0.00627* (0.00338)	0.00528** (0.00214)	.00514** (.00238)
\sum Pre-event	-0.00076 (0.00117)	0.00210 (0.00168)	-0.00006 (0.00100)	-0.00077 (0.00102)	-0.00061 (0.00153)	-0.00105 (0.00110)	0.00028 (0.00124)
N	14,044	13,859	13,422	12,995	14,044	13,390	14,042
Time FE	YES	YES	YES	YES	YES	YES	YES
County FE	NO	NO	NO	NO	NO	NO	YES
Controls	YES	YES	YES	YES	YES	YES	YES
Region-time FE	NO	NO	NO	NO	YES	NO	NO
Establishment FE	NO	NO	NO	NO	YES	NO	NO

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 2 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) includes region-time FE and establishment FE (6) restricts the panel to establishments that are present at least 10 months for a given event. For (7) 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, August 2018-July 2021.

Fig. 13. Retail price effects of minimum wage hikes, all events adjusted



Notes: The figure depicts treatment effects when the dependent variable is trend-adjusted for all three events using an event-specific trend. In both panels, estimates are from equation 2 with time fixed effects but no county fixed effects (since event-specific trends cannot be estimated with county fixed effects). Results are robust to including county fixed effects. Panel a shows the estimated distributed lag coefficients with 90% confidence intervals based on SE clustered at the county level. Panel b displays cumulative effects E_L (as detailed in section 4) with 90% confidence intervals of the sums based on SE clustered at the county level. In panel b, the normalized base period is set to $t - 2$. Data source: Top Shelf Data and Washington ESD.

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 be measured one quarter after the minimum wage hikes. Figure 14 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 subsector (panel a), several counties have relatively high bite for miscellaneous store retailers (panel b). 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 corresponds

⁵⁷The 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.

⁵⁸Eligibility 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.

⁵⁹See Washington State Legislature (1960).

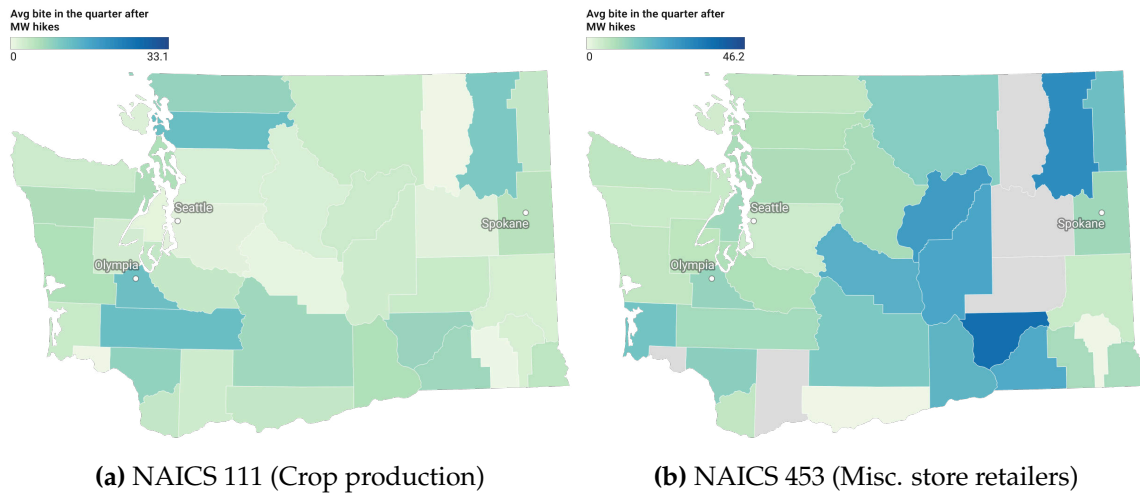
to a wage increase. Moreover, wages slightly above the minimum wage have been shown to be responsive to minimum wage hikes, meaning that the minimum wage hike likely increases wages for exempt employees above the 75% threshold too.⁶⁰

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:

$$\Delta Bite_{k(j)} = Bite_{k(j),Q3,y} - Bite_{k(j),Q1,y+1} \quad (12)$$

This effectively nets out non-compliance and exempt employees at the county level. Tables 15 and 16 show that results are robust to this alternative bite variable.

Fig. 14. Average bite one quarter after the minimum wage hike, 2018-2021



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

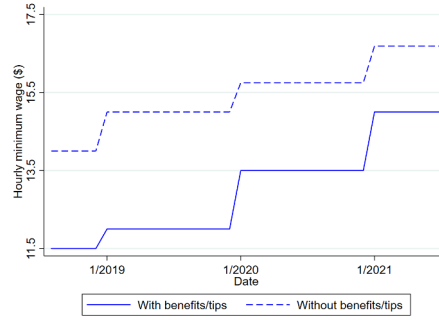
Reverse causality

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

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

which Seattle's minimum wage may be endogenous and reports results that take this potential endogeneity into account.

Fig. 15. 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 Employees who receive health benefits or tips are subject to a lower minimum wage than those who do not (figure 15). 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 entire the sample period. The results in sections 4 and 5 are based on assumption 1.

⁶¹Technically, this only applies to small employers (500 or fewer employees), as large firms (over 500 employees) are subject to a separate minimum wage. However, no cannabis firm has more than 500 employees and the average firm size in King county is 10 employees for NAICS 111 and 11.5 employees for NAICS 453 during the sample period. I therefore omit the large firm minimum wage from my analysis.

⁶²The schedule was determined in 2015.

Assumption 2 (No spillovers to King county)

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

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

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

Assumption 3 (Spillovers to King county)

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

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

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

Table 15 reports results from estimating equation 2 under assumptions 2 and 3 for wholesalers. As columns 3 and 4 illustrate, wholesale price effects are very similar to those obtained in the main paper. Table 16 (columns 3 and 4) shows that the same holds for retail price effects. Taken together, these results suggest that reverse causality from Seattle's minimum wage does not drive my main results.

Bite as treatment intensity

The main results do not rely on interacting bite with the size of the minimum wage hike. To verify this, I estimate a variation of equation 2:

$$\pi_{j,t} = \sum_{l=-6}^5 \beta_l Bite_{k(j),t-l} + X_{k(j),t} + \theta_k + \gamma_t + \epsilon_{j,t}. \quad (13)$$

Here, the treatment intensity is the minimum wage bite and it is not multiplied with $\Delta MW_{j,t-l}$. Figure 16 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 wholesale and retail pass-through is the same as in the main part

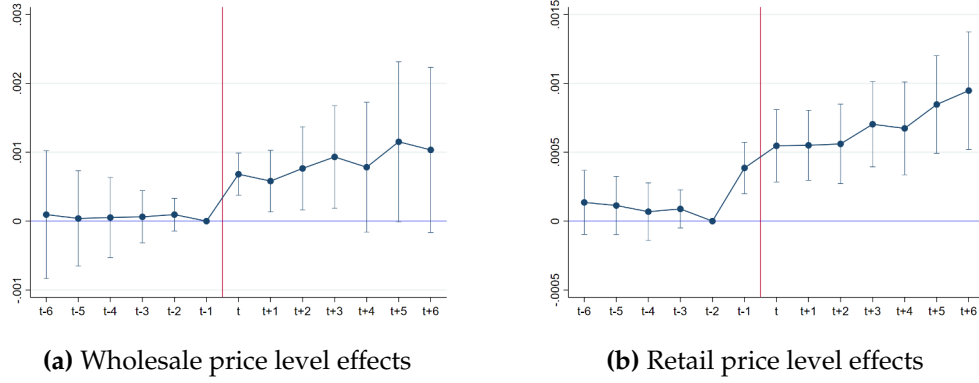
Table 17: Robustness checks for indirect retail price effects

	Trend-adjusted			Unadjusted, establishment FE			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
E_0	0.00546* (0.00289)	0.00512* (0.00269)	0.00726*** (0.00245)	0.00453 (0.00292)	0.00544 (0.00335)	0.00550* (0.00304)	0.00507* (0.00281)
E_2	0.00452 (0.00498)	0.00277 (0.00464)	0.00764* (0.00440)	0.00263 (0.00565)	0.00431 (0.00629)	0.00342 (0.00590)	0.00304 (0.00549)
E_4	0.00491 (0.00428)	0.00218 (0.00379)	0.00732** (0.00364)	-0.000472 (0.00508)	0.00415 (0.00600)	0.00260 (0.00541)	-0.000195 (0.00491)
\sum Pre-event	1.44e-07 (0.00483)	1.01e-07 (0.00503)	-3.08e-08 (0.00512)	0.00427 (0.00528)	0.000251 (0.00518)	-0.000437 (0.00524)	0.00470 (0.00536)
N	13501	13426	13559	13559	13501	13426	13559
Time FE	YES	YES	YES	YES	YES	YES	YES
County FE	NO	NO	NO	NO	NO	NO	NO
Controls	YES	YES	YES	YES	YES	YES	YES
Trend-adjusted	YES	YES	YES	NO	NO	NO	NO

Notes: Dependent variable: monthly establishment-level inflation rate. Listed coefficients are sums E_L of the distributed lag coefficients ψ_l , L months after the minimum wage hikes, relative to the baseline period in $t - 2$. Standard errors of the sums are clustered at the county level and are shown in parentheses. County level controls are the monthly unemployment rate and average monthly wage. For columns 1-3, establishment-level inflation is trend-adjusted. For columns 4-7, establishment-level inflation is unadjusted but establishment FE are included, which removes the pre-trend to a certain extent. (1) uses leads $t - 3$ through $t - 1$ to compute average wholesale expenditure shares. (2) uses all pre-treatment leads for average wholesale expenditure shares. (3) uses leads $t - 4$ through $t - 2$ as in the baseline specification, but jointly weights joint bite: $JB_{r,p,t-l} = \sum_{p=s}^S \alpha_{r,p} \Delta MW_{p,t-l} \times Bite_{k(p),t-l}$. (4) uses the baseline specification, with inflation unadjusted and with establishment FE. (5) is the same as (1) but with inflation unadjusted and with establishment FE. (6) is the same as (2) but with the dependent variable unadjusted and with establishment FE. (7) is same as (3) but with the dependent variable unadjusted and with establishment FE. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Data from Washington ESD and Top Shelf Data, August 2018-July 2021.

of the paper, as wholesale pass-through is approximately twice the size of (direct) retail pass-through two periods after the hikes.

Fig. 16. Direct pass-through with bite-only treatment intensity



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 wholesalers, $t - 2$ 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 13 with time and county fixed effects, estimated separately for wholesalers and retailers. In panel (a), the dependent variable is adjusted for a bite-specific trend as described in Appendix C. Data source: Top Shelf Data and Washington ESD, August 2018-July 2021.

Using a bite variable at the 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 2 with this alternative bite variable in place of the original bite variable in the treatment intensity interaction term $\sum_{l=-6}^5 \beta_l \Delta MW_{j,t-l} \times Bite_{k(j),t-l}$. 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 per-

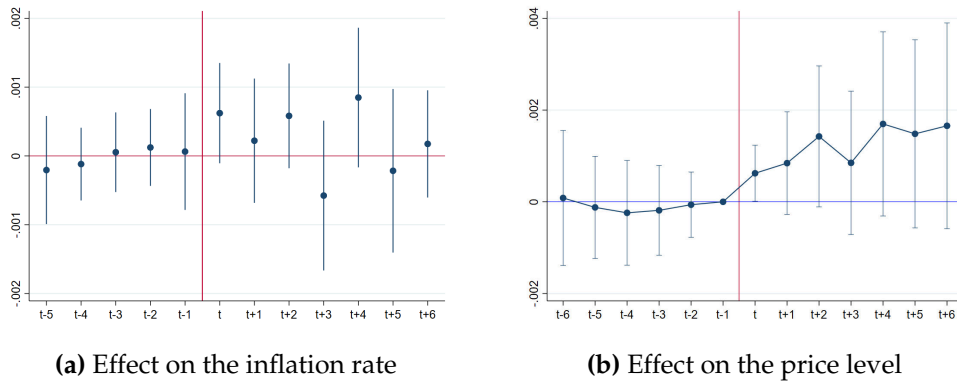
⁶³An alternative would be to use the median wage. Unfortunately, the QCEW does not publish median wages at the detailed industry-by-county level.

mit 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 I 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 17 and 18 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.

Fig. 17. The effect of minimum wage hikes on wholesale prices using 5-digit NAICS bite

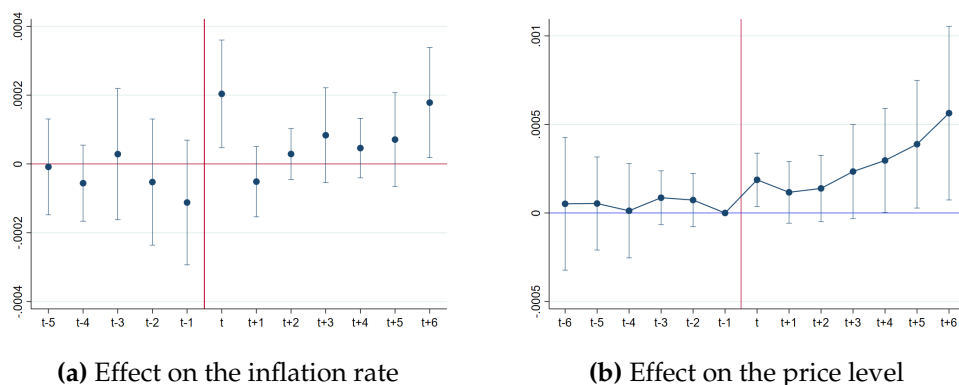


Notes: The figures show estimates from equation 2 with the bite variable based on NAICS 11141 as described in appendix D. Tier 3 producers and producer-processors are omitted from the estimation sample. Equation 2 is estimated with time and county fixed effects. The dependent variable is the establishment-level inflation rate, adjusted for a bite-specific trend as described in section 4.2. 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.

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

⁶⁵NAICS 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, 2017).

Fig. 18. The effect of minimum wage hikes on retail prices using 5-digit NAICS bite



Notes: The figures show estimates from equation 2 with the bite variable based on NAICS 45399 as described in appendix D. Equation 2 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.

E COGS and the labor share of costs for cannabis retailers

This section provides information on the back-of-the-envelope comparison between cannabis retailers' wholesale expenditures and labor expenditures.

Table 18: COGS and the labor share of costs for cannabis retailers

Year	Labor expenditure (per establishment)	COGS (per establishment)	Expenditure ratio
2018	\$324,582	\$501,395	1.54
2019	\$370,897	\$845,563	2.28
2020	\$407,273	\$1,094,652	2.69

Notes: This table compares annual labor expenditure and wholesale expenditure for cannabis retail establishments in Washington state for the years 2018-2020. Labor expenditure equals total wages divided by the number of retail establishments (both obtained from the Washington ESD). 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 excluded. Data from Washington state ESD and Top Shelf Data.

F Construction of the establishment-level markup index

This section describes the construction of the establishment-level markup indexes used in section 6. For each individual product, the markup over marginal input cost (MIC) is defined as:

$$\mu_{i,r,t} = \frac{P_{i,r,t}}{MC_{i,r,t}} \quad (14)$$

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 :

$$I_{c,r,t}^{\mu} = \prod_i \left(\frac{\mu_{i,r,t}}{\mu_{i,r,t-1}} \right)^{\omega_{i,c,y(t)}} \quad (15)$$

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 :

$$I_{r,t}^{\mu} = \prod_c I_{c,r,t}^{\omega_{c,r,y(t)}}. \quad (16)$$

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: $\Delta\mu_{r,t} = \ln I_{r,t}^{\mu}$.

G Labor in the cannabis industry

This section provides a description of labor in the Washington state cannabis industry along with several stylized facts.

The labor intensive nature of cannabis

Cannabis is a labor intensive industry. 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. 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.⁶⁶ This laborious process is compounded by a heavy reliance on indoor cultivation, which is generally considered to be more labor intensive than outdoor production.⁶⁷ 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. Other tasks like filling pre-roll shakers are also largely done by hand.⁶⁸

Cannabis is labor intensive at the retail level as well. Inside every store, a service counter forms a physical barrier between the customer and the products, and the customer can only make a purchase with the help of a qualified sales representative known as a ‘bud-tender’. Budtenders are only permitted to service one customer at a time, and since each consultation can take several minutes depending on the customer’s needs, most retailers employ several budtenders per shift to boost sales volume. These rules are stringently enforced and retailers are subject to frequent compliance audits. Penalties for violations include civil and criminal liability for firm owners, making violations a relatively rare phenomenon (Hansen et al., 2022). Taken together, these regulations suggest that cannabis retailers have high labor intensity compared to similarly-sized retailers in other sectors (Miller & Seo, 2021).

Wages in cannabis

Wages in cannabis are lower than in other industries in Washington state. In 2016, the mean wage for cannabis businesses was less than the overall mean wage in all 36 counties with a cannabis business (Hoagland, Barnes, & Darnell, 2017; Washington State Employment Security Department, 2016). The same pattern held for median wages (Hoagland et al., 2017; Washington State Employment Security Department, 2016). 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-processor level.⁶⁹ Table 19 shows the average annual wage for cannabis establishments for the years 2018-2020 and compares it to the statewide average for all industries. For wholesalers, 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 wholesalers 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 NAICS 453, which amounts to 8% to 11%

⁶⁶Since pollination leads to seed production and inferior buds, the producer is typically forced to discard the entire crop if it becomes cross-pollinated.

⁶⁷Indoor cultivation offers stable growing conditions, year-round harvests, and enables more potent buds (Aizpurua-Olaizola et al. (2016)).

⁶⁸As noted by (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.

⁶⁹Producer-processors 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.

difference.

Table 19: Annual gross wages in the Washington state cannabis industry

Year	Wholesale			Retail			All pri- vate inds.	Min. wage
	Cannabis whole- sale	NAICS 111	NAICS 111419	Cannabis retail	NAICS 453	NAICS 453998		
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.

H Further results

H.1 The geography of wholesale costs

Table 20 shows the percentage of retailers' wholesale costs in relation to a wholesaler's geographic location. Column 1 shows that only 5.22% of retailers' wholesale expenditures go to wholesalers located in the same city as the retailer. Column 2 shows that less than 15% goes to wholesalers 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 wholesalers 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 another region. Column 5 looks at the subset of establishments located in the west and east regions of the state, thus dropping wholesalers in the central region. The east and west regions are non-contiguous and 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.

H.2 Direct retail pass-through with joint bite

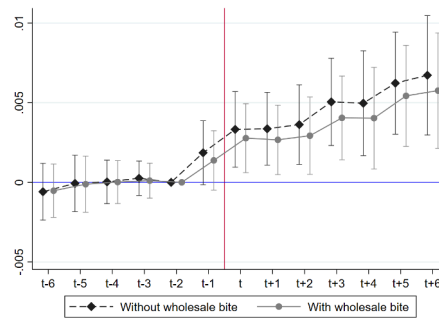
When estimating indirect retail pass-through, one concern is whether the estimates for β_t —the direct pass-through rate—are affected by the inclusion of joint bite as an additional variable. If estimates for direct pass-through were to change, this would cast doubt

Table 20: Share of retailers' wholesale costs by geographic proximity

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

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-months 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.

on the main identification strategy and, by extension, the results from section 4. Figure 19 compares cumulative direct pass-through from equation 4 with and without joint wholesale bite included. Reassuringly, the figure shows that direct pass-through estimates are unaffected by the inclusion of joint wholesale bite. The pre-treatment period is identical and treatment effects appear in $t - 2$ for both specifications. Including joint wholesale bite slightly attenuates the estimates, but the difference is not statistically significant (as evidenced by the overlapping confidence intervals).

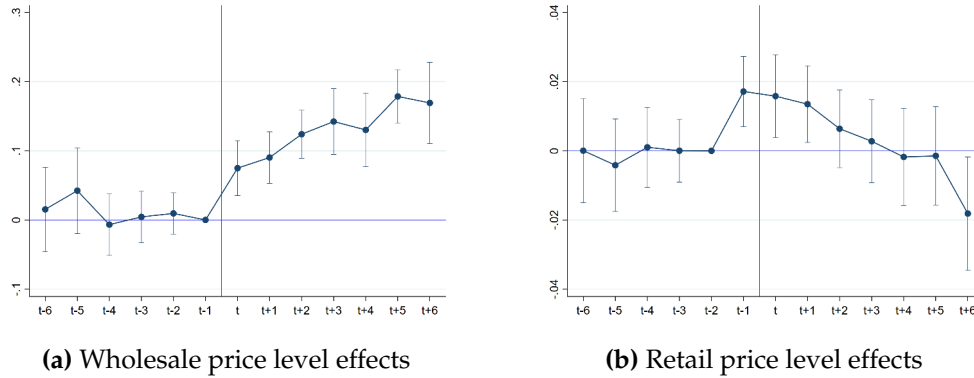
Fig. 19. Direct pass-through with and without joint wholesale bite as a control

Notes: The figure compares cumulative direct price level effects for retailers when joint wholesale bite is included versus omitted from equation 4. 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.

H.3 Legislation vs. implementation for event 3

For event 3, 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 2 for event 3 only. Figure 20 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. 20. Direct pass-through for event 3



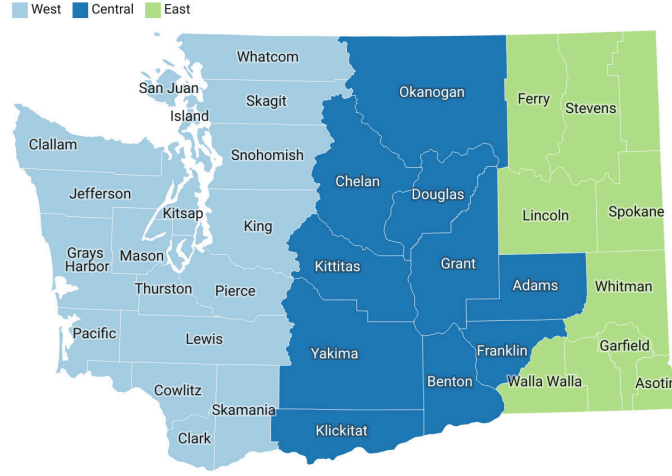
Notes: The figures show cumulative price level effects for event 3 only. Effects are cumulative relative to the normalized baseline period ($t - 1$ for wholesalers, $t - 2$ for retailers). Cumulative effects E_L are obtained by summing the distributed lag coefficients to lead or lag L as detailed in 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 4.2. Data source: Top Shelf Data and Washington ESD, August 2020-July 2021.

I 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 21. To account for time-variant unobserved heterogeneity across these regions I estimate equation 2 with region-time FE (i.e. $\text{region} \times \text{time}$ interaction terms). To ensure the robustness of the $\text{region} \times \text{time}$ specification, I define regions in two different ways. The first is the specification presented in section 5, 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

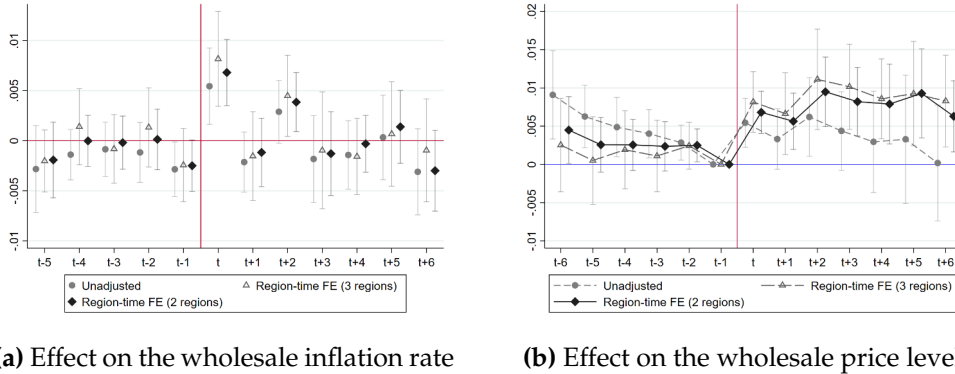
in the average bite depicted in figure 4a in the main part of the paper.

Fig. 21. Socioeconomic regions of Washington state



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

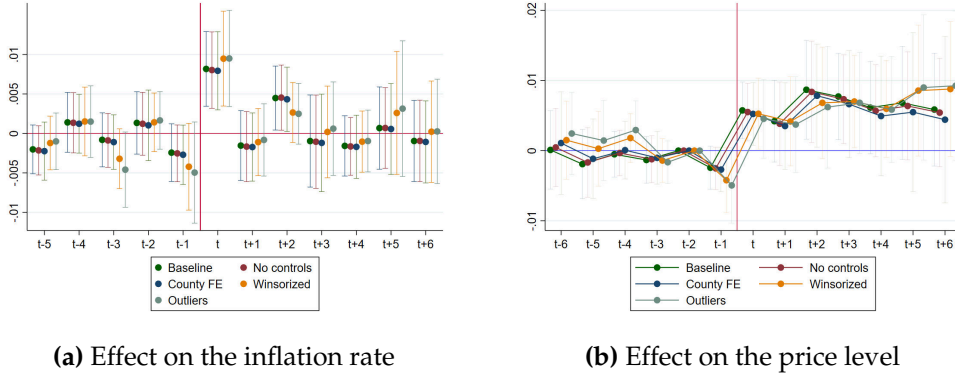
Fig. 22. Comparing region \times time FE specifications



Notes: The figures show estimates from equation 2 under three different specifications: unadjusted, region-time FE based on two regions, and region-time FE based on three regions. The dependent variable is the establishment-level inflation rate. The unadjusted regression is estimated with time and county FE but no controls (as described in the main text); the region-time FE regressions are estimated with county controls but no county FE (as described in the main text). 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) depicts cumulative price level effects (E_L) relative to the baseline period in $t - 2$. 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 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.

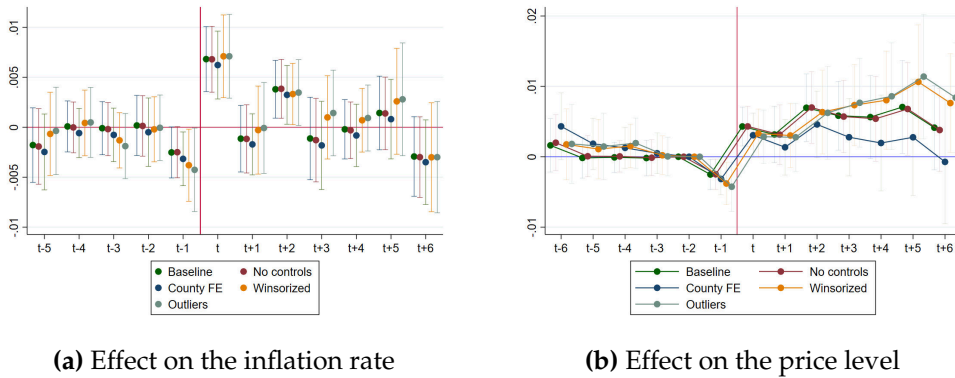
Next, I show that region-time FE remove the pre-trend for the wholesale price pass-through estimates under a variety of other specifications.

Fig. 23. Region-time FE (3 regions)



Notes: The figures show wholesale pass-through estimates from equation 2 with region-time FE. The dependent variable is the establishment-level inflation rate. The three regions are West, Central, and East, as described above. The baseline specification is with county controls; the winsorized specification caps the top/bottom 0.5% of the monthly distribution of inflation rates. Outliers indicates no trimming or winsorizing. Panel (a) shows the estimated distributed lag coefficients, $\hat{\beta}_t$, 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 $t - 2$. 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 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.

Fig. 24. Region-time FE (2 regions)



Notes: The figures show wholesale pass-through estimates from equation 2 with region-time FE. The two regions are West and East, as described above. The dependent variable is the establishment-level inflation rate. The baseline specification is with county controls; the winsorized specification caps the top/bottom 0.5% of the monthly distribution of inflation rates. Outliers indicates no trimming or winsorizing. Panel (a) shows the estimated distributed lag coefficients, $\hat{\beta}_t$, 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 $t - 2$. 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 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.