Title
The Impact of Soda Taxes in the U.S.: Empirical Evidence and Comparison Across Jurisdictions

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The Impact of Soda Taxes in the U.S.: Empirical Evidence and Comparison Across Jurisdictions

By

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DISSERTATION

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Abstract

With obesity rates at epidemic levels in the United States, some public health advocates and policy makers have turned to soda taxes as a way to curb soda consumption. A number of U.S. cities, motivated by health-related or fiscal considerations, have implemented or are considering soda taxes that vary by enactment process, scope, and magnitude. These excise taxes are imposed on sugar-sweetened sodas and non-carbonated drinks such as sport drinks, fruit drinks, and iced teas. In some jurisdictions beverages sweetened with artificial sweeteners are also taxed. This study examines how current soda taxes affect the price and volume sales of soda products and related products. In particular, this study explores not only the short-run effects, but also the long-run effects, an important aspect missing in the literature. I also explore the heterogeneity in the impact across product category and package size, and how responses vary among retailers based on their types. Regressivity is a hotly debated topic surrounding an excise tax. This study investigates whether the tax burdens fall more heavily on the relatively poor and ethnic diverse neighborhoods. I additionally explore whether geographic proximity to neighboring untaxed stores alters the pricing behavior of retailers, as well as their soda sales. Finally, I compare the results among current tax policies and further evaluate their effectiveness.

In the empirical work, I mainly use a large national IRI scanner data and employ both a Difference-in-Differences approach and a Triple-Difference approach to estimate the effects of soda taxes. Results suggest that soda taxes in all taxed jurisdictions had a significantly positive impact on the price of taxed beverages, with pass-through rates less than 100%, and retailers raised prices gradually over time, rather than pass on the tax all at once. These results indicate that the estimates of the impact of soda taxes based on short-term data might be under stated.

The pass-through rates of soda taxes that were passed through a council budget vote are larger than those of taxes that were passed through public referendum. This result is consistent with the hypothesis that the massive media campaigns before an election can draw consumers' attention to the tax and to avoid losing sales, retailers choose not to raise the price significantly in the immediate aftermath. Moreover, the volume sales of soda drinks in Oakland, San Francisco, and Boulder did not decline due to soda taxes, and Berkeley's soda sales did not drop until the fourth year. The main goal of soda taxes enacted by these jurisdictions is to curb soda consumption, and the insignificant consumption effects suggest that establishing soda taxes by public vote may not succeed in achieving the goal of improving public health.

Results also suggest that consumers did not switch to untaxed and healthier beverages in response to soda taxes; they may drive across the city border and purchase sodas in neighboring untaxed stores. In Philadelphia, there is evidence that the sales reduction of taxable beverages was larger in stores near the city border than in those farther away from the border. Given the possibility that the nearby stores of a taxed city can be indirectly affected, future research on soda taxes should be cautious about using them as controls in the empirical analysis.

The impact of soda taxes exhibited huge heterogeneity across store characteristics and product characteristics. Results show that large retailers passed on a smaller proportion of the tax to consumers than small retailers as they may risk losing all profits from consumers' shopping baskets if they raise the price by a large percentage and cause consumers to shop elsewhere, e.g., in untaxed jurisdictions. The pattern of the heterogeneous impact across products also varies by store type. For example, the pass-through rate for large bottles of sodas was greater than that for small bottles at large retailers, while small retailers saw the opposite result. These findings suggest that different types of stores adopt different pricing strategies and consider cross-product relationships when responding to the soda tax. Therefore, it is necessary for future research to analyze at a disaggregated level, or at least distinguish between store types.

Soda taxes might be regressive in relatively large jurisdictions and the results for soda sales suggest that the soda taxes might be less effective at reducing soda consumption among low-income households who tend to purchase more sodas and are more adversely affected by obesity. The tax burdens are also shown to fall more heavily on racially diverse neighborhoods. This study provides insights on how retailers' responses to soda taxes can vary across different dimensions, and how soda taxes of varying scope and objective can differ in terms of the effects and effectiveness. With more jurisdictions considering an introduction of soda tax, the findings of this study provide important implications for future tax design.

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The process of finishing the dissertation was a very valuable experience for me. I have learned a great deal related to academia, and more generally, which affect my future work in positive ways. In particular, the final stage of finishing the dissertation was a special time. I was able to move beyond the anxiety and confusion and am ready to face the new journey.

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Table of Contents

List of Figures

List of Tables

Chapter 1
Introduction

Local communities are increasingly enacting policies to encourage or discourage behaviors. Typical examples include county- or state-level excise taxes levied on the consumption of alcohol, cigarettes, bottled water, etc. Soda taxes are another example. Obesity rates in the United States have been at epidemic levels in recent years. Based on the 2019 data released by the Centers for Disease Control and Prevention's Behavioral Risk Factors Surveillance System (BFRSS), the obesity rate among U.S. adults stands at 42.4% and has increased by 26% since 2008 (TFAH 2020).

Obesity is associated with an increased risk for type 2 diabetes, stroke, high blood pressure, and some cancers and has become a primary public health and policy concern. Parallel to the rise in its rate, obesity imposes a large economic burden on both individuals and nations. Obesity-related diseases cost an estimated $190.2 billion a year, accounting for nearly 21% of annual health care costs in the U.S. (Cawley and Meyerhoefer 2012). Soda drinks are the leading source of added sugar in the American diet and frequent soda consumption is one of the identified contributing factors for the prevalence of obesity (Vartania, Schwartz, and Brownell 2007; USDA and USDHHS 2010).

Public health advocates and policymakers have proposed soda taxes (also called sugar-sweetened beverage or SSB taxes) as a way to curb soda consumption for many years. Currently, several U.S. cities have implemented excise taxes of varying scope and magnitude on soda drinks, including Berkeley, Albany, Oakland, and San Francisco in CA, Boulder in CO, Philadelphia in PA, and Seattle in WA. Most jurisdictions that enacted soda taxes between 2015 and 2018, aimed to improve public health, while others (Philadelphia in PA and Cook County in IL) enacted soda taxes in 2017 primarily to increase revenue. While often called a "soda tax", these taxes apply not only to sodas such as soft drinks or pop, but also apply to many other sugar sweetened drinks such as non-100% juices, sweetened teas, etc. In Philadelphia, a soda tax is also imposed on beverages that contain artificial sweeteners.

The objective of this dissertation is to examine the impact and effectiveness of all soda taxes currently implemented in the U.S.[1] Other jurisdictions (e.g., New York City and Portland) and two states (i.e., Washington and Rhode Island) are considering enacting soda taxes. Thus, an evaluation of the effectiveness of such taxes in reducing SSB consumption and related negative health outcomes is important for providing accurate information on the impact of these taxes as an aid to decision making regarding the imposition of soda taxes in other jurisdictions.

I aim at jointly answering several research questions: (1) How have retailers strategically priced sodas in response to the soda tax policies?; (2) How does a soda tax affect the quantity sold of soda products?; (3) Does a soda tax have "spillover" effects, including the cross-border shopping behavior of consumers and whether the quantity as well as price of untaxed beverages (e.g., bottled water) change due to a soda tax?; (4) The last research question relates to the potential heterogeneous effects on the price and quantity sold of soda products across various dimensions

[1] I do not include Albany in this analysis due to a data limitation. Since the tax in Cook County was repealed four months after it was implemented, I do not include Cook County either.

(e.g., product category, store type, package size). For example, for more popular beverage products (e.g., a 2 Liter Coke), retailers may pass a smaller proportion of a soda tax on to consumers to avoid losing sales. An investigation of how the price and quantity responses vary among products and retailers based on product characteristics and the environment in which retailers operate enables us to fully understand retailers' strategic behavior.

A growing literature has evaluated the effects of soda taxes with mixed results. Most of these studies have focused on the taxes in Berkeley and Philadelphia, while there is little research on the effects of soda taxes enacted by other jurisdictions (Falbe et al. 2016; Cawley and Frisvold 2017; Silver et al. 2017; Rojas and Wang 2021; Seiler, Tuchman, and Yao 2021). I add more evidence of the impact of soda taxes in Oakland, San Francisco, Seattle, and Boulder.

It is difficult to compare the results from these studies due to the wide differences in methodologies, data, and objectives among them. But such comparison is necessary for us to understand how the effects of soda taxes vary across jurisdictions with different demographics and differences in the magnitude and scope of the tax, given that more cities and states are considering a soda tax. To my knowledge, this dissertation is the first comprehensive analysis that includes all taxed jurisdictions.

The data used in the existing research come from self-collected data on price and volume sales, IRI scanner data, and Nielsen scanner data. Self-collected data can introduce measurement errors, selection bias, and potentially underreport soda consumption. More importantly, these studies collected observations or conducted surveys once or twice before the tax implementation and once or twice after that, limiting the data points available for their analyses. As a result, it is difficult to test for the parallel trend assumption as the pre-tax period is too short.

Retail scanner data are more desirable in terms of the sample size and quality of data overall. However, studies that rely on Nielsen data can only identify the 3-digit zip codes for retailers; thus, they have no way to know the specific address of each store (Powell and Leider 2020; Rojas and Wang 2021). As a result, the estimates from using Nielsen data might be biased if an untreated store is considered treated. I utilize IRI store dictionary data that contain the exact address for each store and thus allow me to identify the taxed stores and untaxed stores. The data have been updated to 2019 and cover a large time span (from 2012 to 2019). To my knowledge, this dataset is the largest and most complete so far, which allows me to explore not only the short-run effects of soda taxes but also the long-run effects. It is possible, for example, that retailers adjust their pricing strategy or raise the price gradually to avoid antagonizing consumers.

In terms of the methodology, a difference-in-differences model (DID) is a commonly used approach. But probably due to data limitations, none of the empirical studies that rely on the DID approach has explained how they selected their control groups. The control selection procedure has been largely based on subjective judgment and lacks convincing and quantitative evidence to support the choice. For example, several studies on the Berkeley tax used stores located in nearby cities (San Francisco and Oakland) as controls (Falbe et al. 2015; Falbe et al. 2016; Cawley and Frisvold 2017; Rojas and Wang 2021). But these nearby untaxed stores are likely to be affected by the publicity of pretax campaigns and may also be contaminated by the cross-border shopping behavior. The limited set of stores implies they may not match well with treated stores from Berkeley. Except for Rojas and Wang (2021), none of these studies formally tests for the common-trends assumption during the pretax period. This dissertation illustrates the procedure utilized to select control cities and proves the rationality of the final selection.

The triple-difference approach has been largely ignored by current studies. Its advantage is to remove the potential effects of the unobserved events outside of this study, such as cost increases experienced by treated stores but not by the controls. At the end of the empirical analysis, I conduct additional robustness checks based on triple-difference models.

The analysis regarding the heterogeneity in the impact of a soda tax is relatively rare and no clear patterns emerge. This is, however, of vital importance, especially for our understanding of the strategic pricing behavior of food retailers. Because consumers typically vary in their preferences across products and store types, it is very likely for a retailer to pass more of a tax to one specific product than to another to maximize profits or minimize losses due to the tax. Moreover, the rationale of small food stores might differ from that of supermarkets. These stores target occasional consumption and sell a higher proportion of smaller sizes. In contrast, large supermarkets must be concerned with shoppers' market baskets and potential loss of customers and their market baskets to untaxed rivals.

The heterogeneity in demographics might also influence the impact of the soda tax due to the varying price elasticities. Only two studies about Philadelphia's tax discuss this aspect (Cawley et al. 2020; Seiler, Tuchman, and Yao 2021). Results show that for soda drinks, the pass-through is higher in neighborhoods with a higher rate of poverty – a 10% increase in the poverty rate is associated with a 0.23 cents per ounce increase in the pass-through for regular sodas and a 0.21 cents per ounce increase in the pass-through for diet sodas. Despite its importance, many existing studies that explore the heterogeneity only focused on one or two aspects due to a lack of data. This study explores the heterogeneous effects in greater detail by adding more dimensions to the analysis.

Soda taxes are first imposed on distributors who determine the proportion of the tax they want to pass on to retailers. Due to the lack of data, none of the existing studies on soda taxes has estimated the pass-through rate from distributors to retailers. In other words, the pass-through rates of soda taxes recorded in the literature include both the pass-through of distributors and the pass-through of retailers. Thus, heterogeneity in impacts of soda taxes may be partly due to the heterogeneity in distributors' pass-throughs. For example, distributors' contracts with retailers may be heterogeneous in extent to which price changes are allowed, and also bargaining power relationships will differ across distributors and their downstream buyers.

Previous research on the taxes for tobacco, cigarettes, and food has found evidence that consumers do shop cross-border to avoid a tax (Tosun and Skidmore 2007; Lovenheim 2008; Harding, Leibtag, and Lovenheim 2012). An ignorance of such responses by consumers might lead to an overestimate of a tax's impact on consumption. No studies to date have directly tested for this possibility for Berkeley, Oakland, San Francisco, Boulder, and Seattle by analyzing the untaxed stores adjacent to them. Studies that focus on Philadelphia, however, consider both

Philadelphia and its neighboring areas as treated groups. Results show that the total volume sales of taxed beverages increased by 308 million ounces in the Pennsylvania border zip codes; also, the sales increased by 64,000 ounces per store in stores up to 2 miles away from the city, offsetting the decrease in Philadelphia's volume sales by 24.4% (Roberto et al. 2019; Seiler et al. 2021).

To summarize, this dissertation offers six main contributions to the literature. First, it is the first study to conduct a comprehensive analysis and examine simultaneously the effects of soda taxes that are currently implemented, while the existing empirical studies on soda taxes only focus on one taxed jurisdiction. Especially, I provide comprehensive evidence on the impact of soda taxes in Oakland, San Francisco, Boulder, and Seattle.

Second, the detailed and nationwide scanner data provide multiple feasible controls and allow me to select the most suitable controls based on both graphical and quantitative approaches. With the long time span of the data, I can explore the gradual effects over time. This study is also the first to use scanner sales data to explore the impact of soda taxes in Oakland, and San Francisco. Third, I explain in detail how the control cities were selected and avoid using the nearby untaxed stores as controls.

Fourth, to strengthen the identification strategy, I estimate triple-difference specifications comparing soda to sales of other beverages not affected by taxes. Fifth, this dissertation analyzes the heterogeneity in the impact on the price and quantity of soda taxes across various dimensions simultaneously (e.g., store location, store type, product category, package size). I further discuss retailers' strategic responses to soda taxes based on the results, a crucial aspect missing from the literature. Lastly, this study explores the cross-border shopping behavior for all taxed jurisdictions, especially for Berkeley, Oakland, San Francisco, Boulder, and Seattle.

The remainder of the dissertation is organized as follows. Chapter 2 introduces the main features of each soda tax policy, including tax rates, enactment processes, taxable beverages, taxpayers, etc. Chapter 3 surveys the existing literature on soda taxes and provides a summary of data utilized, methodologies, and findings regarding both the average impact and heterogeneous impact of soda taxes. Chapter 4 provides a theoretical foundation for the analysis. In Chapter 5, I describe the five data sources and the information used in the empirical analysis. This chapter also reports descriptive statistics for all beverages included in the analysis. Chapter 6 describes the empirical approach and specific estimation models. First, I present the difference-in-differences models used to estimate the average impact and heterogeneous impact. I then provide details on the control selection process. A triple-difference approach utilized as an additional robustness check is described at the end of this chapter. Chapter 7 presents the estimation results. I mainly report the results for the average impact and heterogeneous impact based on difference-in-differences models but also include additional robustness checks by estimating the tripe-difference model. Chapter 8 concludes.

Chapter 2
Soda Tax Policies in the U.S.

In November 2014, Berkeley, CA passed the first excise tax on the distribution of sugar-sweetened beverages. Since then, Philadelphia, PA, Oakland, CA, Boulder, CO, San Francisco, CA, and Seattle, WA have implemented similar taxes. This chapter first introduces the essential features common to all taxes, including taxable beverages, taxpayers, etc., and then examines the unique features of each policy, including the voting process, tax rates, advocacy, and advertising efforts.

All but Philadelphia primarily focus on reducing the human and economic costs of health outcomes associated with the consumption of SSBs. They aim at discouraging SSB distribution and consumption by raising the price. In addition, the tax revenue collected will be used to fund programs that expand access to healthy food, nutrition education, etc. For example, Seattle will fund public awareness campaigns that highlight the impact of SSBs on health outcomes. In addition to the price effects, the tax in Seattle may exhibit informational effects. The primary goal for Philadelphia's tax, however, is to raise tax revenue for general purposes such as pre-K, community schools, recreation centers, etc. While current soda taxes are similar in terms of taxpayers, taxable products, etc., they differ in important ways that can influence how taxes are processed and can lead to different health and economic outcomes. These distinctions also influence how this study evaluates the policies.

All soda taxes currently implemented are excise taxes and they are imposed on distributors. This means that soda taxes do not directly apply to the retail sale to consumers. But the distribution can happen within a single business entity, such as by a wholesale or warehousing unit to a retail outlet or between two or more employees. If there is a chain of distribution within a jurisdiction involving more than one distributor, the tax should be levied on the first one. If the tax is not paid, it should be paid by subsequent distributors, provided that the distribution of taxable products is not taxed more than once in the chain of commerce. Distributors must register in each jurisdiction and will be taxed when making their first non-exempt delivery of taxable products within a jurisdiction.

Philadelphia's tax base is larger than the other jurisdictions' because it includes any beverage with added caloric sweeteners or artificial sugar substitutes. In the other five jurisdictions, only sugar-sweetened beverages and added caloric sweeteners are subject to a soda tax.[2] Sugar-sweetened beverages are defined as non-alcoholic beverages to which one or more caloric sweeteners have been added. They include but are not limited to all drinks and beverages commonly referred to as "soda," "pop," "cola," "soft drinks," "sports drinks," "energy drinks," "sweetened ice teas," "non-100% fruit drinks," etc. Thus, Philadelphia is the only jurisdiction that imposes a tax on diet sodas.

Some jurisdictions only tax beverages if the beverage surpasses a calorie minimum (e.g., 2 calories per fluid ounce in California and 0.42 grams of caloric sweeteners per fluid ounce in Boulder). Each jurisdiction exempts some beverages from its tax, including alcoholic beverages, any beverage for medical use, any milk product, infant or baby formula, any liquid sold as a meal replacement, and 100% natural fruit/vegetable juice.[3] Tax rates are based on the volume in ounces.

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[2] An added caloric sweetener means any substance or combination of substances that adds calories to the diet if consumed. Thus, any powder or syrup that contains one or more added caloric sweeteners as an ingredient intended to be used in making an SSB (e.g., fountain drinks from beverage-dispensing machines or made by retailers) by combining with one or more other ingredients is also subject to the tax. Added caloric sweeteners include, without limitation, sucrose, fructose, glucose, other sugars, and high fructose corn syrup but do not include a substance that exclusively contains natural, concentrated, or reconstituted fruit or vegetable juice or any combination thereof. In addition, soda taxes are not imposed on natural common sweeteners that are used by the consumer himself or herself, such as granulated white sugar, brown sugar, honey, molasses, xylem sap of maple trees, or agave nectar.

[3] Milk products are defined as products whose principal ingredient is milk. Rice milk, almond milk, and cashew milk may be taxed in Philadelphia if they contain any sweetener as an ingredient, however.

For added caloric sweeteners, the tax is typically applied to the largest volume of SSBs that could be produced from the sweeteners based on the manufacturer's instructions. The following sections describe the tax rate and passage process of each jurisdiction.

2.1 Berkeley

In November 2014, Berkeley passed the first U.S. soda tax (known as Measure D) of one cent ($0.01) per fluid ounce on SSBs. The Measure D soda tax was approved by 76% of voters and took effect on March 1, 2015. This measure provides a small business exemption for retailers who transport sugar-sweetened beverage products into Berkeley themselves and then sell those products directly to consumers.

The tax was passed after a vigorous campaign. An estimated $1 million was spent by supporters and $2.5 million was spent by the soda industry to defeat the measure, with ads appearing on billboards, in subway stations, at bus stops, on television, and in local newspapers. Even though revenues generated enter the general funds of the City of Berkeley, the measure establishes a Sugar-Sweetened Beverage Product Panel of Experts, comprised of experts in the areas of public health, child nutrition, nutrition education, and food-access programs. The Panel makes recommendations regarding which programs should be established and funded to further reduce the consumption of sugar-sweetened beverages in Berkeley.

2.2 San Francisco

The City and County of San Francisco passed a one-cent ($0.01) per fluid ounce soda tax (Prop V) on November 8, 2016, by public referendum with over 61% in favor. The tax went into effect on January 1, 2018. In addition to the distributors, the measure taxes retailers who obtain SSBs outside the city and bring them into the city themselves (called self-distribution).

As with Berkeley's tax, Prop V was passed after a bitter election campaign. The soda industry spent almost $20 million to defeat the initiative. San Francisco proposed its first soda tax in 2014. Because the referendum received 55% of the vote, short of the two-thirds required for a referendum directing money to a specific item, it was voted down. In 2016, San Francisco used Berkeley's "soft earmark" approach which only required a majority vote, and successfully passed its soda tax. Proceeds of San Francisco's tax are deposited in the general fund. But revenues from the tax are supposed to be directed toward public health.

2.3 Oakland

On November 8, 2016, Oakland voters passed a soda tax (Measure HH) with 61.35% in favor. The magnitude of the tax is also one cent ($0.01) per fluid ounce. Collection of the tax began on July 1, 2017. This measure exempts retailers who transfer SSB products into the city themselves. The passage of Measure HH also occurred after a series of soda tax campaigns. Opponents of the measure argued that the soda tax is a "grocery tax." The American Beverage Association (ABA) paid $7.5 million to the "No on HH" campaign, while proponents raised $10.4 million. The battle over soda taxes in the Bay Area was expensive, with total contributions reaching more than $54 million.

The tax revenue is deposited into the City's general fund and used for any lawful government purpose. However, like Berkeley, Measure HH establishes a Community Advisory Board, which is responsible for making recommendations to the City Council on setting up and/or funding programs that prevent or reduce the health consequences of consuming sugar-sweetened beverages.

2.4 Boulder

On November 8, 2016, the City of Boulder passed the largest SSB tax (Measure 2H) of two cents ($0.02) per ounce with 54% of the vote. This tax went into effect on July 1, 2017. Unlike in

Berkeley and Oakland, small businesses are not exempted from the tax. Measure 2H is called the most expensive municipal election in Boulder's history. A total of $1 million dollars was spent by supporters (e.g., public health activists) and opponents (e.g., ABA). Compared with the three jurisdictions in the Bay Area, there was much less media coverage about the pretax campaign.

Once again, the revenue from the tax is to be used for health promotion, general wellness programs, and chronic disease prevention that improve health equity, and other health programs, especially for residents with low income and those most affected by chronic disease linked to SSB consumption.

2.5 Seattle

The SSB tax passed in Seattle differs from California jurisdictions and Boulder in that it was passed through a city council vote. On June 5, 2017, Seattle's City Council voted 7-1 to pass a 1.75 cents per fluid ounce tax. This tax exempts manufacturers, who are also distributors, with total gross sales of less than $2 million and taxes those with total gross sales between $2 million and $5 million at a lower tax rate (i.e., $0.01 per ounce). Seattle is the only jurisdiction that imposes two levels of tax rates. The tax started on January 1, 2018.

The revenue collected from the tax is to be used to fulfill its goal of expanding the access of low-income families to healthy food. The Sweetened Beverage Tax Community Advisory Board makes recommendations on how and to what extent the City Council should establish programs consistent with the intent of the tax.

2.6 Philadelphia

On June 16, 2016, the Philadelphia City Council approved a 1.5 cents per ounce tax with a vote of 13-4. The tax went into effect on January 1, 2017. This tax was established after months of debate around a proposal by the mayor in March 2016. Small businesses are not exempted from the tax.

Much money was spent on lobbying Philadelphia City Council from both supporters and opponents. The ABA ran local television, radio, and newspaper advertisements against the proposal and spent $10.6 million in 2016 in an effort against the tax. On the other hand, Philadelphians for a Fair Future, a pro-soda tax group founded by allies of Mayor Jim Kenney, forked over $2.2 million. The American Heart Association spent an additional $334,000 in support of the tax. Both sides of the fight paid for TV advertisements, phone banks, and organizing.

2.7 Implications for the Effects of the Tax

The differences among current soda taxes in terms of the magnitude, decision process, tax base, etc. could have important implications for the effectiveness and impact of the tax. One obvious distinction is the magnitude of a soda tax. A larger tax rate (e.g., the two-cent per ounce tax in Boulder) tends to cause a larger price change. If the own-price elasticity for SSBs is constant, this implies a larger reduction in SSB consumption in jurisdictions with higher tax rates. In addition, the price elasticity may vary by the beverage category, the average price, consumer demographics, etc. Additionally, a more salient price change can increase consumers' awareness of the existence of a soda tax, thereby leading to stronger consumer response.

Implementation of soda taxes in the Bay Area and Boulder was preceded with significant debates in the media about the adverse health outcomes of consuming sodas and its association with obesity. A growing literature suggests that informational campaigns and media coverage can influence consumer behavior (e.g., Kiesel 2012; Cornelsen and Smith 2018; Taylor et al. 2019). First, media coverage may reduce the pass-through rate as it draws consumers' attention to the tax and price change. Moreover, consumers may become more aware of the health risks of drinking sodas and reduce their demand for them, despite small price changes. Information effect of a soda tax could therefore play a significant role in the Bay Area and Boulder and might vary in intensity based on the amount of funding allocated to campaigns and media coverage (e.g., $1 million in Boulder vs $18 million in Oakland) and activities.

Instead, I expect the impacts of soda taxes in Philadelphia and Seattle voted through the council to be dominated by price effects. Despite that interest groups spent millions of dollars in a "lobbying war" before the council vote, Philadelphia's soda tax primarily focuses on funding education programs (i.e., pre-K for the city's 3- and 4-year-olds, community schools, and a bond to pay for park and recreation center upgrades), instead of improving health outcomes.

Lastly, the fact that Philadelphia is the only jurisdiction that taxes diet drinks implies different substitution patterns between Philadelphia and other jurisdictions. Diet drinks are considered the closest substitutes for sodas. Therefore, I would expect a substitution towards diet drinks and/or other untaxed beverages in jurisdictions except for Philadelphia. Consumers in Philadelphia, however, may switch to pure water, natural fruit/vegetable juices, or just reduce their soda consumption.

Chapter 3
Literature Review

The soda tax and its effectiveness have been debated for a long time. In the beginning, many studies based their discussion on an estimation of short- and long-term tax elasticities (Zhen et al. 2011; Zhen et al. 2013; Zheng, McLaughlin, and Kaiser 2013; Harding and Lovenheim 2014; Zhen, Brissette, and Ruff 2014; Wang 2015). However, a single estimate of tax elasticities may provide an imprecise measure of how consumers and retailers truly respond to the tax. For example, consumers can cross borders to purchase at nearby untaxed stores.

In recent years, a growing body of literature focuses on evaluating the actual effects of currently implemented soda taxes. Earlier research focuses exclusively on Berkeley's tax. With more soda taxes established, the effectiveness of the taxes in other jurisdictions, including Boulder and Philadelphia, has also been analyzed.

This chapter surveys the existing literature on soda taxes and provides a summary of findings with regard to the impact of soda taxes implemented in Berkeley, Boulder, and Philadelphia. It discusses what is already known about the average pass-through of the tax, as well as heterogeneity in the impact along various dimensions such as store type, product category, etc., documents tax avoidance behavior such as cross-border shopping and concludes with a summary of this study's contributions to the literature.

3.1 Data and Methodology

Existing studies draw on self-collected data on prices from retailers (Falbe et al. 2015; Cawley and Frisvold 2017; Taylor et al. 2019; Cawley et al. 2020), survey data on consumption (Falbe et al. 2016; Cawley et al. 2020), as well as scanner data, including IRI retail data (Roberto et al. 2019;

Seiler, Tuchman, and Yao 2021), Nielsen retail data (Bollinger and Sexton 2018; Rojas and Wang 2021), and Nielsen Homescan (Debnam 2017). Silver et al. (2017) use all three methods to collect data. All but one of the studies employ a difference-in-differences approach, and two of them also use the synthetic control method (Bollinger and Sexton 2018; Rojas and Wang 2021).

A difference-in-differences approach compares the changes in outcomes in a taxed jurisdiction over time to those in the control city. To control for any time-invariant heterogeneity across different dimensions, these studies include fixed effects into their models, such as product fixed effects, store fixed effects, time fixed effects, etc. Debnam (2017), however, uses a "fuzzy" regression discontinuity design. This method identifies changes in aggregate consumption of taxed beverages by residents in Berkeley; there is no need for a control group with this method.

3.2 Average Overall Impact of Soda Taxes on Taxed Beverages

As the first soda tax was implemented, Berkeley's soda tax has been hotly debated both in the literature and in the media. On average, the estimated pass-through rate of the tax for all taxed beverages ranges from 19% through 67%, depending on the data source and methodology used. Analyses based on scanner data (Nielsen retail data) suggest a low pass-through (Rojas and Wang 2021), while studies using hand-collected data report relatively large price changes (Falbe et al. 2015; Falbe et al. 2016; Cawley and Frisvold 2017). This might be due to the limited access to data by these studies. For example, data that were collected came from certain geographical areas and involved a limited number of products. Falbe et al. (2016) used data from low-income neighborhoods and focused on small single products, reporting a pass-through of 47%. This result may be caused by the fact that a single serving (e.g., a 20-oz bottle) is typically intended for impulse buying and thus characterized by the low price elasticity of demand relative to a large size.

In addition, previous studies suggest that lower-income households exhibit a lower own-price elasticity of demand for sodas than higher-income households (Debnam 2017). While these differences clearly suggest a possible heterogeneous impact of taxes, the retail prices for taxed beverages in Berkeley seem to have changed only marginally as a result of the soda tax. One possible explanation is consumers' ability to evade a city-level tax by cross-border shopping. Cross-border shopping is relatively easy in a small city like Berkeley, especially when it is acting as a "bedroom" community for many residents who work, and thus can shop, in other jurisdictions. This possibility would make the demand curve for taxed beverages in Berkeley more elastic. In light of this, Berkeley's retailers may have shifted less of the tax on consumers, fearing loss of sales, not only of taxed products but also other items in consumers' market baskets that could be lost through cross-border shopping. The media coverage during the pre-tax period may also suppress the price increase by drawing consumers' attention to price changes.

Another explanation might be that small food retailers are less likely to insulate consumers from price increases than large supermarket chains, which tend to use Everyday Low Prices as a pricing strategy. Large chains also practice zone pricing; thus, if Berkeley is part of a larger zone for a chain, the price for the zone may not have been changed due to a tax in one location (DellaVigna and Gentzkow 2019).

In terms of the impact on soda consumption, results based on scanner data suggest no significant effects, or if any, a low negative percentage change in volume sales (-9.6%) (Rojas and Wang 2021). This result appears to be consistent with a low average pass-through mentioned above. Results based on survey data, however, report a 21% reduction in soda sales of low-income households (Falbe et al. 2016).

After the passage of the soda tax in Berkeley, several jurisdictions followed the Berkeley model and established similar taxes by public referendum, including Boulder, San Francisco, and

Oakland. But only the tax in Boulder has been analyzed so far. Cawley et al. (2021) based their analysis on hand-collected data from both retailers and restaurants and found that the average pass-through was 50.9% after one month and 51.1% after three months when using posted prices. The authors also collected register prices as some retailers only add the tax at the register; the corresponding pass-through is 78.9% after one month and remains constant thereafter, much larger than the estimates from posted prices. Ignoring these decisions (e.g., whether to include the tax in the shelf price or add at the register) may lead to a substantial underestimate of the pass-through.

These results also point to an important hypothesis – that is, higher prices due to the tax do not necessarily cause a significant change in soda consumption if the price increase is less salient to consumers. Unfortunately, this study didn't explore the impact on consumption for Boulder. Compared with the results for Berkeley, the average pass-through rate of a soda tax is higher in Boulder. Additionally, the magnitude of Boulder's tax is twice as large as Berkeley's, the biggest difference between these two policies. If considering this difference, the average price change due to the tax in Boulder appears to be much larger than in Berkeley.

The tax established in Philadelphia has also been hotly debated and varies from the taxes implemented elsewhere. As previously discussed, Philadelphia is the only jurisdiction that levies an excise tax on both diet and regular sodas and passed the tax through a council budget vote instead of a public referendum. Philadelphia is a large and demographically diverse city that is served by many different types of stores and chains, providing a desirable research setting for analyzing the heterogeneous effects of the tax.

Results show that across all taxed beverages (diet and regular), the pass-through rate of Philadelphia's tax ranges from 97% to 104%, with a 56% estimate at the airport (Cawley et al. 2020; Seiler, Tuchman, and Yao 2021). Thus, Philadelphia's tax has caused a larger increase in the prices of taxed beverages than the tax in Berkeley. Possible reasons for this difference include: (1) the magnitude of Philadelphia's tax is larger; (2) Philadelphia is eight times the size of Berkeley, making it costly for consumers living in the city center to cross the border; (3) diet sodas, which appear to be the closest substitutes for regular sodas, are also taxed. Both (2) and (3) mean that it might be difficult for consumers in Philadelphia to avoid the tax. Additionally, the goal of the tax in Philadelphia is to raise revenue and the tax was passed through a simple council budget vote, instead of a public referendum. Thus, there was less information available in the media and public space about the soda tax and about the health outcomes of drinking sodas. Consumers' preferences for sodas might not have been influenced, unlike in Berkeley.

With respect to the impact on soda consumption, results based on survey data show that the purchases of taxed beverages decreased by 8.9 ounces per shopping trip, with low-income consumers reducing their purchases by 12.7 ounces per shopping trip (Cawley et al. 2020). The study based on IRI retail data reports a large decrease (51%) in the total volume sales of taxed beverages in Philadelphia after the tax implementation (Roberto et al. 2019). These results are in line with the documented large pass-through rate of Philadelphia's tax.

3.3 Impact on Untaxed Beverage Categories

The slight increase in the soda prices of Berkeley implies that the change in the consumption of untaxed beverages is also likely to be small. But besides the price effects, Berkeley's soda tax can also affect consumption through informational effects. The media campaigns before the election may have conveyed information about the unhealthy outcomes of drinking sodas and encouraged consumers to choose healthier beverages, reducing soda consumption. So far, research analyzing the substitution effect caused by the soda tax is relatively rare.

The existing research shows that on average the sales of untaxed beverages rose by only

3.5% in Berkeley stores; the sales of water rose by 15.6%; the sales of untaxed fruit, vegetables, and tea drinks together rose by 4.37%, and plain milk sales rose by 0.63%; the sales of diet soft drinks and energy drinks, however, declined by 9.2% compared to their counterfactuals sold in control stores (Silver et al. 2017). The consumption of untaxed beverages (except water) experienced a small and positive percentage change after the tax implementation. One reason for the relatively larger increase in water sales might be the informational effects discussed earlier. Cawley et al. (2021) estimated the impact of the tax on the price of untaxed beverages in Boulder but did not find significant changes in prices relative to those in control cities. Unfortunately, this study did not explore the change in consumption of untaxed beverages caused by the soda tax.

In Philadelphia, Seiler, Tuchman, and Yao (2021) found a statistically significant increase of 9% in the sales of natural juices compared with the control city, whereas the sales of bottled water did not change significantly. This may imply that natural juices are a closer substitute for soda drinks than water due to their sweet taste. Further, studies that used a difference-in-differences approach found a slight and significant increase in the average price of untaxed beverages, with an increase of 0.1 cents per ounce at supermarkets and an increase of 0.14 cents per ounce at pharmacies (Roberto et al. 2019).

When it comes to the different impact across product categories, Seiler, Tuchman, and Yao (2021) found that prices significantly increased by 0.34 cents per ounce for natural juices and by 0.03 cents per ounce for bottled water relative to the controls, although water experienced no significant change in the sales. The larger increase in the average price of natural juices could be an equilibrium response to the increased demand for natural juices due to consumers substituting away from taxed beverages. Cawley et al. (2020) also estimated the price changes for water and juice relative to those in control cities and reported much larger estimates, with 0.43 cents per ounce increase for water and 1.09 cents per ounce increase for juice. Both coefficients are statistically significant. The results from Cawley et al. (2020) and Seiler, Tuchman, and Yao (2021) point to the same conclusion: juices experienced a larger increase in price than water.

3.4 Heterogeneous Effects by Store Type

The pass-through of the soda tax may vary by store type, beverage category, bottle size, etc. Current studies find no evidence of the impact of Berkeley's tax on prices or volume sales at chain drug stores. Even though Silver et al. (2017) report a 45% pass-through rate, the study used a simple comparison of pre-taxation and post-taxation measures of beverage prices, with the absence of a control group. The possible reasons for this result include that chain drug stores may have used regional (rather than store-specific) pricing, distributed tax-related costs across multiple products, or absorbed costs.

For chain supermarkets, there is evidence of a low pass-through of Berkeley's tax for SSBs (8% - 18%), and limited evidence of the consumption reduction (7% - 12% from DID models, and zero from a synthetic control method) (Rojas and Wang 2021). Compared with the average impact on the price and volume sales across all types of stores, the pass-through rates for chain drug stores and chain supermarkets appear to be low. It may be due to the zone pricing strategy pursued by chain retailers. Previous literature shows that chain retailers tend to perform uniform store pricing due to "managerial menu costs," which constrains their willingness to shift tax costs to consumer prices (DellaVigna and Gentzkow 2019). In small grocery stores and liquor stores, however, the pass-through rates for taxed SSBs are much higher than in other types of stores in Berkeley (42% and 97%, respectively) (Silver et al. 2017).

Consistent with the results for Berkeley, the pass-through estimates for Boulder are smaller at pharmacies (52%) and grocery stores (64%) than at liquor and convenience stores (84% and

99%, respectively) (Cawley et al. 2021). The pass-throughs could vary by store type if the elasticities of demand and supply differ across store types, due to differences in the stores' marginal costs or due to differences in their consumer bases.

Relative to pharmacies, small grocery stores, and gas stations, the pass-through rates at large grocery stores or supermarkets are small (43% - 79%) in Philadelphia (Roberto et al. 2019; Cawley et al. 2020). One possible explanation may be that consumers who shop at large food stores tend to purchase all products on the shopping list; thus, if large retailers increase the prices for one category (e.g., sodas), they may lose consumers' entire shopping baskets. When it comes to chain retailers, the pass-through rates appear to be similar across different stores. Despite relatively low pass-throughs found at large grocery stores and supermarkets, the volume sales of sodas at large food stores declined more than at small retailers. For example, the volume sales of taxed beverages at the mean supermarket decreased by 58.7% and at mass merchandise stores, they decreased by 40.4%; pharmacies, however, experienced a volume decrease of only 12.6% among taxed beverages (Roberto et al. 2019).

It is worth noting once more that soda taxes are imposed on distributors, who possibly absorb some of this tax and only pass part of the tax to retailers. Thus, the partial pass-throughs documented in the literature can be a result of both distributor and retailer behavior. Different contractual agreements might contribute to the heterogeneity in the effects reported in the literature.

3.5 Heterogeneous Effects by Product Characteristics

Only a few studies explored heterogeneity in the impact across beverage categories. I first discuss the results for Berkeley. Compared with other categories, the average pass-through for regular soda ranges from 24% to 30%. For regular soda and energy drinks together, the tax of Berkeley was fully passed through with a 1.09 cents per ounce increase based on scanner data (Silver et al. 2017).

This implies an over-shift of Berkeley's tax for energy drinks. No studies about Berkeley have analyzed the average pass-through rate for fruit drinks or sweetened teas in large sizes. But for small pack-sizes, the pass-through rate for fruit-flavored beverages is 0.47 cents per ounce and the pass-through for sweetened teas is 0.32 cents per ounce, smaller than that of soda drinks (Falbe et al. 2015).

When it comes to the impact on volume sales, the sales change for regular soda due to the tax of Berkeley is the least. Evidence shows that the consumption of regular soda didn't fall or decreased by 26%; the consumption of sports drinks, however, decreased by 36% after Berkeley's tax was implemented (Falbe et al. 2016). All these coefficients are significant. This result might be due to the lower price elasticity for regular soda than that for sports drinks.

Fountain drinks created from added-calorie sweeteners are also subject to a soda tax. In the research on Berkeley, no study has collected the sales data on fountain drinks and analyzed the impact on them. Cawley et al. (2021), however, found that the tax in Boulder was over-shifted onto fountain drinks' retail prices, with a 140% pass-through; this estimate is significant and much higher than that of SSBs.

In Philadelphia, the impact of the tax on sodas (both regular and diet versions) is the largest, among all taxed beverages. The average pass-through for sodas is complete (100%) at large grocery stores after one year (Cawley et al. 2020), and the volume sales decreased by 15.28% per store in Philadelphia after one year and nine months (Seiler, Tuchman, and Yao 2021). But when considering cross-border shopping, this sales decrease for sodas is offset by a larger increase (34.94% per store) in the neighboring untaxed stores (Seiler, Tuchman, and Yao 2021). For taxed juice and tea/coffee, an aggregate reduction in demand (3.33% per store) was seen even after accounting for the sales increase in bordering areas; taxed water (e.g., vitamin water) and energy drinks, however, only experienced a small sales reduction in Philadelphia (3.18% per store) (Seiler, Tuchman, and Yao 2021).

In addition, the literature documents that the distance to a nearby untaxed store is positively correlated with the pass-through rate of Philadelphia's tax for regular soda. A one-minute increase in travel time to the nearest untaxed retailer causes a roughly 0.5 cents per ounce increase in the price. For diet soda, however, the impact of the travel time on the pass-through is not statistically different from zero (Seiler, Tuchman, and Yao 2021). This implies that retailers take consumers' cross-border shopping behavior into account when making pricing decisions for regular soda.

It is worth noting that the results discussed so far are generated from models using neighboring untaxed stores as the control and might be imprecise if cross-border shopping occurs. Roberto et al. (2019), who used Baltimore as control for Philadelphia, report a lower pass-through rate for sodas, with 41% for SSBs and 53% for diet sodas. Also, the volume sales of SSBs sold at supermarkets in Philadelphia are estimated to decline by 2.41 million ounces (about 200,000 12-ounce cans); for artificially sweetened beverages, this estimate is 432,137 ounces (about 36,011 12-ounce cans). A simple comparison between the pass-throughs obtained by Roberto et al. (2019) and those discussed earlier suggests that using neighboring untaxed stores as controls might overestimate the pass-through of the tax in Philadelphia. It is very likely for neighboring untaxed retailers to lower the price to attract more consumers, especially those living far away from the city border.

Two studies based on self-collected data estimate the pass-through rates for popular brands of SSBs in Berkeley. Cawley and Frisvold (2017) find that the pass-throughs for Coke (39%) and Pepsi (41%) are lower than the average rate of 43%; but the estimate for Mountain Dew (PepsiCo) is 45%, a little bit larger than the average pass-through. This is consistent with the hypothesis that retailers pass less of a soda tax to more popular brands to avoid losing sales. Using data collected from low-income neighborhoods, Falbe et al. (2015) report above-average pass-throughs for single bottles of popular brands (55% - 83%). This result might be caused by the low-price elasticity of demand for sodas among poorer consumers. Also, single bottles of beverages are often intended for impulse purchasing and exhibit a low price elasticity.

An analysis of the heterogeneous impact across product sizes has potentially important implications for our understanding of the health and economic outcomes of a soda tax as well. The goal of Berkeley's tax is to curb soda consumption, thereby improving the public health. The key to achieving this goal lies in a considerable decrease in soda drinks consumed by heavy soda drinkers. This type of consumer tends to buy large pack-sizes of soda drinks. Due to the high demand for sodas during one shopping trip, high-consuming individuals might cross the city border and visit retailers located in the untaxed jurisdiction when facing increased prices. Moreover, if consumers prefer "one-stop" shopping – that is, purchasing all products they need at one store, the retailer (e.g., a supermarket) risks a loss of a customer and his or her entire shopping basket if the retailer raises the prices of large pack-sizes of sodas. Therefore, the pass-through rates for large sizes may be lower than those for small ones. This theory is supported by the empirical evidence found by current studies. In Berkeley, the pass-through rate for 20-ounce bottles is estimated to be 45%, while the pass-through for large pack-sizes ranges from 33% to 44% (Falbe et al. 2015; Cawley and Frisvold 2017).

Another possible explanation can be the "Loss Leader" theory (Volpe, Risch, and Boland 2017). Specifically, retailers use large sizes of soda as loss leaders by using temporary promotional pricing, attracting more consumers to purchase soda products or other products needed by consumers. There is evidence in the literature showing that the pass-through for 2-liter bottles of regular soda dropped to 24% (from 46% if using regular prices) when considering promotional prices. In Boulder, however, Cawley et al. (2021) find there is no substantial difference in the pass-through by bottle size; it is roughly 75% for each size.

Studies on Philadelphia's soda tax reach the same conclusion as the Berkeley studies. Across all taxed beverages, results based on hand-collected data point to a larger pass-through rate for small single servings (115%) than that for large containers (93%) (Cawley et al. 2020). This difference is statistically significant for regular sodas (a 29% less pass-through for large containers) but insignificant for diet sodas. Results from using IRI retail data report a slightly larger pass-through rate for small-size containers (1.51 cents per ounce vs 1.42 cents per ounce). This consistent result could also be due to the more elastic demand for large pack sizes documented for Philadelphia (Seiler, Tuchman, and Yao 2021).

In terms of changes in volume sales, there is evidence showing that the demand for large pack-sizes of soda declined more (53%) than for small sizes (10%); a large part of the sales decrease of large sizes is offset by the sales increase at stores outside the city but consumers do not engage in cross-border shopping for small sizes (Seiler, Tuchman, and Yao 2021). This pattern of heterogeneity across pack sizes is intuitive because the costs of traveling to a store outside of the city are presumably too high when purchasing a beverage that is typically intended for impulse purchasing. On the other hand, for large pack sizes, which consumers are more likely to store for future consumption, the benefits in terms of price savings from cross-border shopping are significantly larger.

Current findings regarding the heterogeneity in the impact across products are mixed. Cawley and Frisvold (2017) estimated pass-throughs of Berkeley's tax for 20 oz bottles, 2L bottles, and 12-packs of 12 oz cans of Coke, Pepsi, and Mountain Dew. The results show that the pass-throughs of these products are higher than the average rate (43%), except for Coke and 12-packs of 12 oz cans of Pepsi. This might imply a smaller price increase experienced by more popular products after the tax implementation. Fearing the loss of sales, retailers tend to pass less of the tax to more popular products. These products might also be more likely to be used as "Loss Leaders" by retailers to attract consumers to patronize the stores. Rojas and Wang (2021), however, report a higher pass-through rate and a larger drop in the volume sales for more popular products compared to the less popular products for Berkeley. The authors also find the average pass-through rate for all SSBs is estimated to be 4.8% and insignificant; but if only estimating the pass-through for popular products, 9.4% of the tax is passed onto the price, with 11.7% for the regular soda category. A similar pattern is found for the volume sales. The pooled estimate for popular products suggests a larger drop in volume (8.3% decrease) than that observed in the overall results (1.2% decrease), with a 10.5% decrease for regular sodas.

Overall, the analysis of the heterogeneity in the impact of a soda tax is relatively rare. This is, however, of vital importance, especially for our understanding of the strategic pricing behavior of food retailers. Because consumers tend to vary in their preferences across products, it is very likely for a retailer to pass more of a tax to one specific product than to another to maximize profits or minimize losses due to the tax. Moreover, the rationale of small food stores might differ. They target occasional consumption and sell a higher proportion of smaller sizes. They are less concerned about consumers bundling their purchases or avoiding their store altogether than in larger grocery stores. To fully understand the influence mechanism of a soda tax and how retailers respond strategically, it is necessary to conduct an analysis of the heterogeneity across various dimensions, such as store type, store location, products, in addition to analysis across jurisdictions. Using the same data source eliminates differences in the data collected and analyzed as a reason for observed heterogeneous effects.

3.6 Summary

The prior studies have made great contributions to our understanding of the impacts of soda taxes. First, they provided plenty of empirical evidence about the impacts of Berkeley's and Philadelphia's taxes on the price and volume sales of sodas. But there is relatively little research regarding the impact of soda taxes in the rest of the jurisdictions. Additionally, no study to date has theoretically analyzed and discussed retail pricing and the mechanisms through which a soda tax affects retailers. This dissertation will contribute to the literature by providing evidence of the tax effects for Seattle, Boulder, Oakland, and San Francisco, and providing a theoretical foundation for the empirical work.

A variety of data have been utilized in the literature, including self-collected data and scanner data. But these data have some drawbacks. For example, self-collected data may have measurement errors, and the retail scanner data utilized has lacked key information (e.g., retailers' addresses), or has had limited geographic and temporal coverage. These drawbacks may result in a lack of qualified controls in empirical estimations, and only short-run effects can be analyzed. However, the selection of controls is of vital importance in studies that reply on comparisons. In particular, some studies have used the neighboring cities of a taxed jurisdiction as controls, which may, themselves, be indirectly affected by the tax, e.g., through cross-border shopping. This dissertation draws on detailed and nationwide scanner data that can provide multiple feasible controls and allow me to use both graphical and quantitative methods to make final decisions on choices of controls.

Long-run effects of soda taxes have not been analyzed so far. However, it is important to understand how retailers adjust their prices over time. For example, retailers may raise prices gradually instead of passing all the tax to consumer prices during the first year. Further, retailers themselves do not bear the full impact of a tax if it is passed forward only gradually by distributors. An estimation of the pass-through based on short-term data may underestimate the effects of soda taxes, leading to a biased evaluation. This dissertation draws on data through 2019, and the long time span of data allows me to not only focus on short-run effects but also the long-run effects.

The difference-in-differences model is the most common approach in the literature. But perhaps due to data limitations, current studies have failed to provide sufficient information on how they chose their controls and whether the controls met the common pre-trend assumption. Further, the difference-in-differences approach may be potentially influenced by unobserved events beyond the soda tax. A triple-difference model can rule out this possibility, but no study so far has used it to analyze the tax impact. Another contribution of this dissertation is to present a detailed procedure of control selection and utilize the triple-difference model to strengthen the identification strategy.

Given that more cities and states are considering soda taxes, it is important to compare the impact of soda taxes in different jurisdictions. Such comparisons can help us understand how the effects of soda taxes vary across jurisdictions with differing demographics and differing magnitudes and scopes of the tax. But large differences in data and methodologies used by existing studies make comparisons difficult, and they have only focused on one taxed jurisdiction. This dissertation, to my knowledge, is the first study to conduct a comprehensive analysis and examine simultaneously the effects of all soda taxes imposed in the U.S., eliminating differences in data and methodologies.

The heterogeneous effects of soda taxes have been explored in the existing literature. An analysis of this aspect is important in that it enables us to understand how different retailers respond to the tax, how retailers make decisions across different products, etc. Even so, some of the results were inconsistent, and the prior research mainly focused on the store types and product categories. The geographic location of a store might be also an important predictor of the tax pass-through, and stores closer to the city boundary may see a larger decline in soda sales for a given pass through. Moreover, the impact of a soda tax might vary across demographics due to the different preferences of consumers and, possibly, due to difference in retailer behavior as a function of consumer demographics. In particular, the possibly regressive incidence of a soda tax is also a hotly debated topic. However, analyses about heterogeneous effects across store locations and local demographics, and analyses about the tax regressivity have been largely missing in the literature. This dissertation explores the heterogeneous effects of soda taxes in more detail by incorporating these aspects into the analysis.

Finally, prior studies on cigarette taxes and tobacco taxes have provided evidence of cross-border shopping. However, it is still rare for the literature on soda taxes to discuss cross-border shopping by analyzing the neighboring untaxed stores. This dissertation fills this gap by analyzing cross-border shopping behavior for all jurisdictions.

Chapter 4
Theoretical Background

Under perfect competition, the incidence of a given tax on a single product is a function of the relative supply and demand elasticities, which is given by Eq. (4.1):

$$Tax\ Incidence = \frac{dP_D}{dt} = \frac{\varepsilon_S}{\varepsilon_S - \varepsilon_D} \tag{4.1}$$

where P_D denotes the price paid by consumers and t denotes the tax rate; thus, $\frac{dP_D}{dt}$ defines the incidence of a tax, i.e., the rise in price to consumers for each infinitesimal unit of specific tax imposed; ε_S and ε_D represent the price elasticity of supply and the price elasticity of demand, respectively.[4] The extent to which an excise tax is passed through to consumer prices is unambiguous: assuming the standard upward sloping supply and downward sloping demand, the tax is under-shifted to consumer prices; in particular, the lower the elasticity of demand and the higher the elasticity of supply, the larger the pass-through rate of a tax (Berardi et al. 2016). When the demand is perfectly inelastic or the supply is infinitely elastic, the tax is fully passed on to consumers. However, this result has little relevance for locally enacted soda taxes, given the huge complexity in the structure of a local market, e.g., the existence of market power of retailers, multi-product pricing, cross-border shopping, etc.

As described in Chapter 2, all soda taxes currently in place are levied on distributors. This means that the soda tax would first be passed on in all or part to retailers, who would then determine the pass-through to consumers. Due to lack of sales data on distributors, I am unable to estimate what proportion of the tax is passed on by them. Most of the transactions between a distributor and

[4] The other way to define the incidence of a tax is the fraction of the tax paid by suppliers, which is $\frac{dP_S}{dt} = \frac{\varepsilon_D}{\varepsilon_S - \varepsilon_D}$ and P_S is the price received by suppliers.

a retailer are handled via contracts. Such contracts may specify that prices are fixed for an extended period. Or it may not even be possible for a distributor to adjust the price in response to a tax until the contract is renegotiated. It is also possible that the pass-through of a tax will differ depending upon the type of retailers. For example, large retail chains (e.g., Walmart) are likely to have much greater bargaining power than independent retailers, and thus less of the tax would be passed forward from distributors to chains than from distributors to independent retailers. The changes we observe in shelf prices are a combination of the portion of the tax borne by distributors and the portion further passed on to retailers.

A multitude of factors make the simple tax incidence analysis basically irrelevant to our understanding of the implication of soda taxes, even if the pass-through for distributors is determined. First, there is evidence showing that grocery retailers are "natural oligopolists" and possess some market power within localized food markets (both as buyers from distributors and as sellers to consumers) due to location and high transaction costs, while the basic theory about the tax incidence pertains to competitive markets (Sexton 2000; Li, Sexton, and Xia 2006; Ellickson 2013; DellaVigna and Gentzkow 2019).

For a competitive firm (e.g., retailer) whose price is determined based on the equality between price and marginal cost (i.e., $P = MC$), if the marginal cost rises due to a tax, this pricing rule guarantees that the tax will be passed on fully. Under conditions of imperfect competition, a firm (e.g., food retailer) that faces a downward-sloping demand (i.e., one with market power) always balances the effect on profits margin declining from an incomplete pass-through of the tax, with the loss of sales if the tax is passed on fully. For example, a monopolist that faces demand $P = a - bQ$ and a constant marginal cost $MC = c$. The profit-maximizing price of this seller is given by $P^* = \frac{a+c}{2}$. When an excise tax is implemented, this seller will pass on exactly half of a cost increase due to the tax. The literature on taxation has shown that in markets characterized by imperfect competition, excise taxes may be either under-, fully- or even over-shifted to prices depending on the characteristics of demand (e.g., the curvature of demand function) and of production costs (Anderson, Palma, and Kreider 2001; Berardi et al. 2016).

Discrete pricing of retailers might also matter. When retailers adjust prices, they largely choose prices that end in 9, 99 or 95, and change prices based on fixed increments, such as a whole dollar. For example, the retail price of 2L Coke is 2.39 dollars, if a retailer fully passes a tax of 1 cent per ounce on to consumers, the new price will rise to 3.07 dollars. But retailers may not choose this price. They may adjust the price to 2.99 dollars (charm price) or 3.39 dollars, depending on the specific pricing policy of retailers.[5] Thus, discrete pricing can explain the incomplete or excessive pass-through rate of an excise tax (Conlon and Rao 2020).

The above discussion, however, applies to a single product, and the fact that a retailer sells thousands of products adds additional complexity to the theoretical prediction about the pass-through of a soda tax. Because the demands for products sold at the same retailer are closely interrelated, it is not optimizing behavior for a retailer to set prices for each item based solely on its marginal cost and individual demand (Ostberg, Yoder, and Balderston 1957). Instead, retailers must take cross-product relationships into account when pricing and a consideration of such relationship by retailers (cross-product pricing effect) might reduce the market power (Saitone, Sexton, and Sumner 2015; Thomassen et al. 2017). In particular, if cross-product relationships are mainly complementary, then a price increase in response to the imposition of a tax will be

[5] "Charm prices" make products appear cheaper than they really are. Consumers typically read the price from left to right and display inattention to the decimal digits of the price (i.e., left-digit bias). Retailers utilize this psychological effect and usually set prices that end with 99 cents. For example, $3.01 is close to $2.99. But using $2.99 can increase sales significantly. Price increases that shift the left-most digit upwards, on the other hand, cause a larger decrease in sales of products (Ashton 2014).

diminished due to cross-product effects.

One aspect related to multi-product relationships that complicates the incidence of the soda tax is market-basket shopping, especially in terms of large grocery retailers. Compared with small retailers, customers of large retail chains prefer to concentrate a substantial part of their weekly grocery purchases with a single retailer (Baye, Schlippenbach, and Wey 2018). When facing a substantial price increase for a "staple" item in her/his market basket, the shopper may elect to switch to another store, e.g., a neighboring untaxed store, for market-basket purchases. Retailers that raise prices for soda items thus risk losing not only the soda sales but the profit from the whole market basket of the shopper.

The types of products also matter. Retailers might pass on a larger proportion of the tax to more "staple" products (e.g., 2L Coke) as consumers' demand for this type of products is more inelastic. However, an offsetting effect for "staples" is the store's desire to maintain stable prices for them in order to maintain its customer base. If some products are used as a "Loss Leader" to attract more customers by retailers, the pass-through for them might be smaller than other taxed categories, such as sports drinks.[6]

Retailers that sell sodas might be owned by a large chain. In fact, supermarkets, and general merchandisers such as Target account for almost half the total volume of sodas, making up the largest single market for the sale of soft drinks (NPLAN, 2012).[7] Individual retailers' response to a local policy (e.g., soda tax) might be limited by the management of their parent companies. There is evidence that most large US food, drugstore, and mass merchandise chains set uniform or nearly uniform prices across their stores within broad geographic areas, rather than at the individual store

[6] Loss leading is a practice commonly adopted by large retailers that consist of pricing below cost some of the competitive products (leader products) (Chen and Rey 2012).
[7] NPLAN is short for National Policy & Legal Analysis Network to Prevent Childhood Obesity.

level (DellaVigna and Gentzkow 2019). Using Nielsen retail scanner data, DellaVigna and Gentzkow (2019) document that for the large majority of the 73 chains in the data, measured prices vary very little across stores and for 11 food chains as well as the 2 major drugstore chains, prices vary at the level of large geographic zones but vary much less within them. This study also argues that managerial decision-making costs are the most supported explanation for the uniform pricing; implementing a more flexible pricing policy may result in chain-level fixed costs, such as the effort of upfront managers in pricing design, or the cost for inertial managers deviating from traditional pricing approach in the industry. This has crucial implications for the pass-through of local shocks, such as a soda tax, as uniform pricing causes the price response to local shocks to be smaller than to economy-wide shocks (DellaVigna and Gentzkow 2019). If the cost of deviating from a uniform price to a higher price caused by a soda tax is large, a retailer may not adjust the price or the price adjustment is small, which implies a smaller pass-through rate.

The factors that affect the tax incidence analyzed above have different effects on different types of retailers. For example, the market basket shopping I discussed earlier is less important for convenience stores than for large supermarkets, because consumers use the former mainly for “fill-in” purchases, such as bread, milk, or beverages for immediate consumption. In addition, retailers located near the border of a taxed jurisdiction may pass on a smaller proportion of a tax due to the ability of customers to switch to a nearby untaxed store. Indeed, the literature on cigarette taxes has documented that the pass-through rate of an excise tax is positively associated with the distance to the city border (Harding, Leibtag, and Lovenheim 2012). While chain retailers are likely to adopt uniform pricing or zone pricing and passively react to a soda tax, independent stores may adjust their prices based on the demographics of local customers.

No matter how large or small the retailers’ pass-through rate is, it takes time to complete, which means soda taxes have both short-run and long-run effects. First, distributors may pass through more tax to retailers over time. As discussed earlier, the contracts between distributors and retailers may preclude passing the tax forward in the short run. A contract, for example, may not have ended when a soda tax kicks in. By the time the contract is over, distributors need more time to renegotiate new contracts with retailers. Thus, more tax being passed on at retail may be partly due to distributors passing on more of the tax to the retailers themselves.

Retailers may be reluctant to vary prices for fear of antagonizing customers. Anderson and Simester (2010) used a 28-month randomized field experiment and found evidence that customers reacted to a lower price by making fewer subsequent purchases. In the immediate aftermath of a tax (especially one that was enacted through a well-publicized referendum), if retailers raise prices by large magnitudes, consumers will feel antagonized because the updated price may be far from expected, even if consumers could afford it. To avoid alienating consumers, retailers may choose to pass the tax forward gradually over time, exploiting the consumers' rational inattention (Mackowiak, Matejka, and Wiederholt 2022).[8]

Except for Philadelphia and Seattle, jurisdictions have passed soda taxes by public referendum, resulting in well-funded advocacy campaigns and media coverage. Information provision and media coverage can influence consumer choices and overall economic behaviors (e.g., Kiesel 2012; Cornelsen and Smith 2018; Taylor et al. 2019). They might alter consumers' preferences for sodas by making consumers more aware of the health risks of drinking sodas, but also more attentive to price changes. Retailers could pass on a smaller proportion of a soda tax to the consumer and might even cut the price to avoid demand reductions. Further, consumption

[8] Rational inattention is a concept, proposed by Christopher A. Sims, that economic decision maker cannot absorb all available information due to the high costs of information acquisition but can choose which pieces of information to process and rationally take decisions based on incomplete information (Mackowiak, Matejka and Wiederholt 2022).

reductions due to these information effects may be mistaken for consumer response to the tax itself, causing the analyst to impute a more price elastic response than is warranted.

The multitude of factors discussed so far that affect the pricing of retailers and the pass-through of soda taxes will eventually affect soda consumption. This complexity cannot be dealt with in any single theoretical model. Thus, the incidence of a soda tax and the ultimate effect on consumers is fundamentally an empirical question.

Chapter 5
Data

The primary data of this study include the point-of-sale (POS) data, store and product dictionary data from Information Resources Inc. (IRI). The secondary data include U.S. Census data, and geographic information of stores outside of a taxed jurisdiction web-scraped from Google Maps. I also supplement these data with nutrition information on beverages that I obtained through web searches. Each dataset will be described in detail.

5.1 IRI Point-of-Sale Scanner Store-level Data

Access to retail scanner data collected by IRI was granted through a cooperative arrangement with the U.S. Department of Agriculture, Economic Research Service (ERS). The data are a store-level scanner dataset that covers a large share of stores across the nation, including those in taxed jurisdictions.[9] The types of stores covered include grocery, drug, convenience, mass merchandiser, club, and dollar stores. Not all retail establishments that operate in the U.S. are covered in IRI POS data and not all participating retailers provide the store-level data. CVS, Kroger, etc. only approve the release of their data at the retailer marketing area (RMA) level.[10] Table 5.1 provides an overview of retail establishments that participate.

I use this panel data to extract information on the weekly revenue (in cents) and the number of units sold for all beverage categories (both taxed and untaxed).[11] One observation contains the

[9] The data obtained by ERS do not include smaller and independent stores. They include chain retailers that have agreed to provide sales data for all their stores.
[10] A retailer marketing area (RMA) refers to an aggregate geographic area that a retailer defines as its competitive marketing area, which is unique to retailers.
[11] The weekly total revenue includes discounts obtained using loyalty cards and other promotions.

sales information at the UPC level for a given store and week.[12] Note that IRI POS data do not provide the weekly price for each product (UPC). The price variable is calculated as the ratio of weekly sales in cents to weekly units sold, i.e., as the unit value. These data are currently available from January 2008 through December 2019. I use data from 2012 through 2019 for the taxed jurisdictions in my analysis. The long periods before and after the tax implementation allow me to test for the common pre-trend assumption needed for the difference-in-differences approach and explore the gradual effects of the tax implementation. The soda taxes apply to the taxed beverages sold at retailers, restaurants, vending machines, etc. Because I only obtain the sales data from retailers, the results of this study reflect the impact of the soda tax on the economic behavior observed in the retail sector only.

Table 5.1. Overview of Retail Establishments That Participate

Outlet Type	Definition of Outlet Type	Coverage in Data Received by ERS
Grocery stores	Grocery stores with $2 million or more in annual grocery sales	All stores that provide complete sales data to IRI except the chain HEB; some stores only release data at the RMA level
Drug stores	Chain and independent drug stores	All stores that provide complete sales data to IRI; some stores only release data at the RMA level
Convenience stores with scanning capability	Chain and independent convenience stores with scanning capability	All stores that provide complete sales data to IRI
Mass merchandisers	Mass merchandiser chains	-
Walmart	All Walmart store formats, including supercenters, traditional, and neighbor-hood markets	All stores starting in 2009 (RMA level only)
Club stores	Membership stores	Sam's club (starting in 2009; RMA level only)
Dollar stores	Dollar store chains	-

Source: https://www.ers.usda.gov/publications/pub-details/?pubid=47636

[12] A UPC (or barcode) is a unique product identifier denoting a combination of a brand, unit, and size.

5.2 IRI Product Dictionary and Additional Nutrition Information

IRI provides a set of product dictionaries that contain information on product attributes. Specifically, they contain basic descriptors for UPC food products, including category, UPC description, package size, etc., and nutrition information and claims for many UPCs, including the contents from the Nutrition Facts panel (e.g., calories), and health claims on the packaging. The total number of UPCs that are active can change over time. The IRI dictionary files contain the variable year that denotes when a specific UPC is available in the market.

I select the nutrition data for beverage categories that possibly contain taxed UPCs (e.g., carbonated beverages, sports/energy drinks, bottled water).[13] For each taxed jurisdiction, I then link the selected nutrition data to the POS scanner data by UPC and year. To further determine the tax status of a specific beverage, I use the information on the type of sweeteners. If the type of sweetener of a UPC is an added caloric sweetener (e.g., cane sugar, high fructose corn syrup), this UPC is considered taxed. For the case of Philadelphia, as long as a UPC contains sweeteners, including both added caloric and artificial sweeteners, the policy defines it as a taxed UPC.

However, the nutrition data are incomplete. According to IRI, nutrition data are coded only for edible food and beverage products with significant sales volume. For UPCs that the values of the sweetener type are missing or unclear (e.g., not stated on the package, value not available), I search via Google for their ingredients and nutrition facts based on the UPC description.[14] Table

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[13] It is worth noting that taxable and non-taxable beverages might be included in the same category. For example, the bottled water includes both the sweetened vitamin water and pure water. Even though each tax policy lists the categories that are taxable, such as "soda," "sweetened tea," these categories cannot match with the beverage categories defined by IRI.

[14] IRI product dictionary data do not provide ingredients of products.

5.2 summarizes the characteristics of UPCs sold in each taxed jurisdiction, including a detailed list of beverage categories and the total numbers of taxed and untaxed UPCs within each category.

Table 5.2. Number of UPCs Used in Each Beverage Category

Jurisdictions	Carbonated Beverages		Sports /Energy Drinks	Bottled Water		Juice		Tea/Coffee	
	Taxed Soda	Diet Soda		Taxed Water	Pure Water	Natural Juice	Non-100% Juice	Sweetened Tea/Coffee	Untaxed Tea /Coffee
Berkeley	154	73	173	7	124	55	163	104	24
San Francisco	400	168	327	25	449	255	465	215	98
Oakland	499	197	446	47	569	324	636	275	94
Boulder	477	180	534	58	600	297	480	333	108
Philadelphia	952	0	535	204	767	469	754	367	148
Seattle	274	128	292	21	347	107	234	183	52

Notes: The column for taxed soda contains the quantities of regular soda in all jurisdictions except Philadelphia. Philadelphia's tax applies to both regular and diet soda. Thus, the quantity of taxed sodas for Philadelphia is the sum of the quantity of regular sodas and the quantity of diet sodas.

5.3 IRI Store Dictionary Data

The store dictionary data provide information on the geographic location of each store (e.g., store type, exact address, zip code, city, geographic coordinates) that allows me to identify which retailers are in the taxed jurisdictions and which retailers outside of the taxed jurisdictions are located near the border and can potentially attract consumers seeking to avoid tax-induced price increases on their beverage purchase. I also observe the chain affiliation for each store and use this information to analyze the heterogeneity in the impact of the soda tax across different chains. Table 5.3 describes the characteristics of stores within each taxed jurisdiction, including the store types, and the frequency distribution of stores across the types. The store distribution is also important for the selection of controls in the next chapter because the local market environment or local competition can affect the pricing behavior of retailers.

Table 5.3. Characteristics of Stores Within Each Taxed Jurisdiction

Jurisdictions	Store Types	Number of Stores (N)
Berkeley, CA	Drug stores	6
San Francisco, CA	Grocery stores	2
	Drug stores	56
	Convenience stores	1
	Mass Merchandisers	5
Oakland, CA	Grocery stores	10
	Drug stores	10
	Convenience stores	6
Boulder, CO	Convenience stores	9
	Drug stores	5
	Grocery stores	3
	Mass Merchandiser	1
Philadelphia, PA	Grocery stores	16
	Convenience stores	20
	Dollar stores	39
	Mass Merchandisers	10
	Drug stores	100
Seattle, WA	Convenience	10
	Mass Merchandiser	3
	Drug	23

5.4 Google Map Search Data

As I discussed in Chapter 4, consumers who live near the border of a taxed jurisdiction might shop across the border to avoid the soda tax. To test for the possibility of cross-border shopping and its impact on retailers, I needed information on the distances between the taxed stores and the nearest untaxed stores. Using an API provided by Google Maps, I web-scraped the geographic information for all untaxed stores located in the neighboring cities of a taxed jurisdiction.[15] The data include the name, type, exact address, zip code, and geographic coordinates for each store. I further calculate the geodesic distance between two stores (one taxed and the other untaxed) using Haversine Formula ($hav(\Theta)$), given by Equation (5.1):

$$hav\,(\theta) \; = sin^2(\frac{\theta}{2}) \tag{5.1}$$

[15] Technically, API is an application programming interface that is a set of tools, definitions, and protocols for integrating application software and services.

where θ is equal to $\frac{d}{r}$; d is the distance between two points (i.e., two stores) along a great circle of the sphere (i.e., spherical distance); r equals 6371(km), the radius of the earth.[16] For each taxed store, I choose the untaxed store that is nearest to it and record the corresponding distance. I will use this variable in the next chapters to investigate the heterogeneity in the impact of a soda tax across retailers.

5.5 U.S. Census Data

I also collect the demographics from the U.S. Census Bureau website for all U.S. cities covered in the POS scanner data. The information collected includes the median household income, education attainment, the percentage of people in poverty, population density, etc. These measures are the key determinants of soda demand and retail pricing, based on which I choose cities that are similar to the taxed jurisdictions as controls in the empirical models. The selection of control cities will be described in more detail in Chapter 6. Moreover, I summarize the above demographic characteristics for the taxed jurisdictions in Table 5.4.

The data suggest the considerable variation in population density, median household income, and ethnic diversity. San Francisco and Philadelphia are similar in terms of population density, ethnic diversity, and education level; but the median household income in San Francisco is more than twice that of Philadelphia. Compared with these two jurisdictions, Berkeley features a lower proportion of non-whites and higher education levels; San Francisco's median income is the highest among all taxed cities. Although Berkeley shares a geographical border with Oakland,

[16] Haversine formula is a commonly used way to calculate a spherical distance because the surface of the earth is a spherical surface not flat.

which has a lower population density, they differ a lot in terms of the proportion of non-whites and education levels. Among three jurisdictions in the Bay Area, the income level of Oakland is the lowest. Boulder is a small city with a high level of education and a low proportion of non-whites; its median household income level is close to that of Oakland. Lastly, the demographic characteristics of Seattle are similar to those of Berkeley, except that Seattle has a lower population density.

Given the information on the zip codes where each store is located, I use the U.S. census data (2011-2015 American Community Survey (ACS) from Census Bureau) to obtain local demographics for each taxed store at the zip code level, including the median household income, the percentage of non-whites, and the percentage of African-Americans. I have discussed in Chapter 3 that the response to a soda tax can vary across these consumer characteristics. In the empirical analysis, I will explore this heterogeneity with the demographic data I obtained.

Table 5.4. Characteristics of Taxed Jurisdictions

Jurisdictions	Population Density (N/Sq mi)	Non-whites (%)	High school or higher (%)	Median household income (Dollars)	Poverty Rate (%)	Area in Square Miles
Berkeley, CA	11,558.00	40.70	96.30	80,912.00	20.00	10.50
San Francisco, CA	18,796.00	53.30	88.50	104,552.00	10.90	46.90
Oakland, CA	7,747.00	63.90	81.60	68,442.00	17.60	55.90
Boulder, CO	4,261.00	12.80	96.50	66,117.00	21.30	24.80
Philadelphia, PA	11,804.00	58.80	83.90	43,744.00	24.90	134.20
Seattle, WA	8,994.00	32.00	94.60	85,562.00	11.80	83.80

Source: https://www.census.gov/quickfacts/fact/table/US/PST045219

5.6 Descriptive Analysis

Table 5.5 – Table 5.11 reports the descriptive statistics (i.e., average price and market share) for all products included in this analysis for each taxed jurisdiction. I use the POS data at the UPC/store/week level to do all calculations and focus on the pre-tax period. The weekly volume

sales measured in ounces are first normalized by dividing by 12 (ounces), and then I measure the normalized weekly price as the ratio of weekly revenue to the normalized volume sales.[17] Thus, the average weekly price is the arithmetic mean of weekly prices (normalized) across all UPCs, stores, and weeks during the pre-tax period. I also calculate the volume-weighted average weekly price by using each UPC's volume sales in each store/week as the weight. A volume-weighted average weekly price takes into account volume and assigns more weights to the prices of frequently purchased products. This indicator provides a much more accurate estimate of the average overall price effectively *paid* by consumers (Rojas and Wang 2021).

Table 5.5 contains statistics on taxed and untaxed beverages, which I report separately by tax status and beverage category. I calculate the market share for each category by dividing its volume sales by the total sales of all beverages (taxed and untaxed). All calculations are based on data from the pre-tax period. Also note that these market shares only describe the characteristics of sales in IRI data. Because the soda tax in Philadelphia applies to all beverages that contain added-caloric sweeteners or artificial sweeteners, the statistics for taxed sodas sold in Philadelphia include both regular soda and its diet versions.

Results show that in all jurisdictions, pure water makes up the largest market share among all categories, followed by regular soda. The taxed water (e.g., vitamin water) and unsweetened tea/coffee, however, make up smaller market shares. In terms of pricing, sports/energy drinks are the most expensive category, followed by sweetened tea/coffee and natural juice; pure water and

[17] 12 ounces correspond to a standard can size (Reference Amount Customarily Consumed (RACC)).

diet soda are the cheapest. These results are observed in all jurisdictions. Moreover, regular soda's market share is the largest among all taxed beverages, ranging from 17.08% to 30.81%. The statistics under regular soda also show that the average weekly price for regular soda is the highest in Boulder with 71.09 cents/12 ounces and the lowest in Philadelphia with 54.17 cents/12 ounces. Considering the pre-tax average price, Boulder's soda tax might not be as salient as it appears, although its magnitude is the largest (2 cents/ounce).

Table 5.5. Descriptive Statistics for Both Taxed and Untaxed Beverages by Category During the Pre-tax Period

Jurisdictions	Statistics	Taxed Beverages						Untaxed Beverages				
		Regular Soda	Sports/energy Drinks	Taxed Water	Non-100% Juice	Sweetened Tea/Coffee	All Taxed	Diet Soda	Pure Water	Natural Juice	Untaxed Tea/Coffee	All Untaxed
Berkeley	Average Weekly Price (normalized)	65.51	389.23	91.92	80.27	157.89	185.17	61.48	58.45	137.16	86.47	73.97
	Volume-Weighted Average Weekly Price (normalized)	49.50	127.17	90.01	56.19	77.51	62.64	55.09	31.39	102.09	58.06	39.36
	Market Share (%)	19.33	8.99	1.31	6.57	7.78	43.98	9.96	41.96	2.31	1.77	56.02
Philadelphia	Average Weekly Price (normalized)	54.17	334.04	72.99	71.58	103.20	120.66	-	51.27	107.56	72.89	70.32
	Volume-Weighted Average Weekly Price (normalized)	34.82	73.58	57.59	44.46	48.64	42.26	-	14.81	67.47	42.34	17.86
	Market Share (%)	30.81	5.07	1.60	5.60	5.29	48.37	-	48.06	2.33	1.24	51.63
Boulder	Average Weekly Price (normalized)	71.09	329.37	92.17	108.72	142.64	188.17	65.08	65.19	139.00	92.57	77.19
	Volume-Weighted Average Weekly Price (normalized)	55.60	119.04	80.41	75.07	94.18	83.54	50.34	32.53	89.70	55.73	38.83
	Market Share (%)	21.10	15.92	1.52	3.35	5.39	47.28	11.05	38.60	1.90	1.17	52.72

Oakland	Average Weekly Price (normalized)	60.61	261.31	88.48	82.30	131.23	131.34	53.44	54.07	119.06	61.47	68.85
	Volume-Weighted Average Weekly Price (normalized)	35.31	77.59	78.59	41.96	59.60	45.01	40.15	17.94	72.11	41.77	24.88
	Market Share (%)	27.09	6.80	0.48	8.56	3.51	46.44	6.95	42.19	3.68	0.74	53.56
Seattle	Average Weekly Price (normalized)	66.85	315.83	76.83	97.77	188.25	170.60	61.93	57.59	114.80	82.50	67.57
	Volume-Weighted Average Weekly Price(normalized)	49.93	107.90	55.56	67.18	139.40	69.70	46.52	32.02	85.27	57.15	38.37
	Market Share (%)	26.52	8.63	0.18	3.51	2.96	41.79	16.39	38.91	2.09	0.82	58.21
San Francisco	Average Weekly Price (normalized)	67.75	449.98	91.25	105.76	201.15	199.70	65.92	56.40	123.33	100.95	74.02
	Volume-Weighted Average Weekly Price (normalized)	56.72	125.43	75.18	80.66	151.68	81.08	60.23	34.25	95.57	81.62	42.17
	Market Share (%)	17.08	5.54	0.08	4.31	2.44	29.45	11.98	54.24	3.02	1.31	70.55

Notes: The unit of the average price is cents/12 ounces.

The summary statistics for different types of stores are shown in Table 5.6. The store types analyzed include mass merchandiser, grocery store, drug store, convenience store, and dollar store. The types of stores vary by jurisdiction due to a data limitation. For example, I am only able to access the sales data on drug stores in Berkeley. All calculations are based on data on taxed beverages and the market share of each store type in each jurisdiction is calculated based only on IRI data. The last column of Table 5.6 shows that for large jurisdictions (e.g., Philadelphia, San Francisco, Oakland), the sales of taxed beverages in grocery and drug stores together account for a market share of over 70%. The data do not include the sales information from grocery stores in Seattle, where mass merchandisers control a large share of taxed beverage sales. Unlike other cities, the convenience stores in Boulder exhibit the largest market share for taxed beverages, followed by mass merchandisers.

In terms of pricing, results show that the average pre-tax prices of taxed beverages at drug and convenience stores are much higher than those observed for the other types. One exception is San Francisco, where mass merchandisers charge slightly higher prices for taxed beverages than convenience stores. As expected, dollar stores exhibit the lowest average pre-tax price in Philadelphia, the only jurisdiction for which I obtained sales data for dollar stores. Finally, the average prices at grocery stores are consistently smaller than the prices at mass merchandisers, convenience, and drug stores. Drugstores are the only store type that enables me to make comparisons across all jurisdictions. I find that the average prices at drugstores in Philadelphia and Oakland are lower than those in other jurisdictions.

Table 5.6. Descriptive Statistics by Store Type in Each Taxed Jurisdiction

Jurisdictions	Store Type	Average Weekly Price (normalized)	Volume-Weighted Average Weekly Price (normalized)	Market Share (%)
Berkeley	Drug	185.17	74.59	100.00
Philadelphia	Drug	145.30	55.76	25.32
	Mass Merchandiser	111.26	43.37	14.30
	Dollar	62.87	32.87	8.94
	Grocery	91.92	35.96	47.09
	Convenience	175.64	95.49	0.42
Boulder	Drug	201.51	87.56	12.11
	Convenience	205.34	105.02	46.53
	Grocery	129.65	59.31	12.23
	Mass Merchandiser	134.57	57.72	29.12
Oakland	Drug	161.85	64.17	16.94
	Convenience	180.81	107.11	3.64
	Grocery	92.87	38.08	79.42
Seattle	Drug	184.83	70.28	46.41
	Convenience	149.97	99.80	15.10
	Mass Merchandiser	147.88	56.93	38.25
San Francisco	Drug	217.00	92.43	66.81
	Convenience	153.00	95.81	0.89
	Mass Merchandiser	169.82	64.98	11.32
	Grocery	103.21	52.97	20.98

Notes: The unit of the average price is cents/12 ounces.

Table 5.7. Most Popular Brands With a Total Combined Market Share of Over 50%

Jurisdictions	Brand	Average Weekly Price (normalized)	Volume-Weighted Average Weekly Price (normalized)	Market Share (%)
Berkeley	Brand 1	43.02	39.58	15.59
	Brand 2	66.14	53.78	13.71
	Brand 3	79.02	72.14	9.07
	Brand 4	51.01	40.32	4.68
	Brand 5	62.16	51.92	3.87
	Brand 6	63.90	48.88	3.51
	Brand 7	96.34	90.69	2.99
Philadelphia	Brand 6	56.21	33.73	10.99
	Brand 2	62.99	36.78	8.75
	Brand 4	51.17	31.77	6.61
	Brand 1	38.99	32.99	4.66
	Brand 3	62.22	47.18	4.33
	Brand 8	54.59	38.13	3.82
	Brand 9	56.48	38.86	3.51
	Brand 10	55.87	34.99	3.36
	Brand 5	55.73	36.93	3.33
	Brand 11	87.71	51.93	2.63
Boulder	Brand 2	71.06	53.55	13.44
	Brand 3	80.71	66.66	11.32
	Brand 6	65.41	51.77	5.32
	Brand 1	46.36	41.63	4.42
	Brand 9	62.14	63.50	4.42
	Brand 12	64.51	58.42	4.09
	Brand 13	278.89	269.89	3.93
	Brand 5	67.81	54.53	3.67
Oakland	Brand 2	70.44	39.98	15.94
	Brand 3	72.06	48.07	7.20
	Brand 6	63.20	35.79	5.77
	Brand 1	41.77	35.07	5.51
	Brand 14	26.91	21.59	4.34
	Brand 5	62.99	44.22	4.11
	Brand 15	18.58	15.85	3.96
	Brand 4	53.38	33.28	3.93
Seattle	Brand 2	65.80	48.29	20.79
	Brand 6	62.55	47.95	7.61
	Brand 3	81.09	69.53	6.88
	Brand 5	59.84	50.40	6.80
	Brand 12	63.40	53.56	3.97
	Brand 9	62.76	56.65	3.82
	Brand 4	60.50	45.09	3.29

San Francisco	Brand 2	74.52	59.61	21.26
	Brand 3	82.12	71.06	6.44
	Brand 4	54.92	47.75	5.11
	Brand 5	67.15	60.58	4.98
	Brand 6	68.11	57.03	4.91
	Brand 16	52.91	44.97	3.18
	Brand 17	52.15	48.02	2.73
	Brand 12	73.12	69.73	2.37

Notes: The unit of the average price is cents/12 ounces.

In Table 5.7, I report statistics on the most popular brands that together make up a market share of over 50% within each jurisdiction. The top sellers in a market can control up to 21.26% of the total sales of taxed beverages. The results show that the price of the same brand varies to some extent in different markets and the price variation pattern differs across brands. For example, the average price of Brand 1 ranges from 38.99 cents/12 ounces to 46.36 cents/12 ounces, while the price fluctuation across regions for Brand 3 is more substantial, ranging from 62.22 cents/12 ounces to 82.12 cents/12 ounces.

In Table 5.8, I summarize the statistics for the most popular pack sizes in each jurisdiction, which account for a total pre-tax market share of over 50%. The last column suggests that 2-liter (L), 20 oz, and 12 packs of 12 oz are the sizes that are most often purchased in the taxed jurisdictions. In terms of pricing, a 20-oz beverage is the most expensive on average, followed by 12 packs of 12 oz and 2-liter beverages. The average price of 20-oz beverages varies slightly across jurisdictions, while there is no significant difference in the prices of large sizes.

In addition to the brand and package size, I select the 3 most popular products in each beverage category and report their statistical results in Table 5.9. Despite that at least 13 products are listed for each jurisdiction, only four of them are considered popular in almost all jurisdictions:

Product 2 (regular soda), Product 6 (sports drink), Product 7 (sweetened water), and Product 8 (non-100% juice). While the pricing of Product 6 and Product 7 varies from jurisdiction to jurisdiction, there is little variation in the average prices for Product 2 and Product 8.

Table 5.8. Most Popular Pack-sizes With a Total Market Share of Over 50%

Jurisdictions	Pack-Sizes (total count_ounces per unit)	Average Weekly Price (normalized)	Volume-Weighted Average Weekly Price (normalized)	Market Share (%)
Berkeley	1_20	94.13	84.79	18.31
	1_67.6	29.02	27.69	18.41
	12_12	39.69	36.91	10.49
	1_23	45.41	42.43	10.18
Philadelphia	12_12	35.44	30.73	25.20
	1_67.6	26.28	23.33	23.87
	1_20	97.47	91.29	6.92
Boulder	12_12	37.65	36.00	19.57
	1_20	101.31	96.97	15.73
	1_32	55.97	49.65	7.78
	1_16	151.71	151.26	6.85
Oakland	12_12	36.74	35.19	15.33
	1_67.6	25.79	22.48	22.92
	1_128	27.32	20.20	8.77
	24_12	26.76	25.80	6.42
Seattle	12_12	38.72	34.39	22.57
	1_20	103.79	101.04	15.84
	1_67.6	29.66	27.34	14.16
San Francisco	1_20	97.09	95.87	19.69
	1_67.6	31.49	29.63	19.06
	12_12	42.94	42.09	7.99
	6_12	55.50	47.88	7.58

Notes: The unit of the average price is cents/12 ounces. The market shares are measured based on IRI data.

As I discussed earlier, different types of stores might adopt different pricing strategies. For example, large chains (e.g., supermarkets), tend to use Everyday Low Price as a pricing strategy while small chains (e.g., drug stores) prefer PROMO pricing (Ellickson and Misra 2008). Studies also find that most US food, drugstore, and mass merchandise chains charge nearly uniform prices across stores, despite wide variations in consumer demographics and the level of competition (Della Vigna and Gentzkow 2019).

To test these possibilities, I selected four products (i.e., Product 2, Product 6, Product 7, Product 8) that are often purchased in most of the jurisdictions and reported their statistics (i.e., average price) by store type within each jurisdiction in Table 5.10. Note that not each type of store carries the selected products; for example, Product 8 is not sold at convenience stores in Philadelphia.

The results from Table 5.10 suggest that the price at convenience stores is the highest, followed by drug stores; grocery stores set higher prices for these popular products than mass merchandisers do. In addition, mass merchandisers adopt a nearly uniform pricing strategy for each product across geographic regions, while other types of stores adjust beverage prices to varying degrees in different jurisdictions.

The last table (Table 5.11) displays the market shares of three popular pack sizes (i.e., 20 oz, 67.6 oz, 12 packs of 12 oz) by store type. Not surprisingly, 20-oz taxed drinks make up a larger market share in convenience stores and drug stores than in other types of stores; at large retailers (mass merchandiser and grocery store), taxed drinks in large pack sizes account for a larger proportion of beverage sales than small sizes. With the exception of Boulder, the drug stores in other jurisdictions sell a significant percentage of 2-liter taxed beverages.

Table 5.9. Statistics for Top 3 Most Popular Products in Each Beverage Category

Jurisdictions	Beverage Category	Products	Average Weekly Price (normalized)	Volume-Weighted Average Weekly Price (normalized)	Market Share (%)
Berkeley	Regular Soda	Product 1	33.89	32.73	4.24
		Product 2	40.20	37.18	4.28
		Product 3	104.61	104.55	2.44
	Sports/energy Drinks	Product 4	70.46	66.88	6.00
		Product 5	70.08	66.62	1.72
		Product 6	52.90	48.71	1.62
	Taxed Water	Product 7	91.93	90.03	2.98
	Non-100% Juice	Product 8	71.53	63.81	1.40
		Product 9	44.96	41.45	1.35
		Product 10	58.05	55.91	0.95
	Sweetened Tea/Coffee	Product 11	45.34	42.56	7.89
		Product 12	29.68	28.88	4.78
		Product 13	52.41	50.17	2.63
Philadelphia	Regular Soda	Product 2	36.81	31.86	3.55
		Product 14	35.74	30.75	3.40
		Product 15	26.98	23.21	4.40
	Sports Drinks	Product 6	42.29	36.10	1.51
		Product 4	40.99	36.97	1.39
		Product 16	43.40	39.53	1.26
	Taxed Water	Product 7	78.96	67.50	0.89
		Product 17	83.76	76.76	0.42
		Product 18	73.18	64.49	0.40
	Non-100% Juice	Product 19	22.19	21.49	1.72
		Product 20	57.44	43.49	1.52
		Product 8	65.48	53.73	1.41
	Sweetened Tea / Coffee	Product 11	43.45	36.84	2.33
		Product 21	34.72	31.73	0.50
		Product 22	36.26	35.80	0.91
Boulder	Regular Soda	Product 2	40.61	36.17	5.88
		Product 3	98.31	97.70	2.61
		Product 14	38.17	33.10	2.40
	Sports Drinks	Product 23	83.71	83.04	4.04
		Product 16	40.74	40.26	2.41
		Product 6	54.18	51.15	2.28
	Taxed Water	Product 7	92.29	84.08	2.27
		Product 24	82.36	80.40	0.48
	Non-100% Juice	Product 8	58.76	50.83	0.83
		Product 25	103.67	103.50	0.69
		Product 26	87.05	87.20	0.27
	Sweetened Tea/Coffee	Product 11	48.49	48.27	2.77
		Product 12	26.13	25.72	1.29

		Product 27	78.46	76.21	0.85
Oakland	Regular Soda	Product 28	33.99	32.69	4.69
		Product 2	40.33	38.83	3.89
		Product 1	33.03	28.66	4.51
	Sports/energy Drinks	Product 4	44.52	37.18	2.85
		Product 16	42.93	39.64	2.20
		Product 6	47.11	33.95	1.62
	Taxed Water	Product 7	87.27	79.09	0.88
	Non-100% Juice	Product 29	18.24	15.58	3.87
		Product 8	67.92	56.15	1.27
		Product 30	14.20	13.63	1.11
	Sweetened Tea/Coffee	Product 31	45.58	37.50	2.89
		Product 12	28.60	27.15	1.79
		Product 13	50.50	45.58	0.66
Seattle	Regular Soda	Product 2	40.76	33.75	7.20
		Product 3	99.90	100.40	3.50
		Product 14	38.96	32.27	2.88
	Sports/energy Drinks	Product 6	51.64	48.34	2.84
		Product 23	84.84	82.78	1.75
		Product 16	40.82	39.85	1.18
	Taxed Water	Product 32	51.79	48.83	0.16
	Non-100% Juice	Product 8	64.66	54.52	1.59
		Product 33	112.21	89.38	0.51
		Product 34	43.44	40.83	0.87
	Sweetened Tea/Coffee	Product 35	240.00	237.87	0.73
		Product 36	220.69	219.95	0.71
		Product 37	45.16	41.72	0.62
San Francisco	Regular Soda	Product 3	103.48	103.62	5.31
		Product 38	51.37	46.55	4.14
		Product 2	46.30	43.37	2.76
	Sports/energy Drinks	Product 6	52.34	48.23	2.65
		Product 39	97.12	95.98	1.71
		Product 23	92.58	89.40	1.23
	Taxed Water	Product 7	91.01	87.20	0.15
	Non-100% Juice	Product 8	70.79	61.24	2.06
		Product 40	86.04	80.67	1.56
		Product 41	104.93	104.69	0.47
	Sweetened Tea/Coffee	Product 35	241.77	235.46	0.81
		Product 42	118.57	112.14	0.61
		Product 37	44.00	40.53	0.63

Notes: The unit of the average price is cents/12 ounces.

Table 5.10. The Average Prices of Popular Products Across Store Types Within Each Taxed Jurisdiction

Jurisdictions	Store Type	Product 2		Product 6		Product 7		Product 8	
		Average Weekly Price (normalized)	Volume-Weighted Average Weekly Price (normalized)	Average Weekly Price (normalized)	Volume-Weighted Average Weekly Price (normalized)	Average Weekly Price (normalized)	Volume-Weighted Average Weekly Price (normalized)	Average Weekly Price (normalized)	Volume-Weighted Average Weekly Price (normalized)
Berkeley	Drug Stores	121.72	117.94	52.90	48.71	91.93	90.03	71.53	63.81
Philadelphia	Drug Stores	137.93	136.88	47.19	43.65	84.03	79.08	72.42	65.81
	Mass Merchandisers	117.99	117.81	30.61	29.89	59.34	57.32	48.19	44.80
	Dollar Stores	116.46	111.92	34.87	34.34	77.90	77.87	53.60	53.23
	Grocery Stores	128.75	127.67	35.94	32.62	67.26	61.08	64.71	52.39
	Convenience	145.64	140.62	62.14	59.99	93.15	89.59	--	--
Boulder	Drug Stores	145.71	145.50	51.59	51.03	90.27	88.67	69.42	67.20
	Convenience	167.52	166.10	60.23	55.30	103.08	101.43	--	--
	Grocery Stores	139.03	137.20	33.25	31.92	61.27	59.76	60.03	58.17
	Mass Merchandisers	119.85	119.40	31.23	30.64	59.29	58.56	46.88	45.95
Oakland	Convenience	160.88	161.38	61.66	60.36	106.37	104.64	--	--
	Drug Stores	148.42	149.54	50.31	45.27	88.38	84.26	71.40	62.27
	Grocery Stores	113.95	108.36	35.80	29.38	75.11	71.02	65.80	54.82
Seattle	Convenience	157.74	158.37	58.03	56.97	119.24	119.35	--	--
	Drug Stores	140.54	141.21	50.94	48.45	97.59	90.41	69.34	64.68
	Mass Merchandisers	123.63	123.39	33.17	32.36	--	--	48.36	47.02
San Francisco	Convenience	167.12	165.51	61.23	59.81	--	--	--	--
	Drug Stores	145.50	143.61	54.42	51.36	92.76	89.66	73.02	67.30
	Grocery Stores	120.07	119.75	37.73	34.37	71.62	67.79	67.23	58.60
	Mass Merchandisers	126.07	123.09	33.00	31.46	--	--	48.66	46.87

Notes: The unit of the average price is cents/12 ounces.

Table 5.11. Market Shares of Different Package Sizes Within Each Store Type

Jurisdictions	Store Type	1_20oz	1_67.6oz	12_12oz
Berkeley	Drug stores	18.31%	18.41%	10.49%
Philadelphia	Drug stores	14.69%	25.79%	16.97%
	Mass Merchandisers	8.65%	14.54%	37.03%
	Dollar stores	6.66%	36.42%	6.96%
	Grocery stores	2.49%	25.34%	31.71%
	Convenience stores	42.22%	5.76%	0.84%
Boulder	Drug stores	19.01%	10.11%	17.86%
	Convenience stores	21.84%	5.46%	9.16%
	Grocery stores	4.71%	10.98%	22.45%
	Mass Merchandisers	9.23%	8.42%	35.69%
Oakland	Convenience stores	33.50%	5.33%	0.33%
	Drug stores	18.74%	20.66%	16.72%
	Grocery stores	2.44%	24.20%	15.73%
Seattle	Convenience stores	25.84%	10.36%	3.59%
	Drug stores	17.16%	21.33%	19.25%
	Mass Merchandisers	10.28%	14.04%	34.07%
San Francisco	Convenience stores	25.49%	16.75%	1.83%
	Drug stores	25.75%	18.39%	2.07%
	Grocery stores	3.71%	23.27%	19.86%
	Mass Merchandisers	13.13%	15.24%	21.39%

Notes:1_20oz refers to a 20-ounce bottle. 1_67.6oz refers to a 2-liter bottle. 12_12oz refers to 12 packs of 12-oz beverages. Market shares are calculated based on IRI data.

Chapter 6
Methodology

My main identification strategy is to use a difference-in-differences approach to estimate the responses of both retailers and consumers to a soda tax. This method compares the changes in the outcome measures (i.e., price, quantity) at treated stores to those at stores in the control group. In this analysis, I consider both stores in a taxed jurisdiction and stores in the adjoining region as the treated units. Control stores are those similar to treated ones in terms of demographics, pre-tax trends in prices and quantities, etc., but not affected by a soda tax directly or indirectly. Hence, the candidates include stores located in an untaxed region that does not share the border with a taxed jurisdiction. I provide details on the process of control selection in Subsection 6.2. In all regressions, I include time fixed effects to control for seasonality and cluster the standard errors at the store level to address the potential issue of serial correlation across products within a store. I also explore the heterogeneity in the impact of a soda tax across various dimensions. Further, I conduct triple-difference estimations for robustness purposes.

6.1 Difference-in-Differences Specifications

In this section, I describe the empirical specifications based on which I estimate the average effect and heterogeneous effects of a soda tax.

6.1.1 Average Treatment Effect

I first use the difference-in-differences approach to estimate the average treatment effect of a soda tax on the price and volume sales of taxed beverages sold at treated stores. Here, treated stores refer to those located in a taxed jurisdiction. In the regressions below, the unit of observation is the sales information of a product at a given store in a given week. The basic specification for the regression estimations in this study is given by Eq. (6.1):

$$Y_{isw} = \alpha + \boldsymbol{\beta} \sum_{t=1}^{T^*} Treated_s PostTax_t + \gamma Treated_s + \delta_q + \theta_y + \varepsilon_{isw} \qquad (6.1)$$

where Y_{isw} denotes the outcome variables of interest that can be log(P_{isw}), the natural logarithm of P_{isw}, the price (cents/12 ounces) of product i at store s in week w, or log(Q_{isw}), the natural logarithm of Q_{isw}, the weekly normalized volume sales; $Treated_s$ is a dummy variable that equals one for taxed stores and zero otherwise; $PostTax_t$ denotes a dummy equal to one for the tth year after the tax implementation and t can take the values of $1, 2, \ldots, T^*$, where T^* represents the maximum years a tax has been in effect. δ_q and θ_y represent quarter, and year fixed effects, respectively.

I assume the error term ε_{isw} to be orthogonal to the explanatory variables and cluster errors at the store level. The parameter vector $\boldsymbol{\beta}$ contains the coefficients of interest that measure, in percentage format, the average treatment effect of a soda tax for each year after the tax was implemented. I carry out regressions using the most disaggregate data at the UPC/store/week level.

Cluster-robust standard errors are typically used to account for the potentially correlated error terms across observations within clusters (e.g., a store). In terms of the quantity effects of a soda tax, customers of specific stores tend to have unique demographics that can have a common influence on sales across the various taxed beverages. Other features related to stores, such as size, layout, number of registers, may have similar effects, resulting in a correlation between products sold in the same store. Therefore, I use the strategy of clustering at the store level in the quantity regressions. Chain retailers, as I discussed previously, generally use "zone pricing," wherein product prices are similar or identical for stores belonging to the same chain. This implies that price changes within the same chain might be comparable. Thus, clustering at the chain level appears to be reasonable in the price regressions. However, for most of the taxed jurisdictions, there are just a few clusters (i.e., chains) in the data, which can cause an over-rejection issue (Cameron and Miller 2015). Chain-level clustering therefore might be impractical and, given that it is more desirable to cluster at the store level for quantity effects, I also use store-level clustering in the price regressions to make my clustering strategy consistent. This strategy is consistent with the prior literature on soda taxes, which has largely used store-level clustering as well (Cawley and Frisvold 2017; Rojas and Wang 2021; Seiler, Tuchman, and Yao 2021).

6.1.2 Test for Spillover Effect: Substitution to Untaxed Beverages and Cross-border Shopping

As I discussed earlier, consumers might switch to other untaxed beverages if a soda tax causes an increase in the price of taxed ones. To test this possibility, I explore whether the consumption, as well as the price of untaxed beverages, change due to a soda tax and re-estimate Eq. (6.1) with a focus on the sales data on untaxed beverages (e.g., pure water, 100% natural juices) sold in a taxed jurisdiction and their counterparts in the control group.

In addition to the substitution effect, consumers can avoid the tax by shopping in a nearby untaxed city. If such tax avoidance behavior really matters, the nearby untaxed retailers might also be indirectly affected by the tax. I investigate the cross-border shopping behavior by an estimation of whether the sales of taxed products sold in the untaxed stores near the border are influenced by the tax. The strategy is to estimate a modified version of Eq. (6.1) where I consider the immediate neighboring stores (untaxed) as the treated units. Specifically, I replace $Treated_s$ in Eq. (6.1) with a dummy variable that equals one if the store s is located in a nearby untaxed city. I still draw on data on taxed beverages and use the same controls as those for taxed stores. Thus, the difference-in-differences coefficients (i.e., $\boldsymbol{\beta}$) provide evidence of cross-border shopping behavior. If the coefficient vector is generated from the price model, it suggests whether and how the untaxed retailers near the border strategically respond to a soda tax, e.g., adjust their prices.

6.1.3 Specifications Considering Heterogeneity

I also estimate specifications where a soda tax is allowed to have varying effects on the price and volume sales of taxed beverages across various dimensions, such as beverage category, store type, etc. To this end, I employ the following specification:

$$\begin{aligned} Y_{isw} = {} & \tilde{\alpha} + (Treated_s \times PostTax_w \times \boldsymbol{D})'\widetilde{\boldsymbol{\beta}} + \widetilde{\beta_1}(Treated_s \times PostTax_w) \\ & + (PostTax_w \times \boldsymbol{D})'\widetilde{\boldsymbol{\beta_2}} + (Treated_s \times \boldsymbol{D})'\widetilde{\boldsymbol{\beta_3}} + \widetilde{\beta_4}Treated_s \\ & + \boldsymbol{D}'\widetilde{\boldsymbol{\beta_5}} + \tilde{\delta}_q + \tilde{\theta}_y + \tilde{\varepsilon}_{isw} \end{aligned} \tag{6.2}$$

where I include a set of dummy variables, denoted by vector $\boldsymbol{D}$; each dummy in $\boldsymbol{D}$ represents one value of a dimension, if it is a categorical variable (e.g., store type), and represents a range of values if a dimension is continuous, such as income. I interact vector $\boldsymbol{D}$ with a traditional difference-in-differences term $Treated_s \cdot PostTax_w$ in which $PostTax_w$ is a dummy that equals one for weeks after the tax is implemented. The coefficient vector $\widetilde{\boldsymbol{\beta}}$ on the main interaction terms measure the heterogeneous effects of a tax based on one dimension. Y_{isw} and $Treated_s$ have the same definitions as in Eq. (6.1). To complete this model, I include other relevant interaction terms. Again, $\tilde{\delta}_q$, and $\tilde{\theta}_y$ represent quarter and year fixed effects; $\tilde{\varepsilon}_{isw}$ is the error term. The dimensions that I attempt to explore include store type, beverage category, package size, median household income, ethnic diversity, etc. I estimate specification 6.2 only for one dimension at a time.

6.2 Choices of Control Cities

Given the nationwide coverage of our data, I have a rich set of cities that can serve as controls. In this section, I illustrate the procedure utilized to select the control city for each taxed jurisdiction.

6.2.1 Cluster Analysis

A formal difference-in-differences strategy requires that the treatment stores and controls in this analysis be similar in terms of observable determinants of the prices and consumption of soda drinks. The controls should satisfy the "common pre-trends" assumption – that is, the outcome measures (price and volume sales) for the treated and control stores move in parallel during the pre-tax period.

For each taxed jurisdiction, I first choose cities that are similar to it based on the population reported in the U.S. Census data as a pool of potential controls. For example, I chose cities with a larger population into the control pool for Philadelphia, as large cities are close to each other in terms of composition and distribution of retail outlets, and we might expect similar behavior in terms of cross-border shopping. Further, I employ cluster analysis to choose the candidate controls (cities) for each taxed jurisdiction based on demographics that can affect soda consumption and retail pricing behavior. The demographics considered include the area in square miles, population density, median household income, the percentage of people in poverty, the percentage of non-whites, and the percentage of high school graduates or higher.

Cluster analysis is a data mining method that identifies groups of similar objects within a data set of interest; each group contains observations with a similar profile according to specific criteria. In this analysis, the criteria refer to the demographics mentioned above. The similarity between observations is defined using some inter-observation distance measures including Euclidean and correlation-based distance measures. Here, I rely on a Euclidean Distance and use Wald's method in the agglomeration process.[18]

Figure 6.1 displays the cluster dendrogram for each taxed jurisdiction from the cluster analysis. The objects (cities) on the same branch of the tree have a greater degree of similarity and the lower the height of a branch is, the more similar the objects on its two children are. For example, Portland is the city most similar to Seattle, as the branch connecting them is the shortest. I choose the four or five cities closest to each taxed jurisdiction in terms of demographics as the candidate controls and further test the common trend assumption next step. Table 6.1 contains the demographics for both taxed jurisdictions and their candidate controls.

6.2.2 Check for Common Pre-trends Assumption

With the candidate controls established, I check for the common-trends assumption by plotting the price/quantity series of treated stores against each control group. I first aggregate the weekly sales

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[18] The formula of Euclidean Distance is given by the following:

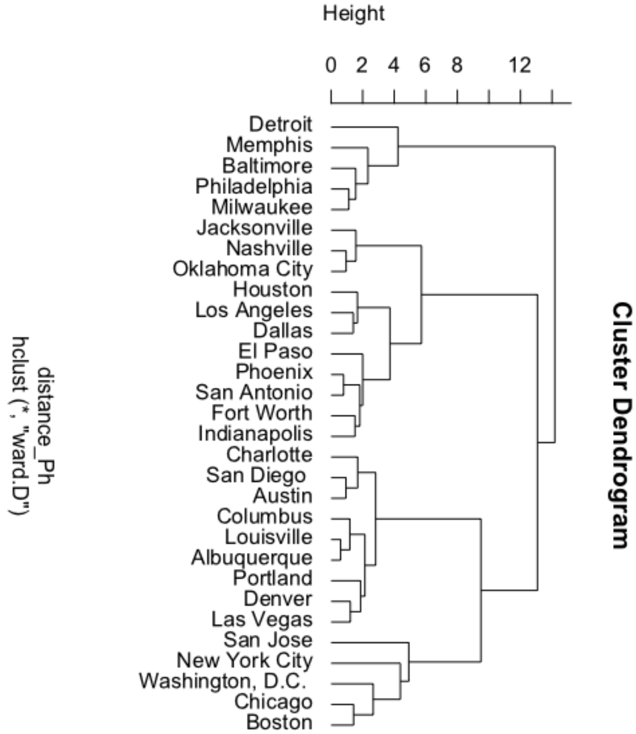
$$d(X,Y) = \sqrt{\sum_{i=1}^{n}(x_i - y_i)^2}$$

X and Y are two points in Euclidean n-space, here, they can be two cities. n is the number of dimensions of the space. x_i and y_i indicate two values in the ith dimension. Ward's method is also known as Ward's minimum variance method, a criterion that minimizes the total within-cluster variance. At each step of this method, the investigator finds a pair of clusters to merge that minimizes the increase in total within-cluster variance.

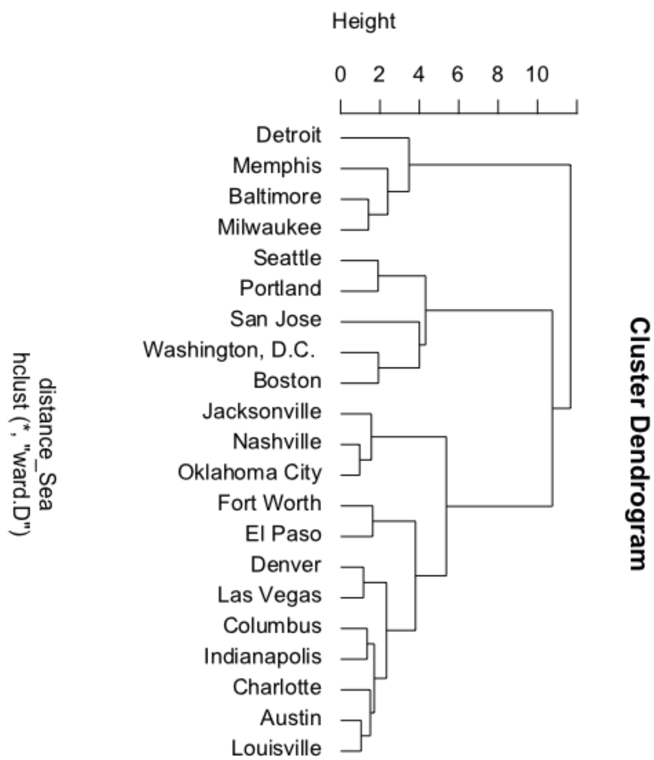
panel data up to the month level and calculate the monthly average price for each UPC/store combination as the ratio of total monthly revenue to total monthly volume sales. To create the price and volume time series, I calculate the monthly volume-weighted average price and monthly average sales across UPCs and stores for each taxed jurisdiction and its control, where the weights are the total monthly volume sales per UPC for each store. I drop observations containing infrequently purchased products, for which only pre-tax or post-tax sales data are available. Keeping products that are sold during both periods ensures that the taxed beverages can be represented throughout the whole period.

Figure 6.2 displays the results of the control selection procedure. By comparing the price/quantity series of a taxed jurisdiction to each candidate control, I determine to use the following cities as our controls: Minneapolis for Berkeley, Baltimore for Philadelphia, New York for San Francisco, Portland for Seattle, Chapel Hill for Boulder, and Long Beach for Oakland, as graphs for these pairs of cities most support the common-trends assumption. In this figure, the vertical line on the right represents the date when a soda tax was implemented. The left line is for the date when the tax was passed.

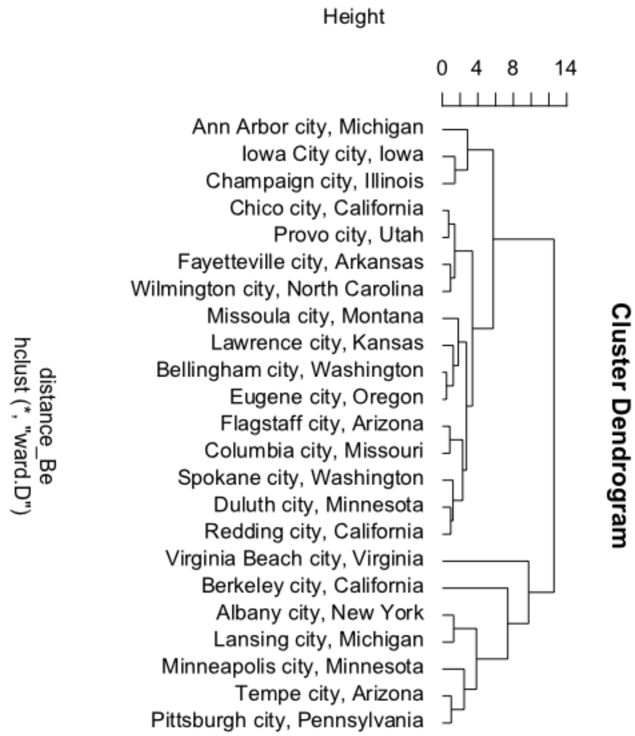
Figure 6.1 Cluster Dendrograms for Taxed Jurisdictions from Cluster Analysis



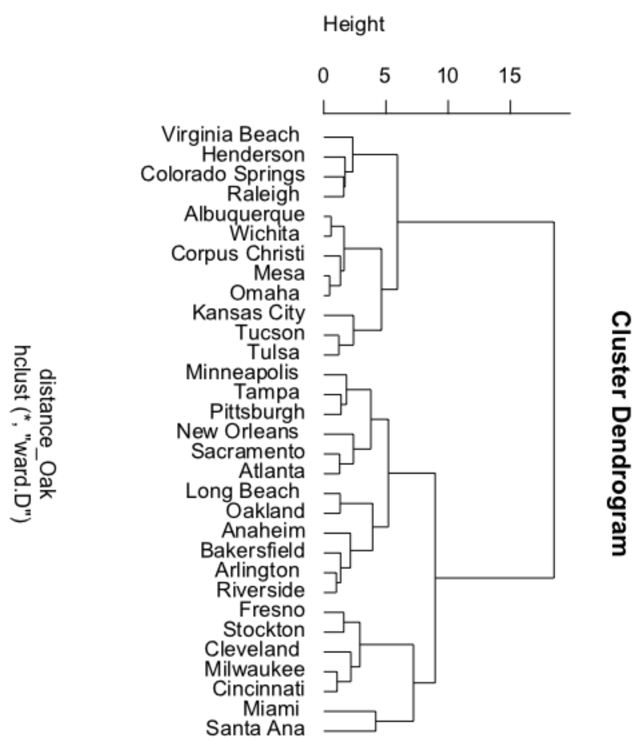
Philadelphia and the Pool of Controls



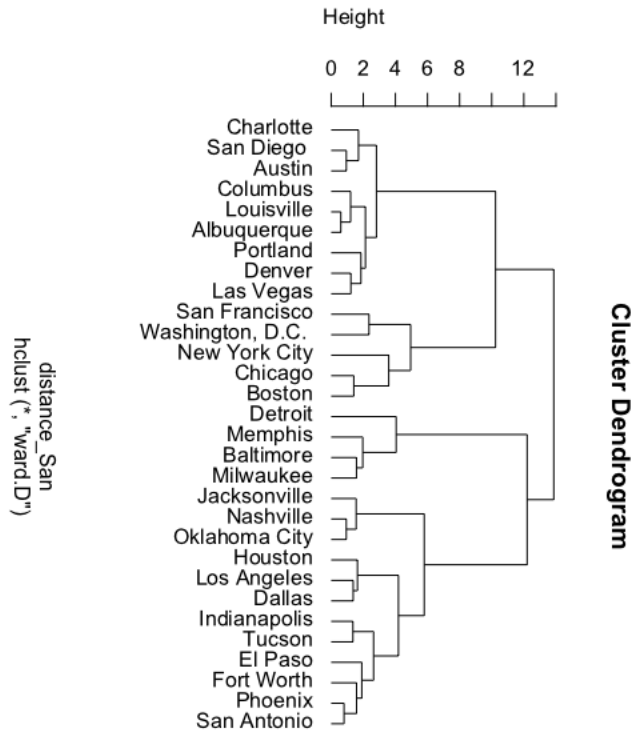
Seattle and the Pool of Controls



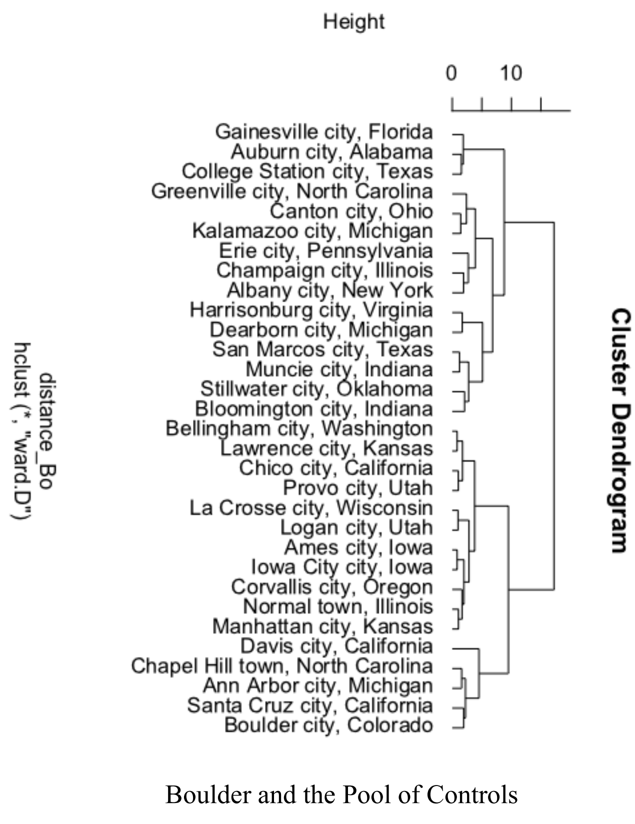
Berkeley and the Pool of Controls



Oakland and the Pool of Controls



San Francisco and the Pool of Controls



Boulder and the Pool of Controls

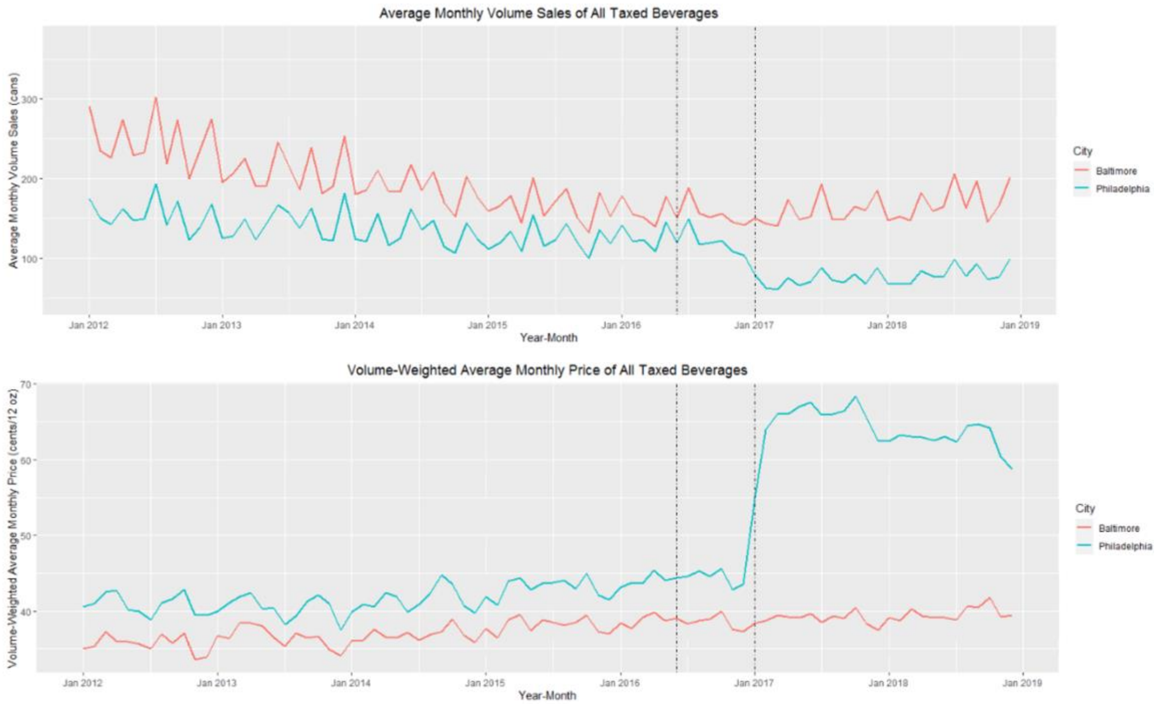
Table 6.1. Demographics of Taxed Jurisdictions and Candidate Controls

Taxed Jurisdictions and Controls	Population (N)	Median Household Income (Dollars)	High School or Over (%)	Nonwhites (%)	Poverty (%)	Population Density (N/Sq mi)	Area (Square Miles)
Philadelphia, PA	1,584,138	40,649	83.27	58.45	25.80	11,683	134.20
Baltimore, MD	602,495	46,641	84.17	69.71	22.43	7,598	80.90
Detroit, MI	672,662	27,838	79.72	85.90	37.85	4,847	138.80
Memphis, TN	650,618	38,230	84.53	70.79	26.93	2,056	317.40
Milwaukee, WI	592,025	38,289	83.02	54.23	27.41	6,186	96.20
Seattle, WA	744,955	79,565	94.20	31.37	12.47	8,405	83.80
Portland, OR	653,115	61,532	91.83	22.63	16.22	4,793	133.50
San Jose, CA	1,030,119	96,662	83.51	59.32	10.04	5,777	177.50
Washington, D.C.	702,455	77,649	90.30	59.30	17.40	11,148	61.10
Boston, MA	694,583	62,021	86.06	47.24	20.54	13,938	48.30
Oakland, CA	429,082	63,251	80.65	63.34	18.74	7,691	55.79
Long Beach, CA	467,354	58,314	79.82	47.53	19.07	9,293	50.29
Anaheim, CA	352,005	65,313	76.58	31.31	15.97	7,062	49.84
Bakersfield, CA	383,579	60,058	80.03	31.65	19.16	2,698	142.16
Arlington, TX	398,112	55,562	84.75	37.02	16.07	4,152	95.88
Riverside, CA	330,063	62,460	79.38	38.10	16.57	4,068	81.14
San Francisco, CA	883,305	96,265	87.90	52.76	11.65	18,569	46.90
New York City, NY	8,398,748	57,782	81.11	57.22	19.57	28,317	301.50
Washington, D.C.	702,455	77,649	90.30	59.30	17.40	11,148	61.10
Boston, MA	694,583	62,021	86.06	47.24	20.54	13,938	48.30
Chicago, IL	2,705,994	52,497	83.80	50.86	20.64	11,900	227.30
Berkeley, CA	121,643	75,709	96.18	39.82	19.81	11,618	10.47
Minneapolis, MN	425,403	55,720	89.30	36.07	20.73	7,882	53.97
Pittsburgh, PA	301,048	44,092	92.09	33.36	22.03	5,437	55.37
Tempe, AZ	192,364	51,829	92.22	31.25	21.34	4,818	39.93
Albany, NY	97,279	43,790	89.56	44.92	24.52	4,548	21.39
Lansing, MI	118,427	38,642	88.82	38.65	27.09	3,285	36.05

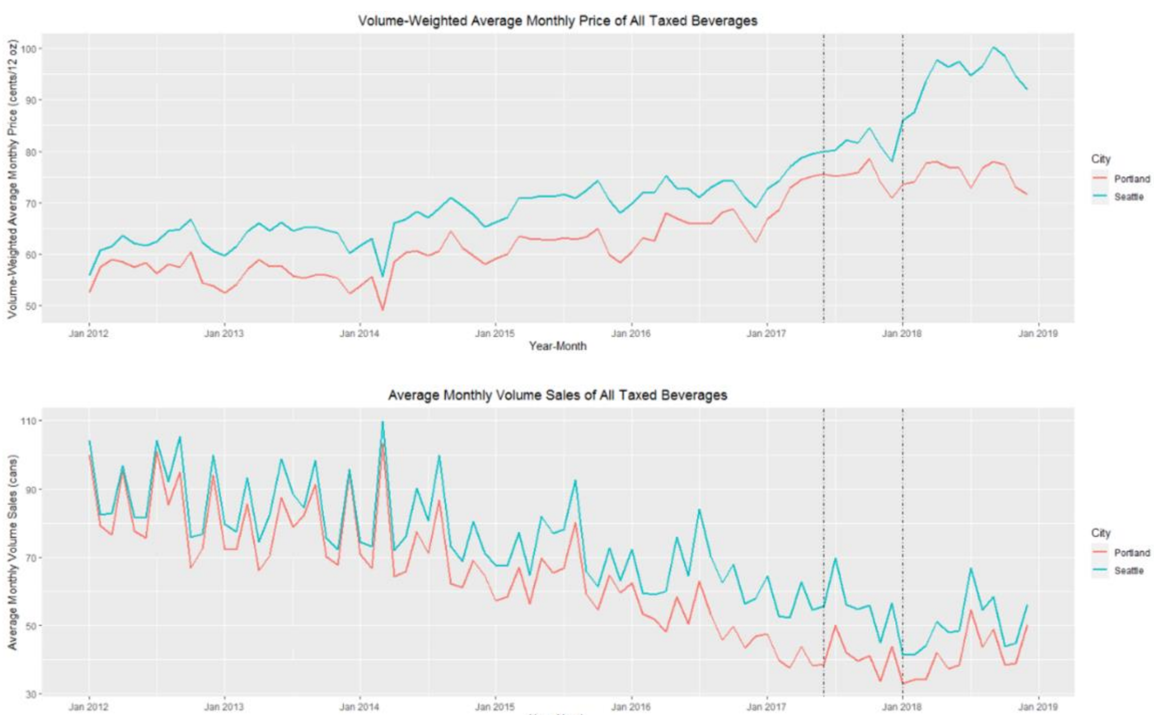
Boulder, CO	107,353	64,183	96.54	12.10	21.58	4,353	24.66
Chapel Hill, NC	60,988	67,426	96.21	27.47	20.31	2,888	21.12
Davis, CA	69,289	63,071	96.94	36.16	29.07	7,006	9.89
Santa Cruz, CA	64,725	65,421	93.72	23.61	24.42	5,080	12.74
Ann Arbor, MI	121,890	61,247	96.84	27.97	22.15	4,380	27.83

Notes: the jurisdictions in bold are taxed.

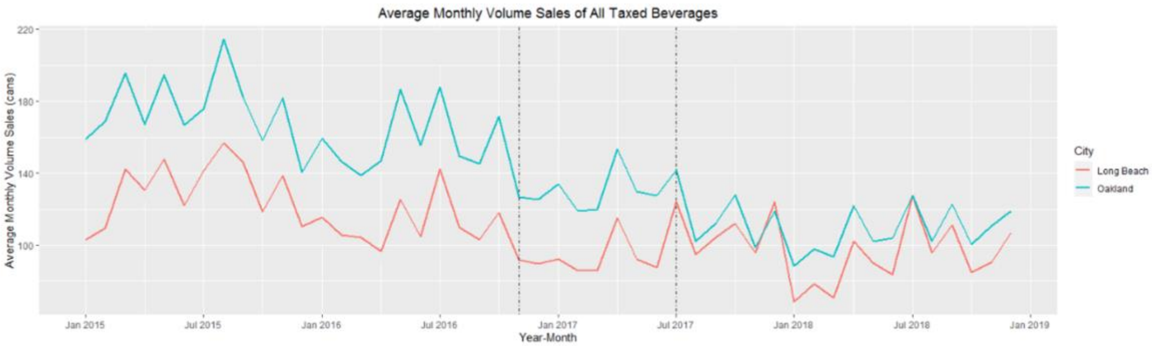
Figure 6.2 Average Monthly Price and Volume Series for Taxed Jurisdictions and Controls



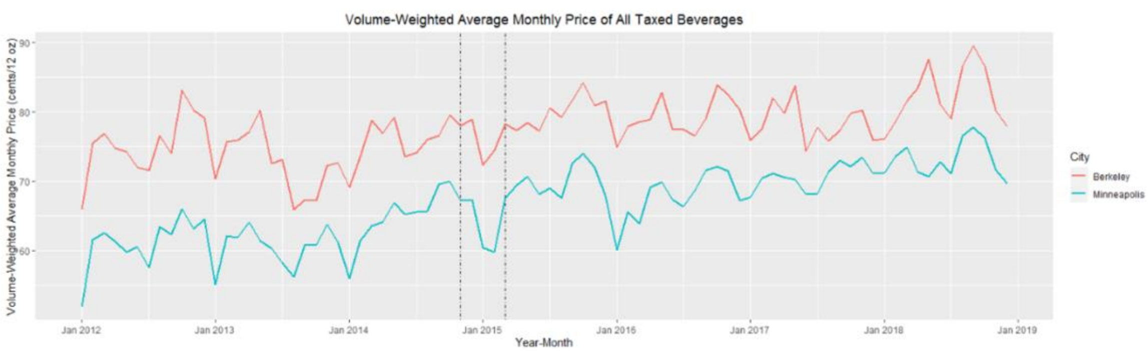
Philadelphia and Baltimore

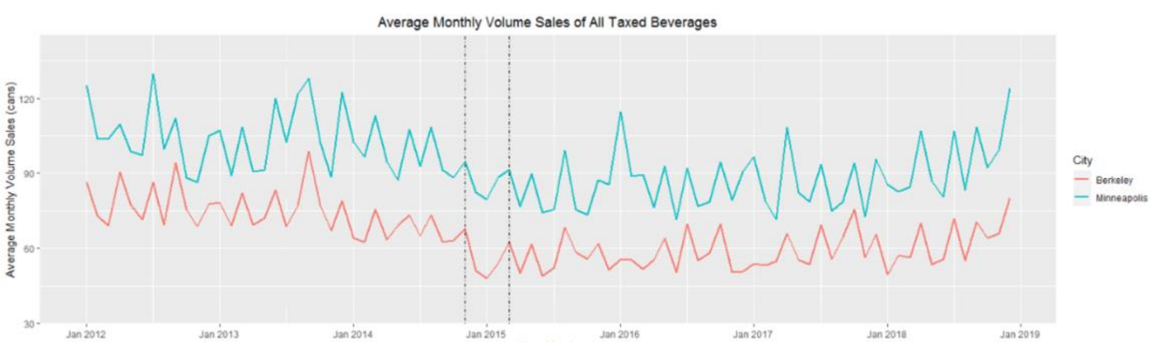


Seattle and Portland

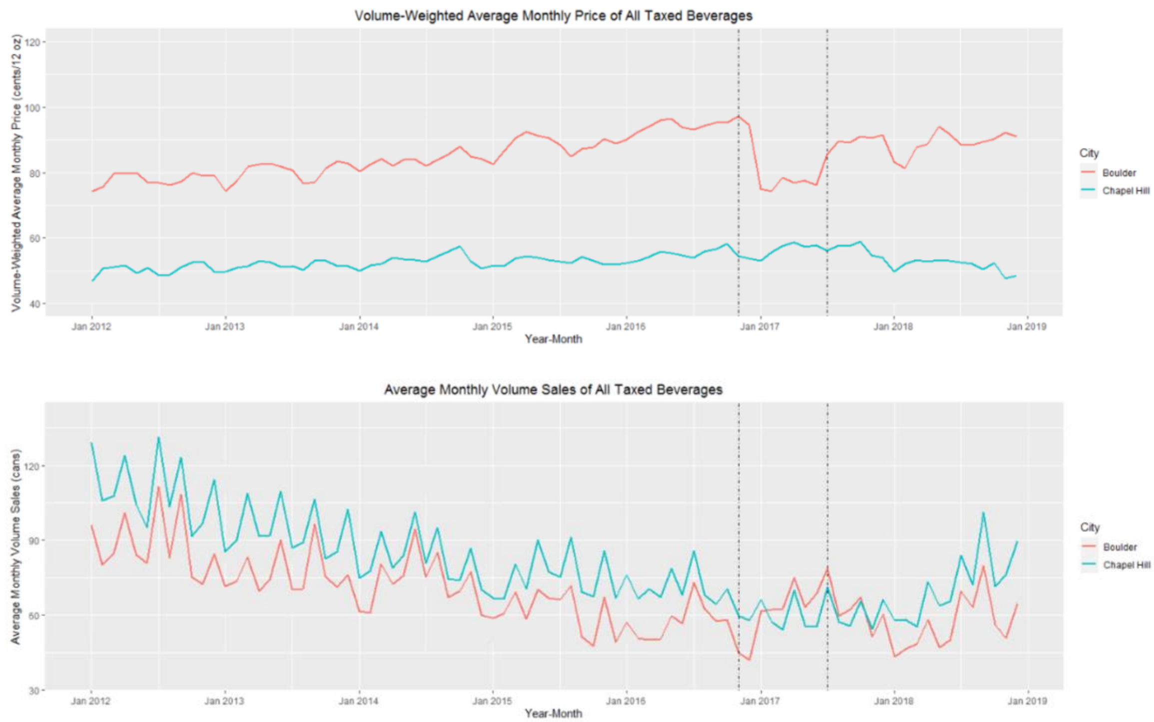


Oakland and Long Beach

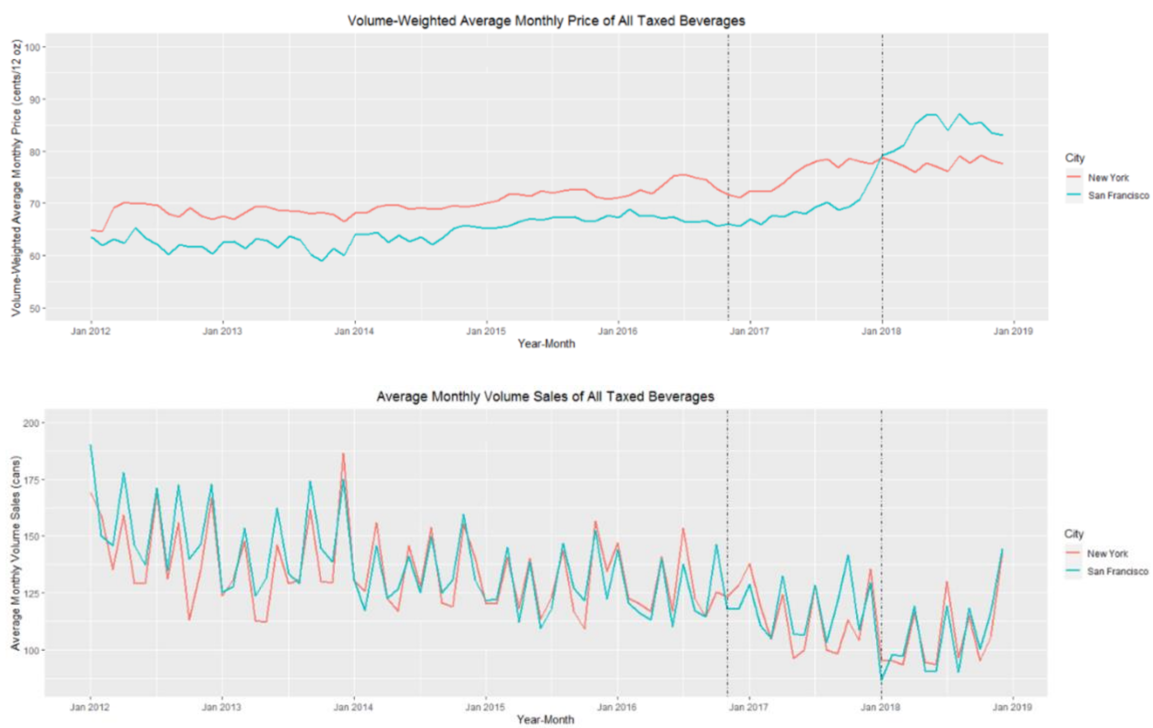




Berkeley and Minneapolis



Boulder and Chapel Hill



San Francisco and New York

Notes: the vertical line on the right represents the date when a soda tax was implemented, and the left vertical line is for the date when a soda tax was passed.

6.3 Triple-difference Estimations

The unobserved events outside of this study, such as cost increases experienced by the treated stores but not by the controls, may have similar effects on the price and sales as the imposition of a soda tax. The difference-in-differences estimator can attribute these effects to the tax imposition. A triple-difference approach, however, avoids this issue by removing time-by-place effects that are largely common to products sold in the same store (Berck et al. 2016). I perform robustness checks on the results by estimating a series of triple-difference specifications and repeating the analysis I have conducted so far. If the results obtained from the triple-difference estimations are consistent with those I got from the DID estimations, the results of this study are robust and do not suffer from the issue I just mentioned.

In a triple-difference specification, I include bottled water as an additional counterfactual, as well as other variables in the difference-in-differences equations. One issue relating to the triple-difference approach is that the untaxed beverage products might also be affected by the tax, through the cross-price elasticity of demand. Close substitutes for soda, such as diet soda, thus cannot be used as controls. Compared with other untaxed beverages, bottled water appears to be a desirable choice, given the relatively low documented cross-price elasticity between soda and water (Zhen et al. 2011; Dharmasena and Capps 2012). But since water is suggested as a healthy alternative to drinking sodas, e.g., by the Berkeley advocacy efforts, its sales may increase due to the tax. Thus, I will test whether pure bottled water is affected by the tax by estimating Eq. (6.1) and if the sales of water significantly change after the tax is implemented, I use other bottled beverages such as juice and tea as another comparison group. The basic specification for the triple-difference approach is given by:

$$
\begin{aligned}
Y_{isw} = \bar{\alpha} + \overline{\boldsymbol{\beta}} \sum_{t=1}^{T^*} Treated_s PostTax_t Category_i + \overline{\boldsymbol{\beta_1}} \sum_{t=1}^{T^*} Treated_s PostTax_t \\
+ \overline{\boldsymbol{\beta_2}} \sum_{t=1}^{T^*} PostTax_t Category_i + \overline{\beta_3}(Treated_s \times Category_i) \\
+ \overline{\beta_4} Category_i + \overline{\beta_5} Treated_s + \overline{\delta_q} + \overline{\theta_y} + \overline{\varepsilon_{isw}} \qquad (6.3)
\end{aligned}
$$

in which $Category_i$ is a dummy variable equal to one if beverage i is in the taxed beverage category and zero otherwise; other variables are the same as in Eq. (6.1). The coefficient vector of interest is $\overline{\boldsymbol{\beta}}$, which captures the causal effect of a soda tax on the price and volume sales of taxed beverages sold at taxed stores after removing the potential effects of any unobservable event.

Chapter 7
Results

In this chapter, I first report the estimation results for the average impact on taxed beverages in section 7.1, including percentage changes in the price and volume sales. I further report the empirical results in section 7.2 for the average impact on untaxed beverages sold in taxed jurisdictions and the effects on the nearby stores that are not taxed. In section 7.3, I estimate the specification (6.2) and discuss the estimation results for the heterogeneous effects of soda taxes across different dimensions, including store type, beverage category, package size, and local demographics.

7.1 Average Impact on Taxable Beverages

I first report and describe the estimation results on average impacts from DID regressions and perform robustness checks using the triple-difference approach.

7.1.1 Results Based on Difference-in-Differences Regressions

In this subsection, I focus on the impact of a soda tax on taxed beverages and estimate the specification (6.1) separately for each taxed jurisdiction. The existing literature on the effects of soda taxes is largely based on short time series (e.g., one-year post implementation) and thus, only is capable of capturing short-run impacts of a tax. As I have noted, longer-term impacts may differ for multiple reasons. The analysis reported here covers a longer after-tax period than other studies. This allows me to examine not only the short-term effects but also the effects in the long run. Unlike prior studies, I estimate the treatment effect each year after the tax implementation to examine how a soda tax affects retailers and consumers gradually over time.

When the outcome variable is Ln (Price) (or Ln (Volume)), the difference-in-differences (DID) regression coefficient approximates the percentage change in price (or volume sales) after the tax for small values (<0.01).[19] To correctly interpret the coefficient as a percentage change across a wider range of values, I adjust each coefficient associated with switching a dummy variable from 0 to 1 using the following formula,

$$100 \times (e^{\beta} - 1)$$

where β is the estimated DID regression coefficient (Halvorsen and Palmquist 1980). For small price or sales changes, this correction does not affect the reported coefficients in significant ways but for large changes it does. The corrected DID coefficients (i.e., the treatment effects) are reported in the first row associated with each year in Table 7.1. For significant coefficients in the price column, I further calculate the pass-through rates using the tax rate and the average price during the pre-tax period and present them in square brackets.[20] Additionally, I report the implied price elasticities of demand in square brackets in the quantity column, regardless of whether the price and quantity coefficients are significant. If the coefficients in both the price and quantity columns are statistically significant, I bold the corresponding price elasticity.

Results show that except for the first three years Berkeley's tax was implemented, soda taxes in all jurisdictions had statistically significant effects on prices each year, and the pass-through rates for all taxes were less than 100% (i.e., under-shifted). This result is consistent with the theoretical prediction that retailers with market power may not fully shift the tax to consumers.[21] I consistently find that the pass-through rate increased over time, implying retailers in the taxing jurisdictions gradually raised the prices of taxed beverages in response to soda taxes. This result is also in line with expectations. When retailers increase prices, they run the risk of

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[19] All current studies on soda taxes, if they estimate percentage changes, report the DID coefficients without correction.
[20] In the first year after the implementation of Philadelphia's tax, for example, the average price of taxed beverages rose by 30.36%. The formula used to calculate the pass-through rate is to divide the product of the average price (e.g., 3.59 cents/ounce in Philadelphia) and the percent change in price by the tax rate (e.g., 1.5 cents/ounce in Philadelphia), yielding a pass-through rate of 72.69% in Philadelphia.
[21] Interpretation of the pass through rates is clouded, however, by lack of knowledge as to whether the tax was fully passed forward from distributors to retailers.

antagonizing consumers. To lessen the blow of a large price rise and to exploit rational consumer inattention, they might raise their prices gradually (Anderson and Simester 2010; Mackowiak, Matejka, and Wiederholt 2022). This finding is also consistent with distributors passing a greater percentage of the tax forward to retailers over time. For example, as noted, contracts may have limited pass forward from distributors in the short run.

When comparing the pass-through rates of different jurisdictions for each year after the tax was implemented, I find that Philadelphia and Seattle, which passed the taxes through a council vote, exhibit higher pass-through rates than other jurisdictions. This result is consistent with the hypothesis that retailers in jurisdictions that passed their taxes through public referendum tend to pass a smaller proportion of the tax on to consumers as such taxes were typically accompanied by extensive media campaigns that attracted widespread attention among consumers. To avoid losing customers, retailers in these jurisdictions may have chosen to not raise prices substantially in the immediate aftermath of tax implementation.

The results for the impact on Berkeley's prices are particularly important given the extensive studies on Berkeley's tax in the literature. The early studies on Berkeley, which drew on short-term data (e.g., less than one year after the tax), found no significant impact on prices (Rojas and Wang 2021). This is consistent with my results for the first three years in Berkeley. However, I also find in the fourth and fifth years after the soda tax was implemented, prices started to rise, and the coefficients are statistically significant, with 45.1% and 76.86% of the tax being passed on to consumers, respectively.

As the first jurisdiction to impose a soda tax, Berkeley attracted a lot of attention when the tax went into effect, not to mention a massive media campaign during the election. So instead of raising prices immediately after the tax implementation, retailers chose a delay in passing the tax on to consumers to avoid a loss in sales. In addition, Berkeley is a small jurisdiction relative to most of the other taxing jurisdictions and would have likely been more vulnerable to cross-border shopping, which may have deterred retailers from raising prices. But as more Bay Area jurisdictions implemented soda taxes and raised prices, consumers from Berkeley got less benefit from cross-border consumption, likely prompting retailers in Berkeley to raise prices as well. Additionally, consumers were less attentive to price changes over time as they were not exposed to the media coverage anymore. Also note that I only have access to data on Berkeley drug stores, so I am unable to generalize this type of store behavior to traditional grocery stores.

In terms of the impact on volume sales, the soda taxes in Philadelphia and Seattle had a statistically significant impact on volume sales each year after the implementation of the tax. The percentage change in volume sales also varies over time and as expected, the larger the pass-through rate, the larger decline in volume sales. Moreover, the implied price elasticities of demand for Philadelphia and Seattle are consistently larger than -1, indicating that the aggregated demand for taxed beverages is inelastic. If the main goal of the soda tax is to reduce soda consumption by raising the price and improve public health, these results suggest that the tax may not be very effective.

In Oakland, San Francisco, and Boulder, however, the soda taxes had no significant impact on soda sales in any given year. Berkeley's tax had a significant effect on volume sales only in Year 4 and Year 5, a result that echoes the previous results on the impact on prices. The findings in the Bay Area and Boulder may suggest that the soda taxes that were passed through public vote were not effective in their goal of improving public health, at least at the outset.

Table 7.1. Average Impact on the Taxed Beverages After the Tax Implementation Based on DID Regressions for Each Jurisdiction and Each Year

Taxed Jurisdictions	Years	(1) Ln (Price)	(2) Ln (Volume)
Philadelphia	Year 1	0.3036***	-0.1807***
		(0.0197)	(0.0337)
		[0.7269]	**[-0.5953]**
	Year 2	0.3627***	-0.2430***
		(0.0276)	(0.0598)
		[0.8687]	**[-0.6698]**
	Year 3	0.3635***	-0.2553***
		(0.0309)	(0.0601)
		[0.8703]	**[-0.7025]**
Seattle	Year 1	0.1663***	-0.0719***
		(0.0140)	(0.0226)
		[0.6047]	**[-0.4325]**
	Year 2	0.1962***	-0.1020***
		(0.0245)	(0.0340)
		[0.7134]	**[-0.5198]**
Oakland	Year 1	0.0634**	-0.0062
		(0.0294)	(0.0482)
		[0.2521]	[-0.0923]
	Year 2	0.0754**	-0.0224
		(0.0363)	(0.0660)
		[0.2982]	[-0.2931]
	Year 3	0.0794*	0.0167
		(0.0421)	(0.0733)
		[0.3173]	
Berkeley	Year 1	0.0153	-0.0212
		(0.0135)	(0.0385)
			[-1.3856]
	Year 2	0.0364	-0.0533
		(0.0244)	(0.0473)
			[-1.4642]
	Year 3	0.0102	-0.1034
		(0.0303)	(0.0649)
			[-10.1373]
	Year 4	0.0865***	-0.1956***
		(0.0309)	(0.0796)
		[0.4510]	**[-2.2613]**
	Year 5	0.1474***	-0.2022**
		(0.0379)	(0.0934)
		[0.7686]	**[-1.3718]**
San Francisco	Year 1	0.0384**	-0.0045
		(0.0151)	(0.0222)
		[0.2651]	[-0.1172]
	Year 2	0.0833***	-0.0041
		(0.0192)	(0.0301)
		[0.5758]	[-0.0497]
	Year 1	0.0983***	-0.0209
		(0.0211)	(0.0396)
		[0.3526]	[-0.2128]
	Year 2	0.1511**	-0.1168

Boulder		(0.0628)	(0.1035)
		[0.5418]	[-0.7729]
	Year 3	0.1581**	-0.0858
		(0.0690)	(0.1041)
		[0.5670]	[-0.5424]

*Notes: I estimate specification (6.1) here. Columns (1) - (2) contain results for the impact (i.e., percentage change) on the price and volume sales, respectively. Year 1 ~ Year 5 refer to the single year for which I report the treatment effects. For ease of comparison, I only report the corrected DID coefficients (β), i.e., the average treatment effects. Standard errors are in parentheses. The calculated pass-through rates and implied price elasticities are in square brackets. ***, **, and * denote 1%, 5%, and 10% significance levels, respectively.*

7.1.2 Results Based on Triple-Difference Regressions

One concern of the difference-in-differences estimates is that policies and events beyond the soda tax, such as a cost increase experienced only by a treated jurisdiction but not by a control, may have an impact on the soda sales in the treated jurisdiction as well. A triple-difference approach can remove the time-by-place effects that are largely common to the taxed beverages and their substitutes (Berck et al. 2016). In this subsection, I repeat the analysis of the impact of soda taxes on the price and volume sales by estimating the triple-difference specification (Eq. (6.3)) and examine whether the results are sensitive to changing the model.

Specifically, I use pure bottled water as an additional comparison group due to the relatively low cross-price elasticity between soda and water documented in the literature (Zhen et al. 2011; Dharmasena and Capps 2012). For San Francisco, however, I find that both the price and volume sales of pure water are affected significantly by the tax based on an estimation of Eq. (6.1) (i.e., an analysis of the treatment effect) for pure water.[22] I therefore use unsweetened tea instead of pure water as the additional counterfactual in the triple-difference estimations for San Francisco because the unsweetened tea has been proved immune to the indirect effects of the soda tax (see the results in Appendix).

The estimation results are reported in Table 7.2 and suggest that the estimates from the triple-difference regressions are not substantially different from those obtained from the DID regressions, with only a few coefficients being more than 5% different from the corresponding values in Table 7.1. My previously reported results are robust – that is, the soda taxes have statistically significant impact on the price in all taxed jurisdictions, with less than 100% pass-through rates, but in Berkeley, only the most recent two years see the significant impact; retailers

[22] I examined the impact of soda taxes on pure water in all taxed jurisdictions and reported the estimation results in the Appendix. Except in San Francisco, I did not find an effect on pure water sales.

gradually raise prices of the taxed beverages over time in response to the soda tax; in most cases, a soda tax passed by public referendum appears to have a lower pass-through rate than a tax that is passed by a council budget vote; the impact on volume sales is far smaller than that on the price, especially in Oakland, San Francisco, and Boulder, where the soda sales do not respond significantly to the tax.

Table 7.2. Average Impact on the Taxed Beverages After the Tax Implementation Based on Triple-Difference Regressions for Each Jurisdiction and Each Year

Taxed Jurisdictions	Years	(1) Ln (Price)	(2) Ln (Volume)
Philadelphia	Year 1	0.3028***	-0.1921***
		(0.0169)	(0.0266)
		[0.7251]	**[-0.6344]**
	Year 2	0.3657***	-0.2366***
		(0.0240)	(0.0498)
		[0.8757]	**[-0.6470]**
	Year 3	0.3996***	-0.2433***
		(0.0227)	(0.0699)
		[0.9568]	**[-0.6088]**
Seattle	Year 1	0.1529***	-0.0793***
		(0.0107)	(0.0236)
		[0.5559]	**[-0.5186]**
	Year 2	0.1895***	-0.1517***
		(0.0174)	(0.0441)
		[0.6891]	**[-0.8005]**
Oakland	Year 1	0.0512**	-0.0641
		(0.0227)	(0.0565)
		[0.2052]	[-1.2505]
	Year 2	0.0746***	-0.0091
		(0.0201)	(0.0776)
		[0.2989]	[-0.1215]
	Year 3	0.0856***	-0.0302
		(0.0282)	(0.0792)
		[0.3429]	[-0.3524]
Berkeley	Year 1	0.0199	-0.0609
		(0.0236)	(0.0480)
			[-3.0613]
	Year 2	0.0294	-0.0856
		(0.0343)	(0.0659)
			[-2.9062]
	Year 3	-0.0049	-0.1036
		(0.0379)	(0.0782)
	Year 4	0.0652**	-0.1622*
		(0.0380)	(0.0984)
		[0.3406]	**[-2.4859]**
	Year 5	0.1540***	-0.1828**
		(0.0480)	(0.0931)
		[0.8037]	**[-1.1870]**
San Francisco	Year 1	0.0464**	0.0371
		(0.0183)	(0.0311)
		[0.3136]	
	Year 2	0.0921***	0.0071
		(0.0263)	(0.0402)
		[0.6224]	
Boulder	Year 1	0.1041**	-0.0585
		(0.0407)	(0.0434)
		[0.3734]	[-0.5623]
	Year 2	0.1354***	0.0008
		(0.0273)	(0.1105)
		[0.4856]	

Year 3	0.1427^{***}	-0.0749
	(0.0343)	(0.1021)
	[0.5117]	[-0.5248]

*Notes: I estimate specification (6.3) here. Columns (1) - (2) contain results for the impact (i.e., percentage change) on the price and volume sales, respectively. Year 1 ~ Year 5 refer to the single year for which I report the treatment effects. For ease of comparison, I only report the corrected DID coefficients (β), i.e., the average treatment effects. Standard errors are in parentheses. The calculated pass-through rates and implied price elasticities are in square brackets. ***, **, and * denote 1%, 5%, and 10% significance levels, respectively.*

7.2 Spillover Effects: Substitution Toward Untaxed Beverages and Cross-border Shopping

I further explore whether consumers substitute toward untaxed beverages (e.g., diet sodas, 100% natural juices, pure water) following the implementation of a soda tax. The health benefits of a soda tax depend not only on a decline in soda consumption but also on consumers' substitution patterns (Grogger 2017). If consumers switch to healthier drinks such as water when the price of taxable products rises, a soda tax may have a positive impact on consumers' drinking habits. I re-estimate specification (6.1) and consider beverages sold in taxed jurisdictions that are not liable to the tax as treated units.

The regression results in Table 7.3 show that among all taxed jurisdictions, only San Francisco's tax had a statistically significant and positive impact on volume sales of untaxed beverages (20.96%), which occurred in the second year. But as the volume sales of taxable beverages in San Francisco did not drop significantly after the tax implementation, the rise in volume sales of untaxed beverages may be mainly due to its own price change rather than the substitution effect, given the significant decrease in the price of untaxed beverages (-5.84%).

Regarding the impact on the price, the basic economic theory predicts that in a perfectly competitive setting, an increase in the price of a product causes the demand curve for its substitute to shift outward, resulting in an increase in the price of its substitute. However, as I discussed in Chapter 4, pricing in reality is more complicated in oligopolistic retail food markets than is suggested by this simple theory. Based on the results in Table 7.3, I find little evidence that soda taxes affected the price of untaxed beverages, except for a few significant coefficients that occasionally appear in the price column. Overall, there is no convincing evidence that consumers substituted towards untaxed beverages or retailers changed the price of untaxed beverages due to the tax.

Table 7.3. Average Impact on the Untaxed Beverages After the Tax Implementation Based on DID Regressions for Each Jurisdiction and Each Year

Taxed Jurisdictions	Years	(1) Ln (Price)	(2) Ln (Volume)
Philadelphia	Year1	0.0174 (0.0107)	0.0033 (0.0264)
	Year2	-0.0409** (0.0179)	-0.0042 (0.0521)
	Year3	-0.0241 (0.0218)	-0.0054 (0.0736)
Seattle	Year1	0.0226** (0.0108) **[0.0404]**	-0.0058 (0.0182) [-0.2566]
	Year2	0.0219 (0.0204)	0.0265 (0.0378)
Oakland	Year1	0.0252** (0.0097) **[0.0519]**	0.0022 (0.0246)
	Year2	0.0016 (0.0190)	-0.0116 (0.0673) [-7.2500]
	Year3	-0.0046 (0.0253)	0.0070 (0.0793)
Berkeley	Year1	0.0026 (0.0161)	-0.0079 (0.0475) [-3.0385]
	Year2	0.0156 (0.0198)	-0.0777 (0.0584) [-4.9808]
	Year3	0.0110 (0.0269)	-0.0699 (0.0684) [-6.3545]
	Year4	-0.0229 (0.0415)	-0.0324 (0.0807)
	Year5	0.0590 (0.0467)	-0.1331 (0.0968) [-2.2559]
Boulder	Year1	-0.0357* (0.0215)	0.0455 (0.0335)
	Year2	-0.0401 (0.0472)	0.0111 (0.1525)
	Year3	-0.0354 (0.0578)	0.0701 (0.1581)
San Francisco	Year1	-0.0434*** (0.0076)	0.0246 (0.0189)
	Year2	-0.0584*** (0.0153)	0.2096*** (0.0280) [-3.5985]

*Notes: I estimate specification (6.1) here and consider untaxed beverages as treated. Columns (1) - (2) contain results for the impact (i.e., percentage change) on the price and volume sales, respectively. Year 1 ~ Year 5 refer to the single year for which I report the treatment effect. For ease of comparison, I only report the corrected DID coefficients (β), i.e., the average treatment effects. Standard errors are in parentheses. The pass-through rates and implied price elasticities are in square brackets. ***, **, and * denote 1%, 5%, and 10% significance levels, respectively.*

The literature on cigarette taxes suggests that consumers can avoid local taxes by driving to another low-tax state to purchase cigarettes; such tax avoidance may reduce the effectiveness of cigarette taxes as a corrective policy since the reduction in in-state cigarette consumption is offset by an increase in out-of-state consumption (Lovenheim 2008; Harding, Leibtag and Lovenheim 2012). Unlike cigarette taxes which are implemented statewide, soda taxes are local policies, and implemented within single jurisdictions, giving consumers the ability to avoid it by shopping beyond the boundaries of the taxed jurisdiction and not necessitating crossing state boundaries, as with cigarette-tax avoidance. I consider the neighboring cities that share the border with a taxed jurisdiction as treatment areas and re-estimate specification (6.1) to examine how the retailers in these neighboring untaxed cities respond to a soda tax and test for the presence of cross-border shopping.

Results in Table 7.4 show that most of the coefficients are not statistically significant at any level, indicating that there is not broad scale evidence of cross-border shopping. In aggregate, retailers located in the neighboring untaxed cities did not strategically respond to the tax by changing prices.[23] However, such effects may exist for individual stores located near the boundary of a taxed jurisdiction. If this is the case, I would expect retailers in a taxed jurisdiction and close to the city boundary to see a greater decline in volume sales than stores located in the city's central area.

[23] I consider the neighboring cities as a whole.

Table 7.4. Average Impact on the Nearby Untaxed Stores After the Tax Implementation Based on DID Regressions for Each Jurisdiction and Each Year

Taxed Jurisdictions	Years	(1) Ln (Price)	(2) Ln (Volume)
Philadelphia	Year1	-0.0230** (0.0114)	0.0538*** (0.0182) [-2.3440]
	Year2	0.0182 (0.0146)	-0.0213 (0.0360) [-1.1703]
	Year3	-0.0078 (0.0189)	0.0548 (0.0552)
Seattle	Year1	0.0043 (0.0114)	-0.0381 (0.0328)
	Year2	-0.0033 (0.0210)	-0.0578 (0.0370)
Oakland	Year1	0.0116 (0.0149)	0.0102 (0.0262)
	Year2	-0.0582** (0.0311)	0.1387* (0.0629) [-2.3828]
	Year3	-0.0995*** (0.0405)	0.2415*** (0.0779) [-2.4278]
Berkeley	Year1	0.0130 (0.0133)	0.0244 (0.0337)
	Year2	-0.0088 (0.0242)	0.0839* (0.0453)
	Year3	-0.0041 (0.0287)	0.0344 (0.0585)
	Year4	-0.0156 (0.0299)	-0.0104 (0.0623)
	Year5	0.0555 (0.0371)	-0.0498 (0.0689) [-0.8973]
San Francisco	Year1	0.0007 (0.0163)	-0.0225 (0.0230)
	Year2	0.0123 (0.0232)	-0.0536 (0.0372) [-4.3577]
Boulder	Year1	0.0118 (0.0158)	-0.0372 (0.0236) [-3.1525]
	Year2	0.0925*** (0.0308) **[0.3138]**	-0.0848 (0.0648) [-0.9168]
	Year3	0.0755* (0.0413) **[0.2558]**	0.0200 (0.0707)

Notes: I estimate specification (6.1) and consider the nearby untaxed stores as treated units. Columns (1) - (2) contain results for the impact (i.e., percentage change) on the price and volume sales, respectively. Year 1 ~ Year 5 refer to the single year for which I report the treatment effect. For ease of comparison, I only report the corrected DID coefficients (β), i.e., the average treatment effects. Standard errors are in parentheses. The pass-through rates and *implied price elasticities are in square brackets.* ***, **, *and* * *denote 1%, 5%, and 10% significance levels, respectively.*

The ability to evade taxation may change the way the tax burden is distributed between retailers and consumers. Previous research on cigarette taxes showed that cross-border shopping declined with distance to a low-tax border and the price was 0.076 cents larger for every one percent increase in distance from the state border, suggesting geographic variations in consumer price responses to cigarette taxes driven by the opportunities for tax avoidance (Harding, Leibtag, and Lovenheim 2012). Thus, I expect retailers closer to the boundary of the city to pass on a smaller percentage of soda tax to consumers than those farther away from the boundary, as the closer to the boundary of the city, the easier it is for cross-border shopping, and retailers located in the boundary of the city have limited room to raise prices if they do not want to lose consumers. Few studies on soda taxes have looked at how retailers at different distances from the city boundary respond to the tax differently in terms of pricing. Philadelphia is the largest city by geographic area, and I use it as the test site for cross-border shopping to fill this gap.

I classify stores by their geographic locations: if they are in zip codes bordering the city limits, I classify them as near the boundary; if the zip codes are inside the city, I classify them as far from the boundary. I then create dummy variables for both types of stores and use them to interact separately with the DID interaction terms in the specification (6.1) to estimate the different effects on prices and volume sales between stores with different distances from the city boundary. Again, I calculate the pass-through rates and the implied price elasticities of demand based on each pair of percentage changes in price and volume sales, reporting them in square brackets. The results are shown in Table 7.5.

I do not find any noticeable difference in the tax pass-through rate between stores close to the city boundary and those farther from it. This result is unsurprising, given that prior research has shown that prices do not vary substantially among stores in different locations that belong to the same chain because retailers tend to adopt a uniform pricing strategy, which reduces or eliminates price adjustments by single stores (Della Vigna and Gentzkow 2019).

The estimates in the volume column, however, show that there are relatively significant differences in sales reductions between stores with varying distances from the city boundary, with the difference in sales percentage change ranging from 6% to 9%. This finding, together with previous results, may indicate that consumers closer to the city border avoid the soda tax by making cross-border purchases, but the scope of out-of-border consumption is limited to individual untaxed stores that border Philadelphia.

Table 7.5. Heterogeneous Impact Across the Distance to the Boundary After the Tax Implementation Based on DID Regressions for Philadelphia and for Each Year

Taxed Jurisdictions	Geographical Locations	Years	(1) Ln (Price)	(2) Ln (Volume)
Philadelphia	Near the Boundary	Year 1	0.3037***	-0.2095***
			(0.0218)	(0.0415)
			[0.7186]	**[-0.6899]**
		Year 2	0.3429***	-0.2844***
			(0.0433)	(0.0838)
			[0.8116]	**[-0.8293]**
		Year 3	0.3758***	-0.2909***
			(0.0370)	(0.0816)
			[0.8892]	**[-0.7743]**
	Far from the Boundary	Year 1	0.3038***	-0.1496***
			(0.0289)	(0.0477)
			[0.7361]	**[-0.4924]**
		Year 2	0.3494***	-0.1955***
			(0.0380)	(0.0825)
			[0.8467]	**[-0.5595]**
		Year 3	0.3817***	-0.2270***
			(0.0400)	(0.0815)
			[0.9249]	**[-0.5947]**

*Notes: I estimate a modified version of specification (6.1) by interacting the dummy variables for stores at two different distances with the DID terms. I use Philadelphia since it is the largest taxed jurisdiction and the distance from stores to the boundary varies widely. Columns (1) - (2) contain results for the impact (i.e., percentage change) on the price and volume sales, respectively. Year 1 ~ Year 3 refer to the single year for which I report the treatment effect. For ease of comparison, I only report the corrected DID coefficients (β), i.e., the average treatment effects. Standard errors are in parentheses. The pass-through rates and implied price elasticities are in square brackets. ***, **, and * denote 1%, 5%, and 10% significance levels, respectively.*

7.3 Heterogeneous Impacts

The average impacts may mask potential heterogeneity in impacts across store characteristics, product characteristics, and socio-economic characteristics. I estimate the specification (6.2) in this subsection to explore the heterogeneity in impacts of a soda tax across a variety of dimensions.

For each taxed jurisdiction, I estimate a set of regressions examining each dimension at a time. Each set of regressions differs in the dependent variables (i.e., Ln (Price), Ln (Volume)). I analyze potential heterogeneous impacts by store type, beverage category, package size, median household income, and ethnic diversity. As I did when analyzing the average impact, I calculate the pass-through rates of a soda tax in the price column and the implied price elasticities of demand in the volume column, based on the percentage changes in price and volume sales. Both values are shown in square brackets.

7.3.1 Store Type

Table 7.6 reports the regression results for the heterogeneous impact on price and volume sales across store types. The previous results on average impact suggest that retailers pass the soda tax on to consumers not all at once, but gradually over time. The first post-tax year appears to represent a period of price adjustment. I, thus, exclude the data from the first year after the implementation of the tax here and only use data from the subsequent years, when the pass-through rate has stabilized, and I can best determine how much tax retailers eventually pass to consumers.

Store types involved in the data vary by jurisdiction. For example, in Philadelphia, the store types include grocery stores, which are not available in Seattle's data. Berkeley is omitted from this analysis because I can only have access to the sales data from drug stores in Berkeley. I generate a set of dummy variables for store types of each taxed jurisdiction and interact them with the DID term. For ease of comparison, I only report the treatment effects (i.e., the percent changes in price and volume sales) by store type in Table 7.6.[24]

I find evidence that in all jurisdictions, different store types respond to the tax differently in terms of pricing taxable beverages. In line with expectations, the pass-through rates at large retailers (e.g., grocery stores and mass merchandisers) are consistently lower than those at smaller retailers (e.g., drug stores and convenience stores). I formally test whether the difference in coefficients for two store types is statistically significant. The values of chi-squared statistics and test results are displayed in the tables of full regression results in the Appendix. In Philadelphia and Seattle, the price coefficients for large retailers (i.e., grocery stores and mass merchandisers) are significantly different from those for drug stores and convenience stores.

Compared with small retailers, customers of large retail chains tend to purchase other products on a shopping list besides beverages, i.e., exhibit market-basket shopping behavior. Large retailers run the risk that some consumers may switch to another store, e.g., a neighboring untaxed supermarket, to avoid a price increase for products that may be staples for them. Large retailers that raise prices thus risk losing not only sales of sodas but the profit from the whole market basket of that customer.[25] Conversely, this market-basket effect is less important for convenience stores and drug stores, thereby incentivizing them to pass a larger share of the tax forward to consumers.

Additionally, the soda taxes are imposed on distributors and the changes observed in shelf prices are a combination of the portion of the tax passed forward by distributors and the portion further passed on by retailers. Large retailers that have bargaining power with distributors might bear a smaller portion of a tax than convenience and drug stores.

In terms of volume sales, I find that the impact for grocery stores is negative and

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[24] Same as previous sections, I use formula $e^{\beta} - 1$ to transform the DID coefficients to percentage changes. I also report the corresponding pass-through rates and the implied price elasticity of demand in square brackets.

[25] Thomassen et al. (2017) document that supermarkets face relatively large cross-category effects and thus exhibit a maximal level of internalization, as a single seller sets prices for all categories sold at the same store.

statistically significant in Philadelphia and Oakland (-27.09% and -6.98%, respectively), and the decline in soda sales of grocery stores is larger than that of other types of stores within jurisdictions. Moreover, the tax in Seattle has a significantly negative impact on soda sales of mass merchandisers (-13.14%), while the taxes in Philadelphia and San Francisco have no significant impact. For drug stores, only those in Seattle experienced a reduction in sales, and the impact of the soda tax is not significant in the rest of the jurisdictions. Drug stores tend to sell single-bottle sodas, which are usually intended for impulse buying and thus have a low price elasticity of demand. Convenience stores, which have the largest pass-through rates in most cases, experienced moderate sales reductions in Oakland and San Francisco (-5.91% and -14.75%, respectively).

Table 7.6. Heterogeneous Impact of a Soda Tax Across Store Type Based on DID Regressions

Taxed Jurisdictions	Store Type	(1) Ln (Price)	(2) Ln (Volume)
Philadelphia	Grocery	0.3771***	-0.2709***
		(0.0130)	(0.0390)
		[0.7698]	**[-0.7184]**
	Drug	0.2884***	-0.0382
		(0.0155)	(0.0282)
		[0.9181]	[-0.1326]
	Mass Merchandisers	0.1151*	-0.1221
		(0.0570)	(0.1410)
		[0.2839]	[-1.0615]
Seattle	Drug	0.2256***	-0.0901***
		(0.0151)	(0.0334)
		[0.8180]	**[-0.3995]**
	Mass Merchandisers	0.1121***	-0.1314***
		(0.0314)	(0.0529)
		[0.3318]	**[-1.1721]**
	Convenience	0.2014***	-0.0759
		(0.0312)	(0.0763)
		[1.0454]	[-0.3767]
Oakland	Grocery	0.0761***	-0.0698***
		(0.0202)	(0.0250)
		[0.2571]	**[-0.9181]**
	Drug	0.0863***	0.0836
		(0.0227)	(0.0599)
		[0.4944]	
	Convenience	0.0900***	-0.0591***
		(0.0310)	(0.0234)
		[0.8327]	**[-0.6573]**
Boulder	Drug	0.1594***	0.0974
		(0.0211)	(0.0868)
		[0.6234]	
	Convenience	0.0905***	0.0593
		(0.0199)	(0.0917)
		[0.4186]	
San Francisco	Drug	0.0790***	0.0154
		(0.0152)	(0.0239)
		[0.6177]	
	Convenience	0.0927***	-0.1475***
		(0.0212)	(0.0052)
		[0.7456]	**[-1.5904]**
	Grocery	0.1195***	0.0380
		(0.0333)	(0.0542)
		[0.5669]	
	Mass Merchandisers	0.0687**	-0.2279
		(0.0298)	(0.2033)
		[0.3697]	[-3.3173]

*Notes: I estimate the heterogenous effects of the soda tax across the store type based on specification (6.2). Columns (1) - (2) contain results for the impact on price and volume sales, respectively. For ease of comparison, I only report the percent changes in price and volume sales due to a soda tax. Standard errors are in parentheses. The calculated pass-through rates and implied price elasticities are in square brackets. ***, **, and * denote 1%, 5%, and 10% significance levels, respectively.*

The estimates reported in Table 7.6 measure the impact on the average price across stores that belong to the same store type. But effects may also vary across different retail chains. I selected three retail chains from Philadelphia and estimated the heterogeneous impact of the soda tax along beverage category for each of them. In Table 7.7, retailer A is a mass merchandiser, and the other two retailers are grocery stores. The estimation results suggest that retailers may respond differently to a soda tax, even within the same store type.

Retailer A only raised the prices of carbonated drinks and sweetened water with relatively low pass-through rates, while the two grocery retailers passed forward the tax to all beverage categories. Moreover, sports drinks are the category with the largest pass-through rate at Retailer B but exhibit the lowest pass-through at Retailer C. These results suggest that an analysis at the aggregate level (e.g., the overall average impact) is not sufficient to reveal the full picture of the impact of the soda tax. The heterogeneity in the impact of a soda tax across store types can stem from heterogeneity among retailers or at a more aggregate level, from heterogeneity among beverage categories. I explore the latter in the next subsection.

In addition, two grocery chains (B and C) raised the price of taxed categories more than retailer A and experienced a sharp drop in the volume sales. The sales of taxed products for retailer A, however, were not significantly affected by the tax. This may imply that consumers did not need to avoid the tax by cross-border shopping. They could have largely avoided the tax by switching to chains within Philadelphia that did not raise the price significantly. Shopping outside of Philadelphia may have also imposed additional costs for transportation and search that were avoided to some extent if price-elastic shoppers were able to largely avoid the tax by shopping at local retailers that did not raise price in response to the tax.

Table 7.7. Heterogeneous Responses by Retail Chains to Philadelphia's Soda Tax

Chain	Beverage Category	(1) Ln (Price)	(2) Ln (Volume)
Retailer A	Carbonated Drinks	0.3054^{***}	-0.1527
		(0.0599)	(0.1448)
		[0.6158]	[-0.4999]
	Sports/Energy	0.0554	0.0121
		(0.0614)	(0.0784)
		[0.1974]	
	Sweetened Water	0.0203^{**}	-0.0457
		(0.0090)	(0.0570)
		[0.0617]	[-2.2507]
	Non-100% Juice	0.0387	0.0349
		(0.0347)	(0.1269)
		[0.1099]	
	Sweetened Tea/Coffee	0.0499	-0.1394
		(0.0511)	(0.2013)
		[0.1572]	[-2.7930]
Retailer B	Carbonated Drinks	0.3850^{***}	-0.3901^{***}
		(0.0120)	(0.0340)
		[0.6817]	**[-1.0130]**
	Sports/Energy	0.3277^{***}	-0.3617^{***}
		(0.0201)	(0.0431)
		[0.8827]	**[-1.1038]**
	Sweetened Water	0.2329^{***}	-0.4759^{***}
		(0.0093)	(0.0415)
		[0.7026]	**[-2.0439]**
	Non-100% Juice	0.3128^{***}	-0.2679^{***}
		(0.0091)	(0.0337)
		[0.8764]	**[-0.8566]**
	Sweetened Tea/Coffee	0.3028^{***}	-0.3628^{***}
		(0.0209)	(0.0374)
		[0.7381]	**[-1.1981]**
Retailer C	Carbonated Drinks	0.4165^{***}	-0.3678^{***}
		(0.0067)	(0.0196)
		[0.7082]	**[-0.8831]**
	Sports/Energy	0.1440^{***}	-0.3736^{***}
		(0.0169)	(0.0352)
		[0.4111]	**[-2.5940]**
	Sweetened Water	0.3528^{***}	-0.5278^{***}
		(0.0073)	(0.0312)
		[0.9557]	**[-1.4959]**
	Non-100% Juice	0.2656^{***}	-0.2626^{***}
		(0.0075)	(0.0116)
		[0.7432]	**[-0.9889]**
	Sweetened Tea/Coffee	0.1911^{***}	-0.3262^{***}
		(0.0097)	(0.0261)
		[0.4500]	**[-1.7071]**

Notes: I estimate the heterogenous effects of Philadelphia's soda tax across the beverage category based on specification (6.2) for three retailers separately. Columns (1) - (2) contain results for the impact on price and volume sales, respectively. For ease of comparison, I only report the percent changes in price and volume sales due to a soda tax. Standard errors are in parentheses. The pass-through rates and implied price elasticities are in square brackets. ***, **, *and* * *denote 1%, 5%, and 10% significance levels, respectively.*

In Table 7.8, I further explore cross-regional differences in response to soda taxes within the same retail chain. Della Vigna and Gentzkow (2019) showed that chain retailers tend to adopt a "uniform store pricing" strategy within regions or zones but vary prices across large regions. I consider Philadelphia, Seattle, and San Francisco as different "regions" and use the data from a chain (Chain A) that operates in each of these regions.

Again, I estimate the heterogeneous effects across beverage categories for each jurisdiction. The results presented in Table 7.8 show that retailers within the same chain but located in different regions raised the prices of carbonated drinks by similar amounts (0.82 - 0.92 cents/ounce), although they responded to different tax policies. For other beverage categories, the reaction of the same chain also varies little across different jurisdictions. Thus, chain retailers acted similarly in widely geographically diverse cities which implemented different tax policies.

Table 7.8. Heterogeneous Responses by the Same Chain Across Geographical Locations

Taxed Jurisdictions	Beverage Category	(1) Ln (Price)	(2) Ln (Volume)
Philadelphia	Carbonated Drinks	0.3054***	-0.1527
		(0.0599)	(0.1448)
		[0.6158]	[-0.4999]
	Sports/Energy	0.0554	0.0121
		(0.0614)	(0.0784)
	Sweetened Water	0.0203**	-0.0457
		(0.0090)	(0.0570)
		[0.0617]	[-2.2507]
	Non-100% Juice	0.0387	0.0349
		(0.0347)	(0.1269)
	Sweetened Tea/Coffee	0.0499	-0.1394
		(0.0511)	(0.2013)
			[-2.7930]
Seattle	Carbonated Drinks	0.2447***	-0.1468*
		(0.0306)	(0.0727)
		[0.5297]	**[-0.5998]**
	Sports/Energy	0.0528*	-0.0656*
		(0.0290)	(0.0315)
		[0.1972]	**[-1.2427]**
	Sweetened Water	-0.0131	0.3150
		(0.1014)	(0.1911)
	Non-100% Juice	0.1158***	-0.0890
		(0.0288)	(0.0994)
		[0.4092]	[-0.7686]
	Sweetened Tea/Coffee	0.0064	0.0070
		(0.0346)	(0.0664)
San Francisco	Carbonated Drinks	0.1904***	-0.0627
		(0.0344)	(0.1466)
		[0.8182]	[-0.3293]
	Sports/Energy	-0.1041*	-0.0323
		(0.0599)	(0.1896)
	Sweetened Water	0.0257	0.5049***
		(0.0902)	(0.1330)
	Non-100% Juice	0.1185	0.0003
		(0.1493)	(0.1233)
	Sweetened Tea/Coffee	0.1433***	-0.2197
		(0.0334)	(0.1912)
		[1.3875]	[-1.5331]

*Notes: I estimate the heterogeneous effects of the soda tax across the beverage category based on specification (6.2) for retailers that belong to the same chain but are in different jurisdictions. Columns (1) - (2) contain results for the impact on price and volume sales, respectively. For ease of comparison, I only report the percent changes in price and volume sales due to a soda tax. Standard errors are in parentheses. The pass-through rates and implied price elasticities are in square brackets. ***, **, and * denote 1%, 5%, and 10% significance levels, respectively.*

7.3.2 Beverage Category

Figure 7.1 displays the results for the heterogeneous effects of soda taxes across beverage categories. The current literature on this subject tends to aggregate the decisions of different store types within one beverage category. As shown earlier, the response to a soda tax varies across retailers, and consumer preferences for different categories of beverages may also vary across store types and retail chains. Therefore, it is necessary to conduct an analysis of heterogeneous impacts across beverage categories within each store type. I focus on five categories: carbonated drinks, sports/energy drinks, sweetened water, non-100% juice, and sweetened tea/coffee.

For each category, I present a graph of percentage change in price at the top and a graph of percentage change in volume sales at the bottom. The vertical axis shows the percentage changes in the price or volume sales after soda taxes were implemented, which are calculated by multiplying the estimated coefficients from DID regressions by 100. The horizontal axis represents the four store types. Within each store type, I report the estimated treatment effects for jurisdictions that have the data on that store type. The height of a bar measures the impact of each jurisdiction's soda tax on the price or volume sales of each product category sold in each type of store.

I display at the top of the bar the calculated pass-through rate for each positive price coefficient and, if any, the implied price elasticity for each quantity coefficient.[26] If a coefficient is statistically significant, its corresponding pass-through or elasticity will be bolded. For example, Philadelphia's soda tax (in green) raised the price of carbonated drinks in drug stores by about 30% with an estimated pass-through of 0.79. In this way, we could compare the results across store types and across jurisdictions more easily. The full regression results are reported in the tables contained in the Appendix.

[26] The implied price elasticity is calculated only when prices increase and volume sales decrease.

The estimation results support heterogeneous effects of a soda tax across beverage categories. The pattern of heterogeneity in the impact varies by store type and by jurisdiction. Of the 78 coefficients in the price column, 60 are statistically significant. Mass merchandisers have the fewest significant price increases among the store types. Combined with the results in Table 7.6, Figure 7.1 confirms that mass merchandisers increase prices on fewer products with a lower pass-through rate than other store types.

Additionally, the pass-through rates of sports/energy drinks and sweetened tea/coffee are larger than those of other categories in most cases. It is also worth reiterating that a soda tax is first passed on to retailers by distributors before it is passed on to consumers. The heterogeneity in pass-through rates among different categories might be partially attributed to different pass through rates between distributors and retailers.

Although the pass-through rate can vary widely across categories, the percentage change in price fluctuates little within the same store type. For example, the pass-through rate within grocery stores ranges from 40.73% to 97.43% but the percentage increase in price is between 13.8% and 41%. Retailers that typically sell tens of thousands of products likely do not attempt to calculate the optimal price for each individual item or category. Instead, they may use broad heuristics to set prices, e.g., raising the prices of some categories by a fixed percentage, even though this results in quite different pass-through rates.[27]

The impact on volume sales also varies among different categories, with the pattern of difference in the percentage change in sales differing by store type and by jurisdiction. Compared with the price responses, however, I did not see significant volume decreases in many cases. Only

[27] McMillan (2007) suggests that retailers typically set the same price across a wide range of products from the same manufacturer due to managerial menu costs. When a retail store faces even a relatively small cost to determine optimal non-uniform prices, it may be optimal to charge the same price for many products.

36 coefficients out of a total of 78 that measured changes in volume sales were statistically significant. In the analysis of the average impact of the soda tax in Oakland, I found that the coefficients for volume sales were insignificant in all three years. In contrast, the estimates reported in Figure 7.1 show that in grocery stores in Oakland, the sales of taxed beverages dropped substantially, especially for sweetened bottled water (-37.9%).

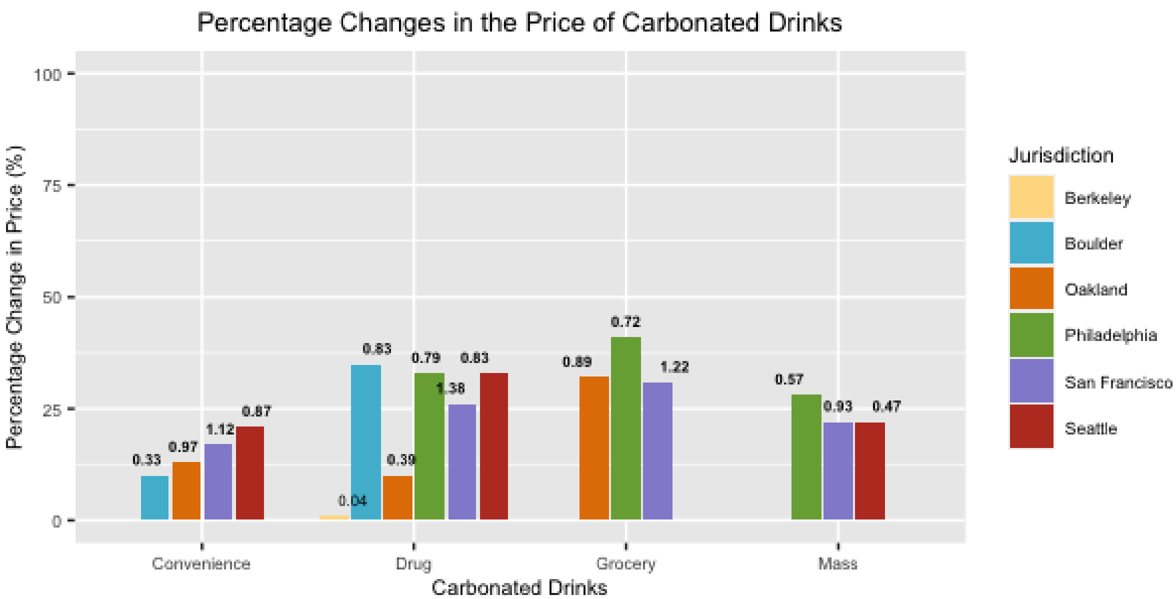
How much retailers raise the prices for different categories of beverages depends in part on consumers' responsiveness to price increases for different categories, i.e., the price elasticity of demand. For beverages with relatively high price elasticity, consumers are more sensitive to a price increase. With this in mind, retailers may not raise the prices by large magnitudes. I conduct a simple OLS regression to test for the correlation between the implied price elasticity of demand and the pass-through rate.

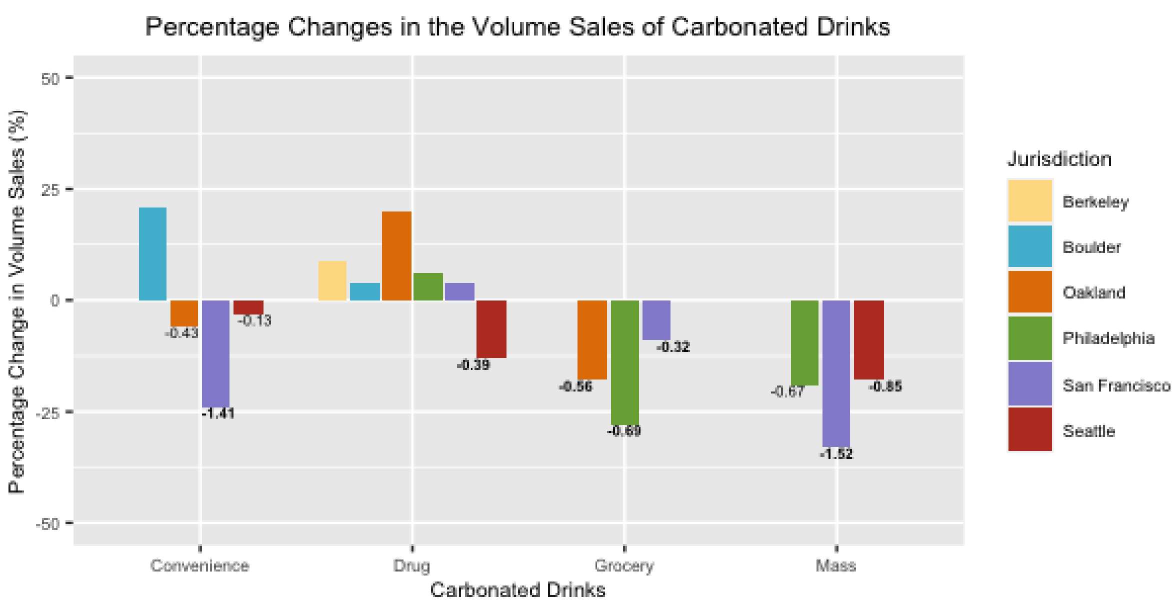
The data come from the pass-through rates and implied price elasticities (in absolute values) presented in the square brackets in the tables in the Appendix. I exclude rows with insignificant coefficients. The estimated model is given by the equation below:

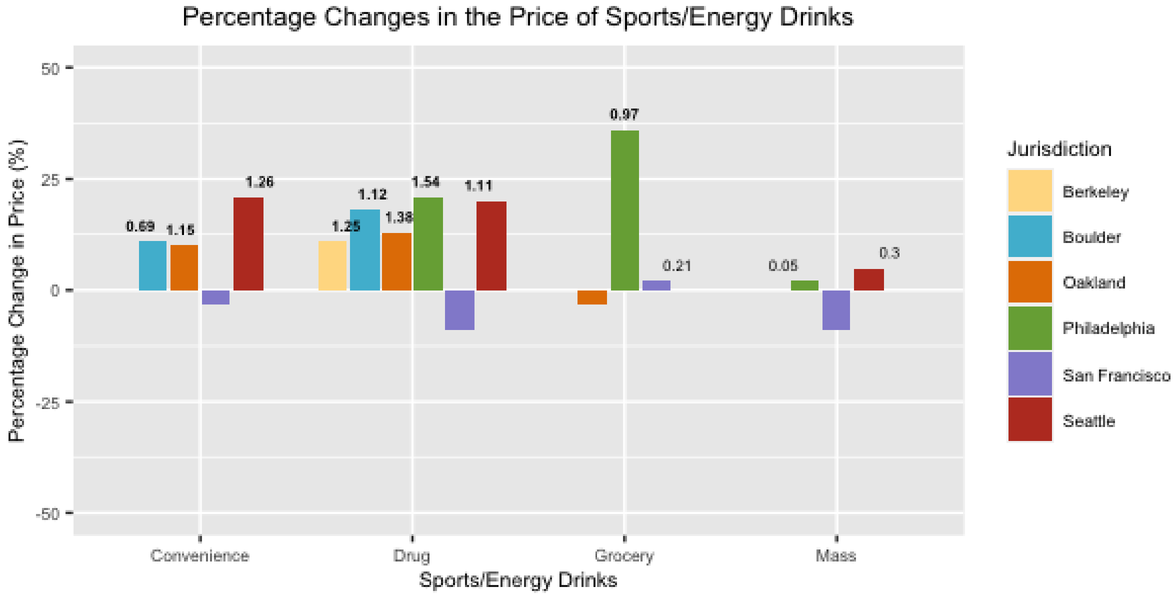
$$\widehat{pass\ through} = \underset{(0.10)}{1.12} - \underset{(0.06)}{0.15} * |price\ elasticity|$$

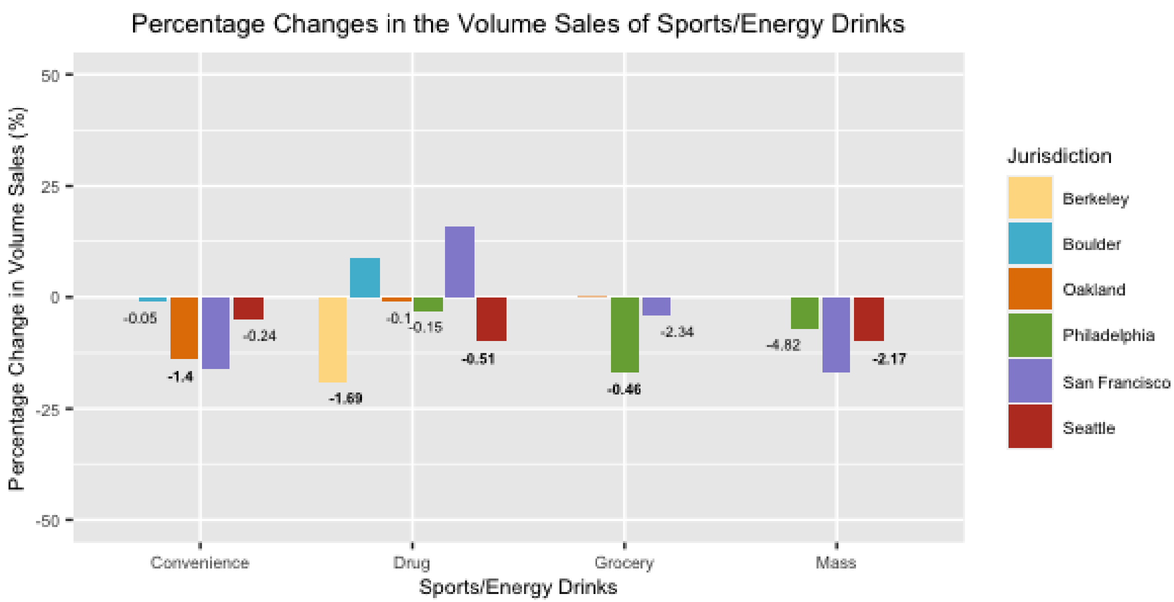
The statistically significant coefficient on the price elasticity confirms that the higher the price elasticity, the lower the pass-through rate.

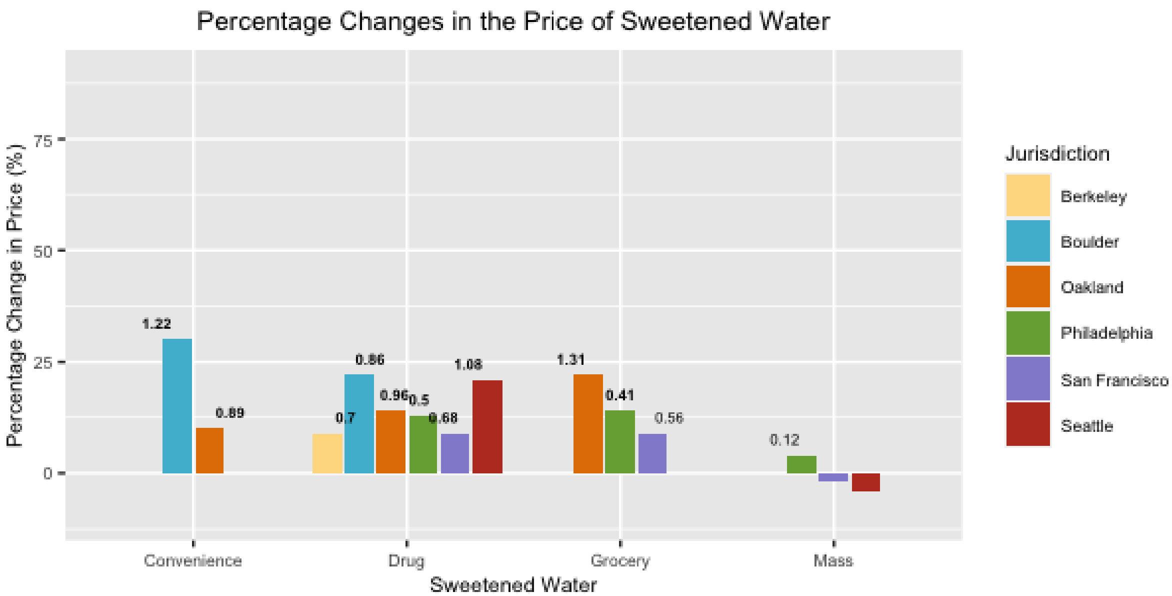
Figure 7.1 Heterogeneous Impact of Soda Taxes Across Product Category in Each Store Type

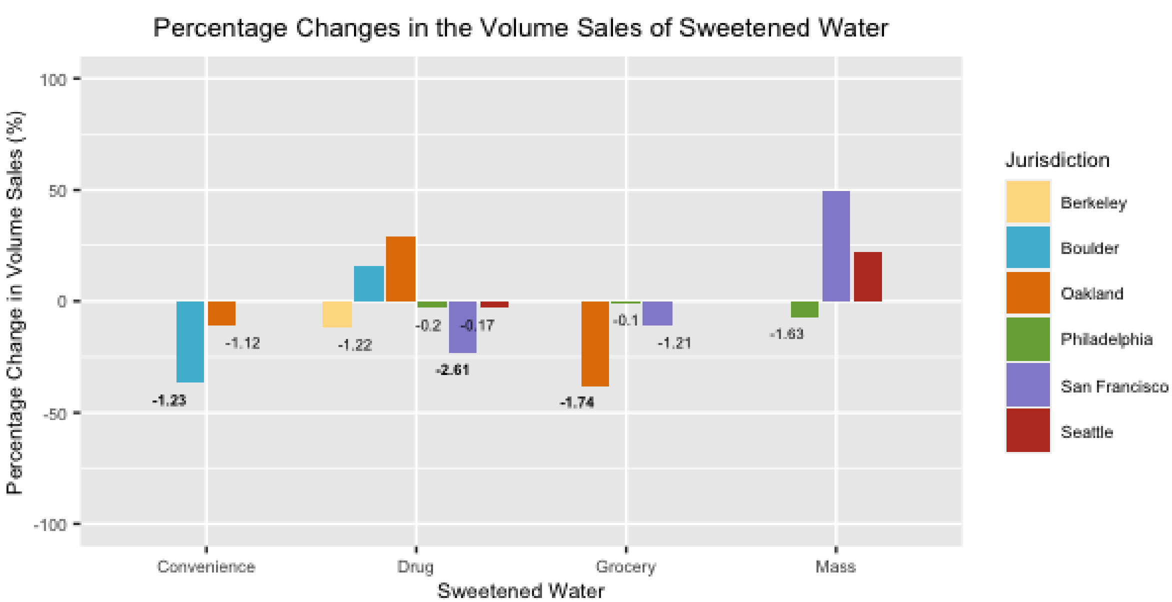


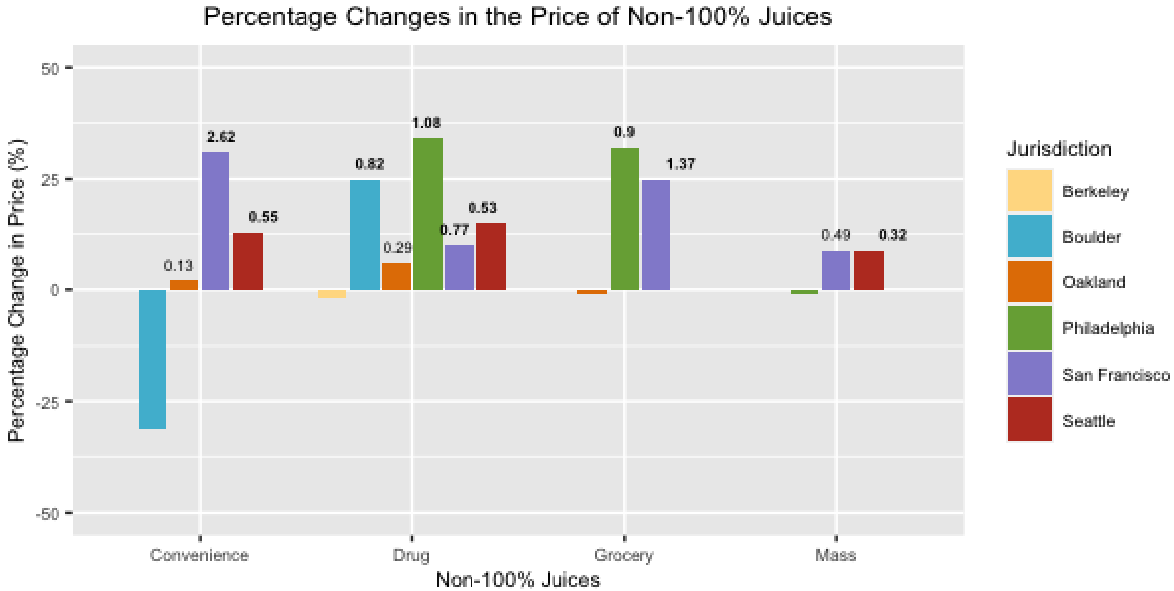



Percentage Changes in the Price of Sports/Energy Drinks
Percentage Change in Price (%)
50
25
0
-25
-50
0.69
1.15
1.26
1.25
1.12
1.38
1.54
1.11
0.97
0.21
0.05
0.3
Convenience
Drug
Grocery
Mass
Sports/Energy Drinks
Jurisdiction
Berkeley
Boulder
Oakland
Philadelphia
San Francisco
Seattle

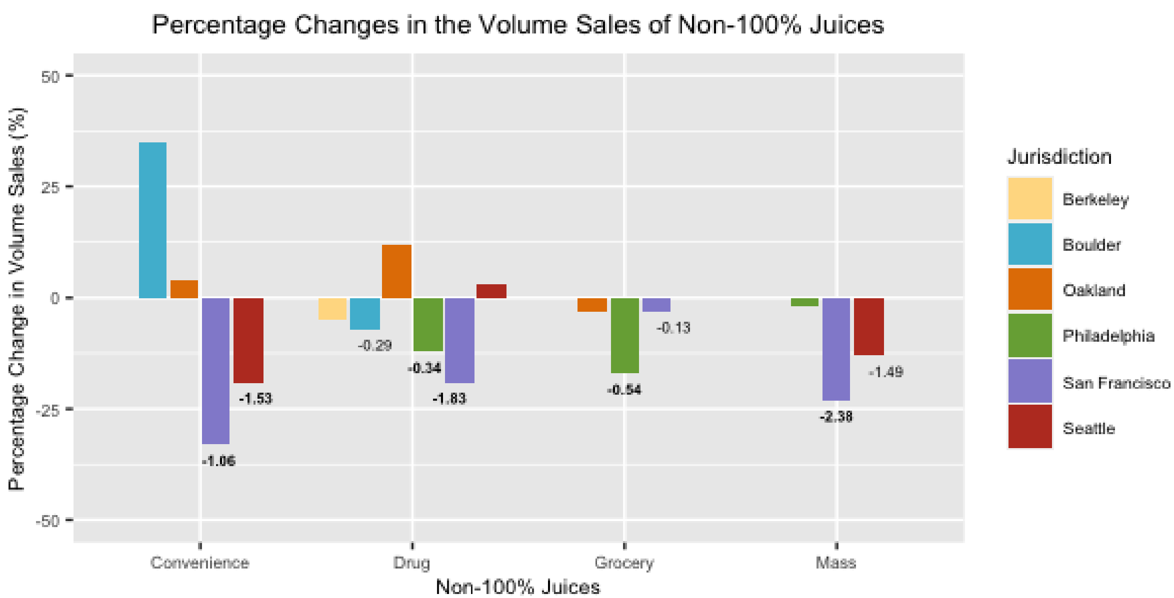

Percentage Changes in the Volume Sales of Sports/Energy Drinks
Percentage Change in Volume Sales (%)
50
25
0
-25
-50
-0.05
-1.4
-0.24
-1.69
-0.1
-0.15
-0.51
-0.46
-2.34
-4.82
-2.17
Convenience
Drug
Grocery
Mass
Sports/Energy Drinks
Jurisdiction
Berkeley
Boulder
Oakland
Philadelphia
San Francisco
Seattle


Percentage Changes in the Price of Sweetened Water
Percentage Change in Price (%)
75
50
25
0
1.22
0.89
0.86
1.08
0.7
0.96
0.5
0.68
1.31
0.41
0.56
0.12
Convenience
Drug
Grocery
Mass
Sweetened Water
Jurisdiction
Berkeley
Boulder
Oakland
Philadelphia
San Francisco
Seattle

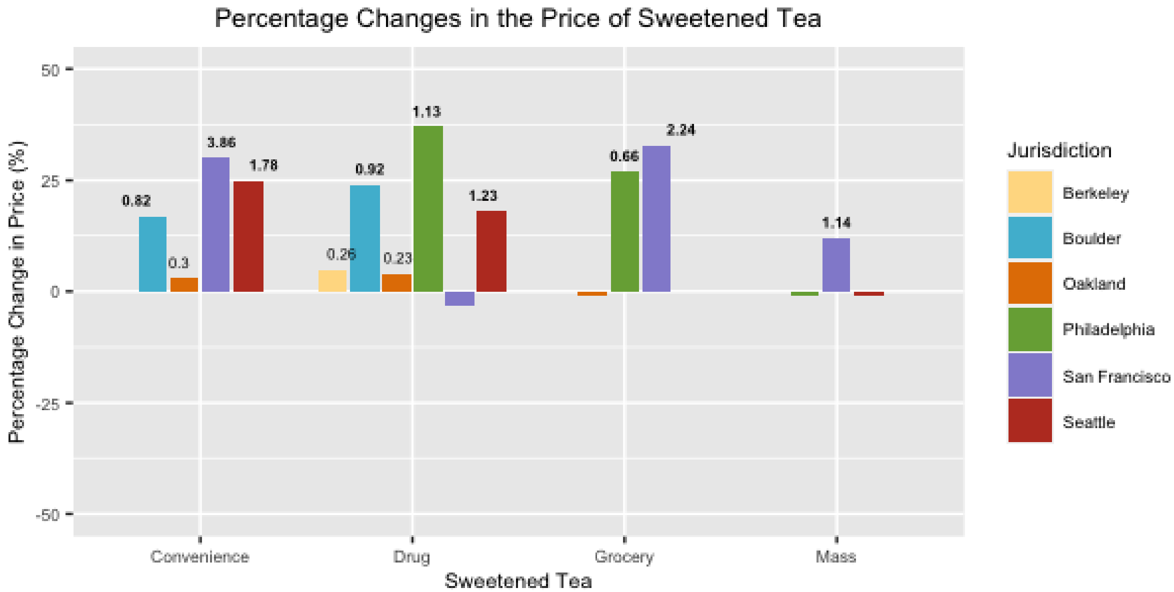

Percentage Changes in the Volume Sales of Sweetened Water
Percentage Change in Volume Sales (%)
100
50
0
-50
-100
-1.12
-1.23
-1.22
-0.2
-0.17
-2.61
-0.1
-1.21
-1.74
-1.63
Convenience
Drug
Grocery
Mass
Sweetened Water
Jurisdiction
Berkeley
Boulder
Oakland
Philadelphia
San Francisco
Seattle

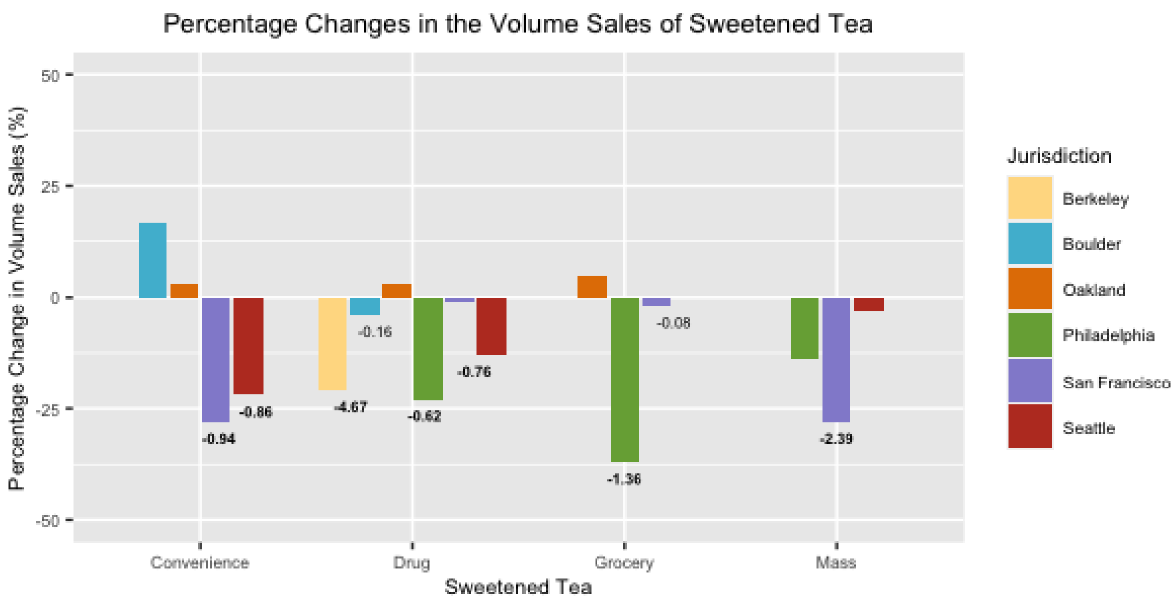


Percentage Changes in the Price of Non-100% Juices
Percentage Change in Price (%)
50
25
0
-25
-50
2.62
0.13
0.55
0.82
0.29
1.08
0.77
0.53
0.9
1.37
0.49
0.32
Convenience
Drug
Grocery
Mass
Non-100% Juices
Jurisdiction
Berkeley
Boulder
Oakland
Philadelphia
San Francisco
Seattle



Percentage Changes in the Volume Sales of Non-100% Juices
Percentage Change in Volume Sales (%)
50
25
0
-25
-50
-1.53
-1.06
-0.29
-0.34
-1.83
-0.13
-0.54
-1.49
-2.38
Convenience
Drug
Grocery
Mass
Non-100% Juices
Jurisdiction
Berkeley
Boulder
Oakland
Philadelphia
San Francisco
Seattle





Notes: the vertical axis represents the percentage changes in price or volume sales, and they are calculated by multiplying the DID coefficients by 100; the value on the top of each bar in the graph for the price denotes the pass-through rate which is calculated only for the positive price change; if a price coefficient is statistically significant, the pass-through is bolded; in the graph for the volume sales, the value under each bar represents the implied price elasticity which is calculated only for the case when the price increases and the volume sales decreases; when both the price coefficient and volume coefficient are statistically significant, the implied elasticity is bolded.

7.3.3 Package Size

In this part, I investigate the heterogeneous effects of soda taxes on price and volume sales across different package sizes. To avoid mixing the effects on different categories and store types, I focus on carbonated drinks sold by grocery stores and mass merchandisers. Because I did not have access to the data of such retailers for Boulder and Berkeley, I analyze drug stores instead for these two jurisdictions. I generate dummy variables for the most popular package sizes that make up a total market share of over 50% and estimate specification (6.2). As I did earlier, I drop the first year after the tax implementation in this analysis.

The results are reported in Table 7.9. I obtain relatively consistent results across jurisdictions for the same store type. The soda taxes enacted by Philadelphia, Seattle, Oakland, and Boulder had statistically significant impacts on the prices of all package sizes. Column (1) shows that for large retailers, the pass-through rates of large sizes (e.g., 2 liter bottles or more) were higher than those of small sizes (i.e., 20 ounce bottles or cans); but in Boulder and Berkeley, the results are reversed, with the small size having a higher pass-through rate.

In terms of the impact on volume sales, the taxes in Berkeley and Boulder had no statistically significant impact on the volume sales of any size. In the rest of the jurisdictions, however, I saw a significant sales decline for large sizes (e.g., a 36.99% sales decline for 2-liter bottles in Philadelphia). In Philadelphia, the tax can affect the volume sales of taxed drinks both in large packages and in small ones. In addition to seeing a decline in sales of large packages, I also saw a 23.2% increase in the volume sales of single bottles of 20 ounces. This implies that consumers may choose to scale back in response to the higher prices of large packages. Although the prices of both large and small sizes rise, it is less so for smaller ones, which may induce consumers to buy smaller package sizes. Consumers may also reduce their planned purchases but increase their impulse purchases as a result. Considering that soda taxes aim to reduce the consumption of sugary sodas, the mostly insignificant impact on volume sales of these taxes may suggest that they are less than ideal at achieving their goals.

Table 7.9. Heterogeneous Impact of a Soda Tax Across the Package Size of Carbonated Drinks Sold by Large Retailers

Taxed Jurisdictions	Package Size	(1) Ln (Price)	(2) Ln (Volume)
Philadelphia	1 bottle of 20 ounces	0.1013***	0.2320**
		(0.0211)	(0.0960)
		[0.6029]	
	12 cans of 12 ounces	0.4635***	-0.4935***
		(0.0763)	(0.1440)
		[0.7741]	**[-1.0646]**
	1 bottle of 2 Liters	0.5660***	-0.3699***
		(0.0522)	(0.0780)
		[0.6857]	**[-0.6535]**
Seattle	1 bottle of 20 ounces	0.0252***	-0.1507
		(0.0068)	(0.1744)
		[0.1296]	[-5.9706]
	12 cans of 12 ounces	0.3125***	-0.2042
		(0.0216)	(0.1723)
		[0.4748]	[-0.6533]
	1 bottle of 2 Liters	0.3997***	-0.2351**
		(0.0131)	(0.1351)
		[0.4638]	**[-0.5880]**
Oakland	1 bottle of 20 ounces	0.0790***	-0.0406
		(0.0294)	(0.2120)
		[0.7014]	[-0.5144]
	12 cans of 12 ounces	0.3814***	-0.3453***
		(0.0637)	(0.1562)
		[1.1236]	**[-0.9054]**
	1 bottle of 2 Liters	0.6636***	-0.4428***
		(0.0548)	(0.1215)
		[1.2241]	**[-0.6672]**
Berkeley	1 bottle of 20 ounces	0.1912***	0.1392
		(0.0236)	(0.1417)
		[1.3806]	
	12 cans of 12 ounces	0.0322	0.0782
		(0.0238)	(0.1101)
	1 bottle of 2 Liters	0.0353**	0.2554
		(0.0176)	(0.1462)
		[0.0823]	
Boulder	1 bottle of 20 ounces	0.1895***	-0.0941
		(0.0387)	(0.1557)
		[0.8088]	[-0.4965]
	12 cans of 12 ounces	0.2418***	0.1747
		(0.0626)	(0.1059)
		[0.3889]	
	1 bottle of 2 Liters	0.4627***	0.1535
		(0.0858)	(0.1454)
		[0.5848]	
San Francisco	1 bottle of 20 ounces	-0.0225	-0.1337
		(0.0214)	(0.1740)
	12 cans of 12 ounces	0.2994***	-0.1438
		(0.0487)	(0.1315)
		[1.0396]	[-0.4802]

1 bottle of 2 Liters	0.4693***	-0.1991*
	(0.0647)	(0.1230)
	[1.0697]	**[-0.4242]**

*Notes: I estimate the heterogeneous effects of the soda tax across the package size based on specification (6.2) for carbonated drinks sold by large retailers (i.e., mass merchandisers and grocery stores). Columns (1) - (2) contain results for the impact on price and volume sales, respectively. For ease of comparison, I only report the percent changes in price and volume sales due to a soda tax. Standard errors are in parentheses. The pass-through rates and implied price elasticities are in square brackets. ***, **, and * denote 1%, 5%, and 10% significance levels, respectively.*

7.3.4 Demographics: Median Household Income and Ethnicity

One of the hotly debated issues surrounding "sin taxes" is whether they are regressive – that is, whether the tax burden often falls disproportionately on low-income consumers.[28] The main purpose of most soda taxes is to reduce the prevalence of obesity and improve health outcomes, particularly among low-income households who tend to be more adversely affected by the negative health outcomes of soda consumption and suffer from the highest rates of obesity-related chronic illnesses. Low-income households tend to have stronger preferences for soda drinks which appear to be cheaper than relatively healthy beverages, and they might also lack nutritional knowledge (Allcott, Lockwood, and Taubinsky 2019).

Moreover, low-income households typically spend a larger share of their income on groceries – specifically on products like sugar-sweetened beverages. For example, households with above $100K contribute about 25% of the total expenditures on sugary drinks, while those with $20K-$50K contribute about 30% (Ross and Lozano-Rojas 2018). Prior studies have shown that retailers raised the price of sodas by a larger pass-through rate in low-income neighborhoods than in high-income neighborhoods (Cawley et al. 2020; Seiler, Tuchman, and Yao 2021). Moreover, high-SSB consumers (often low-income) were less responsive to soda price changes than low-SSB consumers (Valizadeh and Ng 2021). Thus, low-income households may tend to bear a larger burden from soda taxes relative to higher-income households, making soda taxes regressive in their incidence.

Previous literature on cigarette taxes has documented that the incidence of cigarette taxes varies by household income (Harding, Leibtag, and Lovenheim 2012). High-income consumers paid more for cigarettes than low-income consumers did. But in the literature on soda taxes, little

[28] A "sin tax" is a corrective tax levied on goods that are thought to be over-consumed, such as cigarettes, alcohol, and sugary drinks (Allcott, Lockwood, and Taubinsky 2018).

attention has been paid to the distributional effects and whether such taxes can reduce soda consumption by a larger margin among low-income households. Only studies on Philadelphia's tax reported that prices increased more in low-income areas (Cawley et al. 2020; Seiler, Tuchman, and Yao 2021). Soda taxes in jurisdictions that aim to reduce soda consumption have not been widely studied in terms of the heterogeneity in demographics. This research examines whether prices and volume sales of taxed beverages respond differently to the soda tax across the median household income for four large jurisdictions: Philadelphia, San Francisco, Seattle, and Oakland.

As I have discussed, large retailers may utilize "zone pricing", meaning that there may be little variation in prices of large retail chains located in areas within a jurisdiction with different socio-economic characteristics. Thus, I only use data for drug stores and convenience stores in this analysis and estimate the heterogeneous effects separately for these two types of stores. I classify stores based on the median household income of zip codes in which they are located as low-income, middle-income, and high-income levels. Table 7.10 describes the median household income range corresponding to each income level.

For each jurisdiction, I rank the median household income of stores' zip codes from lowest to highest, defining the bottom third as low income, the middle third as middle income, and the top third as high income. Note that when I do not have access to data on convenience stores (e.g., in Philadelphia) or the number of convenience stores is too few to cover all income levels, I only focus on an analysis of drug stores. Given that retailers were adjusting prices in the first year after the tax was imposed, I drop the data on the first year from the treatment period. I create dummy variables for three income levels and interact them with the DID term in the specification (6.1). The coefficients on these interaction terms measure the heterogeneous effects on price and volume sales across the median household income. The estimation results are reported in Table 7.11.

I find that in Philadelphia and Seattle soda taxes had a significant impact on the prices of retailers at low-, middle- and high-income levels. Retailers from high-income zip codes had higher pass-through rates than those at the low-income level. This result appears to reduce the regressive nature of soda taxes. But also note that these taxes did not have significant impacts on the volume sales of sodas in low-income zip codes. This could be due to the fewer alternatives to sodas available to low-income households and, as discussed above, that they tend to have stronger preferences for sugar-sweetened beverages. Regardless of the reason, low-income households' inelastic response to the price changes mean that they bear a disproportionate burden of the soda tax, and the tax policies might not have reduced soda consumption for this population.

The results in the price column are similar for San Francisco and Oakland. But they are different from those for Philadelphia and Seattle. Soda taxes had significantly positive impacts on prices for retailers from low- and middle-income zip codes, while retailers from high-income zip codes did not respond to soda taxes by raising the price. Additionally, I did not find any significant impact of either tax on volume sales of taxed beverages at any income level. In other words, only retailers from low- and middle-income areas raised prices and they did not see sales reductions of taxed beverages. Thus, the tax burdens in San Francisco and Oakland are also disproportionately borne by consumers from low- and middle-income areas.

It is also worth noting that the tax policies in Seattle, Oakland, and San Francisco aim to curb obesity by reducing soda consumption. However, soda sales in low-income zip codes did not decline significantly as intended by the policies in these three jurisdictions. If tax revenues are used for health programs such as nutrition education, or providing access to healthier drinks, the policy ineffectiveness in term of reducing soda consumption might be remedied to some extent.

Table 7.10. Definitions for Median Household Income Levels and Ethnic Diversity Levels

Taxed Jurisdictions	Income Levels	Income Range ($/household)	Ethnic Diversity Levels	Percentage of Non-whites Range (%)	Percentage of African-Americans Range (%)
Philadelphia	Low-income	14,185 – 33,966	Low-diversity	9.2 – 33.2	3.4 – 18.7
	Middle-income	34,795 – 46,462	Middle-diversity	33.6 – 76.8	20.1 – 57.3
	High-income	47,138 – 93,720	High-diversity	84.1 – 98.3	62.5 – 94.9
Seattle	Low-income	26,637 – 53,044	Low-diversity	12.9 – 17.5	1.6 – 2.4
	Middle-income	53,570 – 67,566	Middle-diversity	18.3 – 33.2	2.6 – 7.7
	High-income	75,763 – 96,054	High-diversity	33.3 – 69.1	11.8 – 25.9
San Francisco	Low-income	22,517 – 60,722	Low-diversity	15.7 – 40.8	0.8 – 2.2
	Middle-income	69,223 – 83,407	Middle-diversity	41.5 – 57.0	2.2 – 6.7
	High-income	89,722 – 163,949	High-diversity	58.6 – 87.9	8.6 – 33.7
Oakland	Low-income	26,054 – 38,305	Low-diversity	23.3 – 53.4	4.9 – 18.4
	Middle-income	38,363 – 56,944	Middle-diversity	54.4 – 74.6	18.6 – 29.6
	High-income	71,510 – 116,604	High-diversity	78.1 – 80.1	33.2 – 52.7

Notes: I collected information on the median household income, the percentage of non-whites, and the percentage of African-Americans from the U.S. Census Bureau website for each zip code in Philadelphia, Seattle, San Francisco, and Oakland.

Table 7.11. Heterogeneous Impact Across the Median Household Income Level After the Tax Implementation Based on DID Regressions for Drug Stores in Large Jurisdictions

Taxed Jurisdictions	Income Levels	(1) Ln (Price)	(2) Ln (Volume)
	Low-income	0.3245***	-0.0406
		(0.0212)	(0.0469)
		[0.9279]	[-0.1251]
	Middle-income	0.2090***	-0.0469
Philadelphia		(0.0322)	(0.0487)
		[0.6828]	[-0.2242]
	High-income	0.3373***	-0.1205***
		(0.0211)	(0.0425)
		[1.1627]	**[-0.3571]**
	Low-income	0.1764***	-0.0478
		(0.0241)	(0.0431)
		[0.6340]	[-0.2709]
	Middle-income	0.2542***	-0.1222*
Seattle		(0.0247)	(0.0679)
		[0.8884]	**[-0.4806]**
	High-income	0.2319***	-0.0918***
		(0.0297)	(0.0274)
		[0.9286]	**[-0.3959]**
	Low-income	0.0828***	-0.0188
		(0.0259)	(0.0412)
		[0.6400]	[-0.2271]
	Middle-income	0.1472***	-0.0209
San Francisco		(0.0244)	(0.0413)
		[1.0009]	[-0.1420]
	High-income	0.0293	0.0509*
		(0.0265)	(0.0268)
		[0.2753]	
	Low-income	0.0751*	0.2269
		(0.0417)	(0.1275)
		[0.4167]	
	Middle-income	0.1240***	0.0606
Oakland		(0.0325)	(0.0687)
		[0.7520]	
	High-income	0.0316	-0.0203
		(0.0320)	(0.0281)
		[0.1751]	[-0.6419]

Notes: I estimate the heterogeneous effects of the soda tax across the median household income level based on specification (6.2) for Philadelphia, Seattle, San Francisco, and Oakland. Columns (1) - (2) contain results for the impact (i.e., percentage change) on the price and volume sales, respectively. For ease of comparison, I only report the corrected DID coefficients (β), i.e., the average treatment effects. Standard errors are in parentheses. The pass-through rates and implied price elasticities are in square brackets. ***, **, and * denote 1%, 5%, and 10% significance levels, respectively.

Finally, I estimate the heterogeneous effects of soda taxes across different levels of ethnic diversity, with a specific focus on the percentage of African-Americans living in specific neighborhoods. The existing research reports that sugary drink consumption is disproportionately high among certain ethnic groups; non-white populations tend to drink more sugary beverages, such as African-American, Hispanic consumers, with African-Americans having the highest level of sugary drink consumption (Bleich et al. 2018; Jiang et al. 2020). Soda consumption is a key contributor to the epidemic of obesity and non-white populations, especially African-Americans, also have the highest obesity rates and tend to be most negatively affected by the chronic illnesses associated with obesity (Petersen, Pan, and Blanck 2019).

I first explore whether soda taxes can reduce more soda consumption among neighborhoods with a larger non-white population and whether the tax burden falls most heavily on certain ethnic groups. In the regressions, I define the variable of ethnic diversity as the percentage of non-whites of stores' zip codes and treat it in the same way I did in analyzing the heterogeneous effects across household income.

Table 7.10 contains information on how I define low, medium, and high ethnic diversity, as well as the range of non-white percentages for each category. The dummy variables created for the diversity levels interact with the DID term in the specification (6.1), yielding three coefficients that capture the heterogeneous effects across the ethnic diversity level. Again, I use data on drug stores and convenience stores as large retailers such as mass merchandisers likely adopt "zone pricing" strategies across zip codes. If the data on convenience stores do not cover all diversity levels, I only estimate the heterogeneous effects for drug stores. I present the results by store type in Table 7.12.

The results show that in Philadelphia, retailers from zip codes with a higher percentage of non-whites passed on a larger proportion of the soda tax to consumers (i.e., a larger pass-through). I conduct a formal test for the hypothesis of the equality of price coefficients for different diversity levels and the results for Philadelphia suggest that the percentage change in the price in zip codes at the high-diversity level is significantly different from that in less diverse zip codes (see the test results in Appendix). In addition, volume sales of sodas declined significantly in the highly racially diverse areas of Philadelphia. It seems that the tax in Philadelphia caused soda consumption to drop in these neighborhoods.

In Oakland and Seattle, the estimates in the price column show that the pass-through rates of drug stores in zip codes with a primarily white population are larger than those in zip codes with a greater share of non-white populations. In terms of the impact on volume sales, soda taxes in Oakland, Seattle, and San Francisco did not have significant impacts on soda sales in zip codes with a higher proportion of non-whites. Considering that nonwhite consumers purchase more sugary drinks, this implies the taxes might have not been effective in reducing soda consumption for these populations.

I further examine the heterogeneous effects of soda taxes across the percentage of African-Americans. My approach is to replace the percentage of the non-white population of stores' zip codes in regressions for the ethnic diversity with the percentage of African-Americans. The regression estimates are shown in Table 7.13. For Philadelphia, the results seem close to those on ethnic diversity. That is, retailers (drug stores) in zip codes with a higher percentage of the African-American population had a larger pass-through rate, indicating a larger tax burden for African-Americans. Similar results are found for the drug stores in San Francisco and Oakland. The taxes had significant impacts on soda prices in neighborhoods with the highest percentage of African-Americans. These neighborhoods are seeing the largest pass-through rates. These results provide evidence that the tax burden may fall more heavily on African-American consumers in Oakland, San Francisco, and Philadelphia.

I also consistently find convenience stores passed on a higher proportion of the tax in zip codes with a larger proportion of African-Americans. According to current studies, neighborhoods with higher proportions of African-Americans have a significant presence of convenience stores and limited access to supermarkets and grocery stores (Hilmers, Hilmers, and Dave 2012). The relatively larger pass-through rate of the tax among convenience stores and their prevalence in African-American communities indicates that African-American consumers may bear a larger share of the tax burden.

Regarding the impact on volume sales, I did not find any significant impacts of taxes in San Francisco and Oakland on soda sales among zip codes with a higher proportion of African-Americans. Once more, these taxes may be ineffective in reducing soda consumption among these ethnic groups. However, I do find a significant reduction in the volume sales (-13.48%) at drug stores of Seattle that are located in neighborhoods with a larger proportion of African-Americans residents. To a certain degree, Seattle's soda tax appears to be effective in achieving its goal of curbing soda consumption among these populations.

Table 7.12. Heterogeneous Impact Across the Ethnic Diversity Level After the Tax Implementation Based on DID Regressions for Large Jurisdictions

Taxed Jurisdictions	Store Type	Ethnic Diversity Levels	(1) Ln (Price)	(2) Ln (Volume)
Philadelphia	Drug Stores	Low-diversity	0.2871^{***}	-0.0549
			(0.0223)	(0.0419)
			[0.9529]	[-0.1912]
		Middle-diversity	0.2313^{***}	-0.0215
			(0.0309)	(0.0521)
			[0.7661]	[-0.0928]
		High-diversity	0.3653^{***}	-0.1035^{**}
			(0.0209)	(0.0452)
			[1.0323]	**[-0.2834]**
Seattle	Drug Stores	Low-diversity	0.2579^{***}	-0.0643^{*}
			(0.0186)	(0.0334)
			[1.0419]	**[-0.2492]**
		Middle-diversity	0.2296^{***}	-0.1181^{***}
			(0.0291)	(0.0460)
			[0.8384]	**[-0.5145]**
		High-diversity	0.1953^{***}	-0.0867
			(0.0279)	(0.0756)
			[0.6462]	[-0.4439]
San Francisco	Drug Stores	Low-diversity	0.0759^{***}	-0.0488
			(0.0151)	(0.0296)
			[0.6078]	[-0.6430]
		Middle-diversity	0.0780^{***}	-0.0294
			(0.0239)	(0.0361)
			[0.6342]	[-0.3769]
		High-diversity	0.0835^{**}	-0.0514
			(0.0349)	(0.0519)
			[0.6609]	[-0.6155]
Oakland	Drug Stores	Low-diversity	0.0883^{***}	-0.0593^{*}
			(0.0278)	(0.0326)
			[0.5298]	[-0.6709]
		Middle-diversity	0.0731^{**}	0.2462^{*}
			(0.0352)	(0.1191)
			[0.4135]	
		High-diversity	0.0802^{*}	0.0475
			(0.0431)	(0.0936)
			[0.4212]	
	Convenience Stores	Low-diversity	0.1200^{***}	-0.0060
			(0.0209)	(0.0223)
			[1.0713]	[-0.0500]
		Middle-diversity	-0.0238	0.1270^{***}
			(0.1330)	(0.0252)
		High-diversity	0.1188^{***}	-0.0703
			(0.0137)	(0.0472)
			[1.0859]	[-0.5922]

Notes: I estimate the heterogenous effects of the soda tax across the ethnic diversity level based on specification (6.2) for Philadelphia, Seattle, San Francisco, and Oakland. The ethnic diversity is measured by the percentage of non-whites population. Columns (1) - (2) contain results for the impact (i.e., percentage change) on the price and volume sales, respectively. For ease of comparison, I only report the corrected DID coefficients (β), i.e., the average treatment *effects. Standard errors are in parentheses. The pass-through rates and implied price elasticities are in square brackets.* ***, **, *and* * *denote 1%, 5%, and 10% significance levels, respectively.*

Table 7.13. Heterogeneous Impact Across the Percentage of African-Americans After the Tax Implementation Based on DID Regressions for Large Jurisdictions

Taxed Jurisdictions	Store Type	Percentage of African-Americans	(1) Ln (Price)	(2) Ln (Volume)
Philadelphia	Drug Stores	Low-diversity	0.2720***	-0.0576
			(0.0247)	(0.0370)
			[0.8832]	[-0.2116]
		Middle-diversity	0.2431***	-0.0434
			(0.0303)	(0.0585)
			[0.7440]	[-0.1784]
		High-diversity	0.3653***	-0.1035**
			(0.0209)	(0.0452)
			[1.0324]	**[-0.2833]**
Seattle	Drug Stores	Low-diversity	0.2607***	-0.0812
			(0.0238)	(0.0567)
			[1.0953]	[-0.3113]
		Middle-diversity	0.1754***	-0.0557**
			(0.0229)	(0.0287)
			[0.6722]	**[-0.3174]**
		High-diversity	0.2397***	-0.1348**
			(0.0282)	(0.0731)
			[0.7801]	**[-0.5624]**
	Convenience Stores	Low-diversity	0.1286***	-0.0355*
			(0.0245)	(0.0212)
			[0.6848]	**[-0.2762]**
		Middle-diversity	0.2292***	-0.1388***
			(0.0112)	(0.0315)
			[1.2112]	**[-0.6054]**
		High-diversity	0.2187***	-0.0675
			(0.0380)	(0.1014)
			[1.1065]	[-0.3087]
San Francisco	Drug Stores	Low-diversity	0.0275*	-0.0627***
			(0.0145)	(0.0234)
			[0.2386]	**[-2.2812]**
		Middle-diversity	0.1144***	-0.0007
			(0.0258)	(0.0358)
			[0.8067]	[-0.0057]
		High-diversity	0.1171***	-0.0458
			(0.0342)	(0.0555)
			[0.9030]	[-0.3911]
Oakland	Drug Stores	Low-diversity	0.0949***	-0.0197
			(0.0260)	(0.0398)
			[0.5119]	[-0.2071]
		Middle-diversity	0.0592	0.2870**
			(0.0371)	(0.1154)
			[0.3572]	
		High-diversity	0.1138***	0.0296
			(0.0209)	(0.0567)
			[0.6383]	
		Low-diversity	0.0035	-0.0189
			(0.0846)	(0.0195)

		[0.0345]	[-5.3717]
Convenience Stores	Middle-diversity	0.1486***	-0.0576
		(0.0179)	(0.0555)
		[1.3624]	[-0.3878]
	High-diversity	0.1057***	-0.0858
		(0.0104)	(0.0570)
		[0.9402]	[-0.8119]

*Notes: I estimate the heterogenous effects of the soda tax across the levels of percentages of the African-American population based on specification (6.2) for Philadelphia, Seattle, San Francisco, and Oakland. Columns (1) - (2) contain results for the impact (i.e., percentage change) on the price and volume sales, respectively. For ease of comparison, I only report the corrected DID coefficients (β), i.e., the average treatment effects. Standard errors are in parentheses. The pass-through rates and implied price elasticities are in square brackets. ***, **, and * denote 1%, 5%, and 10% significance levels, respectively.*

7.4 Summary

All soda taxes have significant effects on prices, and the pass-through rates of taxes are incomplete (less than 100%). To avoid antagonizing consumers, retailers may raise prices gradually over time rather than pass on the tax all at once. A comparison across jurisdictions shows that Philadelphia and Seattle have higher pass-through rates than other jurisdictions, where soda taxes were passed on ballot measures and received more attention, forcing retailers not to raise prices substantially.

Although numerous studies have shown that retailers had a relatively small response to the soda tax in Berkeley, the first city in the U.S. that implemented a soda tax, this study reports significant increases in pass-through in the fourth and fifth years after the tax was implemented. One possible reason may be that other Bay Area cities also implemented soda taxes, making cross-border shopping less beneficial for Berkeley's consumers. In terms of volume sales, the effects of taxes on soda sales are much smaller than on prices, especially in the Bay Area and Boulder, where taxes were introduced to reduce soda consumption and improve health, suggesting that taxes may be not effective in achieving these goals.

There is no convincing evidence that consumers are switching to untaxed beverages after soda drinks were priced higher. I do find, however, that in Philadelphia, the volume sales of taxable beverages declined more in stores located on the city's border than in stores located farther away from the city boundary, and the volume sales of taxable beverages rose significantly in the untaxed stores near Philadelphia, providing evidence that consumers avoid taxes by cross-border shopping. Importantly, the results imply that the neighboring untaxed stores are indirectly affected by the tax. Thus, studies that used stores near the taxed cities as controls may overstate the effects of taxes.

I also find large amounts of heterogeneity in retailers' price responses to soda taxes along store type, product characteristics, and local demographic characteristics. Specifically, larger retailers have lower pass-through rates than smaller retailers. I attribute this to the "market basket effect," in which larger retailers tend not to raise soda prices much for fear of losing all the profits from consumers' shopping baskets if they elect to shop elsewhere in response to higher prices.

Taking advantage of the rich data, I further validate the findings of Della Vigna and Gentzkow (2019) that retailers belonging to the same chain set nearly uniform prices within regions. I used data from Philadelphia, Seattle, and San Francisco to conduct the analysis and found that retailers belonging to the same chain but located in different cities raised the prices of soda drinks by similar amounts after the three different soda taxes were implemented. That said, for soda drinks, I found evidence of "uniform pricing".

I also explored the heterogeneous effects of soda taxes along the beverage category within each store type in each jurisdiction. Different types of retailers responded differently to the soda tax. Mass merchandisers tend to raise prices of the least number of products. If aggregating different store types to analyze the heterogeneity along the beverage category, the results for the beverage category would be mixed with the heterogeneous effects for the store type. In fact, most of the existing studies on soda taxes have analyzed the heterogeneous effects of taxes at an aggregate level. The richness of my data allows me to further explore the heterogeneity of price and volume changes across different beverage categories within different store types.

I find that the pass-through rate of a soda tax varies significantly along the beverage category and that the pattern of price increases for different beverage categories varies by store type and by jurisdiction. But I find little variation in the percent changes of prices for different categories. The possible reason is that retailers that sell tens of thousands of products do not attempt to calculate the optimal price of each item; instead, they choose to raise the price of all products by a fixed proportion.

The impact on volume sales also varies across beverage categories, but not as much as price. Some beverages (e.g., sodas) are "staple" products for some households, while others (e.g., functional drinks) are for specific occasions or circumstances. Consumers are likely to have different preferences for different categories of beverages, and thus react differently to price increases. I examine the relationship between the pass-through rate of the soda tax and the implied price elasticity and find that beverages with greater elasticity tend to have lower tax pass-through rates.

Another beverage characteristic I explored is the package size. I find that at the larger retailers, the pass-through rate is higher for larger sizes than for smaller ones, while the opposite result is found for smaller retailers. In other words, the impact of the soda tax on the price of different sizes of bottled drinks depends on the store type, again highlighting how different types of retailers respond to the tax differently. In addition, the volume sales of large bottles of soda drinks fall significantly as prices rise, while the tax has no significant impact on the volume sales of small bottles.

As a "sin tax", an important topic in the research related to the soda tax is whether the tax is regressive. Moreover, the main goal of most soda tax policies is to reduce soda consumption, especially for low-income households. In Philadelphia and Seattle, the low-income areas where soda taxes had no significant impact on soda sales may bear more tax burdens, i.e., the soda taxes are regressive. Although I find a slightly larger pass-through rate in high-income areas, the difference in the price coefficients of different areas is not significant. The results in San Francisco and Oakland show clearly that retailers in low-income neighborhoods passed on a larger percentage of the soda tax to consumers than retailers in high-income neighborhoods, and the tax had no significant impact on soda sales in all zip codes, meaning that much of the burdens of San

Francisco's soda tax and Oakland's soda tax fell on low-income consumers.

Finally, I analyze the heterogeneous effects along ethnic diversity as non-white populations tend to consume more sodas and thus be more negatively affected by obesity. I find that in Philadelphia, zip codes with higher racial diversity bear a higher tax pass-through rate and more tax burdens. Also, Philadelphia's tax has significantly negative impact on the volume sales in racially diverse areas. The results for Oakland and Seattle, however, did not support a larger tax burden in more racially diverse areas. In Oakland, Seattle, and San Francisco, the taxes did not have significant impacts on soda sales in zip codes with a higher proportion of non-whites.

I also investigate the relationship between the percentage of African-Americans and the impact on price and volume sales. The results obtained for Philadelphia are similar to those for ethnic diversity. That is, the tax burdens fall more heavily on areas with more non-white populations, especially on areas with more African-American consumers. Although I did not find a larger pass-through rate for more racially diverse drug stores in Seattle, the convenience stores in areas with more African-Americans tend to pass on a larger proportion of the soda tax to the consumer price. The findings for Oakland's and San Francisco's soda taxes are consistent with those for Philadelphia, i.e., areas with a high percentage of African-Americans may bear a large percentage of the tax burden. The results for the volume sales, however, suggest that the taxes in Oakland and San Francisco did not have impact on soda sales in zip codes with a higher proportion of African-Americans.

Chapter 8
Conclusions and Further Discussion

Motivated by health concerns or tax revenue increases, several jurisdictions have imposed soda taxes and received extensive attention. This study evaluates the impact of soda taxes on the price and volume sales of taxed beverages based on a large IRI scanner dataset. Specifically, I first examined the average impact of soda taxes on prices and volume sales, and whether they have "spillover" effects -- that is, whether consumers switch to untaxed drinks or the nearby untaxed stores. Then I examined whether a soda tax has heterogeneous impacts based on different product characteristics and different store types. Finally, I investigated whether the tax burden is borne disproportionately by certain groups of consumers and whether it is regressive in its impacts.

Prices of taxed beverages rose in all jurisdictions after soda taxes were implemented, and retailers passed a proportion of a tax on to consumers (i.e., an incomplete pass-through). Rather than pass all the tax on to consumer prices at once, retailers raised prices gradually over time. This suggests that future research should not only focus on the short-term impact, but also pay more attention to the long-term policy impact, otherwise the impact might be underestimated. Earlier studies on Berkeley's tax have shown that retailers raised the prices of taxed beverages by slight magnitudes. But in the most recent two years, soda prices in Berkeley have risen significantly, perhaps as neighboring cities (e.g., Oakland, San Francisco) have also implemented soda taxes that reduce opportunities for Berkeley consumers to avoid taxes by cross-border shopping.

The soda taxes voted through the city council vote have higher pass-through rates than those passed on ballot measures. The latter is often accompanied by massive media campaigns and has received more attention, discouraging retailers from raising prices substantially. Philadelphia and Seattle that passed the taxes through a council budget vote saw relatively larger declines in the volume sales of taxable beverages, while the Bay Area and Boulder (except the most recent two years in Berkeley) experienced almost no sales reduction of soda drinks. Given that soda taxes in the Bay Area and Boulder seek to reduce soda consumption and improve public health, this result suggests that they have been rather ineffective in achieving their goals.

There is little evidence supporting a substitution towards untaxed beverages when consumers face an increase in the price of soda drinks. However, consumers may have avoided the tax by making cross-border purchases. In Philadelphia, stores near the city limits saw a larger decline in the volume sales of taxable beverages than stores farther away from the city limits; meanwhile, the volume sales of taxable beverages rose significantly in the untaxed stores near Philadelphia. Moreover, there is weak evidence that retailers close to the city boundary passed a smaller proportion of the soda tax to consumers, indicating that retailers may take into account the possibility of cross-border shopping when responding to the tax. These results suggest that future research should be cautious about using nearby cities as controls as they may be indirectly affected by the tax.

The impact of soda taxes on retail prices is also highly heterogeneous in terms of store types, product characteristics, and local demographics. First, large retailers (e.g., mass merchandisers, grocery stores) seem to absorb a larger share of taxes than small retailers as they risk losing all the profits from consumers' shopping baskets if they raise soda prices by large magnitudes and cause some consumers to shop elsewhere. An analysis of data from Philadelphia, Seattle, and San Francisco suggests that the "uniform pricing" strategy performed by chain retailers, a finding by Della Vigna and Gentzkow (2019), holds for soda drinks. Retailers belonging to the same chain but located in different cities raised the prices of soda drinks by similar amounts, even though they responded to different tax policies.

Retailers may raise prices in similar proportions for different beverage categories in response to the soda tax. Retailers selling tens of thousands of items may not calculate the optimal price of each item; instead, they choose to raise the price of all products by a fixed percentage. Because the pre-tax prices of different beverage categories are different, the pass-through rate of a soda tax varies along the beverage category. The finding of the heterogeneity in the responses of different store types within the same beverages category highlights the importance of an analysis at a disaggregated level, an issue largely ignored by prior studies.

In analyzing the heterogeneity of the soda tax's impact, previous studies focused on either store types or beverage categories. Without distinguishing between store types, the results about the heterogeneous effects across beverage categories may be mixed with the heterogeneous effects across store types. The impact of the soda tax on prices of different package sizes also varies by store type. At larger retailers, the pass-through rate is higher for large bottles than for small ones, while the opposite result is found at smaller retailers, further illustrating the need for an analysis of heterogeneity at the disaggregated level.

Opponents of the soda tax claim that the tax is regressive and can hurt low-income consumers. The findings presented in this study suggest that the taxes are indeed regressive. In

Philadelphia and Seattle, soda sales in the low-income zip codes were less responsive to the price increase due to the soda tax, and in San Francisco and Oakland, retailers in the low-income areas passed on a larger proportion of the soda tax to consumers. Moreover, most of current soda taxes aim to reduce the consumption of soda drinks, especially among low-income households who tend to purchase more sodas and are more adversely affected by obesity. The results for the heterogeneous impact on volume sales, however, suggest that the soda taxes are not effective at reducing soda consumption among low-income neighborhoods.

Additionally, the soda taxes may not be effective at reducing soda consumption among non-white populations, who tend to drink more sugary beverages. The neighborhoods in Philadelphia with higher racial diversity and with a higher proportion of African Americans in particular had a higher tax pass-through rate and thus bore more tax burdens. In San Francisco, Oakland and Seattle, the findings suggest that African-American consumers may bear a relatively large percentage of the tax burden.

The media campaigns before the tax referendum may have played a vital role in terms of altering consumers' behavior. The information conveyed in the newspapers, advertisements, etc. reminds consumers of the adverse health outcomes of drinking soda, and they also alert consumers to potential price increases. Therefore, the implemented soda taxes may have informational effects in addition to price effects. For example, the reductions in soda sales, in addition to being a response to higher prices, may also be a result of consumers' weakened preference for soda drinks after they learn about their potential health risks. Not isolating the informational effects from price effects will falsely amplify the price effects of a soda tax. However, only a few studies on soda taxes have addressed this point to date (Taylor et al. 2017; Cornelsen and Smith 2018; Ahn and Lusk 2020). This is also one of the main limitations of this study. To fully understand effects of a soda tax, an analysis of informational effects deserves more attention in future work.

Another limitation of the existing literature, including this study, on the impact of soda taxes is the lack of analysis of the tax pass-through from distributors to retailers. As discussed earlier, the incomplete pass-through and reported heterogeneous effects of soda taxes may be in part due to the behavior of distributors in the first stage of passing the tax. Thus, to understand how a soda tax affects the pricing behavior of retailers, it is necessary to investigate distributors' behavior and how they respond to a soda tax. If distributors, for example, pass all the tax to retailers, the pass-through rates estimated in this study only represent the effects of the tax on retailers. Finally, it is worthwhile to emphasize again that future research should focus more on the analysis of heterogeneous effects, especially at a more disaggregated level. This study has demonstrated significant heterogeneous effects of soda taxes across various dimensions. The results of studies at high aggregation levels might obscure these details that could help us precisely understand the effects and effectiveness of soda taxes.

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Appendix

A: Regression Results

Table A.1: Full Regression Results for the Average Impact on Taxed Beverages After the Tax Implementation Based on DID Regressions

Variables	Philadelphia		Seattle		Oakland		Berkeley		San Francisco		Boulder	
	(1) Ln (Price)	(2) Ln (Volume)	(1) Ln (Price)	(2) Ln (Volume)	(1) Ln (Price)	(2) Ln (Volume)	(1) Ln (Price)	(2) Ln (Volume)	(1) Ln (Price)	(2) Ln (Volume)	(1) Ln (Price)	(2) Ln (Volume)
Year 1 * Treated	0.304***	-0.181***	0.166***	-0.072***	0.063**	-0.006	0.015	-0.021	0.038**	-0.005	0.098***	-0.021
	(0.019)	(0.034)	(0.014)	(0.023)	(0.029)	(0.048)	(0.014)	(0.039)	(0.015)	(0.022)	(0.021)	(0.040)
	[0.727]	**[-0.595]**	**[0.605]**	**[-0.433]**	**[0.252]**	[-0.092]		[-1.386]	**[0.265]**	[-0.117]	**[0.353]**	[-0.213]
Year 2 * Treated	0.363***	-0.243***	0.196***	-0.102***	0.075**	-0.022	0.036	-0.053	0.083***	-0.004	0.151**	-0.117
	(0.028)	(0.060)	(0.025)	(0.034)	(0.036)	(0.066)	(0.024)	(0.047)	(0.019)	(0.030)	(0.063)	(0.103)
	[0.869]	**[-0.670]**	**[0.713]**	**[-0.520]**	**[0.298]**	[-0.293]		[-1.464]	**[0.576]**	[-0.050]	**[0.542]**	[-0.773]
Year 3 * Treated	0.364***	-0.255***			0.079*	0.017	0.010	-0.103			0.158**	-0.086
	(0.031)	(0.060)			(0.042)	(0.073)	(0.030)	(0.065)			(0.069)	(0.104)
	[0.870]	**[-0.703]**			**[0.317]**			[-10.137]			**[0.567]**	[-0.542]
Year 4 * Treated							0.087***	-0.196***				
							(0.031)	(0.080)				
							[0.451]	**[-2.261]**				
Year 5 * Treated							0.147***	-0.202**				
							(0.038)	(0.093)				
							[0.769]	**[-1.372]**				
Treated	0.118***	-0.207**	0.086*	0.157	-0.069	0.154	0.029	-0.112	-0.060	-0.036	0.343***	-0.082
	(0.032)	(0.087)	(0.052)	(0.117)	(0.097)	(0.196)	(0.042)	(0.120)	(0.047)	(0.068)	(0.128)	(0.151)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Constant	4.023***	2.796***	4.460***	2.065***	4.428***	2.522***	4.586***	2.473***	4.643***	2.419***	4.337***	2.214***
	(0.025)	(0.068)	(0.038)	(0.073)	(0.074)	(0.138)	(0.032)	(0.100)	(0.019)	(0.028)	(0.120)	(0.106)
# of obs.	14,171,784	14,171,784	1,796,654	1,796,654	2,723,701	2,723,701	977,164	977,164	7,803,218	7,803,218	1,109,609	1,109,609

*Notes: The dependent variables are the log of weekly price and the log of weekly normalized volume. Year1 – Year5 are dummy variables for years after the tax implementation. Treated is a dummy variable for stores in a taxing jurisdiction. Standard errors are in parentheses. ***, **, and * denote 1%, 5%, and 10% significance levels, respectively.*

Table A.2: Full Regression Results for the Average Impact on Untaxed Beverages After the Tax Implementation Based on DID Regressions

Variables	Philadelphia		Seattle		Oakland		Berkeley		San Francisco		Boulder	
	(1) Ln (Price)	(2) Ln (Volume)	(1) Ln (Price)	(2) Ln (Volume)	(1) Ln (Price)	(2) Ln (Volume)	(1) Ln (Price)	(2) Ln (Volume)	(1) Ln (Price)	(2) Ln (Volume)	(1) Ln (Price)	(2) Ln (Volume)
Year 1 * Treated	0.017 (0.011)	0.003 (0.026)	0.023** (0.011) **[0.040]**	-0.006 (0.018) [-0.257]	0.025** (0.010) **[0.052]**	0.002 (0.025)	0.003 (0.016)	-0.008 (0.048) [-3.039]	-0.043*** (0.008)	0.025 (0.019)	-0.036* (0.022)	0.046 (0.034)
Year 2 * Treated	-0.041** (0.018)	-0.004 (0.052)	0.022 (0.020)	0.027 (0.038)	0.002 (0.019)	-0.012 (0.067) [-7.25]	0.016 (0.020)	-0.078 (0.058) [-4.981]	-0.058*** (0.015)	0.210*** (0.028) [-3.599]	-0.040 (0.047)	0.011 (0.153)
Year 3 * Treated	-0.024 (0.022)	-0.005 (0.074)			-0.005 (0.025)	0.007 (0.079)	0.011 (0.027)	-0.070 (0.068) [-6.355]			-0.035 (0.058)	0.070 (0.158)
Year 4 * Treated							-0.023 (0.042)	-0.032 (0.081)				
Year 5 * Treated							0.059 (0.047)	-0.133 (0.097) [-2.256]				
Treated	0.003 (0.016)	0.035 (0.062)	0.061** (0.029)	0.229 (0.165)	0.056 (0.046)	-0.054 (0.157)	0.085*** (0.026)	0.041 (0.116)	-0.069*** (0.017)	-0.069 (0.066)	0.242*** (0.077)	-0.089 (0.172)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Constant	4.065*** (0.013)	2.919*** (0.055)	3.927*** (0.019)	2.607*** (0.096)	3.957*** (0.030)	3.171*** (0.157)	3.950*** (0.021)	2.919*** (0.064)	4.127*** (0.010)	3.236*** (0.028)	3.904*** (0.052)	2.804*** (0.090)
# of obs.	4,378,799	4,378,799	999,176	999,176	1,449,110	1,449,110	462,422	462,422	6,918,475	6,918,475	596,782	596,782

*Notes: The dependent variables are the log of weekly price and the log of weekly normalized volume. Year1 – Year5 are dummy variables for years after the tax implementation. Treated is a dummy variable for stores in a taxing jurisdiction. Standard errors are in parentheses. ***, **, and * denote 1%, 5%, and 10% significance levels, respectively.*

Table A.3: Full Regression Results for the Average Impact on the Nearby Untaxed Stores After the Tax Implementation Based on DID Regressions

Variables	Philadelphia		Seattle		Oakland		Berkeley		San Francisco		Boulder	
	(1) Ln (Price)	(2) Ln (Volume)	(1) Ln (Price)	(2) Ln (Volume)	(1) Ln (Price)	(2) Ln (Volume)	(1) Ln (Price)	(2) Ln (Volume)	(1) Ln (Price)	(2) Ln (Volume)	(1) Ln (Price)	(2) Ln (Volume)
Year 1 * Treated	-0.023** (0.011)	0.054*** (0.018) [-2.344]	0.004 (0.011)	-0.038 (0.033)	0.012 (0.015)	0.010 (0.026)	0.013 (0.013)	0.024 (0.034)	0.001 (0.016)	-0.023 (0.023)	0.012 (0.016)	-0.037 (0.024) [-3.153]
Year 2 * Treated	0.018 (0.015)	-0.021 (0.036) [-1.170]	-0.003 (0.021)	-0.058 (0.037)	-0.058** (0.031)	0.139** (0.063) [-2.383]	-0.009 (0.024)	0.084* (0.045)	0.012 (0.023)	-0.054 (0.037) [-4.358]	0.093*** (0.031) **[0.314]**	-0.085 (0.065) [-0.917]
Year 3 * Treated	-0.008 (0.019)	0.055 (0.055)			-0.100*** (0.040)	0.242*** (0.078) [-2.428]	-0.004 (0.029)	0.034 (0.059)			0.076* (0.041) **[0.256]**	0.020 (0.071)
Year 4 * Treated							-0.016 (0.030)	-0.010 (0.062)				
Year 5 * Treated							0.056 (0.037)	-0.050 (0.069) [-0.897]				
Treated	0.041 (0.026)	0.074 (0.075)	0.086 (0.055)	0.047 (0.104)	-0.045 (0.078)	0.126 (0.153)	-0.18*** (0.066)	0.229 (0.149)	-0.332*** (0.051)	0.321*** (0.106)	0.324*** (0.114)	-0.096 (0.115)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Constant	4.017*** (0.024)	2.826*** (0.064)	4.467*** (0.036)	2.061*** (0.070)	4.441*** (0.072)	2.510*** (0.135)	4.491*** (0.033)	2.512*** (0.071)	4.622*** (0.018)	2.453*** (0.029)	4.347*** (0.116)	2.180*** (0.093)
# of obs.	27,342,619	27,342,619	1,991,091	1,991,091	3,262,512	3,262,512	970,558	970,558	6,607,494	6,607,494	1,613,501	1,613,501

Notes: The dependent variables are the log of weekly price and the log of weekly normalized volume. Year1 – Year5 are dummy variables for years after the tax implementation. Treated is a dummy variable for stores in the nearby untaxed cities. Standard errors are in parentheses. ***, **, *and* * *denote 1%, 5%, and 10% significance levels, respectively.*

Table A.4: Full Regression Results for the Heterogeneous Impact Across the Distance to the Border After the Tax Implementation Based on DID Regressions for Philadelphia

Variables	(1) Ln (Price)	(2) Ln (Volume)
Year 1 * Near	0.304***	-0.210***
	(0.022)	(0.042)
	[0.719]	**[-0.690]**
Year 2 * Near	0.343***	-0.284***
	(0.043)	(0.084)
	[0.812]	**[-0.829]**
Year 3 * Near	0.376***	-0.291***
	(0.037)	(0.082)
	[0.889]	**[-0.774]**
Year 1 * Inside	0.304***	-0.150***
	(0.029)	(0.048)
	[0.736]	**[-0.492]**
Year 2 * Inside	0.349***	-0.196***
	(0.038)	(0.083)
	[0.847]	**[-0.560]**
Year 3 * Inside	0.382***	-0.227***
	(0.040)	(0.082)
	[0.925]	**[-0.595]**
Near	0.107***	-0.207*
	(0.035)	(0.109)
Inside	0.130***	-0.206**
	(0.040)	(0.100)
Year FE	Yes	Yes
Quarter FE	Yes	Yes
Constant	4.023***	2.796***
	(0.025)	(0.068)
# of obs.	14,171,784	14,171,784

*Notes: The dependent variables are the log of weekly price and the log of weekly normalized volume. Year1 – Year3 are dummy variables for years after the tax implementation. Near is a dummy variable for stores in a taxing jurisdiction and near the border. Inside is a dummy for stores in a taxing jurisdiction and far away from the border. Standard errors are in parentheses. ***, **, and * denote 1%, 5%, and 10% significance levels, respectively.*

Table A.5: Full Regression Results for the Heterogeneous Impact Across Store Type After the Tax Implementation Based on DID Regressions

Variables	Philadelphia		Seattle		Oakland		San Francisco		Boulder	
	(1) Ln (Price)	(2) Ln (Volume)	(1) Ln (Price)	(2) Ln (Volume)	(1) Ln (Price)	(2) Ln (Volume)	(1) Ln (Price)	(2) Ln (Volume)	(1) Ln (Price)	(2) Ln (Volume)
Mass	0.115*	-0.122	0.112***	-0.131***			0.069**	-0.228		
*Treated	(0.057)	(0.141)	(0.031)	(0.053)			(0.030)	(0.203)		
*Taxed	**[0.284]**	[-1.062]	**[0.332]**	**[-1.172]**			**[0.370]**	[-3.317]		
Grocery	0.377***	-0.271***			0.076***	-0.070***	0.120***	0.038		
*Treated	(0.013)	(0.039)			(0.020)	(0.025)	(0.033)	(0.054)		
*Taxed	**[0.770]**	**[-0.718]**			**[0.257]**	**[-0.918]**	**[0.567]**			
Convenience			0.201***	-0.076	0.090***	-0.059***	0.093***	-0.148***	0.091***	0.059
*Treated			(0.031)	(0.076)	(0.031)	(0.023)	(0.021)	(0.005)	(0.020)	(0.092)
*Taxed			**[1.045]**	[-0.377]	**[0.833]**	**[-0.657]**	**[0.746]**	**[-1.590]**	**[0.419]**	
Drug	0.288***	-0.038	0.226***	-0.090***	0.086***	0.084	0.079***	0.015	0.159***	0.097
*Treated	(0.016)	(0.028)	(0.015)	(0.033)	(0.023)	(0.060)	(0.015)	(0.024)	(0.021)	(0.087)
*Taxed	**[0.918]**	[-0.133]	**[0.818]**	**[-0.400]**	**[0.494]**		**[0.618]**		**[0.623]**	
Mass	0.054	0.157	0.081**	0.447***			0.067	0.088		
*Treated	(0.052)	(0.162)	(0.040)	(0.145)			(0.068)	(0.332)		
Grocery	0.077***	0.040			0.129*	-0.297	-0.050	0.037		
*Treated	(0.014)	(0.055)			(0.069)	(0.190)	(0.094)	(0.186)		
Convenience			-0.01	0.090	-0.034	-0.097	0.042***	0.246***	-0.015	-0.015
*Treated			(0.038)	(0.074)	(0.041)	(0.096)	(0.001)	(0.001)	(0.012)	(0.152)
Drug	-0.015	0.089**	0.046	0.149*	-0.006	0.107	-0.048	-0.113**	0.083***	-0.090
*Treated	(0.015)	(0.043)	(0.034)	(0.083)	(0.035)	(0.108)	(0.031)	(0.052)	(0.021)	(0.234)
Mass	0.187***	-0.078	0.092***	-0.050			0.152***	0.191		
*Taxed	(0.041)	(0.122)	(0.031)	(0.054)			(0.016)	(0.183)		
Grocery	0.220***	-0.204***			0.218***	-0.256***	0.164***	-0.251***		
*Taxed	(0.011)	(0.017)			(0.007)	(0.015)	(0.032)	(0.050)		
Convenience			0.111***	-0.173***	0.156***	-0.221***	0.155***	-0.020***	-0.045	-0.140*
*Taxed			(0.030)	(0.059)	(0.011)	(0.021)	(0.001)	(0.001)	(0.028)	(0.084)
Drug	0.128***	-0.138***	0.117***	-0.033*	0.136***	-0.204***	0.076***	-0.024**	-0.047***	-0.085
*Taxed	(0.010)	(0.020)	(0.010)	(0.019)	(0.014)	(0.041)	(0.007)	(0.012)	(0.018)	(0.070)
Mass	0.102**	-0.206	-0.576***	0.544***			-0.472***	1.106***		
	(0.048)	(0.147)	(0.047)	(0.100)			(0.050)	(0.275)		
Grocery					-0.828***	1.275***	-0.430***	0.960***		
					(0.014)	(0.064)	(0.092)	(0.105)		

Convenience	-	-	-	-	-	-	-	-	-	-
Drug	0.465***	-1.159***	-0.259***	-0.151**	-0.297***	0.003	0.055***	0.628***	-0.205***	-0.456**
	(0.015)	(0.050)	(0.039)	(0.070)	(0.021)	(0.087)	(0.016)	(0.024)	(0.014)	(0.199)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Constant	3.882***	3.241***	4.757***	2.053***	4.796***	2.189***	4.722***	1.649***	4.829***	2.160***
	(0.010)	(0.043)	(0.032)	(0.056)	(0.016)	(0.065)	(0.001)	(0.003)	(0.013)	(0.047)
# of obs.	10,222,108	10,222,108	1,261,347	1,261,347	1,914,470	1,914,470	2,526,091	2,526,091	4,464,406	4,464,406

*Notes: The dependent variables are the log of weekly price and the log of weekly normalized volume. Mass, Grocery, Drug, and Convenience are dummy variables for mass merchandisers, grocery stores, drug stores, and convenience stores. Treated is a dummy variable for stores in a taxing jurisdiction. Taxed is a dummy variable for weeks after the tax implementation. I did not analyze Berkeley because I only got access to data on drug stores for Berkeley. Standard errors are in parentheses. ***, **, and * denote 1%, 5%, and 10% significance levels, respectively.*

Table A.6: Full Regression Results for the Heterogeneous Impact Across Product Category Within Each Store Type for Philadelphia

Variables	Mass Merchandisers		Grocery Stores		Drug Stores	
	(1) Ln (Price)	(2) Ln (Volume)	(1) Ln (Price)	(2) Ln (Volume)	(1) Ln (Price)	(2) Ln (Volume)
Carbonated	0.281***	-0.187	0.410***	-0.283***	0.325***	0.059*
*Treated	(0.059)	(0.170)	(0.014)	(0.045)	(0.014)	(0.032)
*Taxed	**[0.568]**	[-0.666]	**[0.723]**	**[-0.691]**	**[0.793]**	
Sports	0.015	-0.073	0.359***	-0.166***	0.207***	-0.030
*Treated	(0.081)	(0.129)	(0.034)	(0.053)	(0.023)	(0.028)
*Taxed		[-4.822]	**[0.974]**	**[-0.462]**	**[1.540]**	[-0.147]
Water	0.042	-0.069	0.138***	-0.014	0.125***	-0.025
*Treated	(0.055)	(0.163)	(0.017)	(0.104)	(0.008)	(0.041)
*Taxed		[-1.628]	**[0.407]**	[-0.099]	**[0.497]**	[-0.200]
Juice	-0.007	-0.022	0.321***	-0.173***	0.344***	-0.117***
*Treated	(0.072)	(0.152)	(0.015)	(0.041)	(0.025)	(0.041)
*Taxed			**[0.900]**	**[-0.539]**	**[1.077]**	**[-0.340]**
Tea	-0.007	-0.136	0.274***	-0.372***	0.370***	-0.231***
*Treated	(0.099)	(0.207)	(0.021)	(0.054)	(0.018)	(0.034)
*Taxed			**[0.664]**	**[-1.359]**	**[1.132]**	**[-0.623]**
Carbonated	0.006	0.101	0.037***	0.097	-0.027**	0.026
*Treated	(0.028)	(0.159)	(0.012)	(0.063)	(0.012)	(0.046)
Sports	0.051	0.214	0.037	0.112	-0.023	0.204***
*Treated	(0.073)	(0.198)	(0.037)	(0.075)	(0.018)	(0.050)
Water	0.030	0.405	0.076***	0.042	-0.005	0.301***
*Treated	(0.046)	(0.324)	(0.021)	(0.125)	(0.016)	(0.081)
Juice	0.081	0.213	0.105***	-0.030	-0.042***	0.102*
*Treated	(0.069)	(0.155)	(0.018)	(0.065)	(0.014)	(0.054)
Tea	0.078	0.177	0.187***	-0.239***	-0.054***	0.144***
*Treated	(0.083)	(0.121)	(0.026)	(0.063)	(0.020)	(0.048)
Carbonated	0.141***	-0.082	0.257***	-0.214***	0.115***	-0.121***
*Taxed	(0.018)	(0.118)	(0.017)	(0.023)	(0.009)	(0.022)
Sports	0.132***	0.102	0.117***	-0.267***	-0.067***	0.069***
*Taxed	(0.050)	(0.107)	(0.013)	(0.028)	(0.016)	(0.021)
Water	0.041	0.048	0.173***	0.026	0.028***	-0.001
*Taxed	(0.054)	(0.122)	(0.011)	(0.037)	(0.008)	(0.029)
Juice	0.113**	-0.075	0.214***	-0.252***	-0.066***	0.051*
*Taxed	(0.054)	(0.121)	(0.011)	(0.026)	(0.014)	(0.028)
Tea	0.204***	0.078	0.319***	-0.273***	0.131***	-0.129***
*Taxed	(0.078)	(0.166)	(0.016)	(0.029)	(0.010)	(0.020)
Sports	0.691***	-0.995***	0.780***	-0.934***	1.262***	-1.404***
	(0.039)	(0.055)	(0.017)	(0.026)	(0.016)	(0.026)
Water	0.289***	-0.841***	0.324***	-0.765***	0.394***	-1.093***
	(0.017)	(0.146)	(0.020)	(0.046)	(0.014)	(0.039)
Juice	0.369***	-0.893***	0.391***	-0.835***	0.409***	-0.796***
	(0.036)	(0.029)	(0.006)	(0.056)	(0.011)	(0.027)
Tea	0.609***	-0.999***	0.332***	-0.320***	0.514***	-0.620***
	(0.048)	(0.068)	(0.013)	(0.067)	(0.015)	(0.017)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes
Constant	3.670***	3.583***	3.609***	3.693***	3.965***	2.589***
	(0.024)	(0.143)	(0.008)	(0.054)	(0.010)	(0.035)
# of obs.	940,531	940,531	3,959,307	3,959,307	5,322,270	5,322,270

Notes: The dependent variables are the log of weekly price and the log of weekly normalized volume. Carbonated, Sports, Water, Juice, and Tea are dummy variables for carbonated drinks, sports/energy drinks, sweetened water, non-100% juices, and sweetened tea/coffee. Treated is a dummy variable for stores in a taxing jurisdiction. Taxed is a dummy variable for weeks after the tax implementation. Standard errors are in parentheses. ***, **, *and* * *denote 1%, 5%, and 10% significance levels, respectively.*

Table A.7: Full Regression Results for the Heterogeneous Impact Across Product Category Within Each Store Type for Seattle

Variables	Mass Merchandisers		Drug Stores		Convenience Stores	
	(1)	(2)	(1)	(2)	(1)	(2)
	Ln (Price)	Ln (Volume)	Ln (Price)	Ln (Volume)	Ln (Price)	Ln (Volume)
Carbonated	0.2163***	-0.1844**	0.3279***	-0.1271***	0.2104***	-0.0281
*Treated	(0.0357)	(0.0987)	(0.0192)	(0.0413)	(0.0404)	(0.0991)
*Taxed	**[0.4681]**	**[-0.8524]**	**[0.8250]**	**[-0.3877]**	**[0.8682]**	[-0.1336]
Sports	0.0457	-0.0990*	0.1970***	-0.1011***	0.2147***	-0.0509
*Treated	(0.0293)	(0.0559)	(0.0253)	(0.0336)	(0.0369)	(0.1073)
*Taxed		[-2.1669]	**[1.1134]**	**[-0.5133]**	**[1.2645]**	[-0.2371]
Water	-0.0363	0.2151	0.2066***	-0.0341		
*Treated	(0.1072)	(0.2004)	(0.0301)	(0.0829)		
*Taxed			**[1.0845]**	[-0.1651]		
Juice	0.0901**	-0.1344	0.1487***	0.0301	0.1251*	-0.1908*
*Treated	(0.0382)	(0.1094)	(0.0432)	(0.0493)	(0.0740)	(0.1095)
*Taxed	**[0.3185]**	[-1.4917]	**[0.5256]**		**[0.5532]**	**[-1.5250]**
Tea	-0.0124	-0.0259	0.1774***	-0.1339***	0.2530***	-0.2172***
*Treated	(0.0402)	(0.0741)	(0.0274)	(0.0415)	(0.0426)	(0.0771)
*Taxed			**[1.2295]**	**[-0.7552]**	**[1.7796]**	**[-0.8583]**
Carbonated	-0.023	0.433***	0.027	0.177	0.044	0.031
*Treated	(0.045)	(0.155)	(0.033)	(0.112)	(0.038)	(0.109)
Sports	0.153**	0.443***	0.049	0.161**	-0.057	0.126
*Treated	(0.065)	(0.150)	(0.038)	(0.074)	(0.041)	(0.098)
Water	-0.007	0.579***	0.007	-0.006		
*Treated	(0.035)	(0.118)	(0.032)	(0.117)		
Juice	0.092*	0.545***	-0.009	0.172*	-0.106	0.153
*Treated	(0.049)	(0.178)	(0.040)	(0.095)	(0.083)	(0.126)
Tea	0.065	0.499***	0.040	0.162**	-0.001	0.196**
*Treated	(0.071)	(0.175)	(0.035)	(0.080)	(0.076)	(0.093)
Carbonated	0.116***	-0.030	0.167***	-0.095***	0.168***	-0.150*
*Taxed	(0.031)	(0.082)	(0.012)	(0.030)	(0.037)	(0.082)
Sports	-0.021	0.054	0.072***	0.018	0.035	-0.248**
*Taxed	(0.029)	(0.063)	(0.017)	(0.018)	(0.034)	(0.097)
Water	0.310***	-0.028	-0.012	-0.083		
*Taxed	(0.093)	(0.182)	(0.023)	(0.058)		
Juice	0.131***	-0.202**	-0.046*	0.098***	0.199***	0.047
*Taxed	(0.034)	(0.093)	(0.027)	(0.025)	(0.075)	(0.083)
Tea	0.214***	-0.259***	0.044***	0.022	0.128***	-0.094
*Taxed	(0.034)	(0.082)	(0.013)	(0.025)	(0.021)	(0.061)
Sports	0.671***	-0.860***	0.966***	-1.009***	0.588***	-0.531***
	(0.051)	(0.078)	(0.021)	(0.040)	(0.024)	(0.061)
Water	0.152***	-0.704***	0.655***	-1.338***		
	(0.024)	(0.095)	(0.024)	(0.088)		
Juice	0.477***	-0.993***	0.453***	-0.956***	0.344***	-0.914***
	(0.032)	(0.060)	(0.018)	(0.034)	(0.081)	(0.064)
Tea	0.935***	-1.140***	0.978***	-1.134***	0.639***	-0.947***
	(0.049)	(0.045)	(0.034)	(0.037)	(0.064)	(0.047)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes
Constant	3.772***	3.193***	3.992***	2.520***	4.369***	2.520***
	(0.036)	(0.143)	(0.022)	(0.053)	(0.035)	(0.071)
# of obs.	259,000	259,000	765,818	765,818	236,400	236,400

Notes: The dependent variables are the log of weekly price and the log of weekly normalized volume. Carbonated, Sports, Water, Juice, and Tea are dummy variables for carbonated drinks, sports/energy drinks, sweetened water, non-100% juices, and sweetened tea/coffee. Treated is a dummy variable for stores in a taxing jurisdiction. Taxed is a dummy variable for weeks after the tax implementation. Standard errors are in parentheses. ***, **, *and* * *denote 1%, 5%, and 10% significance levels, respectively.*

Table A.8: Full Regression Results for the Heterogeneous Impact Across Product Category Within Each Store Type for Oakland

Variables	Grocery Stores		Drug Stores		Convenience Stores	
	(1) Ln (Price)	(2) Ln (Volume)	(1) Ln (Price)	(2) Ln (Volume)	(1) Ln (Price)	(2) Ln (Volume)
Carbonated	0.316***	-0.176***	0.096***	0.196***	0.130***	-0.056
*Treated	(0.015)	(0.033)	(0.031)	(0.069)	(0.026)	(0.067)
*Taxed	**[0.886]**	**[-0.557]**	**[0.392]**		**[0.969]**	[-0.433]
Sports	-0.028	-0.002	0.130***	-0.012	0.097***	-0.136***
*Treated	(0.027)	(0.030)	(0.028)	(0.051)	(0.017)	(0.034)
*Taxed			**[1.379]**	[-0.096]	**[1.148]**	**[-1.402]**
Water	0.217***	-0.379***	0.140***	0.286	0.096*	-0.107
*Treated	(0.046)	(0.120)	(0.027)	(0.157)	(0.047)	(0.124)
*Taxed	**[1.308]**	**[-1.744]**	**[0.963]**		**[0.887]**	[-1.121]
Juice	-0.005	-0.031	0.058	0.116	0.020	0.036
*Treated	(0.023)	(0.034)	(0.040)	(0.086)	(0.080)	(0.085)
*Taxed			[0.286]		[0.130]	
Tea	-0.011	0.049	0.035	0.031	0.031	0.029
*Treated	(0.020)	(0.067)	(0.036)	(0.095)	(0.085)	(0.088)
*Taxed			[0.229]		[0.296]	
Carbonated	0.032	-0.100	0.003	0.118	0.066	-0.122
*Treated	(0.094)	(0.213)	(0.041)	(0.137)	(0.041)	(0.115)
Sports	0.093***	-0.668***	-0.112***	0.161*	-0.021	-0.077
*Treated	(0.031)	(0.172)	(0.040)	(0.091)	(0.043)	(0.070)
Water	0.321***	-0.577***	-0.025	0.317**	0.148**	-0.247
*Treated	(0.021)	(0.097)	(0.018)	(0.157)	(0.070)	(0.159)
Juice	0.216***	-0.025	0.079*	0.078	-0.006	-0.036
*Treated	(0.070)	(0.216)	(0.040)	(0.119)	(0.65)	(0.173)
Tea	0.211**	-0.684***	0.186***	-0.034	0.022	-0.193
*Treated	(0.094)	(0.246)	(0.034)	(0.115)	(0.082)	(0.173)
Carbonated	0.158***	-0.198***	0.160***	-0.190***	0.193***	-0.181***
*Taxed	(0.016)	(0.026)	(0.013)	(0.042)	(0.014)	(0.028)
Sports	0.149***	-0.388***	-0.022	-0.048	0.053***	-0.173***
*Taxed	(0.015)	(0.027)	(0.022)	(0.040)	(0.015)	(0.026)
Water	0.117***	-0.062***	0.068***	-0.279***	0.088***	0.084
*Taxed	(0.013)	(0.024)	(0.020)	(0.078)	(0.023)	(0.108)
Juice	0.237***	-0.210***	0.016	-0.060	0.233***	-0.260***
*Taxed	(0.016)	(0.027)	(0.028)	(0.066)	(0.031)	(0.057)
Tea	0.479***	-0.639***	0.256***	-0.290***	0.393***	-0.477***
*Taxed	(0.016)	(0.027)	(0.017)	(0.072)	(0.030)	(0.052)
Sports	0.583***	-0.280***	1.161***	-1.082***	0.716***	-0.411***
	(0.001)	(0.001)	(0.025)	(0.053)	(0.035)	(0.059)
Water	0.229***	-0.375***	0.518***	-0.775***	0.154**	-0.220**
	(0.001)	(0.002)	(0.020)	(0.062)	(0.064)	(0.103)
Juice	0.203***	-0.780***	0.350***	-0.725***	0.155***	-0.470***
	(0.001)	(0.001)	(0.026)	(0.035)	(0.028)	(0.069)
Tea	0.282***	-0.299***	0.562***	-0.734***	0.383***	-0.297***
	(0.001)	(0.002)	(0.025)	(0.045)	(0.035)	(0.068)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes
Constant	3.708***	3.838***	3.978***	2.776***	4.386***	2.460***
	(0.010)	(0.029)	(0.014)	(0.068)	(0.019)	(0.084)
# of obs.	636,294	636,294	799,198	799,198	478,978	478,978

Notes: The dependent variables are the log of weekly price and the log of weekly normalized volume. Carbonated, Sports, Water, Juice, and Tea are dummy variables for carbonated drinks, sports/energy drinks, sweetened water, non-100% juices, and sweetened tea/coffee. Treated is a dummy variable for stores in a taxing jurisdiction. Taxed is a dummy variable for weeks after the tax implementation. Standard errors are in parentheses. ***, **, *and* * *denote 1%, 5%, and 10% significance levels, respectively.*

Table A.9: Full Regression Results for the Heterogeneous Impact Across Product Category at Drug Stores of Berkeley

Variables	Drug Stores (1) Ln (Price)	Drug Stores (2) Ln (Volume)
Carbonated *Treated *Taxed	0.009	0.093
	(0.024)	(0.110)
Sports *Treated *Taxed	0.111*	-0.186***
	(0.062)	(0.068)
	[1.254]	**[-1.686]**
Water *Treated *Taxed	0.094***	-0.115
	(0.031)	(0.108)
	[0.703]	[-1.222]
Juice *Treated *Taxed	-0.019	-0.052
	(0.047)	(0.120)
Tea *Treated *Taxed	0.046	-0.213*
	(0.043)	(0.126)
		[-4.665]
Carbonated *Treated	0.091***	-0.436***
	(0.029)	(0.153)
Sports *Treated	-0.123***	0.226*
	(0.046)	(0.130)
Water *Treated	-0.003	0.313*
	(0.009)	(0.187)
Juice *Treated	0.139***	-0.102
	(0.048)	(0.132)
Tea *Treated	0.207***	-0.239
	(0.038)	(0.159)
Carbonated *Taxed	0.121***	-0.119
	(0.027)	(0.123)
Sports *Taxed	-0.128**	0.105
	(0.052)	(0.070)
Water *Taxed	-0.010	-0.064
	(0.033)	(0.099)
Juice *Taxed	0.150***	-0.046
	(0.051)	(0.098)
Tea *Taxed	0.037	0.135
	(0.046)	(0.101)
Sports	1.466***	-1.608***
	(0.036)	(0.082)
Water	0.608***	-0.923***
	(0.021)	(0.083)
Juice	0.182***	-0.838***
	(0.024)	(0.041)
Tea	0.654***	-0.845***
	(0.034)	(0.139)
Year FE	Yes	Yes
Quarter FE	Yes	Yes
Constant	3.944***	3.288***
	(0.022)	(0.136)
# of obs.	805,477	805,477

Notes: The dependent variables are the log of weekly price and the log of weekly normalized volume. Carbonated, Sports, Water, Juice, and Tea are dummy variables for carbonated drinks, sports/energy drinks, sweetened water, non-100% juices, and sweetened tea/coffee. Treated is a dummy variable for stores in a taxing jurisdiction. Taxed is a dummy variable for weeks after the tax implementation. Standard errors are in parentheses. ***, **, *and* * *denote 1%, 5%, and 10% significance levels, respectively.*

Table A.10: Full Regression Results for the Heterogeneous Impact Across Product Category Within Each Store Type for Boulder

Variables	Drug Stores		Convenience Stores	
	(1) Ln (Price)	(2) Ln (Volume)	(1) Ln (Price)	(2) Ln (Volume)
Carbonated	0.347^{***}	0.041	0.102^{***}	0.214
*Treated	(0.080)	(0.199)	(0.035)	(0.142)
*Taxed	**[0.833]**		**[0.328]**	
Sports	0.183^{***}	0.088	0.113^{*}	-0.005
*Treated	(0.043)	(0.125)	(0.058)	(0.106)
*Taxed	**[1.123]**		**[0.692]**	[-0.045]
Water	0.224^{***}	0.164	0.304^{***}	-0.374^{*}
*Treated	(0.050)	(0.205)	(0.016)	(0.271)
*Taxed	**[0.855]**		**[1.220]**	**[-1.228]**
Juice	0.249^{***}	-0.071	-0.311^{***}	0.354
*Treated	(0.048)	(0.168)	(0.090)	(0.199)
*Taxed	**[0.818]**	[-0.287]		
Tea	0.244^{***}	-0.039	0.171^{***}	0.169
*Treated	(0.044)	(0.125)	(0.060)	(0.180)
*Taxed	**[0.918]**	[-0.159]	**[0.816]**	
Carbonated	0.054	-0.125	0.051^{***}	-0.275^{*}
*Treated	(0.087)	(0.263)	(0.018)	(0.146)
Sports	0.004	0.014	-0.089^{***}	0.117
*Treated	(0.035)	(0.221)	(0.015)	(0.140)
Water	0.042	-0.024	0.034^{***}	0.467^{*}
*Treated	(0.026)	(0.268)	(0.008)	(0.250)
Juice	0.131^{**}	-0.166	0.195^{***}	-0.461^{**}
*Treated	(0.063)	(0.297)	(0.030)	(0.198)
Tea	0.026	0.020	-0.048	0.108
*Treated	(0.039)	(0.241)	(0.052)	(0.197)
Carbonated	0.025	0.050	0.081^{***}	-0.125
*Taxed	(0.056)	(0.236)	(0.027)	(0.137)
Sports	-0.007	-0.036	0.177^{***}	-0.159^{*}
*Taxed	(0.041)	(0.166)	(0.054)	(0.091)
Water	0.018	-0.186	0.157^{***}	-0.022
*Taxed	(0.027)	(0.226)	(0.014)	(0.159)
Juice	-0.236^{***}	0.354^{*}	0.327^{***}	-0.328^{***}
*Taxed	(0.031)	(0.206)	(0.057)	(0.096)
Tea	0.110^{***}	0.011	0.352^{***}	-0.373^{***}
*Taxed	(0.037)	(0.167)	(0.029)	(0.137)
Sports	1.141^{***}	-0.890^{***}	1.054^{***}	-0.989^{***}
	(0.088)	(0.078)	(0.001)	(0.001)
Water	0.393^{***}	-0.611^{***}	0.220^{***}	-0.449^{***}
	(0.052)	(0.059)	(0.001)	(0.003)
Juice	0.326^{***}	-0.757^{***}	0.259^{***}	-0.716^{***}
	(0.034)	(0.051)	(0.002)	(0.001)
Tea	0.653^{***}	-0.809^{***}	0.545^{***}	-0.541^{***}
	(0.094)	(0.073)	(0.001)	(0.001)
Year FE	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes
Constant	4.102^{***}	2.279^{***}	4.205^{***}	2.796^{***}
	(0.063)	(0.272)	(0.009)	(0.051)
# of obs.	182,263	182,263	155,187	155,187

Notes: The dependent variables are the log of weekly price and the log of weekly normalized volume. Carbonated, Sports, Water, Juice, and Tea are dummy variables for carbonated drinks, sports/energy drinks, sweetened water, non-100% juices, and sweetened tea/coffee. Treated is a dummy variable for stores in a taxing jurisdiction. Taxed is a dummy variable for weeks after the tax implementation. Standard errors are in parentheses. ***, **, *and* * *denote 1%, 5%, and 10% significance levels, respectively.*

Table A.11: Full Regression Results for the Heterogeneous Impact Across Product Category Within Each Store Type for San Francisco

Variables	Mass Merchandisers		Grocery Stores		Drug Stores		Convenience Stores	
	(1) Ln (Price)	(2) Ln (Volume)	(1) Ln (Price)	(1) Ln (Volume)	(1) Ln (Price)	(2) Ln (Volume)	(1) Ln (Price)	(2) Ln (Volume)
Carbonated	0.216***	-0.328*	0.306***	-0.096***	0.259***	0.041	0.173***	-0.244***
*Treated	(0.039)	(0.207)	(0.019)	(0.027)	(0.014)	(0.025)	(0.0004)	(0.011)
*Taxed	**[0.929]**	**[-1.519]**	**[1.216]**	**[-0.315]**	**[1.376]**		**[1.120]**	**[-1.413]**
Sports	-0.099**	-0.168	0.017	-0.040	-0.094***	0.162***	-0.033***	-0.156***
*Treated	(0.052)	(0.194)	(0.045)	(0.065)	(0.024)	(0.029)	(0.001)	(0.010)
*Taxed				[-2.335]				
Water	-0.018	0.505**	0.093	-0.113	0.090**	-0.234**		
*Treated	(0.095)	(0.159)	(0.060)	(0.131)	(0.042)	(0.119)		
*Taxed				[-1.208]	**[0.679]**	**[-2.608]**		
Juice	0.097	-0.231**	0.255***	-0.033	0.101***	-0.185***	0.307***	-0.326***
*Treated	(0.122)	(0.115)	(0.028)	(0.036)	(0.017)	(0.027)	(0.002)	(0.009)
*Taxed		[-2.385]	**[1.370]**	[-0.128]	**[0.770]**	**[-1.826]**	**[2.615]**	**[-1.062]**
Tea	0.118**	-0.282*	0.329***	-0.025	-0.032**	-0.011	0.299***	-0.280***
*Treated	(0.051)	(0.179)	(0.028)	(0.056)	(0.015)	(0.027)	(0.001)	(0.012)
*Taxed	**[1.143]**	**[-2.386]**	**[2.242]**	[-0.076]			**[3.862]**	**[-0.936]**
Carbonated	-0.039	-0.027	-0.107*	0.179	-0.084***	-0.043	0.144***	0.139***
*Treated	(0.067)	(0.374)	(0.065)	(0.172)	(0.022)	(0.058)	(0.001)	(0.010)
Sports	0.172***	0.082	0.044	0.005	0.027	-0.210***	0.063***	0.318***
*Treated	(0.066)	(0.217)	(0.049)	(0.179)	(0.038)	(0.051)	(0.001)	(0.009)
Water	0.032	-0.085	-0.123	0.343	-0.136***	0.368***		
*Treated	(0.105)	(0.361)	(0.091)	(0.216)	(0.039)	(0.138)		
Juice	0.103	0.249	-0.31***	0.132	-0.097***	0.054	-0.037***	0.575***
*Treated	(0.071)	(0.286)	(0.095)	(0.189)	(0.028)	(0.057)	(0.001)	(0.007)
Tea	0.135**	0.102	-0.253**	0.374	0.040*	-0.097	-0.049***	0.343***
*Treated	(0.065)	(0.257)	(0.114)	(0.268)	(0.021)	(0.059)	(0.001)	(0.011)
Carbonated	0.117***	0.094	0.068	-0.234***	0.166***	-0.172***	0.189***	0.063***
*Taxed	(0.031)	(0.204)	(0.059)	(0.073)	(0.008)	(0.016)	(0.001)	(0.007)
Sports	0.155***	0.068	0.094	-0.363***	0.011	0.028	0.248***	-0.116***
*Taxed	(0.021)	(0.197)	(0.075)	(0.076)	(0.012)	(0.020)	(0.002)	(0.008)
Water	0.170***	-0.018	0.107	0.230***	0.031***	0.063*		
*Taxed	(0.043)	(0.105)	(0.083)	(0.077)	(0.010)	(0.036)		
Juice	0.283**	-0.176	-0.036	-0.286***	0.010	0.131***	0.027***	0.200***
*Taxed	(0.116)	(0.121)	(0.065)	(0.078)	(0.012)	(0.018)	(0.001)	(0.007)
Tea	0.267***	-0.111	0.053	-0.340***	0.122***	-0.065***	-0.025***	-0.011
*Taxed	(0.055)	(0.159)	(0.070)	(0.070)	(0.009)	(0.020)	(0.005)	(0.012)
Sports	0.585***	-0.859***	0.234***	-0.383***	1.050***	-0.823***	0.519***	-0.549***
	(0.023)	(0.184)	(0.034)	(0.051)	(0.014)	(0.018)	(0.001)	(0.002)
Water	0.216**	-0.671***	0.597***	-1.122***	0.528***	-0.654***		
	(0.092)	(0.065)	(0.035)	(0.084)	(0.013)	(0.067)		
Juice	0.407***	-0.943***	0.519***	-0.639***	0.536***	-0.966***	0.415***	-0.744***
	(0.034)	(0.089)	(0.040)	(0.043)	(0.008)	(0.015)	(0.001)	(0.001)
Tea	0.777***	-1.062***	0.732***	-0.983***	0.969***	-1.081***	0.729***	-0.660***
	(0.075)	(0.260)	(0.058)	(0.097)	(0.007)	(0.016)	(0.005)	(0.006)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Constant	3.827***	3.588***	4.125***	3.052***	4.157***	2.930***	4.303***	2.110***
	(0.060)	(0.354)	(0.054)	(0.068)	(0.014)	(0.028)	(0.004)	(0.015)

# of obs.	334,365	334,365	664,439	664,439	4,982,248	4,982,248	38,479	38,479

Notes: The dependent variables are the log of weekly price and the log of weekly normalized volume. Carbonated, Sports, Water, Juice, and Tea are dummy variables for carbonated drinks, sports/energy drinks, sweetened water, non-100% juices, and sweetened tea/coffee. Treated is a dummy variable for stores in a taxing jurisdiction. Taxed is a dummy variable for weeks after the tax implementation. Standard errors are in parentheses. ***, **, *and* * *denote 1%, 5%, and 10% significance levels, respectively.*

Table A.12: Full Regression Results for the Heterogeneous Impact Across the Bottle Sizes of Carbonated Drinks in Large Retailers

Variables	Philadelphia (1) Ln (Price)	Philadelphia (2) Ln (Volume)	Seattle (1) Ln (Price)	Seattle (2) Ln (Volume)	Oakland (1) Ln (Price)	Oakland (2) Ln (Volume)	San Francisco (1) Ln (Price)	San Francisco (2) Ln (Volume)	Boulder (1) Ln (Price)	Boulder (2) Ln (Volume)	Berkeley (1) Ln (Price)	Berkeley (2) Ln (Volume)
Size_1_20	0.101***	0.232**	0.025***	-0.151	0.079***	-0.041	-0.023	-0.134	0.190***	-0.094	0.191***	0.139
*Treated	(0.021)	(0.096)	(0.007)	(0.174)	(0.029)	(0.212)	(0.021)	(0.174)	(0.039)	(0.156)	(0.024)	(0.142)
*Taxed	**[0.603]**		**[0.130]**	[-5.971]	**[0.701]**	[-0.514]			**[0.809]**	[-0.497]	**[1.381]**	
Size_1_68	0.566***	-0.370***	0.400***	-0.235**	0.664***	-0.443***	0.469***	-0.199*	0.463***	0.154	0.035**	0.255
*Treated	(0.052)	(0.078)	(0.013)	(0.135)	(0.055)	(0.122)	(0.065)	(0.123)	(0.086)	(0.145)	(0.018)	(0.146)
*Taxed	**[0.686]**	**[-0.654]**	**[0.464]**	**[-0.588]**	**[1.224]**	**[-0.667]**	**[1.070]**	**[-0.424]**	**[0.585]**		**[0.082]**	
Size_12_12	0.464***	-0.494***	0.313***	-0.204	0.381***	-0.345***	0.299***	-0.144	0.242***	0.175	0.032	0.078
*Treated	(0.076)	(0.144)	(0.022)	(0.172)	(0.064)	(0.156)	(0.049)	(0.132)	(0.063)	(0.106)	(0.024)	(0.110)
*Taxed	**[0.774]**	**[-1.065]**	**[0.475]**	[-0.653]	**[1.124]**	**[-0.905]**	**[1.040]**	[-0.480]	**[0.389]**		[0.098]	
Size_1_20	0.182***	-0.144	0.016*	0.660***	-0.042	-0.660***	-0.047*	0.258	0.004	-0.106	-0.173***	-0.295
*Treated	(0.017)	(0.096)	(0.008)	(0.160)	(0.040)	(0.151)	(0.025)	(0.289)	(0.016)	(0.212)	(0.026)	(0.187)
Size_1_68	0.045***	0.009	0.012	0.519*	-0.255***	0.094	-0.301***	0.301*	0.044*	-0.050	0.049	-0.399**
*Treated	(0.013)	(0.092)	(0.016)	(0.272)	(0.055)	(0.313)	(0.065)	(0.177)	(0.026)	(0.278)	(0.030)	(0.196)
Size_12_12	-0.009	0.246***	0.027	0.235	-0.042	0.520***	-0.164**	-0.103	0.082**	0.037	0.035*	-0.608***
*Treated	(0.021)	(0.075)	(0.024)	(0.160)	(0.074)	(0.194)	(0.082)	(0.266)	(0.040)	(0.093)	(0.019)	(0.167)
Size_1_20	0.065***	-0.200***	0.112***	0.063	0.057	-0.224***	0.098***	-0.098	0.087***	0.082	0.188***	-0.237*
*Taxed	(0.013)	(0.054)	(0.004)	(0.067)	(0.035)	(0.038)	(0.033)	(0.135)	(0.012)	(0.192)	(0.028)	(0.130)
Size_1_68	0.071***	-0.207***	0.134***	-0.051	-0.008	-0.207***	0.053	-0.089	0.083***	-0.169	0.275***	-0.459***
*Taxed	(0.009)	(0.023)	(0.013)	(0.090)	(0.035)	(0.037)	(0.033)	(0.087)	(0.016)	(0.181)	(0.034)	(0.145)
Size_12_12	0.039**	-0.079*	0.071***	-0.049	0.010	0.192***	0.055	-0.034	0.027	-0.124	0.127***	-0.105
*Taxed	(0.016)	(0.047)	(0.010)	(0.073)	(0.036)	(0.036)	(0.046)	(0.128)	(0.027)	(0.139)	(0.027)	(0.117)
Size_1_20	0.993***	-0.615***	1.151***	-0.314***	1.116***	-2.118***	0.927***	-0.69***	1.296***	-0.636***	0.962***	-1.128***
	(0.021)	(0.029)	(0.009)	(0.101)	(0.001)	(0.002)	(0.057)	(0.238)	(0.010)	(0.084)	(0.011)	(0.109)
Size_1_68	-0.394***	-0.135**	-0.300**	-0.316***	-0.264***	-0.462***	-0.305***	-0.318*	0.277***	0.501***	-0.337***	-0.366***
	(0.026)	(0.058)	(0.007)	(0.070)	(0.001)	(0.001)	(0.054)	(0.168)	(0.012)	(0.171)	(0.013)	(0.061)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Constant	3.499***	4.139***	3.500***	3.500***	3.586***	4.838***	3.845***	3.737***	3.331***	2.647***	3.639***	4.033***
	(0.022)	(0.057)	(0.017)	(0.126)	(0.028)	(0.034)	(0.082)	(0.255)	(0.013)	(0.239)	(0.013)	(0.138)
# of obs.	1,136,701	1,136,701	64,552	64,552	110,252	110,252	201,567	201,567	39,979	39,979	133,260	133,260

Notes: The dependent variables are the log of weekly price and the log of weekly normalized volume. Size_1_20, Size_1_68, and Size_12_12 are dummy variables for 20 ounce bottles, 2 liter bottles, and 12 pack 12 ounce cans, respectively. Treated is a dummy variable for stores in a taxing jurisdiction. Taxed is a dummy variable for weeks after the tax implementation. Due to a data limitation, I analyzed the small retailers (e.g., drug stores and convenience stores) for Boulder and Berkeley. Standard errors are in parentheses. ***, **, *and* * *denote 1%, 5%, and 10% significance levels, respectively.*

Table A.13: Full Regression Results for the Heterogeneous Impact Across the Median Household Income Level for Drug Stores of Large Jurisdictions

Variables	Philadelphia		Seattle		Oakland		San Francisco	
	(1) Ln (Price)	(2) Ln (Volume)	(1) Ln (Price)	(2) Ln (Volume)	(1) Ln (Price)	(2) Ln (Volume)	(1) Ln (Price)	(2) Ln (Volume)
Low-income	0.325***	-0.041	0.176***	-0.048	0.075*	0.227	0.083***	-0.019
*Treated	(0.021)	(0.047)	(0.024)	(0.043)	(0.042)	(0.128)	(0.026)	(0.041)
*Taxed	**[0.928]**	[-0.125]	**[0.634]**	[-0.271]	**[0.417]**		**[0.640]**	[-0.227]
Middle-income	0.209***	-0.047	0.254***	-0.122*	0.124***	0.061	0.147***	-0.021
*Treated	(0.032)	(0.049)	(0.025)	(0.068)	(0.033)	(0.069)	(0.024)	(0.041)
*Taxed	**[0.683]**	[-0.224]	**[0.888]**	**[-0.481]**	**[0.752]**		**[1.001]**	[-0.142]
High-income	0.337***	-0.121***	0.232***	-0.092***	0.032	-0.020	0.029	0.051*
*Treated	(0.021)	(0.043)	(0.030)	(0.027)	(0.032)	(0.028)	(0.027)	(0.027)
*Taxed	**[1.163]**	**[-0.357]**	**[0.929]**	**[-0.396]**	[0.175]	[-0.642]	[0.275]	
Low-income	-0.043**	0.009	0.031	0.022	0.021	0.013	0.080	0.068
*Treated	(0.021)	(0.058)	(0.077)	(0.139)	(0.066)	(0.123)	(0.053)	(0.090)
Middle-income	-0.015	0.144*	0.058	0.223*	-0.004	0.002	-0.240***	-0.232***
*Treated	(0.030)	(0.075)	(0.050)	(0.119)	(0.035)	(0.108)	(0.040)	(0.075)
High-income	0.008	0.133	0.055	0.140	-0.006	0.275	0.050	-0.282***
*Treated	(0.020)	(0.088)	(0.044)	(0.160)	(0.071)	(0.385)	(0.036)	(0.067)
Low-income	0.096***	-0.090***	0.129***	-0.011	0.105***	-0.169**	0.096***	0.036
*Taxed	(0.012)	(0.032)	(0.017)	(0.028)	(0.014)	(0.070)	(0.015)	(0.023)
Middle-income	0.146***	-0.148***	0.097***	-0.023	0.087***	-0.190***	0.056***	-0.039
*Taxed	(0.018)	(0.026)	(0.016)	(0.039)	(0.013)	(0.050)	(0.011)	(0.027)
High-income	0.106***	-0.079***	0.082***	0.007	0.152***	-0.123***	0.073***	-0.051***
*Taxed	(0.015)	(0.029)	(0.010)	(0.014)	(0.018)	(0.033)	(0.005)	(0.014)
Low-income	-0.045*	0.133*	-0.007	0.172*	-0.094***	0.350***	-0.173***	-0.096*
	(0.024)	(0.069)	(0.069)	(0.099)	(0.031)	(0.052)	(0.039)	(0.057)
Middle-income	0.006	0.045	-0.063	0.108	-0.055*	0.366***	0.038	0.077
	(0.026)	(0.074)	(0.042)	(0.107)	(0.029)	(0.096)	(0.032)	(0.058)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Constant	4.378***	1.984***	4.541***	1.793***	4.569***	1.924***	4.815***	2.285***
	(0.016)	(0.054)	(0.027)	(0.086)	(0.021)	(0.049)	(0.017)	(0.032)
# of obs.	5,322,270	5,322,270	765,818	765,818	799,198	799,198	2,063,429	2,063,429

Notes: The dependent variables are the log of weekly price and the log of weekly normalized volume. Low-income, Middle-income, *and* High-income *are dummy variables for the three median household income levels of zip codes where taxed stores are located. Treated is a dummy variable for stores in a taxing jurisdiction. Taxed is a dummy variable for weeks after the tax implementation. Standard errors are in parentheses.* ***, **, *and* * *denote 1%, 5%, and 10% significance levels, respectively.*

Table A.14: Full Regression Results for the Heterogeneous Impact Across the Ethnic Diversity Level for Large Jurisdictions

	Philadelphia		Seattle		Oakland				San Francisco	
	Drug Stores		Drug Stores		Drug Stores		Convenience Stores		Drug Stores	
Variables	(1) Ln (Price)	(2) Ln (Volume)	(1) Ln (Price)	(2) Ln (Volume)	(1) Ln (Price)	(2) Ln (Volume)	(1) Ln (Price)	(2) Ln (Volume)	(1) Ln (Price)	(2) Ln (Volume)
Low-diversity	0.287***	-0.055	0.258***	-0.064*	0.088***	-0.059*	0.120***	-0.006	0.076***	-0.049
*Treated	(0.022)	(0.042)	(0.019)	(0.033)	(0.028)	(0.033)	(0.021)	(0.022)	(0.015)	(0.030)
*Taxed	**[0.953]**	[-0.191]	**[1.042]**	**[-0.249]**	**[0.530]**	[-0.671]	**[1.071]**	[-0.050]	**[0.608]**	[-0.643]
Middle-diversity	0.231***	-0.022	0.230***	-0.118***	0.073**	0.246*	-0.024	0.127***	0.078***	-0.029
*Treated	(0.031)	(0.052)	(0.029)	(0.046)	(0.035)	(0.119)	(0.133)	(0.025)	(0.024)	(0.036)
*Taxed	**[0.766]**	[-0.093]	**[0.838]**	**[-0.515]**	**[0.414]**				**[0.634]**	[-0.377]
High-diversity	0.365***	-0.104**	0.195***	-0.087	0.080*	0.048	0.119***	-0.070	0.084**	-0.051
*Treated	(0.021)	(0.045)	(0.028)	(0.076)	(0.043)	(0.094)	(0.014)	(0.047)	(0.035)	(0.052)
*Taxed	**[1.032]**	**[-0.283]**	**[0.646]**	[-0.444]	**[0.421]**		**[1.086]**	[-0.592]	**[0.661]**	[-0.616]
Low-diversity	-0.022	0.171**	0.009	0.075	-0.030	0.252	-0.044	-0.020	-0.059	-0.134*
*Treated	(0.018)	(0.085)	(0.033)	(0.076)	(0.051)	(0.184)	(0.029)	(0.061)	(0.038)	(0.072)
Middle-diversity	0.002	0.117	-0.001	0.256	0.017	0.048	-0.042	-0.082	-0.081*	-0.171**
*Treated	(0.028)	(0.074)	(0.063)	(0.158)	(0.047)	(0.144)	(0.135)	(0.056)	(0.043)	(0.077)
High-diversity	-0.037*	-0.032	0.109**	0.086	-0.008	0.016	-0.008	-0.254	0.019	-0.036
*Treated	(0.020)	(0.052)	(0.043)	(0.144)	(0.073)	(0.139)	(0.036)	(0.206)	(0.072)	(0.109)
Low-diversity	0.124***	-0.095***	0.074***	-0.035	0.152***	-0.121***	0.169***	-0.226***	0.072***	-0.050***
*Taxed	(0.013)	(0.019)	(0.014)	(0.029)	(0.017)	(0.033)	(0.017)	(0.026)	(0.007)	(0.019)
Middle-diversity	0.118***	-0.110***	0.091***	0.018	0.090***	-0.196***	0.163***	-0.220***	0.076***	-0.038**
*Taxed	(0.020)	(0.034)	(0.021)	(0.028)	(0.011)	(0.041)	(0.015)	(0.014)	(0.013)	(0.015)
High-diversity	0.099***	-0.102***	0.131***	-0.015	0.101***	-0.108	0.171***	-0.228***	0.080***	0.035
*Taxed	(0.011)	(0.037)	(0.009)	(0.031)	(0.015)	(0.083)	(0.017)	(0.047)	(0.013)	(0.026)
Low-diversity	0.097***	-0.142**	0.181***	-0.113	0.126***	-0.386***	0.017	-0.375***	0.152***	0.089
	(0.021)	(0.068)	(0.024)	(0.078)	(0.033)	(0.047)	(0.029)	(0.122)	(0.040)	(0.060)
Middle-diversity	0.066***	-0.020	0.178***	-0.127	0.070**	-0.035	0.031	-0.301**	0.193***	0.252***
	(0.025)	(0.064)	(0.050)	(0.117)	(0.028)	(0.065)	(0.031)	(0.124)	(0.041)	(0.063)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Constant	4.306***	2.103***	4.405***	1.956***	4.443***	2.310***	4.782***	2.417***	4.642***	2.141***
	(0.016)	(0.037)	(0.008)	(0.055)	(0.026)	(0.015)	(0.019)	(0.111)	(0.034)	(0.051)
# of obs.	5,322,270	5,322,270	765,818	765,818	799,198	799,198	478,978	478,978	2,063,429	2,063,429

Notes: The dependent variables are the log of weekly price and the log of weekly normalized volume. Low-diversity, Middle-diversity, *and* High-diversity *are dummy variables for the three ethnic diversity levels of zip codes where taxed stores are located. Treated is a dummy variable for stores in a taxing jurisdiction.*

Taxed is a dummy variable for weeks after the tax implementation. Standard errors are in parentheses. ***, **, *and* * *denote 1%, 5%, and 10% significance levels, respectively.*

Table A.15: Full Regression Results for the Heterogeneous Impact Across the Percentage of African-Americans for Large Jurisdictions

Variables	Philadelphia		San Francisco		Seattle				Oakland			
	Drug Stores		Drug Stores		Drug Stores		Convenience Stores		Drug Stores		Convenience Stores	
	(1) Ln (Price)	(2) Ln (Volume)	(1) Ln (Price)	(2) Ln (Volume)	(1) Ln (Price)	(2) Ln (Volume)	(1) Ln (Price)	(2) Ln (Volume)	(1) Ln (Price)	(2) Ln (Volume)	(1) Ln (Price)	(2) Ln (Volume)
Low-diversity	0.272***	-0.058	0.028*	-0.063***	0.261***	-0.081	0.129***	-0.036*	0.095***	-0.020	0.004	-0.019
*Treated	(0.025)	(0.037)	(0.015)	(0.023)	(0.024)	(0.057)	(0.025)	(0.021)	(0.026)	(0.040)	(0.085)	(0.020)
*Taxed	**[0.883]**	[-0.212]	**[0.239]**	**[-2.281]**	**[1.095]**	[-0.311]	**[0.685]**	**[-0.276]**	**[0.512]**	[-0.207]	[0.035]	[-5.372]
Mid-diversity	0.243***	-0.043	0.114***	-0.001	0.175***	-0.056**	0.229***	-0.139***	0.059	0.287**	0.149***	-0.058
*Treated	(0.030)	(0.059)	(0.026)	(0.036)	(0.023)	(0.029)	(0.011)	(0.032)	(0.037)	(0.115)	(0.018)	(0.056)
*Taxed	**[0.744]**	[-0.178]	**[0.807]**	[-0.006]	**[0.672]**	**[-0.317]**	**[1.211]**	**[-0.605]**	[0.357]		**[1.362]**	[-0.388]
High-diversity	0.365***	-0.104**	0.117***	-0.046	0.240***	-0.135**	0.219***	-0.068	0.114***	0.030	0.106***	-0.086
*Treated	(0.021)	(0.045)	(0.034)	(0.056)	(0.028)	(0.073)	(0.038)	(0.101)	(0.021)	(0.057)	(0.010)	(0.057)
*Taxed	**[1.032]**	**[-0.283]**	**[0.903]**	[-0.391]	**[0.780]**	**[-0.562]**	**[1.107]**	[-0.309]	**[0.638]**		**[0.940]**	[-0.812]
Low-diversity	0.008	0.151*	0.009	-0.264***	0.007	0.023	-0.023	-0.079	-0.065	0.174	0.030	0.030
*Treated	(0.021)	(0.078)	(0.038)	(0.062)	(0.031)	(0.099)	(0.024)	(0.063)	(0.069)	(0.243)	(0.087)	(0.048)
Mid-diversity	-0.031	0.133	-0.172***	-0.238***	0.064	0.199*	-0.064**	-0.036	0.069*	0.095	-0.052	-0.251***
*Treated	(0.026)	(0.083)	(0.045)	(0.080)	(0.067)	(0.117)	(0.029)	(0.063)	(0.035)	(0.152)	(0.032)	(0.076)
High-diversity	-0.037*	-0.032	0.081	0.224**	0.074*	0.127	0.009	0.200**	-0.038	0.075	-0.066	-0.210
*Treated	(0.020)	(0.052)	(0.065)	(0.093)	(0.043)	(0.146)	(0.045)	(0.095)	(0.062)	(0.070)	(0.077)	(0.205)
Low-diversity	0.129***	-0.102***	0.073***	-0.039**	0.073***	-0.022	0.216***	-0.406***	0.153***	-0.120***	0.167***	-0.228***
*Taxed	(0.016)	(0.019)	(0.006)	(0.016)	(0.014)	(0.029)	(0.007)	(0.018)	(0.018)	(0.033)	(0.016)	(0.026)
Mid-diversity	0.113***	-0.115***	0.078***	-0.039**	0.122***	-0.005	0.089***	-0.064***	0.080***	-0.182***	0.157***	-0.214***
*Taxed	(0.019)	(0.038)	(0.008)	(0.020)	(0.017)	(0.026)	(0.007)	(0.017)	(0.017)	(0.054)	(0.012)	(0.012)
High-diversity	0.099***	-0.103***	0.068***	0.030	0.105***	0.001	0.133***	-0.180***	0.106***	-0.174***	0.183***	-0.248***
*Taxed	(0.011)	(0.037)	(0.022)	(0.025)	(0.013)	(0.034)	(0.038)	(0.065)	(0.014)	(0.056)	(0.012)	(0.061)
Low-diversity	0.088***	-0.086	0.189***	0.106*	0.163***	-0.195**	0.125***	0.290***	0.071**	-0.431***	0.010	-0.381**
	(0.022)	(0.061)	(0.044)	(0.058)	(0.025)	(0.091)	(0.034)	(0.057)	(0.033)	(0.069)	(0.032)	(0.169)
Mid-diversity	0.073***	-0.070	0.187***	0.179***	0.107*	-0.047	0.058*	0.081	-0.005	-0.195***	0.014	-0.249
	(0.026)	(0.074)	(0.047)	(0.064)	(0.060)	(0.096)	(0.034)	(0.057)	(0.028)	(0.070)	(0.032)	(0.175)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Constant	4.306***	2.104***	4.623***	2.161***	4.429***	1.961***	4.694***	2.026***	4.498***	2.355***	4.791***	2.425***
	(0.016)	(0.037)	(0.040)	(0.049)	(0.017)	(0.068)	(0.039)	(0.061)	(0.025)	(0.056)	(0.023)	(0.161)
# of obs.	5,322,270	5,322,270	2,063,429	2,063,429	765,818	765,818	765,818	765,818	799,198	799,198	478,978	478,978

Notes: The dependent variables are the log of weekly price and the log of weekly normalized volume. Low-diversity is a dummy variable for zip codes with the lowest percentages of African-Americans. Middle-diversity is a dummy for zip codes at the middle level in terms of the percentage of African-Americans. High-diversity is a dummy for zip codes with the highest percentages of African-Americans. Treated is a dummy variable for stores in a taxing jurisdiction. Taxed is a dummy variable for weeks after the tax implementation. Standard errors are in parentheses. ***, **, *and* * *denote 1%, 5%, and 10% significance levels, respectively.*

Table A.16: Treatment Effects of Soda Taxes on Pure Water or Unsweetened Tea Based on DID Regressions

Variables	Philadelphia (Pure Water)		Seattle (Pure Water)		Oakland (Pure Water)		Berkeley (Pure Water)		Boulder (Pure Water)		San Francisco (Unsweetened Tea)	
	(1) Ln (Price)	(2) Ln (Volume)	(1) Ln (Price)	(2) Ln (Volume)	(1) Ln (Price)	(2) Ln (Volume)	(1) Ln (Price)	(2) Ln (Volume)	(1) Ln (Price)	(2) Ln (Volume)	(1) Ln (Price)	(2) Ln (Volume)
Year 1 *Treated	-0.007 (0.014)	0.023 (0.030)	0.012 (0.009)	0.008 (0.019)	0.016 (0.020)	0.081 (0.049)	-0.015 (0.013)	0.064 (0.066)	0.002 (0.034)	0.032 (0.052)	-0.008 (0.016)	-0.045 (0.034)
Year 2 *Treated	-0.010 (0.018)	0.000 (0.056)	0.006 (0.023)	0.057 (0.046)	-0.004 (0.029)	-0.006 (0.095)	0.015 (0.020)	0.017 (0.081)	0.009 (0.059)	-0.106 (0.196)	-0.008 (0.024)	-0.015 (0.040)
Year 3 *Treated	-0.034 (0.029)	-0.007 (0.086)			0.008 (0.040)	0.001 (0.097)	0.015 (0.022)	0.018 (0.089)	0.009 (0.075)	0.006 (0.184)		
Year 4 *Treated							-0.001 (0.023)	-0.012 (0.105)				
Year 5 * Treated							0.005 (0.027)	0.009 (0.113)				
Treated	0.069*** (0.021)	0.056 (0.077)	0.046* (0.025)	0.251 (0.178)	-0.005 (0.057)	-0.022 (0.172)	0.006 (0.022)	0.153 (0.148)	0.268 (0.091)	-0.126 (0.199)	-0.122*** (0.040)	0.014 (0.067)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Constant	3.704*** (0.021)	3.318*** (0.071)	3.812*** (0.016)	2.776*** (0.105)	3.828*** (0.040)	3.374*** (0.172)	3.925*** (0.022)	3.049*** (0.105)	3.739*** (0.077)	3.057*** (0.123)	4.545*** (0.026)	2.374*** (0.039)
# of obs.	2,577,265	2,577,265	813,077	813,077	1,080,020	1,080,020	410,581	410,581	441,291	441,291	446,945	446,945

Notes: The dependent variables are the log of weekly price and the log of weekly normalized volume. Year1 – Year5 are dummy variables for years after the tax implementation. Treated is a dummy variable for stores in a taxing jurisdiction. I also estimated the treatment effect of San Francisco's tax on pure water. The estimation results showed that the impact on pure water was significant in San Francisco. Thus, I used unsweetened tea in the triple-difference estimations for San Francisco as the tax had an insignificant impact on the sales of unsweetened tea. Standard errors are in parentheses. ***, **, *and* * *denote 1%, 5%, and 10% significance levels, respectively.*

Table A.17: Full Regression Results for the Average Impact on Taxed Beverages After the Tax Implementation Based on Triple-Difference Regressions

Variables	Philadelphia		Seattle		Oakland		Berkeley		San Francisco		Boulder	
	(1) Ln (Price)	(2) Ln (Volume)	(1) Ln (Price)	(2) Ln (Volume)	(1) Ln (Price)	(2) Ln (Volume)	(1) Ln (Price)	(2) Ln (Volume)	(1) Ln (Price)	(2) Ln (Volume)	(1) Ln (Price)	(2) Ln (Volume)
Year 1 *Treated *Taxed Product	0.303***	-0.192***	0.153***	-0.079***	0.051**	-0.064	0.020	-0.061	0.046**	0.037	0.104**	-0.059
	(0.017)	(0.027)	(0.011)	(0.024)	(0.023)	(0.057)	(0.024)	(0.048)	(0.018)	(0.031)	(0.041)	(0.043)
	[0.725]	**[-0.634]**	**[0.556]**	**[-0.519]**	**[0.205]**	[-1.251]		[-3.061]	**[0.314]**		**[0.373]**	[-0.562]
Year 2 *Treated *Taxed Product	0.366***	-0.237***	0.190***	-0.152***	0.075***	-0.009	0.029	-0.086	0.092***	0.007	0.135***	0.001
	(0.024)	(0.050)	(0.017)	(0.044)	(0.020)	(0.078)	(0.034)	(0.066)	(0.026)	(0.040)	(0.027)	(0.111)
	[0.876]	**[-0.647]**	**[0.689]**	**[-0.801]**	**[0.299]**	[-0.122]		[-2.906]	**[0.622]**		**[0.486]**	
Year 3 *Treated *Taxed Product	0.400***	-0.243***			0.086***	-0.030	-0.005	-0.104			0.143***	-0.075
	(0.023)	(0.070)			(0.028)	(0.079)	(0.038)	(0.078)			(0.034)	(0.102)
	[0.957]	**[-0.609]**			**[0.343]**	[-0.352]					**[0.512]**	[-0.525]
Year 4 *Treated *Taxed Product							0.065**	-0.162*				
							(0.038)	(0.098)				
							[0.341]	**[-2.486]**				
Year 5 *Treated *Taxed Product							0.154***	-0.183**				
							(0.048)	(0.093)				
							[0.804]	**[-1.187]**				
Year1 *Taxed Product	0.051***	-0.032**	0.001	-0.008	0.015	-0.066***	-0.021	0.055*	-0.036***	-0.073***	0.017	-0.023
	(0.009)	(0.014)	(0.006)	(0.013)	(0.012)	(0.021)	(0.021)	(0.031)	(0.012)	(0.020)	(0.018)	(0.023)
Year2 *Taxed Product	0.097***	-0.060***	-0.131***	0.149***	-0.018	0.007	-0.030	0.036	-0.056***	0.026	-0.075***	0.050*
	(0.012)	(0.022)	(0.013)	(0.028)	(0.015)	(0.048)	(0.026)	(0.053)	(0.015)	(0.021)	(0.015)	(0.029)
Year3 *Taxed Product	-0.093***	0.161***			-0.100***	0.119**	0.014	-0.007			-0.112***	0.161***
	(0.016)	(0.040)			(0.024)	(0.053)	(0.032)	(0.074)			(0.021)	(0.039)
Year4 *Taxed Product							0.029	0.156*				
							(0.034)	(0.085)				

Year5 *Taxed Product							-0.162^{***}	0.259^{**}				
							(0.044)	(0.095)				
Year1 * Treated	-0.0003	0.015	0.012	0.008	0.016	0.081^{*}	-0.014	0.060	-0.008	-0.041	-0.003	0.037
	(0.014)	(0.030)	(0.009)	(0.019)	(0.019)	(0.046)	(0.013)	(0.065)	(0.016)	(0.034)	(0.036)	(0.053)
Year2 * Treated	-0.003	-0.007	0.006	0.057	-0.004	-0.008	0.016	0.015	-0.008	-0.011	0.013	-0.121
	(0.018)	(0.056)	(0.023)	(0.046)	(0.029)	(0.100)	(0.019)	(0.080)	(0.024)	(0.040)	(0.060)	(0.202)
Year3 * Treated	-0.027	-0.015			0.008	-0.0003	0.016	0.016			0.013	-0.008
	(0.030)	(0.086)			(0.040)	(0.101)	(0.021)	(0.088)			(0.076)	(0.191)
Year4 * Treated							0.001	-0.015				
							(0.023)	(0.104)				
Year5 * Treated							0.006	0.006				
							(0.026)	(0.112)				
Treated * Taxed Product	0.057^{***}	-0.271^{***}	0.040	-0.094	-0.065	0.172	0.027	-0.269^{***}	0.062^{**}	-0.046	0.079	0.026
	(0.016)	(0.048)	(0.033)	(0.081)	(0.047)	(0.139)	(0.037)	(0.084)	(0.027)	(0.042)	(0.058)	(0.085)
Treated	0.062^{***}	0.064	0.046^{*}	0.251	-0.005	-0.021	0.005	0.156	-0.122^{***}	0.010	0.265^{***}	-0.111
	(0.021)	(0.077)	(0.025)	(0.178)	(0.057)	(0.172)	(0.022)	(0.147)	(0.040)	(0.067)	(0.091)	(0.198)
Taxed Product	0.470^{***}	-0.642^{***}	0.679^{***}	-0.774^{***}	0.640^{***}	-0.960^{***}	0.655^{***}	-0.587^{***}	0.104^{***}	0.037	0.613^{***}	-0.939^{***}
	(0.010)	(0.032)	(0.026)	(0.050)	(0.035)	(0.061)	(0.026)	(0.074)	(0.018)	(0.023)	(0.055)	(0.058)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Constant	3.571^{***}	3.425^{***}	3.791^{***}	2.818^{***}	3.798^{***}	3.460^{***}	3.927^{***}	3.055^{***}	4.539^{***}	2.382^{***}	3.728^{***}	3.126^{***}
	(0.019)	(0.066)	(0.015)	(0.107)	(0.041)	(0.163)	(0.020)	(0.101)	(0.021)	(0.032)	(0.074)	(0.123)
# of obs.	16,749,049	16,749,049	2,609,731	2,609,731	3,803,721	3,803,721	1,387,745	1,387,745	8,250,163	8,250,163	1,550,900	1,550,900

Notes: The dependent variables are the log of weekly price and the log of weekly normalized volume. Year1 – Year5 are dummy variables for years after the tax implementation. Treated is a dummy variable for stores in a taxing jurisdiction. Standard errors are in parentheses. ***, **, *and* * *denote 1%, 5%, and 10% significance levels, respectively.*

B: Hypothesis Test Results

Table B.1: Linear Hypothesis Test for Difference in Price Coefficients for Different Store Types in Philadelphia and Seattle

Null Hypothesis	Philadelphia		Seattle	
	Chi-squared	P-values	Chi-squared	P-values
Drug Stores = Mass Merchandisers	5.99	0.01	7.75	0.01
Drug Stores = Grocery Stores	10.90	0.00	-	-
Mass Merchandisers = Grocery Stores	13.05	0.00	-	-
Drug Stores = Convenience Stores	-	-	0.33	0.57
Convenience Stores = Mass Merchandisers	-	-	3.04	0.08

Notes: I conducted a linear hypothesis test for each pair of store types in Philadelphia and Seattle and the null hypothesis is that the DID coefficients for two store types reported in Table 7.6 are equal to each other. I reported the chi-squared statistics and p-values separately. When the p-values are smaller than 0.1, it means the two coefficients are statistically different.

Table B.2: Linear Hypothesis Test for Difference in Price Coefficients for Different Ethnic Diversity Levels in Philadelphia

Null Hypothesis	Philadelphia	
	Chi-squared	P-values
Low-Diversity Level = Middle-Diversity Level	1.36	0.24
Low-Diversity Level = High-Diversity Level	3.74	0.05
Middle-Diversity Level = High-Diversity Level	7.72	0.01

Notes: I conducted a linear hypothesis test for each pair of ethnic diversity levels in Philadelphia and the null hypothesis is that the DID coefficients for two diversity levels reported in Table 7.12 are equal to each other. I reported the chi-squared statistics and p-values separately. When the p-values are smaller than 0.1, it means the two coefficients are statistically different.