The Impact of Soda Taxes: Pass-through, Tax Avoidance, and Nutritional Effects*

Stephan Seiler
UCLA

Anna Tuchman
Northwestern University

Song Yao
Washington University in St. Louis

This draft: October 26, 2019

We analyze the impact of a tax on sweetened beverages, often referred to as a "soda tax," using a unique dataset of prices, quantities sold, and nutritional information across several thousand taxed and untaxed beverages for a large set of stores in Philadelphia and its surrounding area. We find the tax is passed through at an average rate of 97%, leading to a 34% price increase. Demand in the taxed area decreases by 46% in response to the tax. We find no significant substitution to bottled water and modest substitution to (untaxed) natural juices. A large amount of cross-shopping to stores outside of Philadelphia off-sets more than half of the reduction in sales in the city and reduces the net decrease in sales of taxed beverages to only 22%. Among taxed beverages, demand decreases more strongly for relatively healthier products. Due to cross-shopping and compositional changes in demand, we find that calories and sugars decrease by only 16% and 15% (p-values of 0.07 and 0.09). Based on these findings, we discuss implications for tax policy design.

Keywords: Sin Taxes, Pass-Through, Tax Avoidance, Policy Evaluation, Tax Design.
JEL codes: D04, D12, I18.

*We thank Hunt Allcott, Eric Anderson, Michael Best, Bryan Bollinger, Chris Conlon, Brett Gordon, Brett Hollenbeck, Sylvia Hristakeva, Carl Mela, Martin O'Connell, Peter Rossi, Bradley Shapiro, Yossi Spiegel, and Dmitry Taubinsky for insightful comments, as well as seminar participants at Boston University, CKGSB, Cornell University, Federal Trade Commission, INSEAD, Northwestern University, NYU, Santa Clara University, Stanford, UCL, UC Riverside, University of Minnesota, University of Rochester, University of Washington, Washington University in St. Louis, and Yale SOM, and participants at the 2017 Marketing in Israel, 2018 Yale Customer Insights, 2018 INFORMS Marketing Science, and 2019 QME conferences for useful comments. Yuanchen Su and Weihong Zhao provided excellent research assistance. Thanks also to Piyush Chaudhari and the team at IRI for providing the data used in our analysis. All estimates and analyses in this paper based on Information Resources, Inc. data are by the authors and not by Information Resources, Inc. Please contact Seiler (stephan.a.seiler@gmail.com), Tuchman (anna.tuchman@kellogg.northwestern.edu), or Yao (songyao@wustl.edu) for correspondence. None of the authors received external funding for this paper.

1 Introduction

The US has the highest rate of obesity among all developed countries (OECD (2017)). According to the Centers for Disease Control and Prevention (CDC), 36% of Americans are clinically obese and another third are overweight (Ogden et al. (2015)). The increasing prevalence of obesity in the US has become a serious public health concern because obesity has been linked to adverse health outcomes, including heart disease, type 2 diabetes, stroke, and certain cancers. In 2008, the estimated annual medical cost of obesity in the US was $147 billion (CDC (2016), Finkelstein et al. (2009)). Due to the prevalence of obesity in the US, taxes on sugar-sweetened beverages (SSBs) have recently gained in popularity. SSBs have been singled out for taxation because sugary drinks are the single largest source of added sugar in the average American's diet (National Cancer Institute (2018)). Berkeley, CA, was the first municipality to implement a 1-cent-per-ounce tax in March 2015. More recently, other cities and counties have implemented similar taxes, including Philadelphia, PA, Cook County, IL (covering Chicago and most of its suburbs)[1], San Francisco, CA, Boulder, CO, and Seattle, WA. In the case of Philadelphia and Cook County, not just sugar sweetened beverages, but all beverages with *any* added sweetener, are taxed. Several other cities (e.g., Washington, D.C., New York, NY, and Portland, OR) and the state of Connecticut have contemplated introducing similar taxes, and hence understanding their impact is important when considering whether and how to implement such taxes.

In this paper, we use the case of Philadelphia as a testbed for understanding the impact of a tax on sweetened beverages. Philadelphia presents a particularly rich setting to study a sweetened-beverages tax, because it is a large and demographically diverse city that is served by many different types of stores and chains. We base our analysis on a unique panel dataset that covers sales and prices of thousands of taxed and untaxed beverages at several hundred stores, ranging from small convenience stores to wholesale clubs. We complement these data with local demographic information and hand-coded product-level nutritional information. To fully understand the impact of the tax, we analyze its impact along various adjustment margins. The tax, which is levied at the distributor level, might not necessarily be passed through to consumers. Furthermore, consumers might substitute to untaxed beverages, or they might engage in tax avoidance by substituting purchases of sweetened drinks from Philadelphia stores to stores outside the taxed zone. The overall impact of the tax on nutritional intake as well as the ability to generate revenue depends on these various margins of adjustment. Therefore, to paint a complete picture of the impact of the tax, we analyze price and demand responses for taxed products, as well as substitutes in geographic and product space. Our analysis is based on a difference-in-differences framework that compares changes before and after the tax took effect in Philadelphia relative to a control group of stores outside of Philadelphia (we only include stores located at least 6 miles outside Philadelphia in the control group).[2]

Several key findings emerge from our analysis: (1) The tax is passed through at an average

[1]The Cook County, IL, tax went into effect in August 2017 and was repealed four months later in December 2017.
[2]We provide evidence later in this paper that this distance threshold is appropriate.

rate of 97%, which corresponds to a 34% price increase. Pass-through is remarkably similar across different types of stores, chains, and products. (2) The large increase in prices leads to a 46% reduction in the quantity purchased of taxed beverages in Philadelphia. However, over half of this reduction is offset by an increase in quantity purchased at stores up to 6 miles outside of the city border. After taking into account cross-shopping, we find that net sales of taxed beverages only fell by 22%. (3) We find no significant change in demand for bottled water, but a modest increase in sales of (untaxed) natural juices. Within the set of taxed products, demand decreases more for relatively healthier products. Due to compositional changes in the demand for beverages (as well as cross-shopping), the change in calorie and sugar intake is smaller than the quantity reduction and the estimated reduction falls just short of statistical significance at the 5% level (p-values of 0.07 and 0.09). The estimated reduction in calories is 16% with a 95% confidence interval of [-33%, 1%] and the estimated reduction in sugars is 15% with a 95% confidence interval of [-33%, 2%]. (4) Purchase quantity decreases less in low-income neighborhoods. This finding suggests low-income consumers either reduce their consumption of taxed beverages less, or they are less likely to engage in cross-shopping.

We draw several lessons about the effectiveness of the Philadelphia tax from these analyses. First, the tax was less effective at reducing consumption of unhealthy products due to tax avoidance through cross-shopping and compositional changes in demand toward relatively less healthy products. Second, in terms of revenue generation, the tax was only partly effective, due to consumers substituting to stores outside of Philadelphia. Third, low-income households are more likely to continue to purchase taxed products at a higher price at stores in Philadelphia. The lower propensity for low-income households to reduce consumption and/or avoid the tax through cross-shopping leads to a relatively larger tax burden for those households. In summary, the tax is limited in its ability to improve calorie and sugar intake and to raise revenue, and it affects low-income households more severely.

Based on our analysis of the Philadelphia tax, we discuss implications for the design of soda taxes more broadly. We show that the soda tax rate cannot be set very high if the tax is levied locally and the objective is to maximize tax revenue, because cross-shopping leads to a high elasticity of demand in the taxed area. Furthermore, we analyze the consequences of changing the tax base by widening the geographic area or narrowing the range of products being taxed. Several patterns in the data suggest that if the tax were levied over a larger area and cross-shopping became more difficult, consumers would continue to buy taxed beverages rather than switching to healthier options. When taxing only SSBs, a larger degree of substitution to diet drinks would be likely. Therefore, widening the geographic area would help raise more tax revenue, whereas narrowing the range of taxed products would help improve nutritional intake at the expense of lower tax revenue.

Our work contributes to the growing body of research that seeks to evaluate the effects of soda taxes on consumption decisions, firm pricing, and consumer health. A first set of papers relevant to our analysis are studies that use structural models and pre-tax data to predict the impact of a (hypothetical) soda tax (Wang (2015), Kifer (2015), Dubois et al. (2017), Allcott et al. (2019a)).[3] These papers vary in their estimated demand elasticities and their assumptions with regard to pass-through, and hence they differ in their conclusions about the effectiveness of soda taxes. A second set of studies analyzes the impact of soda taxes after their implementation. Within the US context, the most well-studied tax is the one implemented in Berkeley in 2014.[4] A series of papers studies price pass-through and quantity reaction using manually collected prices (Falbe et al. (2015), Cawley and Frisvold (2017)), survey-based measures of consumption (Falbe et al. (2016)), and scanner data on prices and quantities sold (Silver et al. (2017), Rojas and Wang (2017), Bollinger and Sexton (2018)). They find pass-through rates between 25% and 47% depending on the data source and methodology employed.[5] With regards to the estimated quantity reduction, the effect is small and insignificant in some studies and as large as 21% in others. In the case of Boulder, Cawley et al. (2018a) document a somewhat higher pass-through rate of 81%.

More recently, several studies focus on evaluating the soda tax in Philadelphia. Some early studies use manually collected prices and consumption surveys (Cawley et al. (2018d), Cawley et al. (2018b), Cawley et al. (2018c)). In line with our findings, these papers find the tax is fully passed through, but they find no significant reduction in demand at stores in Philadelphia. A recent paper by Roberto et al. (2019) uses store scanner data to analyze the impact of Philadelphia's soda tax on prices and volume sales,[6] although unlike our paper, it does not consider policy-relevant issues such as nutritional outcomes and the distributional effects of the tax across income groups. Moreover, many of this paper's key findings differ substantially from ours and lead to very different conclusions with regards to the effectiveness of the tax. These differences are likely due to Roberto et al. (2019)'s relatively limited (and non-representative) sample: they observe a more limited set of stores (grocery stores, mass merchants, and drug stores) and only analyze substitution to stores in Pennsylvania within 3 miles outside of the city border. Our cross-shopping effects are twice as large as theirs because our analysis allows for (and finds evidence of) cross-shopping at stores up to 6 miles outside of Philadelphia in both Pennsylvania and New Jersey.[7] Furthermore, to the best of our knowledge, our paper is the first to directly study nutritional outcomes (calories and sugars). Crucially, we find nutritional effects do not track quantity changes one-for-one because of compositional changes in demand. As a result, calories and sugars only decrease by 16% and

[3] Khan et al. (2016) and Griffith et al. (2018) apply a similar approach to analyze the impact of a tax on fat content.

[4] Another set of papers study soda taxes outside of the US. Grogger (2017), Aguilar et al. (2016), and Colchero et al. (2017) investigate the effects of SSB taxes in Mexico. Berardi et al. (2016) and Bergman and Hansen (2017) analyze soda taxes in France and Denmark. Relative to the more localized taxes in the US, the national implementation of these taxes makes inference more difficult because there is no obvious control group of stores that are unaffected by the tax.

[5] The studies based on scanner data are based on a larger set of products but a smaller set of stores. These differences in coverage might explain the differences in measured pass-through rates.

[6] We publicly posted our working paper on SSRN in December 2018. Roberto et al. (2019) did not post a working paper version of their draft online. We first became aware of their work when the paper was published in May 2019.

[7] Roberto et al. (2019) also find a price increase of 12% to 24% depending on store format, whereas we find a larger price change of 34%, which corresponds to a 97% pass-through rate and is in line with the evidence from manually collected prices (Cawley et al. (2018b)). Differences in estimates of both price and quantity response lead to drastically different elasticities of -1.7 in Roberto et al. (2019) relative to -0.6 in our data.

15%, respectively. Our findings therefore have very different implications for future policy. While Roberto et al. (2019) interpret their estimated 38% quantity decrease as evidence for an effective soda tax policy, our evaluation is much more cautious because we estimate a smaller nutritional impact, because large cross-shopping effects hinder the tax's revenue generation and health goals, and because low income consumers shoulder more of the tax's financial burden.[8]

Our comprehensive and representative retail scanner data from Philadelphia provide a rich setting to study the impact of a soda tax. First, our data contain a large set of 357 stores from 11 different chains in Philadelphia (compared to the 10 stores from three chains in the Nielsen-Kilts store-level data for Berkeley), and our data cover all beverages sold at these stores. In addition, our data is representative of the universe of stores in terms of geographic and format coverage (see Appendix C). Second, Philadelphia is a large and demographically diverse city. Both aspects together allow us to explore heterogeneity across stores, chains, and consumer demographics in more detail. Furthermore, Philadelphia represents a useful testbed for studying soda taxes, because its demographic composition is similar to the US average. Sixty-eight percent of Philadelphia's 1.5 million residents are considered overweight or obese (CDC (2013)), which is close to the rate of 66% for the entire US and much higher than the rate of 36% in Berkeley.[9]

Our analysis is also related to the literature on various other sin taxes such as taxes on alcohol (e.g. Conlon and Rao (2015), Conlon and Rao (2016), Miravete et al. (2017), Miravete et al. (2018)), cigarettes (e.g. Lovenheim (2008), Merriman (2010), Harding et al. (2012)), and cannabis (e.g. Jacobi and Sovinsky (2016), Hollenbeck and Uetake (2018)). Within this broader literature, our analysis is particularly relevant to the analysis of pass-through (Conlon and Rao (2016), Hollenbeck and Uetake (2018), Miravete et al. (2018)) and tax avoidance through cross-shopping (Ferris (2000), Asplund et al. (2007), Lovenheim (2008), Merriman (2010)).

The rest of this paper is organized as follows. In section 2, we provide additional details on the tax's implementation, and in section 3, we describe the data and provide descriptive statistics. Section 4 presents the empirical approach and provides results for the impact of the tax on prices and quantities of taxed and untaxed products, on cross-shopping behavior, and on nutritional intake. Section 5 discusses the implications of our findings for the design of soda taxes. Section 6 concludes.

2 Institutional Context

In March 2016, Philadelphia Mayor Jim Kenney proposed a tax of 3 cents per ounce on sweetened beverages. After months of debate around the proposal, the Philadelphia City Council voted on a scaled-down version of the tax in June 2016, and approved it with a vote of 13-4. A tax of 1.5

[8]We also provide a host of additional analysis not included in Roberto et al. (2019) such as an analysis of heterogeneity across product categories, pack-sizes, and nutritional content as well as an analysis of the implications of our findings for tax policy design.

[9]City of Berkeley, "Health Status Report", City of Berkeley Public Health Division (2013). https://www.cityofberkeley.info/Health_Human_Services/Public_Health/2013_Health_Status_Report.aspx, accessed on January 20, 2018.

cents per ounce went into effect on January 1, 2017. According to a spokeswoman for the mayor, Kenney's primary reason for proposing the tax was to raise tax revenue, but she noted that the tax could also bring health benefits if it reduces the consumption of sweetened beverages (Esterl (2016)). In Philadelphia, pre-implementation projections predicted the tax would raise $92 million in tax revenue in 2017.[10] In practice, the city collected $79 million in 2017, falling short of the projection.

In terms of implementation, the tax is structured as a tax of 1.5 cents per ounce, which, for example, amounts to a tax of $1.01 on a 2-liter bottle. In our data, the average pre-tax price of a 2-liter of soda in Philadelphia is $1.56; thus, the tax is equal to almost two thirds of the pre-tax price of this product. Note that the tax is levied on distributors, not directly on consumers. Thus, the extent to which consumers feel the tax depends on how much of the tax is passed through the supply chain. Finally, Philadelphia's tax applies to both sugar-sweetened and artificially-sweetened beverages. Thus, both diet and regular soft drinks are taxed, as well as pre-sweetened coffee and tea drinks, sports drinks, energy drinks, and non-100% fruit drinks that contain a caloric sweetener or non-nutritive sweetener.[11]

The decision to tax artificially sweetened beverages might seem surprising. From a health perspective, if the goal is to reduce calories consumed, taxing diet drinks that are a close substitute to SSBs could be counter-productive. In the case of Philadelphia, the mayor's office has acknowledged the primary purpose of the tax is to raise tax revenue, and hence the decision to include artificially-sweetened drinks was likely driven by financial motivations. Also note that many other municipalities that introduced similar taxes (several Bay Area cities, Boulder, CO, and Seattle, WA) only tax drinks with caloric sweeteners.

3 Data

We analyze retail point-of-sale data collected by IRI, a large market-research firm.[12] We supplement these data with nutrition information on products and demographic data. Each of these datasets is described in more detail below.

3.1 Data sources

Retail Point-of-Sale Data The data cover the period from January 2015 through September 2018 and contain information on prices[13] and quantity sold at the UPC/store/week level. We

[10]The funds are earmarked to go to pre-K education programs, community schools, and improvements to parks, libraries, and recreation centers.

[11]Examples of caloric sweeteners include cane sugar, high fructose corn syrup, and honey. Examples of non-nutritive sweeteners include stevia, aspartame, sucralose, or saccharin. Drinks that are exempt from the tax include alcoholic beverages, beverages that are 100% juice, and drinks that are more than 50% milk by volume.

[12]IRI and the Nielsen Company are the two major producers of retail point-of-sale data in the US. Both companies compile price, sales and item description data from the scanner systems of cooperating retail outlets.

[13]We compared our pricing data to a sample of manually collected prices in Philadelphia stores in order to verify that the price recorded in our data does include the tax. We found this to be the case for all but one retailer. At this retailer, the shelf price does include the tax, but the checkout receipt reports the tax as a separate line item.

obtained data for all beverage categories, including untaxed beverages, which constitute potential substitutes. We observe the location and chain affiliation for each store.[14] We focus our analysis on stores located in the city of Philadelphia and the four 3-digit ZIP codes that surround Philadelphia. We restrict the sample to stores that (i) entered the panel before January 1, 2016, and were tracked through at least December 31, 2017, and (ii) that belong to one of the 11 chains / groups of stores that operate stores both within the city and outside of the city. Our final dataset includes 357 stores located in Philadelphia and 870 stores located in the surrounding area around Philadelphia.[15] Panel B of Table 1 lists the types of stores (grocery stores, drugstores, etc.) covered in our data and the number of stores observed for each chain. Figure 1 shows the geographic location of all stores. Philadelphia stores are shown in blue, whereas stores 0-6 miles outside the city limits are shown in green, and stores more than 6 miles outside the city are shown in red. More detailed descriptive statistics are provided in the next section. Note that our data cover sales at retail stores. We do not observe sales of taxed beverages at vending machines, restaurants or bars. Thus, our analysis should be interpreted as measuring the effect of the tax on purchases made in the retail sector.

At the most granular level, the data record sales at the UPC/store/week level. Across all stores and weeks, we observe a total of 17,582 individual UPCs due to the fact that many products are sold in various pack sizes and flavors. In our empirical analysis, we use data at a higher level of aggregation. We define a product as a brand/diet-status/pack-size combination and aggregate the UPC-level data up to this level, calculating total units sold and quantity-weighted prices at the product/store/week level. Thus, different flavors of the same brand (e.g., Cherry Coke and Vanilla Coke) are aggregated together.[16] After dropping infrequently purchased products for which prices are often missing, we are left with a total of 861 products (489 taxed and 372 untaxed).[17] We then further aggregate the data from the product/store/week level to the tax-status/store/week level. That is, we compute total quantity sold and average price separately for all taxed products and all untaxed products. We compute tax-status/store/week-level prices as a weighted average of product-level prices, where the weights are equal to market shares (in terms of total ounces sold) of products at each store in the pre-tax period. Total volume sold is obtained by aggregating product/store/week-level volume up to the tax-status/store/week level. When analyzing hetero-

Consequently the IRI data records price net of the tax for this retailer (see Appendix B for more details and sample receipts). In order to recover this retailer's shelf price, which is the effective price paid, we add the 1.5 cents/oz tax to the prices recorded in our data for this retailer beginning in January 2017. We note that we were able to discover this price difference because we observe retailer identities, which are typically unavailable for researchers working with IRI or similar data.

[14]For most stores, we observe the exact street address of each store and the exact chain affiliation. For the remaining stores, we only observe the location at the 5-digit ZIP-code level and the retailer type (Mass Merchant, Dollar Store, or Convenience Store). For the latter set of stores, we assume they are located at the centroid of their ZIP code. We ran robustness checks that exclude stores with noisy location information for all regressions that involve distance variables and found results to be similar in all cases. When performing analyses at the chain level, we treat the unidentified mass merchants, dollar stores, and convenience stores as separate groups. We anonymize the chain affiliation per the request of our data provider.

[15]We provide details on how we select stores and UPCs in section A of the Appendix.

[16]Different flavors of the same brand are typically priced uniformly, and hence little information is lost when aggregating prices at this level.

[17]See section A in the Appendix for details regarding the data cleaning.

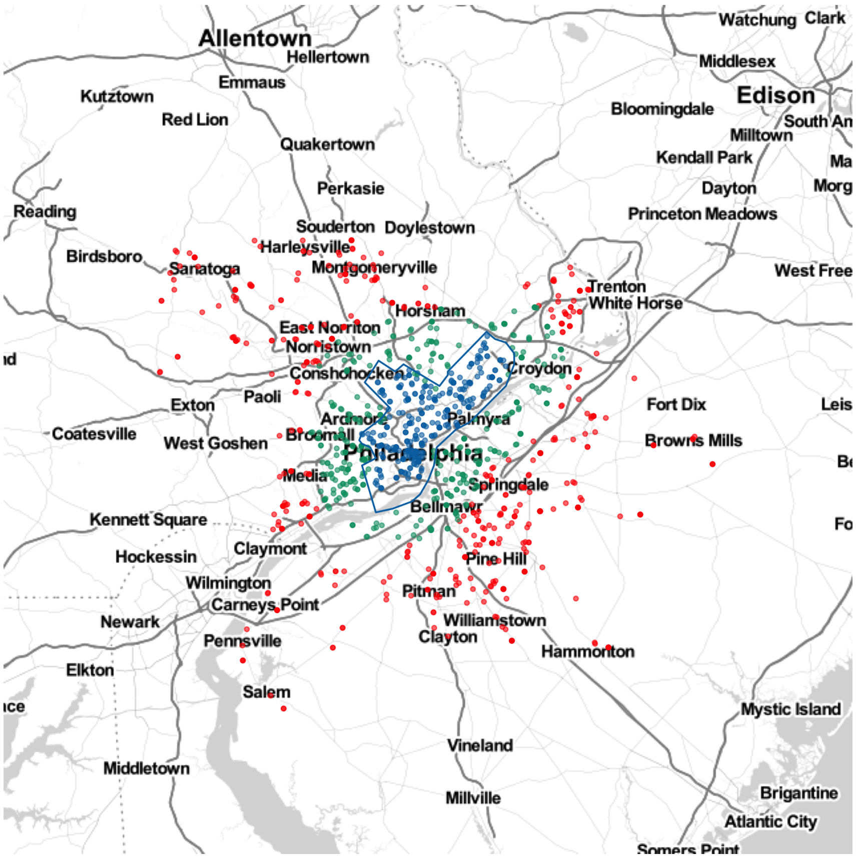

Figure 1: **Stores Within and Outside of Philadelphia.**

geneity in the response to the tax, we separately compute (at the store/week level) average prices and total sales volume for different product pack sizes, for products with different calorie and sugar content, for individual categories (e.g., soda, energy drinks, water, etc.), and for different store formats.

Finally, we note that IRI does not track the full universe of stores in Philadelphia, but it does track a relatively large share of stores. In section C of the appendix, we show that the set of stores in our data is representative along several key dimensions, namely, in terms of geographic coverage and coverage across different store formats.

Demographic Data We supplement the store-level sales data with highly localized demographic data. These data allow us to determine the demographics of each store's catchment area and explore whether the response to the tax varies as a function of local population characteristics. We obtained data on median household income from the Census Bureau's 2011-2015 American Community Survey (ACS), and data on obesity rates from the CDC (Manson et al. (2017)).[18]

[18]The CDC reports model-based estimates of obesity rates at the census-tract level as part of their 500 Cities Project. Further detail on the CDC's methodology is available in its report, "500 Cities Project: Local Data for Better Health. Philadelphia, PA. 2014" (CDC/NCCDPHP/DPH/ESB (2016)). Obesity data are only available for census tracts within the city of Philadelphia. We do not observe data for tracts in our control regions.

Both datasets vary at the census-tract level.[19] We focus on these two sociodemographic measures because (i) past work suggests income may be correlated with price sensitivity and preference for sweetened beverages (Wang (2015)) and (ii) because obesity data allow us to analyze whether the consumers who could reap the largest health benefits from reducing consumption actually do so. We assign demographic profiles to individual stores by calculating average income and obesity rates in each store's catchment area. To this end, we identify all census tracts that are within a 1-mile radius of each store in our data, and calculate (population-weighted) average demographics for each store.[20]

Ingredient and Nutrition Data Finally, we manually collect ingredient and nutrition information on all taxed and untaxed products contained in the retail sales data.[21] For each product, we search for the list of ingredients and the nutrition facts label on the manufacturer and retailers' websites, and we record grams of sugars and calories for each UPC in the data. These data serve two purposes. First, the retail sales data do not have a field that indicates whether each product is subject to Philadelphia's tax. We use the ingredients list to determine the taxed status of each product. Second, we use the nutrition data to evaluate the overall effect of the tax on sugar and calorie consumption.

3.2 Descriptive Statistics

Panel A in Table 1 provides descriptive statistics on the categories included in our data. As described earlier, despite the fact that the tax is often referred to as a "soda tax," it applies to all beverages that contain added sugar or an artificial sweetener (e.g., diet Mountain Dew made with sucralose and diet Snapple Peach Tea made with aspartame). We report market shares based on pre-tax sales in ounces in the first row of Panel A. Among taxed products, soda makes up about one third of all purchases, followed by juice and tea/coffee. Energy drinks, sports drinks, and taxed water (e.g., sweetened "Vitamin Water") make up a smaller market share. We provide a list of sample brands in each category at the bottom of Panel A. Notably, the three larger taxed categories are similar in terms of pricing and nutritional content. Sports drinks and taxed water are healthier and more expensive. Energy drinks are significantly more expensive.

Two types of beverages are not taxed. Out of those two, pure water constitutes the bulk of purchases in the pre-tax period. The second category is natural juices.[22] They make up only 11%

[19]The city of Philadelphia has 384 census tracts. Census tracts cover, on average, 4,000 people, with individual census tracts ranging between 2,500 and 8,000 inhabitants.

[20]We also experiment with a 2-mile radius when computing store demographics and find results to be similar.

[21]We are not able to locate nutrition information for all products in the data. Private-label products are especially challenging because we do not observe the name of the private-label brand. We drop products from our analysis if we are unable to obtain their ingredients list in order to confirm whether they are subject to the tax (such products make up less than 2% of the market share). If we can confirm a product's taxed status, but are not able to find its exact nutrition information, we fill in the nutrition information for that product with the average across similar products produced by the same brand (such imputation is necessary for products that make up 4.8% of the market share).

[22]Juice products from concentrate are included in this untaxed category as long as the sugar content is comparable

Panel A: Category Level	*Taxed Categories*						*Untaxed Categories*	
Market Share	0.457						0.543	
	Soda	Taxed Juice	Tea / Coffee	Sports Drinks	Taxed Water	Energy Drinks	Pure Water	Natural Juice
Market Share (Within Taxed / Untaxed Categories)	0.352	0.256	0.224	0.108	0.031	0.030	0.891	0.109
Price: Cents/Oz	3.71	3.50	3.74	4.53	5.13	19.32	1.35	6.37
Grams Sugar/Oz	2.65	2.71	2.33	1.62	0.71	2.22	0	2.98
Calories/Oz	9.95	10.99	9.81	6.45	2.64	9.14	0	13.87
Example Brands	Coke, Pepsi, Sprite	Ocean Spray, Minute Maid	Lipton, Snapple, Starbucks	Gatorade, Powerade	Glaceau Vitamin Water, Propel	Red Bull, Monster	Deer Park, Fiji	Tropicana, Naked Juice

Panel B: Store Level	#Stores Inside Phil.	#Stores Outside Phil.	Average Weekly Volume (Oz) Per Store	Phil. Market Share		Average Price/Oz	Price/Oz of a Popular Soda Brand 2L	Median Pack-Size (Oz)
Grocery A	15	46	377,774	0.13		3.53	2.52	59
Grocery B	1	38	781,050	0.02		3.27	2.35	48
Grocery C	16	32	1,035,115	0.38	0.53	3.10	2.47	51
Mass Merchant M	6	21	223,869	0.03		4.10	2.16	46
Other Mass Merchants	5	28	920,248	0.11	0.14	3.19	1.97	59
Drugstore X	45	128	29,536	0.03		5.15	2.42	20
Drugstore Y	80	122	15,436	0.03		5.79	2.40	23
Drugstore Z	17	51	42,265	0.02	0.08	5.30	3.00	20
Convenience St.	116	324	72,619	0.19		8.02	2.69	18
Wholesale Club W	2	8	717,375	0.03		3.46	1.94	192
Dollar Stores	54	72	29,721	0.04		3.25	2.20	32
All Stores	357	870	122,409					

Panel C: Catchment-Area Demographics	#Stores	Mean	Min	Median	Max
Median Household Income ($1,000s)	357	44.1	20.0	41.9	76.2
Obesity Rate	357	0.29	0.20	0.28	0.42

Table 1: **Descriptive Statistics.** Market shares and prices in Panels A and B are based on pre-tax data.

to freshly extracted juice and no sweetener is added.

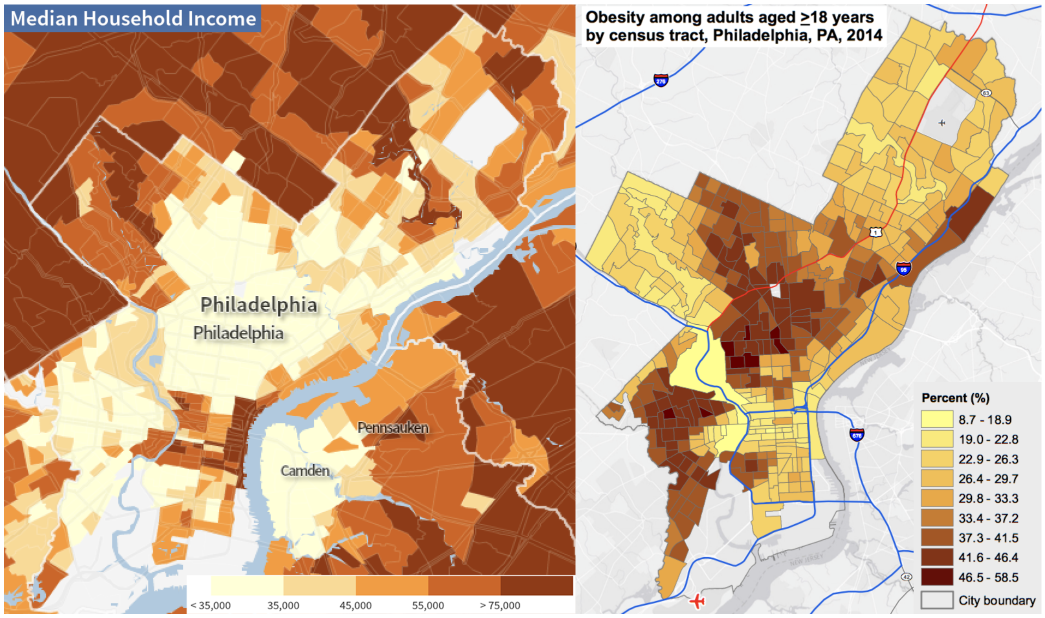

Figure 2: **Variation in Income and Obesity Rates in Philadelphia.**

of untaxed beverage sales, but are notable because they contain similar amounts of sugar and more calories relative to taxed juices. In terms of overall market share, untaxed products are purchased slightly more frequently than taxed beverages.

Panel B in Table 1 describes the characteristics of different stores in our sample. These can broadly be divided into grocery stores, mass merchants, drugstores, and convenience stores. Two residual types of stores with smaller market shares are dollar stores and a wholesale club chain. Each row in the table indicates an anonymized chain belonging to one of these categories of stores. As mentioned previously, the categories "other mass merchants," convenience stores, and dollar stores pool together stores with different chain affiliations and independent stores. The first two columns in the table report counts of stores within and outside the city of Philadelphia. The third column displays the average weekly volume per store of taxed beverages in the pre-tax period,[23] and the fourth column reports total market share. Grocery stores, mass merchants, and the wholesale club all sell a relatively large volume on a per-store basis, and despite the fact that relatively fewer of these stores exist, these retail formats account for over two thirds of purchase volumes. Drugstores, dollar stores, and convenience stores sell a much lower volume per store. Due to the relatively larger number of stores, they jointly account for about 30% of sales. Finally, the average price per ounce is significantly higher in the smaller stores, largely because they tend to sell smaller pack sizes that are significantly more expensive on a per-unit basis. We illustrate this difference in assortment across store types in the final two columns of Panel B. These columns show that the price for the same product, in this case a 2-liter bottle of a popular soda brand, only differs marginally across stores, but the smaller stores tend to sell smaller pack sizes.[24]

[23] Relative volume differences and market shares across chains / types of stores are similar for untaxed products.

[24] Many beverages are priced in a highly non-linear way. For example, a 2-liter (67.6 oz) bottle of Coca-Cola is

Finally, Panel C in Table 1 summarizes the variation in local demographics for the stores in Philadelphia. We see significant variation in income and obesity rates, and these two measures are highly correlated (corr. = -0.8). We provide some graphical evidence for this negative correlation in Figure 2.[25] North Philadelphia and West Philadelphia are lower-income neighborhoods that have a higher obesity prevalence, whereas Center City, Manayunk, Chestnut Hill, and Northeast Philadelphia are higher-income neighborhoods that have lower obesity rates. In section D of the appendix, we show most chains are present in neighborhoods with different income levels and obesity rates.

Descriptive Graphical Evidence: Impact of the Tax on Price and Quantity Sold of 2-Liter Bottles of a Popular Soda Brand As a precursor to the more systematic empirical analysis below, we illustrate the effect of the tax on price and quantity sold for one of the most popular products in our sample: a 2-liter bottle of a popular soda brand. The top graph in Figure 3 plots the average weekly prices of the product at stores in Philadelphia and surrounding control stores outside Philadelphia from January 2015 to September 2018. The product was priced at a similar level both within and outside the city before January 2017, and the weekly price series appear to be highly correlated. When the tax went into effect on January 1, 2017, the average price in the city increased significantly, while the price remained at a lower level in control stores outside the city. Correspondingly, the bottom graph of Figure 3 depicts the average weekly unit sales of the same product at stores in Philadelphia and control stores outside the city. The weekly unit sales inside and outside Philadelphia followed parallel trends over time before the tax. After January 1, 2017, unit sales experienced a substantial drop inside the city.[26]

4 Estimation and Results

Our identification strategy is based on a difference-in-differences approach that compares the change in various outcome measures at stores in Philadelphia against stores in the surrounding 3-digit ZIP codes. In all regressions (unless stated otherwise), we only include stores that are located more than 6 miles away from the city limits in the control group (see Figure 1), to ensure the control group is not affected by the treatment through cross-shopping behavior. We later show 6 miles constitutes an appropriate choice of distance (see column (2) in Table 5). Our choice of control group has two advantages. First, the control-group stores are relatively close to the city of Philadelphia and hence likely to experience similar demand shocks. For example, we expect our control group stores will experience similar weather patterns and similar surges in demand due to local events like sports games. In addition, all of our treatment and control group stores are located in the same

often sold at the identical (or only marginally different) price as a 20-oz bottle.

[25]Map Source: CDC/NCCDPHP/DPH/ESB (2016).

[26]The bottom graph in Figure 3 shows the week-on-week variation in sales decreases in the post-tax period. We explore this pattern further in section G of the appendix.

Figure 3: **Unit Price and Sales of 2-Liter Bottles of a Popular Soda Brand.**

television DMA and thus consumers in both groups will be exposed to the same TV advertising. In section E in the appendix, we test this "parallel trends" assumption formally and find no evidence for differential time trends in the treatment and control groups. Second, choosing stores from a nearby area ensures the chain affiliations of stores in the city are represented in the control group. Therefore, we are able to use stores of the same chain outside of Philadelphia as a control group for stores of the same chain in the city.

Formally, we estimate regressions based on the following general structure:

$$y_{st} = \alpha(Philly_s \times AfterTax_t) + \gamma_s + \delta_t + \varepsilon_{st}, \tag{1}$$

where the unit of observation is a store/week (s, t) combination. γ_s and δ_t are store and week fixed effects, and ε_{st} denotes the regression error. $Philly_s$ denotes a dummy that is equal to 1 if store s

is located in Philadelphia, and $AfterTax_t$ is a dummy that is equal to 1 for any week after the tax went into effect. The difference-in-differences coefficient α is the main coefficient of interest. y_{st} denotes various outcome variables such as price, quantity sold, and so on.

We also explore heterogeneity in the impact of the tax along various dimensions such as chain identity, local demographics, and the competitive environment. To this end, we implement the following regression framework:

$$\begin{aligned} y_{st} \quad = \quad & \tilde{\alpha_0}(Philly_s \times AfterTax_t) + (Philly_s \times AfterTax_t \times \mathbf{X}_s)'\tilde{\boldsymbol{\alpha}}_1 \\ & +(AfterTax_t \times \mathbf{X}_s)'\tilde{\boldsymbol{\beta}} + \tilde{\gamma}_s + \tilde{\delta}_t + \tilde{\varepsilon}_{st}, \end{aligned} \tag{2}$$

where $\tilde{\gamma}_s$ and $\tilde{\delta}_t$ are store and week fixed effects and $\tilde{\varepsilon}_{st}$ denotes the regression error. The vector $\mathbf{X}_s$ denotes a set of store characteristics, and $\tilde{\boldsymbol{\beta}}$ denotes a vector of coefficients capturing the change in the outcome in stores outside of Philadelphia after the tax took effect as a function of $\mathbf{X}_s$. The vector $\tilde{\boldsymbol{\alpha}}_\mathbf{1}$ captures the differential change in the outcome in Philadelphia stores relative to stores outside of the city as a function of $\mathbf{X}_s$. The coefficient $\tilde{\alpha_0}$ denotes the baseline, that is, an un-interacted, difference-in-differences estimate.[27]

We employ two-way clustered standard errors at the store and the week level in all regressions. In section F in the appendix, we show robustness to higher levels of clustering both along the geographical dimension and along the time dimension. Finally, we note a brief adjustment period during which price pass-through and the quantity decrease are slightly lower. After the first 4 months, the impact of the tax does not vary over time for any of the outcomes we analyze below. We analyze these dynamic adjustment patterns in detail in Appendix H. In our main regressions, we omit the first four months after the tax went into effect in order to focus on the impact of the tax after the initial adjustment.

We first analyze the impact of the tax on prices and quantities sold of taxed products. We then turn to analyzing substitution to untaxed beverages and to stores outside of Philadelphia, which are not subject to the tax. Finally, we analyze the impact of the tax on calories and sugar intake.

4.1 Price Reaction and Pass-Through

To measure pass-through, we use price in cents/oz at store s in week t as the outcome measure. The difference-in-differences coefficient in this regression denotes the estimated price change in cents per ounce due to the tax. Remember the tax is equal to 1.5 cents per ounce. Hence, a coefficient of 1.5 would correspond to full pass-through, and dividing the coefficient by 1.5 yields the percentage pass-through rate. All results below are based on the average price for all taxed products. Results from the base specification in equation (1) are reported in column (1) of Table 2 and show an average pass-through of 1.45 cents per oz, corresponding to a 97% average pass-through rate. Relative to

[27] In some specifications below, we include an exhaustive set of dummies along a specific dimension, and hence no $\tilde{\alpha_0}$ term is included. Also, note that the estimation equation does not include "un-interacted" $\mathbf{X}_s$ terms because we include a full set of store dummies.

an average pre-tax price of 4.26 cents per oz, this pass-through rate constitutes a 34% increase in price.

Next, we explore heterogeneity by allowing the pass-through coefficient to differ along various dimensions. In column (2) of Table 2, we report results from a regression that includes interactions of the after-tax dummy times the Philadelphia dummy with a full set of of chain dummies for the 11 different chains / groups of stores in our sample.[28] We find pass-through rates are remarkably consistent across chains. Apart from two exceptions, the increase in price per ounce lies between 1.16 and 1.78 (77% and 119% pass-through). Mass Merchant M and Drugstore Z have significantly smaller pass-through rates.[29] However, the two chains jointly make up less than 5% of market share and hence are not the primary focus of our analysis. Column (3) shows the same regression using the log of price per ounce as the dependent variable. The pass-through rate documented in column (2) translates into a 30%-40% price increase in most stores. The price increase in percentage terms is somewhat lower in convenience stores and drugstores, despite a similar pass-through rate. This difference in the price increase occurs because those retail formats tend to sell smaller pack sizes, which, on average, have a higher price per ounce (see the last column in Table 1, Panel B). We note that due to large coefficient values in the log specifications in columns (3) - (6), applying the transformation $\exp(coefficient) - 1$ is necessary to obtain the percentage change. When discussing percentage results in the paper, we always apply this transformation.[30]

In the remaining columns of Table 2, we explore other factors that may drive within-chain variation in pass-through. In column (4), we investigate whether the competitive environment affects pass-through. In our context, the strongest competition for stores in Philadelphia originates from stores outside of the city. These stores are not subject to the tax and, as we show later, they do not adjust prices after the tax. As a simple measure of competition, we therefore include distance to the city border (with the appropriate interactions) in the regression. We find the distance to the border does not predict a differential price reaction, and the estimated coefficient is small in magnitude. The effect remains small and insignificant when we estimate the regression without chain interactions. We conclude that competitive pressure does not affect pass-through.

In the final two columns of Table 2, we investigate whether income and obesity rates in the store's catchment area (1-mile radius around the store) are predictive of pass-through. To facilitate interpretation, we use re-scaled versions of the income and obesity variables that range from 0 to 1 across all stores in Philadelphia. We find that prices increase more in low-income and high-obesity areas. Hence, from a health policy point of view, one might be encouraged by the higher increase in high-obesity areas. On the other hand, the differential price increase leads to a higher financial burden for low-income households. Further, although both coefficients are statistically significant,

[28]To simplify exposition, we do not report the coefficient vector $\tilde{\boldsymbol{\beta}}$ pertaining to $(AfterTax_t \times \mathbf{X}'_s)$ terms for any of the regressions in columns (2) - (6) of Table 2.

[29]Mass Merchant M and Drugstore Z pass the tax through only for soda, and not for other taxed categories. Furthermore, Drugstore Z initially does not increase soda prices and then increases them by approximately 1 cent per ounce in late May 2017.

[30]For example, the estimated effect for Grocery A in column (3) corresponds to a 37.3% change: $\exp(0.317) - 1 = 37.3\%$.

Dependent Variable	(1) Price/Oz	(2) Price/Oz	(3) Log Price/Oz	(4) Log Price/Oz	(5) Log Price/Oz	(6) Log Price/Oz
Philadelphia × AfterTax	1.449***					
	(0.022)					
Grocery A		1.355***	0.317***	0.319***	0.332***	0.308***
× Philadelphia × AfterTax		(0.018)	(0.008)	(0.012)	(0.009)	(0.008)
Grocery B		1.290***	0.320***	0.324***	0.330***	0.311***
× Philadelphia × AfterTax		(0.003)	(0.001)	(0.014)	(0.003)	(0.002)
Grocery C		1.780***	0.442***	0.444***	0.450***	0.424***
× Philadelphia × AfterTax		(0.051)	(0.011)	(0.013)	(0.012)	(0.012)
Mass Merchant M		0.655***	0.144***	0.145***	0.156***	0.131***
× Philadelphia × AfterTax		(0.122)	(0.027)	(0.029)	(0.027)	(0.029)
Other Mass Merchants		1.155***	0.308***	0.310***	0.319***	0.295***
× Philadelphia × AfterTax		(0.011)	(0.005)	(0.009)	(0.006)	(0.005)
Drugstore X		1.492***	0.258***	0.260***	0.271***	0.247***
× Philadelphia × AfterTax		(0.038)	(0.009)	(0.011)	(0.010)	(0.009)
Drugstore Y		1.377***	0.216***	0.218***	0.226***	0.200***
× Philadelphia × AfterTax		(0.025)	(0.006)	(0.009)	(0.007)	(0.007)
Drugstore Z		0.342***	0.062***	0.064***	0.072***	0.048***
× Philadelphia × AfterTax		(0.031)	(0.006)	(0.010)	(0.007)	(0.007)
Wholesale Club		1.398***	0.336***	0.339***	0.346***	0.326***
× Philadelphia × AfterTax		(0.013)	(0.003)	(0.012)	(0.005)	(0.006)
Dollar Stores		1.557***	0.389***	0.391***	0.396***	0.369***
× Philadelphia × AfterTax		(0.032)	(0.007)	(0.011)	(0.007)	(0.010)
Convenience Stores		1.626***	0.183***	0.185***	0.194***	0.170***
× Philadelphia × AfterTax		(0.032)	(0.003)	(0.009)	(0.006)	(0.004)
Distance (in Miles) to Border				-0.001		
× Philadelphia × AfterTax				(0.003)		
Income					-0.024***	
× Philadelphia × AfterTax					(0.009)	
Obesity Rate						0.033***
× Philadelphia × AfterTax						(0.009)
$(AfterTax_t \times \mathbf{X}_s)'$ Interactions	n/a	Yes	Yes	Yes	Yes	Yes
Store FE	Yes	Yes	Yes	Yes	Yes	Yes
Week FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	144,700	144,700	144,700	144,700	144,700	144,700
Stores	832	832	832	832	832	832
Weeks	176	176	176	176	176	176

Table 2: **Impact on Prices / Pass-through Rate Estimates.** Interactions with an after-tax dummy (the $(AfterTax_t \times \mathbf{X}'_s)$ term) are included in columns (2) - (6), but not reported separately. One exception is the obesity variable in column (6). We have no obesity data outside of Philadelphia and hence no $(Obesity_s \times AfterTax_t)$ term is included.

they are relatively small in magnitude.[31]

In summary, we find pass-through is similar across chains, the competitive environment does not predict differential pass-through, and local demographics explain a small part of the variation in pass-through across store locations.[32]

4.2 Quantity Reaction

Next, we analyze changes in quantity sold after the tax. As in the previous section, we use the framework outlined in equations (1) and (2), but use quantity sold (measured in ounces) as the dependent variable. The first column of Table 3 shows an average decrease of 56,000 ounces per store in the total volume of taxed beverages that were sold in Philadelphia. This effect is large in magnitude and constitutes a 46% reduction relative to the average pre-tax level of weekly sales of 122,000 ounces per store (see Panel B in Table 1).

Notable heterogeneity exists in this effect across chains. In columns (2) and (3), we report results using total quantity and the logarithm of total quantity as the dependent variable, respectively. The chains that sold large quantities prior to the tax, namely, grocery stores, mass merchants, and the wholesale club, all experience large decreases in sales of 41% to 69%.[33] Among the smaller-volume chains, only dollar stores experience a similar decrease. Drugstores and convenience stores instead experience a more modest decrease or no decrease in volume sold. Looking at the patterns documented in Panel B of Table 1 and the price results in Table 2, this pattern has two likely explanations. First, price increased less in percentage terms at drugstores and convenience stores due to a higher pre-tax price level. Second, those stores tend to sell smaller pack sizes, which are more likely to be impulse purchases, and hence consumers might be less price sensitive for such purchases. Finally, we singled out Mass Merchant M and Drugstore Z in the previous section because those two chains are characterized by significantly lower price increases. Consistent with this pattern, we find that Mass Merchant M experiences no quantity decrease, and quantity sold actually increases at Drugstore Z. This increase is likely due to the fact that Drugstore Z has the lowest pass-through rate of all stores, and hence becomes relatively more attractive to consumers after the tax goes into effect.

Next, we turn to the competitive environment and find that distance to the city border does not have a significant impact on quantity changes (see column (4)). Results based on interactions with income and obesity rates are reported in columns (5) and (6). We find quantity decreases more in high-income areas, whereas obesity rates do not predict a differential quantity response. The relationship between income and changes in quantity is relatively large in magnitude. Quantity de-

[31] The standard deviation of the re-scaled income (obesity) variable is equal to 0.26 (0.27). Hence, a one-standard-deviation shift in either variable leads to a change in the price adjustment of less than 1%. We re-iterate that income and obesity are strongly negatively correlated (correlation coefficient of -0.8), and hence when we include both variables, estimates become noisier.

[32] We also report the same analysis using the price for soda rather than all taxed products as the dependent variable in Table A5 in the appendix and find largely similar results.

[33] As mentioned before, we apply the transformation $\exp(coefficient) - 1$ to translate the regression coefficients into percentage changes.

Dependent Variable	(1) Ounces Sold	(2) Ounces Sold	(3) Log Ounces	(4) Log Ounces	(5) Log Ounces	(6) Log Ounces
Philadelphia × AfterTax	-56,192***					
	(9,740)					
Grocery A		-207,363***	-0.733***	-0.747***	-0.652***	-0.725***
× Philadelphia × AfterTax		(34,502)	(0.068)	(0.077)	(0.077)	(0.070)
Grocery B		-369,605***	-0.674***	-0.700***	-0.609***	-0.665***
× Philadelphia × AfterTax		(9,753)	(0.011)	(0.062)	(0.027)	(0.015)
Grocery C		-728,854***	-1.173***	-1.187***	-1.111***	-1.157***
× Philadelphia × AfterTax		(82,272)	(0.068)	(0.070)	(0.071)	(0.074)
Mass Merchant M		-23,083	-0.110	-0.121	-0.033	-0.098
× Philadelphia × AfterTax		(24,797)	(0.109)	(0.119)	(0.112)	(0.110)
Other Mass Merchants		-406,541***	-0.529***	-0.544***	-0.461***	-0.518***
× Philadelphia × AfterTax		(65,340)	(0.078)	(0.087)	(0.080)	(0.080)
Drugstore X		-7,899***	-0.290***	-0.304***	-0.212***	-0.280***
× Philadelphia × AfterTax		(1,232)	(0.041)	(0.054)	(0.048)	(0.043)
Drugstore Y		-610***	-0.002	-0.015	0.063	0.013
× Philadelphia × AfterTax		(202)	(0.034)	(0.048)	(0.041)	(0.039)
Drugstore Z		26,169***	0.558***	0.542***	0.621***	0.570***
× Philadelphia × AfterTax		(4,310)	(0.079)	(0.088)	(0.080)	(0.083)
Wholesale Club		-423,042***	-0.878***	-0.899***	-0.796***	-0.869***
× Philadelphia × AfterTax		(35,987)	(0.062)	(0.079)	(0.074)	(0.061)
Dollar Stores		-16,234***	-0.568***	-0.583***	-0.519***	-0.550***
× Philadelphia × AfterTax		(1,669)	(0.034)	(0.048)	(0.038)	(0.040)
Convenience Stores		-7,131***	-0.108***	-0.122***	-0.035	-0.096***
× Philadelphia × AfterTax		(1,530)	(0.019)	(0.037)	(0.034)	(0.023)
Distance (in Miles) to Border				0.006		
× Philadelphia × AfterTax				(0.014)		
Income					-0.106**	
× Philadelphia × AfterTax					(0.044)	
Obesity Rate						-0.030
× Philadelphia × AfterTax						(0.041)
$(AfterTax_t \times X_s)'$ Interactions	n/a	Yes	Yes	Yes	Yes	Yes
Store FE	Yes	Yes	Yes	Yes	Yes	Yes
Week FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	144,700	144,700	144,700	144,700	144,700	144,700
Stores	832	832	832	832	832	832
Weeks	176	176	176	176	176	176

Table 3: **Impact on Quantity Sold.** Interactions with an after-tax dummy (the $(AfterTax_t \times \mathbf{X}'_s)$ term) are included in columns (2) - (6), but not reported separately. One exception is the obesity variable in column (6). We have no obesity data outside of Philadelphia and hence no $(Obesity_s \times AfterTax_t)$ term is included.

creases by approximately 10% more in the highest-income area relative to the lowest-income area.[34] The direction of the correlation with income is surprising because we would expect high-income households to be less price sensitive, and hence reduce consumption less in response to the tax. Moreover, we saw in Table 2 that prices increased somewhat less in high-income areas, and hence the smaller price increase should lead to a lower-quantity reaction. Furthermore, prior research (see Wang (2015) and Dubois et al. (2017)) predicts a larger reaction of low-income households to a counterfactual soda tax. One possible explanation for our finding is that high-income households have easier access to transportation, and thus are able to avoid the tax by driving to stores outside of the city to stock up on sweetened drinks. We return to this point after presenting results relating to cross-shopping in section 4.4.[35]

4.3 Substitution to Untaxed Beverages

So far we have documented that the tax was almost fully passed through to retail prices and that the quantity of taxed beverages sold in Philadelphia decreased by 46%. We now turn to analyzing whether consumers substitute to other products in reaction to the tax-induced price increase. In our context, two channels for substitution are possible. Consumers might substitute to other untaxed beverages or drive outside of the city to purchase sweetened beverages at stores that are not subject to the tax.

We first analyze demand for untaxed beverages as a potential channel of substitution based on the same regression framework as in the case of prices and quantities of taxed products. When analyzing quantity sold of *all* untaxed beverages in column (1) of Table 4, we find a decrease of 4,521 ounces in the average store in Philadelphia. The effect is statistically insignificant and small in magnitude. Relative to the average pre-tax volume of untaxed beverages of 146,000, the decrease constitutes a 3.1% change in demand (95% CI [-13%, 6%]).

When analyzing bottled water and natural juice separately in columns (2) and (3), we find a statistically significant increase of 9% in the sales of natural juice, whereas sales of bottled water do not change significantly (95% CI [-15%, 6%]). We note the market share of natural juice is relatively small, and hence the increase of 1,400 ounces per store is modest when compared to the decrease of 56,000 ounces of taxed beverages documented earlier. Nevertheless, it is interesting that among untaxed beverages, natural juices, which contain more calories and sugar than most

[34] We also directly test whether price elasticities differ as a function of income by regressing log quantity on log price where price is instrumented with the $Philadelphia \times AfterTax$ dummy. We further include an interaction of log price and income and an additional instrument that interacts the $Philadelphia \times AfterTax$ dummy with income. Using this framework allows us to relate quantity response as a function of income to the relevant price changes induced by the tax. The coefficient on the log price interaction with income is negative and significant (coefficient of -0.52 and standard error of 0.24, the baseline un-interacted log price coefficient is equal to -0.66), and hence demand is more elastic in high income areas.

[35] In Table A6 in the appendix, we present results from the quantity regression for the soda category. Results are very similar with regards to overall effect magnitude and patterns of heterogeneity along various dimensions. The only meaningful difference is a larger quantity reaction for Mass Merchant M in the soda category. This finding is consistent with the earlier finding that Mass Merchant M only increased prices for soda and not for other taxed categories.

	All Untaxed Beverages	*Water*	*Natural Juice*	*All Untaxed Beverages*	*Water*	*Natural Juice*
	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable	Ounces Sold	Ounces Sold	Ounces Sold	Price/Oz	Price/Oz	Price/Oz
Average Pre-tax Quantities / Prices	146,017	130,736	15,281	1.88	1.35	6.37
Philadelphia × AfterTax	-4,521	-5,740	1,387**	0.063***	0.027***	0.343***
	(7,118)	(7,194)	(542)	(0.010)	(0.010)	(0.032)
Store FE	Yes	Yes	Yes	Yes	Yes	Yes
Week FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	144,209	143,931	133,005	144,209	143,931	133,005
Stores	829	827	764	829	827	764
Weeks	176	176	176	176	176	176

Table 4: **Price and Quantity Reaction of Untaxed Beverages.** Some stores do not offer all categories of beverages, and hence the number of observations differs slightly across columns.

taxed beverages, experience an increase in demand.

In columns (4) to (6), we analyze price changes among untaxed beverages. We find that, on average, prices increase slightly by 0.027 cents per ounce for bottled water and by 0.343 cents per ounce for natural juices. Although both coefficients are statistically significant, the effect for water is small in magnitude.[36] The larger increase in the price of natural juice could be an equilibrium response to increased demand for natural juices due to consumers substituting away from taxed beverages.[37]

4.4 Geographic Substitution

Next, we analyze whether consumers chose to drive outside of Philadelphia to purchase at stores that were not subject to the tax. To analyze the extent of cross-shopping at stores outside of Philadelphia, we employ the basic framework laid out in equation (1). However, rather than excluding stores within 6 miles of the city border, we now include those stores and estimate separate treatment effects for them. Thus, we continue to treat stores more than 6 miles outside of the city as the control group.

We first estimate a regression that allows for separate effects within 0-2, 2-4, and 4-6 miles outside of the city. Results from this regression are reported in the first column of Table 5 and

[36] In comparison, the price for taxed beverages went up by 1.45 cents per ounce (see column (1) of Table 2).

[37] Some retailers might have mistakenly applied the tax to some products that are not intended to be taxed. For example, Karen Meleta, a vice president at the ShopRite grocery chain, acknowledged in a January 2017 interview with *Philadelphia* magazine that some products (including plain mineral water and a natural lime juice) had been mislabeled (Fiorillo (2017)). In the article, Meleta explains that "we literally had to go through all of our drink products by hand to determine which ones would be subject to the tax. It's very confusing and complicated. If you read the original regulations, where there was some confusion was that the original regulation actually says that caloric sweeteners may also include sugars from concentrated fruits or vegetable juices that are in excess of what would be expected from fruits or vegetables. [...] We reached out to the city and asked how were were [sic] supposed to calculate this. How do we know if something has been reconstituted to its original sweetness level?"

Dependent Variable	(1) Ounces Sold Taxed Beverages	(2) Ounces Sold Taxed Beverages	(3) Price Per Oz Taxed Beverages	(4) Ounces Sold Untaxed Beverages	# Stores in Geogr. Area
Philadelphia × After Tax	-56,193***	-56,797***	1.449***	-4,481	357
	(9,740)	(9,774)	(0.022)	(7,111)	
0-2 Miles Outside	63,650***	63,046***	-0.022**	6,323	106
City Border × After Tax	(20,733)	(20,748)	(0.011)	(7,610)	
2-4 Miles Outside	18,364***	17,760**	0.006	4,648	140
City Border × After Tax	(7,031)	(7,081)	(0.011)	(9,472)	
4-6 Miles Outside	8,640**	8,036*	0.002	19,877	149
City Border × After Tax	(4,196)	(4,259)	(0.009)	(16,274)	
6-8 Miles Outside		2,790			118
City Border × After Tax		(3,711)			
8-10 Miles Outside		-5,995*			103
City Border × After Tax		(3,044)			
Store FE	Yes	Yes	Yes	Yes	
Week FE	Yes	Yes	Yes	Yes	
Change in Aggregate	-9,456**				
Quantity (Unit: 1,000 Ounces)	(4,358)				
Change in % of Pre-tax Volume in	-0.216**				
Philadelphia **w/ Cross-Shopping**	(0.100)				
Change in % of Pre-tax Volume in	-0.459***				
Philadelphia **w/o Cross-Shopping**	(0.080)				
Observations	213,499	213,499	213,499	212,556	
Stores	1,227	1,227	1,227	1,221	
Weeks	176	176	176	176	

Table 5: **Quantity and Price Reaction in Stores Near the City Border.** Some stores do not offer all categories of beverages, and hence the number of observations differs slightly across columns. Reported store counts in the right-most column are for stores that sell taxed products.

show that quantity sold increases significantly for stores up to 6 miles outside of the city limits. Compared to the decrease of 56,000 ounces of taxed beverages at the average store in Philadelphia, we find an even larger increase of 64,000 ounces/store in stores up to 2 miles away from the city. Stores up to 4 and 6 miles outside of Philadelphia experienced smaller increases in sales. Figure 4 presents a graphical representation of the regression in column (1). The top graph shows the level of sales in each group, whereas the bottom graph shows the difference between sales in the two "treated" groups and the control group (6+ miles out). These graphs show that sales in the 0-2 miles band near the city increase relative to stores more than 6 miles away, whereas sales in stores in Philadelphia decrease. To avoid clutter, we do not show the two curves for stores 2-4 and 4-6 miles outside of the city.

Because the number of stores varies across geographical areas, we need to weigh the different coefficients in column (1) appropriately in order to assess the aggregate change in quantity. In

Figure 4: **Taxed Beverage Sales in Philadelphia and Surrounding Area Stores over Time.**

the lower panel of Table 5, we report the total weekly decrease in quantity in Philadelphia and stores less than 6 miles outside of the city, which is equal to 9.5 million ounces and constitutes a 22% decrease relative to the total weekly volume sold in Philadelphia prior to the tax. The effect is statistically significant at the 5% level.[38] Notably (as reported earlier), when focusing on stores in Philadelphia only, we find a much larger decrease in quantity of 46%. The difference between the "Philadelphia only" and "Philadelphia plus 6 miles outside" percentage decrease is statistically significant. Therefore, to measure the aggregate quantity change correctly, accounting for cross-shopping behavior is important.

We next probe whether stores farther than 6 miles outside of Philadelphia also experience an increase in quantity by adding two additional terms for stores 6-8 and 8-10 miles outside of the city, respectively. Stores that are located more than 10 miles away from the city limit serve as the control group. Results from this regression are reported in column (2) of Table 5 and show no significant increase in quantity at stores farther than 6 miles away from the city border.[39] These estimates provide evidence that stores more than 6 miles away from the city constitute a valid control group that is not indirectly affected by the tax due to cross-shopping.

In column (3) of Table 5, we assess whether prices react differently in areas within a specific distance of the city. Interestingly, we find that prices at stores within 0-6 miles outside of Philadelphia remain almost unchanged despite the large increase in quantity sold in those geographic areas. In fact, in stores within 2 miles, we find a small but statistically significant decrease in prices. We also find that sales of untaxed products do not change at stores near the city border (see column (4)). When using sales of *all* products as the dependent variable (not reported in the table), we find the aggregate change is not significantly different from zero.

We also analyze possible changes in cross-shopping behavior after the tax goes into effect. Such changes in behavior could occur because consumers might engage in cross-shopping immediately after the tax goes into effect but find that doing so in the long run is inconvenient. We present this analysis in Appendix H. Our analysis shows that after a brief adjustment period of four months (which are omitted from our main regressions above), quantities sold stabilized and show no sign of further adjustments between May 2017 and September 2018.

Finally, we investigate whether we can detect patterns suggesting that different income groups are more or less likely to engage in cross-shopping. To this end, we analyze heterogeneity in the demand increase at stores outside of the city border as a function of the income level of nearby census tracts *inside* Philadelphia. If high-income consumers are more likely to travel to stores outside of Philadelphia, we should see a larger increase in demand at border stores near high-income areas in Philadelphia. We test this prediction by adding interactions of the border-store dummies with income in nearby areas in Philadelphia (and the post-tax dummy). We find that

[38]In an earlier working paper version, we found an effect that was significant only at the 10% level (but similar in magnitude to the effect reported above). The key difference is that we now omit the first four months of the tax from our analysis (because patterns during this adjustment period are slightly different from the longer-run impact of the tax).

[39]The 8- to 10-mile coefficient is significant only at the 10% level and has a negative sign.

higher income in nearby areas in Philadelphia leads to a larger increase in demand at border stores. However, in most specifications, the interaction effects are not statistically significant, most likely due to a lack of sufficient power to identify such effects.[40]

Heterogeneity across Pack Sizes and Categories In this section, we separately investigate the demand response for small and large pack sizes, as well as for the various taxed categories presented in Panel A of Table 1. We focus on results for quantity because price pass-through is similar across pack sizes and categories.[41]

We start by investigating heterogeneity in the demand response for small versus large pack sizes. We define small pack sizes as products with a container size of 20 ounces or less. Such products can be qualified as on-the-go beverages and are often sold close to the checkout register in supermarkets or in convenience stores or drugstores. Columns (1) and (2) of Table 6 replicate the cross-shopping regression specification from the previous section, based on sales of only small or large pack sizes, respectively. We find that consumers do not engage in cross-shopping for small pack-sizes. Demand for such products decreases by 10% at stores in Philadelphia, but there is no increase in sales outside of the city. This finding is in contrast to larger pack sizes, for which demand decreases by 53% in Philadelphia. However, a large part of this decrease is offset by an increase at stores outside of the city. 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 intended for immediate consumption. 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 are significantly larger.[42]

Despite the difference in demand response, the elasticity of demand is quite similar for large and small pack sizes, when cross-shopping is taken into account. To see that the two demand elasticities are similar, note that the average pre-tax price per ounce is equal to 10.10 (3.54) for small (large) pack sizes due to highly non-linear pricing across pack sizes. The tax is passed through at an almost identical rate across pack sizes and leads to a 15% (40%) increase in price and thus an elasticity of -0.64 (-0.60) for small (large) pack sizes. Therefore, elasticities are similar when cross-shopping is accounted for. However, when focusing on Philadelphia sales only, demand for large pack sizes appears more elastic.

[40] Specifically, we estimate a regression that replicates column (1) in Table 5, but add two interaction terms for the 0- to 2-mile and 2- to 4-mile distance bands of border stores with the income level in nearby areas in Philadelphia (and the post-tax dummy). We compute this income variable by calculating the average of the median income level at all census tracts in Philadelphia that are within 4 miles of the store. The coefficients on both interaction terms are positive but not statistically significant. We then split the distance bands more finely into bands of 0-1, 1-2, 2-4, and 4-6 miles and include interactions of the three closest bands with the income level in nearby areas in Philadelphia (and the post-tax dummy). In that specification, we find the coefficient on the interaction term for stores in the 0- to 1-mile distance band is statistically significant at the 5% level.

[41] The average increase in price per ounce due to the tax is equal to 1.51 (1.42) for small (large) pack sizes and varies between 1.30 (taxed juice) and 1.68 (sports drinks) across categories.

[42] A similar picture emerges when we analyze cross-shopping separately for larger (grocery stores, mass merchants, wholesale clubs) and smaller (drug stores, conveniences stores, dollar stores) store formats. We find a decrease before (after) accounting for cross-shopping of 61% (27%) for larger store formats and 10% (6%) for smaller store formats. The decrease in purchases after accounting for cross-shopping is statistically significant in both cases.

	Taxed Beverages		*Untaxed Beverages*	
	≤20 Oz Pack-sizes	*>20 Oz Pack-sizes*	*≤20 Oz Pack-sizes*	*>20 Oz Pack-sizes*
	(1)	(2)	(3)	(4)
Dependent Variable	Ounces Sold	Ounces Sold	Ounces Sold	Ounces Sold
Average Ounces Sold Pre-tax	19,439	105,530	3,726	146,795
Philadelphia × After Tax	-1,914***	-54,824***	19	-4,469
	(306)	(9,658)	(65)	(7,201)
0-2 Miles Outside	649	63,498***	118	6,094
City Border × After Tax	(453)	(20,692)	(72)	(7,603)
2-4 Miles Outside	-52	18,416***	-45	4,744
City Border × After Tax	(373)	(6,950)	(73)	(9,531)
4-6 Miles Outside	-240	8,886**	45	20,503
City Border × After Tax	(343)	(4,092)	(66)	(16,584)
Store FE	Yes	Yes	Yes	Yes
Week FE	Yes	Yes	Yes	Yes
Change in Aggregate	-653***	-8,783**	19	2,728
Quantity (Unit: 1,000 Ounces)	(178)	(4,274)	(34)	(4,440)
Change in % of Pre-tax Volume in	-0.096***	-0.238**	0.015	0.054
Philadelphia **w/ Cross-Shopping**	(0.026)	(0.116)	(0.026)	(0.088)
Change in % of Pre-tax Volume in	-0.100***	-0.525***	0.005	-0.031
Philadelphia **w/o Cross-Shopping**	(0.016)	(0.092)	(0.018)	(0.049)
Observations	211,774	212,767	209,805	210,175
Stores	1,217	1,222	1,205	1,206
Weeks	176	176	176	176

Table 6: **Demand Response for Small and Large Pack Sizes.** Some stores do not offer all pack sizes, and hence the number of observations differs slightly across columns.

We also test whether the absence of cross-shopping for small pack sizes leads to substitution to healthier beverages. As shown in columns (3) and (4), which report cross-shopping regressions based on small and large pack sizes of untaxed beverages, we find no evidence of substitution toward untaxed beverages for either set of products. When focusing on demand for natural juice (not reported in the table), we do find a significant positive effect for large pack sizes. We find no significant change in the sales of bottled water for either small or large pack sizes.

Next, we investigate heterogeneity in demand response across product categories. Similar to our analysis of differences across pack sizes, we now estimate the cross-shopping regression specification separately for the six categories of beverages that are subject to the tax. The results are presented in Table 7. Interestingly, we find heterogenous demand patterns across categories. For soda, the change in aggregate volume is close to zero and not statistically significant. For taxed juice and tea/coffee, we find an aggregate reduction in demand even after accounting for cross-shopping. The remaining three categories, which are smaller in terms of market share, are characterized by a much smaller degree of cross-shopping. Also note that taxed water and sports drinks experience a larger

	Soda	*Taxed Juice*	*Tea Coffee*	*Sports Drinks*	*Taxed Water*	*Energy Drinks*
	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable	Ounces Sold	Ounces Sold	Ounces Sold	Ounces Sold	Ounces Sold	Ounces Sold
Average Ounces Sold Pre-tax	43,529	32,950	28,638	14,229	5,015	3,693
Philadelphia × After Tax	-18,711***	-16,903***	-12,213***	-6,609***	-3,341***	-552***
	(3,909)	(3,364)	(2,131)	(1,184)	(463)	(111)
0-2 Miles Outside	42,768***	8,790***	9,658***	2,881**	245	-140
City Border × After Tax	(13,400)	(3,106)	(3,404)	(1,416)	(649)	(121)
2-4 Miles Outside	12,818***	2,733**	2,208**	928	17	-165
City Border × After Tax	(4,245)	(1,061)	(1,017)	(939)	(473)	(119)
4-6 Miles Outside	6,424**	1,132	226	910	189	-162
City Border × After Tax	(2,538)	(690)	(730)	(667)	(423)	(122)
Store FE	Yes	Yes	Yes	Yes	Yes	Yes
Week FE	Yes	Yes	Yes	Yes	Yes	Yes
Change in Aggregate	605	-4,371***	-2,878***	-1,674***	-852***	-258***
Quantity (Unit: 1,000 Ounces)	(2,141)	(1,222)	(865)	(487)	(186)	(65)
Change in % of Pre-tax Volume in	0.039	-0.392***	-0.294***	-0.356***	-0.634***	-0.199***
Philadelphia **w/ Cross-Shopping**	(0.139)	(0.110)	(0.088)	(0.104)	(0.138)	(0.050)
Change in % of Pre-tax Volume in	-0.434***	-0.521***	-0.432***	-0.469***	-0.669***	-0.151***
Philadelphia **w/o Cross-Shopping**	(0.090)	(0.104)	(0.075)	(0.084)	(0.093)	(0.030)
Observations	213,499	205,997	211,183	198,208	169,429	212,619
Stores	1,227	1,183	1,213	1,139	971	1,222
Weeks	176	176	176	176	176	176

Table 7: **Demand Response across Categories.** Some stores do not offer all categories of beverages, and hence the number of observations differs slightly across columns.

reduction in sales than most categories. As shown earlier (Panel A of Table 1), those two categories are characterized by lower levels of sugar and calories per volume; hence, the largest reduction in demand occurs for categories that are relatively healthy (among all taxed beverages).

Cross-shopping and Basket-level Effects When engaging in cross-shopping to purchase sweetened beverages, consumers might also start purchasing other (non-taxed) products outside of Philadelphia. Thomassen et al. (2017) highlight the importance of such basket-level substitution effects.[43] To assess the importance of basket-level substitution effects, we study purchase patterns for milk in Philadelphia, as well as in border stores. We choose this category because milk is one of the most frequently purchased consumer packaged goods categories and not a direct substitute for or complement to sweetened beverages. Because we are not able to obtain data across an exhaustive set of categories in all stores, we treat milk as a stand-in for other items in a consumer's basket. We

[43]To the extent that consumers substitute purchases of other products to stores outside of the city, this could lead to lower overall sales-tax revenue in Philadelphia and lower revenue for Philadelphia retailers. The city of Philadelphia charges a 2% sales tax but food (not ready to eat), candy, and gum are excluded from the tax.

use the same specification as in our cross-shopping regression (see column (1) of Table 5), but use milk sales as the dependent variable. We start by examining all types of stores in Philadelphia, and find that store-level demand for milk decreases by a small amount of 1,100 ounces per week relative to an average pre-tax level of 58,870 ounces per week. The effect is not statistically significant.[44] In line with this finding, we also find no evidence that milk sales at stores near the city border experience a change in demand.[45]

If we focus only on grocery stores and wholesale club stores, where consumers tend to buy large pack sizes and engage more in cross-shopping, we find a statistically significant substitution effect. However, even for this subset of stores, the effect is small in magnitude and corresponds to a substitution of 5% of milk sales from stores in Philadelphia to stores just outside the city. Based on these findings, we conclude that only to a very limited extent do consumers substitute other parts of their basket to stores just outside the city.

4.5 Nutritional Intake

Finally, to analyze nutritional intake, we calculate the total number of calories and grams of sugar sold via beverage sales at the store/week level. Both variables are obtained by simply adding up calories and sugar across all beverage products sold in a given store/week. We intentionally do not distinguish between taxed and untaxed categories, because we want to analyze changes in total calories and grams of sugar from all beverage sales. We analyze the impact of the tax on nutritional intake in Table 8 based on the specification used in the previous sections to account for cross-shopping. We find that calories from beverages drop by only 16%, and the effect is not statistically significant at the 5% level (the 95% confidence interval ranges from -33% to 1%).

The calories regression differs in effect magnitude from the earlier cross-shopping regression based on quantity sold (see Table 5) for two reasons. First, a small increase occurs in the sales of untaxed natural juices, which are high in calories (see Table 4). Second, nutritional content varies within the set of taxed products. Therefore, if the decrease in quantity is driven predominantly by a decrease in healthier, low-calorie variants of taxed products, the percentage decrease in calories will be lower than the raw quantity decrease. We documented in the previous section that taxed water and sports drinks, which have fewer calories and less sugar, experienced a larger decrease in quantity relative to most other taxed categories. In this section, we test more directly for differences in demand patterns across products with different nutritional content by classifying products that contain less than/more than 10 calories per ounce as low-/high-calorie products.[46] We replicate the cross-shopping regression specification from the previous section, based on sales of only high- or low-calorie taxed products, respectively. The results in columns (2) and (3) of Table 8 show that high-calorie products experienced a smaller net decrease after accounting for cross-shopping

[44] We also find no price adjustment for milk either inside or outside the city.

[45] The changes in average weekly milk sales in the 0- to 2-mile, 2- to 4-mile, and 4- to 6-mile distance bands are 1,553 ounces, -1,131 ounces, and -336 ounces, respectively, and none of these estimates are statistically significant at the 5% level.

[46] 10 calories/oz is the median value for calorie content across all taxed products.

	All Beverages	*Low Calorie Taxed Beverages*	*High Calorie Taxed Beverages*
	(1)	(2)	(3)
Dependent Variable	Calories	Ounces Sold	Ounces Sold
Average Pre-Tax Quantities / Calories	1,392,713	52,280	71,836
Philadelphia * After Tax	-523,176***	-24,755***	-31,509***
	(95,942)	(4,200)	(5,906)
0-2 Miles Outside	636,965***	19,184***	44,630***
City Border * After Tax	(204,747)	(6,950)	(14,349)
2-4 Miles Outside	192,558***	5,538*	12,826***
City Border * After Tax	(68,390)	(2,899)	(4,253)
4-6 Miles Outside	93,293**	2,741	5,899**
City Border * After Tax	(42,894)	(1,806)	(2,542)
Store FE	Yes	Yes	Yes
Week FE	Yes	Yes	Yes
Change in Aggregate Quantity	-78,396*	-5,615	-3,843
(Unit: 1,000 Ounces / Calories)	(42,939)	(1,799)	(2,722)
Change in % of Pre-tax Volume in	-0.158*	-0.306***	-0.152
Philadelphia **w/ Cross-Shopping**	(0.087)	(0.098)	(0.107)
Change in % of Pre-tax Volume in	-0.377***	-0.480***	-0.444***
Philadelphia **w/o Cross-Shopping**	(0.069)	(0.081)	(0.083)
Observations	213,499	213,008	213,499
Stores	1,227	1,224	1,227
Weeks	176	176	176

Table 8: **Impact on Nutritional Intake.** High calorie beverages are defined as products with $\geq$10 calories/oz (the median value for calorie content across all taxed products).

compared to low-calorie products. In Table A7 in the appendix, we replicate the set of regressions in Table 8 using sugar content as the dependent variable and find quantitatively very similar results.

In summary, the decrease in calories and sugar is not statistically significant (p-values of 0.07 and 0.09) and is smaller than the decrease in demand for taxed beverages. This difference in effect magnitude is due to substitution to untaxed natural juices that are high in calories and sugars and because within the set of taxed products, demand decreases more for relatively healthier products.

4.6 Summary of Results

The analysis in the preceding sections demonstrated that the tax on sweetened beverages was passed through at an average rate of 97%. As a consequence, demand decreased in most stores in Philadelphia. We find that consumers do not switch to bottled water, but demand increases modestly for natural juices and increases substantially in stores just outside the city boundary. The latter channel of substitution offsets over half of the decrease in demand for taxed products in the city.

Nutritional Intake We find that nutritional intake in terms of total calories and grams of sugar from beverages does not change significantly, because consumers engage in cross-shopping and primarily decrease their consumption of relatively healthier beverages (within the set of taxed products) and because demand for high-calorie/high-sugar natural juices increases. The reduction in calories and sugar is equal to 16% at the point estimate, but the effect is not statistically significant.

Tax Revenue Second, we find tax revenue is substantially reduced by the fact that consumers engage in cross-shopping. The 46% reduction in quantity sold of taxed beverages leads to an equivalent percentage reduction in tax revenue relative to the case in which consumers continue to consume at pre-tax levels. Hence, any projection of tax revenue for a local sweetened-beverage tax of this kind needs to take into account the extent of consumers' cross-shopping behavior.

Welfare and Distributional Effects Finally, we glean several implications for consumer welfare from our analysis. First, consumers are able to partially avoid the financial burden of the tax by driving to stores outside of the city. Second, we find that quantity decreases more in stores that are located in high-income areas (see column (5) of Table 3), which seems surprising because we would expect low-income consumers to be more price sensitive. This pattern could occur if low-income consumers have a higher intrinsic preference for sweetened beverages; however, Wang (2015) and Dubois et al. (2017) allow for such heterogeneity in preferences and find that the price-sensitivity effect dominates and low-income consumers react more strongly to a soda tax. The key difference in our setting (relative to Wang (2015) and Dubois et al. (2017)) is the localized nature of the tax and the importance of the cross-shopping behavior that results from it. Therefore, a likely explanation for the smaller reaction to the tax in low income areas is that geographic substitution is more costly for low-income households. This explanation is also consistent with the fact that car ownership is negatively correlated with income. Specifically, across census tracts in Philadelphia, the correlation between median income and the percentage of households that do not have access to a car is -0.38.[47] Regardless of the specific explanation, the main take-away is the same: low-income households are more likely to continue purchasing sweetened beverages at stores in the taxed area, and hence the tax imposes a disproportionate financial burden on low-income households.

Comparison to Other Studies Various studies of the Berkeley tax find more limited pass-through between 25% and 47% and a small (sometimes statistically insignificant) reduction in quantity. Several papers (Cawley and Frisvold (2017), Rojas and Wang (2017), Bollinger and Sexton (2018)) speculate that lower pass-through occurs because cross-shopping constrains retailers from raising their prices. Because Berkeley is much smaller than Philadelphia (18 vs. 134 square miles), cross-shopping could be a stronger force in this setting. Bollinger and Sexton (2018) document some amount of cross-shopping, but based on only one supermarket outside of the taxed area. Notably,

[47]The data on car ownership comes from the American Community Survey (ACS). The survey asks, "How many automobiles, vans, and trucks of one-ton capacity or less are kept at home for use by members of this household?"

the elasticity implied by the estimates in Bollinger and Sexton (2018) is quite high (between -2 and -3), which might indicate a greater degree of cross-shopping. Our results are also largely in line with three recent studies analyzing the Philadelphia tax. Cawley et al. (2018b) document full pass-through at stores in Philadelphia,[48] and Cawley et al. (2018c) use a consumption survey to assess changes in purchases. The latter paper finds an insignificant decrease in Philadelphia and a significant increase at stores just outside of the city. Directionally, these findings are consistent with our study, but we estimate a significant decrease in purchases in Philadelphia, in addition to a significant increase in demand at stores near the city.

One study that warrants a more detailed comparison is Roberto et al. (2019)'s analysis of the Philadelphia tax using similar (but less comprehensive and representative) retail scanner data. Roberto et al. (2019) do not analyze nutritional outcomes, nor do they explore heterogeneity in how the tax affects different income groups. Moreover, where their analysis overlaps with ours, effect magnitudes differ substantially and hence our conclusions with regard to the effectiveness of the tax are very different. Roberto et al. (2019) estimate that volume sales of taxed beverages in Philadelphia decrease by 51%. We estimate a smaller (46%) decrease in sales in Philadelphia because, in addition to the retail formats they observe, we also observe 116 convenience stores, 54 dollar stores, and 2 wholesale club stores that together account for 26% of the pre-tax volume sales in our Philadelphia data (see Table 1). Further, we find that convenience stores which account for 19% of our pre-tax volume sales experience only a 10% decrease in sales, which is substantially smaller than the decrease at grocery stores and mass-merchants (see Table 3). Roberto et al. (2019) also test for cross-border shopping by analyzing Pennsylvania stores within 3 miles of the Philadelphia border, however they do not observe data for stores more than 3 miles outside the city limits, nor do they observe nearby stores in New Jersey. They conclude that 24% of their estimated reduction in sales in Philadelphia is offset by an increase in sales in these border stores, whereas we find that 52% of the sales reduction in Philadelphia is offset by cross-shopping. We find evidence of significant increases in sales up to 6 miles outside of the city limits, as well as evidence of cross-shopping at stores in both Pennsylvania and New Jersey. When we restrict our sample to the non-representative subset of stores analyzed by Roberto et al. (2019), we come close to replicating their quantity effects (we estimate a 36% reduction). Roberto et al. (2019) also find smaller price effects than we do (between 12% and 24% relative to 34% in our sample).[49] Differences in the estimated impact on both prices and quantities lead to drastically different elasticities of -1.7 in Roberto et al. (2019) versus -0.6 in our data. Unlike Roberto et al. (2019), we also analyze nutritional outcomes. We document compositional changes within different types of taxed and untaxed beverages which lead to nutritional effects that are smaller than the corresponding quantity effects. In summary, while Roberto et al. (2019) interpret their estimated 38% quantity decrease as evidence for an effective

[48] Prices for 38 taxed products were manually collected once before the tax and once after the tax.

[49] This is partly driven by sample differences, but also by differences in how average prices are computed. We use volume-weights based on pre-tax data, while Roberto et al. (2019) compute a simple average across UPCs. Furthermore, we adjust prices at one retailer that records the soda tax separately so that our price variable reflects the actual price paid by consumers (see footnote 13 and Appendix B).

policy, our evaluation is much more cautious because we estimate a significantly smaller nutritional impact,[50] because large cross-shopping effects hinder the tax's revenue generation and health goals, and because low income consumers shoulder more of the tax's financial burden.

5 Policy Design

Next, we take the insights gained from the Philadelphia tax and assess what we can learn from those findings about the design of sweetened-beverage taxes more broadly. The handful of taxes that have gone into effect in recent years vary along three dimensions. First, the tax rate varies from 1 cent per ounce in Berkeley and San Francisco to 2 cents per ounce in Boulder, CO. Second, the geographic area covered by the tax varies from small cities such as Berkeley or Boulder to larger cities such as Philadelphia and even an entire county (Cook County, IL). Third, some cities tax only *sugar*-sweetened beverages, whereas others also apply the tax to artificially (non-sugar) sweetened beverages (i.e., diet drinks). While most of the prior literature has focused on the impact of different tax rates, we add to the policy discussion by also analyzing changes in coverage. Due to the large cross-shopping effects and lack of substitution to most untaxed beverages, the choice of tax base along the geographic and product dimensions is likely to be an important policy lever.

Below, we discuss how changes along each of the three dimensions impact tax revenue and nutritional intake, the two outcomes emphasized by the Philadelphia mayor's office (Esterl (2016)). Due to the nature of our data, we do not attempt to integrate these elements into a unified framework (such as the one provided by Allcott et al. (2019a)), but instead trace out the effect of changes in tax policy design on both outcomes.

Before assessing policy design choices, we first discuss why the evidence regarding the impact of the Philadelphia tax is particularly useful to assess the impact of counterfactual tax policy choices.

5.1 Inference Regarding the Impact of Tax-Policy Design

One way to learn about the likely impact of a tax is to first estimate demand-side drivers of choice and supply-side drivers of price setting from pre-tax data and then simulate the impact of a hypothetical tax (see, e.g., Wang (2015), Kifer (2015), Dubois et al. (2017), and Allcott et al. (2019a)). One key challenge with such an approach is the extrapolation from observed short-term price changes at the individual product-level to a hypothetical long-term price change (induced by the tax) that affects all products and stores within a given geographic area. In contrast to an approach that estimates elasticities from pre-tax data, our analysis uses the price variation induced by the Philadelphia tax, which is closer in nature to price changes that other counterfactual taxes would induce.

A first well-known obstacle for assessing the impact of a soda tax using pre-tax data is how to infer long-run elasticities of demand from short-run variation in prices. In the context of storable

[50] Roberto et al. (2019)'s estimated 38% volume decrease lies outside the 95% confidence interval for our estimated 16% (15%) reduction in calories (sugars).

products such as sweetened beverages, short-term elasticities tend to be larger than long-run elasticities due to stockpiling behavior of consumers (Erdem et al. (2003), Hendel and Nevo (2006)).[51] Because we estimate price and quantity changes (and therefore the elasticity) based on a long-run price change, dealing with the difference between short- and long-run elasticities is unnecessary. A second obstacle to evaluating the impact of localized taxes based on pre-tax price variation is the fact that short-term product-specific price variation is unlikely to trigger consumers to switch stores. Identifying the extent of cross-shopping with pre-tax data is difficult, because most short-run price changes are due to promotions on individual items, and store switching due to those price changes is predicated on consumers knowing about prices prior to the store-switching decision. Indeed, when using only the pre-tax period of our data, we find that the elasticity of store-level demand with respect to the average price of competing stores within a 2-mile radius is equal to 0.083 and not statistically significant. In contrast, when basing the same elasticity on variation in prices induced by the tax, we estimate a statistically significant elasticity of 0.963.[52] Therefore, our data allow us to better assess the extent of cross-shopping triggered by a permanent price increase. Finally, we note that the two papers (Dubois et al. (2017) and Kifer (2015)) that use a supply-side model of price setting to simulate pass-through both find high pass-through rates of around 150%, which are significantly higher than our pass-through estimate of 97%.

5.2 Counterfactual Tax-Policy Design

Tax Rate To calculate the impact of counterfactual tax rates, we need to make an assumption about out-of-sample price setting and demand response. Throughout this section, we assume a constant pass-through rate and a constant elasticity of demand (and nutritional intake) with respect to price.[53]

These assumptions lead to the following expression for tax revenue as a function of the tax rate τ:

[51] Other papers that estimate demand for sweetened beverages based on pre-tax data employ various approaches to obtain long-run price elasticities. Wang (2015) estimates a dynamic demand model with inventory holdings. Dubois et al. (2017) focus on on-the-go beverage sizes for which stockpiling is less likely to occur. Allcott et al. (2019a) use quarterly data and show that there are no effects of lagged prices. They conclude that quarterly level estimation seems to be sufficient for estimating longer-run elasticities.

[52] In detail, we first use only the pre-tax period of our data (i.e., from January 2015 to December 2016) and regress log quantity on the log of own-price and the log of the average price of all competitors within a 2-mile radius around the store (and store fixed effects). We find the impact of competitors' prices is equal to 0.083 (standard error of 0.057) and not statistically significant. We then estimate the same regression, but isolate the variation in prices induced by the tax using instrumental variables. Specifically, we estimate the same regression as before, but use pre- and post-tax data and instrument own- and competitor price with interactions of all store dummies with the post-tax dummy. We find that in the latter case, the cross-price elasticity between stores is equal to 0.963 (standard error of 0.174).

[53] In this analysis, we assume that the quantity responses we documented in Section 4 are the result of price changes. In addition to prices, other marketing variables might also adjust in response to a tax, and these marketing variables could have an impact on quantity sales. It is also possible that the announcement of the tax and the discussion surrounding health effects of sweetened beverages could lead consumers to switch to healthier beverages independent of price changes (Taylor et al. (2019)). In Appendix I, we discuss these potential non-price effects in more detail and explain why the quantity changes we observe are likely to be a direct response to a change in price.

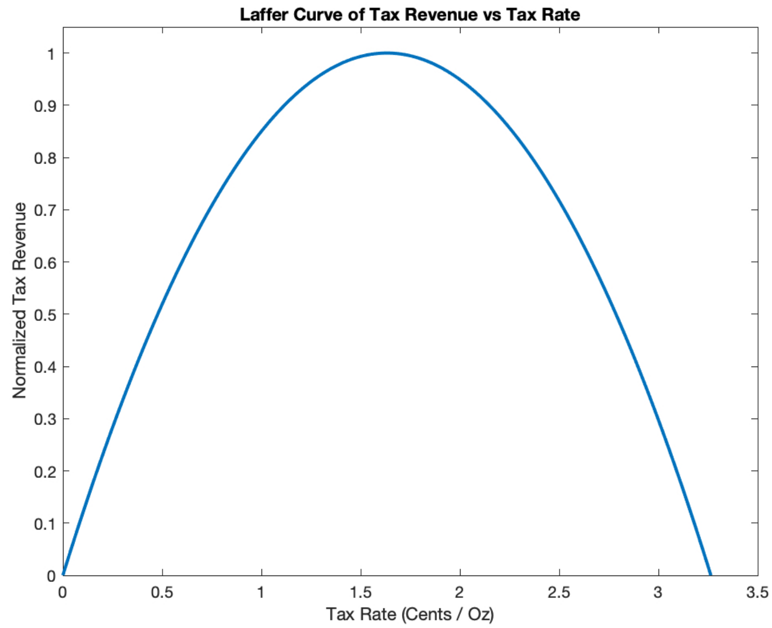

Figure 5: **Predicted Tax Revenue as a Function of the Tax Rate.**

$$\begin{aligned} TaxRevenue &= \tau \times Q(0) \times \left[1 + \eta_{qp} \frac{p(\tau) - p(0)}{p(0)}\right] \\ &= \tau \times Q(0) \times \left[1 - 1.35 \frac{0.97\tau}{4.26}\right], \end{aligned} \tag{3}$$

where $Q(0)$ denotes quantity sold when no tax is imposed ($\tau = 0$), η_{qp} denotes the elasticity of quantity sold in Philadelphia with respect to price, and $p(\tau)$ denotes the price level at tax rate τ. The second line follows by substituting in the following quantities from our earlier regressions: (1) elasticity: $\eta_{qp} = -46\%/34\% = -1.35$; (2) pass-through rate: $p(\tau) - p(0) = \frac{1.45}{1.5}\tau = 0.97\tau$; and (3) pre-tax price level: $p(0) = 4.26$.

Based on the calculation above, we can derive the revenue-maximizing tax rate and, more generally, the relationship between tax revenue and the tax rate τ. We plot this relationship in Figure 5. We find the revenue-maximizing tax rate is equal to 1.63 cents per ounce. The Laffer curve shows that the actual tax rate of 1.5 cents per ounce in Philadelphia generates revenue that is equal to 99.3% of the revenue generated at the revenue-maximizing tax rate. A lower tax rate of 1 cent per ounce generates 85.0% of potential revenue, whereas a higher rate of 2 cents per ounce generates 94.9% of potential tax revenue. A tax as high as 3 cents per ounce (which was contemplated in Philadelphia) would generate 29.8% of the maximum possible tax revenue. Therefore, even if the sole goal of the tax is revenue generation, high tax rates are sub-optimal because they severely shrink the tax base.

Using a similar calculation, we can also trace out the relationship between calorie and sugar intake and the tax rate. Based on the assumptions above, this relationship is linear in the tax rate. At the revenue maximizing rate of 1.63 cents per ounce, calorie intake is reduced by 17%. A tax rate of either 1 or 2 cents per ounce would reduce calorie intake by 11% and 21% respectively. The analogous calculations for sugar intake produce similar results. We note that the extrapolation of nutritional effects to other tax rates is based on the insignificant point estimate of the effect of the Philadelphia tax on calorie and sugar intake.

Geographical Coverage One of our key empirical findings is the large amount of substitution to sweetened beverages purchased at stores outside of Philadelphia. The extent to which consumers are able to engage in such cross-shopping is, of course, a function of the proximity of stores that are not subject to the tax. If a soda tax were to be implemented, for instance, at the state or even national level, engaging in cross-shopping would become much harder for consumers (Allcott et al. (2019b)).

To assess the change in purchases of taxed beverages when cross-shopping is eliminated, we first note that the aggregate demand change can be bounded. If all consumers who currently cross-shop instead ceased to purchase taxed beverages altogether, the reduction in sales would be equal to 46% (the impact at stores in Philadelphia). Instead, if all cross-shoppers continued to buy taxed beverages, sales would decrease by only 22% (the impact at Philadelphia stores minus the increase at border stores - see Table 5).

Our primary piece of evidence with regards to a counterfactual scenario without cross-shopping comes from the patterns of heterogeneity in demand response across pack sizes.[54] Because consumers engage in cross-shopping for large pack sizes but not for small pack sizes (see Table 6), we can analyze the change in demand for small pack sizes to gain insight into the change in total demand that would occur if cross-shopping were not possible. As reported earlier, we find a demand elasticity of -0.64 for small pack sizes. When applying this elasticity to total demand, the observed price increase of 34% translates into a quantity reduction of 22%, which is equal to the magnitude of the decrease in demand under the scenario where cross-shoppers continue to buy taxed beverages instead. We also note that we observe no substitution to bottled water, even for small pack-size sales where cross-shopping does not occur. Therefore, demand patterns for small pack sizes suggest the reduction in sales without cross-shopping would likely be close to 22% and cross-shoppers would continue to purchase taxed beverages in Philadelphia if cross-shopping were not available. These patterns suggest that applying the tax to a larger geographical area would likely not change nutritional intake.

[54] We also explore how purchase behavior at stores in Philadelphia differs as a function of distance to the city border. Intuitively, consumers who shop in stores near the city center are less likely to engage in cross-shopping because they are farther away from the city border. Thus, we would expect that demand decreases less at Philadelphia stores that are farther away from the border. Although we estimate the effect of distance to the border on demand to be positive (see Table 3), the effect is small in magnitude and not statistically significant. We also find that demand for untaxed beverages at stores in Philadelphia does not increase more with increasing distance from the city border. We conjecture that these null results with respect to distance are largely due to the fact that variation in distance to the border is limited, because the farthest stores are only 4 miles away from the city border.

To quantify the impact of tax avoidance via cross-shopping on tax revenue, we compute the Laffer curve based on a quantity reduction of 22% (the impact net of cross-shopping) rather than 46% (the impact in Philadelphia). In this scenario, the implied elasticity would be $\eta_{qp} = -22\%/34\% = -0.65$. At the implemented tax rate of 1.5 cents per ounce, tax revenue would be 1.45 times higher than the tax revenue generated currently. The revenue-maximizing tax rate would be much higher at 3.47 cents per ounce and would raise 2.13 times more revenue than the revenue-maximizing tax rate when demand is more elastic because consumers can cross-shop. We therefore conclude that applying the tax to a larger geographical area would raise significantly more tax revenue.

Our results on cross-shopping also generate predictions on how consumer welfare and distributional effects would be impacted by broadening the geographic coverage of the tax. With regards to welfare, consumers would be worse off, because the option of purchasing outside of the city at a lower price would no longer be available.[55] In terms of distributional effects, whether soda taxes are regressive depends on several factors such as preference heterogeneity across income groups and whether low-income consumers end up benefiting more from the re-investment of tax revenue (Allcott et al. (2019a)). Furthermore, a differential ability across the income spectrum to avoid the tax via cross-shopping is an additional force that may make local taxes regressive. Our results suggest that broadening the tax's geographic coverage would have a bigger impact on high-income consumers, since low-income consumers are less likely to engage in cross-shopping. Therefore, applying the tax over a larger geographic area may distribute the tax burden more equally across the income spectrum.

Product Coverage Apart from the geographic dimension, the tax base could also be altered along the product dimension. Among the existing soda-tax regimes, most cities tax only sugar-sweetened beverages, but exclude artificially sweetened drinks, whereas Philadelphia also taxes artificially sweetened drinks. To understand the impact of excluding diet drinks, we need to understand substitution patterns between diet and regular drinks. Unfortunately, because the Philadelphia tax does not induce a change in the relative price of diet and SSBs, estimating cross-elasticities between those two sets of products is difficult. However, elsewhere in the literature, estimates show that consumers are more likely to substitute from SSBs to diet drinks than to bottled water (see, e.g., Wang (2015) and Allcott et al. (2019a)). Therefore, if taxing only SSBs, we would expect a larger degree of substitution away from SSBs. The smaller tax base would therefore lead to lower tax revenue. At the same time, nutritional intake would likely improve.

Finally, even outside of the sugar-sweetened / diet distinction, large heterogeneity is present in calorie and sugar content across products, and hence one could implement a tax scheme that more directly targets nutritional content, such as a tiered tax structure based on calorie / sugar levels or even a direct tax on calories or sugars (rather than a volume tax conditional on any caloric or artificial sweetener). The fact that consumers in Philadelphia predominantly substitute away from

[55] Because broader geographic coverage would likely not lead to changes in nutritional intake (as we argue above), externalities and internalities (see Allcott et al. (2019a)) related to nutritional intake would also remain unaltered.

healthier taxed beverages indicates such a scheme might be more effective at moving consumers toward healthier beverages.

6 Conclusion

We use detailed scanner data from a large set of stores in Philadelphia to evaluate the impact of a sweetened-beverage tax. We find the tax was almost fully passed through at most stores. Although some decrease occurs in the aggregate consumption of taxed beverages, the magnitude of the decrease is reduced considerably because consumers avoid the tax by cross-shopping. Due to cross-shopping and compositional changes in demand, we are not able to detect a significant improvement in nutritional intake. Furthermore, the large amount of cross-shopping reduces the tax base and therefore limits the ability to raise tax revenue. Finally, the tax imposes a relatively larger financial burden on low-income households that are less likely to engage in cross-shopping at stores outside of the city.

Our findings in the case of the Philadelphia tax also provide lessons with regards to the broader question of how to design soda taxes or other types of sin taxes. If taxes are localized (as is the case for all current soda taxes), high tax rates will be sub-optimal for generating revenue because they lead to cross-shopping, which reduces the tax base. Furthermore, altering the tax base along the geographic or product dimension will likely have a different impact on tax revenue and nutritional intake. A larger geographic coverage will make cross-shopping more difficult and therefore generate greater tax revenue. A narrower product coverage instead will improve nutritional intake at the expense of lower tax revenue.

References

Aguilar, A., E. Gutierrez, and E. Seira (2016): "Taxing to Reduce Obesity," *Working Paper.*

Allcott, H., B. B. Lockwood, and D. Taubinsky (2019a): "Regressive Sin Taxes, with an Application to the Optimal Soda Tax," *Quarterly Journal of Economics.*

——— (2019b): "Should We Tax Soda? An Overview of Theory and Evidence," *Journal of Economic Perspectives.*

Asplund, M., RichardFriberg, and F. Wilander (2007): "Demand and distance: Evidence on cross-border shopping," *Journal of Public Economics*, 91, 141–157.

Berardi, N., P. Sevestre, M. Tepaut, and A. Vigneron (2016): "The impact of a 'soda tax' on prices: evidence from French micro data," *Applied Economics*, 48, 3976–3994.

Bergman, U. M. and N. L. Hansen (2017): "Are Excise Taxes on Beverages Fully Passed Through to Prices? The Danish Evidence," *Working Paper, University of Copenhagen.*

Bollinger, B. and S. E. Sexton (2018): "Local Excise Taxes, Sticky Prices, and Spillovers: Evidence from Berkeley's Soda Tax," *Working Paper.*

Cawley, J., C. Crain, D. Frisvold, and D. Jones (2018a): "The Pass-Through of the Largest Tax on Sugar-Sweetened Beverages: The Case of Boulder, Colorado," *NBER Working Paper.*

Cawley, J. and D. Frisvold (2017): "The Incidence of Taxes on Sugar-Sweetened Beverages: The Case of Berkeley, California," *Journal of Policy Analysis and Management*, 36, 302–326.

Cawley, J., D. Frisvold, A. Hill, and D. Jones (2018b): "The Impact of the Philadelphia Beverage Tax on Prices and Product Availability," *NBER Working Paper.*

——— (2018c): "The Impact of the Philadelphia Beverage Tax on Purchases and Consumption by Adults and Children," *NBER Working Paper.*

Cawley, J., B. Willage, and D. Frisvold (2018d): "Pass-Through of a Tax on Sugar-Sweetened Beverages at the Philadelphia International Airport," *JAMA, the Journal of the American Medical Association*, 319.

CDC (2013): "Communities Putting Prevention to Work: Philadelphia, Pennsylvania," Online Report.

——— (2016): "Adult Obesity Facts," Online Report.

CDC/NCCDPHP/DPH/ESB (2016): "500 Cities Project: Local Data for Better Health. 2014. Philadelphia, PA," Tech. rep.

Colchero, M. A., J. Rivera-Dommarco, B. M. Popkin, and S. W. Ng (2017): "In Mexico, Evidence of Sustained Consumer Response Two Years after Implementing a Sugar-Sweetened Beverage Tax," *Health Affairs*, 36, 564–571.

Conlon, C. T. and N. S. Rao (2015): "The Price of Liquor is Too Damn High: Alcohol Taxation and Market Structure," Working Paper.

——— (2016): "Discrete Prices and the Incidence and Efficiency of Excise Taxes," Working Paper.

Dubois, P., R. Griffith, and M. O'Connell (2017): "How well targeted are soda taxes," *Working Paper.*

Erdem, T., S. Imai, and M. P. Keane (2003): "Brand and Quantity Choice Dynamics Under Price Uncertainty," *Quantitative Marketing and Economics*, 1, 5–64.

Esterl, M. (2016): "Philadelphia Mayor to Propose Soda Tax," *The Wall Street Journal.*

Falbe, J., N. Rojas, A. H. Grummon, and K. A. Madsen (2015): "Higher Retail Prices of Sugar-Sweetened Beverages 3 Months After Implementation of an Excise Tax in Berkeley, California," *American Journal of Public Health*, 105, 2194–2201.

Falbe, J., H. R. Thompson, C. M. Becker, N. Rojas, C. E. McChulloch, and K. A. Madsen (2016): "Impact of the Berkeley Excise Tax on Sugar-Sweetened Beverage Consumption," *American Journal of Public Health.*

Ferris, J. S. (2000): "The Determinants of Cross Border Shopping: Implications for Tax Revenues and Institutional Change," *National Tax Journal*, 53, 801–824.

Finkelstein, E. A., J. G. Trogdon, J. W. Cohen, and W. Dietz (2009): "Annual Medical Spending Attributable to Obesity: Payer- and Service-Specific Estimates," *Health Affairs*, 28.

Fiorillo, V. (2017): "ShopRite VP Respondes to Accusations of Price-Gouging and Fraud," *Philadelphia.*

Griffith, R., L. Nesheim, and M. O'Connell (2018): "Income effects and the welfare consequences of tax in differentiated product oligopoly," *Quantitative Economics*, 9, 305–341.

Grogger, J. (2017): "Soda Taxes and the Prices of Sodas and Other Drinks: Evidence from Mexico," *American Journal of Agricultural Economics*, 99, 481–498.

Harding, M., E. Leibtag, and M. F. Lovenheim (2012): "The Heterogeneous Geographic and Socioeconomic Incidence of Cigarette Taxes: Evidence from Nielsen Homescan Data," *American Economic Journal: Economic Policy*, 4, 169–198.

Hendel, I. and A. Nevo (2006): "Measuring the Implications of Sales and Consumer Inventory Behavior," *Econometrica*, 74, 1637–1673.

Hollenbeck, B. and K. Uetake (2018): "Taxation and Market Power in the Legal Marijuana Industry," Working Paper.

Jacobi, L. and M. Sovinsky (2016): "Marijuana on Main Street? Estimating Demand in Markets with Limited Access," *American Economic Review*, 106, 2009–2045.

Khan, R., K. Misra, and V. Singh (2016): "Will a Fat Tax Work?" *Marketing Science*, 35, 10–26.

Kifer, A. (2015): "The Incidence of a Soda Tax, in Pennies and Pounds," *Working Paper*.

Lovenheim, M. F. (2008): "How far to the border?: The extent and impact of cross-border casual cigarette smuggling." *National Tax Journal*, 61, 7–33.

Manson, S., J. Schroeder, D. Van Riper, and S. Ruggles (2017): *IPUMS National Historical Geographic Information System: Version 12.0 [Database]*, Minneapolis: University of Minnesota.

Merriman, D. (2010): "The Micro-geography of Tax Avoidance: Evidence from Littered Cigarette Packs in Chicago," *American Economic Journal: Economic Policy*, 2, 61–84.

Miravete, E. J., K. Seim, and J. Thurk (2017): "One Markup to Rule Them All: Taxation by Liquor Pricing Regulation," Working Paper.

——— (2018): "Market Power and the Laffer Curve," *Econometrica*, 86, 1651–1687.

National Cancer Institute (2018): "Sources of Calories from Added Sugars among the U.S. Population, 2005-06," Tech. rep., Epidemiology and Genomics Research Program website.

OECD (2017): "Obesity Update 2017," Technical Report.

Ogden, C. L., M. D. Carroll, C. D. Fryar, and K. M. Flegal (2015): "Prevalence of Obesity Among Adults and Youth: United States, 2011-2014," *NCHS Data Brief*, 219.

Roberto, C., H. Lawman, M. LeVasseur, N. Mitra, A. Peterhans, B. Herring, and S. N. Bleich (2019): "Association of a Beverage Tax on Sugar-Sweetened and Artificially Sweetened Beverages with Changes in Beverage Prices and Sales at Chain Retailers in a Large Urban Setting," *JAMA, the Journal of the American Medical Association*, 321, 1799–1810.

Rojas, C. and E. Wang (2017): "Do Taxes for Soda and Sugary Drinks Work? Scanner Data Evidence from Berkeley and Washington," *Working Paper*.

Silver, L. D., S. W. Ng, S. Ryan-Ibarra, L. S. Taillie, M. Induni, D. R. Miles, J. M. Poti, and B. M. Popkin (2017): "Changes in prices, sales, consumer spending, and beverage consumption one year after a tax on sugar-sweetened beverages in Berkeley, California, US: A before-and-after study," *PLoS Medicine*.

Taylor, R. L. C., S. Kaplan, S. B. Villas-Boas, and K. Jung (2019): "Soda Wars: The Effect of a Soda Tax Election on University Beverage Sales," *Economic Inquiry*, forthcoming.

Thomassen, Ø., H. Smith, S. Seiler, and P. Schiraldi (2017): "Multi-Category Competition and Market Power: A Model of Supermarket Pricing," *American Economic Review*, 107, 2308–2351.

Wang, E. Y. (2015): "The impact of soda taxes on consumer welfare: implications of storability and taste heterogeneity," *The RAND Journal of Economics*, 46, 409–441.

A Selection of Stores and Products for the Analysis

Our raw data cover geographic areas with three-digit ZIP codes of 080, 081, 190, 191, and 194, which cover Philadelphia and its surrounding areas in Pennsylvania and New Jersey. We remove 28 stores in Ocean County, NJ, which is 50 miles away from Philadelphia. The resulting data contain 1,538 stores and 17,582 UPCs that belong to 462 brands. A portion of the stores and UPCs are not contained in our final data set. We detail the criteria for dropping those observations below.

First, some stores enter or exit during the sample period. We choose to drop stores that entered after January 1, 2016, or exited before December 31, 2017. As a result, for each remaining store, we have at least one year of data both before and after the tax went into effect on January 1, 2017. Next, we remove stores affiliated with retail chains that only operate within Philadelphia or only outside the city. Finally, some UPCs were purchased infrequently at a given store. Given the nature of scanner data, infrequent purchases render it difficult to reliably measure a product's price over time.[56] We choose to only keep product/stores (a product is defined as a brand/diet-status/pack-size combination) that had sales in at least 40 weeks each year during 2016 and 2017 and to only keep UPC/store-combinations that had sales in at least 85% of the weeks that the corresponding product/store was observed in the data.[57] Our final data contain 1,227 stores and 5,070 UPCs that belong to 101 brands. This final dataset represents 89% of the unit sales in the raw data.

B Heterogeneity in Price Recording Across Retailers

As outlined in footnote 13, one retailer in our dataset reported the soda tax as a separate item on the checkout receipt rather than reporting the total price including the tax. Figure A1 shows two receipts from this retailer – one from 1/1/2017 and a recent receipt from 10/18/2019 – in which the Philadelphia soda tax is reported as a separate item (see highlighted area of the receipt). In contrast, Figure A2 shows that on the shelf tags, this retailer reports the price inclusive of the tax, along with a note that that price includes the Philadelphia beverage tax. We have confirmed that the shelf tag price is equal to the sum of the price and the tax as reported on the receipt. Further, we compared receipts to the relevant prices in the IRI data to establish that throughout our sample period, our data reports price net of the tax for this retailer. Thus, in order to recover the effective price paid by consumers, we need to add the 1.5 cents/oz tax onto this retailer's prices starting in January 2017.

Figure A3 shows two additional receipts from two other retailers, where the tax is not broken out as a separate item, but the price includes the tax. We verified that all retailers except for the

[56] Price is not recorded in weeks in which a specific product was not sold in a given store. Therefore, prices need to be interpolated from adjacent weeks in which prices were recorded.

[57] If a UPC is dropped, but other UPCs belonging to the same product are maintained because they meet the criteria described above, we include quantity information for the dropped UPC in the aggregate-volume calculation for the specific product. We do not use its price information (and only use maintained UPCs to form the product-level average price).

one in Figure A1 report the tax in this fashion. We also compared receipts from these retailers to the relevant prices in the IRI data to establish that our data reports total price inclusive of the tax for all retailers except the one in Figure A1.

Figure A1: **Sample receipts with separate beverage tax reporting.** The two receipts are from the same retailer on two different dates. (The day the tax was introduced 1/1/2017 and a recent receipt from 10/18/2019. See dates in the top left corner of both receipts.)

Figure A2: **Sample price tag with beverage tax included in price.** The price tag shown above is from the retailer that reports the tax separately on the receipt (see Figure A1). Unlike the reporting on the receipt, the shelf tag includes the tax in the product price. This shelf tag was photographed on 10/18/2019 and corresponds to the item purchased in the receipt on the right side of Figure A1. The shelf tag reports a price of $2.25, which is the sum of the price $1.95 and the tax $0.30 reported on the receipt.

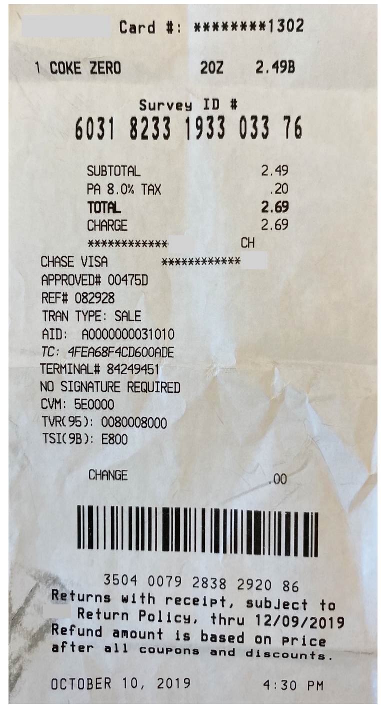

Figure A3: **Sample receipts from two retailers where the beverage tax is not reported separately.** These receipts were both obtained on 10/10/2019. Unlike the receipts in Figure A1, both retailers do not report the beverage tax as a separate item on the receipt or on the shelf tags. This method of reporting prices inclusive of the tax occurs at all retailers in our data except for the one retailer in Figure A1.

C Store Coverage in IRI Data

In this section, we assess whether our sample of stores is representative of all stores located in Philadelphia and the surrounding areas. To this end, we obtained data from IRI on the universe of stores in the relevant geographic areas. Out of all the stores listed, only a subset have their volume and prices tracked by IRI. Relative to the primary data used throughout the paper, the list of stores is cross-sectional and does not contain information on store entry and exit. We therefore assess how tracked stores (regardless of when they enter or exit) compare to the universe of stores.

Due to concerns about data privacy, we do not report coverage rates in levels, but only report *differences* in coverage rates between various groups of stores. Out of all potential stores, more than 60% are tracked by IRI. We assess selection into being tracked along two dimensions that are particularly relevant for our analysis, namely, along the geographic dimension and across store formats. Geographic coverage, which we analyze in the top panel of Table A1, is particularly important because we contrast the change in demand in Philadelphia with the change in stores near the city border when calculating the change in total demand. When considering all types of stores, we find the coverage is very similar. The difference between coverage in Philadelphia and coverage up to 6 miles outside the city is only 0.6 percentage points, and this difference is not

Geographic Coverage	Diff. in Coverage Rate Phil. Minus 6 Miles Outside		P-value Diff. in Means Test			
All Stores	0.006		0.793			
Small Format	0.034		0.220			
Large Format	-0.098		0.068			
Grocery Stores	-0.096		0.128			
Mass Merchants	0.000		1.000			
Wholesale Clubs	0.111		0.588			
Drug Stores	-0.059		0.147			
Convenience Stores	0.039		0.322			
Dollar Stores	0.068		0.315			
Store Format Coverage	Diff. in Coverage Rate Small Minus Large Format					P-value Diff. in Means Test
	0.019					0.535
	Diff. in Coverage Rate Relative to Convenience Stores					
	Drug Stores	Dollar Stores	Grocery Stores	Mass Merch.	Wholes. Club	
	0.247	0.117	0.001	0.325	0.367	0.000

Table A1: **Coverage in IRI (versus Universe of Stores).** Coverage rates calculated as the fraction of stores (of a particular type) that are tracked by IRI.

statistically significant. We also assess geographic coverage by store format, first by small versus large stores (small-format stores comprise Drug Stores, Convenience Stores, and Dollar Stores; large-format stores comprise Grocery Stores, Mass Merchants, and Wholesale Club Stores) and then separately for the six different formats. We do not find a significant difference in coverage for any of these groups of stores.

In the lower panel of Table A1, we focus on the format dimension. The most important aspect in terms of coverage by format is larger versus smaller formats of stores, because larger stores sell significantly more quantity of taxed beverages than do smaller stores as documented in Table 1. Furthermore, quantity decreases more at larger stores in reaction to the tax (see Table 3), partly because large stores tend to sell larger pack sizes (which are more affected by cross-shopping). We find that coverage rates for large- and small-format stores are not significantly different from each other. We note that when splitting the sample more granularly into six separate formats, we do find significant differences in coverage. However, we regard this more granular split as less relevant for our main regression results, because coverage at the more aggregate level of large- and small-format stores does not differ.

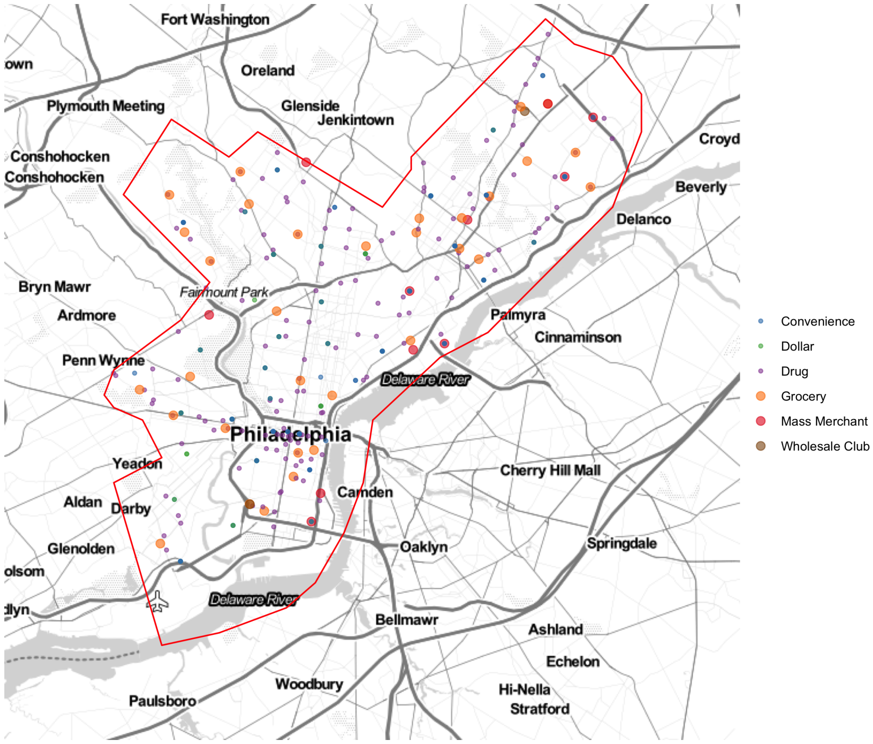

Figure A4: Philadelphia Stores by Retail Format

As a final check, we use the coverage rates by format and geography to re-weight the results from some of our main regressions. First, we apply geography-specific weights to the cross-shopping regressions. In the case of column (1) of Table 5, we find the re-weighted effect with (without) cross-shopping is equal to a 21% (46%) reduction in quantity compared to a 22% (46%) reduction without re-weighting. We also compute the average pass-through and quantity change based on weighting the chain-specific results in column (2) of Table 2 and column (2) of Table 3 by the appropriate format-specific weights (based on the six different formats). We find the weighted (unweighted) pass-through is equal to 1.481 (1.449) and the weighted (unweighted) quantity change is equal to -61,959 (-56,192).

D Additional Store Descriptive Statistics

Figure A4 shows a map of Philadelphia stores, color-coded by retail format. The map shows the stores in our sample are geographically dispersed. Table A2 summarizes the within- and across-chain variation in demographics. Looking at the average income across chains shows that Grocery A and Drugstore X stores are, on average, located in higher-income neighborhoods, whereas Grocery C and dollar stores tend to be located in lower-income neighborhoods. Moreover, the chain-specific standard deviations tend to be only slightly smaller than the standard deviations across all stores. Therefore, we are able to analyze the impact of demographics based on the variation in demograph-

	# Stores in Phil.	Median Income ($1,000s) Mean	Std. Dev	Obesity Rate Mean	Std. Dev
Grocery A	15	53.7	11.7	0.26	0.03
Grocery B	1	41.6	-	0.26	-
Grocery C	16	37.9	11.2	0.32	0.06
Mass Merchant M	6	47.7	7.7	0.28	0.05
Other Mass Merchants	5	45.8	10.5	0.28	0.04
Drugstore X	45	50.9	14.5	0.27	0.05
Drugstore Y	80	43.2	14.7	0.30	0.06
Drugstore Z	17	44.0	15.6	0.29	0.07
Convenience St.	116	45.2	14.6	0.28	0.06
Wholesale Club W	2	41.4	7.8	0.27	0.05
Dollar Stores	54	36.1	11.8	0.33	0.05
All Stores	357	44.1	14.5	0.29	0.06

Table A2: **Within and Across-Chain Variation in Demographics.**

ics within stores of the same chain.

E Robustness Check: Parallel Time Trends

Our difference-in-differences approach to estimating the impact of the tax relies on the assumption that the treatment and control groups would follow the same time trend in the absence of treatment. Because we have data for multiple time periods before and after treatment, we are able to flexibly control for differential time trends while still estimating the treatment effect of the tax. We do so by including a third-order polynomial time trend interacted with the Philadelphia dummy variable in several of our main regressions. (In all regressions, we control for a "baseline" time trend using a full set of week fixed effects.) We find the differential time-trend variables are jointly insignificant in all of our main regressions and the estimated coefficients of interest do not change significantly when including the additional time-trend controls. For example, when including time-trend controls in the regressions reported in column (1) of Tables 2, 3, and 4, we find p-values of 0.50, 0.16, and 0.33 for a test of joint significance of the differential time-trend variables. We conclude that the similarity between time trends in the treatment and control groups before and after the tax is implemented supports the parallel-trends assumption underlying our identification strategy.

F Robustness Check: Clustering

In our main specification, we use two-way clustering at the store and week level. As an additional robustness check, we explore the sensitivity of our results to clustering at a higher level of ag-

	Taxed Products				*Untaxed Products*		*Cross-shopping*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent Variable	Price/Oz	Price/Oz	Ounces Sold	Ounces Sold	Ounces Sold	Ounces Sold	Ounces Sold	Ounces Sold
Clustering	Store and Week	Store and Month	Store and Week	Store and Month	Store and Week	Store and Month	Store and Week	Store and Month
Philadelphia	1.449***	1.449***	-56,192***	-56,192***	-4,521	-4,521	-56,193***	-56,193***
× After Tax	(0.022)	(0.024)	(9,740)	(9,865)	(7,118)	(7,063)	(9,740)	(9,864)
0-2 Miles Outside							63,650***	63,650***
City × After Tax							(20,733)	(20,706)
2-4 Miles Outside							18,364***	18,364**
City × After Tax							(7,031)	(7,022)
4-6 Miles Outside							8,640**	8,640**
City × After Tax							(4,196)	(4,166)
Store FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Week FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	144,700	144,700	144,700	144,700	144,209	144,209	213,499	213,499
Stores	832	832	832	832	829	829	1,227	1,227
Weeks	176	176	176	176	176	176	176	176
Months	41	41	41	41	41	41	41	41

Table A3: **Robustness Check: Store/Month Clustering.** Some stores do not offer all categories of beverages and hence the number of observations differs slightly across columns.

gregation. Table A3 reports results for our main specifications, when clustering standard errors at the store and *month* level. In columns (1), (3), (5), and (7), we replicate our earlier results with store/week clustering, and the remaining columns show results when using a higher level of clustering. Across all four regressions, standard errors only change minimally. In most cases, they are slightly larger when clustering at the month level, but not uniformly so (see, e.g., the three distance-band coefficients in the cross-shopping regression). We also probed robustness to clustering at the county rather than the store level (not reported in the table). We find that county-level clustering (combined with either week- or month-level clustering) leads to lower standard errors in almost all regressions.

G Volatility of Purchases and Changes in Price Sensitivity

Figure 3 shows that in the case of 2-liter bottles of a popular soda brand, purchase volume decreases in Philadelphia relative to the control group. Furthermore, the volatility of sales in stores in Philadelphia also decreases after the tax goes into effect. In this section, we show this pattern occurs for taxed beverages more broadly, and it is driven by the fact that the most price-sensitive consumers start to engage in cross-shopping after the tax went into effect. Therefore, the set of consumers who continue to purchase taxed beverages in Philadelphia after the tax constitutes a selected set of less price-sensitive consumers. Those price-insensitive consumers react less to temporal movements in price, and therefore the volatility in sales decreases.

We first turn to analyzing the change in volatility across all taxed beverages. To this end, we compute the variance of sales for each store/product combination separately for the pre- and post-tax period. We find that in Philadelphia, the standard deviation of sales decreased by more than 50%, falling from 1,270 to 443, whereas in control stores outside of the city (excluding the buffer zone), the variance of sales decreased only slightly from 1,206 to 1,130. We also note that during the same time, the volatility of price movements remained unchanged in Philadelphia and outside of the city. This finding suggests the nature of demand changed in a way that led to a decrease in the volatility of sales over time.

To assess the cause of this change more directly, we estimate the average product-level elasticity separately for the pre-/post-tax period and for stores inside and outside of the city. We estimate the elasticity by regressing (at the product level) log quantity on log price, store/product-pair fixed effects, and week fixed effects. We find the product-level elasticity of demand dropped from -2.00 to -1.23 at stores in Philadelphia, whereas at control-group stores, it decreased by a more modest amount from -2.21 to -1.87. We therefore conclude that consumers who continue to purchase taxed beverages in Philadelphia are less price sensitive than the average pre-tax consumer who purchased in Philadelphia. This pattern is consistent with the idea that the consumers who start to engage in cross-shopping are the most price-sensitive consumers. Hence, consumers who continue to purchase sweetened beverages in Philadelphia will tend to be less price sensitive.

H Dynamics

Dynamic adjustment patterns could occur because retailers and consumers take some time to adjust their behavior in response to the tax. Furthermore, consumers might engage in tax avoidance via cross-shopping immediately after the tax goes into effect but find doing so in the long run is inconvenient. To investigate the importance of changes in the impact of the tax over time, we categorize the post-tax data into four time periods: January to April of 2017, May to August of 2017, September to December of 2017, and January 2018 to September 2018 (the end of our sample period). We then re-estimate several of our main regressions, allowing for different treatment effects in the four post-tax time periods. Table A4 reports the results of these analyses. For ease of comparison, in columns (1), (3), (5), and (7), we replicate the results in column (1) of Tables 2, 3, 4, and 5, respectively (which exclude the first four months after the tax was introduced).

In column (2), we test for changes in the pass-through rate over time. Besides the interaction of the Philadelphia dummy with the after-tax dummy, we now add further interactions of the Philadelphia dummy with dummies for the time periods January-April of 2017, May-August of 2017, and September-December of 2017. Accordingly, the Philadelphia times after-tax coefficient now captures the long-run impact of the tax on prices for the final period of our sample from

January to September 2018. The other three interaction terms capture differences in short-term price adjustments relative to the long-run pass-through rate. The interaction term of January-April of 2017 is equal to -0.31 and significant, which indicates that the pass-through rate was at a slightly lower level during the first four months after the tax. In comparison, the interaction terms for May-August and September-December of 2017 are small in magnitude and not significantly different from the pass-through rate in 2018, which implies pass-through remained stable after May 2017.

The remaining columns present similar specifications regarding the impact of the tax on quantity sales of taxed products, quantity sales of untaxed products, and cross-shopping behavior. In the case of quantity sold, we find the sales reduction in the first four months is smaller than the long-run decrease of 55,600 ounces. After May 2017, the change in quantity is not distinguishable from the long-run decrease. Sales of untaxed products are unresponsive in the long run and show no short-run reaction to the tax either. With regards to cross-shopping effects, we find most estimates to be insignificant for the various time periods in 2017. A few individual coefficients in this regression are significant. When we run a set of joint-significance tests of the three distance-band coefficients in each time period, we find that in all cases, we cannot reject that the three coefficients are equal to zero. We conclude that after a brief adjustment period of four months, prices and quantities sold stabilized and show no sign of further adjustments between May 2017 and September 2018.

I Non-Price Effects of the Tax

Apart from prices, other marketing variables such as advertising and in-store product displays might also adjust in response to the tax, and these marketing variables could have an impact on quantity sales. In terms of advertising, all stores in the treatment and control groups are located in the Philadelphia DMA, and hence the effects we measure are not driven by differences in television ad exposure. We do not observe data on in-store planograms that detail shelf layouts, nor do we observe the frequency of feature advertising or end-of-aisle displays. Thus, the effects we measure may reflect changes in these marketing variables in Philadelphia relative to the control group.

In addition, the tax was discussed publicly before it was passed and was therefore likely salient to many consumers. We might expect that the announcement of the tax and the discussion surrounding health effects of sweetened beverages might lead consumers to switch to healthier beverages independent of price changes (Taylor et al. (2019)). However, the null effect we find with regards to substitution to healthier beverages speaks against such a salience effect playing a large role, because we would expect salience to trigger switches to healthier beverages, but not cross-shopping for sweetened beverages. We also find no evidence that demand for taxed beverages in Philadelphia decreased in the months prior to January 2017, when the upcoming introduction of the tax would already have been salient to consumers. For these reasons, we believe that the quantity decreases we observe are primarily a direct response to price changes.

	Taxed Products				Untaxed Products		Cross-shopping	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent Variable	Price/Oz	Price/Oz	Ounces Sold	Ounces Sold	Ounces Sold	Ounces Sold	Ounces Sold	Ounces Sold
Philadelphia × AfterTax	1.449***	1.451***	-56,192***	-55,612***	-4,521	-5,437	-56,193***	-55,612***
	(0.022)	(0.025)	(9,740)	(10,162)	(7,118)	(7,041)	(9,740)	(10,163)
Philadelphia × Jan-April 2017		-0.309***		18,982***		3,847		18,982***
		(0.064)		(5,145)		(4,097)		(5,150)
Philadelphia × May-Aug 2017		-0.002		-6,230		4,990		-6,230
		(0.020)		(4,791)		(3,184)		(4,817)
Philadelphia × Sept-Dec 2017		-0.008		3,537		-929		3,537
		(0.015)		(4,661)		(3,270)		(4,704)
0-2 Miles Outside × AfterTax							63,650***	61,787***
							(20,733)	(20,164)
0-2 Miles Outside × Jan-April 2017								-12,196
								(8,007)
0-2 Miles Outside × May-Aug 2017								2,846
								(2,498)
0-2 Miles Outside × Sept-Dec 2017								4,736***
								(1,818)
2-4 Miles Outside × AfterTax							18,364***	17,017**
							(7,031)	(6,664)
2-4 Miles Outside × Jan-April 2017								-2,538
								(3,187)
2-4 Miles Outside × May-Aug 2017								3,029
								(2,575)
2-4 Miles Outside × Sept-Dec 2017								2,544**
								(1,017)
4-6 Miles Outside × AfterTax							8,640**	7,819*
							(4,196)	(4,267)
4-6 Miles Outside × Jan-April 2017								1,334
								(2,739)
4-6 Miles Outside × May-Aug 2017								2,800
								(2,944)
4-6 Miles Outside × Sept-Dec 2017								617
								(1,778)
Store FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Week FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	144,700	159,676	144,700	159,676	144,209	159,131	213,499	235,585
Stores	832	832	832	832	832	832	1,227	1,227
Weeks	176	194	176	194	176	194	176	194

Table A4: **Dynamic Adjustment Patterns.** Columns (1), (3), (5), and (7) replicate earlier results and are based on a sample that excludes the first four months after the tax was introduced.

J Additional Tables

Dependent Variable	(1) Price/Oz	(2) Price/Oz	(3) Log Price/Oz	(4) Log Price/Oz	(5) Log Price/Oz	(6) Log Price/Oz
Philadelphia × AfterTax	1.459***					
	(0.020)					
Grocery A		1.308***	0.344***	0.340***	0.351***	0.342***
× Philadelphia × AfterTax		(0.025)	(0.010)	(0.015)	(0.012)	(0.010)
Grocery B		1.518***	0.414***	0.406***	0.417***	0.411***
× Philadelphia × AfterTax		(0.004)	(0.001)	(0.021)	(0.005)	(0.004)
Grocery C		1.838***	0.466***	0.462***	0.469***	0.461***
× Philadelphia × AfterTax		(0.055)	(0.015)	(0.019)	(0.017)	(0.017)
Mass Merchant M		1.441***	0.318***	0.315***	0.323***	0.314***
× Philadelphia × AfterTax		(0.270)	(0.061)	(0.064)	(0.061)	(0.060)
Mass Merchant N		1.084***	0.303***	0.298***	0.308***	0.299***
× Philadelphia × AfterTax		(0.029)	(0.008)	(0.015)	(0.009)	(0.009)
Drugstore X		1.536***	0.300***	0.296***	0.306***	0.297***
× Philadelphia × AfterTax		(0.039)	(0.012)	(0.016)	(0.014)	(0.013)
Drugstore Y		1.321***	0.250***	0.246***	0.254***	0.245***
× Philadelphia × AfterTax		(0.020)	(0.007)	(0.012)	(0.009)	(0.009)
Drugstore Z		0.935***	0.179***	0.174***	0.183***	0.174***
× Philadelphia × AfterTax		(0.068)	(0.008)	(0.015)	(0.010)	(0.010)
Wholesale Club		1.411***	0.439***	0.433***	0.442***	0.436***
× Philadelphia × AfterTax		(0.073)	(0.009)	(0.019)	(0.010)	(0.009)
Dollar Stores		1.456***	0.360***	0.355***	0.362***	0.353***
× Philadelphia × AfterTax		(0.042)	(0.012)	(0.017)	(0.012)	(0.016)
Convenience Stores		1.602***	0.197***	0.193***	0.202***	0.193***
× Philadelphia × AfterTax		(0.025)	(0.005)	(0.013)	(0.007)	(0.007)
Distance (in Miles) to Border				0.002		
× Philadelphia × AfterTax				(0.005)		
Income					-0.013	
× Philadelphia × AfterTax					(0.012)	
Obesity Rate						0.010
× Philadelphia × AfterTax						(0.014)
($AfterTax_t \times \mathbf{X}'_s$) Interactions	n/a	Yes	Yes	Yes	Yes	Yes
Store FE	Yes	Yes	Yes	Yes	Yes	Yes
Week FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	144,700	144,700	144,700	144,700	144,700	144,700
Stores	832	832	832	832	832	832
Weeks	176	176	176	176	176	176

Table A5: **Impact on Prices / Pass-through Rate Estimates for the Soda Category.** Interactions with an after-tax dummy (the ($AfterTax_t \times \mathbf{X}'_s$) term) are included in columns (2) - (6), but not reported separately. One exception is the obesity variable in column (6). We have no obesity data outside of Philadelphia, and hence no ($Obesity_s \times AfterTax_t$) term is included.

Dependent Variable	(1) Ounces Sold	(2) Ounces Sold	(3) Log Ounces	(4) Log Ounces	(5) Log Ounces	(6) Log Ounces
Philadelphia × AfterTax	-18,713***					
	(-3,909)					
Grocery A		-103,199***	-0.810***	-0.818***	-0.671***	-0.826***
× Philadelphia × AfterTax		(-18,693)	(0.080)	(0.092)	(0.091)	(0.083)
Grocery B		-163,577***	-0.891***	-0.906***	-0.787***	-0.909***
× Philadelphia × AfterTax		(-5,162)	(0.012)	(0.079)	(0.030)	(0.018)
Grocery C		-237,319***	-1.024***	-1.032***	-0.928***	-1.057***
× Philadelphia × AfterTax		(-42,436)	(0.087)	(0.092)	(0.088)	(0.093)
Mass Merchant M		-33,289	-0.554**	-0.560**	-0.427*	-0.578***
× Philadelphia × AfterTax		(-20,205)	(0.222)	(0.236)	(0.225)	(0.211)
Mass Merchant N		-130,308***	-0.492***	-0.501***	-0.378***	-0.515***
× Philadelphia × AfterTax		(-26,802)	(0.104)	(0.116)	(0.105)	(0.106)
Drugstore X		-2,692***	-0.229***	-0.237***	-0.098*	-0.250***
× Philadelphia × AfterTax		(-779)	(0.047)	(0.063)	(0.055)	(0.052)
Drugstore Y		-427***	0.004	-0.004	0.110**	-0.026
× Philadelphia × AfterTax		(-146)	(0.038)	(0.057)	(0.048)	(0.045)
Drugstore Z		16,053***	0.701***	0.692***	0.806***	0.676***
× Philadelphia × AfterTax		(-3,048)	(0.104)	(0.116)	(0.101)	(0.106)
Wholesale Club		-73,161***	-0.802***	-0.814***	-0.679***	-0.821***
× Philadelphia × AfterTax		(-6,733)	(0.065)	(0.090)	(0.083)	(0.067)
Dollar Stores		-8,085***	-0.416***	-0.425***	-0.338***	-0.453***
× Philadelphia × AfterTax		(-1,349)	(0.039)	(0.059)	(0.044)	(0.049)
Convenience Stores		385	-0.024	-0.033	0.094**	-0.048*
× Philadelphia × AfterTax		(-322)	(0.021)	(0.048)	(0.039)	(0.027)
Distance (in Miles) to Border				0.003		
× Philadelphia × AfterTax				(0.018)		
Income					-0.202***	
× Philadelphia × AfterTax					(0.053)	
Obesity Rate						0.062
× Philadelphia × AfterTax						(0.052)
$(AfterTax_t \times \mathbf{X}'_s)$ Interactions	n/a	Yes	Yes	Yes	Yes	Yes
Store FE	Yes	Yes	Yes	Yes	Yes	Yes
Week FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	144,700	144,700	144,700	144,700	144,700	144,700
Stores	832	832	832	832	832	832
Weeks	176	176	176	176	176	176

Table A6: **Impact on Quantity Sold for the Soda Category.** Interactions with an after-tax dummy (the $(AfterTax_t \times \mathbf{X}'_s)$ term) are included in columns (2) - (6), but not reported separately. One exception is the obesity variable in column (6). We have no obesity data outside of Philadelphia, and hence no $(Obesity_s \times AfterTax_t)$ term is included.

	All Beverages	*Low Sugar Taxed Beverages*	*High Sugar Taxed Beverages*
	(1)	(2)	(3)
Dependent Variable	Gram of Sugar	Ounces Sold	Ounces Sold
Average Pre-Tax Quantities / Grams of Sugar	342,807	54,290	69,832
Philadelphia × After Tax	-132,129***	-26,132***	-30,136***
	(24,315)	(4,408)	(5,740)
0-2 Miles Outside	166,074***	20,613***	43,213***
City Border × After Tax	(53,314)	(7,741)	(13,775)
2-4 Miles Outside	50,600***	5,633*	12,775***
City Border × After Tax	(18,026)	(2,954)	(4,221)
4-6 Miles Outside	24,572**	2,751	5,888**
City Border × After Tax	(11,086)	(1,849)	(2,501)
Store FE	Yes	Yes	Yes
Week FE	Yes	Yes	Yes
Change in Aggregate Quantity	-18,821*	-5,946***	-3,512
(Unit: 1,000 Ounces / Grams of Sugar)	(10,979)	(1,907)	(2,634)
Change in % of Pre-tax Volume in	-0.154*	-0.311***	-0.143
Philadelphia **w/ Cross-Shopping**	(0.090)	(0.100)	(0.107)
Change in % of Pre-tax Volume in	-0.386***	-0.487***	-0.437***
Philadelphia **w/o Cross-Shopping**	(0.071)	(0.082)	(0.083)
Observations	213,499	212,871	213,499
Stores	1,227	1,223	1,227
Weeks	176	176	176

Table A7: **Impact on Nutritional Intake: Sugar.** High-sugar-content beverages are defined as products with $\geq$2.4 grams/oz (the median value for sugar content across all taxed products).