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

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Abstract

The authors analyze the impact of a tax on sweetened beverages using a unique data set 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. 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. Cross-shopping to stores outside of Philadelphia offsets more than half of the reduction in sales in the city and decreases the net reduction in sales of taxed beverages to only 22%. There is no significant substitution to bottled water and modest substitution to untaxed natural juices. The authors show that tax avoidance through cross-shopping severely constrains revenue generation and nutritional improvement, thus making geographic coverage an important policy decision.

Keywords

pass-through, policy evaluation, sin taxes, tax avoidance, tax design

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The United States has the highest rate of obesity among all developed countries (Organisation for Economic Co-operation and Development 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). In 2008, the estimated annual medical cost of obesity in the United States was $147 billion (https://www.cdc.gov/obesity/data/adult.html; Finkelstein et al. 2009). Due to the prevalence of obesity in the United States, taxes on sugar-sweetened beverages (SSBs) have recently gained in popularity. Policy makers have singled out SSBs for taxation because sugary drinks are the single largest source of added sugar in the average American's diet (National Cancer Institute 2018). Berkeley, California, was the first municipality to implement a 1¢ per ounce tax in March 2015. More recently, other cities and counties have implemented similar taxes, including Philadelphia; Cook County, Illinois (covering Chicago and its suburbs)[1]; San Francisco; Boulder, Colorado; and Seattle. Several other cities (e.g., Washington, D.C.; New York; and Portland, Oregon) and the state of Connecticut have contemplated introducing similar taxes. Understanding the impact of such taxes is therefore important for policy makers, as well as affected firms in the beverage industry and the retail sector.

In this article, we use the case of Philadelphia as a test bed for understanding the impact of a tax on sweetened beverages. Philadelphia presents a particularly rich setting to study a sweetened-beverage 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 data set that covers sales and prices of thousands of taxed and untaxed beverages at hundreds of stores ranging from small convenience stores to wholesale clubs in Philadelphia and its surrounding areas.[2] 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

[1] The Cook County, Illinois, tax went into effect in August 2017 and was repealed four months later in December 2017.

[2] Our data do not cover point-of-consumption sales such as restaurants and bars.

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substitute to untaxed beverages or 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).[3]

Several key findings emerge from our analysis:

1. The tax is passed through at an average rate of 97%, which corresponds to a 34% price increase.
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, though the differential change in sales for healthier taxed products is not statistically significant. Due to the compositional changes in the demand for beverages, calories and sugars decrease by only 16% and 15% respectively, but these effect sizes are not statistically distinguishable from the 22% reduction in total quantity.
4. Purchase quantity decreases less in low-income neighborhoods, most likely because low-income households are less likely to engage in cross-shopping. This explanation is supported by several data patterns, such as lower car ownership rates and a lower share of residents who commute outside of the city for work in low-income neighborhoods.

Next, based on our estimates of demand elasticities and the pass-through rate, we analyze changes in tax policy design. We show that the current tax rate of 1.5¢ per ounce is close to the revenue-maximizing tax rate, but a slightly higher tax rate of 2¢ per ounce would lower sales of unhealthy taxed beverages at a modest cost in terms of tax revenue. The initially proposed rate of 3¢ per ounce would have drastically shrunk tax revenue by 70% relative to the current tax rate. We also investigate the impact of eliminating cross-shopping by broadening the geographic coverage of the tax. We find that applying the tax to a larger geographical area has the potential to raise more tax revenue and at the same time lower the sales of taxed beverages. Thus, limiting cross-shopping by expanding the geographic coverage enhances the health and revenue benefits of the tax.

We draw several lessons from these analyses about the effectiveness of the Philadelphia tax and localized soda taxes more generally. First, the tax's effectiveness at reducing consumption of unhealthy products is hindered by tax avoidance through cross-shopping and compositional changes in demand toward relatively less healthy products. Second, geographic substitution to stores outside of Philadelphia also limits the tax's revenue-generation capabilities. These findings undermine the position often taken by advocates of soda taxes: that they are a win-win because the city earns tax revenue if consumers keep buying, or consumers obtain health benefits if they reduce consumption. Third, we find that low-income households are less likely to engage in tax avoidance via cross-shopping, potentially due to their limited access to transportation. While an analysis of consumer welfare across different income groups is outside of the scope of this article, this finding highlights an important behavioral difference between low- and high-income households in how they react to a localized tax.

Our work contributes to an emerging literature at the intersection of marketing, economics, and health policy (e.g., Khan, Misra, and Singh 2016; Kim and KC 2019; Tuchman 2019; Wang, Lewis, and Singh 2016). More specifically, our article fits into a stream of research that seeks to evaluate the effects of soda taxes on consumption decisions, firm pricing, and consumer health. A first set of articles relevant to our analysis includes studies that use structural models and pretax data to predict the impact of a (hypothetical) soda tax (Allcott, Lockwood, and Taubinsky 2019a; Dubois, Griffith, and O'Connell 2017; Kifer 2015; Wang 2015).[4] These articles vary in their estimated demand elasticities and their assumptions with regard to pass-through, and thus 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 U.S. context, the most well-studied tax is the one implemented in Berkeley, California, in 2014.[5] A series of articles studies price pass-through and quantity reaction using manually collected prices (Cawley and Frisvold 2017; Falbe et al. 2015), survey-based measures of consumption (Falbe et al. 2016), and scanner data on prices and quantities sold (Bollinger and Sexton 2018; Rojas and Wang 2017; Silver et al. 2017). They find pass-through rates between 25% and

[3] We provide evidence in Table 5 that this distance threshold is appropriate.

[4] Khan, Misra, and Singh (2016) and Griffith, Nesheim, and O'Connell (2018) apply a similar approach to analyze the impact of a tax on fat content.

[5] Another set of articles study soda taxes outside of the United States. Grogger (2017), Aguilar, Gutierrez, and Seira (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 United States, 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.

47% depending on the data source and methodology employed.[6] With regard 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, Colorado, Cawley et al. (2018) 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. 2019, 2020; Cawley, Willage, and Frisvold 2018). In line with our findings, these articles find that the tax is fully passed through, but they find no significant reduction in demand at stores in Philadelphia.

Compared with studies of the Berkeley tax and research using surveys or hand-collected prices, our retail scanner data from Philadelphia provide a particularly rich setting to study the impact of a soda tax. Our data contain a large set of 357 stores from 11 different chains in Philadelphia (compared with the 10 stores from 3 chains in the Nielsen-Kilts store-level data for Berkeley), and our data cover all beverages sold at these stores. In addition, our data are representative of the universe of traditional retail stores in terms of geographic and format coverage (see Web Appendix A). Moreover, Philadelphia is a large and demographically diverse city. Both aspects together enable us to explore heterogeneity across stores, chains, and consumer demographics in more detail. Finally, Philadelphia represents a useful test bed for studying soda taxes because its demographic composition is similar to the U.S. 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 United States and much higher than the rate of 36% in Berkeley (City of Berkeley 2013).

A recent study by Roberto et al. (2019) also uses store scanner data to analyze the impact of Philadelphia's soda tax on prices and volume sales. However, unlike our article, it does not consider policy-relevant issues such as nutritional outcomes and the differential impact of the tax across income groups, nor does it analyze the implications for tax policy design. Moreover, many of their key findings differ substantially from ours and lead to different conclusions with regard to the effectiveness of the tax. Roberto et al. observe a more limited set of stores (grocery stores, mass merchants, and drugstores) 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. Roberto et al. also estimate a smaller 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. 2020). Differences in estimates of both price and quantity response lead to drastically different elasticities (net of cross-shopping) of -1.7 in Roberto et al. relative to $-.6$ in our analysis. In Web Appendix B, we provide a detailed comparison of our research that decomposes the impact of various modeling choices and differences in the underlying data on the empirical results.

As mentioned previously, our research contributes to a burgeoning stream of literature that studies questions at the intersection of marketing and health policy such as a fat tax (Khan, Misra, and Singh 2016) and restrictions on advertising for hospitals (Kim and KC 2019) and nicotine products (Tuchman 2019; Wang, Lewis, and Singh 2016). This literature studies consumers' reactions to marketing causal variables (price and advertising) and considers the implications for consumers, firms, and regulators. Our analysis is also related to the extensive marketing literature on retail pass-through (e.g., Ailawadi and Harlam 2009; Besanko, Dubé, and Gupta 2005; McShane et al. 2016; Nijs et al. 2010) as well as the literature on store choice and consumer search (e.g., Mojir and Sudhir 2020; Yavorsky, Honka, and Chen 2020). Finally, our research is related to the literature on various other sin taxes, such as taxes on alcohol (e.g., Conlon and Rao 2015, 2020; Miravete, Seim, and Thurk 2017, 2018), cigarettes (e.g., Harding, Leibtag, and Lovenheim 2012; Lovenheim 2008; Merriman 2010), and cannabis (e.g., Hollenbeck and Uetake 2018; Jacobi and Sovinsky 2016). Within this broader literature, our analysis is particularly relevant to the analysis of tax pass-through (Conlon and Rao 2020; Hollenbeck and Uetake 2018; Miravete, Seim, and Thurk 2018) and tax avoidance through cross-shopping (Asplund, Friberg, and Wilander 2007; Ferris 2000; Lovenheim 2008; Merriman 2010).

Institutional Context

In March 2016, Philadelphia Mayor Jim Kenney proposed a tax of 3¢ per ounce on sweetened beverages. After months of debate, 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¢ per ounce went into effect on January 1, 2017. According to a spokesperson for the mayor, Kenney's primary reason for proposing the tax was to raise tax revenue, but she noted that the tax could also result in health benefits if it reduces the consumption of sweetened beverages (Esterl 2016). In Philadelphia, preimplementation projections predicted that the tax would raise $92 million in tax revenue in 2017.[7] The city actually collected $79 million in 2017, falling short of the projection.

In terms of implementation, the tax is structured as a tax of 1.5¢ per ounce, which, for example, amounts to a tax of $1.01 on a 2-liter bottle. In our data, the average pretax price of a 2-liter bottle of soda in Philadelphia is $1.56; thus, the tax is equal to almost two-thirds of the pretax price of this product.

[6] The studies that analyze 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.

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

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 presweetened coffee and tea drinks, sports drinks, energy drinks, and non-100% fruit drinks that contain a caloric sweetener or nonnutritive sweetener.[8]

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 counterproductive. In the case of Philadelphia, the mayor's office has acknowledged that the primary purpose of the tax is to raise tax revenue, and thus the decision to include artificially sweetened drinks was likely driven by financial motivations. Many other municipalities that introduced similar taxes (e.g., several Bay Area cities; Boulder, Colorado; Seattle) only tax drinks with caloric sweeteners.

Data

We analyze retail point-of-sale data collected by IRI, a large market-research firm.[9] We supplement these data with nutrition information on products and demographic data. We describe each data set in more detail next.

Data Sources

Retail point-of-sale data. The data cover the period from January 2015 through September 2018 and contain information on prices[10] and quantity sold at the Universal Product Code (UPC)/store/week level. We obtained data for all beverage categories, including untaxed beverages, which constitute potential substitutes. We observe the location and chain affiliation for each store.[11] We focus our analysis on stores located in the city of Philadelphia and the four three-digit zip codes that surround Philadelphia. We restrict the sample to stores that (1) entered the panel before January 1, 2016, and were tracked through at least December 31, 2017, and (2) that belong to one of the 11 chains/groups of stores that operate stores both within and outside of the city. Our final data set includes 357 stores located in Philadelphia and 870 stores located in the surrounding area.[12] 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, stores 0–6 miles outside of 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.

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 (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.[13] 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).[14] 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 pretax period. Total volume sold is obtained by aggregating product/store/week–level volume up to the tax-status/store/week level. When analyzing heterogeneity 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), 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 Web Appendix A, we show that the set of stores in

[8] Examples of caloric sweeteners include cane sugar, high-fructose corn syrup, and honey. Examples of nonnutritive 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.

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

[10] We compared our pricing data with a sample of manually collected prices in Philadelphia stores 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. Consequently, the IRI data records price net of the tax for this retailer (for more details and sample receipts, see Web Appendix C). To recover this retailer's shelf price, which is the effective price paid, we add the 1.5¢ per ounce tax to the prices recorded in our data for this retailer beginning in January 2017. 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.

[11] 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 five-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. In Web Appendix D, we show that our findings are not sensitive to measurement error that might arise from imprecise location data. 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.

[12] For details on how we select stores and UPCs, see Web Appendix E.

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

[14] For details regarding the data cleaning, see Web Appendix E.

Table 1. Descriptive Statistics.

A: Category Level

Market share	Taxed Categories .46						Untaxed Categories .54	
	Soda	**Taxed Juice**	**Tea/ Coffee**	**Sports Drinks**	**Taxed Water**	**Energy Drinks**	**Pure Water**	**Natural Juice**
Market share (within taxed/untaxed categories)	.35	.26	.22	.11	.03	.03	.89	.11
Price (¢/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	.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

B: Store Level

	# Stores in Philadelphia	**# Stores Outside Philadelphia**	**Average Weekly Volume (oz) Per Store**	**Philadelphia Market Share**		**Average Price/oz**	**Price/oz of a Popular 2-L Soda Brand**	**Median Pack Size (oz)**
Grocery A	15	46	377,774	.13		3.53	2.52	59
Grocery B	1	38	781,050	.02		3.27	2.35	48
Grocery C	16	32	1,035,115	.38	.53	3.10	2.47	51
Mass Merchant M	6	21	223,869	.03		4.10	2.16	46
Other mass merchants	5	28	920,248	.11	.14	3.19	1.97	59
Drugstore X	45	128	29,536	.03		5.15	2.42	20
Drugstore Y	80	122	15,436	.03		5.79	2.40	23
Drugstore Z	17	51	42,265	.02	.08	5.30	3.00	20
Convenience stores	116	324	72,619	.19		8.02	2.69	18
Wholesale Club W	2	8	717,375	.03		3.46	1.94	192
Dollar stores	54	72	29,721	.04		3.25	2.20	32
All stores	357	870	122,409					

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	.29	.20	.28	.42
Share of households with a car	357	.71	.46	.70	.95
Share of outbound commuters	357	.08	.02	.07	.19

Notes: Market shares and prices in Panels A and B are based on pretax data.

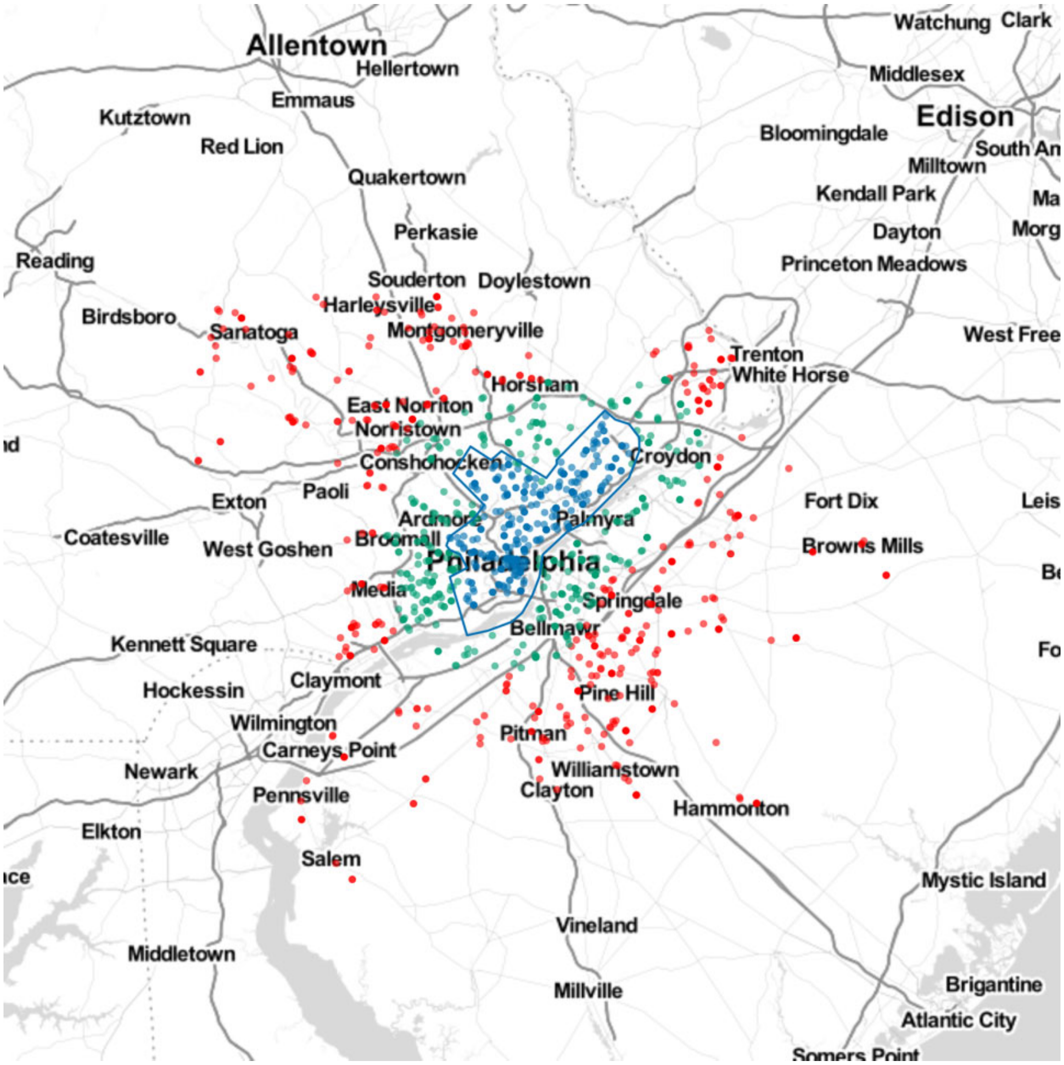

Figure 1. Stores within and outside of Philadelphia.

our data is representative along key dimensions, namely, geographic coverage and coverage across different store formats. However, although our sample is representative with regard to retail formats and geographic coverage, the tax also affects sales of sweetened beverages at restaurants and vending machines, which are not covered in our data.[15] Our data also do not include sales of fountain drinks at retail stores. Drawing on publicly reported tax revenues (which are proportional to ounces sold of taxed beverages), we calculate that our data cover 28% of sales of taxed beverages. Using coverage rates (for more details, see Web Appendix A), we estimate that the retail sector as a whole accounts for a market share of 40%–50% of all sales of sweetened beverages.[16] Our analysis should be interpreted as measuring the effect of the tax on purchases made in the retail sector.

Demographic data. We supplement the store-level sales data with highly localized demographic data. These data enable 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, car ownership, and commute patterns from

[15] The impact of the tax in the retail sector is likely to be different from the impact on restaurants. For instance, we find a large degree of cross-shopping at stores outside of Philadelphia. The ability to purchase large quantities and stockpile products is specific to the retail sector, and we would therefore expect geographic tax avoidance to be a relatively less important issue for the restaurant sector, where beverages are purchased for immediate consumption. Pricing and purchase patterns for small pack sizes, which tend to be for immediate consumption, are likely to be most similar to patterns we expect to find at restaurants.

[16] Due to data privacy concerns, we are not able to report exact coverage rates and therefore report only a range for the retail sector market share.

the Census Bureau's 2011–2015 American Community Survey and data on obesity rates from the CDC (Manson et al. 2017).[17] Both data sets vary at the census-tract level.[18] We focus on income and obesity as sociodemographic measures because (1) previous work suggests that income may be correlated with price sensitivity and preference for sweetened beverages (Wang 2015) and (2) obesity data allow us to analyze whether the consumers who could reap the largest health benefits from reducing consumption actually do so. We use the car ownership and commuter patterns data as measures of transportation costs. We assign demographic profiles to individual stores by calculating average characteristics (income, obesity rate, car ownership rate, and commuter share) 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.

Ingredient and nutrition data. Finally, we manually collect ingredient and nutrition information on all taxed and untaxed products contained in the retail sales data.[19] 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.

Descriptive Statistics

Table 1, Panel A, provides descriptive statistics on the categories included in our data. As described previously, 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, Diet Snapple Peach Tea made with aspartame). We report market shares based on pretax 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 pretax period. The second category is natural juices.[20] They make up only 11% 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.

Table 1, Panel B, 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 pretax period,[21] 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.[22]

[17] Regarding car ownership, 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?" We define car ownership as having access to at least one vehicle. The commute data are collected in collaboration with the Department of Transportation. The survey asks respondents to report the location of their home and workplace. Accordingly, for every pair of census tracts in our data, we are able to extract an estimate of the number of inbound and outbound commuters. The CDC reports model-based estimates of obesity rates at the census-tract level as part of its 500 Cities Project (https://www.cdc.gov/500cities/index.htm). Obesity data are only available for census tracts within the city of Philadelphia.

[18] 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.

[19] 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 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).

[20] Juice products from concentrate are included in this untaxed category as long as the sugar content is comparable to freshly extracted juice and no sweetener is added.

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

[22] Many beverages are priced in a highly nonlinear way. For example, a 2-liter (67.6 oz) bottle of Coca-Cola is often sold at the identical (or only marginally different) price as a 20 oz bottle.

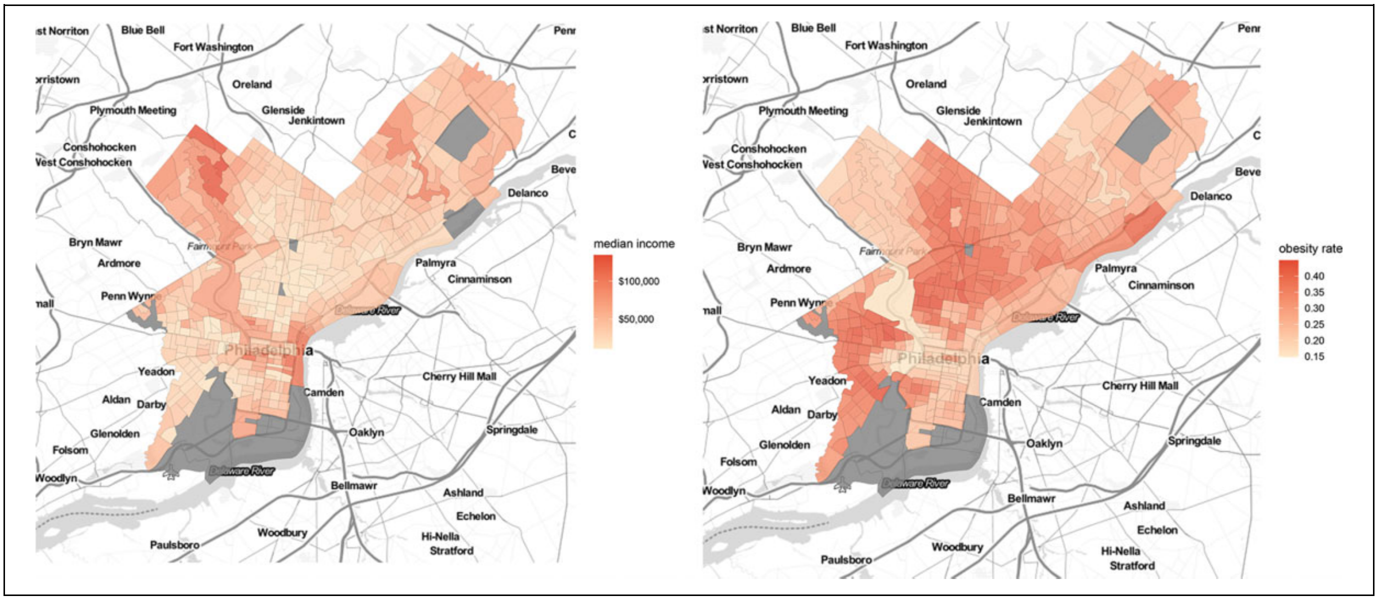

Figure 2. Variation in income and obesity rates in Philadelphia.
Notes: Gray regions indicate census tracts where no income and obesity information is recorded because there is little or no residential population.

Finally, Table 1, Panel C, summarizes the variation in local demographics for the stores in Philadelphia. We see significant variation in income and obesity rates, as well as car ownership rates and the share of residents that commute to locations outside of Philadelphia. Income and obesity rates are highly correlated (corr. $= -.8$). We provide some graphical evidence for this negative correlation in Figure 2. In Web Appendix F, we show most chains are present in neighborhoods with different income levels.

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.[23] 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.[24]

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 three-digit zip codes. In all regressions (unless stated otherwise), we include only stores that are located more than 6 miles away from the city limits in the control group (see Figure 1) to ensure that the control group is not affected by the treatment through cross-shopping behavior. We subsequently show that 6 miles constitutes an appropriate choice of distance. Our choice of control group has two advantages. First, the control-group stores are relatively close to the city of Philadelphia and thus 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 television designated market area and thus consumers in both groups will be exposed to the same TV advertising. In Web Appendix I, we provide supporting evidence for the validity of the parallel trends assumption based on pretax data. Second, choosing stores from a nearby area ensures the chain affiliations of stores

[23] Prices continue to increase in the treatment group in the first four months after the tax is implemented. This pattern is visible in Figure 3 and also holds true on average across all taxed products. The pattern is driven by heterogeneity in adoption behavior across retailers. While some retail chains adjust prices immediately, others adjust with a delay or in a gradual fashion. In our main analysis we omit this four-month adjustment period. In Web Appendix G, we show that the price and quantity reaction to the tax is stable over time after the first four months.

[24] The bottom graph in Figure 3 shows the week-on-week variation in sales decreases in the posttax period. We explore this pattern further in Web Appendix H.

Figure 3. Unit price and sales of 2-liter bottles of a popular soda brand.

Notes: The black vertical lines indicate the tax's introduction and the end of the four-month "adjustment period" that we omit from our main analyses (see the "Estimation and Results" section and Web Appendix G).

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:

$$y_{st} = \alpha(\text{Philly}_s \times \text{AfterTax}_t) + \gamma_s + \delta_t + \epsilon_{st}, \quad (1)$$

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:

$$y_{st} = \tilde{\alpha}_0(\text{Philly}_s \times \text{AfterTax}_t) + (\text{Philly}_s \times \text{AfterTax}_t \times \mathbf{X}_s)'\tilde{\boldsymbol{\alpha}}_1$$
$$+ (\text{AfterTax}_t \times \mathbf{X}_s)'\tilde{\boldsymbol{\beta}} + \tilde{\gamma}_s + \tilde{\delta}_t + \tilde{\epsilon}_{st}, \quad (2)$$

where $\tilde{\gamma}_s$ and $\tilde{\delta}_t$ are store and week fixed effects and $\tilde{\epsilon}_{st}$ 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 $\tilde{\boldsymbol{\alpha}}_1$ 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 uninteracted, difference-in-differences estimate.[25]

[25] In some specifications, we include an exhaustive set of dummies along a specific dimension, and thus no $\tilde{\alpha}_0$ term is included. Note that the estimation equation does not include "uninteracted" $\mathbf{X}_s$ terms because we include a full set of store dummies.

We employ two-way clustered standard errors at the store and the week level in all regressions. In Web Appendix J, we show robustness to higher levels of clustering both along the geographic and time dimensions. Finally, we note a brief adjustment period during which price pass-through and the quantity decrease are slightly lower. After the first four months, the impact of the tax does not vary over time for any of the outcomes we analyze.[26] We analyze these dynamic adjustment patterns in detail in Web Appendix G. In our main regressions, we omit the first four months after the tax went into effect 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.

Price Reaction and Pass-Through

To measure pass-through, we use price in cents per ounce 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¢ per ounce. Thus, 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 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¢ per ounce, corresponding to a 97% average pass-through rate. Relative to an average pretax price of 4.26¢ per ounce, 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 × the Philadelphia dummy with a full set of chain dummies for the 11 different chains/groups of stores in our sample.[27] We find that pass-through rates are fairly 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,[28] and overall mass merchandisers have a lower pass-through rate than other retail formats. 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 this article, we always apply this transformation.[29]

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. To evaluate the reaction to a change in the competitive environment, we analyze whether the distance to the nearest untaxed store outside of Philadelphia predicts price changes. We show that stores outside of Philadelphia that are not subject to the tax do not adjust prices after the tax, and therefore the tax generates a large price wedge between stores inside Philadelphia versus outside of the city.[30] As a simple measure of competition, we therefore include the distance to the nearest untaxed store (with the appropriate interactions) in the regression. We find that the distance to the nearest untaxed store does not predict a differential price reaction, and the estimated coefficient is small in magnitude.

In the final two columns of Table 2, we investigate whether income and obesity rates in the store's catchment area (a 1-mile radius around the store) are predictive of pass-through. To facilitate interpretation, we use rescaled 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.[31] Thus, from a health policy point of view, one might be encouraged by the higher increase in high-obesity areas. However, the differential price increase suggests that low-income households need to pay a relatively higher price to purchase taxed beverages. Furthermore, although both coefficients are statistically significant, they are relatively small in magnitude.[32]

[26] We also test for differences in behavior in the months immediately before the tax but find no evidence for behavioral changes during this time period. We therefore retain the entire pretax period in our sample. See Web Appendix G for details.

[27] To simplify exposition, we do not report the coefficient vector $\tilde{\beta}$ pertaining to $(\text{AfterTax}_t \times \mathbf{X}_s)'$ terms for any of the regressions in Columns 2–6 of Table 2.

[28] The two chains jointly make up less than 5% of market share. 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¢ per ounce in late May 2017.

[29] For example, the estimated effect for Grocery A in Column 3 corresponds to a 37.3% change: $\exp(.317) - 1 = 37.3\%$.

[30] We prefer this metric to other measures of competition, such as the number of nearby stores, because the latter measures differences in competition that exist before and after the tax. In contrast, our metric captures changes in the competitive environment. We did also estimate regressions using the number of competing stores within a certain radius (we consider radii of one, two, three, and four miles) around the focal store and find that for most radius definitions, the number of competing stores does not predict a differential price or quantity reaction.

[31] Due to the imprecise location information for some chains, both demographic variables (which are based on stores' catchment areas) might be subject to measurement error. We explore the possibility of such measurement error in Web Appendix D.

[32] The standard deviation of the rescaled income (obesity) variable is equal to .26 (.27). Therefore, a one-standard-deviation shift in either variable leads to a change in the price adjustment of less than 1%. We reiterate that income and obesity are strongly negatively correlated (correlation coefficient of −.8), and thus when we include both variables, estimates become noisier.

Table 2. Impact on Prices and Pass-Through Rate Estimates.

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***					
	(.022)					
Grocery A		1.355***	.317***	.326***	.332***	.308***
× Philadelphia × AfterTax		(.018)	(.008)	(.011)	(.009)	(.008)
Grocery B		1.290***	.320***	.332***	.330***	.311***
× Philadelphia × AfterTax		(.005)	(.001)	(.009)	(.004)	(.003)
Grocery C		1.780***	.442***	.450***	.450***	.424***
× Philadelphia × AfterTax		(.051)	(.011)	(.012)	(.012)	(.012)
Mass Merchant M		.655***	.144***	.151***	.156***	.131***
× Philadelphia × AfterTax		(.122)	(.027)	(.031)	(.027)	(.029)
Other mass merchants		1.155***	.308***	.315***	.319***	.295***
× Philadelphia × AfterTax		(.011)	(.005)	(.008)	(.006)	(.005)
Drugstore X		1.492***	.258***	.266***	.271***	.247***
× Philadelphia × AfterTax		(.038)	(.009)	(.011)	(.010)	(.009)
Drugstore Y		1.377***	.216***	.224***	.226***	.200***
× Philadelphia × AfterTax		(.025)	(.006)	(.009)	(.007)	(.007)
Drugstore Z		.342***	.062***	.072***	.072***	.048***
× Philadelphia × AfterTax		(.031)	(.006)	(.010)	(.007)	(.007)
Wholesale club		1.398***	.336***	.349***	.346***	.326***
× Philadelphia × AfterTax		(.013)	(.003)	(.011)	(.005)	(.006)
Dollar stores		1.557***	.389***	.397***	.396***	.369***
× Philadelphia × AfterTax		(.032)	(.007)	(.010)	(.007)	(.010)
Convenience stores		1.626***	.183***	.192***	.194***	.170***
× Philadelphia × AfterTax		(.032)	(.003)	(.008)	(.006)	(.004)
Distance (in miles) to nearest untaxed store				−.004		
× Philadelphia × AfterTax				(.003)		
Income					−.024***	
× Philadelphia × AfterTax					(.008)	
Obesity rate						.033***
× Philadelphia × AfterTax						(.009)
$(\text{AfterTax}_t \times \mathbf{X}_s)'$ Interactions	N.A.	Yes	Yes	Yes	Yes	Yes
Store FEs	Yes	Yes	Yes	Yes	Yes	Yes
Week FEs	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

*$p < .1$.
**$p < .05$.
***$p < .01$.
Notes: N.A. = not applicable; FEs = fixed effects. Interactions with an after-tax dummy (the $(\text{AfterTax}_t \times \mathbf{X}_s)'$ 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 thus we do not include the $(\text{Obesity}_s \times \text{AfterTax}_t)$ term. Standard errors are clustered at the store and week levels and reported in parentheses.

In summary, we find some heterogeneity in pass-through 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.[33] Next, we analyze changes in quantity sold after the tax.

Quantity Reaction

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 pretax level of weekly sales of 122,000 ounces per store (see Table 1, Panel B).

Notable heterogeneity exists in this effect across chains. In Columns 2 and 3 of Table 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%.[34] Among the smaller-volume chains, only dollar stores experience a similar decrease. Drugstores and convenience stores instead experience a more modest decrease or no

[33] We also report the same analysis using the price for soda rather than all taxed products as the dependent variable in Table A11 in the Web Appendix and find largely similar results.

[34] As mentioned previously, we apply the transformation $\exp(\text{coefficient}) - 1$ to translate the regression coefficients into percentage changes.

Table 3. Impact on Quantity Sold.

Dependent Variable	(1) Ounces Sold	(2) Ounces Sold	(3) Log Ounces	(4) Log Ounces	(5) Log Ounces	(6) Log Ounces
Philadelphia × AfterTax	−56,192***					
	(9,742)					
Grocery A		−207,363***	−.733***	−.819***	−.652***	−.725***
× Philadelphia × AfterTax		(34,502)	(.068)	(.077)	(.077)	(.070)
Grocery B		−369,605***	−.674***	−.792***	−.609***	−.665***
× Philadelphia × AfterTax		(10,383)	(.012)	(.042)	(.027)	(.016)
Grocery C		−728,854***	−1.173***	−1.251***	−1.111***	−1.157***
× Philadelphia × AfterTax		(82,272)	(.068)	(.065)	(.071)	(.074)
Mass Merchant M		−23,083	−.110	−.181	−.033	−.098
× Philadelphia × AfterTax		(24,797)	(.109)	(.130)	(.112)	(.110)
Other mass merchants		−406,541***	−.529***	−.609***	−.461***	−.518***
× Philadelphia × AfterTax		(65,340)	(.078)	(.085)	(.080)	(.080)
Drugstore X		−7,899***	−.290***	−.371***	−.212***	−.280***
× Philadelphia × AfterTax		(1,234)	(.041)	(.051)	(.048)	(.043)
Drugstore Y		−610***	−.002	−.082*	.063	.013
× Philadelphia × AfterTax		(188)	(.034)	(.043)	(.041)	(.039)
Drugstore Z		26,169***	.558***	.457***	.621***	.570***
× Philadelphia × AfterTax		(4,311)	(.079)	(.083)	(.080)	(.083)
Wholesale club		−423,042***	−.878***	−1.004***	−.796***	−.869***
× Philadelphia × AfterTax		(35,987)	(.062)	(.081)	(.074)	(.061)
Dollar stores		−16,234***	−.568***	−.656***	−.519***	−.550***
× Philadelphia × AfterTax		(1,670)	(.034)	(.044)	(.038)	(.041)
Convenience stores		−7,131***	−.108***	−.196***	−.035	−.096***
× Philadelphia × AfterTax		(1,531)	(.019)	(.035)	(.034)	(.023)
Distance (in miles) to nearest untaxed store				.040***		
× Philadelphia × AfterTax				(.014)		
Income					−.106**	
× Philadelphia × AfterTax					(.044)	
Obesity rate						−.030
× Philadelphia × AfterTax						(.041)
$(\text{AfterTax}_t \times \mathbf{X}_s)'$ Interactions	N.A.	Yes	Yes	Yes	Yes	Yes
Store FEs	Yes	Yes	Yes	Yes	Yes	Yes
Week FEs	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

*$p < .1$.
**$p < .05$.
***$p < .01$.
Notes: N.A. = not applicable; FEs = fixed effects. Interactions with an after-tax dummy (the $(\text{AfterTax}_t \times \mathbf{X}_s)'$ 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 thus do not include the $(\text{Obesity}_s \times \text{AfterTax}_t)$ term. Standard errors are clustered at the store and week levels and reported in parentheses.

decrease in volume sold. Given 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 pretax price level. Second, those stores tend to sell smaller pack sizes, which are more likely to be for immediate consumption, and thus 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 because Drugstore Z has the lowest pass-through rate of all stores and thus 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 nearest untaxed store does have a positive and significant impact on quantity changes (see Column 4 of Table 3). Therefore, stores that are further away from untaxed stores outside of the city experience a smaller decrease in quantity. The least competitive location according to this measure is a store that is located 3.7 miles from the nearest untaxed store. Other things equal, the quantity decrease at this store is 15% smaller than the would-be decrease if that store were instead located just next to an untaxed store. We later show that stores outside of the city experience an increase in demand after the tax is implemented. Our results suggest that quantity declines more at Philadelphia stores where geographic substitution is more likely.

Results based on interactions with income and obesity rates are reported in Columns 5 and 6 of Table 3. We find that 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 decreases by approximately 10% more in the highest-income area relative to the lowest-income area.[35] The direction of the correlation with income is surprising because we would expect high-income households to be less price sensitive, and thus reduce consumption less in response to the tax. Moreover, we saw in Table 2 that prices increased somewhat less in high-income areas, and thus the smaller price increase should lead to a lower-quantity reaction. Furthermore, prior research (see Dubois, Griffith, and O'Connell 2017; Wang 2015) predicts a larger reaction of low-income households to a counterfactual soda tax. One possible explanation for our finding is that high-income households face lower transportation costs, either because they tend to live closer to an untaxed store or because they have easier access to transportation. We find that the income effect remains of similar magnitude when we also include an interaction with the distance to the nearest untaxed store in the regression and is not statistically significantly different from the estimate reported in Column 5 of Table 3. Thus, distance alone does not explain the income effect. However, it is possible 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 discuss this point in more detail in the "Differential Impact by Income" section.[36]

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 pretax volume of untaxed beverages of 146,000, the decrease constitutes a 3.1% change in demand.

When analyzing bottled water and natural juice separately in Columns 2 and 3 of Table 4, we find a statistically significant increase of 9% in the sales of natural juice, whereas sales of bottled water do not change significantly. We note the market share of natural juice is relatively small, and thus the increase of 1,400 ounces per store is modest when compared with the decrease of 56,000 ounces of taxed beverages documented previously. Nevertheless, it is interesting that among untaxed beverages, natural juices, which contain more calories and sugar than most taxed beverages, experience an increase in demand.

In Columns 4–6 of Table 4, we analyze price changes among untaxed beverages. We find that, on average, prices increase by .027¢ per ounce for bottled water and by .343¢ per ounce for natural juices. Although both coefficients are statistically significant, the effect for water is small in magnitude.[37] 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.[38]

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[35] We find similar income effects when estimating the regression separately for each store format (e.g., grocery stores, drugstores), but most estimates are not statistically significant, due to smaller sample sizes in the format-level regressions. 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 × AfterTax dummy. We further include an interaction of log price and income and an additional instrument that interacts the Philadelphia × 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 −.52 and standard error of .24, the baseline uninteracted log price coefficient is equal to −.66), and thus demand is more elastic in high-income areas. Note that here we reference store-level elasticities to illustrate differential responsiveness across the income spectrum, whereas the elasticity reported in the "Summary and Implications for Policy Design" section compares the aggregate volume change with the average price change across all stores in Philadelphia.

[36] In Table A12 in the Web Appendix, we present results from the quantity regression for the soda category. Results are very similar with regard 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 prior finding that Mass Merchant M only increased prices for soda and not for other taxed categories.

[37] In comparison, the price for taxed beverages goes up by 1.45¢ per ounce (see Column 1 of Table 2).

[38] 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 [we] were supposed to calculate this. How do we know if something has been reconstituted to its original sweetness level?"

Table 4. Quantity and Price Reaction of Untaxed Beverages.

Dependent Variable	(1) Ounces Sold All Untaxed Beverages	(2) Ounces Sold Water	(3) Ounces Sold Natural Juice	(4) Price/oz All Untaxed Beverages	(5) Price/oz Water	(6) Price/oz Natural Juice
Average pretax quantities/ prices	146,017	130,736	15,281	1.88	1.35	6.37
Philadelphia × AfterTax	−4,521	−5,740	1,388**	.063***	.027***	.343***
	(7,125)	(7,201)	(543)	(.010)	(.010)	(.032)
Store FEs	Yes	Yes	Yes	Yes	Yes	Yes
Week FEs	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

*$p < .1$.
**$p < .05$.
***$p < .01$.
Notes: FEs = fixed effects. The number of observations differs across columns because some stores offer fewer categories of beverages. Standard errors are clustered at the store and week levels and reported in parentheses.

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 show that quantity sold increases significantly for stores up to 6 miles outside of the city limits. Compared with 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 per 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. Because the number of stores varies across geographical areas, we need to weigh the different coefficients in Column 1 appropriately to assess the aggregate change in quantity. In the lower part 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. 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 + 6 miles outside" percentage decrease is statistically significant. Therefore, to measure the aggregate quantity change correctly, accounting for cross-shopping behavior is important.

Figure 4 presents a graphical representation of the regression in Column 1 of Table 5. The top graph shows the average store-level sales in each group. This graph shows that average sales in all three geographic areas track each other closely prior to the tax. Once the tax is implemented, sales in stores 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. The bottom graph of Figure 4 illustrates the aggregate change in volume by plotting the total sales volume in Philadelphia and the entire "cross-shopping area" 0–6 miles outside of the city relative to the control group. Specifically, we isolate the change in the two "treated" regions by differencing average store sales in the control group from average store sales in Philadelphia and from average store sales in the 0–6 mile band outside the city. Then we scale up average store sales by the number of stores within each area (both time series are normalized to zero in the first week). Consistent with our previous regression analysis, the graph shows that the increase in volume in the area 0–6 miles outside of the city offsets about half of the volume reduction in Philadelphia.

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. We

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

Table 5. Quantity and Price Reaction in Stores Near the City Border.

Dependent Variable	(1) Ounces Sold Taxed Beverages	(2) Ounces Sold Taxed Beverages	(3) Price/oz Taxed Beverages	(4) Ounces Sold Untaxed Beverages	# Stores in Geographic Area
Philadelphia × AfterTax	−56,193***	−56,797***	1.449***	−4,481	357
	(9,742)	(9,775)	(.022)	(7,115)	
0–2 miles outside city border × AfterTax	63,650***	63,046***	−.022**	6,323	106
	(20,734)	(20,748)	(.011)	(7,613)	
2–4 miles outside city border × AfterTax	18,364***	17,760**	.006	4,648	140
	(7,032)	(7,082)	(.011)	(9,474)	
4–6 miles outside city border × AfterTax	8,640**	8,036*	.002	19,877	149
	(4,198)	(4,261)	(.009)	(16,275)	
6–8 miles outside city border × AfterTax		2,790			118
		(3,713)			
8–10 miles outside city border × AfterTax		−5,995*			103
		(3,047)			
Store FEs	Yes	Yes	Yes	Yes	
Week FEs	Yes	Yes	Yes	Yes	
Change in aggregate quantity (unit: 1,000 oz)	−9,456**				
	(4,358)				
Change in % of pretax volume in Philadelphia with cross-shopping	−.216**				
	(.100)				
Change in % of pretax volume in Philadelphia without cross-shopping	−.459***				
	(.080)				
Observations	213,499	213,499	213,499	212,556	
Stores	1,227	1,227	1,227	1,221	
Weeks	176	176	176	176	

*$p < .1$.
**$p < .05$.
***$p < .01$.
Notes: FEs = fixed effects. The number of observations differs across columns because some stores offer fewer categories of beverages. Reported store counts in the right-most column are for stores that sell taxed products. Standard errors are clustered at the store and week levels and reported in parentheses.

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).

Finally, to better understand the consumer behavior underlying the documented cross-shopping effects, we analyze data on commuter patterns. We first note that 64,500 individuals commute from Philadelphia to the region 0–6 miles outside of the city, and thus, it is plausible that part of the cross-shopping effect is driven by outbound commuters that are able to relatively easily shift their purchases to stores outside of Philadelphia. Next, we explore whether stores in Philadelphia that serve a higher proportion of commuters experience a larger decrease in purchase quantity. To do so, we reestimate our previous quantity regression (see Column 3 of Table 3), but also include an interaction of the share of commuters in each store's catchment area with the Philadelphia × AfterTax dummy. We find that the commuter share is predictive of the demand decrease, and the quantity decrease is about 14% larger in the store with the highest commuter share relative to the store with the lowest commuter share.[40] Taken together, these patterns suggest that commuters likely play an important role in generating the large cross-shopping effect documented herein.

In the Web Appendix we include additional analyses related to geographic substitution. First, we 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 Web Appendix G. Our analysis shows that after a brief adjustment period of four months (which are omitted from our main regressions), quantities sold stabilized and show no sign of further adjustments between May 2017 and September 2018.

[40] We use a rescaled version of the commuter share variable that ranges from 0 to 1 across all stores in Philadelphia. The coefficient on the interaction between rescaled commuter share and the Philadelphia × AfterTax dummy is −.155, and the standard error is .047. We also explore how the number of commuters from Philadelphia is related to the change in purchase quantity at stores outside of the city. We find that stores located in areas with more commuters from Philadelphia experience a larger increase in purchases; however, commuter counts have limited explanatory power with regard to changes in demand in stores outside of Philadelphia once we account for each store's distance to Philadelphia.

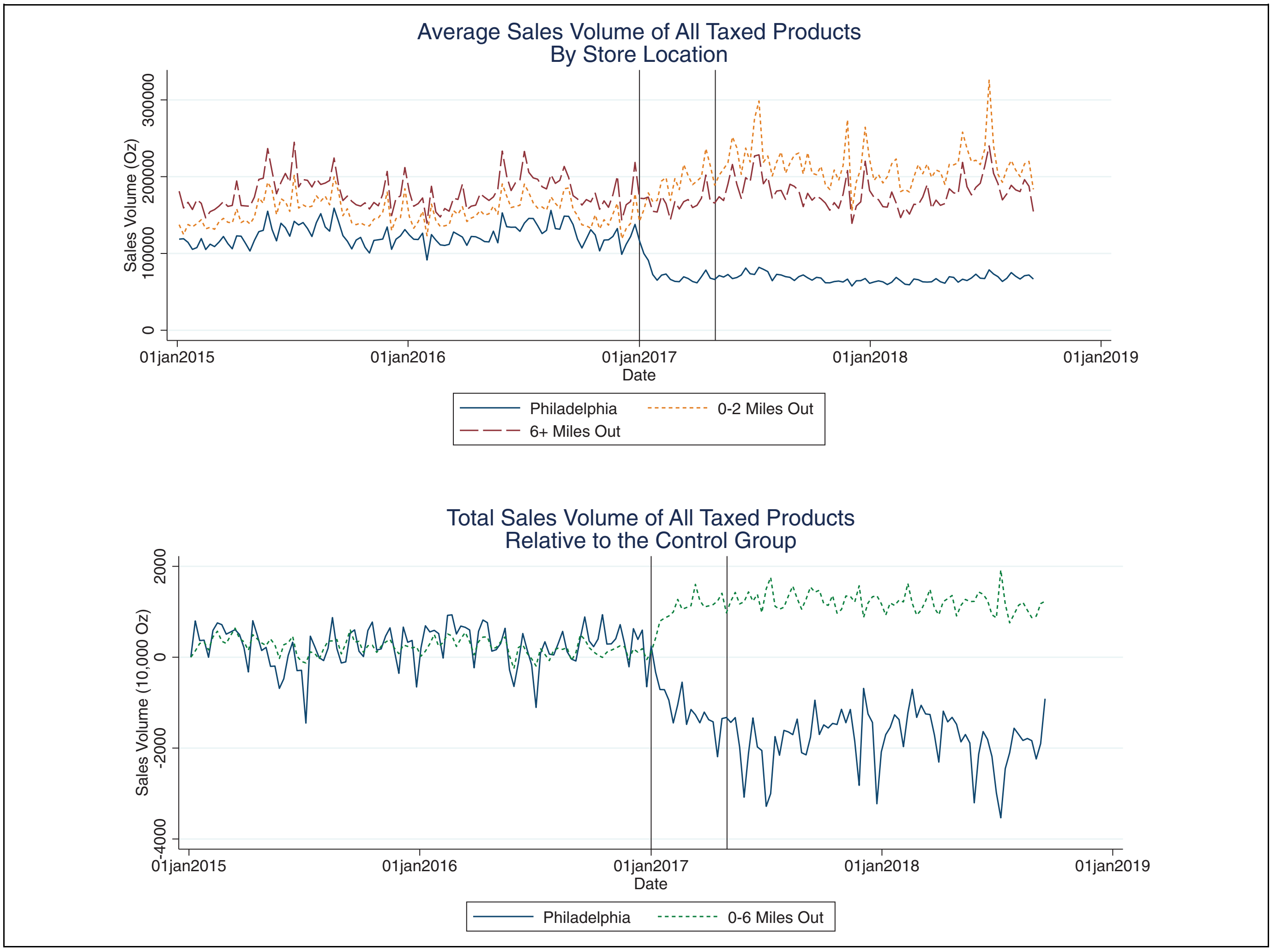

Figure 4. Taxed beverage sales in Philadelphia and surrounding area stores over time.

Notes: The top graph shows average sales at stores in the three different geographic areas. The bottom graph shows total sales at stores in each area relative to the control group. (Each week displays the difference between average sales in the control group and each of the two areas, multiplied by the number of stores in each area.) Both time series in the lower graph are normalized to zero in the first week. The black vertical lines indicate the tax's introduction and the end of the four month "adjustment period" that we omit from our main analyses (see the "Estimation and Results" section and Web Appendix G).

Second, we look for evidence of basket-level substitution by analyzing sales of milk in Web Appendix K. We find some evidence of basket-level substitution at grocery stores and wholesale clubs but do not find evidence of this substitution at other store formats. Furthermore, even at grocery stores and wholesale clubs, 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.

Heterogeneity across pack sizes and categories. In this subsection, 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]

[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.

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 significantly smaller reductions in sales of small pack sizes compared with large pack sizes at stores in Philadelphia, and we find that consumers do not engage in cross-shopping for small pack sizes. Demand for small-pack-size 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

Table 6. Demand Response for Small and Large Pack Sizes.

	Taxed Beverages		Untaxed Beverages	
Dependent Variable	**(1) Ounces Sold ≤20 oz Pack Sizes**	**(2) Ounces Sold >20 oz Pack Sizes**	**(3) Ounces Sold ≤20 oz Pack Sizes**	**(4) Ounces Sold >20 oz Pack Sizes**
Average ounces sold pretax	19,439	105,530	3,726	146,795
Philadelphia × AfterTax	−1,914***	−54,824***	19	−4,469
	(306)	(9,659)	(65)	(7,205)
0–2 miles outside city border × AfterTax	649	63,498***	118	6,094
	(453)	(20,693)	(72)	(7,606)
2–4 miles outside city border × AfterTax	−52	18,416***	−45	4,744
	(373)	(6,951)	(73)	(9,534)
4–6 miles outside city border × AfterTax	−240	8,886**	45	20,503
	(343)	(4,094)	(66)	(16,585)
Store FEs	Yes	Yes	Yes	Yes
Week FEs	Yes	Yes	Yes	Yes
Change in aggregate quantity (unit: 1,000 oz)	−653***	−8,783**	19	2,728
	(178)	(4,276)	(34)	(4,440)
Change in % of pretax volume in Philadelphia with cross-shopping	−.096***	−.238**	.015	.054
	(.026)	(.116)	(.026)	(.088)
Change in % of pretax volume in Philadelphia without cross-shopping	−.100***	−.525***	.005	−.031
	(.016)	(.092)	(.018)	(.049)
Observations	211,774	212,767	209,805	210,175
Stores	1,217	1,222	1,205	1,206
Weeks	176	176	176	176

*$p < .1$.
**$p < .05$.
***$p < .01$.
Notes: FEs = fixed effects. Some stores do not offer all pack sizes, and thus, the number of observations differs slightly across columns. Standard errors are clustered at the store and week level and reported in parentheses.

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. However, 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 pretax price per ounce is equal to 10.10 (3.54) for small (large) pack sizes due to highly nonlinear 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 −.64 (−.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.

We also test whether the absence of cross-shopping for small pack sizes leads to substitution to healthier beverages. Columns 3 and 4 of Table 6 report cross-shopping regressions based on small and large pack sizes of untaxed beverages. These regressions show 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 heterogeneous demand patterns across categories. For soda, cross-shopping is pervasive: the reduction in soda sales in the city is completely offset by an increase in sales outside the city. The other five categories are characterized by a much smaller degree of cross-shopping. The category with the second-highest proportion of cross-shopped volume is tea/coffee, for which 32% of the volume reduction in

[42] A similar picture emerges when we analyze cross-shopping separately for larger (grocery stores, mass merchants, wholesale clubs) and smaller (drugstores, 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.

Table 7. Demand Response Across Categories.

Dependent Variable	(1) Ounces Sold Soda	(2) Ounces Sold Taxed Juice	(3) Ounces Sold Tea/Coffee	(4) Ounces Sold Sports Drinks	(5) Ounces Sold Taxed Water	(6) Ounces Sold Energy Drinks
Average ounces sold pretax	43,529	32,950	28,638	14,229	5,015	3,693
Philadelphia × AfterTax	−18,711***	−16,902***	−12,213***	−6,609***	−3,341***	−552***
	(3,910)	(3,365)	(2,132)	(1,184)	(463)	(111)
0–2 miles outside city border × AfterTax	42,768***	8,790***	9,658***	2,881**	245	−140
	(13,401)	(3,106)	(3,404)	(1,416)	(649)	(121)
2–4 miles outside city border × AfterTax	12,818***	2,733**	2,208**	928	17	−165
	(4,246)	(1,061)	(1,018)	(940)	(473)	(119)
4–6 miles outside city border × AfterTax	6,424**	1,132	226	910	189	−162
	(2,539)	(691)	(731)	(668)	(423)	(122)
Store FEs	Yes	Yes	Yes	Yes	Yes	Yes
Week FEs	Yes	Yes	Yes	Yes	Yes	Yes
Change in aggregate quantity (unit: 1,000 oz)	605	−4,371***	−2,878***	−1,674***	−852***	−258***
	(2,141)	(1,223)	(865)	(488)	(186)	(65)
Change in % of pretax volume in Philadelphia with cross-shopping	.039	−.392***	−.294***	−.356***	−.634***	−.199***
	(.139)	(.110)	(.088)	(.104)	(.138)	(.050)
Change in % of pretax volume in Philadelphia without cross-shopping	−.434***	−.521***	−.432***	−.469***	−.669***	−.151***
	(.091)	(.104)	(.075)	(.084)	(.093)	(.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

*$p < .1$.
**$p < .05$.
***$p < .01$.
Notes: FEs = fixed effects. Some stores do not offer all categories of beverages, and thus the number of observations differs slightly across columns. Standard errors are clustered at the store and week levels and reported in parentheses.

Philadelphia is offset by cross-shopping. The share of cross-shopping in the soda category is significantly different from the shares of cross-shopping in all other taxed categories.[43]

Nutritional Intake

Next, we analyze the impact of the tax on nutritional intake, namely on calories and grams of sugar. Because there is heterogeneity in calorie and sugar content among both taxed and untaxed beverages, calorie and sugar intake does not necessarily need to change by the same magnitude as sales of taxed beverages. In this section, we therefore investigate whether changes in nutritional intake differ significantly from the observed 22% reduction in quantity (net of cross-shopping).

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 16%. In Table A13 in the web appendix, we replicate this regression using sugar content as the dependent variable and find a decrease of 15%. The estimates for both calories and sugar are smaller in magnitude than the impact on sales of taxed beverages, but contain the sales effect of 22% in their confidence intervals.

The calories regression differs in effect magnitude from the previous cross-shopping regression based on quantity sold (see Table 5) for two reasons. First, we observe a statistically significant increase 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 test for a differential response among more/less healthy taxed products by replicating the cross-shopping regression specification, based on sales of only high- or low-calorie taxed products.[44] 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

[43] We test whether the share of cross-shopping (relative to the decrease at stores in Philadelphia) for soda differs from each of the other categories in Table 7 through a series of pairwise tests. The null hypotheses of equal cross-shopping shares are rejected for all pairwise comparisons.

[44] We classify products that contain less than/more than the median level (10 calories per ounce) as low/high-calorie products.

Table 8. Impact on Nutritional Intake.

Dependent Variable	(1) Calories All Beverages	(2) Ounces Sold Low Calorie Taxed Beverages	(3) Ounces Sold High Calorie Taxed Beverages
Average pretax quantities/calories	1,392,713	52,280	71,836
Philadelphia × AfterTax	−523,176***	−24,755***	−31,537***
	(95,954)	(4,200)	(5,907)
0–2 miles outside city border × AfterTax	636,965***	19,184***	44,625***
	(204,752)	(6,950)	(14,347)
2–4 miles outside city border × AfterTax	192,558***	5,538*	12,817***
	(68,405)	(2,900)	(4,253)
4–6 miles outside city border × AfterTax	93,293**	2,741	5,876**
	(42,916)	(1,807)	(2,543)
Store FEs	Yes	Yes	Yes
Week FEs	Yes	Yes	Yes
Change in aggregate quantity (unit: 1,000 oz/calories)	−78,397*	−5,615	−3,859
	(42,954)	(1,800)	(2,723)
Change in % of pretax volume in Philadelphia with cross-shopping	−.158*	−.306***	−.153
	(.087)	(.098)	(.108)
Change in % of pretax volume in Philadelphia without cross-shopping	−.377***	−.480***	−.445***
	(.069)	(.081)	(.083)
Observations	213,499	213,008	213,499
Stores	1,227	1,224	1,227
Weeks	176	176	176

*$p < .1$.
**$p < .05$.
***$p < .01$.
Notes: FEs = fixed effects. High-calorie beverages are defined as products with ≥10 calories/oz (the median value for calorie content across all taxed products). Standard errors are clustered at the store and week levels and reported in parentheses.

compared with low-calorie products, but the difference in demand response is not statistically significant.[45]

Therefore, while we find a significant increase in sales of high-calorie untaxed natural juices, we do not have enough power to estimate differential effects for low- and high-calorie taxed beverages, nor can we distinguish the calorie and sugar reduction from the somewhat larger estimated reduction in sales of taxed beverages. Despite these power issues, we think the directional results are informative for policy makers who aim to understand the nutritional impact of beverage tax policies. If consumers have a preference for product characteristics such as sugar or calorie content, it follows that substitution will occur between taxed and untaxed products that are more similar along that specific dimension. For example, if consumers substitute from sugary taxed beverages to untaxed beverages that are high in sugar, this behavior will lead to a lower reaction in nutritional content than the corresponding quantity change.[46] The increase in sales of natural juices with high sugar and calorie content provides some evidence that this type of substitution occurs.

We also note that our analysis assesses the impact of the tax on nutrients consumed in the form of beverages purchased at retail stores. It is conceivable that consumers also change their food consumption in response to the tax. On one hand, consumers might substitute away from sweetened beverages to other sugary and high-calorie foods. Alternatively, the tax could lead to a decrease in consumption of sugary and high-calorie foods if the tax makes "eating healthy" more salient. Because we do not have access to data on all food purchases, such an analysis is outside the scope of this paper.

Differential Impact by Income

In this section, we explore how the tax affects different income groups. We documented previously that quantity decreases more in stores that are located in high-income areas (see Column 5 of Table 3). In general, there are three factors that might influence how demand in areas with different income profiles reacts to the tax: (1) price sensitivity, (2) preferences for sweetened beverages, and (3) transportation costs. We take as given that low-income households tend to be more price sensitive, and therefore a lower quantity reaction in low-income areas has to be driven either by differences in

[45] In Table A13 in the Web Appendix, we replicate the set of regressions in Table 8 using sugar content as the dependent variable and find quantitatively very similar results.
[46] As pointed out by Zhen, Brissette, and Ruff (2014) and Grummon et al. (2019), this limitation of the policy may be mitigated by directly taxing sugar content rather than the volume of products with any sugar content.

preferences for sweetened beverages or differences in transport costs. Drawing on a series of data patterns, we argue that the latter channel is likely to be the key driver of the differential reaction by income. In other words, the reason demand decreases less in low-income areas is because low-income households find it more costly to shift their purchases to stores outside of Philadelphia.

A first piece of evidence to support this hypothesis comes from other related studies on demand for sweetened beverages and nutritional inequality. Wang (2015) and Dubois, Griffith, and O'Connell (2017) estimate models of demand that allow for differences in price sensitivity, as well as preferences for sweetened beverages. Both studies find that low-income households are more price sensitive and have a stronger preference for sweetened beverages. When simulating the impact of a tax, they find that the price sensitivity effect dominates and low-income consumers react more strongly to a soda tax. Both studies simulate a national tax, and thus transportation costs do not play a role in their setting. Therefore, if our setting is comparable to theirs, with the exception of transportation costs, it must be the case that higher transport costs make low-income households react less to the tax. In a related article that explores the causes of nutritional inequality, Allcott et al. (2019) analyze data from the 2009 National Household Travel Survey and find that urban households in the bottom income quartile tend to travel shorter distances for shopping trips than urban households in other income groups. This fact is consistent with our hypothesis that low-income consumers face higher transportation costs for shopping trips.

To explore this further, we analyze variables that directly capture transportation costs of Philadelphia residents, namely car ownership and the local share of outbound commuters (traveling outside of the city to work). Both variables are collected at the census-tract level, and we correlate the share of car owners and outbound commuters (relative to local population size) with tract-level median income. We find that both variables are positively correlated with median income across census tracts in Philadelphia. The correlation coefficient of income with share of car ownership (share of commuters) is equal to .38 (.56).

Finally, we investigate whether we detect cross-shopping patterns that suggest 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 AfterTax dummy). We find that higher income in nearby areas in Philadelphia is associated with a larger increase in demand at border stores. However, the interaction effect with income is only statistically significant for stores 0–1 miles outside of the city border. For stores further outside of the city, we find positive interaction effects, but they are imprecisely estimated.[47] We present the results from regressions that allow for heterogeneity in demand effects as a function of income in nearby areas in Philadelphia in Table A14 in the Web Appendix.

To summarize, we provide several pieces of evidence that suggest that the reason demand decreases less in low-income areas is because low-income households face higher transportation costs. We show that low-income households are less likely to own cars or commute outside the city to work. Meanwhile, border stores outside the city that are closer to high-income Philadelphia neighborhoods tend to experience a higher increase in sales after the tax.

Summary and Implications for Policy Design

In this section, we briefly recap the empirical findings presented in the previous sections and contrast them with findings from other studies of soda taxes in the United States. We then explore the implications of observed elasticities, the amount of cross-shopping, and other estimated quantities on tax design choices such as the tax rate and geographic coverage of the tax.

Summary of Results and Comparison to Other Studies

We find a pass-through rate of 97% (a 34% price increase) and a quantity decrease of 46% at stores in Philadelphia. At stores 0–6 miles outside of the city, prices remain unchanged, but demand increases due to cross-shopping. Net of cross-shopping, quantity decreases by only 22%. Taken together, the estimated price and quantity reactions imply an elasticity of aggregate quantity sold in Philadelphia of $-46\%/34\% = -1.35$ and an elasticity net of cross-shopping of $-22\%/34\% = -.65$. While the first elasticity is the relevant one with regard to assessing tax revenue, the latter elasticity is relevant when assessing the impact on total sales of taxed beverages and thus changes in nutritional intake.

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 studies (Bollinger and Sexton 2018; Cawley and Frisvold 2017; Rojas and Wang 2017) 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

[47] Specifically, we estimate a regression that replicates Column 1 in Table 5, but we add two interaction terms for the 0–2 mile and 2–4 mile distance bands of border stores with the income level in nearby areas in Philadelphia (and the posttax 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 posttax dummy). In that specification, we find the coefficient on the interaction term for stores in the 0–1 mile distance band is statistically significant at the 5% level.

outside of the taxed area. Notably, the elasticity (with regard to quantity sold in Berkeley) 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. The difference between the Berkeley studies and our estimates for Philadelphia point to the potentially important role of geographical coverage as a tax design choice, which we explore in more detail below.

Our results are largely in line with two recent studies that analyze the Philadelphia tax based on more limited data. Cawley et al. (2020) manually collect prices for a small set of products before and after the tax and document full pass-through at stores in Philadelphia. Cawley et al. (2019) use a consumption survey to assess changes in purchases. They find an insignificant decrease in Philadelphia and a significant increase in purchases made 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), which also uses retail scanner data to analyze the impact of Philadelphia's sweetened beverage tax. Several of their findings differ substantially from ours because store coverage in our sample is more comprehensive and because of differences in modeling choices.[48] To provide a comprehensive account of what is driving these differences, we conduct a detailed comparison of our findings in Web Appendix B. Here, we focus on highlighting the differences in key estimates. Roberto et al. (2019) find that 24% of their estimated reduction in sales in Philadelphia is offset by an increase in sales in border stores, leading to a net reduction in volume of 38%. In contrast, we find that 52% of the sales reduction in Philadelphia is offset by cross-shopping, leading to a 22% reduction in net volume.[49] Roberto et al. (2019) also find a smaller 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. 2020).[50] Differences in the estimated impact on both prices and quantities lead to drastically different elasticities of −1.7 in Roberto et al. (2019) versus −.6 in our data. As we show in more detail in the "Policy Design" section, the elasticity is an important input to understanding how sales and tax revenue behave as a function of the tax rate. Moreover, the impact of changes in geographic coverage depend crucially on the magnitude of the estimated cross-shopping effect.

To the best of our knowledge, we are the first study to explicitly analyze the impact of a soda tax on nutritional intake, namely sugar and calories. We find evidence for compositional changes in demand that lead to a smaller decrease in sugar and calories relative to the decrease in quantity. While statistically we cannot distinguish the sugar/calorie decrease from the decrease in quantity, we do find (statistically significant) evidence for one of the underlying drivers of the compositional change in demand: an increase in sales of untaxed natural juice. Overall, we cautiously interpret our nutritional findings as suggestive of an overestimation of nutritional effects when using the quantity reaction of taxed beverages as a stand-in for changes in nutritional intake.

Finally, our research documents the heterogeneous impact of the tax on different income segments. We show that stores in high-income neighborhoods in Philadelphia experience a smaller sales reduction than those in low-income neighborhoods. This finding differs from the extant literature's prediction that low-income consumers would reduce their consumption more than high-income consumers in response to a counterfactual national soda tax (Dubois, Griffith, and O'Connell 2017; Wang 2015). Our analysis suggests that the difference is likely driven by the fact that Philadelphia's localized soda tax leads to a significant amount of cross-border shopping and high-income consumers face lower transportation costs and therefore engage more in cross-shopping.

Policy Design

In this section, 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¢ per ounce in Berkeley and San Francisco to 2¢ per ounce in Boulder, Colorado. 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, Illinois). Third, some cities tax only *sugar*-sweetened beverages, whereas others also apply the tax to artificially sweetened beverages (i.e., diet drinks).

[48] 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 pretax volume sales in our Philadelphia data (see Table 1). Furthermore, we find that convenience stores, which account for 19% of our pretax 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).

[49] These differences are due to the fact that Roberto et al. (2019) test for cross-border shopping by analyzing Pennsylvania stores within 3 miles of the Philadelphia border. (They do not observe data for stores more than 3 miles outside the city limits, nor do they observe nearby stores in New Jersey.) In contrast, 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.

[50] Price differences are partially driven by sample differences as well as by differences in how average prices are computed. We use volume weights based on pretax 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 10 and Web Appendix C).

Next, we discuss how changes along each of the three dimensions impact tax revenue and sales of taxed beverages,[51] 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 welfare assessment of the tax (such as the one provided by Allcott, Lockwood, and Taubinsky (2019a) but instead trace out the effect of changes in tax policy design on these outcomes.

Inference regarding the impact of tax-policy design. Before assessing policy design choices, we briefly discuss why the evidence regarding the impact of the Philadelphia tax is particularly useful to assess the impact of counterfactual tax policy choices. In particular, we believe the variation induced by the Philadelphia tax enables us to learn about policy-relevant measures that are harder to estimate using pretax data alone.

An important obstacle to evaluating the impact of localized taxes based on pretax 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 pretax data is difficult because most short-term 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 pretax 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 .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 .963.[52] Therefore, our data allow us to better assess the extent of cross-shopping triggered by a permanent price increase. We also note that the literature on store choice typically does not measure how store choice reacts to price changes for specific products but rather focuses on aspects such as distance to the store and stores' overall pricing strategies (Bell, Ho, and Tang 1998; Hoch, Drèze, and Purk 1994). This emphasis is presumably related to the fact that large and persistent price changes for specific products or categories, such as the one induced by the tax, are rare.[53]

Other challenges that arise when extrapolating from pretax data include how to infer long-run elasticities from short-run price variation and how to model the supply side in such a way to yield accurate estimates of tax pass-through.[54] 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. Similarly, because we observe tax pass-through directly in the data, our counterfactual analysis is less sensitive to specific supply-side modeling assumptions.

Tax rate. To predict outcomes under counterfactual tax rates, we assume a constant pass-through rate and a linear demand curve.[55] The constant pass-through of close to 1 can be seen as the result of a simple cost-based pricing rule where retailers simply pass any cost change through to consumers.[56] Because we want to analyze the impact of different tax rates on aggregate price and quantity reactions and their implication for revenue generation, we focus on capturing the shape of the aggregate demand and supply relationships and do not specify the underlying preferences of consumers and a model of supply-side price setting. We note that several of our findings hold the tax rate constant at the current rate or are based on tax rates close to the current rate. We deem these findings more reliable because our assumptions are more likely to hold locally. Predictions for counterfactual tax rates that are further away from the current rate should be interpreted more cautiously. Finally, when using the observed elasticity of demand in our calculations, we assume that quantity response is entirely driven by price changes rather than changes in other marketing variables. In Web Appendix L, we discuss potential nonprice effects in more detail and explain why the quantity changes we observe are likely to be a direct response to a change in price.

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

[51] We focus on sales of taxed beverages rather than nutritional outcomes in this section because we find changes in calories and sugars are not significantly different from the impact on quantity (see the "Nutritional Intake" section).

[52] In detail, we first use only the pretax 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 that the impact of competitors' prices is equal to .083 (SE = .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 posttax data and instrument own and competitor price with interactions of all store dummies with the posttax dummy. We find that in the latter case, the cross-price elasticity between stores is equal to .963 (SE = .174).

[53] Another related article is Goli and Chintagunta (2019), which studies the impact of a different type of "large shock" on store choice. The authors analyze how CVS's decision to stop carrying tobacco products affects store choice.

[54] In the context of storable products such as sweetened beverages, short-run elasticities tend to be larger than long-run elasticities due to stockpiling (Erdem, Imai, and Keane 2003; Hendel and Nevo 2006). Other articles that estimate demand for sweetened beverages based on pretax data employ various approaches to obtain long-run price elasticities. Wang (2015) estimates a dynamic demand model with inventory holdings. Dubois, Griffith, and O'Connell (2017) focus on on-the-go beverage sizes for which stockpiling is less likely to occur. Allcott, Lockwood, and Taubinsky (2019a) use quarterly data and show that there are no effects of lagged prices, leading them to conclude that quarterly-level estimation seems to be sufficient for estimating long-run elasticities.

[55] Data from the initially lower price increase in January through April 2017 (which are not used in our main empirical analysis) provide support for the linear demand assumption. In particular, when plotting the relationship of price and quantity for three different data points—the pretax period, the period January through April 2017, and the period after April 2017—we find that the relationship is close to linear. Alternative demand functions such as log-linear do not fit the data as well.

[56] Our pass-through assumption is similar to the one in Allcott, Lockwood, and Taubinsky (2019a), who assume a constant pass-through rate of 1. A simple pricing rule that passes any cost increase through to consumers could be the result of managerial costs associated with price setting, similar to the uniform pricing practices documented by DellaVigna and Gentzkow (2019).

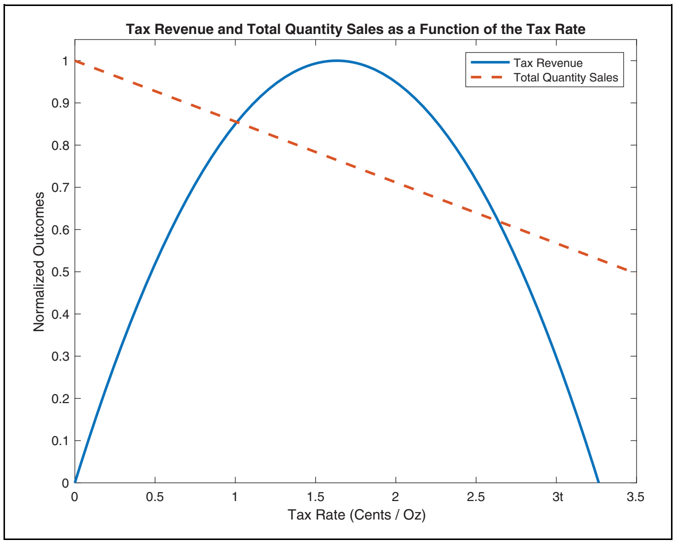

Figure 5. Predicted tax revenue and total quantity sales as a function of the tax rate.

Notes: Tax revenue is normalized relative to the maximum revenue, which is earned with a tax rate of 1.63¢/oz. Total quantity sales are normalized relative to baseline sales without a tax.

$$\text{TaxRevenue} = \tau \times Q(0) \times \left[1 + \eta_{qp}\frac{p(\tau) - p(0)}{p(0)}\right] \quad (3)$$

$$= \tau \times Q(0) \times \left[1 - 1.35\frac{.97\tau}{4.26}\right],$$

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 previous regressions: (1) elasticity: $\eta_{qp} = -46\%/34\% = -1.35$; (2) pass-through rate: $p(\tau) - p(0) = (1.45/1.5)\tau = .97\tau$; and (3) pretax price level: $p(0) = 4.26$.

Similarly, total sales of taxed beverages are given by the following relationship:

$$Q(\tau) = Q(0) \times \left[1 + \tilde{\eta}_{qp}\frac{p(\tau) - p(0)}{p(0)}\right] \quad (4)$$

$$= Q(0) \times \left[1 - .65\frac{.97\tau}{4.26}\right],$$

where $\tilde{\eta}_{qp} = -22\%/34\% = -.65$ denotes the elasticity net of cross-shopping.

Using Equation 3, 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¢ per ounce.[57] The Laffer curve shows that the actual tax rate of 1.5¢ per ounce in Philadelphia generates revenue that is equal to 99% of the revenue generated at the revenue-maximizing tax rate. A lower tax rate of 1¢ per ounce generates 85% of potential revenue, whereas a higher rate of 2¢ per ounce generates 95% of potential tax revenue. A tax as high as 3¢ per ounce (which was contemplated in Philadelphia) would generate 30% of the maximum possible tax revenue. Therefore, even if the sole goal of the tax is revenue generation, high tax rates are suboptimal because they severely shrink the tax base. Based on Equation 4, Figure 5 also shows the relationship between beverage sales and the tax rate. We find that at the revenue-maximizing rate of 1.63¢ per ounce, taxed beverage sales are reduced by 24%. A tax rate of either 1¢ or 2¢ per ounce would reduce sales by 15% and 30%, respectively.

In summary, Philadelphia levied a tax that is close to the revenue-maximizing tax rate, whereas the initially proposed tax of 3¢ per ounce would have shrunk revenue severely. We note that if policy makers pursue the dual goals of raising revenue and lowering consumption of taxed beverages, any increase of the tax rate up to the revenue-maximizing rate would lead to an improvement along both dimensions. Any further increase would continue to lead to lower sales of taxed beverages but at the expense of lower revenues. Thus, if the sole goal was to raise revenue, the current tax is close to optimal. A higher relative weight on nutritional improvement would suggest raising the tax rate further. For instance, raising the tax rate to 2¢ per ounce would lower revenue by only 5 percentage points but decrease sales of taxed beverages by 30% instead of 22%.

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 national level, it would be much more difficult for consumers to engage in cross-shopping (Allcott, Lockwood, and Taubinsky 2019b).

To assess the change in purchases of taxed beverages when cross-shopping is eliminated, we first note that the impact on the aggregate demand elasticity in Philadelphia can be bounded. If all consumers who currently cross-shop instead ceased to purchase taxed beverages altogether, the elasticity of demand in Philadelphia would remain at -1.35. Instead, if all cross-shoppers switched to buying taxed beverages at stores in Philadelphia, the elasticity of demand in Philadelphia would be equal to the elasticity of demand net of cross-shopping, $-.65$. To demonstrate how tax revenue and sales of taxed beverages vary depending on assumptions regarding the behavior of cross-shoppers, we plot how metrics change as we vary the share of cross-shoppers that switch to purchasing in Philadelphia from 0 to 1 and thus the aggregate elasticity of demand in Philadelphia from -1.35 to $-.65$.

Panel A of Figure 6 plots the revenue-maximizing tax rate, which varies between 1.63¢ per ounce at an elasticity of

[57] Because the revenue-maximizing rate is close to the current tax rate and requires only a small extrapolation, this finding is less sensitive to our assumptions of constant pass-through and linear demand.

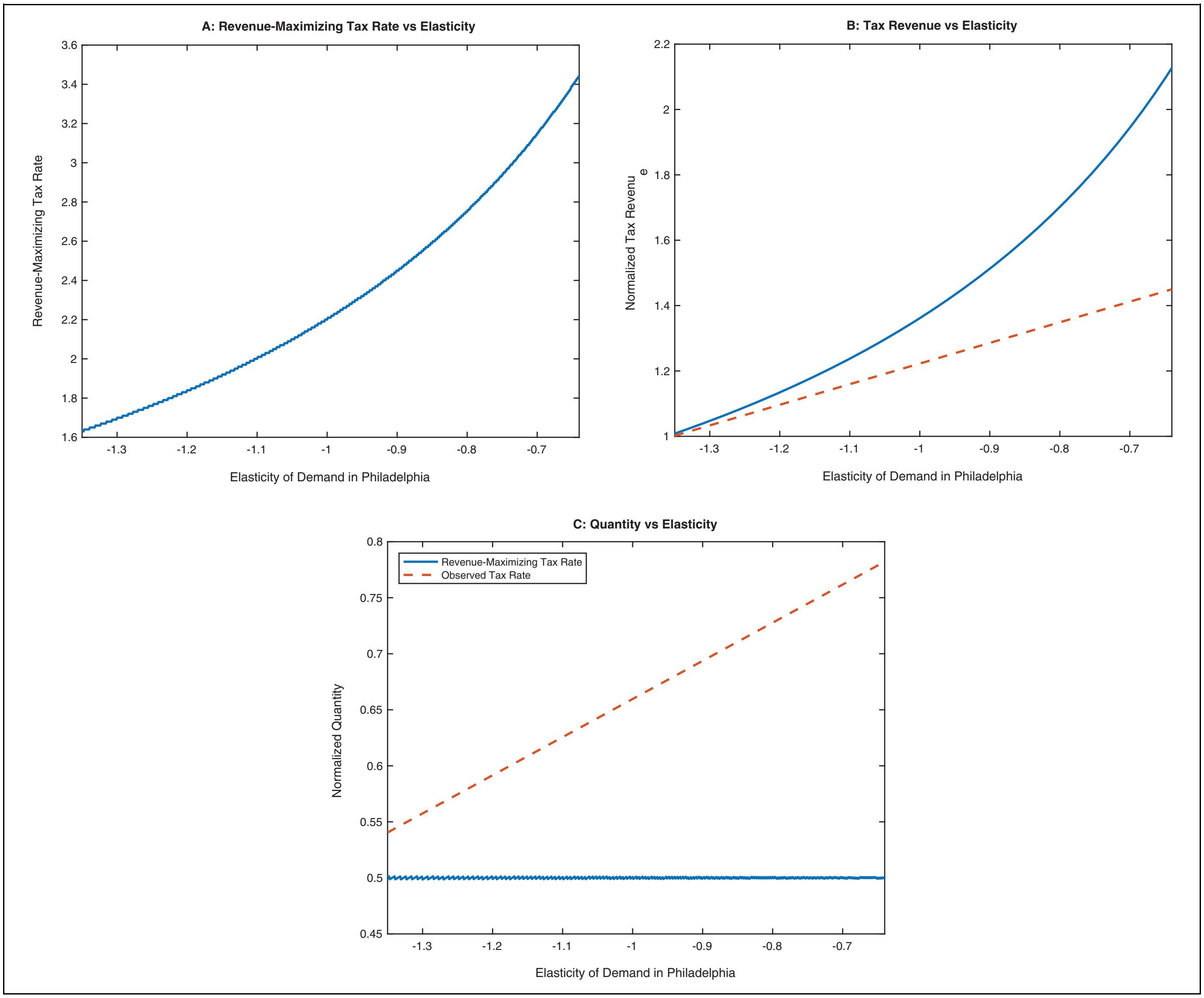

Figure 6. Predicted outcomes as a function of the elasticity of demand in Philadelphia.
Notes: From left to right within each graph, the share of cross-shoppers that switch to purchasing at stores in Philadelphia ranges from 0 to 100%, and thus the implied elasticity of demand in Philadelphia ranges from −1.35 to −.65. Tax revenue is normalized relative to the revenue earned at the observed elasticity of demand in Philadelphia (−1.35) and the observed tax rate (1.5¢/oz). Quantity is normalized relative to total pretax sales at stores in Philadelphia.

−1.35 (as described previously) and 3.47¢ per ounce if the elasticity is equal to −.65. Panel B plots tax revenue as a function of the elasticity of demand in Philadelphia. The dashed line shows tax revenues assuming that the observed tax rate of 1.5¢ per ounce is fixed. Assuming that all cross-shoppers stop buying altogether, tax revenues would be unchanged. As the proportion of cross-shoppers that substitute purchases into Philadelphia increases, demand in Philadelphia becomes more inelastic and tax revenue increases. If all cross-shoppers substituted their purchases back to stores in Philadelphia, tax revenue would be 1.45 times higher than the tax revenue generated currently. Moreover, the solid line shows that the possible gains in terms of tax revenue are substantially higher once we set the tax to the revenue-maximizing rate for a given elasticity value.

Panel C of Figure 6 traces out the implied reduction in quantity sales of taxed beverages under different scenarios with regard to cross-shopping. Again, the dashed line represents the change in sales under the observed tax rate. If cross-shoppers stop buying taxed beverages altogether, the elasticity of demand in Philadelphia is −1.35 and quantity decreases by 46% (the observed decrease in Philadelphia). As the proportion of cross-shoppers who substitute their purchases to Philadelphia stores increases, demand in Philadelphia becomes more inelastic, and we see a smaller decrease in quantity. At the extreme, if all cross-shoppers switched to purchasing taxed beverages at stores in Philadelphia, the demand elasticity would be −.65 and quantity would decrease by only 22%. Interestingly, when we allow the tax rate to be set to the revenue-maximizing level across the spectrum of elasticities, we find that sales of taxed beverages are fairly constant, regardless of whether cross-shoppers continue to buy or not.[58] Under revenue-maximizing tax rates, quantity sales are about 50% lower than pretax levels, which is a large improvement relative to the observed 22% reduction when cross-shopping is feasible and the tax rate is 1.5¢ per ounce.

Based on this analysis, we conclude that applying the tax to a larger geographical area has the potential to raise more tax revenue while lowering the sales of taxed beverages.[59] If cross-shoppers stop buying altogether and the tax is kept fixed at the current rate, tax revenue would remain unchanged, but we would see large improvements in nutritional intake due to a decrease in sales of taxed beverages. If some cross-shoppers substitute their purchases back into the city, tax revenue would increase and we would see more modest improvements in nutritional intake relative to the current setting with cross-shopping. When allowing the tax rate to adjust to the revenue-maximizing rate, both revenue gains and the reduction in sales of taxed beverages are larger than under the current tax rate. These patterns illustrate that regardless of the specific policy goals, cross-shopping is detrimental to both the ability to raise tax revenue and to reduce purchases of unhealthy beverages.

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 SSBs but exclude artificially sweetened drinks, whereas Philadelphia also taxes artificially sweetened drinks. Unfortunately, our data are less suitable to analyze the impact of product coverage because the Philadelphia tax does not induce a change in the relative price of diet and SSBs and estimating cross-elasticities between those two sets of products is therefore 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., Allcott, Lockwood, and Taubinsky 2019a; Wang 2015). 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, but nutritional intake would likely improve. Contrary to changes in geographical coverage, product coverage decisions are therefore likely to affect health and revenue goals differently.

[58] This relationship is driven by two forces that work in opposite directions and cancel each other out. Going from left to right in Figure 6, Panel C, we increase the share of cross-shoppers that switch to purchasing at stores in Philadelphia, thus increasing the sales of taxed beverages when holding the tax rate fixed. However, increasing the share of cross-shoppers that switch to purchasing at stores in Philadelphia also leads to a higher revenue-maximizing tax rate, which acts as an opposing force that depresses sales of taxed beverages.

[59] We note that a full evaluation of a tax that is applied to a larger geographical area, such as a state-level tax, would have to take tax revenue and demand effects in areas outside of Philadelphia into account as well. Such an analysis is outside of the scope of this research.

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 that the tax is almost fully passed through at most stores. Although the aggregate consumption of taxed beverages decreases, the magnitude of the decrease is reduced considerably because many consumers avoid the tax by cross-shopping. Furthermore, the large amount of cross-shopping reduces the tax base and therefore limits the ability to raise tax revenue. To the best of our knowledge, we are the first to explore the impact of a soda tax on nutritional intake. We document compositional changes in demand, which lead to smaller nutritional gains relative to quantity gains, though the difference is not statistically significant. Finally, purchase quantity decreases less in low-income neighborhoods, most likely because low-income households 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 regard to the broader question of how to design soda taxes or other types of sin taxes. If taxes are levied over a small geographic area (as is the case for all current U.S. soda taxes), cross-shopping leads to relatively elastic demand, and thus high tax rates will be suboptimal for generating revenue because they reduce the tax base. Furthermore, broadening the tax base along the geographic dimension will likely generate greater tax revenue and lead to larger nutritional improvements.

Our results are also relevant for managers in the retail sector who aim to understand the consequences of soda taxes that might be implemented in other cities. First, the prevalence of cross-shopping suggests that retail chains are losing less revenue than the sales decrease in Philadelphia. All chains in our sample have a footprint in both Philadelphia and the area near the city border, and thus they may recover some of the lost sales in Philadelphia at stores outside the city. Second, stores outside of Philadelphia (especially those within 2 miles of the border) experience large increases in demand, and thus cross-shopping effects are important for inventory management. Finally, it appears that stores in our sample are not responding to differences in the threat of cross-shopping by adjusting their pricing differentially. Most notably, stores that are located close to the city border in Philadelphia lose more demand but exhibit similar pass-through rates to stores further away from the border. Our findings suggest that more granular pricing strategies that take cross-shopping into account could benefit retailers.

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