Web Appendix: The Impact of Soda Taxes: Pass-through, Tax Avoidance, and Nutritional Effects (Seiler, Tuchman, Yao)

A Store Coverage in IRI Data

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

Due to concerns about data privacy, we do not report coverage rates in levels, but only report *differences* in coverage rates between various groups of stores. Out of all potential stores, more than 60% are tracked by IRI. We assess selection into being tracked along two dimensions that are particularly relevant for our analysis, namely, along the geographic dimension and across store formats. Geographic coverage, which we analyze in the top panel of Table A1, is particularly important because we contrast the change in demand in Philadelphia with the change in stores near the city border when calculating the change in total demand. When considering all types of stores, we find the coverage is very similar. The difference between coverage in Philadelphia and coverage up to 6 miles outside the city is only .6 percentage points, and this difference is not statistically significant. We also assess geographic coverage by store format, first by small versus large stores (small-format stores comprise Drug Stores, Convenience Stores, and Dollar Stores; large-format stores comprise Grocery Stores, Mass Merchants, and Wholesale Club Stores) and then separately for the six different formats. We do not find a significant difference in coverage for any of these groups of stores.

In the lower panel of Table A1, we focus on the format dimension. The most important aspect

Geographic Coverage	Diff. in Coverage Rate Phil. Minus 6 Miles Outside		P-value Diff. in Means Test			
All Stores	.006		.793			
Small Format	.034		.220			
Large Format	-.098		.068			
Grocery Stores	-.096		.128			
Mass Merchants	.000		1.000			
Wholesale Clubs	.111		.588			
Drug Stores	-.059		.147			
Convenience Stores	.039		.322			
Dollar Stores	.068		.315			
Store Format Coverage	Diff. in Coverage Rate Small Minus Large Format					P-value Diff. in Means Test
	.019					.535
	Diff. in Coverage Rate Relative to Convenience Stores					
	Drug Stores	Dollar Stores	Grocery Stores	Mass Merch.	Wholes. Club	
	.247	.117	.001	.325	.367	.000

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

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

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

B Detailed Comparison to Roberto et al. (2019)

Roberto et al. (2019)'s analysis of the Philadelphia tax uses similar retail scanner data, but their data is less comprehensive than ours both geographically and in terms of the retail formats observed in the data. Where their analysis overlaps with ours, effect magnitudes sometimes differ substantially and hence our conclusions with regard to the effectiveness of the tax are different. This web appendix describes how our samples differ and explains why these differences generate different estimates for price pass-through, quantity reduction, and price elasticities.

Differences in the Data Although Roberto et al. (2019) also analyze retail scanner data from the same data provider (IRI), our samples are not identical.

1. In addition to the retail formats observed by Roberto et al. (2019), we also observe 116 convenience stores, 54 dollar stores, and 2 wholesale club stores that together account for 26% of the pre-tax volume sales in our Philadelphia data (see Table 1). In Web Appendix A, we show that our sample is representative of the Philadelphia retail landscape in terms of the distribution of large- and small-format retailers.

2. With regard to stores outside of Philadelphia that may benefit from cross-shopping, Roberto et al. (2019) observe stores in Pennsylvania that are located within 3 miles of the Philadelphia

[1] In order to re-weight the cross-shopping regression, we multiply the various geography-specific coefficients (Philadelphia, 0-2 mile band, etc.) by the number of stores in the universe of stores rather than the number of stores in the sample.

[2] We re-weight quantity and price effects using the estimates from the price and quantity regressions with chain-specific effects (column (2) of Tables (2) and (3)). Based on those regression estimates, we compute the average effect by taking a weighted average of the chain-specific effects. The weights are given by the number of stores associated with a given format in the store universe, adjusted for the prevalence of chains within a given format. For example, for grocery stores, the calculation would be: $(\beta_A \times w_A + \beta_B \times w_B + \beta_C \times w_C) \times \#GroceryStoresInUniverse$ where $w_j = (\frac{\#storesj}{\#storesA + \#storesB + \#storesC})$. This calculation re-scales the effects up to the full universe of stores, while giving more weight to estimates for formats that are more prevalent within the city.

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. Furthermore, within the geography 0-3 miles outside Philadelphia in Pennsylvania, we observe 72 pharmacies, supermarkets, and mass merchant stores while Roberto et al. (2019) only observe 51.[3]

3. Roberto et al. (2019) analyze data from 2016 – 2017. Our sample runs from 2015 through September 2018, so we observe an additional year of data in the pre-tax period and an additional 9 months of data in the post-tax period. Our longer panel allows us to document evidence of a short-term adjustment period and omit this adjustment period from our main estimates, while Roberto et al. (2019)'s post-tax period includes this four-month adjustment period.[4]

4. We compute and analyze volume-weighted prices, while Roberto et al. (2019) compute a simple average price across UPCs.

5. We observe retail chain identities (which are typically anonymized), which allows us to account for the fact that one retailer's price was recorded net of the tax in the data (see footnote 11 and the discussion in Web Appendix C).

6. The two papers use different control groups. Roberto et al. (2019) use Baltimore stores as a control group, while we use stores 6+ miles outside Philadelphia that lie in the 3-digit zip codes that surround Philadelphia. Price and quantity trends in both of our control groups seem fairly similar, so we do not expect this to drive substantial differences in our analyses.

7. We conduct some data cleaning (described in Web Appendix E) to remove stores and products that are observed infrequently and to fill in missing prices in weeks when no purchase is made. Differences in how we clean the data could generate slight differences in the samples that we analyze. For example, Roberto et al. (2019) drop energy drinks from their sample, but we retain these products because they were subject to the tax.

[3]Our store counts are very similar within the city of Philadelphia (our final sample includes 180 pharmacies, supermarkets, and mass merchant stores and Roberto et al. (2019) analyze 185 of these store types.) Thus, we hypothesize that the differences in our store counts for Pennsylvania stores are due to how we define this geography. Roberto et al. (2019) analyze cross-border shopping in "zip codes within approximately 3 miles of Philadelphia's border in 3 Pennsylvania counties (Bucks, Delaware, and Montgomery)." In contrast, we observe precise store addresses for all pharmacies, supermarkets, and mass merchant stores, and measure distance from the city border using this precise location information.

[4]The longer panel also allows us to show that the important force of cross-shopping is not just a short-run response to the tax.

Price Pass-Through The aforementioned differences in our sample lead us to estimate differences in the effect of the tax on prices. Specifically, Roberto et al. (2019) estimate lower pre-tax prices and lower pass-through than we do.[5]

In an effort to understand why the differences in our samples lead to different estimates of pass-through, we attempt to replicate Roberto et al. (2019)'s analysis of prices by restricting our sample of stores and time periods to be consistent with theirs. We retain the first 4 months after the tax went into effect and we drop the additional 9 months of data we have from 2018, so the post-tax period is all of 2017. We also remove the correction to the prices for Grocery C as described in footnote 11. In Table A2, we report pass-through estimates separately by retail format in order to facilitate comparison with Roberto et al. (2019). Column (1) reports Roberto et al. (2019)'s estimates. Column (2) reports our replication attempt in which we give equal weight to all UPCs. Column (3) reports our replication attempt where we make the aforementioned restrictions to our sample, but we use volume-based weights in the analysis. Finally, column (4) reports our estimates based on our own dataset using the full set of retail chains, time periods, product categories, and volume-weights.

Columns 2 and 3 help us understand what differences in the data are driving the differences in our results. Our replication shows that differences in pre-tax prices are largely due to the fact that Roberto et al. (2019) give equal weight to all UPCs, while we construct a volume-weighted price variable that gives more weight to the prices of UPCs that have higher pre-tax sales volume (and tend to be cheaper on a per oz basis). Furthermore, the correction we make to Grocery C prices does not fully account for the difference in estimated pass-through at grocery stores, but it does explain 85% of the difference. [6]

After attempting to replicate Roberto et al. (2019)'s data to the best of our abilities, we still find some differences in pre-tax prices and estimated pass-through. Comparing columns 1 and 2, our calculated pre-tax prices still tend to be lower than Roberto et al. (2019)'s, and although Roberto et al. (2019)'s estimated pass-through at mass merchandisers and supermarkets lies within our replication attempt's 95% CI, their estimate for pharmacies lies outside our confidence interval.

When we compute the percentage price increase that results from the tax (an input into the price elasticity calculation), we are able to come close to approximating Roberto et al. (2019)'s results.

[5]The difference in estimated pass-through is notable because Roberto et al. (2019)'s finding of incomplete pass-through suggests that the city can extract some "free" revenue from companies without distorting market outcomes for consumers. In contrast, our estimate of complete pass-through suggests that this is not the case.

[6]With the Grocery C price adjustment, we estimate average pass-through at Philadelphia grocery stores to be 1.57 cents per oz. Without the price adjustment but holding the rest of our sample fixed, our estimate of pass-through is .79 cents per oz. Roberto et al estimate grocery pass-through of .65 cents per oz.

	(1) Roberto et al. (2019)	(2) Replication Simple Average	(3) Replication Volume-Weighted	(4) Our Estimate
Drug Stores / Pharmacies				
Av. Pre-Tax Price/Oz	6.60	6.79	5.56	5.52
Pass-Through	1.56	1.08	1.16	1.30
	(1.50, 1.62)	(.96, 1.19)	(1.06, 1.26)	(1.23, 1.37)
% Price Change	23.5%	21.1%	20.7%	23.7%
	-	(18.5%, 23.8%)	(18.6%, 22.8%)	(22.0%, 25.3%)
Mass Merchandisers				
Av. Pre-Tax Price/Oz	5.28	4.50	3.67	3.68
Pass-Through	.87	.72	.80	.88
	(.72, 1.02)	(.51, .93)	(.63, .98)	(.68, 1.08)
% Price Change	16.4%	18.9%	22.1%	24.4%
	-	(13.1%, 25.0%)	(16.0%, 28.4%)	(17.5%, 31.6%)
Grocery Stores / Supermarkets				
Av. Pre-Tax Price/Oz	5.43	4.23	3.33	3.28
Pass-Through	.65	.62	.77	1.57
	(.60, .69)	(.41, .84)	(.55, .99)	(1.48, 1.66)
% Price Change	11.8%	16.1%	20.4%	46.3%
	-	(10.7%, 21.8%)	(14.7%, 26.4%)	(42.9%, 49.9%)
Other Retail Formats				
Av. Pre-Tax Price/Oz	-	7.27	6.49	6.36
Pass-Through	-	1.45	1.47	1.58
	-	(1.38, 1.51)	(1.40, 1.54)	(1.53, 1.64)
% Price Change	-	25.4%	25.9%	28.3%
	-	(23.7%, 27.2%)	(23.9%, 27.59%)	(26.4%, 30.3%)

Table A2: **Estimated Pass-Through and Percent Price Increase by Retail Format.** 95% confidence intervals are reported in parentheses.

Comparing columns 1 and 2, Roberto et al. (2019)'s reported percentage price increase estimates lie within our 95% confidence intervals for all three retail formats where our samples overlap.

Quantity Effects Roberto et al. (2019) estimate that volume sales of taxed beverages in Philadelphia decrease by 51%. We estimate a smaller 46% decrease in sales in Philadelphia. This is partially driven by the fact that our data includes additional retail formats, and the quantity response at these formats differs from the response at the sub-set of retail formats observed by Roberto et al. (2019). For example, convenience stores (which account for 19% of our pre-tax volume sales) experience only a 10% decrease in sales, which is substantially smaller than the decrease at grocery stores and mass-

	Roberto et al. (2019)	Replication	Our Estimate
Change in % of Pre-Tax Volume in Philadelphia **w/o Cross-Shopping**	-51%	-50% (-69%, -32%)	-46% (-62%, -30%)

	Roberto et al. (2019)	Replication Restricting to 51 stores in PA 0-3 mile band	Replication Using all 72 stores in PA 0-3 mile band	Our Estimate
Change in % of Pre-Tax Volume in Philadelphia **w/ Cross-Shopping**	-38%	-34% (-54%, -14%)	-28% (-49%, -6%)	-22% (-42%, -2%)

Table A3: **Estimated Quantity Response.**

merchants (see Table 3). Furthermore, our main analyses drop the first four-months of the post-tax period. Dropping these months leads us to estimate a larger effect of the tax, because the quantity response in the first few months was smaller than the long-run response (see Web Appendix G). If we restrict our data to both the time periods and retail formats analyzed by Roberto et al. (2019), these two forces partially off-set each other and we estimate a 50% reduction in volume sales of taxed beverages in Philadelphia. This is very close to Roberto et al. (2019)'s estimate of 51% (see the top panel of Table A3).

Roberto et al. (2019) also test for cross-border shopping by analyzing Pennsylvania stores within 3 miles of the Philadelphia border; however, they do not observe data for stores more than 3 miles outside the city limits, nor do they observe nearby stores in New Jersey. They conclude that 24% of their estimated reduction in sales in Philadelphia is offset by an increase in sales in these border stores, resulting in an overall reduction of 38%. We find that 52% of the sales reduction in Philadelphia is offset by cross-shopping, resulting in an overall reduction of 22%. We find evidence of significant increases in sales up to 6 miles outside of the city limits, as well as evidence of cross-shopping at stores in both Pennsylvania and New Jersey. When we restrict our sample to the retail formats observed by Roberto et al. (2019), the limited geographies, and the same time period they analyze, we estimate a 28% overall reduction. As we describe under bullet-point (2) above, within the Pennsylvania 0-3 mile band, we observe 72 stores, while Roberto et al. (2019) only observe 51 stores. If we "restrict" our sample to only have 51 stores in that band, and assume that these stores have the average per store increase from cross-shopping (i.e. assume stores are missing at random in Roberto et al. (2019)'s sample), we come close to replicating Roberto et al. (2019)'s quantity effects. Under this additional

Dependent Variable	Ounces Sold Taxed Beverages	# Stores in Geogr. Area	Share of Cross-Shopped Volume
Philadelphia × After Tax	-56,193***	357	
	(9,742)		
0-3 Miles Outside City Border ×	61,922***	135	79%
PA × After Tax	(17,407)		
3-6 Miles Outside City Border ×	8,954*	108	9%
PA × After Tax	(4,898)		
0-3 Miles Outside City Border ×	11,908	45	5%
NJ × After Tax	(9,262)		
3-6 Miles Outside City Border ×	6,985*	107	7%
NJ × After Tax	(3,883)		
Store FE	Yes		
Week FE	Yes		
Change in Aggregate	-9,451**		
Quantity (Unit: 1,000 Ounces)	(4,354)		
Change in % of Pre-tax Volume in	-.216**		
Philadelphia **w/ Cross-Shopping**	(.100)		
Change in % of Pre-tax Volume in	-.459***		
Philadelphia **w/o Cross-Shopping**	(.080)		
Observations	213,499		
Stores	1,227		
Weeks	176		

Table A4: **Quantity Reaction in Stores Near the City Border.**

restriction, we estimate an overall reduction of 34% in volume sales, while they estimate an overall reduction of 38%. This analysis indicates that failing to account for cross-shopping in other geographic regions, as well as missing stores in Pennsylvania 0-3 miles outside Philadelphia, both contribute to Roberto et al. (2019)'s under-estimate of the extent to which cross-shopping occurs in the data (see the lower panel of Table A3).

To further decompose the sources of the difference in our cross-shopping estimates, we break-down the fraction of all cross-shopped volume that was substituted to different geographic regions (see Table A4). Specifically, we estimate a regression similar to what is reported in Table 5, but to facilitate comparison with Roberto et al. (2019), we separately measure cross-shopping at stores 0-3 and 3-6 miles outside the city border in both Pennsylvania and New Jersey. We perform these calculations working off our full sample of stores and time periods. We find that 79% of cross-shopped volume is to Pennsylvania stores 0-3 miles outside Philadelphia, 9% of cross-shopped volume is to Pennsylvania stores 3-6 miles outside Philadelphia, and 12% of cross-shopped volume is to New Jersey stores.

Price Elasticities Differences in the estimated impact of the tax on both prices and quantities lead to substantially different elasticities. Roberto et al. (2019) report a price elasticity of -1.7, which is the price elasticity of demand after accounting for cross-shopping. Because we estimate a smaller percent reduction in volume and a larger percent increase in price, we estimate the elasticity of overall consumption (i.e. net of cross-shopping) to be much smaller, $\eta_{qp} = -22\%/34\% = -0.6$.

We attempt to replicate Roberto et al. (2019)'s elasticity calculation by restricting our sample of stores and time periods to be comparable. Using our "replication sample," the implied elasticity of overall consumption is $\eta_{qp} = -34\%/18\% = -1.9$ when using simple average prices and $\eta_{qp} = -34\%/21\% = -1.6$ when using volume-weighted prices. These are reasonably close to Roberto et al. (2019)'s estimated elasticity of -1.7.[7]

C Heterogeneity in Price Recording Across Retailers

As outlined in footnote 11, one retailer in our dataset reported the soda tax as a separate item on the checkout receipt rather than reporting the total price including the tax. Figure A1 shows two receipts from this retailer – one from 1/1/2017 and a recent receipt from 10/18/2019 – in which the Philadelphia soda tax is reported as a separate item (see highlighted area of the receipt). In contrast, Figure A2 shows that on the shelf tags, this retailer reports the price inclusive of the tax, along with a note that that price includes the Philadelphia beverage tax. We have confirmed that the shelf tag price is equal to the sum of the price and the tax as reported on the receipt. Further, we compared receipts to the relevant prices in the IRI data to establish that throughout our sample period, our data reports price net of the tax for this retailer. Thus, in order to recover the effective price paid by consumers, we need to add the 1.5 cents/oz tax onto this retailer's prices starting in January 2017.

Figure A3 shows two additional receipts from two other retailers, where the tax is not broken out as a separate item, but the price includes the tax. We verified that all retailers except for the one in Figure A1 report the tax in this fashion. We also compared receipts from these retailers to the relevant prices in the IRI data to establish that our data reports total price inclusive of the tax for all retailers except the one in Figure A1.

[7] According to Roberto et al. (2019)'s supplementary appendix, they arrive at this estimate by separately computing an elasticity for each retail format - beverage size combination, and then computing the volume-weighted average of these elasticities. In contrast, we compute elasticities by dividing the overall reduction in quantity by the average increase in price.

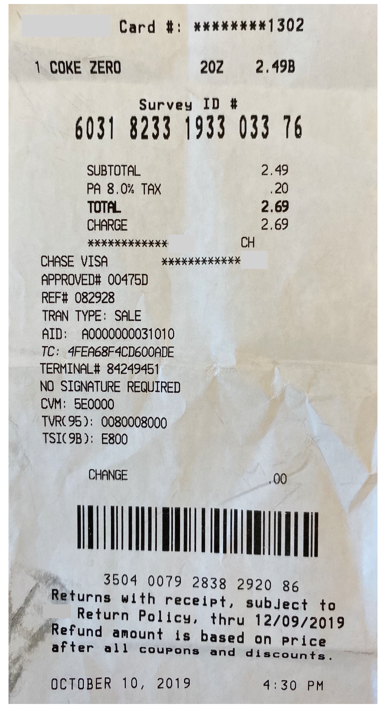
Figure A1: **Sample Receipts with Separate Beverage Tax Reporting.** The two receipts are from the same retailer on two different dates. (The day the tax was introduced 1/1/2017 and a recent receipt from 10/18/2019. See dates in the top left corner of both receipts.)

Figure A2: **Sample Price Tag with Beverage Tax Included in Price.** The price tag shown above is from the retailer that reports the tax separately on the receipt (see Figure A1). Unlike the reporting on the receipt, the shelf tag includes the tax in the product price. This shelf tag was photographed on 10/18/2019 and corresponds to the item purchased in the receipt on the right side of Figure A1. The shelf tag reports a price of \$2.25, which is the sum of the price \$1.95 and the tax \$.30 reported on the receipt.

D Measurement Error in Demographic Variables

As discussed in the "Data" section, we do not observe precise store addresses and instead only observe store zip codes for three types of stores that together account for 34% of pre-tax volume sales in Philadelphia. This coarser location information might lead to measurement error in our demographic variables (income and obesity) which are defined based on a catchment area of 1 mile around the store's

Cashier #111246578 10/10/19 4:49PM
1 COKE ZERO PET NGL 20Z 2.29 TF
1 Items Subtotal $2.29
Tax $.18
Total $2.47
VISA SALE $2.47
VISA card * #XXXXXXXXXXXX
App #AA APPROVAL AUTO
Ref # 08172D
Entry Method: Chip
Application Label: VISA CREDIT
AID: A0000000031010
TVR: 0080008000
TSI: E800
AC: 3D72C7CE9642A382
ARC: 00
Tendered $2.47
Cash Change $.00



Card #: ********1302
1 COKE ZERO 20Z 2.49B
Survey ID #
6031 8233 1933 033 76
SUBTOTAL 2.49
PA 8.0% TAX .20
TOTAL 2.69
CHARGE 2.69
************ CH
CHASE VISA ************
APPROVED# 00475D
REF# 082928
TRAN TYPE: SALE
AID: A0000000031010
TC: 4FEA68F4CD600ADE
TERMINAL# 84249451
NO SIGNATURE REQUIRED
CVM: 5E0000
TVR(95): 0080008000
TSI(9B): E800
CHANGE .00
3504 0079 2838 2920 86
Returns with receipt, subject to
Return Policy, thru 12/09/2019
Refund amount is based on price
after all coupons and discounts.
OCTOBER 10, 2019 4:30 PM

Figure A3: **Sample Receipts from Two Retailers that Do Not Report the Beverage Tax Separately.** These receipts were both obtained on 10/10/2019. Unlike the receipts in Figure A1, both retailers do not report the beverage tax as a separate item on the receipt or on the shelf tags. This method of reporting prices inclusive of the tax occurs at all retailers in our data except for the one retailer in Figure A1.

location. For stores without precise location information, we use a radius around the centroid of the zip code that they are located in. To help visualize the nature of our data, Figure A4 overlays the stores in our data with the median income across census tracts in Philadelphia. Stores with precise location information are represented by green circles, while stores without precise addresses in our data are represented by squares located at the centroid of the store's zip code. The map suggests that income is relatively homogenous within zip codes and hence imprecise location information is unlikely to lead to large measurement error. The three large zip codes in the south of Philadelphia (19153, 19145, and 19148) may be the most problematic in terms of measurement error because parts of these zip-codes are non-residential and hence the centroid approximation is less appropriate.

In Table A5 we explore the possibility that measurement error affects our estimates related to demographics. Specifically, we investigate the robustness of our findings for the income and obesity interactions in the price and quantity regressions in Tables 2 and 3. Columns (1) and (5) in both panels replicate our baseline findings and correspond to columns (4) and (5) in Tables 2 and 3, respectively.

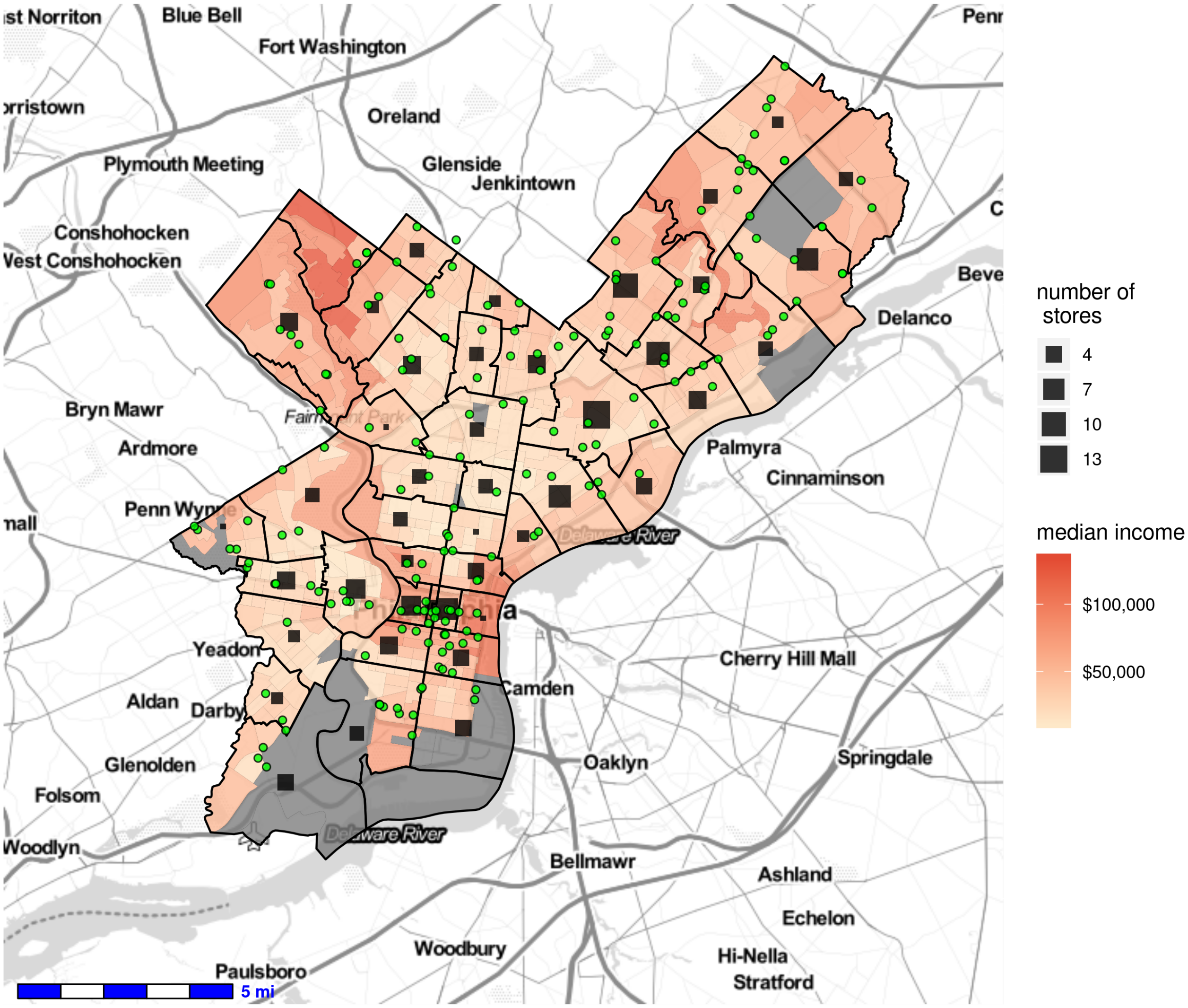


Figure A4: **Store Locations and Median Income by Census Tract in Philadelphia.** Stores with precise addresses are represented by green circles. Stores for which we only observe zip code are represented by squares. These stores are assumed to be located at the centroid of their zip code. The size of the square indicates the number of stores without precise addresses that are located in that zip code (the legend displays 4 examples showing how the number of stores corresponds to the size of the square). Black outlines denote zip code boundaries and the red fill indicates the median income across census tracts. Grey regions indicate census tracts where no income information is recorded because there is little or no residential population.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Sample	Full Sample	Stores w/ Precise Address	Full Sample	Full Sample	Full Sample	Stores w/ Precise Address	Full Sample	Full Sample
Radius	1 Mile	1 Mile	1.5 Miles	2 Miles	1 Mile	1 Mile	1.5 Miles	2 Miles
	Log Price/Oz	Log Price/Oz	Log Price/Oz	Log Price/Oz	Log Price/Oz	Log Price/Oz	Log Price/Oz	Log Price/Oz
Income × Philadelphia × AfterTax	-.024***	-.030***	-.031***	-.033***				
	(.009)	(.011)	(.009)	(.012)				
Obesity Rate × Philadelphia × AfterTax					.033***	.034***	.032***	.031***
					(.009)	(.011)	(.009)	(.009)
	Log Ounces	Log Ounces	Log Ounces	Log Ounces	Log Ounces	Log Ounces	Log Ounces	Log Ounces
Income × Philadelphia × AfterTax	-.106**	-.151**	-.105**	-.175***				
	(.044)	(.071)	(.044)	(.054)				
Obesity Rate × Philadelphia × AfterTax					-.030	-.021	-.031	-.033
					(.041)	(.064)	(.041)	(.041)
Observations	144,700	75,400	144,700	144,700	144,700	75,400	144,700	144,700
Stores	832	431	832	832	832	431	832	832
Weeks	176	176	176	176	176	176	176	176

Table A5: **Robustness to Different Catchment Area Definitions.** Each column reports results from a regression on store and week fixed effects as well as chain-specific effects. We only report the the demographic interactions in each case. Columns (1) to (4) replicate the regression specification in column (4) of Tables 2 and 3. Columns (5) to (8) replicate the regression specifications in column (5) of Tables 2 and 3. Standard errors are clustered at the store and week level and reported in parentheses. *** $p < .01$, ** $p < .05$, * $p < .1$.

In columns (2) and (6) we restrict the sample to only stores for which we observe a precise address. In all cases, point estimates are similar and not significantly different from our baseline estimates. We then further probe robustness to using a wider catchment area radius of 1.5 or 2 miles (instead of 1 mile in our baseline specification). Again, for all income and obesity interactions in the price and quantity regressions, we find results to be similar. The robustness to using a larger radius is particularly relevant because the measurement error presumably declines when the catchment area is large relative to the zip code used to define store location. Overall, these findings show that measurement error is not a major concern with regard to the estimated impact of demographics on price pass-through and quantity reaction.

E Selection of Stores and Products for the Analysis

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

First, some stores enter or exit during the sample period. We choose to drop stores that entered after January 1, 2016, or exited before December 31, 2017. As a result, for each remaining store, we have at least one year of data both before and after the tax went into effect on January 1, 2017. Next, we remove stores affiliated with retail chains that only operate within Philadelphia or only in the areas more than 6 miles outside the city. (This selection criterion leads to a sample of retail chains that are present in all three geographic areas that are relevant to our analysis: Philadelphia, the area less than 6 miles outside of the city, and the area more than 6 miles outside of the city). Finally, some UPCs were purchased infrequently at a given store. Given the nature of scanner data, infrequent purchases render it difficult to reliably measure a product's price over time.[8] We choose to only keep product/stores (a product is defined as a brand/diet-status/pack-size combination) that had sales in at least 40 weeks each year during 2016 and 2017 and to only keep UPC/store-combinations that had sales in at least 85% of the weeks that the corresponding product/store was observed in the data.[9] Our final data contain 1,227 stores and 5,070 UPCs that belong to 101 brands. This final dataset represents 89% of the unit sales in the raw data.

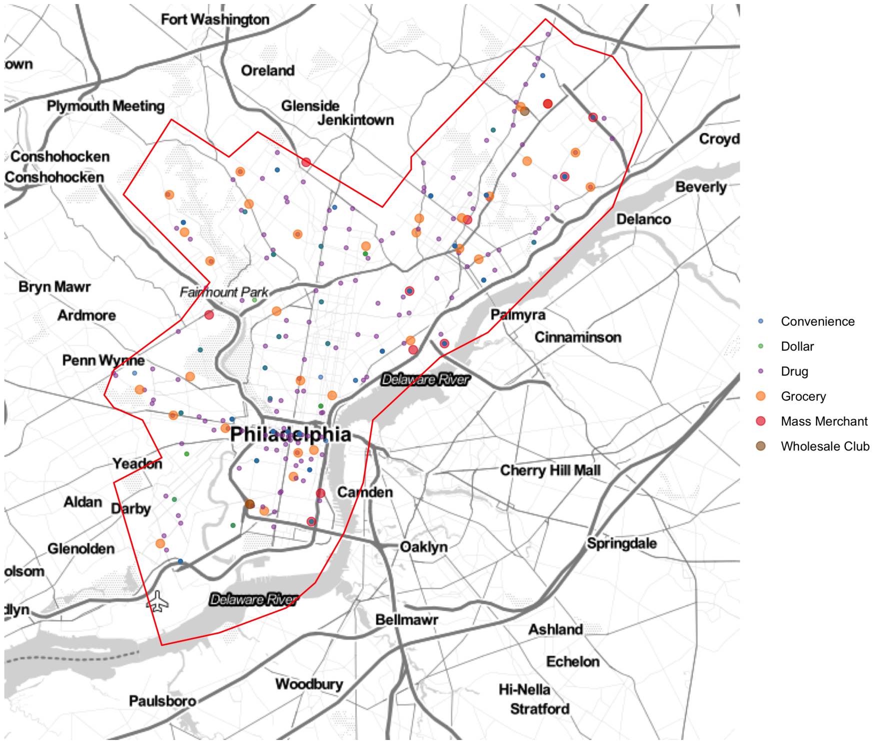


Figure A5: **Philadelphia Stores by Retail Format.**

F Additional Store Descriptive Statistics

Figure A5 shows a map of Philadelphia stores, color-coded by retail format. The map shows the stores in our sample are geographically dispersed. Table A6 summarizes the within- and across-chain variation in demographics. Looking at the average income across chains shows that Grocery A and Drugstore X stores are, on average, located in higher-income neighborhoods, whereas Grocery C and dollar stores tend to be located in lower-income neighborhoods. Moreover, the chain-specific standard deviations tend to be only slightly smaller than the standard deviations across all stores. Therefore, we are able to analyze the impact of demographics based on the variation in demographics within stores of the same chain.

	# Stores in Phil.	Median Income ($1,000s) Mean	Std. Dev	Obesity Rate Mean	Std. Dev
Grocery A	15	53.7	11.7	.26	.03
Grocery B	1	41.6	-	.26	-
Grocery C	16	37.9	11.2	.32	.06
Mass Merchant M	6	47.7	7.7	.28	.05
Other Mass Merchants	5	45.8	10.5	.28	.04
Drugstore X	45	50.9	14.5	.27	.05
Drugstore Y	80	43.2	14.7	.30	.06
Drugstore Z	17	44.0	15.6	.29	.07
Convenience St.	116	45.2	14.6	.28	.06
Wholesale Club W	2	41.4	7.8	.27	.05
Dollar Stores	54	36.1	11.8	.33	.05
All Stores	357	44.1	14.5	.29	.06

Table A6: **Within and Across-Chain Variation in Demographics.**

G Dynamics

Dynamic adjustment patterns could occur because retailers and consumers take some time to adjust their behavior in response to the tax. Furthermore, consumers might engage in tax avoidance via cross-shopping immediately after the tax goes into effect but find doing so in the long run is inconvenient. To investigate the importance of changes in the impact of the tax over time, we categorize the post-tax data into four time periods: January to April of 2017, May to August of 2017, September to December of 2017, and January 2018 to September 2018 (the end of our sample period). We then re-estimate several of our main regressions, allowing for different treatment effects in the four post-tax time periods. Table A7 reports the results of these analyses. For ease of comparison, in columns (1), (3), (5), and (7), we replicate the results in column (1) of Tables 2, 3, 4, and 5, respectively (which exclude the first four months after the tax was introduced).

In column (2), we test for changes in the pass-through rate over time. Besides the interaction of the Philadelphia dummy with the after-tax dummy, we now add further interactions of the Philadelphia

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

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

	Taxed Products				*Untaxed Products*		*Cross-shopping*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent Variable	Price/Oz	Price/Oz	Ounces Sold	Ounces Sold	Ounces Sold	Ounces Sold	Ounces Sold	Ounces Sold
Philadelphia	1.449***	1.451***	-56,192***	-55,612***	-4,521	-5,437	-56,193***	-55,612***
× AfterTax	(.022)	(.025)	(9,742)	(10,164)	(7,125)	(7,046)	(9,742)	(10,163)
Philadelphia		-.309***		18,982***		3,847		18,982***
× Jan-April 2017		(.064)		(5,147)		(4,099)		(5,146)
Philadelphia		-.002		-6,230		4,990		-6,230
× May-Aug 2017		(.020)		(4,793)		(3,195)		(4,795)
Philadelphia		-.008		3,537		-929		3,537
× Sept-Dec 2017		(.015)		(4,663)		(3,274)		(4,669)
0-2 Miles Outside							63,650***	61,787***
× AfterTax							(20,734)	(20,164)
0-2 Miles Outside								-12,196
× Jan-April 2017								(8,009)
0-2 Miles Outside								2,846
× May-Aug 2017								(2,651)
0-2 Miles Outside								4,736**
× Sept-Dec 2017								(2,009)
2-4 Miles Outside							18,364***	17,017**
× AfterTax							(7,032)	(6,663)
2-4 Miles Outside								-2,538
× Jan-April 2017								(3,184)
2-4 Miles Outside								3,029
× May-Aug 2017								(2,558)
2-4 Miles Outside								2,544**
× Sept-Dec 2017								(988)
4-6 Miles Outside							8,640**	7,819*
× AfterTax							(4,198)	(4,266)
4-6 Miles Outside								1,334
× Jan-April 2017								(2,737)
4-6 Miles Outside								2,800
× May-Aug 2017								(2,932)
4-6 Miles Outside								617
× Sept-Dec 2017								(1,774)
Store FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Week FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	144,700	159,676	144,700	159,676	144,209	159,131	213,499	235,585
Stores	832	832	832	832	832	832	1,227	1,227
Weeks	176	194	176	194	176	194	176	194

Table A7: **Dynamic Adjustment Patterns.** Columns (1), (3), (5), and (7) replicate earlier results and are based on a sample that excludes the first four months after the tax was introduced. Standard errors are clustered at the store and week level and reported in parentheses. *** $p < .01$, ** $p < .05$, * $p < .1$.

dummy with dummies for the time periods January-April of 2017, May-August of 2017, and September-December of 2017. Accordingly, the Philadelphia times after-tax coefficient now captures the long-run impact of the tax on prices for the final period of our sample from January to September 2018. The other three interaction terms capture differences in short-term price adjustments relative to the long-run pass-through rate. The interaction term of January-April of 2017 is equal to -.31 and significant, which indicates that the pass-through rate was at a slightly lower level during the first four months after the tax. In comparison, the interaction terms for May-August and September-December of 2017 are small in magnitude and not significantly different from the pass-through rate in 2018, which implies pass-through remained stable after May 2017.

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

We also investigate whether there are changes over time in the lack of reaction of prices to competitive pressure (measured by the distance to the nearest untaxed store). As documented in the "Price Reaction and Pass-Through" section, we find that firms do not charge different prices when a store is located close to the city border, despite the fact that such stores experience stronger competition and a stronger decline in sales after the tax is implemented. This finding is surprising, and we hence investigate whether firms start charging different prices depending on a store's distance to the city border in the later part of our sample period. In order to analyze such patterns, we implement a similar regression to the ones outlined above, but also include an interaction of the distance to the nearest store with dummies for the various time periods used above. We find that for the time periods used in our main sample, the coefficients on the distance interaction terms are small in magnitude and

Dependent Variable	(1) Ounces Sold	(2) Ounces Sold	(3) Ounces Sold	(4) Ounces Sold	(5) Ounces Sold
Philadelphia × AfterTax	-56,192***	-55,434***	-55,699***	-56,050***	-56,158***
	(9,742)	(9,750)	(9,785)	(9,731)	(9,752)
Philadelphia × 0-4 months BeforeTax		4,519			
		(2,790)			
Philadelphia × 0-3 months BeforeTax			3,847		
			(3,211)		
Philadelphia × 0-2 months BeforeTax				1,802	
				(4,319)	
Philadelphia × 0-1 months BeforeTax					865
					(7,072)
Store FE	Yes	Yes	Yes	Yes	Yes
Week FE	Yes	Yes	Yes	Yes	Yes
Observations	144,700	144,700	144,700	144,700	144,700
Stores	832	832	832	832	832
Weeks	176	176	176	176	176

Table A8: **Impact on Quantity Sold Before the Tax.** Standard errors are clustered at the store and week level and reported in parentheses. *** $p < .01$, ** $p < .05$, * $p < .1$.

not statistically significant. Therefore, it appears that the firms in our data do not learn to charge different prices in more or less competitive markets over time.

Finally, we also test whether there are changes in quantity sold *before* the tax goes into effect. Because the tax was publicly discussed and salient to consumers, one might expect consumers to stockpile, i.e. purchase larger quantities, immediately before the tax goes into effect. In Table A8 we report results from regressions that allow for separate effects for 0-4 months, 0-3 months, 0-2 months, and 0-1 month before the tax. In all cases, we find no evidence of a significant change in quantity sold prior to the tax.

H Volatility of Purchases and Changes in Price Sensitivity

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

We first turn to analyzing the change in volatility across all taxed beverages. To this end, we compute the variance of sales for each store/product combination separately for the pre- and post-tax period. We find that in Philadelphia, the standard deviation of sales decreased by more than 50%, falling from 1,270 to 443, whereas in control stores outside of the city (excluding the buffer zone), the variance of sales decreased only slightly from 1,204 to 1,128. We also note that during the same time, the volatility of price movements remained unchanged in Philadelphia and outside of the city. This finding suggests the nature of demand changed in a way that led to a decrease in the volatility of sales over time.

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

We also find that price sensitivity in the area 0-6 miles outside of Philadelphia decreases, but by a smaller amount than in the control group. (The elasticity changes from -2.22 to -2.02 in the 0-6 mile band versus a change from -2.21 to -1.87 in the control group). Moreover, the volatility of sales increases slightly in the 0-6 mile band (the standard deviation of sales changes from 1,208 to 1,374) whereas it decreases in the control group (the standard deviation of sales changes from 1,204 to 1,128). These patterns provide further evidence that price sensitive customers stop purchasing taxed beverages in Philadelphia and start shopping at stores in the 0-6 mile band.[10]

[10]We find that in the case of soda (the category for which consumers are more likely to engage in cross-shopping) the volatility of sales in the area 0-6 miles outside of Philadelphia increases more relative to the volatility of sales of all taxed beverages.

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable	Price/Oz	Price/Oz	Price/Oz	Ounces Sold	Ounces Sold	Ounces Sold
Time Trend Adjustment	n/a	No	Yes	n/a	No	Yes
Sample	2015-2017 (Pre-Tax Period)	Full Sample	Full Sample	2015-2017 (Pre-Tax Period)	Full Sample	Full Sample
Philadelphia × Time Trend	.00048***			75*		
	(.00015)			(44)		
Philadelphia × After Tax		1.449***	1.399***		-56,192***	-64,118***
		(.022)	(.022)		(9,742)	(9,744)
Store FE	Yes	Yes	Yes	Yes	Yes	Yes
Week FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	85,041	144,700	144,700	85,041	144,700	144,700
Stores	832	832	832	832	832	832
Weeks	104	176	176	104	176	176

Table A9: **Analysis of Differential Pre-Tax Trends.** Columns (1) and (4) analyze only pre-tax data. "Time Trend" is a variable ranging from 0 in the first week of the sample to 103 in the week before the tax is implemented. Columns (3) and (6) adjust the outcome variable for the differential time trend in Philadelphia estimated in Columns (1) and (4) respectively (see text for more details on the adjustment to the outcome variable). Standard errors are clustered at the store and week level and reported in parentheses. *** $p < .01$, ** $p < .05$, * $p < .1$.

I Robustness Check: Parallel Time Trends

Our difference-in-differences approach to estimating the impact of the tax relies on the assumption that the treatment and control groups would follow the same time trend in the absence of treatment. In order to assess the plausibility of the parallel trends assumption, we provide three pieces of analysis, which are reported in Table A9. First, we test whether the outcomes of interest follow similar trends in the pre-tax period. To do this, we restrict our sample to the pre-tax time period only and regress each outcome on a set of store and week fixed effects as well as an interaction of the Philadelphia dummy with a linear time trend (i.e. a variable that runs from 0 in the first week of the sample to 103 in the last week of the pre-tax period). Second, to assess the impact of any differential time trends on our estimates, we assume that the estimated pre-tax trend persists in the post-tax period and obtain de-trended outcomes by subtracting the linear trend from outcomes in Philadelphia stores. We then re-estimate our baseline regression using the de-trended data and compare the results using de-trended

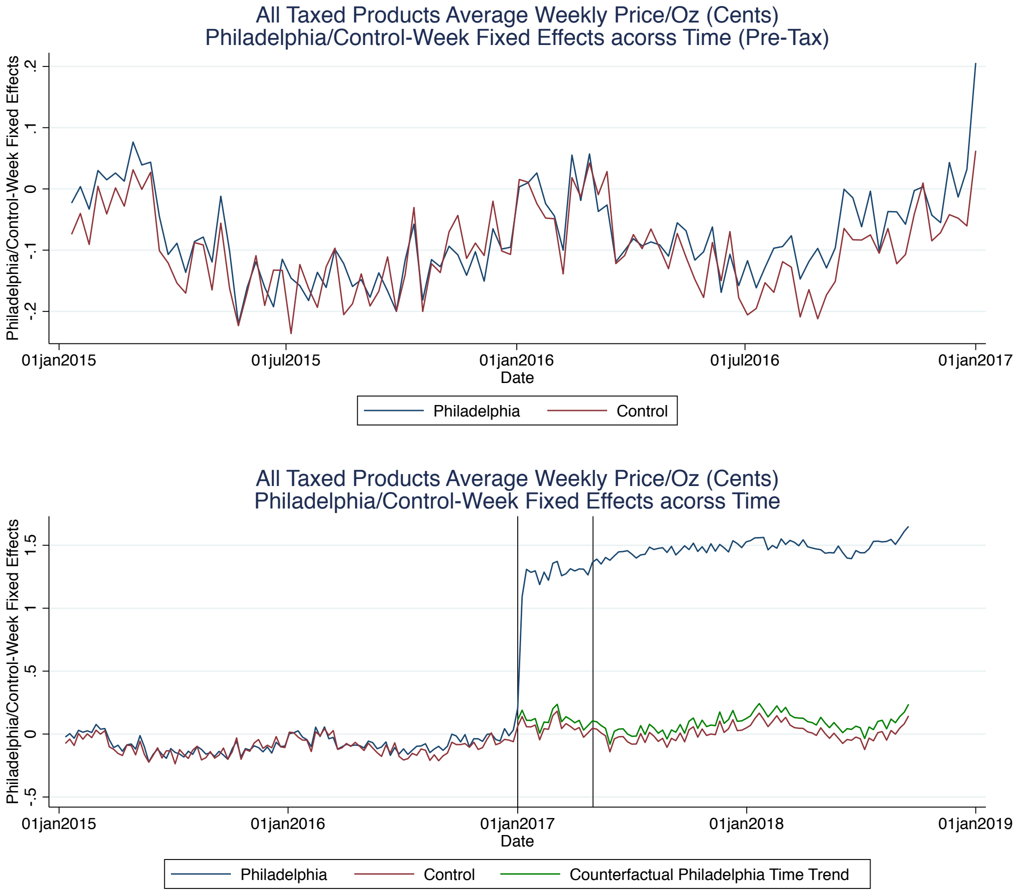


Figure A6: **Differential Pre-Tax Time Trends: Price/Oz.** The black vertical lines indicate the tax's introduction and the end of the four month "adjustment period" that we omit from our main analyses (see the "Estimation and Results" section and Web Appendix G).

data to our main regression results.

Focusing on price/oz first, we find a positive and statistically significant, but economically small differential time-trend in prices in Philadelphia relative to the control group. The point estimate is equal to .00048 and corresponds to a .025 cents/oz ($\approx 52 \times 0.00048$) increase in prices per year. This change is several orders of magnitude smaller than the price increase of 1.45 cents/oz that we estimated as the causal impact of the tax. Next, in order to assess the impact of the time trend on our estimate, we remove the Philadelphia-specific time trend by subtracting the estimated differential trend from prices in all Philadelphia stores. Columns (2) and (3) report estimates from our baseline regression and from a regression where prices in Philadelphia were adjusted for the differential time trend. As expected, given the small magnitude of the time trend, the change in magnitude of the difference-in-difference estimate is small.

Figure A6 further illustrates the influence of the time trend in prices relative to the change in price that is triggered by the tax. In order to isolate the time trends in Philadelphia and the control group, we regress price on store fixed effects and separate sets of week dummies for Philadelphia and the control group. We then plot the coefficients on the Philadelphia weekly dummies and the control group's weekly dummies over time. The top graph reports the estimated time-trends in pre-tax prices in Philadelphia and in the control group, and shows that the two time-series track each other closely. The bottom graph plots the entire time series for treatment and control and shows that the price increase caused by the tax eclipses any pre-tax price movements. The green line overlaid on the plot shows the counterfactual post-tax time trend for Philadelphia stores based on the estimated differential pre-tax time trend (i.e. we add the estimated differential time trend in Philadelphia stores to the estimated weekly dummies for the control group in the post-tax time period). Consistent with the regression analysis discussed above, the counterfactual post-tax time series of prices lies only slightly above the post-tax price series for the control group, and hence the small pre-existing time trend cannot account for the large change in prices caused by the tax.

We repeat the same set of regressions using quantity as the outcome variable as well. In the case of the quantity regression, we do not find a significant pre-tax time trend (see Table A9 Column (4)). The magnitude of the point estimate of the time trend coefficient is again small in magnitude and removing the time trend leads to a small increase (in absolute terms) in the difference-in-difference coefficient in our main regression. We also report graphs that depict the pre-tax and overall time trends for quantities in Figure A7. Similar to the patterns for prices, we find that quantity movements in Philadelphia and the control group track each other closely in the pre-tax period. Compared to prices, quantities show a larger amount of seasonal fluctuations, but seasonality patterns are similar in Philadelphia and outside of the city. The green line shows that the counterfactual post-tax time trend in Philadelphia (based on the estimated pre-tax time trend) lies slightly above above the post-tax time series for the control group.

J Robustness Check: Clustering

In our main specification, we use two-way clustering at the store and week level. As an additional robustness check, we explore the sensitivity of our results to clustering at a higher level of aggregation. Table A10 reports results for our main specifications, when clustering standard errors at the store

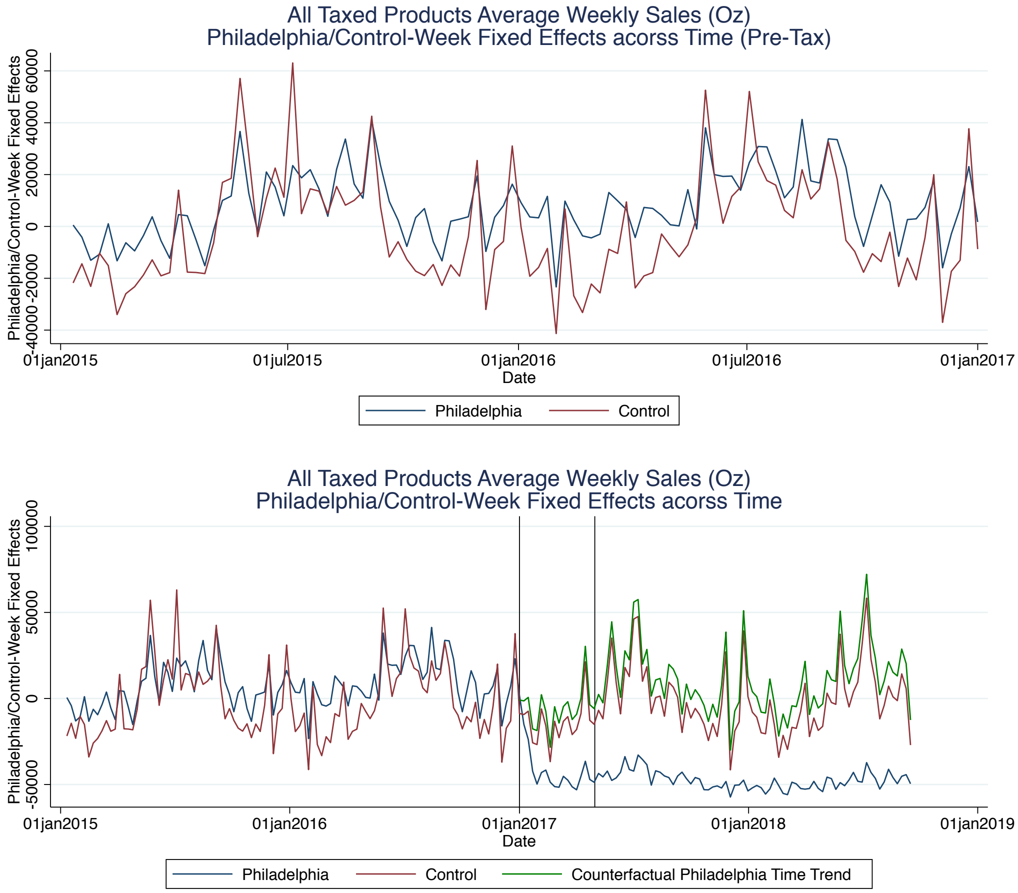


Figure A7: **Differential Pre-Tax Time Trends: Quantity (Ounces).** The black vertical lines indicate the tax's introduction and the end of the four month "adjustment period" that we omit from our main analyses (see the "Estimation and Results" section and Web Appendix G).

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

K Cross-shopping and Basket-level Effects

When engaging in cross-shopping to purchase sweetened beverages, consumers might also start purchasing other products outside of Philadelphia. Thomassen et al. (2017) highlight the importance of

	Taxed Products				*Untaxed Products*		*Cross-shopping*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent Variable	Price/Oz	Price/Oz	Ounces Sold	Ounces Sold	Ounces Sold	Ounces Sold	Ounces Sold	Ounces Sold
Clustering	Store and Week	Store and Month	Store and Week	Store and Month	Store and Week	Store and Month	Store and Week	Store and Month
Philadelphia	1.449***	1.449***	-56,192***	-56,192***	-4,521	-4,521	-56,193***	-56,193***
× After Tax	(.022)	(.024)	(9,742)	(9,871)	(7,125)	(7,080)	(9,742)	(9,868)
0-2 Miles Outside							63,650***	63,650***
City × After Tax							(20,734)	(20,707)
2-4 Miles Outside							18,364***	18,364**
City × After Tax							(7,032)	(7,026)
4-6 Miles Outside							8,640**	8,640**
City × After Tax							(4,198)	(4,172)
Store FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Week FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	144,700	144,700	144,700	144,700	144,209	144,209	213,499	213,499
Stores	832	832	832	832	829	829	1,227	1,227
Weeks	176	176	176	176	176	176	176	176
Months	41	41	41	41	41	41	41	41

Table A10: **Robustness Check: Store/Month Clustering.** Some stores do not offer all categories of beverages and hence the number of observations differs slightly across columns. *** $p < .01$, ** $p < .05$, * $p < .1$.

such basket-level substitution effects.[11] To assess the importance of basket-level substitution effects, we study purchase patterns for milk in Philadelphia, as well as in border stores. We choose this category because milk is one of the most frequently purchased consumer packaged goods categories and not a direct substitute for or complement to sweetened beverages. Because we are not able to obtain data across an exhaustive set of categories in all stores, we treat milk as a stand-in for other items in a consumer's basket. We use the same specification as in our cross-shopping regression (see column (1) of Table 5), but use milk sales as the dependent variable. We start by examining all types of stores in Philadelphia, and find that store-level demand for milk decreases by a small amount of 1,100 ounces per week relative to an average pre-tax level of 58,870 ounces per week. The effect is not statistically significant.[12] In line with this finding, we also find no evidence that milk sales at stores near the city

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

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

border experience a change in demand.[13]

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

L Non-Price Effects of the Tax

Apart from prices, other marketing variables such as advertising and in-store product displays might also adjust in response to the tax, and these marketing variables could have an impact on quantity sales. In terms of advertising, all stores in the treatment and control groups are located in the Philadelphia DMA, and hence the effects we measure are not driven by differences in television ad exposure. Moreover, feature advertising conducted by retailers is typically implemented uniformly across large geographies and hence it is also likely to be the same for treatment and control stores (Seiler and Yao (2017); Blattberg and Neslin (1990)). Therefore, the only marketing activities that might change more locally and could affect treatment and control differently are changes to in-store displays and shelf layouts. We do not have data on display advertising or in-store planograms and hence cannot directly investigate changes in these marketing activities, but in-store displays are typically used less frequently than price promotions and feature advertising and thus are a tool that generally plays a more minor role relative to price changes (Seiler and Yao (2017)). To the extent that in-store displays or product availability do change when the tax goes into effect, we anticipate that the availability of taxed products would decrease in Philadelphia stores relative to control stores. The best source of data on this comes from Cawley et al. (2020), who manually collected data from 65 stores in Philadelphia and found that the probability that a Philadelphia store carried a given taxed product fell by 4 percentage points ($p<.1$). Thus, while unobserved changes in product assortment could lead us to over-estimate the demand elasticity, we believe any such bias would be small.

In addition, the tax was discussed publicly before it was passed and was therefore likely salient

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

to many consumers. We might expect that the announcement of the tax and the discussion surrounding health effects of sweetened beverages might lead consumers to switch to healthier beverages independent of price changes (Taylor et al. (2019)). However, the null effect we find with regard to substitution to healthier beverages speaks against such a salience effect playing a large role, because we would expect salience to trigger switches to healthier beverages, but not cross-shopping for sweetened beverages. We also find no evidence that demand for taxed beverages in Philadelphia decreased in the months prior to January 2017 (see Table A8), when the upcoming introduction of the tax would already have been salient to consumers. For these reasons, we believe that the quantity decreases we observe are primarily a direct response to price changes.

M Additional Tables

Dependent Variable	(1) Price/Oz	(2) Price/Oz	(3) Log Price/Oz	(4) Log Price/Oz	(5) Log Price/Oz	(6) Log Price/Oz
Philadelphia × AfterTax	1.459***					
	(.020)					
Grocery A		1.308***	.344***	.343***	.351***	.342***
× Philadelphia × AfterTax		(.025)	(.010)	(.015)	(.012)	(.010)
Grocery B		1.518***	.414***	.412***	.417***	.411***
× Philadelphia × AfterTax		(.006)	(.002)	(.015)	(.005)	(.004)
Grocery C		1.838***	.466***	.465***	.469***	.461***
× Philadelphia × AfterTax		(.055)	(.015)	(.018)	(.017)	(.017)
Mass Merchant M		1.441***	.318***	.317***	.323***	.314***
× Philadelphia × AfterTax		(.270)	(.061)	(.066)	(.061)	(.060)
Mass Merchant N		1.084***	.303***	.302***	.308***	.299***
× Philadelphia × AfterTax		(.029)	(.008)	(.013)	(.009)	(.009)
Drugstore X		1.536***	.300***	.299***	.306***	.297***
× Philadelphia × AfterTax		(.039)	(.012)	(.016)	(.014)	(.013)
Drugstore Y		1.321***	.250***	.249***	.254***	.245***
× Philadelphia × AfterTax		(.020)	(.007)	(.011)	(.009)	(.009)
Drugstore Z		.935***	.179***	.177***	.183***	.174***
× Philadelphia × AfterTax		(.068)	(.008)	(.015)	(.010)	(.010)
Wholesale Club		1.411***	.439***	.437***	.442***	.436***
× Philadelphia × AfterTax		(.073)	(.009)	(.018)	(.010)	(.009)
Dollar Stores		1.456***	.360***	.358***	.362***	.353***
× Philadelphia × AfterTax		(.042)	(.012)	(.017)	(.012)	(.016)
Convenience Stores		1.602***	.197***	.196***	.202***	.193***
× Philadelphia × AfterTax		(.025)	(.005)	(.012)	(.007)	(.007)
Distance (in Miles) to Nearest Untaxed Store				.001		
× Philadelphia × AfterTax				(.005)		
Income					-.013	
× Philadelphia × AfterTax					(.012)	
Obesity Rate						.010
× Philadelphia × AfterTax						(.014)
$(AfterTax_t \times \mathbf{X}_s')$ Interactions	n/a	Yes	Yes	Yes	Yes	Yes
Store FE	Yes	Yes	Yes	Yes	Yes	Yes
Week FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	144,700	144,700	144,700	144,700	144,700	144,700
Stores	832	832	832	832	832	832
Weeks	176	176	176	176	176	176

Table A11: **Impact on Prices / Pass-through Rate Estimates for the Soda Category.** Interactions with an after-tax dummy (the $(AfterTax_t \times \mathbf{X}_s')$ term) are included in columns (2) - (6), but not reported separately. One exception is the obesity variable in column (6). We have no obesity data outside of Philadelphia, and hence no $(Obesity_s \times AfterTax_t)$ term is included. Standard errors are clustered at the store and week level and reported in parentheses. *** p < .01, ** p < .05, * p < .1.

Dependent Variable	(1) Ounces Sold	(2) Ounces Sold	(3) Log Ounces	(4) Log Ounces	(5) Log Ounces	(6) Log Ounces
Philadelphia × AfterTax	-18,713***					
	(3,910)					
Grocery A		-103,199***	-.810***	-.904***	-.671***	-.826***
× Philadelphia × AfterTax		(18,693)	(.080)	(.093)	(.091)	(.083)
Grocery B		-163,577***	-.891***	-1.021***	-.787***	-.909***
× Philadelphia × AfterTax		(5,472)	(.014)	(.059)	(.032)	(.020)
Grocery C		-237,319***	-1.024***	-1.109***	-.928***	-1.057***
× Philadelphia × AfterTax		(42,436)	(.087)	(.088)	(.088)	(.093)
Mass Merchant M		-33,289	-.554**	-.632**	-.427*	-.578***
× Philadelphia × AfterTax		(20,205)	(.222)	(.254)	(.225)	(.211)
Mass Merchant N		-130,308***	-.492***	-.579***	-.378***	-.515***
× Philadelphia × AfterTax		(26,802)	(.104)	(.113)	(.105)	(.106)
Drugstore X		-2,692***	-.229***	-.318***	-.098*	-.250***
× Philadelphia × AfterTax		(781)	(.047)	(.063)	(.055)	(.052)
Drugstore Y		-427***	.004	-.084	.110**	-.026
× Philadelphia × AfterTax		(145)	(.038)	(.054)	(.048)	(.045)
Drugstore Z		16,053***	.701***	.590***	.806***	.676***
× Philadelphia × AfterTax		(3,048)	(.104)	(.110)	(.101)	(.106)
Wholesale Club		-73,161***	-.802***	-.940***	-.679***	-.821***
× Philadelphia × AfterTax		(6,732)	(.065)	(.091)	(.083)	(.067)
Dollar Stores		-8,085***	-.416***	-.513***	-.338***	-.453***
× Philadelphia × AfterTax		(1,349)	(.039)	(.057)	(.044)	(.049)
Convenience Stores		385	-.024	-.121**	.094**	-.048*
× Philadelphia × AfterTax		(322)	(.021)	(.048)	(.039)	(.027)
Distance (in Miles) to Nearest Untaxed Store				.044**		
× Philadelphia × AfterTax				(.020)		
Income					-.202***	
× Philadelphia × AfterTax					(.053)	
Obesity Rate						.062
× Philadelphia × AfterTax						(.052)
($AfterTax_t \times \mathbf{X}'_s$) Interactions	n/a	Yes	Yes	Yes	Yes	Yes
Store FE	Yes	Yes	Yes	Yes	Yes	Yes
Week FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	144,700	144,700	144,700	144,700	144,700	144,700
Stores	832	832	832	832	832	832
Weeks	176	176	176	176	176	176

Table A12: **Impact on Quantity Sold for the Soda Category.** Interactions with an after-tax dummy (the ($AfterTax_t \times \mathbf{X}'_s$) term) are included in columns (2) - (6), but not reported separately. One exception is the obesity variable in column (6). We have no obesity data outside of Philadelphia, and hence no ($Obesity_s \times AfterTax_t$) term is included. Standard errors are clustered at the store and week level and reported in parentheses. *** p < .01, ** p < .05, * p < .1.

	All Beverages	*Low Sugar Taxed Beverages*	*High Sugar Taxed Beverages*
	(1)	(2)	(3)
Dependent Variable	Gram of Sugar	Ounces Sold	Ounces Sold
Average Pre-Tax Quantities / Grams of Sugar	342,807	54,290	69,832
Philadelphia × After Tax	-132,129***	-26,132***	-30,164***
	(24,318)	(4,409)	(5,741)
0-2 Miles Outside	166,074***	20,613***	43,207***
City Border × After Tax	(53,315)	(7,741)	(13,773)
2-4 Miles Outside	50,600***	5,633*	12,766***
City Border × After Tax	(18,030)	(2,954)	(4,220)
4-6 Miles Outside	24,572**	2,751	5,866**
City Border × After Tax	(11,091)	(1,851)	(2,501)
Store FE	Yes	Yes	Yes
Week FE	Yes	Yes	Yes
Change in Aggregate Quantity	-18,821*	-5,946***	-3,527
(Unit: 1,000 Ounces / Grams of Sugar)	(10,982)	(1,907)	(2,635)
Change in % of Pre-tax Volume in	-.154*	-.311***	-.143
Philadelphia **w/ Cross-Shopping**	(.090)	(.100)	(.107)
Change in % of Pre-tax Volume in	-.386***	-.487***	-.438***
Philadelphia **w/o Cross-Shopping**	(.071)	(.082)	(.083)
Observations	213,499	212,871	213,499
Stores	1,227	1,223	1,227
Weeks	176	176	176

Table A13: **Impact on Nutritional Intake: Sugar.** High-sugar-content beverages are defined as products with ≥2.4 grams/oz (the median value for sugar content across all taxed products). Standard errors are clustered at the store and week level and reported in parentheses. *** $p < .01$, ** $p < .05$, * $p < .1$.

Dependent Variable	(1) Ounces Sold	(2) Ounces Sold	(3) Ounces Sold	(4) Ounces Sold
Philadelphia * After Tax	-56,193***	-56,193***	-56,194***	-56,194***
	(9,742)	(9,742)	(9,742)	(9,742)
0-1 Miles Outside City Border			182,184**	-176,298
× After Tax			(71,962)	(143,948)
0-1 Miles Outside City Border				1,157,703**
× After Tax * Income-in-Philadelphia				(576,934)
1-2 Miles Outside City Border			22,420***	16,258
× After Tax			(8,257)	(14,781)
1-2 Miles Outside City Border				13,747
× After Tax * Income-in-Philadelphia				(27,738)
0-2 Miles Outside City Border	63,650***	35,957		
× After Tax	(20,734)	(32,678)		
0-2 Miles Outside City Border		67,140		
× After Tax * Income-in-Philadelphia		(75,752)		
2-4 Miles Outside City Border	18,364***	12,126	18,364***	12,124
× After Tax	(7,032)	(10,415)	(7,032)	(10,415)
2-4 Miles Outside City Border		13,049		13,050
× After Tax * Income-in-Philadelphia		(17,325)		(17,325)
4-6 Miles Outside City Border	8,640**	8,640**	8,640**	8,640**
× After Tax	(4,198)	(4,198)	(4,198)	(4,198)
Store FE	Yes	Yes	Yes	Yes
Week FE	Yes	Yes	Yes	Yes
Observations	213,499	213,499	213,499	213,499
Stores	1,227	1,227	1,227	1,227
Weeks	176	176	176	176

Table A14: **Cross-Shopping as a Function of Income in Nearby Philadelphia Census Tracts.** Column (1) replicates column (1) in Table 5. Income-in-Philadelphia is computed by taking the average of the median income level in all census tracts in Philadelphia that are within a 4 mile radius around a store (outside of the city). The median income level is re-scaled such that it varies between 0 and 1 across all census tracts in Philadelphia. The standard deviation of the Income-in-Philadelphia variables is equal to .15, .20, and .30 for stores within 0-1 miles, 1-2 miles, and 2-4 miles outside of Philadelphia respectively. For the 0-1 miles distance band, the income variable varies between .20 and .66. Standard errors are clustered at the store and week level and reported in parentheses. *** $p < .01$, ** $p < .05$, * $p < .1$.

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