The Impact of Sugar-Sweetened Beverage Taxes on Purchases: Evidence from Four City-Level Taxes in the U.S.*

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Abstract

Since 2017, many U.S. cities have implemented taxes on sugar-sweetened beverages (SSBs) to decrease consumption of sugary beverages and raise revenue. In this paper, we analyze household receipt data to examine the impact of SSB taxes on households' purchases of taxed and untaxed beverages in the four largest U.S. cities with such taxes: Philadelphia, Pennsylvania; San Francisco, California; Seattle, Washington; and Oakland, California. We estimate the impact of these taxes by comparing changes in monthly household purchases in the treatment cities to changes in one of two comparison groups: 1) areas adjacent to the treatment cities; or 2) a matched set of households nationally. We find that an increase in the beverage tax rate of 1 cent per ounce decreases household purchases of taxed beverages by 53.0 ounces per month or 12.2 percent. This impact is small in magnitude and consistent with a reduction in individual consumption of 5 calories per day per household member and eventual reduction in weight of 0.5 pounds. When we examine results separately by city, we find that the decline was concentrated in Philadelphia, where the tax decreased purchases by 27.7 percent. We do not find impacts of the taxes in the other three cities combined.

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I. Introduction

Since 2015, cities across the U.S. have enacted taxes on sugar-sweetened beverages (SSBs).[1] The introduction of these local taxes reflects a broader global trend of countries enacting nationwide SSB taxes, motivated by the association of SSB consumption with obesity, diabetes, and their related externalities (Cawley et al., 2019a).

A growing literature examines the impact of these beverage taxes. The research on national taxes generally concludes that these taxes are fully passed through, or even overshifted, to retail prices and that they result in large reductions in purchases. However, these studies of national taxes lack a geographic control group and are in some cases unable to separate the effect of the SSB tax from other similarly intentioned policies enacted at the same time (Cawley et al., 2019).

The literature on U.S. taxes does use geographic comparison areas, typically a nearby city or the areas surrounding the taxing city. This literature finds pass-through rates of SSB taxes that vary from less than 50 percent in Berkeley, California (Falbe et al., 2015; Cawley and Frisvold, 2017; Rojas and Wang, 2017; Bollinger and Sexton, 2018) to full pass-through in Philadelphia, Pennsylvania (Cawley et al., 2019a; Seiler et al., 2019; Roberto et al., 2019).

Several studies have examined the impact of such taxes on purchases in individual cities. For Philadelphia, Cawley et al., (2019b), Seiler et al. (2019) and Roberto et al. (2019) find that the tax lead to reductions in purchases of taxed beverages within Philadelphia but increases in purchases of those beverages outside of Philadelphia. For Berkeley, Rojas and Wang (2017) and Bollinger and Sexton (2018) find limited evidence that the tax reduced the sales volume of SSBs in national chain retailers. For Oakland, Cawley et al. (2019c) find that the tax resulted in a slight decrease in the volume of SSBs purchased in Oakland.

In this paper, we use unique household receipt data to estimate the impact of sweetened beverage taxes on household purchases of taxed and untaxed beverages in the four largest cities with such taxes: Philadelphia, Pennsylvania; Oakland, California, San Francisco, California; and Seattle, Washington.[2] The household receipt data are collected by InfoScout, which is a

[1] Berkeley, CA, was the first to impose a tax on SSBs, in 2015. In 2017, Albany, CA; Oakland, CA; Philadelphia, PA; and Boulder, CO introduced taxes. In 2018, San Francisco, CA, and Seattle, WA introduced taxes. Cook County, IL, briefly imposed a tax on SSBs in 2017, but it was repealed after a few months.

[2] Philadelphia implemented a tax of 1.5 cents per ounce on SSBs and non-caloric sweetened beverages on January 1, 2017. Oakland implemented a tax of 1 cent per ounce on SSBs on July 1, 2017. San Francisco and Seattle implemented a tax on SSBs on January 1, 2018 of 1 cent per ounce and 1.75 cents per ounce, respectively.

marketing panel similar to the Nielsen consumer panel. We acquired complete records for all panelists in the four cities. One benefit of the InfoScout consumer panel is that the number of panelists in these cities is approximately twice as large as that in the Nielsen consumer panel, which gives us more statistical power to detect effects of these taxes and allows us to measure them with greater precision. The data include 12 months of household purchases of all beverages, covering 6 months before and 6 months after the implementation of each tax. We estimate the impact of these taxes by comparing changes in household monthly purchases in the treatment cities (that is, cities implementing beverage taxes) to changes in two comparison groups: 1) households in the surrounding metropolitan statistical areas (MSA) but outside of the treatment cities; and 2) a matched set of urban households throughout the nation. We examine the trends in beverage purchases prior to the implementation of the taxes to assess the appropriateness and suitability of the two comparison groups.

By taking this approach, we overcome four limitations of the previous literature. First, all prior papers focus on a tax in a single city in the U.S. or a national tax.[3] This is the first paper to study multiple cities' beverage taxes with a single data source and consistent methods. Examining multiple cities provides a richer and more complete understanding of how the taxes affect purchases across a range of settings. Including multiple treatment cities and comparison areas also improves the precision of the estimates by adding additional clusters to the analysis. Second, we examine household data and observe beverages purchased at all retail locations. In contrast, the previous literature that uses secondary data on sales observes only the large, national chains in the Nielsen retail scanner data or sales at a limited set of large stores or chains that provided data to the researchers. By using unique data with the full range of locations of household purchases and a larger sample size that is well-suited to examining city-level taxes in the US, we are able provide a more complete understanding of the impact of these taxes on households' purchases. Third, we test the sensitivity of the results to the use of alternate comparison groups. Much of the previous literature uses either areas adjacent to the taxing city or a nearby city; we are also able to examine a matched sample of households nationwide that resemble the households in the taxing cities. With six months of purchase data prior to the

[3] Rojas and Wang (2017) examine the SSB tax in Berkeley and the state-level tax in Washington on carbonated beverages separately. Fletcher et al. (2010a, 2010b, 2015) examine multiple state-level taxes on soda, but their analysis pre-dates the larger SSB taxes in the U.S.

implementation of the taxes, we are able to assess the validity of the parallel trends assumption for each comparison group. Fourth, we are able to cluster the standard errors at the geographic level and compute confidence intervals using the wild-cluster bootstrap method (Cameron, Gelbach, and Miller, 2008). A consistent limitation of the prior literature is that the standard errors could not be clustered at the geographic level due to the samples consisting of two geographic clusters or less. With a sample consisting of households in the four largest cities with beverage taxes in the U.S. and their multiple comparison groups, we are able to examine the sensitivity of the results to clustering the standard errors at the geographic level.

We find that, on average across the four taxing cities, the SSB taxes reduced household purchases of taxed beverages by 53 ounces per month or 12 percent. The reduction in household purchases is concentrated in Philadelphia, which has a relatively high tax rate (1.5 cents per ounces), is the only city to tax non-caloric sweetened beverages (such as diet soft drinks), and is the city with the highest pre-tax purchases and consumption. We do not find an impact of the taxes on beverage purchases in the other three cities combined. In addition, the estimates based on each of the two comparison groups indicate that the reduction in purchases is larger when using the national comparison group, rather than the surrounding MSA. This result is consistent with the taxes having spillover effects to neighboring communities, which could be due to suburban residents occasionally shopping in the city (and being exposed to the tax) or from information about SSBs being disseminated throughout the media market during the implementation of the tax.

II. Methods

A. Overview

We estimate the impact of city-level beverage taxes on households' monthly beverage purchases using a difference-in-differences (DiD) approach, in which we compare the changes in purchases by households in the cities with taxes to those by households in areas without taxes. Specifically, we estimate the following equation:

$$Y_{hct} = \alpha_0 + \alpha_1 Tax_{ct} + \delta_h + \gamma_t + \varepsilon_{hct}, \tag{1}$$

where Y_{hct} is the monthly beverage purchases (in ounces) of household h in city c in month t. We estimate the impact of the taxes on the purchases of all taxed beverages and all untaxed beverages separately. *Tax* is the city-specific tax rate in cents per ounce, which is equal to 1 for San Francisco and Oakland, 1.5 for Philadelphia, and 1.75 for Seattle after each tax takes effect and zero beforehand. It equals zero in every month for households in the comparison groups. δ_h is a vector of household fixed effects, γ_t is a vector of month fixed effects, and ε is an error term. α_1 is the coefficient of interest, which represents the change in ounces of beverages purchased per household per month, after the tax relative to before the tax, for households in the taxing cities relative to those in the comparison areas. In addition to estimating the impact of the taxes on purchases, we also estimate the impacts on the number of shopping trips with beverage purchases per month per household, the number of beverage purchases (taxed and untaxed separately) per month per household, and whether households had any purchases in the month (taxed and untaxed separately).[4] We estimate the equation above using ordinary least squares (OLS).

We examine the four largest cities with a beverage tax: Philadelphia, Oakland, San Francisco, and Seattle. We examine the largest cities with a tax because they may be of the greatest interest and because the largest cities tend to have the most households in the InfoScout panel, providing us with the greatest possible statistical power. Information about each of these city taxes is provided in Table 1. All four cities tax SSBs, which includes regular soda, sports drinks, energy drinks, sweetened iced tea, and juice drinks. Philadelphia is unique in that it also taxes non-caloric sweetened beverages, including diet soda, diet sports drinks, sugar-free energy drinks, and diet iced teas. As a result, the outcome variable measuring purchases of taxed beverages includes SSBs for all cities and, for Philadelphia, includes non-caloric sweetened beverages as well.

We estimate the pooled impact across all four cities. We additionally estimate the impact separately for Philadelphia and for the other three cities combined because Philadelphia also taxes non-calorie sweetened beverages, unlike the other three cities.[5]

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[4] For the number of beverages purchased, multi-packs (such as 12-packs of 12-ounce cans) that come in a single package are considered one beverage.

[5] Because the elasticity of demand may vary by household characteristics (such as race, ethnicity, and income), we estimate impacts of the tax separately based on income, household size, marital status, the age of the primary shopper, the race/ethnicity of the primary shopper, and the educational attainment of the primary shopper. These results are shown in the appendix.

B. Selection of Comparison Groups

We estimate the impact of the SSB taxes alternately using one of two geographic comparison groups, each with its own strengths and limitations. The first comparison group consists of households in the same state and MSA as the taxing city, but outside of the taxing city itself. The primary advantage of using such households as a comparison is that they experience the same state policies, are in the same media market (and thus experience similar advertising and information), and have similar local economic conditions. Thus, the estimates are likely to reflect the impact of the tax as opposed to local economic shocks or the impact of public messaging (such as media attention and public health campaigns) surrounding the tax. A potential disadvantage of using such households as a comparison is that there may be spillover effects of the tax to nearby areas. For example, households that live outside the taxing city might occasionally shop in the taxing city and thus be exposed to the tax at times, which could bias downward the estimated impact of the tax.

The second comparison group is a matched set of households nationally. At our request, InfoScout matched households in urban areas without SSB taxes to the households in the treatment group (cities with taxes) based on income, race/ethnicity, household size, and education.[6] The advantage of this national comparison group is that households in these distant communities will not be affected by the taxes in the treatment cities. A potential disadvantage, however, is that these more distant households may not experience the same local shocks to purchases that are experienced by the households in the treatment group. Such shocks could be due to local economic fluctuations, entry and exit of grocery chains, differing media coverage, or changes in advertising. Each of the two comparison groups has its strengths and weaknesses. By examining the sensitivity of the results to the choice of comparison group, we gain a richer understanding of the robustness of our results and the extent to which results in the past literature may have been influenced by their choice of comparison group.

For each comparison group, valid inference depends on whether the household purchases in the comparison households represent a reasonable counterfactual outcome for the purchases that households in the treatment cities would have made in the absence of the SSB taxes. Thus,

[6] The urban areas from which the households are drawn include cities such as New York City, Detroit, Charlotte, Houston, San Jose, San Diego, and Los Angeles.

an important assumption of the DiD strategy is parallel trends in the outcome for the treatment and comparison group. A strength of our panel data is that it allows us to examine whether trends in beverage purchases prior to the taxes were indeed parallel between the treated households and our two comparison groups. To this end, we examine the level and trends in purchases per household in the six months prior to each tax in the cities with taxes and the comparison communities. Additionally, we estimate event study regressions that show the (conditional) differences in purchases in households in the treatment and comparison groups for each month before and after implementation of the taxes.

C. *Standard Errors*

Initially, we cluster standard errors at the household level to account for correlations over time in the purchases of each household. However, error terms may also be correlated *across* households within a city. A consistent limitation of the previous literature is the inability to adjust the standard errors to account for correlations between observations within geographic areas. In the previous literature, all studies of national taxes lack a geographic control group and thus examine only a single cluster (the taxing nation), so it is not possible to cluster the standard errors by area. Past studies of city-level taxes in the U.S. typically examine one taxing city and one comparison area; these studies are also not able to cluster standard errors by area because standard errors with only two clusters are degenerate (Donald and Lang, 2007). The lack of clustering may result in standard errors that are biased downward; i.e., the papers may overstate the true precision of their estimates (Cameron and Miller, 2015).

By estimating regressions using data from four treated cities, each with two comparison groups (for a total of twelve clusters), we are able to overcome this limitation and cluster the standard errors based on geography to account for any correlation in errors among households in the same city or area. However, with 12 clusters, hypothesis tests based on cluster-robust standard errors can over-reject the null hypothesis. Thus, we estimate confidence intervals using the wild-cluster bootstrap (Cameron, Gelbach, and Miller, 2008; Cameron and Miller, 2015). Next, we take an even more cautious step of allowing for the possibility that observations within a treatment city and its two comparison groups are not independent, and we estimate confidence intervals using the wild-cluster bootstrap with four geographic clusters, one for each treatment city.

III. Data

To estimate the impact of SSB taxes on beverage purchases, we use unique data on households' beverage purchases collected by InfoScout (now part of Numerator), a marketing intelligence firm that collects consumer panel data.[7] InfoScout maintains a panel of roughly 400,000 households nationwide, who purchase items from roughly 44,000 retailers across all categories of stores (including online purchases). The primary shopper for each household records all the households' purchases by uploading photos of their receipts to InfoScout. InfoScout uses the receipts to create a database that includes a record for each item purchased, including a description of the item, the quantity purchased, price, household characteristics, and household and shopping trip identifiers.

An important advantage of the InfoScout data, relative to the Nielsen consumer panel, for our purposes is that it includes a larger number of households in cities with SSB taxes, which allows us to estimate the effect of the taxes with greater power and precision. An important advantage of the InfoScout data, relative to retail scanner data, is that it includes purchases by households from all stores, instead of sales volume for a selection of large chain retailers.

We acquired records for all beverage purchases for all households with children included the InfoScout panel database that live in four cities with sweetened beverage taxes: Oakland, Philadelphia, San Francisco, and Seattle. We restrict the sample to households with children because children are at an elevated risk of the health effects from consuming sugary beverages. We also acquired records for all beverage purchases for comparison households, which are: (1) InfoScout's household panelists with children living in the same state within the MSAs of the four cities with taxes but outside of the cities' borders, and (2) household panelists with children in other cities across the country that match the households in the treatment cities based on income, race/ethnicity, household size, and education.

The records for treatment and comparison households span the six months prior to and the six months after implementation of the taxes. For example, the tax was implemented in

[7] See https://www.numerator.com/infoscout-omnipanel for more information on InfoScout. Numerator states that the panel is nationally representative. In Appendix Table 1, we compare the demographic characteristics of households and the primary shopper in our InfoScout sample of households with children in specific geographic locations to households with children in the American Community Survey in the same geographic regions and in all urban areas.

Philadelphia on January 1, 2017; thus, the records for Philadelphia and its two comparison areas include all purchases from July 1, 2016 to June 30, 2017. We sum the ounces of beverages purchased for each household for each month. If a household did not purchase a beverage of a given type in a month, the value for the household/month record is zero. Thus, we have a balanced panel of twelve months of observations for every household.

Table 2 lists the number of households in our sample in each city with a tax, the number of households in each of our comparison groups, and the period of the data for each city. Overall, we have data on the purchases of 1,447 households, which includes 483 households in the cities with taxes: 277 in Philadelphia, 34 in Oakland, 123 in San Francisco, and 49 in Seattle. The data also include 480 households in the comparison areas in the same MSAs (274 in Philadelphia, 34 in Oakland, 123 in San Francisco, and 49 in Seattle) plus 484 matched urban households nationally (278 for Philadelphia, 34 for Oakland, 123 for San Francisco, and 49 for Seattle).

For both the neighboring and national comparison groups, households were matched to those in the taxing cities based on race/ethnicity, income, household size, and education.[8] Because of the matching process, the characteristics of the panelists in the treatment cities are close to those of the panelists in both of the comparison groups, as shown in Appendix Table 2. The primary shoppers in comparison households in the matched urban comparison group are less likely to be married and more likely to have never been married than the primary shoppers in households in the treatment cities, but no other differences in the characteristics are statistically significant. The primary shoppers in comparison households in the same MSA are older, more likely to be white, more likely to be married or previously married, and less likely to be living with a partner or never married than the primary shoppers in households in the treatment cities, but are comparable on income and household size. The similarities in the demographic characteristics, particularly between the households in the treatment cities and matched urban

[8] For Oakland and San Francisco, the sample for the MSA excluded households in Berkeley and Albany because both of these cities also taxed SSBs. For Oakland, households in the MSA exactly matched households in the city on race/ethnicity, the lower two income categories, and the percentage of the sample with less than a high school degree. For San Francisco, almost all households in the MSA exactly matched households in the city on race/ethnicity, income, household size, and education. For Seattle, households in the MSA exactly matched households in the city on race/ethnicity and the lower two income categories. For Philadelphia, the number of households in the MSA was three fewer than the number of households in the city because of the limited number of black and Hispanic households in the MSA in the consumer panel.

households, are consistent with the notion that the households in the comparison samples provide a reasonable counterfactual estimate.

Additional comparisons of the treated and comparison areas are provided in Table 2. Prior to the implementation of the taxes, households in the treatment cities purchased a greater amount (in ounces) of taxed beverages than either comparison group.[9] Specifically, households in the treatment cities purchased 6.32 taxed beverages totaling 468.05 ounces per month over the six months leading up to the tax. Households in the MSAs purchased 5.98 taxed beverages per month, totaling 429.64 ounces, and households in the matched urban sample purchased 6.16 taxed beverages per month, totaling 436.18 ounces. Thus, the *levels* of beverage purchases are slightly higher in the treatment cities similar prior to the taxes.

We also examine the *trends* in beverage purchases prior to the taxes, which relates to the identifying assumption of the DiD design, as described previously. Figure 1 displays the trends in average monthly purchases of taxed (top panel) and untaxed (bottom panel) beverages for the six months before and six months after the implementation of the taxes. The trend in purchases of taxed beverages is generally similar for the treatment and comparison groups prior to the tax. The *level* of purchases is slightly higher in the treatment cities in Figure 1, but the identifying assumption concerns the *trends* in purchases, and those are similar.

Figure 2 presents results from an event study; in none of the months prior to the tax is purchases for a control group significantly different from the purchases of the treatment group. Figure 3 shows the trends in purchases of taxed beverages in Philadelphia and its comparison groups (top panel) and in Oakland, San Francisco, and Seattle combined and their comparison groups (bottom panel). In both cases, the trends in purchases for the treatment and comparison communities are similar prior to the taxes. These results are consistent with the DiD assumption that the comparison households represent a suitable counterfactual for the treated households.

[9] The InfoScout data include the information on receipts and do not identify whether a beverage is taxed. Additionally, all records do not include the volume of the purchased beverage. We describe the method used to identify whether the beverage is taxed and the volume of beverages purchased in the appendix. We used natural language processing to attempt to identify this information from the item description on the receipt and a random forest model to impute missing records. The measurement error inherent in imputation would not bias the estimates if the errors do not systematically differ between households in the treatment and comparison groups before the tax relative to after the implementation of the tax. If households in the treatment cities reduce their purchases of taxed beverages after the tax, and the true values of observations are greater than the imputed values in the pre-tax period and less than the imputed values in the post-tax period, then the estimates reported in this paper would be underestimates of the impacts of the taxes.

IV. Results

A. Impacts on Beverage Purchases

Table 3 reports estimates of equation (1). To illustrate the effect of a beverage tax, we use the example of a one-cent-per-ounce tax, which is the most common rate (used by San Francisco and Oakland) in our sample. Based on all observations, including households in all four taxing cities and all eight comparison areas (Table 3, column 1), an increase in the beverage tax rate of 1 cent per ounce decreased household purchases of taxed beverages by 53.0 ounces per month, which represents a 12.2 percent decrease relative to the pre-tax mean of the comparison group households. When clustering standard errors at the household level, the 95 percent confidence interval of the decrease in purchases ranges from -86.04 to -19.97 ounces, and the point estimate is statistically significant at the 5 percent level.

As discussed above, clustering at the household level adjusts for potential correlation in error terms within households over time but does not adjust for correlation in error terms across households within geographic areas; as a result, the standard errors could be biased towards zero. We next cluster our standard errors using the 12 geographic areas (four treatment cities and the two comparison groups for each city) and estimate the confidence intervals using the wild-cluster bootstrap because of the limited number of clusters (Cameron, Gelbach, and Miller, 2008; Cameron and Miller, 2015). As expected, clustering by geographic area increases the standard errors and widens the confidence intervals. The estimate is no longer statistically significant at the 5 percent level, and the 95 percent confidence interval ranges from a decrease in purchases of 93.35 ounces to an increase in purchases of 41.74 ounces. The lower bound of the confidence interval is similar to that based on clustering at the household level, and we can rule out decreases of more than 93.35 ounces per month, or 22 percent of the pre-tax comparison group mean, from a tax increase of 1 cent per ounce. When we cluster the standard errors by the four cities, accounting for the potential lack of independence of observations across the treatment city and their two comparison groups, the wild-cluster bootstrapped confidence interval narrows, ranging from -84.76 to 17.67 ounces.

Table 3 also reports the estimates for untaxed beverages. The tax on SSBs may affect the demand for untaxed beverages if consumers switch away from the taxed drinks and towards those that are untaxed. The point estimate suggests that the tax led to a reduction of 28.1 ounces in monthly purchases of untaxed beverages, but this estimate is not statistically significant, either when clustering at the household level or when clustering by geographic area.[10] Thus, the results do not provide evidence that the SSB taxes lead consumers to switch to untaxed beverages.

The trends in Figure 1 and the event study results in Figure 2 provide more granular information about the timing of the impact of the taxes. As shown in Figure 1, after the implementation of the taxes, purchases of taxed beverages fell by more than 75 ounces per household in the treatment cities in the first month and by more than an additional 50 ounces per household in the second month but rebounds somewhat thereafter. In contrast to the treatment group, purchases among households in the national comparison group remained stable after the implementation of the taxes, while those in the nearby comparison group also decreased in the two months following the taxes (but by much less than the decreases among treated households), which could reflect a spillover effect of the taxes to households in the nearby communities.[11]

The lower panel of Figure 1 shows the trends in purchases of untaxed beverages. For all three groups (treated and the two comparison groups), sales of untaxed beverages are largely unchanged after the tax, relative to the month before the tax.

The event study graph, Figure 2, displays the change in monthly purchases of taxed beverages relative to the month prior to the implementation of the taxes. The first panel reports the results for the two comparison groups combined, and the next two panels report the results for each comparison group separately. Focusing on the top panel, which uses both comparison groups, purchases in each month post-tax (1 through 6) are lower than the month prior to implementation; two of the decreases are statistically significant at the five percent level and an additional two are statistically significant at the 10 percent level. The largest relative decline in purchases occurred in the second month after implementation (a decrease in purchases of 90 ounces relative to the month before implementation). The results are consistent when using only one of the two comparison groups; although, when using only the matched national comparison

[10] The results are also negative and generally not statistically significant for the ounces of untaxed beverages purchased in Philadelphia alone and the other three cities combined (Appendix Table 4) and the number of untaxed beverages or whether the household purchased any untaxed beverages (Appendix Table 5). The estimates are statistically significant for Philadelphia when using both comparison groups and clustering at the geographic level, but we caution that the purchases of untaxed beverages in Philadelphia are decreasing relative to those in the comparison areas prior to the tax (Appendix Figure 1).

[11] The patterns shown in these figures by month are consistent with the differences in pre- and post-tax means (for all cities combined; Philadelphia; and Oakland, San Francisco, and Seattle combined) shown in Appendix Tables 3a, 3b, and 3c.

group, the declines in purchases of taxed beverages are larger and the differences are more precisely estimated.

B. Differences by Comparison Group

Columns 2 and 3 of Table 3 present results from equation (1) when the comparison group comprises matched households in the MSA (column 2) or matched urban households nationwide (column 3). A one cent per ounce increase in the beverage tax rate reduces monthly household purchases by 48.7 ounces (11.3 percent) when using matched households in the same MSA as the comparison group and by 61.7 ounces (14.1 percent) when using matched households from urban areas throughout the country as the comparison group. Both estimates are statistically significant at the five percent level when clustering the standard errors at the household level but not when clustering at the area and city levels. The difference in point estimates found when using each of the comparison groups separately is not statistically significant.

The lower panel of Table 3 likewise presents results for untaxed beverages when we use only one of the two comparison groups. The estimates are very consistent when using either comparison group or both together; in each case, the point estimate is between -27.6 ounces/month and 29.0 ounces/month, and is mostly not statistically significant. This is consistent with the lower panel of Figure 1, which showed with unconditional data that the purchases of untaxed beverages did not change after the tax in either the treatment or comparison households.

C. Impact on Related Shopping Outcomes

We also estimate the impact of the taxes on the number of monthly shopping trips to purchase beverages, the number of taxed beverages purchased per month, and the percentage of households that purchased any taxed beverage in a month. The estimated impacts of the taxes on these outcomes, shown in Table 4, are fairly small. The number of shopping trips to purchase beverages declined by 0.43 per month or 6.1 percent. The number of taxed beverages purchased per month declined by 0.55 (9.1 percent), and the percentage of households purchasing any taxed beverages in a month declined by 1.83 percentage points (2.6 percent). Thus, households made both fewer trips to purchase taxed beverages after implementation of the taxes and fewer beverage purchases during those shopping trips. Similar to the findings on the ounces of taxed beverages purchased, the estimates are statistically significant when clustering the standard errors at the household level but not when clustering at the area or city levels.

D. *Differences by City*

Our preferred estimates are based on the sample of all cities pooled, in the interests of maximizing statistical power. However, these estimates combine the results for Philadelphia, which taxes diet beverages, with the three other cities. We estimate the impact of the beverage tax in Philadelphia separately from the impact of the SSB taxes in Oakland, San Francisco, and Seattle combined.[12,13]

We present these results in Table 5. These results indicate that the decline in monthly household purchases found for the pooled sample is concentrated in Philadelphia. As shown in the first column, the estimate using data from Philadelphia and both of its comparison groups indicates that an increase in the beverage tax rate of 1 cent per ounce decreased household purchases of taxed beverages in Philadelphia by 84.1 ounces or 18.5 percent.[14] The decline is 58.6 percent greater than the estimated decline across all four cities (84.07 versus 53.00). Philadelphia's tax, however, was not 1 cent per ounce but 1.5 cents per ounce, which implies a decline in monthly purchases of 126.1 ounces or 27.7 percent.[15] The large reduction in purchases of taxed beverages in Philadelphia can be seen clearly in the top panel of Figure 3, which shows

[12] Given the sample sizes, we are able to estimate the impact separately for Philadelphia but not the other cities. We note that there are other distinguishing characteristics of Philadelphia besides the coverage of its tax; for example, the average monthly household purchases of SSBs is highest in Philadelphia, compared to the other three cities, prior to the implementation of the tax.

[13] In addition to examining heterogeneity based on breadth of the different taxes, we investigate whether there are heterogeneous impacts by household characteristics. Different population subgroups may have different price elasticities of demand for SSBs. We estimate the impact of beverage taxes separately by the age of the primary shopper, the race/ethnicity of the primary shopper, the educational attainment of the primary shopper, marital status, and household income. The results are shown in Appendix Table 6. We do not find any consistent and statistically significant relationships between these characteristics and the impact of the taxes.

[14] The results are statistically significant when clustering at the household and area level. We cannot cluster the standard errors at the city level because we would only have two clusters. In addition, as we observed for the results for all cities combined, the decline in purchases is larger when estimating the impact using the matched national comparison group, although the difference in estimates using the two comparison groups is not statistically significant.

[15] We also estimated the impacts of the tax on SSBs and non-caloric sweetened beverages separately. Because SSBs contain many more calories than non-caloric taxed beverages, the health effects of the estimated 126-ounce reduction in purchases in Philadelphia can vary greatly depending on which taxed beverages saw declines. In results not shown, we find that the tax decreased monthly purchases of SSBs by 98.8 ounces (26.1 percent) and non-caloric taxed beverages by 27.3 ounces (35.4 percent). The larger decline in SSBs in Philadelphia (98.8 ounces) compared to the other cities (14.75 ounces) suggests that the overall decrease in Philadelphia is not solely due to the types of beverages that are taxed.

stable trends in purchases in households in the comparison areas and a large decline in purchases in Philadelphia in the months immediately following implementation of the tax.

By comparison, there is not a clear relative decline in purchases of taxed beverages in the other three cities combined (see the second panel of Figure 3). There was a slight decline in purchases in the two months after implementation of the taxes; however, there were also declines for households in the comparison areas, and purchases in the treatment cities increased to pre-tax levels three months after the tax. Thus, we do not find that the taxes had measurable impacts on the purchases of taxed beverages in the other three cities combined; the point estimate is positive, small, and not statistically significant (Table 5).

V. Discussion

This paper examines the impact of the taxes on sugar-sweetened beverages in the four largest U.S. cities to implement such a tax: Philadelphia, San Francisco, Seattle, and Oakland. Using a difference-in-difference design with data of household purchases of beverages from those four cities plus two comparison areas for each city, we find that a tax of 1 cent per ounce decreased the average monthly household purchases of taxed beverages by 53.0 ounces or 12.2 percent. This magnitude is not large, and a tax of 1 cent per ounce is not likely to lead to a substantial reduction in obesity. A back-of-the-envelope calculation suggests that this effect translates into a reduction in consumption of 21 calories per day per household, which is a reduction of 5 calories per day per household member and a reduction in weight of 0.5 pounds per household member after roughly three years.[16] To clarify the effect sizes that we can rule out, the largest decrease within the confidence intervals (which is a reduction of 93.4 ounces per

[16] An average 20-ounce regular soda contains roughly 60 grams of sugar, and there are roughly 4 calories in a gram of sugar; thus, 53 fewer ounces of regular soda is roughly equivalent to 159 fewer grams and 636 fewer calories per household per month, or 21 fewer calories per day. Given the average household in the data has four members, if we assume each member consumes an equal share of taxed beverages, and that the reduction in purchases translates into a 1:1 reduction in calories consumed, the impact of the tax translates to roughly 159 fewer calories per household member per month, or 5 fewer per day. Hall et al. (2011) estimate that a sustained reduction in consumption of 10 calories per day translates into a reduction in weight of 1 pound, 95% of which is achieved after three years. Thus, a five-calorie reduction in consumption is expected to result in an eventual reduction in weight of 0.5 pounds. Applying the same estimated effect of the tax (a reduction in purchases of 12.2 percent) for households with purchases at the 90th percentile (1,150 ounces per month prior to the taxes), translates to a reduction of roughly 105 ounces, which is 420 grams of sugar and almost 1,700 calories per household per month, or 56 calories per day. This decline is roughly 14 few calories per day per household member. Note, these estimates also assume that the entire decline in beverage purchases was for regular sodas, although the decline includes other beverages with varying number of calories, including zero-calorie drinks in Philadelphia.

month, based on the wild-cluster bootstrap with area-level clusters) translates to a reduction of 9 fewer calories per day per household member and weight loss of nearly one pound.[17] Thus, even the most extreme plausible value still indicates a small change in steady state weight.

The decline is larger in Philadelphia, where the 1.5 cents per ounce tax reduced monthly purchases of taxed beverages by 126.1 ounces or 27.7 percent. This impact implies a reduction in consumption of roughly 10 calories per day per household member and a reduction in weight of roughly one pound after three years. In contrast, purchases of taxed beverages in Oakland, San Francisco, and Seattle combined did not decline after their taxes took effect; the point estimate in those three cities was less than 20% the absolute magnitude of that in Philadelphia, the opposite sign (positive), and not statistically significant. The differences in the impacts across cities could be due to Philadelphia taxing non-caloric sweetened beverages in addition to SSBs, the greater amount of household purchases in Philadelphia relative to the other cities prior to the taxes, or the relatively larger tax in Philadelphia of 1.5 cents per ounce.

A strength of the paper is the two different comparison groups for each city: 1) households outside the city but in the same MSA and state, and 2) matched urban households nationally. We find that the estimated effect of the tax on purchases of taxed beverages is roughly 20 percent smaller when using the nearby comparison group. This difference may be due to spillovers of the tax, such as suburban households occasionally shopping in the taxing jurisdiction, which would bias downward the estimated effect of the tax. However, households from the nearby comparison group, which are within the same media market as the taxing cities, may be exposed to the information campaigns that accompany the tax. Likewise, if beverage companies change their local advertising after a tax, that might affect the nearby comparison households as well as the households in the taxing city. Our results do not indicate whether one comparison group is preferable than the other. The trends in both comparison groups were similar to the trend in the treatment cities prior to the taxes. The national comparison group would include the impact of the tax and any changes in information or advertising surrounding the tax. The nearby comparison group is more likely to isolate the impact of the tax alone. However, both sets of estimates tell a consistent story that the taxes reduced purchases of taxed

[17] Following the logic of the previous calculation, a reduction of 93.4 ounces is approximately 1120 fewer calories per month or 37 fewer calories per day per household and 9 fewer calories per day per household member.

beverages and the difference in the estimates by comparison group are modest and not statistically significant.

These multi-city estimates add to a growing literature on the impacts of beverage taxes in individual cities on purchases and consumption. Regarding purchases, Roberto et al. (2019) and Seiler et al. (2019) estimate that the tax in Philadelphia led to a 38 percent and 22 percent reduction in sales of taxed beverages at large chain stores, respectively. Based on interviews of consumers exiting all types of stores in Philadelphia, Cawley et al. (2019b) found that consumers at stores in Philadelphia reduced purchases of taxed beverages per shopping trip by roughly 20 percent, although the reduction was not statistically significant. In comparison, based on household data that includes all beverage purchases at all types of stores regardless of the location, we find that the 1.5 cent per ounce tax in Philadelphia decreased purchases by 27.7 percent using both comparison groups and up to 29.7 percent using the national comparison group.[18] For Oakland, based on interviews of consumers exiting stores, Cawley et al. (2019c) found that the SSB tax did not significantly change purchases of taxed beverages per trip. This result is consistent with the finding in this paper that there was no statistically significant change in taxed beverage purchases in Oakland, San Francisco, and Seattle combined.

This study makes several important contributions to the literature. First, we examine unique data on household purchases that include all beverage purchases from all types of stores, rather than using sales data from a subset of stores in the treatment cities. Past studies that used store sales data tend to have only data from large chains and lack information on sales from small and independent stores that may be important sources of SSBs for urban households. Second, by examining the impact in the four largest U.S. cities with beverage taxes, we gain a better understanding of the effects of beverage taxes across multiple settings. Finally, we compare the impact estimates using two comparison groups commonly used in analyses of beverage taxes, each with their merits and limitations. By using both comparison groups, we have a more complete understanding of the impacts, greater confidence in our findings, and a perspective on the estimates provided by studies that use one comparison group.

[18] Although we don't have information on consumption, our finding of declines in purchases in Philadelphia are consistent with the findings in Cawley et al. (2019b) and Zhong et al. (2018), which found evidence of declines in consumption of soda among adults in Philadelphia.

Our results also suggest reasons for caution in the interpretation of the estimated impacts of SSB taxes in the prior literature. Most estimates in the literature are based on data drawn from two geographic clusters or less; as a result, standard errors are not clustered at the geographic level and may be underestimated. By pooling data on four cities with sweetened beverage taxes and a total of eight comparison areas (for a total of 12 clusters), we are able to compute confidence intervals that account for correlations among households within areas using the wild-cluster bootstrap method (Cameron, Gelbach, and Miller, 2008; Cameron and Miller, 2015; Roodman et al., 2019). We find that clustering the standard errors at the geographic level can substantially reduce the precision of the estimated impacts. Nevertheless, the qualitative findings remain consistent: across all four cities, a one-cent per ounce beverage tax is not likely to lead to a meaningful reduction in purchases that might translate to a significant reduction in body weight. This overall result combines a larger decrease in purchases in Philadelphia that remains statistically significant after clustering and no impact of the tax in Oakland, San Francisco, and Seattle combined. Even with the reduced precision from clustering by geography, we still find that there is a significant reduction in purchases of taxed beverages after the tax in Philadelphia.

This study has several limitations. Our longitudinal data are consistent with parallel trends in purchases six months before implementation of the taxes, but ideally, we would also have data from further back. However, prior studies have demonstrated consistent parallel trends in beverage purchases 12 months before the implementation of beverage taxes; for example, Cawley et al. (2019b) for Philadelphia and Cawley et al. (2019c) for Oakland.

A related limitation is that our estimates reflect data from up to 6 months after the tax. Households could adjust their responses to the taxes over time; for example, those that reduced consumption at first might slowly revert to their former consumption patterns, or conversely, households could reduce consumption even more over time as they change their habits. Examining long-run effects could be particularly important for children as they become adults and develop their patterns of consumption.

There is an unknown degree of measurement error is any panel of consumer purchases (Fricker et al., 2015). The InfoScout data collection process involves participants uploading photographs of their receipts, and there could be an unknown amount of underreporting due to errors of omission. However, measurement error should bias our estimates only if the degree of measurement error changes after the implementation of SSB taxes and differs in the treatment cities than the comparison areas. Underreporting would influence the estimate of the implied reduction in calories and weight based on the estimated reduction in purchases. If we conservatively assumed that the InfoScout data included 50 percent of all purchases made by households, then the estimated decrease of 53.0 ounces would translate to 10 fewer calories per day per household member and a steady-state weight loss of one pound. Even the most extreme decrease within the confidence interval based on the wild-cluster bootstrap with area-level clusters would only translate to a weight loss of two pounds. Similarly, the estimate for Philadelphia would translate to a weight loss of two pounds. Thus, even substantial underreporting is not likely to influence the overall conclusion.

Finally, although we have enough households to conduct a meaningful analysis of the beverage tax in Philadelphia, we do not have enough households to estimate the impact of the taxes separately for Oakland, San Francisco, or Seattle. Although the pooled analysis is a strength of this paper for the reasons mentioned above, the literature on beverage taxes taken together has shown that there can be substantial differences in the effects of the taxes in different locations. Thus, future research should continue to compare taxes and settings to assess their impacts, which will help policymakers in different settings and geographies design effective policies.

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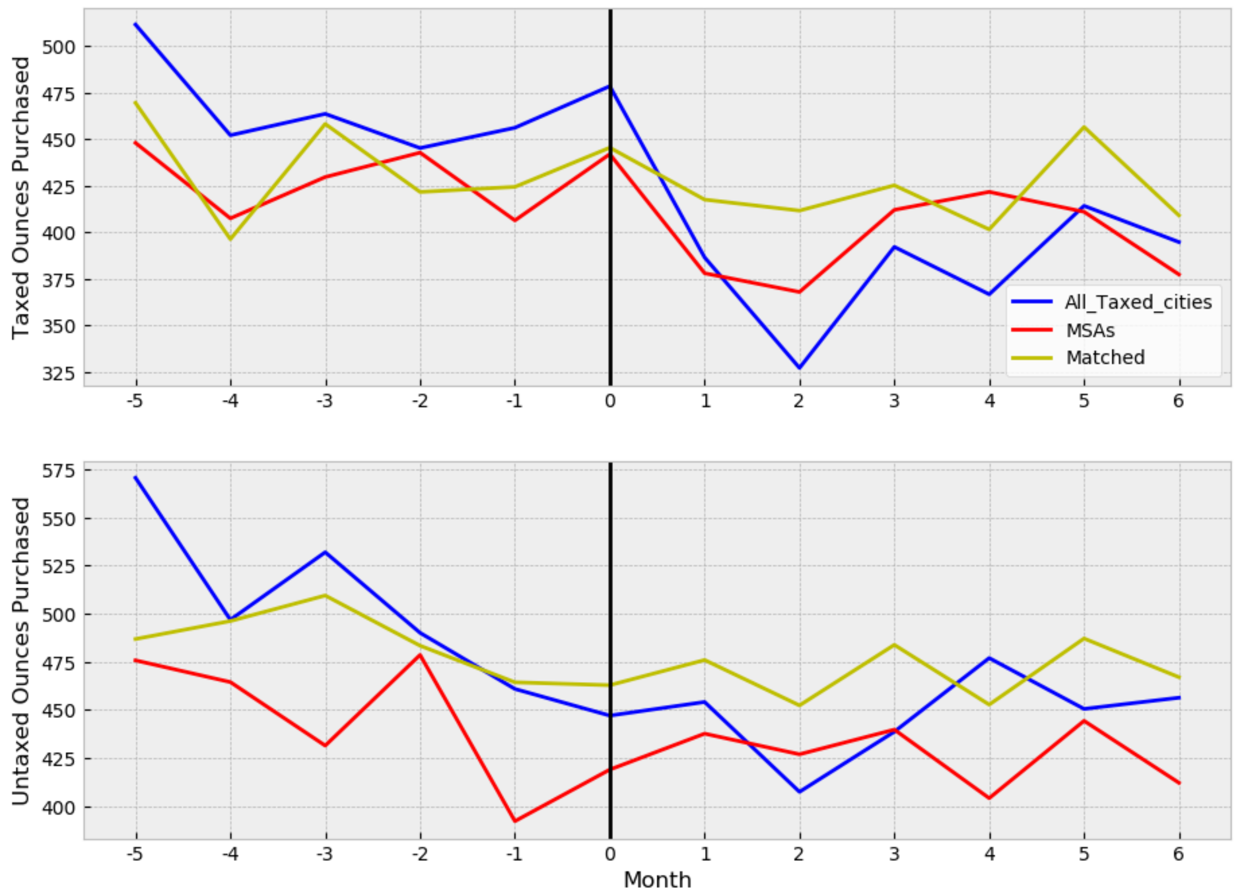
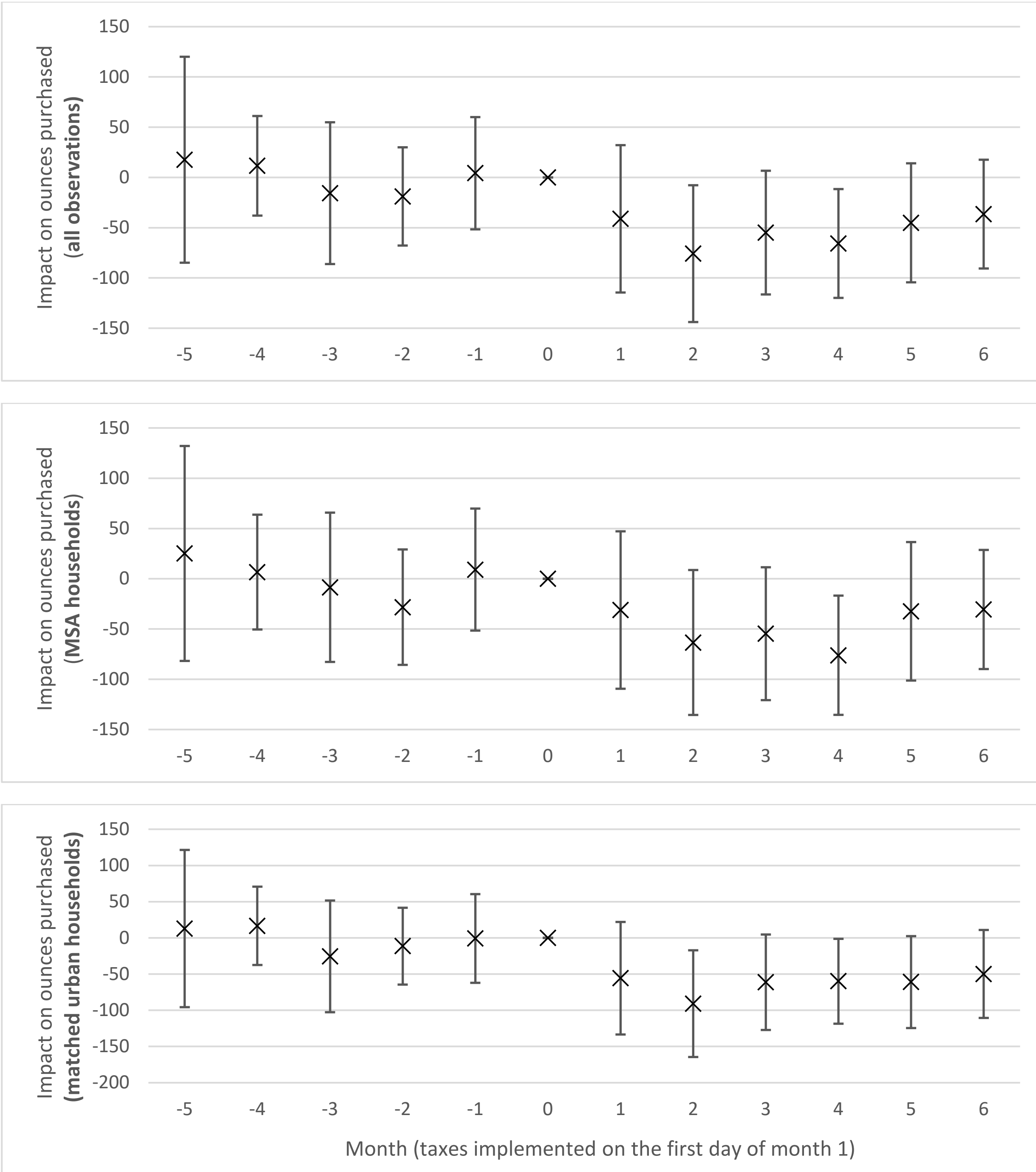
Figure 1: Average Household Purchases (in Ounces/Month) of Taxed and Untaxed Beverages



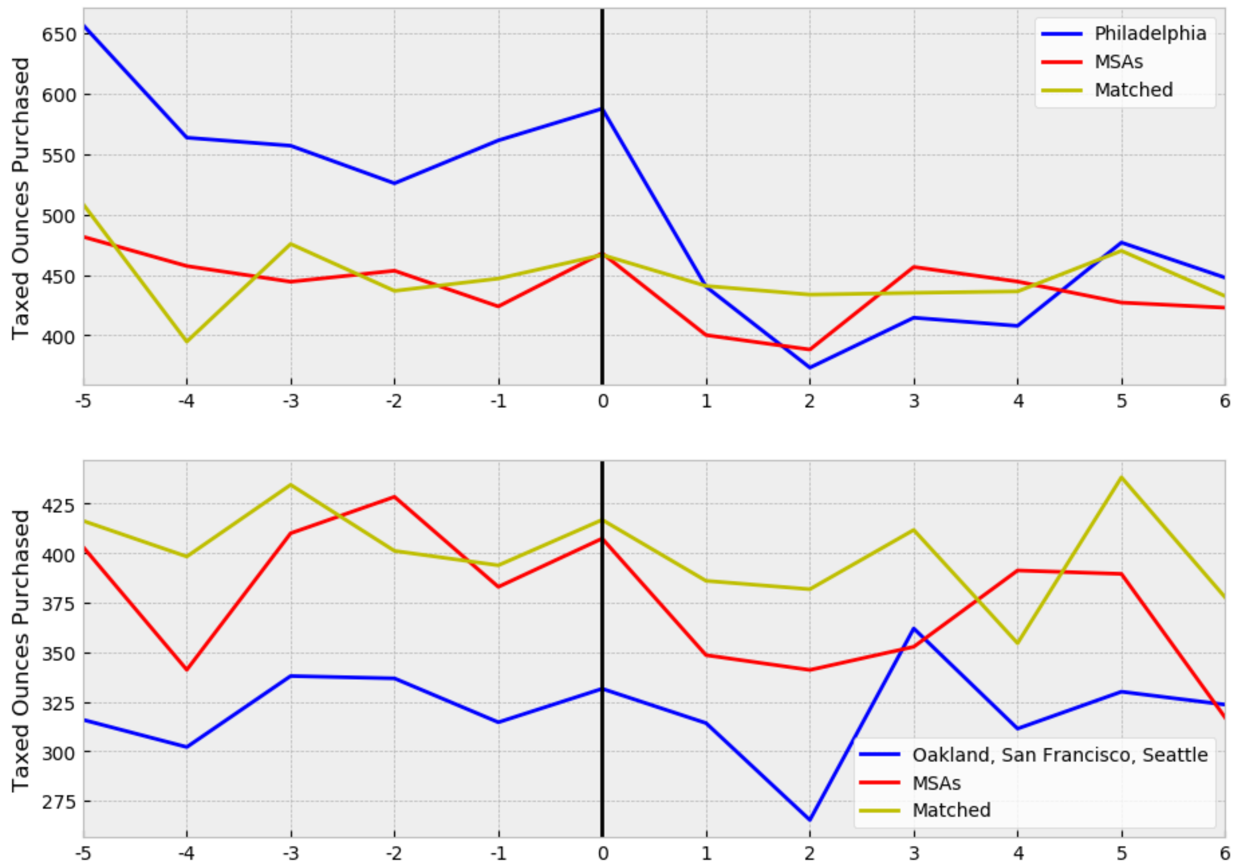
Notes: This figure shows the trends in the monthly average volume of taxed beverages purchased (top panel) and untaxed beverages purchased (bottom panel) for the six months before and six months after the implementation of each city beverage tax for households in the cities and the two comparison groups: (1) households in the MSAs but outside of the treatment cities and (2) matched households in cities nationwide. The taxes were implemented on the first day of month 1. The vertical line at month 0 distinguishes between the pre- and post-tax periods.

Figure 2: Impact of SSB Taxes on Purchases of Taxed Beverages (Ounces/Month)



Notes: This figure shows the point estimates and 95% confidence intervals, based on clustering at the household level, for each month relative to the month prior to the implementation of the taxes. The top panel compares households in the treatment cities to all comparison group households; the middle panel uses only households in the MSAs as the comparison group; the bottom panel uses only households in the matched urban sample as the comparison group.

Figure 3: Average Household Purchases (in Ounces/Month) of Taxed Beverages for Different Cities



Notes: This figure shows the trends in the monthly average volume of taxed beverages purchased for the six months before and six months after the implementation of the beverage tax in Philadelphia (top panel) and the SSB taxes in Oakland, San Francisco, and Seattle (bottom panel). The panels show the trends for households in the taxed cities, households in the MSAs but outside of the taxed cities, and matched households in cities nationwide. The taxes were implemented on the first day of month 1. The vertical line at month 0 distinguishes between the pre- and post-tax periods.

Table 1: Details of City Beverage Taxes

City	Implementation date	Taxed beverages	Amount of the tax
Philadelphia	January 1, 2017	SSBs and non-caloric sweetened beverages	1.5 cents/ounce
Oakland	July 1, 2017	SSBs	1 cent/ounce
San Francisco	January 1, 2018	SSBs	1 cent/ounce
Seattle	January 1, 2018	SSBs	1.75 cents/ounce

Notes: Sugar-sweetened beverages (SSBs) include regular soda, sports drinks, energy drinks, juice drinks, and sweetened iced tea. Additional cities in the United States with beverage taxes that are not included in the analysis are Boulder, Colorado; Berkeley, California; and Albany, California.

Table 2. Number of Households and Summary Statistics of Outcomes

		Cities with a Tax (Treatment Cities)	MSA (Comp. Group 1)	Matched Sample (Comp. Group 2)
Number of unique households: 1,447				
All locations		483	480	484
Philadelphia	(July 1, 2016 – June 30, 2017)	277	274	278
Oakland	(January 1, 2017 – December 31, 2017)	34	34	34
San Francisco	(July 1, 2017 – June 30, 2018)	123	123	123
Seattle	(July 1, 2017 – June 30, 2018)	49	49	49
Shopping trips per month		7.19	7.01	7.17
		(0.17)	(0.16)	(0.15)
Taxed beverages purchased per month		6.32	5.98	6.16
		(0.26)	(0.18)	(0.19)
Ounces of taxed beverages purchased per month		468.05	429.64	436.18
		(22.83)	(12.83)	(14.67)
Untaxed beverages purchased per month		4.26	3.98	4.02
		(0.12)	(0.10)	(0.10)
Ounces of untaxed beverages purchased per month		499.90	443.96	484.20
		(16.56)	(13.06)	(15.15)

Notes: The time periods for each city span six months before to six months after the implementation of each tax. Monthly averages of the number of shopping trips per household, number of taxed and untaxed beverages purchased per household, and ounces of taxed and untaxed beverages purchased per household are based on the six months prior to the implementation of each tax.

Table 3: Impact of SSB Taxes on Beverage Purchases (Ounces/Month)

	All Observations	Comparison Group: Treatment City MSAs	Comparison Group: National Matched Households
Taxed Beverages			
Tax Rate			
Point Estimate	-53.00	-48.74	-61.71
95% Confidence Interval			
Clustered at household level	[-86.04, -19.97]	[-84.57, -12.92]	[-96.98, -26.44]
Wild-cluster bootstrap, area clusters	[-93.35, 41.74]	[-109.20, 97.77]	[-114.00, 77.27]
Wild-cluster bootstrap, city clusters	[-84.76, 17.67]	[-79.68, 34.19]	[-91.50, 14.05]
Pre-Tax Mean	432.92	429.64	436.18
Observations	17,364	11,556	11,604
Households	1,447	963	967
Untaxed Beverages			
Tax Rate			
Point Estimate	-28.07	-27.62	-29.02
95% Confidence Interval			
Clustered at household level	[-61.28, 5.13]	[-64.49, 9.26]	[-67.57, 9.52]
Wild-cluster bootstrap, area clusters	[-66.39, 2.63]	[-88.04, 56.27]	[-52.52, -15.51]
Wild-cluster bootstrap, city clusters	[-50.51, 73.02]	[-141.8, 42.23]	[-72.78, 141.30]
Pre-Tax Mean	464.16	443.96	484.20
Observations	17,364	11,556	11,604
Households	1,447	963	967

Notes: The table reports the estimates of the impact of the taxes. The point estimates represent the change in the average ounces of beverages purchased by households per month due to a 1 cent per ounce beverage tax. We also included household and month fixed effects in the regressions. The columns represent the impact estimates using both comparison groups, the MSA comparison group, and the matched national comparison group. The pre-tax mean is the mean of the outcome variable for the comparison group over the six months prior to the implementation of the tax.

Table 4: Impact of SSB Taxes on Shopping Trips, Number of Beverages Purchased, and Any Beverages Purchased

	All Observations	Comparison Group: Treatment City MSAs	Comparison Group: National Matched Households
Shopping Trips Per Month			
Point Estimate	-0.43	-0.46	-0.43
95% Confidence Interval			
Clustered at household level	[-0.72, -0.14]	[-0.78, -0.15]	[-0.77, -0.09]
Wild-cluster bootstrap, area clusters	[-0.79, 0.37]	[-0.86, 0.63]	[-0.88, 0.42]
Wild-cluster bootstrap, city clusters	[-0.70, 0.17]	[-2.16, 0.46]	[-0.59, 0.14]
Pre-Tax Mean	7.09	7.01	7.17
Number of Taxed Beverages Purchased			
Point Estimate	-0.55	-0.58	-0.56
95% Confidence Interval			
Clustered at household level	[-0.95, -0.15]	[-1.01, -0.15]	[-1.01, -0.11]
Wild-cluster bootstrap, area clusters	[-1.04, 0.55]	[-1.32, 0.94]	[-1.23, 0.96]
Wild-cluster bootstrap, city clusters	[-0.95, 0.53]	[-2.21, 0.75]	[-0.89, 0.43]
Pre-Tax Mean	6.07	5.98	6.16
Any Taxed Beverages Purchased			
Point Estimate	-1.83	-2.05	-1.82
95% Confidence Interval			
Clustered at household level	[-3.51, -0.16]	[-3.98, -0.11]	[-3.74, 0.09]
Wild-cluster bootstrap, area clusters	[-2.85, 1.97]	[-6.42, 1.53]	[-2.82, 1,92]
Wild-cluster bootstrap, city clusters	[-2.62, 2.17]	[-6.51, 1.38]	[-2.78, 3.19]
Pre-Tax Mean	69.61	68.33	70.87
Observations	17,364	11,556	11,604
Households	1,447	963	967

Notes: The table reports the estimates of the impact of the taxes. The point estimates represent the change in the outcome due to a 1 cent per ounce beverage tax. We also included household and month fixed effects in the regressions. The columns represent the impact estimates using both comparison groups, the MSA comparison group, and the matched national comparison group. The pre-tax mean is the mean of the outcome variable for the comparison group over the six months prior to the implementation of the tax.

Table 5: Impact of Beverage Taxes on Purchases (Ounces/Month) in Philadelphia and Other Cities

	All Observations	Comparison Group: Treatment City MSAs	Comparison Group: National Matched Households
Philadelphia			
Tax Rate			
Point Estimate	-84.07	-78.03	-90.02
95% Confidence Interval			
Clustered at household level	[-130.08, -38.05]	[-127.29, -28.76]	[-138.20, -41.84]
Wild-cluster bootstrap, area clusters	[-101.9, -65.99]	[N/A]	[N/A]
Pre-Tax Mean	455.13	455.03	455.23
Observations	9,948	6,612	6,660
Households	829	551	555
Oakland, San Francisco, Seattle			
Tax Rate			
Point Estimate	14.75	20.84	5.76
95% Confidence Interval			
Clustered at household level	[-15.12, 44.62]	[-13.71, 55.38]	[-28.43, 39.94]
Wild-cluster bootstrap, area clusters	[-15.97, 54.37]	[-49.34, 79.43]	[-9.27, 42.25]
Wild-cluster bootstrap, city clusters	[-2.13, 25.42]	[-34.51, 37.43]	[-1.76, 28.86]
Pre-Tax Mean	382.25	383.37	381.15
Observations	7,416	4,944	4,944
Households	618	412	412

Notes: The table reports the estimates of the impact of the taxes on purchases of taxed beverages separately for Philadelphia and the other three cities combined. The point estimates represent the change in the average ounces of taxed beverages purchased by households per month due to a 1 cent per ounce beverage tax. We also included household and month fixed effects in the regressions. The columns represent the impact estimates using both comparison groups, the MSA comparison group, and the matched national comparison group. The pre-tax mean is the mean of the outcome variable for the comparison group over the six months prior to the implementation of the tax. We do not report standard errors clustered at the area level for the regressions comparing Philadelphia to single comparison groups because there are only two clusters in these cases.

Appendix

This appendix (1) describes the process to determine whether each beverage in the data is taxed and the volume of beverages purchased and (2) includes supplementary figures and tables describing the data and displaying additional results.

A. Determining taxed status and volume of beverages purchased

Because the InfoScout data does not directly identify whether each beverage purchase is taxed, we used the item description and other contextual information to classify whether each beverage was taxed or untaxed. For example, a common item description is “Coca Cola Soda Cola Bx 12 Ct 144 Oz”. We used the description to determine that the beverage is regular soda based on its name, and that the volume purchased is 144 ounces (12 ounces per can multiplied by 12 cans). We developed a process that combines automated parsing of the item description combined with manual review of the descriptions to identify whether each beverage is taxed or untaxed and the volume of the purchase in ounces.

First, we identified an initial set of keywords to classify every beverage purchase by beverage type. For example, we identified keywords for soda brands, such as Coke and Pepsi, and other keywords, such as diet and unsweetened that span across beverage types. The taxed beverage types are: regular soda, energy drinks, sports drinks, sweetened coffee, sweetened tea, and juice drinks. The untaxed beverage types are: bottled water, unsweetened coffee, unsweetened iced tea, 100% juice, and milk. Diet versions of soda, sports drinks, energy drinks, and tea are taxed in Philadelphia but not in Oakland, San Francisco, and Seattle. We also identified drink mixes, alcohol, and nondrinks (the latter of which are in the dataset by mistake). We applied the keywords to identify beverage type for as many records as we can. We also parsed the volume from the item description to create a separate variable that indicates volume in ounces; for example, “24 Ct 568.8” indicates a 24-count multi-pack of bottled water, 23.7 ounces per bottle. We converted all beverage sizes to ounces and multiplied each beverage purchase by the number of units purchased; for example, if the purchase is for four 20-ounce bottles of the same beverage, the total volume for the beverage purchase is 80 ounces.

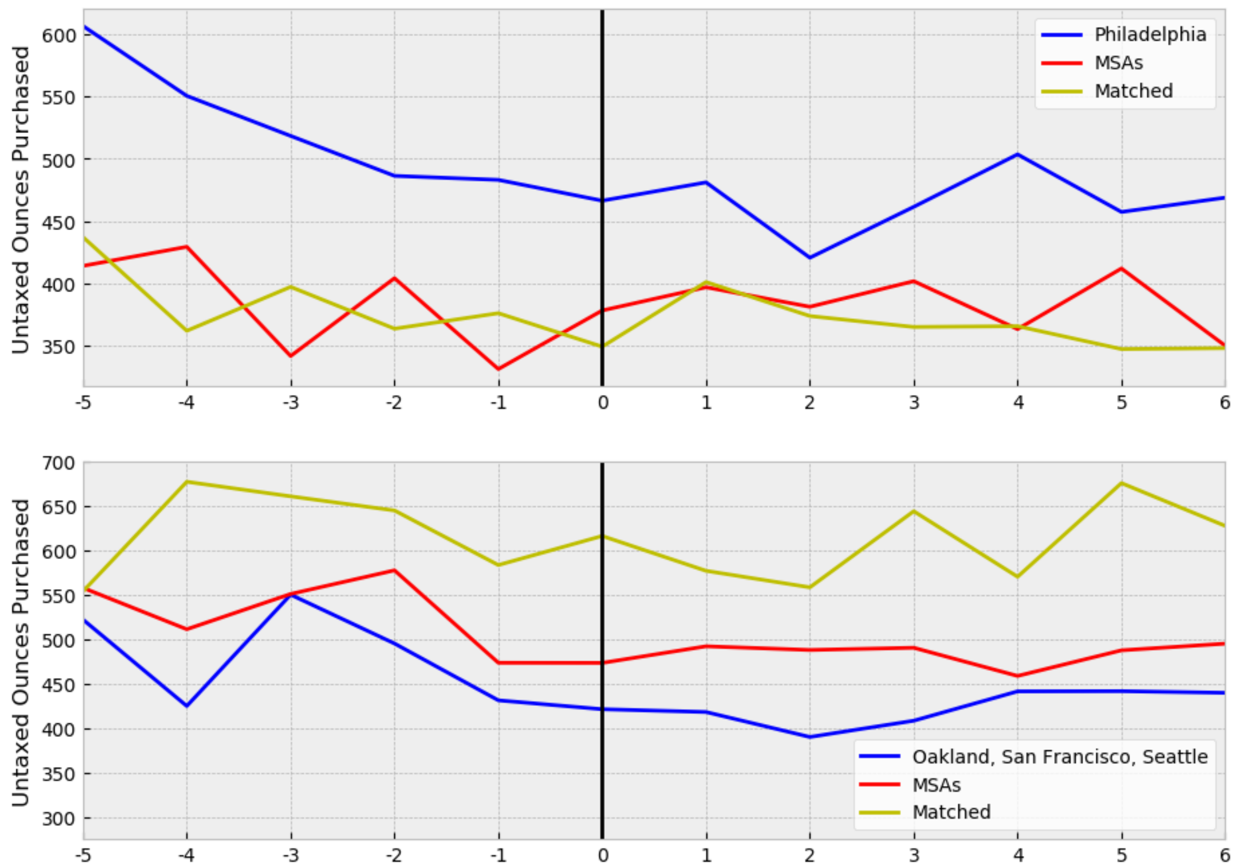
Next, we developed a function to predict the type of beverage using a random forest model for each purchase missing beverage type at this stage in the process. The function uses the dataset of beverage purchases restricted to beverages for which we have determined the beverage type using keywords. To improve the predictions, for beverage purchases missing beverage type assignment, we identified the beverage description in the dataset that is closest based on the item description. For example, if we are missing the beverage type with a description "Dt Mt Dew", we might identify another beverage with the description "Diet Mountain Dew" and labeled as diet soda. We calculated distances to the closest item descriptions using two approaches, Levenshtein and Jaccard distances (Gomaa and Fahmy, 2013). We then generated a random forest model to predict the beverage type that includes the Levenshtein and Jaccard distances, the beverage types predicted by the distances, state, household characteristics (age, ethnicity, education, marital status, income) and information on the shopping trip (trip type, beverages of the specific type purchased per trip, dollars spent per beverage, dollars spent per trip, beverages purchased per trip). We used cross validation among the records with beverage type filled in to tune the model parameters. The model outputs the predicted probability and beverage type with the highest predicted probability. We reviewed the predicted probabilities to identify a threshold above which the imputations clearly identify the correct beverage type, and we accepted the imputed beverage types above the threshold.

Next, we made a list of the remaining unclassified beverage descriptions sorted by how common the beverage is in the dataset to identify beverage type for these purchases. We reviewed the most common beverage descriptions and assigned them beverage types, reviewing nutritional information for the beverages online for cases in which the beverage type is not clear based on the description. Through the review, we also identified additional keywords used to identify beverage types and revised existing keywords when needed. We then repeated the process starting with applying the keywords to identify beverage type. We repeated the process until beverage purchases appearing in the data ten times or more were assigned a beverage type. We accepted the imputed beverage types for the remainder of the beverage purchases, which is less than ten percent of purchases. Thus, beverage type is filled in for every purchase in the dataset. Finally, we used the beverage types to identify whether each beverage is taxed or untaxed for each city. We identified sweetened non-caloric beverages (diet versions of soda, energy drinks, sports drinks, and tea) as taxed in Philadelphia but not in the other three cities to reflect differences in the taxes across the cities in the study.

Because not every beverage description includes the volume of the beverage, we used the other information available for the purchase to impute the volume. We estimated a similar random forest model as the model described above for beverage type to predict the volume for all purchases missing volume. We estimated the model on beverage purchases with nonmissing volume. We included beverage type, state, household characteristics (age, ethnicity, education, marital status, income) and information on the shopping trip (trip type, beverages of the specific type purchased per trip, dollars spent per beverage, dollars spent per trip, beverages purchased per trip). We used a subset of purchases with nonmissing volume to cross-validate and tune the model parameters. We filled in the predicted values for all purchases missing volume, which is roughly 60 percent of the nearly 120,000 records. The prediction accuracy was roughly 63 percent, which can be thought of as a measure of explained variance.

B. Supplemental Figures and Tables

Appendix Figure 1: Average Monthly Ounces of Untaxed Beverages Purchased by Households in Different Cities



Notes: This figure shows the trends in the monthly average volume of taxed beverages purchased (top panel) and untaxed beverages purchased (bottom panel) for the six months before and six months after the implementation of the beverage tax for households in Philadelphia and the two comparison groups: (1) households in the MSA but outside of Philadelphia and (2) matched households in cities nationwide. The taxes were implemented on the first day of month 1. The vertical line at month 0 distinguishes between the pre- and post-tax periods.

Appendix Table 1. Characteristics of Households in the InfoScout Sample and the American Community Survey

	Treatment cities		Comparison Group 1 (Treat. City MSAs)		Comparison Group 2 (Nat. Matched Households)	
	InfoScout	ACS	InfoScout	ACS	InfoScout	ACS
Generation						
Millennials (1982-2004)	35.61	27.35	29.58	17.62	39.46	24.29
Gen. X (1965-1981)	56.11	29.74	61.46	32.58	55.17	32.40
Boomers (1945-1964)	7.45	31.50	8.54	36.69	4.96	32.33
Seniors (before 1945)	0.41	11.41	0.21	13.11	0.21	10.98
Unknown	0.41	n.a.	0.21	n.a.	0.21	n.a.
Race/Ethnicity						
Asian	31.26	15.09	28.54	13.69	31.20	9.57
African American	10.35	21.40	8.13	6.63	10.74	19.18
Hispanic	9.73	10.73	6.25	9.75	9.30	26.51
White	41.41	49.19	52.08	66.35	41.53	42.19
Other	7.25	0.76	5.00	0.99	7.23	0.71
Multiple race	n.a.	2.83	n.a.	2.60	n.a.	1.85
Education						
< high school degree	3.93	10.38	1.88	5.95	3.51	14.68
High school degree	29.81	41.36	25.21	47.05	27.48	45.73
College degree	43.89	27.47	50.00	27.08	46.90	23.59
Graduate degree	16.98	20.79	16.25	19.92	17.15	16.01
Unknown	5.38	n.a.	6.67	n.a.	4.96	n.a.
Marital Status						
Married	62.94	34.38	71.04	56.32	69.22	42.35
Living with partner	9.32	7.29	4.79	5.93	8.26	6.28
Was married	6.63	21.90	11.88	23.20	7.23	23.98
Never married	19.67	36.43	11.67	14.54	14.88	27.40
Unknown/refused	1.45	n.a.	0.63	n.a.	0.41	n.a.
Income						
< $40,000	29.40	34.85	27.92	20.63	29.75	35.73
$40,000-$80,000	26.09	21.94	28.13	24.15	28.31	26.79
$80,000-$125,000	22.15	16.11	22.71	20.53	25.00	16.90
> $125,000	18.01	27.10	21.25	34.69	16.94	20.58
Unknown	4.35	n.a.	0.00	n.a.	0.00	n.a.
Household size	4.00	[]	4.00		4.06	

Notes: The table reports characteristics of the primary shoppers for households in the InfoScout data and reference person for households in the 2017 American Community Survey (ACS) data. The table reports the figures for households in the treatment cities and the two comparison groups: (1) households in the MSAs but outside of the treatment cities and (2) matched households in cities nationwide. For the second comparison group, the ACS figures cover the cities included in the matched comparison group. The figures are percentages of households with the given characteristics, with the exception the mean values for household size.

Appendix Table 2. Characteristics of Households in the Treatment and Comparison Groups

	Treatment Cities	Comp. Group 1 (Treat. City MSAs)	Comp. Group 2 (Nat. Matched Households)	Difference (T-C1)	Difference (T-C2)
Generation					
Millennials (1982-2004)	35.61	29.58	39.46	6.03	-3.85
	(2.18)	(2.08)	(2.22)	(3.02)	(3.11)
Gen. X (1965-1981)	56.11	61.46	55.17	-5.35	0.94
	(2.26)	(2.22)	(2.26)	(3.17)	(3.20)
Boomers (1945-1964)	7.45	8.54	4.96	-1.09	2.50
	(1.20)	(1.28)	(0.99)	(1.75)	(1.55)
Seniors (before 1945)	0.41	0.21	0.21	0.21	0.21
	(0.29)	(0.21)	(0.21)	(0.36)	(0.36)
Unknown	0.41	0.21	0.21	0.21	0.21
	(0.29)	(0.21)	(0.21)	(0.36)	(0.36)
Race/Ethnicity					
Asian	31.26	28.54	31.20	2.72	0.07
	(2.11)	(2.06)	(2.11)	(2.95)	(2.98)
African American	10.35	8.13	10.74	2.23	-0.39
	(1.39)	(1.25)	(1.41)	(1.87)	(1.98)
Hispanic	9.73	6.25	9.30	3.48	0.43
	(1.35)	(1.11)	(1.32)	(1.74)	(1.89)
White	41.41	52.08	41.53	-10.68	-0.12
	(2.24)	(2.28)	(2.24)	(3.20)	(3.17)
Other	7.25	5.00	7.23	2.25	0.02
	(1.18)	(1.00)	(1.18)	(1.54)	(1.67)
Education					
Less than high school degree	3.93	1.88	3.51	2.06	0.42
	(0.89)	(0.62)	(0.84)	(1.08)	(1.22)
High school degree	29.81	25.21	27.48	4.61	2.33
	(2.08)	(1.98)	(2.03)	(2.87)	(2.91)
College degree	43.89	50.00	46.90	-6.11	-3.01
	(2.26)	(2.28)	(2.27)	(3.21)	(3.20)
Graduate degree	16.98	16.25	17.15	0.73	-0.17
	(1.71)	(1.68)	(1.71)	(2.40)	(2.42)
Unknown	5.38	6.67	4.96	-1.28	0.42
	(1.03)	(1.14)	(0.99)	(1.53)	(1.42)
Marital Status					
Married	62.94	71.04	69.22	-8.10	-6.28
	(2.20)	(2.07)	(2.10)	(3.02)	(3.04)
Living with partner	9.32	4.79	8.26	4.53	1.05
	(1.32)	(0.98)	(1.25)	(1.64)	(1.82)
Was married	6.63	11.88	7.23	-5.25	-0.61
	(1.13)	(1.48)	(1.18)	(1.86)	(1.63)
Never married	19.67	11.67	14.88	8.00	4.79
	(1.81)	(1.47)	(1.62)	(2.33)	(2.43)
Unknown/refused	1.45	0.63	0.41	0.82	1.04
	(0.54)	(0.36)	(0.29)	(0.65)	(0.62)

Income					
< $40,000	29.40	27.92	29.75	1.48	-0.35
	(2.07)	(2.05)	(2.08)	(2.91)	(2.94)
$40,000-$80,000	26.09	28.13	28.31	-2.04	-2.22
	(2.00)	(2.05)	(2.05)	(2.86)	(2.86)
$80,000-$125,000	22.15	22.71	25.00	-0.56	-2.85
	(1.89)	(1.91)	(1.97)	(2.69)	(2.73)
> $125,000	18.01	21.25	16.94	-3.24	1.07
	(1.75)	(1.87)	(1.71)	(2.56)	(2.44)
Unknown	4.35	0.00	0.00	-4.35	-4.35
	(0.93)	n.a.	n.a.	n.a.	n.a.
Household size	4.00	4.00	4.06	0.00	0.06
	(0.06)	(0.06)	(0.06)	(0.09)	(0.09)
Number of households	483	480	484	963	967

Notes: The table reports the percentage of primary shoppers in the households with the given characteristics for the treatment cities and the two comparison groups: (1) households in the MSAs but outside of the treatment cities and (2) matched households in cities nationwide. We also report the mean household size for these groups.

Appendix Table 3a. Average Ounces Purchased, Shopping Trips, and Beverages Purchased Per Month Before and After Implementation of Beverage Taxes

	Treatment Cities			Comparison Group			
	Pre-tax	Post-Tax	Difference	Pre-tax	Post-Tax	Difference	DiD
Comp. Group 1							
Any purchases taxed (%)	67.12	62.56	-4.56	68.26	65.94	-2.33	-2.23
	(0.87)	(0.90)	(1.25)	(0.87)	(0.88)	(1.24)	(1.76)
Any purchases untaxed (%)	65.32	64.01	-1.31	66.70	63.47	-3.23	1.92
	(0.88)	(0.89)	(1.26)	(0.88)	(0.90)	(1.26)	(1.78)
Ounces purchased taxed	468.05	380.54	-87.51	429.64	394.97	-34.68	-52.84
	(22.83)	(15.84)	(27.79)	(12.83)	(12.26)	(17.75)	(33.02)
Ounces purchased untaxed	499.90	447.72	-52.18	443.96	427.90	-16.06	-36.12
	(16.56)	(13.94)	(21.64)	(13.06)	(14.63)	(19.61)	(29.22)
Trips per household	7.19	6.14	-1.05	7.01	6.51	-0.50	-0.55
	(0.17)	(0.14)	(0.22)	(0.16)	(0.15)	(0.22)	(0.31)
Taxed beverages purchased	6.32	5.18	-1.14	5.98	5.51	-0.47	-0.67
	(0.26)	(0.18)	(0.32)	(0.18)	(0.18)	(0.26)	(0.41)
Observations	2,898	2,898	5,796	2,880	2,880	5,760	11,556
Comp. Group 2							
Any purchases taxed (%)	67.12	62.56	-4.56	70.76	68.11	-2.65	-1.90
	(0.87)	(0.90)	(1.25)	(0.84)	(0.87)	(1.21)	(1.74)
Any purchases untaxed (%)	65.32	64.01	-1.31	67.94	67.36	-0.59	-0.73
	(0.88)	(0.89)	(1.26)	(0.87)	(0.87)	(1.23)	(1.76)
Ounces purchased taxed	468.05	380.54	-87.51	436.18	420.53	-15.64	-71.87
	(22.83)	(15.84)	(27.79)	(14.67)	(14.45)	(20.59)	(34.58)
Ounces purchased untaxed	499.90	447.72	-52.18	484.20	470.20	-14.00	-38.18
	(16.56)	(13.94)	(21.64)	(15.15)	(14.90)	(21.25)	(30.33)
Trips per household	7.19	6.14	-1.05	7.17	6.62	-0.55	-0.50
	(0.17)	(0.14)	(0.22)	(0.15)	(0.15)	(0.21)	(0.31)
Taxed beverages purchased	6.32	5.18	-1.14	6.16	5.67	-0.49	-0.65
	(0.26)	(0.18)	(0.32)	(0.19)	(0.18)	(0.26)	(0.41)
Observations	2,898	2,898	5,796	2,904	2,904	5,808	11,604

Notes: The table reports the pre- and post-tax mean values in the treatment cities and the two comparison groups, the pre- and post-tax differences, and the unadjusted DiD estimates. The two comparison groups are: (1) households in the MSAs but outside of the treatment cities and (2) matched households in cities nationwide.

Appendix Table 3b. Average Ounces Purchased Per Month Before and After Implementation of Beverage Taxes (Comparison Group 1)

	Treatment Cities			Comparison Group 1 (Treatment City MSAs)			
	Pre-tax	Post-Tax	Difference	Pre-tax	Post-Tax	Difference	DiD
Taxed Beverages	468.05	380.54	-87.51	429.64	394.97	-34.68	-52.84
	(22.83)	(15.84)	(27.79)	(12.83)	(12.26)	(17.75)	(33.02)
	[2,898]	[2,898]	[5,796]	[2,880]	[2,880]	[5,760]	[11,556]
Philadelphia	575.57	427.02	-148.54	455.03	423.53	-31.50	-117.04
	(38.06)	(25.42)	(45.77)	(19.46)	(18.90)	(27.13)	(53.34)
	[1,662]	[1,662]	[3,324]	[1,644]	[1,644]	[3,288]	[6,612]
All other cities	323.47	318.03	-5.44	395.87	356.97	-38.89	33.45
	(14.78)	(14.36)	(20.61)	(14.94)	(13.48)	(20.12)	(28.80)
	[1,236]	[1,236]	[2,472]	[1,236]	[1,236]	[2,472]	[4,944]
Untaxed Beverages	499.90	447.72	-52.18	443.96	427.90	-16.06	-36.12
	(16.56)	(13.94)	(21.64)	(13.06)	(14.63)	(19.61)	(29.22)
	[2,898]	[2,898]	[5,796]	[2,880]	[2,880]	[5,760]	[11,556]
Philadelphia	518.74	465.68	-53.06	383.37	384.41	1.04	-54.10
	(22.49)	(20.03)	(30.11)	(17.85)	(20.75)	(27.38)	(40.72)
	[1,662]	[1,662]	[3,324]	[1,644]	[1,644]	[3,288]	[6,612]
All other cities	474.56	423.56	-50.99	524.54	485.73	-38.81	-12.18
	(24.33)	(18.49)	(30.56)	(18.79)	(19.91)	(27.38)	(41.03)
	[1,236]	[1,236]	[2,472]	[1,236]	[1,236]	[2,472]	[4,944]

Notes: The table reports the pre- and post-tax mean values in the treatment cities and the comparison group comprising households in the MSAs, the pre- and post-tax differences, and the unadjusted DiD estimates. The table reports the values for all treatment cities, Philadelphia separately, and the other three cities combined (Oakland, San Francisco, and Seattle).

Appendix Table 3c. Average Ounces Purchased Per Month Before and After Implementation of Beverage Taxes (Comparison Group 2)

	Treatment Cities			Comparison Group 2 (National Matched Households)			
	Pre-tax	Post-Tax	Difference	Pre-tax	Post-Tax	Difference	DiD
Taxed Beverages	468.05	380.54	-87.51	436.18	420.53	-15.64	-71.87
	(22.83)	(15.84)	(27.79)	(14.67)	(14.45)	(20.59)	(34.58)
	[2,898]	[2,898]	[5,796]	[2,904]	[2,904]	[5,808]	[11,604]
Philadelphia	575.57	427.02	-148.54	455.23	441.71	-13.52	-135.03
	(38.06)	(25.42)	(45.77)	(21.88)	(21.64)	(30.77)	(55.11)
	[1,662]	[1,662]	[3,324]	[1,668]	[1,668]	[3,336]	[6,660]
All other cities	323.47	318.03	-5.44	410.46	391.95	-18.51	13.07
	(14.78)	(14.36)	(20.61)	(17.78)	(17.29)	(24.80)	(32.25)
	[1,236]	[1,236]	[2,472]	[1,236]	[1,236]	[2,472]	[4,944]
Untaxed Beverages	499.90	447.72	-52.18	484.20	470.20	-14.00	-38.18
	(16.56)	(13.94)	(21.64)	(15.15)	(14.90)	(21.25)	(30.33)
	523.54	468.44	-55.10	523.27	485.08	-38.19	-16.91
Philadelphia	518.74	465.68	-53.06	381.15	367.10	-14.05	-39.02
	(22.49)	(20.03)	(30.11)	(17.18)	(17.31)	(24.39)	(38.73)
	[1,662]	[1,662]	[3,324]	[1,668]	[1,668]	[3,336]	[6,660]
All other cities	474.56	423.56	-50.99	623.28	609.34	-13.94	-37.05
	(24.33)	(18.49)	(30.56)	(26.51)	(25.55)	(36.81)	(47.85)
	[1,236]	[1,236]	[2,472]	[1,236]	[1,236]	[2,472]	[4,944]

Notes: The table reports the pre- and post-tax mean values in the treatment cities and the comparison group comprising matched households in cities nationwide, the pre- and post-tax differences, and the unadjusted DiD estimates. The table reports the values for all treatment cities, Philadelphia separately, and the other three cities combined (Oakland, San Francisco, and Seattle).

Appendix Table 4: Impact of SSB Taxes on Monthly Untaxed Beverage Purchases

	All Observations	Comparison Group: Treatment City MSAs	Comparison Group: National Matched Households
Philadelphia			
Tax Rate			
Point Estimate	-31.00	-36.07	-26.01
95% Confidence Interval			
Clustered at household level	[-69.08, 7.07]	[-79.21, 7.07]	[-69.21, 17.19]
Wild-cluster bootstrap, area clusters	[-46.16, -16.06]	[N/A]	[N/A]
Pre-Tax Mean	403.16	395.87	410.46
Observations	9,948	6,612	6,660
Households	829	551	555
Oakland, San Francisco, Seattle			
Tax Rate			
Point Estimate	-25.14	-17.17	-35.57
95% Confidence Interval			
Clustered at household level	[-89.65, 39.38]	[-88.10, 53.76]	[-112.07, 40.92]
Wild-cluster bootstrap, area clusters	[-106.90, 173.50]	[-185.30, 123.30]	[-90.19, 155.80]
Wild-cluster bootstrap, city clusters	[-47.52, 94.11]	[-94.73, 20.81]	[-80.63, 182.10]
Pre-Tax Mean	573.91	524.54	623.278
Observations	7,416	4,944	4,944
Households	618	412	412

Notes: The table reports the estimates of the impact of the taxes on purchases of untaxed beverages separately for Philadelphia and the other three cities combined. The point estimates represent the change in the average ounces of untaxed beverages purchased by households per month due to a 1 cent per ounce beverage tax. We also included household and month fixed effects in the regressions. The columns represent the impact estimates using both comparison groups, the MSA comparison group, and the matched national comparison group. The pre-tax mean is the mean of the outcome variable for the comparison group over the six months prior to the implementation of the tax. We do not report standard errors clustered at the area level for the regressions comparing Philadelphia to single comparison groups because there are only two clusters in these cases.

Appendix Table 5: Impact of SSB Taxes on the Number of Untaxed Beverages Purchased and Any Untaxed Beverages Purchased

	All Observations	Comparison Group: Treatment City MSAs	Comparison Group: National Matched Households
Untaxed Beverages Purchased			
Point Estimate	-0.25	-0.24	-0.25
95% Confidence Interval			
Clustered at household level	[-0.48, -0.02]	[-0.50, 0.03]	[-0.52, 0.01]
Wild-cluster bootstrap, area clusters	[-0.58. 0.05]	[-0.79, 0.47]	[-0.39, -0.20]
Wild-cluster bootstrap, city clusters	[-0.36, 0.16]	[-1.01, 0.32]	[-0.53, -0.20]
Pre-Tax Mean	4.00	3.98	4.02
Any Untaxed Beverages Purchased			
Point Estimate	0.44	1.35	0.45
95% Confidence Interval			
Clustered at household level	[-1.12, 2.01]	[-0.47, 3.18]	[-2.26, 1.35]
Wild-cluster bootstrap, area clusters	[-3.62, 4.74]	[-7.26, 3.90]	[-1.84, 2.62]
Wild-cluster bootstrap, city clusters	[< -0.01, 6.34]	[-2.73, 2.42]	[-1.53, 10.94]
Pre-Tax Mean	67.31	66.67	67.94
Observations	17,364	11,556	11,604
Households	1,447	963	967

Notes: The table reports the estimates of the impact of the taxes. The point estimates represent the change in the outcome due to a 1 cent per ounce beverage tax. We also included household and month fixed effects in the regressions. The columns represent the impact estimates using both comparison groups, the MSA comparison group, and the matched national comparison group. The pre-tax mean is the mean of the outcome variable for the comparison group over the six months prior to the implementation of the tax.

Appendix Table 6: Impact of the SSB Tax on Monthly Purchases in Ounces by Household Characteristics

	All Observations	Comparison Group: Treatment City MSAs	Comparison Group: National Matched Households
Generation			
Millennials (1982-2004)	-16.85	-26.45	-12.11
	[-49.11, 15.42]	[-64.45, 11.55]	[-47.08, 22.87]
	[-38.58, 37.59]	[-49.00, 47.60]	[-48.95, 83.85]
Gen X (1965-1981)	-80.37	-69.54	-97.72
	[-134.35, -26.38]	[-127.05, -12.02]	[-155.09, -40.36]
	[-139.20, 50.77]	[-150.70, 135.00]	[-178.30, 86.80]
Boomers (1945-1964)	-32.94	-30.41	-46.38
	[-100.739, 34.85]	[-100.67, 39.86]	[-155.68, 62.92]
	[-88.54, 115.20]	[-95.73, 183.90]	[-85.70, 88.11]
Seniors (before 1945)	70.86	-15.98	112.47
	[-323.78, 465.51]	[-518.34, 486.38]	[-507.97, 732.92]
	[-73.89, 577.50]	[-562.40, 967.60]	[-562.40, 1327.00]
Race/Ethnicity			
Asian	-59.83	-55.42	-73.22
	[-156.37, 36.70]	[-159.77, 48.92]	[-175.54, 29.10]
	[-202.8, 50.42]	[-225.70, 101.40]	[-234.30, 69.32]
African American	-19.80	5.60	-40.14
	[-89.89, 50.29]	[-73.03, 84.24]	[-120.54, 40.25]
	[-65.50, 79.93]	[-4.37, 65.03]	[-91.81, 48.33]
Hispanic	-90.56	-142.40	-59.84
	[-209.19, 28.07]	[-290.81, 6.01]	[-180.55, 60.87]
	[-173.70, 91.23]	[-229.70, 110.70]	[-140.90, 179.10]
White	-53.37	-40.59	-70.69
	[-90.33, -16.40]	[-80.78, -0.39]	[-113.12, -28.26]
	[-101.00, 31.88]	[-90.33, 55.66]	[-101.00, 15.50]
Other	-31.38	-91.64	0.28
	[-99.50, 36.75]	[-188.22, 4.94]	[-69.71, 70.27]
	[-142.10, 148.20]	[-268.90, 78.16]	[-108.40, 306.60]
Education			
Less than high school degree	-124.14	-80.29	-156.07
	[-298.79, 50.52]	[-281.50, 120.92]	[-334.29, 22.14]
	[-230.70, 205.20]	[-290.30, 337.60]	[-716.40, 263.70]
High school degree	-85.05	-77.45	-92.05
	[-137.95, -32.15]	[-140.41, -14.48]	[-148.04, -36.05]
	[-151.00, -57.33]	[-153.90, -64.06]	[N/A]
College degree	-35.83	-23.81	-53.60
	[-92.80, 21.14]	[-84.06, 36.44]	[-114.63, 7.43]
	[-98.25, 68.57]	[-98.99, 114.40]	[-120.80, 80.35]
Graduate degree	-43.76	-63.89	-28.90
	[-119.21, 31.70]	[-143.16, 15.38]	[-110.09, 52.29]
	[-99.90, 67.27]	[-116.90, 133.60]	[-106.30, 145.40]
Marital Status			
Married	-74.52	-68.49	-84.15

	[-122.66, -26.37]	[-119.64, -17.34]	[-134.92, -33.38]
	[-128.70, 32.23]	[-141.80, 75.69]	[-154.40, 61.55]
Living with partner	5.44	-16.24	6.62
	[-62.91, 73.78]	[-111.60, 79.13]	[-72.26, 85.51]
	[-57.30, 190.40]	[-296.40, 625.70]	[-66.95, 216.10]
Was married	7.62	13.95	-5.09
	[-86.56, 101.80]	[-85.59, 113.49]	[-120.08, 109.90]
	[-43.48, 195.70]	[-19.55, 158.20]	[-51.99, 292.90]
Never married	-58.47	-66.21	-58.91
	[-113.50, -3.45]	[-136.99, 4.58]	[-117.29, -0.53]
	[-113.20, 26.49]	[-139.40, -2.95]	[-185.30, 42.70]
Income			
< $40,000	-51.23	-39.70	-62.59
	[-98.01¸ -4.45]	[-90.80, 11.41]	[-114.10, -11.09]
	[-76.92, -8.66]	[-55.67, 4.46]	[-80.16, -11.54]
$40,000-$80,000	-60.59	-65.79	-62.26
	[-143.23, 22.05]	[-153.40, 21.82]	[-148.22, 23.70]
	[-118.60, 153.10]	[-242.10, 152.20]	[-139.50, 167.20]
$80,000-$125,000	-90.71	-88.83	-98.29
	[-166.66, -14.77]	[-168.83, -8.83]	[-180.57, -16.02]
	[-130.30, 27.56]	[-136.90, 102.60]	[-150.70, 68.67]
> $125,000	-9.08	-0.68	-24.70
	[-66.33, 48.17]	[-69.50, 68.14]	[-84.24, 34.83]
	[-109.30, 61.85]	[-126.10, 92.67]	[-150.40, 47.61]

Notes: The table reports the estimates of the impact of the taxes by subgroups based on household characteristics. The point estimates represent the change in the average ounces of beverages purchased by households in the given group per month due to a 1 cent per ounce beverage tax. We also included household and month fixed effects in the regressions. The columns represent the impact estimates using both comparison groups, the MSA comparison group, and the matched national comparison group. The first confidence interval is calculated clustering the standard errors at the household level; the second is calculated clustering at the area level. The pre-tax mean is the mean of the outcome variable for the comparison group over the six months prior to the implementation of the tax. A confidence interval for the subgroup high school degree using comparison group 2 when clustering by area could not be calculated.