# Soda Wars: The Effect of a Soda Tax Election on University Beverage Sales*

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**Abstract**

We examine how soda sales changed due to the campaign attention and election outcome of a local excise tax on sugar-sweetened beverages (SSB), most commonly referred to as a soda tax. Using panel data of beverage sales from university retailers in Berkeley, California, we estimate that soda purchases relative to control beverages significantly dropped immediately after the election, months before the tax was implemented in the city of Berkeley or on campus. Supplemental scanner data from off-campus drug stores reveal this result is not unique to the university setting. Our findings suggest soda tax media coverage and election outcomes can have larger effects on purchasing behavior than the tax itself.

**Keywords:** Sugar-Sweetened Beverage Tax, Berkeley, Election, Difference-in-differences, Event Study

**JEL Classification:** D12, H20, C23, I38, Q18

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## I Introduction

With the current trend of sugar consumption, exercise, and dietary habits, it is estimated that 40% of Americans born from 2000 to 2011 will get diabetes in their lifetimes, with the percentages for African-American women and Hispanics placed even higher at 50% (Gregg et al. 2014). While researchers and industry participants agree on the health dangers of sugar, and in particular sugar-sweetened beverages (SSB), there is disagreement on how to design laws and policies to change behavior. According to the Center for Disease Control (CDC), SSBs are defined as drinks with added sugar, which includes sweeteners like brown sugar, raw sugar, and corn syrup, among others.[1] Policy proposals to address SSB consumption include bans (James et al. 2004; Fernandes 2008; Huang and Kiesel 2012), taxes (Brownell and Frieden 2009), nutrition education programs (James et al. 2004; Fernandes 2008), and warning labels on SSBs advising the dangers of obesity, diabetes, and tooth decay (Roberto et al. 2016). This raises the empirical questions: how do consumers react to such policies and are there differences between direct regulations and informational campaigns? This paper examines how consumers alter their purchasing behavior due to the campaign attention and election outcome of a local excise tax aimed at curbing SSB consumption.

We take advantage of a tax policy change—referred to as Measure D—in the city of Berkeley, California. Measure D imposes a penny-per-fluid-ounce tax to be paid by distributors of SSBs. The aim of the policy is to lower the consumption of SSBs, or if demand is deemed to be unresponsive,[2] to raise tax revenues which could fund nutritional programs

[1] Added sugar is distinctly different from naturally-occurring sugar, and a beverage with naturally-occurring sugar is not classified as a SSB.

[2] There is suggestive evidence that in the first month of the tax, tax revenues increased by $116,000, which is consistent with demand having not responded in an elastic fashion to the one-cent-per-ounce increase in price (*The Daily Californian*. "1st Month of Berkeley 'Soda Tax' Sees $116,000 in Revenue," May 19, 2015.

and education. On November 4, 2014, Measure D was put to a vote and passed with 75% of voters in favor. An aggressive campaign war preceded this vote, dubbed "Berkeley vs. Big Soda." This campaign cost $3.4 million, with roughly $1 million spent in favor of Measure D and $2.4 million spent against it.[3]

The specific objective of this paper is to examine how consumers reacted to the Measure D media campaign and to the election outcome. There is evidence suggesting that highlighted news coverage can lead to sharp information updates (Huberman and Regev, 2001; Lusk, 2010), and local elections can publicly reveal to individuals previously unknown information about the preferences of their neighbors and peers (Goldstein et al., 2008). Investigating whether campaigns and elections also lead to behavioral changes—whether through information or social norm channels—has important policy implications about how and where SSB policies will be effective in altering SSB consumption. This is especially true if campaigns and elections cause behavioral changes that happen before the policy implementation.

Our study employs detailed data from university retailers in Berkeley, consisting of monthly beverage sales. We use a difference-in-differences (DID) strategy to measure the change in ounces of soda purchased against untreated products (i.e., comparable control beverages) and untreated months (i.e., the pre-campaign period).[4] Additionally, we estimate an event study model to test the identifying assumption of parallel trends in the pre-campaign period. We verify that soda would have evolved in the same trend as other beverage products had there not been a tax campaign or affirmative election outcome.

There are two major advantages of using this empirical setting for our research design. First, products offered, as well as the promotional effort and posted prices, are uniform across

Online, *accessed May 21, 2016*).

[3] *Berkeleyside.* "Around $3.4M spent on Berkeley soda tax campaign," Feb. 5, 2015. Online, *accessed May 21, 2016*.

[4] We focus on soda instead of SSB products more broadly because the media campaign focused on soda.

campus retail locations. Second, we know exactly when and by how much the SSB tax is passed on to consumers, and do not have to infer the pass-through from the data. Since SSB taxes are often levied on the distributors of SSBs—who have a choice on how much of the tax they will pass on to consumers[5]—there is an empirical literature asking who bears the SSB tax burden.[6] In the university setting, we know exactly when and by how much the campus retailer adjusts prices. In particular, due to the costs of changing prices, campus retailers chose not to pass-through the tax to consumers for a year after receiving the tax invoices. Thus, we are able to look at how soda demand changes on-campus when the prices off-campus react to the tax implementation, yet the prices on-campus remain unchanged.

Our primary findings reveal no significant difference in on-campus retail soda sales compared to control beverage groups during the campaign period before the election (July 2014-October 2014). Conversely, soda sales fell significantly compared to control beverage groups in the period immediately following the election (November 2014-February 2015), decreasing by between 10-20% compared to pre-campaign levels. We also find that on-campus soda sales continued to fall when the tax was implemented in the city but not on campus (March 2015-July 2016)—decreasing by 18-36% compared to the pre-campaign period—and remained at this depressed level after the tax implementation on campus (August 2016-December 2016).

[5]One reason to tax distributors instead of customers is to make the price change more salient. There is a growing literature providing empirical evidence that consumers have an attenuated response to non-salient costs. With a labeling experiment, Chetty et al. (2009) find that the sales of taxable products at a grocery store are reduced when their tax-inclusive price is displayed in addition to the tax-exclusive price. Thus by taxing distributors of SSBs, if the tax is passed on to consumers, this will affect the displayed price and be more salient than a tax at the point-of-sale.

[6]Cawley and Frisvold (2015) and Falbe et al. (2015) examine the incidence effects of the Berkeley soda tax and both studies find that roughly half of the tax was passed on to consumers four to five months after the election. Grogger (2017) estimates the incidence of a sugary drink tax in Mexico and finds more than full price pass-through of the tax for sugary drinks.

Additionally, we find evidence that consumers substituted towards diet beverages as a result of the election outcome.

It is important to note that the university retailers in our analysis are not representative of the average U.S. retail outlet, especially in terms of clientele, and this could have large implications for whether our results will generalize to other locations. For this reason, we supplement the on-campus analysis with an analysis of beverage sales off-campus—at drug stores in the city of Berkeley and eight comparable cities with University of California campuses. Using retail scanner data, we estimate a triple-difference model measuring the change in soda sales in Berkeley during the campaign and election periods relative to untreated beverage products, untreated cities, and the pre-campaign period. The results of this analysis show that the drop in soda sales starting after the election was not unique to campus retailers.

Our paper fits into a growing literature informing policymakers about the potential impacts of SSB taxes.[7] Silver et al. (2017) use weekly panel-level scanner data from two supermarket chains in Berkeley and adjacent cities and find soda consumption fell by 9.6% in Berkeley stores, but rose 6.9% in non-Berkeley stores after the tax. We expand on this study by providing campaign- and election-specific treatment effects in addition to post-tax treatment effects on sales. Additionally, in our drug store analysis, we do not use adjacent cities as control cities, since they could have been affected by the media campaign around the election, confounding the results. Another related study, Falbe et al. (2016), uses survey-based evidence on SSB consumption—comparing the responses of survey participants in Berkeley to survey participants in neighboring Oakland and San Francisco. Falbe et al. (2016) estimate that the quantity of SSBs purchased in Berkeley dropped by 21%. We

[7] Please see Cornelsen and Smith (2018) for a recent review of the literature on ex-post soda tax evaluations, and Paarlberg et al. (2017) for a discussion of the spread of local SSB excise taxes in the U.S.

extend their analysis by using actual purchase data, rather than stated consumption levels, which could be biased. Furthermore, Falbe et al. (2016) conduct the surveys in two separate blocks of time—before the campaign and after the tax implementation—and thus they cannot distinguish between the election's effect and the tax's effect on SSB consumption.

A third related study is Debnam (2017), which uses the Nielsen© Homescan Consumer Panel instead of retail scanner data, to analyze the effect of Measure D on soda purchases. An important contribution of Debnam (2017) is the ability to study consumer heterogeneity, and in particular high- and low-SSB consumption households. A drawback is that the location of households is limited to the county level, so the author uses all households in Alameda County as the treated units. However, Berkeley comprises less than 8% of the population of Alameda County, and there is no way to guarantee the sampled households are located in Berkeley, especially since the Nielsen Homescan sample is designed to be nationally representative and not necessarily representative at the county level. Debnam (2017) finds that high-consuming households living in Alameda County *increase* their weekly SSB consumption by 7.41 ounces relative to other U.S. households. Similar to our results, the change occurs after the election, before the tax implementation. Given we find a significant decrease in soda sales at on- and off-campus retailers in Berkeley, our results together with Debnam (2017) and Silver et al. (2017), suggest more work needs to be done to understand border shopping behavior and spillover effects.[8]

The rest of the paper proceeds as follows. Section II reviews the literature on potential

[8] There have also been several studies examining SSB taxes outside of Berkeley. In the context of the U.S., Fletcher et al. (2010) use the variation in soda taxes across states and estimate that a one percentage point increase in the soda tax implies a reduction of 6 soda-calories per day, accounting for 5% of daily caloric intake from soft drinks. Colchero et al. (2016), Colchero et al. (2017), and Aguilar et al. (2017) examine the effects of a countrywide sugary drink tax in Mexico and find a 6–9% reduction in demand for sugary drinks compared to untaxed products.

mechanisms behind policy-induced behavioral change. Section III describes the setting and summarizes the data, while Section IV outlines the empirical design (i.e. the DID and event study strategies). Section V presents the results from the analysis of the on-campus data while section VI presents the results using off-campus data. Section VII discusses policy implications and future research.

## II Literature Review

### *A Mechanisms Behind Behavioral Change*

While our results will suggest the election caused a change in purchasing behavior well before the tax led to a price increase (both on- and off-campus), this paper—similar to other natural experiments—cannot distinguish the exact mechanism behind these changes (e.g., media information effects, rational addiction effects, and social norms effects). First, our results are consistent with models where consumers update their beliefs and behaviors based on information provided by the media and by advisory campaigns. Several studies show that new information about food-related health problems, food-safety, and animal-safety can alter preferences and consumer demand (Chavas 1983; Brown and Schrader 1990; Van Ravenswaay and Hoehn 1991; Yen and Jensen 1996; Schlenker and Villas-Boas 2009; Lusk 2010). The approach of our analysis is particularly close to Lusk (2010), who uses scanner data to examine how consumer demand for eggs changed in the months leading up a statewide election on whether to bar the use of cages in California egg production. The author finds that demand for the types of eggs associated with higher animal welfare standards increased over time in response to articles on the vote, whereas demand for other types of eggs fell.

Second, our results are consistent with models of rational addiction (Becker and Murphy, 1988), which model consumption of addictive products as a function of past and future prices, and where permanent price changes can curb addictive behavior for a product. The rational addiction model has been applied to many common consumer goods, including coffee (Olekalns and Bardsley 1996), alcohol (Waters and Sloan 1995), and cigarettes (Chaloupka 1991). Gruber and Köszegi (2001) study consumers addicted to cigarettes using a rational addiction model with time-inconsistent preferences and find that excise tax legislation that has been enacted but not yet implemented can cause immediate behavioral changes, instead of behavioral changes that only result once the tax is actually put into place. With the case of the SSB legislation in Berkeley, the effect we witness may be the result of forward-looking consumers adjusting their behavior as soon as the election outcome was reached, knowing that the SSB tax would go into effect in the near future.

Third, the results in this paper are consistent with the rich literature on peer effects and social norms, and how these effects may lead to changes in consumption behavior (Rosenquist et al. 2010; Christakis and Fowler 2008; Allcott 2011). The Measure D election revealed that 75% of Berkeley voters were in favor of a SSB tax. As many university consumers are not originally from Berkeley, the election may have revealed new information that was not previously known about the social norms of peers and neighbors in Berkeley. Social norms have been found to have the greatest effect when the comparison group is most similar to the treated individual (Goldstein et al., 2008). Thus, we might expect local elections to have stronger social norm effects than state and national elections. In a university setting, peer communication can be a large influence in the way norms spread among groups, especially with respect to "sin" products (Real and Rimal, 2007; Kremer and Levy, 2008). Thus, it may be the case that university students and staff are more susceptible than other populations to social norms about products deemed to be unhealthy.

## III Empirical Setting and Data

### *A Background on Measure D*

Since 2009, the soda industry has spent more than $117 million to stop soda tax initiatives in the U.S., such as those considered by the U.S. Congress and in states such as Maine, Texas, and New York.[9] For Berkeley's Measure D in particular, the American Beverage Association of California contributed almost $2.5 million to defeat the tax, while supporters of Measure D spent just under $1 million.[10] One of the strongest supporters of Measure D—"Berkeley vs. Big Soda"—gathered industry, individual, and lawmaker support and funded an aggressive advertising campaign promoting "Yes on D" and emphasizing the need to fight "Big Soda." While the SSB tax in Measure D affects all beverages containing added sugar at a rate of $0.01 per ounce,[11] the City of Berkeley and the media paid particular attention to soda, rather than SSB products in general (see Figure A.1 in the Appendix for examples). For instance, we found that a vast majority of popular articles and op-eds written on Measure D in Berkeley refer to a soda tax rather than a sugar-sweetened beverage tax.[12] Thus, we will look at the effects of the campaign war and election on regular soda separately from other SSB products.

Given that time series data on campaign expenditures are not available, we investigate

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[9] *The New York Times*. "Berkeley Officials Outspent but Optimistic in Battle Over Soda Tax." Oct. 7, 2014. Online, *accessed May 21, 2016*.

[10] *Berkeleyside*. "Around $3.4M Spent on Berkeley Soda Tax Campaign," Feb. 5, 2015. Online, *accessed May 21, 2016*.

[11] Official legislation regarding which beverages fall under the tax and which beverages are exempt can be found Online, *accessed February 21, 2018*.

[12] Of the 34 articles on Measure D archived at *Berkeleyside*, an independent and reputable news site that reports on Berkeley and the East Bay, 32 articles refer to the tax as a "soda tax" (or Berkeley taking on "Big Soda") while 2 articles refer to it as a "sugar tax."

the intensity of the campaign over time by examining web search data. Figure 1 depicts Google Trends data for web searches of the terms "soda tax", "sugar tax", and "beverage tax" in the San Francisco-Oakland-San Jose area in the weeks before and after the election.[13] Numbers on the y-axis represent search interest relative to the highest point on the chart for the given region and time. A value of 100 is the peak popularity for a term. A value of 50 means that a term is half as popular. Likewise a score of 0 means a term was less than 1% as popular as the peak. Figure 1 shows that the relative search interest for "soda tax" reached 5% in July after the election was first announced. It grew to 7% and 17% in September and October respectively, suggesting the campaign war led to increased awareness of a potential soda tax. Web searches then spiked to 100% in early November, after Measure D was voted on and passed into law. The interested reader can adjust the Google Trends query dates closer to the election in order to see that the web search spike occurred on November 5, the day after the election. Conversely, when analyzing search trends for the terms "sugar tax" and "beverage tax," we find only modest increases in search interest for these terms around the election, evidence that attention was focused on soda rather than SSBs more broadly.[14]

This increased search interest after the election may have several explanations: (1) voters searching for the outcome of the election, (2) prominent national and local news coverage leading to more searches, as Berkeley historically became the first city in the U.S. to pass a SSB tax, and (3) a delay in exposure to campaign information and searching for more details on the tax. Overall, Figure 1 indicates that the campaign did raise some interest in soda taxes in the two months before the election, however, this is dwarfed by interest shown after the election outcome.

[13]Source: Google Trends. Online, *accessed February 13, 2018.*

[14]We also examine Google Trends data for web searches of the terms "sugar-sweetened beverage" and "sugar-sweetened beverage tax" in the same region and time period. We find zero interest in these alternate phrases.

## B *University Retail Data*

We use a unique data source to estimate the effect of a media campaign and election on consumer purchasing decisions: a retail dataset from dining locations at the University of California, Berkeley. This dataset includes monthly data on the total quantities sold and revenue sales at the product level—i.e., campus sold $x$ ounces of product $z$ in month $m$, where a product is represented by a unique bar-code. The dataset includes all beverage products for the period January 2013 through December 2016. We categorize products into eight product groups: 1) soda, 2) water, 3) juice, 4) energy drinks, 5) milk, 6) coffee, 7) tea, and 8) diet drinks. We focus on beverage products in order to have a common unit of analysis—fluid ounces.

While the university retailers in our empirical analysis may not be representative of the average U.S. food outlet, there are several advantages of using this empirical setting for our experimental design. First, we have strong institutional knowledge of our setting. Our data come from on-campus retailers, which are open to all people on campus and do not include residential dining halls. Beverages are sold à la carte with individual product prices posted (i.e., drink prices are not hidden in the price of a meal). The products offered, promotional effort, and posted prices are uniform across campus locations, which would otherwise be a concern because we only have aggregate campus retail sales and not sales by individual campus locations. Customers can use cash, credit and debit cards, and university ID cards loaded with "meal points" to make purchases. However, our data does not include information on payment type or customer identifiers, and thus we cannot track customers over time or look at customer heterogeneity. Second, we know exactly when and by how much the soda tax is passed on to consumer, as told to us directly by the campus retail staff. Unlike other studies (i.e., Cawley and Frisvold 2015; Falbe et al. 2015; and Silver et al.

2017), we do not have to infer tax pass-through from observing shelf prices or using scanner data. Third, the university is an important, yet previously unstudied, retailer in the context of Berkeley and Measure D, with the student population more than a third the size of the city population.[15]

We define soda as the treated product category, which we will compare to the seven other beverage groups. It is important to note that some of our beverage categories, aside from soda, may include products that fall under the regulation (in particular, juice, energy drinks, coffee, and tea). Given the wording of Measure D ("The City hereby levies a tax of one cent ($0.01) per fluid ounce on the privilege of distributing sugar-sweetened beverage products in the city"), any drinks with added sweeteners are taxed.[16] So for example, 100% juices are not taxed, but juices with sugar or corn syrup added are taxed. In an attempt to distinguish regulated beverages from unregulated beverages, we group products into the separate diet drinks category if they have "diet", "low-calorie", "zero-calorie", or "unsweetened" in their name. While this works well for categorizing soda and energy drinks into taxed and untaxed, it does a poor job for juices—which tend not to distinguish between natural sugars and added sugars—and for tea and coffee drinks—which do not always have the sugar content in the name, nor do we know if taxed syrups were added.[17] Thus the juice, tea, and coffee categories

[15] Source: According the U.S. Census Bureau, the population of Berkeley was 121,240 in 2016. There were 40,173 students enrolled at UC Berkeley in 2016-2017 (Office of Planning and Analysis, UC Berkeley. Online, *accessed Febrary 21, 2018*).

[16] The following beverage products are taxed: regular soda, sport and energy drinks, sweetened tea, and lemonade. Exempted are the following: water (without added sugar), diet drinks (drinks sweetened with zero/low-calorie sweeteners), beverages containing only natural fruit and vegetable juice, beverages in which milk is the primary ingredient, beverages or liquids sold for purposes of weight reduction as a meal replacement, medical beverages (used as oral nutritional therapy or oral rehydration electrolyte solutions for infants and children), and alcoholic beverages, although the last two categories are not sold on campus.

[17] Unfortunately, we do not have information on which juices have natural or added sugar.

may contain both regulated and unregulated products.

The substantial amount of advertising and campaigning directed at soda, and not sugar-sweetened-beverages, may have affected consumption of soda differently from other SSBs. For this reason, we focus first on soda sales as the treated product; however, we we also examine the effect of the soda tax election on other beverages, namely, energy drinks (another SSB) and diet drinks (a substitute for SSBs).

### *C Summary Statistics*

We use the pre-campaign period data to investigate pre-existing trends in demand for soda versus the control beverage groups. Table 1 presents the average monthly ounces sold by beverage group in the academic year before the campaign (August 2013 to July 2014). Figure 2(a) unpacks these averages and plots the monthly ounces sold by beverage group over time, both before and after the campaign. The highest selling categories in the pre-campaign period are juice, water, energy drinks, followed by soda and coffee. Milk, tea, and diet drinks experience the lowest levels of sales. While the various products differ in levels, their seasonal patterns are quite similar, with sales peaking in April—the weeks leading up to final exams—and plummeting in June—after the Spring semester ends. While soda has different quantities sold than the other products, to the extent that these differences are constant over time, product-group fixed effects will control for all possible time-invariant determinants of beverage sales, and month fixed effects will control for seasonality in sales. Additionally, Figure 2(b) plots the linear trend in average monthly sales by product category, both before and after the start of the campaign. Before the campaign, all categories share similar trends, with the exception of coffee.[18] For this reason, we will examine the sensitivity

[18] As a more rigorous test of parallel trends, we regress quantity sold on a time trend interacted with the eight products. We find that the point estimates of the product trends are not statistically different from each

of our results with and without the inclusion of coffee. After the campaign, juice, water, tea, and milk remain on similar trends as before the campaign while soda and energy drinks experience a decline in trends and diet drinks and coffee experience an increase in trends. This is consistent with consumers substituting coffee and diet drinks for soda and energy drinks after the soda tax campaign and election. While these figures are visually suggestive, we will test this relationship formally in our regression analysis. As a final descriptive statistic, Table 1 also presents the average price per ounce by beverage group. Water, soda, tea, and diet drinks are between 8 and 12 cents per ounce while energy drinks, juice, milk, and coffee as between 19 and 27 cents per ounce.

In evaluating the effects of the soda tax campaign, we will compare the pre-campaign period to four separate post-campaign periods: (1) the pre-election campaign period—July 2014-October 2014, (2) the post-election and pre-tax implementation period—November 2014-February 2015, (3) the tax implementation period in City of Berkeley but not on campus—March 2015-July 2016, and (4) the tax implementation period on campus—August 2016-December 2016. It is important to note here that while the City of Berkeley implemented the SSB tax in March 2015, campus retailers did not start receiving the SSB tax on invoices from their vendor until August 2015, and did not pass the tax on to consumers in any form until August 2016.[19] Furthermore, when prices increased on campus, they increased by roughly a penny per ounce for all beverage groups, except water and milk which have no added sugar and are exempted by the SSB tax. Interestingly, diet drinks saw the same price increase as soda (i.e., the price of Diet Pepsi and Pepsi both increased by one cent per ounce). The tax is set up such that it is paid by the distributor, who may or may not pass the cost

other, again with the exception of coffee. Furthermore, the time series correlation of the sample averages of soda and the other products is high, suggesting that the products share broadly similar time varying patterns in the pre-campaign period.

[19]This was reported to us by campus retail staff and confirmed in the data.

on to their consumers. Both Falbe et al. (2015) and Cawley and Frisvold (2015)—who examine prices at non-campus retailers in the City of Berkeley—find incomplete pass through of Berkeley's SSB tax on to consumers three months after the policy implementation, with roughly half of the tax passed through. In our setting, campus food and beverage prices are sticky and only change once per year, occurring during the summer months of June, July, or August. Since the tax was not passed through to consumers on campus for almost two years after the campaign, this paper examines how the soda tax campaign, election, and increase in prices off-campus affect the sales of soda on-campus.

## IV Empirical Strategy

Our approach has two parts. First, we use a difference-in-differences (DID) strategy to measure the change in soda sales due to the soda tax campaign and election. Secondly, we estimate an event study model to test the identifying assumption of the DID model, namely that soda sales would have continued on the same trend as the other products had it not been for the campaign and election.

### *A Difference-in-Differences Model*

The DID model compares purchases of soda (i.e., the treated category) with purchases of the seven other beverage groups (i.e., the control categories), in the four policy periods. Using data from January 2013 through December 2016, we compare the pre-campaign period to four subsequent periods: (1) pre-election campaign, (2) post-election and pre-policy implementation, (3) post-policy implementation in the City of Berkeley, and (4) post-policy implementation on campus. For shorthand, we refer to these periods as: pre-campaign, campaign, post-election, post-city, and post-campus. By comparing the soda purchase behavior in the pre-period to each of these policy periods, we attempt to distinguish the effects of the campaign from the effects of the election and the effects of prices increasing off- and on-campus. The DID model specification is as follows:

$$
\begin{aligned}
Q_{im} = {} & \beta_1(\text{Soda*Campaign})_{im} + \beta_2(\text{Soda*PostElection})_{im} + \beta_3(\text{Soda*PostCity})_{im} \\
& + \beta_4(\text{Soda*PostCampus})_{im} + \alpha_i + \alpha_m + \epsilon_{im}. \qquad (1)
\end{aligned}
$$

where $Q_{im}$ is the quantity sold (measured in ounces) of beverage group $i$ in month-of-sample $m$. We estimate equation (1) with quantities both in levels and in logs (i.e., $Q_{im}$ and $ln(Q_{im})$). $Soda_i$ is an indicator for beverage group $i$ being in the treated soda group. Four time indicators—$Campaign_m$, $PostElection_m$, $PostCity_m$, and $PostCampus_m$—define the four policy periods. Finally, we include fixed effects for the eight product groups $\alpha_i$ and for the month-of-sample $\alpha_m$. It should be noted that price is not included in this estimating equation due to endogeneity concerns. Moreover, since prices are adjusted only once every year in either June, July, or August, the month-of-sample fixed effects pick up much of the price variation that may have biased results.

The coefficients of interest are those on the interactions of $Soda_i$ and the policy periods. The coefficient for $Soda * Campaign_{im}$ is the effect of the campaign on soda sales relative to the control product categories, the coefficient on $Soda * PostElection_{im}$ is the effect of the election, the coefficient on $Soda * PostCity_{im}$ is the effect of the SSB tax change in the city of Berkeley, and the coefficient on $Soda * PostCampus_{im}$ is the effect of the SSB tax change on campus.

## *B Event Study Model*

The identifying assumption of the DID model is that of parallel trends, where soda sales would have continued on the same trend as the other product groups had it not been for the campaign, election, and tax implementation. To directly test this assumption, we complement the DID model with the following event study model:

$$Q_{imt} = \sum_{t=-5}^{7} \beta_t D_{t,im} + \alpha_i + \alpha_m + \epsilon_{imt} \quad (2)$$

where $D_{t,im}$ is a dummy variable equaling one if product group $i$ is in the soda product group and month-of-sample $m$ is $t$ time periods from the election. Once again, we include fixed effects for the eight product groups $\alpha_i$ and for the month-of-sample $\alpha_m$. The time periods $t$ are four month intervals centered at the election (i.e., t=0 is Nov 2014 to Feb 2015). We omit the period immediately preceding the election (t=-1) to avoid perfect collinearity. Thus, equation (2) is the same as equation (1), except instead of splitting the sample into 5 periods of unequal length, we instead compare soda sales to the untreated products in every 4-month interval of the sample. The $\beta_t$ vector contains the coefficients of interest, which we plot over time to trace out the adjustment path from before the soda tax campaign through the election and policy implementations. Importantly, if soda is trending parallel to the control products before the policy periods, there should be no trend in the $\beta_t$ coefficients in the pre-campaign period and they should be statistically indistinguishable from zero.

### *C Estimation Concerns*

An important potential limitation of our analysis is that any beverage could be a substitute for soda. For example, diet drinks sales may increase due to the Measure D election as regular soda sales decrease. Since we are examining soda sales relative to the other beverage groups, an increase in sales of beverages in one of the other beverage categories would bias our estimates in the same direction as a drop in soda sales. In other words, our effects would be biased in the correct direction but they would be biased larger in magnitude. To address this concern, we estimate equation (1) seven times, excluding one of the other beverage groups each time, in order to evaluate whether substitution towards one of the other products is biasing our results. In this way, we are able to gain some clarity on whether consumers are substituting towards certain beverage products more than others as a result of the election. However, while having a potential substitute as a control may create an upward bias in our treatment effect, it should not bias the timing of when the effect occurs, which is one of our main research objectives.

A second limitation of the university data is that they do not contain a comparison location unaffected by the soda tax campaign and election. To address this limitation, we supplement our campus analysis with an analysis of beverage sales at drug stores in Berkeley and eight comparable cities with University of California campuses. With these data, we measure the change in soda sales in Berkeley during the campaign and election periods relative to untreated beverage products, untreated cities, and the pre-campaign period. This analysis provides evidence that the drop in soda sales starting after the election was not unique to campus retailers.

## V Results

### *A Effect of the Soda Tax Campaign on Soda Purchases (Campus Retail Analysis)*

We present the results from the reduced form specification of equation (1) in Table 2, where the dependent variable is the quantity sold (in ounces) of product group $i$ and month-of-sample $m$, in levels (column 1) and in logs (column 2). The parameters of interest are the four interactions of the soda indicator and the policy period indicators. Standard errors are clustered at the product group by academic year level, to account for the possibility that the errors are correlated within a given product group and academic year, but not across product groups or years.[20]

There are three main takeaways from Table 2. First, in both columns the coefficients on the *Campaign* interaction are positive, small in magnitude, and are not statistically different from zero. This suggests the campaign did not alter soda sales, on average, relative to the control beverage groups. We acknowledge that consumer heterogeneity may lead to a statistically insignificant average effect—where decreased consumption by consumers relating more to the "Yes" side of Measure D could cancel increased consumption of consumers relating more to the "No" side. However, without customer level data, we can say little about heterogeneous effects. Second, the coefficients on the other three interactions are negative, much larger in magnitude, and statistically different from zero at the 10% significance level, with the exception of the *Post-Election* interaction in column (1). Moreover, the coefficients on the *Post-City* and *Post-Campus* interactions are nearly double the coefficients for the *Post-Election* interaction. Translating the coefficients into percent changes shows that soda sales were 10-20% lower post-election compared to pre-campaign and 18-36% lower post-tax implementation compared to pre-campaign.[21] These results suggest that soda sales began to deviate below the sales of the control beverage groups after the election and this decrease continued through the tax implementation periods. Third, the coefficients on the *Post-City* and *Post-Campus* interactions are nearly equal to one another, suggesting that the price changes that occurred on-campus almost two years after the election did not lead to any additional decreases in sales.

[20] We use the academic year instead of the calendar year since campus retail product and pricing decisions are made at the beginning of the academic year.

[21] In column 1, the mean of the dependent variable is 112,376 ounces per month, thus a coefficient of -11,172 on $Soda \times PostElection$ translates to a 10% decrease in quantity sold. In column 2, the percent change in the dependent variable can be found using $100 \times (\exp^{\beta_1} - 1)$, thus the coefficient of $-0.433$ on $Soda \times PostCampus$ translates to 36% decrease in quantity sold.

To understand whether the use of the other beverage groups as controls for soda is biasing our results away from zero, we estimate equation (1) seven times, excluding one of the control beverage groups each time. Comparing column (2) in Table 2 to each of the columns of Table 3, the only beverage group when excluded that alters the results is diet drinks (shown in column 2). In particular, while the coefficients estimated excluding diet drinks follow the same sign and pattern as the coefficients including diet drinks, the coefficients estimated without diet drinks are nearly half the size. This is an interesting result in and of itself, suggesting some consumers substituted diet drinks for regular soda after the election. Since these results raise support for the concern that diet drinks may not be a valid control for soda, the specification in column (2) of Table 3 is our preferred specification.[22] Translating the coefficients in column (2) into percent changes, soda sales relative to the remaining six beverage groups were 9% lower post-election compared to pre-campaign and 23-24% lower post-tax implementation compared to pre-campaign.

Thus far we have focused on soda, since soda was the target of the election campaign. However, Figure 2 suggests other beverage categories were affected by the soda tax election—in particular, energy drinks and diet drinks. In Table 4, we estimate the effects of the soda tax campaign on soda, energy drinks, and diet drinks relative to one control category, water. We use water as a comparison because there should be little confusion about whether water is taxed (unlike juice, tea, coffee, and milk) and because trends in water sales are not statistically different from soda, energy drinks, and diet drinks in the pre-campaign period (as shown in Figure 2). Columns (1) and (2) of Table 4 show that soda and energy drinks experience similar declines in sales (relative to water) after the election but not during the campaign period. Therefore, in column (3) we jointly compare soda and energy drinks to water and find that sales of these SSB drinks fell by 24.9% after the election compared to

[22]We replicate Table 2 without diet drinks in Appendix Table A.1.

the pre-campaign period. Sales dropped another 10 percentage points after the tax was implemented in the city and another 10 percentage points after the tax was implemented on campus. Conversely, column (4) shows that diet drink sales increased relative to water in all four treatment periods. For instance, diet drink sales are 77.5% greater in the post-election period than the pre-campaign period. This, once again, suggests some customers substituted diet drinks for SSBs.

In summary, even though the tax was not implemented on campus during the *Post-Election* and *Post-Policy City* periods, we find consumers purchased less soda relative to the other beverage groups. The result in the *Post-Policy City* period is particularly surprising, given soda on-campus would have been relatively cheaper when prices increased off-campus. Our results are consistent with the election causing consumers to update their beliefs and change behavior. In particular, the election revealed a social norm that 75% of people in Berkeley were in favor of the SSB tax. If, instead, sales fell due to rational addiction, two years is a long time for sales to remain depressed without a change in price. Changes in purchasing behavior did not occur during the $3.4 million campaign period; however, we cannot rule out that delays in receiving campaign information led to changes in consumption after the election or that a lack of average effects during the campaign period reflects the consumption changes from the opposing sides of the campaign canceling each other.

### *B Event Study Results*

Given the interesting patterns we find in the DID results, we next explore the parallel trends assumption and the dynamics of the treatment effects over time using our event study model. Figure 3 plots the estimates we obtain from equation (2), excluding diet drinks, with the $\beta_t$ plotted in black and the 95 percent confidence intervals plotted in gray. Standard errors are once again clustered at the product group by academic year level. Vertical red lines separate the sample into the four treatment periods. The omitted dummy is $D_{-1}$, which corresponds to the four month interval of the campaign period.

In the periods before the election, we find parallel trends, with each of the $\beta_t$ not statistically different from zero at the 95% significance level. After the election in November 2014, the $\beta_t$ estimates begin to decline, indicating that soda sales dropped relative to the control beverage groups. By a year after the election, the $\beta_t$ are no longer declining, but are at a constant level significantly lower than the pre-campaign period. These event study results suggest that the decline in soda sales on-campus relative to the other beverage categories began after the election, and that sales of soda remained depressed after the tax was implemented off- and on-campus. The event study results also provide evidence against the alternative hypothesis that slowly declining preferences for soda, rather than the campaign and election, are driving the decreases in consumption described. In particular, there are no downward trends in the pre-campaign period to suggest the results are driven by declining preferences for soda. Instead, the downward trend in sales begins only after the election and stabilizes by a year later.

## VI Supplemental Analysis Using Nielsen Scanner Data

While the university retail data has the benefit of institutional knowledge (i.e., we know exactly when the tax was implemented and the exact pass-through amount), a major drawback of these data is that they do not contain a comparison location unaffected by the soda tax campaign and election. To address this limitation, we supplement our campus analysis with an analysis of beverage sales at drug stores in Berkeley and eight comparable cities with University of California campuses. With these data, we extend the DID model above to a triple-difference model—measuring the change in soda sales in Berkeley during the campaign and election periods relative to untreated beverage products, untreated cities, and the pre-campaign period. This analysis provides evidence that the drop in soda sales starting after the election was not unique to campus retailers.

### *A Drug Store Scanner Data*

The drug store scanner data are collected by Nielsen© and made available through the Kilts Center at The University of Chicago Booth School of Business.[23] These data include weekly price and quantity information at the product-by-store level—i.e., store $j$ sold $x$ units of product $z$ in week $w$, where a product is represented by a universal product code (UPC)—from January 2012 through December 2015.[24]

While the Nielsen database includes several types of retail food outlets selling soda and other beverages (e.g. supermarkets, grocery stores, and mass merchandising stores, among others),[25] we focus on drug stores because these are the only stores in the sample we could uniquely identify as being located in Berkeley. This is because the scanner data do not contain the exact street address of each store in the sample; instead, the county and three-digit zip code of each store is provided. There are two cities in Alameda County and zip code 947 (Albany and Berkeley), and we select the five drug stores we can verify are in Berkeley and not Albany using the retailer codes provided.

Given our goal is to compare the drug store analysis to the campus analysis, we select

[23] Researchers own analyses calculated (or derived) based in part on data from The Nielsen Company (US), LLC and marketing databases provided through the Nielsen Datasets at the Kilts Center for Marketing Data Center at The University of Chicago Booth School of Business. The conclusions drawn from the Nielsen data are those of the researchers and do not reflect the views of Nielsen. Nielsen is not responsible for, had no role in, and was not involved in analyzing and preparing the results reported herein."

[24] At the time of this study, the Nielsen data span 2006 to 2015. Since our event of interest took place towards the end of the available data, we chose to subset the data from 2012 onwards for computational ease.

[25] The Nielsen data covers more than 50,000 individual stores in 90 participating retail chains across the entire United States.

control drug stores as those in counties and 3-digit zip codes containing one of the nine University of California campuses, other than UC Berkeley. Specifically, we use drug stores in the same counties and 3-digit zip codes as UC Davis, Irvine, Los Angeles, Merced, Riverside, San Diego, Santa Barbara, and Santa Cruz.[26] We do not use drug stores near UC San Francisco for three reasons: (1) UCSF does not have an undergraduate program, (2) UCSF persuaded every vendor on campus to stop selling SSBs in 2015, and (3) San Francisco had a SSB tax election at the same time as Berkeley which did not pass. The second two reasons would particularly confound changes in soda sales. We decided to restrict this analysis to locations within California, since they will be identical in terms of state policies that may affect consumption of beverages differently across states.

Finally, we select data for product groups similar to the ones used in the campus analysis—soda, water, coffee, tea, milk, and juice—and aggregate the week-store-upc data to the month-store-product-group level in order to match the level of analysis used with the campus data.[27] Thus in total we have 48 months, 80 stores (5 treated and 75 control), and six product groups (1 treated and 5 control).

Table 5 shows the average monthly ounces sold by beverage group per store, averaged across all stores in Berkeley and in the control cities, during 2012–2013.[28] The higher selling categories in this pre-campaign period are milk, water, juice, and soda, while the lower selling

[26]We use the following counties and 3-digit zip codes to select control stores: UCD = Yolo County and 956; UCI = Orange County and 926; UCLA = Los Angeles County and 900; UCM = Merced County and 953; UCR = Riverside County and 925; UCSD = San Diego County and 920; UCSB = Santa Barbara County and 931; UCSC = Santa Cruz County and 950.

[27]While we also have data for diet drinks, we choose not to use them in this analysis given the results in the previous section.

[28]The observations in Berkeley and the control cities reflect the number of stores multiplied by 24 months, since the summary statistics are calculated for 2012–2013 for each product group.

categories are tea and coffee. The stores in Berkeley sell more ounces per month across all beverage category than the control stores.

### B Drug Store Empirical Specification and Results

For the drug store analysis, we extend the DID models in equation (1) and (2) to include an additional dimension—store $s$, which is city-specific. We estimate the following triple-difference model:

$$Q_{ims} = \beta_1 (Soda \times Berkeley \times Campaign)_{ims} + \beta_2 (Soda \times Berkeley \times PostElection)_{ims} + \beta_3 (Soda \times Berkeley \times PostCity)_{ims} + \alpha_{im} + \alpha_{ms} + \alpha_{is} + \epsilon_{ims}. \tag{3}$$

where $Q_{ims}$ is now the quantity sold (measured in ounces) of beverage group $i$ in month-of-sample $m$ in store $s$, and the fixed effects are product group by month-of-sample ($\alpha_{im}$), month-of-sample by store ($\alpha_{ms}$), and product group by store ($\alpha_{is}$). The coefficients of interest are the interactions of $Soda_i$, $Berkeley_s$ and the three policy periods: $Campaign_m$, $PostElection_m$, and $PostCity_m$. There are only three policy periods in the drug store analysis because our sample ends in 2015, before the tax was implemented on campus. Similarly, we extend the event study equation (2) as follows:

$$Q_{imst} = \sum_{t=-8}^{4} \beta_t D_{t,ims} + \alpha_{im} + \alpha_{ms} + \alpha_{is} + \epsilon_{imst} \tag{4}$$

where $D_{t,ims}$ is now a dummy variable equaling one if product group $i$ is in the soda product group, store $s$ is in Berkeley, and month-of-sample $m$ is $t$ time periods from the election.

Table 6 and Figure 4 present the results of equations (3) and (4). There are three main takeaways from the drug store analysis. First, the results follow the same pattern as the campus analysis in that there is no statistically significant change in soda sales during the campaign period, there is a significant drop in soda sales in the post-election period, and this drop in sales continues into the post-city period. Second, the magnitude of the effects using the drug store data are similar in size to the Berkeley campus data analysis excluding diet drinks. For instance, the coefficient on the post-election interaction is -0.108 in Table 6 and -0.094 in Table 3 column (2). This suggests the lack of control cities in the campus analysis is not biasing the results away from zero when diet drinks are excluded. Third, converse to the campus analysis, the drug store results show the decrease in soda sales might have begun in the campaign period; however this decrease is not statistically significant in either Table 6 or Figure 4.

## VII Discussion

This paper is motivated by the growing adoption of local excise taxes on sugar-sweetened beverages in the U.S. (e.g., Berkeley, San Francisco, Oakland, Boulder, Seattle, and Philadelphia). In particular, Berkeley made history by being the first city to vote and pass a soda tax in a local election. This paper uses a detailed scanner dataset in a university setting to measure the response of the soda tax campaign and election on soda sales. Our results show that soda purchases significantly drop relative to other beverage products immediately after the election, months before the tax is implemented in the city of Berkeley or on campus. Additionally, using scanner data from off-campus retailers in the same city as the campus, we find similar drops in soda sales after the election. Thus, our results are not unique to the university setting. Specifically, we find a 10.8% drop in off-campus sales in the period immediately following the election.

While other studies have examined the effects of the Berkeley SSB tax on beverage sales (Falbe et al., 2016; Silver et al., 2017; Debnam, 2017), this study is unique across multiple dimensions. First, we focus on an understudied yet important setting—university food retailers. This setting is especially important in the context of Berkeley because the student population is more than 1/3 the size the population of the city.[15] A second contribution of this paper is that instead of a simple pre- and post-policy comparison, we explore changes in purchasing behavior during several periods before and after the election (e.g., campaign, post-election, post-implementation in the City of Berkeley, and post-implementation on campus), in an effort to disentangle the mechanisms behind the behavioral changes. Our results show that soda sales fell on-campus after the SSB tax election yet before prices changed due to the tax. This suggests that comparing pre-campaign to post-implementation sales may confound a price elasticity effect with media and social norm effects. Finally, we provide evidence that consumers substituted towards diet drinks. This is particularly interesting given prices on-campus changed for diet soda in the same fashion as regular soda.

An important policy implication of our study is that the effects of election campaigns and outcomes on behaviors may be larger than the effects of the policy itself. In the university setting, we find that sales dropped 10-20% in the post-election period compared to the pre-campaign period. Sales dropped another 8-16% when the policy was implemented off-campus, but did not fall any further when the policy was implemented on-campus. Thus, the change in soda sales post-election but pre-implementation was similar (if not slightly larger) to the change in soda sales post-implementation. These results are consistent with findings from other highly publicized elections. For instance, in the context of standards in egg production, Lusk (2010) finds that the publicity surrounding a vote to pass a proposition pertaining to animal welfare *in itself* had a significant impact on consumer behavior, beyond the effect the policy had once implemented.

The Berkeley tax differs from other soda taxes in several ways, which could have important implications for comparing the results in Berkeley to SSB taxes in other jurisdictions. In particular, it was voted on and passed by the people of Berkeley, and there was an extensive campaign to inform voters about the tax. Conversely, when the Mexican government announced their SSB tax in September 2013, it took the soda industry and the public by surprise, according to media reports.[29] If the Berkeley SSB tax is replicated elsewhere without a proceeding campaign war and affirmative election outcome, its effects on sales may differ substantially. The Berkeley soda tax was also a local election, as opposed to statewide or countrywide election. If the social norm revealed by the election was the driving mechanism behind the consumption change, and not the information revealed by the media campaign, we might expect larger effects when consumers identify more closely with other voters. If instead, rational addiction was the main mechanism behind the reduction in soda consumption after the election, then the size of the jurisdiction, the level of media coverage, and whether the policy is implemented by direct democracy or republic may not matter for external validity.

This study scratches the surface with respect to the mechanisms behind the reduced soda demand. We can reject that SSB taxes only affect beverage demand through current price changes, but there is still much to uncover. We echo the sentiments of Cornelsen and Smith (2018) in that more needs to be done to understand the mechanisms behind the behavioral changes—especially the media, rational addiction, and social norm effects—and how these might vary across heterogeneous consumers. It may be that a combination of these mechanisms is at play for different types of consumers. We suggest three areas of future research. First, to address the limitations of natural experiments such as the one in this paper, laboratory experiments simulating elections could be designed to parse out information effects from social norm effects. Second, rational addiction models could be tested and compared across price changes specifically resulting from elections and price

[29] *The Guardian.* "How One of the Most Obese Countries on Earth Took on the Soda Giants," November 3, 2015. Online, *accessed February 21, 2018.*

changes resulting from other sources—for instance, an individual may be more likely to believe a tax will be permanent if the vast majority of people voted for it as opposed to the officials currently in power. Lastly, this paper highlights a need for additional research on border shopping behavior and substitution effects from SSB taxes.

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Table 1: Average Monthly Ounces Sold and Price per Ounce by Beverage Group (2013-2014 Academic Year)

| | Coffee<br>mean/sd | Diet<br>mean/sd | Energy<br>mean/sd | Juice<br>mean/sd | Milk<br>mean/sd | Soda<br>mean/sd | Tea<br>mean/sd | Water<br>mean/sd |
|---|---|---|---|---|---|---|---|---|
| Quantity Sold (Oz) | 123846.73 | 20977.97 | 167810.88 | 213262.79 | 72516.03 | 114172.37 | 69005.18 | 217998.35 |
| | (74436.02) | (11260.22) | (76218.38) | (109572.74) | (31448.30) | (56332.02) | (40961.09) | (108092.20) |
| Price per Oz ($) | 0.27 | 0.12 | 0.19 | 0.23 | 0.24 | 0.10 | 0.11 | 0.08 |
| | (0.03) | (0.02) | (0.02) | (0.01) | (0.04) | (0.01) | (0.01) | (0.00) |
| N | 12 | 12 | 12 | 12 | 12 | 12 | 12 | 12 |

Standard deviations in parentheses. The academic year begins August 2013 and ends July 2014.

Table 2: Difference-in-Difference: Effect of Soda Tax Campaign and Election on Campus Retail Soda Sales Relative to Other Beverage Products

| | (1)<br>Oz Sold | (2)<br>Log Oz Sold |
|---|---|---|
| Soda × Campaign | 3339.766 | 0.035 |
| | (12267.539) | (0.112) |
| Soda × Post-Election | -11172.373 | -0.227$^{*}$ |
| | (10735.197) | (0.126) |
| Soda × Post-Policy City | -19958.315$^{*}$ | -0.441$^{***}$ |
| | (10663.730) | (0.145) |
| Soda × Post-Policy Campus | -23253.053$^{*}$ | -0.443$^{**}$ |
| | (12644.855) | (0.168) |
| Mean of Dep. Variable | 112376.308 | 11.179 |
| Num of Obs. | 384 | 384 |
| R squared | 0.839 | 0.928 |
| Product Group FE | X | X |
| Month-of-Sample FE | X | X |

Standard errors in parentheses are clustered at the product group by academic year level. The outcome variable is ounces sold of product group $i$ in month $m$, in logs (column 1) and in levels (column 2). Asterisks indicate the following: $*p < 0.10$, $**p < 0.05$, $***p < 0.01$

Table 3: Robustness by Control Beverages: Effect of Soda Tax Campaign and Election on Campus Retail Soda Sales Relative to Other Beverage Products

| | (1) Excl. Coffee | (2) Excl. Diet | (3) Excl. Energy | (4) Excl. Juice | (5) Excl. Milk | (6) Excl. Tea | (7) Excl. Water |
|---|---|---|---|---|---|---|---|
| Soda × Campaign | -0.003 | 0.088 | 0.054 | 0.040 | 0.013 | 0.013 | 0.043 |
| | (0.100) | (0.091) | (0.135) | (0.127) | (0.122) | (0.116) | (0.125) |
| Soda × Post-Election | -0.242* | -0.094 | -0.264* | -0.208 | -0.305** | -0.250* | -0.223 |
| | (0.124) | (0.100) | (0.145) | (0.147) | (0.128) | (0.141) | (0.148) |
| Soda × Post-Policy City | -0.424*** | -0.266** | -0.463*** | -0.439** | -0.597*** | -0.470*** | -0.425** |
| | (0.151) | (0.116) | (0.169) | (0.170) | (0.135) | (0.163) | (0.169) |
| Soda × Post-Policy Campus | -0.382** | -0.277* | -0.515*** | -0.423** | -0.594*** | -0.477** | -0.433** |
| | (0.171) | (0.148) | (0.185) | (0.197) | (0.161) | (0.191) | (0.197) |
| Mean of Dep. Variable | 11.126 | 11.312 | 11.123 | 11.076 | 11.289 | 11.247 | 11.066 |
| Num of Obs. | 336 | 336 | 336 | 336 | 336 | 336 | 336 |
| R squared | 0.933 | 0.948 | 0.927 | 0.918 | 0.944 | 0.922 | 0.918 |
| Product Group FE | X | X | X | X | X | X | X |
| Month-of-Sample FE | X | X | X | X | X | X | X |

Standard errors in parentheses are clustered at the product group by academic year level. This table replicates column (2) of Table 2, with one of the seven control beverage groups excluded in each column. Asterisks indicate the following: $*p < 0.10$, $**p < 0.05$, $***p < 0.01$

Table 4: Effect of Soda Tax Campaign and Election on SSB and Diet Drink Sales Relative to Water

| | (1) $Soda\|Water$ | (2) $Energy\|Water$ | (3) $S\&E\|Water$ | (4) $Diet\|Water$ |
|---|---|---|---|---|
| Soda × Campaign | -0.011 | | | |
| | (0.080) | | | |
| Soda × Post-Election | -0.249*** | | | |
| | (0.003) | | | |
| Soda × Post-Policy City | -0.534*** | | | |
| | (0.036) | | | |
| Soda × Post-Policy Campus | -0.501*** | | | |
| | (0.003) | | | |
| Energy × Campaign | | 0.066 | | |
| | | (0.139) | | |
| Energy × Post-Election | | -0.249*** | | |
| | | (0.003) | | |
| Energy × Post-Policy City | | -0.231*** | | |
| | | (0.014) | | |
| Energy × Post-Policy Campus | | -0.487*** | | |
| | | (0.003) | | |
| S&E × Campaign | | | 0.028 | |
| | | | (0.108) | |
| S&E × Post-Election | | | -0.249*** | |
| | | | (0.066) | |
| S&E × Post-Policy City | | | -0.382*** | |
| | | | (0.083) | |
| S&E × Post-Policy Campus | | | -0.494*** | |
| | | | (0.061) | |
| Diet=1 × Campaign | | | | 0.268* |
| | | | | (0.128) |
| Diet=1 × Post-Election | | | | 0.775*** |
| | | | | (0.141) |
| Diet=1 × Post-Policy City | | | | 0.953*** |
| | | | | (0.141) |
| Diet=1 × Post-Policy Campus | | | | 0.939*** |
| | | | | (0.141) |
| Num of Obs. | 96 | 96 | 144 | 96 |
| Product FE | X | X | X | X |
| Month-of-Sample FE | X | X | X | X |

Standard errors in parentheses are clustered at the product group by academic year level. The outcome variable is ounces sold of product group $i$ in month $m$. In columns 1-4, soda, energy, and diet drinks are compared to the control cateogry (water). In this table, S&E is an abbreviation for Soda and Energy Drinks. Asterisks indicate the following: $*p < 0.10$, $**p < 0.05$, $***p < 0.01$

Table 5: Average Monthly Ounces Sold per Store by Beverage Group (2012-2013 Nielsen Data)

| Quantity Sold (Oz) | Coffee mean/sd | Juice mean/sd | Milk mean/sd | Soda mean/sd | Tea mean/sd | Water mean/sd |
|---|---|---|---|---|---|---|
| Berkeley [N=120] | 6380.86 | 96030.75 | 158783.86 | 76072.18 | 34546.53 | 97766.22 |
| | (2718.95) | (41491.24) | (66403.39) | (29953.56) | (20529.05) | (50248.46) |
| Control Cities [N=1,800] | 4362.25 | 43825.13 | 59158.68 | 44824.30 | 26695.99 | 70161.95 |
| | (2546.88) | (29852.22) | (60161.21) | (27798.79) | (14836.58) | (66173.17) |

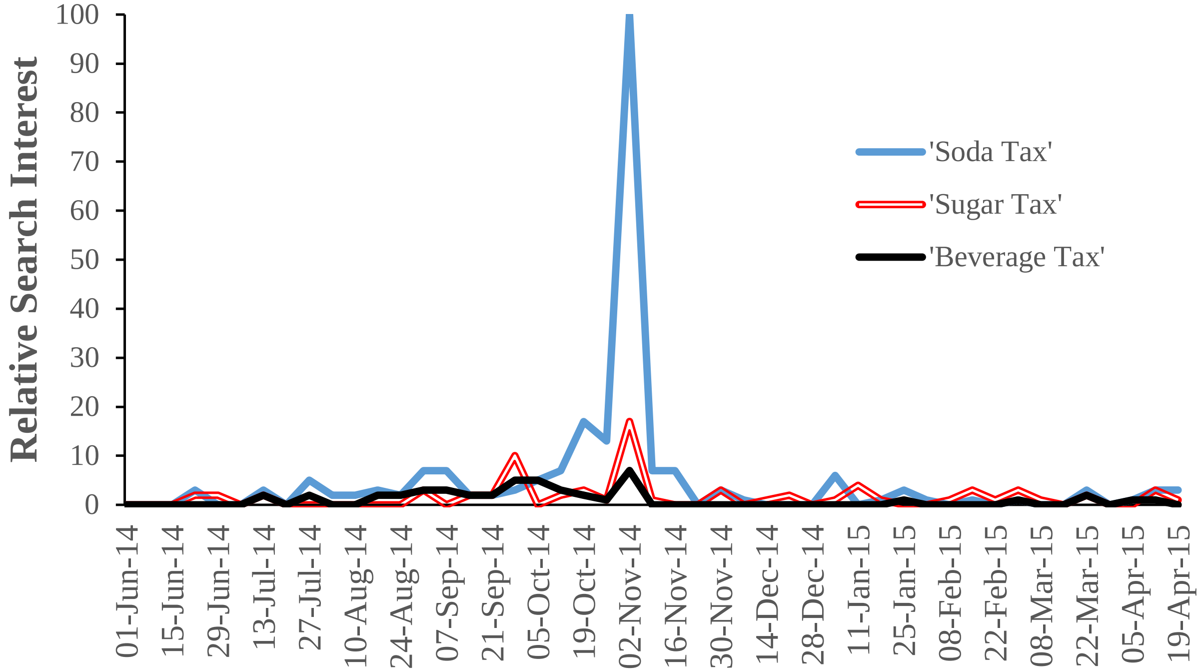
Standard deviations in parentheses.

Table 6: Triple-Difference: Effect of Berkeley Soda Tax Election on Drug Store Soda Sales Relative to Other Beverage Products and Other Cities

| | (1)<br>Oz Sold | (2)<br>Log Oz Sold |
|---|---|---|
| Soda × Campaign × Berkeley | -2313.329 | -0.033 |
| | (1884.994) | (0.036) |
| Soda × Post-Election × Berkeley | -782.082 | -0.108*** |
| | (2086.024) | (0.028) |
| Soda × Post-Policy City × Berkeley | -3610.394* | -0.125*** |
| | (1981.229) | (0.023) |
| Mean of Dep. Variable | 42306.065 | 6.687 |
| Num of Obs. | 23040 | 23040 |
| R squared | 0.976 | 0.983 |
| Product Group × Store FE | X | X |
| Store × Month-of-Sample FE | X | X |
| Month-of-Sample × Product Group FE | X | X |

Standard errors in parentheses are clustered at the product group by year by 3-digit zip code level. The outcome variable is ounces sold of product group $i$ in month-of-sample $m$, store $s$, and city $c$, in levels (column 1) and in logs (column 2). Asterisks indicate the following: $*p < 0.10$, $**p < 0.05$, $***p < 0.01$

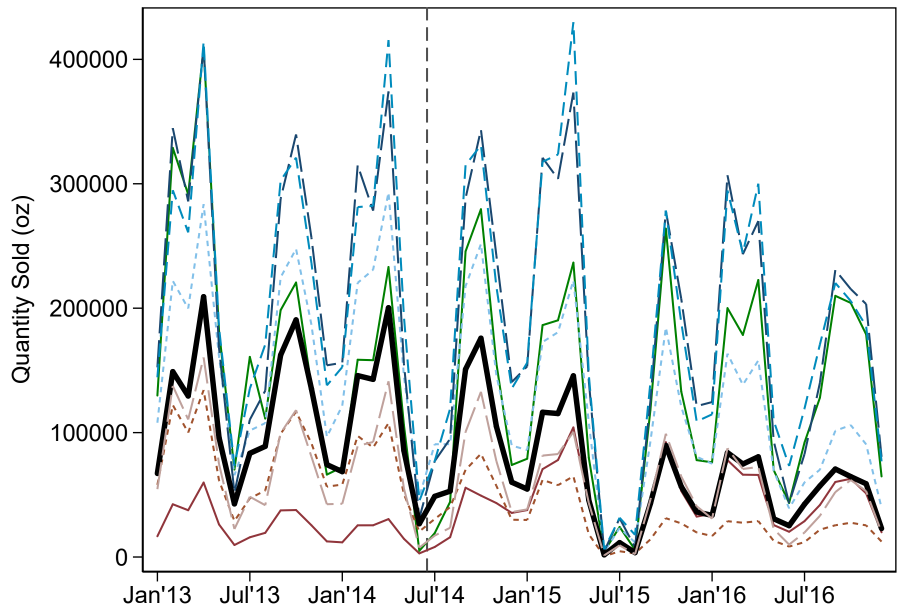
Figure 1: Google Trends Web Search Interest of “Soda Tax”, “Sugar Tax”, and “Beverage Tax” in the San Francisco Bay Area Over Time



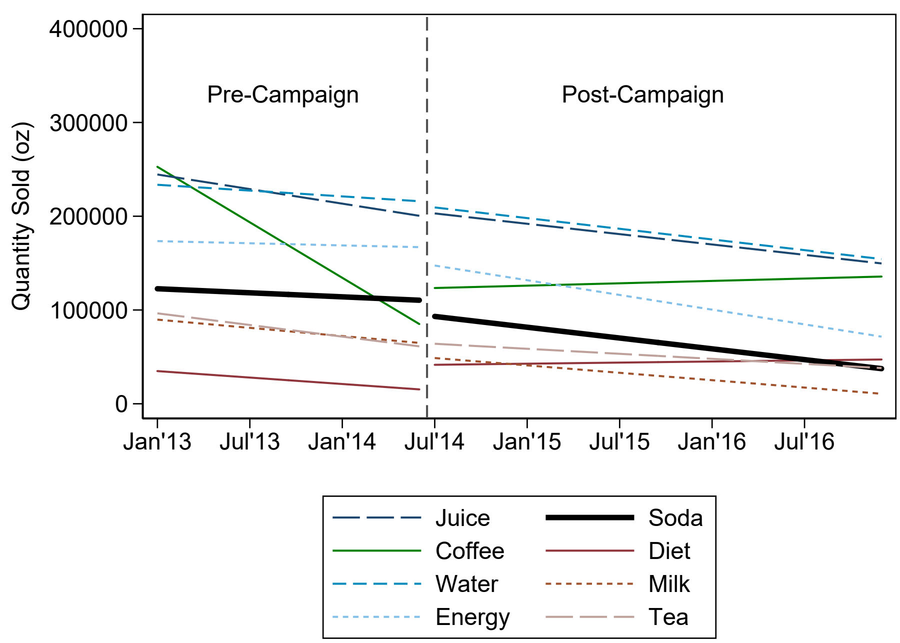

Source: Google Trends. Online, *accessed Aug. 2, 2018.* Note: Numbers on y-axis represent search interest relative to the highest point on the chart for the given region and time. A value of 100 is the peak popularity for the term. A value of 50 means that the term is half as popular. Likewise a score of 0 means the term was less than 1% as popular as the peak.

Figure 2: Monthly Quantities Sold by Product Group (Pre-Campaign Period)

(a) Monthly Quantities Sold (Oz)

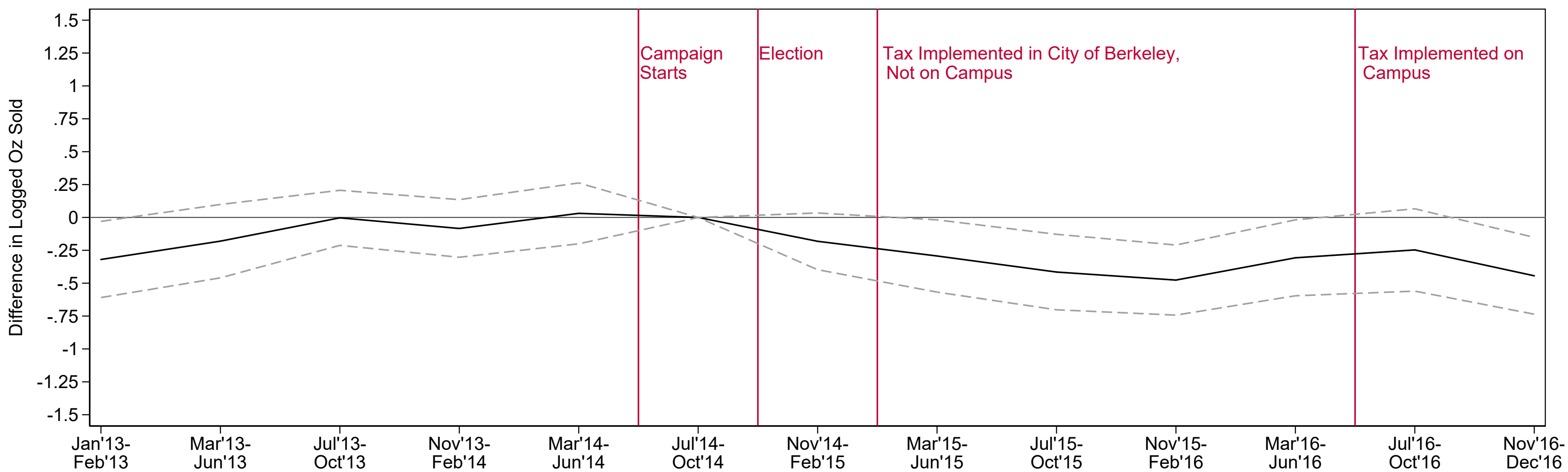



(b) Linear Trend in Monthly Quantities Sold



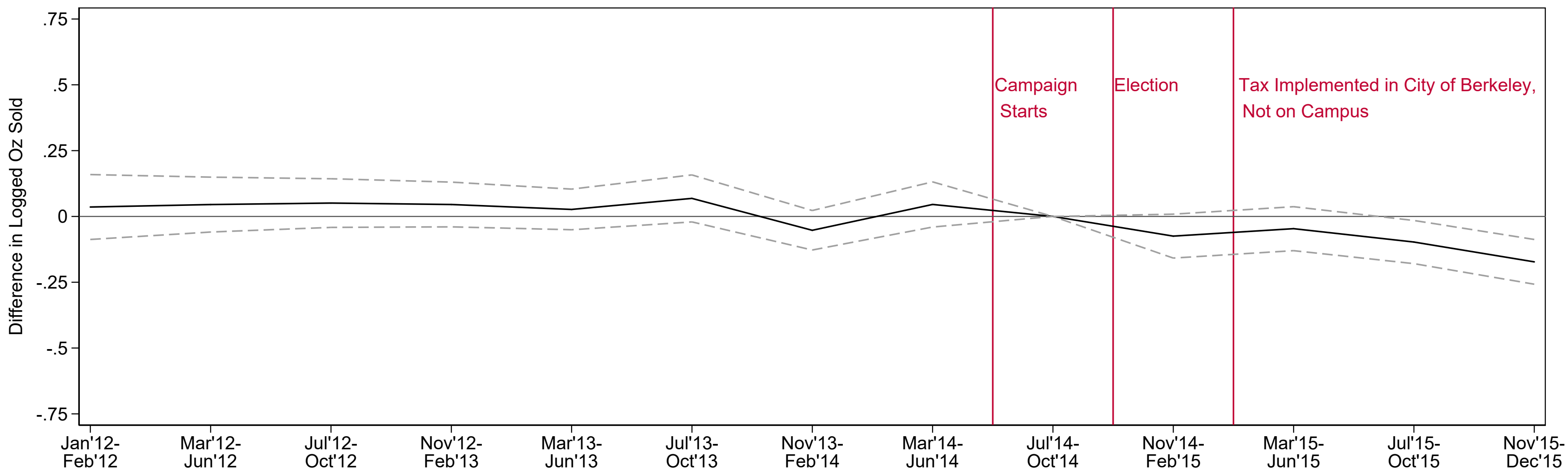

*Note*: Beverage products are categorized into eight groups: 1) Juice, 2) Coffee, 3) Water, 4) Energy Drinks, 5) Soda, 6) Diet Drinks, 7) Milk, and 8) Tea.

Figure 3: Event Study: Effect of Soda Tax Campaign and Election on Campus Retail Soda Sales Relative to Other Beverage Products




*Note*: The figure displays the $\beta_t$ coefficient estimates from event study equation 2. The dependent variable is the logged quantity sold (in ounces) of product group $i$ and month-of-sample $m$. Upper and lower 95% confidence intervals are depicted in gray, estimated using standard errors clustered at the product group by academic year level.

Figure 4: Triple-Difference Event Study: Effect of Berkeley Soda Tax Campaign and Election on Drug Store Soda Sales Relative to Other Beverage Products and Other Cities




*Note*: The figure displays the $\beta_t$ coefficient estimates from event study equation 4. The dependent variable is the logged quantity sold (in ounces) of product group $i$, in month-of-sample $m$, store $s$, and city $c$. Upper and lower 95% confidence intervals are depicted in gray, estimated using standard errors clustered at the product group by year by 3-digit zip code level.

# A Appendix

Table A.1: Replication of Table 2, excluding Diet Drinks

| | (1) Oz Sold | (2) Log Oz Sold |
|---|---|---|
| Soda × Campaign | 6663.962 | 0.088 |
| | (12515.253) | (0.091) |
| Soda × Post-Election | -3996.684 | -0.094 |
| | (10883.869) | (0.100) |
| Soda × Post-Policy City | -9420.324 | -0.266** |
| | (9778.545) | (0.116) |
| Soda × Post-Policy Campus | -13156.130 | -0.277* |
| | (12476.123) | (0.148) |
| Mean of Dep. Variable | 123120.144 | 11.312 |
| Num of Obs. | 336 | 336 |
| R squared | 0.865 | 0.948 |
| Product Group FE | X | X |
| Month-of-Sample FE | X | X |

Standard errors in parentheses are clustered at the product group by academic year level. The outcome variable is ounces sold of product group $i$ in month $m$, in logs (column 1) and in levels (column 2). Asterisks indicate the following: $*p < 0.10$, $**p < 0.05$, $***p < 0.01$

Figure A.1: Measure D Campaign Advertisements

(a) Yes on D Advertisement


(b) Yes on D Advertisement


(c) No on D Advertisement


(d) No on D Advertisement


Sources: (a) Berkeleyside, Online, (b) Clancey Bateman, MPH, Online. (c) Berkeleyside, Online. (d) MotherJones, Online. *Accessed February 18, 2018.*