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Expenditure Effects from the 2010 Washington Soda Tax*

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Abstract: This study examines the 2010 Washington excise soda tax and its effects on household soda expenditures. Findings show that soda expenditures fell by approximately \$2.26 per week following the tax. When matched with households in Oregon using a propensity score matching model to create a control group, the decrease in Washington household spending on soda exceeded that of Oregon households in all model specifications. Matched against households located anywhere in the U.S., Washington households' expenditures fell by between \$1.24 and \$1.92 more per week than control group households following the soda tax. After the tax's repeal, Washington soda expenditures remained below their previous pre-tax level. The evidence suggests the tax acted similar to a public health warning against soda and snack foods, as snack food expenditures also declined despite no added tax.

Keywords: soda tax, propensity score matching, tax expenditure effects, consumer expenditure survey, excise tax policy, Washington tax

JEL Codes: D12, I18, H2

1. INTRODUCTION

On July 1, 2010, the Washington State Legislature implemented a \$0.02 per ounce excise tax on soda. The tax was unpopular with Washington voters. The day following the tax's implementation, voters submitted nearly 400,000 signatures to place the Repeal Tax Law Amendments initiative on the state ballot in November. In November 2010, the Repeal Tax Law Amendments initiative passed with more than 60 percent of the popular vote, removing the excise tax on carbonated beverages effective December 1, 2010.

We utilize the temporary tax increase on soda in Washington as a natural experiment to study soda expenditures. We use household-level expenditure microdata from the Bureau of Labor Statistics' Consumer Expenditure Survey and a difference-in-differences two-stage

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propensity score matching model to determine whether the temporary soda excise tax had a causal effect on Washington residents' soda expenditures. This study's findings contribute to the heated and rapidly growing literature regarding the health and welfare effects of excise soda taxes by studying household expenditure responses.

Our aim in this study is to capture the change in soda expenditures resulting from the soda tax increase. The 2010 Washington soda tax was one of the first soda taxes to utilize an excise tax instead of sales tax. Our findings weigh into the policy debate by providing additional information on consumer expenditure responses to an excise tax and consumer response to a tax on only soda. The literature is split on the price elasticity of soda so understanding household expenditure responses to a new tax can provide additional information for future public policy. Expenditure responses are important to understand not only to form hypotheses on if taxation can help battle obesity, but to also better ascertain impacts at the household level. Based on past research on the elasticity of soda, a new excise tax could increase or decrease household soda expenditures. Excise taxes are generally considered to be more effective at altering consumer behavior than sales taxes, possibly making the Washington tax results even more pronounced (Zheng et al., 2012). We find that soda expenditures fell in Washington following the implementation of the excise tax. Average household expenditures fell \$2.23 per week from 2009 to 2010 in Washington. Matched similar households in Oregon experienced no significant decline in soda expenditures over the same time span. Matched households from anywhere in the U.S. spent an average of \$0.29 less in 2010. The decline in household soda spending in Washington exceeded matched households from anywhere in the U.S. between \$1.24 and \$1.92 per week. In 2011, following the tax's repeal, soda expenditures remained at the lower, 2010 levels, rather than returning to their 2009 levels. We suggest three possible channels the soda tax impacted consumer expenditures through: the tax served as a public health warning and adjust behavior, the excise tax increased tax salience and had a large shift after the tax and exhibited sticky prices after the tax was removed, and that menu costs also impacted expenditures.

2. SODA TAXES, CONSUMPTION, EXPENDITURES, AND ELASTICITY

Several studies have calculated the price elasticity for soda with conflicting findings. Andreyeva et al. (2010) conducted a meta-analysis of 160 of these studies and they report the median price elasticity estimate for soda is 0.79.¹ Powell et al. (2013) found more recent empirical studies report soda elasticity as more price sensitive and found in a smaller meta-analysis, only looking at papers published after 2008, that the mean price elasticity of regular soda is -1.25. Whether soda demand is price elastic or inelastic needs additional investigation and it remains unclear how consumer expenditures will change based on new taxes due to elasticity.

The Washington soda tax is uncommon in that it explicitly targets soda as opposed

¹A range of estimates are reported. More recently, Dharmasena and Capps Jr. (2012) found soda demand is price elastic, while preliminary results from a much-publicized recent working paper exploring the effects of 10 percent 2014 soft drink tax in Mexico suggest soda consumption is inelastic as quantity consumed fell by only six percent. Preliminary results of the study led by Shu Wen Ng were released in June, 2015, but as of the writing of this manuscript, the working paper has been removed from the MNIPH website.

to the broader category of all sugary sweetened beverage (SSB) taxes on which much of the discussion is focused. SSBs include soda plus certain kinds of iced tea, flavored lattes, energy, sports, and juice drinks. While soda demand has traditionally been discussed as price-inelastic Andreysva et al. (2010) estimated SSBs to be price elastic (1.20). Powell et al. (2013) note that subcategories are more price elastic in their analysis. Additionally, the fact that the Washington soda tax was an excise tax instead of a sales tax makes it uncommon. Researchers have begun to advocate for excise taxes over sales tax to reduce imperfect tax knowledge (Brownell et al., 2009). Zheng et al. (2012) provide theoretical and empirical evidence that previous studies may overestimate consumption elasticities because many consumers have imperfect tax knowledge and are sometimes inattentive to sales tax. Further, some consumers are rationally ignorant of tax changes because eligible food or beverages are tax exempt if using food stamps. Since excise taxes are placed at the producer or distributor level, the new price due to the tax will be reflected in the retail price and seen by the consumer when they make the purchasing decision, reducing the possibility of tax ignorance.

The ability of soda taxes to improve public welfare and health remains highly debated among economists and health professionals. Chaloupka et al. (2017) and Zheng et al. (2012) provide an excellent summary. In short, tax proponents see soda taxes as a means to decrease soda consumption and generate tax revenue (Brownell et al., 2009; ?). In particular, if household soda expenditures decrease after a tax, soda consumers can substitute to either less caloric drinks or more nutritionally dense foods.

Tax opponents highlight that when consumers substitute away from soda, they simply replace the calories given up in an alternative form, thus negating most of the proposed health benefits (Dharmasena and Capps Jr., 2012). Soda taxes are also regressive (Shughart, 1997; Brownell et al., 2009). If a tax increases household soda expenditures, soda consumers will find it more difficult to prioritize long-run health because gym memberships, low-preserved foods, and organic foods are expensive (Hoffer et al., 2014).²

This study offers a direct estimate of the effects of the Washington excise soda tax on household soda expenditures. The effect on consumer budgets provides a direct connection to consumer welfare.

3. DATA AND METHODS

We examine the expenditure effects of the 2010 Washington soda excise tax. Given previous estimates that soda demand is price-inelastic (Andreysva et al., 2010) and estimates that soda demand is price-elastic (Powell et al., 2013), it is unclear if we will observe an increase or decrease in household soda expenditures following the tax.

Additionally, we propose other channels through which consumer expenditures might be altered. If consumers see the tax as a public health warning about the effects of soda, we expect expenditures to decline after the tax and to not return to previous levels after its repeal. If people are responding to the tax's implementation, the high level of shifting in excise taxes means expenditures will likely decrease after the tax's implementation and

²Tax revenues from soda are also subject to political economy factors (Ghosh and Hall, 2015).

not return to previous levels after the repeal. If sticky prices are a factor, this may mean no change in expenditures after the tax introduction if producers do not change the price because they are anticipating its repeal.

We utilize household micro data from the 2009-2011 Bureau of Labor Statistics' (BLS) Consumer Expenditure (CE) surveys. Data are collected at the household level by asking participants to log in a diary every expenditure made during a two week period. The CE surveys provide a continuous and comprehensive flow of data on the buying habits of American consumers and are thus an excellent source of microdata for studying household consumption in the U.S (Blundell et al., 2008).

Tables 1 and 2 provide definitions and summary statistics of the variables utilized in the empirical analysis. The data include 663 observations from Washington households, 582 observations from Oregon households, and 37,807 U.S. non-Washington households.

The Washington state soda tax acts as a natural experiment to analyze the household expenditure effects induced by the excise tax. We use the soda tax as an experimental treatment and calculate the difference-in-differences of mean expenditures on soda.

The impact of a soda tax can be measured by comparing the observed mean household expenditure on soda (Y) with the mean household expenditures those households would have consumed had there been no tax. The difference between these expenditures is the average treatment effect on the treated (ATT):

$$ATT = E(Y^1|A = 1) - E(Y^0|A = 1) \quad (1)$$

where Y^1 is the soda expenditure of a treated household and Y^0 is the soda expenditure of the identical, non-treated household. $A = 1$ indicates that the households belong to the same adoption group. Unfortunately, $Y^1|A = 1$ and $Y^0|A = 1$ cannot be observed for the same household because time flows in a single direction and we cannot directly observe the counterfactual - soda expenditures of Washington if no soda tax had been implemented.

A common second-best approach is to manufacture a counterfactual by identifying a control group not subject to the tax whose outcomes can be compared to those in the treatment group. However, this task is complicated because public policies are not random events. As such, data based on household location (inside or outside of a policy's geographical applicability) suffer from selection bias (Heckman et al., 1997). A particular policy may have been implemented as a result of the preferences and characteristics of households the policy will affect. In such a situation, standard treatment effects would be endogenous.

Thus, if Washington households in the treatment group were fundamentally different than households outside of Washington composing a control group, empirical estimation results could not be interpreted as causal. We attempt to assuage such concerns in two ways. First, the events surrounding the 2010 Washington soda tax suggest Washington households are not fundamentally different from households in other states regarding soda tax preferences. The tax was legislatively implemented and so unpopular with voters that the tax was later repealed via voter referendum.

Table 1: Summary Statistics

Variable	All				Washington				Oregon				US Non-Washington			
	Obs	Mean	Std.	Obs	Mean	Std.	Obs	Mean	Std.	Obs	Mean	Std.	Obs	Mean	Std.	
Cola	38,470	1.68	3.65	663	1.29	3.12	582	1.54	3.64	37,807	1.69	3.66	37,807	1.69	3.66	
Age	38,470	49.62	17.05	663	47.81	17.30	582	50.54	17.36	37,807	49.66	17.04	37,807	49.66	17.04	
Income	38,470	53,628	67,574	663	62,146	65,000	582	54,028	62,641	37,807	53,479	67,609	37,807	53,479	67,609	
Family Size	38,470	2.54	1.50	663	2.46	1.42	582	2.46	1.38	37,807	2.54	1.50	37,807	2.54	1.50	
White	38,470	0.82	0.38	663	0.81	0.39	582	0.93	0.26	37,807	0.82	0.38	37,807	0.82	0.38	
Hispanic	38,470	0.12	0.33	663	0.05	0.22	582	0.08	0.28	37,807	0.12	0.33	37,807	0.12	0.33	
Other Race	38,470	0.06	0.24	663	0.16	0.36	582	0.07	0.25	37,807	0.06	0.24	37,807	0.06	0.24	
Married	38,470	0.53	0.50	663	0.54	0.50	582	0.57	0.50	37,807	0.53	0.50	37,807	0.53	0.50	
College	38,470	0.62	0.49	663	0.76	0.43	582	0.74	0.44	37,807	0.62	0.49	37,807	0.62	0.49	
Male	38,470	0.46	0.49	663	0.51	0.50	582	0.51	0.50	37,807	0.46	0.50	37,807	0.46	0.50	
Hours	10,651	80.20	17.11	208	80.11	19.69	296	76.42	18.22	10,443	80.18	17.05	10,443	80.18	17.05	

Table 2: Variable Description

Variable	Description
Cola	Cola expenditures (in dollars)
Age	The age of the individual responding to the survey
Income	Annual household income after taxes during the past 12 months. (in dollars)
Family Size	Number of members in CU (household)
White	= 1 if the individual responding to the survey is White
Hispanic	= 1 if the individual responding to the survey is Hispanic
Other Race	= 1 if the individual responding to the survey is not White, Hispanic, or Black
College	= 1 if one year of college, 0 otherwise
Male	= 1 if male, 0 otherwise
Hours	The number of hours worked by all the members in the household the previous week
Snack	Expenditures (in dollars) on cakes, cookies, and donuts

Second, we use a propensity score matching (PSM) model to match households based on the CE survey's rich set of demographic data. PSM can control for selection bias due to observed differences between treatment and control groups (Rosenbaum and Rubin, 1983; Michalopoulos et al., 2004). While this method still does not guarantee a perfect counterfactual against which to estimate treatment effects, households in the control and treatment groups can be statistically indistinguishable based on observed household demographics after matching.

We construct a quasi-panel data set from the repeated cross-sectional data - the participating households change from year to year - using a two-stage PSM model and then estimate the ATT using a difference-in-differences (DID) estimator. The propensity score is defined as the conditional probability that household i is in the treatment group, given a set of household characteristics, X_i :

$$p(X) = Pr(A = 1|X_1) \quad (2)$$

where $A = (0, 1)$ is an adoption dummy.

In stage one of the two stage matching model, treatment is defined as 2010. The goal of stage one is to match households within the state in order to form a quasi-panel dataset. In stage two, households are matched across states to determine the effects of the tax policy.

Propensity scores can be estimated using logit or probit models. We use a probit model for our analysis below.³

PSM imposes two conditions, balancing and common support. The balancing property is achieved when households with the same (or similar) propensity scores have the same distribution of X , irrespective of the treatment status. Balanced PSM reduces the influence of possible confounding factors (Rosenbaum and Rubin, 1983; Dehejia and Wahba, 2002).

Common support, or overlap, assures that households with the same (or similar) X

³Logit models results did not differ significantly from the probit results presented below. Logit model results are available upon request.

values have a positive probability, $p(X) \in (0, 1)$ both being in the treatment and control groups (Heckman et al., 1997). Thus, the treatment effect calculation is performed only for treatment and control households that share a common support in their estimated propensity scores, excluding the tails of the distribution of $p(X)$.

With cross-sectional data, selection bias is addressed based on a single difference matching estimator. Treatment and control group households with similar observable characteristics are matched (Dehejia and Wahba, 2002). After estimating the propensity score, the ATT is estimated as:

$$ATT = E[Y^1|A = 1, p(X)] - E[Y^0|A = 0, p(X)|A = 0] \quad (3)$$

With panel data, PSM can be combined with a DID estimator to further control for time invariant unobservable factors (Smith and Todd, 2005). Thus, the two-stage combination of PSM and DID can significantly improve the quality of the quasi-experimental evaluation (Blundell and Costa Dias, 2000).

The DID estimator exploits the fact that household observations in both the control and treatment groups are available for two time periods, 2009 and 2010. This is then repeated for 2010 and 2011. The ATT of the Washington soda tax is calculated by comparing the changes in household soda expenditures among Washington households ($Y_{2010}^1 = Y_{2009}^1$) with the changes among their non-Washington control group matches ($Y_{2010}^0 = Y_{2009}^0$),

$$ATT = E[Y^1_{2010} - Y^1_{2009}|A = 1, p(X)] - E[Y^0_{2010} - Y^0_{2009}|A = 0, p(X)|A = 0] \quad (4)$$

$$= \frac{1}{N_1} \left[\sum_{i=1}^N (Y^1_{2010} - Y^1_{2009}) - \sum_{i=1}^N (Y^0_{2010} - Y^0_{2009}) \right] \quad (5)$$

where N_1 is the number of matches.

Several matching algorithms are available for PSM (Caliendo and Kopeinig, 2008). The most common method is nearest neighbor (NN) matching. NN matches adopters with non-adopters with the nearest propensity score, while controlling for differences between adopters and non-adopters (Abadie and Imbens, 2006).

For the stage one matches, we present the results from one-to-one NN matching. For the state two matches we present the results from one-to-one NN matching along with the results from a five NN match, local linear regression (LLR) matches, and radius matching (0.1), in which household matches (control groups) reflect the average values of households matched with propensity scores within 0.1 of the matching (treatment group) observation.

We consider two separate groups within the BLS data in which to form (and match) a control group. We first consider only households in Oregon. Oregon borders Washington and is relatively similar in terms of demographics and political ideology. Border states are commonly used as control groups for policy analysis. We then create a separate control group based on households located anywhere outside of Washington in the U.S. Households for all matches use the same set of observable demographic variables.

Table 3: Propensity Score Stage One Matching Probit,
Treatment = Year 2010

Variable	Washington		Oregon		All US, non-WA	
	Coefficient	SE	Coefficient	SE	Coefficient	SE
Age	0.01	0.01	0.01	0.01	-0.00	0.01**
Income	-0.00	0.00	-0.00	0.00	-0.00	0.00
Family Size	0.22	0.10**	0.26	0.13*	-0.02	0.01*
White	-6.30	0.35***	0.52	0.39	-0.12	0.06**
Hispanic	-0.18	0.45	-0.66	0.67	0.12	0.05***
Other Race	-	-	-	-	-0.07	0.08
College	0.41	0.27	-0.20	0.35	-0.05	0.03
Male	0.25	0.22	0.19	0.27	-0.02	0.03
Hours	0.01	0.01***	0.00	0.01	0.00	0.00***
Constant	3.35	1.04***	-2.56	1.13**	0.01	0.13
N	148		112		7,035	
Log-Likelihood	-94		-72		-4853	

Note: *, **, *** denote statistical significance at the 10, 5, and 1 percent levels, respectively.

4. RESULTS

Stage one of the two-stage propensity score matching model constructs the quasi-panel data from repeated cross-sections. Table 3 presents the results from the probit models used to generate propensity scores for stage one (intrastate) match. The treatment effect in Table 3 is participating in the survey in 2010. Larger families were more likely to be surveyed in both Washington and Oregon in 2010 than in 2009. White households were less likely to appear in the survey in 2010 (the omitted race group is Black).

Prior to implementing the matching model, in-state households were very similar demographically in 2009 and 2010. After matching, the households in 2009 and 2010 are nearly statistically indistinguishable.⁴ Propensity scores were not statistically different, but Washington households post-match were slightly larger on average in 2010; family size was 0.4 persons greater on average post-match.

Table 4 shows the average treatment effects on treated households for soda expenditures in 2009 to 2010. Interestingly, average household cola expenditures declined following the soda tax implementation. Between the full sample of Washington households, households spent an average of \$1.16 less in 2010 and 2009. After matching, the gap grew to \$2.23. This could potentially align with the understanding that soda is price elastic in demand. However, since we are using expenditures for our analysis, it could also show the impact of other channels, such as additional health awareness. Under the excise tax present in 2010, household expenditures on soda decreased. Due to using annual data rather than quarterly data and the fact that the tax was only present for about half the year, our results may be biased. However, in this case our results would show a smaller change in expenditure.

⁴Full propensity score estimates and pre- and post-matching balancing tests are available upon request.

Table 4: Average Treatment Effects on Treated Households,
Treatment = 2010

	Washington		Oregon		All US, non-WA	
	ATT	SE	ATT	SE	ATT	SE
unmatched	-1.16	0.656*	0.12	0.75	-0.26	0.094***
matched	-2.23	1.21*	-0.84	1.10	-0.29	0.16*
N	148		112		7,035	

Note: *, **, *** denote statistical significance at the 10, 5, and 1 percent levels, respectfully.

One possible explanation is that the decline in soda expenditures was part of a nationwide trend. Oregon households experienced no significant decline in expenditures on soda over the same time span, however. Across the U.S., households spent less on soda in 2010 than in 2009, but the average decrease in expenditures was only \$0.29 after matching, much less than the decline seen in Washington.

In Stage two (of the two stage matching model), we matched households across states and estimated a second PSM to calculate the DID in household expenditures. We first matched Washington households with Oregon households. We then separately matched Washington households with households from anywhere within the U.S. other than Washington to create a separate counterfactual. Table 5 shows the DID average treatment effects based on the matching models described above.

All of the estimated coefficients are negative, suggesting that the decline in household soda expenditures in Washington exceeded the decline in soda expenditures from households in other U.S. states. Using Oregon as a control group, all of the unmatched coefficients were statistically significant, but only the radius matching method produced statistically significant results for matched households.

Table 5: Difference-in-Difference Average Treatment Effects on Treated Households,
Treatment = Washington

	One Nearest Neighbor		Five Nearest Neighbor		Local Linear Regression		Radius (0,1)	
	ATT	SE	ATT	SE	ATT	SE	ATT	SE
Control = Oregon								
unmatched	-1.50	0.73**	-1.50	0.73**	-1.50	0.73**	-1.50	0.73**
matched	-0.27	0.94	-0.92	0.85	-0.66	0.94	-1.35	0.80*
N	148		148		148		148	
Control = Any U.S. Non-Washington								
unmatched	-1.32	0.52**	-1.32	0.52**	-1.32	0.52**	-1.33	0.52**
matched	-1.46	0.72**	-1.92	0.52***	-1.24	0.72*	-1.33	0.43***
N	7,035		7,035		7,035		7,035	

Note: *, **, *** denote statistical significance at the 10, 5, and 1 percent levels, respectfully.

Table 6: Average Treatment Effects on Treated Households,
Control = 2010, Treatment = 2011

	Washington		Oregon		All US, non-WA	
	ATT	SE	ATT	SE	ATT	SE
unmatched	0.17	0.50	-0.40	0.69	0.11	0.09
matched	0.90	0.60	0.25	0.87	-0.06	0.16
N	148		112		7,035	

Note: *, **, *** denote statistical significance at the 10, 5, and 1 percent levels, respectfully.

When matched against households from anywhere in the U.S. other than Washington, all matched and unmatched DID average treatment effect estimates were negative and statistically significant. Washington households decreased soda expenditures between \$1.24 and \$1.92 more per week in 2010 than their matched non-Washington counterparts.

By 2011, however, the tax was removed. Repeating the identical two stage propensity score matching model above, we calculated the DID average treatment effect estimates for 2010 to 2011. Table 6 shows the matched inter-household change in soda expenditures from 2010 to 2011. Table 7 shows those households matched across states

We found no statistically significant change in consumption in 2011 compared to 2010 in any region. Soda expenditures in Washington did not return to their previous levels following the removal of the tax. The DID estimates for Washington households compared to Oregon households and any household across the U.S. similarly showed no statistical difference.

Within the analysis, we use annual data. The excise soda tax was in effect for half of 2010; it took effect on July 1, 2010 and was repealed effective December 1, 2010. Due to this, our results examining the change in expenditures from 2009 to 2010 may be biased

Table 7: Difference-in-Difference Average Treatment Effects on Treated Households,
Treatment = Washington

	One Nearest Neighbor		Five Nearest Neighbor		Local Linear Regression		Radius (0,1)	
	ATT	SE	ATT	SE	ATT	SE	ATT	SE
Control = Oregon								
unmatched	0.60	0.66	0.60	0.66	0.60	0.66	0.60	0.66
matched	1.43	0.88	1.20	0.99	0.62	0.88	0.64	0.81
N	148		148		148		148	
Control = Any U.S. Non-Washington								
unmatched	0.52	0.56	0.52	0.56	0.52	0.56	0.52	0.56
matched	0.87	0.61	0.47	0.49	0.87	0.61	0.61	0.90
N	7,035		7,035		7,035		7,035	

Note: *, **, *** denote statistical significance at the 10, 5, and 1 percent levels, respectfully.

downward. Even with this, our results suggest that the excise soda tax significantly decreased household soda expenditures. Our results examining the change in expenditures from 2010 to 2011 suggest that expenditures are not significantly different - thus there is likely a small increase in expenditures in 2011 but not enough to return to previous levels. This suggests that the change in expenditure may be larger than reported.

5. DISCUSSION

Soda expenditures fell in Washington in 2010 following the implementation of a \$0.02 per ounce soda tax. The decline in expenditures in Washington equaled or exceeded the expenditure decline of matched households in neighboring Oregon and matched households from anywhere in the U.S. This finding suggests soda demand is price elastic. However, without price and quantity data, this cannot be directly tested and it remains unclear how much of the tax was passed onto the consumer in the form of higher prices. Additionally, the fact that expenditures did not return to 2009 levels after the tax removal suggests additional mechanisms are important to consider.

The most attractive feature of the expenditure model estimated above is that it supplies a direct measure of changes in household spending and the factors that influence those expenditures. The model provides a direct means of quantifying the way in which expenditures vary in response to changes in a number of factors, such as income, rather than attempting to approximate expenditure effects from a single point estimate from an own-price elasticity estimation. In addition, the model absorbs quality changes that may not be encompassed by traditional income elasticity or price elasticity estimates.

However, the expenditure model also introduces other interpretative restrictions. The most notable limitation is that by utilizing total expenditures as the dependent variable, the effects of price and quantity are combined and thus inseparable. That is, the expenditure model cannot disentangle the effects on price and quantity from a policy change, such as those following the imposition of a tax. We cannot identify how much of the tax was passed onto consumers. We therefore discuss three possible explanations for the decline in soda in expenditures in Washington - the tax acted as a public warning, tax shifting, and sticky prices.

5.1. Public Warning Hypothesis

The highly publicized tax signaled to certain Washington soda consumers that soda consumption should be reduced or eliminated. If a group of consumers treated the tax similarly to a public health warning on soda, ceasing consumption following the tax, those consumers likely would not have started drinking soda again when the tax was repealed. The result would mirror the empirical findings above - a decrease in expenditures following the tax and no change in expenditures following the tax repeal.

Researchers have provided ample evidence that information campaigns and public health warnings can influence consumer behavior in a similar manner. Examining cigarette cessation in pregnant women in Nigeria, Harris et al. (2015) found each of the non-price experimental treatments - banning of advertising nationwide, rotating warnings with pictograms on each

Table 8: Snack Expenditure Difference in Difference Average Treatment Effects on Treated Households, Treatment = Washington

	One Nearest Neighbor		Five Nearest Neighbor		Local Linear Regression		Radius (0,1)	
	ATT	SE	ATT	SE	ATT	SE	ATT	SE
	Control = Oregon							
unmatched	-1.70	0.44***	-1.70	0.44***	-1.70	0.44***	-1.70	0.44***
matched	-2.03	0.68***	-1.85	0.59***	-1.96	0.60***	-1.89	0.46***
N	148		148		148		148	
	Control = Any U.S. Non-Washington							
unmatched	-1.47	0.36***	-1.47	0.36***	-1.47	0.36***	-1.47	0.36***
matched	-1.37	0.48***	-1.35	0.39***	-1.54	0.49***	-1.46	0.31***
N	7,035		7,035		7,035		7,035	

Note: *, **, *** denote statistical significance at the 10, 5, and 1 percent levels, respectfully.

pack, restriction of brands to a single presentation, and an increase in the size of pictograms to 80 percent of the front and back of each pack - was separately associated with a significant decline in smoking.

Reger et al. (1998) ran a “1 percent or Less” consumer education campaign for spent seven weeks in Clarksburg, West Virginia, encouraging consumers to switch from higher-fat to lower-fat milk. After the campaign ended, the market share of low-fat milk had risen from 18 percent to 41 percent and persisted at elevated levels for a full year.

To test the public warning hypothesis, we replicated the DID ATT procedure for other high-sugar snack foods on which the BLS collects data - cake, cookies, and donuts. No tax was applied to these goods. If the tax acted similarly to a public health warning against sugary food consumption, we would expect to observe a decline in snack expenditures as well.

Table 8 shows that household expenditures in Washington similarly declined compared to the matched control group households in Oregon and across the U.S. This finding suggests that the selective tax on soda may have acted similar to a public health warning. It could also be that soda and these snack foods are complements in consumption, in which case previous soda tax estimates may have underestimated the caloric effect of soda tax.

As a second test of the public warning hypothesis, we repeat the DID PSM procedure for households in 2009 and in 2011, removing the tax year of 2010 from the analysis. We have no reason to believe that soda prices in Washington changed differently than soda prices in Oregon between 2009 and 2011. Therefore, the observed effects should isolate quantity changes, with the treatment effect of the public warning (tax then repeal) in Washington. Table 9 presents these results.

We estimate a negative treatment effect in all model specifications. The ATT effects were statistically different from zero in all model specifications other than when matched to a single nearest neighbor. Washington households decreased their soda expenditures from 2009 to 2011 more than similarly matched households from Oregon or elsewhere in the U.S.

Table 9: 2009 to 2011 Difference in Difference Average Treatment Effects on Treated Households, Treatment = Washington (Public Warning Hypothesis Test)

	One Nearest Neighbor		Five Nearest Neighbor		Local Linear Regression		Radius (0,1)	
	ATT	SE	ATT	SE	ATT	SE	ATT	SE
	Control = Oregon							
unmatched	-1.83	0.82***	-1.83	0.82***	-1.83	0.82***	-1.83	0.82***
matched	-0.42	0.85	-1.67	1.00*	-1.81	0.85**	-1.91	0.88**
N	124		124		124		124	
	Control = Any U.S. Non-Washington							
unmatched	-1.93	0.72***	-1.93	0.72***	-1.93	0.72***	-1.93	0.72***
matched	-1.44	1.00	-1.67	0.73**	-1.77	1.00*	-1.92	0.69***
N	7,035		7,035		7,035		7,035	

Note: *, **, *** denote statistical significance at the 10, 5, and 1 percent levels, respectfully.

We believe this gives support to the temporary tax's effect as a public warning hypothesis.

5.2. Tax Shifting and Sticky Prices

The fact that the Washington soda tax was an excise tax increased its tax salience compared the tax salience of sales taxes. Since excise taxes are levied at the producer or wholesale level, they are more likely to be passed through to the consumer at the point of purchase (Brownell et al., 2009). Berardi et al. (2016) found that an excise tax on soda in France was gradually shifted to consumers and fully shifted within six months. Grogger (2017) found evidence of overshifting of the excise soda tax in Mexico. In the U.S., Cawley and Fisvold (2017) found that just under half of the 2014 soda tax in Berkley, CA was passed onto consumers. If similar tax shifting occurred in Washington, we would expect to see large expenditure effects. Additionally, Bergman and Hansen (2016) examined both the tax implantation shift and the tax cut shift on beverages and found that tax cuts on beverages are consistent with sticky prices and that there is an undershift after the tax's removal. This is consistent with our results that expenditures do not change after the tax repeal.

Producers may have absorbed the burden of the soda tax due to sticky prices (menu costs) or an extremely inelastic soda supply curve. Vending machine operators may have been hesitant to manually adjust the prices at all of its machines. Further, the tax was clearly unpopular with voters. The Repeal Tax Law Amendments initiative gathered nearly 400,000 votes and was submitted one day after the tax's implementation. Soda vendors may have expected that the tax would be repealed and thus, rather than changing prices twice - increasing with the added tax and then decreasing once the tax was repealed - soda sellers may have simply maintained their price through the duration of the tax or initiated the initial tax and then failed to lower the prices after the taxes repeal.

Through looking at an expenditure model of household behavior after an excise soda tax implementation and then repeal, it is clear households adjusted their initial behavior. We suggest that households adjusted their behavior due to one of three channels: becoming

more aware of the health effects of soda, high levels of tax shifting impacting prices, and sticky prices. Regardless of the actual cause of the decline in soda expenditures, we find no support that households spent more of their discretionary budgets on soda following the tax.

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