Evaluation of changes in prices and purchases following implementation of sugar-sweetened beverage taxes across the United States

Scott Kaplan, PhD*[1], Justin S. White, PhD[2], Kristine A. Madsen, MD[3], Sanjay Basu, MD, PhD[4,□], Sofia B. Villas-Boas, PhD[5,□], and Dean Schillinger, MD[6]

[1] Department of Economics, United States Naval Academy, Annapolis, Maryland
[2] Philip R. Lee Institute for Health Policy Studies, University of California, San Francisco, San Francisco, California
[3] School of Public Health, University of California, Berkeley, California
[4] Institute of Health Policy, Management and Evaluation, University of Toronto
[5] Department of Agricultural & Resource Economics, University of California, Berkeley, California
[6] Division of General Internal Medicine, Center for Vulnerable Populations, San Francisco General Hospital/University of California San Francisco, San Francisco, California

[□] These authors contributed equally to this work.
* Corresponding author: Scott Kaplan, 106 & 107 Maryland Avenue, Annapolis, MD, 21402, USA; skaplan@usna.edu; +1 (410) 293-6971

Presented at the 2024 ASSA Annual Meeting, January 5-7, 2024

ABSTRACT

Sugar-sweetened beverage (SSB) excise taxes are promoted as a key policy to reduce cardiometabolic diseases and other conditions, but comprehensive analyses of US SSB taxes have been difficult due to the absence of suitable methodologies to account for confounding factors. We use recent advances in synthetic control methods to estimate the changes in SSB prices and purchases in five large cities in the US in the two years following their respective implementation of SSB taxes. We find prices of SSB products increased by an average of 33.1% over the 2 years following tax implementation, corresponding to an average price increase of 1.3 cents per ounce and a price pass-through rate of 92% from distributors to consumers. SSB purchases in total volume declined by an average of 33.0%, corresponding to a price elasticity of demand of -1.00. The observed price increase and corresponding volume decrease immediately followed tax implementation, and both were sustained in the months thereafter. We also assessed changes in adjacent, untaxed areas to detect any increase in cross-border purchases, finding no evidence of increased cross-border purchases following tax implementation. Relating our findings to the medical literature suggests that SSB taxation would likely generate significant improvements in population health and large cost-savings, which would apply across a diverse set of demographic and geographic settings.

INTRODUCTION

Sugar-sweetened beverages (SSBs) are a major source of non-nutritional calories and associated with serious adverse health outcomes, including type 2 diabetes, obesity, cardiovascular disease, gum disease, caries, and other conditions contributing to morbidity and mortality.[1-2] Because of these relationships, excise taxes on SSBs have been proposed in the US and around the world, with 8 US jurisdictions and more than 50 countries having implemented some form of an SSB tax as of November, 2022.[3] Several systematic reviews and meta-analyses have examined the association of SSB excise taxes with both prices and consumption.[4-6] The most recent international review finds a pass-through rate from distributors to consumers of 82% (95% CI:66%,98%), a mean reduction in SSB sales of 15% (95% CI:-20%,-9%), and an average demand elasticity of -1.59 (95% CI = [-2.11,-1.08]).[7]

Yet, nearly all U.S-based SSB tax studies analyzed a single taxed city and compared it to a control city. To our knowledge, two existing studies have evaluated joint estimates of SSB taxes across multiple taxed cities.[8-9] However, recent statistical advances suggest that these estimates likely suffer from bias associated with conventional two-way fixed effects (TWFE) approaches that cannot account for time-varying confounders that differ between experimental and control populations.[10] Unbiased estimation of a composite effect, which provides a pooled estimate of SSB taxes across multiple taxed cities, is critical for understanding the generalizability of SSB tax impacts to different localities featuring heterogeneous characteristics; such an estimate is complementary to existing estimates from individual localities with SSB taxes in place. This estimate, while imperfect, also better informs the potential effectiveness of a nationwide tax, which was recommended by a recent federal commission on diabetes,[11] and is especially relevant considering the beverage industry's recent efforts to preempt localities from levying SSB taxes.[12]

In this study, retail sales data from five taxed cities in the US were used to estimate the composite effect of SSB taxes in the US on SSB prices and volume purchased. We applied recent advances in statistical methods to estimate an augmented synthetic control (ASC) model with staggered adoption, which produces joint estimates from taxes in several treated cities despite different timing of policy implementation. Unlike conventional TWFE approaches, an ASC model with staggered adoption addresses time-invariant and time-varying unobserved confounders that differ between taxed cities and their untaxed comparators.[10,13-14] We also estimate composite cross-border shopping in untaxed adjacent areas following SSB taxes to examine if consumers offset SSB purchases following tax implementation.

METHODS

Retail scanner data on SSB prices and volume sold and a staggered adoption ASC approach were used to estimate the composite change in prices and purchases following implementation of SSB taxes in five cities. We also estimated composite changes in cross-border shopping using adjacent, untaxed areas. This study followed the STROBE reporting guidelines for cross-sectional studies.[15] The research was determined not to meet the criteria for human participant research by the institutional review board at the University of California, San Francisco.

Data

The primary dataset was The Nielsen Company retail scanner data. It consisted of product-week-store observations from selected chain stores in nearly all 3-digit zip codes across the US (871) over the study period from January 1, 2012, through February 29, 2019. The data included total units sold and average sale price per unit for each observation. Beverage products from this dataset were supplemented with nutritional and general product information from Label Insight[16] and hand-coded nutritional information. This enabled the classification of individual beverage products as SSBs or not based on tax regulations across the five cities. Artificially sweetened beverages were not included in the analysis, despite being covered by Philadelphia's SSB tax. The eMethods contains additional details on product selection and tax status classification procedures.

Table 1 provides summary information about the study's localities. There were five taxed 3-digit zip codes examined: 803 (Boulder, CO), 191 (Philadelphia, PA), 946 (Oakland, CA), 981

(Seattle, WA), and 941 (San Francisco, CA). Each of these 3-digit zip codes formed the full set of taxed jurisdictions. Berkeley, CA and Albany, CA (947) were not included because they were taxed at different times and could not be separately identified from one another (more detail is included in the Limitations subsection). Localities with sales taxes, which include Washington, DC and Navajo Nation, were omitted because they tend to be smaller in magnitude and less likely to change purchasing behavior. Among the five treated localities studied, SSB taxes were implemented at three different times and differed by city: January 1, 2017 (Philadelphia); July 1, 2017 (Boulder, Oakland); and January 1, 2018 (San Francisco, Seattle). Tax amounts ranged from $0.01/ounce to $0.02/ounce. Cross-border purchasing was examined in all immediately adjacent 3-digit zip codes, of which there were 13 (**Table 1**). These areas did not contain any taxed jurisdictions.

Outcome Variables

Two primary outcome measures were examined: the monthly change in (1) average shelf price per ounce of SSB products and (2) total ounces sold of SSB products in treated localities compared to the "synthetic" control localities following tax implementation. Total ounces sold of SSB products was the outcome used in the cross-border shopping analysis.

Statistical Analyses

This study used an augmented synthetic control (ASC) approach.[17] The original synthetic control method uses a data-driven approach to construct a "synthetic" control unit as a weighted average of all potential control units that best match the treated unit on both the pre-treatment outcome and prognostic factors.[18] The ASC approach extends this method by (i) allowing for multiple treated units experiencing treatment at different times and (ii) providing a robust correction procedure when the synthetic unit's pretreatment outcomes do not closely match those of the treated units. Using a donor pool of untaxed, non-bordering 3-digit zip codes, a "synthetic" treated unit was constructed for each of the five treated cities using pre-tax SSB prices and purchases (respectively) and a set of time-invariant characteristics from the 2010 Decennial Census and 2016 American Community Survey.

Analysis of Price Pass-Through and Volume Purchases

The primary ASC analyses was estimated at the 3-digit zip code-by-month level. We used the weighted average shelf price of SSBs and aggregated the total ounces purchased of SSBs at this unit of observation. Then, separate estimations assessed the composite post-tax implementation change in (i) shelf prices and (ii) volume sold in treated localities compared to a "synthetic" locality for each. Each individual city was given equal weight in calculating the composite effect. The percent change in shelf prices (volume sold) was computed using pre-tax average shelf prices (volume sold) in the treated localities.

Following Abadie (2021),[19] the donor pool was limited to units with similar characteristics, namely jurisdictions within one standard deviation (0.35) of the average urbanicity level of the five treated localities (0.98), following the US Census definition of urban vs. rural. 284 3-digit zip codes remained, including the five treated localities but omitting the 13 border localities. Sociodemographic and geographic characteristics used in constructing the synthetic units are shown in **Figure 1** and described in Section 2 of the eMethods. These characteristics were chosen based on prior research examining SSB taxes.[20-23]

To determine the statistical significance of the ASC average treatment effects, which are calculated as the average post-tax percent change in SSB prices (purchases) for treated units relative to that of the synthetic control units, placebo estimates were generated for each donor unit one by one as if each of those units had been treated.[18] Because treated localities implemented taxes at different times, this procedure was repeated for each treated locality, generating 279×5=1,395 placebo estimates. To generate p-values, the ratio of mean squared prediction error (RMSPE) in the post-tax vs. pre-tax period was computed for the composite unit estimate and each placebo estimate, which were then ranked from largest to smallest.[24] The p-value was calculated as the ratio of the composite unit ranking with respect to the total number of units (1,396).

More details are provided in the eMethods.

Analysis of Cross-Border Purchasing

To fully quantify the changes following SSB taxes in treated cities, we also explored whether purchasing behavior changed in adjacent 3-digit border zip codes.

The same ASC procedure was implemented, except all adjacent border localities were considered “treated” and taxed cities were excluded. Because border localities tended to be semi-urban or suburban, the subsample of donor pool units was modified to those featuring an urbanicity level within one standard deviation (0.35) of the mean urbanicity of the 13 border localities (0.75). 369 3-digit zip codes remained, including the 13 border localities. This analysis used the same Census characteristics and p-value calculation approach.

Sensitivity Analyses

To assess sensitivity, two different urbanicity cutoffs were used to determine the donor pool subsample: an urbanicity level of 0.9 and 0.85, which reduced the donor pool of 3-digit zip codes to 204 and 226, respectively.

RESULTS

Sample Composition and Comparison of Treated and Donor Units

The main analytic sample included 28,512 3-digit zip code-by-month observations from 297 3-digit zip codes across 98 months. Using nutritional information from the supplementary hand-coded and Label Insight data, 5,500 unique UPCs were confirmed as SSBs under the tax designations. The sample included 26,338 stores: 496 in treated localities, 1,340 in border localities, and 24,502 in the donor pool. **Table 1** provides summary information for each group of localities.

Figure 1 compares the similarity of each treated unit and corresponding synthetic unit for the volume analysis based on pre-tax means of volume purchases and values for the 12 sociodemographic and geographic covariates (eFigure 1 displays the price analysis comparisons). Variables were scaled to be between 0 and 100 so the units of measure were comparable. In most instances these values were highly similar (within five index points), and no comparisons differed by more than 14 index points. eFigure 2 displays sample distributions of each Census characteristic.

Augmented Synthetic Control Analyses of SSB Prices and Volume Sales

In the composite treated locality, shelf prices of SSB products increased by an average of 33.1% (95% CI: 14.0%,52.2%; p<0.001) in the 2 years following tax implementation, relative to the average percent change in the composite synthetic locality. This corresponded to an average price increase of 1.3 cents per ounce (**Figure 2**) and a 92% price pass-through rate (eFigure 3). SSB volume purchases declined by an average of 33.0% (95% CI: -2.2%,-63.8%; p=0.035) over the same timeframe, relative to the average percent change in the composite synthetic locality. This corresponded to an average monthly change of 18,534 ounces per store-month (**Figure 2**). Together, these estimates yielded a price elasticity of demand of -1.00, suggesting SSB purchasing behavior was relatively responsive to changes in shelf prices (**Figure 2**). **Figure 2** also shows changes in shelf prices and volume purchases for the five taxed localities individually. The demand elasticity estimates were relatively consistent across taxed localities, ranging from -0.80 (Philadelphia) to -1.37 (Seattle). Shelf price changes for individual cities were significant at the 10% level, while we failed to reject null changes in volume purchased for each city at the 10% level.

Figure 3 shows the time-varying ASC results for SSB shelf prices (Panel A) and volume sales (Panel B). The purple line indicates the difference between the composite treated unit and synthetic unit, while the grey lines represent each placebo estimate. In both analyses, there was a close fit between the composite treated unit and synthetic unit in the pre-tax period.

There was a steep, immediate increase (decrease) in shelf prices (volume sales) following tax implementation, which was sustained in the months thereafter.

Each city in the composite analysis is equally weighted, since the procedure and context through which each city introduced an SSB tax varies, and the findings are intended for policy-makers considering tax implementation in specific geographies. The population-weighted composite estimates are similar (eFigures 8 and 9).

The analyses for different urbanicity cutoffs generated similar results (eFigures 10 and 13). eFigures 5 and 6 show the individual city ASC analyses.

ASC Analyses of Cross-Border Shopping

Figure 4 shows the time-varying ASC results for cross-border SSB volume sales. There was no statistically significant average change in cross-border purchases of SSBs following tax implementation (-2.4%, 95% CI: -12.8%,8.1%; p=0.671), which remained stable in the years following the tax. No significant change in cross-border SSB volume purchases was observed in each taxed city individual (eFigure 4). Estimates for different urbanicity cutoffs provided similar findings (eFigures 12 and 15). eFigure 7 displays the time-varying cross-border analyses for each taxed city.

DISCUSSION

SSB excise taxes led to large, consistent declines in SSB purchases across five US taxed cities following tax-driven price changes. Quasi-experimental methods were used to estimate the overall changes following SSB taxes implemented at different times and locations relative to a synthetic control of untaxed areas. The results show shelf prices of SSB products increased by an average of 33.1% (1.3 cents/ounce) in the years following SSB tax implementation, corresponding to a 92% price pass-through rate from distributors to consumers. Volume sales fell by 33.0% over the same timeframe, without evidence of changes in cross-border shopping in untaxed adjacent areas.

While the estimates generally support prior estimates from single-city studies, they help answer the critical question of how much variation across taxed localities is due to unique characteristics of a locality versus the generalizable effect of a tax. Compared to a recent international meta-analysis of SSB taxes, the results suggest slightly higher pass-through, a substantially larger reduction in volume purchased, and moderately less demand-responsiveness to price changes.[6] These modest discrepancies may reflect differences in geographic areas of comparators, store sample composition, and greater accounting of unmeasured confounders in this analysis than in prior studies. Additionally, there have been conflicting findings concerning cross-border purchasing following SSB taxes, with some studies pointing to significant increases and others finding no changes.[25-28] The results provided no evidence of changes in cross-border purchasing.

To further contextualize the findings, we estimated a TWFE event-study model (eMethods Section 3), which has been the primary approach taken in previous SSB tax evaluation studies. eTable 3 shows the point estimates are generally comparable to the ASC estimates, although there are some moderate differences. Inspection of the pre-policy coefficients in the event-study plots suggests these estimates suffer from varying degrees of bias associated with imperfect pre-trends (eFigures 16-19).[29-30] The TWFE estimates are much more precisely estimated than the ASC estimates, in part because the TWFE confidence intervals may be overly narrow.[31-33] Nevertheless, this tradeoff highlights this study's focus on generating unbiased estimates at the partial expense of precision.

It is important to interpret these estimates in the context of projected health benefits. Several studies have found that a 15-20% increase (decrease) in price (consumption) generates significant health benefits, including reductions in myocardial infarction events, ischemic heart disease, coronary heart events, strokes, diabetes, and obesity.[34-36] This study estimated a 33.1% increase in price and corresponding 33.0% decrease in volume, suggesting health benefits at least as large as those found previously.

Additionally, studies have suggested that SSB taxes are highly cost effective.[20,35,37] Wang et al. found a nationwide tax could have avoided $17 billion in medical costs between 2010-20. Lee et al. found approximately $53 billion in cost-savings over an average individual lifetime. More recently, White et al. found a 27% reduction in consumption in Oakland, CA is expected to accrue more than $100,000 per 10,000 residents in societal cost savings over a 10-year period. This study's findings suggest SSB taxation would likely generate significant improvements in population health and large cost-savings.

Limitations

First, the retail scanner data identifies purchasing behavior and not direct consumption. It is possible, though unlikely, that taxed populations consumed a different share of purchased SSBs than did untaxed control populations (e.g., wasting more). Second, the data were geocoded by 3-digit zip code. This prevented Berkeley, CA and Albany, CA (3-digit zip code 947) from being included because they (i) could not be separately identified and (ii) were taxed at different times. The 3-digit zip codes for included taxed cities contained a small number of untaxed jurisdictions, accounting for <7% of the total population of these areas (eTable 2). However, this misclassification should only lead to an under-estimate of the changes following the taxes.

We also lacked nutritional information for certain beverage UPCs. Of the UPCs in the scanner data falling under categories considered to contain SSBs, we successfully matched on 84.0% of sales volume (in ounces) using the Label Insight and hand-coded data featuring nutritional information. To the extent the set of unmatched UPCs was similar across taxed and untaxed jurisdictions, the findings should be unaffected. Additionally, the scanner data only contained a sub-sample of all stores in each zip code, and thus did not include all volume sales. Using SSB tax revenues to estimate total volume sales in treated localities, coverage from this set of products was 12.7% (eTable 1). The coverage estimates are similar but slightly lower than recent SSB tax evaluations using Nielsen data.[27,38] Lower coverage in Philadelphia was partially due to exclusion of artificially sweetened beverages from this analysis. Coverage could not be calculated in donor zip codes since there were no SSB taxes in place. However, the ASC estimation generated a reliable counterfactual from the existing sample of donor zip codes, which should mitigate any unintended bias caused by unequal SSB coverage across treatment and control localities.

Next, while the ASC estimates for each individual city in the volume analysis (**Figure 2**; eFigure 6) were similar to those in prior studies,[7] they were relatively imprecise and a null effect could not be rejected at the 5% level. Furthermore, while the composite estimates for the volume analysis were much more precise, reductions in purchases as small as 2% or large as 64% could not be ruled out at a 95% confidence level. While synthetic control methods deliver less biased estimates than difference-in-differences (DiD) approaches, they also generate less statistical power.[39] However, DiD studies involving a small number treated units may underestimate the true variance of effect estimates.[31-33] As more localities introduce SSB taxes, staggered adoption synthetic control methods will feature greater precision.

Only posted shelf prices were observed in the scanner data, which may lead to underestimates of pass-through. While excise taxes are generally reflected in shelf prices, certain retailers may have only included the tax once products were scanned at the register.[40] Moreover, the scanner data was primarily composed of large chain stores. Thus, these results may not extend to independent stores, although similar estimates have been found in those settings.[41] Finally, the five treated localities studied here, while geographically distinct and racially, ethnically, and socioeconomically diverse, were not fully representative of the US population. Therefore, the findings may not be fully generalizable on a national scale, a limitation most relevant to less urban populations.

CONCLUSION

SSB taxes in Boulder, Philadelphia, Oakland, San Francisco, and Seattle led to composite increases in SSB prices (33.1%, 92% pass-through) and reductions in SSB purchases (33.0%), with no offset through cross-border purchases of SSBs. The changes in prices and purchases remained stable in the years following tax implementation. The findings have important implications for the potential efficacy of SSB taxes across larger geographic jurisdictions, and even at the national level. Scaling SSB excise taxes across the US would likely generate significant population health benefits and cost savings.

ACKNOWLEDGEMENTS

Author Contributions. Dr. Kaplan had full access to all of the data in the study and takes responsibility for the integrity of the data and accuracy of the data analysis.
Concept and design: All authors. Acquisition, analysis, or interpretation of the data: Kaplan. Statistical analysis: Kaplan. Visualization: Kaplan, White. Drafting of the manuscript: Kaplan, White. Critical revision of the manuscript for important intellectual content: All authors. Obtained funding: Basu, Schillinger. Supervision: Kaplan, White, Schillinger. The authors would like to thank seminar participants at the University of Illinois, Urbana-Champaign and Cornell University for helpful comments.

Funding: This work was supported by grants from the National Institute on Diabetes and Digestive and Kidney Diseases (R01 DK116852 and 2P30 DK092924), and the Centers for Disease Control and Prevention's National Center for Chronic Disease Prevention and Health Promotion (U18DP006526). The funders had no role in design and conduct of the study; collection, management, analysis, and interpretation of the data; preparation, review, or approval of the manuscript; and decision to submit the manuscript for publication.

Additional Contributions: We thank UC Berkeley undergraduate students Youssef Andrawis, Ryan Andresen, Anqi Chen, Matthew Chill, Maggie Deng, Amanda Gold, Drake Hayes, Nan Hou, Liam Howell, Hongxian Huang, Zixia Huang, Jason Liu, Julie Maeng, Michael Quiroz, Emaan Saddique, Caroline Wu, Yuemin Xu, April You, Haolin Zhang, and Yihui Zhu for assistance with entering and cleaning the nutritional data and beverage classification.

Additional Information. All estimates and analyses in this article are by the authors and not by The Nielsen Company. Researchers' own analyses calculated (or derived) based in part on data from Nielsen Consumer LLC and marketing databases provided through the NielsenIQ Datasets at the Kilts Center for Marketing Data Center at The University of Chicago Booth School of Business. The conclusions drawn from the NielsenIQ data are those of the researchers and do

not reflect the views of NielsenIQ. NielsenIQ is not responsible for, had no role in, and was not involved in analyzing and preparing the results reported herein.

Data Sharing Statement. The Nielsen retail scanner dataset can be accessed by purchasing a subscription with the Kilts Center for Marketing at the University of Chicago Booth School of Business. Replication code may be requested from Dr. Kaplan directly via email.

REFERENCES

1. Dietary Guidelines Advisory Committee. Scientific report of the 2015 Dietary Guidelines Advisory Committee. Washington, DC: U.S. Department of Agriculture, Agricultural Research Service; 2015.
2. Jackson KE, Hamad R, Karasek D, White JS. Sugar-sweetened beverage taxes and perinatal health: A quasi-experimental study. American Journal of Preventive Medicine. 2023 Mar 25.
3. Global Food Research Program, University of North Carolina. 2022. https://www.globalfoodresearchprogram.org/wp-content/uploads/2022/11/Sugary_Drink_Tax_maps_2022_11.pdf
4. Teng AM, Jones AC, Mizdrak A, Signal L, Genç M, Wilson N. Impact of sugar-sweetened beverage taxes on purchases and dietary intake: systematic review and meta-analysis. Obesity Reviews. 2019 Sep;20(9):1187-204.
5. Powell LM, Marinello S, Leider JU, Andreyeva T. A review and meta-analysis of the impact of local US sugar-sweetened beverage taxes on demand. Chicago, IL: Policy, Practice, and Prevention Research Center: University of Illinois Chicago. 2021 Aug.
6. Powell, Lisa M., Samantha Marinello, and Julien Leider. "A Review and Meta-analysis of Tax Pass-through of Local Sugar-Sweetened Beverage Taxes in the United States." *Chicago, IL: Policy, Practice and Prevention Research Center: University of of Illinois Chicago* (2021).
7. Andreyeva T, Marple K, Marinello S, Moore TE, Powell LM. Outcomes following taxation of sugar-sweetened beverages: a systematic review and meta-analysis. JAMA Network Open. 2022 Jun 1;5(6):e2215276-.
8. Barker, A.R., Mazzucca, S., & An, R. (2022). The impact of sugar-sweetened beverage taxes by household income: A multi-city comparison of Nielsen purchasing data. Nutrients, 14(5), 922.
9. Cawley, J., Frisvold, D., & Jones, D. (2020). The impact of sugar-sweetened beverage taxes on purchases: Evidence from four city-level taxes in the United States. Health Economics, 29(10), 1289–1306. https://doi.org/10.1002/hec.4141
10. De Chaisemartin C, d'Haultfoeuille X. Two-way fixed effects estimators with heterogeneous treatment effects. American Economic Review. 2020 Sep 1;110(9):2964-96.
11. National Clinical Care Commission. Report to Congress on Leveraging Federal Programs to Prevent and Control Diabetes and its Complications. US Department of Health and Human Services, 2021.
12. Crosbie E, Pomeranz JL, Wright KE, Hoeper S, Schmidt L. State Preemption: An Emerging Threat to Local Sugar-Sweetened Beverage Taxation. Am J Public Health. 2021;111(4):677-86. doi: 10.2105/ajph.2020.306062. PubMed PMID: 33600243.
13. Borusyak, K., Jaravel, X. & Spiess, J. (2021) Revisiting event study designs: robust and efficient estimation.
14. Goodman-Bacon, A. (2021) Difference-in- differences with variation in treatment timing. *Journal of Econometrics*. In press. https://doi.org/10.1016/j.jecon om.2021.03.014
15. Von Elm E, Altman DG, Egger M, Pocock SJ, Gøtzsche PC, Vandenbroucke JP. The Strengthening the Reporting of Observational Studies in Epidemiology (STROBE) statement: guidelines for reporting observational studies. The Lancet. 2007 Oct 20;370(9596):1453-7.
16. The Open Data Initiative. 2020. https://www.labelinsight.com/open-data. Accessed December 9, 2020.
17. Ben-Michael, Eli, Avi Feller, and Jesse Rothstein. "The augmented synthetic control method." *Journal of the American Statistical Association* 116, no. 536 (2021): 1789-1803.

18. Abadie A, Diamond A, Hainmueller J. Synthetic control methods for comparative case studies: Estimating the effect of California's tobacco control program. *Journal of the American Statistical Association.* 2010;105(490):493-505.
19. Abadie A. Using Synthetic Controls: Feasibility, Data Requirements, and Methodological Aspects. *J Econ Lit.* 2021;59(2):391-425.
20. White JS, Basu S, Kaplan S, Madsen KA, Villas-Boas SB, Schillinger D. Evaluation of the sugar-sweetened beverage tax in Oakland, United States, 2015–2019: A quasi-experimental and cost-effectiveness study. PLoS medicine. 2023 Apr 18;20(4):e1004212.
21. Han E, Powell LM. Consumption Patterns of Sugar-Sweetened Beverages in the United States. J Acad Nutr Diet. 2013;113(1):43-53. doi: https://doi.org/10.1016/j.jand.2012.09.016.
22. Powell ES, Smith-Taillie LP, Popkin BM. Added Sugars Intake Across the Distribution of US Children and Adult Consumers: 1977-2012. J Acad Nutr Diet. 2016;116(10):1543-50.e1. Epub 2016/08/06. doi: 10.1016/j.jand.2016.06.003. PubMed PMID: 27492320; PubMed Central PMCID: PMCPMC5039079.
23. Rosinger A, Herrick KA, Gahche JJ, Park S. Sugar-sweetened beverage consumption among U.S. youth, 2011-2014. NCHS data brief. 2017;(271). PubMed PMID: cdc:44039.
24. Wiltshire, Justin C. "allsynth: Synthetic Control Bias-Correction Utilities for Stata." In *In2021 Stata Conference*, no. 15. 2021.
25. Cawley J, Frisvold D, Hill A, Jones D. The impact of the Philadelphia beverage tax on purchases and consumption by adults and children. Journal of Health Economics. 2019 Sep 1;67:102225.
26. Seiler S, Tuchman A, Yao S. The impact of soda taxes: Pass-through, tax avoidance, and nutritional effects. Journal of Marketing Research. 2021 Feb;58(1):22-49.
27. Powell LM, Leider J. The impact of Seattle's sweetened beverage tax on beverage prices and volume sold. Economics & Human Biology. 2020 May 1;37:100856.
28. Cawley J, Frisvold D, Hill A, Jones D. Oakland's sugar-sweetened beverage tax: impacts on prices, purchases and consumption by adults and children. Economics & Human Biology. 2020 May 1;37:100865.
29. Roth J. Pretest with caution: Event-study estimates after testing for parallel trends. American Economic Review: Insights. 2022 Sep 1;4(3):305-22.
30. Rambachan A, Roth J. A more credible approach to parallel trends. Review of Economic Studies. 2023 Feb 15:rdad018.
31. Bertrand M, Duflo E, Mullainathan S. How much should we trust differences-in-differences estimates?. The Quarterly journal of economics. 2004 Feb 1;119(1):249-75.
32. Cameron AC, Gelbach JB, Miller DL. Bootstrap-based improvements for inference with clustered errors. The review of economics and statistics. 2008 Aug 1;90(3):414-27.
33. Conley TG, Taber CR. Inference with "difference in differences" with a small number of policy changes. The Review of Economics and Statistics. 2011 Feb 1;93(1):113-25.
34. Wilde P, Huang Y, Sy S, Abrahams-Gessel S, Jardim TV, Paarlberg R, Mozaffarian D, Micha R, Gaziano T. Cost-effectiveness of a US national sugar-sweetened beverage tax with a multistakeholder approach: who pays and who benefits. American journal of public health. 2019 Feb;109(2):276-84.
35. Wang YC, Coxson P, Shen YM, Goldman L, Bibbins-Domingo K. A penny-per-ounce tax on sugar-sweetened beverages would cut health and cost burdens of diabetes. Health Affairs. 2012 Jan 1;31(1):199-207.
36. Long MW, Gortmaker SL, Ward ZJ, Resch SC, Moodie ML, Sacks G, Swinburn BA, Carter RC, Wang YC. Cost effectiveness of a sugar-sweetened beverage excise tax in the US. American journal of preventive medicine. 2015 Jul 1;49(1):112-23.
37. Lee Y, Mozaffarian D, Sy S, Liu J, Wilde PE, Marklund M, Abrahams-Gessel S, Gaziano TA, Micha R. Health impact and cost-effectiveness of volume, tiered, and absolute sugar content sugar-sweetened beverage tax policies in the United States: a microsimulation study. Circulation. 2020 Aug 11;142(6):523-34.
38. Leider J, Powell LM. Longer-term impacts of the Oakland, California, sugar-sweetened beverage tax on prices and volume sold at two-years post-tax. Social Science & Medicine. 2022 Jan 1;292:114537.
39. O'Neill S, Kreif N, Grieve R, Sutton M, Sekhon JS. Estimating causal effects: considering three alternatives to difference-in-differences estimation. Health Services and Outcomes Research Methodology. 2016 Jun;16:1-21.
40. Cawley J, Frisvold D, Jones D, Lensing C. The pass-through of a tax on sugar-sweetened beverages in Boulder, Colorado. American Journal of Agricultural Economics. 2021 May;103(3):987-1005.

41. Bleich SN, Lawman HG, LeVasseur MT, Yan J, Mitra N, Lowery CM, Peterhans A, Hua S, Gibson LA, Roberto CA. The Association Of A Sweetened Beverage Tax With Changes In Beverage Prices And Purchases At Independent Stores: Study compares changes in sweetened beverage prices and purchases before and twelve months after tax implementation, at small, independent stores in Philadelphia. Health Affairs. 2020 Jul 1;39(7):1130-9.

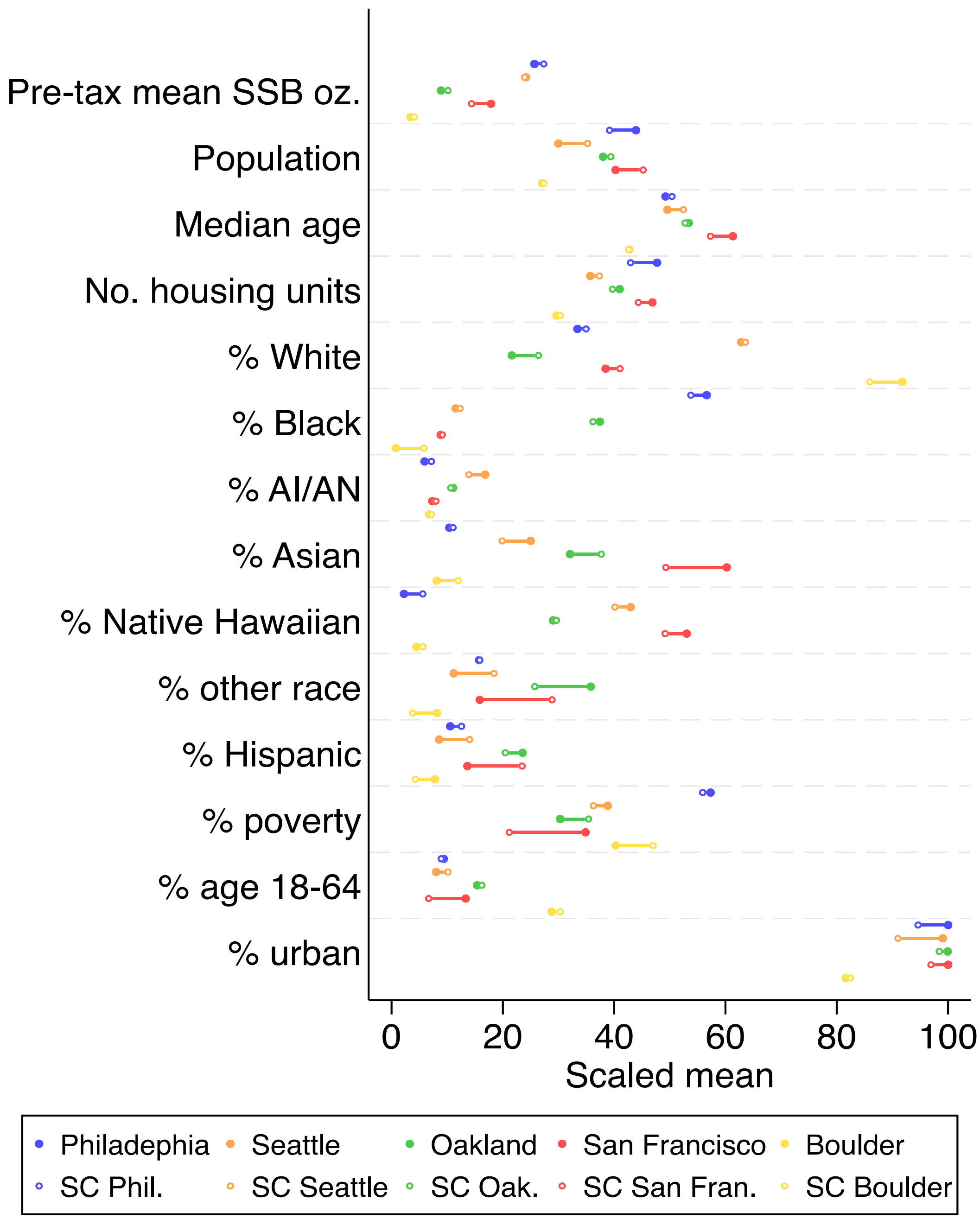
Table 1. Descriptive Statistics of 3-Digit Zip Codes in Primary Analysis

	3-Digit Zip Code						
	941 (SF)	946 (Oak.)	191 (Phil.)	803 (Boul.)	981 (Sea.)	Borders[a]	Donors[b]
Number of 3-Digit Zips	1	1	1	1	1	13	279
Number of Stores	103	41	213	26	113	1,340	24,502
Date Tax Implemented	1/1/18	7/1/17	1/1/17	7/1/17	1/1/18	---	---
# Months (in Data) Pre-Tax	72	66	60	66	72	---	---
# Months (in Data) Post-Tax	24	30	36	30	24	---	---
$/Ounce	0.01	0.01	0.015	0.02	0.0175	---	---

[a] Border 3-digit zip codes comprise all immediately adjacent 3-digit zip codes to each of the five treated zip codes, and include 800, 804, 805, 945, 948, 080, 081, 940, 949, 980, 982, 983, 984.

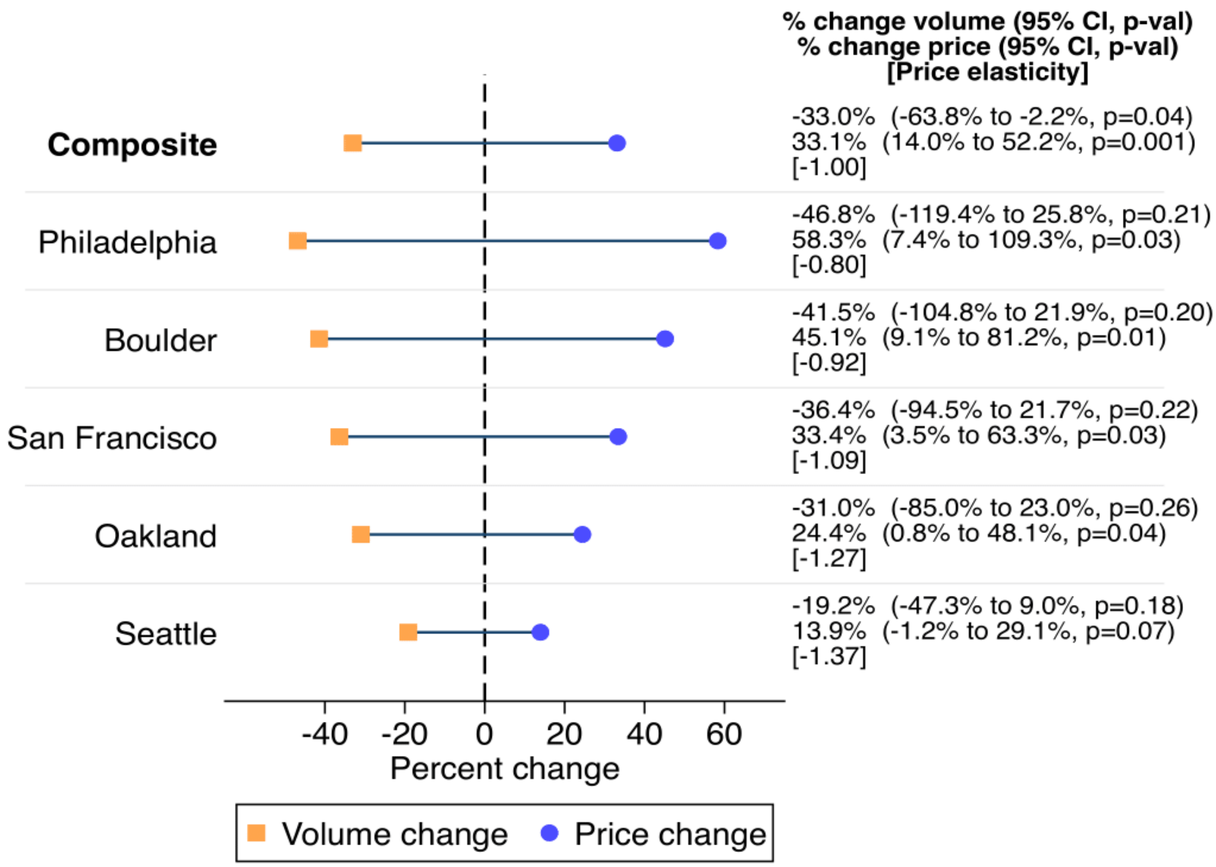
[b] Donor zip codes consist of all 3-digit zip codes with a "% Urban" value within one standard deviation (0.35) of the mean urbanicity of the five treated localities (0.98).

Figure 1. Comparing Treated and Synthetic Values of Prognostic Factors from the Analysis of SSB Volume Purchased



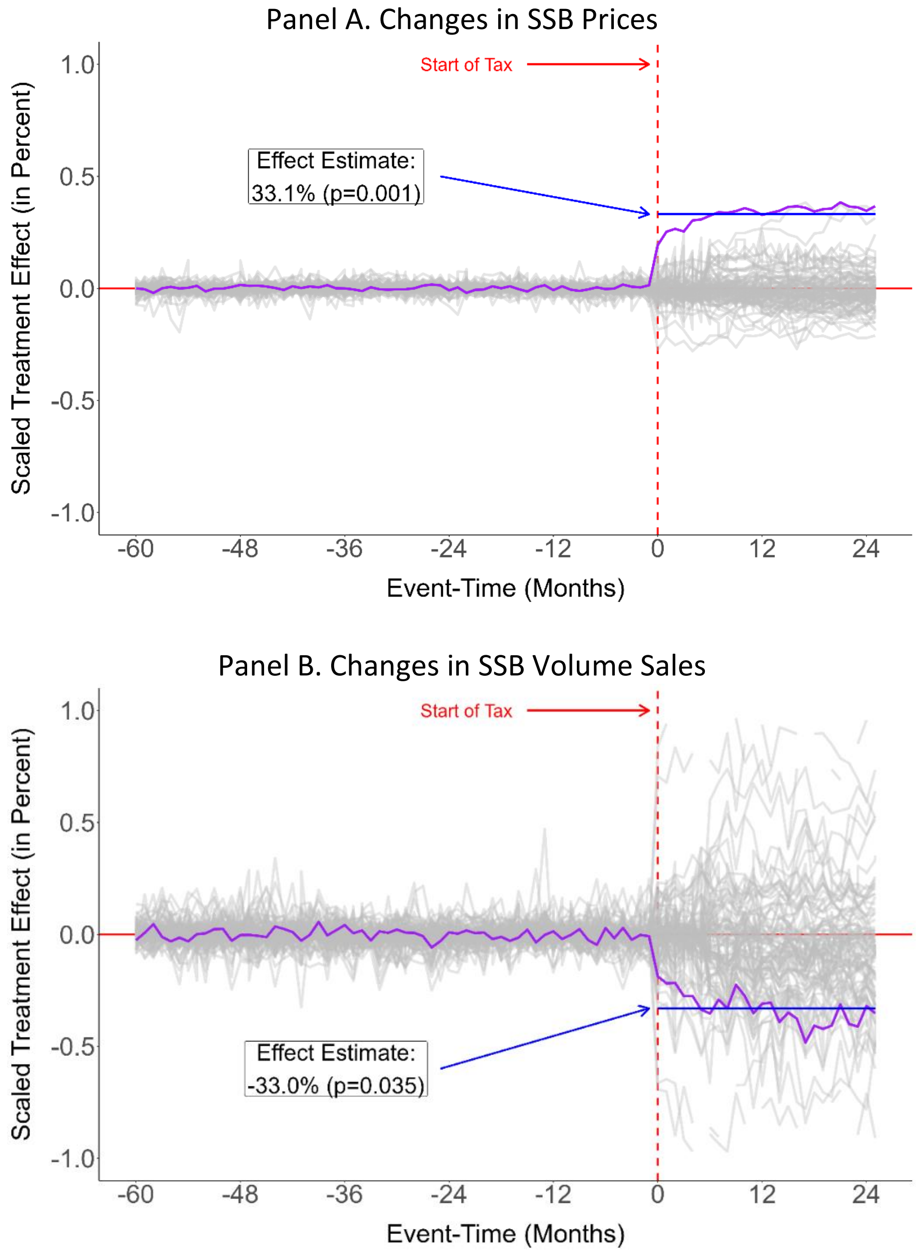
Note: This plot shows the scaled mean values of pre-tax outcomes and prognostic covariates included in the synthetic control analysis of SSB volume purchased. Mean values are scaled to be between 0 and 100 based on each variable's maximum and minimum values found in the primary sample. Shaded dots correspond to the mean value for a treated city, and hollow dots correspond to its synthetic control ("SC").

Figure 2. Composite and Individual Locality Demand Elasticity Estimates



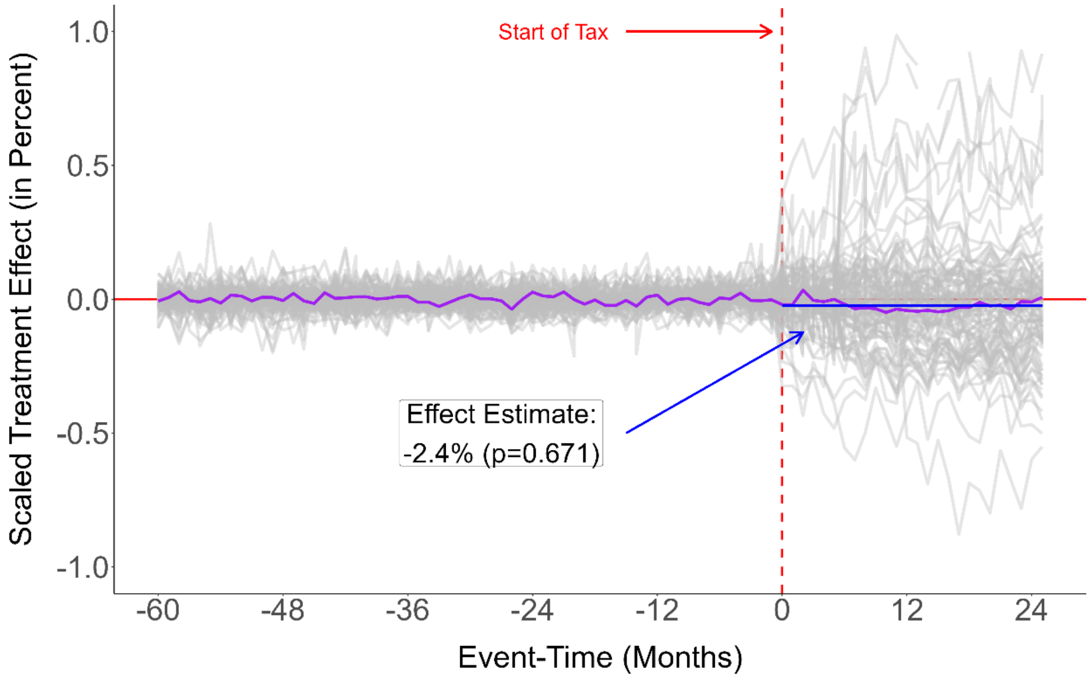
Note: This plot shows the % change in volume sold (in ounces) and % change in shelf prices for the augmented synthetic control staggered adoption composite analysis and the same information for augmented synthetic control analyses of the five treated localities individually. Price elasticities of demand are provided in brackets, and 95% confidence intervals and p-values for each estimation are provided in parentheses.

Figure 3. Augmented Synthetic Control Estimates for Composite Changes in Price and Volume Sales of SSB Products



Note: Panel a) shows the % change in volume sold (in ounces), and panel b) the % change in shelf prices in response to implementing an excise SSB tax for the staggered adoption composite analysis. The bolded purple line represents the composite treated unit, while the lightly shaded grey lines represent in-space placebo estimates from the donor pool. % changes are calculated with respect to the average of the pre-treatment means of each of the five treated localities. The composite effect size estimates and p-values are provided in the designated box of each panel.

Figure 4. Augmented Synthetic Control Estimates of Composite Changes in Volume Sales of SSB Products in Border Areas



Note: This figure shows the staggered adoption composite analysis % change in volume sold (in ounces) in immediately adjacent bordering 3-digit zip codes in response to implementing an excise SSB tax in the five treated zip codes. The bolded purple line represents the composite adjacent border unit, while the lightly shaded grey lines represent in-space placebo estimates from the donor pool. % changes are calculated with respect to the average of the pre-treatment means of each of the twelve adjacent border localities. The composite effect size estimates and p-values are provided in the designated box.

Supplementary Material

eMETHODS

1. Nielsen Data and Beverage Classification Procedure

The Nielsen retail scanner dataset was made available through a subscription to the Kilts Marketing Center at the University of Chicago Booth School of Business. The data is comprised of ten general product categories: HEALTH AND BEAUTY CARE, DRY GROCERY, FROZEN FOODS, DAIRY, DELI, PACKAGED MEAT, FRESH PRODUCE, NON-FOOD GROCERY, ALCOHOLIC BEVERAGES, GENERAL MERCHANDIZE, and UNCLASSIFIED. We query all products falling within the DRY GROCERY category. Within the DRY GROCERY category, we select the following subcategories of products, which form the sample of UPCs we initially work with: JUICE DRINKS – CANNED, BOTTLED; CARBONATED BEVERAGES; and SOFT DRINKS – NON-CARBONATED.

Because the Nielsen data provides limited nutritional information about each product, we leverage 10-digit UPC data from Label Insight (LI) and hand-coded data from a previous study[1] to classify individual products into SSB vs. non-SSB status. These two datasets include information regarding total calories, total sugar, added sugar per serving, serving size, and presence of artificial sweeteners, which allowed us to ascertain the SSB status for each UPC. Because the Nielsen data provides UPCs in EAN-13 format (with the check digit dropped), and LI and the hand-coded data contain information at the 10-digit UPC level, we convert the Nielsen 12-digit UPCs into 10-digit UPCs. We do so by following the procedure laid out in documentation from Label Insights to merge their UPC data with Nielsen UPC data, i.e. we drop the first two digits of Nielsen UPCs and the first and last digits of Label Insights UPCs. Any UPCs that become duplicated due to this procedure are simply aggregated together. We also took this classification approach with the hand-coded data.

We were successful in matching on 18,147 10-digit UPCs, which make up 84.0% of the total sales volume in the Nielsen beverage data over our study period. Of the matched 10-digit UPCs, 5,500 are classified as SSBs, which account for 39.7% of volume sales in the matched data. This forms the set of products we use in our analyses.

2. Augmented Synthetic Control Model

Synthetic control models have become widely used in panel data analyses to assess policy changes.[2-3] These models are advantageous because they algorithmically create a counterfactual unit that can be directly compared to a treated unit of interest without worrying about fundamental differences in outcomes or characteristics of the two groups, by construction. Using our setting as an example, the base synthetic control model matches pre-tax outcomes and covariates of taxed and untaxed units by weighting each untaxed unit in such a way that the "synthetic" unit(s) closely match the taxed unit(s) on both the outcome measure of interest and covariate characteristics. In particular, for each outcome Y_{im} for 3-digit zip code i in month m, and X_i 3-digit zip code-level covariates (which in our case are time-invariant), the method chooses weights for each untaxed 3-digit zip code j (w_j) to minimize the distance $(Y_{im}, X_i) - \sum_j w_j \left(Y_{jm}, X_j\right)$.[4]

Recent work has extended the initial synthetic control approach in various ways.[5-7] There are two notable enhancements of the original synthetic control method that we leverage in this study. First, the original synthetic control framework was designed to estimate the impact of an intervention on a single treated unit. In our setting, we study multiple treated units that experience treatment at different times, referred to as a "staggered adoption" setup.[8] Second, the use of the original synthetic control method was recommended only when the synthetic unit's pretreatment outcomes closely matched the pre-treatment outcomes of the treated unit. Our study takes advantage of recent work that relaxes this requirement by introducing a "bias-correction" procedure. This estimation framework is called the augmented synthetic control (ASC) model, since it augments the original synthetic control approach with an outcome model that is used to determine bias as a result of a relatively poor pretreatment fit between the treated and synthetic units, and then uses the output to remove the bias in the pretreatment period.[9] While there are several different outcome models that can be used to de-bias the synthetic control model, the primary method used is a ridge regression model.[9] A ridge regression model estimates a linear regression of post-treatment outcomes of the control units $(Y_{jm} | m \geq T)$, where T indicates the month of tax implementation, on the centered pretreatment outcomes of the control units ($Y_{jm}|m < T$). This modification allows certain donor units to be assigned negative weights (whereas the original synthetic control procedure restricts all weights on donor units to be ≥ 0), which can improve pretreatment fit. Additional structure and details of this procedure can be found in sections 2-4 of Ben-Michael, Feller, and Rothstein (2021).

Sociodemographic and geographic characteristics used in constructing synthetic units were taken from the 2010 Decennial Census and 2016 American Community Survey. Characteristics included population size (2010), median household income (2016), racial/ethnic composition (proportion non-Hispanic White, non-Hispanic Black, Hispanic, Asian, and American Indian/Alaska Native 2010), proportion in poverty (household income <$10,000K, 2016), proportion of individuals 18 to 64 years old (2010), number of housing units (2010), and percentage of the population defined as urban (2010).

One important implication of the use of synthetic control methods is the importance of a donor pool consisting of units that could plausibly act as reasonable controls for the treated units.[4] Failure to do so can lead to substantial bias in the estimation. Because of this, we decide to limit the donor pool of 3-digit zip codes to those with urbanicity levels that are similar to the treated units. Using a measure of urbanicity is desirable for different reasons. First, it's easily defined by and computed using information and data from the US Census.[10] Second, urbanicity captures several observed and unobserved characteristics that are likely to influence the relative similarity among control and treated units, including characteristics we include like population, median household income, number of housing units, etc., as well as characteristics we do not observe, like housing prices, police presence, and voter party alignment. Finally, our five treated localities have an average urbanicity level of 0.98, which ranges from a minimum of 0.94 (Boulder) to 1 (Philadelphia and San Francisco). This relative similarity between the treated units' urbanicity allows for the construction of a donor pool that could plausibly act as reasonable controls for each of the treated units, while keeping the donor pool the same for each.

In the primary augmented synthetic control estimation, we use a subsample of control (donor) units that fall within one standard deviation of the average urbanicity level of the five treated localities. In the cross-border shopping analyses, we use a subsample of donor units that fall within one standard deviation of the average urbanicity level of the thirteen adjacent border localities. Robustness checks, which are included in eFigures 10-15, include control units with urbanicity levels >0.85 and >0.9. The results from these supplementary estimations are qualitatively unchanged.

Because implementation of the tax happened at different times across the five treated localities (hence the “staggered adoption” nature of the BCSC procedure), calendar time is converted to event-time, which normalizes time = 0 to the month when the tax went into place in each treated locality. Therefore, in event time we observe a different number of total time periods for each taxed locality. Consequently, we provide results from a “balanced” estimation, which only considers event-time periods when all treated localities are present in the sample. This is done to avoid biasing the estimation in favor of taxed localities that are observed in the data during event-time periods when other taxed localities may not be observed.

To determine the statistical significance of our augmented synthetic control average treatment effects, which are calculated as the average post-tax percent change in SSB purchases (shelf prices), we use an in-space placebo generation inference procedure.[3,11] For each of the five treated localities, we generate in-space placebo estimates for each donor pool unit one-by-one as if each unit had been treated. Because treated localities implement taxes at different times, we repeat this procedure for each of the five different treated localities, which generates 279*5=1,395 placebo estimates. To generate p-values, we compute the ratio of mean squared prediction error (RMSPE) in the post-tax vs. pre-tax period for the composite unit estimate and each of the placebo unit estimates, and rank them from largest to smallest.[11] The p-value for the estimation is calculated as the ratio of the composite unit numerical ranking with respect to the total number of units (1,396). Each of the BCSC plots takes 100 quasi-randomly selected placebo lines from the universe of 1,396 placebos for the composite estimation and 279 for each of the individual city estimations. This selection procedure is quasi-random in the sense that the universe of eligible placebos to be chosen is “pruned” to those that exhibit a pre-period MSPE that is no greater than five times the pre-period MSPE of the treated unit. Confidence intervals were obtained from p-values using the method outlined by Altman.[12]

3. Two-Way Fixed Effects (TWFE) Model

Two-way fixed effects (TWFE) models make up one of the most common empirical approaches to identifying the impact of a treatment (e.g. policy intervention) using panel data.[13-14] This approach has also been often used in the SSB tax evaluation literature.[1,16-17]

Using this conventional approach, we estimate a series of TWFE models and TWFE event study models. The simple TWFE model takes the following form:

$$Y_{it} = \beta Tax_{it} + \alpha_i + \delta_t + \epsilon_{it}$$

Where Y_{it} indicates the outcome variable of interest (i.e. volume purchased or shelf prices) in 3-digit zip code i in month-year t, Tax_{it} is a binary variable =1 if 3-digit zip code i has an SSB tax in place during month-year t and =0 otherwise, and α_i and δ_t represent 3-digit zip code and month-year fixed effects, respectively. β measures the treatment effect associated with the implementation of an SSB tax on the outcome variable of interest. We estimate such a TWFE model to determine both the composite effect (by including all five treated 3-digit zip codes) as well as individual city effects (separate estimations for each of the five treated 3-digit zip codes). eTable 3 presents the TWFE estimates for each of these specifications.

We also estimate a TWFE event study specification, which estimates individual coefficients for each month-year in event-time, which is normalized to 0 at the month-year when an SSB tax is implemented in 3-digit zip code i. Again, we estimate a TWFE event study to determine both the composite effect (by including all five treated 3-digit zip codes) as well as individual city effects (separate estimations for each of the five treated 3-digit zip codes). The TWFE event-study model takes the following form:

$$Y_{ite} = \sum_{e=-a \setminus \{-1\}}^{b} \beta_e \, Tax_{e,it} + \alpha_i + \delta_t + \epsilon_{ite}$$

Where e represents the month-year in event-time, ranging from $-a$ to b. The period prior to implementation of an SSB tax (-1) is omitted. β_e is a vector of coefficients indexed by event-time that can be interpreted relative to the omitted event-time period. $Tax_{e,it} = 1$ if 3-digit zip code i has been treated at event-time e. eFigure 16 presents the event study results for the composite estimation, while eFigures 17-19 present the event study results for the individual city estimations.

REFERENCES

1. White JS, Basu S, Kaplan S, Madsen KA, Villas-Boas SB, Schillinger D. Evaluation of the sugar-sweetened beverage tax in Oakland, United States, 2015–2019: A quasi-experimental and cost-effectiveness study. PLoS medicine. 2023 Apr 18;20(4):e1004212.
2. Abadie A, Gardeazabal J. The economic costs of conflict: A case study of the Basque Country. American economic review. 2003 Mar 1;93(1):113-32.
3. Abadie A, Diamond A, Hainmueller J. Synthetic control methods for comparative case studies: Estimating the effect of California's tobacco control program. Journal of the American Statistical Association. 2010;105(490):493-505.
4. Abadie A. Using Synthetic Controls: Feasibility, Data Requirements, and Methodological Aspects. J Econ Lit. 2021;59(2):391-425.
5. Abadie A, L'hour J. A penalized synthetic control estimator for disaggregated data. Journal of the American Statistical Association. 2021 Oct 2;116(536):1817-34.
6. Athey S, Bayati M, Doudchenko N, Imbens G, Khosravi K. Matrix Completion Methods for Causal Panel Data Models. arXiv e-prints, page. arXiv preprint arXiv:1710.10251. 2017.
7. Arkhangelsky D, Athey S, Hirshberg DA, Imbens GW, Wager S. Synthetic difference-in-differences. American Economic Review. 2021 Dec 1;111(12):4088-118.
8. Ben-Michael E, Feller A, Rothstein J. Synthetic controls with staggered adoption. Journal of the Royal Statistical Society Series B: Statistical Methodology. 2022 Apr;84(2):351-81.
9. Ben-Michael, Eli, Avi Feller, and Jesse Rothstein. "The augmented synthetic control method." *Journal of the American Statistical Association* 116, no. 536 (2021): 1789-1803.
10. Urban and Rural. US Census. https://www.census.gov/programs-surveys/geography/guidance/geo-areas/urban-rural.html
11. Wiltshire, Justin C. "allsynth: Synthetic Control Bias-Correction Utilities for Stata." In *In2021 Stata Conference*, no. 15. 2021.
12. Altman DG, Bland JM. How to obtain the confidence interval from a P value. Bmj. 2011 Aug 8;343.
13. De Chaisemartin C, d'Haultfoeuille X. Two-way fixed effects estimators with heterogeneous treatment effects. American Economic Review. 2020 Sep 1;110(9):2964-96.
14. Roth J. Pretest with caution: Event-study estimates after testing for parallel trends. American Economic Review: Insights. 2022 Sep 1;4(3):305-22.
15. Cawley, J., Frisvold, D., & Jones, D. (2020). The impact of sugar-sweetened beverage taxes on purchases: Evidence from four city-level taxes in the United States. Health Economics, 29(10), 1289–1306. https://doi.org/10.1002/hec.4141
16. Taylor RL, Kaplan S, Villas-Boas SB, Jung K. Soda wars: The effect of a soda tax election on university beverage sales. Economic Inquiry. 2019 Jul;57(3):1480-96.

eTable 1. Total Coverage of SSB Ounces Sold in Matched Nielsen Retail Scanner Data

City (first complete fiscal year of SSB tax)	Tax Revenue ($000's)	Tax ($/Ounce)	Total SSB Sales (1000s of Ounces)	SSB Sales of Nielsen UPCs (1000s of Ounces)	Coverage (%)
Boulder (2018)	$4,868	$.02	243,400	50,781	20.86%
Oakland (Jul 2017–Jun 2018)	$11,076	$.01	1,107,600	171,850	15.52%
Philadelphia (Jul 2017–Jun 2018)	$77,421	$.015	5,161,400	240,146	4.65%
San Francisco (Jul 2018–Jun 2019)	$16,098	$.01	1,609,800	287,089	17.83%
Seattle (2018)	$22,254	$.0175	1,271,657	404,600	31.82%
Composite	$131,717	$.0145	9,083,931	1,154,468	12.71%

*Note***:** Tax revenues taken from Krieger et al. (2021).[1] Coverage estimates use the first fiscal year of each city's respective tax implementation. Lower coverage in Philadelphia is in part due to the exclusion of artificially sweetened beverages in our analysis. The tax amount for the Composite geographic unit is the unweighted average of the tax amounts across the five taxed cities.

[1] Krieger J, Magee K, Hennings T, Schoof J, Madsen KA. How sugar-sweetened beverage tax revenues are being used in the United States. Preventive Medicine Reports. 2021 Sep 1;23:101388.

eTable 2. Total Population (2010) by City within Taxed 3-Digit Zip Codes

3-Digit Zip Code	City	Tax Status	Population (2010)	% of 3-Digit Zip Code Population	% of Overall Population
803	Boulder	Tax	97,724	100.00%	2.66%
946	Oakland	Tax	391,350	94.95%	10.64%
	Emeryville	No Tax	10,110	2.45%	0.27%
	Piedmont	No Tax	10,709	2.60%	0.29%
191	Philadelphia	Tax	1,528,000	99.61%	41.53%
	Manayunk	No Tax	5,913	0.39%	0.16%
941	San Francisco	Tax	805,519	100.00%	21.89%
981	Seattle	Tax	610,654	73.57%	16.60%
	Tukwila	No Tax	19,161	2.31%	0.52%
	Bainbridge Island	No Tax	23,062	2.78%	0.63%
	Shoreline	No Tax	53,182	6.41%	1.45%
	Burien	No Tax	48,224	5.81%	1.31%
	Des Moines	No Tax	29,775	3.59%	0.81%
	Normandy Park	No Tax	6,335	0.76%	0.17%
	Seatac	No Tax	26,999	3.25%	0.73%
	Lake Forest Park	No Tax	12,639	1.52%	0.34%
	TOTAL	Tax	3,433,247	--	**93.31%**
	TOTAL	No Tax	246,109	--	**6.69%**

Note: Population estimates for each city taken from 2010 (source: US Census Bureau). In the "981" 3-digit zip code, some untaxed cities (e.g. Bainbridge Island) overlap with other untaxed 3-digit zip codes (e.g. Bainbridge Island includes areas in the 980 and 983 zip codes). Therefore, population estimates for untaxed cities in the "981" 3-digit zip code may include people from untaxed 3-digit zip codes. Because of this, the estimate of the % of the population covered by an SSB tax in the "981" 3-digit zip code is conservative (underestimated).

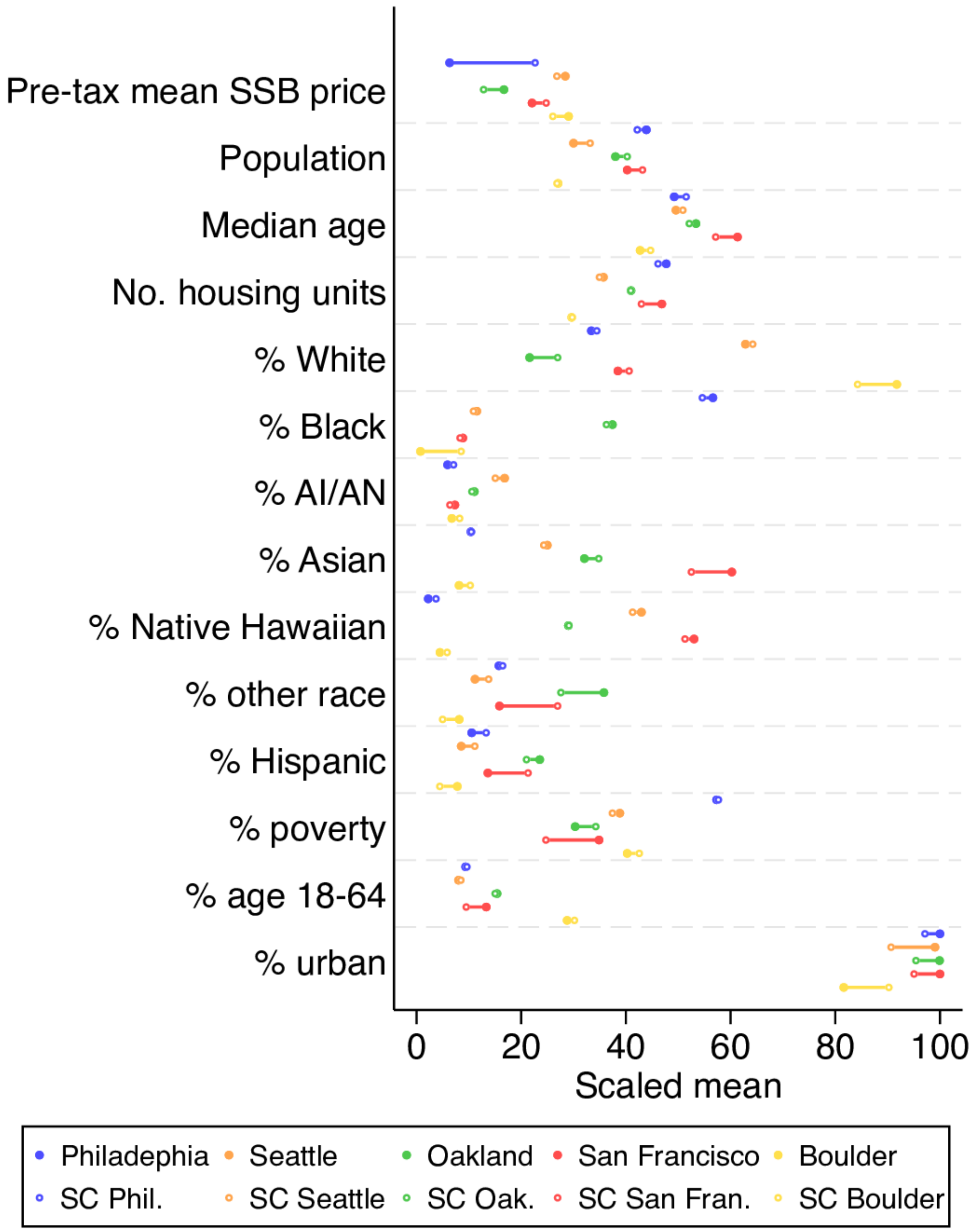
eTable 3. Two-Way Fixed Effects Estimation Results for Composite and Individual City Analyses

	Volume Purchases (Oz.)	Avg. Price per Oz.	Border Volume Purchases (Oz.)
Composite (Balanced)	-10,087,267.0***	0.0123***	-2,486,372.0**
	(2,711,915.0)	(0.0026)	(883,365.0)
Dep. Var. Pretreatment Mean	27,850,700	0.041	42,345,118
Percent Change (%)	-36.22	30.38	-5.87
Observations	16,980	16,980	21,984
Boulder	-4,509,647.0***	0.0215***	906,480.0
	(507,729.0)	(0.0003)	(1,000,519.0)
Dep. Var. Pretreatment Mean	6,112,734	0.045	3,378,103
Percent Change (%)	-73.77	47.39	26.83
Observations	27,440	27,440	36,162
San Francisco	-12,068,979.0***	0.0142***	-5,667,943.0**
	(586,217.0)	(0.0003)	(1,929,936.0)
Dep. Var. Pretreatment Mean	31,0762,01	0.041	72,366,839
Percent Change (%)	-38.84	34.33	-7.83
Observations	27,440	27,440	36,162
Philadelphia	-24,102,200.0***	0.0215***	-4,808,432.0
	(453,161.0)	(0.0003)	(2,464,901.0)
Dep. Var. Pretreatment Mean	44,5485,78	0.032	42,233,523
Percent Change (%)	-54.1	66.1	-11.39
Observations	27,440	27,440	36,162
Oakland	-4,114,207.0***	0.0092***	-5,022,741.0
	(507,729.0)	(0.0003)	(2,668,559.0)
Dep. Var. Pretreatment Mean	15,561,263	0.038	16,544,381
Percent Change (%)	-26.44	23.87	-30.36
Observations	27,440	27,440	36,162
Seattle	-12,428,611.0***	0.0057***	-1,366,042.0
	(586,217.0)	(0.0003)	(1,543,743.0)
Dep. Var. Pretreatment Mean	41,954,725	0.045	38,603,038
Percent Change (%)	-29.62	12.59	-3.54
Observations	27,440	27,440	36,162

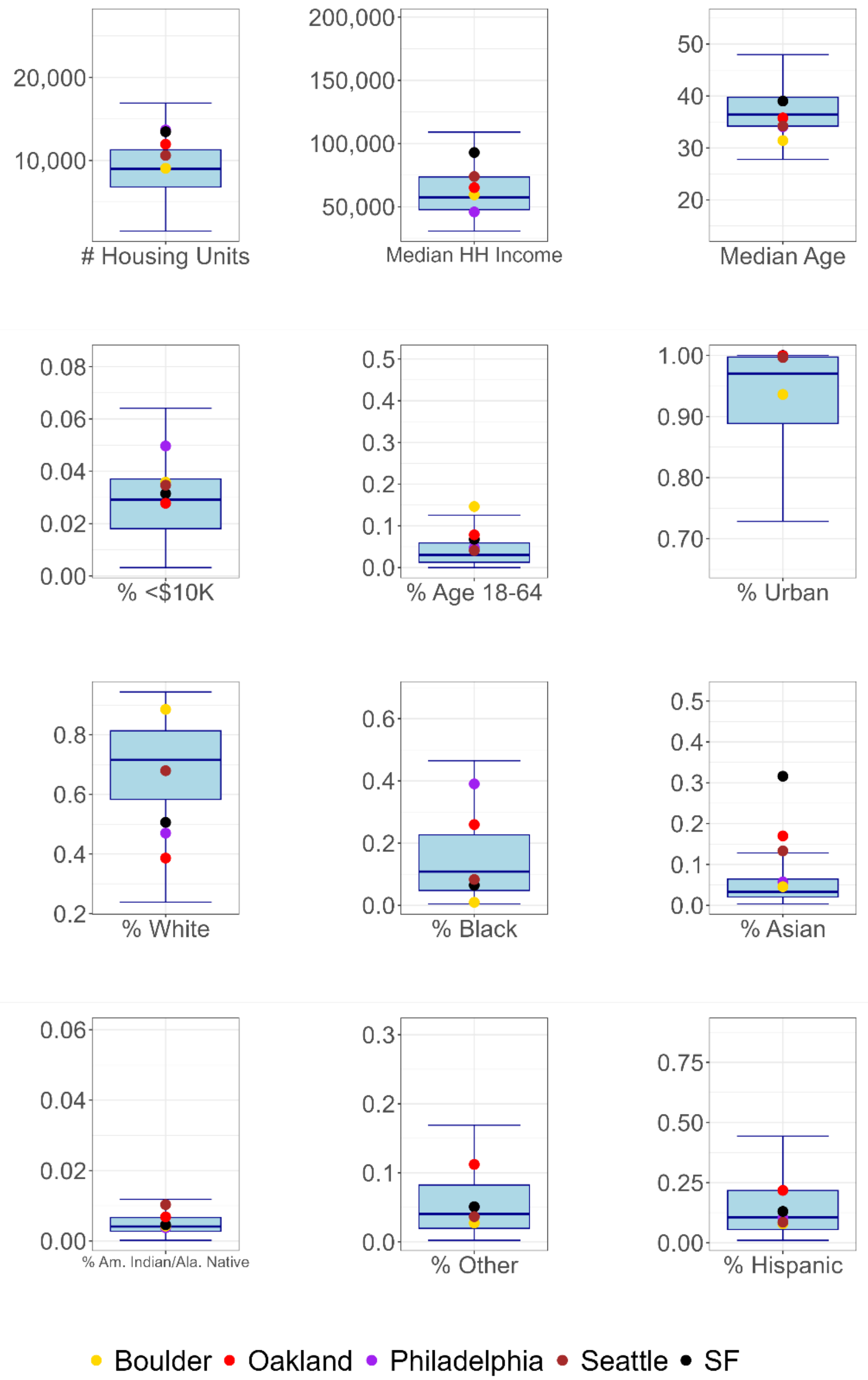
Note: *p<0.05**p<0.01***p<0.001

All specifications include 3-digit zip code and month-year fixed effects. Standard errors are robustly estimated and clustered at the 3-digit zip code level.

eFigure 1. Comparing Treated and Synthetic Values of Prognostic Factors from the Analysis of SSB Shelf Prices

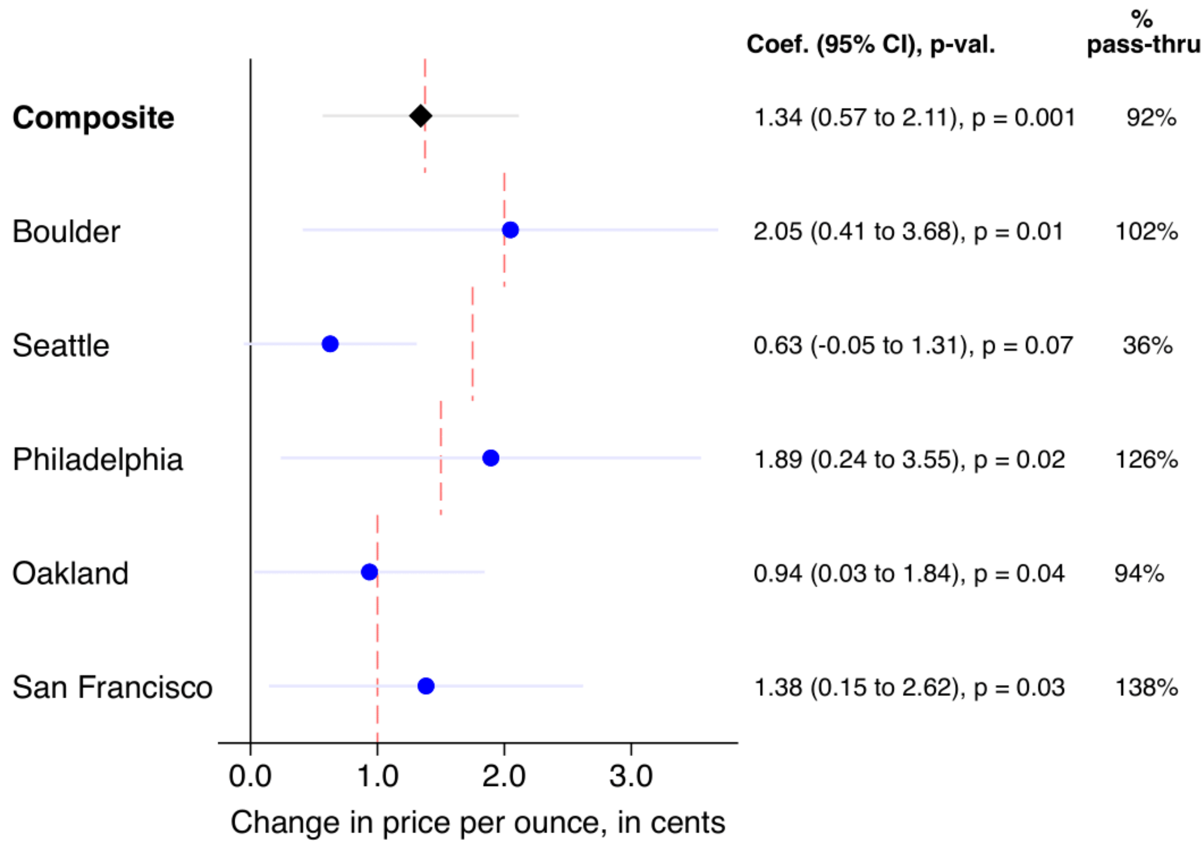


eFigure 2. Overlap of US Census Sociodemographic Characteristics between each taxed city and the donor pool of control 3-digit zip codes



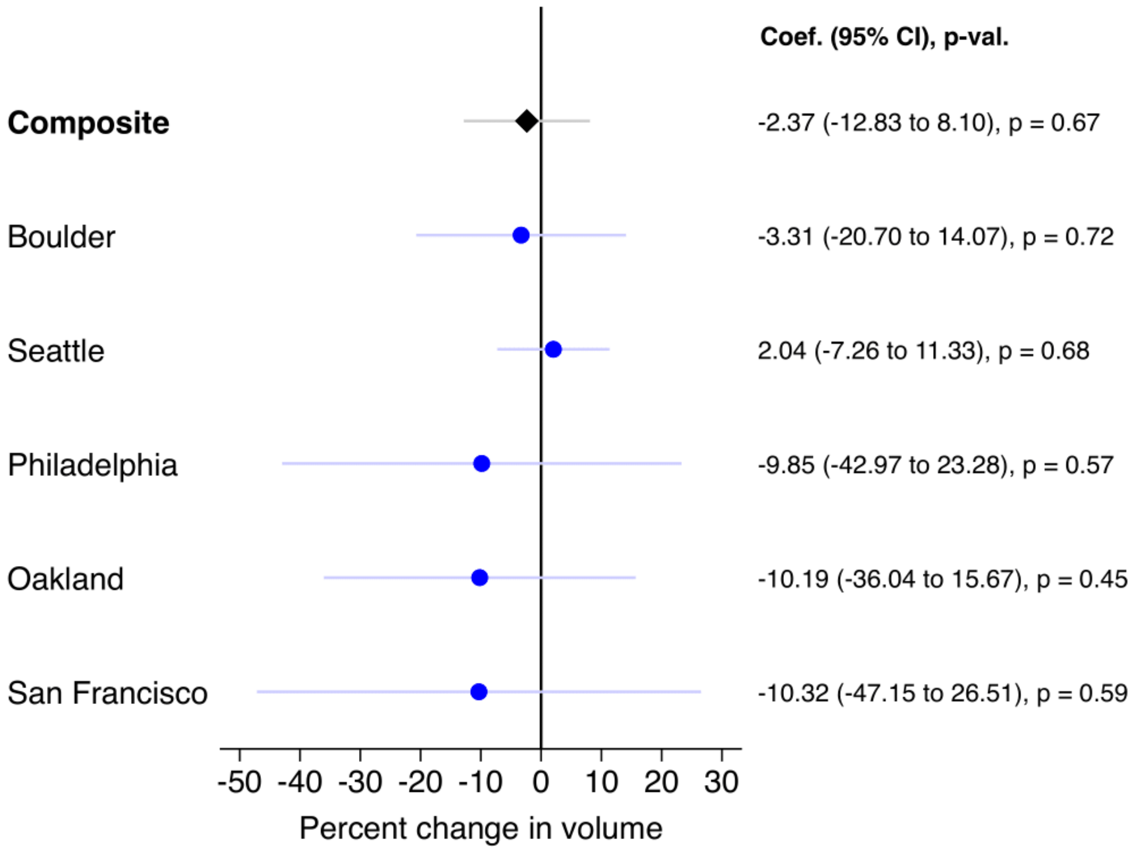
Note: Metrics for each 3-digit zip code were taken from either the 2010 Census or 2016 American Community Survey (ACS). Colored points on each plot represent values for each of the five treated localities. Box plots for each characteristic are formed from the distribution within the subsample of 3-digit zip codes used in the primary analysis.

eFigure 3. Composite and Individual Locality Price Pass-Through



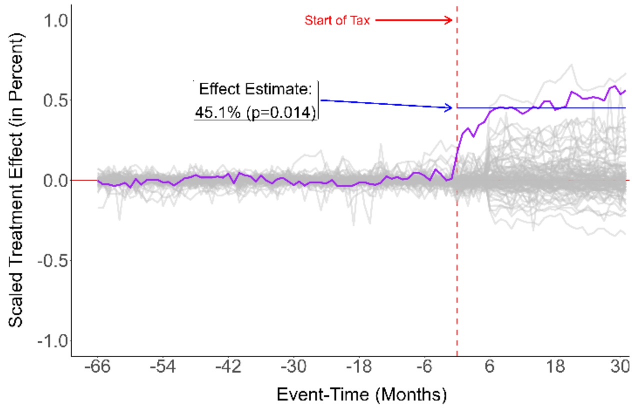
Note: Coefficient estimates represent the average total number of cents per ounce passed through to shelf prices of SSB products in the composite estimation and each individual treated locality. Dotted red lines denote full (100%) pass-through. Lightly shaded horizontal lines through each coefficient indicate 95% confidence intervals. % pass-thru indicates the % of the per-ounce tax in the composite estimation and each individual treated locality that was reflected in changes in shelf prices.

eFigure 4. Composite and Individual Changes in Volume Sales in Adjacent Border Zip Codes

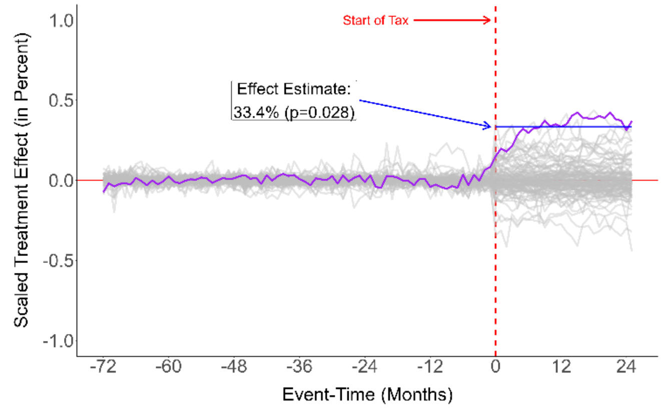


Note: Coefficient estimates represent the % change in SSB purchases in immediately adjacent border localities to each treated locality, and all borders in the composite estimation. Lightly shaded horizontal lines through each coefficient indicate 95% confidence intervals. Corresponding 95% confidence intervals and p-values are indicated next to each coefficient.

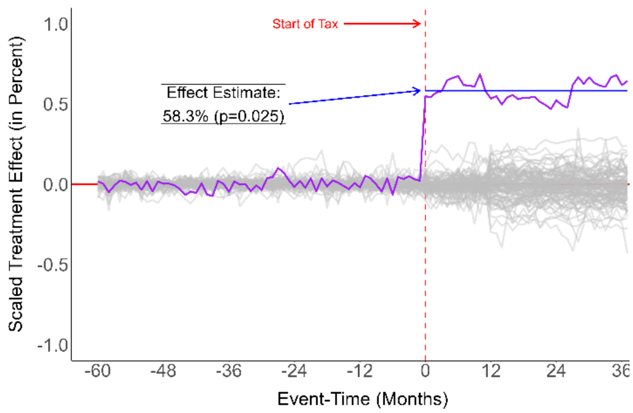
eFigure 5. Augmented Synthetic Control Estimates for Individual Locality Changes in Price



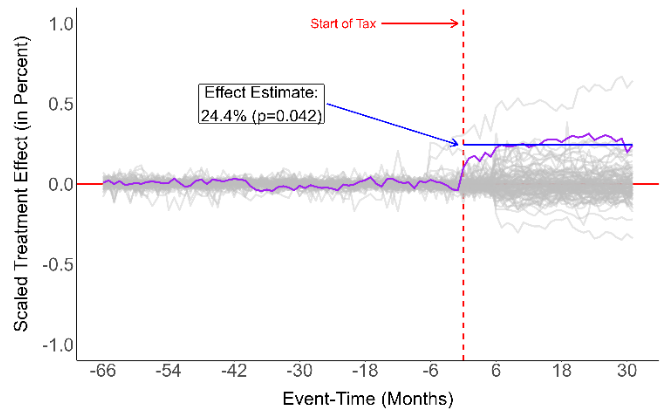
(a) 803 (Boulder)



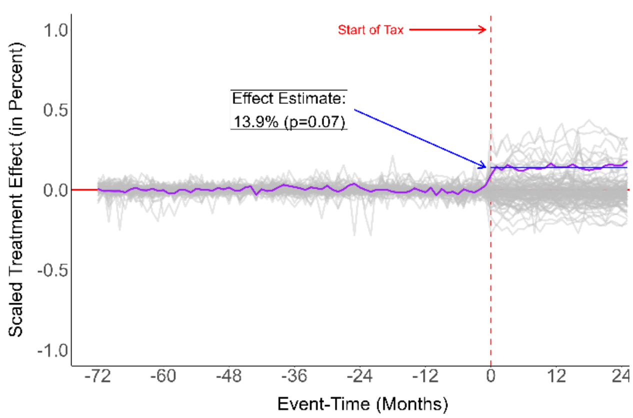
(b) 941 (San Francisco)



(c) 191 (Philadelphia)

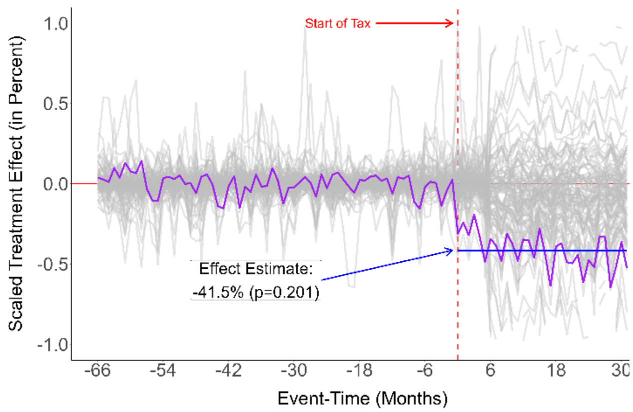


(d) 946 (Oakland)

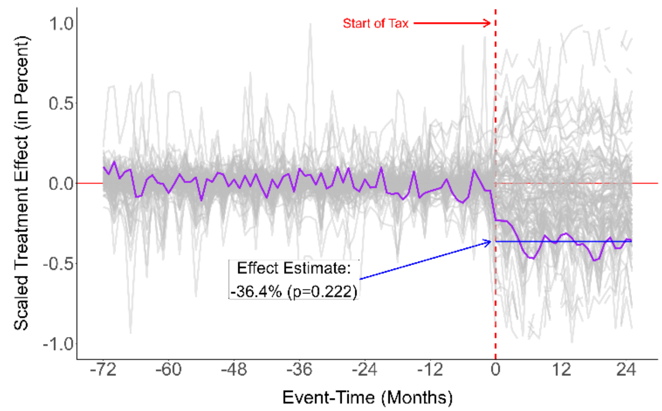


(e) 981 (Seattle)

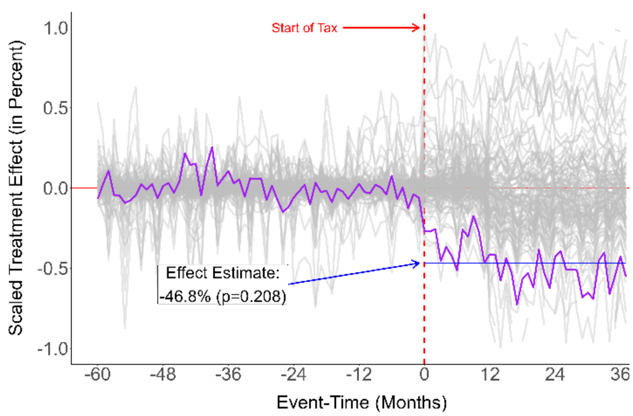
eFigure 6. Augmented Synthetic Control Estimates for Individual Locality Changes in Volume Sales



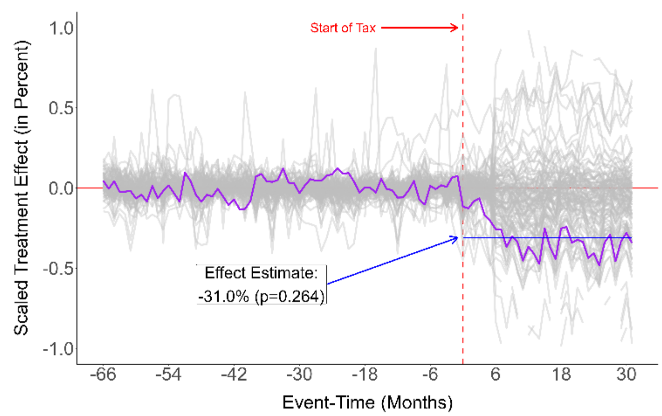
(a) 803 (Boulder)



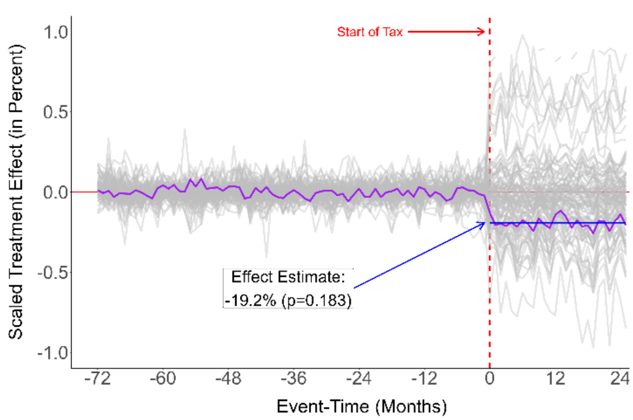
(b) 941 (San Francisco)



(c) 191 (Philadelphia)

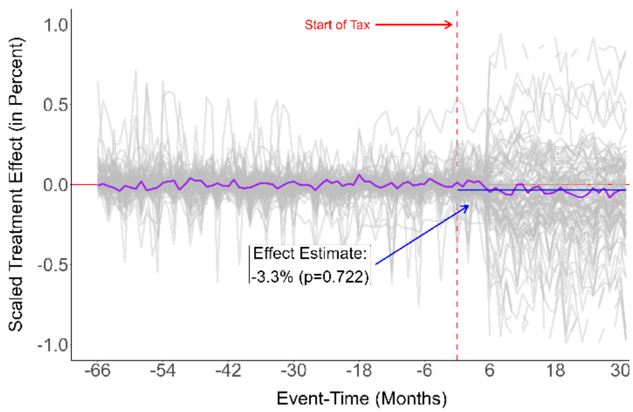


(d) 946 (Oakland)

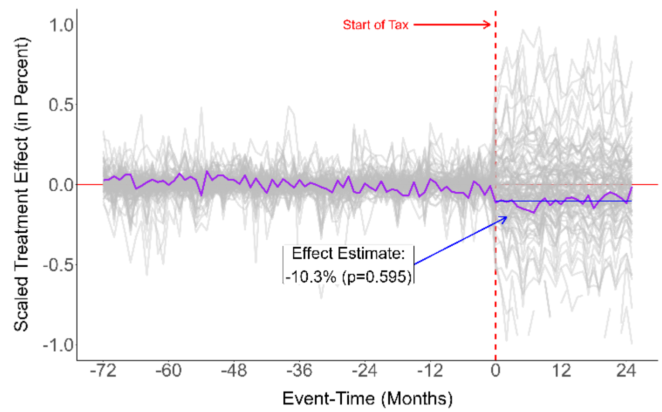


(e) 981 (Seattle)

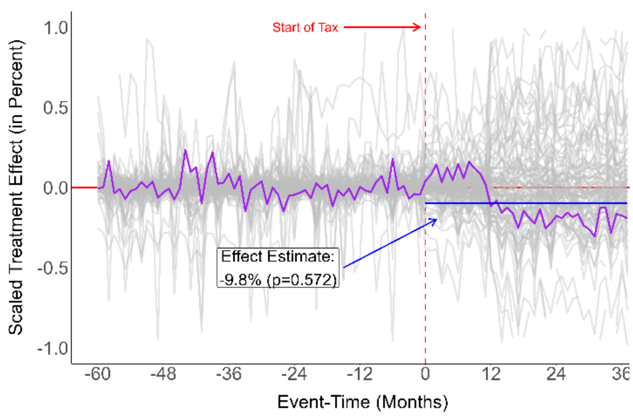
eFigure 7. Augmented Synthetic Control Estimates of Individual Locality Changes in Volume Sales of SSB Products in Border Areas



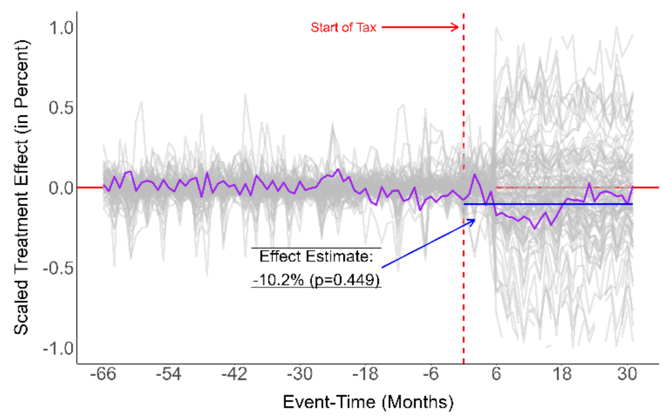
(a) 803 (Boulder)



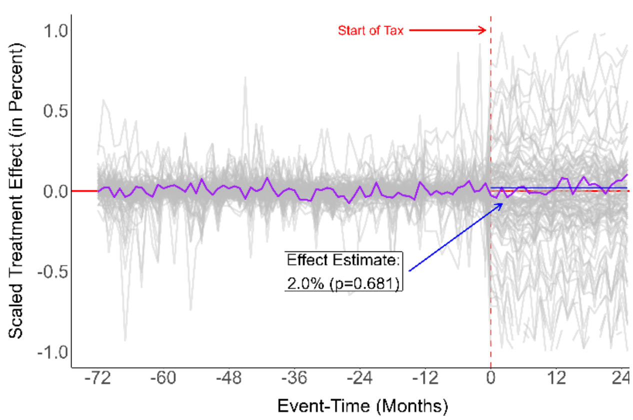
(b) 941 (San Francisco)



(c) 191 (Philadelphia)



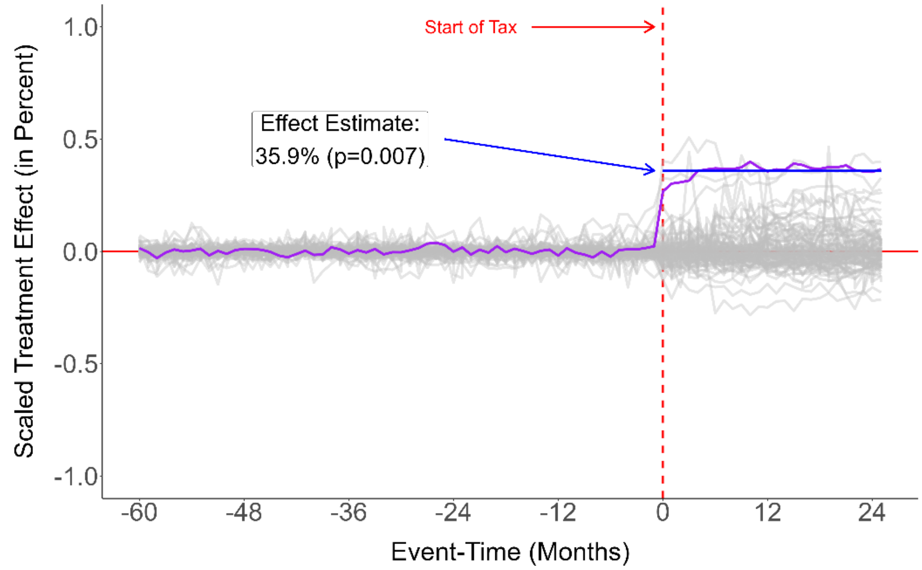
(d) 946 (Oakland)



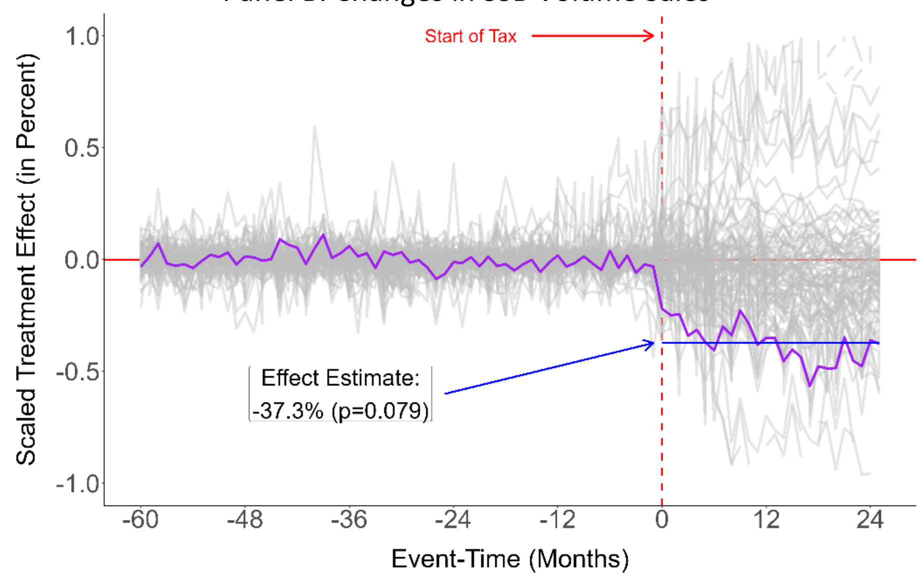
(e) 981 (Seattle)

eFigure 8. Augmented Synthetic Control Estimates for Composite Changes in Price and Volume Sales of SSB Products (Population Weighted)

Panel A. Changes in SSB Prices

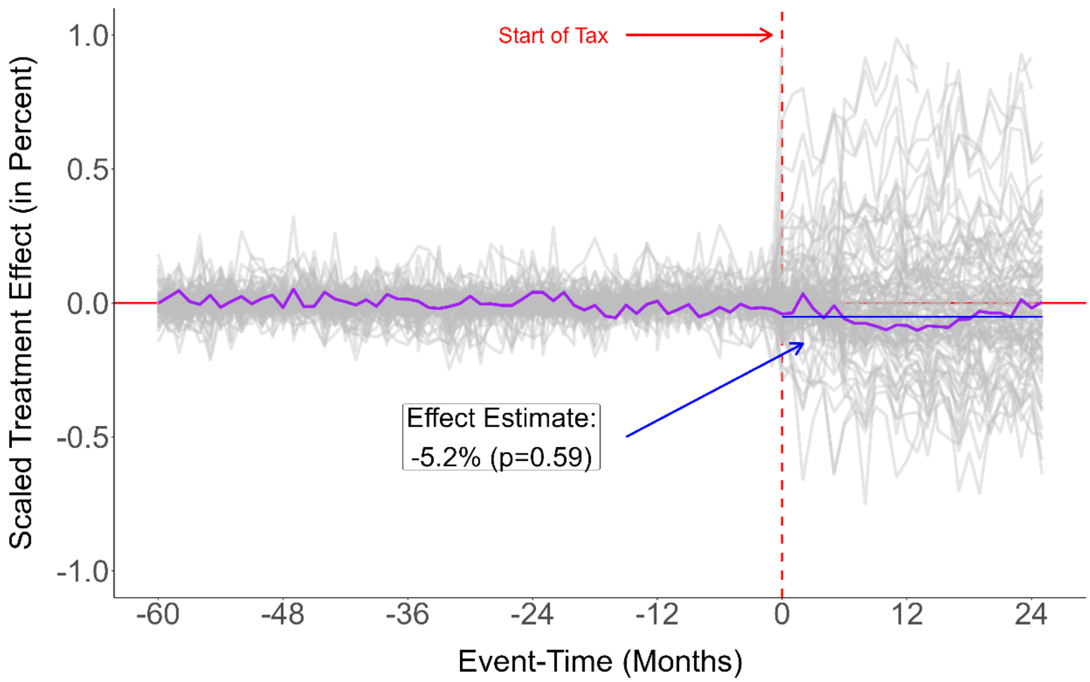


Panel B. Changes in SSB Volume Sales



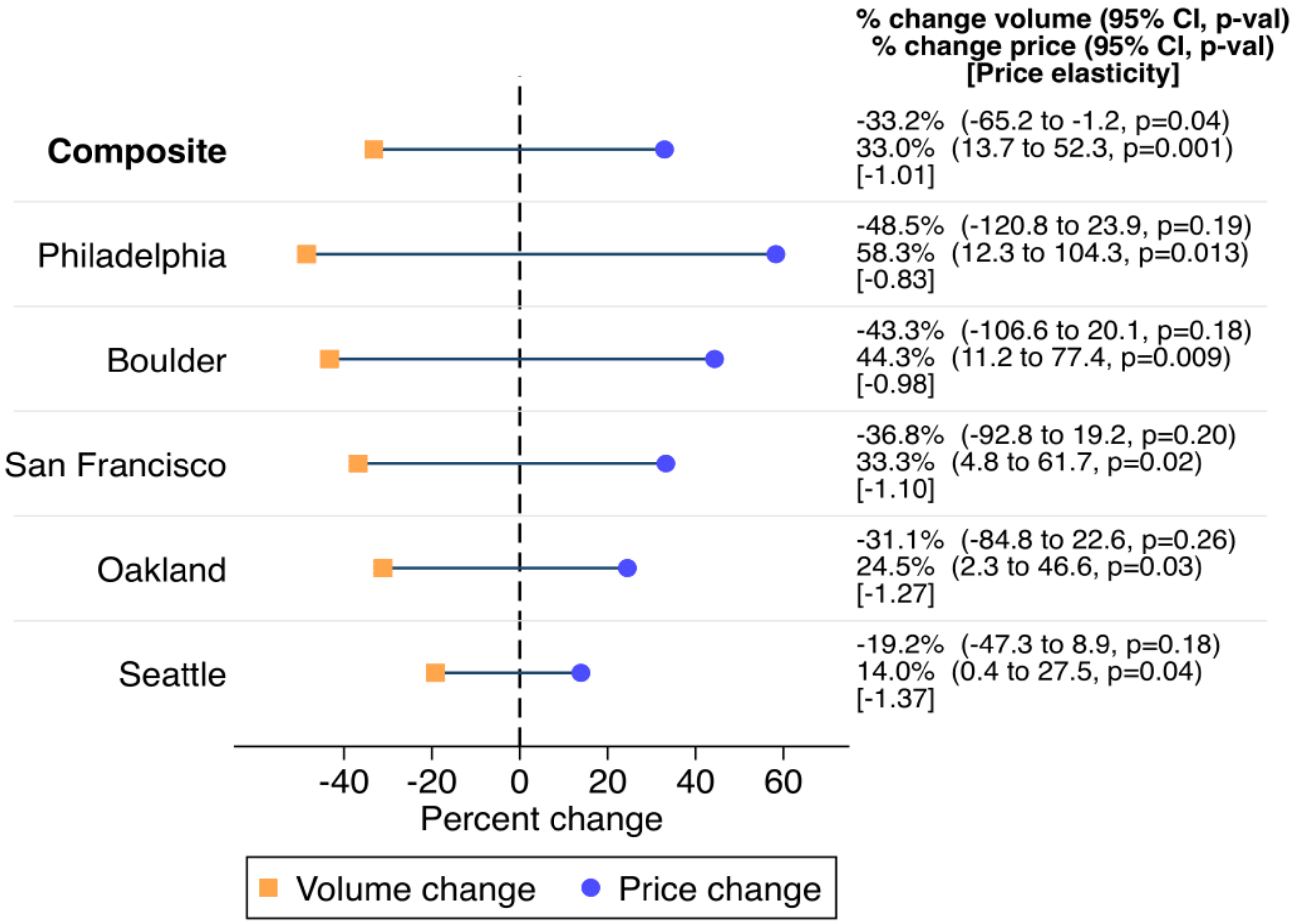
Note: Panel a) shows the % change in volume sold (in ounces), and panel b) the % change in shelf prices in response to implementing an excise SSB tax for the staggered adoption composite analysis. The bolded purple line represents the composite treated unit, while the lightly shaded grey lines represent in-space placebo estimates from the donor pool. The composite effect is explicitly weighted by the population of each individual treated city. % changes are calculated with respect to the population-weighted average of the pre-treatment means of each of the five treated localities. The composite effect size estimates and p-values are provided in the designated box of each panel.

eFigure 9. Augmented Synthetic Control Estimates of Composite Changes in Volume Sales of SSB Products in Border Areas (Population Weighted)



Note: This figure shows the staggered adoption composite analysis % change in volume sold (in ounces) in immediately adjacent bordering 3-digit zip codes in response to implementing an excise SSB tax in the five treated zip codes. The bolded purple line represents the composite adjacent border unit, while the lightly shaded grey lines represent in-space placebo estimates from the donor pool. The composite effect is explicitly weighted by the population of each individual treated city. % changes are calculated with respect to the population-weighted average of the pre-treatment means of each of the twelve adjacent border localities. The composite effect size estimates and p-values are provided in the designated box.

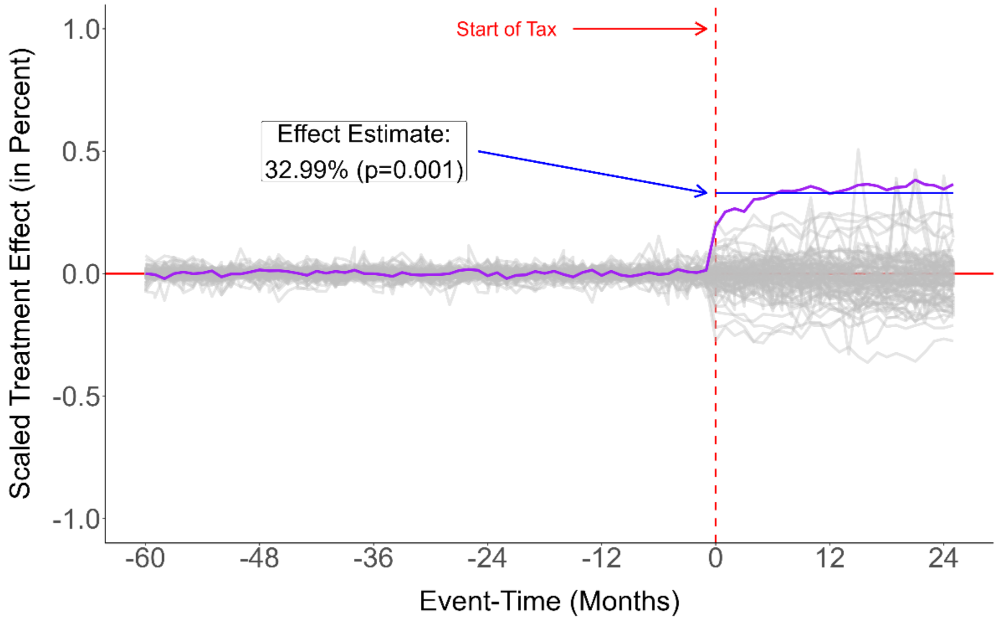
eFigure 10. Composite and Individual Locality Demand Elasticity Estimates (Urbanicity > 0.85)



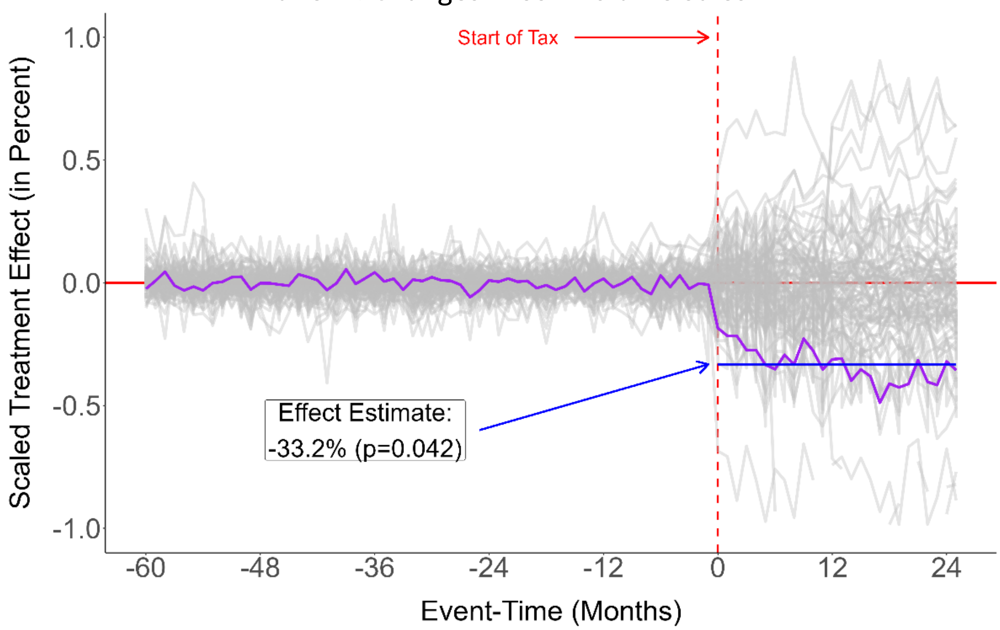
Note: This plot shows the % change in volume sold (in ounces) and % change in price for the augmented synthetic control composite analysis, and the same information for augmented synthetic control analyses of each of the five treated localities individually. Price elasticities of demand are provided in brackets, and 95% confidence intervals and p-values for each estimation are provided in parentheses.

eFigure 11. Augmented Synthetic Control Estimates for Composite Changes in Price and Volume Sales of SSB Products (Urbanicity > 0.85)

Panel A. Changes in SSB Prices

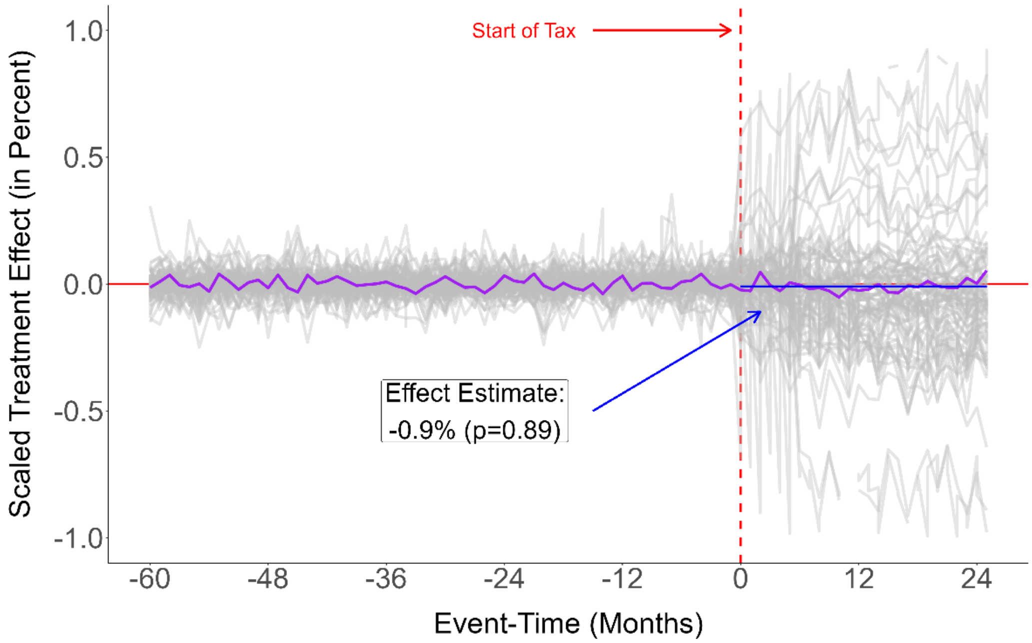


Panel B. Changes in SSB Volume Sales



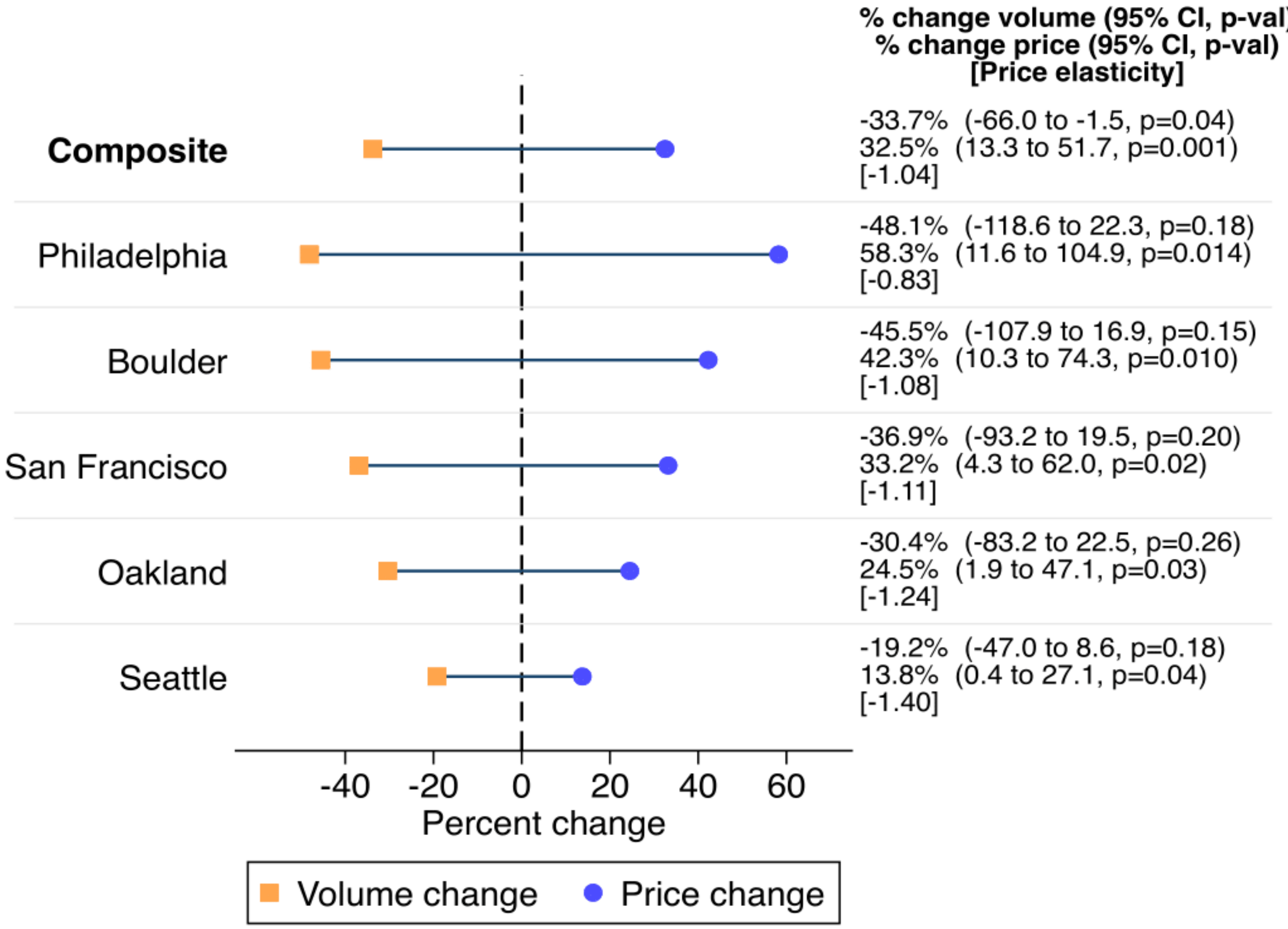
Note: Panel a) shows the % change in price and panel b) the % change in volume in response to the implementation of an excise SSB tax for the composite analysis. The bolded purple line represents the composite treated unit, while the lightly shaded grey lines represent in-space placebo estimates from the donor pool. % changes are calculated with respect to the average of the pre-treatment means of each of the five treated localities. The average composite effect estimates and p-values are provided in the designated box of each panel.

eFigure 12. Augmented Synthetic Control Estimates of Composite Changes in Volume Sales of SSB Products in Border Areas (Urbanicity > 0.85)



Note: This figure shows the composite analysis % change in volume sold in immediately adjacent bordering 3-digit zip codes in response to the implementation of an excise SSB tax in the five treated zip codes. The bolded purple line represents the composite adjacent border unit, while the lightly shaded grey lines represent in-space placebo estimates from the donor pool. % changes are calculated with respect to the average of the pre-treatment means of each of the twelve adjacent border localities. The average composite effect estimates and p-values are provided in the designated box.

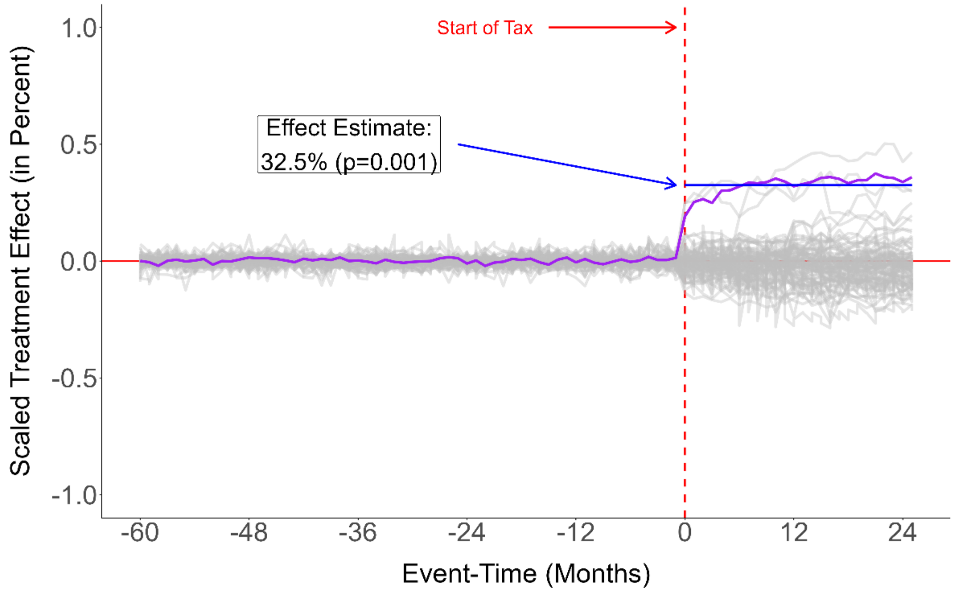
eFigure 13. Composite and Individual Locality Demand Elasticity Estimates (Urbanicity > 0.9)



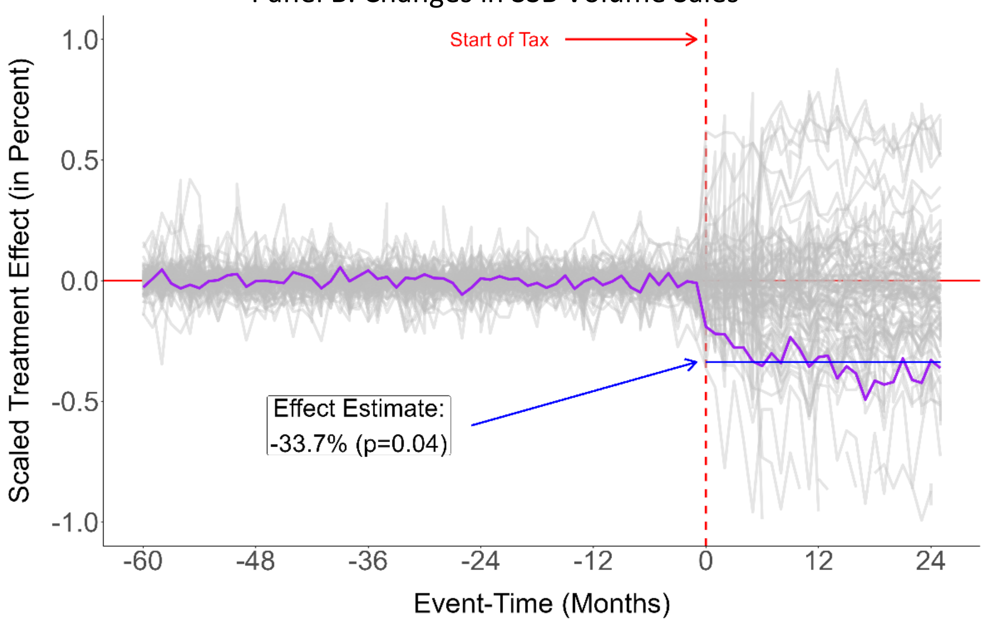
Note: This plot shows the % change in volume sold (in ounces) and % change in price for the augmented synthetic control staggered adoption composite analysis, and the same information for augmented synthetic control analyses of each of the five treated localities individually. Price elasticities of demand are provided in brackets, and 95% confidence intervals and p-values for each estimation are provided in parentheses.

eFigure 14. Augmented Synthetic Control Estimates for Composite Changes in Price and Volume Sales of SSB Products (Urbanicity > 0.9)

Panel A. Changes in SSB Prices

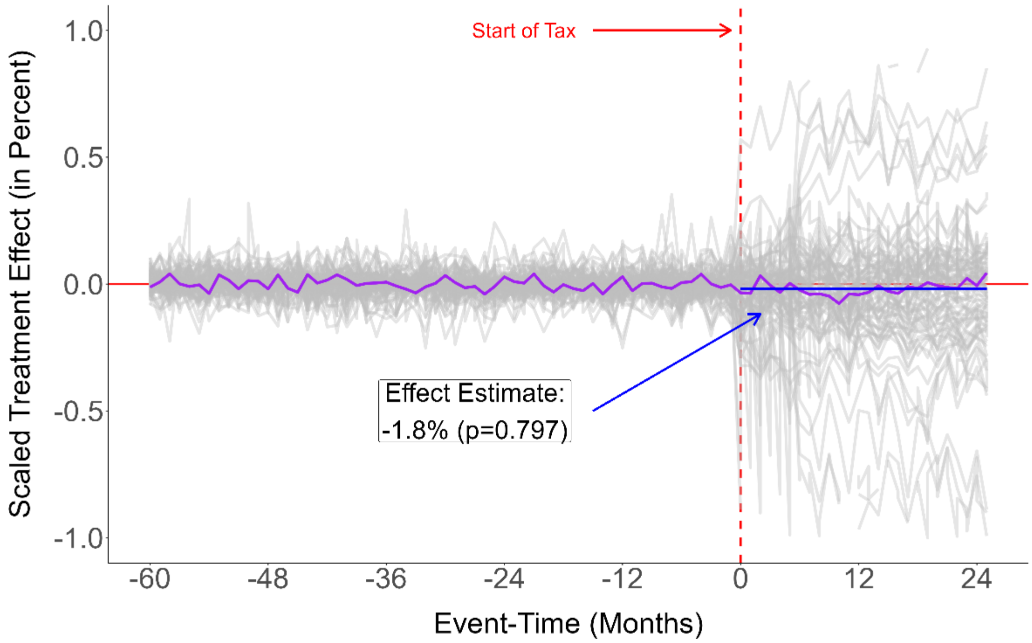


Panel B. Changes in SSB Volume Sales



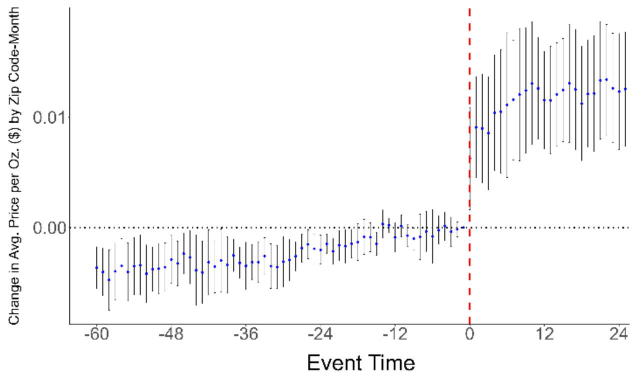
Note: Panel a) shows the % change in price and panel b) the % change in volume sold (in ounces) in response to the implementation of an excise SSB tax for the composite analysis. The bolded purple line represents the composite treated unit, while the lightly shaded grey lines represent in-space placebo estimates from the donor pool. % changes are calculated with respect to the average of the pre-treatment means of each of the five treated localities. The average composite effect estimates and p-values are provided in the designated box of each panel.

eFigure 15. Augmented Synthetic Control Estimates of Composite Changes in Volume Sales of SSB Products in Border Areas (Urbanicity > 0.9)

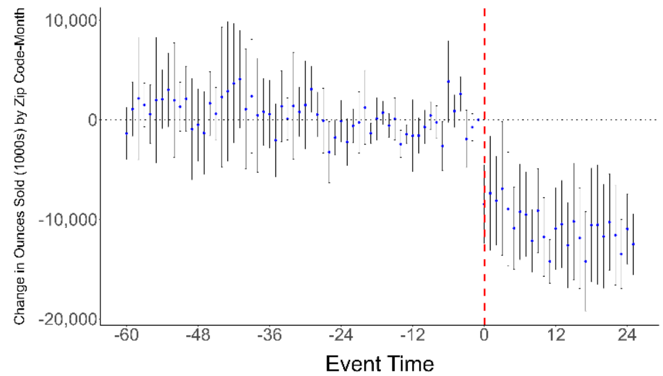


Note: This figure shows the composite analysis % change in volume sold in immediately adjacent bordering 3-digit zip codes in response to the implementation of an excise SSB tax in the five treated zip codes. The bolded purple line represents the composite adjacent border unit, while the lightly shaded grey lines represent in-space placebo estimates from the donor pool. % changes are calculated with respect to the average of the pre-treatment means of each of the twelve adjacent border localities. The average composite effect estimates and p-values are provided in the designated box.

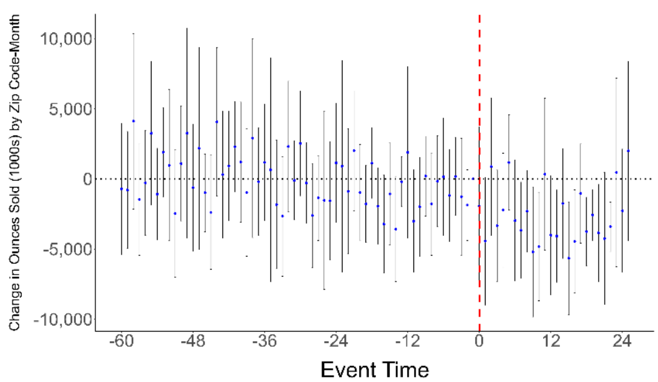
eFigure 16. TWFE Estimates of Composite Changes in Prices, Volume Sales, and Border Volume Sales



Panel A. Changes in SSB Prices



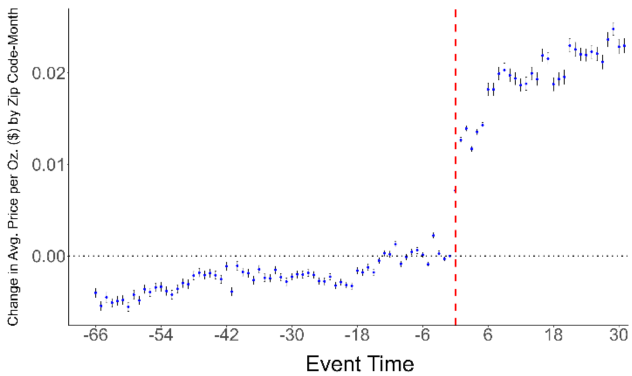
Panel B. Changes in SSB Volume Sales



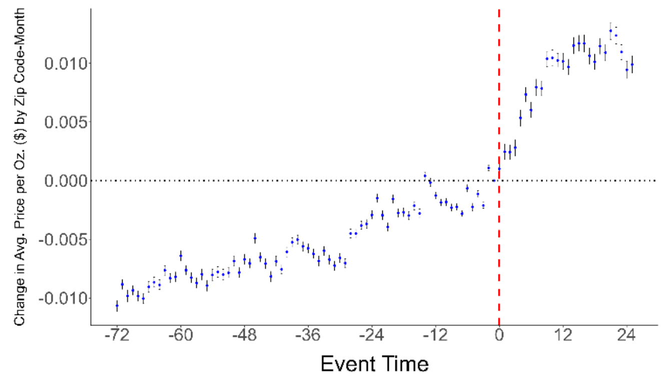
Panel C. Changes in Border SSB Volume Sales

Note: All point estimates should be interpreted relative to the omitted event-time period (-1). 95% CIs are depicted with each point estimate. The red dashed line indicates timing of policy enactment.

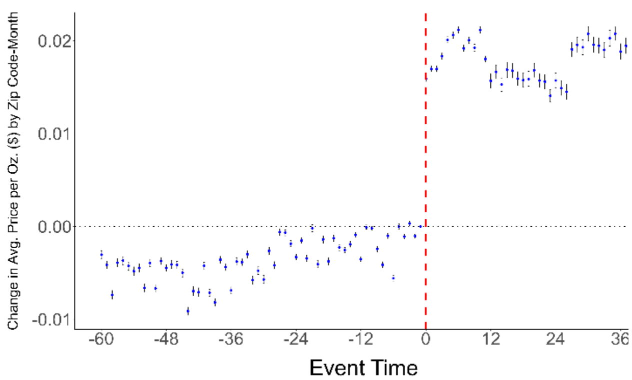
eFigure 17. TWFE Estimates of Individual Locality Changes in Prices



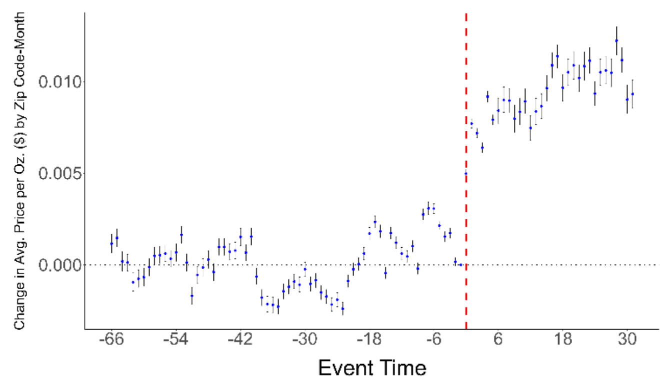
(a) 803 (Boulder)



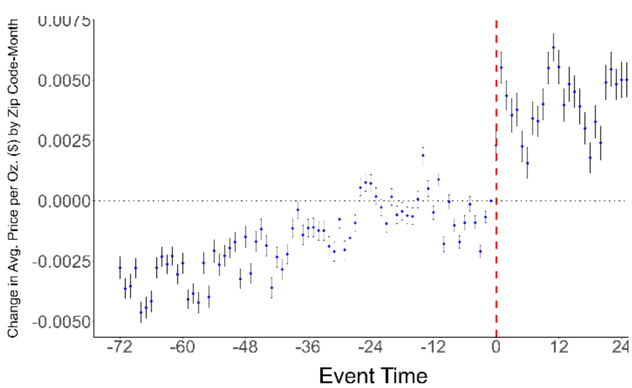
(b) 941 (San Francisco)



(c) 191 (Philadelphia)



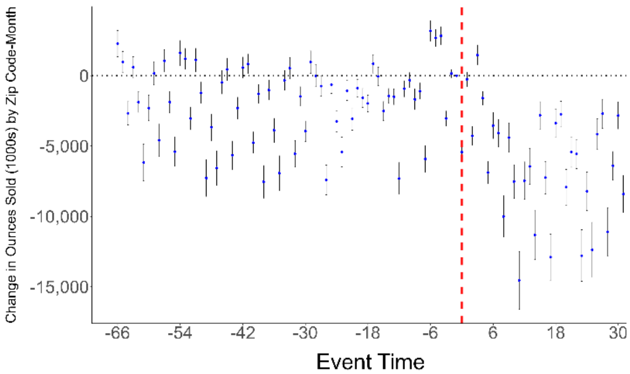
(d) 946 (Oakland)



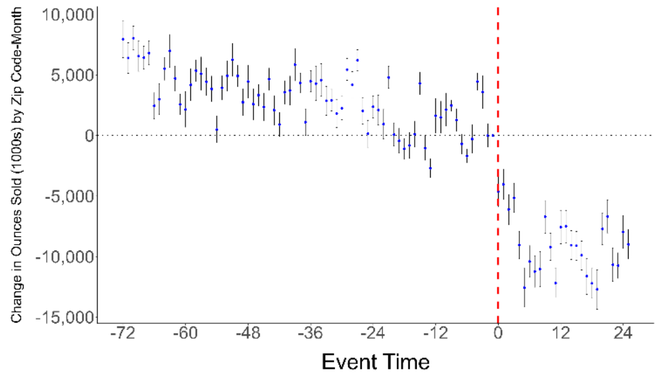
(e) 981 (Seattle)

Note: All point estimates should be interpreted relative to the omitted event-time period (-1). 95% CIs are depicted with each point estimate. The red dashed line indicates timing of policy enactment.

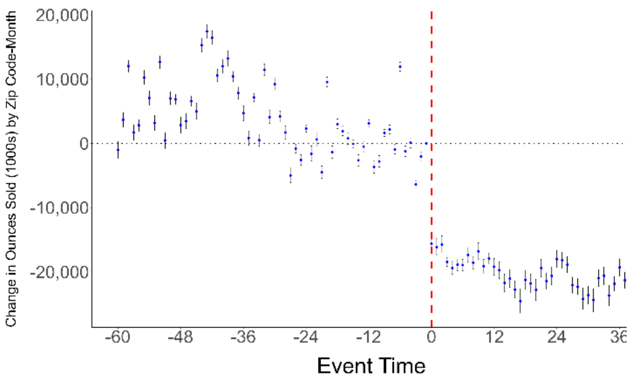
eFigure 18. TWFE Estimates of Individual Locality Changes in Volume Sales



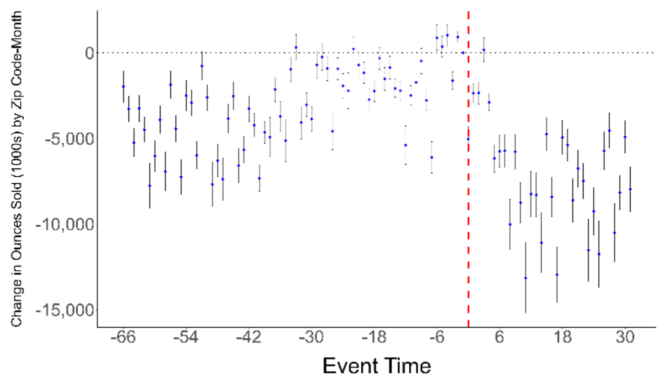
(a) 803 (Boulder)



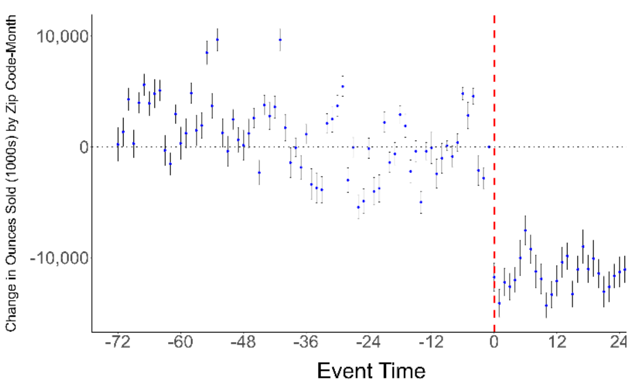
(b) 941 (San Francisco)



(c) 191 (Philadelphia)



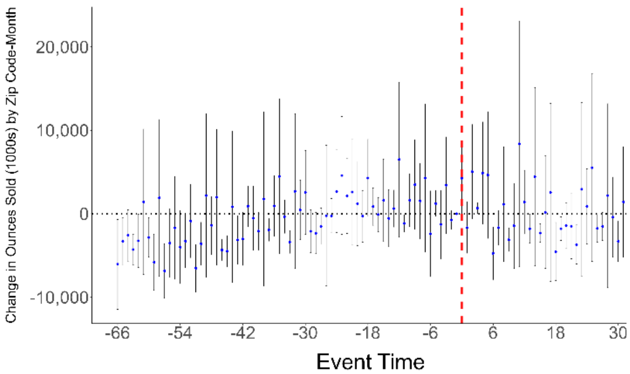
(d) 946 (Oakland)



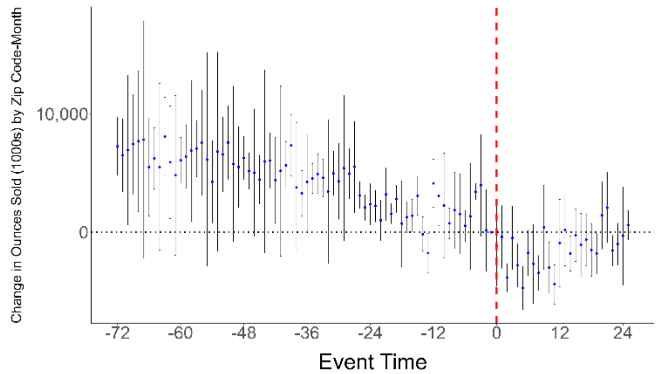
(e) 981 (Seattle)

Note: All point estimates should be interpreted relative to the omitted event-time period (-1). 95% CIs are depicted with each point estimate. The red dashed line indicates timing of policy enactment.

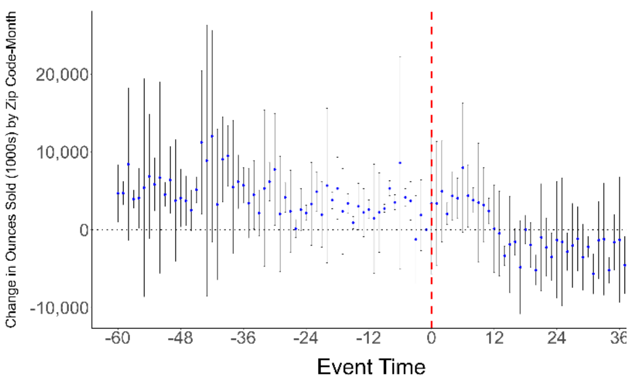
eFigure 19. TWFE Estimates of Individual Locality Changes in Volume Sales of SSB Products in Border Areas



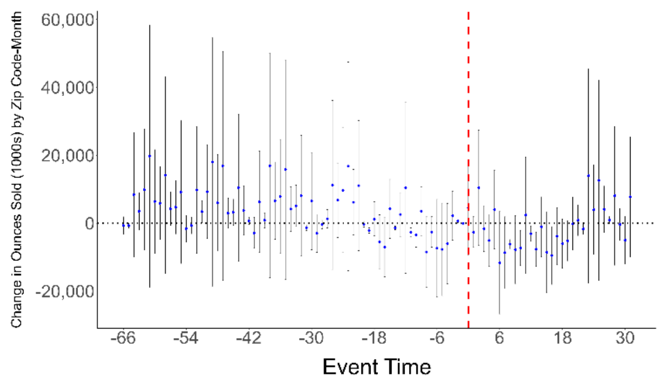
(a) 803 (Boulder)



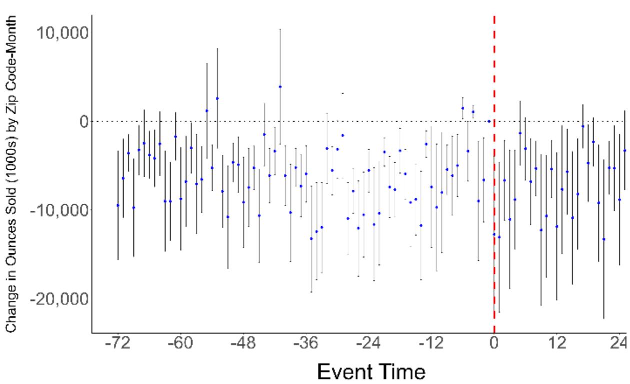
(b) 941 (San Francisco)



(c) 191 (Philadelphia)



(d) 946 (Oakland)



(e) 981 (Seattle)

Note: All point estimates should be interpreted relative to the omitted event-time period (-1). 95% CIs are depicted with each point estimate. The red dashed line indicates timing of policy enactment.