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How do parents respond to regulation of sugary drinks in child care? Evidence from California☆

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ABSTRACT

Excessive sugar intake is associated with higher risk for a range of diseases in children, including childhood obesity. To reduce sugar intake in children, California regulates the provision of sugar-sweetened beverages and juice by child care facilities. The regulation may reduce the consumption of beverages high in sugar in the short run and weaken their preferences for sugary drinks in the long run. Whether these objectives are achieved depends on how parents respond to the regulation by providing sugary drinks at home. Using detailed scanner data on grocery purchases, we find that affected California households increased their juice purchases right after the regulation became effective. However, this increase disappears after 1 year. Moreover, we do not find an increase in the purchases of sugary substitutes. Our findings suggest that parents provide more juice for their children after child care facilities limit their provision of sugary beverage, but such offsetting behavior disappears after 1 year. Regulating the consumption of sugary drinks in child care facilities may be an effective policy to lower children's preferences for sugary drinks.

1. Introduction

The social and economic consequences of obesity have generated concern among academics, policymakers, and decision makers due to its high prevalence in developed countries, and especially in the U.S. In particular, the obesity rate has more than doubled in the U.S. in the past 30 years (Cawley, 2015). Obesity imposes a large economic burden on individuals and their families that can take the form of lost productivity and forgone economic growth as a result of lost working days, lower productivity at work, mortality, and permanent disability. Also, obesity is found to be associated with lower wages, lower probability of being employed, and higher medical care costs (Cawley, 2015). The standard American diet is generally characterized by excessive consumption of calories from high-fat products and high-sugar drinks, and it has long been criticized for contributing to obesity and related health issues, such as type 2 diabetes (Grotto and Zied, 2010).

☆ This paper reports our own analyses calculated (or derived) based in part on data from the Nielsen Company (US), LLC and marketing databases provided through the Nielsen Datasets at the Kilts Center for Marketing Data Center at the University of Chicago Booth School of Business. The conclusions drawn from the Nielsen data are ours and do not reflect the views of Nielsen. Nielsen is not responsible for, had no role in, and was not involved in analyzing and preparing the results reported herein. We do not have significant competing financial, professional, or personal interests that might have influenced the work described in this manuscript. We thank Cahit Guven, Jinhu Li, Lionel Page, Jing Zhang, participants at the Econometric Society Australasian Meeting 2019, and anonymous reviewers for their helpful comments. Any errors are our own. Declarations of interest: none for all co-authors.

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Various policies and interventions have been implemented to promote a healthy and balanced diet and prevent obesity, including a tax on sugar-sweetened beverages, informational campaigns, labeling laws, and child nutrition programs (Fletcher et al., 2010; Bollinger et al., 2011; Taber et al., 2011). The main challenge in evaluating these interventions is that offsetting behavior is difficult to observe (Cawley, 2015). For example, at school, children might consume healthy food due to a school-mandated restriction on sugar-sweetened food. Nevertheless, these students might purchase unhealthy foods outside of school hours, which will eventually lead to higher consumption of unhealthy foods at home (Cawley, 2015) or even higher overall consumption of unhealthy food due to psychological reactance (Debnam, 2017).

In this paper, we study the effects of a California regulation that prohibits child care facilities from providing sugar-sweetened beverages to children and limits their daily provision of juice to no more than one serving per day. The regulation was enacted by the California Healthy Beverages in Child Care Act (AB 2084) and became effective on January 1, 2012. Using detailed home-scan consumption data, we investigate the potential offsetting responses and behaviors in preschool children's overall consumption of sugar-sweetened beverages.

The rationale behind the California's regulation, as reported in the legislation, is that almost 20% of children between 2 and 5 years in California are over-weight or obese, a pattern that is difficult to reverse in adolescence or adulthood.[1] The legislation requires California child care facilities, which include "all licensed child care homes and centers," to comply with the stricter standard for beverage serving.[2] Since 2012, California has become the first and only state in the U.S. that restricts the provision of juice and sugar-sweetened beverages in child care facilities. Juice provision is restricted to no more than one serving per day of 100% juice, and other sugar-sweetened beverages, such as soda, are banned. Before the regulation, juice in daycare facilities in California was typically served up to three or four times per day with each meal, including breakfast, lunch, dinner, and snack time.[3] One advantage of studying the consumption behavior of preschool children is that the places where they consume food and beverages are relatively limited to child care facilities and home, compared with those of adults. Therefore, household purchase of beverages and their provision in child care account for most beverage consumption by children.

We then construct appropriate counterfactuals to compare with households whose children were affected by the sugar-sweetened regulation. To do so, we target households from the Nielsen Consumer Panel Dataset between the years 2004 and 2016 that are in unaffected states and have at least one preschool child with working parents. We do this because we believe that children who have working parents are more likely to attend child care facilities. We adopt an exact matching algorithm to identify the matched unaffected/control households. After we identify appropriate unaffected households, we use a difference-in-differences methodology that compares the annual juice purchase of affected households with that of matched unaffected households. We later demonstrate that we satisfy the crucial assumption for the validity of our identification strategy, which is that prior to the regulation, the trends in affected versus unaffected households were the same in their consumption of sugar-sweetened beverages. The results we present in this paper show a significant increase in juice purchased in the year the regulation was activated for treated households compared with households with preschool children and other similar characteristics in control states, while controlling for state-specific unobserved heterogeneity. Since juice consumption is limited to one serving per day at the daycare facility, this pattern indicates that affected children consume more juice at home. However, this offsetting behavior disappears the following year, in 2013, and since then it follows the same pattern as that in unaffected households. This indicates that overall juice consumption for preschool children in California dropped after the regulation was enacted. What is striking is that a few years later, in 2016, home consumption of juice in affected households decreased compared with that of unaffected households. We later provide evidence that our identification assumptions and main results are highly robust to an extensive battery of robustness exercises.

We also study whether there is any switching behavior from juice to other sugary products for affected compared with unaffected households. We do not find significant substitution effects in affected households' expenditures on other sugary products – such as soda, ice cream, candy, and cookies, or ingredients for homemade juice, such as raw sugar and fruits – as a result of the juice ban compared with unaffected households. We then look at subgroups of the sample. First, we find that households in areas with high child care availability are those that drive the increase in home-consumption of sugary beverages 1 year after the act was in effect. This indicates that these preschool children are likely to have stronger preferences for sweetened beverages, and it might be harder for them to adjust their preferences and habits. Second, we report heterogeneous treatment effects of juice consumption by household income level. We find that households with incomes above the median are those that exhibit stronger offsetting behavior and slower adjustment of their preferences. Households in which parents work long hours or have more high-paying occupations are likely to rely more on child care facilities. As a result, these preschool children are likely to find it harder to adjust to the restriction on sugar-sweetened beverage consumption at their child care facility.

[1] The legislation can be found here: http://leginfo.legislature.ca.gov/faces/billNavClient.xhtml?bill_id=200920100AB2084 (last accessed August 2020).

[2] The regulation applies regardless of whether the child care facility participates in the federal Child and Adult Care Food Program (CACFP). The CACFP is a federal program that provides reimbursement for nutritious meals and snacks for children and adults who are enrolled for nutritional meals at specific childcare centers, daycare homes, and adult daycare centers. The CACFP also provides reimbursement for meals served to children and youth participating in after-school care programs, children residing in emergency shelters, and older adults or chronically impaired persons with disabilities in their care facilities. Details about the Child and Adult Care Food Program can be found here: https://www.fns.usda.gov/cacfp/meals-and-snacks.

[3] Details about the Healthy Beverages in Child Care Act (AB 2084) can be found at http://www.healthybeveragesinchildcare.org/ (last accessed August 2020).

The implications of sugar consumption for children's diet and health are serious, and the importance of identifying which interventions best predict behavioral changes is of paramount importance for policy makers. Previous studies that evaluate the effectiveness of various interventions on sugary drinks report conflicting findings. In particular, the most commonly analyzed policy in this literature is a tax on unhealthy food products, such as a "fat tax" or "sugar tax." In addition to the mixed empirical results regarding those interventions' effectiveness and the consequences for consumer welfare loss, the main concern is that food taxes are "regressive" (e.g., Cawley, 2015; Muller et al., 2017) – that is, taxes that take a larger percentage of income from low-income earners rather than from high-income earners. In a recent study, using experimental data, Muller et al. (2017) find that not only unhealthy food taxes are regressive, but also thin subsidies on healthy foods favor high-income households over low-income ones. In this case, subsidies on healthy food are technically not regressive, but are still highly ineffective, since there is extensive evidence that high-income earners eat healthier than low-income earners (Guenther et al., 2008; Drewnowski, 2009). Additionally, high-income households exhibit more elastic demand and are responsive to changes in the price of sugar-sweetened beverages (Zhen et al., 2011). We believe that our paper makes a substantial contribution to this literature. The evidence we provide in this paper suggests that interventions that target and reduce early-stage consumption of sugary drinks can be highly effective for preschool children. These interventions are budget-neutral and have the potential to lead to a substantial reduction of sugar consumption by adults in the future.

Our paper also contributes to the literature on early childhood interventions. Unlike previous findings that primary or secondary schooling has little impact on obesity prevention (e.g., Brunello et al., 2013; Clark and Royer, 2013), Frisvold and Lumeng (2011) and Carneiro and Ginja (2014) provide evidence that early childhood education could help preschool children formulate good health habits and prevent obesity. These two papers evaluate the impact of the Head Start Program in the U.S., which provides comprehensive services related to the education, health, and nutrition behaviors of low-income children who are below 5 years old and their families. Children are eligible to participate in this program if they are between 3 and 5 years and their family income is below the federal poverty line[4] or the family is eligible for public assistance (Carneiro and Ginja, 2014). Carneiro and Ginja (2014) provide evidence for the program's effectiveness, which nevertheless consists of multiple treatments in terms of education, health, and nutrition on a particular subgroup (i.e., low-income families). Our paper complements their design and findings, and provides evidence of a statewide intervention that targeted preschool children from the whole distribution of income and socioeconomic backgrounds. Additionally, we demonstrate that the type of food served at child care facilities plays an important role in the development of children's health and diet preferences.

2. Background

Sugar-sweetened beverages contain high added sugar content and little nutritional value, and are considered to be highly associated with poor health outcomes, including obesity, diabetes, fractures, and tooth decay (Malik et al., 2006). Sugar supplementation increases one's preferences for sweet foods, and this has been found to be the case even for those who initially dislike sucrose (Sartor et al., 2011). Sugar-sweetened beverages could lead to obesity in preschool children because there is imprecise and incomplete compensation for energy consumed in liquid form (Ludwig et al., 2001). Juice is traditionally considered to be healthy. However, it has many potential detrimental effects, such as obesity, energy imbalances, diarrhea, over-nutrition or under-nutrition, and development of dental issues (Baker et al., 2001). Moreover, preschool children are still at the stage in which they develop their food preferences, and a preference for sweet foods could easily be acquired if juice is served frequently (Birch, 1999).

In 2012, California led other states by passing legislation to establish nutrition standards for beverages served in licensed child care centers and home facilities. The legislation's goal was to improve the nutritional environment of child care, because millions of children in California enter school with unhealthy taste preferences and dietary habits that developed in early childhood environments, including child care facilities.[5]

Starting on January 1, 2012, California passed the Healthy Beverages in Child Care Act, which requires all child care facilities to limit juice to no more than one serving per day. At the same time, it promotes water consumption and prohibits all sugar-sweetened beverages. Under the law, no beverages with added sweeteners, either natural or artificial, can be served to children in child care facilities. Additionally, after January 2012, all child care facilities in California must ensure that water is available all the time and only fat-free or low-fat milk (1%) can be served to children over the age of 2. These requirements are more stringent than those of the federal Child and Adult Care Food Program (CACFP[6]) that is in place in all states. For example, CACFP imposes no restriction on the provision of sugar-sweetened beverages. Only starting in 2016 did the CACFP dietary guidelines suggest that sugar-sweetened beverages contribute to the overconsumption of sugar by children. These dietary guidelines recommend that program participants should avoid providing sugar-sweetened products to eligible children, but state that it is beyond their statutory authority to prohibit the provision of sugar-sweetened beverages. Moreover, although juice is considered to be less healthy, it was still included in the standard list of reimbursable beverages, meals, and snacks under the CACFP before 2017.

[4] Federal poverty lines depend on family size. For details, see https://aspe.hhs.gov/poverty-guidelines (last accessed August 2020).

[5] More details about the goals of the legislation can be found at http://leginfo.legislature.ca.gov/faces/billNavClient.xhtml?bill_id=200920100AB2084 (last accessed August 2020).

[6] See footnote 2 for more detailed information.

However, California's Healthy Beverages in Child Care Act (AB 2084) strictly prohibits all licensed child care facilities from serving sugar-sweetened beverages and restrict the provision of juice to no more than one serving per day.[7] Households with a preschool child in other states are unaffected by this intervention. This provides us with a group of affected and unaffected households we will compare to evaluate the effectiveness of this intervention.

3. Data and empirical approach

3.1. Data description

Our primary dataset is the Nielsen Consumer Panel Data from the Kilts Center for Marketing at the University of Chicago. The Consumer Panel Data is a panel dataset of household purchases, and includes approximately 40,000 to 60,000 households each year from year 2004 to 2016. Nielsen, which is a marketing company, provided bar-code scanners to participating households to record their purchases. The dataset includes information on the date, retailer location, and quantity of products each household purchased.

Purchases are identified at the bar-code level. We know a plethora of characteristics for each recorded purchase: price, quantity, and product attributes. Nielsen categorizes products according to different hierarchies. There are about 1400 product modules, which are then grouped into 122 product groups. For example, grape juice is categorized in the product module of "Fruit Juice - Grape," which in turn belongs to the module group of "Juice, Drinks - Canned, Bottled." For the purpose of this study, we include all purchases of products that fall into group "Juice, Drinks - Canned, Bottled" as juice purchases.

Households were recruited across the U.S. and remained in the Consumer Panel for 1 to several years. Nielsen only retained households whose purchase data satisfy minimum quality criteria in the Consumer Panel Data. We therefore have an unbalanced panel of households from the 48 continental states and Washington, D.C. We have demographic information for households such as age, household composition, income, labor force participation, residential county, etc. Since California's regulation on healthy beverages became effective January 1, 2012, we aggregate household purchases by year so that our analysis is conducted at the household-year level.

We also obtained information on childcare availability across the U.S. from the Census. Data on the availability of child care facilities per county are available for the years 2002, 2007, and 2012. Child care facilities include "facilities with an employee payroll that pay income taxes (e.g., a child care center for profit)"; "facilities with an employee payroll that are tax-exempt (e.g., a non-profit based child care center)"; and "non-employer businesses (e.g., a family daycare home)."[8] Using the estimated number of children below 5 years, we measure child care availability in each county by the number of child care facilities per 100 children.[9] Since the number of child care facilities per county is only available every 5 years in the Census, we also use the Quarterly Census of Employment and Wages (QCEW). QCEW reports the total number of employees for those facilities with an employee payroll for each year during our sample period in a robustness exercise.

3.2. Empirical approach

3.2.1. Exactmatching process

The legislation regulates which beverages child care facilities in the state of California can provide to preschool children. Households that send at least one child to a child care facility in California are potentially affected by the law. If preschool children's preferences for sugary drinks are weakened by the law, we may see a reduction in the purchase of juice in these households. On the other hand, parents may purchase more juice for their children so that they can bring it to the child care facility for consumption. If such a compensating behavior exists, we may see that juice purchases increase after enactment of the regulation. Ideally, we focus on these households as our treatment group.

Unfortunately, the Consumer Panel data do not include information on the use of child care facilities by households. However, households report the number of children under the age of 6 and parents' work hours.

The average school starting age is 6 in California and 6.12 in other states.[10] Accordingly, we consider children under the age of 6 to be preschool children who are most likely to be in child care. In total, 44,592 households in the data have at least one preschool-age (under 6 years) child in the family at the time of the survey.

Working parents are more likely to use child care services. Therefore, we focus on households in which both parents work at least 30 h a week and, in the case of single parent households, the single parent works at least 30 h per week.

In the absence of information on actual child care usage, we define our *target group* as California households that have at least one preschool-age child and all parents work at least 30 h per week. In total, 15,681 household-year observations satisfy the criteria of working parents with at least one preschool-age child. Of these observations, 1485 are California residents and 14,196 live outside of California.

[7] The only exception to the policy is when the child's parent or legal guardian provides sugar-sweetened beverages to the child care facility for their own child's consumption.

[8] Source: https://www.census.gov/library/working-papers/2013/econ/2013_child_care.html.

[9] Since the Consumer Panel Data only provides the number of children below 6, ideally, we could also have population estimates for children under age 6. However, the Census uses 5 as the cutoff for the population grouping of different ages.

[10] https://nces.ed.gov/programs/statereform/tab5_1.asp (last accessed August 2020).

These out-of-California households could potentially serve as our control group. However, such a control group could be quite different from the treated group due to location-related characteristics. California is more urban than other states in the U.S., and average household income and expenditure are higher than elsewhere. Moreover, states differ in child care availability. The average number of child care facilities per 100 children under the age of 5 is 4.3 in California counties and 3.5 elsewhere.[11] Children attending child care more regularly may have different dietary preferences compared with those who do not. General awareness of healthy diet practices may also differ across states. Indeed, we find that the trend of household juice purchases in California is different from other states in the country.

In order to obtain a comparable control group, we perform an exact matching exercise. This technique matches each treated household to all possible control households with the same values on all covariates, forming subclasses such that within each subclass all units (treatment and control) have the same covariate values. We match households from other states to households in California that have the same demographic and consumption characteristics. Our exact matching variables are the following: presence and age of children, parents' working hours per week, total expenditure[12] excluding drinks and coupons, child care availability[13] in the county where households are located and whether households purchase any tobacco. One of the covariates is the presence and age of children, which is divided into eight categories in the Consumer Panel Data. These categories are under 6 only; 6–12 only; 13–17 only; under 6 & 6–12; under 6 & 13–17; 6–12 & 13–17; under 6 & 6–12 & 13–17; and no children under 18. Four of these categories include at least one child under 6. The composition of children of different ages may affect beverage consumption and how they change due to factors such as sibling influences. We match exactly on this categorical variable to ensure that children under 6 have similar sibling influence for both treated and control groups.

Five categories characterize the working hours per week for male/female head: under 30 h; 30–34 h; 35+ h; not employed for pay; and no male/female head. We focus on parents who work 30 h or more – since these are more likely to send their children to child care – and exclude those parents who work less than 30 h per week, or are not employed for pay. We match out-of-California parents or single parents to California parents in the same working-hours category. Exact matching on this variable allows us to compare households whose children spend similar time in daycares.

Consumption in general and juice consumption specifically is likely to be correlated with income. People with higher income tend to have healthier dietary habits and are more responsive to price changes (Finkelstein et al., 2010 and Muller et al., 2017). In the Consumer Panel Data, household income is recorded 2 years before the panel year that records the purchase. Moreover, the income variable's values are integers that represent ranges of income, and the top range is not consistent across panel years. Since there has been literature using the household's overall expenditure as a better measurement for their income level than their self-reported income level, we use households' overall expenditure as one of the matching criteria.[14] To avoid the confound from expenditure on drinks and coupons, we use a household's total expenditure excluding drinks and coupons. We divide households in each panel year into deciles and match households within an expenditure decile in each year.

Since our focus is preferences for a healthy diet, we want to compare households with similar awareness of healthy lifestyles. We create a binary variable that indicates whether the household purchases any tobacco, and include this variable in our matching criteria.

Child care use depends on the availability of child care facilities in the local area. To measure child care availability, we obtain county-level numbers of child care facilities per 100 children using the most recent data from the Census. To compare households living in areas with similar child care accessibility, we sort households into three groups based on this measure of child care availability. We drop 258 observations for which there were no exactly matched controls, which left us with 1227 observations in the treated group.

Table 1 reports descriptive statistics for the key variables we use in our analysis. There are 1227 matched treated households and 4972 matched control households. The average quantity of juice purchases in treated households is 2858 ounces, and the average quantity of juice purchases in control households is 2836 ounces. Juice expenditure, overall expenditure, and expenditure per capita (in dollars) between matched treated and control households are very similar. The household size in treated households is 4.1 and in control households is 3.9. Moreover, the average number of child care facilities per 100 children under the age of 5 in the control group is 4.2, which is very close to the corresponding number for treated counties, which is 4.3. To contextualize the differences between the treated and control households, we pool the treated and control households and regress several demographic and consumption variables on the treatment indicator. Because multiple control households may be matched to one treated household, we re-weight the control households by the inverse of the number of matched controls. Re-weighting ensures that treated households and control households have equal weights and are consistent with the difference-in-differences design to be described in the next section.

[11] The number of child care facilities per 100 children for each county is calculated from the data we collected from Economic Census and Intercensal Population Estimates.

[12] In particular, we match based on the decile of total expenditure.

[13] In particular, we match based on the tertile of child care availability.

[14] It is standard to use consumption to measure material well-being rather than income for the developing countries (Banerjee and Duflo, 2007 and Deaton, 1997). In terms of the U.S., Meyer and Sullivan (2003) find evidence that consumption is a better measurement than income for the poor in the U.S., and Gottschalk and Moffitt (2009) find that the measurement error of reported income from U.S. surveys is "mean reverting," which means that the rich tend to report lower income than their actual earnings and the poor to report higher.

Table 1
Summary statistics.

Matched Sample:	Treated Group	Control Group	Weighted Difference
	Households		
Juice purchased (OZ)	2858.4	2835.6	−91.9
	(2742.4)	(2802.8)	(192.4)
Expenditure on fruit juice ($)	104.9	101.5	2.1
	(93.2)	(98.2)	(5.6)
Expenditure on soda ($)	61.2	58.6	−0.5
	(86.1)	(91.2)	(4.2)
Expenditure on alternative drinks ($)	206.0	212.3	1.9
	(175.1)	(184.6)	(9.2)
Expenditure on sugary food ($)	363.5	374.7	−11.5
	(249.4)	(253.5)	(15.2)
Overall expenditure ($)	4625.0	4656.7	63.4
	(2636.3)	(2492.7)	(128.5)
Expenditure per capita ($)	1200.5	1243.4	22.6
	(723.3)	(684.6)	(35.9)
Household size	4.1	3.9	0.01
	(1.2)	(1.0)	(0.1)
Male age	40.8	39.5	0.8***
	(7.8)	(7.5)	(0.3)
Female age	39.1	37.8	0.4
	(7.7)	(7.4)	(0.4)
Single parent portion (%)	9.4	4.3	0.0
	(0.3)	(0.2)	(0.0)
Number of households	1227	4972	6199
	Counties		
Child care availability	4.3	4.2	−0.2
	(1.0)	(1.8)	(0.3)

Notes: The middle two columns report the means and standard deviations (in parentheses) of the key variables and several demographic variables. The last column reports the differences between treated households and matched control households for expenditure and demographic variables listed in the first column. We pool treated and control households and regress the covariates on the treatment indicator. The difference is captured by coefficient estimates of the treatment indicator. Control households are weighted 1/M if M control households are matched to the same treated household. Robust standard errors clustered by county are in parentheses. Upper-panel statistics are at the household level and the lower panel reports county-level child care availability, which is approximated by the number of childcare facilities available per 100 children under the age of 5 in the county.

In the last column of Table 1, we report the coefficient estimates of the treatment indicator, which captures the difference between treated and control households. Robust standard errors clustered by county are reported in parentheses. Differences between treated and control households are small and statistically insignificant in terms of the quantity of juice purchases; expenditure on fruit juice, on soda, on sugary foods, etc.; overall household expenditure; household size; age of female head of household; single parenthood; and county-level child care availability. There is a small but statistically significant difference in the age of male household heads between treated and control households. However, the age of male household heads is never a significant predictor of juice consumption. Our empirical analysis is quantitatively similar with or without controlling for the age of household heads. Overall, treated and control households are similar and, as shown in the next section, follow a parallel trend before implementation of the California Healthy Beverage in Child Care Act in 2012.

3.2.2. Difference-in-differences design

Our main empirical strategy relies on the difference-in-differences matching estimator (Heckman et al., 1998; Smith and Todd, 2005). Let C_{it} be California household i's total juice purchases in year t. Let $\mathbf{M}_{it}$ be the set of non-California households matched to household i in year t. Suppose $n_{it} = |\mathbf{M}_{it}|$ – that is, there are n_{it} matched non-California households for household i in year t. Also let C_{jt} be non-California household j's total juice purchases in year t. Then, we construct ΔQ_{it} as the treatment-control difference in juice consumption:

$$\Delta Q_{it} = C_{it} - \frac{1}{n_{it}} \sum_{j \in \mathbf{M}_{it}} C_{jt}. \tag{1}$$

In our preferred specification, we regress the treatment-control difference ΔQ_{it} on a set of year indicators:

$$\Delta Q_{it} = \sum_{t=2004}^{2016} \delta_t Y_t + \epsilon_{it} \tag{2}$$

Table 2
Juice regulation and household purchase of juice.

	Matched Samples				Full Samples
Dependent Variable:	Juice Purchase Difference		Juice Purchase		Juice Purchase
(Unit: Ounce)	(1)	(2)	(3)	(4)	(5)
Year 2004	−375.978	−247.989	−259.563	−177.901	58.117
	(364.137)	(474.944)	(488.579)	(462.148)	(463.343)
Year 2005	228.939	356.928	222.664	286.422	300.957
	(411.401)	(512.081)	(404.738)	(373.775)	(315.197)
Year 2006	−126.652	1.337	34.196	−157.160	579.347
	(472.902)	(562.683)	(534.030)	(514.170)	(363.441)
Year 2007	−189.159	−61.170	−20.008	60.565	38.179
	(276.645)	(411.714)	(307.477)	(286.051)	(283.976)
Year 2008	−48.847	79.142	117.899	251.471	176.111
	(286.945)	(418.704)	(277.253)	(273.291)	(284.526)
Year 2009	−170.236	−42.247	−24.693	140.238	−107.409
	(308.923)	(434.062)	(288.206)	(244.364)	(277.811)
Year 2010	−95.623	32.366	−46.156	101.378	−238.294
	(264.923)	(403.931)	(283.239)	(271.925)	(160.451)
Year 2011	−127.989				
	(304.920)				
Year 2012	958.801**	1086.791**	856.397**	789.203**	629.364*
	(414.475)	(514.554)	(392.400)	(365.241)	(365.100)
Year 2013	−284.545	−156.555	−62.790	−27.397	178.635
	(432.472)	(529.158)	(576.066)	(517.847)	(363.423)
Year 2014	1.875	129.864	83.609	149.686	457.450
	(343.619)	(459.402)	(426.600)	(362.832)	(367.121)
Year 2015	−118.617	9.373	−112.532	78.166	−61.754
	(278.380)	(412.882)	(337.604)	(293.714)	(325.479)
Year 2016	−434.163*	−306.174	−388.800	−241.797	−119.188
	(257.869)	(399.340)	(454.777)	(396.100)	(257.220)
R^2	0.011	0.010	0.313	0.358	0.333
Observations	1227	1227	6199	6199	15681
Demographic controls	No	No	No	Yes	Yes
Pair Fixed Effects	No	No	Yes	Yes	N/A
County Fixed Effects	No	No	No	No	Yes

Notes: The dependent variable in the first two columns is the weighted quantity difference of juice purchase between matched treated and control groups. Both report coefficients from equation 2. The first column reports the coefficients of all year dummies with no constant included in the regression model. Column (2) reports the coefficients of year dummies except for year 2011, treating it as base year to be compared with. The dependent variable in columns (3), (4), and (5) is juice purchase within a year for a household. The three columns report the coefficients for year dummy interacted with the treatment dummy, using year 2011 as the base year. Estimation in columns (3) and (4) uses the matched samples, with pair fixed effect included in the models. Households' demographic controls from equation (3) are included in the estimation for column (4). Estimation in column (5) uses the full target sample, including county fixed effects and households' demographic controls from equation (4). Standard errors in parentheses are clustered by matched pair in columns (1) and (2) and by county in columns (3), (4), and (5). N/A means not applicable. * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$.

where Y_t is a set of binary indicator variables that indicate years 2004 to 2016; δ_t are the parameters of interest; and ϵ_{it} are error terms. In this specification, observations include the set of California households for which at least one non-California household match was found.

Alternatively, we could include pair fixed effects instead of calculating the treatment-control difference first. Here, a pair is a match of a treated California household i and its non-California control household j.[15] In particular, we estimate the following specification:

$$C_{it} = \sum_{t \neq 2011} \delta_t D_{it} Y_t + d_{it} + X_{it}\beta + \epsilon_{it} \tag{3}$$

where C_{it} is the total juice consumption in year t for household i, D_{it} is an indicator variable indicating whether household i lives in California, Y_t is a year indicator, d_{it} is the pair fixed effect, ϵ_{it} is the error term, and δ_t and β are coefficients to be estimated. X_{it} is a vector of demographic variables of household i in year t, which include log total expenditure excluding drinks, and binary variables that indicate education level of the male head, education level of the female head, race, the presence of household member of Hispanic origin, household size and type of residence. Since we conduct a year-by-year matching, year fixed effects are subsumed by pair fixed effects. In this specification, observations include the set of California

[15] See, e.g., Dube et al. (2010), who use pair fixed effects to implement a difference-in-differences strategy.

households and matched households outside of California. We chose year 2011 as the base year – that is, the year before the legislation was enacted in California – and thus we will compare juice consumption in any other year to that in 2011.

For comparison, we also run a regression similar to Eq. (3) but instead use the full sample of households. In the full sample, treated and untreated households are not matched. Therefore, when we use the full sample, no pair fixed effects are included in the regression. Instead, we include county fixed effects to control for time-invariant location-related heterogeneity:

$$C_{it} = \sum_{t \neq 2011} \delta_t D_{it} Y_t + \mu_c + X_{it}\beta + \epsilon_{it} \tag{4}$$

where C_{it} is the total juice purchased in year t for household i, D_{it} is an indicator variable indicating whether household i live in California, Y_t is a year indicator, μ_c is a county fixed effect, and ϵ_{it} is an error term. The coefficient of interest is δ_t, which captures the treatment-control difference before and after enactment of California's regulation related to the base year (2011). X_{it} is a vector of demographic variables, which include log total expenditure excluding drinks, child care availability, binary variables that indicate the age and presence of children in the households, working hours per week of male head and working hours per week of female head, tobacco usage, alcohol usage, and binary variables that indicate household composition, age of the male head, age of the female head, education level of the male head, education level of the female head, marital status, race, the presence of household member of Hispanic origin, household size, type of residence, kitchen appliances, and whether the household has an internet connection.

We cluster standard errors at the county level for both the pair fixed effects specification of Eq. (3) and county fixed effects specification of Eq. (4). Ideally, we would cluster at the state level, given that the policy treatment varied at the state level in our case (Bertrand et al., 2004). However, we only have one treated state, and the consistency of cluster robust standard errors depends on a large number of clusters. Therefore, we would not be able to get consistent estimates of standard errors with clustering at the state level. To the extent that there is positive correlation of errors among California's counties, our standard errors may be biased downward and falsely reject the null hypothesis. Reassuringly, we find no evidence that there is a substantial positive within-state correlation in the error term. If we run the fixed effects regressions without the interaction terms of California and year indicators, clustering at the state level and clustering at the county level would give similar standard errors for the demographic controls. While these specifications may be misspecified, the similarity of the standard errors suggests that the within-state correlation of the error terms is unlikely to be substantial and, in turn, bias our estimates of standard errors.

4. Results

4.1. Main results

To see whether the parallel trends assumption that underlies our difference-in-differences approach is valid, we plot the average annual juice purchases of California households, as well as the average of their matched non-California households, in Fig. 1.

Recall that Eq. (1) specifies the treatment-control difference for household i in California:

$$\Delta Q_{it} = C_{it} - \frac{1}{n_{it}} \sum_{j \in \mathbf{M}_{it}} C_{jt}.$$

Suppose that in year t, N_t California households get at least one matched non-California household. The solid line in Fig. 1 plots the average juice purchase in ounces by these California households between 2004 and 2016:

$$\overline{C}_t^* = \frac{1}{N_t} \sum_{i=1}^{N_t} C_{it}.$$

Similarly, the dashed line in Fig. 1 plots the average annual juice purchases in ounces by the matched non-California households:

$$\overline{C}_t^o = \frac{1}{N_t} \sum_{i=1}^{N_t} \left[\frac{1}{n_{it}} \sum_{j \in \mathbf{M}_{it}} C_{jt} \right].$$

As shown in Fig. 1, there are clean parallel trends for California households and their matches before 2012: matched California households and their controls share a downward trend in juice consumption. However, there was a sharp increase in juice consumption in California related to the control in 2012. This juice purchase gap decreased after 1 year. Fig. 1 suggests that in the short run, parents provided more juice at home or from home when child care facilities regulated juice consumption in 2012.

Table 2 presents the main OLS estimates for the year indicators (δ_t) from the specification based on Eqs. (2) to (4). The dependent variable in columns (1) and (2) is ΔC_{it}, i.e., the treatment-control difference in the quantity of juice purchases. These estimates come from estimation model (2). The specification in column (1) does not include a constant, and reports coefficient estimates for all year indicators.

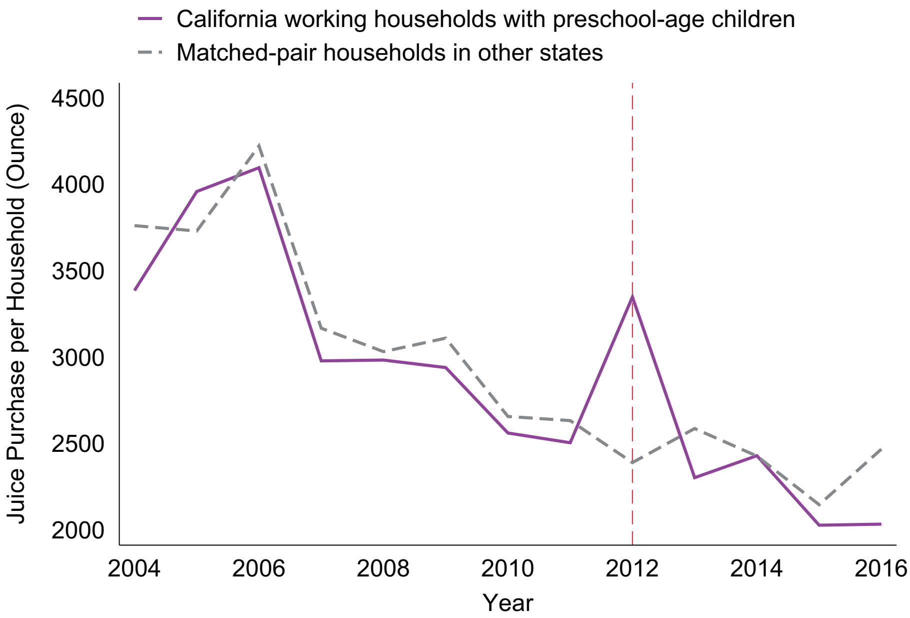

Fig. 1. Juice-purchasing California Households and Matched Non-California Households. Notes: The figure plots trends for average annual fruit juice purchases by households in which there is at least one preschool-age child and all parents are working. The solid purple line represents California households. The dashed gray line represents matched households that share the exact demographic and other matching characteristics but reside outside of California. The vertical red dashed line indicates the first year the California regulation on beverages in child care was enacted, i.e., Year 2012.

The estimate in column (1) shows that for most years before 2012 – the year the juice regulation started – household juice purchase in California was slightly smaller than in matched control states. In 2012, there was a significant increase in juice purchased by treated households; the juice purchase difference changes from negative to positive and increases to a difference equal to 958.8 ounces at the 5% level of significance. The increase is about one-third of average annual juice purchased by treated households. Using the price elasticity of juice from Andreyeva et al. (2010), the increase found in this paper is comparable to a 26% drop in juice price. The standard error of the point estimator is 414.5, which is 15.1% of the standard deviation of annual juice purchases in treated households or 14.5% of their average juice purchases per year.

Given that the sugar-sweetened beverage consumption was regulated at daycares after 2011, our finding suggests that preschool children in California might have demanded more home-purchased juice in 2012 to substitute for their reduced consumption at daycares. Alternatively, this could mean that preschool children in affected households demand that their parents buy juice and bring it to the child care facility. For the following years until year 2016, estimates become statistically insignificant, with most of the signs becoming negative and small in magnitude. In 2016, the difference decreases by 434.2 ounces at the 10% level of significance.

Column (2) is based on a specification similar to column (1), but now a constant is included in the regression model and year 2011 is the omitted baseline year. Column (2) reports coefficient estimates for all year indicators except 2011. The pattern we observe is the same as that in column (1); that is, the difference in juice consumption between affected and unaffected households is statistically significant in 2012.

We also tested the 2012 coefficient against other baseline years. Juice purchase is significantly higher at the 5% level in 2012 compared with each of the prior years except for years 2005 and 2006. Juice purchase in 2012 is higher than in 2006, but the difference is only statistically significant at the 10% level. Juice purchase in 2012 is also higher than in 2005, but the difference is statistically insignificant.

In Table 2 columns (3), (4), and (5), the outcome variable is the household's juice purchase. These three columns report the estimated effects for all year indicators interacted with the treatment indicator; 2011 is the baseline year and is thus omitted. Estimations in columns (3) and (4) come from Eq. (3) and use the matched samples, while pair fixed effect is included in the models. Household demographic controls are included in the regressions used in column (4). Juice consumption in 2012 in affected compared with unaffected households increased by 856.4 ounces compared with 2011 – the baseline year – when we only include pair fixed effects in column (3). This increase is statistically significant at the 5% level. When we include demographic controls in column (4), the magnitude drops slightly to 789.2 ounces and is statistically significant at the 5% level. The two sets of estimates point to the same conclusion as the estimates in columns (1) and (2).

The estimated effects in column (5) correspond to Eq. (4) and use the full target sample, including county fixed effect and household demographic controls. As we would expect, when the matching process is not implemented and we instead use the full sample, the estimate and standard error decrease for the interaction term in 2012, leading to a loss in precision. However, as we can see, the juice ban still induces a large increase in juice consumption in affected compared with unaffected households by 629.4 ounces.

Notice that, although the California regulation limits juice provision by child care facilities, it does not prohibit parents from providing their children with juice from home. One reason parents buy more juice in 2012 is so that their children could still consume juice at child care at an amount similar to prior years. Such offsetting behaviors may be due to demand by their own children or peer pressure from other parents. Another reason that parents buy more juice in 2012 is that their children now consume more juice at home due to reactance (Debnam, 2017). While we are not able to separate or apportion the two channels, our findings suggest that there was some short-term persistence in children's juice consumption. However, such persistence was short-lived.[16] This finding is consistent with the reactance response to the Berkeley Soda Tax Debnam (2017) examined; she finds a short-term response after the soda tax vote but before the policy was implemented.

In the years following 2012, children seem to adjust their preferences and stop demanding more juice or their parents stop supplying juice for their children to consume at the child care facility. In the meantime, their consumption of juice at daycares dropped as a result of the regulation, which means that overall juice intake for treated children in California decreased. In 2016, the children's juice consumption significantly decreased. The effect, compared with year 2011, is not statistically significant but is still negative with a large magnitude. Therefore, although the coefficients are insignificant, there is suggestive evidence that the juice regulation leads to a gradual decrease in overall juice intake for California children affected by the policy.

Fig. A.1 shows coefficient estimates for all years in the sample that stem from the regression in which the treatment-control difference is the dependent variable. Subplots A to D in Fig. A.2 show coefficient estimates for each year that correspond to columns (2) to (5) in Table 2. These regressions use 2011 as the base year. Both figures reassure us that there are no significant differences in juice purchase between affected/treated and unaffected/control households in any year prior to the policy change. In 2012, there was a spike, which indicates that affected households react and purchase significantly more juice compared with unaffected households. In the years following the policy change, the juice purchase pattern converges back to that of pre-policy years and shows similar juice consumption patterns between affected and unaffected households. In 2016, we observe a decrease in juice purchases for affected households in Fig. A.1. These patterns are compatible with the regression estimates we find in Table 2.

4.2. Impact on consumption of substitutes

4.2.1. Different types of substitutes

To examine whether treated children switch their preferences to other types of drinks or sugary food, we examine the impact of the policy on the consumption of goods that are substitutes for juice. We define three types of substitutes. The first is soda, which is also a main contributor to sugar-sweetened beverages. The second and broader type of substitutes includes both soda and ingredients for homemade juice. Homemade juice may be healthier, but still is sugary and may also include added raw sugar. Thus, we define the second type of substitutes as "Alternative Drinks," which include soda, raw sugar, and fruits. Our third type of substitutes is any other solid sugary food such as ice cream, candy, and cookies, and we call these "Sugary Foods." Unlike the unit of the quantity of juice consumed which is all "OZ (Ounce)," the unit of quantity for these types of substitutes purchased by households is not consistent. Thus, instead of the quantity of juice consumed (in ounces), we use expenditure to measure the purchase of these substitutes. Accordingly, the dependent variables in dollar terms are the weighted difference in purchases, similar to that in Eq. (2).

Table 3 presents the annual difference between affected and unaffected households in expenditures on soda, alternative drinks, and sugary foods in columns (2), (3), and (4), respectively. For comparison, we also regress differences in expenditures on fruit juice on annual indicators in column (1). We find that there is no significant change in expenditure differences on substitute goods between paired households during the sample period. These results are robust to different compositions of sugary foods.

4.2.2. Trends in expenditure on juice and other substitutes

In Appendix Fig. A.3, we plot the annual weighted average expenditure per household for the matched treated and control samples for all years between 2004 and 2016. From the top left to the bottom right figures, the vertical axis represents expenditure in dollars on fruit juice, soda, alternative drinks and sugary food, respectively. The solid pink line represents working households in California with at least one preschool child – that is, the affected group. The dashed gray line represents matched control pairs in other states, which constitute the unaffected group.

Fig. A.3 panel A shows the average expenditure on fruit juice for treated and matched control households. We observe that annual trends in expenditure on fruit juice follow a different pattern, especially in the year of the regulation. As discussed earlier, there is a significant increase in juice consumption in 2012, the year the juice ban policy was implemented. In the other three figures, the time trends in expenditure (in dollars) on the potential substitutes for fruit juice follow a very

[16] Columns (1) to (5) in Appendix Table A.1 correspond to each regression in columns (1) to (5) in Table 2, except that pre-2012 and post-2012 observations are grouped together. We find that the estimates for post-2012 are significantly different from year 2012 at the 1% level for Columns (1) to (3), at the 5% level for Column (4), and at the 10% level for Column (5). The Wald-test results suggest that the significant increase in juice purchases in year 2012 for California households did not persist after 2012.

Table 3
The juice regulation's impact on potential substitute goods.

Dependent Variable:	Weighted Difference in Expenditure			
	Fruit Juice (1)	Soda (2)	Alternative Drinks (3)	Sugary Foods (4)
Year 2004	−6.322	−13.878	−16.193	−15.266
	(11.639)	(13.642)	(17.152)	(16.698)
Year 2005	17.276	14.098	−17.816	−9.299
	(12.437)	(12.480)	(20.124)	(20.765)
Year 2006	12.059	0.770	−18.354	−9.963
	(15.322)	(13.669)	(20.071)	(24.694)
Year 2007	−1.190	−8.186	8.268	3.644
	(9.110)	(7.500)	(12.706)	(14.685)
Year 2008	3.412	−4.339	18.326	−13.611
	(9.487)	(9.627)	(12.957)	(21.999)
Year 2009	−3.885	3.816	13.440	−0.544
	(9.954)	(8.703)	(11.369)	(16.082)
Year 2010	−3.854	−8.445	2.322	−31.737
	(8.206)	(7.784)	(14.984)	(19.886)
Year 2011	−5.426	0.280	11.132	−6.155
	(9.562)	(8.513)	(13.960)	(17.092)
Year 2012	32.435**	11.387	−11.206	1.336
	(13.743)	(11.567)	(30.005)	(40.820)
Year 2013	−4.674	−5.570	−16.203	−19.202
	(15.190)	(8.063)	(18.784)	(29.045)
Year 2014	2.503	8.203	17.003	2.479
	(12.403)	(10.342)	(17.236)	(23.833)
Year 2015	9.528	−3.138	10.446	−48.789
	(10.682)	(8.607)	(21.337)	(38.702)
Year 2016	−9.225	6.849	−2.228	−12.872
	(8.217)	(10.402)	(18.712)	(27.088)
R^2	0.011	0.007	0.007	0.007
Observations	1227	1227	1227	1227

Notes: Columns (1) to (4) report coefficients of the yearly impact on average expenditure differences between paired households on fruit juice, soda, alternative drinks, and sugary foods. Apart from soda, we include potential homemade juice in the alternative drinks category. For sugary foods, we also include some potential solid sugary foods such as candy and cookies. Standard errors are clustered by pair. * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$.

similar trend, and show no evidence of substitution behavior. In Fig. A.3 panel B, we notice that the expenditure difference (in dollars) on soda between affected and unaffected households, which is a substitute for fruit juice, follows parallel trends over the sample period. The same applies to panel C, in which the expenditure trends for "Alternative Drinks" follow similar time trends in the sample period. The Alternative Drinks group combines soda, raw sugar, and fruits. The same applies to case D, in which we observe that annual trends in expenditure on "Sugary Food" follow common time trends. Sugary food includes any solid sugary food such as ice cream, candy, and cookies.

Summarizing the patterns we observe in terms of the three types of substitutes, their figures follow parallel trends in expenditure during the sample period. We find further evidence, therefore, that it is unlikely that children substitute fruit juice in favor of other sugary foods and drinks.

4.3. Heterogeneous treatment effects by child care availability and income

4.3.1. By child care availability

In order to gain further insight into the effects of the child care juice ban on children's behavior and preferences, we examine heterogeneous effects across different dimensions. The juice regulation applies to all child care facilities in California. It is worthwhile to examine whether the regulation had a differential impact on household juice purchase across regions with different levels of child care availability and income.

In Table 4 column (1), we report the baseline estimates shown in Table 2 as a point of comparison, using all matched observations. In columns (2)–(5), we present the estimated effects of the regulation on juice consumption and expenditure for different levels of child care availability, based on different stratifications of the sample. In particular, in columns (2) and (3), we group households based on the three quantiles of child care availability in the county in which the household lives. The upper quantile is defined as households in counties with the highest level of child care availability, while the lower quantile is defined as households in counties with the lowest level of child care availability. The middle quantile consists of households in counties with a middle level of child care availability. Since California in general has a high level of child care availability, the number of observations that are categorized as being in the upper quantile in the matched sample is 760, which accounts for more than 60% of the total number of matched observations. We also use a complementary method to

Table 4
Heterogeneous effect.

Dependent Variable:	Juice Purchase Difference						
		Level of Child Care Availability				Level of Expenditure	
	Baseline (1)	High Level (2)	Middle&Low Level (3)	Above Median (4)	Below Median (5)	Above Median (6)	Below Median (7)
Year 2004	−375.978	−204.665	−730.419	−331.005	−431.070	−567.791	−219.396
	(364.137)	(436.831)	(680.106)	(451.784)	(637.488)	(794.883)	(232.722)
Year 2005	228.939	494.717	−555.105	588.143	−178.805	308.907	111.486
	(411.401)	(450.459)	(920.637)	(561.928)	(572.658)	(648.667)	(392.205)
Year 2006	−126.652	103.950	−1363.521	128.586	−615.859	−321.358	3.152
	(472.902)	(483.372)	(1546.220)	(603.824)	(747.672)	(851.411)	(493.966)
Year 2007	−189.159	118.200	−766.993*	123.402	−501.720	−310.578	−92.023
	(276.645)	(332.917)	(451.376)	(384.994)	(356.627)	(444.312)	(343.583)
Year 2008	−48.847	189.961	−486.662	159.409	−276.328	95.450	−193.145
	(286.945)	(366.015)	(432.270)	(370.719)	(358.771)	(526.215)	(202.086)
Year 2009	−170.236	−239.151	−48.815	−110.308	−241.472	−617.072	192.818
	(308.923)	(422.661)	(431.180)	(482.024)	(355.702)	(508.247)	(335.999)
Year 2010	−95.623	−38.966	−203.626	115.557	−375.437	−315.337	119.416
	(264.923)	(319.032)	(470.565)	(288.625)	(458.961)	(439.250)	(271.496)
Year 2011	−127.989	−158.807	−56.973	−48.029	−257.579	−177.955	−75.322
	(304.920)	(381.853)	(502.444)	(429.567)	(414.071)	(477.049)	(386.066)
Year 2012	958.801**	1488.683**	143.599	1269.134*	666.723	1827.445***	−83.571
	(414.475)	(586.280)	(474.233)	(705.906)	(502.646)	(638.028)	(421.026)
Year 2013	−284.545	169.457	−616.741	−800.259	−37.431	−617.194	98.506
	(432.472)	(873.475)	(387.645)	(693.132)	(548.493)	(768.699)	(286.542)
Year 2014	1.875	374.655	−397.533	167.669	−120.813	−232.537	230.959
	(343.619)	(550.273)	(375.408)	(488.844)	(427.840)	(574.176)	(397.585)
Year 2015	−118.617	38.122	−246.567	−475.119	101.767	−35.612	−235.272
	(278.380)	(492.462)	(310.078)	(444.917)	(364.743)	(439.231)	(232.232)
Year 2016	−434.163*	31.257	−925.440**	−331.303	−504.295	−420.776	−450.495*
	(257.869)	(340.942)	(374.175)	(351.539)	(372.558)	(431.322)	(233.967)
R^2	0.011	0.017	0.043	0.020	0.015	0.025	0.009
Observations	1227	760	467	614	613	614	613

Notes: The dependent variable is the weighted quantity difference in juice purchase between the matched treated and control groups. In column (1), we report baseline estimates from Table 2, column (1) for comparison, using all matched observations. Columns (2) and (3) report the effect on households from regions with a high level of child care availability and middle and low levels of child care availability, respectively. The criteria for segments are consistent with the matching process. Columns (4) and (5) report the effects for households in counties with above-median child care availability and counties with below-median child care availability, respectively. Columns (6) and (7) report the effect for households with upper median overall expenditure level and those with below median overall expenditure level, respectively. Standard errors in parentheses are clustered by matched pair. * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$.

divide regions as being above and below the median based on child care availability in the county in which the household is located, and the estimated effects are reported in columns (4) and (5), respectively.

Estimates in columns (2) to (5) show that the increase in juice consumption in 2012 was driven by households in counties with a high level of child care availability. In particular, the estimate in column (2) suggests that California households in counties with a high level of child care availability purchased 1488.7 ounces more juice in 2012 relative to their matched pairs in other states with the same level of child care availability. Interestingly, the juice consumption pattern for households in areas with high child care availability is different from that in the baseline model in column (1). The estimated effects in column (2) are all positive and the magnitudes decrease over time, which means that the juice purchase level for treated households living in counties with a high level of child care availability are still higher than that for their matched unaffected households, even after 2012.

This could imply that children on average in counties with high child care availability attend child care more regularly, which means that those children are more likely to have more persistent juice preferences and it might be harder for them to switch tastes when the juice regulation is enacted. On the other hand, column (3) suggests that there is no significant substitution effect in 2012 for treated children who live in counties with middle or low levels of child care availability. In the following years, the estimated effects become negative but insignificant. However, in 2016 the juice purchase for treated households in counties with middle or low levels of child care availability is significantly lower than that for their matched control pairs by 925.4 ounces, at a 5% level of significance. This reinforces the argument that children who attend child care more regularly have a strong preference for juice, while children who attend child care less regularly would more easily switch their preferences.

However, the number of observations in column (3) is smaller than those in column (2). As a robustness check, we run the regressions separately for households above (column 4) and below (column 5) median child care availability, and the number of observations is similar in the two groups. Also, the pattern is similar to that in columns (2) and (3). Comparing the estimates in columns (4) and (5), we can see that the increase in juice consumption in 2012 was driven by households in counties with above-median child care availability. Consistent with the previous results, the increase in juice consumption in 2012 was driven by children who are likely to attend child care more regularly.

4.3.2. By income

In columns (6) and (7) of Table 4, we report separate estimates for children in households with income above and below the median, respectively. To deal with inconsistencies in how the income variable is categorized,[17] we use overall expenditure on non-drinks to approximate the household income level and report the heterogeneous effect by income.

The estimates in column (6) suggest that treated California households with income above the median consume on average 1827.4 ounces more juice relative to their matched pair households in other states in 2012. On the other hand, the estimated effect in 2012 for households in counties with income below the median is negative and insignificant.

Overall, the increase in juice purchases in 2012 seems to be driven by households that live in counties with high child care availability and that have above-median incomes. This may be due to two reasons. First, the effects might be driven by children in urban areas. Households in urban areas may earn relatively more, and urban areas tend to have higher child care availability. Second, households in which the parents(or single parent) work longer hours tend to earn more, and they are likely to put their children in child care due to the long working hours.

Therefore, preschool-age children from households with a higher income level might attend child care more regularly and find it harder to adjust to the new juice policy at child care. Households with lower income levels might send their preschool-age children less regularly to child care due to limited child care availability or, if they do not work, may keep their children home rather than using child care. These households did not seem to experience an increase in their juice consumption in 2012. Instead, they decreased their juice purchases in 2016. As a result, children who attend child care less regularly are more likely to adjust to the new child care policy more quickly and might consume less juice at home at the end of the sample period.

4.4. Robustness of the estimates

In this section, we present a set of robustness checks and alternative specifications that support the causal interpretation of our main findings.

4.4.1. Different specifications of dependent variables

The baseline definition of juice we have used so far includes only fruit juice. However, there are also other types of juice, including fruit punch, syrups, cider, clam juice, and vegetable juice. In Table 5, we present our main estimates but change the definition of juice, and rerun the main specification. In column (1) we report the main specification using the baseline definition. In column (2) we include fruit punch and syrups in the juice category, rerun the main specification, and report the estimates for each year indicator. In column (3), we further include cider and clam juice in the juice definition and re-estimate the year indicators on the juice purchase differences between affected and unaffected households.

The pattern in Table 5 is very similar to that in Table 2, and does not depend on the adopted definition of juice. In particular, there was a statistically significant increase in juice consumption in 2012. The estimates based on all three definitions of juice are very similar and insensitive to the different definitions of juice: 958.8, 958.1 and 942.0 in columns (1), (2), and (3), respectively. All three estimates are significant at the 5% level. In the following years, juice consumption drops substantially. In 2016, this drop in juice consumption of affected compared with unaffected households becomes statistically significant and does not depend on which definition of juice we adopt.

In columns (4) to (6), we examine the impact of the regulation on juice expenditure rather than juice purchase quantity. We report the estimates for all year indicators that represent annual changes in juice expenditures between affected and unaffected households. In column (4), we restrict juice to fruit juice, in column (5), we include fruit punch and syrups, and in column (6), we further include cider and clam juice in the juice definition. As we can see in Table 5, whether we include the extra juice types does not much affect the estimates between columns (1) and (3) or columns (4) and (6). In contrast, in columns (4) to (6) we use juice expenditure as the dependent variable, and the substitution effect in year 2012 is still large and significant. The juice regulation increased the difference in expenditure between affected and unaffected households by more than 30 dollars per household on average in 2012. This difference is significant at the 5% level. In the following years, juice expenditure dropped substantially. In 2016, we observe a negative effect on juice consumption between affected and unaffected households, which is statistically significant. The difference in juice expenditure is also negative in the following years, but statistically insignificant. These patterns remain unchanged regardless of which definition of juice we adopt.

4.4.2. Truncated samples

To ensure that outliers are not driving our estimates, we perform an exercise that truncates the sample and re-estimate the main specification; the outcome variable is the difference in juice purchase between affected and unaffected households. In particular, we exclude households that purchase excessive quantities of juice and are above the 99th percentile for juice consumption, as well as those households that do not purchase juice at all (more than 1st percentile), according to the distribution of juice purchase in the full sample.

[17] The income variable is a categorical variable, and the values for the top range of household income change over time.

Table 5
Robustness exercises for the main estimates.

Dependent Variable:	Juice Purchase Difference			Juice Expenditure Difference			Juice Purchase Difference		
							Truncated samples		
	Baseline (1)	Include punch (2)	All juice (3)	Fruit juice (4)	Include punch (5)	All juice (6)	Fruit juice (7)	Include punch (8)	All juice (9)
Year 2004	−375.978	−375.793	−383.061	−6.322	−6.746	−6.360	−104.461	−108.107	−105.905
	(364.137)	(365.570)	(364.765)	(11.639)	(11.707)	(11.647)	(254.999)	(255.275)	(256.264)
Year 2005	228.939	225.610	225.044	17.276	16.880	17.070	327.937	323.510	348.788
	(411.401)	(411.754)	(408.604)	(12.437)	(12.494)	(12.422)	(341.152)	(341.418)	(331.871)
Year 2006	−126.652	−125.873	−167.706	12.059	11.972	11.454	−26.893	−26.010	−65.149
	(472.902)	(472.901)	(474.307)	(15.322)	(15.311)	(15.333)	(357.191)	(357.142)	(357.946)
Year 2007	−189.159	−189.852	−202.746	−1.190	−1.494	−1.502	77.414	77.559	88.375
	(276.645)	(277.033)	(279.934)	(9.110)	(9.137)	(9.189)	(240.205)	(240.259)	(241.175)
Year 2008	−48.847	−47.696	−62.638	3.412	3.337	2.955	198.047	180.732	185.998
	(286.945)	(286.580)	(286.803)	(9.487)	(9.447)	(9.474)	(227.244)	(225.960)	(227.109)
Year 2009	−170.236	−170.567	−192.012	−3.885	−4.116	−4.587	−35.843	−36.187	−66.384
	(308.923)	(309.149)	(310.089)	(9.954)	(9.982)	(9.973)	(304.110)	(304.425)	(302.496)
Year 2010	−95.623	−97.755	−120.470	−3.854	−4.144	−4.538	−15.463	−16.721	−40.313
	(264.923)	(265.178)	(266.083)	(8.206)	(8.219)	(8.247)	(243.078)	(242.947)	(243.323)
Year 2011	−127.989	−128.678	−144.932	−5.426	−5.337	−5.319	−116.203	−161.572	−131.955
	(304.920)	(304.950)	(304.930)	(9.562)	(9.524)	(9.677)	(261.129)	(261.128)	(260.515)
Year 2012	958.801**	958.117**	941.966**	32.435**	31.994**	31.994**	740.344*	739.716*	724.287*
	(414.475)	(414.400)	(413.840)	(13.743)	(13.594)	(13.693)	(384.719)	(384.496)	(384.135)
Year 2013	−284.545	−284.524	−291.338	−4.674	−3.682	−5.012	−333.377	−334.694	−274.360
	(432.472)	(433.191)	(433.009)	(15.190)	(16.560)	(15.262)	(254.099)	(253.986)	(271.073)
Year 2014	1.875	0.364	−23.537	2.503	2.451	1.846	−25.359	−19.997	−51.203
	(343.619)	(343.691)	(344.709)	(12.403)	(12.456)	(12.386)	(310.341)	(311.174)	(311.773)
Year 2015	−118.617	−119.767	−135.427	9.528	8.692	8.968	−210.845	−209.739	−228.148
	(278.380)	(277.504)	(277.245)	(10.682)	(10.496)	(10.611)	(254.020)	(254.082)	(252.283)
Year 2016	−434.163*	−434.754*	−460.891*	−9.225	−9.968	−10.232	−177.555	−178.175	−288.907
	(257.869)	(257.502)	(257.438)	(8.217)	(8.204)	(8.194)	(204.457)	(203.971)	(196.012)
R^2	0.011	0.011	0.011	0.011	0.011	0.011	0.009	0.009	0.010
Observations	1227	1227	1227	1227	1227	1227	1188	1190	1187

Notes: The dependent variable is the weighted difference of juice purchase between matched treated and control groups. In column (1), we report baseline estimates from column (1) of Table 2 for comparison. The baseline definition of juice is fruit juice. Column (2) adds fruit punch and syrups. Column (3) further adds cider and clam juice. Columns (4) to (6) report the coefficients of the yearly impact on average expenditure differences between paired households, using the same definition of juice as in Columns (1) to (3). Columns (7) to (9) report estimates from truncated samples, using the same definition of juice as in Columns (1) to (3). * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$.

The Nielsen Company, which collected the consumer panel dataset, targeted households that are demographically diversified and geographically dispersed to cover a large range of socioeconomic backgrounds and geographic accessibility to services and facilities. In our paper, we only target a specific group of households – that is, households in which parents are working and have at least one preschool-age child; we consider these to be our targeted households.

The full sample includes those targeted households, but also households that are not in our targeted group. The mean quantity of juice purchased between targeted households and non-targeted households is quite different. On average, targeted households purchase more juice than non-targeted households. Thus, the idea behind this is that by truncating the sample of matched households, the sample becomes more similar to the full sample.

After we drop those households, we rerun our main specification which is equivalent to the one in columns (1), (2), and (3), and we report estimates for the year indicators in columns (7), (8), and (9). In column (7), we adopt the baseline definition of juice; in column (8), we also include fruit punch juices, and syrups; in column (9), we include all other types of juice.

The juice regulation affected juice purchased in affected and unaffected households in 2012, which is the same pattern we observed in columns (1), (2), and (3). The difference in juice consumption is significant at the 10% level, while the magnitude of the estimate is slightly smaller. However, the impact on juice purchase in year 2016 is no longer significant and the magnitude drops, which implies that by truncating the sample, we lose efficiency.

We can see that the broadest definition of juice used in columns (3), (6), and (9) does not include vegetable juice. This is because vegetable juice is considered to be a healthy option, and is usually not preferred by children. Even if we include vegetable juice into the broadest juice definition, however, and rerun our main specifications, the estimates are nearly unchanged.

4.4.3. Households with children under 5 years old

Nielsen sends demographic surveys to their recruited households for year t in the last quarter of year $t-1$. Using the demographic surveys, Nielsen codes household composition into a categorical variable, which we use to identify households that have at least one preschool-age child under 6 years. Since we know the birth year and month of each household member, we could alternatively code households with a preschool-age child using their birth dates. Specifically, we code a child as being preschool-age in year t based on whether they have turned 5 by the end of December, November, and October, respectively, in year $t-1$.

Using the alternative coding for treatment households in California, we estimate our main specification again and report the results in Appendix Table A.2. We also report our main result from Column (1) of Table 5 in Column (1) for comparison. As shown in Column (2) of Table A.2, it turns out that using that December in year $t-1$ as the cutoff yields results identical to our baseline results. In other words, the categorical variable we used indicates a child that is less than 6 years by the end of year t. Using a child who is 5 by November or October in year $t-1$ gives quantitatively similar results. We therefore do not report these estimates to conserve space.

The minimum school entry age in California is 6 years by September 1 of a school year. Therefore, children who had not turned 5 by September of year t were not eligible to attend primary school in purchase year t. Using such a cutoff is the most restrictive, as it most likely eliminates false positive identifications of treated households by the childcare juice regulation in California. On the other hand, it omits some treated households with children in child care and reduces the sample size of treated households, which may potentially lower our statistical power. We report our estimates using September of year $t-1$ as our cutoff for child care attendance. The year 2012 coefficient estimate is slightly smaller but remains statistically significant at the 5% level.

In sum, our estimates are robust to alternative coding of treated households using alternative age cutoffs to define households with children in child care.

4.4.4. Matching by alternative childcare availability measurement

In our exact matching process, we use the number of child care facilities per 100 children, which includes those with and without employees. Alternatively, we use two sets of indicators instead in matching to measure child care availability at the county level. The first set of indicators reports the tertile of average annual employment by child care facilities with an employee on payroll per 100 children; the second set indicates the tertile of the number of non-employer businesses per 100 children.

Because we replace one indicator that measures child care availability with two indicators for the matching criteria, we have 329 observations that could not find their exactly matched controls, leaving 898 observations in the treated group. We rerun our main specification using these observations and their matched pair and report the estimates in Column (4) in Table A.2. The results are qualitatively similar to those in column (1) from the main specification. There is a significant increase in juice purchase for California households compared with those from other states in year 2012. The estimate has similar magnitude as before and is significant at the 10 % level. The juice purchase of California households decreased by 761.7 ounces in year 2016, which is statistically significant at the 5% level.

5. Conclusions

In this paper, we exploit a reform that took place in 2012 in California that regulated the provision of sugar-sweetened beverages for preschool children in child care facilities. In particular, child care facilities in California, had to restrict the quantity of juice from four to one serving per day and instead encourage water consumption. This regulation aimed to reduce sugar-sweetened beverage consumption and change children's preferences for sugary drinks from an early age.

The rationale behind the regulation is to promote a healthy and balanced diet and prevent obesity. The prevalence of obesity is a major problem in developed countries and is associated with elevated mortality and other serious health problems, such as type 2 diabetes, hypertension, and asthma (Must et al., 1999). A fundamental aspect of obesity is that people have time-inconsistent preferences. That is, eating is immediately enjoyable, while the benefits of a healthy diet emerge in the distant future (Cawley, 2015; Ruhm, 2012).

The combination of economic and biological factors is likely to result in overeating readily available food (Ruhm, 2012). Ruhm (2012) models two decision systems, and argues that food consumption reflects the interaction between two parts of the brain: a deliberative system that makes decisions, as in traditional models in economics, and an impulsive system that responds rapidly to stimuli, but does not account for long-term consequences. An extensive literature focuses on optimal incentives to help consumers overcome the impulsive decision system (e.g., Cawley and Price, 2013; Just and Price, 2013; Charness and Gneezy, 2009). The basic assumption underlying both decision models is that consumers prefer energy-dense food over healthy food and that food preferences, partially those decided by one's genes, also depend on the eating environment and child-feeding practices (Birch, 1999).

Our paper provides empirical evidence that preschool-age children's preferences for sugar-sweetened beverage could be reversed if they are provided with alternative, healthier options. As a consequence of the regulation, in affected households there was a sudden offsetting behavior to compensate for the quantity of juice children could no longer consume at daycares. This is especially the case for children who attend daycares more regularly and are likely to be used to the original unhealthy serving pattern. In particular, there was a significant increase in juice consumption at home in the year following the reform, compared with unaffected households. However, we find that a couple of years after the regulation was enacted, preschool children in affected households quickly stopped demanding more juice at home and even reduced their juice consumption, suggesting that their preferences are not yet time persistent. This might be a budget-neutral and effective policy to limit the obesity problem, which starts in the early stages of a child's life. Along with increasing female labor force participation, it is important to note that children's preferences can change due to policies implemented in child care facilities. This contributes to the argument that child care services become steadily more important for a child's development.

Declaration of Competing Interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Supplementary material

Supplementary material associated with this article can be found, in the online version, at doi:10.1016/j.jebo.2020.08.014.

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