Endogenous Responses to Paternalism: Examining Psychological Reactance in the Lab and the Field[1]

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Abstract

By accounting for limited human computational ability, willpower, and rationality within economic models, work in behavioral economics has highlighted the ways in which individuals' choices may systematically deviate from their own best interest. As a result, policymakers have considered any number of paternalistic policies (both overt taxes and restrictions, or more subtle "nudges") to move individuals closer to optimal outcomes. Much work, however, remains to characterize optimal design within this new class of policy instruments and to understand their aggregate impact. We present a theoretical framework of individual response to paternalistic interventions which considers, in addition to the set of behavioral responses explicitly incentivized by the policy, an additional behavioral outcome – the agent's impulse to re-establish whatever perceived choice set he had before the intervention occurred. We refer to this behavioral outcome as *psychological reactance*, a concept introduced by Brehm (1966). In support of this framework, we first provide evidence on the nature and magnitude of *reactance* responses from a laboratory experiment designed to measure response to paternalistic advertisements. We then present evidence of consumption responses to paternalistic advertisement in and around New York City during the policy debate surrounding then Mayor Bloomberg's proposed restrictions on sugary drink consumption within city limits (popularly referred to as a "soda ban"). Our findings support the existence of real interaction effects of paternalistic public policies.

JEL Classification: A13, H43, I18, M37, M38

Keywords: Paternalism, nudges, sugar-sweetened beverage taxes, reactance, behavioral economics

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Governments have a long history of restricting choice to protect willing adults from themselves. Examples of choice-restricting policies include minimum drinking ages, helmet-wearing and seat-belt laws, laws restricting the sale and use of illicit drugs, and laws prohibiting the sale of human organs. These policies draw a distinction between individual choices and individual optimality, thereby rejecting standard, revealed-preference notions of optimal choice. Instead, these policies are grounded in evidence from behavioral economics which illustrates the vulnerability of individual choices to systematic errors[2]. To the conversation surrounding choice-restricting policies, we introduce a general model of consumer response which allows for a new feature grounded in psychology– the potential for choice-restricting policies to induce an endogenous adverse reaction to their existence.

By definition, under the standard theory of revealed preference, chosen options are weakly welfare-improving options. However, decades of work in behavioral economics has highlighted the sub-optimal choices that this theory rationalizes. Individuals consume addictive drugs and pay for gym memberships that go unused. Relative to rational agents, agents under-save for retirement (Burman, Gale, Hall & Orszag 2004), wait too long to realize financial losses (Odean 1998), and under-insure against some catastrophic risks such as floods (Michel-Kerjan 2010). Individuals exhibit a strong preference for the default option in a variety of settings, and suffer sub-optimal outcomes as a result (Choi, Laibson, & Madrian 2004; Madrian & Shea 2001; Handel 2013). These mistakes, among others, have led economists to develop alternative frameworks by which to evaluate welfare.

Kahneman, Wakker, and Sarin (1997) provide an early alternative framework to measure welfare improvements without relying on revealed preference. Rather than the notion of utility most commonly used by economists, *decision utility*, they argue that Benthamite (Bentham 1789) *experienced utility* should be applied in decision contexts where individuals may make systematic mistakes. Decision utility is directly inferred from choice orderings; experienced utility is inferred from the feelings of pleasure or pain brought about by a particular choice. The latter welfare notion allows an option to be optimal which makes an individual better off in the long-run (like forced seat-belt wearing) even if the individual would not choose it. Bernheim and Rangel (2009) suggest an alternative criterion for defining a welfare improvement, defining that individual-level optima can be discovered by observing individual choices across time, and then excluding those choices which are the product of 'distorted' decision-making processes, such as addiction and self-control problems. Loewenstein and Ubel (2008) introduce the role of hedonic adaptation, where experienced utility fails to account for the welfare impact of long-term positive or negative events (like winning the lottery or losing a limb) to which individuals ultimately habituate. Loewenstein and Ubel propose a welfare measure based on the decision utility of well-

[2] DellaVigna (2009) reviews the field and experimental evidence supporting the behavioral economic claim that individuals deviate from the behavior of standard agents in important ways.

informed decision-makers who have been intentionally de-biased and informed of policy consequences in terms of experienced utility. Under each of these frameworks, a "nudge" or choice-restriction by a policymaker may be welfare improving in the presence of systematic errors by individuals.

Grounded in these welfare notions, economists have begun to characterize optimal choice-restricting policies. Prominently, Thaler and Sunstein (2003) introduce the idea of *libertarian paternalism* to describe a notion of a choice-modification wherein a policymaker might actively manage biases in individual decision-making without eliminating options from an individual's choice set. By so doing, they argue, policymakers may optimize individuals' (hypothetical) true individual preferences over ultimate outcomes, if those outcomes were salient and explicit to the individual decision maker. O'Donoghue and Rabin (2003) consider a framework with an estimated distribution of rational- and irrational-type agents in an economy, and then suggest that interventions be designed to minimize irrational mistakes while preventing distortions in rational actors' behaviors. This importance of balancing positive outcomes for irrational individuals with minimizing implications for rational ones is also central in the 'asymmetric paternalism' suggested by Camerer, Issacharoff, Loewenstein, O'Donoghue & Rabin (2003). Beshears, Choi, Laibson, & Madrian (2005) describe a set of regulations (which they coin *early decision regulations*) which would increase the cost of socially undesirable behaviors by making them more inconvenient to engage in, for example, by placing geographic or time-based restrictions on the sale of cigarettes to overcome present-biased preferences. Much of the above literature can be characterized as supporting minimally invasive interventions which optimize individual preferences over longer-term outcomes.

Current paternalistic efforts by U.S. policymakers are far-reaching. The White House and Department of Agriculture's Food and Nutrition Service has begun to automatically enroll low-income children in free or reduced-price school meal plans[3]. The Consumer Financial Protection Bureau has discouraged the offerings of complicated 'exotic' mortgage products[4], and has required the choice of overdraft protection be framed to consumers as an opt-in rather than an opt-out decision[5]. Familiar price incentives such as federal, state, and local cigarette taxes exist throughout the country, and sugar-sweetened beverage (SSB) taxes have been implemented in an increasing number of U.S. cities[6]. Other examples include mandated health insurance coverage as part of the Affordable Care Act, seat-belt

[3] Social and Behavioral Sciences Team – Executive Office of the President, *2016 Annual Report*, accessed at https://sbst.gov/download/2016%20SBST%20Annual%20Report.pdf
[4] Consumer Financial Protection Bureau, *Ability-to-Repay and Qualified Mortgage Standards Under the Truth in Lending Act (Regulation Z)* accessed at https://www.federalregister.gov/documents/2013/01/30/2013-00736/ability-to-repay-and-qualified-mortgage-standards-under-the-truth-in-lending-act-regulation-z
[5] H.R.1261 - Overdraft Protection Act of 2013; https://www.congress.gov/bill/113th-congress/house-bill/1261
[6] At the time of this writing, sugar-sweetened beverage taxes have been instituted in the U.S. cities of Philadelphia PA, Berkeley CA, San Francisco CA, Oakland CA, Albany CA, Boulder CO, Portland OR, and Seattle WA.

requirements in 34 states as well as the District of Colombia[7], and helmet requirements for motorcyclists in 19 states[8].

As governments continue to intervene in choice contexts, more work is needed to understand individuals' responses to these interventions. In this paper, we describe a theory of opposition to paternalism, *psychological reactance*, and present relevant experimental evidence. We then substantiate the external validity of reactance behavior using Mayor Michael Bloomberg's 2011- 2013 campaign to restrict soda purchases in New York City as a quasi-experimental shock. Consistent with psychological reactance, we find evidence of *increased* soda consumption following proposed soda restrictions.

I. Psychological Reactance

Brehm (1966) proposes the theory of psychological reactance, which states that if a behavior is reduced or threatened with reduction, the actor will be "directed toward the re-establishment of whatever freedom has been lost or threatened". That is, the simple existence of a (real or implied) behavioral restriction can lead individuals to act in *opposition* to incentivized behavior. While Brehm postulates a number of ways that reactance may be observed, we focus on the particular manifestation most relevant for discussions of empirical economics – an increased tendency for individuals to engage in a restricted or prohibited behavior. In response to some real or proposed limit on consumption, individuals may increase their preference for the restricted good. This has significant implications for economic policy. Importantly, if true, reactance theory predicts that individuals operating in the presence of newly introduced paternalistic policies may respond to them by engaging in the very behavior that the policy has been introduced to correct. Further, if this increase in the targeted behavior is caused by an increased desire, the welfare implications become complicated to determine.

A large literature in psychology provides evidence of *reactance*, in particular evidence that restricted freedoms can lead to an increased likelihood of engaging in a restricted activity. For example, after a ban of phosphate-containing detergents in Florida, Mazis, Settle & Leslie (1973) survey middle-income households in regions both affected and unaffected by the phosphate ban. Consumers for whom phosphate containing detergents were banned expressed more positive attitudes toward these detergents. Further, consumers for whom detergents had been banned expressed more negative attitudes toward government involvement in mitigating pollution. Pennebaker and Sanders (1976) provide the seminal evidence of reactance in their experiment demonstrating that signs prohibiting graffiti on men's bathroom walls resulted in a more graffiti-laden wall. The amount of graffiti found varied positively with the severity of the threat written on the sign and with the authoritativeness of the message source.

[7] Governors Highway Safety Association. http://www.ghsa.org/state-laws/issues/Seat-Belts
[8] Insurance Institute for Highway Safety, Highway Loss Data Institute. http://www.iihs.org/iihs/topics/laws/helmetuse/mapmotorcyclehelmets

Reich and Robertson (1979) conduct a series of field experiments surrounding anti-littering messages, and conclude that direct, threatening messages prohibiting littering resulted in greater littering behavior than those appealing to social norms. Relevant to our work, Cacioppo & Petty (1979), Calder & Sternthal (1980), and Petty & Cacioppo (1986) present evidence of increased consumption of foods associated following the placement of a negative warning label. More recent examples include Vrugt (1992) who observes more negative attitudes toward female faculty members following the implementation of a policy giving women preferential treatment; Plant & Devine (2001) who present similar evidence following the implementation of affirmative action policies favoring blacks; and Allen, Sprenkel, & Vitale (1994) who find evidence of increased teen drinking in response to a non-binding increase in the legal drinking age. Kirchler (1999) surveys employers for attitudes toward taxation, tax avoidance and tax evasion and finds evidence that that employers who had been in business for a relatively short period of time expressed a greater loss of freedom due to taxation and displayed greater reactance than more experienced employers. Recent literature has focused on differentiating features of choice environments in which reactance behaviors are more likely to be observed (cf. Schade & Baum 2007; Laurin, Kay, & Fitzsimons 2012; Laurin, Kay, Proudfoot, Fitzsimons 2013).

Evidence from the psychology literature overwhelmingly supports the notion that authoritarian restrictions on behavior can induce reactance and result in greater tendencies toward the behavior that is threatened. Within the context of economics and public policy, two primary questions are of substantial interest. First, is the effect large enough that policymakers should take note?; and second, how does the existence of reactance (a change in preferences that is potentially endogenous to policy changes) affect welfare analysis and evaluation of efficient policy?

II. Soda Consumption in a Laboratory Experiment

For our empirical work, we turn to an increasingly popular paternalistic policy – restrictions placed on sugar-sweetened beverage (SSB) consumption. Given the well-established link between high-consumption of SSBs and negative health outcomes such as obesity, coronary heart disease, type 2 diabetes[12], the policymaker's paternalistic impulse is well-placed. However, psychological reactance suggests that if consumers have an emotional attachment to these beverages, some consumers may increase their preference for these beverages following anti-SSB legislative measures. If true, this response directly opposes policymakers' objectives.

During the Fall of 2013, we conducted a laboratory experiment to explore the possible existence of psychological reactance in soda consumption. We measured the effect of viewing a graphic anti-soda

[12] In a 2007 meta-analysis of existing research concerning the health effects of sugar-sweetened beverages, Vartanian, Schwartz, & Brownell review 88 articles and find robust associations between increased soft drink consumption and increased body weight and risk of medical problems such as type 2 diabetes, risk of bone fracture, dental caries, and hypocalcemia.

advertisement on the volume of soda participants consumed while completing an unrelated task. Groups of Cornell University undergraduates completed the experiment during one-hour-long periods between 10:30 am and 3:00 pm on each of four days during November 2013. We record observations for 86 participants, 56 women and 30 men. Participants visited the lab on each of two days and were observed once under both treatment and control conditions; whether the participant was assigned to the treatment condition or to the control condition for his or her first visit to the lab was randomly assigned.

When entering the lab, all participants received a sealed 20 oz. bottle of Coca-Cola and a packet of documents. Participants were directed to be seated at a desk with a privacy screen and unscrew the lid of their soda. Participants were not instructed to drink the soda. For participants in the control treatment, the packet of documents contained a set of questionnaires about their lifestyle and level of activity, none of which addressed soda or sugary drinks. Under the treatment condition, participants received a set of documents which contained the same set of surveys completed by participants in the control group, with an additional first page which featured a graphic anti-soda advertisement[13] and a set of questions to focus their attention, gauge their previous exposure to the advertisement, and to distract them from the study's purpose (see web appendix for copy of the experimental treatment). The graphic ad had been part of an anti-soda consumption campaign conducted by the New York City Department of Health and Mental Hygiene in January of 2012. Participants in the treatment setting were asked to report their agreement or disagreement using 7-point Likert scales (with 1 = Strongly Disagree to 7 = Strongly Agree) with each of the following statements: "I think this advertisement is appealing", "I think this advertisement is forced", "I think this advertisement is inappropriate"; "I think this advertisement is invasive"; and "I think this advertisement is funny". In addition, participants were asked whether they had seen the advertisement before. For both groups, after participants completed the entire packet of documents and exited the lab, we measured the quantity of soda remaining in each participants' bottle of Coca-Cola.

For each participant we collected additional control variables including age, race, gender, height, body-mass index, self-assessed hunger level, self-assessed thirst level, household income average daily soda consumption, and how much they enjoyed the soda that they received as part of the experiment.

Table 2: Balance of Covariates across Experimental Treatments

	Condition		*Significance*
	Anti-soda Advertisement (n = 86)	No Advertisement (n = 87)	F-Test (1.64) (p-value)
Volume of soda consumed	4.18 (0.45)	3.54 (0.47)	0.97 (0.33)

[13] Participants in the treatment group were shown a simplified version of a graphic advertisement placed by the New York City Department of Health and Mental Hygiene inside of New York City subway trains during January 2012 which features an image of an overweight man whose right leg has been amputated. On top of this image are superimposed three cups of cola of increasing size. The original poster reads: "Portions have grown: So has type 2 diabetes, which can lead to amputations" with text at the bottom which reads "Cut your portions, cut your risk. Call 311 for your Healthy Eating Packet". To simplify our treatment, we edited the text to read "Portions have grown: Cut your portions" with text at the bottom which read "Don't drink more than 10 oz. of soda" in large print at the bottom of the ad. The original advertisement can be accessed directly from the New York City Department of Health and Mental Hygiene at: http://www.nyc.gov/html/doh/html/pr2012/pr001-12.shtml.

Number of sodas consumed in an average week	1.5 (0.20)	1.73 (0.27)	0.32 (0.57)
"On a scale of 1-5, please indicate how much you enjoyed the soda you drank during the experiment." (1=not at all)	2.72 (0.15.34)	2.95 (0.15)	1.13 (0.29)
"On a scale of 1-5, how thirsty are you now?" (1=not very thirsty)	2.93 (0.18)	3.20 (0.19)	1.10 (0.30)
Measure of individual level reactance	2.46 (0.10)	2.50 (0.12)	0.07 (0.79)
Socio-demographics			
Age	21.42 (0.40)	21.00 (0.36)	0.61 (0.44)
Gender (percent male)	.30 (.05)	.43 (.05)	3.34 (0.07)
Height (inches)	66.46 (.36)	67.40 (.43)	2.83 (0.09)
BMI	22.49 (0.36)	22.71 (0.44)	0.15 (0.70)

Note: Standard errors given in parentheses.

The imbalance of observable characteristics, particularly gender, between the treatment and control group is evident in Table 2. Therefore we use a non-parametric minimum-distance matching estimator developed by Abadie and Imbens (2002) in order to estimate the treatment effect controlling for these imbalances (Table 3). Controlling for preference and socio-demographic variables, we find that anti-soda advertisements *increase* the level of soda consumed by participants in the lab by 2.11 ounces in a result unexplained by differences in participant enjoyment of the soda they consumed. This result is disproportionately driven by the behavior of male participants who drink 2.68 ounces of additional soda under the treatment condition (though this result is not statistically significant when parsing the data this way).

Table 3: Nearest Neighbor Matching Estimates of Effect of Viewing Anti-Soda Advertisement on Consumption of Soda Controlling for Sociodemographic Characteristics

Outcome Variable	Quantity of Soda Consumed (oz.)	Quantity of Soda Consumed (oz.) (Male Only)	Quantity of Soda Consumed (oz.) (Female Only)	Enjoyment of Soda
	(1)	(2)	(3)	(4)
Treatment Condition	2.11**	2.68	0.54	-0.063
	(0.93)	(1.93)	(0.66)	(0.29)
Demographic Controls	YES	YES	YES	YES

Note: Standard errors given in parentheses. Double asterisk (**), triple asterisk (***), and quadruple asterisk (****) denote variables significant at 5%, 1% and .1% respectively.

The advertisement treatment used in our initial experiment included a realistic image of a cup of soda. To ensure that the consumption effect we observed was not due to a pure advertising effect for soda, we replicated the initial experiment using the initial soda advertisement as the treatment condition, and an identical ad with the image of the soda removed as a control. We find no statistically significant differences in consumption following exposure to the two treatments (see Table 4).

Table 4: Effect of Viewing Initial Anti-Soda Advertisement With and Without Image of Soda

Condition		*Significance*
Ad WITH Soda	Ad WITHOUT Soda	F-Test (1.64)
(*n* = 39)		(p-value)

		(n = 43)	
Volume of soda consumed	4.81 (0.63)	5.62 (0.90)	0.54 (0.46)
Number of sodas consumed in an average week	0.91 (0.25)	1.37 (0.22)	1.97 (0.16)
"On a scale of 1-5, please indicate how much you enjoyed the soda you drank during the experiment." (1=not at all)	2.98 (0.22)	2.95 (0.22)	0.00 (0.95)
"On a scale of 1-5, how thirsty are you now?" (1=not very thirsty)	2.98 (0.17)	2.83 (0.19)	0.35 (0.56)
Measure of individual level reactance	0.25 (0.29)	-0.23 (0.26)	1.53 (0.22)
Socio-demographics			
Age	19.05 (0.16)	19.13 (0.15)	0.14 (0.71)
Gender (percent male)	0.50 (0.082)	0.45 (0.082)	0.21 (0.65)

Note: Standard errors given in parentheses.

III. Empirical Evidence of Reactance using Field Data

To explore the external validity of our experimental results, we focus on a salient example of a proposed nudge – restrictions to sugary drink purchases in New York City which were under consideration by the New York City Board of Health beginning in the Fall of 2012. An amendment to Article 81 of the New York City Code of Health restricting the sale of soda in New York City was proposed by Mayor Michael Bloomberg in 2012, unanimously approved by the New York City Board of Health in September of 2012, and ultimately defeated by the New York State Appeals Court in June of 2014. The events surrounding what was popularly referred to as a 'soda ban' were the subject of much national and local media attention. By the authors' count at least 46 articles discussing or describing the legislation were published in the New York Times alone during 2012, and 25 articles were published in 2013. Using a quasi-experimental "fuzzy" regression discontinuity design to identify changes in aggregate consumption, we examine the effects of these policy announcements, and attendant advertisement campaigns on the purchases of soda and sugary drinks in and around New York City. This estimation procedure and surrounding series of robustness checks is similar to the approach of Debnam (2017) to estimate reactance-motivated consumption shifts following the vote into law of a sugar-sweetened beverage tax in Berkeley, California. The consumption effect in which we are interested is one of exposure and absent any purely exogenous shocks (such as those available to us in our experimental treatment), our empirical strategy is subject to several limitations. Our estimation procedure is designed to provide information about shifts in consumption which co-occur with policy dates of interest, controlling for underlying time trends and household-level sociodemographic characteristics. As such, in addition to the usual endogeneity concerns associated with evaluating the impacts of advertising, we are vulnerable to the effects of unrelated co-occurring events on consumption and to the effects of longer term consumption shifts which may or may not be related to the policy of interest, but that are specific to New

York City. While we cannot completely eliminate these concerns, we address them through a series of robustness checks.

The first concern is that any consumption shifts we observe may be due to seasonal fluctuations in SSB and soda consumption. To address this concern, we include week fixed effects and for each policy date of interest, we re-estimate the main specification for a placebo date exactly one year before that date. Second, to address the concern that shifts may be driven by underlying trends in beverage consumption, we re-estimate the main specification for fruit juice – a substitute beverage which would not have been subject to the tax. Third, to address the concern that consumption shifts may have been a national, rather than a New York City-specific behavior, we re-estimate the main specification using three other major cities in the United States as placebo cities. Fourth, to address the concern that behavior may have been driven by promotional behavior on the part of retailers, we re-estimate the main specification using only purchases made by households which occurred as part of retailer promotions. Fifth, to address the possibility that media attention and messaging around the policy dates of interest resulted in a pure advertising effect for soda, we consider a good which would have been advertised in pro-soda campaigns, but which would not have been subject to any purchasing restrictions under the legislation: low-calorie soda. Finally, we address the concern that these effects may be driven by consumers' stockpiling behavior in the face of an anticipated shortage using testable predictions of consumer stockpiling models and examining consumption patterns of SSBs and soda which were single-use purchases.

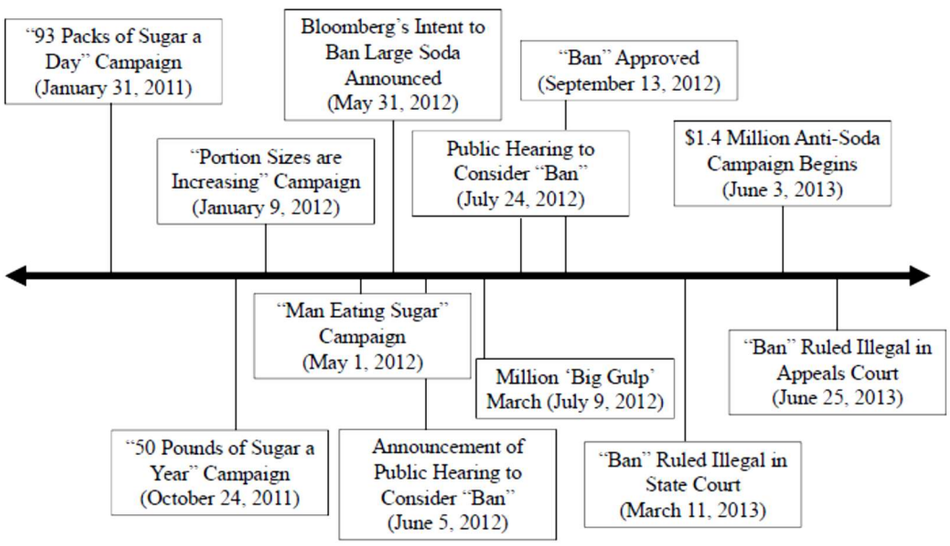
We examine consumption patterns surrounding each key date shown in Figure 1. For the purposes of exposition, we focus here on a few key dates (estimations concerning all dates are available in the online appendix). The proposed restrictions would have applied to retailers under the purview of the New York City Department of Health (DoH). – Local food service establishments and restaurants would have been subject to the restrictions, while national restaurant chains and food retailers operating in New York City would not have been subject to these restrictions.[14] However, there is evidence to suggest that the complex nature and scope of the proposed restrictions were not widely understood. The restrictions were extremely dense in nature, and would have banned the sale of non-diet soda and drinks with sugar added which were greater than 16 ounces, less than 50% alcohol and contained no milk. Diet soda and fruit juice purchases of any size would have remained permissible under the legislation. For example, one well-publicized protest to the proposed legislation – the *Million "Big Gulp" March* – held in City Hall Park on July 9, 2012, referred to a beverage which would not have been subject to the proposed legislation at all.[15] Given the level of confusion and ambiguity surrounding the legislation, we analyze

[14] §81.01 of the New York City Health Code. Food commissaries that are regulated under the State Agriculture and Markets Law are not subject to the provisions of Article 81.

[15] One prominent example of this is the slogan Million 'Big Gulp' March, used by protesters who organized opposition to the ban in City Hall Park on July 9, 2012. Seven-11, the retailer who sells the 'Big Gulp', a 64 oz. sugary beverage, would have in fact been exempt from all regulation by the New York City Department of Health since as a national retailer, it falls under the regulation of the New York State Department of Health. http://www.cbsnews.com/news/million-big-gulp-march-to-protest-proposed-nyc-soda-ban/

changes in the aggregate purchase of sugary beverages in and around New York City both for beverage purchases which would have been prohibited under the proposed amendments to Article 81 and those beverage purchases which would have remained permissible.

Figure 1: Timeline of Relevant Advertising Campaigns and Policy Announcements in and Around New York City between January 31, 2011 and June 25, 2013



Our empirical analyses are conducted using longitudinal purchase data from the Nielsen Homescan Household Panel accessed through the Kilts Center for Marketing at the University of Chicago. In each year of the panel, this dataset records all "household purchases" made by each of roughly 60,000 U.S. households, along with the household-level demographic and socioeconomic characteristics of these households. The panel is unbalanced with about 80% of households being retained in the panel year-over-year. For each purchase, we observe the UPC of the item purchased, product characteristics, whether the item was purchased as part of a store promotion, when and where the purchase was made, and a measure of price[16]. The composition of the Homescan Consumer Panel sample is balanced to the U.S. population along household demographic categories (i.e. size, income, age of head, education of head, occupation of head, presence of children, race, and Hispanic origin). For our primary estimations we use purchasing data from 2010 through 2014. For tractability, we draw a random sample of 20% of the households in each year of the dataset, retaining all households living near New York City for a sample of over 7.11 million purchases made across 1.90 million household purchasing-weeks. The characteristics of these households are summarized in Table 5. With an average household income of $63,494.58 our resulting sample is wealthier and better educated than that of the United States. On average, our households purchase 3.73 household items per week, conditional on making any observed household purchases. While we have no *a priori* reason to believe that purchasing-weeks in our sample should be distributed proportionally along sociodemographic groups, the composition of weekly-purchasers in our sample appears roughly equivalent to the composition of households within the sample at least along the

[16] Unless the price is manually entered by the respondent, prices are imputed by Nielsen based on the average price paid for the item at the retailer during the week in which the purchase was made.

demographic categories we observe. Household summary statistics weighted by the number of purchasing weeks for which we observe these households can be found in the online appendix.

Table 5: Summary Statistics of Sampled Households (2010 – 2014 Sample)

Household Summary Statistics ($n_{households}$ = *21,390)*)	
Household Size	2.49 (.01)
Income[17]	$63,494.58 (286.90)
No Male Household Head	5,375 (25.13%)
Male Household Head Less than High School	786 (3.67%)
Male Household Head High School	3,969 (18.56%)
Male Household Head Some College	4,704 (21.99%)
Male Household Head College	4,638 (21.68%)
Male Household Head Graduate School	1,918 (8.97%)
No Female Household Head	2,043 (9.55%)
Female Household Head Less than High School	477 (2.23%)
Female Household Head High School	4,388 (20.51%)
Female Household Head Some College	6,137 (28.69%)
Female Household Head College	6,119 (28.61%)
Female Household Head Graduate School	2,226 (10.41%)
Married	13,283 (62.10%)
Widowed	1,350 (6.31%)
Divorced/Separated	3,323 (15.54%)
Single	3,434 (16.05%)
White/Caucasian	17,223 (80.52%)
Black/African American	2,363 (11.05%)
Asian	670 (3.13%)
Other	1,134 (5.30%)
New York City	1,370 households
New York Suburbs	260 households
Other locations	19,760 households

To control for unobserved household-level heterogeneity, we estimate the following fixed-effects regression at the household-week level, clustering standard errors at the household level:

$$y_{ht} = \alpha + \gamma_1 D_{Event} + \gamma_2 Time + \gamma_3 (Time \cdot D_{Event}) + \gamma_4 (D_{NYC} \cdot D_{Event}) + \gamma_5 Week + \varepsilon_{ht} \quad (1)$$

Where dependent variable of interest, y_{ht}, is either ounces of sugar-sweetened beverages (SSBs) (drinks that contain added sugar) or soda (carbonated SSBs) consumed in week *t* by household h. We include week fixed effects to control for seasonal fluctuations in soda consumption[18]. For geographic residence dummies, a household is defined as living in New York City ($D_{NYC} = 1$) if the Federal Information Processing Standard (FIPS) state and county codes associated with its address identifies any of the counties in New York City (New York County, Kings County, Richmond County, Bronx County, Queens County). We estimate equation (1) separately for each policy date relevant to the proposed soda restriction:

[17] Nielsen records household income in broad income categories. We recode these as continuous variables by defining each household's income as the median of the income range which they report, save for the lower ($0 - $5,000) and upper bound ($100,000 +) categories which we recode as $5,000 and $150,000 respectively.

[18] In alternative specifications, we have controlled for seasonality using month fixed effects or spectral analysis variables. The main findings are robust to these alternative approaches.

1. The launch of the DoH's television and NYC subway poster advertising campaign touting how one day's worth of sugary drinks is worth 93 packs of sugar ("93 Packs Campaign");
2. The launch of the DoH's television and NYC subway poster campaign stating how one soda a day is worth 50 pounds of sugar a year ("50 lbs. Ad.");
3. The launch of the DoH's graphic NYC subway campaign linking increasing soda portions to negative health outcomes ("Increasing Portions Ad.");
4. The Centers for Disease Control and Prevention begins nationally airing a television advertisement initially produced by the DoH which shows a man eating packets of sugar in a diner ("Man Eating Sugar Ad.");
5. Deputy Mayor Howard Wolfson's statement to the New York Times of the DoH's intent to implement a policy banning sodas of a large size ("Soda 'Ban' Announced");
6. The date the DoH announced a public hearing to provide comments on The Amendment ("Hearing Announcement");
7. The date the DoH's intent to amend Article 81 of the New York City Code of Health was published in the City Record ("City Record");
8. The date of a large public hearing to comment on The Amendment ("Public Hearing");
9. The date of the 'Million 'Big Gulp'' march in City Hall Park to protest The Amendment ("Million March");
10. The passage of The Amendment by the New York City Board of Health ("Ban Passed by DoH");
11. The date which The Amendment was scheduled to take effect ("Start Date");
12. The initial overturn of The Amendment by a New York State Judge on the grounds that it was 'arbitrary and capricious'[19] ("First Block");
13. The launch of a $1.4 million anti-sugary drink television and NYC bus campaign by DoH highlighting the added sugar in sports drinks, sweet teas, and energy drinks ("Sugary Drinks Ad."); and
14. The ruling by the Appeals Court that The Amendment is unconstitutional and that the Board of Health exceeded its legal authority by its passage ("Final Block").

Our coefficient of interest is γ_4, the change in consumption associated with being a resident of New York City immediately following the policy event of interest. We are interested in observing changes in soda and sugary drink consumption in New York City associated with well-publicized anti-sugary drink advertising campaigns and policy events relating to Mayor Bloomberg's proposed "soda ban" – dates which reminded consumers that their soda choice sets may be limited in the future. We

[19] Jaslow, R. (2013, March 11) Bloomberg "confident" soda ban will be upheld. *CBS News.* Retrieved from http://www.cbsnews.com/news/bloomberg-confident-soda-ban-will-be-upheld/

hypothesize that these reminders may have led to increased soda and sugary drink consumption as individuals exhibited *reactance* responses.

For parsimony, we show throughout estimation tables here for the dates which received the most popular attention (according to Google Trends[20]): the start of the New York City Department of Health's advertisement effort with a campaign emphasizing how a day's worth of sugary drinks is worth 93 packs of sugar ("93 Packs Campaign"), the date on which Mayor Bloomberg formally announced the plan to Amend Article 81 of the New York City Health Code restricting purchases of sugary beverages ("'Soda Ban' Announced"), and the date which the proposed amendment was approved by the New York City Board of Health ("Ban Passed by DoH"). Estimations results for all other policy dates can be found in the online appendix.

IV. Main Empirical Results

Consistent with reactance, we find that following the initial announcement of Mayor Bloomberg's intent to place restrictions on the quantity of SSBs purchased in New York City retailers, resident households *increased* their weekly consumption of soda by 8.78 ounces on average. Further, we find that after the successful passage of the amendment to Article 81 limiting SSB purchases, households increased their consumption of soda by 11.94 ounces. These results are summarized in Table 6. We also find statistically significant evidence that, consistent with policymaker goals, following the Department of Health's "93 packs" advertising campaign which called attention to the health consequences of over-consuming SSBs, the average New York City household decreased their weekly soda consumption by 8.50 ounces. This latter result however, is not robust to the investigations into seasonality which follow.

Table 6: Estimation of Equation (1) for All Soda Purchases (oz.)

	"93 Packs" Campaign	Soda "Ban" Announced	"Ban" Passed by DoH
After Event	29.02****	41.79****	32.43****
	(2.584)	(4.009)	(4.359)
Time Trend X After Event	-0.355****	-0.352****	-0.314****
	(0.0477)	(0.0293)	(0.0289)
NYC X After Event	-8.498**	8.777**	11.94***
	(4.009)	(4.083)	(4.077)
Constant	241.2****	242.7****	242.9****
	(3.192)	(3.022)	(3.016)
Time Trend/Seasonal Controls	YES/YES	YES/YES	YES/YES
Observations	1902952	1902952	1902952

Note: Standard errors given in parentheses. Double asterisk (**), triple asterisk (***), and quadruple asterisk (****) denote variables significant at 5%, 1% and .1% respectively.

[20] Google Trends data for search terms "soda tax" and "soda ban" among searches conducted in the New York City area. Accessed at www.google.com/trends.

When we estimate changes in the consumption for the broader category of SSBs (which includes purchases of other non-soda SSBs) following the dates of the 'ban's' announcement and passage, we also find evidence consistent with reactance (shown in Table 7). We find statistically significant evidence of *increased* sugar-sweetened beverage consumption by NYC-resident households following the 'ban's' passage (of 6.62 oz.) but not following its announcement.

Table 7: Estimation of Equation (1) for Sugar-Sweetened Beverage Purchases (oz.)

	"93 Packs" Campaign	Soda "Ban" Announced	"Ban" Passed by DoH
After Event	17.83****	25.64****	20.01****
	(1.459)	(2.261)	(2.438)
Time Trend X After Event	-0.201****	-0.219****	-0.194****
	(0.0269)	(0.0166)	(0.0163)
NYC X After Event	-5.584**	4.862	6.623***
	(2.599)	(2.492)	(2.447)
Constant	140.2****	140.5****	140.8****
	(1.787)	(1.688)	(1.685)
Time Trend/Seasonal Controls	YES/YES	YES/YES	YES/YES
Observations	1902952	1902952	1902952

Note: Standard errors given in parentheses. Double asterisk (**), triple asterisk (***), and quadruple asterisk (****) denote variables significant at 5%, 1% and .1% respectively.

VI.I. Robustness: Seasonality?

A first concern is one of seasonality, and that routine seasonal fluctuations rather than consumer responses to policy events, are driving our main results. Therefore, in addition to the inclusion of weekly fixed effects[21] in our main specification, we re-estimate our household fixed effects regression for both soda and SSBs using a placebo date exactly one year before the date of interest in place of each policy date. These results are summarized in online appendix Tables P1 and P2. We not do find statistically significant evidence of consumption shifts associated with either the placebo date one year in advance of the 'ban's' announcement or the placebo date one year in advance of the 'ban's' passage. We do however find evidence of a large shift in consumption associated with the date of the "93 packs" advertising campaign, suggesting that seasonality may play a role in this result.

VI.II. Robustness: Trends in Beverage Consumption?

Another possibility is that the shifts in consumption we observe reflect general changes in beverage consumption in New York City. We therefore re-estimate the main specification for a substitute good which would not have been subject to restriction under the legislation – no-sugar-added fruit juice. We do not observe corresponding shifts in consumption of juice associated with the policy events of

[21] For robustness, we re-estimate all results using month fixed effects and spectral analysis variables in place of week fixed effects. All results hold and point estimates remain largely unchanged.

interest (as shown in Table 8); this suggests that the main results are not being driven by general beverage consumption trends.

Table 8: Estimation of Equation (1) for Juice (oz.)

	"93 Packs" Campaign	Soda "Ban" Announced	"Ban" Passed by DoH
After Event	5.969****	9.823****	9.760****
	(0.700)	(1.051)	(1.161)
Time Trend X After Event	-0.0873****	-0.0760****	-0.0739****
	(0.0135)	(0.00775)	(0.00762)
NYC X After Event	-0.460	-2.945	-2.277
	(1.875)	(1.658)	(1.596)
Constant	66.43****	67.08****	67.19****
	(0.825)	(0.777)	(0.773)
Time Trend/Seasonal Controls	YES/YES	YES/YES	YES/YES
Observations	1902952	1902952	1902952

Note: Standard errors given in parentheses. Double asterisk (**), triple asterisk (***), and quadruple asterisk (****) denote variables significant at 5%, 1% and .1% respectively.

VI.III. Robustness: A General Trend?

To confirm that observed effects are specific to residents of New York City metro area, the population for which the soda 'ban' represented a legitimate threat, we estimate equation one using the three of the largest cities in the United States – Chicago, and Los Angeles, and Houston to generate household-level geographic dummies (coefficients are summarized in Tables 9 & 10). Since households living in these cities faced no proposed restriction on their SSB purchases during the policy dates of interest, we should not expect to see reactance-driven increases in consumption among these households. Consistent with our hypothesis, for household purchases made by residents in these regions, we do not find evidence of statistically significant increases in consumption of soda or sugar-sweetened beverages associated with any of the policy dates under consideration.

Table 9: Estimation of Equation (1) for All Soda Purchases in Placebo Cities (oz.)

	"93 Packs" Campaign	Soda "Ban" Announced	"Ban" Passed by DoH
Los Angeles X Policy	-4.307	-3.496	-1.941
	(7.060)	(7.774)	(8.347)
Chicago X Policy	-0.201	-7.601	-11.13
	(10.72)	(11.14)	(11.91)
Houston X Policy	-8.907	0.687	-1.695
	(12.25)	(12.85)	(12.82)

Note: This table reports coefficient estimates from estimating equation one with dummies for the residents of the cities Los Angeles, Chicago, and Houston in place of the dummies indicating the residents of New York City. These estimates therefore estimate the impact of living in one of these major cities following the policy dates indicated by each column. Standard errors given in parentheses. Double asterisk (**), triple asterisk (***), and quadruple asterisk (****) denote variables significant at 5%, 1% and .1% respectively.

Table 10: Estimation of Equation (1) for Sugar-Sweetened Beverages in Placebo Cities (oz.)

	"93 Packs" Campaign	Soda "Ban" Announced	"Ban" Passed by DoH
Los Angeles X Policy	-0.939	-1.236	-0.567
	(4.170)	(4.913)	(5.159)
Chicago X Policy	-0.161	-2.203	-3.400
	(5.699)	(5.977)	(6.291)
Houston X Policy	-4.005	-2.236	-3.900
	(6.594)	(6.919)	(6.984)

Note: This table reports coefficient estimates from estimating equation one with dummies for the residents of the cities Los Angeles, Chicago, and Houston in place of the dummies indicating the residents of New York City. These estimates therefore estimate the impact of living in one of these major cities following the policy dates indicated by each column. Standard errors given in parentheses. Double asterisk (**), triple asterisk (***), and quadruple asterisk (****) denote variables significant at 5%, 1% and .1% respectively.

VI.IV. Robustness: A Promotional Effect?

If retailers responded to publicity surrounding the soda 'ban' by holding sales promotions, the empirical results we attribute to reactance could instead be driven by increased soda demand resulting from lower prices. To address this concern, we estimate equation one for soda and SSB purchases which occurred as part of a retailer promotion, where a promotion is defined as a limited time price decrease (Tables 11 & 12).

Table 11: Estimation of Equation (1) for Soda Purchases Which Occurred as Part of a Store Promotion (oz.)

	"93 Packs" Campaign	Soda "Ban" Announced	"Ban" Passed by DoH
After Event	2.251****	3.416****	2.762***
	(0.536)	(0.816)	(0.894)
Time Trend X After Event	-0.0411****	-0.0241****	-0.0238****
	(0.00992)	(0.00598)	(0.00592)
NYC X After Event	-0.679	1.206	1.415
	(0.773)	(0.843)	(0.847)
Constant	26.23****	26.90****	26.78****
	(0.636)	(0.596)	(0.594)
Time Trend/Seasonal Controls	YES/YES	YES/YES	YES/YES
Observations	1902952	1902952	1902952

Note: Standard errors given in parentheses. Double asterisk (**), triple asterisk (***), and quadruple asterisk (****) denote variables significant at 5%, 1% and .1% respectively.

Table 12: Estimation of Equation (1) for Sugar-Sweetened Beverage Purchases Which Occurred as Part of a Store Promotion (oz.)

	"93 Packs" Campaign	Soda "Ban" Announced	"Ban" Passed by DoH
After Event	3.854****	4.621****	3.721****
	(0.597)	(0.908)	(0.982)

Time Trend X After Event	-0.0525****	-0.0391****	-0.0353****
	(0.0109)	(0.00668)	(0.00656)
NYC X After Event	-1.057	1.402	1.631
	(0.976)	(0.983)	(0.987)
Constant	30.03****	30.59****	30.63****
	(0.695)	(0.654)	(0.651)
Time Trend/Seasonal Controls	YES/YES	YES/YES	YES/YES
Observations	1902952	1902952	1902952

Note: Standard errors given in parentheses. Double asterisk (**), triple asterisk (***), and quadruple asterisk (****) denote variables significant at 5%, 1% and .1% respectively.

Despite sufficient statistical power, we find no evidence of increased soda purchases which occurred as part of a store promotion following any of our key policy dates. Similarly, when we estimate our main specification for all SSB purchases which occurred as part of a store promotion we again find no evidence of increased promotional purchases on any of our dates of interest. This suggests that consumer responses to store promotions do not explain the increases in consumption we observe following the announcement of the soda 'ban' and following the 'ban's' passage.

VI.V. Robustness: A Priming Effect?

An alternative hypothesis is that increased media attention and advertising containing images of soda and SSBs during the dates in the policy window resulted in a pure advertising effect, leading to increased consumption. As we cannot directly observe changes in advertising, we cannot directly estimate the behavioral significance of these priming effects. Instead, we identify a consumption category – low-calorie soda – which should also be subject to these priming effects, but should not be subject to reactance, and investigate related consumption shifts. As a sub-category of soda, this product would have been featured in soda advertising surrounding the 'ban', but it was unthreatened by the 'ban' and posited as a "healthy" alternative to full-calorie sodas. As it was unthreatened, we should not expect reactance to play a role in the purchase of low-calorie soda. We estimate equation one for low-calorie soda purchases (Table 13) and find no evidence that advertising effects play a role.

Table 13: Estimation of Equation (1) for Low-Calorie Soda Purchases (oz.)

	"93 Packs" Campaign	Soda "Ban" Announced	"Ban" Passed by DoH
After Event	7.051****	12.78****	11.72****
	(0.874)	(1.345)	(1.458)
Time Trend X After Event	-0.0986****	-0.0960****	-0.0940****
	(0.0151)	(0.00994)	(0.00980)
NYC X After Event	-1.667	2.197	3.192**
	(1.240)	(1.519)	(1.552)
Constant	46.09****	46.70****	46.60****
	(1.022)	(0.972)	(0.965)

Time Trend/Seasonal Controls	YES/YES	YES/YES	YES/YES
Observations	1902952	1902952	1902952

Note: Standard errors given in parentheses. Double asterisk (**), triple asterisk (***), and quadruple asterisk (****) denote variables significant at 5%, 1% and .1% respectively.

VI.VI. Robustness: Intertemporal Substitution?

A rational response in the face of an impending consumption restriction is to store goods for future consumption. This rational behavior is a plausible explanation to the results we observe. Since we cannot observe consumer inventories, we use two approaches to gain insight into possible consumer storage behavior. First, we re-estimate equation one for soda and sugar-sweetened beverage purchases which are by definition, unlikely to be stored. We define such purchases as the purchase of a single soda or SSB in a quantity less than or equal to 16 ounces. These purchases are likely to be consumed before panelists report their purchases to Nielsen and are therefore less likely to be reported in Nielsen Homescan Data than bulk purchases (Einav, Leibtag, & Nevo, 2010). However, we do observe some purchases that meet this criteria in the data. Among these purchases, we do not find evidence of reactance-consistent increased consumption surrounding the policy dates of interest (Tables 14 & 15). Therefore, we cannot reject a role for intertemporal substitution.

Table 14: Estimation of Equation (1) for Single-Use Soda Purchases (oz.)

	"93 Packs" Campaign	Soda "Ban" Announced	"Ban" Passed by DoH
After Event	0.0980***	0.446****	0.541****
	(0.0353)	(0.0683)	(0.0776)
Time Trend X After Event	0.0000113	-0.00277****	-0.00327****
	(0.000583)	(0.000467)	(0.000486)
NYC X After Event	-0.0728	0.0504	0.0752
	(0.0596)	(0.0725)	(0.0775)
Constant	0.417****	0.365****	0.356****
	(0.0311)	(0.0307)	(0.0306)
Time Trend/Seasonal Controls	YES/YES	YES/YES	YES/YES
Observations	1902952	1902952	1902952

Note: Standard errors given in parentheses. Double asterisk (**), triple asterisk (***), and quadruple asterisk (****) denote variables significant at 5%, 1% and .1% respectively.

Table 15: Estimation of Equation (1) for Single-Use Sugar-Sweetened Beverage Purchases (oz.)

	"93 Packs" Campaign	Soda "Ban" Announced	"Ban" Passed by DoH
After Event	0.153****	0.550****	0.632****
	(0.0402)	(0.0734)	(0.0823)
Time Trend X After Event	-0.000860	-0.00345****	-0.00396****
	(0.000692)	(0.000513)	(0.000525)
NYC X After Event	-0.0852	-0.0211	-0.00476
	(0.0696)	(0.0847)	(0.0863)

Constant	0.653**** (0.0356)	0.613**** (0.0350)	0.600**** (0.0349)
Time Trend/Seasonal Controls	YES/YES	YES/YES	YES/YES
Observations	1902952	1902952	1902952

Note: Standard errors given in parentheses. Double asterisk (**), triple asterisk (***), and quadruple asterisk (****) denote variables significant at 5%, 1% and .1% respectively.

Our second approach is to appeal to the testable predictions of consumer inventory models. These models have been developed to examine dynamic consumption in the presence of real and anticipated price changes; we consider a purchasing restriction as an implicit price increase. Originating with the work of Arrow, Harris & Marschak (1951), consumer inventory models have been used to investigate consumer storage behavior including in the Nielsen datasets (Hendel & Nevo 2006). These models explore consumer behavior under normal circumstances and under anticipation of a price change to make inferences about stockpiling behavior. In our case, purchasing restrictions never materialized because the anti-SSB legislation was ultimately overturned. Therefore, we examine changes in consumption before the 'ban's' legislative defeat – if consumers stockpiled beverages in anticipation of a price increase, then once it was clear that prices would not increase consumers would no longer have needed to stockpile. In addition, if consumers were stockpiling purchases during the window of time under which Mayor Bloomberg's soda restriction was under consideration, during this time they would have made either more frequent purchasing trips, purchased a greater volume of SSBs during these purchasing trips, or both. Therefore, in Table 16 we show the average volume of soda purchased by households living in New York City on an average trip, on a trip during the policy window (the time between the Mayor's plan was announced and the ban's legislative defeat), and on the first trip following the legislative defeat of the 'ban'. To provide a baseline, we also include the volume of soda purchased as part of store promotions.

Table 16: Average Volume of Soda and Sugar-Sweetened Beverages Purchased by Households Living in New York City Per Trip in Which Soda Was Purchased

	Soda				Sugar-Sweetened Beverages			
	Any Purchase	Sale Purchase	Purchases During Policy Window	First Purchase Following Legislative Defeat	Any Purchase	Sale Purchase	Purchases During Policy Window	First Purchase Following Legislative Defeat
Quantity of Purchase (oz.)	70.25 (1.819)	73.76 (1.738)	73.74 (6.333)	72.03 (5.580)	87.56 (2.807)	79.25 (2.096)	83.70 (8.456)	79.20 (7.394)

For both soda and SSBs we do not find either statistically significant evidence that consumers purchased more soda per trip during the policy window. While point estimates suggest a slight drop in the volume of soda and SSBs which were purchased following the legislative defeat, this difference is also not

statistically significant. To characterize purchasing behavior during the policy window more fully, we also show the average number of days which elapse between soda and SSB purchases during the policy window, the average trip, and the time which elapses following promotional purchases of soda (Table 17). We do find evidence that during the time at which the ban was under consideration, households made more frequent purchases of soda than usual, but this is not the case for sugar-sweetened beverages. There is therefore some evidence to suggest storage behavior for soda, but not for SSB purchases during the policy window.

Table 17: Average Number of Days Which Elapse Between Soda and Sugar-Sweetened Beverages Purchases by Households Living in New York City

	Soda			Sugar-Sweetened Beverages		
	Any Purchase	Sale Purchase	Purchases During Policy Window	Any Purchase	Sale Purchase	Purchases During Policy Window
Number of Days Between Purchases	76.36 (4.315)	53.61 (3.299)	66.18 (3.650)	58.03 (3.069)	40.25 (2.103)	54.30 (2.913)

Consistent with consumption responses driven by reactance, we observe positive statistically significant changes in consumption of potentially restricted beverages such as soda and sugar-sweetened beverages associated with key policy dates in which the soda 'ban' was legislatively advancing in New York City. None of these changes can be explained by purchases which occurred as part of store or manufacturer promotions.

VII. Discussion and Conclusion

We present a general model of consumer responses to paternalistic consumption regulations, grounded in *psychological reactance*. Consistent with the model's predictions, we find evidence of *increased* SSB consumption following exposure to anti-SSB advertising in the lab and following anti-SSB policy events in New York City. Findings from our laboratory experiment show that simply viewing an anti-soda advertisement can increase the volume of laboratory participants' soda consumption by 2.11 ounces. In New York City, we find evidence of increased household consumption following the announcement of the soda 'ban' of 8.78 ounces of soda (where an average household contains 2.49 members). Following the passage of the legislation by the New York City Department of Health, we find that households living in New York City increased their consumption of soda by 11.94 ounces and their consumption of SSBs by 6.62 ounces. Our consumption results cannot be explained by seasonal, national, or beverage consumption trends, nor can they be attributed to promotional behavior on the part of retailers. In the case of soda consumption, however, we cannot reject a role for intertemporal substitution in partially

explaining the increase we observe. In this setting, we find evidence that individuals respond by increasing their consumption of soda in direct response to the proposal of a paternalistic policy with exactly the opposite behavior of that intended by policymakers. These results are consistent with those of Debnam (2017), who finds evidence of reactance-driven consumption in Berkeley, California following the successful passage of a local sugar-sweetened beverage tax.

Our results suggest that the nature of paternalistic policies, the way in which an individual's choice set is restructured or reframed, matters for the efficacy of an intervention. Outstanding questions for future research include how long induced changes in consumption might last, changes in aggregate welfare associated with reactance responses, and whether this behavioral motivation generalizes to contexts outside of the United States.

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