Advertising Bans and the Substitutability of Online and Offline Advertising

Avi Goldfarb and Catherine Tucker*

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Abstract

We examine whether the growth of the internet has reduced the effectiveness of government regulation of advertising. Specifically, we combine variation in state and local regulation of offline alcohol advertising with data from field tests that randomized exposure to online advertising for 275 different online advertising campaigns to 61,580 individuals. People are 8 percent less likely to say that they will purchase an alcoholic beverage in states that have alcohol advertising bans compared to states that do not. For consumers exposed to online advertising, this gap narrows to 3 percent. We also show similar effects for four changes in local offline alcohol advertising restrictions where we observe advertising effectiveness both before and after the change. Our results suggest that online advertising could reduce the effectiveness of attempts to regulate other advertising channels because online advertising substitutes for (rather than complements) offline advertising. Our results also suggest an informative role for online advertising in places with bans: The effect of online advertising is disproportionately high for new products and for products with low awareness in places that have bans.

Keywords: Advertising, Regulation, Advertising Media Mix, Internet

*Avi Goldfarb is Associate Professor of Marketing, Rotman School of Management, University of Toronto, 105 St George St., Toronto, ON. Tel. 416-946-8604. Email: agoldfarb@rotman.utoronto.ca. Catherine Tucker is Assistant Professor of Marketing, MIT Sloan School of Business, 1 Amherst St., E40-167, Cambridge, MA. Tel. 617-252-1499. Email: cetucker@mit.edu. We thank WPP/Google Marketing Research Awards for providing financial support for this project.

1 Introduction

Advertising regulation is widespread. There are full or partial bans on marketing communications in many industries, including alcohol, pharmaceuticals, video games, gambling, tobacco, and legal services. The existence and severity of these restrictions varies by country, by state, and by city. Such regulations often date from a time when local restrictions on advertising could be expected to have a binding effect on the types of advertising that the local population encountered. Today, widespread use of the internet means that a firm's potential customers may encounter advertising content online that their local government has tried to restrict.

This paper examines how the effectiveness of online ad campaigns for alcoholic beverages changes when there are restrictions on offline advertising. By examining differences in advertising offline exposure that are not due to firm-level advertising decisions, we are also able to shed light on whether online advertising substitutes for or complements offline ads. To measure the effectiveness of the online ad campaigns, we use data from a large-scale set of field experiments that randomized exposure to online display advertisements of alcoholic beverages. These field experiments asked 61,580 US web users about purchase intent for alcoholic products, covering 275 campaigns for different products on different websites from 2001 to 2008. For each of these campaigns, a mean of 223 people completed the survey, of whom roughly half were randomly exposed to an ad for an alcoholic beverage, and the others were exposed to an alternative placebo advertisement.

We contrast how randomized exposure to online advertising affects purchase intent in locations that place restrictions on out-of-home advertising with locations that do not. We do this in two ways. First, we compare the 17 states that regulate out-of-home advertising such as billboards, storefront signage, and transit ads with the 33 states that do not regulate such advertising for alcohol. Table 1 provides descriptive statistics that foreshadow our core findings. Among those who did not see the online ad campaign, the percentage of respondents 'very likely' or 'likely' to purchase is 2.04 percentage points lower, or relatively 8% lower in states with ad bans than in states with no ad ban. The relative difference in purchase intent between states with an ad ban and states without an ad ban narrows to 3% after advertising exposure. Given that ad exposure was randomized, this suggests that online advertising reduces the effectiveness of offline advertising bans. In the body of the paper, we conduct a wide battery of tests to show that the pattern in Table 1 holds with thorough econometric analysis. We check that there were no systematic differences in the kind of ads that people saw and the people who saw the ads across states with and without offline ad bans. We show that the results are robust when allowing the effect of exposure to advertising to vary with a variety of state characteristics such as alcohol consumption, alcohol abuse, and alternative state regulations that restrict the sale, consumption and marketing of alcohol. We also show that there is no similar effect for other CPG categories.

Table 1: Percentages of respondents who say they are likely to purchase in states with and without out-of-home advertising bans

Out-of-home	Exposed	
Ad Ban	No	Yes
No	28.88	31.01
Yes	26.84	30.18

The analysis of state-level regulations provides a large-sample understanding of differences between places with regulation and places without them. However, there still may be systematic differences in how people in states with ad bans respond to alcohol advertising that are not related directly to the ban. Therefore, as a second empirical approach, we examine changes in four local regulations that provide a natural experiment on advertising regulation because they were either enacted or rescinded during the period for which we have data. These local regulations are: A 2003 ban on out-of-home alcohol advertising in Philadelphia; a 2004 elimination of a ban on college newspaper alcohol advertising in Pennsylvania; a 2007 increase in enforcement of an alcohol advertising ban on San Francisco public transit; and a 2007 withdrawal (in New York City) of a self-imposed ban on hard liquor advertisements on broadcast television.

The changes in local regulations complement the state-level results by relying on the weaker identification assumption that the changes in the effectiveness of online alcohol ads within a given location are due to changes in offline advertising rather than location-specific changes over time in online alcohol advertising responsiveness. This assumption is particularly plausible because one Pennsylvania regulatory change increased offline advertising regulation and the other decreased regulation, but both show that more regulation of offline advertising translates to more effective online advertising. Our estimates of the effect of offline advertising bans are reasonably similar, despite the much smaller sample sizes and specificity of local regulatory changes.

Crucially for firms, we also show that the disproportionate effect of online advertising in places with advertising bans is related to levels of product awareness. Online advertising for products that have low levels of awareness is particularly effective in places with out-of-home advertising bans. By contrast, for products that have high levels of awareness, there is little difference in online advertising effectiveness between places with and without bans. This suggests that online display advertising plays an informative role for people in places with out-of-home advertising bans. It also provides some support for the results being driven by diminishing marginal effectiveness of advertising goodwill, since high-awareness products are already at the point of steep diminishing returns (Nerlove and Arrow, 1962; Dube et al., 2005; Dube and Manchanda, 2005; Hitsch, 2006).

Also important for firms is the implication of our research that internet advertising substitutes for offline advertising. Indeed, our results provide evidence on an unusual but useful natural experiment which allows us to identify how offline advertising relative to online advertising affects purchase intent. This is valuable because usually it is hard to separate out the effects of offline and online advertising campaigns, given that online and offline campaigns are launched at the same time. As emphasized by Silk et al. (2001), "[the internet] looms as a potential substitute or complement for all of the major categories of existing media and appears capable of serving a wide range of communications objectives for a broad array of advertisers." This matters because it is not clear whether the unique capacity of the internet to target and interact with users means that it extends and enhances existing external advertising campaigns or acts as a substitute for them.

Our finding that online display advertising is a substitute for offline display (primarily billboard) advertising contributes to a growing literature in marketing that explores the relationship between offline and online environments for customer acquisition (Bell and Choi, 2009), brands (Danaher et al., 2003), word of mouth (Bell and Song, 2007; Forman et al., 2008), purchases (Forman et al., 2009; Brynjolfsson et al., 2009), customized promotions (Zhang and Wedel, 2009), ad pricing (Goldfarb and Tucker, 2009), search behavior (Lambert and Pregibon, 2008) and price sensitivity (Chu et al., 2008). We believe ours is the first study to empirically investigate consumer substitution between online and offline advertising.

Our results more generally provide an expanded framework in which to understand the effectiveness of online advertising. Most of this literature, including Manchanda et al. (2006) and Chatterjee et al. (2003), has focused on measuring the effect of ad exposure and clicks. We contribute to this literature by emphasizing that the effectiveness of online advertising cannot be considered independently from the availability and feasibility of offline media.

Previous research has discussed the difficulties of tailoring local regulations to the internet era in the areas of gambling (Clarke and Dempsey, 2001), tobacco (Cohen et al., 2001), and prescription drugs (Fox and Ward, 2005). However, there has been little systematic empirical work on the internet's influence on the effectiveness of existing local regulation outside of tax policy (Ellison and Ellison, 2009; Goolsbee, 2000; Goolsbee et al., 2010).

This dearth of empirical research extends to marketing regulations. Our results suggest that advertising restrictions are less effective when locals are able to access the internet. Prior research has used aggregate non-experimental data to show that partial advertising bans have negligible effects on total alcohol consumption - arguing that substitution by advertisers between advertising channels plays a role (Nelson, 2003; Young, 1993).[1] Frank (2008) also suggested there may be substitution by advertisers between print, television, and radio advertising channels. Our research provides evidence of a related mechanism that renders ad bans less effective - when one channel is blocked, the alternative channels become *more* effective. In the case of an out-of-home advertising ban, there may be little effect on overall customer consumption, because the ban makes advertising in the non-regulated media outlets more effective. With the advent of the internet, websites whose servers lie outside the ban's jurisdictional boundary provide a persistent alternative advertising outlet, and it therefore seems likely this mechanism for rendering partial advertising bans ineffective will persist.

Our findings suggest that the internet reduces the ability of local authorities to restrict the effect that advertising has on the local population. While the type of substitution that we document between different types of media has been possible previously, never before has there been a media channel that is so pervasive and that has such an ability to reach a local population when outside its political borders. Castells (2001) wrote, "the Internet decisively undermined national sovereignty and state control" of the flow of information to its inhabitants (p. 168). Not only are governments unable to regulate access to online advertising, but our results indicate that the absence of offline advertising actually increases the effectiveness of online advertising.

[1] Though Saffer (1991) found a negative effect of an advertising ban on aggregate demand in an international study, Young (1993) and Nelson and Young (2001) demonstrate that this negative effect reflected a failure to control sufficiently for differences in cultural attitudes to alcohol consumption and serial correlation, and argue that the true effect is positive.

2 Data on Display Advertising

We use data from a large database of surveys collected by a media metrics agency to measure the effectiveness of 275 different online alcohol ad campaigns. The data span campaigns that were run from 2001-2008. The mean campaign lasted 73 days. The shortest campaign lasted 14 days, and the longest lasted 200 days. There were 57 separate products advertised in total and each had an ad that ran on either 3 or 4 websites. In this paper, we use 'campaign' to describe an advertising campaign for a single product run on a single website category.

There were a variety of different creative formats used for these banner ads. 58 percent of ads were "skyscrapers" (tall ads that extend down the webpage), 19 percent were shaped as medium and large rectangles, and 8 percent were "super banners" that crossed the width of the page. 24 percent of ads were displayed on portals, 14 percent on sports websites, and 8 percent on entertainment websites. In our main specifications, we include fixed effects for each campaign to control for such potential heterogeneity in the deployment of creative format.

These 275 campaigns were reasonably evenly distributed across years, though there were slightly more in 2004 and 2007. 34 percent of the campaigns were for beer, 8 percent for wine, and the rest for spirits and other liquor. Individuals browsing the website where the campaign is running are either exposed to the ads or not, based on the randomized numerical algorithm placed on the ad server. Those who were not randomly selected to see the ad saw a dummy ad for a neutral organization. Both exposed and not exposed (control) respondents were recruited via an online survey invitation that is typically issued by a pop-up window. In each case they are not aware they are being monitored, as the survey invitation is neutral and does not make explicit that it will be asking questions about banner ad effectiveness.

Because ads were randomized, both exposed and control groups have the same likelihood of seeing offline ads, if they are permitted by law, as well as other online ads. Furthermore, because respondents are browsing the same website over the course of only a few weeks, they are similar in terms of unobserved dimensions. Thus, the randomization implies that they have the same underlying purchase probability. The only variable of difference between the two groups is the randomized presence of the ad campaign being measured, so differences in consumer attitudes toward the advertiser's brand can be attributed to the online campaign.

The online questionnaire appears as the website visitors try to navigate away from the webpage where the focal or dummy ad is served. This means that the questions measure the immediate effect of seeing the ad. The online questionnaire asked the extent to which a respondent was likely to purchase a variety of products (including the one studied) on a five-point scale. In the main specification in this paper, our dependent variable is whether the respondent said they were 'likely' or 'very likely' to purchase the product. In discretizing our scale into a single dependent measure, we follow the arguments in familiar marketing research textbooks. Malhotra (2007) and Aaker et al. (2004) suggest that discretizing an ordinal scale reduces the issues surrounding treating an interval scale as continuous. However, we recognize that whether such scales should be treated as discrete or continuous is a gray area in marketing practice (Fink, 2009; Kline, 2005). Therefore, in the appendix (Table A-2) and elsewhere, we replicate all our results with the full scale as our dependent measure in a linear regression. Consistent with Bentler and Chou (1987) and Johnson and Creech (1983), we find no qualitative difference between the specifications.

The survey asked three additional questions which we use in our analysis. First, we use respondent ratings on whether they had a favorable opinion of the product as a robustness check on our purchase intent results. Second, we use whether the respondent recalled seeing the ad in question (presented alongside some decoy ads) to inform our understanding of the effect. Third, we use respondent statements about general awareness about the product to distinguish between products where there is likely already media saturation and ones where there is not. Table 2 shows summary statistics for these survey data.

The survey also asked respondents about their gender, income, age, and the number of hours they spent on the internet. Table 2 displays summary statistics for these demographic variables. On average, the survey respondents were disproportionately male, which may reflect the fact that 35 percent of the alcohol ads were shown on websites devoted to gaming, sports, and other specifically 'male' topics. The average age is higher than in the general population, because people under 21 years old are excluded. The 9.4 hours per week that people claimed to spend on the internet is slightly higher than recent data that suggest on average people spend 32.2 hours a month on the internet.[2] Mean income, at $72,603, is not too different from the average mean household income in the US of $77,634. We converted the responses to zero-mean-standardized measures and used these variables as controls in our regressions. This allowed us to "zero out" missing data. The results are robust to both non-parametric specifications that control for the missing data through fixed effects and the omission of these controls entirely. We look at the answers of respondents who were identified as being in the target market for the product. The zipcodes given by the survey respondents allowed us to match them to a location (county or state).

If a respondent was in the exposed condition and returned to that particular webpage, or refreshed that webpage before exiting the website, the respondent is counted as having seen the ad again. The median exposure was to have seen the ad one time (56 percent of respondents who were in the exposed condition). We check the robustness of our results to excluding people who saw the ads multiple times in Table A-1, and show that their exclusion actually increases the relative magnitude of our point estimates for the incremental effects of out-of-home advertising bans on the effectiveness of banner ads.

Survey-based measures offer the advantages of having a large number of respondents and can be collected consistently at different locations and times (Clark et al., 2009). Also, the fact that websites are often unable to sell alcohol directly because of the extensive nature

[2] comScore Networks 2009 data.

of bans on the direct shipping of alcohol to consumers by states[3] means that the website purchase measures used by Manchanda et al. (2006) are not available. Survey responses are weaker measures of advertising success than purchasing or profitability (as used by Reiley and Lewis (2009)), because many users may claim that they intend to purchase but never do so. Still, as long as people reporting higher purchase intent are actually more likely to purchase (and the group that is exposed to ads is truly random), the direction of results holds. In other words, the direction of our core results depends only on whether our survey measures are positively correlated with actual purchase outcomes. This positive correlation between stated purchase intent and purchase outcomes has been well established in many product categories by Bemmaor (1995) and others. In particular, Morwitz et al. (2007) found this correlation to be particularly strong for product-specific surveys such as the ones conducted to generate our data.

Table 2: Summary Statistics for Full Sample

	Mean	Std Dev	Min	Max	Observations
Purchase Intent	0.30	0.46	0	1	61580
Intent Scale	2.51	1.49	1	5	61580
Favorable Opinion	0.36	0.48	0	1	59686
Opinion Scale	3.28	1.14	1	5	59686
Ad Recall	0.23	0.42	0	1	51923
Exposed	0.54	0.50	0	1	61580
Ad Ban	0.30	0.46	0	1	61580
Income	70603.1	55799.0	5000	250000	52839
Female	0.40	0.49	0	1	61580
Age	43.7	14.0	21	99	61562
Weekly Internet Hours	9.42	10.5	0	31	61580
Observations	61580				

[3]http://freethegrapes.org has details on the full set of laws.

3 State-Level Results

We explore how these survey measures were affected by offline advertising restrictions in two ways. In this section we use a straightforward specification that reflects both the variation in state laws governing outdoors advertising of alcohol and the randomized nature of exposure to advertising in our data. In section 4, we examine changes in local laws.

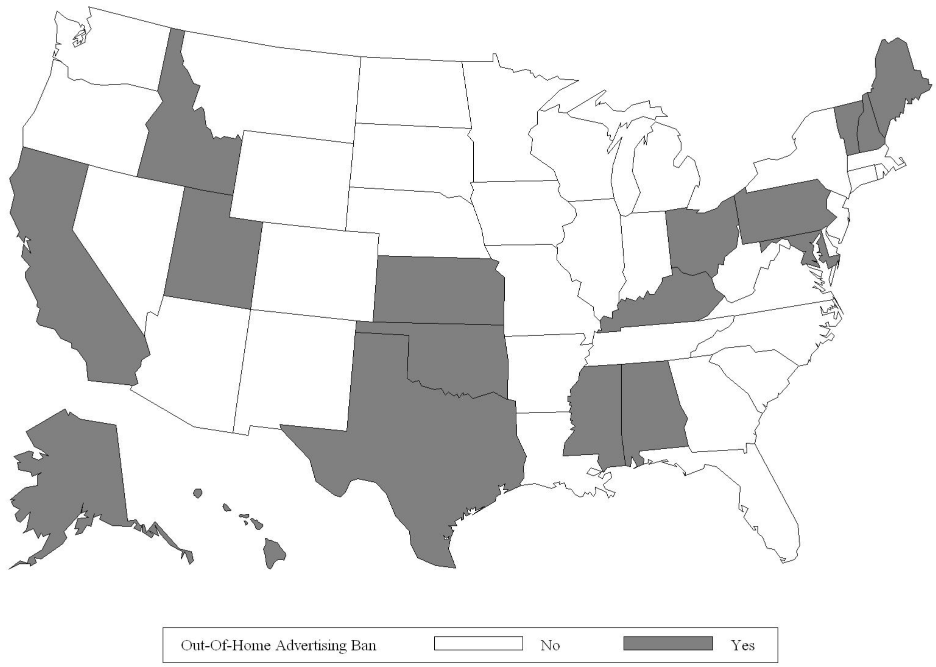
3.1 Discussion of state-level regulations

The Twenty-First Amendment (1933) granted states broad legal powers over the distribution and sale of alcohol as well as some power to regulate advertising for alcoholic beverages.[4] We gathered information on laws restricting the advertising of alcoholic beverages out-of-home using two main sources of regulations: 'State Alcohol Advertising Laws,' a report in conjunction with Georgetown University published by the Center on Alcohol Marketing and Youth in 2003; and 'Out-of-Home Alcohol Advertising: A 21st Century Guide to Effective Regulation,' published by the Marin Institute in 2009. We then verified the current status of laws and whether they imposed offline advertising bans by visiting each state's liquor control website. Appendix Table B-1 gives the sources and histories of each law.

Figure 1 reflects the geographical distribution of the 17 states that had laws. In Maryland and Pennsylvania, the laws were municipal (Baltimore and Philadelphia) rather than statewide, so we include only households that fall within those city limits as being subject to the law. There has been little change to state laws since the widespread adoption of the internet. Philadelphia's ban started in 2003, after the beginning of our data set. In section 4, we exploit this time variation along with three other changes in municipal alcohol advertising regulation.

[4]Limitations on this power stem from First Amendment free speech guarantees. For example, the 44 Liquormart (1996) decision struck down a Rhode Island State law that banned price advertising for alcoholic beverages (Milyo and Waldfogel, 1999). However, the Schmoke (1996) decision upheld an alcohol billboard advertising ban in Baltimore.

Figure 1: Distribution of states with advertising bans during our sample period



The state-level ad restrictions largely affected billboards and signage. In our specifications, we treat all out-of-home advertising bans as a binary variable, whether they relate to storefront signage, billboards, transit advertising, or a blanket ban on advertising. Three of the bans forbid all billboard advertising (Maine, Alaska, and Hawaii) while Vermont restricts all billboard advertising with extra provisions for alcohol advertising. In the appendix Table A-1, we show that our results are robust to excluding these states. In order to prevent challenges on grounds of free speech,[5] the legal language in the alcohol bans typically emphasizes that the measures are designed to prevent under-age drinkers from being exposed to advertising.

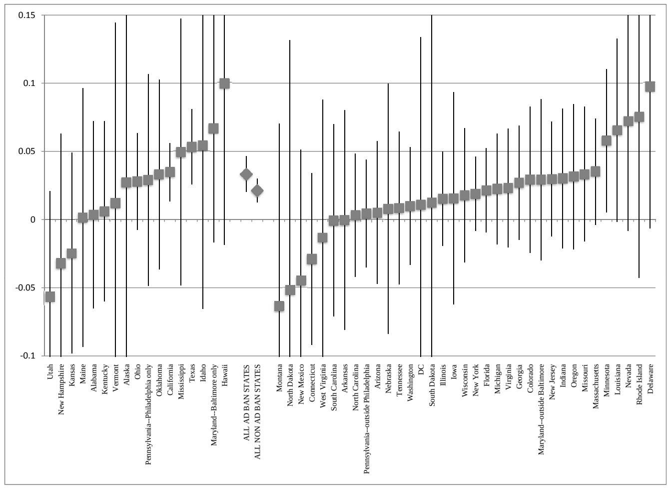
Despite various restrictions on advertising, both externally and internally imposed, alcohol manufacturers and distributors still spend a relatively large amount on advertising in

[5] So far, the bans in our data have not been struck down by the courts. In contrast, the Lorillard (2001) case in Massachusetts struck down a billboard advertising ban for cigarettes that was felt to have overreached the aim of protecting school children from cigarette advertising.

general and out-of-home advertising in particular. They spend an average of 15.2 percent of revenue on ads for liquor, 11.5 percent for wine, and 8.6 percent for beer. This compares with a mean of 7.1 percent for non-alcoholic beverages (Tremblay and Tremblay, 2005). According to Frank (2008), spirit manufacturers spent an average of $348.6m annually in 2004 on out-of-home advertising. Tremblay and Tremblay (2005) find that Anheuser Busch spent 20% of its advertising expenditure on out-of-home advertising. While not the dominant advertising channel, out-of-home appears to be an important channel in this industry. Out-of-home may be a particularly important advertising channel for alcohol because, as discussed by Frank (2008), major television networks do not typically carry ads for spirits (we use a short-term exception to this policy in NYC when we examine changes in local regulations in section 4.) Similarly, the display ads we study are particularly important for online alcohol advertising because Google and other search engines do not accept ads for spirits.

Table 1 summarizes the key empirical insight of the paper. In states that have out-of-home advertising bans, the likelihood that someone says they are likely to purchase a product is 8 percent lower relatively for the group that is not exposed to the online ad. There are many things that could drive this lower purchase intent in ad ban states besides the actual advertising ban (such as anti-alcohol attitudes), which is why we focus on the effect of randomized exposure on these different groups, why we conduct considerable additional analysis, and why we also show an alternative set of results for changes in local regulations. The raw statistics suggest that in ad ban states, underlying purchase intent is relatively 3 percent lower after randomized exposure to internet advertising. In other words, exposure to internet advertising has a larger effect on people who live in ad ban states. Figure 2 displays how the mean differences in purchase intent between participants who were exposed and not exposed to the ads varies across the states with and the states without bans on out-of-home advertising. Each square captures the effect for a different state. There are large 95% confidence intervals (the lines) for the size of the aggregate effect for any one state. While

Figure 2: Distribution of difference in exposed and control conditions for likely purchase intent across states with and without an advertising ban



the average difference between states with ad-bans and states without ad-bans is similar to Table 1, there is considerable heterogeneity within the grouping of states that have ad bans or who do not. This motivates our analysis in Table 7, where we control for different observable sources of heterogeneity at the state level in alcohol attitudes and regulations.

3.2 Specification and main results

For person i who was exposed to advertising campaign j in state s, their purchase intent reflects

$$Intent_{ij}^{s} = I(\alpha Exposure_{ij} + \beta Exposure_{ij} \times AdBan_{i}^{s} + \theta X_{ij} + \gamma^{s} + \delta_{j} + \epsilon_{ij} > 0) \qquad (1)$$

Therefore, α captures the main effect of being exposed to an ad on purchase intent; β captures the core coefficient of interest for the paper - whether exposure is more or less influential in places with an advertising ban; X_{ij} is a vector of controls for gender, age, income, and time online; γ^s is a series of state fixed effects that control for heterogeneity in baseline purchase intent at the state level and includes the main effect of the offline advertising ban ($AdBan_i^s$), which is why this lower-order interaction is not included in our specification; and δ_j controls for heterogeneity in baseline purchase intent for the different campaigns. In these regressions, we assume that the ϵ_{ij} has a type-2 extreme value distribution, implying a logit specification. Standard errors are clustered at the state level in accordance with the simulation results presented by Bertrand et al. (2004), that suggest that state-level clustering in respondent-level panel data, where policy variation occurs at the state level, is an appropriate technique to address potential downwards bias for standard errors. This represents a conservative empirical approach, as in our setting we have randomization at the respondent level as well.

Table 3 reports the results. Columns (1) through (5) use a logit specification. Column (1) simply measures the effect of exposure to the respondent's stated purchase intent. Column (2) presents the basic differences-in-differences specification, while Column (3) presents results for the core specification that includes the product-level fixed effects (δ_j). Column (4) presents results where we use non-parametric controls for customer characteristics. The results are reasonably similar across specifications, though adding product-level controls makes our estimates more precise.

The main result from Table 3 is that the effect of exposure to an online ad in states that ban out-of-home advertising is larger than in states that do not ban out-of-home advertising. A comparison of the predicted probabilities implied by our logit model suggests that, if there were an out-of-home advertising ban in effect, then exposure to an ad would increase purchase intent by 6 percent compared to an increase of 2 percent in states where there was no ban.

One explanation for this result is that offline ad bans mean that firms are advertising to consumers who are at a less steep part of their goodwill response function for advertising. The theory proposed in Nerlove and Arrow (1962) suggests that all forms of advertising contribute to a goodwill stock for the product, and that the marginal effectiveness of advertising goodwill has diminishing returns. This is been empirically documented in recent advertising literature (Dube et al., 2005; Dube and Manchanda, 2005; Hitsch, 2006). Theoretically, an advertising ban could limit the amount of goodwill accumulated by residents of states with ad bans, meaning that the marginal effect of an exposure would be higher in states with bans than in states with no bans.

There are of course many identification assumptions that have to hold to draw such a conclusion. Therefore we devote sections 3.3 and 3.4 to exploring the identification assumptions that are inherent in this result, allow the effect of exposure to vary with pertinent state characteristics, and show that the difference in advertising responsiveness across states is specific to alcohol advertising and does not apply to closely related categories.

In columns (1) to (3), the standardized measures of our demographic variables give us a weakly positive effect from income and internet hours, and a negative effect from age. Column (4) reports the results of a non-parametric specification where we included fixed effects for every age, income, and internet usage group. This has the advantage of letting us control in a non-parametric manner for missing observations. The main effect that we estimated for the interaction between outdoor advertising and exposure was very similar to our previous estimates. Column (5) provides an alternative specification where we include no demographic controls. The main results are statistically similar to (4), though the exclusion of the demographic controls is reflected in a more negative log-likelihood suggesting that the controls do help with efficiency. This is to be expected given the randomized nature of our data.

Another econometric concern is the interpretation of the interaction terms in Table 3. Research by Ai and Norton (2003) suggests that the interaction in a non-linear model may not capture the true cross-derivative. Recent work by Puhani (2008), however, does suggest that the treatment effect in differences-in-differences specifications have the same sign as the

Table 3: People who are exposed less to offline advertising react more to online advertising

	(1) Purchase Intent	(2) Purchase Intent	(3) Purchase Intent	(4) Purchase Intent	(5) Purchase Intent	(6) Intent Scale	(7) Intent Scale	(8) Favorable Opinion	(9) Ad Recall
Exposed × Ad Ban		0.0596**	0.0673**	0.0676**	0.0698***	0.0460***	0.0589***	0.0539	-0.0143
		(0.0293)	(0.0268)	(0.0270)	(0.0263)	(0.0160)	(0.0207)	(0.0351)	(0.0384)
Exposed	0.0997***	0.0823***	0.0684***	0.0613***	0.0734***	0.0459***	0.0600***	0.0925***	0.550***
	(0.0155)	(0.0130)	(0.0161)	(0.0161)	(0.0162)	(0.0104)	(0.0124)	(0.0217)	(0.0299)
Female	0.0851***	0.0848***	0.0900***	0.0880***		0.0293**	0.0270	-0.0976***	-0.196***
	(0.0164)	(0.0164)	(0.0183)	(0.0190)		(0.0135)	(0.0171)	(0.0231)	(0.0219)
Std. Internet Hours	0.0515***	0.0514***	0.0476***			0.0315***	0.0381***	0.0636***	0.0870***
	(0.0105)	(0.0105)	(0.0123)			(0.00843)	(0.0106)	(0.0104)	(0.0125)
Std. Income	-0.000264	-0.000158	-0.0198**			-0.0272***	-0.0319***	0.0279***	-0.0331***
	(0.0100)	(0.0100)	(0.00994)			(0.00646)	(0.00810)	(0.00729)	(0.00969)
Std. Age	-0.135***	-0.135***	-0.167***			-0.146***	-0.189***	-0.143***	-0.239***
	(0.0108)	(0.0108)	(0.0155)			(0.0102)	(0.0143)	(0.0128)	(0.0150)
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Campaign Fixed Effects	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Demo Fixed Effects	No	No	No	Yes	No	No	No	No	No
Observations	61580	61580	61580	61566	61580	61580	61580	58585	51923
Log-Likelihood	-37191.6	-37190.3	-35940.3	-35770.9	-36099.2	-110305.7	-90026.4	-36802.6	-26108.4
R-Squared	NA	NA	NA	NA	NA	0.0519	NA	NA	NA

Robust standard errors clustered at the state level. * $p < 0.10$, ** $p < 0.05$,*** $p < 0.01$. 'Ad Ban' is collinear with state fixed effects and omitted.

logit interaction. In order to ensure that our results are not a function of the nonlinearity of the estimation function, we performed the Ai-Norton specification check for each of our specifications. The results were similar in size and relative magnitude and significant at the 95 percent level. We also focus on the implied changes in marginal probabilities which take into account the potential for non-linearities in the logit model when discussing the economic significance of our results. In the appendix (Table A-1, Column (1)), we show that a linear probability model gives qualitatively similar results, which provides reassurance that the non-linear functional form does not drive our results.

Column (6) of Table 3 replicates our results for a linear model where the dependent variable is the full 5-point scale that we assume to be continuous. The similar results suggest that our discretization of the scale is not driving our results, though as discussed above treating an ordinal scale as continuous is not without problems. Column (7) presents results from an ordered logit that also uses this scale as its dependent variable with similar results. Column (8) presents results for whether the respondent said they had a favorable opinion of the product as the dependent variable.

Column (9) presents results for ad recall as the dependent variable. The main effect of exposure on ad recall is, unsurprisingly, larger. Interestingly, the advertising bans are unrelated to ad recall. Recalling the ad on the website is not related to whether offline alcohol ads are allowed. It suggests that the difference we observe in purchase intent is not a result of ads being more unusual and therefore more noticeable to inhabitants of states where there are advertising bans. Instead, there is a difference in how effective the message of such ads is between the populations of regulated and non-regulated states.

3.3 Controlling for systematic differences between states with ad bans and without ad bans

The random assignment of advertising exposure means that we have a clean measure of advertising effectiveness, so the core remaining identification assumption that underlies Table 3 is that online alcohol advertising is not systematically more effective in states that have an advertising ban compared to states without such a ban for reasons other than the ban.

We received the data after people who had not completed the survey were removed. One potential concern is that there may be different survey attrition or recruitment rates for survey-takers exposed to the ad in states that had bans compared to states that did not have bans, that could influence our results. For example, it would be problematic for our interpretation if states that had ad bans also had inhabitants who were more likely to be motivated to complete surveys enthusiastically if they had seen an alcohol ad. A two-sided t-test comparing the relative proportion of respondents who were exposed to the ad in states that had ad bans and states that did not have ad bans, strongly rejected that the proportions were different (p-value=.77). We also checked for systematic differences in recruitment ability in states that had ad bans by comparing the proportion of survey-takers relative to the population of each state in states that had ad bans. A two-sided t-test (p-

value=.0.97) provided strong evidence that there was no difference in the proportion of the state population recruited. This suggests that there was no difference in the ability of the marketing research firm to generate the appropriate sample size from states that had ad bans and states that did not.

One possible alternative explanation could be that the ads viewed in states with ad bans were designed to be more effective by alcohol beverages companies. Such geographical targeting could take place either if there were regional differences in the viewership of sites in the experiment or if the website matched ads to viewers by the geographical location of their IP address. However, we found no difference in the proportion of viewers exposed to different campaigns in states with ad bans and states without. Furthermore, as shown in Table 4, there is little difference in the creative formats used across states. As one would expect given the sample size, there are some statistically significant differences across the 20 categories. However, for the 6 categories where there was a statistically significant difference, the differences tend not to be *economically* significant. Furthermore, the differences tend to cut in the wrong direction if they were going to provide an alternative explanation of our result. For example, the largest difference across states with bans and without bans is that people in states with ad bans tend to have seen more medium rectangle ads. However, medium rectangles are one of the smaller and less visible ad formats.

Another explanation is that the viewers in states with ad bans and states without ad bans have different demographic profiles. For example, if states with ad bans had a younger population who might be more likely to be influenced by alcohol advertising, then this could explain why online alcohol advertising was more effective in such states. However, as Table 5 shows, there is no statistically significant difference in states with ad bans and without bans in terms of average age, gender, income, or internet usage for survey-takers that were exposed to ads and survey-takers that were not.[6] If we use buckets for these demographic

[6] Analysis at the state level is similar.

Table 4: Relative proportion of different advertising formats for states with and without ad bans

	Mean No Ban	Mean with Ban	T-Test
Banner (468x60)	0.0284	0.0253	1.99
Button (120x90)	0.0055	0.0047	1.14
Large Rectangle/Square (336x280)	0.0372	0.0384	-0.70
Skyscraper (120x600)	0.0267	0.0229	2.56
Vertical Rectangle (240x400)	0.0001	0.0001	-0.13
Half-Banner (234x60)	0.0008	0.0006	0.94
Full page	0.0039	0.0035	0.62
N/A	0.0180	0.0212	-2.43
Interstitial	0.0003	0.0004	-0.48
Super Banner (728x90)	0.0604	0.0557	2.12
Floating	0.0060	0.0067	-1.05
Pop Up (250x250)	0.0012	0.0012	-0.05
Rectangle (180x150)	0.0026	0.0031	-0.88
Medium Rectangle (300x250)	0.1675	0.1915	-6.70
Wide Skyscraper (160x600)	0.6158	0.5986	3.74
Half Page Ad (300x600)	0.0050	0.0049	0.24
Vertical Banner (120x240)	0.0162	0.0166	-0.37
Square Button(125x125)	0.0002	0.0001	0.31
Other	0.0019	0.0026	-1.53
Unknown	0.0023	0.0018	1.12
Rectangle (300x100)	0.0001	0.0002	-0.74
Observations	61,580		

variables rather than a continuous measure, the difference remains statistically insignificant. For example, the average number of people under the age of 35 (who buy the majority of alcohol) is the same across states with ad bans and without.

Another set of concerns lies with the potential for the effects of online advertising exposure to vary in systematic ways connected with the state's characteristics, which are in turn correlated with the ability of alcohol advertising to persuade. Because respondents are

Table 5: Demographic profiles for states with and without ad bans by exposure to ads

	Mean No Ban	Mean with Ban	T-Test
	Exposed to Ad		
Age	43.57	43.55	0.16
Female	0.41	0.42	-1.33
Income	70,820	70,800	0.03
Weekly Internet Hours	9.59	9.62	-0.25
Observations	33,322		
	Not Exposed to Ad		
Age	43.94	43.96	-0.15
Female	0.40	0.39	0.71
Income	70,034	71,067	-1.31
Weekly Internet Hours	9.29	9.08	1.56
Observations	28,258		

randomly assigned to whether or not they are exposed to the ad but are not randomly assigned to whether or not they live in a state with offline advertising bans, there may be other reasons for the systematic difference we observe in the effect of exposure in states that have bans and states which do not. This problem is apparent in our data: respondents who are not exposed to alcohol advertising were 8 percent less likely to express positive purchase intent in states that had billboard bans, suggesting there is indeed an underlying systematic difference. In this section, we address this problem by controlling for the effect of multiple state characteristics on exposure. In section 4, we tackle this identification issue directly by examining locations which experienced changes in advertising bans.

Table 6 summarizes the rich set of observables that we explore. The first seven variables capture different aspects of alcohol consumption and abuse. The idea is to control for consumer-side heterogeneity that may be driving our results. For example, states where people consume more alcohol may be more likely to have people who respond to alcohol ads and to have an alcohol advertising ban. The last five variables reflect different states' attempts to restrict advertising content and the consumption and the sale of alcohol. This helps control for the possibility that we may be picking up something more general about states that take a tough line on alcohol sales, and the likelihood of their population to respond to alcohol advertising.

For each potential source of observable heterogeneity $Observable_i^{st}$, we reran the specification in equation (1) with an additional term $Exposure_{ij} \times Observable_i^{st}$ that captured how this moderated the main effect of being exposed to the ad. Because there was time-variation in these measures, we also included the main effect, $Observable_i^{st}$. Table 7 reports the results. The final column presents results from a combined regression where we include interactions for each of these observables in the same specification. It is apparent that, as would be expected, some of these sources of state variation do impact the effect of exposure on advertising. People from states with large populations of non-drinkers were less likely to be influenced by an online alcohol ad, people from states with more heavy drinkers and youth drinkers were more likely to influence by an online alcohol ad, and people in states with strict restrictions on Sunday alcohol sales were less likely to be influenced by ads.

Crucially, however, in all cases the key effect of interest $Exposure_{ij} \times AdBan_i^s$ remains positive and significant. While not conclusive, these results help rule out the most obvious alternative explanations of our results occasioned by the lack of orthogonality of the state offline ad ban to state characteristics pertaining to alcohol consumption.

Table 6: State Level Variables

Variable	Mean	Std. Dev.	Description	Source
BeerDrunk	21.6	3.27	Gallons (000) of beer drunk per capita in state each year	Alcohol Epidemiologic Data System. LaVallee, R.A.; Williams, G.D.; and Yi, H. Surveillance Report #87: Apparent Per Capita Alcohol Consumption: National, State, and Regional Trends, 19702007. Bethesda, MD: National Institute on Alcohol Abuse and Alcoholism, Division of Epidemiology and Prevention Research (September 2009).
WineDrunk	2.31	0.90	Gallons (000) of wine drunk per capita in state each year	Alcohol Epidemiologic Data System. LaVallee, R.A.; Williams, G.D.; and Yi, H. Surveillance Report #87: Apparent Per Capita Alcohol Consumption: National, State, and Regional Trends, 19702007. Bethesda, MD: National Institute on Alcohol Abuse and Alcoholism, Division of Epidemiology and Prevention Research (September 2009).
SpiritsDrunk	1.40	0.37	Gallons (000) of spirits drunk per capita in state each year	Alcohol Epidemiologic Data System. LaVallee, R.A.; Williams, G.D.; and Yi, H. Surveillance Report #87: Apparent Per Capita Alcohol Consumption: National, State, and Regional Trends, 19702007. Bethesda, MD: National Institute on Alcohol Abuse and Alcoholism, Division of Epidemiology and Prevention Research (September 2009).
BingeDrinkers	15.4	2.65	Percentage of binge drinkers (males having five or more drinks on one occasion, females having four or more drinks on one occasion)	Behavioral Risk Factor Surveillance System annual survey data 2001-2008
HeavyDrinkers	5.28	1.04	Percentage of heavy drinkers (adult men having more than two drinks per day and adult women having more than one drink per day)	Behavioral Risk Factor Surveillance System annual survey data 2001-2008
YouthDrinkDrive	2.34	0.74	Percentage of students who during the past 30 days drove a vehicle 1 or more times when they had been drinking alcohol	Youth Risk Behavior Survey, Centers for Disease Control and Prevention. The YRBS is an in-school survey of students in grades 9 through 12.
YouthDrinking	43.2	3.73	Percentage of students who at least had one drink of alcohol on 1 or more of the past 30 days	Youth Risk Behavior Survey, Centers for Disease Control and Prevention. The YRBS is an in-school survey of students in grades 9 through 12.
Teetotalers	45.3	7.70	Percentage of adults who have not had alcohol in the last 30 days	Behavioral Risk Factor Surveillance System annual survey data 2001-2008
NoKidsAds	0.38	0.79	State bans alcohol ads that depict children. For example CT's law reads [No alcohol advertisement shall include] any scene in which is portrayed a child or objects, such as toys, suggestive of the presence of a child or which in any manner portrays the likeness of a child or contains the use of figures or symbols which are customarily associated with children. [CT Reg. 30-6-A31(a)(6)].	State Advertising Laws: Current Status and Model Policies, Center on Alcohol Marketing and Youth, Georgetown University
NoAthleticAds	0.28	0.69	State bans alcohol ads that Associate Alcohol with Athletic Achievement	State Advertising Laws: Current Status and Model Policies, Center on Alcohol Marketing and Youth, Georgetown University
NoSundayTrading	0.25	0.44	State bans majority of Sunday off-premises alcohol sales	Alcohol Policy Information System, NIH
NoOpenBeverages	0.92	0.27	State prohibits open containers of alcohol in the passenger compartments of non-commercial motor vehicles.	Alcohol Policy Information System, NIH
StateLiquorStores	0.28	0.45	State-run retail distribution system for hard liquor	Alcohol Policy Information System, NIH

Table 7: Allowing effect of exposure to vary with observable state characteristics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Exposed × Ad Ban	0.0695***	0.0691**	0.0783***	0.0685***	0.0620**	0.0667**	0.0712***	0.0762***	0.0671**	0.0588**	0.0554**	0.0699**	0.0679***	0.0758**
	(0.0257)	(0.0268)	(0.0291)	(0.0253)	(0.0267)	(0.0268)	(0.0230)	(0.0251)	(0.0264)	(0.0280)	(0.0253)	(0.0277)	(0.0252)	(0.0385)
Exposed	-0.00382	0.0344	0.000463	-0.0338	-0.0842	0.0590	-0.396*	0.242***	0.0691***	0.0784***	0.0932***	0.0941***	0.0797***	0.323
	(0.111)	(0.0398)	(0.0737)	(0.0819)	(0.0686)	(0.0479)	(0.207)	(0.0823)	(0.0152)	(0.0185)	(0.0201)	(0.0325)	(0.0185)	(0.516)
Exposed× BeerDrunk	0.00330													0.00142
	(0.00525)													(0.00847)
Exposed× WineDrunk		0.0145												-0.0319
		(0.0160)												(0.0334)
Exposed× SpiritsDrunk			0.0463											0.0338
			(0.0525)											(0.0873)
Exposed× BingeDrinkers				0.00660										-0.0152
				(0.00565)										(0.0177)
Exposed× HeavyDrinkers					0.0292**									0.0183
					(0.0136)									(0.0308)
Exposed× YouthDrinkDrive						0.00414								-0.0338
						(0.0218)								(0.0347)
Exposed× YouthDrinking							0.0107**							0.00730
							(0.00476)							(0.00580)
Exposed× Teetotalers								-0.00390**						-0.00611
								(0.00175)						(0.00490)
Exposed× NoKidsAds									-0.00177					0.0122
									(0.0183)					(0.0211)
Exposed× NoAthleticAds										-0.0269				-0.0366
										(0.0210)				(0.0224)
Exposed× NoSundayTrading											-0.0833***			-0.0755**
											(0.0245)			(0.0320)
Exposed× NoOpenBeverages												-0.0288		-0.0549
												(0.0368)		(0.0452)
Exposed× StateLiquorStores													-0.0411	-0.0277
													(0.0259)	(0.0415)
Female	0.0902***	0.0907***	0.0901***	0.0899***	0.0905***	0.0901***	0.0900***	0.0887***	0.0900***	0.0900***	0.0902***	0.0900***	0.0900***	0.0908***
	(0.0183)	(0.0184)	(0.0183)	(0.0184)	(0.0184)	(0.0183)	(0.0184)	(0.0184)	(0.0183)	(0.0183)	(0.0184)	(0.0183)	(0.0183)	(0.0184)
Std. Internet Hours	0.0478***	0.0515***	0.0480***	0.0478***	0.0476***	0.0474***	0.0472***	0.0509***	0.0476***	0.0475***	0.0480***	0.0475***	0.0477***	0.0518***
	(0.0124)	(0.0126)	(0.0121)	(0.0124)	(0.0123)	(0.0123)	(0.0122)	(0.0124)	(0.0123)	(0.0123)	(0.0123)	(0.0123)	(0.0123)	(0.0124)
Std. Income	-0.0199**	-0.0193*	-0.0198**	-0.0198**	-0.0198**	-0.0199**	-0.0198**	-0.0195**	-0.0198**	-0.0198**	-0.0199**	-0.0198**	-0.0199**	-0.0194*
	(0.00993)	(0.00998)	(0.00992)	(0.00994)	(0.00993)	(0.00995)	(0.00994)	(0.00992)	(0.00993)	(0.00993)	(0.00993)	(0.00994)	(0.00994)	(0.00993)
Std. Age	-0.167***	-0.166***	-0.167***	-0.167***	-0.167***	-0.167***	-0.167***	-0.166***	-0.167***	-0.167***	-0.167***	-0.167***	-0.167***	-0.165***
	(0.0154)	(0.0155)	(0.0155)	(0.0155)	(0.0155)	(0.0154)	(0.0154)	(0.0155)	(0.0155)	(0.0155)	(0.0155)	(0.0155)	(0.0155)	(0.0155)
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Campaign Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observable Main Effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	61580	61580	61580	61580	61580	61580	61580	61580	61580	61580	61580	61580	61580	61580
Log-Likelihood	-35936.2	-35934.6	-35939.8	-35939.6	-35938.9	-35940.2	-35937.5	-35932.9	-35940.3	-35939.8	-35938.0	-35940.1	-35939.8	-35922.2

Logit estimates. Robust standard errors clustered at the survey level. * $p < 0.10$, ** $p < 0.05$,*** $p < 0.01$.

3.4 Falsification Checks

As demonstrated in the previous section, observable differences in the ad campaigns, characteristics of respondents, and characteristics of the states with ad bans do not provide an alternative explanation of our results. To explore whether there are *un*observed differences in general advertising responsiveness, we look at other types of closely related advertising that are not subject to the ban to see whether we observe similar results that may be suggestive of unobserved heterogeneity. Specifically, we rerun our main specification on similar data for 'placebo' categories. The first placebo category is high-sugar/high-salt snacks and candies. Like alcoholic beverages, these have been targeted by the US Surgeon General's office as an area of excess consumption with potential negative side-effects. Column (1) in Table 8 reports the results for these snacks. As expected, we see an effect from exposure to an ad; however alcohol ad bans do not change this relationship - the interaction term is negative and insignificant. Column (2) repeats this falsification exercise for non-alcoholic beverages, and again we find no significant effect from out-of-home advertising bans on the effect of exposure. Column (3) repeats the falsification exercise for all food items, again finding no increase in the effect in states with bans. This supports our interpretation that the effect we are measuring is specific to the alcohol category, and therefore more likely to be related to the presence or absence of an advertising ban.

4 Changes in local advertising regulations

The second way we show the impact of offline advertising bans on online advertising effectiveness is by examining four different changes in local alcohol advertising regulations. These help overcome worries that, despite the above checks for underlying state-level heterogeneity and the falsification test, there is still the possibility that there may be unobserved heterogeneity in advertising responsiveness that is specific to the alcohol sector. We focus on several instances where, during the period of our data, alcohol advertising bans were

.

Table 8: Falsification Checks

	High Sugar Foods	Non-Alc. Beverages	All Food
	(1)	(2)	(3)
	Purchase Intent	Purchase Intent	Purchase Intent
Exposed × Ad Ban	-0.00493	-0.00268	-0.00467
	(0.0372)	(0.0549)	(0.0173)
Exposed	0.0712***	0.0835**	0.0758***
	(0.0263)	(0.0340)	(0.0104)
Female	0.164***	0.300***	0.266***
	(0.0269)	(0.0364)	(0.0120)
Std. Internet Hours	0.0514***	0.0646***	0.0465***
	(0.0136)	(0.0168)	(0.00616)
Std. Income	-0.0653***	-0.0106	-0.0465***
	(0.0145)	(0.0181)	(0.00710)
Std. Age	-0.0688***	-0.129***	-0.0338***
	(0.0150)	(0.0155)	(0.00818)
State Fixed Effects	Yes	Yes	Yes
Campaign Fixed Effects	Yes	Yes	Yes
Observations	45310	23983	194351
Log-Likelihood	-28468.6	-13875.9	-117830.4

Robust standard errors clustered at the state level. * $p < 0.10$, ** $p < 0.05$,*** $p < 0.01$.

'Ad Ban' is collinear with state fixed effects and omitted.

enacted or rescinded. The idea here is that such changes in laws allow us to identify precisely the causal effect of the law on responses to online advertising because we can compare (for people living in the same place) the reactions to online ads before and after there is a law prohibiting forms of alcohol advertising. There are four such cases for which we have observations in our data: (1) The enactment of a law in Philadelphia prohibiting alcohol advertising from public property; (2) The repeal of a law in Pennsylvania restricting the ability of student newspapers to publish alcohol advertising; (3) A toughening of regulations by the San Francisco public transit authority meaning that alcohol advertising would be fined at $5,000 a day; and (4) A loosening of a self-imposed policy, which meant that one broadcast TV affiliate in New York started showing liquor ads.

Even when using a change in a law to identify the effect of ad bans, a lingering concern is that the change in law may be connected with an unobserved change in behavior over time that is also systematically connected with how people respond to advertising. To control for this possibility, in each instance of a change in the law we include data on a control group of people who should display a similar time trend because they live in a similar place reasonably nearby. This allows us to use a triple difference specification to difference out changes in advertising responsiveness due to location and changes due to changing time trends. In equation form, this means that our specification in equation (1) becomes for person i who was exposed to advertising campaign j in location l at time t:

$$\begin{aligned} Intent^l_{ijt} = I(&\alpha Exposure_{ij} + \beta_1 Exposure_{ij} \times AffectedGroup^l_i \times AdBanPeriod^l_{it} + \\ &\beta_2 Exposure_{ij} \times AffectedGroup^l_i + \beta_3 Exposure_{ij} \times AdBanPeriod^l_{it} + \\ &\lambda AdBanPeriod_{it} + \eta AffectedGroup^l_i \times AdBanPeriod^l_{it} + " \\ &\theta X_{ij} + \gamma^l + \delta_j + \epsilon_{ij} > 0) \end{aligned} \tag{2}$$

Reflecting the switch from the state level to a more local analysis, γ^l is now a series of county fixed effects that control for heterogeneity in baseline purchase intent at the county level. Again, these substitute for the main effect of $AffectedGroup^l_i$. As we are no longer looking at state-level variation in policy but instead at within-state variation, we cluster standard errors at the campaign level rather than the state level.

Table 9 displays our case-study results. In all cases the sample size is relatively low and as a result our results are sometimes only marginally significant. However, taken together they suggest that offline advertising bans positively influence the effectiveness of online ads.

Column (1) looks at a change in law that occurred in 2003 when the Philadelphia city government decide to ban alcohol ads from all city property. This change in policy is discussed in detail by Haas and Sherman (2003), and appears to have been motivated by policy advocates highlighting the issue of alcohol ads on bus shelters that were commonly used by students going to school. As our control group, we use people who live in the same combined metropolitan statistical area but over the border either in Camden, New Jersey or in 'Wilmington-Newark' in Delaware and Maryland. The results in column (1) suggest that people who live in Philadelphia became incrementally more responsive to online alcohol advertising after the ban, compared both to their previous responsiveness and to people in neighboring states in that time period.

Column (2) looks at a change in law in the opposite direction, also in Pennsylvania. In Pitt News v. Attorney General of Pennsylvania (No. 03-1725), July 29, 2004, the federal court struck down a Pennsylvania law banning advertising of alcoholic beverages in college newspapers. Though this law, which pertains to print media rather than out-of-home advertising, is different from the ones we have studied so far, it should have similar effects if the behavior we observe is due to awareness of the product. To study this law, we looked at the behavior of consumers in college towns such as Waynesburg, PA which is home to Waynesburg University and the 'Yellow Jacket' student newspaper. We defined a 'college town' as one where more than 20 percent of the population are students. We compared responsiveness to online alcohol advertising in these college towns with similarly sized towns in surrounding areas where there was no such newspaper. This focus on college towns and their counterparts meant that our analysis excluded the Philadelphia metropolitan area we studied in column (1). This is attractive from an identification point of view, as it means that we are studying two different types of populations within the same state who during a similar time-frame experience a change in exposure to offline alcohol advertising but in *different* directions. If it were indeed state-level heterogeneity that explained our results, we would not expect to find different directions of effects in these two cases. Consistent with our prior findings, column (2) indicates that in the period prior to the ad ban being struck down, these college-town populations were indeed more responsive to online alcohol advertising that they were afterwards, relative to the population in non-college towns.

Column (3) examines a change in policy regarding alcohol advertising by the San Francisco transit authority. As described by Simon (2008), the Marin Institute, a public pressure group, presented evidence that despite a contract prohibiting alcohol advertising, in at least 15 cases CBS Outdoor had erected alcohol ads on transit authority property that violated their contract. In response to public outcry, at the end of 2007 the San Francisco transit authority issued a new contract that promised to fine advertising companies $5,000 per day if an advertising company violated their contract and displayed alcohol ads. We study how this toughening of regulatory enforcement affected online ad responsiveness. Therefore our estimates in this case should be thought of as comparing the effect of the official ban of the kind that already existed in California that we have studied so far, with increased and improved enforcement of that ban. As a control group, we use respondents in Los Angeles and San Jose, which while geographically distant, are other large metropolitan communities in the state; again, if it is a systematic change in state-specific behavior that is driving our results, we would expect to see similar time trends in how effective online exposure to advertising is across each city. However, the results of column (3) suggest that again there was an incrementally positive effect of the ad ban for San Francisco survey-takers relative to their previous behavior and relative to survey-takers in Los Angeles and San Jose in the same period.

Column (4) focuses on TV advertising, extending our previous focus on outdoor and print advertising. As described by Elliot (2007), at the end of November 2007 WNBC-TV, NBC's flagship New York network affiliate, decided to start showing hard liquor ads on its broadcasts. Liquor ads had been shown on cable before, but it was the first network airing of spirit ads in 6 years. What is attractive about this change in policy is that it represents a change in advertising practices within the alcoholic beverage category. Beer is regularly advertised on broadcast television, but spirits have been subject to self-regulation. This means that we can evaluate whether people living in the same place responded differently to online spirits ads compared to how they did previously, while controlling for general

Table 9: Case studies of instances where alcohol advertising bans changed in our sample period

	(1) Purchase Intent	(2) Purchase Intent	(3) Purchase Intent	(4) Purchase Intent	(5) Purchase Intent
Purchase Intent					
Exposed × Ad Ban × Affected Group	0.881*	1.791***	2.310**	1.005*	0.563***
	(0.489)	(0.666)	(0.943)	(0.581)	(0.215)
Exposed	0.646	0.0958	0.0958	0.909***	0.134
	(0.447)	(0.139)	(0.0948)	(0.302)	(0.0838)
Exposed × Ad Ban	-0.662	-0.0978	-0.469*	-0.940***	-0.219*
	(0.508)	(0.253)	(0.284)	(0.326)	(0.131)
Exposed × Affected Group	-0.678*	-0.657	-0.0740	-0.886	-0.287
	(0.411)	(0.434)	(0.483)	(0.541)	(0.187)
Ad Ban	-0.572	0.822**	0.115	0.712***	0.0573
	(0.552)	(0.365)	(0.269)	(0.218)	(0.0956)
Ad Ban × Affected Group	-0.445	-0.412	-0.439	-0.256	-0.178
	(0.430)	(0.590)	(0.885)	(0.351)	(0.156)
Campaign Fixed Effects	Yes	Yes	Yes	Yes	Yes
Demographics	Yes	Yes	Yes	Yes	Yes
County fixed effects	Yes	Yes	Yes	Yes	Yes
Observations	1429	1957	2524	1991	7927
Log-Likelihood	-781.5	-1063.0	-1430.0	-1172.0	-4552.4
Date of change	December 2003	End July 2004	December 2007	November 2007	Combined
Change in Regulation	Ban of alcohol advertising on Philadelphia's public property	Law banning alcohol ads in student newspapers struck down	Stiff fines for alcohol advertising on San Francisco public transportation	NBC affiliate in New York rescinds self-imposed ban on airing hard liquor ads	Combined
Affected Sample	Respondents in Philadelphia	Respondents in college towns in PA	Respondents in San Francisco	New York respondents asked about liquor campaigns	Combined
Control Group	Respondents in Camden, NJ and Wilmington-Newark, DE	Respondents outside of Philadelphia in non-college towns in PA	Respondents in Los Angeles and San Jose, CA	New York respondents asked about beer campaigns	Combined

Logit estimates. Robust standard errors clustered at the survey level. * $p < 0.10$, ** $p < 0.05$,*** $p < 0.01$.

changes in the effectiveness of online campaigns for alcohol using beer. The results again suggest that before the ban was rescinded, there was a relative incremental advantage in terms of effectiveness for spirits relative to beer. However, after the ban was rescinded this incremental advantage decreased. The fact that we obtain similar results for changes in regulation governing print media and also television media is informative, since it suggests that our results concerning the effect of restrictions on out-of-home advertising campaignscan be applied cautiously to other media.

Column (5) presents results of a specification that combines data from columns (1) - (4). The results are similar, though our estimate of the coefficient of interest $Exposure \times AdBan \times AffectedGroup$ is more precise.

The aim of these case studies is to provide evidence for the robustness of our results in Table 9, so we checked how the economic size of the effects they suggested compared to our earlier estimates. This is again a logit framework with interaction terms, so in order to get reliable estimates of marginal effects, we calculated the population average predicted probabilities and compared them for our different treatment and control groups. The estimates in Table 3 suggest that in states with ad bans the effect of advertising exposure is around 1.2 percentage points higher. In two of our case studies our results are similar. Our estimates for Philadelphia residents suggest that after the ad ban, there was a 2.2 percentage point increase in the effect of exposure to advertising on purchase intent. Our estimates for New York residents suggest that before the spirits ad ban was repealed, there was a 1.8 percentage point greater effect of exposure to online spirits advertising on purchase intent relative to other alcohol. However, in two case studies the estimates of the incremental advantage of online advertising when offline ad bans are in place were far higher. For respondents in San Francisco, there was an incremental lift of 14 percentage points. For respondents in college towns, it was 24 points higher. It could be that these are unusual populations who are easily motivated by stimuli to drink alcohol. However, since these estimates of the marginal effects have large confidence intervals and are statistically indistinguishable from 2.2 percentage points, it is more likely that these high point estimates reflect the relative imprecision of our estimates due to the smaller sample size used in these case studies. In column (5) where we combine data from all 5 case studies, we find evidence of approximately a 4.1 percentage point increase in the likelihood of a respondent stating 'likely' or 'very likely' purchase intent.

In Table 10, we show the robustness of the combined results from column (5) to a similar sequence of robustness checks to those presented in Table 3. Column (1) shows that our results are robust to the exclusion of the controls. Column (2) shows our results are robust to a linear OLS regression for the full intent scale. Column (3) shows our results are robust to an ordered logit specification for the scale. Column (4) shows the results when we use Favorable Opinion as the dependent variable. Interestingly, compared to Table 3 the point estimate for the coefficient of interest is more precisely estimated here, suggesting that offline ad bans can affect attitudes towards the product as well as purchase intent. Column (5) again shows little effect of the offline advertising ban for ad recall. Columns (6)-(8) repeat the falsification checks presented in Table 8 for the combined data from our four case studies. In all cases we do not find any significant effect of offline alcohol ad bans on the purchase intent for categories that are not alcohol.

Table 10: Case studies of instances where alcohol advertising bans changed in our sample period: Robustness and Falsification Check

	(1) No Controls	(2) OLS	(3) Ordered Logit	(4) Favorable Opinion	(5) Ad Recall	(6) Junk Food	(7) Beverages	(8) CPG Category
main								
Exposed × Ad Ban × Affected Group	0.579***	0.297**	0.372**	0.677***	0.188	0.102	0.0236	0.0200
	(0.210)	(0.136)	(0.174)	(0.216)	(0.298)	(0.0840)	(0.125)	(0.0421)
Exposed	0.150*	0.117**	0.162**	0.184**	0.638***	0.0493**	0.0158	0.0290***
	(0.0831)	(0.0562)	(0.0685)	(0.0737)	(0.111)	(0.0193)	(0.0260)	(0.0100)
Exposed × Ad Ban	-0.216	-0.181**	-0.228**	-0.319***	-0.313*	-0.0724**	-0.0474	-0.0396**
	(0.132)	(0.0790)	(0.0963)	(0.0932)	(0.169)	(0.0324)	(0.0413)	(0.0159)
Exposed × Affected Group	-0.305*	-0.148	-0.198	-0.342*	-0.0254	-0.0668	0.0759	0.0102
	(0.183)	(0.128)	(0.163)	(0.207)	(0.180)	(0.0662)	(0.0967)	(0.0338)
Ad Ban	0.0586	0.0616	0.0737	0.0671	0.163	0.0198	0.0697*	0.0230
	(0.0984)	(0.0627)	(0.0801)	(0.0928)	(0.104)	(0.0282)	(0.0373)	(0.0154)
Ad Ban × Affected Group	-0.204	-0.114	-0.144	-0.253	0.291	-0.0211	-0.166	-0.0191
	(0.155)	(0.0954)	(0.135)	(0.162)	(0.247)	(0.0598)	(0.105)	(0.0320)
Survey Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Demographics	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	7927	7937	7937	7480	6781	5111	2895	21791
Log-Likelihood	-4582.2	-14036.6	-11568.9	-4655.8	-3386.3	-3515.8	-1943.7	-15595.3

Columns (1)-(4) repeat robustness checks from Table 3 for column (5) of Table 9. Columns (5)-(7) perform 'falsification' checks for alternative categories for Column (5) of Table 9.

5 Awareness

In this section, we explore one potential mechanism for the result that online advertising is more effective in places where out-of-home advertising is banned. Specifically, we examine the possibility that media bans reduce the likelihood that consumers will be saturated with media images and that therefore online advertising will be more effective in these places. As Ackerberg (2001) points out, the effectiveness of advertising may differ depending on whether consumers are aware of the products. To understand the role of awareness, we divided the products advertised in our sample in two ways: based on stated awareness of people in our sample who had not seen the ads,[7] and based on whether the product is a new product. Table 11 presents the results for the state-level variation in regulation. The effect is far stronger for new products and for products that had below-average awareness levels. For products that had above-average awareness levels, we no longer find a significant effect of ad bans or even for advertising exposure.

Table 12 presents the results for combined data for municipalities where there was a change in regulation during our data. The results again suggest that the effect of the ad ban is stronger for low-awareness products. Interestingly, we do not see such a strong effect for new products or at least the estimates are statistically identical for the effect of the ban. This may be because these were markets where new products gained high awareness levels swiftly. As a robustness check we repeated the estimation in table 11 for the placebo categories of Junk Food, CPG products and other beverages that we observed examined in Table 8 in the appendix in Table A-3. The results for the key coefficients were either insignificant or of the wrong sign.

One explanation for these results is that media saturation levels through 'at home' media

[7]As might be expected, mean awareness levels are lower in states with advertising bans. 70 percent of respondents said they knew about the product in states that had no advertising ban, and 67 percent of respondents said they knew about the product in states that had bans (The difference is statistically significant at the $p<0.001$ level.). We split the sample at average awareness levels for the products.

Table 11: Awareness affects impact of online advertising in advertising ban states

	(1) High Awareness	(2) Low Awareness	(3) Old Product	(4) New Product
Exposed × Ad Ban	0.0194	0.0974**	0.0476*	0.119**
	(0.0349)	(0.0403)	(0.0288)	(0.0470)
Exposed	0.0330	0.107***	0.0572***	0.0557*
	(0.0245)	(0.0293)	(0.0212)	(0.0338)
Female	-0.0306	0.222***	-0.0368	0.267***
	(0.0340)	(0.0256)	(0.0254)	(0.0277)
Std. Internet Hours	0.0351**	0.0663***	0.0481***	0.0816***
	(0.0152)	(0.0193)	(0.0155)	(0.0221)
Std. Income	-0.0225**	-0.0167	-0.0201*	0.00238
	(0.0109)	(0.0177)	(0.0106)	(0.0190)
Std. Age	-0.0592***	-0.294***	-0.106***	-0.268***
	(0.0160)	(0.0188)	(0.0156)	(0.0257)
State Fixed Effects	Yes	Yes	Yes	Yes
Campaign Fixed Effects	Yes	Yes	Yes	Yes
Observations	30671	30908	40362	21218
Log-Likelihood	-19045.9	-16632.3	-23403.2	-12380.8

Logit estimates. Robust standard errors clustered at the state level. * $p < 0.10$, ** $p < 0.05$,*** $p < 0.01$.

'Ad Ban' is collinear with state fixed effects and omitted.

such as television and radio are already high enough for older, high-awareness products so that a ban on offline media makes little difference. Online advertising can only overcome the effects of offline media bans when the media bans have an effect in the first place. This result is consistent with our main result being driven by diminishing marginal effectiveness of advertising goodwill because products with high awareness already have steep diminishing returns to incremental advertising.

6 Implications

This paper uses field experiment data on alcohol advertising to show that online display advertising has the largest impact in locations with restrictions on out-of-home advertising. We interpret this to suggest that the online advertising substitutes for the banned offline ads, thereby reducing the effectiveness of local advertising bans. Many authors have argued that the internet reduces the effectiveness of local regulations, but outside of tax policy there has been little systematic empirical examination of this phenomenon. Our results contribute to this literature, showing how the internet allows firms and consumers to circumvent offline restrictions. While the alcohol advertising bans may still achieve their intended purpose of

Table 12: Awareness affects impact of online advertising in locations with changes in advertising bans

	(1) High Awareness	(2) Low Awareness	(3) Old Product	(4) New Product
Purchase Intent				
Exposed X Ad Ban X Affected Group	0.284	1.123***	0.747***	0.542
	(0.339)	(0.338)	(0.269)	(0.391)
Exposed X Ad Ban	-0.102	-0.499**	-0.246	-0.483**
	(0.178)	(0.220)	(0.164)	(0.241)
Exposed X Affected Group	-0.130	-0.713***	-0.340	-0.418
	(0.281)	(0.260)	(0.224)	(0.380)
Exposed × Ad Ban	-0.197	-0.188	-0.135	-0.235
	(0.161)	(0.181)	(0.128)	(0.269)
Exposed	0.0873	0.449***	0.159	0.449**
	(0.159)	(0.174)	(0.135)	(0.208)
Ad Ban	-0.0707	0.381**	-0.0392	0.562***
	(0.143)	(0.169)	(0.119)	(0.184)
Ad Ban X Affected Group	0.0405	-0.617**	-0.113	-0.426
	(0.347)	(0.295)	(0.227)	(0.317)
Campaign Fixed Effects	Yes	Yes	Yes	Yes
Demographics	Yes	Yes	Yes	Yes
County fixed effects	Yes	Yes	Yes	Yes
Observations	3649	4155	5486	2371
Log-Likelihood	-2170.7	-2289.9	-3117.9	-1376.6

Logit estimates. Robust standard errors clustered at the survey level. * $p < 0.10$, ** $p < 0.05$,*** $p < 0.01$.

reducing the exposure of school children to offline alcohol ads, our results suggest these bans will be relatively ineffective for regular internet users.

Our results also have important implications for managers trying to evaluate the merits of combining online and offline campaigns. They can be viewed as an unusual natural experiment which allows empirical analysis to tease apart the relationship between the presence of offline advertising and the effectiveness of online advertising. Usually such analysis is problematic because the launch of both types of campaigns is contemporaneous, making it hard to tease apart the separate effects of either or how they interact. As discussed by Silk et al. (2001), it has been suggested that online advertising could potentially complement and enhance the effectiveness of offline campaigns. However, our results suggest that instead offline and online advertising appear to be substitutes. Though the majority of our results focus on the effect of outdoors advertising on banner advertising, the fact that our findings appear to extend to both TV and print media in select markets suggests this result is likely robust across media types.

There are of course limitations to our study that suggest potential avenues for future research. First, we rely on stated expressions of purchase intent and not actual purchase data. While these measures allow us to determine that online advertising is more effective in places with out-of-home advertising bans, the use of these measures means that we are unable to define the precise impact of the internet on increasing alcohol sales in places with bans relative to places without. Second, our study focuses on alcohol advertising bans, and it is possible that bans on different products may have different effects. Third, our results are specific to the study of state-specific bans on offline media in the United States. We do not study attempts by national governments or international regulatory bodies to enforce bans on advertising on the internet. It is therefore not clear the extent to which our results apply more broadly to national bans on all alcohol advertising, such as the ban imposed in Sweden, or how our results would apply to laws such as the EU 'Tobacco Advertising Ban' that attempt to restrict internet advertising directly.

A puzzle that we leave for future research is why what appears to be out-of-equilibrium behavior persists. If internet advertising has greater returns in states with ad bans, it is an open question whether alcohol advertising campaign managers are aware of this and, if they are aware, why they do not act upon it by attempting to advertise on geographically targeted websites. One potential explanation is that because alcohol advertising is monitored and tracked by interest groups and governments, alcohol advertising firms have not wanted to court censure by appearing to use the internet to circumvent local jurisdictions. It would be interesting to explore whether this is in fact the case.

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Appendix

A Further Robustness Checks

Column (1) of Table A-1 presents results of a linear probability model. The results are very similar to the logit specification presented in Table 3. This provides more evidence that, despite the concerns expressed by Ai and Norton (2003) about the interpretation of interaction coefficients in non-linear models, our results remain robust.

We then check that our results are robust to dropping states where the laws apply predominantly to dry counties. Because the decision to live in a dry county is endogenous, and related to attitudes to the consumption of alcohol, we also reran our regressions excluding these dry counties. Column (2) of Table A-1 presents the results, which are similar to before.

In our main specifications, we include laws that ban all billboards, not just alcohol advertising. This is the case for Vermont, Maine, Hawaii and Alaska. We show that our results are robust to excluding these states (Column (3) of Table A-1). Their exclusion does not appear to affect our results.

There is also the potential concern that wine may have a slightly different target market from liquor and beer. Column (4) of Table A-1 shows that our results are robust to the exclusion of wine.

Column (5) of Table A-1 shows the robustness of our results to the exclusion of people who saw the ads multiple times either because they refreshed the webpage or because they returned to the webpage later after navigating away from it within the site. If anything, our results are stronger (though slightly less precise) when we exclude these multiply treated individuals. This suggests that out-of-home ad bans have the largest impact on the effectiveness of ads that are only seen once, rather than multiple times. In other words, they make the 'first impression' from an online ad more effective.

Table A-2 shows that are main results are similar to Tables 8 and 11 when we use purchase intent scale as our dependent measure in a linear regression.

Table A-1: Robustness Checks

	Linear Probability (1) Purchase Intent	Dry State (2) Purchase Intent	Excluding No Billboard (3) Purchase Intent	Excluding Wine (4) Purchase Intent	Exposed Once (5) Purchase Intent
Exposed × Ad Ban	0.0127**	0.0605**	0.0674**	0.0641**	0.0608*
	(0.00539)	(0.0267)	(0.0272)	(0.0283)	(0.0336)
Exposed	0.0139***	0.0706***	0.0678***	0.0674***	0.0445*
	(0.00319)	(0.0160)	(0.0161)	(0.0169)	(0.0238)
Female	0.0180***	0.0868***	0.0926***	0.0908***	0.0677***
	(0.00366)	(0.0185)	(0.0184)	(0.0190)	(0.0205)
Std. Internet Hours	0.00955***	0.0479***	0.0468***	0.0479***	0.0497***
	(0.00251)	(0.0125)	(0.0125)	(0.0128)	(0.0127)
Std. Income	-0.00409**	-0.0184*	-0.0195*	-0.0195*	-0.0203*
	(0.00197)	(0.00968)	(0.00998)	(0.0107)	(0.0108)
Std. Age	-0.0323***	-0.167***	-0.167***	-0.184***	-0.155***
	(0.00278)	(0.0156)	(0.0156)	(0.0163)	(0.0141)
State Fixed Effects	Yes	Yes	Yes	Yes	Yes
Campaign Fixed Effects	Yes	Yes	Yes	Yes	Yes
Observations	61580	60526	60672	58981	47026
Log-Likelihood	-37648.5	-35316.7	-35435.0	-34475.3	-27084.4
R-Squared	0.0461				

Robust standard errors clustered at the state level. * $p < 0.10$, ** $p < 0.05$,*** $p < 0.01$.
'Ad Ban' is collinear with state fixed effects and omitted.

As a robustness check we repeated the estimation in Table 11 for the placebo categories of Junk Food, CPG products and other beverages that we observed examined in Table 8 in Table A-3. The results for the key coefficients were either insignificant or of the wrong sign.

Table A-2: Replicating results using the full purchase intent scale

	(1) High Sugar Foods	(2) Non-Alcoholic Beverages	(3) All Food CPG	(4) High Awareness	(5) Low Awareness	(6) Old Product	(7) New Product
Exposed × Ad Ban	0.000752	-0.0339	-0.0130	0.0187	0.0577^{***}	0.0319	0.0797^{**}
	(0.0219)	(0.0321)	(0.00920)	(0.0253)	(0.0182)	(0.0203)	(0.0331)
Exposed	0.0428^{***}	0.0652^{***}	0.0503^{***}	0.0285	0.0613^{***}	0.0401^{***}	0.0330
	(0.0147)	(0.0222)	(0.00587)	(0.0177)	(0.0154)	(0.0130)	(0.0240)
Female	0.0713^{***}	0.127^{***}	0.129^{***}	-0.0482^{**}	0.105^{***}	-0.0647^{***}	0.170^{***}
	(0.0177)	(0.0213)	(0.00827)	(0.0226)	(0.0171)	(0.0170)	(0.0239)
Std. Internet Hours	0.0444^{***}	0.0372^{***}	0.0366^{***}	0.0184^{*}	0.0495^{***}	0.0269^{**}	0.0604^{***}
	(0.00846)	(0.0108)	(0.00416)	(0.0108)	(0.0128)	(0.0116)	(0.0147)
Std. Income	-0.0711^{***}	-0.0215^{*}	-0.0497^{***}	-0.0313^{***}	-0.0225^{**}	-0.0260^{***}	-0.0156
	(0.0104)	(0.0109)	(0.00489)	(0.00792)	(0.00919)	(0.00685)	(0.0103)
Std. Age	-0.0595^{***}	-0.105^{***}	-0.0404^{***}	-0.0703^{***}	-0.216^{***}	-0.0962^{***}	-0.230^{***}
	(0.00955)	(0.0109)	(0.00624)	(0.0122)	(0.00987)	(0.00975)	(0.0174)
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Campaign Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	45310	23983	194351	30671	30909	40362	21218
Log-Likelihood	-78846.0	-42109.3	-340048.1	-56118.7	-53820.0	-72681.7	-37404.7
R-Squared	0.116	0.161	0.171	0.0447	0.0667	0.0446	0.0827

Robust standard errors clustered at the state level. * $p < 0.10$, ** $p < 0.05$,*** $p < 0.01$. 'Ad Ban' is collinear with state fixed effects and omitted.

Table A-3: Results when repeating specification in Table 11 for alternative categories.

	Junk Food				Beverages				CPG Category			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	High Awareness	Low Awareness	Old Product	New Product	High Awareness	Low Awareness	Old Product	New Product	High Awareness	Low Awareness	Old Product	New Product
Exposed × Ad Ban	0.00848	-0.0623	0.0305	-0.0762	0.00313	0.0146	0.0962	-0.204*	-0.00394	-0.0141	0.0305	-0.122**
	(0.0534)	(0.0776)	(0.0484)	(0.0930)	(0.0829)	(0.102)	(0.0715)	(0.112)	(0.0265)	(0.0358)	(0.0236)	(0.0483)
Exposed	0.0634**	0.0593	0.0559*	0.112**	0.0649	0.101*	0.0482	0.145**	0.0647***	0.0816***	0.0527***	0.142***
	(0.0312)	(0.0497)	(0.0317)	(0.0547)	(0.0408)	(0.0612)	(0.0391)	(0.0660)	(0.0155)	(0.0231)	(0.0154)	(0.0298)
Female	0.203***	0.0801	0.198***	0.110*	0.223***	0.339***	0.302***	0.265**	0.204***	0.325***	0.265***	0.271***
	(0.0313)	(0.0551)	(0.0353)	(0.0651)	(0.0691)	(0.0504)	(0.0512)	(0.119)	(0.0215)	(0.0308)	(0.0211)	(0.0431)
Std. Internet Hours	0.0533***	0.105***	0.0261	0.101***	0.0337	0.111***	0.0546**	0.0853**	0.0458***	0.0631***	0.0369***	0.0773***
	(0.0166)	(0.0239)	(0.0210)	(0.0357)	(0.0237)	(0.0309)	(0.0219)	(0.0402)	(0.00870)	(0.0110)	(0.00955)	(0.0193)
Std. Income	-0.0470***	-0.122***	-0.0574***	-0.0732**	-0.00137	-0.0327	-0.00402	-0.0317	-0.0443***	-0.0624***	-0.0450***	-0.0465***
	(0.0165)	(0.0272)	(0.0170)	(0.0284)	(0.0224)	(0.0312)	(0.0240)	(0.0264)	(0.00938)	(0.0122)	(0.00901)	(0.0158)
Std. Age	-0.0610**	-0.0409	-0.0550**	-0.0979**	-0.124**	-0.102***	-0.0735**	-0.274***	-0.0176	-0.0549***	-0.0156	-0.101***
	(0.0242)	(0.0330)	(0.0270)	(0.0385)	(0.0525)	(0.0347)	(0.0294)	(0.0771)	(0.0127)	(0.0164)	(0.0117)	(0.0275)
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Campaign Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	31549	13356	32257	12654	13275	10571	16170	7676	120561	72769	149419	43922
Log-Likelihood	-19481.9	-8330.0	-20134.8	-8009.5	-8537.2	-5118.1	-9249.7	-4488.4	-74617.1	-41066.3	-90944.4	-26185.0

Robust standard errors clustered at the state level. * $p < 0.10$, ** $p < 0.05$,*** $p < 0.01$. ‘Ad Ban’ is collinear with state fixed effects and omitted.

B Sources of Laws

Table B-1: Sources of State and Municipal Laws

Alabama	Section 28-3-16 - Advertising of alcoholic beverages. (Acts 1936-37, Ex. Sess., No. 66, p. 40; Code 1940, T. 29, 12; Acts 1978, No. 434, p. 442.)
Alaska	Sec 19.25.075 - Blanket Ban on billboards on highways
California	California Business and Professions Code Sections 25612.5(c)(7), 25617
Baltimore	Enacted 1994, Upheld by Fourth Circuit US Court of Appeals Penn Advertising of Baltimore, Inc. v. Mayor and City Council of Baltimore, No. 94-2141, 63 F.3d 1318 (4th Cir 1995)
Hawaii	264-72 Control of outdoor advertising (Blanket ban on billboards on highways)
Idaho	23-607 23-607. ADVERTISING.
Kansas	Signs, advertising and other promotional activities Background. K.S.A. 41-714 was amended by the Legislature in 2005 to remove all of the statutory restrictions on advertising and other promotional activities. Instead, the Legislature delegated to the Secretary of Revenue the power to regulate liquor advertising and other promotion activities by administrative regulation. [Subsection (b) of K.S.A. 41-714]
Kentucky	244.540 History: Recodified 1942 Ky. Acts ch. 208, sec. 1, effective October 1, 1942, from Ky. Stat. sec. 2554b-208
Maine	Title 23 Chapter 15: Protection of Highways Subchapter 1: Signs and Markets Article 1 23 S1153 (Blanket ban on billboards on highways)
Missisippi	Section 67-1-85. Regulation of advertising and display of alcoholic beverages. Sources: Codes, 1942, Section 10265-33; Laws, 1966, ch. 540, Section 33; Laws, 1968, ch. 592, Section 1; Laws, 1971, ch. 350, Section 1; Laws, 1982, ch. 419; Laws, 1986, ch. 450, Section 1; Laws, 1988, ch. 562, Section 2; Laws, 1990, ch. 569, Section 5, eff from and after passage (approved April 9, 1990)., Also: 'Chapter 02 Advertising 100 No person, firm or corporation shall originate advertisements in dry counties of this State, pursuant to Miss. Code Ann. 67-1-1, 67-1-13, 67-1-15 and 67-5-5.
New Hampshire	Source. 1990, 255:1. 1991, 355:59. 1992, 195:2. 1996, 275:27, eff. June 10, 1996. 2003, 231:27, 28, eff. July 1, 2003.
Ohio	4301:1-1-44 Advertising. History: Eff. 7-5-50; 9-20-84; 4-16-88; 1-10-99; 7-1-01; Rescinded and reenacted eff. 6-4-04 Rule promulgated under: RC Chapter 119. Rule authorized by: RC 4301.03(B), 4301.03(E) Rule amplifies: RC 4301.03(B), 4301.03(E), 4301.22, 4301.24 R.C. 119.032 review dates: 04/01/2009
Oklahoma	Okla. Const. Article XXVIII , Section 5 or 37 O.S. 516 (1981)
City Of Philadelphia	17-110. Passed by the City Council on December 4, 2003. The Bill was Signed by the Mayor on December 18, 2003.
Texas	Section 108.52. Acts 1977, 65th Leg., p. 521, ch. 194, Section 1, eff. Sept. 1, 1977. Amended by Acts 1979, 66th Leg., p. 501, ch. 231, Section 2, eff. Aug. 27, 1979; Acts 1997, 75th Leg., ch. 62, Section 1, eff. Sept. 1, 1997
Utah	32A-12-401.Amended by Chapter 314, 2003 General Session
Vermont	Sec. 4. 7 V.S.A. Section 666: Approved: June 3, 1994