Online Appendix A - Generalization (Massachusetts tobacco bans)

Tobacco bans in Massachusetts

According to Massachusetts Association of Health Boards[1] (MAHB), as of June 5^{th} 2018, 160 municipalities in Massachusetts had imposed bans on tobacco sales in pharmacies. These regulations were passed at different points in time. In our data set we have 317 drugstores in Massachusetts that belong to two national chains, a subset of 198 were affected by these regulations at different points in time during 2008-2016 period.

Previous research by DellaVigna and Gentzkow (2017), and Hitsch et al. (2019) has verified that prices within the same retail chain across the same market are fairly homogeneous and most of the variation in prices is a result of dispersion across chains within the same market and across different markets. This suggests that marketing mix variables at large retail chains are determined at larger geographic aggregates such as states or designated marketing areas (DMA) rather than at the individual store level, and therefore, the timing of tobacco regulations that affect stores of national pharmacies within individual cities can be taken to be exogenous to each individual store after controlling for county-time fixed effects. Note that advertising intensity varies at the DMA level, and excise taxes in categories such as sugar or tobacco may vary at the county level. Therefore, county-time fixed effects absorb the effect of confounds such as advertising, excise taxes, or other local regulations that vary at the county level.

We investigate the effect of these restrictions on total non-tobacco revenue and total revenue generated within each of the product groups as defined by AC Nielsen. The following difference-in-differences identification strategy is used to determine the effect of the regulation:

$$\log y_{it} = \alpha.\delta_{it} + \eta_i + \eta_{c_i t} + \eta_{r_i d_i t}, \tag{1}$$

where y_{it} is an outcome of interest for store i during week t such as total non-tobacco revenue or total sales in a given product group. The treatment effect is denoted by α and the treatment dummy δ_{it} turns on when the regulation goes into effect for store i. We use an extensive set of fixed effects to absorb confounds that may vary at store level (η_i), local demand trends or policies that may vary county-week level ($\eta_{c_i t},$) and changes in marketing mix decisions at the retail chain-DMA-week level ($\eta_{r_i d_i t}$).

Our identification strategy is to leverage the variation in times at which these regulations went to effect, i.e., using the variation in the times at which δ_{it} dummies turn on for stores in different municipalities.

[1] http://mahb.org/tobaccomaps/

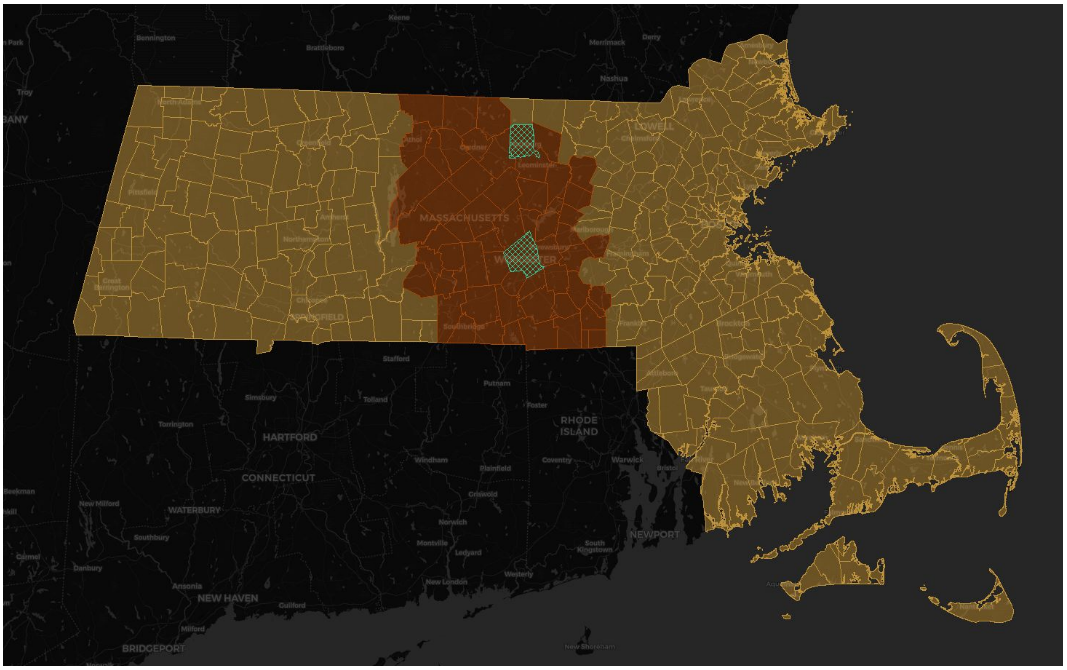


Figure A1: Massachusetts state is plotted in yellow, Worcester county in red along with Fitchburg and Worcester municipalities located within Worcester county that are hashed in green.

We account for county-time fixed effects, so the identification is based on variation within municipalities in counties. For instance, in Figure A1, we have plotted Worcester county in Massachusetts state, and hashed two municipalities, namely Worcester (lower) and Fitchburg (upper), in green that have both implemented pharmacy tobacco bans. However, regulations in Worcester went to effect on 24-06-2011, whereas Fitchburg pharmacy bans were implemented on 01-06-2012. These types of variations enable us to disentangle local time trends, and other county and state regulations that were concomitant with these pharmacy tobacco bans.

Table A1 shows the results of estimating equation (1) using the log total non-tobacco revenue as the dependent variable. Column (1) provides estimates from a regression with no fixed effects or controls. Column (2) adds store-month fixed effects, column (3) additionally adds in chain-DMA-week fixed effects and column (4) presents the full specification which additionally includes the county-week fixed effects. Column (1) shows a statistically significant and positive treatment effect. This could be due to the fact that these pharmacy tobacco bans were first adopted by larger municipalities like Boston that include larger stores. Therefore, the treatment dummy is turned on earlier for larger stores, which creates a positive treatment coefficient. The addition of store fixed effects flips the sign of the point estimate by absorbing the variation in store sizes and makes it insignificant. Including the chain-DMA-week fixed effects amplifies the point estimate, but the estimate remains insignificant. The full specification returns a negative and statistically significant coefficient at $P < 0.05$, which implies that the Massachusetts tobacco bans led to an average $1 - \exp(0.025) = 2.5\%$ loss in the revenue generated by non-tobacco products for pharmacies.

Table A1: The effect of pharmacy tobacco bans imposed by municipalities in Massachusetts on the total non-tobacco revenue generated by pharmacies.

	Dependent variable:			
	log(tot.sales)			
	(1)	(2)	(3)	(4)
Treatment dummy	0.143***	−0.007	−0.018*	−0.025**
	(0.044)	(0.009)	(0.011)	(0.011)
Constant	10.200***			
	(0.031)			
Store FE		X	X	X
Chain-DMA-Week FE			X	X
County-Week FE				X
Observations	136,777	136,777	136,777	136,777
R^2	0.014	0.922	0.946	0.957
Adjusted R^2	0.014	0.922	0.946	0.953
Residual Std. Error	0.555 (df = 136775)	0.156 (df = 136459)	0.130 (df = 135525)	0.121 (df = 124763)

Note: *p<0.1; **p<0.05; ***p<0.01

All standard errors are clustered at store level.

Impact on product groups: Massachusetts ban

To further assess the generalizability of our findings, we use the difference-in-differences identification strategy for the Massachusetts ban in (1) to study the cross-category impact of these regulatory bans on individual categories. As we mentioned in Section 4.5, studying the impact of this policy on individual product groups can be challenging. Specifically, some stores may not offer certain categories or nonzero weekly sales of certain product groups can be very sparse which may bias the OLS estimates if one includes the zero weekly sales for those stores. If our identification strategy is credible and the effects we obtained previously are driven by smokers migrating to rival chains because EC stops selling tobacco, then incumbent chains are expected to see gains at the product category level. Further, we would expect that if tobacco exits have a similar effect on non-tobacco sales across markets, then product groups that yield positive effects in the IV model (i.e., gains by non-EC stores) are expected to have negative estimates for the pharmacy chains prevented from selling tobacco in Massachusetts according to the difference-in-differences model.

We re-estimated the difference-in-differences model in (1) using total revenue for each product group as the dependent variable. Consistent with our IV regressions, for each product group, we only considered those stores that had nonzero sales throughout the time period that they were present in the data. Furthermore, we only kept those product groups that had at least 30 stores that satisfy the above condition which left us with 73 product groups. The 95% confidence intervals for the treatment coefficient estimates of the full specification is depicted in Figure A2.

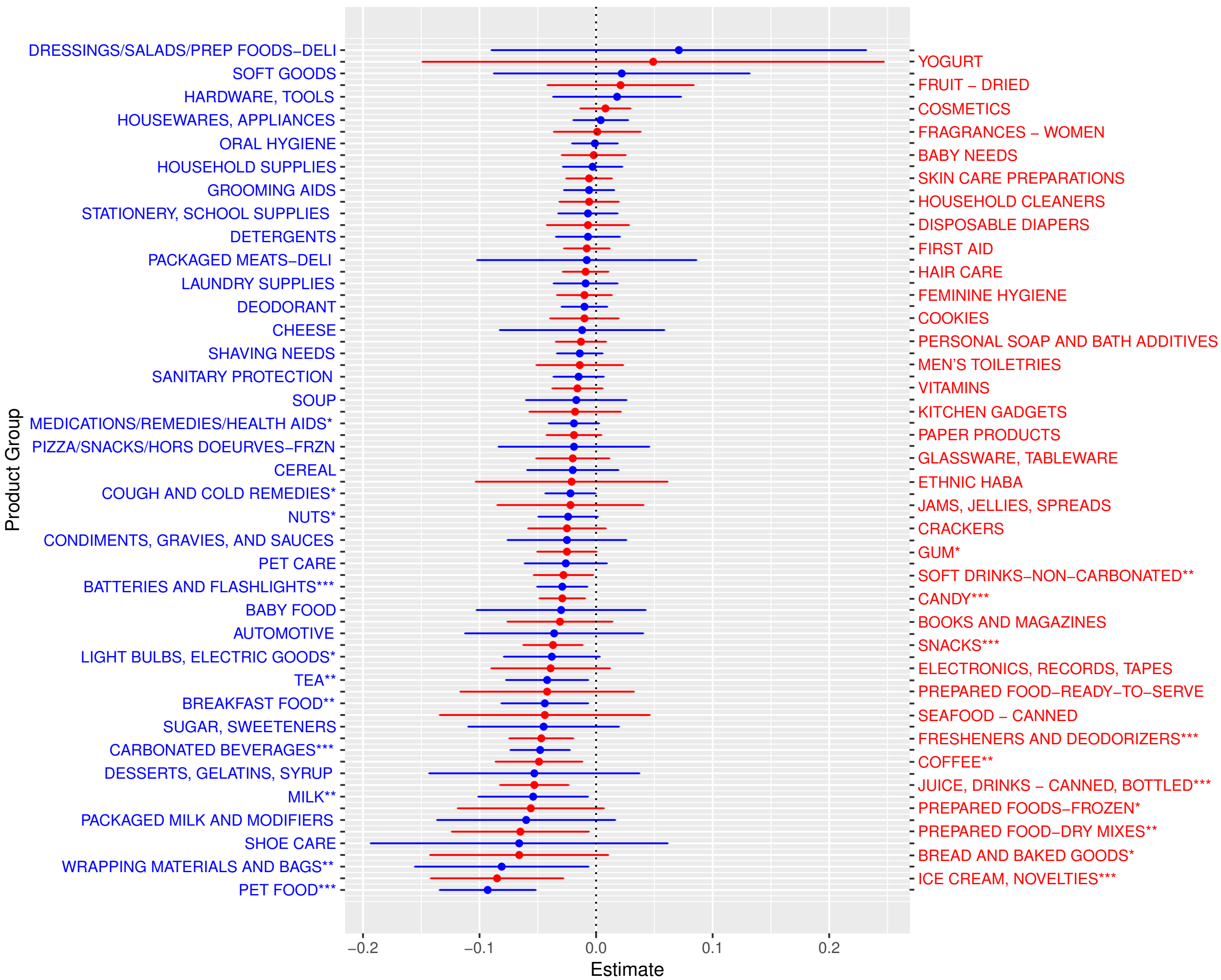


Figure A2: The impact of Massachusetts tobacco ban on the revenue generated by individual product groups. The intervals represent the 95% confidence interval. The majority of point estimates are negative and none of the positive point estimates were significant at $p < 0.10$. The names of corresponding product groups are reported on the sides and colors are only used for ease of exposition.
Note: *p<0.1; **p<0.05; ***p<0.01.

Comparing Figures 6 and A2 reveals that both models detect effects in similar product groups including soft drinks, carbonated beverages, candy, snacks, and milk. Interestingly, our IV model also yields statistically significant coefficients for alcoholic beverages including beer, liquor, and wine, which are frequently co-purchased with tobacco. The difference-in-differences model fails to identify this relationship because pharmacies are prohibited from selling alcoholic beverages in Massachusetts.

Online Appendix B - Extended Panel Data Analysis

In this appendix we take a closer look at the panel data to address some of the concerns that the readers may have with the analysis provided in Section 5. In particular, our analysis here tries to address the following concerns:

- Some of the variables studied in Section 5 are intrinsically discrete or are positive, therefore, in that section we aggregated the observations and used $log(1+y)$ to include periods where the outcome was equal to zero. To allay biases that censored observations with value zero may cause we use the full panel data and account for these limitations by using Bayesian estimation and proper distribution assumptions e.g., Poisson or truncated normal.
- One may be concerned about unobserved heterogeneity in our panel analysis. This concern was address before by including household fixed effects in OLS. However, including household fixed effects in Poisson or logit models to account for unobserved heterogeneity is both computational difficult and leads to biased estimates, see Greene (2004).

To address these issues we use a hierarchical Bayesian model to account for both unobserved heterogeneity and the limited domain of dependent variables. We demonstrate that our findings in Section 5 are robust to these assumptions and the Bayesian estimates are consistent with the aggregate panel data analysis presented before.

Store vs. category loyalty

To demonstrate the robustness of our analysis to distributional assumption and the methodology used in Section 5.2, we use two sets of hierarchical Bayesian regression models to measure the effects on both basket size, and frequency of trips. Consider the following specifications:

$$\begin{aligned} V_{ht} &= \text{Poisson}(\lambda^*_{ht}), \\ \log(\lambda^*_{ht}) &= \alpha_h + \delta_h \left(\mathbb{1}_{t>t^*}\right)\left(\mathbb{1}_{h\in\mathcal{T}}\right) + \tau_{m_t} + \theta \log\left(1+\bar{E}_{ht}\right) + \gamma \log\left(1+\bar{V}_{ht}\right), \end{aligned} \tag{2}$$

and

$$\log(E_{ht_h}) = \alpha_h + \delta_h \left(\mathbb{1}_{t_h>t^*}\right)\left(\mathbb{1}_{h\in\mathcal{T}}\right) + \tau_{m_{t_h}} + \theta \log\left(1+\bar{E}_{ht_h}\right) + \gamma \log\left(1+\bar{V}_{ht_h}\right), \tag{3}$$

where h, t, and t_h index household h, week t, and the t_h^{th} purchase occasion for household h, respectively. V_{ht}, and E_{ht_h} denote the number of trips by household h during week t that included at least one non-tobacco product, and total non-tobacco expenditure of household h in the t_h^{th} trip[2]. $\mathcal{T}$ is the set of households in the treatment group, and the treatment was delivered at $t^* = $ 09-01-2014. $\bar{E}_{ht_h}$, and $\bar{V}_{ht}$ are the total expenditure and visits for household h across all stores during the six weeks prior to week t or occasion t_h. These parameters aim at controlling for income effects and transportation costs for each household. Finally, m_t represents the month of occasion t.

The following prior structure is imposed on the parameters:

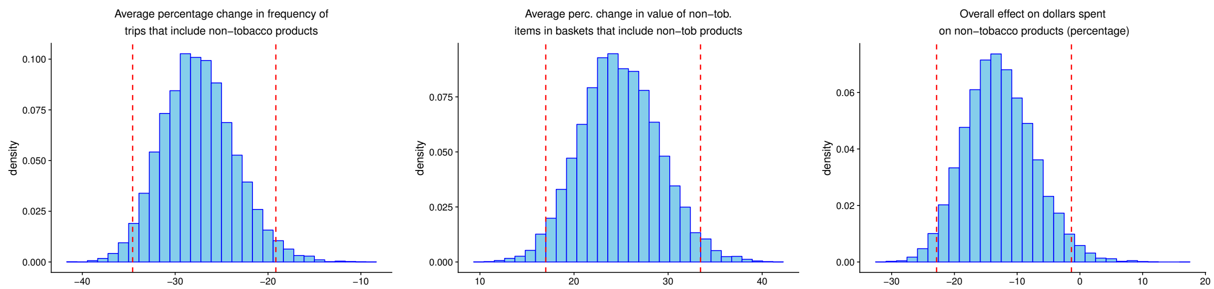
$$\begin{aligned} \alpha_h &\sim \mathcal{N}(\bar{\alpha}_h + \bar{\pi}\mathbb{1}_{h\in\mathcal{T}}, \sigma_\alpha^2), \\ \delta_h &\sim \mathcal{N}(\bar{\delta}_h + \eta \log C_h + \psi \tilde{C}_h, \sigma_\delta^2), \\ \bar{\alpha}_h, \bar{\delta}_h, \bar{\beta}, \eta, \psi, \theta, \gamma, \tau_m &\sim \mathcal{N}(0, 10000), \qquad \forall\, m \in \{2, \dots, 60\} \\ \sigma_\alpha, \sigma_\delta &\sim Unif(0, 100), \end{aligned} \tag{4}$$

where α_h aims at capturing unobserved heterogeneity using a hierarchical model while allowing for the treatment and control groups to have a mean difference by adjusting $\bar{\pi}$. The effect of EC tobacco ban (δ) is allowed to vary by unobservables through $\bar{\alpha}_h$, the total tobacco consumption in the first half of the panel (C_h) and the share of tobacco purchases from EC stores ($\bar{C}_h$). To capture common time trends across treatment and control groups we used random monthly trends (τ_m) throughout our panel. Finally, θ and γ are coefficients whose intent is to absorb distortions to income, transportation costs, or other parameters that affect frequency of shopping trips.

For each of the estimation exercises in this section, we draw 15,000 samples from the posterior distribution with a burn-in period of 5,000 samples using a Gibbs sampler implemented by the RJAGS library in R. The histogram of MCMC draws are included in Figure A7 in the Appendix. The distribution of average treatment effect (ATE) on frequency of visits, size of baskets, and the combined effect on dollar value of non-tobacco products is depicted in Figure A3. Our findings reveal that the frequency of trips that include at least one non-tobacco product were negatively affected when EC ended tobacco sales. However, the average basket size for such trips seem to have grown. Although at first glance this result may suggest that customers are compensating for fewer trips by increasing the basket size, this could merely be a selection problem. In particular, the non-tobacco expenditure in trips that include both tobacco

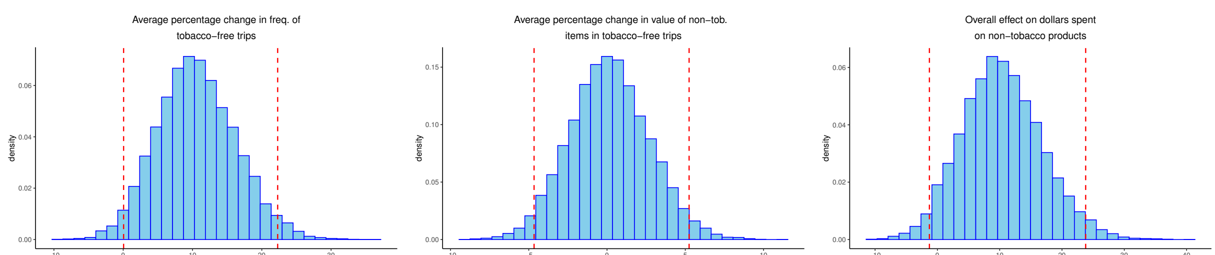
[2] We discard trips that only consist of tobacco products.

and non-tobacco products may be different from those in trips that only include non-tobacco purchases. To further examine this hypothesis we probe into tobacco-free trips and re-estimate models (2)-(3) using frequency and value of tobacco free trips as the outcome variables. The density of MCMC draws are depicted in Figures A9-A10 of the Appendix, and the distribution of ATEs on trip frequency, basket size, and the total dollar value of non-tobacco purchases are plotted in Figure A4. Interestingly, Figure A4 shows that although the basket size for tobacco-free trips is minimally affected by the policy, smokers are generating more tobacco-free trips to EC-stores.



(a) (b) (c)

Figure A3: The distribution of ATE on frequency, basket size, and overall dollar value of non-tobacco items in trips that include at least one non-tobacco product. The red dashed lines signify the 95% credibility intervals. Panel (a) shows that the frequency of trips that include at least one non-tobacco product declines by about 30%. Panel (b) illustrates the change in value of non-tobacco products in those trips. Panel (c) depicts the overall average treatment effect, accounting for both frequency and basket size effects, on dollar value of non-tobacco purchases.



(a) (b) (c)

Figure A4: The distribution of ATE on frequency, basket size, and overall dollar value of non-tobacco items in pure non-tobacco trips. The red dashed lines signify the 95% credibility intervals. Panel (a) shows that the frequency of tobacco-free trips increases by about 15%. Panel (b) demonstrates that the basket size of these trips do not seem to be affected, i.e., note that credible interval does include zero. Panel (c) depicts the distribution of average treatment effect on dollar value of non-tobacco purchases, accounting for both frequency and basket size effects. Although the 95% credibility intervals in the overall effect contain zero, our results seem to suggest that the frequency of tobacco free trips to EC stores has increased which supports our previous results and shows that customers' loyalty transcended loyalty to tobacco category.

Note that after this change all trips made to EC are tobacco-free, and our results in Figure A4 show that frequency of such trips have grown after the end of tobacco sales, while the basket size has remain unchanged. While our results in panel (c) suggest that the overall dollar value of these trips may have grown, this growth in frequency of tobacco-free trips fails to fully offset the losses caused by fewer overall trips to EC stores as demonstrated in Figure A3. Hence, the adverse effect of ceasing tobacco sales is limited to trips that include tobacco purchases and does not seem to extend beyond those trips by affecting the overall loyalty of customers. Nevertheless, these findings also highlight the critical role of tobacco in driving store patronage as the distribution of the average treatment effect on dollar value of non-tobacco purchases is negative and centered around -13.1% as depicted in Panel (c) of Figure A3.

Impact on smoking rates

In this section we provide a Bayesian counterpart to Section 5.2.1. We measure the impact of EC's decision on the frequency of tobacco trips, and quantity of tobacco conditional on purchase after controlling for transaction prices and accounting for heterogeneity through a Bayesian hierarchical model. Consider the following specifications:

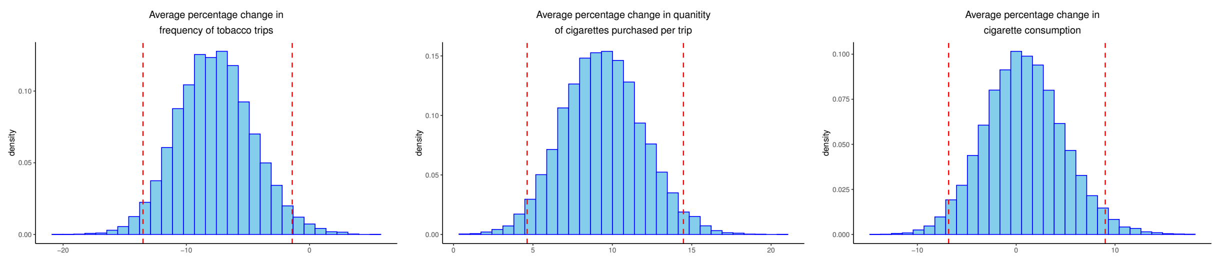
$$\begin{aligned} V_{ht}^{t} &= \text{Poisson}(\lambda_{ht}^{*}), \\ \log(\lambda_{ht}^{*}) &= \alpha_h + \delta_h \left(\mathbb{1}_{t>t^*}\right)\left(\mathbb{1}_{h\in\mathcal{T}}\right) + \beta p_{ht} + \tau_{m_t} + \theta \log\left(1+\bar{E}_{ht}\right) + \gamma \log\left(1+\bar{V}_{ht}\right), \end{aligned} \tag{5}$$

and

$$\log(E_{ht_h}^{t}) = \alpha_h + \delta_h \left(\mathbb{1}_{t_h>t^*}\right)\left(\mathbb{1}_{h\in\mathcal{T}}\right) + \beta p_{hw_{t_h}} + \tau_{m_{t_h}} + \theta \log\left(1+\bar{E}_{ht_h}\right) + \gamma \log\left(1+\bar{V}_{ht_h}\right), \tag{6}$$

where V_{ht}, and E_{ht_h} denote the frequency of trips that include cigarettes in week t, and the total number of cigarette sticks purchased during the t_h^{th} tobacco trip for household h, respectively. p_{ht} and $p_{hw_{t_h}}$ are the average weekly transaction price of tobacco for household h during week t and the week of t_h^{th} tobacco purchase occasion. For weeks where no tobacco purchase was made, the last average weekly transaction price is used for p_{ht} and $p_{hw_{t_h}}$. The rest of the parameters are defined as before in equations (2) and (3). Additionally, we have $\beta \sim \mathcal{N}(0, 10000)$.

We draw $15,000$ samples from the posterior distribution with a burn-in period of $5,000$ using a Gibbs sampler implemented in the RJAGS library in R. The posterior draws are presented in Figures A11-A12 in the Appendix. The distribution of ATE for frequency of tobacco trips, quantity of cigarettes purchased per trip, and overall cigarette consumption are depicted in Figure A5. Although our results in Figure A5(a) reveal that the frequency of tobacco trips is drastically affected by this policy, the increase in quantity of cigarettes purchased per trip, presented in Figure A5(b), fully offsets the frequency effect as demonstrated in Figure A5(c).



(a) (b) (c)

Figure A5: The impact of ending tobacco sales at EC on cigarette consumption. (a) Frequency of trips. (b) Quantity of cigarettes sold per trip. (c) Overall impact on cigarette consumption. While our results in panel (a) show that the frequency of cigarette consumption has decreased, the increase in quantity of cigarettes purchased per trip as demonstrated in panel (b) partially neutralizes the effect of lower trip frequency and reduces the effectiveness of this policy, as manifested in panel (c).

Advertising effect

To further demonstrate the robustness of our findings in Section 5.3, we estimate a set of hierarchical Bayesian models that study the impact of announcing this policy on trips made by non-smokers to drugstores. We seek to capture the heterogeneity in treatment effect across unobservables, and two important observables including income and household head education. In particular, we estimate a Bayesian hierarchical model for store choices made by non-smokers within drug store retailers, and a hierarchical linear model to examine the effect of this policy on the size of transactions made by non-smokers at pharmacies. Consider the following utility model:

$$U_{ht_h i} = \alpha_{hi} + \delta_h \left(\mathbb{1}_{t>t^*}\right)\left(\mathbb{1}_{i=EC}\right) + \epsilon_{ht_h i}, \quad i \in \{Other,\, EC\} \tag{7}$$

where h, and t_h are defined as in (3). Conditional on making a trip to a drugstore, the customer makes a decision between going to EC versus visiting another drugstore (outside option). The utility from visiting outlet i is denoted by $U_{ht_h i}$, and α_{hi} denotes the match value between household h and choice i, where $\alpha_{h,other}$ is normalized to zero. Furthermore, δ_h represents the treatment effect that turns on after $t^* = 02$-01-2014. Since many important variables that may affect the store choice problem including distance from the stores and product prices are not available for all of the stores in HomeScan data, we allow for a very flexible distribution of heterogeneity for both α_{hi}, and δ_h. In particular, we use a mixture of normals distribution with five components to capture unobserved heterogeneity, where *household income* and *education* levels enter in the upper level of hierarchy and capture the heterogeneity in treatment effect across different population groups. Finally, ϵ_{ht_hi} are independent random variables that follow a type-1 extreme value distribution, which yields the familiar multinomial logit model.

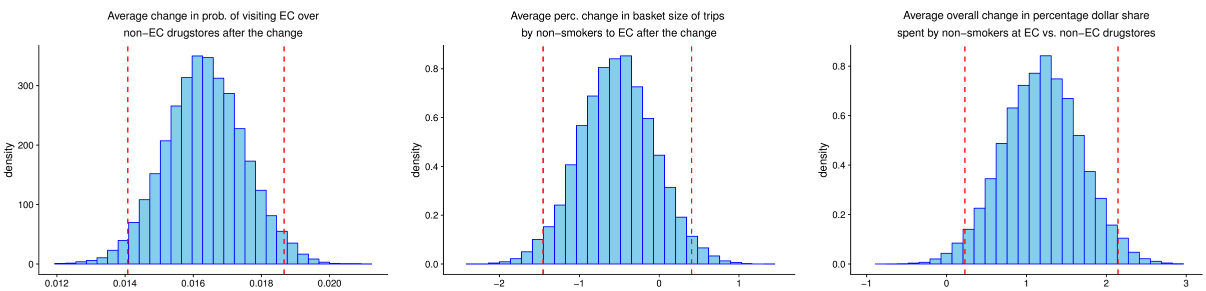
To study the effect on the dollar value of baskets at drugstores, we propose the following model:

$$\log(E^d_{ht_hi}) = \alpha_{hi} + \delta_h \left(\mathbb{1}_{t>t^*}\right)\left(\mathbb{1}_{i=EC}\right) + \tau_{m_{t_h}} + \epsilon_{ht_hi}, \quad i \in \{Other,\, EC\}, \tag{8}$$

where $E^d_{ht_hi}$ is the total expenditure made by household h during its t_h^{th} trip to drugstore $i \in \{Other,\, EC\}$. Furthermore, α_{hi} captures the heterogeneity in baseline expenditures across different options and household pairs. Similar to (7) a hierarchical model with a mixture of normals with five components is used to capture the heterogeneity in α_{hi}. Additionally, $\tau_m \quad m \in \{2, .., 12\}$ are random effects with the same hierarchical prior structure as the rest of variables, which aim at absorbing seasonality in basket sizes.

The posterior distribution for the multinomial logit model and the linear regression model were sampled $15,000$ times after a burn-in period of $5,000$ samples. The samples for the multinomial logit and the linear model were generated using the Bayesm package in R which utilize a hybrid Gibbs sampler with a RW Metropolis step and a Gibbs sampler, respectively. Figure A6 depicts the histogram of the treatment effects on visits and baskets across individuals[3]. Our results illustrate the extent of heterogeneity across individuals. The 95% credibility interval for ATE on probability of visiting EC is [1.41% 1.87%], which shows that customers favored EC to non-EC drugstores after the change. However, the ATE on the basket size reveals that on average the dollar value of baskets generated at EC-drugstores shrunk relative to other drugstores and the 95% credibility interval is [−1.45% 0.41%]. The distribution of ATE on the overall dollar share of baskets generated at EC-stores shows that the combined effect of frequency of basket size changes led to a positive effect on the dollar share of EC-drugstores. In particular, the 95% credibility interval for ATE on dollar share of EC-drugstores is [0.23% 2.15%].

[3]To sample from ATE in each of these cases, we first took a sample from the chains corresponding to each individual, and censored the top and lower 2.5% of the draws to avoid outlier draws and subsequently calculated the average treatment effect in each case.



(a) (b) (c)

Figure A6: The distribution of ATE on change in probability of visit, basket size, and overall dollar share of trips by non-smokers to EC-drugstores within their drugstore trips. The red dashed lines signify the 95% credibility intervals. Panel (a) shows that the probability of visiting EC relative to others raises by about 1.6%. Panel (b) demonstrates that new basket sizes are smaller by about 0.5%. Panel (c) demonstrates that the distribution of average treatment effect on the overall dollar share expenditure at EC-drugstores within the drugstore category for non-smokers is positive and is centered around 1.2%.

Online Appendix C - Histogram of MCMC Draws

In this Appendix, we present the histograms of Markov Chain Monte Carlo (MCMC) draws for the Bayesian models introduced in Sections -5.3.

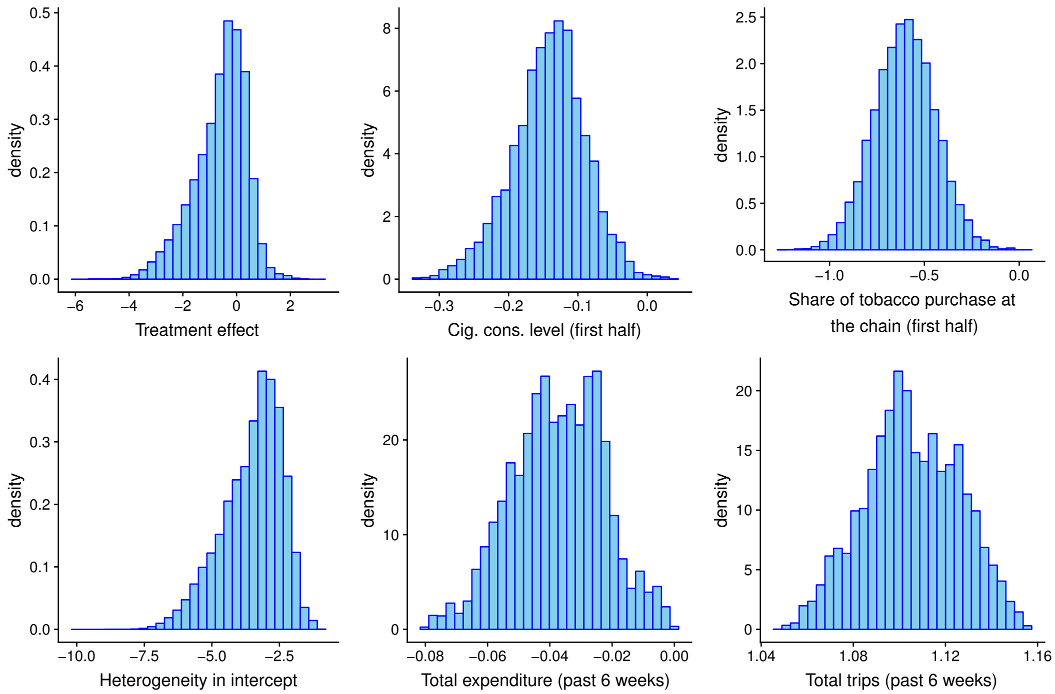


Figure A7: The density plots of the estimated parameters for model (2), where the outcome is the frequency of trips that include at least one non-tobacco purchase.

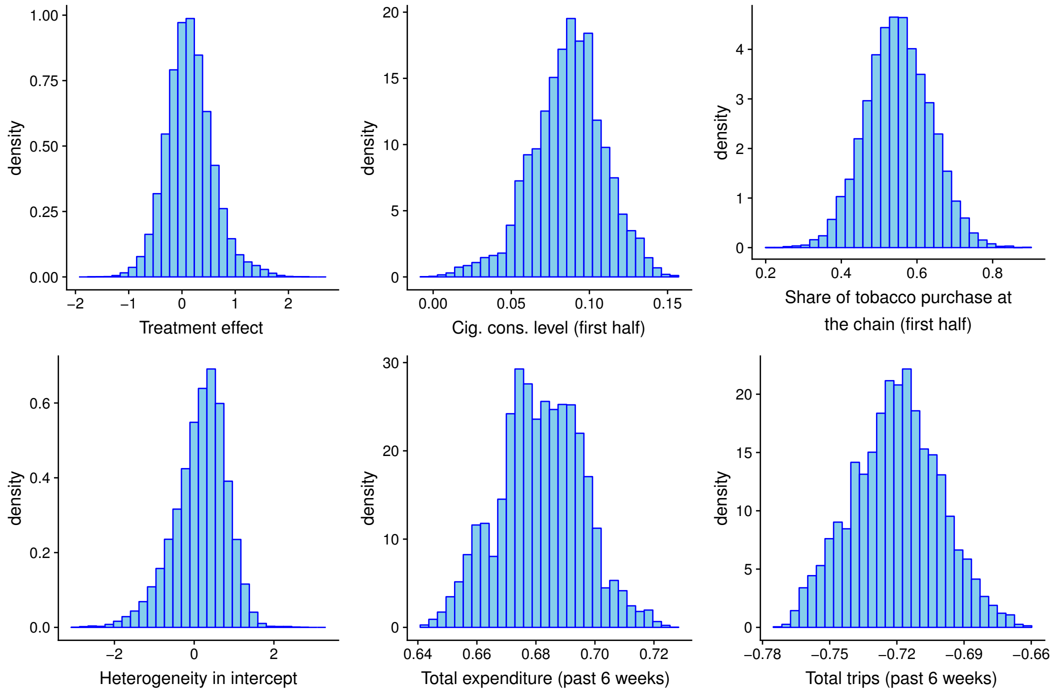


Figure A8: The density plots of the estimated parameters for model (3), where the dependent variable is the dollar value of non-tobacco purchases in trips that include at least one non-tobacco product.

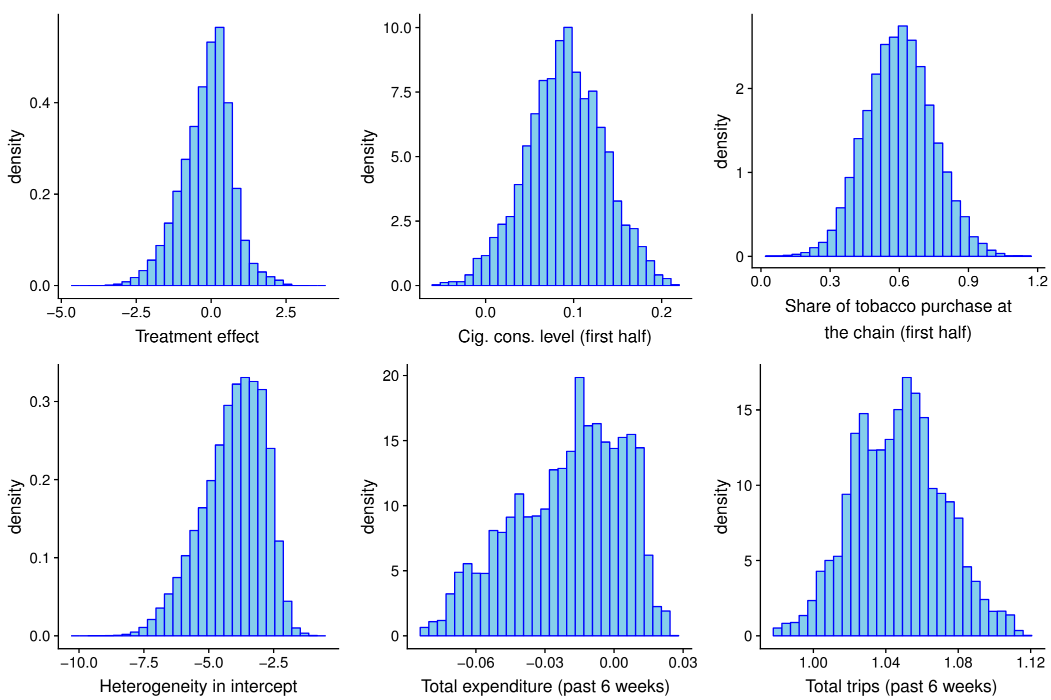


Figure A9: The density plots of the estimated parameters for model (2), where the outcome is the frequency of tobacco-free trips.

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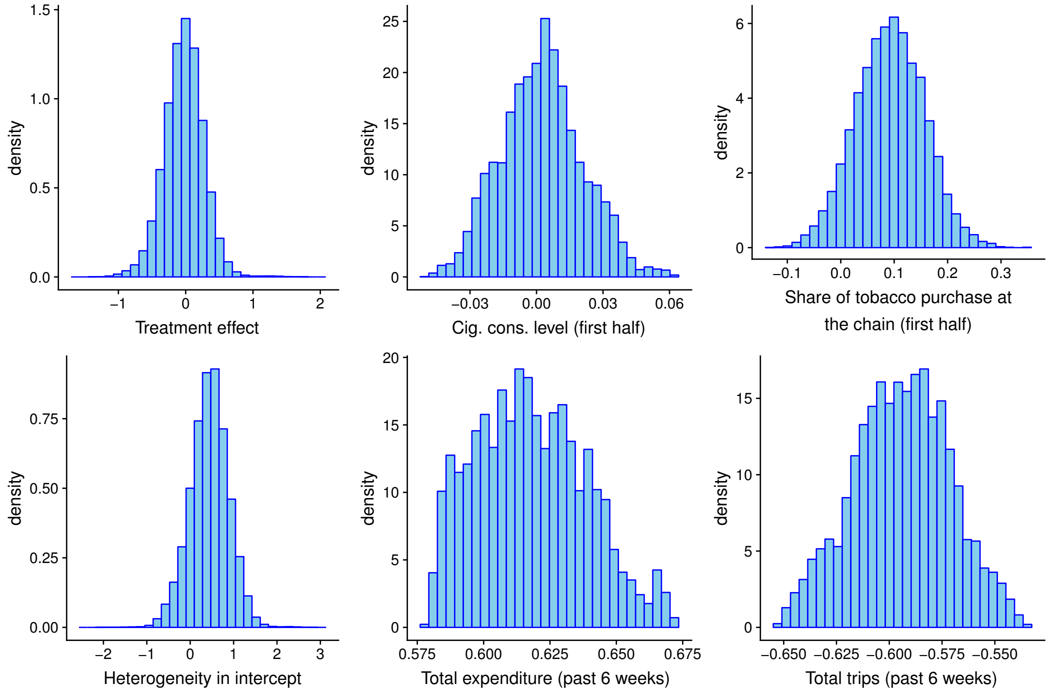


Figure A10: The density plots of the estimated parameters for model (3), where the outcome is the dollar value of tobacco-free trips.

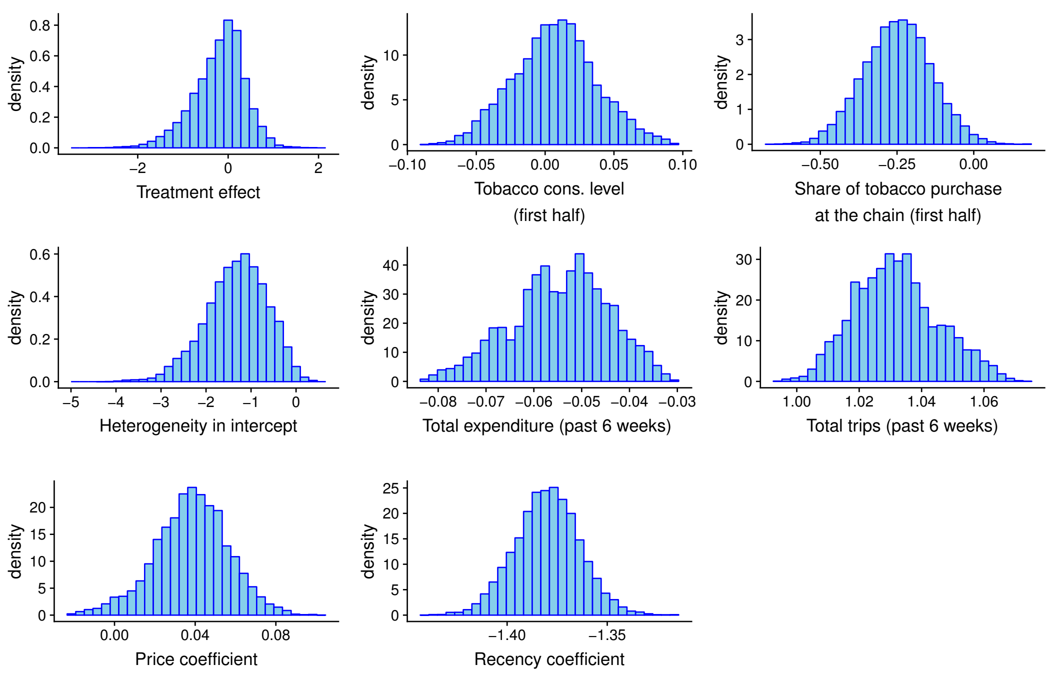


Figure A11: The density plots of the estimated parameters for model (5), where the dependent variables is the total number of trips generated by treatment/control group households that include tobacco products.

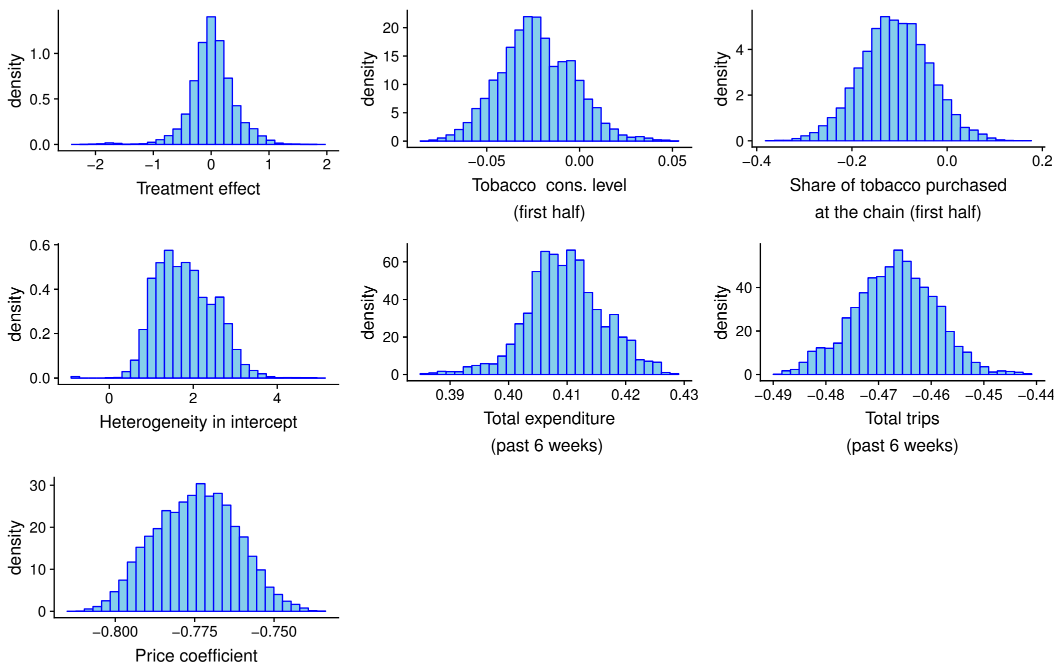


Figure A12: The density plots of the estimated parameters for model (6), where the dependent variables is the total number of cigarettes sticks purchased by households that were in the treatment/control group.

Online Appendix D - Definition of Treatment and Control Stores

In this section, we provide two sets of analyses to ensure that our results are robust to how treatment and control stores are defined. There are two concerns one may have when considering the current definition of treatment and control stores (i) how sensitive are the results to the values selected for the distance bands, i.e., 1 km and 2.5km, also would the results change if one includes the stores within the 1-2.5 km radius of an EC store that were discarded in our analysis above? and (ii) would it be possible to relax the fixed treatment radii feature, since there is heterogeneity in the density of rival stores in different geographic areas? Regarding (i), we use a semi-parametric approach in the first stage regression to relax the "binary" treatment definition. We first illustrate how the treatment intensity, that is the impact of EC's exit on cigarette sales at non-EC stores, decays as a function of distance from the closest EC store. Then we show that our results are robust when we adopt this semi-parametric approach in the first-stage regression and include the stores that were located within the 1-2.5 km radius. For (ii) we devise a novel approach for defining treatment/control stores which adjusts the treatment radius depending upon density of rival stores and again demonstrate that our estimates in the second stage regression remain consistent.

First, we estimate specification (1) while allowing for the effect to vary semi-parametrically as a function of distance. In particular, we group non-EC stores into six buckets, i.e., those located within 500 meters of the closest EC stores, those between 500 and 1000 meters; 1km to 2.5km; 2.5km to 5km; 5km to 10km; and 10km+ from the closest EC store. For stores located in each of these groups, we measure the effect of EC's tobacco ban on cigarette sales at non-EC stores relative to non-EC stores that are located more than 10km away from the closest EC store. The effect (α) for each group is depicted in Figure A13. The figure shows that the effect declines with distance from the treated store. While we find a (declining) effect up to the 2.5km mark, the effect declines subsequently and is indistinguishable from zero[4]. Also note that while the treatment intensity remains stable and statistically indistinguishable for the first two distance bands, it declines significantly for those stores located within the 1km to 2.5km distance band.

This observation motivated us to define treated stores as those stores that are located within 1 km of an EC store, and control stores to be those that are more than 2.5 km away. To keep the analysis simple in our studies we do not allow for heterogeneous treatment intensity in the first stage. Note that

[4]We also examined the change in treatment intensity across other formats namely discount/dollar stores, and groceries/warehouses. However, we did not find any evidence for a statistically significant effect across other formats that depreciates with distance from EC stores. We believe that is because the volume of cigarette sales across other formats is much larger than that of drugstores and that hinders our ability to detect a statistically significant effect especially when the shock is created by a single drugstore chain exiting the tobacco category. Therefore, studying the cross category effects across other formats using the current quasi-experimental variation seems to be infeasible.

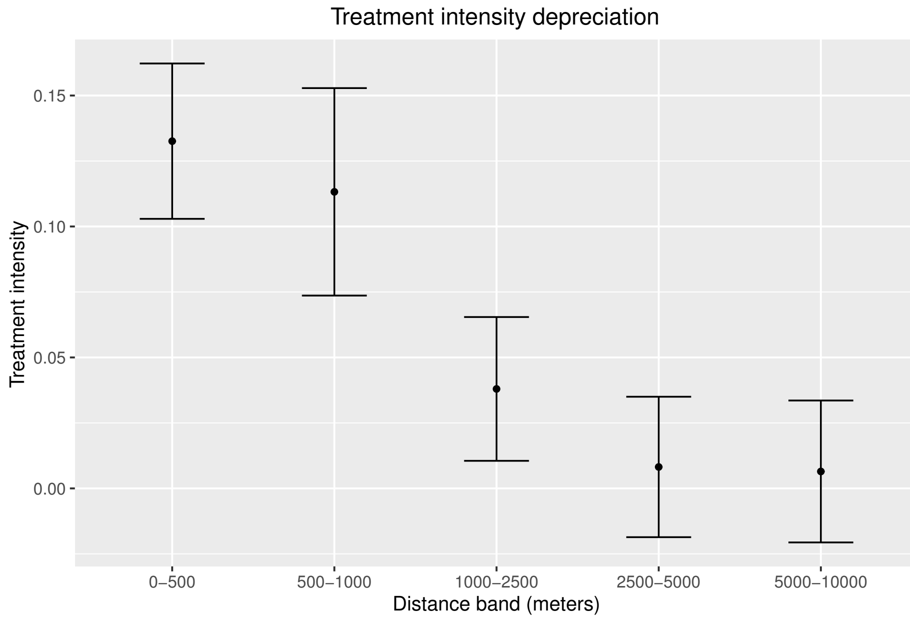


Figure A13: The treatment intensity of EC's tobacco ban on non-EC stores as a function of distance relative to the closest EC store. The intervals indicate the 95% confidence intervals.

as long as the average treatment intensity is strong enough we are able to detect its spillover effect on non-tobacco products. However, to demonstrate that our results are indeed robust to how treatment and control stores are defined we allow the treatment intensity to vary as a semi-parametric function of distance from EC stores. In particular, instead of having a treatment indicator being interacted with a time dummy we interact five treatment indicators with the time dummy that reflect if the closest EC store is within 0-0.5 km, 0.5-1 km, 1-2.5 km, 2-5 km, 5-10 km, or 10+ km from a given store. We also use coarser geographical fixed effects, e.g., state-week and DMA-week, instead of county-week. The coarser fixed effects along with the semi-parametric approach to capturing treatment intensity allow us to include stores that were previously removed across more counties. In particular, we now include 7785 stores that are spread across 1509 counties (previously 481)[5]. The result of this analysis is reported in Table A2. These estimates are inline with our previous findings and indicate that each percentage increase of cigarette sales is accompanied by 0.036 % increase in non-tobacco revenue.

Lastly, while we are only aware of fixed radii definitions in similar setups in the literature, see Ailawadi et al. (2010), Davis (2006), Ozturk et al. (2016), Wang and Goldfarb (2017), we re-estimated our models using a rank-based definition that would allow the treatment radius to adjust based on density of stores. To achieve this we first collect data on all locations that sell tobacco including relevant supermarket chains, drugstores, convenience stores, gas stations, and tobacco shops. In the fixed-radius treatment definition,

[5]The set of all drugstores in Nielsen scanner data are located in 1672 counties.

Table A2: IV,and OLS estimates for the treatment effect on non-tobacco sales. Instead of using a binary dummy to capture the treatment we let the treatment intensity to vary semi-parametrically as a function of distance from EC stores in the first-stage regression.

	Dependent variable:					
	Log non-tobacco revenue					
	OLS	IV	OLS	IV	OLS	IV
Impact of cig. sales (α)	0.1048***	0.0361***	0.1028***	0.0356***	0.1092***	0.0380***
	(0.0020)	(0.0081)	(0.0020)	(0.0088)	(0.0021)	(0.0092)
Cig. price index (θ)	0.1487***	0.0381***	0.1388***	0.0340**	0.1370***	0.0405***
	(0.0101)	(0.0148)	(0.0104)	(0.0154)	(0.0127)	(0.0149)
State-Week FE	X	X				
DMA-Week FE			X	X		
County-Week FE					X	X
Conditional F-stat (first stage)		150.4		125.16		125.92
Observations	2,014,342	2,014,342	2,014,342	2,014,342	2,014,342	2,014,342
R^2	0.9917	0.9915	0.9922	0.9921	0.9941	0.9939
Adjusted R^2	0.9910	0.9908	0.9914	0.9912	0.9920	0.9918
Residual Std. Error	0.0555 (df = 1861719)	0.0561 (df = 1861719)	0.0543 (df = 1810302)	0.0549 (df = 1810302)	0.0524 (df = 1481298)	0.0530 (df = 1481298)
Note:						*p<0.1; **p<0.05; ***p<0.01

every non-EC drugstore chain in our data that is located within a fixed radius of an EC store is considered to be treated by EC's exit. The caveat of this definition is that this radius remains the same as the density of rival stores changes, e.g., moving from rural to urban areas does not change the treatment radius. To address this we redefined the treatment/control groups using a rank-based definition. In particular, for a given non-EC drugstore in our dataset we ranked all stores that sell tobacco based on their proximity to this focal store and if there exists an EC store that is among the top K nearest neighbors of the focal non-EC drugstore we consider that store to be treated by EC's exit otherwise the store is assigned to the control group. Figure A14 demonstrates the difference between a fixed-radius definition and a rank-based definition for treatment/control stores and illustrates this definition accounts for the change in the density of available alternatives in the proximity of each store.

We set K=5 and define the treatment and control non-EC stores accordingly, i.e., a non-EC drugstore is said to be treated by EC's exit if there exists an EC store among its top 5 nearest neighbors. The histogram of distance from the closest EC store is plotted in Figure A15 across the treatment and control groups. Note that there are stores in the treatment group that are as far as 8km away from an EC store, and there are stores in the control group that are as close as 1km to an EC store, which indicates that the treatment radius varies as a function of density of stores. The estimates from the second stage regression are reported in Table A3. While the magnitude of the effect increases slightly compared to Table 5 in the original version (0.0548 compared to 0.0443), the estimates are not statistically different from each other.

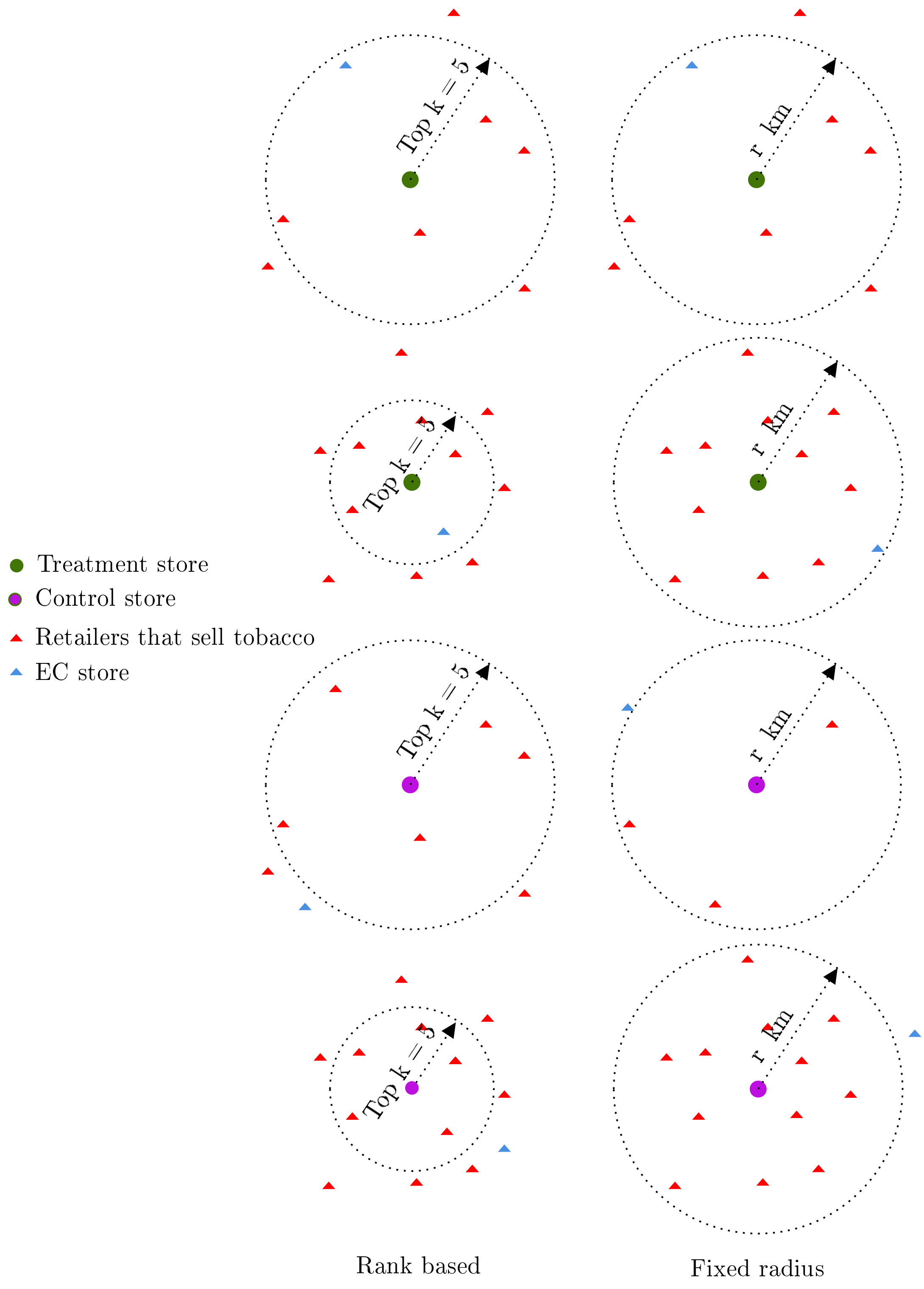
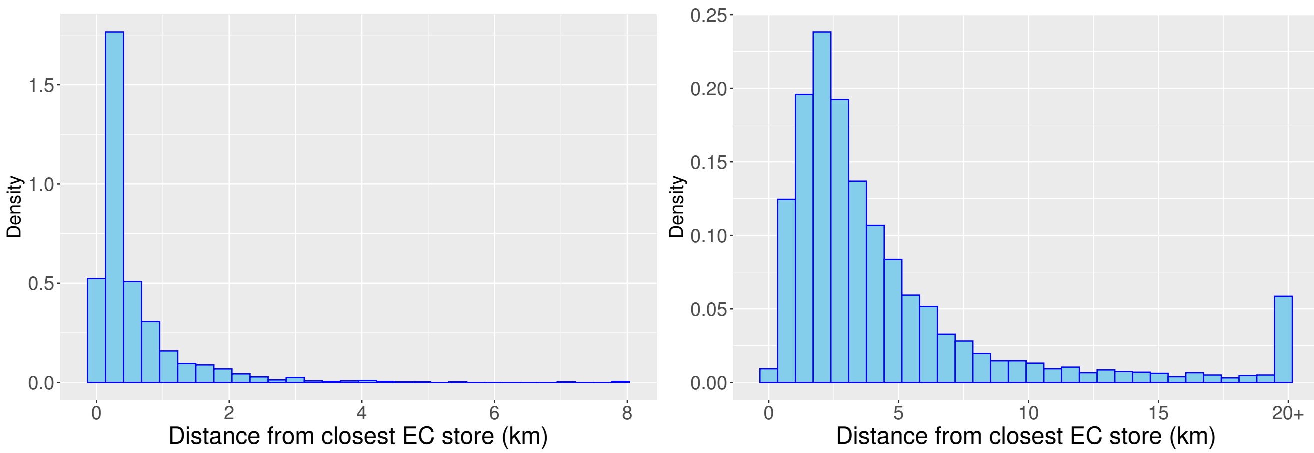


Figure A14: Defining treatment/control stores based on a fixed radius (right) and based on ranking of distance from the focal store (left). On the left panel a non-EC drugstore is said to be treated if there exists an EC store among its K = 5 nearest neighbors.



(a) Treatment

(b) Control

Figure A15: The histogram of distance from the closest EC store for treatment (left) and control (right) stores.

Table A3: IV estimates for the treatment effect on non-tobacco sales. Instead of using a fixed distance radius to define treatment/control (instrumental variables) in the first-stage regression we used the rank-based definition illustrated in Figure A14.

	Dependent variable:			
	Log non-tobacco revenue			
	(2SLS 1)	(2SLS 2)	(2SLS 3)	(2SLS 4)
Impact of cig. sales (α)	0.0425***	0.0475***	0.0556***	0.0548***
	(0.0118)	(0.0107)	(0.0107)	(0.0109)
Cig. price index (θ)	0.0481***	0.0537***	0.0628***	0.0620***
	(0.0154)	(0.0144)	(0.0146)	(0.0147)
Observations	1,346,917	1,346,917	1,346,917	1,346,917
R^2	0.9928	0.9928	0.9928	0.9928
Adjusted R^2	0.9913	0.9913	0.9914	0.9914
Residual Std. Error (df = 1119685)	0.0537	0.0536	0.0535	0.0535
Note:				*p<0.1; **p<0.05; ***p<0.01

Online Appendix E - Treatment Heterogeneity (panel data)

As discussed in the paper since advertising and other marketing activities tend to vary at DMA level, their impact is absorbed by the county-week fixed effects. Nevertheless, one could be concerned that EC stores may change their marketing activities or ad spending to mitigate some of the losses that arose from the tobacco ban. Looking at how the effect evolves in time and examining EC's ad spending could shed some light on the underlying mechanism and improve the validity of our results. In this appendix, we present two sets of analyses (a) we use Nielsen AdIntel data to examine changes in ad spending of EC, and (b) we re-estimate our models in Section 5 allowing the coefficients to vary in different stages, i.e., pre-announcement, between announcement and implementation, and after implementation of the tobacco ban. Our findings are summarized below:

(a) While we do see a temporary increase in EC's ad spending following the implementation of the ban, our results for both smokers and non-smokers remain unchanged when we control for this confound.

(b) We could not detect any statistically significant effect in the period between the announcement and the implementation of the ban and the effect remains stable in the periods following the implementation.

We used Nielsen AdIntel data which records ad spending by firms in different channels including print, radio, and TV (national and spot). We aggregate the ad spending of EC from 2012 to 2016 at weekly level and plot it in Figure A16. As Figure A16 illustrates, it turns out following the implementation of the ban EC indeed started spending more on advertising, however, during the time interval between announcement and implementation the ad spending seems to be fairly stable.

We added an ad stock variable[6] (Berndt et al., 1996, Ching et al., 2016, Dubé et al., 2005, Shapiro, 2018) to control for this confound in our regressions in Section 5. We present the results when controlling for an ad stock with 0.9 weekly discount factor in Tables A4-A5, which are counterparts to Tables 7-9. The consistency of these results reassures us that these changes could not be explained by advertising.

To further investigate the impact of EC's tobacco ban on non-smokers, we also look at how the effect varies in time. We allow the effect to vary depending upon the time period studied while also controlling for ad spending by EC/non-EC chains in Tables A6. The analysis suggests that the announcement *per se* did not have an effect, and that the effect starts growing in the months following the implementation of the ban. Eventually it appears to partially decline and stabilize.

[6] We tried different discount factors including 0.99, 0.95, 0.9, and 0.8 to verify that our results are not sensitive to the definition of the ad stock variable. While the dollars spent on advertising could explain some of the variation in trip frequency and customer expenditure, the magnitude of the coefficient of interest always remained stable.

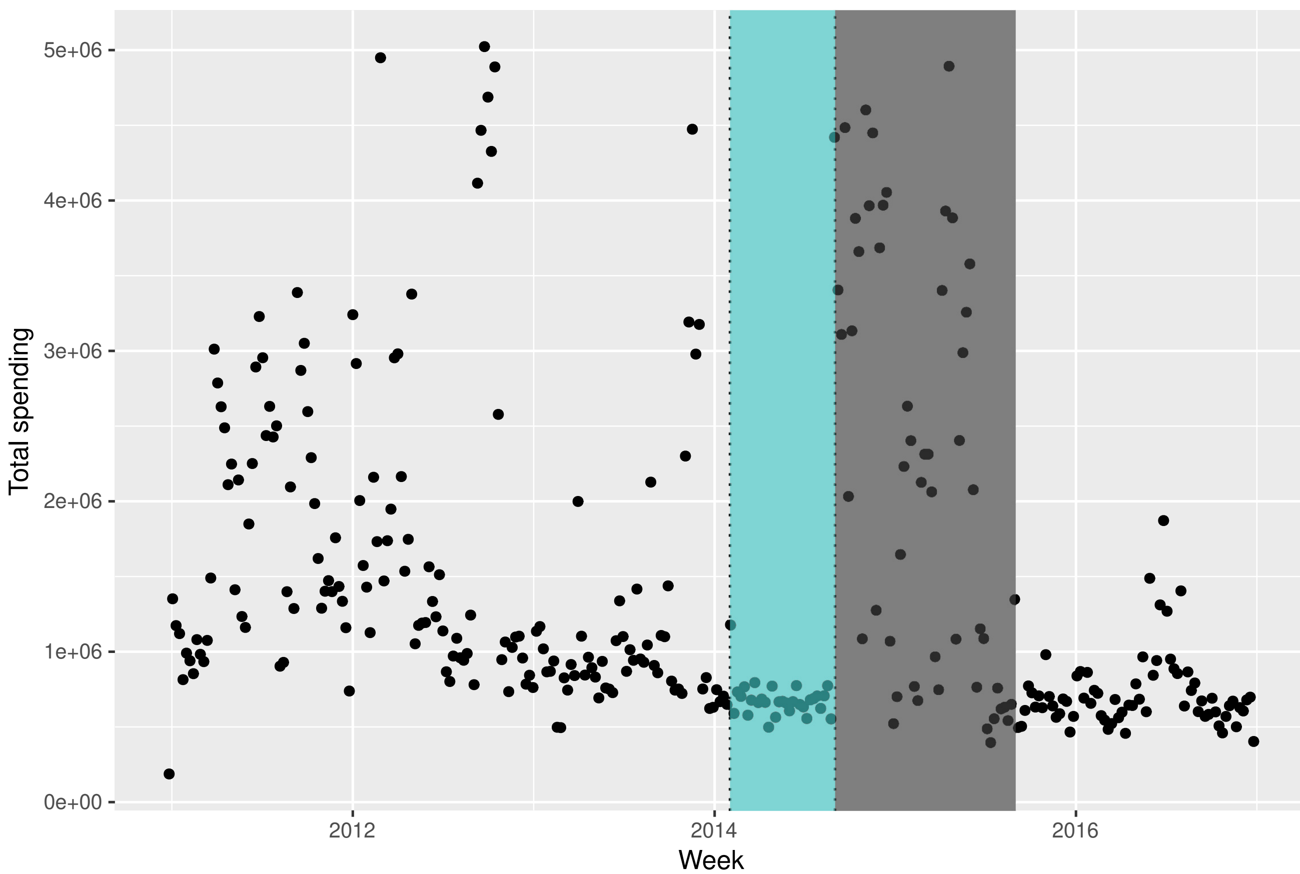


Figure A16: Total weekly ad expenditure (dollars) by EC. The blue highlighted area shows the time interval between announcement and implementation of the tobacco ban. This figure demonstrates that the firm has indeed increased its ad spending in the year following the implementation (gray highlighted area), however, in the time interval between announcement and implementation the ad expenditure has remained fairly stable.

Table A4: The impact of EC's decision to drop tobacco products on weekly non-tobacco expenditure, and weekly dollar value of tobacco-free trips. Column (1) shows that the non-tobacco expenditure by treatment households at EC has dropped relative to control households' non-tobacco expenditure at non-EC drugstores while controlling for ad expenditure

	Dependent variable:			
	Tot. non-tob. exp. at EC (Non-EC)	Tot. non-tob. exp. All stores	Tot. value of pure non-tob. trips at EC (Non-EC)	Tot. value of pure non-tob. trips All stores
	(1)	(2)	(3)	(4)
EC tobacco exit Effect (δ)	-0.189^{***}	-0.021	-0.005	-0.015
	(0.037)	(0.045)	(0.033)	(0.036)
Log ad stock EC/Non-EC	0.059^{***}	0.043	0.059^{***}	0.030
	(0.022)	(0.032)	(0.021)	(0.027)
Observations	186,368	186,368	186,368	186,368
R^2	0.243	0.179	0.215	0.186
Adjusted R^2	0.240	0.176	0.212	0.183
Residual Std. Error	1.285 (df = 185580)	1.882 (df = 185580)	1.204 (df = 185580)	1.914 (df = 185638)
Note:				*p<0.1; **p<0.05; ***p<0.01

To ensure that the impact of EC's tobacco ban on trips made by smokers does not change in the different time periods we also replicated Table 7 while allowing for the coefficient to vary across different periods. The results are presented in Table A7 and again show no effect in the time between announcement and implementation and a stable effect size post-implementation.

Finally, We measure the effect of EC's tobacco ban on consumption of cigarettes, i.e., similar to Table 8. However, we do allow for the coefficient to vary in different time periods. The results are presented in Table A8, and show that the behavior of smokers who were loyal to EC has not changed differentially relative to those smokers who were loyal to non-EC drugstores, the results are independent of the time period studied. The coefficient of the treatment effect both before and after the ban remains statistically insignificant.

To conclude this section, we also investigate how EC's tobacco ban has affected different types of non-smokers. Is the increase in share primarily driven by EC loyal consumers (used to purchase at EC) or new non-smoking customers?[7] To answer this question, we divide households into two groups based on a median split on the number of trips to EC *prior* to 2014. Those households that generated fewer trips than median are referred to as not loyal and the rest as loyal to EC. The results are presented in Table A9 and show that this policy seems to attract new customers, i.e., those who were not loyal prior

[7]We thank an anonymous reviewer for recommending this analysis.

Table A5: Non-smoker household reaction to EC's tobacco ban. The findings remain consistent with Table 9 after we control for the ad spending by EC/Non-EC chains. Note that the first two columns investigate EC trip and expenditure share, while the last column compares the basket size at EC/non-EC stores using a difference-in-differences specification. The observations in column (3) are at trip level, and the ad stock variable corresponds to the chain (EC/non-EC) that the transaction took place at.

	Dependent variable:		
	EC trip share	EC exp. share	log(Exp. per trip)
	(1)	(2)	(3)
EC tobacco exit effect (δ)	0.022***	0.022***	−0.003
	(0.002)	(0.002)	(0.007)
Log EC ad stock	0.001	0.001	
	(0.002)	(0.002)	
Log. Ad stock EC/Non-EC			0.023***
			(0.006)
Indicator for purchase at EC (γ)			0.122***
			(0.013)
Observations	682,575	682,256	996,277
R^2	0.369	0.365	0.251
Adjusted R^2	0.362	0.358	0.245
Residual Std. Error	0.368 (df = 674346)	0.377 (df = 674027)	0.934 (df = 987999)
Note:			*p<0.1; **p<0.05; ***p<0.01

Table A6: Non-smoker household reaction to EC's tobacco ban. The results do not show any statistically significant impact during the announcement to implementation period. The effect increases slightly following the implantation of the ban and stabilizes thereafter. Similar to Table A5, the last regression is at trip level, and the ad stock variable corresponds to the chain (EC/non-EC) that the transaction took place at.

	Dependent variable:		
	EC trip share	EC exp. share	log(Exp. per trip)
	(1)	(2)	(3)
EC tobacco exit effect (Feb 2014 to Sep 2014)	0.001	−0.001	−0.017*
	(0.002)	(0.002)	(0.009)
EC tobacco exit effect (Sep 2014 to Mar 2015)	0.019***	0.019***	−0.025***
	(0.003)	(0.003)	(0.009)
EC tobacco exit effect (Mar 2015 to Sep 2015)	0.026***	0.026***	0.008
	(0.003)	(0.003)	(0.010)
EC tobacco exit effect (after Sep 2015)	0.020***	0.020***	−0.00003
	(0.003)	(0.003)	(0.010)
Log ad stock (EC/non-EC)			0.024***
			(0.009)
Log EC ad stock	−0.001	−0.001	
	(0.003)	(0.003)	
Indicator for purchase at EC			0.125***
			(0.015)
Observations	682,256	682,256	996,277
R^2	0.369	0.365	0.251
Adjusted R^2	0.362	0.358	0.245
Residual Std. Error	0.368 (df = 674024)	0.377 (df = 674024)	0.934 (df = 987996)
Note:			*p<0.1; **p<0.05; ***p<0.01

Table A7: The impact of EC's decision to drop tobacco products on weekly non-tobacco expenditure, and weekly dollar value of tobacco-free trips through time. Column (1) shows that the non-tobacco expenditure by treatment households at EC has dropped relative to control households' non-tobacco expenditure at non-EC drugstores. We are unable to detect statistically significant changes in the effect through time, and we do not detect any effect in the period between announcement and implementation of the tobacco ban. Column (2) demonstrates that the overall non-tobacco expenditure of treatment households has not differentially changed relative to the control ones. Column (3) shows that the value of tobacco-free trips generated by treatment households at EC has not changed relative to the value of tobacco free trips generated by control households at non-EC stores, and the impact is limited to trips that included tobacco products. Finally, column (4) shows that the value of tobacco-free trips to all stores has not changed for the treatment relative to control.

	Dependent variable:			
	Tot. non-tob. exp. at EC (Non-EC)	Tot. non-tob. exp. All stores	Tot. value of pure non-tob. trips at EC (Non-EC)	Tot. value of pure non-tob. trips All stores
	(1)	(2)	(3)	(4)
EC tobacco exit effect (Feb 2014 to Sep 2014)	−0.030	0.025	0.016	0.038
	(0.035)	(0.043)	(0.030)	(0.032)
EC tobacco exit effect (Sep 2014 to Mar 2015)	−0.179***	0.0001	0.028	0.023
	(0.041)	(0.054)	(0.037)	(0.042)
EC tobacco exit effect (Mar 2015 to Sep 2015)	−0.150***	0.050	0.032	−0.010
	(0.043)	(0.055)	(0.038)	(0.042)
EC tobacco exit effect (after Sep 2015)	−0.214***	−0.042	−0.027	−0.015
	(0.045)	(0.057)	(0.040)	(0.043)
Observations	186,368	186,368	186,368	186,368
R^2	0.244	0.180	0.216	0.186
Adjusted R^2	0.240	0.176	0.212	0.183
Residual Std. Error	1.285 (df = 185381)	1.882 (df = 185381)	1.204 (df = 185381)	1.914 (df = 185636)
Note:				*p<0.1; **p<0.05; ***p<0.01

Table A8: The effect of EC's decision to drop tobacco products on cigarette consumption. Column (1)-(2) compares the total number of cigarette sticks and visits before and after the implementation of the policy across the treatment and control households, that are loyal smokers to EC and non-EC drugstores, respectively. Our results do not indicate any statistically significant effect on tobacco consumption in the period after announcement and different periods after treatment. Column (3) shows the quantity of tobacco conditional on purchase for trips made before and after the implementation of policy across treatment and control groups. Results across all columns show that this policy did not have a statistically significant effect on smoking habits in neither of the time periods studied.

	Dependent variable:		
	Log(Weekly cig. cons.)	Log(Weekly cig. visits)	Log(# of sticks purchased)
	(1)	(2)	(3)
EC tobacco exit effect (Feb 2014 to Sep 2014)	−0.041	−0.012	0.029
	(0.064)	(0.015)	(0.033)
EC tobacco exit effect (Sep 2014 to Mar 2015)	−0.023	−0.006	0.028
	(0.073)	(0.017)	(0.037)
EC tobacco exit effect (Mar 2015 to Sep 2015)	0.039	0.001	0.035
	(0.081)	(0.019)	(0.040)
EC tobacco exit effect (after Sep 2015)	−0.011	−0.008	0.006
	(0.084)	(0.020)	(0.049)
Price coefficient (β)	−0.242***	0.013	−0.873***
	(0.066)	(0.011)	(0.071)
Weeks since last purchase (γ)	−0.010***	−0.002***	
	(0.001)	(0.0002)	
Observations	185,640	185,640	115,518
R^2	0.326	0.357	0.587
Adjusted R^2	0.323	0.354	0.584
Residual Std. Error	1.907 (df = 184848)	0.396 (df = 184848)	0.596 (df = 114727)
Note:			*p<0.1; **p<0.05; ***p<0.01

to the policy. Note that the impact on loyal customers is indeed not statistically significant.

Table A9: The heterogeneity in the advertising effect of dropping tobacco products. The policy seems to have attracted new customers rather than inducing the previous loyal customers to visit the stores more often.

	Dependent variable:		
	EC trip share	EC exp. share	log(Exp. per trip)
	(1)	(2)	(3)
EC tobacco exit effect (not loyal)	0.071***	0.071***	0.004
	(0.004)	(0.004)	(0.011)
EC tobacco exit effect (loyal)	−0.003	−0.004	0.004
	(0.003)	(0.003)	(0.008)
Indicator for purchase at EC (not loyal)			−0.030***
			(0.010)
Indicator for purchase at EC (loyal)			0.125***
			(0.010)
Observations	682,256	682,256	996,277
R^2	0.371	0.367	0.252
Adjusted R^2	0.363	0.359	0.246
Residual Std. Error	0.368 (df = 674027)	0.376 (df = 674027)	0.933 (df = 987998)
Note:			*p<0.1; **p<0.05; ***p<0.01

Online Appendix F - Change in nature of trips to EC

It is conceivable that EC may have changed its product offerings or re-positioned itself to recoup some of the losses that they incurred because of dropping the tobacco category. While we do not have access to store data from EC, we do observe trips made to EC by panelists in the Nielsen Homescan data. What we *can* investigate, and do below, is if the nature of trips to EC has changed after the tobacco ban but within the categories for which we do have data.[8] What we *cannot* investigate are changes in revenues of departments such as clinics that are not observed in the data, and as we noted earlier we explicitly acknowledge as a limitation of the analysis.

Nielsen categorizes products into twelve departments including health & beauty, groceries, dairy, and mass merchandise. We investigate if share of trips to EC with purchases from the health and beauty department has changed after the tobacco ban perhaps because of re-positioning itself as a health-care provider. Table A11 presents the top 10 categories in the health and beauty department at the EC stores which account for 96% of the total sales in that department. We consider the trips by non-smokers to all drugstores during the 2012-2016 time period and study three outcomes for each trip: (a) expenditure on health and beauty products, (b) a dummy that reflects if the basket included any purchase from the health and beauty department, and (c) a dummy that indicates if more than half of the basket value is attributed to health and beauty products. The model is cast as a difference in differences model to determine if the nature of trips made to EC has changed over time:

$$\mathbf{Y}_{it} = \delta \cdot (\mathbb{1}_{t>t^*})(\mathbb{1}_{r_{it}=\text{EC}}) + \beta \cdot (\mathbb{1}_{r_{it}=\text{EC}}) + \eta_{m_{it}} + \eta_h, \tag{9}$$

where $\mathbf{Y}_{it}$ is an outcome of interest, e.g., expenditure on health and beauty products, for t^{th} trip of household i. r_{it} indicates the drugstore chain that the trip was made to. Finally, $\eta_{m_{it}}$ and η_h are month and household fixed effects. The estimates for δ and β are reported in Table A10. We could not find any significant change in expenditure or share of trips that involve purchases from the health and beauty department at EC relative to other drugstores in the panel. These suggest that re-positioning EC as a healthcare provider has not differentially changed the share of H&B-related (the closest related category in our data) transactions with EC. Note that this does not mean that H&B-related trips did not increase at EC, rather it means that the composition of trips made to EC has not changed in favor of H&B-related purchases (relative to other categories) and implies that these H&B-related trips increased at the same

[8] We thank an anonymous reviewer for suggesting this analysis.

rate as other ones.

Table A10: The impact of EC tobacco ban on trips that involve purchases made at health and beauty department. We could not find any significant change in expenditure or share of trips that involve purchases from the health and beauty department at EC relative to other drugstores in the panel.

	Dependent variable:		
	Expenditure at H&B department	Includes purchase from H&B	At least Half are H&B products
	(1)	(2)	(3)
EC trip x After tobacco ban (δ)	−0.358	0.003	−0.004
	(0.229)	(0.003)	(0.003)
EC trip (β)	2.907***	0.081***	0.077***
	(0.173)	(0.003)	(0.003)
Observations	996,835	996,835	996,835
R^2	0.185	0.203	0.202
Adjusted R^2	0.178	0.196	0.195
Residual Std. Error (df = 988558)	24.965	0.444	0.448

Note: *p<0.1; **p<0.05; ***p<0.01

All regressions include household and monthly fixed effects

Standard errors are cluster at household level.

Table A11: Top revenue generating product groups within EC's health and beauty department. This table was created using the Nielsen HomeScan panel data.

Product group	Share of H&B	Cumulative share
Medications/Remedies/Health aids	0.4253050	0.4253050
Vitamins	0.1802918	0.6055967
Cough and cold remedies	0.0674240	0.6730207
Oral hygiene	0.0603392	0.7333598
Hair care	0.0583466	0.7917065
Skin care preparations	0.0562210	0.8479275
Cosmetics	0.0510762	0.8990037
Shaving needs	0.0252238	0.9242275
First aid	0.0224114	0.9466389
Deodorant	0.0160156	0.9626545

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