Online Appendix

A. Border strategy demand model

One potential concern when estimating TV ad effects is that advertisers may purchase spot TV ads to target specific DMAs – this could lead to an endogeneity problem if local demand shocks and advertising levels are correlated. This would potentially pose a threat to the validity of estimates from models such as the one we estimated using equation (2). While this is less of an issue for product placement vs. advertising because instances vary at the national level and the estimates rely on variation in viewership, one may still be concerned that brands may be responsive to local demand shocks in a few key markets in each season and that could still create an endogeneity problem. To allay these concerns we take advantage of discontinuities across DMAs, which imply that people living in border counties are exposed to slightly different TV content and schedules. In our case, people on either side of the border are exposed to different amounts of product placement due to live events affecting broadcasting schedules, channel positions being different, and local syndication, all of which could alter program viewership and in turn shift the exposure to product placement.

The idea behind the border strategy is to limit the analysis to stores located in counties that lie on a DMA border, and to compare the cigarette sales of stores on either side of this border. See Card and Krueger (2000), Shapiro (2018), Shapiro, Hitsch, and Tuchman (2020), and Tuchman (2019) for other examples of using geographic discontinuities. Table 1 provides a summary of the set of store and brand on the border markets. We first estimate equation (2) using the subset of stores located in DMA border counties, and following Shapiro, Hitsch, and Tuchman (2020) we then allow for border-brand-month fixed effects to replace the brand-month fixed effects considered previously. This yields the following model, where B indexes the DMA border. Results are presented in Table A1.

$$\begin{aligned}\log(1+Q_{bst}) =&\beta\log(P_{bst}) + \gamma\log(1+\mathcal{G}_{bd_st}) + \gamma_c\log(1+\mathcal{G}_{\text{-}bd_st}) + \\ &\xi f_{bst} + \kappa d_{bst} + \eta_{sw_t} + \eta_{Bbm_t} + \eta_{sby_t} + \epsilon_{bst}\end{aligned} \tag{A1}$$

One concern with the border strategy is that it is measuring the local average treatment effect of exposure to product placement on people who live near DMA borders, and this estimand could be different from the average treatment effect at the national level (Li, Hartmann, and Amano, 2019). To illustrate the extent of change in the placement coefficient as a result of focusing on border markets as opposed to the full dataset, we first replicate the regressions from Table 2 except only using observations from border markets. These estimates are in columns (1)-(4) of Table A1. Comparing the estimates in column (4) of Table A1 with Table 2 suggests that the local average treatment effect could be slightly larger when measured at the border. After controlling for border-brand-month fixed effects in column (5) of Table (A1), the coefficients of own and competitor effects shrink and become statistically indistinguishable from those reported in column (4) of Table 2. Overall, both the estimation on the border counties and our prior estimates suggest that competitor and own product placement have an almost equal effect on demand, and our results remain consistent across both demand models.

A second challenge with using the border strategy in this context is that product placement is a national promotional tool. As a consequence, product placement instances do not

Table A1: The effect of own and competitor product placement using only stores located in DMA border markets. We control for a wide variety of fixed effects to absorb the effect of potential confounds. Columns (1)-(4) correspond to columns (1)-(4) of Table 2. In addition to the fixed effects considered in Table 2, we allow for border-brand-month fixed effects in column (5) to compensate for local demand shocks.

	Dependent variable:				
	Log (Quantity + 1)				
	(1)	(2)	(3)	(4)	(5)
Log own placement goodwill	1.002***	−0.004	0.043**	0.034***	0.018**
	(0.051)	(0.010)	(0.021)	(0.011)	(0.009)
Log competitor placement goodwill	−0.113***	0.073***	0.056***	0.036***	0.018***
	(0.034)	(0.007)	(0.011)	(0.007)	(0.006)
Feature	2.236***	0.417***	0.435***	0.422***	0.438***
	(0.145)	(0.042)	(0.042)	(0.043)	(0.045)
Display	−0.520	4.612***	4.683***	4.604***	4.123**
	(1.322)	(1.686)	(1.713)	(1.712)	(1.785)
Log price index	−1.584***	−0.999***	−0.975***	−1.357***	−1.524***
	(0.195)	(0.057)	(0.070)	(0.069)	(0.083)
Constant	4.823***				
	(0.284)				
Store-Brand		X	X		
Store-Week of Year		X	X	X	X
Brand-Month			X	X	
Store-Brand-Year				X	X
Border-Brand-Month					X
Observations	778,360	778,360	778,360	778,360	778,360
R^2	0.296	0.871	0.872	0.892	0.900
Adjusted R^2	0.296	0.865	0.866	0.884	0.888
Residual Std. Error	1.493 (df = 778354)	0.654 (df = 742982)	0.651 (df = 742525)	0.607 (df = 724927)	0.595 (df = 696510)

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

All standard errors are two-way clustered at the DMA border-brand and brand-week level.

vary across DMAs. However, product placement exposure (and therefore, product placement goodwill) does vary across DMAs due to differences in viewership. Our usage of the border strategy is similar to extant research that uses the border strategy to examine the effect of national TV advertising, which is another setting where instances do not vary across DMAs but viewership and exposure do vary across DMAs (e.g., Thomas, 2020).

A third potential issue is that viewership is measured at the DMA level rather than the county level. Focusing on counties that lie on a DMA border will lead to measurement error in viewership and in placement goodwill. This kind of measurement error in border county viewership would likely lead to slightly conservative estimates of product placement elasticities.

B. Calibration of the carry-over parameter

In our main analysis, we constructed the placement goodwill variables assuming a carry-over parameter $\delta = 0.9$, which is consistent with the values used in the prior literature that estimates TV advertising effectiveness (Dubé, Hitsch, and Manchanda, 2005; Shapiro, Hitsch, and Tuchman, 2020). In this section, we calibrate the carry-over parameter to verify that our results are robust to this assumption. As discussed in Dubé, Hitsch, and Manchanda (2005), the calibration of the carry-over parameter is only feasible because of the volatility in product placement. In other words, if brands were using a constant amount of product placement level throughout the panel, then this carry-over calibration exercise would not be possible. In our context, the product placement instances for each brand vary at the national level, and we are also allowing for brand-month fixed effects. Therefore, the only variation that makes this calibration possible is the placement goodwill decay speed within each month.

Our goal is to examine how model fit (R^2) changes as a function of the carry-over parameter δ. In our earlier analysis we presented estimates from three models: (a) the baseline demand model from equation (2) estimated using data from all stores, (b) the baseline demand model from equation (2) estimated on border markets, and (c) the border strategy demand model from equation (A1) estimated on border markets. Note that (a), (b), and (c) correspond to column (4) of Table 2, and columns (4)-(5) of A1, respectively. Since we used three different models in our analyses, we consider the average R^2 across these models as a measure of fit to calibrate the carry-over parameter. We perform a grid search on the interval $\delta \in [0, 1]$ with increments of 0.025 in the range between 0 and 0.85 and finer increments of 0.005 for the range between 0.85 and 1, and we re-estimate all parameters of the model for each of these δ values. Figure A1 shows how the average R^2 changes as we vary the carry-over parameter δ. The highest R^2 value is attained at a carry-over parameter of 0.94.

We re-estimate each model with the calibrated carry-over parameter of 0.94 and present the results of this analysis in Table A2. The own and competitor goodwill effects estimated using the full set of stores in column (1) of Table A2 remain statistically significant and are statistically indistinguishable from each other – this is consistent with our prior findings in Table 2. The magnitudes of the effects are slightly larger in Table A2 compared to Table 2; however, the results are not statistically different from each other. The calibrated border fixed effect border strategy results in columns (2)-(3) of Table A2 yield a larger magnitude for the competitor placement effect, but the standard errors have also increased in size and the coefficients are still statistically indistinguishable from the coefficient previously reported in Table A1. Overall, this exercise demonstrates that our substantive results remain consistent regardless of whether we use a standardized carry-over parameter of 0.9 (common in the literature) or whether we calibrate it using model fit.

As an additional check, we now repeat the calibration exercise using only the R^2 from the baseline model in equation (2). We report the R^2 of the baseline model as a function of the carry-over parameter in Figure A2. The optimal fit is attained at $\delta = 0.93$ and the estimates across the three models for this carry-over value are reported in Table A3. The results in Table A3 remain consistent with our findings in Table A2.

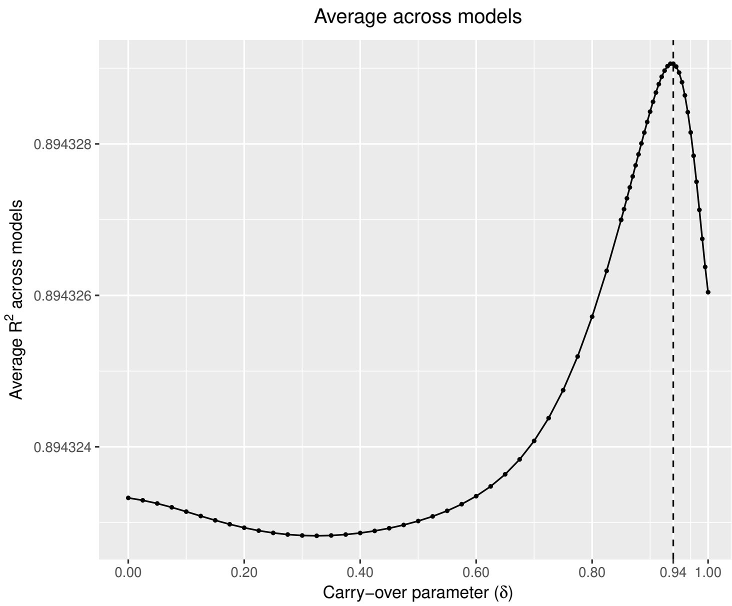

Figure A1: The average model fit across the three models as a function of the carry-over parameter δ. The highest average R^2 is attained for a carry-over parameter of 0.94, which is indicated by the dashed line.

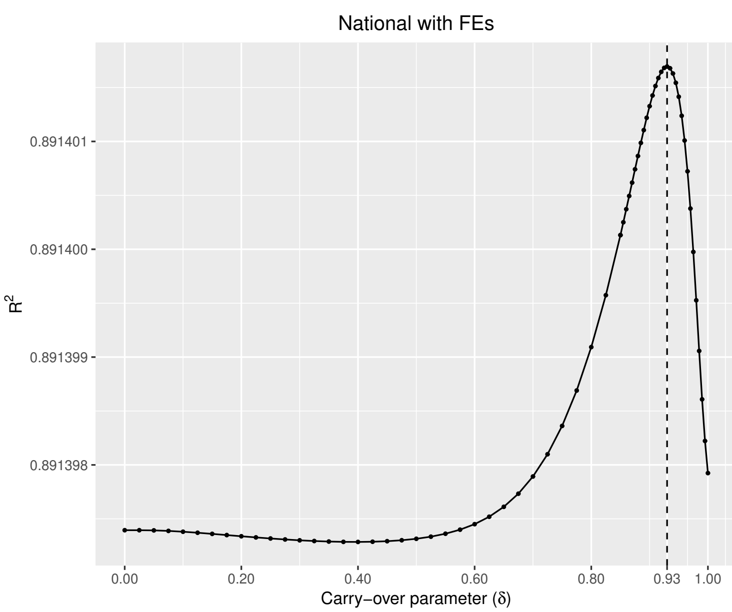

Figure A2: The model fit for the baseline model as a function of the carry-over parameter δ. The highest R^2 is attained for a carry-over parameter of 0.93, which is indicated by the dashed line.

Table A2: The national and border estimates for the calibrated carry-over parameter of 0.94.

	Dependent variable:		
	Log (Quantity + 1)		
	National with FEs	Border with FEs	Border Strategy
Log own placement goodwill	0.027***	0.037**	0.022*
	(0.009)	(0.015)	(0.011)
Log competitor placement goodwill	0.031***	0.055***	0.029***
	(0.006)	(0.010)	(0.009)
Feature	0.446***	0.422***	0.438***
	(0.038)	(0.043)	(0.045)
Display	0.816	4.606***	4.126**
	(0.646)	(1.712)	(1.785)
Log price index	−1.425***	−1.359***	−1.524***
	(0.047)	(0.069)	(0.083)
Border-Brand-Month			X
Observations	2,740,781	778,360	778,360
R^2	0.891	0.892	0.900
Adjusted R^2	0.883	0.883	0.887
Residual Std. Error	0.599 (df = 2551495)	0.607 (df = 724222)	0.597 (df = 691475)

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

Standard errors are two-way clustered at the DMA-brand and brand-week level for the first column and at the DMA border-brand and brand-week level for the last two columns.

Table A3: The national and border estimates for the calibrated carry-over parameter of 0.93.

	Dependent variable:		
	Log (Quantity + 1)		
	National with FEs	Border with FEs	Border Strategy
	(1)	(2)	(3)
Log own placement goodwill	0.028***	0.038***	0.021**
	(0.009)	(0.014)	(0.011)
Log competitor placement goodwill	0.028***	0.049***	0.025***
	(0.006)	(0.009)	(0.008)
Feature	0.446***	0.422***	0.438***
	(0.038)	(0.043)	(0.045)
Display	0.816	4.606***	4.125**
	(0.646)	(1.712)	(1.785)
Log price index	−1.424***	−1.358***	−1.524***
	(0.047)	(0.069)	(0.083)
Border-Brand-Month			X
Observations	2,740,781	778,360	778,360
R^2	0.891	0.892	0.900
Adjusted R^2	0.883	0.883	0.887
Residual Std. Error	0.599 (df = 2551495)	0.607 (df = 724222)	0.597 (df = 691475)

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

Standard errors are two-way clustered at the DMA-brand and brand-week level for the first column and at the DMA border-brand and brand-week level for the last two columns.

C. Heterogeneity in product placement effects

Our results in section 4 demonstrate that tobacco product placement has a positive average effect on sales both for the brand that was displayed on-screen and for its competitors. In this section, we focus on examining the heterogeneity of this effect in order to better understand product placement's efficacy. First, we examine differences across product placement types. As illustrated in Figure 4, there are four types of product placement instances in our data:

- Background: The brand logo or name is present in the background of a scene
- Foreground: The brand logo or name is present in the foreground of a scene
- Dialogue mention: The brand is mentioned verbally
- Prop: A character is holding a cigarette or a pack of cigarettes

Our first analysis is to examine whether the visual salience of the product placement matters. We denote prop and foreground placement as visually salient and the other forms of product placement as non-salient. Next, we create four separate goodwill variables for product placement: salient own-brand, non-salient own-brand, salient competitor-brand, and non-salient competitor-brand. Finally, we modify equation (2) to accommodate these additional variables and estimate the following demand model:

$$\begin{aligned}\log(1+Q_{bst}) =& \beta\log(P_{bst}) + \gamma\log(1+\mathcal{G}^{\text{salient}}_{bd_st}) + \gamma_c\log(1+\mathcal{G}^{\text{salient}}_{\text{-}bd_st})+\\ & \varphi\log(1+\mathcal{G}^{\text{non-salient}}_{bd_st}) + \varphi_c\log(1+\mathcal{G}^{\text{non-salient}}_{\text{-}bd_st})+\\ & \xi f_{bst} + \kappa d_{bst} + \eta_{sw_t} + \eta_{bm_t} + \eta_{sby_t} + \epsilon_{bst}\end{aligned} \tag{A2}$$

Estimation results from this model are presented in Table A4. Columns (1)-(4) of Table A4 correspond to the specifications in Table 2. Our results from column (4) of Table A4 show that both salient and non-salient product placement have a positive effect on sales, and salient product placement has a larger magnitude. However, we do not have enough statistical power to claim that the coefficients are statistically different from each other.

We also examine heterogeneity across product placement types in three further ways:

1. We use an alternative definition of salience where background instances are classified as non-salient and all other types are classified as salient. The patterns remain directionally similar and statistically indistinguishable from the results in table A4.

2. We classify each product placement instance as either verbal (dialogue) or on-screen (prop, foreground, and background) and we estimate the following regression:

$$\begin{aligned}\log(1+Q_{bst}) =& \beta\log(P_{bst}) + \gamma\log(1+\mathcal{G}^{\text{on-screen}}_{bd_st}) + \gamma_c\log(1+\mathcal{G}^{\text{on-screen}}_{\text{-}bd_st})+\\ & \varphi\log(1+\mathcal{G}^{\text{verbal}}_{bd_st}) + \varphi_c\log(1+\mathcal{G}^{\text{verbal}}_{\text{-}bd_st})+\\ & \xi f_{bst} + \kappa d_{bst} + \eta_{sw_t} + \eta_{bm_t} + \eta_{sby_t} + \epsilon_{bst}\end{aligned} \tag{A3}$$

   Results from this regression are presented in table A5. We find that on-screen product placement still has a positive and significant effect, but verbal product placement is statistically insignificant. Given that only 15% of the product placement instances in

Table A4: Investigating the heterogeneous effect of salient vs. non-salient product placement.

	Dependent variable:			
	Log (Quantity + 1)			
	(1)	(2)	(3)	(4)
Log own placement goodwill (salient)	0.315***	−0.013	0.029*	0.024***
	(0.075)	(0.008)	(0.016)	(0.008)
Log own placement goodwill (non-salient)	1.030***	0.001	0.020	0.014*
	(0.079)	(0.009)	(0.016)	(0.009)
Log competitor placement goodwill (salient)	−0.085***	0.042***	0.021***	0.013***
	(0.021)	(0.005)	(0.007)	(0.003)
Log competitor placement goodwill (non-salient)	−0.018	0.001	0.008	0.011***
	(0.042)	(0.005)	(0.007)	(0.004)
Feature	2.171***	0.406***	0.418***	0.447***
	(0.116)	(0.036)	(0.036)	(0.038)
Display	3.415*	1.058*	1.123*	0.818
	(1.753)	(0.628)	(0.640)	(0.646)
Log price index	−1.639***	−1.232***	−1.297***	−1.423***
	(0.142)	(0.048)	(0.053)	(0.047)
Constant	4.752***			
	(0.208)			
Store-Brand		X	X	
Store-Week of Year		X	X	X
Brand-Month			X	X
Store-Brand-Year				X
Observations	2,740,781	2,740,781	2,740,781	2,740,781
R^2	0.312	0.869	0.869	0.891
Adjusted R^2	0.312	0.862	0.863	0.883
Residual Std. Error	1.454 (df = 2740773)	0.650 (df = 2616249)	0.649 (df = 2613707)	0.599 (df = 2551493)

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

All standard errors are two-way clustered at the DMA-brand and brand-week level.

our data are verbal, these results may simply be due to the relatively small sample size. As a result, we choose to use the pooled regression results as the main specification in the paper.

3. We include each of the four product placement types in the regression rather than summarizing the product placement instances as salient vs. non-salient. This approach is unable to find conclusive evidence regarding heterogeneity across these different types of product placement, in large part because there is substantial multicollinearity in this setting. Different types of product placement tend to co-occur in the same show, which means that it is hard to identify separate effects for each type in isolation.

Table A5: Investigating the heterogenerous effect of verbal vs. on-screen product placement.

	Dependent variable:			
	Log (Quantity + 1)			
	(1)	(2)	(3)	(4)
Log own placement goodwill (on screen)	0.840***	0.0005	0.044***	0.026***
	(0.043)	(0.007)	(0.015)	(0.008)
Log placement goodwill (verbal)	1.392***	0.023	−0.016	−0.006
	(0.113)	(0.015)	(0.018)	(0.009)
Log competitor goodwill (on screen)	−0.091***	0.047***	0.030***	0.021***
	(0.027)	(0.005)	(0.007)	(0.004)
Log placement goodwill (verbal)	−0.088*	0.013**	−0.015**	0.002
	(0.047)	(0.005)	(0.007)	(0.004)
Feature	2.165***	0.402***	0.418***	0.446***
	(0.120)	(0.036)	(0.036)	(0.038)
Display	3.359*	1.023	1.120*	0.816
	(1.815)	(0.628)	(0.639)	(0.646)
Log price index	−1.618***	−1.275***	−1.299***	−1.424***
	(0.141)	(0.046)	(0.053)	(0.047)
Constant	4.804***			
	(0.209)			
Store-Brand		X	X	
Store-Week of Year		X	X	X
Brand-Month			X	X
Store-Brand-Year				X
Observations	2,740,781	2,740,781	2,740,781	2,740,781
R^2	0.318	0.868	0.869	0.891
Adjusted R^2	0.318	0.862	0.863	0.883
Residual Std. Error	1.447 (df = 2740773)	0.651 (df = 2616249)	0.649 (df = 2613707)	0.599 (df = 2551493)

Note: *p<0.1; **p<0.05; ***p<0.01
All standard errors are two-way clustered at the DMA-brand and brand-week level.

Apart from examining heterogeneity across placement types, we also examine heterogeneity across different product types. In our sales data, the main product characteristic differentiator between different UPCs is whether they are menthol vs. non-menthol cigarettes. To test whether menthol vs. non-menthol cigarettes have a higher sales lift from product placement, we aggregate the sales data to the store-brand-week-menthol level and re-estimate a version of equation (2) where we interact the goodwill coefficient with the type of cigarettes (menthol/non-menthol).

Note that we do not observe whether any given product placement instance was for a menthol or non-menthol product within a particular brand. Therefore, we have to assume that all products from a particular brand have the same product placement goodwill values. We are able to observe menthols vs. non-menthols in the sales data, because we know whether each UPC corresponds to a pack of menthol cigarettes or a pack of non-menthol cigarettes. Therefore, our analysis of menthols vs. non-menthols relies purely on differences in sales between these two types of products after product placement for the parent brand takes place, rather than any differences in the levels of product placement between these two types of products. Results from this analysis are presented in Table A6 and demonstrate that there is no significant difference in the product placement elasticity between menthol vs. non-menthol cigarettes.[13]

Menthol cigarettes are much more popular among young people and African Americans than with other demographic groups (Villanti et al., 2016). Our results indicate that product placement does not have a noticeably stronger effect on consumers who buy menthol vs. non-menthol cigarettes, which in turn suggests that product placement does not affect young people and African Americans much more (or much less) than it does members of other demographic groups. However, we are unable to directly examine whether young people or African Americans have higher product placement elasticities than members of other demographic groups. Although policymakers may be interested in understanding whether some demographic groups are more responsive to product placement than others, a systematic examination of product placement effect heterogeneity across different demographic groups is difficult due to the limitations of our data. We do not have sales data for each demographic group, nor do we know the product placement exposure levels for each demographic group. These limitations make it infeasible for us to find any conclusive evidence regarding the heterogeneous treatment effect of product placement across different demographic groups.

[13]In our data, there are two brands that only sell menthol products: Kool and Salem. To examine whether product placement elasticities were different for these brands, we repeated the analysis only on these two brands. This substantially reduces the number of observations, and it yields large standard errors on own and competitor product placement coefficients as well as a statistically insignificant own product placement coefficient. However, the results remained directionally similar and were statistically indistinguishable from our main results.

Table A6: Investigating the heterogeneous effect of product placement on menthol vs. non-menthol cigarettes.

	Dependent variable:			
	Log (Quantity + 1)			
	(1)	(2)	(3)	(4)
Log own placement goodwill x Menthol	0.225***	−0.045***	0.026**	0.016**
	(0.039)	(0.009)	(0.011)	(0.007)
Log own placement goodwill x Non-menthol	1.107***	−0.009	0.037**	0.025***
	(0.046)	(0.008)	(0.015)	(0.009)
Log competitor placement goodwil x Menthol	−0.076**	0.035***	0.024***	0.017***
	(0.035)	(0.005)	(0.008)	(0.005)
Log competitor placement goodwil x Non-menthol	−0.141***	0.051***	0.019**	0.019***
	(0.033)	(0.007)	(0.008)	(0.005)
Feature	2.351***	0.492***	0.503***	0.524***
	(0.110)	(0.037)	(0.036)	(0.038)
Display	0.529	0.777***	0.813***	0.630**
	(0.752)	(0.195)	(0.196)	(0.255)
Log price index x Menthol	−1.099***	−1.111***	−1.201***	−1.301***
	(0.149)	(0.050)	(0.057)	(0.048)
Log price index x Non-menthol	−1.564***	−1.223***	−1.189***	−1.309***
	(0.142)	(0.059)	(0.065)	(0.060)
Menthol	−0.848***			
	(0.245)			
Constant	4.503***			
	(0.211)			
Store-Brand-Menthol		X	X	
Store-Menthol-Week of Year		X	X	X
Brand-Month			X	X
Store-Brand-Menthol-Year				X
Observations	4,022,218	4,022,218	4,022,218	4,022,218
R^2	0.294	0.856	0.857	0.879
Adjusted R^2	0.294	0.847	0.848	0.868
Residual Std. Error	1.422 (df = 4022208)	0.661 (df = 3786566)	0.660 (df = 3781637)	0.614 (df = 3690264)

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

All standard errors are two-way clustered at the DMA-brand and brand-week level.

D. Marlboro-specific coefficients

Our main specification stacks the data across brands and estimates a single coefficient for own-brand and cross-brand product placement. Given that product placement instances were disproportionately more for Marlboro relative to other brands, one may be concerned that the product placement effect could differ significantly across brands. To test this, we estimate a model which allows for the own and competitor product placement effects to differ for Marlboro.[14] We estimated an extended model that allows the coefficients to be different for Marlboro:

$$\begin{aligned}\log(1+Q_{bst}) =&\beta\log(P_{bst})+\gamma\log(1+\mathcal{G}_{bd_st})+\gamma_c\log(1+\mathcal{G}_{\text{-}bd_st})+\\&\gamma_m\log(1+\mathcal{G}_{bd_st})\cdot\left(1_{\{b=\text{Marlboro}\}}\right)+\\&\gamma_{c_m}\log(1+\mathcal{G}_{\text{-}bd_st})\cdot\left(1_{\{b=\text{Marlboro}\}}\right)+\\&\xi f_{bst}+\kappa d_{bst}+\eta_{sw_t}+\eta_{bm_t}+\eta_{sby_t}+\epsilon_{bst}\end{aligned} \tag{A4}$$

The parameters in (A4) are defined similar to equations (2). The estimates from this model are report in Table A7. We do not find a statistically significant difference in the effect of product placement across Marlboro and non-Marlboro brands. However, the magnitude of the effect for own product placement is higher for Marlboro relative to other brands, while the magnitude of the effect for competitor product placement is lower for Marlboro relative to other brands. In particular, the own product placement elasticity for Marlboro is equal to 0.018 + 0.014, compared to 0.018 for other brands. For the effect of competitor product placement, we find that the effect is 0.021 - 0.004 for Marlboro compared to 0.021 for other brands.

Although the estimates from Table A7 do not show a statistically significant difference in the effect of product placement across Marlboro vs. non-Marlboro brands, we used the new point estimates and allowed for different coefficients across Marlboro/non-Marlboro brands and repeated the counterfactual exercise performed in Figure 8. The results of this analysis are reported in Figure A3. Overall, the patterns and main conclusions from this figure are in line with our previous findings.

[14]Our estimates are under-powered if we allow for a separate coefficient across all brands.

Table A7: The effect of own and competitor product placement. The results allow the product placement effect to vary for Marlboro relative to other brands. Our results do not show a statistically significant difference in the effect of product placement for Marlboro relative to other brands.

	Dependent variable:			
	Log (Quantity + 1)			
	(1)	(2)	(3)	(4)
Log own placement goodwill	0.304^{***}	-0.011	0.045^{***}	0.018^{**}
	(0.093)	(0.012)	(0.015)	(0.008)
Log own placement goodwill x Is Marlboro	0.205^{*}	0.006	-0.006	0.014
	(0.105)	(0.015)	(0.027)	(0.015)
Log competitor placement goodwill	-0.052^{**}	0.052^{***}	0.026^{***}	0.021^{***}
	(0.026)	(0.006)	(0.008)	(0.004)
Log competitor placement goodwill x Is Marlboro	1.237^{***}	-0.066^{***}	0.003	-0.004
	(0.105)	(0.017)	(0.024)	(0.014)
Feature	2.132^{***}	0.401^{***}	0.418^{***}	0.447^{***}
	(0.124)	(0.036)	(0.036)	(0.038)
Display	2.754^{*}	1.020	1.118^{*}	0.816
	(1.462)	(0.626)	(0.640)	(0.646)
Log price index	-1.596^{***}	-1.268^{***}	-1.299^{***}	-1.423^{***}
	(0.139)	(0.047)	(0.053)	(0.047)
Constant	4.700^{***}			
	(0.202)			
Store-Brand		X	X	
Store-Week of Year		X	X	X
Brand-Month			X	X
Store-Brand-Year				X
Observations	2,740,781	2,740,781	2,740,781	2,740,781
R^2	0.330	0.868	0.869	0.891
Adjusted R^2	0.330	0.862	0.863	0.883
Residual Std. Error	1.435 (df = 2740773)	0.651 (df = 2616249)	0.648 (df = 2615789)	0.598 (df = 2553834)

Note: $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

All standard errors are two-way clustered at the DMA-brand and brand-week level.

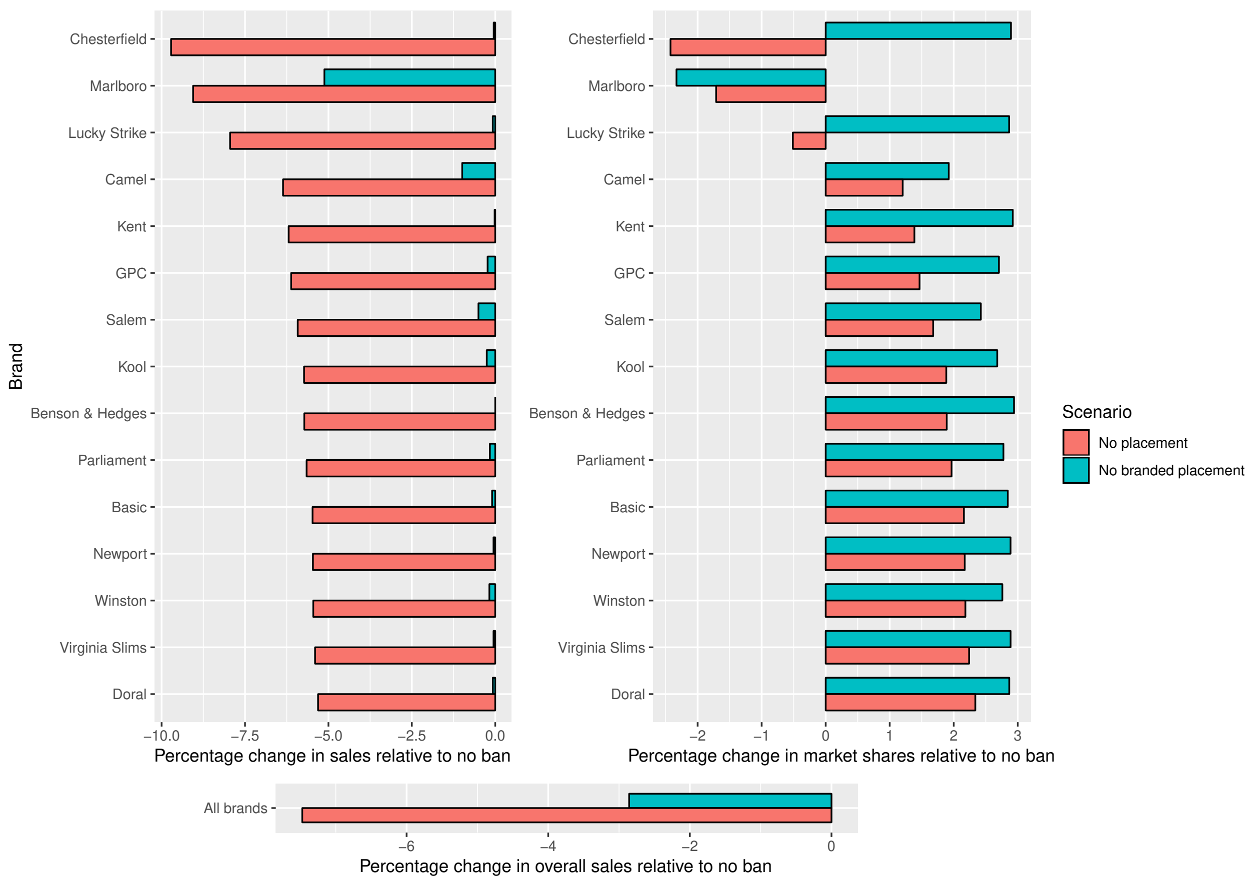

Figure A3: Change in sales and market shares under different scenarios while allowing for the product placement effect to differ for Marlboro.

E. Autoregressive errors

In the presence of autoregressive errors, the estimated coefficients for the goodwill variable could be biased. In this section, we use the Hildreth-Lu grid search procedure (Hildreth and Lu, 1960) to determine the optimal autocorrelation parameter ρ and adjust our variables to mitigate any bias that could arise as a result of autoregressive error.[15] Following the Hildreth-Lu procedure, we construct transformed versions of the dependent variable and independent variables as follows:

$$\tilde{y}_{ijt} = y_{ijt} - \rho y_{ij(t-1)}$$
$$\tilde{x}_{ijt} = x_{ijt} - \rho x_{ij(t-1)}$$

where i and j index store and brands, and t indexes time. The outcome is denoted by y and the independent variables including own/competitor goodwill, price, promotion, and feature are denoted by x. We re-estimate equation (2) and compare the MSE for across different values of ρ in Figure A4.

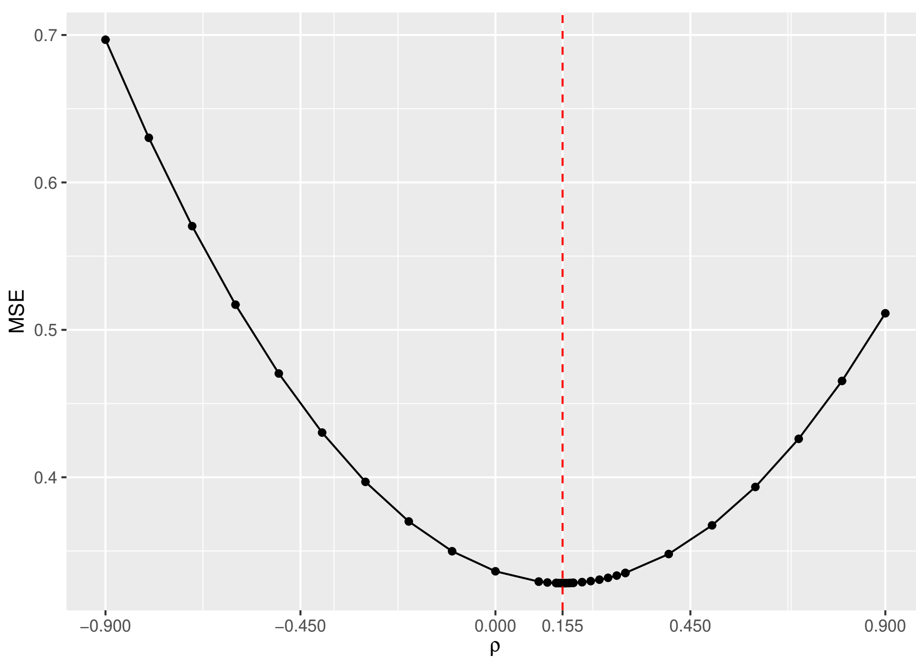

Figure A4: The mean squared errors corresponding to fitted lag models estimated following the Hildreth-Lu procedure.

The optimal MSE is attained for $\rho = 0.155$, which is then used to re-estimate equation (2) in the manuscript using the transformed dependent and independent variables described above. We present the results in Table A8. The own-brand and competitor-brand product elasticity estimates are 0.023 and 0.017, respectively. Overall, the patterns and main conclusions from this model look very similar to the results that we present in the paper.

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

Table A8: The effect of own and competitor product placement adjusting for auto-regressive errors.

	Dependent variable:			
	Log (Quantity + 1)			
	(1)	(2)	(3)	(4)
Log own placement goodwill	0.986***	−0.006	0.037***	0.023***
	(0.045)	(0.007)	(0.013)	(0.007)
Log competitor placement goodwill	−0.124***	0.048***	0.023***	0.017***
	(0.030)	(0.006)	(0.007)	(0.004)
Feature	2.041***	0.415***	0.428***	0.446***
	(0.107)	(0.036)	(0.036)	(0.038)
Display	2.426*	0.821	0.893	0.661
	(1.329)	(0.590)	(0.596)	(0.615)
Log price index	−1.638***	−1.297***	−1.326***	−1.489***
	(0.142)	(0.046)	(0.053)	(0.049)
Constant	4.122***			
	(0.175)			
Store-Brand		X	X	
Store-Week of Year		X	X	X
Brand-Month			X	X
Store-Brand-Year				X
Observations	2,715,161	2,715,161	2,715,161	2,715,161
R^2	0.293	0.836	0.837	0.857
Adjusted R^2	0.293	0.828	0.829	0.848
Residual Std. Error	1.275 (df = 2715155)	0.628 (df = 2590920)	0.626 (df = 2590475)	0.591 (df = 2549619)

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

Standard errors are two-way clustered at the DMA-brand and brand-week level.

F. Permutation test

In this section, we develop a permutation test to check whether our results are driven by spurious correlations. Our goal is to ensure that our results are indeed generated because of exposure to different levels of cigarette product placement, and not merely because of residual correlation in time or correlation between levels of own and competitor cigarette placement instances. Note that placement instances vary at the national level. If our results are being driven by placement instances rather than viewership, then we would be likely to get estimates of similar size if we shuffle the exposure intensity (measured in placement goodwill) at DMA level. In particular, impressions consist of two components: instances and viewership. If our results are a consequence of some spurious correlation in placement instances for brands at the national level and the variation in viewership is not helping in identifying the placement elasticities, then these "placebo" regressions should yield elasticity values that are often as large as those reported in Table 2.

To implement this idea, we permute the own and competitor goodwill at the DMA level and re-estimate the placement elasticities. Instead of using the true goodwill values, we randomly shuffle the goodwill values and replace each DMA's own and competitor goodwill values with those of another DMA. We then re-estimate equation (2) to generate new own-brand and competitor-brand elasticities. This process is repeated 1000 times. The histograms of own and competitor placement elasticities that were generated by this permutation test are displayed in Figure A5. The dashed red lines are the values of our estimate with the true goodwill values. For own-brand placement elasticity, the probability of having an estimate larger than the true estimate is 0.004, and for competitor-brand placement elasticity this value is 0.018. Interestingly, these values are similar to the p-values from Table 2. The fact that the permutation tests do not generate elasticity estimates that are similar to our true estimated elasticities indicates that placement viewership is the key contributor to our estimated elasticity results, rather than placement instances (which are identical across DMAs).

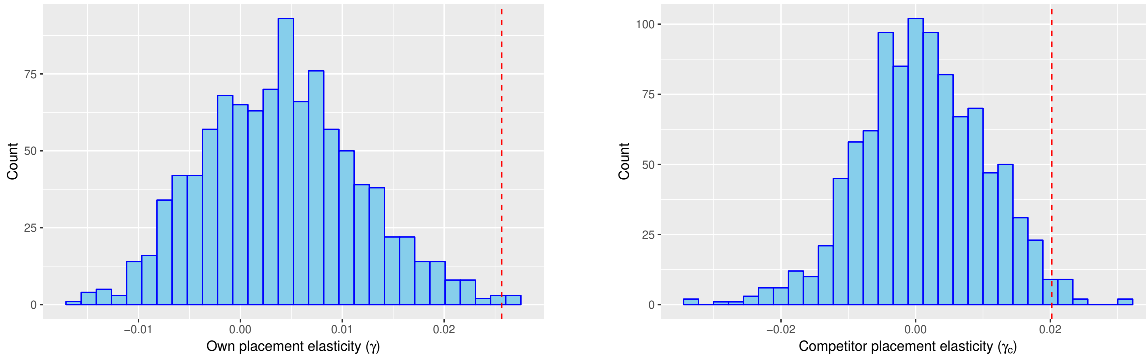

Figure A5: Histogram of elasticity estimates from the permutation test in which goodwill values are randomly shuffled across DMAs before re-estimating equation (2). Each red line represents the true elasticity value previously calculated in Table 2.

G. Time-reversed placement goodwill variables

While we do present a permutation test to show that the measured estimates are not a mere effect of spurious correlation in the time-series data, one might still be concerned about other confounds that could drive both viewership to the shows that depict smoking and smoking behavior across different geographic areas.

If there was a confound that led to both higher smoking rates and higher viewership for the shows that had tobacco product placement instances, then high tobacco sales should be correlated with both past viewership and future viewership of tobacco product placement. This allows us to generate two competing hypotheses:

- If there is a confound that led to higher viewership for shows that have tobacco product placement in some geographic areas, then both future and past tobacco product placement viewership should be correlated with current tobacco sales.
- If tobacco sales were generated because of exposure to tobacco product placement (rather than the confound described above), then exposure to past product placement instances should be correlated with sales but future tobacco product placement instances should remain uncorrelated with current sales.

Our analysis in the paper demonstrates a correlation between tobacco sales and past tobacco product placement. Therefore, to test which of these two hypotheses are correct, we construct time-reversed placement goodwill variables by discounting future instances rather than past ones:

$$\mathcal{G}_{bdt} = \sum_{l=0}^{\infty} \left[\delta^l \times \mathcal{P}_{bd(t+l)}\right] \tag{A5}$$

Subsequently, we re-estimate specification (2), except now using these time-reversed goodwill variables instead of the normal goodwill variables used in the paper. If the results were indeed caused because of spurious correlation between viewership of shows and smoking behavior, then we should still detect effects in this regression. The results are reported in Table A9 and the coefficients of both own and competitor product placement are statistically insignificant at $p < 0.05$.

Overall, these results show that while past exposure to tobacco product placement could explain current sales, future exposure to tobacco product placement does not explain current sales. This suggests that the results documented in the paper could not be due to a confound that gave rise to both viewership and tobacco consumption across geographic areas.

Table A9: The effect of future exposure to tobacco product placement on current sales.

	Dependent variable:			
	Log (Quantity + 1)			
	(1)	(2)	(3)	(4)
Log own placement goodwill (time-reversed)	1.063***	0.002	0.017	0.005
	(0.042)	(0.008)	(0.016)	(0.010)
Log competitor placement goodwill (time-reversed)	−0.187***	0.045***	0.002	−0.007*
	(0.026)	(0.004)	(0.008)	(0.004)
Feature	2.258***	0.399***	0.418***	0.447***
	(0.105)	(0.034)	(0.034)	(0.036)
Display	3.068*	1.053*	1.115*	0.816
	(1.653)	(0.633)	(0.643)	(0.653)
Log price index	−1.594***	−1.298***	−1.296***	−1.425***
	(0.139)	(0.045)	(0.052)	(0.045)
Constant	4.936***			
	(0.196)			
Store-Brand		X	X	
Store-Week of Year		X	X	X
Brand-Month			X	X
Store-Brand-Year				X
Observations	2,740,781	2,740,781	2,740,781	2,740,781
R^2	0.293	0.868	0.869	0.891
Adjusted R^2	0.293	0.862	0.863	0.883
Residual Std. Error	1.474 (df = 2740775)	0.651 (df = 2616251)	0.648 (df = 2615791)	0.598 (df = 2553836)

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

All standard errors are two-way clustered at the DMA-brand and brand-week level.

H. Category-level regressions

Our analysis in Figure 8 shows that a blanket ban on cigarette product placement would reduce cigarette sales by 6.8%. If our model fails to capture substitution between brands, this estimate could be biased. To alleviate this concern, we estimate the following category-level specification:

$$\begin{aligned}\log(1+Q_{st}) =& \beta\log(P_{st}) + \gamma\log(1+\mathcal{G}_{st}) + \xi f_{st} + \kappa d_{st} + \\ & \eta_{sw_t} + \eta_{m_t} + \eta_{sy_t} + \epsilon_{bst}\end{aligned} \tag{A6}$$

where Q_{st} is the total number of cigarette packs sold in week t at store s. The placement goodwill variable $\mathcal{G}_{st}$ is calculated by creating an aggregate goodwill variable for product placement across all brands. P_{st}, f_{st} and d_{st} are the price index, feature and display; these variables are calculated as quantity weighted averages across the brands.[16] The estimates are reported in Table A10 and indicate that a blanket ban on all placement instances would lead to a 6.3% reduction in category sales ($exp(0.061) - 1 = 0.063$). This number is very similar to the predictions from the brand-level regressions in Table 2 and Figure 8: 6.3% from the category-level regression vs. 6.8% from the brand-level regressions.

Table A10: The effect of product placement on weekly cigarette category sales.

	Dependent variable:			
	Log (Quantity + 1)			
	(1)	(2)	(3)	(4)
Log placement goodwill (all brands)	0.038	0.212**	0.075*	0.061**
	(0.028)	(0.096)	(0.043)	(0.025)
Feature	1.861***	1.882***	1.234***	1.626***
	(0.171)	(0.168)	(0.285)	(0.368)
Display	11.396**	11.628*	2.475	−1.384
	(5.785)	(6.237)	(1.654)	(1.784)
Log price index	−1.027***	−1.054***	−0.890***	−0.844***
	(0.213)	(0.216)	(0.164)	(0.258)
Constant	7.289***			
	(0.301)			
Month		X	X	X
Store-Week of Year			X	X
Store-Year				X
Observations	228,435	228,435	228,435	228,435
R^2	0.161	0.165	0.772	0.827
Adjusted R^2	0.161	0.165	0.590	0.676
Residual Std. Error	1.137 (df = 228430)	1.135 (df = 228399)	0.795 (df = 127425)	0.707 (df = 122471)

Note: *p<0.1; **p<0.05; ***p<0.01
All standard errors are two-way clustered at the DMA and week level.

The category-level specification in equation A6 helps address concerns about possibly over-estimating demand elasticities if the demand model does not properly account for cross-brand substitution. Another possible way to over-estimate demand elasticities would be if

[16] The weights were fixed in the panel and were calculated at store level.

our model does not properly account for forward-buying behavior. For instance, if exposure to product placement reminds customers to buy cigarettes and causes them to buy them before they have used up the cigarettes they already have at home, then our demand model would yield artificially high elasticities even though customers may not actually be increasing their smoking consumption.

We can address this potential concern by aggregating our weekly category-level demand model to the monthly level and re-estimating the demand model.[17] If this regression yields a null or statistically insignificant effect of product placement, that would suggest that the previous results may have been driven by forward-buying rather than actual increases in cigarette sales. The specification is identical to the previous category-level regression (equation A6), except now the time subscript t indexes months rather than weeks. The results of this regression are reported in table A11.

Table A11: The effect of product placement on monthly cigarette category sales.

	Dependent variable:			
	Log (Quantity + 1)			
	(1)	(2)	(3)	(4)
Log placement goodwill (all brands)	0.0001	0.172*	0.027	0.082**
	(0.024)	(0.092)	(0.025)	(0.034)
Feature	1.897***	1.911***	0.565***	0.779***
	(0.171)	(0.168)	(0.141)	(0.220)
Display	21.632***	22.280***	5.710***	0.504
	(4.848)	(5.383)	(1.028)	(1.165)
Log price index	−0.987***	−1.029***	−0.901***	−0.744***
	(0.186)	(0.191)	(0.139)	(0.155)
Constant	4.954***			
	(0.258)			
Store-Brand		X	X	
Store-Month of Year		X	X	X
Brand-Month			X	X
Store-Brand-Year				X
Observations	74,134	74,134	74,134	74,134
R^2	0.264	0.271	0.896	0.937
Adjusted R^2	0.264	0.270	0.834	0.888
Residual Std. Error	0.813 (df = 74129)	0.809 (df = 74096)	0.386 (df = 46643)	0.317 (df = 41683)

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

All standard errors are two-way clustered at the DMA and month block level.

The results from this monthly category-level regression indicate that a blanket ban

[17] Although we refer to this as a monthly regression, it is actually a "four-week level" regression. Note that the IRI sales data is reported at the weekly level. Aggregating to the true month level means that a given month could consist of four or five weeks depending on weekend endings, and this could change across years. Therefore, aggregation at the monthly level could create measurement error which would be exacerbated in the presence of month of the year (seasonality) fixed effects. To fix this issue, we aggregated the data to four-week blocks which we refer to as "months" in this analysis.

on all placement instances would lead to an estimated 8.5% reduction in category sales ($exp(0.082) - 1 = 0.085$). This is slightly higher than the results from the weekly category-level regressions we estimated above. However, the standard errors do also increase in size as the number of observations shrink and the estimates are statistically indistinguishable from each other. The results of this model demonstrate that overall category sales do in fact go up when product placement increases, which in turn suggests that brand switching and forward buying cannot be the primary factors behind our main results.

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