The Salience of Promotional Prices with In-Store Advertising

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Abstract

Using a data set from a natural field experiment conducted by a food retailer, this study estimates the effect of promotional signage on consumer demand for items on promotion. In the study, products were placed on a promotional discount for a two week period. At treatment locations the items received promotional signage, where at control locations the items had standard, non-promotional signage. The unique setting allows for us to provide a contribution to research on how attributes like promotional price are made 'salient' by contextual features, like point-of-sale advertising. We find placing promotional signage on an item increased the amount sold by 20-25%, independent of the effect of the promotional price.

Introduction

Retail consumers buying items off of a shelf with well-marked prices might reasonably be expected to be informed and aware of price. Yet, firms generally pair promotional prices with advantaged in-store placement and separate advertising. They do so under the theory the additional contextual features provided with the promotion help draw consumers' attention to the price discount. The effort expended by firms in this area is not small. Some estimates place large retailers' spending on promotions and promotional advertising at as much as 10-20% of their sales.[1] Further, based on survey evidence, the expenditure seems warranted.

[1]Delaney, L., & Blasberg, B. (2006, June 12). Trade Promotion Management: Why 'Crash Diets' Don't Work. Paper presented at GMA Executive Conference, Bain & Company.

Previous surveys have found a range of 50% or more of consumers who cannot remember the prices of retail items immediately after buying them (Dickson et al., 1990). These attention-directing contextual features are often described as attribute 'salience.' A product attribute is more salient when the framing of a decision directs consumers' attention to it. In-store promotional signage is meant to increase the salience of the discounted promotional price. This study provides a natural experiment of the price salience effect of in-store promotional advertising in a market setting, where changes in consumer demand can be causally attributed to in-store promotional signage. We are able to do this through a unique set of data from a field experiment. We find unit sales were between 20-25% higher for items in treatment store locations where promotional items had promotional signage relative to the same items with the same price in control store locations. This suggests price salience is, in fact, a major part of consumer demand and helps close a gap in the existing literature between laboratory studies and studies utilizing market data.

Existing estimates of price salience in advertising have twin problems associated with both laboratory investigations and research with market data. A concern with laboratory investigations is subjects often behave differently in the 'hypothetical' settings constructed in laboratories when compared to behavior in actual markets (Murphy, et al., 2005). Meta-analyses of the wording and presentation of pricing in advertising found laboratory studies find larger effects than studies conducted in the field (Krishna, et al., 2002). However, when looking at market data, effectively controlling the estimates becomes a concern. Not least among the potential confounding variables is price, which almost always changes alongside promotional advertising in a real market. It is difficult in practice to separately identify the effect of the price change and the salience effect (Cuellar, Noland, and Kirkwood 2012). Many studies with market data have an inability to observe promotional status at all, and have to infer promotional status from the extent of price discounts. There are two existing studies that most closely resemble our setting. First, Chetty Looney, and Kroft (2009) conducted an experiment at a grocery chain in which the researchers posted tax-inclusive prices for a group of treatment products. They found signage that displayed the tax-inclusive price of an item had an 8% decrease in demand. Based on this evidence and a model of boundedly-rational behavior, the authors conclude the assumption consumers optimize fully with respect to the incentives created by tax policies is complicated by the salience of the tax policy in purchase decisions. Second, Bemmaor and Mouchoux (1991) conducted a similar in-store experiment with twelve grocery items. The treatment items in their study were advertised with mailed leaflets, advantaged in-store placement, and were announced as 'on sale' over the stores broadcast system. They found a large difference between the sales lift associated with only a price change (20%) and the sales lift associated with a price change and promotional efforts (180%). The results presented later are consistent with both of these findings.

These results also tie to other research on contextual cues in retail. One of the most consistent results in research about retail consumers is that many cannot recall the exact price they pay for items (Zeithaml, 1988). Some survey evidence has indicated that price knowledge also decreases quickly after consumers make a choice (Dickenson and Sawyer, 1990). This suggests that many product attributes and contextual factors compete for consumers' attention. For instance, Phillips, et al. (2015) found that in-store product demonstrations can compete with promotions if they are close to one another in a store. If this is the case, in-store promotional signage would work mostly by out-competing other potential sources of consumer attention. This stands somewhat in contrast with models of price salience that attempt to explain differences in terms of price-quality trade-offs. Bordalo, Gennaioli, and Shleifer (2012) describe promotions that anchor consumers' expectation of prices by modelling consumers as reactive to 'quality salience' and 'price salience.' High regular prices increases the salience of price, and inflates a consumer's valuation of the item's quality. A testable implication of their model that is relevant to this experiment is that a price salient promotion only boosts demand for high-quality goods that are not available elsewhere. This theoretical result was meant to explain earlier empirical results that price decreases on higher-price retail items led to substitution from similar items in all price levels, but price decreases in lower-price retail items generally led to substitution from only other similar low-price items (Blattberg and Wisniewski, 1989). Interestingly, the highest priced items in our data did not see the largest increase in sales during the promotional weeks. Additionally, no particular qualitative factor seems to tie together the items that saw large increases with promotional signage. This suggests that price substitution and price salience do not necessarily act by inflating consumers' valuation of quality and presents a challenge for future research in this area.

The study proceeds in three sections. First, a discussion of the experimental design and the data. Next, a presentation of the empirical specification. Last, a presentation of the results and discussion.

Data

This study analyses a natural field experiment carried out by a food retailer. It was conducted in the field at five store locations in three states over three calendar weeks. The experiment was 'natural,' in the sense that items were sold as they would be in the absence of an experiment, and the subjects were those consumers who came into the locations for their regular business. Nine items were randomly chosen from the set of top one hundred selling dry-packaged foods for a two week promotional period. During the promotional period, all the locations placed the items at a price discount, but two treatment locations placed promotional signage on these items as would be done in typical promotion. In control locations the items were given standard, non-promotional signage.

The promotional signage was larger than regular signage, brightly colored, and featured the promotional price, alongside a description of how much the promotional price discounted the regular price. Also, the signage presented the start and end dates of the promotion. Most features of the sales context were the same across all locations. The food retailer charged the same price for all of its items across all the locations, sourced the products centrally, and distributed to all the locations. In-store placement was subject to a placement plan common to all locations, however, differences in the size and orientation of each location still exist. These differences, as well as differences between the market context of each location, motivate the inclusion of location-item level controls in the methodology discussed later.

Since the identifying variation between treatment and control comes from differences between location-item pairs, its worth discussing the differences between locations further. Two treatment and three control locations were selected by the food retailer so that the treatment locations had similar location and market characteristics as the locations acting as controls.[2] This selection was made by the firm, not the authors, so to evaluate the selections, Table 1 presents a comparison of the demographics of the markets for the treatment and control locations. For this comparison, markets were defined as the census ZCTA (Census-defined zip code tabulation areas) of the location, as well as all the immediately bordering ZCTAs. The data for the Table come from the 2010 Census and indicate the zip codes and neighboring zip codes of the treatment locations are broadly similar in population, mean and median income, and age distribution. The control locations are slightly wealthier, on average, and skew slightly younger. Overall, however, the selection of treatment and control locations appears independent of broad demographic features, making the likely consumers in these locations similar in observable characteristics.

The data are at a weekly level, spanning a calendar week, for fourteen weeks. The time period includes seven calendar weeks before the promotion and four calendar weeks after the promotion. The promotional changes, both in terms of prices and signage, were made in the middle of the calendar week. So, the whole promotional period appears in the data over a three calendar week period. In the first calendar week, the items were on promotion for the last five days, including both days of the weekend. In the second calendar week, the items were on promotion for all seven days. In the third calendar week, the items were on

[2]Originally, three treatment locations had been chosen. However, one of the treatment locations ran out of stock of several of the promotional items during the full promotional week, and was dropped from the analysis.

Table 1: Neighboring Zip codes by Treatment

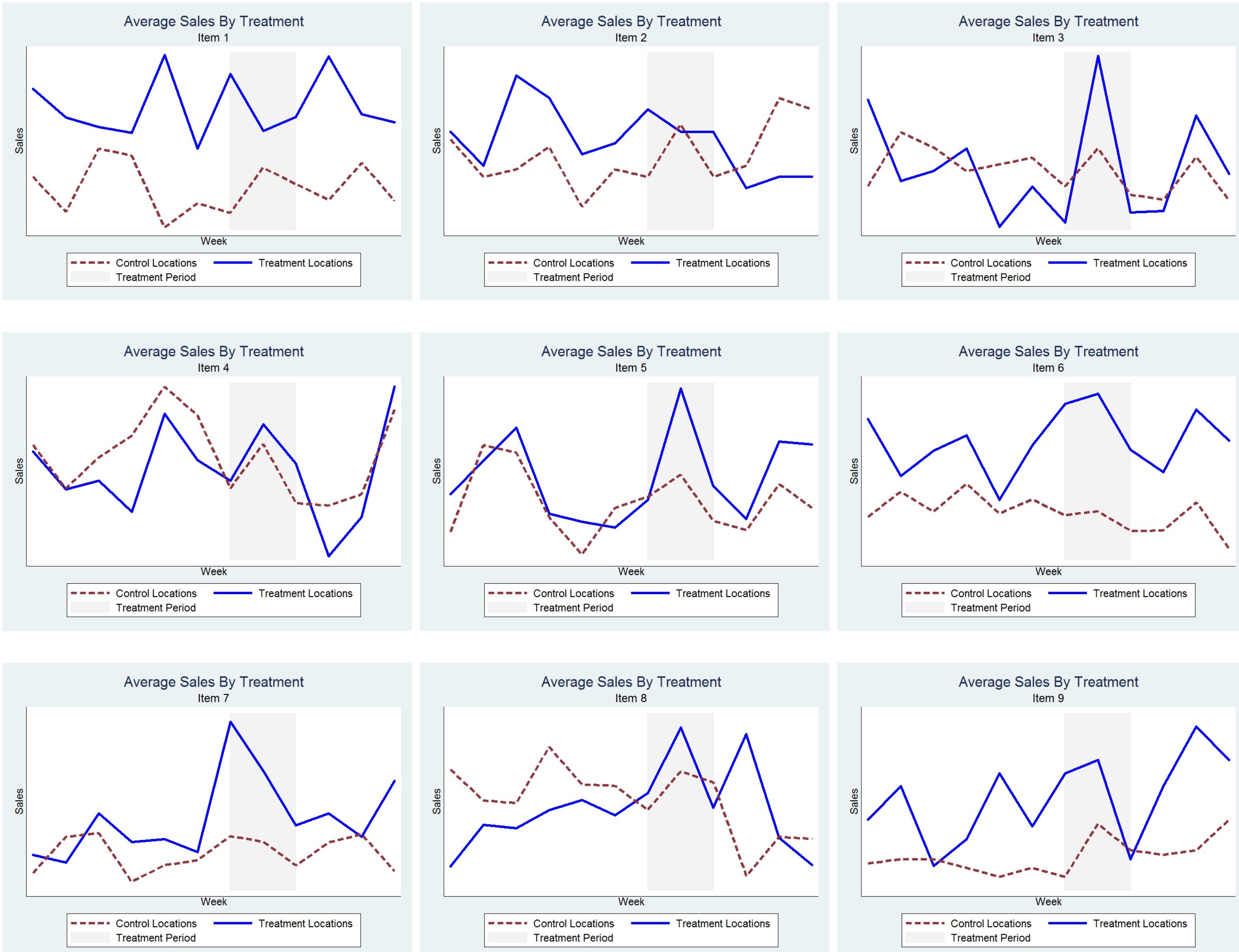
	(1) Treatment Locations	(2) Control Locations
Total Population	260,169	224,743
Median Household Income	$64,563	$68,301
Mean Household Income	$76,567	$79,746
Proportion less than 21 years old	0.17	0.23
Proportion 21-64 years old	0.69	0.67
Proportion 65+ years old	0.12	0.09

promotion for the first two days. How this difference in timing is handled in estimation is discussed below. The only other variable that changed mid-week along with the promotional signage was price. For the calendar weeks where the items were on a promotional price for a portion of the week, we assign the price its average weighted by the number of days the product was available at that price.[3]

Prices and quantities sold are observed across locations for the nine grocery items as a panel at weekly intervals. The proprietary nature of the data limit the extent to which identifying or descriptive information about the items can be presented, but in general the items are unrelated to each other, and represent a wide range of products. Three of the items are refrigerated, and the items are split between five 'scannable' items (items with a barcode) and three 'scalable' items (items which are weighed at the register). Three of the items are non-perishable, two of the items are perishable, but have long shelf-lives, and three of the items are perishable with short shelf-lives.

[3]Consumer purchasing patterns change across the week and other weighting schemes may be preferred but estimation is robust to alternative price constructions.

Figure 1: Average Sales by Item and Treatment



The graphs in Figure 1 present the mean unit sales for each item across weeks for the treatment locations and control locations. The three calendar week period is indicated as a shaded 'treatment period.' Seven of the nine items saw an increase in mean unit sales in treatment locations between the calendar week prior to the promotional period and the first calendar week that included promotional days. Of those items, only two items also saw a similar increase in control locations. By the second calendar week of the promotion, all the items in treatment locations had a higher level of mean unit sales than in the week prior to promotion, compared to all but two items in the control locations. Overall, the raw differences in the data indicate a potential differential effect in control and treatment locations in the treatment period. Items 3, 5, 6, and 7 in particular see a evidently larger increase in mean units sales relative to control locations in the promotional period. Items 4, 8, and 9 also appear to have some differential promotional outcome, but the scale is less clear. As discussed above, qualitative aspects of the items which are potentially associated with price salience are of theoretical interest. However, in the raw observed differences there is little pattern in the types of items which had larger increases in unit sales in treatment locations. The highest priced items at regular price were items 1, 2 and 7. The items with larger sales increases are also split in terms of department, perishability, 'scalable' vs 'scannable,' and type of packaging. Items 3 and 5 had placement on the sides of the stores, where items 6 and 7 had placement in the middle of the stores. In general, the relatively small number of items in the study make it difficult to generalize about how qualitative aspects of the items affected the promotions, but we did not observe high-priced items, or items with common obvious qualitative factors preform better over the promotional period.

Next, we describe our methodological approach to test these raw differences and control for potential unobserved factors.

Empirical Strategy

We employ a difference-in-differences framework, where 'individuals' in the experiment are item-location combinations, and the treatment is having a promotional sign. The outcome of interest for an item-location pair is log sales, q. The treatment is a promotional sign for an item-location, but has some complexity with the amount of time the item had the sign. The observations run Monday-Sunday. The first week that experiences any treatment had the promotion and signage for five days (Wednesday-Sunday), the second week had the promotion and signage during the entirety of the week and the final week, only experienced the promotion and signage on Monday and Tuesday. The timing of the start and end of the promotion potentially creates an incentive to time shopping trips differently in different weeks. Consumers average one and a half trips per week for retail food with the median consumer shopping once per week.[4] Most consumers likely experienced the treatment condition during the full seven-day promotional week, but some set of consumers could have timed their weekly trip differently in the first and third calendar weeks of the promotion.

To construct the treatment, first consider the nine items j in the five locations l across weeks w. For simplicity, let τ index the item-location-week combinations, with τ_1 representing item-location-week combinations that received treatment and τ_0 representing item-location-week combinations that did not receive treatment. The half-calendar-weeks, both before t_b and after t_a are given their own treatment indicator, along with the full week t_f. This could be interpreted as three separate treatment conditions: one treatment condition of promotional signage for the last five of seven days in a week (including both days of the weekend), one for a full week, and another for the first two days of the week.

In our framework, potential consumer demand is a function of observable characteristics μ_τ, mean-zero unobservable characteristics ϵ_τ, and the treatment condition t_i where $i = \{b, f, a\}$ indicates the three periods of treatment

[4] U.S. Grocery Shopper Trends 2015, Food Marketing Research Institute.

$$q(t_i|\mu_\tau) = \mu_\tau + \sum_i (t_i\beta_i) + \epsilon_\tau \tag{1}$$

.

The average treatment treatment effect t_i, is

$$E(q(t_i = 1|\mu_\tau) - q(t_i = 0|\mu_\tau)) = \beta_i \tag{2}$$

.

For item-location-week combinations in the treatment condition τ_1, we do not observe $q(t = 0|\mu_{\tau_1})$. That is, we do not observe log unit sales for item-location-week pairs in the treatment group absent treatment. For item-location-week combinations in the control condition τ_0, we do not observe $q(t = 1|\mu_{\tau_0})$. However, we do observe

$$q(t = 1|\mu_{\tau_1}) - q(t = 0|\mu_{\tau_0}) \tag{3}$$

,

which forms our estimate of the treatment effect. The standard difference-in-differences assumption is that this observed difference, conditional on observables, corresponds to the average treatment effect. This assumption has several parts. The first is the Stable Unit Treatment Value Assumption (Rubin, 1974). This assumption requires there be no "spillover" interactions between members of the control and treatment groups. One way this assumption would be violated is if the same consumers shopped at both treatment and control locations in the same week. Considering the closest treatment and control locations are more than four hours of driving apart, this is unlikely. The framework also assumes the treatment was not anticipated. Anticipation of treatment is unlikely for a few reasons. First, promotional items are not advertised as such until the beginning of the promotional period. Second, since the items were randomly selected, there would be no way for consumers to anticipate which items would be on promotion based on historical promotions.

The last and strongest assumption is the exogenous assignment to treatment. This assumption means expected differences in non-treatment demand are independent of belonging to the treatment condition τ_1, conditional on μ_{τ_1}. Under the assumption, if the treatment locations had regular signage during the promotional period, expected demand would have been μ_{τ_0}. We address the credibility of this assumption in three ways. First, we include seven pre-treatment weeks and four post-treatment weeks in order to capture information on underlying differences between treatment and control locations. Secondly, we specify our estimate of μ_τ to control for the broadest possible set of unobservable differences between treatment and control conditions. We also test the robustness of our results to specification choices by varying the control set to confirm our main results do not change. Last, we perform a randomization inference with 'placebo' trials under our preferred specification to conclude the actual week-location treatment combination has a large estimated effect size relative to a randomly selected treatment reassignment.

The assumption of exogenous assignment is violated if any unobserved factors of consumer demand vary between the treatment and control conditions either across time or in the treatment week in particular. As an example of this type of unobservable characteristic, which has the benefit of not fitting the particulars of these data, consider if both treatment locations happened to be near large universities, and the control locations were not. Additionally, if the promotional week happened to fall exactly during the time students were moving into dormitories and items on promotion were themselves related to stocking up a college refrigerator, then that effect would be wrongly attributed to promotional signage. We can control for some potential unobserved factors that would threaten identification through the specification of μ_τ. First, to control for differences between items, we include item-level fixed effects. These control for item-level differences in consumer demand that are time-and-location invariant. Next, to control for time-varying effects across all locations and items, we include week fixed effects. Last, to control for time-invariant differences across items in the different locations, we include location-level fixed effects. This makes the baseline specification of μ_τ

$$\mu_\tau = \lambda_l + \omega_w + \iota_j \tag{4}$$

at location l, in week w, for item j.

Although this is similar in form to other difference-in-difference studies (Bertrand, Duflo, and Mullainathan, 2004) the inclusion of different items across stores introduces variation that warrants a better control than separate item and location fixed effects. As discussed above, the 'individuals' in the study are location-item pairs, and so to more clearly control for 'individual-level' heterogeneity, we include location-item fixed effects in some specifications. This accounts, for instance, for time-invariant local variation in tastes, and differences in the specific layout and orientation of locations which might make certain items more or less popular.

Mechanically, it is possible to include item-week fixed effects as well, which we do in a separate specification for comparison and to test the robustness of results. This would control for any potential unobservable factor making the treatment items more popular in the treatment week than other weeks. However, even in the case this type of unobservable did exist, it would need to have a differential effect on treatment and control locations to bias the results. One particular type of item-week variation we consider separately is log price. Price cannot be included alongside item-week effects, but is the item-week-level factor with the most theoretical relevance to consumer demand. Together, the separate specifications including item-week fixed effects and price test the robustness of our main effect to item-week-level variation.

Our preferred specification of μ_τ combines week-level fixed effects ω_w, item-location-level fixed effects θ_{lj} and log price by item.

$$\mu_\tau = \theta_{lj} + \omega_w + \alpha_j ln(p_{wj}) \tag{5}$$

.

This specification best controls for the relevant unobserved factors that could violate the crucial exogenous assignment assumption. To return to the university example discussed earlier, the item-location-level fixed effects would control for the time-invariant differences in item-level demand between locations. In the 'move-in weekend' scenario, of course, no combination of fixed effects would make the assumption valid, since they would be perfectly co-linear with treatment. In the preferred specification, we control for general week-level shocks to demand across items and locations through week-level fixed effects, and item-week variations in price.

Outside of testing the robustness of the results to these specification choices, we provide another source of evidence for the credibility of our estimates through a randomization inference from placebo trials. Randomization inference was originally described by Fisher (1935), and has seen a resurgence of usage within economics with the increase in studies utilizing random assignment. One of the benefits of the approach is it is valid even when the number of groups in the data are small, as is the case here. The approach is to create a set of 'placebo' treatment conditions across all of the week-location combinations outside of the real treatment condition. Then, for each placebo trial, we estimate $\hat{\beta}$ as if it was the actual treatment condition. Let t_p be one of these placebo treatment conditions, and β_p the equivalent term as for the real treatment condition in Equation 1. From the empirical cumulative distribution function $\hat{F}(\hat{\beta}_p)$ of $\hat{\beta}_p$, across all the placebo trials, we test the null hypothesis $\beta_p = 0$ by finding the location of the estimated effect of the actual treatment $\hat{\beta}_p$ in $\hat{F}$. That is, the proportion of placebo trials with an estimated effect larger than what was found with the actual treatment is an exact p-value of the null hypothesis of no treatment effect and rejection of the null at any level of significance is the same as with a standard p-value (Athey and Imbens, 2017).

In order to include the before and after promotional week indicators, we could not simulate a placebo treatment in the first or last week of the data. With 10 potential weeks, and 2 treatment locations chosen from 5 possibilities, there are 100 total possible location-week combinations. 30 of those combinations include at least one of the treatment locations in at least one of the treatment weeks, leaving 70 unique placebo treatment combinations. Another result of randomization inference suggested by Bertrand, Duflo, and Mullainathan (2004) is the rate at which the null hypothesis is rejected across placebo trials. In testing a set of existing difference-in-difference estimates, the authors found an alarmingly large amount (45%) of placebo trials rejected the null hypothesis of no effect. In the results below, we report on both the p-value formed from the empirical distribution of $\hat{\beta}_p$ and the rejection rate across placebo trials.

Results

The results for OLS estimates of several candidate specifications are presented in Table 2. All specifications include the treatment condition t_f, for items in the treatment stores for the full seven-day promotional week. Also, all specifications include indicators for the 'before treatment' condition t_b, and for the 'after treatment' condition t_a. The 'before treatment' condition is an indicator for items in the treatment locations for the calendar week preceding the full promotional week. In this week, items were on the promotion for five days. The 'after treatment' condition is an indicator for items in the treatment locations for the calendar week after the full promotional week. In this week, items were on the promotion for two days.

In the first specification (1), the treatment indicators are included with item-level, week-level, and location-level fixed effects. Having a promotional sign was associated with a 19% increase in unit sales, but we cannot reject a null hypothesis of no effect. The second (2) and third (3) specifications test the robustness of this estimate to different sets of controls. Specification (2) includes week-level fixed effects and location-item-level fixed effects. Specification (3) includes week-item-level and location-item-level fixed effects. For both, the treatment indicator is only significant at a 10% level, and the effect is estimated to be a 22% and 20% increase in sales, respectively. Location-item effects are included in all subsequent specifications.

Week-item-level fixed effects, especially in combination with store-item-level effects, are more problematic, since they are highly co-linear with treatment. Their inclusion in specification (3) indicates a positive effect for promotional signage and is robust to controlling for this variation, but it is possible some of the actual effect is being included in the estimates of week-item-level fixed effects. Notably, specification (3) has the smallest estimated effect size for promotional signage. The largest source of week-item-level variation not due to treatment is price, and specification (4) controls for this directly. As expected, the estimate for the effect of promotional signage is larger, at 25%. In specification (4), we can reject the null hypothesis of no effect at a 5% level.

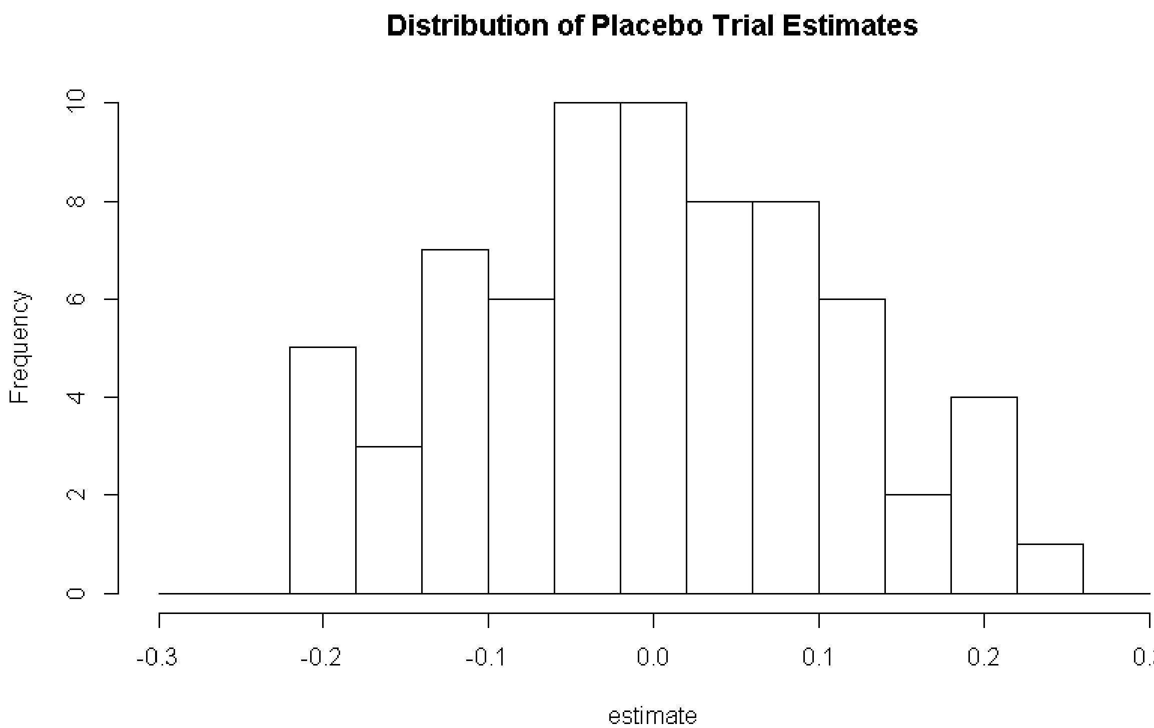
Across all specifications, the estimate associated with the 'before treatment' indicator is larger than the main treatment indicator, and we may reject the null hypothesis in all cases at least at a 5% level. Any interpretation of this effect should be be done cautiously, since the first promotional week mixed promotional and non-promotional sales. Some speculation around the potential reasons for an effect of this kind are discussed below, but if anything, the results for the 'before treatment' indicator likely re-enforce the finding that promotional signage has a positive effect on consumer demand. The first promotional week had five of seven days on promotion, including high-sales weekend days, so a 31-36% increase in unit sales is consistent with a positive effect for promotional signage. The 'after treatment' condition had only two days of promotion, both weekdays, and even though estimates are positive across all specifications, we cannot reject a null hypothesis of no effect. Both the estimates for the 'before treatment' condition and the 'after treatment' condition suggest having promotional signage for more days of a week increases item sales.

Table 2: OLS Results

	Dependent variable:			
	Log Unit Sales			
	(1)	(2)	(3)	(4)
Treatment	0.196	0.223*	0.199*	0.254**
	(0.144)	(0.126)	(0.112)	(0.107)
Before Treatment (5 days)	0.314**	0.341**	0.312**	0.364***
	(0.187)	(0.133)	(0.129)	(0.131)
After Treatment (2 days)	0.0653	0.0925	0.0674	0.105
	(0.170)	(0.144)	(0.141)	(0.146)
Item Fixed Effects	X			
Week Fixed Effects	X	X		X
Location Fixed Effects	X			
Location-Item Fixed Effects		X	X	X
Week-Item Fixed Effects			X	
Log Price-Item				X
Observations	506	506	506	506
R^2	0.810	0.911	0.929	0.915
Note:	*p<0.1; **p<0.05; ***p<0.01			

Next, as another test of the robustness of the results, we estimated our preferred specification (4) across 70 placebo trials. This created a set of treatment effect size estimates ranging from -0.213 to 0.257, with a mean and median of 0. The distribution of the estimates in included in Figure 2. The p-value of the actual results against the null formed by the placebo trials is 0.014, which means we can reject the null hypothesis of no treatment effect at the 5% level. Additionally, for none of the estimates in the placebo trials would we have

Figure 2



been able to reject the null hypothesis of $\beta_p = 0$ at a 5% significance level. Both these randomization inference results indicate none of the other week-store combinations in the data would have led to treatment effect estimates found in the OLS results.

Discussion

One surprising finding was the strong effect on unit sales for promotional signage in the five-day promotional week preceding the full seven-day promotional week. The fact we found a large positive effect for the five-day week is consistent with our findings for the full promotional week, but that the estimated effect would be larger is not expected. There are a few potential explanations for this. First, consumers demand for the promotional items could be less than one unit per week on average, so many consumers who had already purchased them in the five promotional days of the first promotional week did not want them again in the second. Although the average consumer grocery shops once a week, it is possible the standard size for these items lasts the average consumer more than a week, or the items are not the sort of item consumers typically buy weekly. Although some of the items are perishable, consumers could be 'stocking up' on the non-perishable items when they first encounter the promotion as part of their home inventory process. Last, it is possible the promotional price encourages marginal consumers to try a new product, and those who find they do not like it do not follow up with a purchase in the second week.

Unlike Blattberg, and Wisniewski (1989), we did not find a differential effect between high-price items and low-price items, which has implications for some theories of salience like Bordalo, Gennaioli, and Shleifer (2012). We did not observe higher price items have the largest increase in sales in promotional weeks, as would be predicted by a model with a price-quality trade-off. The small number of items under study means that the results here are far from dispositive on any particular explanation, but do seem most consistent with a general price salience effect, in which consumers notice the item and the potential for a 'deal' based on promotional signage. Further, we were unable to discern any important qualitative features that might have driven the between-item differences in the size of the effect. It possible that a range of other contextual factors provides the best basis for explaining our results, relative to a explanation that describes a revaluation of quality based on a reference price.

Overall, the results are indicative of a 20-25% increase in unit sales for items caused by the addition of promotional signage. The promotional signage appears to have increased demand for these items by drawing consumers' attention to the discounted promotional price. Given the unique setting, the results here do not suffer from a potential 'hypothetical bias' that might be the case in laboratory studies, but also separately identify the effect of the signage alone, as studies using market data are generally not able to do. Retailers' efforts and expenditures in placing items on promotion make sense when consumers' attention is drawn in many directions. Even when provided with clear information at the point of purchase, a change in price might go unnoticed without a brightly colored, attention grabbing assist.