The Impact of Advertising along the Conversion Funnel*

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We assemble a unique dataset that combines information on supermarket feature advertising with path-tracking data on consumers' movement within the store as well as purchase information. Using these novel data, we trace out how advertising affects consumer behavior along the path-to-purchase. We find that advertising has no significant effect on the number of consumers visiting the category being advertised. The null effect is precisely estimated, and even at the upper bound of the confidence interval, a one-standard-deviation shift in advertising increases category traffic by only 1.3%. On the other hand, we do find a significant effect at the lower end of the conversion funnel. A one-standard-deviation change in advertising (evaluated at the point estimate) increases category-level sales by 10%. We further decompose the impact on sales and find the increase is driven by the same number of consumers buying a larger number of products. We find no evidence of spillover effects of advertising between categories that are stocked in proximity of each other, nor between different products in the same category. Based on these patterns, we discuss potential mechanisms.

Keywords: Advertising, Conversion Funnel, Spillovers, Path-tracking Data

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1 Introduction

Advertising is an important part of firms' marketing mix and plays a crucial role in many industries. In the retail industry, advertising spending was over 10 billion dollars in 2014, which equals 1.4% of gross sales (in comparison, net profits were 1.5% of gross sales).[1] Feature advertising, whereby stores promote specific products using newspaper inserts and store fliers, accounts for 42% of the advertising budget and is the focus of our analysis. Given the large amount of spending, marketers must understand the effectiveness of feature advertising at different stages of the conversion funnel. Does advertising bring consumers to the store? Does it increase traffic to specific parts of the store? Does advertising increase product sales? Understanding which parts of the conversion funnel are affected is relevant for managers in assessing the overall impact of their advertising efforts. If advertising a specific product brings additional consumers to the store, those additional consumers also likely purchase other (non-advertised) products in the store. If, instead, advertising is most effective at the lower part of the conversion funnel and increases purchases by consumers that would have visited the category even in the absence of the ad, the impact of advertising will be narrower and confined to the category or even only the specific product being advertised.

Despite the practical relevance of decomposing the conversion funnel and the perception that advertising can benefit the store more broadly by increasing traffic (Bodapati and Srinivasan (2006), Chan et al. (2006)), the empirical evidence on this issue is relatively scant. We posit that this sparsity in research is most likely due to a lack of appropriate data. Marketing researchers have traditionally only been able to observe consumers' purchases, and hence most of the advertising literature focuses on analyzing the impact of advertising on sales, but does not explore the different stages of the conversion process.

In this paper, we make use of a novel data set that allows us to observe consumer behavior in a brick-and-mortar store at a greater level of detail. Specifically, we use a data set of consumer "path-tracking" information obtained from radio-frequency identification (RFID) tags that are attached to consumers' shopping carts. This data set allows us to track precisely which path the consumer took through the store as well as where she was located in the store at each point in time. Combined with data on product locations, this approach allows us to measure whether a consumer visited a particular product category, at what time during her trip she made the visit, and how much time she spent in front of the shelf. For the same set of consumers, we also observe purchases as well as the feature advertising they were exposed to across a large set of categories. Using all these pieces of data together allows us to investigate the impact of advertising onto parts of the consumer's decision process that are typically not observed.

More specifically, the research questions we address in this paper are the following. First, we analyze at what stage of the conversion process advertising has the largest impact on consumers. Second, we analyze the consequences of this decomposition in terms of cannibalization and/or spillover effects from advertising. To answer the first question, we make use of the path-tracking

[1] "The Food Industry Speaks 2015," Food Marketing Institute.

data on consumers' movement within the store and analyze whether advertising affects the number of consumers visiting a particular category. We then analyze the impact of advertising on purchase behavior conditional on visiting the category. To answer the second question, we analyze spillover effects across categories that are stocked near each other in the store, as well as between individual products within each category.

Our paper establishes several key findings. First, we investigate *whether advertising drives foot traffic* to the advertised category. We implement this analysis by regressing the number of consumers visiting a specific category on a given day on the number of advertised products in that category, while controlling for category fixed effects and other marketing activity, namely, price reductions and product displays. Surprisingly, we find that feature advertising *does not* increase traffic toward featured categories. The null effect is precisely estimated and, even at the upper bound of the confidence interval, a one-standard-deviation shift in the number of advertised products increases daily category traffic by only 1.3%. Therefore, any possible increase in sales must be driven by an effect of advertising on purchase behavior conditional on visiting the category.

Second, we analyze *whether (and how) advertising affects purchases*. We find the number of advertised products in the category has a significant impact on category sales. A one-standard-deviation increase in advertising leads to a 10% increase in purchase quantity. When decomposing the effect, we find the increase in sales originates almost entirely from one specific margin of adjustment: consumers purchase a larger number of different products from a given brand in response to advertising. However, the number of consumer purchasing in the category remains unchanged, and so does the number of brands individual consumers purchase. The quantity purchased of a given product is also not significantly affected by advertising. Together with the results from the traffic data, this sales decomposition paints a detailed picture along which margins advertising is able to affect consumer behavior. Advertising does have a significant impact, both in a statistical and economic sense, on the final outcome variable of interest, quantity sold. However, along the conversion funnel, advertising is ineffective at various stages of the process. It does not increase category traffic, nor does it convert a higher number of consumers to buying in the category or to purchase a specific brand. Instead, the effect is primarily driven by the same number of consumers purchasing additional products (i.e. different flavors or varieties) from the same brand.

Third, we investigate advertising spillover effects both within categories (between different products) as well as across categories that are stocked in proximity of each other in the store. We implement the latter analysis based on a detailed map of the store that allows us to define the location as well as the set of nearby products for each category. We find no evidence that advertising in a specific category increases purchases in other nearby product categories. Furthermore, within categories, we do not find evidence of positive advertising spillovers between different products. Instead, the impact of advertising is confined to the specific product being advertised and sales for other products in the category are unaffected.

Finally, we discuss behavioral mechanisms that are consistent with the data patterns. We posit two alternative scenarios that can explain our set of findings and especially the fact that advertising is only effective at the lower end of the conversion funnel. One possible explanation is that the consumer might be exposed to an ad without taking any immediate action. Instead, she only retrieves the memory of the ad when she is in front of the aisle and interacts with the advertised category and brand. This type of memory retrieval based on an external stimulus can thus explain the presence of an effect only at the lower end of the conversion funnel. Alternatively, it could be the case that only consumers that were already planning to purchase the brand choose to pay attention to the ads for products belonging to the specific brand. Such self-selection into advertising consumption will similarly lead to an absence of an effect in earlier stages of the conversion funnel.

Our paper contributes to several strands of literature. First, it extends the work using data on consumers' within-store movement, such as Hui et al. (2009a) who document shoppers' deviations from the most efficient path through the store, and Seiler and Pinna (2016) who estimate the benefits from search in terms of price saving from longer in-store search. Hui et al. (2013a) and Hui et al. (2013b) both analyze unplanned shopping behavior using video-tracking and RFID tracking technology, respectively. To the best of our knowledge, none of the prior papers in this literature combined advertising data with data on consumers' movement within the store. Understanding how marketing activity affects consumers' path-to-purchase can yield important new insights, and we see this paper as a first foray into this research area.

Apart from path-tracking studies in a brick-and-mortar store context, another application of similar methods is from online browsing data. A range of papers have investigated consumer search behavior in this realm (see, e.g., Kim et al. (2010), De Los Santos et al. (2012), Bronnenberg et al. (2016), and Chen and Yao (2016)), but mostly focus on estimating the primitives of the search process such as consumer search costs and preferences. The impact of advertising and other marketing tools is not typically the focus of the analysis. We conjecture that certain patterns we find in the physical store setting of our paper might look different in an online context due to the fact that navigating through a brick-and-mortar store is more costly and less flexible than online browsing.

A third stream of literature that we contribute to is the literature on measuring advertising spillovers. Sahni (2016) quantifies spillovers effects in the context of online advertising and analyzes between which types of products spillovers tend to occur. Lewis and Nguyen (2014) provide evidence for spillovers to competing firms in online search behavior following an ad exposure. Anderson and Simester (2013) show spillovers exist for products sold by catalog and that they are most prevalent in categories with higher switching costs. Shapiro (2016) estimates spillovers in the context of pharmaceutical advertising. Sahni et al. (2016) find that email coupons generate spillovers to products to which the coupons do not apply. By contrast, in this paper, we find no evidence for advertising spillovers either between categories or between products within the same category. The divergence in our results from the prior literature on spillovers could be due to the more rigid nature of product search and discovery in a physical store relative to online or catalog search.

Finally, we contribute to the literature on measuring advertising effects more broadly (see the summary in Bagwell (2007)). However, we differ from most of the prior literature by focusing not only on the impact of advertising on purchases, but also on consumers' movement through the store. Furthermore, we provide a decomposition of the sales effect of advertising into the impact on the number of consumers purchasing in the category, as well as the number of products, brands and quantity purchased per consumer. Two papers that also investigate the role of advertising along different stages of the consumer's decision process are Honka et al. (2016) and Johnson et al. (2016) in the context of financial services and internet advertising, respectively.

The remainder of the paper is structured as follows. In Section 2, we present the data and descriptive statistics. In Section 3, we analyze the impact of advertising on category traffic and sales. In Section 4, we discuss identification and provide an extensive set of robustness checks. Section 5 investigates spillover effects. Section 6 discusses the possible underlying mechanisms that are consistent with our empirical findings and Section 7 concludes.

2 Data and Descriptive Statistics

Our data come from two sources. First, we obtained data from a large store in Northern California that belongs to a major supermarket chain (we are not able to disclose the identity of the supermarket). The store has a fairly typical format with a trading area of about 45,000 square feet and a product range of 30,000 products. For this store, we observe individual-level purchases as well as data on the path a consumer took through the store for a subset of shopping trips over a period of 26 days (8/24/2006 - 8/29/2006 and 9/7/2006 - 9/26/2006). In terms of the purchase data, we have information on all consumers that visited the store during these 26 days. For each shopping trip, we observe the full basket of products as well as the price paid for each item. Furthermore, we are able to link the path data to the corresponding purchase baskets. In section (A.1) of the appendix, we provide details on how the two pieces of data are combined. Finally, we have detailed information on the location at which each product is stocked in the store.

We complement these data with a second piece of data containing information on feature advertising from the IRI data set (see Bronnenberg et al. (2008)). Below, we provide more details on the path data as well as how the feature advertising and path data are merged to form the final data set.

2.1 Path Data

We record the paths consumers take when walking through the store, using RFID tags that are attached to their shopping carts and baskets (see Sorensen (2003) and Hui et al. (2009b)). Each RFID tag emits a signal approximately every four seconds that is received by a set of antennas throughout the store. Based on the signal, triangulation from multiple antennas is used to pinpoint the consumer's precise location. The consumer's location is then assigned to a particular point on a grid of "traffic points," which are overlaid onto the store map and are about four feet apart from each other, thus allowing for a fairly granular tracking of the consumer. For every path, we observe a sequence of consecutive traffic points that the consumer passed on her shopping trip with a time stamp associated with each point.[2] We also note that not all shopping carts and baskets in the store are equipped with RFID tags, and we therefore only observe path data for 7% of all store visits. We use the path data to derive our key outcome variable: the daily number of consumers visiting a particular product category.

To define category visits, we first find the locations of all relevant products for a given category and the traffic points associated with the set of products belonging to that specific category.[3] For each shopping trip, we consider the consumer to have visited the category if, during her trip, she was located on a certain number of traffic points associated with the category. In our baseline definition of a visit, we require a trip to cross at least three traffic points pertaining to the category. We also compute how far into the trip the consumer walked past a specific category, as well as how much time the consumer spent at a specific category's location. The former is obtained by calculating the time elapsed between the beginning of the trip and the moment at which the consumer is first located on a traffic point associated with the category. The latter records the total time a consumer spends on traffic points associated with the category.

Figure 1 illustrates the definition of these variables for a specific trip (indicated by the dashed line) and category. The figure depicts an illustrative aisle of the supermarket that stocks the focal category at the lower right-hand side of the aisle. A series of traffic points inside the aisle, as well as at the lower end, are considered to be in the vicinity of the category and are used to identify whether the consumer visited the category. In this example, the consumer passed six traffic points associated with the category, and hence her trip qualifies as a visit to the category. To compute the timing of the category visit, we retrieve the time stamp when the consumer is first located on one of the relevant traffic points (in this case, the lowermost traffic point inside the aisle) and calculate the time elapsed since the start of her trip. Finally, dwell-time is measured by the total amount of time spent on traffic points belonging to the specific category (i.e., the six traffic points in the lower part of the aisle).

We aggregate all three variables (category visits, visit timing, and dwell-time) to the category/day level for our empirical analysis. In the case of category visits, we calculate the total number of consumers visiting the specific category each day. With regards to visit timing and dwell-time, we compute the average value of the respective variable at the category/day level.

Finally, we define a product "pick-up" as our purchase outcome. A pick-up is recorded if a product is observed in the consumer's checkout basket *and* the consumer visited the relevant category. We therefore do not count purchases for which no path data are available. Hence, both traffic and purchase outcomes are based on the same sample of consumers. Due to the fact that the traffic data only cover 7% of all store visits, we re-scale the daily traffic count and the number of product pick-ups by (1/0.07).

[2] If a consumer moves farther than to an adjacent traffic point between signals, the movement over traffic points in between the signals is interpolated. Because the signal is emitted at a high frequency, little interpolation is necessary for most trips.

[3] The data provide the linkage between traffic points and product locations. Most product locations are associated with two or three traffic points.

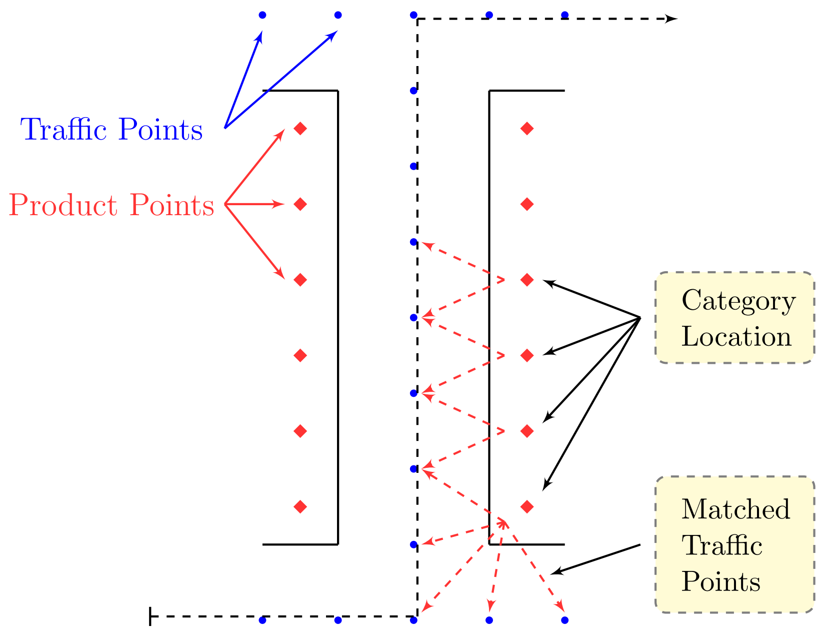

Figure 1: **Data Structure.** Consumer location within the aisle is recorded on a grid of traffic points. Products are located at specific locations on the shelf, which are coded up as a grid of product points. Product points are matched to nearby traffic points. The dashed black line depicts the consumer's path when traversing the aisle.

2.2 Feature Advertising Data

We supplement the purchase and path data with additional information on feature advertising, which we obtain from the IRI data set. The store-level IRI data contain purchase information, feature advertising at the product/store/week level, as well as information on price and product displays. We only make use of the IRI data in a limited way by complementing our main data set with the relevant feature advertising (and display) information, which is missing from the path-tracking data. A product is considered to be featured if it appears in the supermarket's weekly advertising leaflet such as the one displayed in Figure 2 (we display half of a page of a feature advertising leaflet for a store similar to the one in our data).

Unfortunately, the store for which we have the path data is not itself contained in the IRI data, which only contain a sample of stores. However, for the purpose of obtaining information on feature advertising, this issue is not problematic, because feature advertising is usually implemented at the market level, which allows supermarket chains to only provide one advertising leaflet for the entire market (Mela et al. (1997), Blattberg and Neslin (1990)). We are hence able to infer the relevant feature advertising information from several stores of the same chain that are located in the same market (Northern California) and are contained in the IRI data set. We note that although most of our analysis is conducted at the daily level, feature advertising only changes at weekly intervals. Our final data set covers four weeks and hence contains four sets of featured products per category.

One relevant aspect of feature advertising is that it is frequently implemented at brand (Yoplait

Figure 2: **Example of a Feature Advertising Leaflet (half a page is shown).**

yogurt) rather than the product level (Yoplait vanilla flavor).[4] In all regressions reported in the paper, we use the number of featured *products* as the main measure of feature advertising activity in the category. We also implemented all of our key regressions using the number of featured *brands* as the main regressor and found results to be similar both directionally and in terms of effect magnitude.

We also use the IRI data to compute a proxy for product displays at the path-data store. Product displays, in contrast to feature advertising, are often store specific, and hence we cannot perfectly predict product displays from other stores of the same chain. We nevertheless compute

[4]We find that for 88% of all brand/week combinations in our data, feature advertising status is identical across all products within the specific brand/week (i.e., products within the same brand are all featured or not featured).

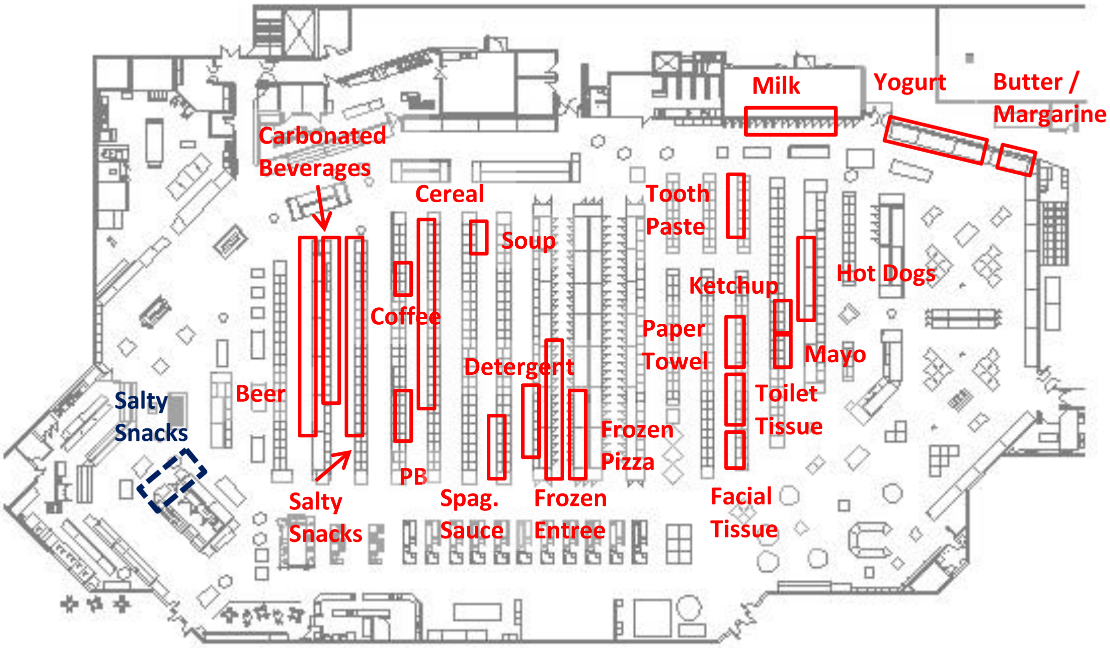

Figure 3: **Store Map with Category Locations.** Primary locations for all 21 categories are depicted using solid rectangles. An illustrative secondary location for the salty snacks category is depicted using a dashed rectangle.

a proxy for product displays by calculating the fraction of stores of the same chain that displayed a particular product in a given week, which we interpret as the display probability for the specific product at our focal store. To the extent that stores display similar products,[5] this proxy will allow us to capture the likelihood of a specific product being displayed. We later run a set of robustness checks (in Section 4.3) to assess whether the imperfect measurement of displays affects our results.

2.3 The Final Data Set: Merging and Category Selection

Our final data set comprises 1,200 products in 21 categories across 26 days. The IRI data set constraints our data set in terms of categories, whereas the path-tracking data limit the time horizon. IRI contains information on 31 categories, but some of those contain very few products that are rarely purchased and other categories are never featured, and thus do not provide relevant variation for our analysis. We therefore end up with 21 categories in our final data set.[6]

The primary locations of these categories within the store are displayed in Figure 3. As the figure shows, the categories in our data are fairly spread out in terms of their locations, and they also cover a broad set of category "types" such as food and household items, storable and perishable

[5]The average pairwise correlation of displays (across all categories and weeks) between stores of the same chain in the same market is equal to 0.50.

[6]The categories in IRI not included in our analysis are razors, razor blades, cigarettes, deodorant, diapers, household cleaner, photo, shampoo, sugar substitutes, and tooth brushes.

items, etc. The one omission from the set of categories is fresh food, such as produce or fresh meat, which is missing from the set of categories provided in IRI and hence is not part of our analysis. We also note that many categories are stocked in several different parts of the store with a "primary" location in an aisle in the center section of the store as well as additional "secondary" locations in the open areas to the left and right of the primary aisles, as well as on top of the aisles, opposite the entry/exit of the store. The map in Figure 3 depicts all primary locations and an illustrative secondary location for the salty snacks category in the open area in the left part of the store.

2.4 Descriptive Statistics

We start by providing an overview of the traffic and sales patterns across the categories in our data. The first two columns of Table 1 report total daily category traffic as well as the share of traffic relative to the total number of consumers visiting the store. For simplicity of exposition, the 21 categories are ordered in descending order by their traffic share. We find substantial heterogeneity across categories in terms of the amount of traffic they are exposed to, ranging from over 90% for carbonated beverages to below 10% for butter and margarine.

Columns (1) and (2) are based on all product locations of each category in the store, that is, both the primary location in the aisle as well as any secondary locations. Because secondary locations generally tend to receive more traffic, presumably because they need to be traversed to reach other parts of the store, we also provide an alternative definition of traffic based on primary locations only. Traffic numbers are generally lower, but still vary substantially across the different categories. For some categories, the difference between total traffic and primary location traffic is large, and the gap can be up to 70 percentage points in the case of carbonated beverages. Due to these pronounced differences, we later analyze traffic flows separately for primary and secondary locations. Furthermore, we note that primary locations are typically signposted with the names of the categories stocked in the specific aisle, and hence visits to those locations are likely to be more indicative of consumers explicitly seeking out the category.

We also report category-level sales in terms of total quantity as well as the number of consumers purchasing in the category. We find large heterogeneity in sales levels as well as conversion rates of visiting consumers (captured by the traffic count) to sales.

Finally, we report the number of products / UPCs (universal product codes), in each category and the average number of featured products as well as the standard deviation of featured products. On average, about 10%-20% of UPCs are featured on any given day within a category. Importantly for our empirical analysis, substantial variation exists in the number of featured products (within categories). We also note that different types of marketing activity are not strongly correlated with each other, and we can hence isolate the effect of feature advertising from the impact of other marketing variables such as promotions and displays. The correlation between the number of feature ads and price promotions (displays) in our sample is equal to 0.35 (0.36) after controlling for category fixed effects. In a larger sample of comparable stores in the IRI data (which we use in a robustness check in Section 4), these correlations are even lower and equal to 0.19 (0.09). Furthermore, featured products are not labeled differently in the store (see Figure A1 in the appendix). Correspondingly, featured products are not visually more salient than other products in the store.

3 Decomposing the Impact of Advertising

3.1 Category Traffic

We start by analyzing the impact of feature advertising on category traffic. As noted earlier, researchers have typically not analyzed this part of the conversion funnel because of the lack of information on consumers' movement within the store. The path-tracking data provide us with a unique opportunity to unpack the effect of advertising by analyzing this "upper level" of the conversion funnel. To the best of our knowledge, this paper is the first to provide such an analysis.

Our empirical strategy is to regress daily category traffic onto the number of featured products within that category, as well as category and day fixed effects, and controls for other marketing activity. Standard errors are clustered at the category level.[7] Formally, we estimate the following regression:

$$Traffic_{ct} = \alpha \times FeatureNum_{ct} + X'_{ct}\beta + \delta_c + \theta_t + \varepsilon_{ct}, \tag{1}$$

where $Traffic_{ct}$ denotes category traffic, that is, the number of consumers visiting category c on day t. $FeatureNum_{ct}$ denotes the number of featured UPCs, X_{ct} denotes a vector of other marketing variables. Specifically, we include the number of promoted items in the category, the average category-level price,[8] and the number of displayed items. δ_c and θ_t denote category and day fixed effects, respectively. ε_{ct} is the regression error term. We would not expect price to be an important control in the traffic regression, because product prices are usually not known to the consumer before reaching the shelf. We nevertheless maintain the number of promotions and average price as control variables in the traffic regression in order to make the regression as comparable as possible with the later regression of sales onto feature advertising and other marketing variables (where price controls are more important).[9]

Our baseline specification defines a category visit as a trip that passes at least three traffic points which are associated with the category and is based on all product locations of the category. Column (1) of Table 2 reports the results from this regression. We find that the number of features has *no statistically significant impact* on category traffic (p-value of 0.707). Furthermore, the coefficient on the number of features is not only insignificant, but also small in magnitude. Featuring one

[7]We also implement the wild bootstrap method that Cameron et al. (2008) propose for settings with a small number of clusters. For our baseline regressions (for the impact on traffic as well as sales), we find the level of precision is slightly *higher* when applying the bootstrap procedure.

[8]The price information is obtained from the purchase data. A promotion is defined as a reduction of at least 15% relative to the base price. The average price level is computed as the average (unweighted) price of all products in the category, and captures promotional price fluctuation over time in a more continuous fashion (relative to the number-of-promotions variable).

[9]The inclusion of marketing controls does not play a role in driving the null effect (see Table A1 in the appendix).

	Traffic (# Cons. Visiting the Cat.)	Traffic Share (Fraction of Cons. Visiting)	Traffic (Primary Location Only)	Traffic Share (Primary Location Only)	# Cons. Purch. in the Cat.	Total Quantity Purchased	# UPCs	# Feat.	S.D. Features
Carb. Bev.	4,866	97.4	1,392	27.8	371	458	108	16.8	2.9
Salty Snacks	4,429	88.6	1,237	24.7	182	229	129	18.8	6.7
Beer	4,321	86.5	654	13.1	174	187	67	6.0	1.6
Soup	3,582	71.7	1,695	33.9	64	153	79	13.3	8.2
Spaghetti Sauce	3,534	70.7	1,630	32.6	33	45	43	5.1	5.4
Detergent	2,985	59.7	1,052	21.1	17	19	21	3.4	2.2
Milk	2,950	59.0	1,062	21.2	90	118	44	4.6	3.7
Must./Ketch.	2,616	52.3	685	13.7	17	18	17	0.3	0.5
Toothpaste	2,427	48.6	1,022	20.4	13	14	18	0.5	0.9
Cereal	2,229	44.6	2,045	40.9	157	238	130	32.0	9.7
Frozen Dinner	2,188	43.8	1,305	26.1	103	282	201	66.2	22.2
Yogurt	1,988	39.8	1,266	25.3	135	372	143	26.2	23.7
Coffee	1,783	35.7	1,783	35.7	21	25	21	1.5	1.5
Hot Dog	1,631	32.6	1,548	31.0	18	23	21	2.4	2.3
Frozen Pizza	1,359	27.2	1,217	24.4	32	50	44	7.1	4.5
Paper Towels	1,123	22.5	1,123	22.5	22	23	16	0.8	1.4
Toilet Tissue	1,095	21.9	928	18.6	34	37	15	1.1	1.0
Facial Tissue	948	19.0	584	11.7	18	28	13	1.6	2.7
Peanut Butter	850	17.0	850	17.0	13	14	17	0.5	0.9
Mayonnaise	562	11.2	562	11.2	31	33	20	0.5	0.9
Butter/Marg.	165	3.3	165	3.3	18	22	33	1.0	1.0

Table 1: **Traffic, Sales, and Feature Advertising Across Categories.** Each column (except for the last) displays the daily average value for the respective variable. The number of UPCs (universal product codes) does not vary over time.

additional product leads to 0.631 additional consumers visiting the category. Relative to an average of 2,270 daily category visits, this effect is small.

To further illustrate the magnitude of the effect, consider a one-standard-deviation increase in the number-of-features variable, which is equal to eight additional products being featured.[10] Such an increase in the feature advertising variable leads to about five additional visitors (0.631*8), a 0.22% increase (5/2270). Even evaluated at the upper bound of the 95% confidence interval (i.e., two standard deviations above the point estimate), the effect magnitude is still small. A one-standard-deviation increase in the number of features will lead to 31 additional visitors, a mere 1.3% increase in the number of visits. A final way to assess the relevance of the effect in terms of magnitude is to compare it to the effect of feature advertising onto sales, which we present below. When running the equivalent regression to the one above, but using category sales as the dependent variable, we find a statistically significant increase in sales of 10%. Therefore, the effect of feature advertising on sales (evaluated at the point estimate) is an order of magnitude larger than its effect on traffic (evaluated at the upper bound of the 95% confidence interval).

To probe the robustness of the null result regarding the impact of features onto traffic, we run several additional specifications. We first implement a set of regressions that use different definitions of category traffic. Instead of assuming a consumer visited a category when her path passed at least three traffic points, we consider several more conservative definitions, which require the consumer to pass a larger number of associated traffic points. Columns (2) and (3) of Table 2 report the results from two regressions that base the category-visit definition on at least five and seven traffic points, respectively. Results are similar to our baseline specification. The point estimates are close to zero and in fact slightly negative, and the standard errors are smaller relative to the baseline specification reported in column (1). We also run an even larger set of regressions using between 1 and 15 traffic points as the basis for the category traffic definition. Across all 15 specifications, the effect is consistently statistically insignificant with an average (minimum) p-value of 0.841 (0.616) and small in magnitude.

In a second set of robustness checks, we narrow the category definition down to only the primary location of the each category. As described in Section (2.4), many categories are stocked at different locations in the store. Typically, the primary location is either in an aisle or at the back wall of the store (for perishable items), and secondary locations are in the open areas of the stores. Secondary locations often experience higher traffic volume. Furthermore, if consumers who see a feature ad are explicitly seeking out the featured category, we might expect an effect on traffic to show up mostly for the primary locations because those are typically labeled and signposted with the category names. We therefore construct traffic measures using only the primary location of each category for the three, five, and seven traffic-point definitions used above. The results from those three regressions are reported in columns (4) to (6) of Table 2, and again show a clear null effect with point estimates and standard errors that are of similar magnitude as the estimates in the first three

[10]We compute the standard deviation of features *within* categories by regressing the feature variable onto category fixed effects and then calculating the standard deviation of the residuals from this regression.

	All Locations			*Only Primary Category Locations*		
Dependent Variable	(1) # Cat. Visits	(2) # Cat. Visits	(3) # Cat. Visits	(4) # Cat. Visits	(5) # Cat. Visits	(6) # Cat. Visits
Category Visit Definition	$\geq$ 3 Traffic Points Visited	$\geq$ 5 Traffic Points Visited	$\geq$ 7 Traffic Points Visited	$\geq$ 3 Traffic Points Visited	$\geq$ 5 Traffic Points Visited	$\geq$ 7 Traffic Points Visited
Mean	2,270	1,589	1,124	1,133	743	533
S.D.	1,397	1,239	1,100	532	448	425
# Features	0.631	-0.428	-0.295	-0.148	-0.105	-0.250
	(1.654)	(1.029)	(1.005)	(0.963)	(0.969)	(1.059)
Category FEs	Yes	Yes	Yes	Yes	Yes	Yes
Day FEs	Yes	Yes	Yes	Yes	Yes	Yes
Marketing Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	546	546	546	546	546	546
Categories	21	21	21	21	21	21
Days	26	26	26	26	26	26

Table 2: **The Impact of Advertising on Category Traffic.** The unit of observation is a category/day combination. Marketing controls are the number of promoted items in the category, the average category-level price, and a proxy for the number of displayed items. Standard errors are clustered at the category level.

columns.

We also investigate a set of alternative measures of category traffic (not reported) that take time spent on traffic points associated with a category into account. Across various metrics such as total time spent or visit definitions based on a minimum amount of time spent, we again find consistently small and insignificant coefficient estimates.

Furthermore, due to the store layout, some categories have generally higher traffic than others. As the first column in Table 1 shows, this scenario is particularly true for the traffic definition based on all locations. For instance, over 90% of consumers walk through parts of the store where carbonated beverages are stocked. When products in such a high traffic category are featured, the marginal effect on traffic could potentially be small because of the already high level of baseline traffic. To address this concern, we re-estimate the baseline specification in column (1), but exclude categories with high traffic volume. When we exclude categories with more than 80% or 60% average traffic volume, we find the estimated effect remains small and insignificant. The coefficient (standard error) is 2.044 (1.745) when using an 80% cutoff and 0.115 (1.157) based on a 60% cutoff.

Finally, traffic effects could be masked due to specific patterns of feature advertising activity at nearby categories. For instance, if two categories are stocked on two sides of the same aisle, any consumer visiting the aisle would count as a visitor to both categories. If feature advertising alternates between the two categories such that exactly one category advertises during a given week, then advertising in any specific category will appear not to affect traffic, because total advertising for the two categories is constant. We test whether such spatial correlations in advertising activity exist and find no evidence for any systematic spatial pattern. Feature advertising in categories that are stocked in the same aisle is not correlated, nor does distance between pairs of categories predict the correlation in their advertising. More details on the analysis of spatial correlation in advertising is provided in Section A.2 of the appendix.

We conclude that across a wide variety of alternative specifications, the impact of advertising on category traffic is statistically insignificant and small in magnitude. Consequently, feature advertising does not seem to be able to attract an economically meaningful number of additional consumers to areas of the store where the advertised category is stocked.

3.2 Category Sales

In this section, we estimate the effect of feature advertising on sales. The objective of this analysis is two-fold. First, we aim to establish whether advertising has any impact on purchases and how the magnitude of the effect compares to the null effect on traffic. Second, we decompose the effect of advertising onto purchases into different adjustment margins.

Similar to our analysis in the previous section, we implement the following regression:

$$Sales_{ct} = \alpha \times FeatureNum_{ct} + X'_{ct}\beta + \delta_c + \theta_t + \varepsilon_{ct}, \tag{2}$$

where $Sales_{ct}$ denotes a measure of product purchases in category c on day t. $FeatureNum_{ct}$ denotes the number of featured UPCs in category c on day t. X_{ct} denotes a vector of other marketing variables. δ_c and θ_t denote category and day fixed effects, respectively. ε_{ct} is the regression error term. The specification is identical to the one used for analyzing category traffic, but now we use sales instead of traffic as the dependent variable. We "re-use" the same notation for the regression coefficients used in the traffic regression in the interest of simplicity.[11] We also note that we focus on the contemporaneous impact of advertising on sales. In Section A.3 of the appendix we explore whether advertising leads to intertemporal substitution in sales and find no evidence for such effects.

To decompose the effect of advertising into different margins of adjustment, we use four different measures of purchase outcomes (i.e., $Sales_{ct}$), that gradually include further steps in the conversion funnel. First, we compute a simple count of the number of consumers purchasing in the category. We then expand this metric to also capture consumers buying different brands and products (UPCs) from the same category as well as consumers purchasing multiple units of the same product. To separately capture these different dimensions, we compute the number of con-

[11] One could also use the share of purchases divided by the number of consumers visiting the category as the dependent variable. Due to the null effect on traffic, conditioning on category visits will not materially affect the results. For simplicity we therefore focus on the unconditional number of purchases.

Dependent Variable	(1) # Cons. Purchasing	(2) # Cons.-Brand Pairs	(3) # Cons.-UPC Pairs	(4) Quantity
Mean	74.4	80.4	96.7	113.7
S.D.	94.2	106.3	123.5	145.2
# Features	0.133	0.237	1.153**	1.427**
	(0.194)	(0.152)	(0.469)	(0.599)
Category FEs	Yes	Yes	Yes	Yes
Day FEs	Yes	Yes	Yes	Yes
Marketing Controls	Yes	Yes	Yes	Yes
Observations	546	546	546	546
Categories	21	21	21	21
Days	26	26	26	26

Table 3: **The Impact of Advertising on Purchases.** The unit of observation is a category/day combination. Marketing controls are the number of promoted items in the category, the average category-level price, and a proxy for the number of displayed items. Standard errors are clustered at the category level.

sumer/brand and consumer/UPC pairs as well as total quantity purchased in the category. To illustrate the decomposition, consider a consumer that purchased two units of product A and one unit of product B in the same category. We code this purchase bundle as one consumer, two consumer/UPC pairs, and three units of total quantity. Depending on whether product A and B belong to the same brand, e.g. two flavors of the same yogurt brand, this bundle is considered to contain either one or two consumer/brand pairs.

We start by reporting the results from a regression using the count of consumers as the dependent variable in column (1) of Table 3. We find the estimated effect is insignificant, which complements our earlier finding regarding the null effect of features on category traffic. Not only are no additional consumers visiting the category due to advertising, but for those consumers whose shopping paths overlap with the category, advertising also does not convert them into purchasing in the category. In column (2) we analyze the impact of advertising on the number of consumer-brand pairs and again find no significant effect. Next, we analyze the impact of advertising on the number of consumer-UPC pairs sold within a category on a given day. The results from this regression are reported in column (3) and show that features significantly increase the number of consumer-UPC pairs. Together with the null result from the first two columns, this significant result implies advertising leads to the same number of consumers buying a larger number of different products from the same brand. Finally, we also include multi-unit purchases into our outcome variable by using the total number of purchases as the dependent variable. The results are reported in column (4) of Table 3. The coefficient is statistically significant and shows only a slight increase in coefficient magnitude relative to column (3).

To assess the magnitude of the estimated effect on total quantity purchased, consider an increase of eight units in the number of products featured (a one-standard-deviation shift). Such an increase leads to an 11.4 additional units sold (1.427*8), a 10% increase (1.427*8/114). This effect is large in magnitude and in particular much larger than the corresponding increase in traffic. As reported in the previous section, an additional eight products being featured increases traffic by only 0.22%.

Taken together, the results presented in Table 3 show that feature advertising enhances sales by increasing the order size of consumers rather than the likelihood of purchasing in the category. Furthermore, the increase in order size originates from consumers buying multiple products of the same brand. We therefore conclude that advertising leads consumers that already intended to purchase a given brand to buy a larger number of products of that brand. We note that the category-level regressions do not directly tie the sales increase to the specific product being advertised. In Section 5, we run a regression at the product-level and find advertising leads to an increase in sales for the advertised product, but does not affect sales of other products in the same category. Therefore, advertising a specific product leads consumers that are already planning to buy other products of the same brand to add the advertised product to their purchase baskets.

3.3 Store Traffic

Our findings provide some evidence against advertising increasing traffic to the store as a whole, albeit only indirectly because we have only one store in our data. However, under the assumption that the impact of advertising is weakly increasing throughout the conversion funnel, we can rule out an effect on store traffic due to the absence of a category-traffic effect. In other words, if additional consumers visit the store with the intention to purchase in a specific category as a result of the feature advertising, we would expect these additional consumers to visit the advertised category once they are in the store. The null effect of advertising on category traffic therefore rules out a positive store-traffic effect. However, we cannot rule out that feature ads bring consumers to the store without them necessarily having the intention to purchase a product in the advertised category. Such a store-level effect can occur if feature advertising affects the general price image of the store (Mela et al. 1997, Jedidi et al. 1999) or serves to build the brand of the store as a whole. The analysis of such effects that are not specific to the categories being featured is beyond the scope of this paper.

3.4 Other Pre-Purchase Behavior

To complement the previous analysis, we explore the impact of advertising on two other outcomes related to consumers' pre-purchase behavior, namely the timing of category visits and the amount of time spent in front of the category. Neither of those two outcomes have typically been observable to researchers in the past, but can be tracked here based on the path data. The analysis of visit timing can shed light on whether advertising leads to consumers planning to purchase in the category and hence category visits might occur earlier in the trip in reaction to advertising. Changes in dwell-time in front of the category might occur if advertising affects the process of product search and discovery.

We find that advertising affects neither of those two outcomes. Advertising does not lead consumers to alter the timing of their purchase nor does it affect the duration of search in the category as measured by the consumer's dwell-time in front of the shelf. We present a more detailed analysis with regards to these issues in sections A.4 and A.5 of the appendix.

4 Identification and Robustness Checks

We do not have access to random variation in advertising, and thus the identification of the impact of feature advertising on traffic, sales, and other outcomes relies on variation in marketing activity within categories over time. This empirical strategy leaves two possible factors that could cause bias in our estimates. First, different forms of marketing activity might be correlated over time, and second, advertising could be correlated with time-varying demand shocks (e.g., turkey is more likely to be advertised around Thanksgiving).

A priori, we think both issues are unlikely to be a concern in our setting. First, we control for other marketing activity in all our regressions. Second, our data cover only a short time window and do not contain major holidays or other special events, and hence the scope for demand fluctuations over time is limited. Third, feature advertising is typically determined in advance by the retailer and manufacturers as part of the promotional calendar, and is therefore unlikely to be altered in response to short-term demand shocks (see Anderson et al. (2016), Rossi (2014), Quelch and Court (1983)). Finally, both possible confounds would tend to overstate sales effects, because advertising is most likely positively correlated with demand shocks and other marketing activity. It is, however, less clear how either of the two channels can spuriously generate a null effect on category traffic *and* a positive effect on sales. Nevertheless, we turn to further investigate both issues in a battery of robustness checks below.

We also briefly discuss the related issues of the role of other marketing activity that is delivered at a more aggregate level, such as TV advertising, as well as the impact of measurement error in the variables used in our baseline regressions.

4.1 Time-Varying Demand Shocks

To control for time-varying demand shocks in a flexible way, we would ideally want to include category/time-period specific dummies in the regression. However, the unit of observation in our data is a category/day combination, and hence we are not able to control for demand fluctuations at such a granular level.

To circumvent this shortcoming of our main data set, we use additional data for a set of comparable stores from the IRI data (for the same set of categories and over the same time period). Specifically, we select all stores in California that belong to one of four major chains. Our focal store is also located in the same geographical area and belongs to one of the four chains. Using data from multiple stores allows us to control for marketing activity, while at the same time being able to back out category-specific time trends that are common across stores.

We run the following regression:

$$Sales_{sct} = \alpha \times FeatureNum_{sct} + X'_{sct}\beta + \xi_{ct} + \lambda_{sc} + \varepsilon_{sct}, \tag{3}$$

where s denotes a specific store, c denotes the category, and t denotes a week (IRI reports data at the weekly rather than daily level). $Sales_{sct}$, $FeatureNum_{sct}$, and X_{sct} are defined as before, but are store-specific now. X_{sct} contains the number of promoted products, average price, and the number of products on display. Because we have store-category-week-level data, we can allow for a category-week-specific demand shock ξ_{ct}. Furthermore, we also control for store-category fixed effects λ_{sc}. Having recovered the demand shocks from the IRI data, we then include the fitted values $\hat{\xi}_{ct}$ into our baseline regression for the focal store.

We report results with the demand shock as additional control variable for both traffic and sales regressions in columns (3) and (4) of Table 4, which are based on the specifications in column (1) of Table 2 and column (4) of Table 3, respectively. For easier comparison, we also replicate the baseline results for the impact on traffic and sales in columns (1) and (2) of Table 4. The impact of including the demand-shock control on the feature-advertising coefficient in both regressions is minimal, and the null result for traffic as well as the positive and significant effect on sales are robust to the inclusion of this additional variable. Furthermore, the effect of the market-level demand shock on sales is positive but statistically insignificant.[12] Although we only report one traffic- and sales-based regression respectively, results are similar when we control for demand shocks using any of the other specifications reported in Tables 2 and 3.

4.2 Market-level Marketing Activity

Apart from categories and products being promoted at individual stores via feature advertising, several other types of marketing activity occur at a higher level of aggregation. Such activity comprises advertising by manufacturers in different media such as TV, radio, and newspaper advertising. Importantly, such advertising is typically delivered at the level of relatively large geographic units (e.g., media markets in the case of TV advertising) and therefore does not vary across stores within a confined geographic area. For this reason, we would expect the category/week demand-shock term estimated from the IRI data in the previous section to also include any demand shifts that such market-level marketing activity induces. Therefore, similar to taste-based shifts of demand over time, any variation in marketing activity over time that is common across stores in the same local market will be controlled for via the imputed demand shock.

Another form of marketing that might occur during our sample period is store ads run by the retailer. Such advertising might change traffic to the whole store and is unlikely to have a

[12] Because the across-store regression is estimated at the weekly level, we divide the estimated demand shocks by 7.

differential impact across categories. Our baseline regressions control for such time-varying effects that are common across categories via a set of day fixed effects.

4.3 Correlation in Marketing Activity

A further issue could arise from a correlation of feature advertising with other marketing activity, namely, price promotions and product displays. We note that we control for both price and displays in our main regression. In terms of price controls, we include both the average category price level and the number of promoted items. The most problematic element regarding our attempt to control for other marketing activity is arguably the display variable. As mentioned in Section 2.2, we do not observe display information for the focal store. We therefore approximate product displays by calculating the weekly fraction of stores that display a specific product in stores of the same chain in the same local market. The product-specific fraction of displays is then added up across products within a category to yield the number-of-displayed-products proxy variable for each category. This variable is a noisy proxy for displays, and we hence run a set of additional robustness checks.[13]

First, to assess the possible impact of correlated displays on our estimates, it is useful to consider a few basic descriptive statistics on the usage of displays. Among all three observed marketing activities (i.e., displays, promotions, and feature advertising), displays are the least frequently used. Across all products and stores in the IRI sample used in the previous section, the fraction of product/store/week combinations during which each marketing tool is used is as follows: promotions (40%), feature ads (20%), and displays (9%). Furthermore, the correlation between the different marketing tools is not particularly high. The correlation of feature ads and displays at the category/store level is equal to only 0.09 after controlling for category/store-pair fixed effects. Therefore, the potential for display mis-measurement to bias our estimated effect of feature advertising onto sales and traffic is not particularly large.

Nevertheless, we implement a set of additional regressions to assess possible effects from the imperfect display control variable on our regressions. One thing to note is that the impact of displays onto the traffic and sales regression might be different. In case of the sales regression, one might worry that when controlling imperfectly for displays, the effect of features might be overestimated. In the case of the traffic regression, the direction of the bias is less clear. Conceivably, displays occur in other parts of the stores rather than the typical location of the category. End-of-aisle displays are the most prominent example. Therefore, we might record fewer consumers walking past a specific category because they are able to pick the product up elsewhere. This specific mechanism could therefore lead to a bias toward zero in the traffic regression and an upward bias in the sales regression. We implement two tests below to address this issue.

First, we note that the sales variable used in our estimation is based on product "pickups" (see Section 2.1). In other words, we are only recording the sale of a product if the product appears

[13]We emphasize that the display variable in the academic IRI data set is recorded for each store individually. Although industry practice is to sometimes impute display information from other stores, we did confirm with IRI directly that the display information is *not* imputed for the data used here.

in the consumer's checkout basket *and* the consumer walked through the aisle where the product is stocked. Therefore, if displays divert consumers away from aisles, because consumers pick up the product elsewhere, both the traffic and sales results will be biased towards zero. Hence, the divergence of traffic and sales effects cannot originate from this mechanism.

We nevertheless further probe our data to test whether displays do lead to more purchases from temporary locations rather than the main category locations. We can implement such a test by computing for each category/day-pair the number of purchased products (from the checkout data) as well as the number of items picked up from the typical category locations. If displays divert traffic and sales away from the typical category locations, we would expect the number of pickups relative to purchases to decrease as a function of displays. We test this hypothesis by regressing the ratio of pickups to purchases onto features, displays, and other control variables (following the specification used in our baseline regressions). We run this regression using the ratio of both primary location and all location pickups relative to total purchases. The results are reported in columns (5) and (6) of Table 4. We find that in both cases, the display proxy variable (not reported in the table) has no significant effect on the pickup/purchase ratio, and feature advertising (which might be correlated with unobserved display variation) has a marginally significant effect in one of the two specification. However, the effect is small in magnitude,[14] and the sign of the effect is *positive*; that is, features led to more pickups relative to total purchases. Hence, these regressions provide evidence against displays diverting traffic away from permanent locations.

In a second test, we confine our analysis to perishable products (frozen entrees, frozen pizza, milk, yogurt, butter / margarine, and hot dogs), for which displays are rare due to the need for these products to be stored in refrigerators, and hence they cannot be moved to different locations such as end-of-aisle placements. We re-run the main traffic and sales regressions based on categories containing perishable products only, and find the results are robust to using this sub-sample of categories. Results for both traffic and sales regressions are reported in columns (7) and (8) of Table 4. As for the previous robustness checks, we only report one traffic- and sales-based regression, respectively. Results are similar for the other specifications reported in Tables 2 and 3.

Finally, one might also worry that the impact of feature advertising on sales is incorrectly attributed to the effect of displays that are located at the category's main location. The robustness test based on perishable products, which are rarely displayed, provides evidence against such a scenario. Furthermore, we find a significant effect on sales of similar magnitude (relative to our baseline regression for the focal store) when estimating our main regression based on the IRI data, which contain a correctly measured display variable. The coefficient (standard error) for the effect of feature advertising on sales at the daily level based on the IRI data is equal to 1.077 (0.105).

[14] The standard deviation of the pickup/purchase ratio (based on all locations) is 0.306, and hence a one-standard-deviation shift in the number of features (eight additional products) leads to an increase of 4% of a standard deviation (0.0017*8/0.306).

	Baseline		*Demand Shock Control*		*Pickup/Purchase Ratio*		*Only Perishable Categories*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent Variable	# Category Visits	Quantity Purchased	# Category Visits	Quantity Purchased	Pickup to Purchase Ratio (Prim. Loc.)	Pickup to Purchase Ratio (All Loc.)	# Category Visits	Quantity Purchased
Category Visit Definition	All Locations $\geq$3 Traffic Points Visited	n/a	All Locations $\geq$3 Traffic Points Visited	n/a	n/a	n/a	All Locations $\geq$3 Traffic Points Visited	n/a
# Features	0.631	1.427**	0.963	1.568***	0.0003	0.0017**	-0.998	2.085***
	(1.654)	(0.599)	(1.569)	(0.472)	(0.0007)	(0.0008)	(1.261)	(0.301)
Imputed Demand Shock			0.872	0.369				
			(0.642)	(0.439)				
Category FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Day FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Marketing Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	546	546	546	546	546	546	156	156
Categories	21	21	21	21	21	21	6	6
Days	26	26	26	26	26	26	26	26

Table 4: **Robustness Checks.** The unit of observation is a category/day combination. Marketing controls are the number of promoted items in the category, the average category-level price, and a proxy for the number of displayed items. Standard errors are clustered at the category level.

4.4 Measurement Error

Finally, we assess the potential for measurement error to bias our estimates. The two variables that are most likely to be subject to measurement error are the category-traffic count and the number-of-displays proxy variable. Importantly, our key explanatory variable, the number of featured products in the category, is less likely to be mis-measured, and hence typical concerns about attenuation bias do not apply here. Instead, any concerns about mis-measurement apply only to a control variable (displays) and the dependent variable in the traffic regressions. As we outline below, measurement error in either case is less problematic.

With regards to (classical) measurement error in the traffic count,[15] some amount of mis-measurement is likely. The primary source of such error occurs due to consumers leaving their carts behind while visiting a specific category. However, traffic is used as a dependent variable, and hence any measurement error in traffic will decrease the degree of precision of the regression, but will not lead to biased estimates. As we outlined in detail in Section 3.1, the coefficient on feature advertising in the traffic regression is precisely estimated and the effect size is small even at the upper bound of the confidence interval.

A second variable that might be plagued by measurement error is the display variable we discussed extensively in the previous section. Because we include displays only as a control variable, the impact of measurement error on the main coefficient of interest, feature advertising, is indirect. Nevertheless, measurement error in displays can potentially lead to a biased estimate of the impact of feature advertising. If controlling for displays is important to isolate the effect of feature advertising (because feature ads are correlated with displays), the mis-measured display proxy will not be able to control fully for the variation in the *actual* number of displays.

For several reasons, we think such a scenario is unlikely to be problematic for the traffic and sales results presented earlier. First, as documented above, displays tend to be positively correlated with feature advertising, and we would expect them to have a positive effect on traffic and sales.[16] Therefore, not controlling fully for displays will bias the feature coefficient upwards. Mis-measured displays can therefore not account for the null effect on traffic, but they could lead to an overstatement of the impact of feature ads on sales. The latter, however, is unlikely, because the impact of feature ads on sales using the IRI data, where we can control for displays without mis-measurement, is similar to the effect we find in the data for our focal store.

Finally, we re-iterate that displays and features are not strongly correlated (as discussed at the beginning of the previous section), and hence the impact of the display control on the feature effect is likely to be minimal. In this regard, we also note the feature coefficient in the traffic and sales regression does not change much when displays are included as a control variable versus when displays are omitted as we show in Table A1 in the appendix.

[15] All of our discussion in this section focuses on classical, that is, additively separable, measurement error.

[16] As discussed above, one could imagine that displays divert traffic away from main category locations, and hence the impact on traffic might be negative. However, our analysis in the previous section provides evidence against such an effect.

5 Spillover Effects

Having established and probed the robustness of the effect of advertising along the conversion funnel, we now turn to analyzing the consequences of this decomposition in terms of spillovers to other products and categories. First, we explore whether advertising in the focal category affects sales in categories that are stocked close to a featured category. This analysis makes use of the detailed information on product locations within the store. To the best of our knowledge, such "micro-geographic" spillovers within a store have not previously been explored, because data on store layout and product locations have not typically been available to researchers. Based on our previous finding that advertising does not affect the number of consumers visiting a specific category, we conjecture that a spillover effect onto nearby categories is not likely to occur.

Second, we explore whether, within categories, advertising leads to category expansion, substitution, or positive spillovers between products.

5.1 Cross-Category Spillovers

Our analysis of cross-category advertising effects proceeds in a similar fashion as the analysis of sales within the category (see equation 2), except that we substitute sales of nearby products for sales within the category as the dependent variable. Apart from the change in the dependent variable, we employ the same regression framework as earlier and control for category and day fixed effects and a set of marketing controls.

One downside with this analysis is that we relate sales in nearby categories to feature advertising in the focal category, but do not control for feature advertising in those nearby categories. This is due to data limitations. Most of the 21 categories in our sample are not stocked next to each other and therefore, for most nearby categories, we do not have information on advertising.[17] However, the omission of such feature information for nearby categories is problematic only if it is correlated with feature advertising in the focal category. In Section 3.2 (and in more detail in Section A.2 of the appendix), we showed that there is no systematic correlation of feature advertising in categories that are stocked close to each other. This allows us to implement the regression outlined above without a control for feature advertising in nearby categories.

To define which products are stocked near the set of 21 categories for which we observe advertising, we first find all locations at which products of a particular category are stocked. Based on these sets of coordinates for each category, we then find all *other* product locations that are within a certain distance of any product point belonging to the category. Our baseline specification uses all category locations and defines vicinity as a 15-foot radius around each product location. In other words, for, say, the beer category, we find all locations at which beer is stocked and then draw a 15-foot radius around each location and find all possible locations within this radius at which other products might be stocked. We make sure vicinity is only defined in open spaces of the

[17] We note that we have sales data for all products and categories in the store, but the advertising data (from IRI) is limited to only 21 categories. See Section 2.3 for more details.

	Cross-Category Spillovers			Within-Category Spillovers
	(1)	(2)	(3)	(4)
Dependent Variable	Quantity	Quantity	Quantity	Quantity
Unit of Observations	Category	Category	Category	Product
Definition of Nearby Products	≤15 Feet All Loc.	≤10 Feet All Loc.	≤10 Feet Primary Loc.	n/a
Mean	1407	676	203	1.99
S.D.	1125	592	137	6.11
# Features	-2.800	-1.200	-0.467	
	(1.732)	(1.191)	(0.596)	
Feature Dummy				0.738***
				(0.257)
Fraction of Other Products				-0.380
(of the Same Brand) Featured				(0.295)
Fraction of Other Products				0.347
(from Different Brands) Featured				(0.378)
Category FEs	Yes	Yes	Yes	No
Product FEs	No	No	No	Yes
Day FEs	Yes	Yes	Yes	Yes
Marketing Controls	Yes	Yes	Yes	Yes
Observations	546	546	546	31,200
Products	n/a	n/a	n/a	1,200
Categories	21	21	21	21
Days	26	26	26	26

Table 5: **Spillover Effects across and within Categories.** The unit of observation is a category/day combination in columns (1) to (3) and a product/day combination in column (4). Marketing controls are the number of promoted items in the category, the average category-level price, and a proxy for the number of displayed items in columns (1) to (3). In column (4), marketing controls also include "fraction of other" versions of each marketing variable. Standard errors are clustered at the category level in columns (1) to (3) and the product level in column (4).

store; that is, we do not consider adjacent aisles within 15 feet as nearby locations, because they are separated by a wall. Having defined nearby locations, we find all products that are stocked at these locations, and compute the total daily sales volume across all products. We hence end up with a count of sales of nearby products at the daily level for all 21 categories.

The results using this baseline definition are reported in column (1) of Table 5. We find a negative but insignificant effect, which is consistent with our prior that cross-category spillover effects are unlikely to occur. Evaluated at the point estimate, the estimated effect corresponds to a 1.6% decrease in sales per featured product, and the 95% confidence interval ranges from a

3.5% decrease to a 0.3% increase. We note that relative to the traffic regressions, our estimates are noisier. Nevertheless, the range of effect magnitudes within the confidence interval are economically relatively small and we can rule out large positive effects.

We also probe the robustness of our results to alternative definitions of nearby categories. We first narrow the radius to 10 feet, and then also employ a definition that is based only on the primary locations of each category rather than all product locations (using a 10-foot radius). Results from both specifications are reported in columns (2) and (3) of Table 5 and show negative and insignificant effects. In further robustness checks, we use every combination of a 5-, 10-, 15-, and 20-foot radius and primary versus all locations to define the vicinity of categories. We find no significant effect in any of those eight regressions. Finally, we also run a set of regressions (not reported in the table) where we distinguish nearby products by their relationship with the focal category. Specifically, we divide nearby products into substitutes, complements, and unrelated products, and run regressions separately for each type. Consistently across all three product types, we find small and insignificant effects.[18]

We conclude that advertising does not lead to significant spillovers across categories and therefore, advertising decisions for individual categories can be taken in isolation without a need to coordinate such decisions across categories.

5.2 Within-Category Substitution and Spillover Effects

We next proceed to analyze the response to feature advertising at the individual product (UPC) level. In order to relate the results to our analysis of different adjustment margins (see Section 3.2), we provide a framework that captures advertising effects at the product-, brand- and category-level. Subscripts j, b and c refer to product, brand and category. Subscript b_j refers to the brand that product j belongs to, and subscript c_b refers to the category that brand b belongs to.

We estimate the following linear regression:

$$Sales_{jt} = \alpha_1 Feature_{jt} + \alpha_2 \frac{Feature_{-jt}}{N_{b_j} - 1} + \alpha_3 \frac{Feature_{-bt}}{N_{c_b} - N_{b_j}} + Z'_{jt}\beta + \gamma_j + \vartheta_t + e_{jt}, \tag{4}$$

where $Feature_{jt}$ is a dummy variable equal to one if product j is featured on day t. $Feature_{-jt}$ denotes the number of other products featured of the same brand product j belongs to, excluding the focal product j. The denominator $N_{b_j} - 1$ represents the number of other products of brand b_j. The variable therefore represents the fraction of *other* products featured of the same brand. Similarly, $Feature_{-bt}$ denotes the number of products featured in category c_b that b belongs to, but excludes all products of brand b. Dividing by the number of products of other brands, this variable yields the fraction of featured products of other brands in the same category. Z'_{jt} denotes other marketing controls and contains the same variables as previous regressions, but also includes

[18] We manually code whether categories are substitutes, complements or unrelated to each other. For instance, in the vicinity of beer, one substitute category (wine) is stocked as well as several complementary categories (chips, popcorn, etc.). We also note that the majority of nearby products belongs to unrelated categories (88 percent) and only a small subset of products are either substitutes or complements of the focal category.

analogues to the two additional feature advertising terms for each of the other marketing variables. γ_j and ϑ_t denote product and day fixed effects, and e_{jt} is the error term. Standard errors are clustered at the product level.

To see why the formulation above is useful for analyzing product substitution, spillover, and category expansion effects, consider the predicted change in sales when product j is featured. At the individual product level, the change in sales is given by

$$E(\triangle Sales_{jt}|\triangle Feature_{jt} = 1) = \alpha_1.$$

We can similarly compute the predicted change for other products of the same brand. For any other product belonging to the same brand, $Feature_{-jt}$ increases by one unit, and hence the predicted change is equal to $\alpha_2/(N_{b_j} - 1)$. Because $(N_{b_j} - 1)$ other products exist within the brand, the predicted change aggregated to the brand level is given by

$$E(\sum_{k \in b_j} \triangle Sales_{kt}|\triangle Feature_{jt} = 1) = \alpha_1 + (N_{b_j} - 1)\frac{\alpha_2}{(N_{b_j} - 1)} = \alpha_1 + \alpha_2.$$

Similarly, the total sales effect at the category level is given by $(\alpha_1 + \alpha_2 + \alpha_3)$. This framework is simple, yet flexible enough to allow for substitution as well as positive spillover effects at the brand and category level. For instance, if featured products within a given brand steal sales from other products of the same brand, the regression would yield $\alpha_2 < 0$. In contrast, in the case of positive advertising spillovers within a given brand we would obtain $\alpha_2 > 0$. A similar interpretation applies to α_3.

We present results from this regression in column (4) of Table 5. In Table A2 in the appendix, we report the full set of estimates for the other marketing variables. We find a positive and significant effect at the product level (α_1) and insignificant effects at the brand and category level (α_2 and α_3). Hence, feature advertising leads to higher sales for the product being advertised, but does not significantly affect sales of other products in the category. We find neither evidence for positive advertising spillovers between products nor for substitution between products in response to advertising.[19]

6 Mechanism

We summarize our main findings here: (1) Advertising does not affect traffic to the category being advertised. (2) Advertising affects quantity sold due to an increase in the number of different products purchased from a given brand by the same number of consumers. (3) Advertising does not affect sales in other nearby categories, nor does advertising for a specific product affect sales of other products in the same category. (2) and (3) together show that when a product is advertised,

[19] We note however that our estimates from this regression are noisy and only the coefficient on the feature dummy variable is precisely estimated. The confidence interval for the brand-level effect of advertising $(\alpha_1 + \alpha_2)$ ranges from -0.45 to 1.17 and therefore includes a null effect of advertising at the brand level as well as a modest positive spillover effect.

consumers that were already planning to buy a product from the same brand will add the advertised product to their purchase basket. Therefore, advertising affects total sales by increasing the the purchase basket size of consumers conditional on those consumers buying the specific brand. Based on these results, we posit two explanations as to why we observe an impact of advertising only at the lower end of the conversion funnel.

One possible scenario is that consumers who are exposed to the ad do not take any explicit action and only retrieve the memory of the ad when they are in front of the shelf and engage with the category and the specific brand. Merely entering the store or walking past the category is not sufficient to trigger the memory. Such a mechanism is consistent with the literature on memory and retrieval cues (e.g., Keller (1987), Lee (2002), and Lee and Labroo (2004)).

Alternatively, the feature ad might only alter consumers' purchase intentions at the product but not the category or brand level. In other words, consumers who did not intend to purchase the specific brand will not be converted by the ad, but consumers already wanting to purchase the brand might change their purchase intention with regards to the advertised product. Such an effect could occur if advertising is informative in nature and can serve to increase awareness for the specific product. Alternatively, such a pattern could be due to a specific type of selection into ad consumption. Consumers might only pay attention and hence react to feature advertising for a brand they are already planning to purchase. Therefore, advertising will only affect purchases of advertised products conditional on purchasing the brand they belong to.

7 Conclusion

In this paper, we leveraged a new data set that combines advertising information with path-tracking data of consumers' movements in a brick-and-mortar store. This unique data set provides a closer look at the different stages of the conversion process that have typically been unobserved.

We find that although advertising has a significant impact on total quantity sold, it is ineffective at various stages of the process. Specifically, advertising does not influence traffic patterns, nor does it convert a higher number of consumers to buy in the category. The null result regarding traffic is precisely estimated, and even at the upper bound of the confidence interval, advertising shows a limited effect on category traffic. Instead, the overall advertising effect is mostly driven by consumers already visiting the category purchasing a larger number of different products within the category. We further investigate the spillover effects of advertising across and within categories, but find no evidence for spillovers along either dimensions. Together, both pieces of the analysis present a detailed picture of advertising impact along the conversion funnel. Advertising does not increase category traffic, and hence the impact of advertising does not spill over to other nearby categories. At the category level, there are no spillovers between individual products. Instead, advertising leads to an increase in sales only for the advertised product. This increase in purchases originates from consumers that are already planning to buy the brand adding the advertised product to their purchase baskets.

Our findings suggest managers need to pay little attention to coordinate advertising across categories or products. Furthermore, advertising does not cannibalize sales of other products in the same category and hence retailers are able to grow category sales through advertising. Finally, the absence of spillovers, especially within a category, is good news for manufacturers who do not want to benefit competitors through their advertising.

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A Appendix

A.1 Linking Sales and Path Data

One important features of our data set is the linkage of sales to trip records. As part of the RFID tracking process, the data report when the consumer arrives at the checkout. Independently, the sales data also have a time stamp for each shopper's transaction at the checkout. Comparing the time stamp of a particular path with the sales data allows us to define a set of "candidate" checkout product baskets that occurred at a similar point in time.[20] Matching which trip goes with which specific transaction involves considering the physical location of all the UPCs in each candidate basket. Based on how many of those locations lie on the path we are trying to match, a score is created for the baskets and the highest-scoring one is matched to the path.[21] The matches do not necessarily yield a perfect score, because consumers might occasionally leave the cart and pick up an item. Therefore, we might not see the path of the consumer going past a specific item, even if the item was in her matched purchase basket.

A.2 (Lack of a) Spatial Correlation in Feature Advertising

In this section, we explore spatial correlation patterns in feature advertising activity in different categories. Correlation in feature advertising could have an impact on our results with regards to the lack of an effect of advertising on category traffic. Specifically, if feature advertising in categories that are stocked near each other is negatively correlated over time, this could mask an effect of advertising on traffic for any individual category.

To study spatial correlation, we first compute correlations between pairs of categories which are stocked in the same aisle. Among the 21 categories in our sample there 11 such pairs and no systematic patterns emerges regarding the pairwise correlations. Out of 11 correlations, 5 are positive and 6 are negative.

Next, to assess the relationship between categories more systematically, we calculate the distance between each pair of categories in our sample. We then estimate a regression at the category-pair level where we regress the correlation coefficient (of features) for the category pair on the distance between the categories. Doing so we find a small and insignificant coefficient for the distance variable. A one standard deviation change in distance (about 51 feet) leads to an (insignificant) increase in the correlation coefficient of 0.027. This corresponds to a 0.05 standard deviation increase in the correlation coefficient. We also implement regression specifications that include a

[20] The path-data time stamp that records the arrival at the checkout can be noisy because the consumer will be stationary when standing in line at the cashier. Therefore, checkout baskets within a certain time window after the consumer became stationary in the checkout area qualify as possible matches.

[21] The data provider did not disclose the precise algorithm to us.

"same-aisle" dummy and higher order terms for the distance variable. Across all such specifications we find consistently small and insignificant effects of distance (and other measures of vicinity) on the correlation in features between category pairs.

A.3 Intertemporal Effects of Advertising

Our main analysis of advertising impact on product sales in Section 3.2 investigates the effect of advertising on category-level sales in the same time period. It is conceivable that any increase in contemporaneous purchases is offset by lower levels of purchases in subsequent periods. Such intertemporal demand effects are well documented for price promotions (see Erdem et al. (2003), Hendel and Nevo (2006) and Osborne (2011)) and could also occur in response to advertising.

To look at intertemporal advertising effects, we amend our regression framework in a simple way. Namely, we add lagged feature advertising, as well as similar terms for the other marketing variables, to our main regression which regresses category-level sales on marketing variables (feature advertising, display, promotion dummy, and average price), category and day fixed effects. Such a regression will show a "post-advertising dip" in sales if intertemporal effects are important and hence a negative effect of lagged advertising would provide evidence for intertemporal substitution.

In Table A3 we present results for the two sales measures on which advertising has a significant impact: the number of consumer/UPC pairs and total quantity (the dependent variables used in columns (3) and (4) of Table 3). The baseline regressions without lagged variables are replicated in the first two columns, followed by the corresponding regressions with lagged terms.[22] For both outcome variables, we find the effect of lagged advertising to be insignificant and small in magnitude. The magnitude of the contemporaneous advertising effects do not change significantly relative to the specifications without lagged terms. However, adding the lagged variables makes the effect of contemporaneous advertising insignificant in the specification based on total quantity (column (4)). Results stay significant when using consumer/UPC pairs as the dependent variable. We also note that when we run the traffic regressions with lagged terms (not reported) both contemporaneous and lagged advertising effects are insignificant.

We take the results from these regressions as evidence that intertemporal advertising effects do not occur in our setting.

A.4 The Impact of Feature Advertising on Visit Timing

In this section, we describe in more detail the analysis of category-visit timing summarized briefly in section 3.4. To analyze the timing of visits, we compute for each shopping trip the point in time at which the consumer is for the first time walking past a specific product category. We then compute the average time since the start of the trip during which a specific category was visited

[22]We have path data for only 26 days, but we have data on feature advertising and other marketing variables for a longer time period. As a result, our lagged regressions have the same number of observations as the main regressions.

at the category/day level.[23] We first regress the time of the visit (measured in minutes since the start of the trip) and fraction of total shopping time elapsed on the number of featured products in a particular category. Both regressions include category and day fixed effects and marketing controls, and hence mirror the traffic regression (equation 1).

We start by implementing the analysis based on all product locations for each category. In other words, we define visit timing as the point in time at which a consumer first passes any location in the store associated with the particular category. The results using both minutes elapsed and the fraction of shopping time elapsed are reported in columns (1) and (2) of Table A4. Columns (3) and (4) replicate the same regressions, but base the visit timing only on the primary locations of each category. Across all four specifications, we find effects of feature advertising that are consistently small in magnitude and mostly insignificant. Take, for example, the results in column (1). According to the (insignificant) point estimates, a one-standard-deviation increase in the number of features (eight additional features) in a particular category delays the visit to the category by 0.016 minutes (i.e., about 1 second) or shifts the visit timing back by 0.05 percentage points relative to the total time spent in the store.[24] The marginally significant effect in column (4) is similarly small in magnitude and does not constitute an economically meaningful shift in the timing of the category visit.

Finally, advertising might only affect a small set of consumers who are planning to purchase within the category due to the feature ad. When analyzing the visit timing of all consumers in the store, the unaltered behavior of the majority of visitors to the store might mask a significant effect for this group of consumers. We hence isolate the group of consumers who are most likely to be affected, by computing the daily average time of a category visit based only on consumers who purchase in the specific category. The results from regressions based on this measure of visit timing are reported in columns (5) and (6) of Table A4. We again find a null effect of feature advertising on visit timing, and the confidence intervals do not contain effect sizes that are economically important.[25]

We hence conclude feature advertising does not influence when consumers visit a specific category.

A.5 The Impact of Feature Advertising on Dwell-Time

In this section, we provide further details on the impact of advertising on dwell-time in front of the category. Based on the path data, we calculate the total time a consumer spends on traffic points belonging to the specific category for each category in which she purchased during a given shopping trip. Similar to other parts of our analysis, we aggregate this variable to the category/day level and

[23] We can only define visit timing for consumers who actually pass the category at all during their trip. The day/category average therefore represents the average visit time for the subset of consumers who visit the specific category.

[24] We also ran the same set of regressions based on distance walked before reaching a specific category (rather than time elapsed), and found similarly small and insignificant results.

[25] The confidence interval for columns (5) and (6), respectively, are equal to [-0.050,0.021] minutes and [-0.089,0.149] percentage points.

regress the average daily dwell-time onto the number of features (and control variables). Results from this regression are reported in column (7) of Table A4 and show a small and insignificant effect. We note that dwell-time is measured in seconds, and average daily dwell-time has a mean (standard deviation) of 53 (41) seconds. A one-standard-deviation shift in the number of features changes dwell-time by only 0.29 seconds ($0.29 = 0.036 * 8$).

We note that we would ideally like to measure the time a consumer spent contemplating which product to buy in the category. Total time spent in the vicinity of a given category is likely to be a noisy measure of search time (see Seiler and Pinna (2016) for a detailed discussion of the measurement error associated with path-tracking-based dwell-time measures). We therefore assess robustness of the null effect to using an alternative measure that only captures the amount of time spent near the specific product that was picked up (rather than the entire category). Results from this regression are reported in column (8) of Table A4 and also yield an insignificant result and an effect size that is small in magnitude.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
				Traffic *Baseline*				*Sales* *Baseline*
Dependent Variable	# Category Visits (3 Traffic Point Def.)	# Category Visits (3 Traffic Point Def.)	# Category Visits (3 Traffic Point Def.)	# Category Visits (3 Traffic Point Def.)	Quantity Purchased	Quantity Purchased	Quantity Purchased	Quantity Purchased
# Features	0.431	0.296	0.853	0.631	2.050***	1.501**	1.633**	1.427**
	(1.216)	(1.460)	(1.467)	(1.654)	(0.622)	(0.613)	(0.667)	(0.599)
# Promotions		0.475		1.514		1.518***		1.290***
		(2.212)		(1.504)		(0.449)		(0.301)
Av. Price		9.174		12.724		1.351		0.574
		(60.674)		(59.145)		(9.708)		(9.182)
# Displays (Proxy)			-7.694	-12.757			7.587*	2.794
			(23.503)	(24.551)			(3.889)	(3.899)
Category FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Day FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	546	546	546	546	546	546	546	546
Categories	21	21	21	21	21	21	21	21
Days	26	26	26	26	26	26	26	26

Table A1: **Traffic and Sales Regressions: Impact of Marketing Controls.** Columns (4) and (8) represent the baseline traffic and sales regressions. Columns (1) to (4) and (5) to (8) show how results change when using different sets of marketing controls in the traffic and sales regression, respectively. The unit of observation is a category/day combination. Standard errors are clustered at the category level.

	(1)			
Dependent Variable	Quantity			
		Marketing Variable		
	Feature	Display	Promotion	Price
Own Dummy	0.738***	5.645***	0.833**	-0.299
	(0.257)	(0.949)	(0.350)	(0.237)
Fraction of Other Products of the Same Brand	-0.380	0.208	-0.368	-0.137
	(0.295)	(0.786)	(0.340)	(0.223)
Fraction of Other Products of the Different Brand	0.347	-2.452	0.647	0.483
	(0.378)	(1.897)	(0.964)	(0.513)
Product FEs	Yes			
Day FEs	Yes			
Marketing Controls	Yes			
Observations	31,200			
Products	1,200			
Categories	21			
Days	26			

Table A2: **Spillover Effects within Categories: Full Results.** The unit of observation is a product/day combination. The table is an extension of column (4) in Table 5, that displays the full set of estimates of other marketing controls. Results are from one regression, but arranged across four columns for the four different marketing variables.

	(1)	(2)	(3)	(4)
Dependent Variable	# Cons.-UPC Pairs	Quantity	# Cons.-UPC Pairs	Quantity w/ Lagged
Mean	96.7	113.7	96.7	113.7
S.D.	123.5	145.2	123.5	145.2
# Features	1.153**	1.427**	0.873**	0.890
	(0.469)	(0.599)	(0.412)	(0.608)
One-week Lagged # Features			-0.027	0.004
			(0.307)	(0.400)
Category FEs	Yes	Yes	Yes	Yes
Day FEs	Yes	Yes	Yes	Yes
Marketing Controls	Yes	Yes	Yes	Yes
Lagged Marketing Controls	Yes	Yes	Yes	Yes
Observations	546	546	546	546
Categories	21	21	21	21
Days	26	26	26	26

Table A3: **The Impact of Lagged Advertising on Purchases.** The unit of observation is a category/day combination. Marketing controls are the number of promoted items in the category, the average category-level price, and a proxy for the number of displayed items. Standard errors are clustered at the category level.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent Variable	Minutes Elapsed Since Start of the Trip	Fraction of Time Elapsed (0 to 100)	Minutes Elapsed Since Start of the Trip	Fraction of Time Elapsed (0 to 100)	Minutes Elapsed Since Start of the Trip	Fraction of Time Elapsed (0 to 100)	Dwell-Time (Sec.)	Dwell-Time (Sec.)
Variable Definition	All Locations	All Locations	Primary Location	Primary Location	Conditional on Purchase	Conditional on Purchase	Category Level	Product Level
# Features	0.002	0.006	0.005	0.010*	-0.014	0.030	0.036	-0.015
	(0.002)	(0.005)	(0.003)	(0.006)	(0.017)	(0.057)	(0.133)	(0.016)
Category FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Day FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Marketing Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	546	546	546	546	546	546	503	503
Categories	21	21	21	21	21	21	21	21
Days	26	26	26	26	26	26	26	26

Table A4: **The Impact of Advertising on Visit Timing and Dwell-Time.** The unit of observation is a category/day combination. Marketing controls are the number of promoted items in the category, the average category-level price, and a proxy for the number of displayed items. Standard errors are clustered at the category level. Dwell-time is not observed for some category/day combinations. Therefore, the number of observations is smaller in columns (7) and (8).

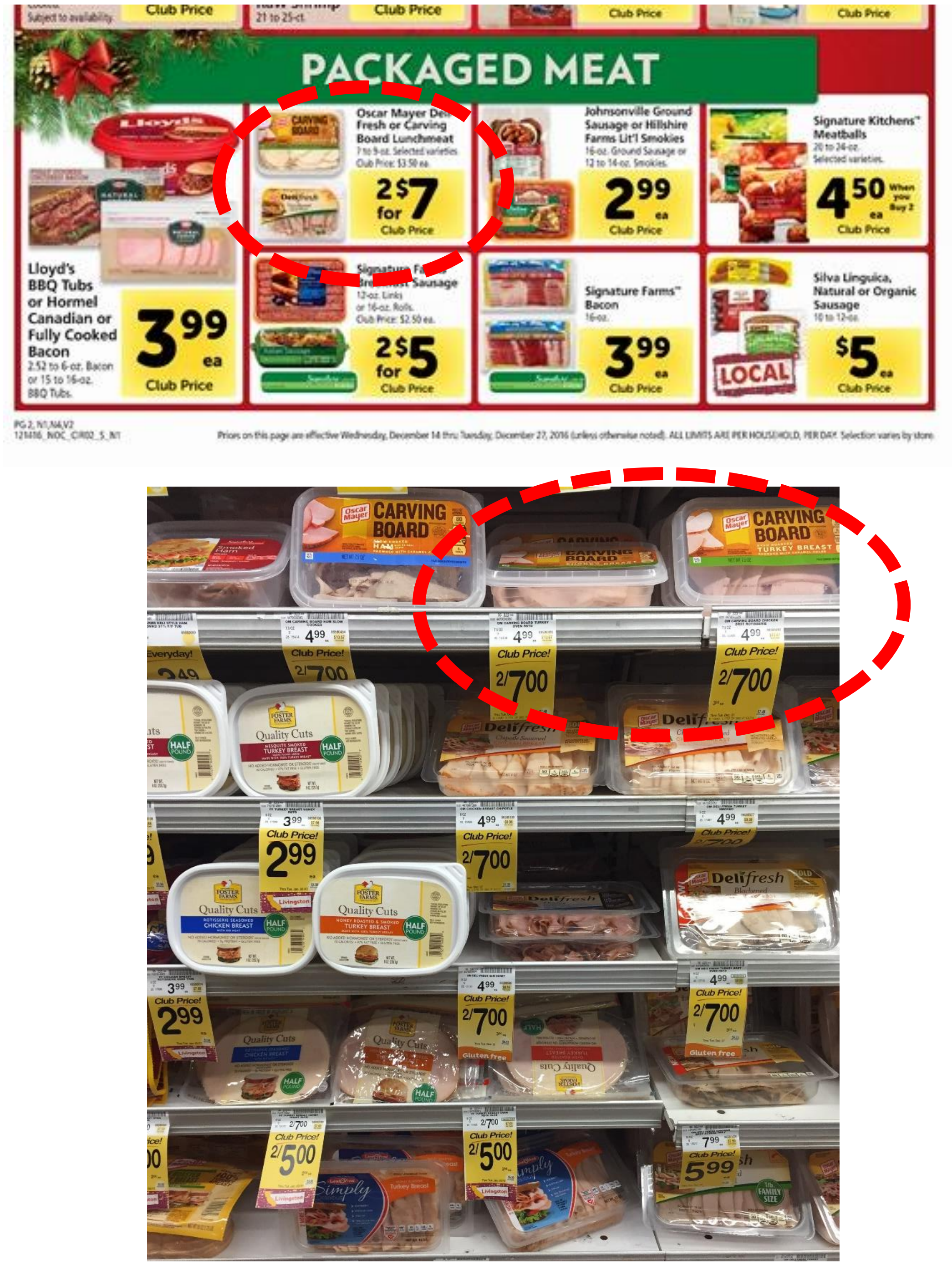

Figure A1: **Example: Feature Advertising and Shelf Labeling.** The top picture shows part of the weekly feature advertising leaflet of a store comparable to the one in our data. The bottom picture shows the labeling on the shelf in the same week. The dashed circle highlights the advertised product. The advertised product is not labeled more saliently. For example, right below the advertised product, another product (which is not featured in the advertising leaflet) has an identical label (showing "2 for 7 dollars").