ANITA RAO and EMILY WANG*

Firms sometimes make selective or deceptive claims, which can have negative consequences for consumers, especially if consumers are not fully informed and the claims are hard to verify. This study aims to measure the decline in demand that a firm making such claims faces when caught. In addition, it seeks to understand which type of consumer these claims primarily affect. Using a panel data set of consumer purchases and firm advertising, the authors measure this impact by exploiting the fact that four popular products settled charges raised by the Federal Trade Commission. They further control for and document firm responses in terms of price and advertisement changes around the date of the settlement. Findings indicate a significant decline in demand following the termination of the claims, resulting in a 12%–67% monthly loss in revenue across the four products, which amounts to a $.40 million–$3.82 million loss in monthly revenue. They also find that these claims primarily affect consumers who are newcomers.

Keywords: public policy, advertising, FTC, natural experiments, firm deception

Online Supplement: http://dx.doi.org/10.1509/jmr.15.0398

Demand for "Healthy" Products: False Claims and FTC Regulation

Firms often have incentives to make deceptive or selective claims, especially when such claims can lead to an increase in demand or can prevent substitution away from their products. Examples appear in our day-to-day world: products selectively claiming to be made with whole wheat even when whole wheat is not a main ingredient; products claiming to be "all natural" even when they contain synthetic compounds. The Federal Trade Commission (FTC) states, "In recent years there has been a trend in food advertising toward making unproven claims that eating certain foods can improve health and even reduce the risk of serious illnesses such as prostate cancer and heart disease."[1] Although regulatory bodies such as the FTC exist to safeguard consumers from deception, the vast number of companies and advertisements suggests that some claims are likely to go unnoticed for prolonged durations.

This study asks what (if any) consequences, in terms of consumer demand, firms that make false claims face when caught. The extent to which consumers respond to information about a firm's deceptiveness is unclear: if consumers were completely informed and knew the claims were false either through search (e.g., reading the ingredient list) or experience (e.g., having consumed the product once), any additional information should have no impact on demand. However, if consumers were uninformed, especially if the claims were hard to verify, demand could decline in response to new information about the firm's deception.

*Anita Rao is Associate Professor of Marketing, Booth School of Business, University of Chicago (email: anita.rao@chicagobooth.edu). Emily Wang is Assistant Professor of Resource Economics, University of Massachusetts, Amherst (email: emilywang@resecon.umass.edu). The authors thank the James M. Kilts Center for Marketing at the University of Chicago Booth School of Business for its help with the Nielsen Homescan, Retail Scanner, and Media data. They also thank seminar participants at Chicago, Duke, Indian School of Business, North Carolina State University, and University of Zurich; and participants at the 2015 International Industrial Organization Conference, 2015 Summer Institute in Competitive Strategy Big Data Conference, 2015 Yale Customer Insights Conference, 2014 Marketing Science Conference, 2014 Marketing Dynamics Conference, and 2014 Marketing and Public Policy Conference. Finally, they thank discussants Thomas Blake and Yeşim Orhun, Mary Sullivan, and James Lacko of the Federal Trade Commission, and especially Günter Hitsch and Sam Peltzman, for their helpful comments and suggestions. All errors are the authors' own. Coeditor: Sachin Gupta; Associate Editor: Carl Mela.

[1]See https://www.ftc.gov/news-events/media-resources/truth-advertising/health-claims. In addition, the Center for Science in the Public Interest has a list of litigation projects, most of which deal with deceptive, false, and misleading claims; see https://cspinet.org/protecting-our-health/courts.

ISSN: 0022-2437 (print)
1547-7193 (electronic)

Journal of Marketing Research
Vol. LIV (December 2017), 968–989
DOI: 10.1509/jmr.15.0398

To measure how consumers respond to firm deception, we exploit the fact that, in our data, the FTC investigated four products for making false health claims. These companies reached agreements with the FTC that required a termination of the false claims on or before the dates of the publicly issued consent orders. The FTC issued each of these agreements with an accompanying press release. We use the timings of these consent orders as exogenous shocks that (1) reduced the levels of the false-advertising campaigns to zero and (2) led to widespread information diffusion, through national press coverage, about these misleading claims.

As an example, Kellogg's Frosted Mini-Wheats started making the claim in January 2008 that the cereal was "clinically shown to improve kids' attentiveness by nearly 20%" without making any change to the composition of its product. On April 20, 2009, the FTC issued a consent order that required Kellogg's to stop making this claim. For Frosted Mini-Wheats, this order implied both a discontinuation of television advertisements and a change in their front-of-the-box labeling. Associated Press, Reuters, the *Wall Street Journal*, and three other news services picked up the FTC press release later that day.

Our empirical strategy is based on comparing market shares before and after these FTC-issued public statements to give us a measure of the impact of consumers' response to firm deception. This measure combines the effect of the removal of the false claims with any negative publicity and any remaining unobservable supply-side factors that followed the FTC consent order. Our measure therefore provides an upper bound on the impact of false claims, the measurement of which is challenging for multiple reasons in an empirical context. First, little variation exists in whether a claim is present, because most claims accompany a product's introduction. Second, even if variation exists—for instance, a claim is introduced or removed after a certain date—these changes are likely to be endogenous and an active part of a brand's positioning strategy. Our empirical strategy, which uses exogenously determined claim termination as enforced by the FTC, overcomes these challenges.

Using household-level purchase data from products across four categories—Kellogg's Frosted Mini-Wheats cereal, Dannon's Activia yogurt, Dannon's DanActive yogurt drink, and the nutritional supplement Airborne—that were issued consent orders by the FTC, we find the revelation of firm deception led to a significant decline in demand. Because competitors as well as the impacted brand can choose to respond strategically around the date of the consent order, we further account for these responses. We find the decline in market share persists even after we control for the competitive environment, prices, and advertisements.

Our findings further indicate that exposure of firms' deceptive activities primarily affects newcomers rather than existing consumers. For Kellogg's Frosted Mini-Wheats, for which we have a start date of the false claim, we find evidence corroborating this heterogeneity: the sharpest decline in demand is for those households that, prior to the start of the false claims, had not purchased the product. These consumers are likely to have been the most influenced by the new—albeit false—information presented in the advertisements and front-of-the-box packaging. We also find directional evidence that markets that received more false advertisements prior to the consent order saw sharper drops in market share.

Quantifying the economic impact of the potential penalty a firm faces when caught, the drop in monthly revenue (compared with the peak sales prior to the consent order) ranges from \$.40 million to \$3.82 million—a 12%–67% monthly loss—across the four brands, with Airborne and Frosted Mini-Wheats the most affected. These figures show consumers severely penalize deceptive practices in these four product categories.

To understand whether firms benefited from the presence of false claims prior to the FTC order, we need to know how much of the revenue gain can be solely attributed to the false-claims campaigns. Frosted Mini-Wheats offers a case study because it is the only product that started making the claim without any change in product composition. A back-of-the-envelope calculation shows Frosted Mini-Wheats is likely to have gained between \$59 million and \$144 million in revenue in the 12–32-month window surrounding the FTC order, a substantial amount compared with the \$4 million fund it settled to in a 2013 class-action lawsuit. Although these numbers control for all observable marketing activities (e.g., changes in price, ads, competitors' response), they should be regarded as an upper bound because firms might have launched other marketing activities independent of the false-claims campaign during this period.

CONTRIBUTION

Although studies have looked at the role of information on consumer decisions, the consequences of false or misleading health claims on consumer purchases have received little empirical attention. A large body of literature has studied the role of information, using shifts in regulatory policies. For example, Ippolito and Mathios (1990, 1995) find that after a regulatory ban on advertising health benefits was lifted, information acquisition became easier and more people were able to purchase healthier products; Dhar and Baylis (2011) find that a regulatory ban on advertising targeting children had a positive impact on reducing fast-food consumption. Jin and Leslie (2003) find that a policy change requiring restaurants to display hygiene-quality grade cards resulted in consumers becoming more sensitive to this information. Moorman (1996) studies the influence of front-of-package information on consumers after the implementation of the Nutrition Labeling and Education Act. This literature has, to our knowledge, not focused on the impact of misleading information.

Stated purchase intentions and consumer beliefs about brands making deceptive claims have been studied in lab settings: Olson and Dover (1978) experimentally create deception and measure the pre- and post-trial effects on brand beliefs, attitudes, and purchase intentions; Dyer and Kuehl (1978) measure brand beliefs and find that a one-time corrective advertisement is insufficient to restore brand beliefs to the correct levels. This literature has also looked at the type of claims that impact consumers: Burke et al. (1988) find that strongly positive but ambiguous claims performed better than true claims in terms of stated purchase intentions and beliefs; Snyder (1989) finds that implied-superiority claims were more misleading than noncomparative claims and that subjects were more likely to believe claims made by familiar brands as opposed to fictitious brands. More recently, this literature has examined various mechanisms to explain why consumers buy products that make false claims. Skurnik et al. (2005) find that letting consumers know a claim is false leads them to misremember it as true. LaTour and LaTour (2009) show that although a positive mood can impact detection of false claims, it also enhances positive feelings toward the brand. Johar (1995) finds consumers' involvement can explain whether or not they are deceived by an ad. Darke and Ritchie (2007) show that deceptive advertising creates distrust, which undermines the credibility of further advertising. We contribute to this stream of literature by empirically measuring consumers' response to firms making false health claims, using actual purchase data.

Perhaps closest to our study is that of Peltzman (1981), who studies the effects of FTC regulation on the capital market, advertising expenditure, and market share of the impacted brands. Peltzman postulates that false advertisements should affect first-time buyers rather than loyal buyers. However, data limitations at the time forced Peltzman to rely on an aggregated yearly autoregressive market-share model that imposes strong assumptions on individual behavior, as pointed out in Givon and Horsky (1985). On the other hand, our study exploits within-household variation to identify heterogeneity in consumer responses. Heterogeneity in information processing has been shown by Bronnenberg et al. (2015), who find experts in a certain category were more likely to buy the generic versions, whereas novices were more likely to buy the branded versions of an otherwise homogeneous product. Our study finds evidence supporting heterogeneity in consumers' responses to misleading information, whereby existing users are more likely to continue purchasing the product even after revelation of the firm's deceptive claims.

Researchers have studied firm deception in other contexts, such as buyers being misled by sellers' quality claims in the baseball card market (Jin and Kato 2006), ski resorts deceptively reporting more snowfall on weekends when demand is likely to be higher (Zinman and Zitzewitz 2016), and restaurants committing review fraud on Yelp (Luca and Zervas 2016). Unlike these studies, which focus on experience goods where quality is observed postpurchase, our work focuses on products with credence attributes, where verifying the claims is difficult, even with repeated purchases. Our study is also related to the product-recall literature (e.g., Liu and Shankar 2015). Whereas product recalls are in effect when the product presents a tangible danger to consumers, we study a very different effect whereby the product is not harmful per se. In other words, the product is safe to consume and continues to be sold, and only the specific false-advertising messages have been recalled.

In the next section, we describe the data and provide reduced-form evidence on the impact of misleading claims on consumer demand. We then describe the demand estimation that controls for prices, advertisements, and the competitive environment to quantify the impact of these misleading claims. We also document firm responses in terms of price and advertisement changes around the date of the consent order.

DATA

We use the Nielsen Homescan data, which contain households' purchases at the UPC-date level; the Nielsen Retail Scanner data (RMS), which contain the weekly price at the UPC-store level for participating retailers; and the Nielsen Media data, which contain the ad spend, airtime, and frequency of campaigns at the creative-title level for each brand. The Homescan data consist of a panel of about 40,000–60,000 households, and the RMS data are generated by point-of-sale systems at about 35,000 participating stores across the United States. Both the Homescan and Media data span the years 2004–2012, whereas the RMS data span 2006–2012.

We combine these data with the dates on which the FTC issued consent orders pertaining to various companies in the ready-to-eat (RTE) cereal, yogurt, yogurt drink, and nutritional supplement categories. To the extent the FTC focuses on cases that involve a high amount of injury to consumers,[2] our sample focuses on the more serious false claims. The relevant population of firms affected by the FTC consent orders can be found on the FTC website[3] and consists of cases and proceedings classified under the mission "Consumer Protection" and the topic "Health Claims." As of August 2015, 135 such cases existed. We restrict our attention to four of these cases on the basis of the following inclusion criteria (numbers in parentheses refer to the number of existing cases that fail to meet the listed criteria):

- The cases should not pertain to Internet scams or products sold only online (54).
- The case-filing date should be after 2003 and before 2012 to be within the time frame of the Homescan data (39).
- The cases should involve consumer product goods (e.g., insurance, tanning services, etc., were excluded) and be present in the Nielsen products file (22).
- The total number of category-specific purchase occasions across households in the panel should be at least 1,000 to ensure statistically meaningful measures (16).

This leaves us with four well-established products that are sold in retail stores.[4] Table 1 lists these products, the claims they were making, and the date the FTC asked them to terminate these claims. Figure 1 shows the front-of-the-package claims highlighted on these products prior to the FTC consent order.

Another product that satisfies these criteria is Kellogg's Rice Krispies. In July 2009, Rice Krispies was modified to include higher amounts of Vitamins A, B, C, and E, and it began making the claim that it "helps support your child's immunity." We do not include this product in our main analysis, because of a confounding factor that led to a firm-determined claim termination prior to the FTC order. The brand chose to remove the claim in November 2009 in response to the swine flu epidemic, issuing the following statement, which the press picked up: "While science shows that these antioxidants help support the immune system, given the public attention on H1N1, the Company decided to make this change." As a result, the claim terminated prior to the FTC order, which was issued in June 2010. Another confound arose because the FTC press release for Kellogg's Rice Krispies also emphasized Kellogg's Frosted Mini-Wheats, which had recently been issued a consent order, and Kellogg's, instead of just the impacted brand, Rice Krispies. We examine Rice Krispies in the "Discussion" section, wherein we discuss possible mechanisms that drive consumers response to deceptive claims.

The FTC typically conducts a private investigation of firms' claims prior to the release of a formal and public complaint.

[2]The FTC's advertising FAQ (https://www.ftc.gov/tips-advice/business-center/guidance/advertising-faqs-guide-small-business) states that the FTC concentrates on national advertising, leaving local matters to state, county, or city agencies. It also focuses on cases that have a high amount of injury—to consumers' health, safety, and wallets—and does not focus on individual disputes between a consumer and a business but looks at ads that represent a pattern of deception.

[3]See http://www.ftc.gov/enforcement/cases-proceedings/advanced-search.

[4]A large portion of FTC cases (>30%) pertain to weight-loss products. Cawley et al. (2013) study this category, in which deception is common and the products are ineffective and often harmful, using the National Consumer Survey to obtain measures of consumption and deceptive ad exposure. Instead, we focus on popular and well-established products for which deception is less common (note the distinction between deception and puffery, which is more common and legal) and products cater to the mass market.

Table 1
PRODUCTS ASKED BY THE FTC TO TERMINATE MISLEADING CLAIMS

Product	*Category*	*Claim*	*Date of Consent Order*
Kellogg's Frosted Mini-Wheats	RTE cereal	Product improves kids' attentiveness by 18%.	April 2009
Dannon Activia	Yogurt	Product helps with slow intestinal transit.	December 2010
Dannon DanActive	Yogurt drink	Product helps strengthen body's defenses.	December 2010
Airborne	Nutritional supplement	Product offers guaranteed cold-fighting protection.	August 2008

The FTC does not disclose the start date of the investigation. Although the firm is made aware of the investigation, consumers and members of the press have no knowledge of it while it is being conducted. Thus, the informational impact of the termination notice occurs only after the date of the consent order. However, firms can choose to make changes (for the better) to their marketing activities prior to the announcement. We verify that normal advertising activities do not cease after the FTC order; that is, the brand replaces the false ads with other ads, ensuring our measure is one of false claims and not overall ad exposure. We explore this further in the "Firm Response" section.

We now provide some preliminary evidence that revelation of firms' deceptive claims can affect consumer demand.

Reduced-Form Evidence: Market Shares

Figures 2–4 plot the aggregate unit market shares of the impacted products over time.[5] The vertical lines in Figures 3 and 4 and the right vertical line in Figure 2 indicate the date of the consent order; the left vertical line in Figure 2 corresponds to the start of the misleading claims.

RTE cereal. Figure 2 plots the unit market shares of Frosted Mini-Wheats over time. The rightmost vertical line in the graph corresponds to April 2009, when the FTC issued a consent order to Kellogg's to stop making claims that eating the cereal increased children's attentiveness by nearly 20%. The plot indicates a fairly sharp decrease in market share after this event and a symmetric increase in market share before this event.[6,7]

Yogurt and yogurt drinks. In the refrigerated-yogurts category, Dannon was issued a consent order in December 2010 to stop making the claims that Activia "relieves irregularity" and that DanActive "helps people avoid catching colds or the flu." These claims had been present in the brands' advertising and packaging since their introduction—for Activia, in February 2006, and for DanActive, in January 2007. The specific settlement required that, unless the FDA approved such claims, Dannon could not state that its products reduce the likelihood of getting a cold or flu. In addition, Dannon could not claim their products had digestive benefits unless (1) it was clearly stated that three servings of Activia must be taken every day to obtain these benefits, or (2) reliable scientific evidence or two well-designed human clinical studies backed these claims. Figure 3 plots unit market shares for both Dannon products.

Nutritional supplements. Airborne is a dietary supplement that since its introduction had claimed it had "guaranteed cold-fighting properties" and stated, "If taken at the first sign of a cold symptom, its herbal formulation is clinically proven to nip most colds in the bud." The FTC in August 2008 issued a consent order to the company to stop making these unsubstantiated claims. Figure 4 shows a marked decrease in market shares following the FTC request to terminate these claims.

These figures indicate a perceptible impact of the FTC consent order and provide preliminary evidence of how much firms making misleading claims stand to lose. We next estimate aggregate market-share regressions that reflect the patterns depicted in these figures. These equations will form the basis of our disaggregate household-level analysis in the next section. Because market share of a brand already measures relative sales (the denominator is sales of all other brands), we first perform a reduced-form regression of market shares on the time since the consent order. We then perform a difference-in-differences (diff-in-diff) analysis using monthly sales as the dependent variable and all other brands as a synthetic control. In the next section, we control for all likely substitutes and the competitive environment using a parsimonious utility framework.

Aggregate Regression: Market Shares

To reflect the patterns in Figures 2 and 3, we estimate the following reduced-form regression for the brands affected by the consent order:

$$(1) \qquad s_{jt} = \overbrace{\alpha_j^{pre}}^{\text{Pre False}} + \overbrace{\alpha_j^{false} I(False_t) + \beta_j^{false} I(False_t) \times (t - \tau)}^{\text{During False Claims}}$$
$$\overbrace{+ \alpha_j^{ftc} I(FTC_t) + \beta_j^{ftc} I(FTC_t) \times (t - \tau)}^{\text{After FTC}} + \varepsilon_{jt},$$

[5]Figures using sales (both quantity and volume in ounces) are consistent with the unit share plots depicted in Figures 2–4.

[6]We check whether package changes or additional varieties were introduced close to the date of the FTC consent order. We find an increase in unique UPC counts prior to the FTC consent order, followed by a decrease a few months later. To check whether the market share increase and decrease correspond to this movement in UPCs, we limit our attention to the UPCs that are present in January 2008 and continue being present until January 2010. In other words, we hold constant the set of products since the false claims were introduced, ignoring any UPCs that were introduced or removed during the period of interest. The resulting market share closely resembles the pattern displayed in Figure 2, providing evidence that the UPC additions/removals were not responsible for this market-share change. It is possible that the additional UPCs resulted in greater in-store features and displays, leading to an increase in demand, but to the extent that the greater shelf space featured the false claims on the packages, this result would still have been driven by the firm's deceptive practices.

[7]The peak in the month immediately following the FTC consent order might reflect a lag in consumers' response to the order/press release. This is somewhat corroborated by the fact that the FTC usually offers a 30-day public comment period, and the Kellogg's case had many comments, as evidenced in the Letters to Public Commenters the FTC issued in July 2009 (https://www.ftc.gov/sites/default/files/documents/cases/2009/07/090731kellogggletter.pdf). Our demand estimates in the "Data" and "Household-Level Demand Estimation" sections are robust to removal of this month's data.

Figure 1
FRONT-OF-THE-PACKAGE LABELS OF PRODUCTS CONTAINING THE FALSE CLAIMS

where s_{jt} is the market share of brand j in month t, $I(False_t)$ is 1 if t belongs to the period when the focal brand f was making the false claims, and $I(FTC_t)$ is 1 if t belongs to the period after the FTC has issued the consent order to focal brand f. The terms $I(False_t) \times (t - \tau)$ and $I(FTC_t) \times (t - \tau)$ capture any upward/downward time trends before and after the event date τ, respectively. In this specification, α_j^{false} and β_j^{false} measure the intercept and slope during the false-claims period; α_j^{ftc} and β_j^{ftc} measure the intercept and slope after the FTC press release. If the termination of the false claims affects demand for the impacted brand, we should see a significantly large drop in market-share levels as measured by $\alpha_{j=f}^{ftc} - \alpha_{j=f}^{false}$ and/or a significantly large change in trend as measured by $\beta_{j=f}^{ftc} - \beta_{j=f}^{false}$.

To capture the seasonal patterns associated with Airborne (Figure 4), we estimate the following reduced-form regression for the nutritional supplement category:

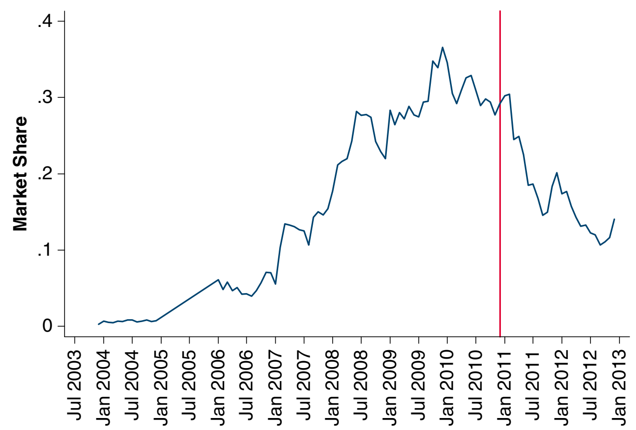
Figure 2
FROSTED MINI-WHEATS MARKET SHARE OVER TIME

Notes: Right reference line indicates date of FTC order. Left line indicates start of false claims.

Figure 3
DANNON ACTIVIA (YOGURT) AND DANACTIVE (YOGURT DRINK) MARKET SHARE OVER TIME

A: Dannon Activia

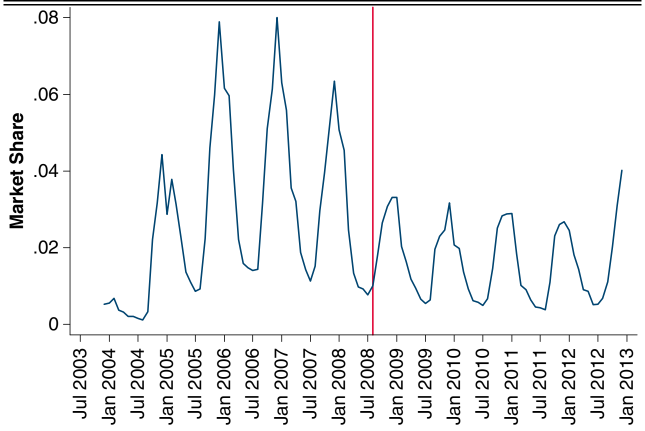
B: Dannon DanActive



Notes: Reference lines indicate dates of FTC orders.

Figure 4

AIRBORNE MARKET SHARE OVER TIME



Notes: Reference line indicates date of FTC order.

$$s_{jt} = \alpha_j^{false} I(False_t) + \alpha_j^{false,notFlu} I(notFlu) \times I(False_t) + \alpha_j^{ftc} I(FTC_t) + \alpha_j^{ftc,notFlu} I(notFlu) \times I(FTC_t) + \varepsilon_{jt}. \quad (2)$$

Here, $I(notFlu)$ is an indicator for being outside the flu season. The drop in market share levels during the flu season, if any, is captured by the difference $\alpha_j^{ftc} - \alpha_j^{false}$.

Table 2 reports the results of these regressions. The estimates indicate all the impacted brands faced a decline in market share after the issuance of the consent order, as measured by both the drop in level and the declining trend after the order. This finding is largely consistent with Figures 2–4 as well.

We next show how the focal brand's monthly sales compare to a synthetic control.

Diff-in-Diff Analysis: Sales

Here, we treat all "other" brands in the category as the control and measure how far the focal brand is from this synthetic control after the FTC consent order. We specify the diff-in-diff estimator as

Table 2

REDUCED-FORM EVIDENCE: MARKET SHARES OF IMPACTED BRANDS DROP AFTER CONSENT ORDERS

	Coefficient	*t-Statistic*
Kellogg's Frosted Mini-Wheats		
Drop in level after FTC order, $\alpha^{ftc} - \alpha^{false}$	−.008	−2.73
Months since FTC warning letter, $\beta^{ftc} - \beta^{false}$	−.002	−6.72
Dannon Activia		
Drop in level after FTC order, $\alpha^{ftc} - \alpha^{false}$	−.010	−2.94
Months since FTC warning letter, $\beta^{ftc} - \beta^{false}$	−.002	−8.13
Dannon DanActive		
Drop in level after FTC order, $\alpha^{ftc} - \alpha^{false}$	−.115	−6.51
Months since FTC warning letter, $\beta^{ftc} - \beta^{false}$	−.014	−12.57
Airborne		
Drop in level after FTC order, $\alpha^{ftc} - \alpha^{false}$	−.017	−4.48

Note: Dependent variable is market share of brand j in month t.

Table 3

DIFF-IN-DIFF ESTIMATES FOR THE FOCAL BRANDS: SALES OF IMPACTED BRANDS DROP AFTER CONSENT ORDERS

	Coefficient	*t-Statistic*
Kellogg's Frosted Mini-Wheats		
Level diff-in-diff, $\alpha^{ftc,F}$	−944.59	−13.89
Trend diff-in-diff, $\beta^{ftc,F}$	−233.33	−5.74
Dannon Activia		
Level diff-in-diff, $\alpha^{ftc,F}$	490.45	.52
Trend diff-in-diff, $\beta^{ftc,F}$	−295.63	−30.26
Dannon DanActive		
Level diff-in-diff, $\alpha^{ftc,F}$	−794.34	−21.42
Trend diff-in-diff, $\beta^{ftc,F}$	−56.80	−50.37
Airborne		
Level diff-in-diff, $\alpha^{ftc,F}$	−132.32	−9.19

Notes: Dependent variable is monthly sales of brand j. Level diff-in-diff is the change in intercept after the FTC order, relative to all other brands, Trend diff-in-diff is the change in slope after the FTC order, relative to all other brands. Standard errors cluster at the brand level.

$$sales_{jt} = \kappa^{false} + \kappa^{false,F} I(Focal_t) + \kappa^{ftc} I(FTC_t) + \kappa^{ftc,F} I(FTC_t) \times I(Focal_t), \quad (3)$$

where $\kappa^{ftc,F} = [\alpha^{ftc,F}, \beta^{ftc,F}]$ is the vector of the diff-in-diff estimates comprising the intercept and slope for the focal brand (the increment over all other brands and the increment over the false-claims period) and $sales_{jt}$ is the monthly sales of brand j.

Table 3 reports the relevant diff-in-diff estimates for the impacted brands, using sales as the dependent variable. All brands impacted by the FTC order exhibit a significant decline in sales as measured using this diff-in-diff estimator.

These regressions provide consistent evidence that the drop in market share and sales for each of the impacted brands is not likely to be due to other unobservable events that coincided with the issuance of the consent order. We now turn to a consumer choice model that controls for the competitive environment.

HOUSEHOLD-LEVEL DEMAND ESTIMATION

We now analyze the data treating each household as a unit of observation. In estimation, we focus on households that have purchased the focal brand at least once in the entire panel. For these households, we retain all purchases made within the category. We account for multiple purchases by treating each additional unit as a separate observation: when a household purchases more than one unit of a product, we replicate those observations N times, where N is the number of units purchased of that product on that purchase occasion (similar to Shum 2004).

Within a product category, we assume a household makes a choice from C options every time a transaction occurs. The price of the chosen option is directly observed from the purchase panel data. We use the RMS data to construct the prices of the other options in the consumer's consideration set. For each brand, we construct an average within-market weekly price by averaging prices (per pound) of the brand across all stores and UPCs each week. We then match the products in each household's choice set to these market-level prices. For market weeks for which this observation is missing, we use the weekly national average (across markets). We aggregate prices to the market level because of missing price information at the store-brand level. The RMS data cover only a percentage of U.S. stores, whereas the Homescan data are representative

Table 4
DEMAND ESTIMATES: RTE CEREAL

	Model Specification					
	S1		*S2*		*S3*	
	Coefficient	*t-Statistic*	*Coefficient*	*t-Statistic*	*Coefficient*	*t-Statistic*
Kellogg's Frosted Mini-Wheats						
α^{false}	−1.994	−138.44	−2.434	−139.89	−2.429	−133.73
β^{false}	.037	27.67	.034	24.01	.034	23.63
α^{ftc}	−2.159	−152.57	−2.564	−153.45	−2.562	−141.64
β^{ftc}	−.008	−15.47	−.017	−27.92	−.016	−26.57
GM Cheerios						
α^{false}	−2.799	−143.98	−.660	−23.34	−.646	−21.23
β^{false}	−.012	−8.38	−.073	−48.06	−.073	−47.91
α^{ftc}	−2.744	−150.96	−1.035	−41.43	−1.021	−36.71
β^{ftc}	−.005	−7.51	−.004	−6.96	−.004	−7.07
GM Honey Nut Cheerios						
α^{false}	−2.817	−142.21	−1.726	−78.80	−1.716	−72.45
β^{false}	.004	2.58	.000	−.13	.000	−.31
α^{ftc}	−2.763	−162.90	−1.868	−100.72	−1.859	−87.73
β^{ftc}	.002	4.20	.023	40.22	.023	39.25
Post Honey Bunches of Oats						
α^{false}	−3.065	−134.07	−3.010	−127.33	−3.000	−121.99
β^{false}	−.025	−15.25	−.005	−3.05	−.006	−3.19
α^{ftc}	−2.860	−144.18	−2.785	−136.45	−2.772	−124.44
β^{ftc}	−.007	−9.69	−.015	−19.66	−.015	−19.19
Kellogg's Frosted Flakes						
α^{false}	−3.040	−147.38	−2.797	−142.38	−2.782	−132.40
β^{false}	.025	14.33	.032	18.98	.032	18.87
α^{ftc}	−3.188	−168.88	−3.328	−170.92	−3.313	−156.11
β^{ftc}	.001	1.62	.034	46.42	.034	45.39
GM Cinnamon Toast Crunch						
α^{false}	−3.418	−140.40	−2.801	−112.25	−2.787	−105.09
β^{false}	.000	.23	−.012	−5.89	−.013	−5.97
α^{ftc}	−3.330	−162.53	−2.814	−136.96	−2.799	−120.71
β^{ftc}	.000	−.05	.014	18.98	.014	18.63
Kellogg's Rice Krispies						
α^{false}	−3.572	−153.51	−2.486	−100.00	−2.474	−92.23
β^{false}	.003	1.55	−.028	−13.67	−.028	−13.70
α^{ftc}	−3.491	−156.98	−2.527	−111.83	−2.520	−99.81
β^{ftc}	−.007	−9.18	−.009	−11.22	−.009	−10.53
Kellogg's Raisin Bran						
α^{false}	−3.497	−144.31	−4.615	−154.05	−4.598	−153.27
β^{false}	.011	5.03	.008	3.68	.008	3.57
α^{ftc}	−3.422	−161.60	−4.605	−165.63	−4.590	−164.34
β^{ftc}	−.003	−3.70	−.005	−5.87	−.005	−5.54
Store Brand Frosted Mini-Wheats						
α^{false}	−3.896	−105.21	−5.707	−125.13	−5.762	−121.77
β^{false}	−.002	−.72	−.010	−3.44	−.010	−3.46
α^{ftc}	−3.985	−124.28	−5.782	−140.96	−5.838	−136.25
β^{ftc}	.006	5.31	.002	1.48	.002	1.60
Log-likelihood	−6,382,328		−5,240,808		−5,240,736	
Prices			Yes		Yes	
Ads					Yes	

Notes: Dependent variable is household's brand choice on a given purchase occasion. Specification S1 estimates an alternative-specific logit model controlling for the relevant set of competitors. S2 adds controls for the price of the purchased brand as well as prices of the competing brands. S3 adds controls for advertisements at both the national and regional (DMA) levels. All specifications cluster at the household level; $N_{household} = 44{,}544$; $N_{observations} = 5{,}177{,}394$. The estimation uses data from the entire panel. For definitions of the parameters in the various specifications, see the main text.

of the entire U.S. population. As a result, other than for the purchased product, we do not have price information at the brand-store level for many stores.

We also construct a measure of ad seconds aired in each household's designated market area (DMA). Most of the brands advertise heavily at the national level. However, some advertising occurs at the local level. Specifically, the Nielsen data contain information on ads aired via national, network clearance, spot, and syndicated clearance TV. National represents all nationally aired advertisements via cable, network, and syndicated TV. Spot TV corresponds to ads aired directly in local markets. Network clearance and syndicated

Table 5
DEMAND ESTIMATES: YOGURT

	Model Specification					
	S1		*S2*		*S3*	
	Coefficient	*t-Statistic*	*Coefficient*	*t-Statistic*	*Coefficient*	*t-Statistic*
Dannon Activia						
α^{false}	−1.359	−58.70	−.455	−15.61	−.458	−15.45
β^{false}	.023	24.65	.008	8.54	.008	8.50
α^{ftc}	−1.447	−59.31	−.560	−18.62	−.534	−17.28
β^{ftc}	−.010	−6.57	−.019	−12.82	−.020	−13.38
Yoplait						
α^{false}	−.216	−9.78	−.212	−10.18	−.178	−8.17
β^{false}	.011	12.88	.004	4.40	.004	5.38
α^{ftc}	−.435	−18.31	−.400	−17.96	−.369	−15.67
β^{ftc}	.008	5.91	.012	9.32	.011	8.43
Stonyfield						
α^{false}	−2.485	−49.93	−.561	−9.60	−.561	−9.62
β^{false}	.067	39.30	.077	46.98	.077	46.96
α^{ftc}	−2.883	−55.24	−1.014	−17.33	−1.006	−17.17
β^{ftc}	−.010	−2.66	−.005	−1.38	−.004	−1.26
Dannon						
α^{false}	−1.034	−41.65	−.915	−38.48	−.941	−39.35
β^{false}	.015	15.23	.007	6.86	.006	6.17
α^{ftc}	−1.062	−40.98	−.753	−30.06	−.753	−30.04
β^{ftc}	.004	2.32	.021	12.18	.020	11.88
Yoplait Whips!						
α^{false}	−2.372	−48.72	−1.206	−22.37	−1.206	−22.34
β^{false}	.003	1.52	−.010	−6.09	−.010	−5.99
α^{ftc}	−2.584	−44.94	−1.206	−19.26	−1.193	−19.06
β^{ftc}	.002	.42	−.015	−4.46	−.014	−4.38
Chobani						
α^{false}	−2.288	−44.45	.441	6.27	.442	6.29
β^{false}	.102	31.99	.111	35.90	.111	35.98
α^{ftc}	−2.018	−51.13	.537	8.76	.553	9.00
β^{ftc}	.029	12.92	.013	5.93	.013	5.83
Log-likelihood	−8,335,325		−7,772,024		−7,770,710	
Prices			Yes		Yes	
Ads					Yes	

Notes: Dependent variable is household's brand choice on a given purchase occasion. Specification S1 estimates an alternative-specific logit model controlling for the relevant set of competitors. S2 adds controls for the price of the purchased brand as well as prices of the competing brands. S3 adds controls for advertisements at both the national and regional (DMA) levels. All specifications cluster at the household level; $N_{household} = 35{,}837$; $N_{observations} = 5{,}697{,}053$. The estimation uses data from the entire panel. For definitions of the parameters in the various specifications, see the main text.

clearance correspond to commercials that are fed from the satellite with the network or syndicated program and aired by the local affiliate in the market (Nielsen 2012). We use the duration[8] (in seconds) of advertisements aired per DMA and match it to each household's DMA. Nielsen divides the United States into 210 DMAs. We do not observe other kinds of advertising, such as in-store displays, features, and non-TV advertising. To the extent these relate to the false-advertising campaigns, our coefficients of interest will capture these effects.

To construct the choice set, for each category, we start with the top nine brands by total sales. We then exclude any products that were discontinued. This approach leaves us with eight brands in the cereal category, six in the yogurt category, four in the yogurt drink category, and three in the nutritional supplement category. For cereal, we also add the store-brand frosted shredded wheat as an additional control. We also ensure these brands are likely to be purchased by the heavy[9] consumers of the impacted brand so that our analysis contains all likely substitutes. We aggregate the rest of the products into a composite brand called "Other." Note that doing so ensures our analysis still contains all products, including the discontinued ones. Following Shum (2004), we construct a composite measure of prices and advertisements for the Other brand. For price, $p_{Other,t}$, we use the sales-volume-weighted average price across all other brands,

$$p_{Other,t} = \sum_{j \notin C} \frac{\upsilon_{jt}}{\sum_{k \notin C} \upsilon_{kt}} p_{jt},$$

[8]Other measures, such as expenditure and gross rating points, are highly correlated with duration. However, ad expenditure is not available at the local level in all cases because time slots for local ads are purchased nationally and then assigned to clearance TV.

[9]Heavy consumers are those who purchased more than 16 units of the brand over the time span of the data.

Table 6
DEMAND ESTIMATES: YOGURT DRINKS

	Model Specifications					
	S1		*S2*		*S3*	
	Coefficient	*t-Statistic*	*Coefficient*	*t-Statistic*	*Coefficient*	*t-Statistic*
Dannon DanActive						
α^{false}	1.131	10.25	1.330	13.43	1.384	13.87
β^{false}	.032	11.46	.032	11.98	.037	12.91
α^{ftc}	1.003	8.09	1.266	9.94	1.301	10.18
β^{ftc}	−.037	−4.08	−.042	−4.59	−.043	−4.65
Dannon Danimals						
α^{false}	−.711	−5.75	−.700	−5.90	−.677	−5.69
β^{false}	.042	11.65	.042	12.06	.042	12.24
α^{ftc}	−.545	−3.88	−.500	−3.63	−.464	−3.38
β^{ftc}	−.025	−2.59	−.029	−2.99	−.030	−3.07
Lifeway						
α^{false}	−1.299	−6.91	−1.354	−7.19	−1.332	−7.06
β^{false}	.054	9.10	.056	9.45	.056	9.37
α^{ftc}	−1.244	−7.14	−1.196	−6.94	−1.160	−6.73
β^{ftc}	.013	1.30	.006	.60	.006	.54
Stonyfield						
α^{false}	−3.495	−9.73	−3.355	−9.27	−3.320	−9.27
β^{false}	−.012	−1.19	−.013	−1.24	−.013	−1.25
α^{ftc}	−2.535	−7.14	−2.354	−6.62	−2.319	−6.53
β^{ftc}	−.020	−1.35	−.025	−1.73	−.026	−1.77
Log-likelihood	−176,861		−176,154		−175,882	
Prices			Yes		Yes	
Ads					Yes	

Notes: Dependent variable is household's brand choice on a given purchase occasion. Specification S1 estimates an alternative-specific logit model controlling for the relevant set of competitors. S2 adds controls for the price of the purchased brand as well as prices of the competing brands. S3 adds controls for advertisements at both the national and regional (DMA) levels. All specifications cluster at the household level; $N_{household}$ = 8,828; $N_{observations}$ = 166,080. The estimation uses data from the entire panel. For definitions of the parameters in the various specifications, see the main text.

and for advertisement, $\mathbf{Ad}_{Other,t}$, we use the total air duration of all other advertisements,

$$\mathbf{Ad}_{Other,t} = \sum_{j \notin C} \mathbf{Ad}_{jt},$$

where υ_{jt} is the sales volume of brand j at time t.

Utility Specification

We specify individual i's utility from purchasing brand j at time t as

$$u_{ijt}(\theta) = \overbrace{\alpha_j^{pre}}^{\text{Pre False}} + \overbrace{\alpha_j^{false} I(False_t) + \beta_j^{false} I(False_t) \times (t - \tau)}^{\text{During False Claims}}$$
$$\overbrace{+ \alpha_j^{ftc} I(FTC_t) + \beta_j^{ftc} I(FTC_t) \times (t - \tau)}^{\text{After FTC}} + \gamma \mathbf{F}_{jt} + \varepsilon_{ijt},$$

where α_j^{pre} is the consumer's preference for brand j before the focal brand began making the false claim; $I(False_t)$ is 1 if t belongs to the period when the brand was making the false claims; $I(FTC_t)$ is 1 if t belongs to the period after the FTC order; τ is the date of the FTC order; $t - \tau$ measures how far t is from the date of the consent order;[10] α_j^{false} and β_j^{false} measure the intercept and slope during the false-claims period; α_j^{ftc} and β_j^{ftc} measure the intercept and slope in the period after the FTC press release, that is, once the consumer (potentially) knows the claim is false; β^{false} captures the cumulative effect of consumers' exposure to false claims; and β^{ftc} captures any effects of information dissemination or lagged consumer response. A priori, a brand might face either a level change, a trend change, or both. The vector $\mathbf{F}_{jt} = \{p_{jt}, \mathbf{Ad}_{jt}\}$ includes prices and the duration of aired national and local advertisements; γ indicates a consumer's sensitivity to these firm-side variables $\mathbf{F}_{jt}$. θ is the set of parameters $\{\alpha_j^{pre}, \alpha_j^{false}, \beta_j^{false}, \alpha_j^{ftc}, \beta_j^{ftc}, \gamma\}$ governing a consumer's decision; and ε_{ijt} is the unobserved (to the researcher) shock, assumed to be independent across time and across all available options.[11] We estimate a different specification for each category to capture the specifics of that category.

The yogurt and yogurt drinks categories have no "before" period because the claims were present since product introduction. Hence, we use the following specification:

[10]Because we capture declining market share via a time trend and specifically model the seasonality effects in the nutritional supplement category, we do not include time fixed effects.

[11]To capture a more flexible substitution pattern, we also estimated a nested logit model, including all trips that are likely to be associated with a purchase within a similar category. For example, for cereal, we included all products that can be thought of as "breakfast goods": breakfast bars, granola, hot cereal, frozen waffles, pancakes, French toast, frozen/refrigerated breakfast, granola and yogurt bars, hominy grits, instant breakfast—powdered, toaster pastries, and wheat germ. All these modules belong to the broader product groups of "Breakfast Food," "Breakfast Foods—Frozen," and "Cereal" per Nielsen's category definitions. The resulting market-share changes using the nested logit estimates were qualitatively and quantitatively consistent with our logit model.

Table 7
DEMAND ESTIMATES: NUTRITIONAL SUPPLEMENTS

	Model Specification					
	S1		*S2*		*S3*	
	Coefficient	*t-Statistic*	*Coefficient*	*t-Statistic*	*Coefficient*	*t-Statistic*
Airborne						
α^{false}	−.849	−31.91	−.227	−3.48	−.038	−.49
$\alpha^{false,notFlu}$	−1.077	−40.75	−1.041	−39.23	−1.152	−35.75
α^{ftc}	−1.404	−46.78	−.857	−14.01	−.694	−9.30
$\alpha^{ftc,notFlu}$	−1.039	−28.84	−.999	−27.49	−1.043	−26.78
Nature Made						
α^{false}	−2.221	−48.39	−2.254	−48.89	−2.081	−34.49
$\alpha^{false,notFlu}$	−.074	−2.29	−.054	−1.67	−.162	−4.26
α^{ftc}	−2.161	−62.91	−2.185	−63.08	−2.034	−42.08
$\alpha^{ftc,notFlu}$	−.051	−2.18	−.053	−2.23	−.090	−3.29
Nature's Bounty						
α^{false}	−2.725	−43.68	−2.796	−44.35	−2.607	−35.58
$\alpha^{false,notFlu}$	−.020	−.46	−.009	−.20	−.120	−2.53
α^{ftc}	−2.305	−49.95	−2.373	−50.48	−2.216	−38.68
$\alpha^{ftc,notFlu}$	.035	1.37	.034	1.29	−.007	−.23
Emergen-C						
α^{false}	−3.466	−45.64	.149	.47	.350	1.09
$\alpha^{false,notFlu}$	−.335	−4.65	.110	1.40	−.007	−.08
α^{ftc}	−3.183	−54.52	1.516	4.04	1.686	4.39
$\alpha^{ftc,notFlu}$	−.457	−8.68	−.145	−2.92	−.192	−3.83
Log-likelihood	−273,725		−264,189		−263,976	
Prices			Yes		Yes	
Ads					Yes	

Notes: Dependent variable is household's brand choice on a given purchase occasion. Specification S1 estimates an alternative-specific logit model controlling for the relevant set of competitors. S2 adds controls for the price of the purchased brand as well as prices of the competing brands. S3 adds controls for advertisements at both the national and regional (DMA) levels. All specifications cluster at the household level; $N_{household} = 10{,}738$; $N_{observations} = 285{,}603$. The estimation uses data from the entire panel. For definitions of the parameters in the various specifications, see the main text.

$$u_{ijt}(\theta) = \alpha_j^{false} I(False_t) + \beta_j^{false} I(False_t) \times (t - \tau) + \alpha_j^{ftc} I(FTC_t) + \beta_j^{ftc} I(FTC_t) \times (t - \tau) + \gamma \mathbf{F}_{jt} + \varepsilon_{ijt}.$$

Finally, in the nutritional supplement category, seasonality (whether it is the flu season or not) is captured using the following specification:

$$u_{ijt}(\theta) = \alpha_j^{false} I(False_t) + \alpha_j^{false,notFlu} I(notFlu) \times I(False_t) + \alpha_j^{ftc} I(FTC_t) + \alpha_j^{ftc,notFlu} I(notFlu) \times I(FTC_t) + \gamma \mathbf{F}_{jt} + \varepsilon_{ijt}.$$

Assuming that the unobserved (to the researcher) shocks follow a Type I extreme-value distribution, the probability that individual i chooses brand j at time t is given by

$$Pr_{ijt}(\theta) = \frac{e^{u_{ijt}(\theta)}}{\sum_{k \in C} e^{u_{ikt}(\theta)}}.$$

When we aggregate the probabilities over all purchase occasions that i makes, the individual-level probability is

$$Pr_i(\theta) = \prod_{t=1}^{T} \prod_{j=1}^{C} Pr_{ijt}^{I_{ijt}}(\theta),$$

where I_{ijt} is 1 if individual i purchased brand j on purchase occasion t.

The overall log-likelihood across all individuals can then be written as

$$LL(\theta) = \sum_{i=1}^{N} \log Pr_i(\theta).$$

Discussion on Endogeneity

We do not use instrumental variables for prices and advertisements for the following reason. Endogeneity concerns typically arise from omitted variable biases. Of particular relevance to us are two cases. In the first case, a variable exists that is unobserved by the researcher but is observed by both consumers and the firm. In our analysis, this is less of a concern, especially in the local window around the FTC order: if firms and consumers respond to the FTC press release, then explicitly including the before and after variables, $I(False_t)$ and $I(FTC_t)$, in our analysis reduces such endogeneity concerns. The second case pertains to firms changing strategic variables in response to anticipated demand. For example, a firm may add a "gluten-free" label on its packaging responding to an increasing consumer trend for gluten-free products. A before–after analysis following this change will lead to an upward bias in the impact of the label. However, in our analysis, inference is based on the termination of the false claims and the revelation of deception to consumers, both of which are outside the firm's control.

RESULTS

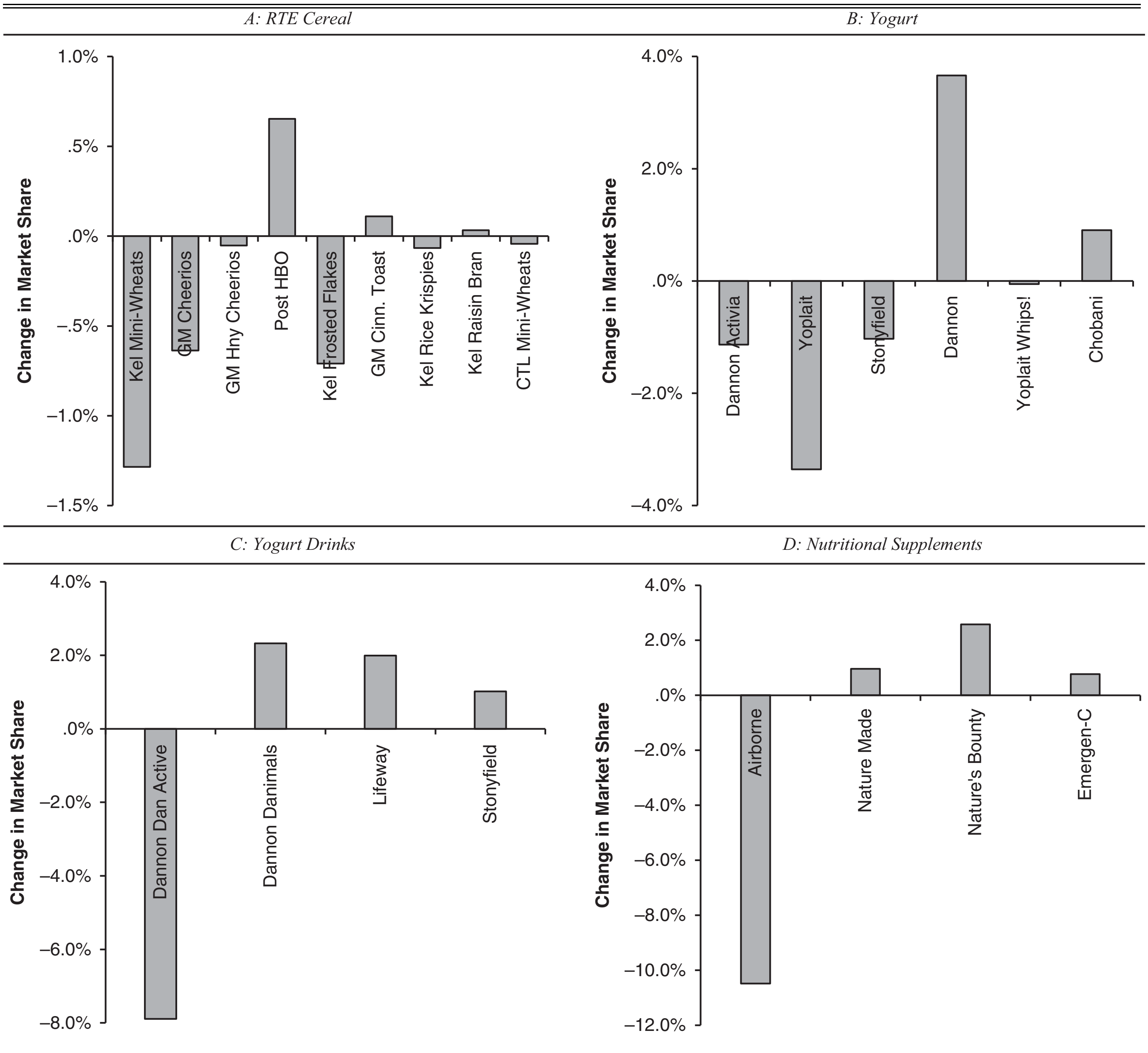
We estimate the model including all the data postevent, which gives us a long-run measure of the impact of the event. This estimate is likely to be conservative, especially if market shares rebound quickly. We also estimate the model using shorter six-month and two-month periods after the event, and in the next subsection, we conduct a placebo test, pretending the event occurred at a different date.

The results of the demand estimation for all categories are reported in Tables 4–7. In specification S1, we verify that the patterns presented in Section 2 hold even after we include the relevant set of competitors. Specification S2 controls for the price of the purchased brand as well as prices of the competing brands. Specification S3 adds controls for advertisements at both the national and regional (DMA) levels. All specifications cluster at the household level.

Because the relative magnitudes of all estimates matter, we simulate the market shares of all brands using the estimates from specification S3 to highlight the decline in demand. Figure 5 plots the percentage point decline in market share four months after the focal brand was required to terminate the false claims, relative to the market share just before. These figures provide further evidence of the decline in demand after the FTC consent order. All impacted brands face a large decline in demand even after we control for prices, advertisements, and the competitive environment. In the yogurt category, we find Yoplait faces a steeper decline than Activia, possibly because of spillovers to Yoplait, which caters to a similar segment as Activia. This

Figure 5

CHANGE IN BRAND MARKET SHARES FOUR MONTHS AFTER FTC ORDER



Notes: The decline in market share four months after the FTC consent order is steepest for three of four focal brands: Frosted Mini-Wheats, DanActive, and Airborne.

Table 8

CHANGE IN MARKET SHARE USING SMALLER WINDOWS, AND COMPARED WITH A PLACEBO WINDOW

	Six-Month Window		*Two-Month Window*[a]	
	FTC Order	*Placebo*	*FTC Order*	*Placebo*
RTE Cereal				
Kellogg's Frosted Mini-Wheats	**−1.96%**	−.02%	**−1.27%**	−.34%
GM Cheerios	−.25%	.37%	.16%	.11%
GM Honey Nut Cheerios	−.08%	−.23%	−.19%	−.19%
Post Honey Bunches of Oats	.76%	−.14%	.74%	−.03%
Kellogg's Frosted Flakes	−.75%	.02%	−.54%	−.19%
GM Cinnamon Toast Crunch	.09%	−.06%	.08%	−.10%
Kellogg's Rice Krispies	.17%	.73%	.02%	.13%
Kellogg's Raisin Bran	−.51%	−.01%	−.33%	−.12%
Store Brand Frosted Mini-Wheats	.02%	−.19%	.01%	−.04%
Other	2.50%	−.46%	1.31%	.78%
Yogurt				
Dannon Activia	**−1.13%**	−.62%	**−.93%**	.42%
Yoplait	−4.36%	1.65%	−5.42%	2.01%
Stonyfield	.13%	.60%	.20%	.12%
Dannon	3.14%	1.56%	4.53%	−.20%
Yoplait Whips!	−.28%	−.90%	−.84%	−.39%
Chobani	1.93%	.15%	2.00%	−.04%
Other	.57%	−2.43%	.46%	−1.91%
Yogurt Drinks				
Dannon DanActive	**−7.61%**	−.88%	**−8.87%**	4.35%
Dannon Danimals	4.67%	−.69%	7.50%	−1.02%
Lifeway	1.12%	.15%	−1.36%	−.06%
Stonyfield	.55%	.11%	.31%	.17%
Nutritional Supplements				
Airborne			**−12.10%**	−8.16%
Nature Made			6.45%	1.29%
Nature's Bounty			3.54%	5.03%
Emergen-C			2.10%	1.84%

[a]For nutritional supplements, instead of a two-month window, a three-month window that spanned flu season is used.

Notes: The table presents the simulated drop in market shares after the FTC order or the placebo window, using estimates in Tables 1-4 in the Web Appendix. Boldface indicates impacted brands. Data used in the analysis are limited to six- and two- months before and after the FTC order/placebo windows.

hypothesis is partly supported by the fact that Yoplait in 2010 was advertising the digestive benefits of its yogurt.[12]

Placebo Tests

Because market shares can fluctuate for many reasons, we conduct a placebo test to measure the drop in market share in a placebo period and compare it with the drop in market share after the termination of the claims. Whereas the previous analysis used all the data after the FTC consent order, we limit our analysis here to periods of six and two months before and after the order. For nutritional supplements, we use the three-month peak flu season, from December to February. This provides an additional validity check on shorter-term effects.

[12]The Wayback Machine (archive.org/web) contains a copy of the page at http://www.yoplait.com/health_spotlight.aspx on July 2, 2010, on which Yoplait stated, "Yoplait yogurt may naturally support healthy digestion, with live and active cultures."

Table 9

REVENUE DECLINE RELATIVE TO PEAK

	In-Sample	*Projected (in Millions)*	
Brand	$r_{j,\tau+4} - r_{j,\tau}$	$R_{j,\tau+4} - R_{j,\tau}$	*Monthly Revenue*
Kellogg's Frosted Mini-Wheats	−$4,035	−$3.51	$12.44
Dannon Activia	−$4,398	−$3.82	$31.27
Dannon DanActive	−$464	−$.40	$2.57
Airborne	−$4,172	−$3.63	$5.44

Notes: In-sample revenue decline four months after the FTC order, computed using estimates from specification S3 of the demand model. Projected numbers project these estimates to the U.S. population.

We choose the placebo event as conservatively as possible, picking the date prior to the FTC consent order when the market-share dip appears to be the steepest in Figures 2–4. For Frosted Mini-Wheats, this date corresponds to August 2006; for Activia, October 2008; for DanActive, May 2008; and for Airborne, the previous year.

Table 8 reports the market-share changes four and two months after the FTC order/placebo, in the shorter periods using the six- and two-month windows, respectively. Demand estimates are provided in the Web Appendix. In all four cases, we find the drop in market share for the impacted brand following the consent order is steeper than the drop after the placebo event; that is, controlling for prices and advertisements explains most of the market-share decline following the placebo event but does not explain the decline after the FTC order. Moreover, the drop in market share for the impacted brand relative to other brands after the FTC order follows the same pattern qualitatively when we use the six-month or two-month windows compared with the longer time frame, providing us with a robustness check.

Economic Significance

To determine the magnitude of revenue decline relative to the peak, we project the estimated revenue decline to the U.S.

Figure 6

HETEROGENEITY IN RESPONSES TO THE WITHDRAWAL OF THE MINI-WHEATS CLAIMS

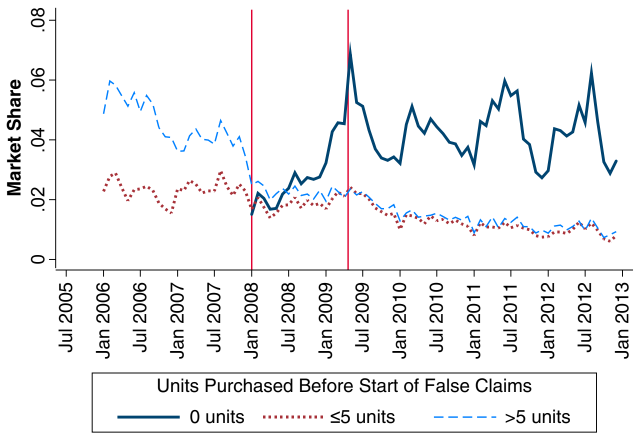


Table 10

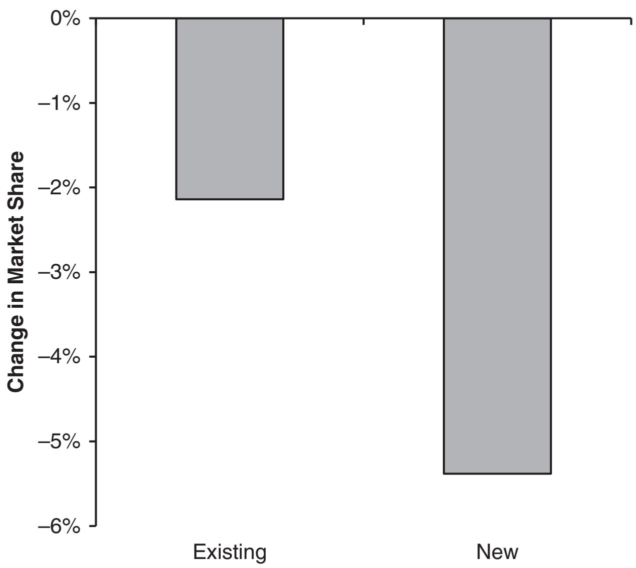
HETEROGENEOUS DEMAND ESTIMATES: RTE CEREAL

	Existing Users: θ		*New Users: θ_{new}*	
	Coefficient	*t-Statistic*	*Coefficient*	*t-Statistic*
Kellogg's Frosted Mini-Wheats				
α^{false}	−.876	−37.56	.039	1.22
β^{false}	.012	6.42	.051	16.38
α^{ftc}	−.875	−36.65	−.149	−4.87
β^{ftc}	−.019	−20.67	.004	3.45
GM Cheerios				
α^{false}	.629	17.56	−.187	−4.92
β^{false}	−.070	−35.36	−.024	−7.97
α^{ftc}	.448	13.21	−.059	−1.66
β^{ftc}	−.007	−7.66	.000	−.38
GM Honey Nut Cheerios				
α^{false}	−.292	−9.28	−.115	−2.89
β^{false}	−.012	−5.54	−.004	−1.01
α^{ftc}	−.211	−7.52	−.090	−2.78
β^{ftc}	.019	21.44	.000	.20
Post Honey Bunches of Oats				
α^{false}	−1.501	−45.90	−.235	−4.93
β^{false}	−.012	−5.59	−.014	−3.99
α^{ftc}	−1.246	−40.01	−.115	−2.80
β^{ftc}	−.014	−12.77	.001	.90
Kellogg's Frosted Flakes				
α^{false}	−1.404	−46.28	.022	.54
β^{false}	.021	9.21	.006	1.74
α^{ftc}	−1.787	−60.42	−.008	−.21
β^{ftc}	.032	28.86	.001	1.04
GM Cinnamon Toast Crunch				
α^{false}	−1.412	−38.52	.010	.21
β^{false}	−.024	−8.84	−.002	−.38
α^{ftc}	−1.281	−38.12	.062	1.53
β^{ftc}	.008	7.07	.002	1.42
Kellogg's Rice Krispies				
α^{false}	−1.137	−31.64	−.040	−.86
β^{false}	−.037	−14.08	.000	−.11
α^{ftc}	−1.056	−30.88	.065	1.62
β^{ftc}	−.011	−9.35	.000	−.11
Kellogg's Raisin Bran				
α^{false}	−3.025	−82.39	−.146	−2.85
β^{false}	−.001	−.27	−.003	−.58
α^{ftc}	−2.889	−83.93	−.133	−2.97
β^{ftc}	−.007	−5.94	.002	1.05
Store Brand Frosted Mini-Wheats				
α^{false}	−2.264	−43.21	−.154	−2.01
β^{false}	.008	2.17	−.004	−.74
α^{ftc}	−2.065	−42.43	−.131	−2.00
β^{ftc}	−.008	−4.94	.000	.17
Log-likelihood	−5,718,196			
Prices	Yes			
Ads	Yes			

Notes: Dependent variable is household's brand choice on a given purchase occasion. The specification estimates an alternative-specific logit model for two types of households: those with a history of prior usage and those who had never purchased before the false claim was made. The parameters for those who had not purchased prior to the claim: $\theta + \theta_{new}$. The specification clusters at the household level; $N_{household} = 44{,}544$; $N_{observations} = 5{,}177{,}394$. The estimation uses data from the entire panel. For definitions of the parameters in the various specifications, see the main text.

Figure 7

HETEROGENEITY IN RESPONSES BETWEEN NEW AND EXISTING CUSTOMERS



Notes: As illustrated in the figure, existing customers are impacted less than new customers by the FTC order.

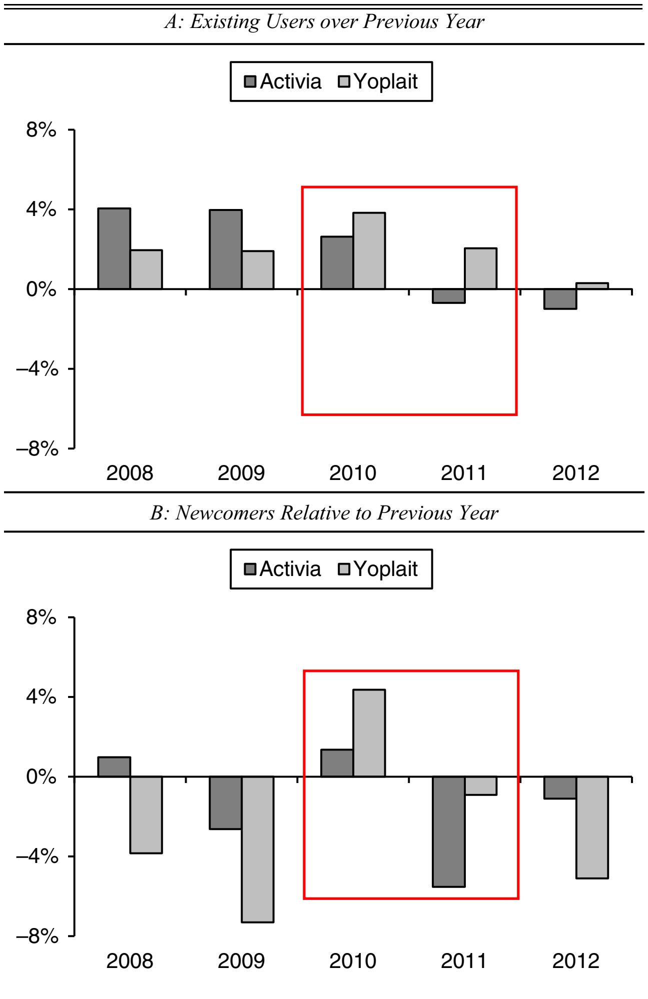
population. To do so, we first compute the share of households, hh_{cat}/hh_{panel}, that consume products in a given category (cat). We do so because not every household participates in a given category (e.g., not everyone buys RTE cereal). We then multiply this amount by the total households in the United States, HH_{US}, to get the relevant household population for each category. We then compute the average units consumed per household. To do so, we infer the total units $q_{cat,t}$ sold in month t in category cat directly from the data; $q_{cat,t}/hh_{cat}$ gives us the average category consumption per household. Last, the share of the focal brand, s_{jt}, is computed from our demand estimates. Multiplying this by the average brand price p_j, where j is the impacted brand, gives us the estimated revenue R_{jt}:

$$R_{jt} = \left(\frac{hh_{cat}}{hh_{panel}} \times HH_{US}\right)\frac{q_{cat,t}}{hh_{cat}} s_{jt} \times p_j.$$

Table 9 lists the in-sample revenue drop $r_{j,\tau+4} - r_{j,\tau}$ and the projected monthly revenue drop $R_{j,\tau+4} - R_{j,\tau}$ for the impacted brand, where $r_{j,\tau}$ and $R_{j,\tau}$ are the in-sample and projected revenues in the month of the FTC order, respectively. The table also lists the estimated monthly revenues, which closely correspond to the revenue/sales figures reported in news media. Frosted Mini-Wheats and Airborne are the hardest hit in terms of the decline relative to their peak sales.

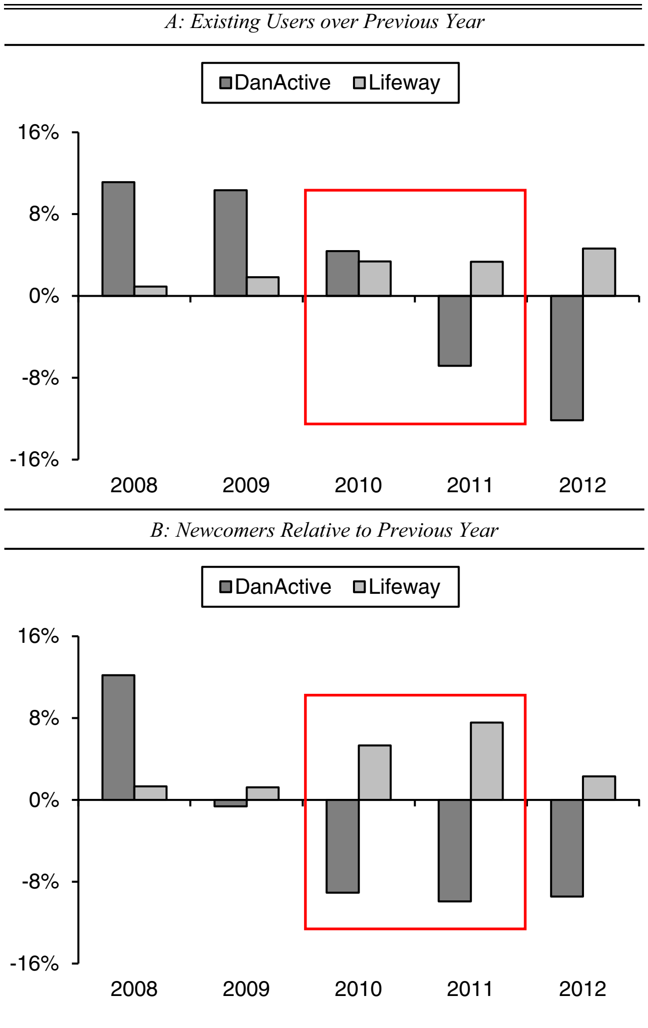
Perhaps more important is the firm's potential revenue gain from the false claims. To quantify this, we calculate the revenue gain for Frosted Mini-Wheats, for which we have a start date of the claims. If we assume the entire gain in market share from January 2008 (start of the false claims) to September 2010 (when

Figure 8
YOGURT: CHANGE IN USERS BEFORE AND AFTER FTC ORDER, BY BRAND



Notes: Activia attracted fewer newcomers in 2011; aditional existing users also declined, but at a lower rate.

Figure 9
YOGURT DRINKS: CHANGE IN USERS BEFORE AND AFTER FTC ORDER, BY BRAND



Notes: Fewer newcomers purchased DanActive the year of and the year after the FTC order.

market shares seem to stabilize to pre-2008 levels) was due to the presence of the false claims, the total revenue gain for these 32 months from these claims is $105 million. While this number controls for all observable marketing activities (e.g., changes in price, ads, competitors' response), we cannot rule out other unobserved marketing activities conducted during this period that were independent of the false-claims campaigns. We therefore provide a few robustness checks to our revenue calculations. Using a shorter time frame, to capture the possibility of a short-lived effect, the revenue estimates indicate a gain of $59 million over one year surrounding the FTC order. Using raw market-share data from the sample of households used in the estimation (i.e., households that purchased Mini-Wheats at least once over the panel), the estimate is $100.4 million. Using a more predictive/flexible modeling approach,[13] our revenue estimate is $144 million. Thus, our estimate of the revenue gain is in the range of $59 million–$144 million. This gain is substantial, even after we control for advertisement expenses (which were similar to those in 2008), relative to the 2013 class-action settlement in which Kellogg's agreed to a $4 million settlement fund.

These calculations show firms stand to gain from making false claims even if they are eventually caught. However, whether this is true in the long run is unclear, because consumers may begin to lose trust in the brand, and class-action settlements may involve larger sums.

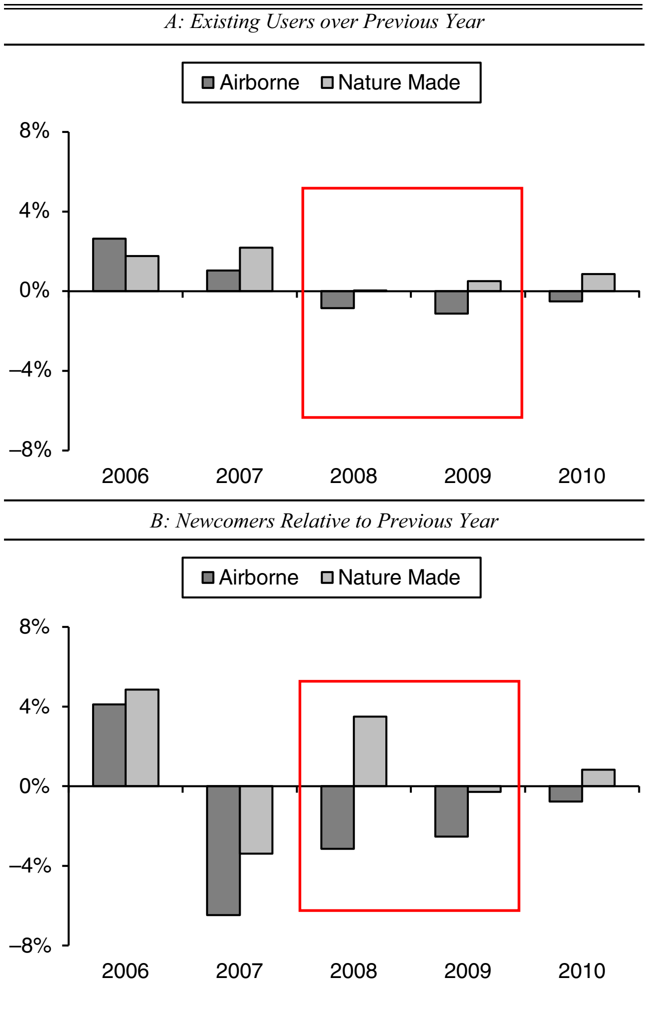
Heterogeneity in Consumer Responses

We next explore heterogeneity in consumer responses to determine whom these claims affect the most. We first explore heterogeneity in consumer responses to the Frosted Mini-Wheats claim withdrawal, because we have a start and end date for the false claims for this product. At the start date of the claims (January 2008), the only addition to the product was the front-of-the-package label without any product composition change. This

[13]The model incorporates all competitors' strategic variables in each brand's focal equation and accounts for weekly search volume using Google Trends.

Figure 10

NUTRITIONAL SUPPLEMENTS: CHANGE IN USERS BEFORE AND AFTER FTC ORDER, BY BRAND



Notes: Fewer newcomers purchased Airborne around date of FTC order.

enables us to investigate the behavior of the households that had never purchased the brand before the false claims were made.[14]

We classify households into two types according to whether they had purchased any units before the start of the claims. This is indicative of households that began purchasing Frosted Mini-Wheats because of the claims. Our hypothesis is that the drop in market share for this group is higher than for consumers who were already purchasing the product before the start of the false claims. Figure 6 plots the market share by the number of units purchased prior to January 2008. By virtue of our classification, we expect to see a regression to the mean, where heavy users' consumption levels drop and nonusers' consumption levels increase. However, our inference relies on the pattern at the date of the FTC consent order, when the market-share decline is starkest for the users who had not purchased prior to January 2008.

We further quantify this heterogeneity by estimating a demand system that specifically accounts for these two types of households:

$$
\begin{aligned}
u_{ijt}(\theta) = {} & \overbrace{\alpha_j^{false} I(False_t) + \beta_j^{false} I(False_t) \times (t - \tau) + \alpha_{new,j}^{false} I(False_t) + \beta_{new,j}^{false} I(False_t) \times (t - \tau)}^{\text{During False Claims}} \\
& \overbrace{+ \alpha_j^{ftc} I(FTC_t) + \beta_j^{ftc} I(FTC_t) \times (t - \tau) + \alpha_{new,j}^{ftc} I(FTC_t) + \beta_{new,j}^{ftc} I(FTC_t) \times (t - \tau)}^{\text{After FTC}} \\
& + \gamma \mathbf{F}_{jt} + \varepsilon_{ijt},
\end{aligned}
$$

where $\theta = \{\alpha_j^{false}, \beta_j^{false}, \alpha_j^{ftc}, \beta_j^{ftc}\}$ is the parameter vector for households with a history of prior usage, or existing consumers, and $\theta_{new} = \{\alpha_{new,j}^{false}, \beta_{new,j}^{false}, \alpha_{new,j}^{ftc}, \beta_{new,j}^{ftc}\}$ is the additional change over θ for households that had not purchased prior to the claim. In this specification, the relevant parameter vector for the new households is $\theta + \theta_{new}$. Table 10 reports the estimates for the existing and new households. Figure 7 plots the change in market share four months after the FTC consent order, relative to the market share just before. This figure shows that the termination of the false claims has the biggest impact on newcomers.

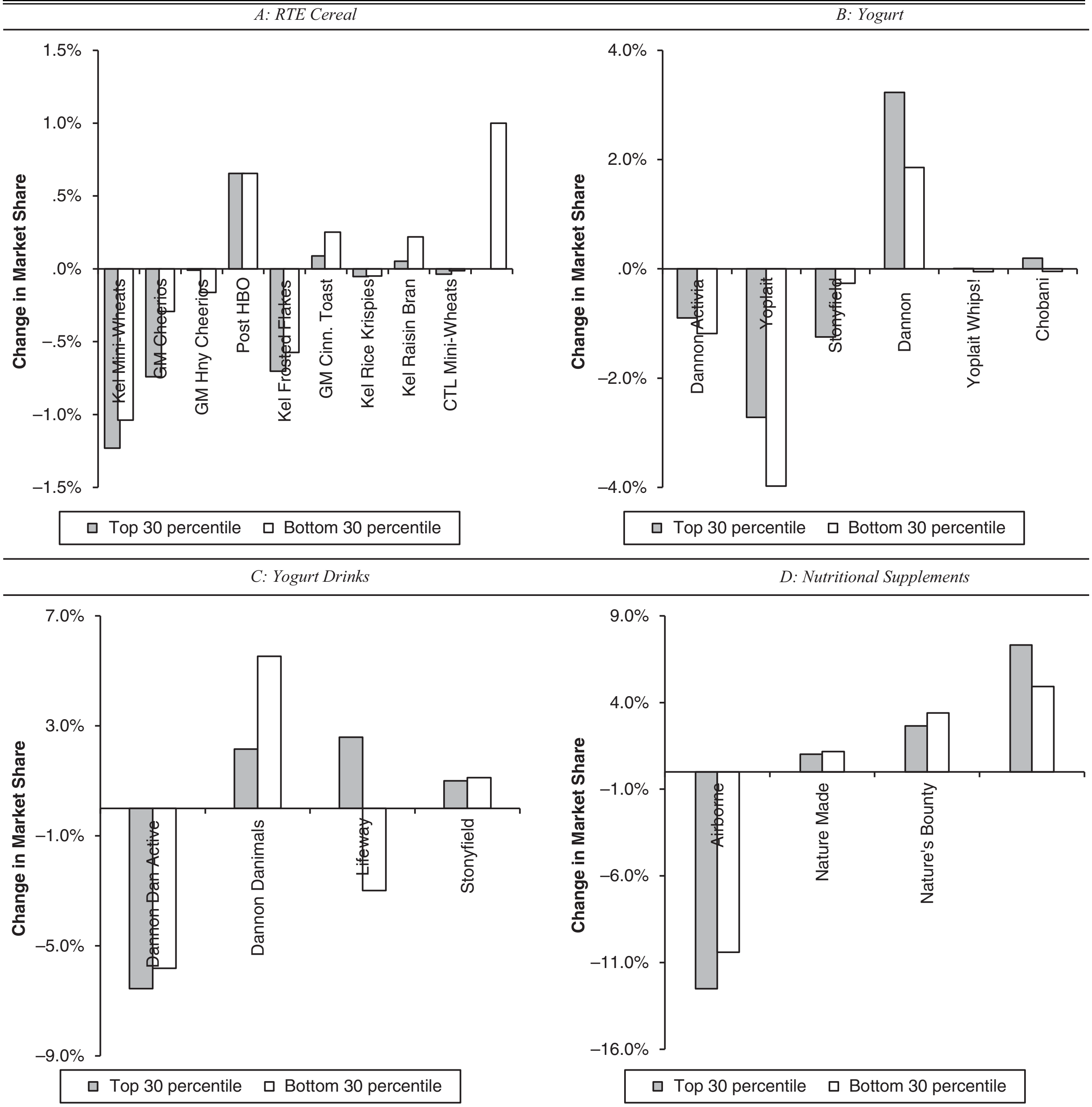
For the other three categories, we cannot make such a direct comparison because the claims were present in the products' messages since inception. Therefore, we cannot identify which consumers are likely to have purchased the brand because of the claim and which consumers purchased the product for other reasons (e.g., taste, brand loyalty). Instead, we examine the brand's ability to attract newcomers and retain existing users in the year of and the year after the FTC order. For each year, we define newcomers as those who had not purchased the brand in any previous year but purchased it that year. Existing users are those who had purchased the brand in any of the previous years and continued to purchase that year.

Figures 8–10 plot the additional percentage of newcomers the focal brand (and the top competitor) received each year compared with the previous year. The figures also plot the increase/decrease in the firm's existing users each year. Across all brands, we find that the percentage of newcomers joining the brand is lower for the impacted brand (and not the competitor brands) the year of or the year after the consent order. Existing users also begin to decline, but the drop is not as steep. Note that the competitors appear to be increasing their base of existing users at a stable rate. These figures provide additional evidence that the impacted brand's ability to attract newcomers drops drastically following the FTC order and that loyalty can play a big role in a firm's ability to retain its consumers.

[14]In most categories, we find that consumers who differed on observable demographics exhibit little difference in purchase behavior. This finding is consistent with Bucklin and Gupta (1992) and Rossi, McCulloch, and Allenby (1996), who find that purchase histories are more informative than observable demographics. In the nutritional supplement category, we find evidence that households with members over the age of 65 continue purchasing Airborne, and households with children under 12 stop purchasing Airborne (vs. households with no children).

We also estimated a model that incorporates unobserved heterogeneity using the latent class logit approach, similar to Gupta and Chintagunta (1994). However, this approach picks up differences not central to our analysis. For example, using a class membership equation of the form $\eta_c z_i$, where z_i includes a constant, prior usage, and demographics, leads to recovery of unobserved classes that differ most in the estimated price coefficient. Although useful in estimating the overall underlying heterogeneity in the population, this approach does not help recover a specific type of heterogeneity, which in our case is aimed at understanding which group leaves the focal brand and which does not.

Figure 11
CHANGE IN MARKET SHARE FOR TOP VS. BOTTOM DMAS BY AD DURATION



Notes: The figure shows the change in brands' market shares four months after termination of the false claims.

Heterogeneity Across Markets

We next explore whether different markets respond differently to the false-advertising campaigns. To do so, we estimate the household-level choice model for DMAs where advertising is high (comprising the top 30th percentile of advertising duration) and for those where advertising is low (comprising the bottom 30th percentile). Figure 11 plots the market-share changes for the top and bottom DMAs. Although this is still a correlational exercise, DMAs that face more advertising seem to see the sharpest drops in market shares (except for the yogurt category).

Discussion

Mechanism. One limitation of this study is that the data enable us to analyze only four products. Future research will benefit from documenting market-share changes as more FTC orders are issued, and highlighting patterns indicative of the

Table 11

REDUCED-FORM REGRESSION: MARKET SHARE OF RICE KRISPIES

	Coefficient	*t-Statistic*
All Data		
Drop in level after FTC order	−.001	−.23
Months since FTC order	.000	1.18
Two-Month Window		
Drop in level after FTC order	−.001	−1.17
Months since FTC order	−.003	−4.14

magnitude and duration of the effect, along with who is impacted. For instance, using our limited sample, it appears that products that have the backing of big brands regain market share in the long run (e.g., Kellogg's), whereas products without such backing suffer longer-lasting consequences (e.g., Airborne). Another hypothesis is that brands whose false-credence claim is central to their differentiation strategy fare worse. This can be seen in the case of Airborne, which was hurt most in the long run. Brands that differ from their competitors on attributes other than the claim (e.g., in terms of taste) seem to fare better. The parent brand also might lose some equity (e.g., we see some evidence that Kellogg's Frosted Flakes was also affected when Frosted Mini-Wheats was issued a consent order). Finally, related to who is impacted, consumers with a history of usage prior to the FTC order are not as affected by the removal of the false claim, compared with newcomers. All of these patterns could serve as potential hypotheses to test as more products come under scrutiny, providing the researcher with a larger sample of products affected by the FTC regulation.

Future research will also benefit from documenting what happens to firms that proactively remove deceptive claims prior to receiving an FTC order. Kellogg's Rice Krispies is one such case that provides us with preliminary evidence that such brands are penalized less by consumers. Table 11 shows the market-share regressions using both the entire panel and a short-term two-month window. We do not find evidence that the brand was negatively impacted by the FTC order in the long run. This could be because the proactive withdrawal led to less penalty in terms of consumer response or because the product had the backing of a big parent brand, making it easy to rebound in the long run.

Strategic responses. If a firm responded strategically to an FTC order, for example, by advertising more or reducing prices, then in the absence of these strategic actions, the market-share drop would be higher. Thus, our estimate is a conservative one. We explore possible strategic actions further in the next section. In contrast, if the ads aired postorder were less effective, our estimate would overstate the effect. However, this scenario is less likely. As we show in the "Advertisements and False Claim Termination" section, some of the ad copies after the consent order were mostly identical to the ad copies before, with the only change that the voiceover made weaker claims. Moreover, our approach measures not only the effect of the aired ads but also the

Figure 12

AVERAGE PRICES (PER LB) OF THE IMPACTED BRANDS

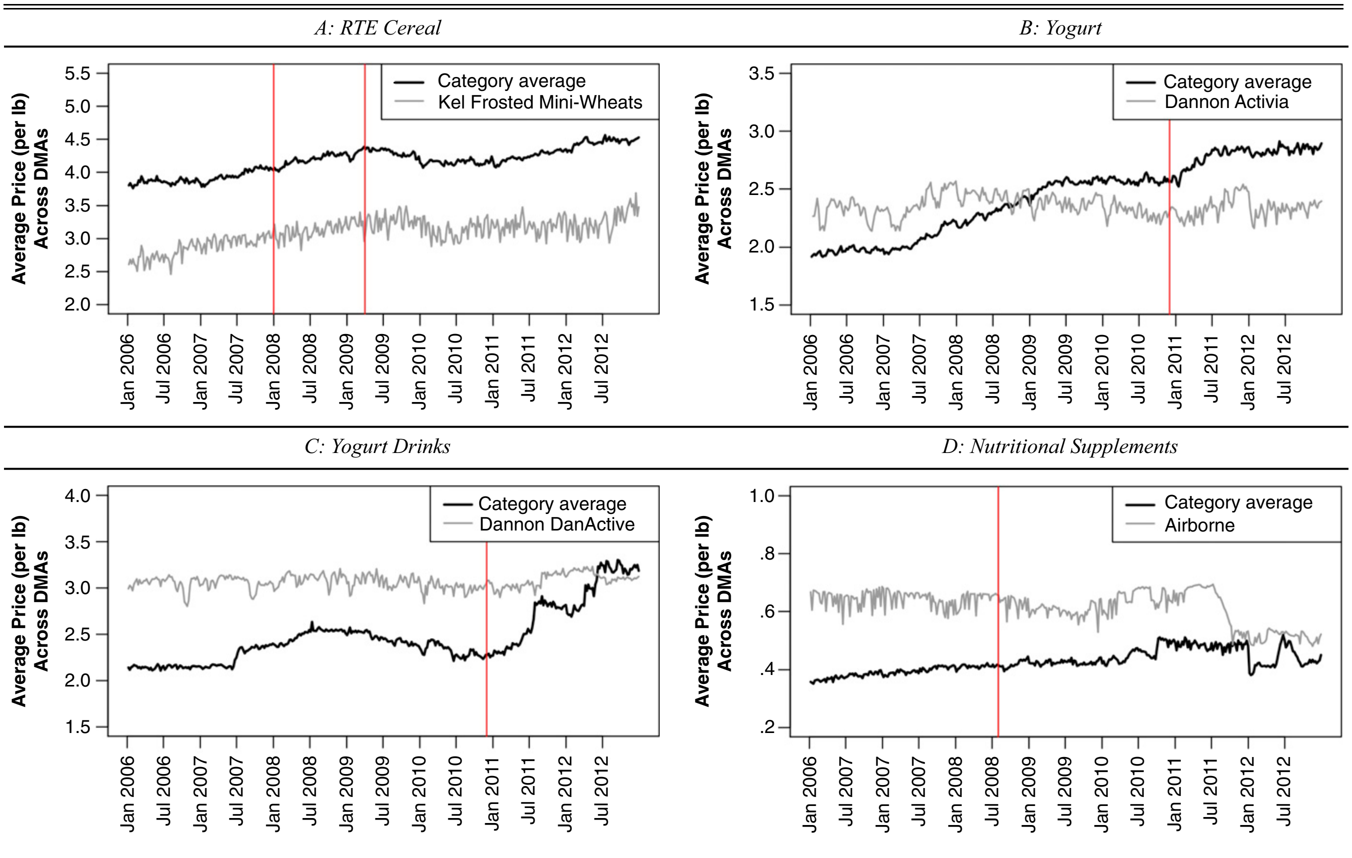


Figure 13

CREATIVES CONTAINING THE FALSE CLAIMS

A: Mini-Wheats on Backpacks and First Day of School[a]

Kellogg's Frosted Mini-Wheats - Back to School (2008) :30 (USA)

B: Teacher Loses Place, Attentive Boy Reminds[b]

Noah Munck - Frosted Mini Wheats Commercial (2009)

[a]http://adland.tv/commercials/kelloggs-frosted-mini-wheats-back-school-2008-30-usa

[b]https://www.youtube.com/watch?v=uXQKM7gxxo8

effect of the messages on the front of the box, which, apart from the claim in question, were identical before and after the consent order. Similarly, if the firm chose to dedicate fewer resources to the impacted brand and instead focused on other brands, our estimate would overstate the effect. In our case, Airborne provides a counterexample to this scenario: being a single-product firm, Airborne does not have another product to focus on, making the estimate unlikely to be an overestimate.

We now turn to exploring possible firm-side changes in terms of price and advertisement responses.

FIRM RESPONSE

Prices

Figure 12 plots the price per pound of the impacted brands, averaged across DMAs. Across all brands, little perceptible change occurs in prices around the time of the consent order. To test for such a change, we regress the impacted brand's prices on the average category price and test whether the price coefficient postorder is different from the coefficient preorder. Specifically, we test whether $\theta_j^{pre} = \theta_j^{post}$ in the following equation:

$$(4)\qquad p_{jt} = \theta_j \bar{p}_t + \theta_j^{pre} I(\text{preFTC}) \times \bar{p}_t + \theta_j^{post} I(\text{FTC}) \times \bar{p}_{st} + \varepsilon_{jt},$$

where j is the brand and t is the unit of time (week, month, or quarter)[15]; I(FTC) is 1 if t is after the FTC press release, and I(preFTC) is 1 if t is before the FTC press release; and $\theta = [\alpha, \beta]$ captures the level (intercept) and trend (slope) during the relevant period of interest. For nutritional supplements, we also test for level differences in the flu season versus the rest of the year, consistent with the demand analysis. We define the average category price, $\bar{p}_t$, as the weekly sales-volume-weighted average across all brands; that is,

$$\bar{p}_t = \sum_{j \in J_t} \frac{\upsilon_{jt}}{\sum_k \upsilon_{kt}} p_{jt},$$

where υ_{jt} is the sales volume of brand j sold in week t and J_t is the set of all brands sold that week.

We further restrict our attention to shorter 6- and 12-month windows and perform a placebo test to test whether the difference

$$\left(\theta_j^{post} - \theta_j^{pre}\right)\Big|_{\text{Event=FTC}} - \left(\theta_j^{post} - \theta_j^{pre}\right)\Big|_{\text{Event=Placebo}}$$

is statistically significant, where Event = Placebo is defined as one year before the FTC consent order.

Overall, we find no systematic increase/decrease in prices around the timing of the FTC consent order for the impacted brands. The regression estimates are provided in the Web Appendix. Testing for differences across competitors, we find evidence of competitor response in the RTE cereal category: General Mills (GM) Honey Nut Cheerios and Cinnamon Toast Crunch exhibit increases in price. Although not conclusive or causal, this finding further highlights the importance of controlling for prices in the demand analysis.

Advertisements

In this section, we first explore when the focal firm might have changed its advertising. We use the Nielsen Media data and the Wayback Machine (https://archive.org/web/) to infer when the company might have taken action. In the "Total Ads" section, we quantify total ad quantity changes to measure changes after the FTC order.

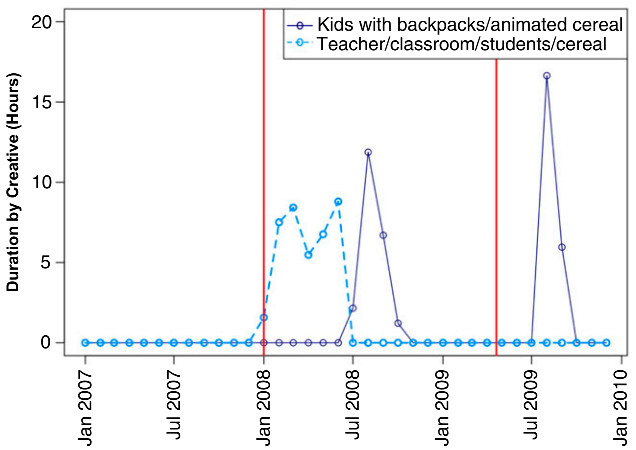
Advertisements and false-claim termination. The FTC complaint for Frosted Mini-Wheats highlights two specific TV ads, shown in Figure 13. In the Nielsen Media data, we can identify these ads based on the creative title, air date, and corresponding video files available on YouTube and Adland. Figure 14 plots the total airtime duration of these creatives at the national level for Kellogg's Frosted Mini-Wheats. Although the two creatives, which started in 2008, had already stopped at the time of the FTC order, existing packages containing the misleading claims were likely not replaced until the date of the order; a Flickr photo of a cereal aisle taken on January 27, 2009, confirms this.[16] The FTC order requires these claims be absent only after the order. Moreover, the ad that reappeared in

[15]We aggregate the weekly price data to the quarterly level to avoid inconsistent standard errors that can result from the presence of serially correlated observations (Bertrand et al. 2004).

[16]See https://www.flickr.com/photos/rexroof/3243790269.

Figure 14

DURATION OF KELLOGG'S ADS BEFORE AND AFTER FTC ORDER



Notes: The Kellogg's creatives that contained false claims ended prior to the consent order. The ad that reappears in 2009 has the same creative but makes weaker claims. Right reference line indicates date of FTC order. Left line indicates start of false claims.

2009 had the same creative but a different voiceover that made weaker claims. The 2008 script states, "A clinical study showed kids who had a filling breakfast of Frosted Mini-Wheats cereal improved their attentiveness by nearly 20% when compared to kids who missed out on breakfast. Now available in blueberry muffin. Keeps them full, keeps them focused." The 2009 script no longer makes the false claim, stating, "Packed with fiber and nearly a day's worth of whole grains, Kellogg's Frosted Mini-Wheats helps keep your kid full and focused all morning long. Now available in fruit raspberry. Keeps them full, keeps them focused." Therefore, apart from the claim in question, the ad copies did not change drastically, making the pre- and postorder periods more comparable.

For the remaining brands, all ad campaigns have the false claims. To identify when the brands might have responded to the order, we use the Wayback Machine. For Dannon, a perceptible change occurs in the website text between December 2010 and January 2011 (before and after the consent order). In particular, we see that the words "shown in several clinical studies," which were the subject of the FTC complaint, had been removed. The positioning of Activia largely remained the same.

For DanActive, national TV ads were discontinued in early 2009. Airborne seemed to have responded around the date of the FTC consent order, as evidenced in Figure 15 as well as in Airborne's website text. In April 2008, the text contained many details on how Airborne "combats the airborne germs and viruses that are all around in places like classrooms, offices and airplanes." In May 2008, the text no longer contained these claims.

This discussion indicates brands largely responded around the date of the consent order. More importantly, the informational impact to consumers occurred at the date of the consent order. We now examine whether these terminations led to overall changes in ad levels.

Total ads. Figure 15 plots the impacted brands' advertising duration in hours. To quantify possible changes in advertisements, we estimate the following regression equations for ad spend, duration, and frequency of advertisements at the brand-week level:

$$\text{(5)} \qquad \text{Ad}_{jt} = \gamma_j + \gamma_j^{\text{pre}} I(\text{preFTC}) + \gamma_j^{\text{post}} I(\text{FTC}) + \varepsilon_{jt},$$

where Ad_{jt} is the vector of ad-related variables $\{\text{AdSpend}, \text{Duration}, \text{Frequency}\}$; I(FTC) is 1 if t is after the FTC press release, and I(preFTC) is 1 if t is before the FTC press release; and $\gamma = [\alpha, \beta]$ captures the level (intercept) and trend (slope) during the relevant period of interest. For nutritional supplements, we test only for level differences in the flu season versus the rest of the year, consistent with the demand analysis.

As with prices, we test whether $\gamma_j^{\text{pre}} = \gamma_j^{\text{post}}$, using all data, and whether

$$\left(\theta_j^{\text{post}} - \theta_j^{\text{pre}}\right)\Big|_{\text{Event=FTC}} - \left(\theta_j^{\text{post}} - \theta_j^{\text{pre}}\right)\Big|_{\text{Event=Placebo}},$$

using 6- and 12-month windows. We find that although Mini-Wheats faces a decline in ads following the order, the decline is not statistically significant in comparison with the drop in a placebo period. Activia, on the other hand, seems to face a decline when we use all data as well as in the placebo test using a 12-month window. There is no statistically significant change in ads in the yogurt drink and nutritional supplement categories. Note that the regression equations overstate significance because each observation is a week, resulting in highly correlated observations. Therefore, the significance of the decline we measure is an overestimate. The regression estimates are provided in the Web Appendix.

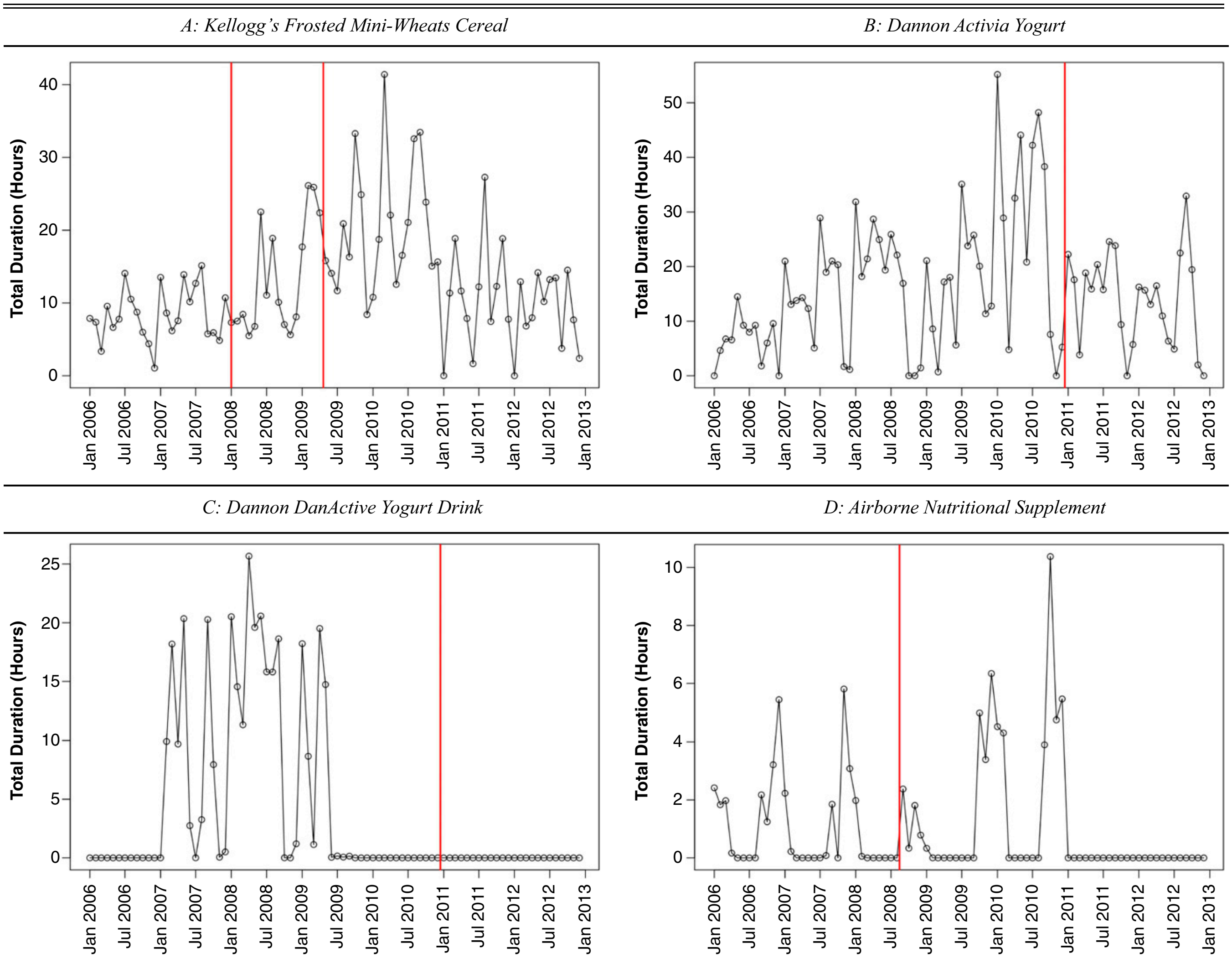
In the cereal category, only Kellogg's Frosted Flakes exhibits a significant increase in advertisements in this period. In the yogurt category, Stonyfield appears to have increased ad levels after the FTC order. These findings provide some evidence of possible strategic competitor response and highlight the importance of controlling for ads in the demand analysis.

Availability

After the FTC consent order, the impacted products might be unavailable in stores (e.g., manufacturers might need a few weeks to replace the packaging of their existing products). Lack of availability of the impacted products could explain the patterns observed in the data. However, a withdrawal of products from the shelves would imply an immediate sharp drop and a subsequent increase in market share. Because we do not observe this pattern in any of the products, our findings are likely not associated with lack of availability. However, the brand might be unavailable only in some stores, which could cause this gradual decline. To ensure that demand-side factors, rather than product unavailability, drive the decline in market shares following the FTC consent order, we check for discontinuity patterns in store availability.

Although the RMS data do not contain measures of availability, we infer store availability by exploiting the nature of the missing data. An observation in the RMS data can be missing if (1) the store did not report sales of the UPC that week (if this occurs, it should occur randomly and not systematically after the FTC consent order), (2) the UPC had no sales in that store-week, or (3) the product was not available. Although separating out the second and third possibilities is hard given the data, we take advantage of the fact that brand-level unavailability, if it occurs, should affect the entire brand and not just a single UPC. This is

Figure 15
ADVERTISING DURATION FOR FOCAL BRANDS BEFORE AND AFTER FTC ORDERS



because per the FTC order, a firm must remove all units associated with the impacted brand, not just a single UPC. A brand typically has 20–70 UPCs associated with it. Aggregating UPCs to the brand level, Figure 16 plots the number of stores that sold at least one unit of the brand during the sample period.

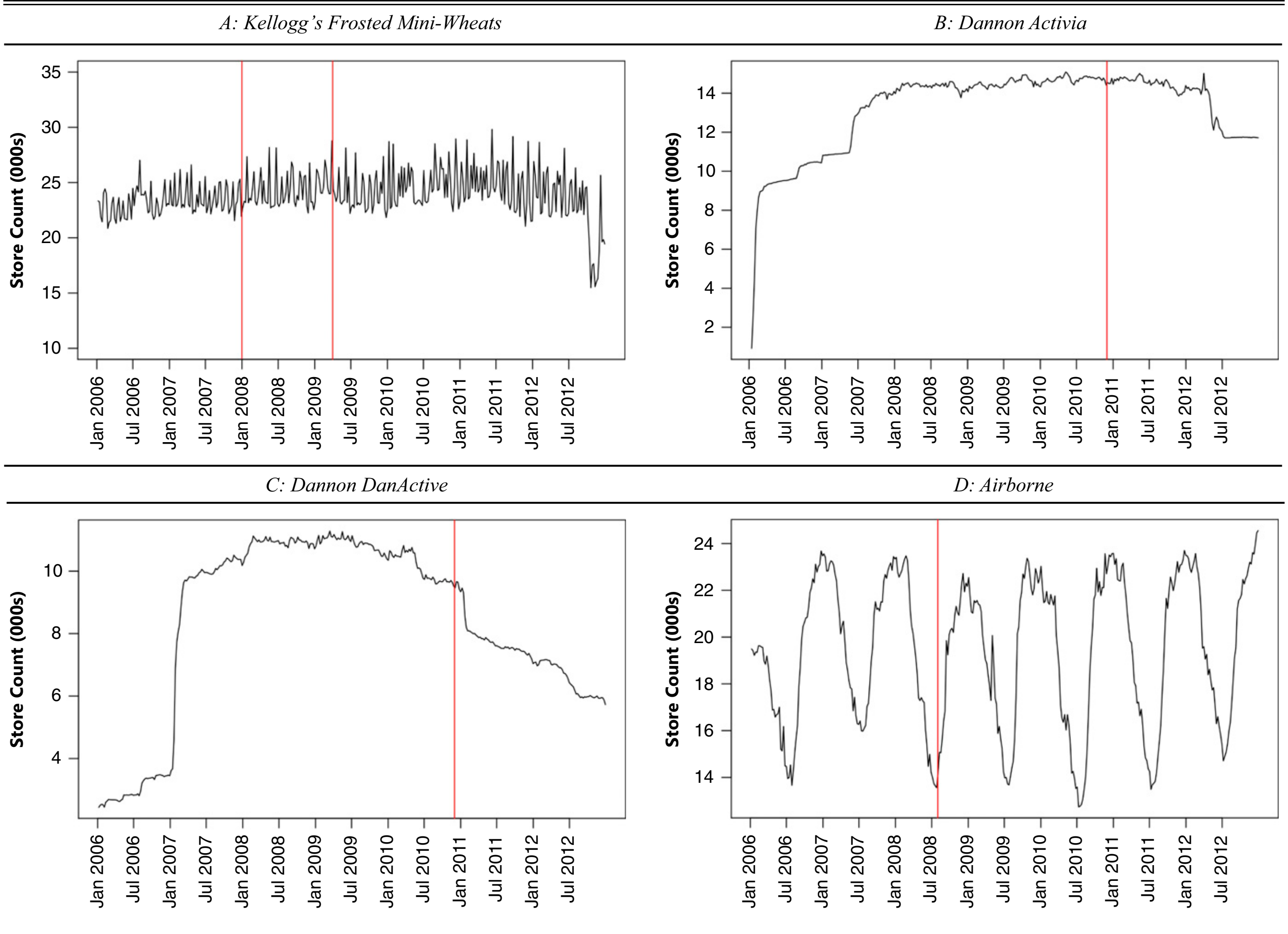
Across all brands, only Dannon's DanActive exhibits a sharp drop in the count of stores that sold at least one unit of the product. We next verify whether the demand patterns documented thus far hold for DanActive, controlling for availability. To do so, we exclude stores where a product was available in early 2010 but unavailable late 2010 and early 2011. We restrict attention to only those stores present in the RMS data. The resulting market shares shown in Figure 17 are remarkably close to the ones plotted in Figure 3, indicating the decline in market share is likely to be due to demand-side factors. We further verify for this subset whether reduction in retail support could explain the decline in demand. Controlling for feature and display, which are available for some stores in the RMS data, we find that our conclusions still hold.

Overall, we do not find a systematic change in policy. Our supply-side analysis shows a decline in ads for one of the brands (Activia), a reduction in number of stores where the product is available for another (DanActive), and a possible competitor price response in the cereal category.

CONCLUSION

This study finds that revelation of firms' deceptive practices can have a significant impact on consumer demand. The findings have implications for consumers, firms, and regulatory authorities. From a firm's perspective, making false claims appears lucrative, especially in the short run. A back-of-the-envelope calculation shows Kellogg's Frosted Mini-Wheats gained between \$59 million and \$144 million in revenue in the 12- to 32-month window surrounding the FTC order. Similarly, Airborne, whose main differentiator was its false-credence attribute, experienced fairly high market share prior to the FTC order. However, as more firms get caught making false statements and as class-action lawyers sue for even larger compensations, whether this revenue gain will hold for future brands is unclear.

Figure 16
NUMBER OF STORES THAT SOLD AT LEAST ONE UNIT OF FOCAL BRANDS BEFORE AND AFTER FTC ORDERS

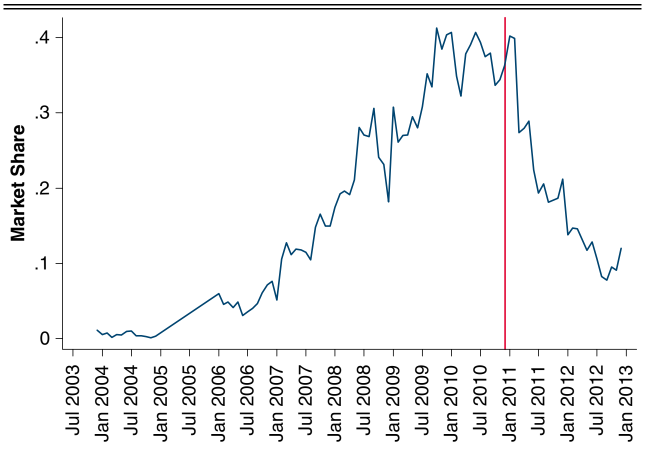


Furthermore, multiple violations by a single brand can cause consumers to lose trust in the brand. Measuring the effects of repeatedly misleading consumers will add to the literature on the long-run effects of advertising and is a suggestion for future work.

Regulatory bodies clearly play a big role, especially in the case of claims a consumer cannot reasonably verify. The role of the authority lies in ensuring the false claims are terminated, as well as ensuring consumers are made aware that a deception has occurred. Our work does not emphasize the mechanism by which consumers internalize the claims, namely, whether it is a response to the claims no longer being present or a response to information (via national press coverage) that the claims are false. The effect we measure is an aggregate of consumer responses to the termination of the claims, as well as to possible strategic firm-side responses in terms of price and advertisement changes. The firm-side data provide some evidence that competitors, especially in the cereal and yogurt categories, respond with price and/or ad changes.

Finally, because randomizing the presence of a false claim is nearly impossible in practice, our work provides an identification strategy that can be used in other contexts. In-lab studies are limited to hypothetical brands: one cannot credibly vary the presence of a false claim in a real brand because respondents can easily verify whether the brand makes that claim. For example, presenting respondents with a real brand but a hypothetical false claim can be easily checked in store or online, which could lead subjects to guess the experimental

Figure 17
DANNON DANACTIVE MARKET SHARES, EXCLUDING STORES WHERE IT WAS LIKELY NOT AVAILABLE



manipulation created by the researchers, invalidating the study. We exploit the timing of the FTC consent orders and measure aggregate market-share responses and individual-level purchase behaviors before and after this event, controlling for prices, advertising, and the competitive environment. We find evidence suggesting response to the termination of the false claims is heterogeneous: newcomers are most impacted by these false claims, whereas longtime users persist in their purchases even after the false claims have been identified and removed. Moreover, markets that saw more ads respond more strongly once they know the claims are misleading.

REFERENCES

Bertrand, Marianne, Esther Duflo, and Sendhil Mullainathan (2004), "How Much Should We Trust Differences-in-Differences Estimates?" *Quarterly Journal of Economics*, 119 (1), 249–75.

Bronnenberg, Bart J., Jean-Pierre Dubé, Matthew Gentzkow, and Jesse M. Shapiro (2015), "Do Pharmacists Buy Bayer? Sophisticated Shoppers and the Brand Premium," *Quarterly Journal of Economics*, 130 (4), 1689–726.

Bucklin, Randolph E., and Sachin Gupta (1992), "Brand Choice, Purchase Incidence, and Segmentation: An Integrated Approach," *Journal of Marketing Research*, 29 (2), 201–15.

Burke, Raymond R., Wayne S. DeSarbo, Richard L. Oliver, and Thomas S. Robertson (1988), "Deception by Implication: An Experimental Investigation," *Journal of Consumer Research*, 14 (4), 483–94.

Cawley, John, Rosemary Avery, and Matthew Eisenberg (2013), "The Effect of Deceptive Advertising on Consumption of the Advertised Good and Its Substitutes: The Case of Over-the-Counter Weight Loss Products," National Bureau of Economics Research Working Paper No. 18863, http://www.nber.org/papers/w18863.

Darke, Peter R., and Robin J.B. Ritchie (2007), "The Defensive Consumer: Advertising Deception, Defensive Processing, and Distrust," *Journal of Marketing Research*, 44 (1), 114–27.

Dhar, Titha, and Kathy Baylis (2011), "Fast-Food Consumption and the Ban on Advertising Targeting Children: The Quebec Experience," *Journal of Marketing Research*, 48 (5), 799–813.

Dyer, Robert F., and Philip G. Kuehl (1978), "A Longitudinal Study of Corrective Advertising," *Journal of Marketing Research*, 15 (1), 39–48.

Givon, Moshe, and Dan Horsky (1985), "Homogeneity and Heterogeneity in Stochastic Models of Brand Choice Behavior," *International Journal of Research in Marketing*, 2 (4), 263–72.

Gupta, Sachin, and Pradeep Chintagunta (1994), "On Using Demographic Variables to Determine Segment Membership in Logit Mixture Models," *Journal of Marketing Research*, 31 (1), 128–36.

Ippolito, Pauline M., and Alan D. Mathios (1990), "Information, Advertising, and Health Choices: A Study of the Cereal Market," *RAND Journal of Economics*, 21 (3), 459–80.

Ippolito, Pauline M., and Alan D. Mathios (1995), "Information and Advertising: The Case of Fat Consumption in the United States," *American Economic Review*, 85 (2), 91–95.

Jin, Ginger Zhe, and Andrew Kato (2006), "Price, Quality, and Reputation: Evidence from an Online Field Experiment," *RAND Journal of Economics*, 37 (4), 983–1005.

Jin, Ginger Zhe, and Philip Leslie (2003), "The Effects of Information on Product Quality: Evidence from Restaurant Hygiene Grade Cards," *Quarterly Journal of Economics*, 118 (2), 409–51.

Johar, Gita Venkataramani (1995), "Consumer Involvement and Deception from Implied Advertising Claims," *Journal of Marketing Research*, 32 (3), 267–79.

LaTour, Michael S., and Kathryn A. LaTour (2009), "Positive Mood and Susceptibility to False Advertising," *Journal of Advertising*, 38 (3), 127–42.

Liu, Yan, and Venkatesh Shankar (2015), "The Dynamic Impact of Product-Harm Crises on Brand Preference and Advertising Effectiveness: An Empirical Analysis of the Automobile Industry," *Management Science*, 61 (10), 2514–35.

Luca, Michael, and Georgiao Zervas (2016), "Fake It Till You Make It: Reputation, Competition, and Yelp Review Fraud," *Management Science*, 62 (12), 3412–27.

Moorman, Christine (1996), "A Quasi Experiment to Assess the Consumer and Informational Determinants of Nutrition Information Processing Activities: The Case of the Nutrition Labeling and Education Act," *Journal of Public Policy & Management*, 15 (1), 28–44.

Nielsen (2012), "National Agency Report: Ad*Views," http://en-us.nielsen.com/sitelets/cls/documents/adviews/AdViews-NationalAgency-ReportGuide-Dec2012.pdf.

Olson, Jerry C., and Philip A. Dover (1978), "Cognitive Effects of Deceptive Advertising," *Journal of Marketing Research*, 15 (1), 29–38.

Peltzman, Sam (1981), "The Effects of FTC Advertising Regulation," *Journal of Law & Economics*, 24 (3), 403–48.

Rossi, Peter E., Robert E. McCulloch, and Greg M. Allenby (1996), "The Value of Purchase History Data in Target Marketing," *Marketing Science*, 15 (4), 321–40.

Shum, Matthew (2004), "Does Advertising Overcome Brand Loyalty? Evidence from the Breakfast-Cereals Market," *Journal of Economics & Management Strategy*, 13 (2), 241–72.

Skurnik, Ian, Carolyn Yoon, Denise C. Park, and Norbert Schwarz (2005), "How Warnings About False Claims Become Recommendations," *Journal of Consumer Research*, 31 (4), 713–24.

Snyder, Rita (1989), "Misleading Characteristics of Implied-Superiority Claims," *Journal of Advertising*, 18 (4), 54–61.

Zinman, Jonathan, and Eric Zitzewitz (2016), "Wintertime for Deceptive Advertising?" *American Economic Journal: Applied Economics*, 8 (1), 177–92.