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The Short- and Long-term Impact of an Assortment Reduction on Category Sales

Laurens M. Sloot, Dennis Fok, and Peter C. Verhoef

To understand the sales effects of assortment reduction, retailers should take a long view. Over time, the negative sales effect of reducing variety decreases: Customers find it easier and quicker to find the product they want, and new buyers are attracted by the streamlined selection.

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Report Summary

Retailers usually assume that reducing the variety of goods they offer will have a negative effect on sales and cause customers to shop in another store. To address this assumption, authors Sloot, Fok, and Verhoef investigate three questions:

1. What are the short- and long-term effects of reducing a particular category of products on a store's shelves?
2. Do the effects on sales differ between former buyers and those who had not previously bought the item?
3. Does reducing assortment affect the percentage of sales to new buyers in that category?

This study, a collaborative effort with a Dutch retailer, shows that reducing the variety of an item may actually boost sales. In this case, the retailer offered fewer types of detergent items. While initially lowering sales for the short term, the effort did not lessen sales in the long run. The reduced selection may have also aided consumers; they found it easier and quicker to make a selection when the variety was reduced.

The reduced sales are caused initially by former buyers who purchase fewer items in the category. But sales losses are offset by new buyers attracted by the streamlined selection.

The authors also studied buyers' perceptions of the smaller assortment and the time they spent searching for an item. They found that customers felt they used less time searching; this was verified by an independent team that clocked individual customers' time in the detergents aisle of the supermarkets.

The authors' application of cubic splines methodology in a marketing context is one of the few times it has been used in marketing research. They offer it as a useful model when, for example, studying the effect of a single event, such as assortment reduction, on sales over time. ■

Introduction

Since the early 1990s, increased competition from "category killers" such as Wal-Mart and Aldi has forced traditional grocery retailers to implement cost-saving programs (Basuroy, Mantrala, and Walters 2001; Dhar, Hoch, and Kumar 2001). One common way to reduce costs has been to reduce the number of offered items. Some assortment-reduction projects use a "cutting the tail" strategy in which low-selling items in a category are eliminated (see, for example, Boatwright and Nunes 2001; Drèze, Hoch, and Purk 1994; Food Marketing Institute [hereafter, FMI] 1993). These assortment-reduction projects can be regarded as natural experiments to assess the impact of cuts. However, the results of these experiments are mixed.

In Table 1, we provide a schematic overview of prior literature on assortment reductions in which we classify existing studies according to two dimensions: (1) the type of experimental method (laboratory versus natural experiment), and (2) the dependent variables chosen (assortment perceptions versus purchase behavior or category sales). Laboratory experiments tend to focus on perceptions (e.g., Oppewal and Koelemeijer 2005), although some studies have considered both assortment perceptions and stated purchase behavior (e.g., Broniarczyk, Hoyer, and McAlister 1998). Perceptions that have been studied include perceived assortment variety and assortment satisfaction or evaluation. In contrast, natural experiments focus only on category or store sales.

Neither the current laboratory experiments nor the studies based on natural experiments distinguish between the short- and long-term effects of assortment reductions. However, this essential distinction can provide valuable insights into the effectiveness of marketing tactics (Dekimpe and Hanssens 1995; Dekimpe et al. 2005). Moreover, ignoring the short- and long-term effects of assortment reductions may lead to the wrong conclusions. For example, if the long-term effect on category sales is less than the short-term effect, the retailer may overstate the negative consequences of an assortment reduction, which would lead to an overly restrictive policy on assortment reductions and in turn to inefficient assortments. Therefore, it becomes essential to study both the short- and the long-term effects of assortment reductions.

In this working paper, we use a natural experiment to measure the effect of an assortment reduction in a single category (detergents) on that category's sales. We extend current assortment-reduction studies that employ assortment sales as the dependent variable by distinguishing between the short- and long-term effects of assortment reductions. Furthermore, we add to this literature stream by analyzing the entrance of new buyers. Our research questions in this collaborative study can be summarized as follows:

1. What are the short- and long-term effects of a major assortment reduction on total category sales?
2. Do the short- and long-term effects on category sales differ between former buyers and former nonbuyers of delisted items?
3. Does the assortment reduction affect the sales percentage accounted for by new category buyers?

Our study was conducted in close cooperation with a major Dutch retailer. We used customer loyalty-card data from more than 25,000 households in two test stores and two control stores to assess the short-term and long-term category sales effects of an assortment reduction, where the category sales are defined in monetary units. To provide insights into the explanations of the sales effects we found, we executed an additional study in which we investigated changes in assortment perceptions (i.e., assortment variety, search efficiency, and assortment satisfaction) and actual search time due to the assortment reduction.

The remainder of this paper is structured as follows: In the next section, we discuss in detail the collaborative research project that underlies this study. Subsequently, we discuss the theory,

Table 1
Overview of Prior Studies on Assortment Reductions

	Assortment Perceptions	Purchase Behavior/Category Sales
Laboratory experiment, hypothetical reductions	Broniarczyk, Hoyer, and McAlister (1998) Oppewal and Koelemeijer (2005) Van Herpen and Pieters (2002)	Broniarczyk, Hoyer, and McAlister (1998)
Natural experiment, real reductions	Broniarczyk, Hoyer and McAlister (1998)	Food Marketing Institute (1993) Drèze, Hoch, and Purk (1994) Boatwright and Nunes (2001) De Clerck et al. (2001) Borle et al. (2005) Zhang and Krishna (2005)
	This paper: Additional study, with inclusion of actual search time	This paper: Main study with addition of short- and long-term impact, and entrance of new buyers

methodology, and results. We then examine the results of our additional study, followed by a discussion of the results and managerial implications. We specifically focus on the implications for our collaborative research partner. We end with research limitations and resulting future research issues.

Collaborative Assortment-Reduction Project

A team consisting of a retailer, a brand manufacturer, and academics carried out this assortment-reduction project. The retailer aimed to reduce costs in the supply chain and reduce complexity by lowering the number of items in various categories, particularly those defined as "routine categories" (Dhar, Hoch, and Kumar 2001). The retailer's primary objective was to lower the total number of store items by approximately 1,500, which would enable it to close a warehouse. The associated cost savings were estimated to be approximately €4.5 million per year. However, the retailer feared that such an assortment reduction might affect its category sales. Therefore, we conducted a pilot project in one category to investigate the impact of a major assortment reduction.

In this paper, we focus on the results of the pilot project that considered an assortment reduction in detergents. The retailer previously offered 150 detergent items. Despite this large number, the category performed below its market share compared with a price-aggressive competitor that offered only 80 detergent items. Hence, the retailer decided to remove 37 of the total 150 items, which constituted 25% of the total number of detergents and 14% of the sales of detergents. Thus, in general, the low-selling items were removed. For each delisted item, the assortment manager verified that there was at least one reasonable alternative item within the remaining assortment. The 37 delisted items included brand delistings (i.e., all items of one brand were delisted) and item delistings (e.g., a package format or variety within a brand was delisted).[1] Overall, the assortment reduction resulted in the delisting of six complete brands, corresponding with 17 different items. All the delisted brands can be considered as low-equity brands. Of the other 20 delisted items, consumers could still switch within the brand. The selection of items that were delisted in the test stores was based on item turnover statistics and a consumer decision-tree analysis provided by the manufacturer. First, items that did not meet regular turnover demands were selected for reduction. Second, the consumer decision-tree analysis showed in which detergent segments there was potential overlap between items. The retailer's category manager made the final decision on which items should be delisted. In the case of clearly duplicated items, the category manager preferred to delist the item with the lowest gross margin.

Category space was held constant by giving the remaining items more shelf space and keeping the overall structure (e.g., location of items on the shelf) of the presented assortment constant. Furthermore, no new items were introduced during the test. Sales data before and after the assortment reduction were collected from two test stores and two control stores. The perception data were collected in the two test stores before and after the assortment reduction occurred. On the basis of the outcomes of this project, the retailer decided whether the assortment reduction would be rolled out nationwide and, if necessary, which adaptations it needed to make.

Theoretical Background

Sales effects of assortment reductions

Several studies have considered the category sales effects of assortment reductions. Drèze, Hoch, and Purk (1994) report positive sales effects. These positive effects may, however, be due to other changes to the assortment presentation in their study. Using six categories, the Food Marketing Institute (1993) reports both negative and positive sales effects of assortment reductions, though the negative effects mainly occurred in categories with deep cuts. Boatwright and Nunes (2001, 2004) report, on average, a neutral sales effect of reductions for an online grocery store, though they also find negative sales effects in categories with very deep assortment cuts. Borle et al. (2005) use household panel data of an online grocer that executed a large-scale assortment reduction in most categories. They conclude that overall store sales are reduced and that less frequently purchased categories are more adversely affected by assortment reductions. Zhang and Krishna (2005) also report sales decreases of assortment reductions in three categories in an online-retail context. Moreover, practical experiences show the negative effects of assortment reductions. For example, in 2001, the leading Dutch grocery retailer Albert Heijn deleted almost 1,500 items across categories, which caused widespread consumer complaints (*Foodmagazine* 2002).

Negative sales effects may occur because, after the assortment reduction, a certain percentage of buyers will no longer be able to find their preferred item (Broniarczyk, Hoyer, and McAlister 1998). These buyers may initially postpone their purchase but eventually may decide to switch items or switch stores (Campo, Gijsbrechts, and Nisol 2000, 2004). If the customer switches to another item, no category sales losses will occur. However, when he or she decides to switch stores, category sales will decrease.

On the other hand, assortment reductions may have positive sales effects. Previously,the general belief was that a larger assortment is always better (Oppewal and Koelemeijer 2005). Recently, however, it has been claimed that the opposite may be true (Broniarczyk and Hoyer 2005). Several studies in consumer research and psychology have shown the negative effects of assortments that are too large and the positive effects of small assortments (e.g., Gourville and Soman 2005; Iyengar and Lepper 2000). The negative effects may occur because of the complexity in searching large assortments (Botti and Iyengar 2004), which may prevent retail customers from buying products, and they defer their purchase (Huffman and Kahn 1998). Reducing assortment size would decrease search complexity, which might induce nonbuyers in the category to start buying products.[2] As a result, positive sales effects might occur, which might explain why Drèze, Hoch, and Purk (1994) and Boatwright and Nunes (2001) find either positive sales effects or no sales effects. In the latter case, positive sales effects due to the entrance of new buyers might offset negative sales effects among former buyers.

In summary, there is ample empirical and theoretical evidence for the negative sales effect of an assortment reduction, especially among former buyers of delisted items. This negative sales effect might, however, be (partially) offset by the attraction of new buyers, which may compensate for the initial negative effect in the long run. The latter sales effect has not been empirically investigated.

Short- versus long-term effects

Numerous studies in marketing science have considered the short- and long-term sales effects of marketing-mix variables, such as advertising, promotions, pricing, and new product introductions (Bijmolt, van Heerde, and Pieters 2005; Dekimpe and Hanssens 1995; Nijs et al. 2001; Pauwels, Hanssens, and Siddharth 2002; Pauwels et al. 2004). However, the literature on assortment reductions contains no studies that distinguish between short- and long-term effects.

The available evidence clearly indicates that the short- and long-term effects of marketing-mix instruments may differ. For example, Nijs et al. (2001) demonstrate a short-term effect of price promotion that dissipates in the long run. According to Hanssens, Parsons, and Schultz (2000), most effects of marketing actions dissipate over time. The question is whether these findings hold for assortment reductions as well. An assortment reduction, fundamentally different from the earlier studied promotions, is a one-time permanent change, whereas promotions occur regularly and are temporary. Long-run effects of an assortment reduction are therefore more likely to be present.

To understand the short- and long-term effects of assortment reductions further, we first focus on the reactions of former buyers of delisted items because we expect that negative sales effects will occur mainly for them (Broniarczyk, Hoyer, and McAlister 1998). As we noted previously, negative sales effects are manifested when these former buyers switch to another store to buy the preferred item or brand or postpone their purchase. Because most consumers visit several stores to buy their grocery products, switching stores for detergent purchases may lead to permanent sales effects. Postponement generally results in a short-term effect; following the terminology of Van Heerde, Leeflang, and Wittink (2000). This effect may be labeled the post–assortment reduction dip. After a certain amount of time, the customer must buy the product because the stock at home has been depleted. At that time, the customer, who initially postponed purchasing the product, has to decide whether to switch stores or switch to another item. As a consequence, the downward peak in sales early on might be followed by an upward peak some weeks after the assortment reduction, due to item switching. To investigate such a pattern, we must study not only the direct and the long-run effect, but also the effect of the reduction during the period in between. Thus, in principle, we expect a relatively large negative sales effect in the short run due to postponement and store switching and a smaller sales effect in the long run because part of the group that initially postponed will switch to another item in the store. Overall, the total resulting sales effect among former buyers might remain negative and significant in the long run.

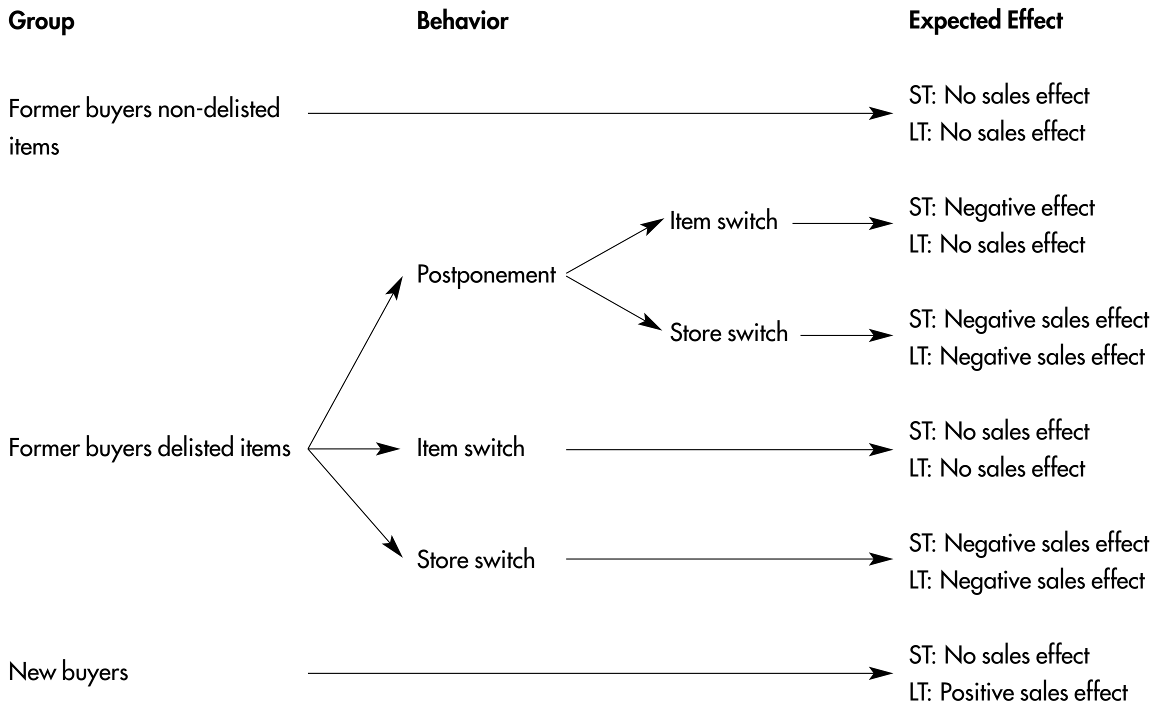
A complicating factor is the entrance of new buyers due to the assortment reduction. One might expect this will occur gradually over time. Hence, the entrance of new buyers will not compensate for negative sales effects directly after the reduction. The possible negative sales effects in the long run among former buyers might, however, be compensated for. Hence, on a total category sales level, one might question the existence of a long-run negative sales effect of an assortment reduction. Figure 1 shows the expected sales effect among different groups of category buyers.

Research Methodology

Data

We analyzed customer loyalty-card data to measure the effect of the assortment reduction on category sales. Data on household purchases are available from two stores in which the assort-

Figure 1
Expected Sales Effects of an Assortment Reduction among Different Groups of Buyers



ment reduction took place and two control stores in which the assortment remained unchanged. These control stores are essential for distinguishing between the effects of the delisting and other exogenous changes in sales. The selected stores are geographically quite far apart. It is therefore unlikely that people would visit more than one store in our sample.

The retailer supplied a database detailing the detergent purchases of 26,941 households in the four stores. The data are based on purchases by individual households participating in the retailer's customer loyalty-card program that account for more than 80% of total store sales. The data cover 52 weeks, 26 weeks before and 26 weeks after the assortment was reduced.

Decomposition of sales effects

The theoretical discussion outlines how sales effects might differ across groups of retail customers. To formally investigate this, we decompose the sales effects for these different groups of buyers. Previous studies decomposed sales effects of marketing actions, such as promotions, based on the behavioral source of this effect (i.e., category expansion, brand switching, stockpiling, etc.) (see Van Heerde, Gupta, and Wittink 2003; Van Heerde, Leeflang, and Wittink 2004). In this study, we decompose sales effects based on the type of customer, where type is determined based on the customers' behavior before the delisting. That is, we consider three customer groups in our database: (1) former category buyers of delisted items before the assortment reduction, (2) former category buyers of non-delisted items before the assortment reduction, and (3) new category buyers after the assortment reduction (noncategory buyers in the 26 weeks before the assortment reduction). The category sales after the assortment reduction at time T_1 can then be formally decomposed as:

$$\text{Sales}_t = \text{Sales}_{t,1} + \text{Sales}_{t,2} + \text{Sales}_{t,3} \qquad t \geq T_1, \quad (1)$$

where the subscripts refer to the above defined customer groups.

In our analysis, we first consider the total sales effects. Subsequently, we focus on former category buyers only, thereby distinguishing between buyers of delisted items and nonbuyers of delisted items. Households that have not bought detergents before the delisting cannot be assigned to either subpopulation. Our final analysis focuses on the entrance of these new buyers.

It is expected that the percentage of preferred items that is delisted will also affect the impact of the assortment reduction on household behavior. Although an interesting hypothesis to investigate, we lack sufficient data to test it. We therefore focus on the binary distinction outlined above; that is, we consider households that have bought a delisted item and households that have not.

Econometric modeling

To estimate the effect of the assortment reduction on the category sales in the test stores, we specify an econometric model in terms of log category sales for specific sets of households. Thus, the parameters should be interpreted as relative effects; that is, they represent percentage changes. An advantage of such a specification is that sales of populations of different size can be compared easily. For ease of exposition, we start by specifying the model to compare the total category sales across the four stores. For this case, the model can be presented compactly as follows:

$$\log S_{it} = \alpha_i + \beta' X_{it} + I[t \geq T_1]\,\{f(t\,|\gamma) + g(t\,|\theta)\} + \varepsilon_{it} \quad i = 1,3 \text{ (test stores)} \tag{2a}$$

$$\log S_{it} = \alpha_i + \beta' X_{it} + I[t \geq T_1]\,\{f(t\,|\gamma)\} + \varepsilon_{it} \quad i = 2,4 \text{ (control stores)}, \tag{2b}$$

where S_{it} denotes the sales for store $i = 1,2,3,4$ at time $t = 1, \ldots, T$, X_{it} denotes a vector of explanatory variables, such as promotion dummies or dummies for aberrant observations; $I[t \geq T_1]$ denotes an indicator function that equals 0 before the time of delisting (T_1) and 1 after the delisting; and $f(t|\gamma)$ and $g(t|\theta)$ denote flexible functions of the time index that measure the change in category sales in the period after the delisting. These functions depend on unknown parameters γ and θ. In this specification, we explicitly use the control stores to identify the effect of the delisting. The function $f(t|\gamma)$ gives the baseline changes in category sales in all stores, irrespective of the delisting, whereas $g(t|\theta)$ gives the (additional) change in the test stores due to the assortment reduction. These functions capture everything that is different after the delisting versus before the delisting. They are therefore not specified for $t < T_1$. We estimate the model based on the entire sample ($t = 1,..,T$), the *estimates* of $f()$ and $g()$ do therefore depend on the observed sales prior to the delisting. This final function is the key point of our analysis because it indicates the change in sales unique to the test stores. This model contains four equations, one for each store. For the error terms, we assume a joint normal distribution with an unrestricted covariance matrix, namely,

$$(\varepsilon_{1t}, \varepsilon_{2t}, \varepsilon_{3t}, \varepsilon_{4t}) \sim N(0,\Sigma).$$

To complete the model specification, we specify $f(t|\gamma)$ and $g(t\,|\theta)$. There are several possibilities with varying degrees of flexibility and sophistication. The most straightforward specification assumes a constant effect, that is,

$$f(t|\gamma) = \gamma \text{ and } g(t\,|\theta) = \theta \text{ for all } t.$$

However, the change in category sales after the delisting may not be the same for all time periods. Earlier, we highlighted the need to study the intermediate points between the short-run and the long-run effects. One may also opt to include time dummies; however, as we consider category sales on a weekly basis, this will yield too many parameters. Instead we choose a specification somewhere in between assuming a constant effect and using a time dummy for each period.

In our model, we opt for a cubic spline approach. The resulting function is a smooth, piecewise, cubic function. To illustrate this technique, we first consider the simplest form of the cubic spline. We introduce two parameters representing the function value at T_1 and T (referred to as the knots); the function value for $T_1 < t < T$ is obtained by simple linear interpolation. In this case, the cubic spline reduces to a linear trend, that is,

$$g(t|\theta) = \theta_1 + (\theta_2 - \theta_1)(t - T_1)/(T - T_1);$$

that is, we estimate the instantaneous (short-term) effect at the time of the delisting ($t = T_1$) by θ_1 and estimate the effect at the end of the sample ($t = T$) by θ_2. Between these two extremes, we interpolate the effect using a straight line; for example, halfway between T_1 and T, the function value equals $.5(\theta_1 + \theta_2)$. In a regression context, it is easy to estimate θ_1 and θ_2 because they appear linearly in the function specification. In many cases, the assumption of linearity may be too restrictive.

The above idea can be extended by adding more parameters and increasing the flexibility of the function, that is, we introduce more knot points. Furthermore, instead of linear interpolation, we use a smooth, piecewise, cubic function (for a general discussion of cubic splines, see Monahan [2001] Poirier [1976]; for an application, see Koopman and Ooms [2003]; for an application of linear splines in marketing, see Wedel and Leeflang [1998]). For this technique, we must select additional knot points, next to the points T_1 and T used in the linear case. A model specification with time-dummies is obtained if we place a knot at every time period. Unfortunately, there is no guideline to select the number and the placement of knots in a practical situation. The optimal placement of knots is at the (unknown) locations where one expects the derivative of the curve to change the most. The number of knots should be chosen relative to the number of available observations. Using few knots may result in too restrictive a model; using too many knots may result in estimation problems. In our application, we use five knot points distributed evenly over the period after the delisting. The first knot is located at the start of the delisting, and the final knot is the end of our observation sample. In this paper, we interpret the function value at the end of the sample as the long-term effect of the assortment reduction. This long-term effect depends, of course, on the sample. If one collected an even longer sample, one might find a different long-term effect. The function $f(t|\gamma)$ is specified analogously. The resulting complete model can be estimated using generalized least squares, as the cubic spline is linear in the parameters.

Decomposition Analysis. To study the category sales of subpopulations within a store, we can easily extend (2a,b) by including additional equations (one equation for every subgroup) and including more spline functions. Within the group of detergent buyers before T_1, we decompose the sales effects between former buyers and nonbuyers of delisted items as follows: On the basis of their observed purchase behavior before T_1, we assign each household to one of two groups, that is, (1) those that bought at least one item involved in the delisting or (2) the group of households that has not bought such an item. There is no reason to believe there will be differences in the composition of each group across stores, because the assignment is based on behavior before the assortment reduction became effective. We now have to consider only households that have bought at least one detergent item during the period prior to delisting. Every selected household makes at least one purchase before T_1, but we are not sure that they will also make a purchase afterward. Our selection therefore introduces a selection or survival bias in the data; that is, the sales generally will show a negative trend. However, this trend occurs for the test stores as well as the control stores and therefore does not interfere with the estimate of the effect of the delisting. This again illustrates the need for data on control stores along with data on the stores in which the assortment reduction occurred.
For each group, we calculate the total sales per week, denoted by S_{ijt}, where $j = 1$ corresponds to

Table 2
Change of Category Sales after the Delisting in Model (3)

	Former Nonbuyers of Delisted Items	Former Buyers of Delisted Items
Control stores	$f(t)$	$f(t)+h(t)$
Test stores	$f(t)+g(t)$	$f(t)+h(t)+g(t)+k(t)$

former buyers of detergent items that were not delisted and $j = 2$ corresponds to the former buyers of a delisted item. The model we use is a straightforward extension of (2a,b), in which we introduce an additional dummy variable for the former buyers and two additional spline functions, as follows:

Former non-buyers ($j = 1$)

$$\log S_{i1t} = \alpha_i + \beta' X_{i1t} + I[t \geq T_1]\{f(t|\gamma)\} + \varepsilon_{i1t} \quad i \in \text{Control} \tag{3a}$$

$$\log S_{i1t} = \alpha_i + \beta' X_{i1t} + I[t \geq T_1]\{f(t|\gamma) + g(t|\theta)\} + \varepsilon_{i1t} \quad i \in \text{Test} \tag{3b}$$

Former buyers ($j = 2$)

$$\log S_{i2t} = \alpha_i + \delta + \beta' X_{i2t} + I[t \geq T_1]\{f(t|\gamma) + h(t|\phi)\} + \varepsilon_{i2t} \quad i \in \text{Control} \tag{3c}$$

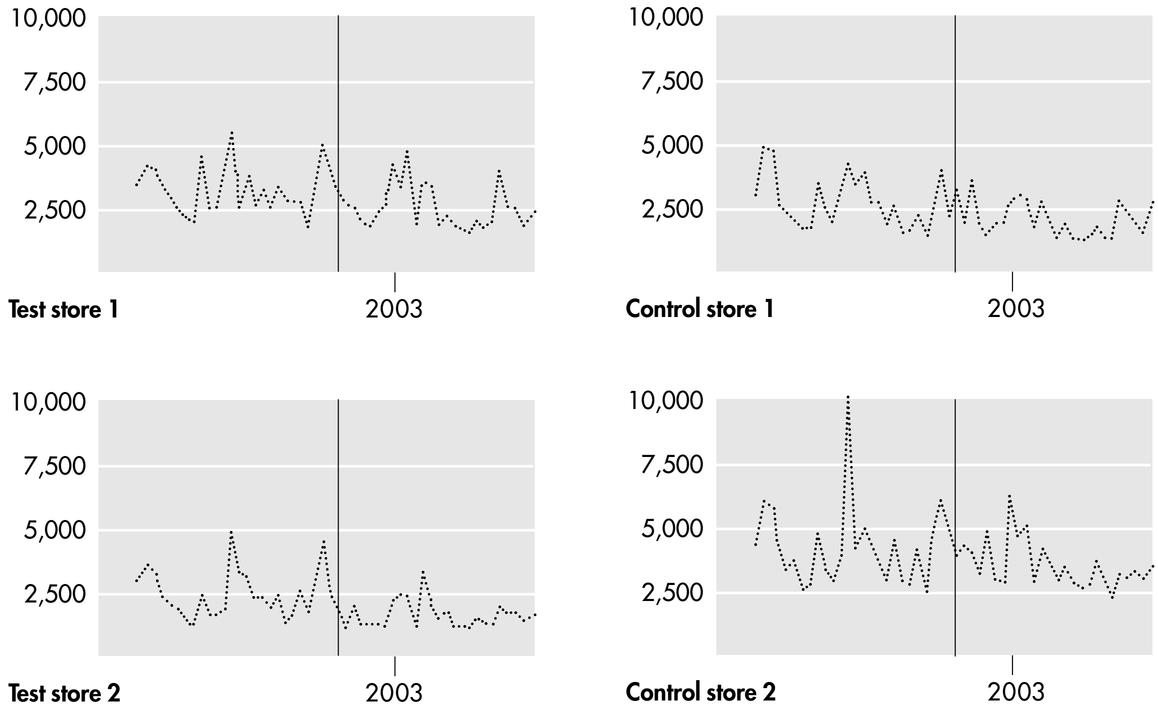
$$\log S_{i2t} = \alpha_i + \delta + \beta' X_{i2t} + I[t \geq T_1]\{f(t|\gamma) + h(t|\phi) + g(t|\theta) + k(t|\nu)\} + \varepsilon_{i2t} \quad i \in \text{Test} \tag{3d}$$

where i denotes the store ($i = 1,2,3,4$), δ denotes an additional intercept for the former buyers of delisted items, and Control and Test denote the sets of control and test stores, respectively (Test stores = $\{1,3\}$). We now have eight equations instead of four (2 groups times 4 stores). To economize on the number of parameters in the covariance matrix of the residuals, we now assume the disturbances to be independent across stores. We do allow for correlation within a store.

Table 2 shows an overview of the interpretation of the four spline function in Equation 3. The functions $f(t|\gamma)$ and $g(t|\theta)$ have the same interpretation as before; the first function captures the general pattern of detergent sales after the delisting, and the second function gives the sales development specific to the test stores. Former buyers of delisted items are expected to behave differently from former nonbuyers, irrespective of the assortment reduction. The simple reason is that they are selected to show specific behavior. The function $h(t|\phi)$ measures this difference in behavior; it gives the specific effect for former buyers of delisted items in general, that is, across test stores and control stores. The function $k(t|\nu)$ specifies in what way former buyers in the test stores are different from former buyers in general. We are most interested in the estimates for $k(t|\nu)$. Again we model the entire time path of the (possible) changes in category sales. We are interested not only in the size of the effect, but also in its timing and duration.[3]

Controlling for Other Marketing Interventions. Although no detailed price information is available in our database, this is not a serious problem. As we stated previously, the database pertains to purchases in four different stores. The delisting took place in two stores, whereas all detergent items remained on the shelf in the other two. The control stores were rather similar to the test stores in terms of assortment policy, square meters (large supermarkets), intensity of competition (five or more competitive supermarkets within a range of four kilometers), and urbanization (urban areas). Furthermore, basic marketing efforts were the same across these stores; that is, the same (price) promotions occurred in all stores at the same time. This implies that delisted items were not promoted in the control stores. Changes in the price level therefore did not influence a relative comparison across brands. Of course, estimates for the development of category sales in the control stores are affected by promotions. To correct for the presence of promotions, we construct a promotional indicator. We know that promotions occurred in all stores at the same time, so we base the promotional indicator

Figure 2
Total Weekly Category Sales per Store
(vertical line indicates start of the delisting in the test stores)



on the total sales across all stores. To identify the weeks in which a promotion (of some sort) took place, we estimate a model with a cubic spline function for the total sales across all stores. We assume that a promotion occurred for each observation with a large positive error. We then re-estimate the same model, now including the promotion indicator, to identify promotions that had a smaller impact. The definition of this promotional indicator is irrelevant for the effects of the delisting we estimate. Again this is due to the fact that promotions occurred in all stores and that we use control and test stores to identify the effect of the delisting.

Empirical Results

Analysis 1: Total category sales

We first focus on the weekly total category sales for each store, which can be directly obtained from the database by simple aggregation. In Figure 2, we show time series plots for the category sales in each store, which demonstrate a slight decrease in sales for all four stores. This overall decrease in detergent sales cannot be attributed to the delisting because, in the control stores, the number of available items remained constant. To assess the actual effect of the assortment reduction, we must compare the changes in the test stores to changes in the control stores.

In Table 3, we provide the parameter estimates for (2a,b), with which we model the total category sales per store. We include the earlier discussed promotional indicator as regressors to control for promotional effects. We also include a dummy variable to correct for an influential outlier. This outlier corresponds to a week of extremely low reported sales in one of the stores. The retailer informed us that this was due to an error in the data collection system; the actual sales were higher, but the exact figure was un-

Table 3

Estimated Parameters for Log Weekly Category Sales (Equations 2a,b)

	Estimate	Standard Error
Store Dummies and Regressors		
Test store 1	7.928	(.036)
Control store 1	7.640	(.042)
Test store 2	7.540	(.039)
Control store 2	8.184	(.039)
Promotion	.501	(.051)
Outlier dummy	–.404	(.155)
Baseline Sales Change $f(t\|\gamma)$		
2002:46	.241	(.082)
2002:52	–.069	(.081)
2003:06	–.164	(.081)
2003:12	–.302	(.079)
2003:19	–.028	(.119)
Additional Change in Test Stores $g(t\|\theta)$		
2002:46	–.243	(.055)
2002:52	–.194	(.055)
2003:06	.007	(.055)
2003:12	–.061	(.053)
2003:19	–.098	(.081)

known. Although the stores were selected in advance for similarities in detergent shelf metrics, the estimated store intercepts show some differences in baseline sales across the four stores, which may be explained by the unique characteristics and environment of each store.

The most interesting results appear in the final lines of Table 3, which display the estimated function value of $f(t|\gamma)$ and $g(t|\theta)$ at the chosen knot points, as well as the associated standard errors. The results clearly show that the effect changes over time. A model in which the effect of the delisting is captured by a single dummy variable is therefore not valid. In Figure 3, we depict the same values together with the interpolated values, with a 95% confidence interval to indicate the uncertainty in these estimates. The function value of the spline at any point in time is a linear combination of the parameters. The confidence intervals can therefore easily be obtained from the covariance matrix of the parameter estimates. The first graph in Figure 3 shows that the decrease in overall detergent sales, in the test stores as well as in the control stores, occurs mainly during several weeks in early 2003. For this period, $f(t|\gamma)$ is significantly different from 0 and negative.

The second graph portrays the effect that may be attributed to the delisting. As we expected, the drop in category sales reaches its maximum negative sales effect in the first few weeks after the delisting took place. In later periods, sales recover, and at the end of the sample, the decrease in sales is only significant at 10%. These results seem to indicate that the delisting had a mainly (substantive) short-term effect. In contrast, we find only weak evidence for a long-term effect. Based on the estimation results, we can easily test the hypothesis that the long-run effect equals the immediate effect (H_0) against the one-sided alternative that the long-run effect is smaller in magnitude (H_a). For this comparison, we can directly compare the parameter estimates for the spline function $g(t|\theta)$ at the points 2002:46 and 2003:19. Based on the results, we reject H_0 at a 5% significance level.

These results also show that a model with a single intervention dummy or a linear function would not have captured the changes in sales adequately. The effect we find is obviously nonlinear.

Analysis 2: Decomposition—Former buyers versus former nonbuyers

The parameter estimates for Equation 3 appear in Table 4. In this case, we consider only sales generated by households that made at least one detergent purchase prior to T_1. The sales of the group of former nonbuyers in the control and test stores do not differ significantly after the delisting, as demonstrated by the estimates for $g(t|\theta)$. Former buyers of delisted items in the control stores also do not behave significantly differently from the other households in the

Figure 3
Effect of the Delisting on Detergent Category Sales (95% confidence bounds)

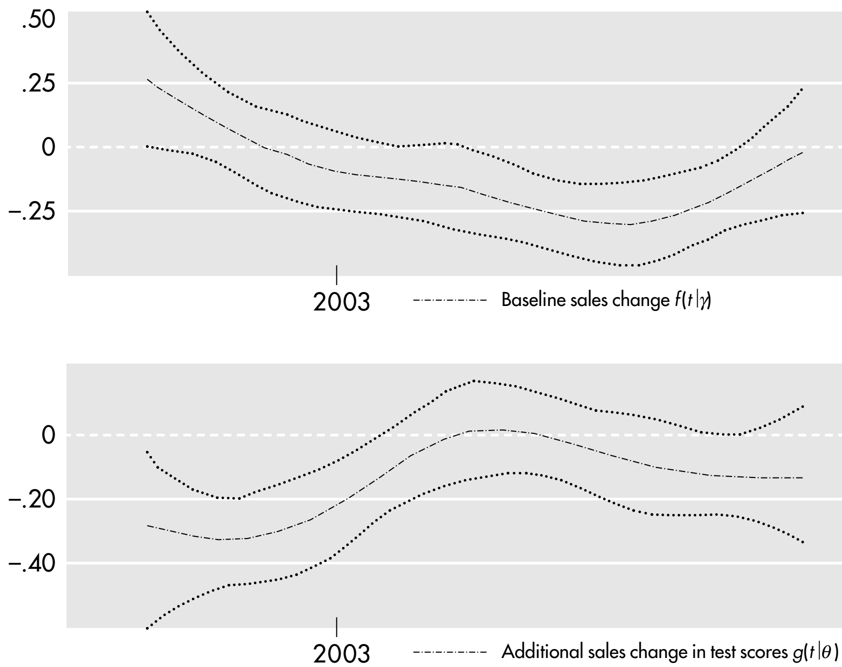


Table 4
Estimated Parameters for Log Weekly Category Sales Split into Former Buyers of Delisted Items and Former Nonbuyers (Equation 3)

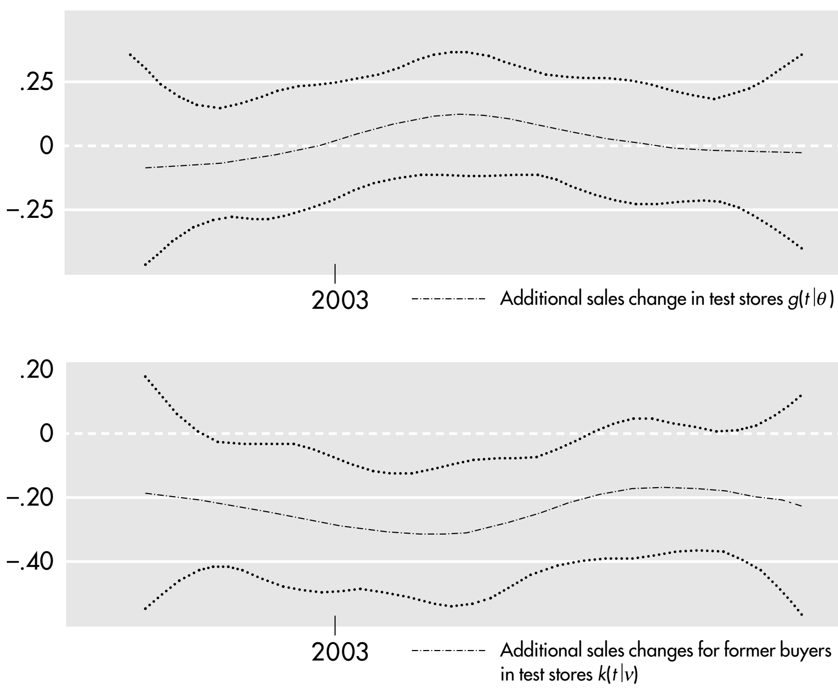
	Estimate	Standard Error
Store Dummies and Regressors		
Test store 1		(.036)
Control store 1		(.039)
Test store 2		(.040)
Control store 2		(.038)
Promotion		(.032)
Dummy former buyers (δ)		(.023)
Outlier dummy		(.215)
Baseline Sales Change all stores $f(t\|\gamma)$		
2002:46		(.143)
2002:52		(.093)
2003:06		(.092)
2003:12		(.089)
2003:19		(.137)
Additional Change in Test Stores $g(t\|\theta)$		
2002:46		(.191)
2002:52		(.122)
2003:06		(.121)
2003:12		(.117)
2003:19		(.183)
Additional Change: Former Buyers All Stores $h(t\|\phi)$		
2002:46		(.129)
2002:52		(.083)
2003:06		(.082)
2003:12		(.080)
2003:19		(.125)
Additional Change: Former Buyers in Test Stores $h(t\|\nu)$		
2002:46		(.168)
2002:52		(.105)
2003:06		(.104)
2003:12		(.101)
2003:19		(.162)

control stores. This is demonstrated by the estimates for the function $h(t|\phi)$. However, in the most interesting case, for consumers actually confronted with the removal of their preferred item or brand, we do find a significant decrease in sales, which is reflected in the estimates for $k(t|\nu)$. We also show this finding in Figure 4. The two graphs in Figure 4 indicate the changes in sales in the test stores relative to the control stores. The top graph shows that for households in the test stores that had not bought a delisted item prior to T_1, we find no significant effect on sales $[g(t|\theta)]$, whereas the lower graph $[k(t|\nu)]$ shows that for the group of former buyers of delisted items in these stores, there is a strong and significant decrease in sales a few weeks after the delisting. At the end of our sample, the effect of the delisting remains rather negative, though again only significant at a .10 level. Thus, we find only some weak evidence for a long-term sales effect of the assortment reduction among former category buyers of delisted items.[4]

Analysis 3: Sales of new category buyers

In this analysis, we only consider new category buyers. These buyers only purchased detergents after the assortment reduction and thus did not

Figure 4
Effect of the Delisting on Detergent Category Sales, Split into Sales Change due to Delisting for Former Buyers and Former Nonbuyers (95% confidence bounds)



purchase in the weeks before. Note, however, that the term *new category buyer* is not totally justified, because our observation period before the assortment reduction on which we base our grouping is only 26 weeks. Thus, our subsample of new buyers may also include some households that buy detergent very infrequently in the considered stores and therefore did not make a purchase before the delisting in the considered time period. Naturally, the detergent sales of new buyers equal 0 before the delisting. We therefore cannot apply the same methodology as we did previously. Instead, we consider the detergent sales generated by the new buyers relative to the total detergent sales. For all stores, we expect this percentage to increase over time, as some households that bought detergent before the delisting may stop purchasing detergents at the store, and more and more new households will enter. The stores in which the assortment reduction actually took place may attract more new buyers relative to the control stores, which will lead to a larger percentage of purchases made by new buyers.

To quantify the difference, we again perform a regression analysis using spline functions (see Figure 5). We initiate our analysis two weeks after the start of the delisting. In Figure 5, the top graph shows the estimated baseline effect, which demonstrates that, regardless of the delisting, the percentage of sales attributed to new buyers tends to increase over time. The bottom graph shows the additional effect in the test stores, namely, the additional share of sales by the new buyers in the test store. Immediately following the delisting, there is no significant difference between the control and the test stores. However, at the conclusion of our sample, the new buyers generate 28% of the sales in the control store and 38% in the test stores, which is a significant difference ($p < .01$). A possible explanation for this finding is that the reduced assortment enhanced search efficiency and thereby attracted more new buyers than did the nonreduced assortment. In an additional study executed within the test stores, we will verify if the reduced assortment indeed created more search efficiency.

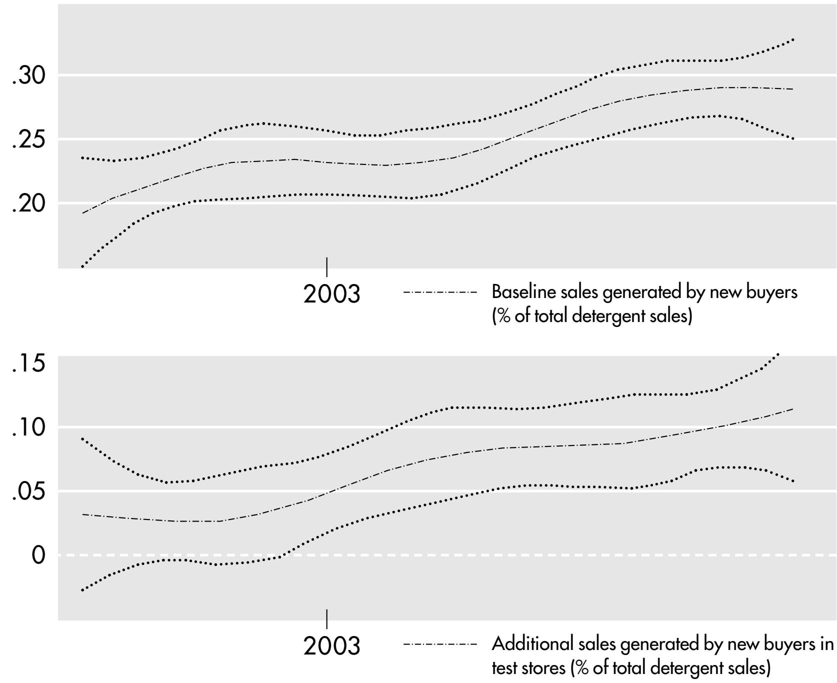
Additional Study

To further understand the sales effects we found, we conducted an additional study. In the test-stores, we investigated whether perceived assortment variety, perceived search efficiency, assortment satisfaction, and actual search time changed due to the assortment reduction. In particular, the search efficiency might be increased, while search time might be decreased. This might explain why we find a significant increase in category sales originating from new buyers in the test stores.

Research methodology

Data Collection. In the two test stores, data were collected one week before and 12 weeks (week 6 of 2003) after the assortment reduction occurred.[5] Experienced interviewers from a

Figure 5
Sales Effect Due to Entrance of New Buyers



research agency observed customers as they made their detergent purchases . Directly after the customer bought detergents, the interviewer contacted the detergent buyer to determine whether she or he would participate in this study. In total, 333 respondents participated in the instore survey (before purchase: 179, after purchase: 154; total response percent-age 65%).[6]

To assess whether the participants in the before and after surveys had the same backgrounds, we collected demographic variables such as gender, age, and household size, and buying behavior variables such as primary buyer, store loyalty, store visit frequency, and detergent buying frequency. Using pairwise t-tests and chi-square tests, we did not find any significant differences between the two samples for these variables.

In the week of the second measurement, the total category sales in the test stores are not significantly different from the total category sales in the control stores (see Table 3, estimate of $g(t|\theta)$ for t = 2003:6). However, former buyers of delisting items in the test stores did purchase significantly less compared to the control stores [see Table 4, estimate of $k(t|\nu)$]. Therefore, it must be that in this week, new buyers account for a larger share of the purchases in the test stores; unreported estimates of the spline functions for the new buyers confirm this.

Measurement. Following Hoch, Bradlow, and Wansink (1999) and Van Herpen and Pieters (2002), we measure perceived assortment variety with a four-item, five-point Likert scale (1 = strongly disagree, 5 = strongly degree). The items include "This assortment offers a wide variety of detergent," "I definitely miss some detergent items on this shelf," "This shelf offers the full range of detergent items," and "There are no important detergent items missing in this shelf." The coefficient alpha of the scale was .79. We defined perceived search efficiency as the ease with which customers perceived finding the preferred item (Broniarczyk, Hoyer, and McAlister 1998). We again used a four-item, five-point Likert scale, with a coefficient alpha of .67. The items we used include, "In this product assortment, it is easy to find the detergent item I prefer," "This is an orderly, organized assortment," "Some items are difficult to find in this assortment," and "This shelf offers the detergent items in a logical order." We conducted a confirmatory factor analysis to assess whether both factors are unidimensional and find sufficient scores for the fit statistics. The fit statistics are as follows: root mean squared error of approximation (RMSEA) = .06, goodness-of-fit index (GFI) =.96, and confirmatory fit index (CFI) = .95 (Bagozzi and Yi 1988; Baumgartner and Homburg 1996). The factor loadings were all significant and larger than .5. Therefore, we formed composites of the underlying items of perceived variety and perceived search efficiency. Finally, we used a single item to measure assortment satisfaction with which we asked consumers to evaluate the detergent shelf with a grade of one to ten.

We also collected the actual time (in seconds) consumers spent searching for an item in front of the detergent shelf. The interviewer started the time measurement when the detergent buyer entered the aisle and started looking at the detergent shelf and then stopped the clock when the customer picked the first detergent item off the shelf.

Results

The perceived assortment variety does not change after the assortment was reduced (3.9 before versus 3.9 after; p-value > .10), which indicates that even the large cut of 25% of the items did not lead to perceptions of less choice among detergent buyers. However, in line with Broniarczyk, Hoyer, and McAlister (1998), detergent buyers in the after-reduction group evaluated the detergent shelf significantly more easily in terms of perceived search efficiency than did the before-reduction group (4.1 versus 3.7, respectively; p-value < .01). This finding is confirmed by the results for actual search time in front of the detergent shelf, which demonstrate that the after group used significantly less time to buy the first detergent item than did the before group (14 seconds versus 20 seconds; p-value < .05). This finding reveals that a "cleaned-up" shelf lowers search costs among buyers. Because the perceived assortment variety did not change and perceived shelf efficiency increased, we also might expect an increase in the assortment evaluation, which our results confirm. Assortment evaluation significantly increased from 7.4 before the reduction to 7.6 after the assortment reduction (p-value < .05).[7]

Thus, the main results of this additional study are that the assortment reduction increases search efficiency (both perceived and actual search time), without lowering assortment variety among detergent buyers. As a consequence, assortment satisfaction increases. This result fits our finding that more new buyers were attracted to the stores where the assortment reduction was implemented. There is, however, one cautionary note. As this experiment includes only buyers of detergents, we did not include any evaluations of nonbuyers either before or after the assortment reduction; this could have two potential effects. First, former buyers of delisted items who switched stores or postponed their purchase because their detergent product was unavailable are not included in the after-reduction sample. As a consequence, evaluations measured after the reduction could be overstated because the dissatisfaction of these former buyers is not included. Second, because of the reduced complexity of the assortment, new detergent buyers could be attracted to the category, even though these buyers were not included in the before-reduction survey. Because this group should have a lower assortment satisfaction, evaluations in the before-reduction survey could be inflated as well.

Discussion

In this collaborative research project, we investigated the short- and long-term sales effects of an assortment reduction. Although this study pertains only to a single category, it clearly contributes to the literature on assortment reductions, in that we (1) investigate the short- and long-term sales effects of this reduction, (2) decompose the short- and long-term sales effects between former buyers and nonbuyers of the delisted items, (3) consider the entrance of new buyers as an explanation for the finding of neutral or positive sales effects in prior studies, and (4) investigate differences in actual search time before- and after the assortment reduction. We additionally executed a more qualitative study, which showed that increases in search efficiency might explain the increasing sales from new buyers.

The main conclusions of our study are as follows: First, on an aggregate level, we find a short-term negative sales effect and no strong significant long-term negative sales effect. Thus, reducing an assortment by delisting mainly low-selling items and brands has a negative sales effect in the short run. Second, extending the findings of Boatwright and Nunes (2001), we find that strong short-term negative sales effects occur mainly among former buyers of delisted items, probably due to their initial postponement and store switching. In the long term, the negative sales effect dissipates very slowly. Within the timeframe of our database, the results indicate some evidence (but not strong) of a long-term negative sales effect among former buyers of delisted items. Third, our study reveals that the assortment reduction may induce noncategory buyers to start buying within the category. We assume that the improved search efficiency, as shown in the additional study and reflected in increases in perceived search efficiency and a decreased search time, induces noncategory buyers to start purchasing detergents in this store. This finding provides an important empirical confirmation, in a natural experiment, of the findings by several experimental studies in consumer research and psychology that too large an assortment may inhibit retail customers from buying products because of the search complexity (Botti and Iyengar 2004; Gourville and Soman 2005). Of course, this is only a single study in one category. Hence, more research is required to generalize this finding. One final methodological contribution of this research is that, to our knowledge, this is the first application of a cubic splines methodology in marketing. It is a very useful model when researchers aim to study the effect of a single event such as an assortment reduction on, for instance, sales over time.

Management Implications

On the basis of this study, our partner retailer decided to roll out the assortment reduction nationwide. The results of our study gave it confidence that the assortment reduction would not significantly harm its detergent category sales in the long run. On the basis of consumer complaints in the pilot study, some small adaptations in the number of items to delist were made, so that, in total, 32 of the 37 tested items were delisted in the final rollout. The results from this rollout indicate that detergent sales, measured as a percentage of total store sales, were not significantly affected by the assortment reduction in the long term.

The collaborative research project also provides the retailer with some information regarding the execution of assortment-reduction projects. One key lesson is that a sole focus on short-term sales effects leads to incorrect conclusions. Instead, the time span for analyzing these effects must be lengthy enough to include long-run effects. Another key lesson is that assortment satisfaction apparently can be improved by reducing assortments and that new category buyers can be attracted. Therefore, the retailer has continued its assortment-reduction projects in other categories in which customers might find too large an assortment.

Research Limitations and Future Research

Our study has some limitations that may provide interesting opportunities for further research. First, the study is based on a single product category. Obviously, assortment-reduction effects may differ across categories, as already shown by Borle et al. (2005). Therefore, additional studies should include more categories to determine if our findings can be generalized to other products. For example, researchers could study hedonic categories, categories of products that cannot be stockpiled, impulse categories, and so on (Narasimhan, Neslin, and Sen 1996). Second, because each store has different characteristics and environment, further research should be based on data collected from more than four stores. Third, this study considered the effects of an assortment reduction at the aggregated category level. Other studies have investigated consequences of an assortment reduction at the disaggregated customer level. For example, Borle et al. (2005) consider how assortment reductions affect shopping frequency and purchase quantity, providing a deeper insight of consequences at the customer level. Clearly more research is required both at the aggregated and the disaggregated levels, as the number of studies on the effects of assortment reductions is still limited. Fourth, in practice, assortment reduction mainly implies delistings of low-selling items and brands, but delistings also may include high-equity brands, for example, when the retailer has a conflict with a national brand manufacturer. Additional research should study the short- and long-run effects of these delistings. A related possibility is that the manufacturer decides to delist an item. In this case, the item will not be available in any store. Such a delisting will probably have a different effect. Fifth, consumers may be confronted with multiple delistings in one or more categories. Future research efforts could focus on how multiple delistings affect category- and store sales. ■

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Notes

1. In the remainder of this paper, we do not distinguish between brand- and item delistings. Therefore, as we refer to item delistings in the remainder of this paper, we imply both brand- and item delistings. The only exception is the analysis reported in note 3, where we explicitly consider differential sales effects between brand- and item delistings.

2. In the remainder of this article, we refer to new buyers to the category as "new category buyers." This group may consist of several types of customers, such as new-to-the-store customers, infrequent category buyers, and buyers formerly buying the category products at other stores.

3. Usually researchers have used other models, such as the VAR model and Vector Error Correction Models, in studies distinguishing between short- and long-term effects of marketing actions (e.g., Nijs et al. 2001; Paap and Franses 2000), However, in these studies, one does not study a one-time event, such as assortment reduction. Instead, for example, one considers the effect of price promotions, where multiple promotions are observed in the sample. Because an assortment reduction is a one-time event, these models cannot be used. Hence, we have decided to use spline regression.

4. Following Boatwright and Nunes (2001), we also did a further decomposition by investigating sales effect differences between former category buyers of delisted brands and former category buyers of delisted items. Our results did not, however, show any significant differences between these two groups of former category buyers. This contrasts the findings of Boatwright and Nunes (2001), who reported different effects. The estimation results of this analysis can be requested from the authors.

5. Given an inter-purchase time of approximately four weeks for detergents, we have chosen to conduct the after-survey 12 weeks after the delisting took place to give consumers enough time to get used to the new shelf.

6. As approximately 80% of the test store sales were covered by loyalty card purchases, the additional study may be seen as an in-depth survey of a subset of the households studied in Study 1.

7. We also conducted a regression analysis in which assortment satisfaction was the dependent variable, and perceived assortment variety, perceived search efficiency, and actual search time were explanatory variables. This analysis reveals significant effects of assortment variety ($p < .10$), perceived search efficiency ($p < .01$), and actual search time ($p < .05$).

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