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The Effect of Survey Participation on Consumer Behavior: The Moderating Role of Marketing Communication

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Past research has established that just surveying individuals or measuring consumers' intentions can influence their subsequent behaviors. Building on self-generated validity theory and extant studies on the survey participation effect, we examine the behavioral phenomenon in a setting where consumers repeatedly participate in brand-specific surveys of all competing brands in a product category. We also investigate the existence and magnitude of the survey participation effect at the individual decision maker level while accounting for marketing communication efforts of the focal and competing brands. We test our proposed individual-consumer-level model using unique behavioral panel data with survey participation and marketing communication information. Our results suggest that the survey participation effect exists in a competitive marketplace setting where consumers' intentions toward a focal brand and all the competing brands are measured. We find evidence of a *backlash effect* wherein survey participation and marketing communication work against each other. We also find that consumers' participation in surveys of competing brands does not positively spill over to their choice of the focal brand. Based on our results, we suggest important implications for coordination between marketing communication efforts and marketing research activities.

Keywords: survey participation effect; mere-measurement effect; marketing communication; sales response models; multivariate Poisson-lognormal model; physician prescription behavior; detailing; Bayesian hierarchical models
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1. Introduction

It is typical for a firm to survey consumers to seek information on their purchase intentions and other information related to the firm's products. Since the study by Morwitz et al. (1993), which established that merely measuring consumers' purchase intentions related to a product category in a survey can increase the likelihood of a first-time purchase (commonly referred to as the "mere-measurement effect" or the "survey participation effect"), several studies have demonstrated that measurement of consumers' intentions or consumers' participation in surveys would positively influence consumers' brand-level choice within a product category (Fitzsimons and Morwitz 1996) as well as other aspects of consumer behavior, such as incidence and responsiveness to price promotions (Borle et al. 2007), and relational behaviors, such as customer retention and profitability (Chandon et al. 2004). The rationale for the mere-measurement effect is grounded in the self-generated validity theory (Feldman and Lynch 1988). This theory suggests that survey participation can cause respondents to form favorable judgments about the focal brand that they would not otherwise form and also help them to recall information about the brand.

Despite support for the presence of the survey participation effect, there are important issues that remain unanswered. First, many extant studies have investigated the existence of the survey participation effect at the overall product category level (e.g., Morwitz et al. 1993) or by using surveys pertaining to a single firm (e.g., Dholokia and Morwitz 2002). As market research firms such as ForeSee, J.D. Power, and Forrester Research collect customer satisfaction data from all competing brands in a product category, consumers are likely to participate in surveys of not only a focal brand but of all the competing brands in a particular product category.[1] In such contexts, it is unclear

[1] Other than third-party market research companies, firms also survey their customers to gauge opinions about their own and other competing products and services. For example, United Airlines and American Airlines distribute in-flight surveys in which they ask customers to rate their services against other competing airlines.

whether the survey participation effect still exists. A key mechanism by which the survey participation effect operates is that of increased accessibility of cognition on the part of consumers. When consumers participate in surveys of competing brands, regardless of whether their opinions about competing brands are measured simultaneously in a single survey or in different brand-specific surveys, we believe that increased accessibility of a focal brand can come at the expense of accessibility of a competing brand (Alba and Chattopadhyay 1985). As a result, the net mere-measurement effect toward any single brand may not exist in such a competitive setting. In other words, the increased accessibility of the competing brands (as a result of consumers' survey participation) may mitigate the survey participation effect toward the focal brand. However, one can also argue that similar to perception spillovers across competing brands (Ahluwalia et al. 2001, Janakiraman et al. 2009), measuring consumers' intent toward a competing brand can spill over and positively influence consumers' behavior toward the focal brand. To our knowledge, no study has examined the existence and magnitude of the survey participation effect toward a focal brand in a competitive setting, where respondents could participate in different (brand-specific) surveys for all the competing brands in a product category. Studying the survey participation effect pertinent to a focal brand using a panel of the *same* consumers participating in *different* surveys of competing brands in a product category would also help us understand the nature of the effect, whether it is because of category expansion or brand switching.

Second, survey participation effects are attributed to increased accessibility of attitudes toward the product category and the focal brand (Feldman and Lynch 1988). Extant work has established that the process of measurement helps consumers retrieve information from their memory (Janiszewski and Chandon 2007) and that the mere-measurement effect is automatic and nonconscious (Fitzsimons and Williams 2000). Many of the mechanisms behind the survey participation effect are similar to those behind the effects of marketing communication on consumers (Berger and Mitchell 1989, Keller 1987). Given the similar mechanism underlying the effects of marketing communication and survey participation on consumers' choices, it becomes imperative to study the presence and measure the magnitude of the survey participation effect after accounting for the effects of marketing communication. From an econometric perspective, the estimate of the mere-measurement effect can be biased if the effect of marketing communication on consumer choice is not accounted for. Although a few limited studies rule out the effects of marketing communication of a focal firm (e.g., Dholokia and Morwitz 2002), no study to our knowledge thus far has accounted for the presence of marketing communication efforts of the focal firm and its competitors when studying consumer response to survey participation.

Third, survey participation could have an indirect effect on consumer behavior through its interaction with the marketing communication efforts of the focal firm. On the one hand, survey participation and marketing communication can have a synergistic effect, wherein survey participation can help consumers recall the marketing communication efforts of a focal brand, which can make consumers respond more positively to the focal firm's marketing communication efforts. On the other hand, prior literature has established that sustained repetition of messages can lead to decreased effectiveness of messages or even backfire as repeated messages can lead to boredom when consumers have less opportunity to learn (Sawyer 1981, Campbell and Keller 2003). Along these lines, we believe that repeated engagement, in the form of exposure to marketing communication and measurement of consumers' intentions could lead to a *backlash effect*, whereby consumers respond negatively to marketing communication as a result of survey participation. In other words, there could be a significant interaction effect between the survey participation effect and consumer response to marketing communication, and the direction of such an effect is largely empirical in nature. Prior literature has emphasized the importance of planning marketing-mix strategies in the presence of interaction effects (e.g., Naik et al. 2005). Against this background, note that, to our knowledge, no study thus far has explicitly examined the interaction effect between marketing communication and the survey participation effect using market data. We believe examining this effect is important because it has implications for the coordination between targeted marketing communication efforts and marketing research efforts. Prior studies have established the practical relevance of the survey participation effect—for example, Borle et al. (2007) argue that measuring customer opinions in the form of surveys should be considered an investment and not purely a cost-incurring marketing research activity, as it can mean additional revenues in the form of increased brand loyalty, shorter interpurchase time, and greater response to promotional activities. Although the proportion of customers who participate in surveys is generally low, it is becoming increasingly feasible to solicit consumers' opinions as a result of consumers' participation in firms' social media efforts. However, for a firm to enjoy such benefits from its customers' participation in surveys, it is critical to know the moderating role of marketing communication on consumer response to survey participation effect. Last, whereas almost all the extant studies have examined the phenomenon of the survey participation effect in a business-to-consumer setting, we document the survey participation effect in a business-to-business setting.[2] On a related point, many of the extant studies have examined the survey participation effect in nonroutine behaviors, such as blood donation (Godin et al. 2008), voting (Greenwald et al. 1987), and durable goods purchase behavior (Morwitz et al. 1993), but we examine the survey participation effect in a fairly stable context á la Wood et al. (2005), one in which decision makers make routine decisions. We hope that the uniqueness of our empirical context lends new evidence on the scope of the behavioral phenomenon.

The objectives of our study are to address the above issues and extend our understanding of the survey participation effect. We examine the effects of consumers' *repeated* participation in brand-specific surveys pertinent to multiple and competing brands on their choices to determine whether the survey participation effect holds in the presence of marketing communication and how the effects (if any) interact with marketing communication efforts. We examine this in a competitive marketplace setting where consumers can choose a product from a set of competing alternatives and consumers' intentions toward a focal brand and all the competing brands in the particular product category are measured repeatedly in (different) brand-specific surveys. To accomplish our objectives, we leverage a unique data set in which we observe consumers' choices from a set of competing brands in a product category over time. We have access to disaggregate consumer-level targeted marketing communication efforts by all the competing brands in a product category. In our context, the consumers repeatedly participate in surveys administered on behalf of all the competing brands in the product category. Leveraging this unique data set, we build a disaggregate-level consumer model to examine the main effect of survey participation and its interaction effect with the marketing communication efforts of a focal brand while accounting for consumers' participation in surveys administered by competing brands and their marketing communication efforts. We account for unobserved heterogeneity in consumers' response to marketing communication and survey participation effect by casting our model in a hierarchical Bayesian framework. In our study context, with respect to marketing communication activities, firms typically target decision makers based on their responsiveness to such efforts. Accordingly, we account for endogeneity of firms' targeted marketing communication efforts by specifying a marketing communication allocation model, which we estimate jointly with our proposed consumer response model.

Our empirical findings show evidence of a survey participation effect in a competitive marketplace setting where consumers participate repeatedly in surveys of competing brands. We find evidence of a backlash effect, whereby survey participation and marketing communication work against each other. We also find that measuring consumers' intentions toward competing brands does not positively spill over to their choice of the focal brand. Instead, our results suggest that survey participation effects are primarily due to brand switching. Our study makes the following contributions: From a theoretical perspective, our study confirms the presence of the survey participation effect toward a focal brand under stringent conditions wherein consumers' intentions toward all competing brands are measured (repeatedly in different brand-specific surveys) and in the presence of marketing communication efforts by both the focal and the competing brands. Our study also demonstrates how survey participation can interact with marketing communication efforts. From a practical standpoint, our results confirm that survey participation helps consumers recall information about the focal brand and positively influences their behavior. However, our finding also suggests caution over surveying, and we quantify the negative returns due to repeated surveying and overexposure to marketing communication. Based on our results, we shed light on the coordination between two consumer touchpoints—namely, surveying customers and targeting them with marketing communication—so that the impact of the negative interaction between these two can be minimized. This insight on coordination is especially important if firms perceive and manage marketing communication and market research as two entirely independent functions.

The remainder of the paper is organized as follows. First, we review the background literature on the survey participation effect. We then describe the empirical setting of our study, present the formulation of the model, and discuss its specification and estimation issues. We then present and discuss the results of our model. In the subsequent sections, we present model comparisons and the robustness checks that we performed. We then discuss the managerial implications of our study and conclude with limitations and future research opportunities.

2. Background

In this section, we review the pertinent background literature and delineate the positioning of our research vis-à-vis the findings from extant research on the survey participation effect.

2.1. Survey Participation Effect

A rich set of studies in psychology and marketing have demonstrated that simply asking or measuring an individual's intent about future behavior can change the individual's underlying behavior itself. Using a series of experiments, Sherman (1980) established that

[2] We thank an anonymous reviewer for pointing out this distinction.

participants who were asked about their willingness to participate in socially desirable activities (i.e., volunteering for a cancer society) are more likely to do so than those who were not asked about their intentions. Sherman termed this effect "self-erasing behavior," as respondents are more likely to behave in conformance with their stated behavior. Subsequently, a number of studies have established the presence of the survey participation effect in various behavioral domains, such as respondents' likelihood of voting (Greenwald et al. 1987) and the long-term use of a health club (Spangenberg and Obermiller 1997). Unlike these experimental studies, Morwitz et al. (1993) demonstrated the effect by using a series of quasi-experimental studies set in a marketing context. They found that the act of measuring intentions leads to increased purchase rates for durable goods, such as automobiles and personal computers, among those whose intentions were measured. At this point, we note that researchers have employed different nomenclatures to refer to the survey participation effect such as, the self-erasing error of prediction (Sherman 1980), the mere-measurement effect (Morwitz et al. 1993), the self-prophecy effect (Spangenberg and Greenwald 1999), and the question-behavior effect (Sprott et al. 2006). Sprott et al. (2006) referred to the *question-behavior* effect as "any phenomenon whereby questioning of a person (whether it be through an intention measure, self-prediction, a measure of satisfaction or other means) influences the future performance of the focal behavior" (p. 129). Regardless of the terminology or what was measured in a particular survey, the main result established by all these studies is that surveying individuals or measuring consumers' intentions influences their subsequent actual behavior.

Given that the survey participation effect has been established as a robust behavior by earlier studies, more recent studies have taken two different approaches. One set of studies looks at examining other behavioral consequences and the persistence of the survey participation effect, and the other set is concerned with understanding the processes behind the survey participation effect. We first briefly discuss the studies that extend the survey participation effect to other related behaviors. In the following subsection, we discuss in detail the studies that have examined the reasons behind the survey participation effect. Dholokia and Morwitz (2002) explored the scope and persistence of the survey participation effect and found that measuring satisfaction not only affects one time purchase behavior but also other relational behaviors, such as customers' likelihood of termination and profitability. They also found that the survey participation effect can persist several months after measuring the respondents' intentions. Chandon et al. (2004) found a long-term effect of measuring intention and showed that the survey participation effect sustains through repeat purchases. Borle et al. (2007) leveraged a longitudinal field study of customer satisfaction to help document the practical implications of the survey participation effect. They found that customers' participation in a satisfaction survey is positively associated with various customer behaviors, such as responsiveness to price promotions, coupon offerings, interpurchase duration, and the total amount spent by the customers. It is worth noting that whereas the earlier studies worked with surveys in which consumers' intention to purchase was measured (e.g., Morwitz et al. 1993), some of the more recent studies (e.g., Dholokia and Morwitz 2002, Borle et al. 2007) have worked with surveys in which subjects' satisfaction was measured.

Although these studies shed further light on the scope of the survey participation effect, they do not examine the behavior in a competitive setting where consumers choose a focal product from a set of competing options and where consumers' participation in surveys pertinent to all the competing options (either in the form of a single survey or different brand-/firm-specific surveys) are measured. Furthermore, they do not account for marketing communication efforts of either the focal product or the competing products. Our study aims to address both of these critical gaps, which will enhance our understanding of the survey participation effect.

2.2. The Survey Participation Effect—What Is Behind It?

As mentioned earlier, the second set of recent studies was concerned with understanding the processes that make the survey participation effect work. Sherman (1980) first suggested that the act of measuring intention evokes a cognitive representation of the behavior, which then helps the respondents follow through with the behavior. Gregory et al. (1982) proposed that measuring intentions related to a behavior can increase the salience and accessibility of information that is related to the behavior. They explained that asking respondents to imagine an outcome helps them give more weight to information related to that outcome. Feldman and Lynch (1988) also argued that asking respondents about a behavior makes them form judgments about the behavior that they would not have formed otherwise. In other words, the increased accessibility of the judgments influences the subsequent response. Later studies (Morwtiz et al. 1993, Fitzsimons and Morwitz 1996) confirmed that increased accessibility is the operational force behind the survey participation effect. The finding that increased accessibility is the reason behind the response to an intention question implies that such a response is automatic. To confirm this, Fitzsimons and Williams (2000), using a series of experiments, established that the mere-measurement effect is a result of an automatic and nonconscious process on the part of the respondents rather than the result of their forming a deliberate intention and then following through with that intention at a later time. It is worth noting that the survey participation effect need not be induced solely by intention measurement questions but may also be induced by participation in surveys where subjects' other opinions are measured. (For example, Borle et al. 2007 worked with surveys in which customers' satisfaction was measured.) Building on the arguments by Feldman and Lynch (1988), Dholokia and Morwitz (2002) contended that questioning essentially serves to form an opinion about a firm and its products, which in turn results in behavior that is consistent with subjects' evaluations.

Many of the mechanisms that are attributed to the observed survey participation effect, such as automatic activation and easier accessibility of judgments, can also be induced by marketing communication. For example, extant studies have established that advertising increases the accessibility of attitudes (Berger and Mitchell 1989), triggers a subliminal response (Moore 1982, Petty et al. 1983), and helps consumers recall their judgments about the products that they have experienced (Keller 1987). Given the similarity in behavioral mechanisms behind the effects of survey participation and marketing communication, and given that consumers regularly participate in surveys and are constantly exposed to marketing communication, the following three questions are important: (1) Does the survey participation effect pertinent to a focal brand hold if the effects of marketing communication related to the focal brand and the competing brands are accounted for? (2) How do the survey participation effect and marketing communication interact with each other? (3) If questioning can increase the accessibility of consumers' attitudes toward the category brand and the focal brand, would the survey participation effect associated with a focal brand hold if consumers participate in surveys of competing brands? The first two questions related to the survey participation effect and marketing communication are important because cognitive responses to marketing communication and survey participation are similar. From a theoretical perspective, understanding the interaction between marketing communication and the survey participation effect is particularly important. Building on the persuasion knowledge model by Friestad and Wright (1995), Williams et al. (2004) found support for their hypothesis that when consumers perceive the act of measuring intention to be persuasive, the survey participation effect is attenuated. This finding has important implications for consumers' response to repeated survey participation in the presence of marketing communication. As consumers are repeatedly exposed to marketing communication, there are fewer opportunities to obtain new information. This leads to annoyance, which in turn can lead to a response against repeated messages (Anand and Sternthal 1990, Campbell and Keller 2003). In a similar vein, repeated survey participation and exposure to marketing communication can lead to a backlash effect that may result in negative returns on marketing communication. However, one could argue, based on the arguments noted earlier, that there could be a synergistic effect between survey participation and marketing communication. Thus, although we believe that the direction of the interaction effect is empirical, no study to our knowledge has explicitly examined such an interaction effect between the two in a context where consumers repeatedly participate in surveys and are exposed to marketing communications by competing firms in a product category. This study aims to fill this critical gap in our understanding of the survey participation effect and explores the contingency factors in consumer response to survey participation.

The other highlight of our study is that we examine the survey participation effect in a setting where consumers' opinions about competing brands in a product category are measured. This brings the brand spillover effect into play; at this point, we would like to clarify that, with respect to the measurement of consumers' intentions about competing brands, intentions may or may not be measured in the same survey as the focal brand. We work within a context where consumers' opinions of the focal and the competing brands are measured in *different* brand-specific surveys. Regardless of how consumers' intentions toward competing brands are measured, it is possible that measuring consumers' intentions toward competing brands and the subsequent increased accessibility of a competing brand comes at the expense of the accessibility of consumers' attitudes toward the focal brand (Alba and Chattopadhyay 1985). In such a scenario, if consumers were to be surveyed about all the brands in a category, the net survey participation effect toward any single brand may not be significant. However, arguments based on the accessibility-diagnosticity framework (Feldman and Lynch 1988) suggest that if one brand were to be informative of another brand, then the perceptions of that brand would spill over to another brand (Roehm and Tybout 2006, Janakiraman et al. 2009). This in turn suggests that consumer participation in surveys related to a focal brand helps them to access information and/or form judgments not only about the focal brand but also about other competing brands. In other words, the survey participation effect associated with a focal brand can positively spill over to a competing brand, and such effects could be asymmetric across the different brands. It is worth noting that much of the extant work in the area has examined the survey participation effect at a product category level or a single brand/firm level. We attempt to document the survey participation effect in the context of a competitive marketplace.

To summarize, our study contributes to the literature by examining whether the survey participation effect toward a focal brand still exists when (1) consumers participate in surveys of competing brands and (2) consumers are exposed to the marketing communication efforts of both the focal brand and the competing brands in the same product category. We use an individual-level behavioral data set in the context of a business-to-business setting and thus complement and extend previous research in the area of the survey participation effect.

3. Data and Research Context

The context of our study is physicians' prescription behavior, and we use data from a single therapeutic category. The data were collected and made available to us by ImpactRx, a firm that specializes in tracking the effect of targeted promotional activities employed by pharmaceutical firms on physicians' prescription behavior. We believe that the context is well suited to studying our research questions for the following three reasons: First, the level of involvement on the part of the decision makers in this context (i.e., the physicians) is high; physicians are often surveyed repeatedly about the various brands they prescribe by both pharmaceutical firms and third-party market research companies. Second, physicians are often targeted with marketing communication by all of the competing brands, and they regularly prescribe from a set of competing drugs. Third, the context allows for the tracking of marketing communication at the individual decision maker level. Although individual-level transaction data are typically available in a variety of contexts, accurate individual-level marketing communication data are not available in many other contexts. For example, with respect to ACNielsen television advertising data, one cannot be sure whether the advertisements were actually viewed by the decision makers. In our data, all the marketing communication activities directed at individual physicians by all the competing brands in a category are accurately recorded. For the above-stated reasons, after controlling for factors such as physicians' inherent propensity to prescribe a branded drug, promotional activities of the focal and competing drug companies, and physicians' persistence in prescribing a drug, if we find a significant effect of survey participation on physicians' prescription behavior, then it would point to the potency of the survey participation effect on the behavior of decision makers.

The data contain the monthly prescription decisions of a nationally representative panel of primary care physicians during the two-year period between January 2008 and December 2009. The data represent the monthly new prescriptions[3] written by the panel of physicians in the oral anti-diabetics category, as well as detail visits from all of the competing pharmaceutical companies in the category.[4] We focus our analysis on the top three brands in the category during the data period. They are Actos (marketed by Takeda), Avandia (marketed by GlaxoSmithKline), and Januvia (marketed by Merck). Avandia and Actos belong to the same subclass of anti-diabetics drugs (called thiazolidinediones), whereas Januvia belongs to another subclass (called dipeptidyl peptidase-4 inhibitors). These three branded drugs account for 89% of all prescriptions in the category (in our data).[5] Since we are interested in individual physician-level estimates, we exclude those physicians who did not prescribe any of the three brands from the analysis. Applying this filter leaves us with a panel of 503 physicians as our estimation sample for the two-year period.

A novel aspect of our data is that along with individual physician-level marketing communication information from competing drugs, we also have access to surveys that were completed by the panel of physicians.[6] Each survey is drug specific and asks nine questions, all rated on a 1–7 scale. Of these nine questions, five are about the sales representative the physician met on the previous detailing call, three are about the message delivered on the previous detailing call, and the final question is about the overall intention of prescribing the detailed drug. The survey participants indicated a high intention to prescribe for all three drugs under study, with the average intention to prescribe being 5.0 (S.D. = 0.96) for Actos, 4.9 (S.D. = 1.0) for Avandia, and 5.1 (S.D. = 0.97) for Januvia. We note that these drug-specific surveys are automatically generated right after every fourth detailing visit (for a particular drug, but *across* physicians) is recorded, and these surveys are conducted by ImpactRx. Since the surveys were administered by a third-party firm, as per this simple rule, we do not have to worry about a potential "demand" effect that may contaminate the

[3] We consider only new prescriptions in our analysis. Whereas new prescriptions usually entail decision making on the part of physicians, who are impacted by marketing tools, renewals are primarily driven by nonmarketing factors, such as patient–drug match, which are beyond the scope of our study.

[4] Detailing refers to the one-on-one sales calls made by pharmaceutical representatives. Although pharmaceutical firms employ other forms of marketing communication, such as advertising in journals, detailing accounts for the single largest expenditure by pharmaceutical firms (Wittink 2002, IMS Health 2011).

[5] We note that all of the three branded drugs were under patent protection during the study period, and thus, there were no generic alternatives available. Henceforth, we use the terms "brands" and "drugs" interchangeably.

[6] ImpactRx started sending and collecting surveys from the physicians' panel before the start of our data period.

survey participation effect at the individual physician level. A highlight of our data is that we have a 100% participation rate from all the physicians in our panel.[7] Another key data feature is that all physicians get to participate in the surveys of all the competing drugs in the therapeutic category. Among 503 physicians in our estimation sample, 235 received surveys for more than one drug in at least one given month during the two-year data period. Table 1 provides summary statistics about the estimation sample. Actos accounts for the largest market share at 52%, followed by Januvia with a market share of 33%. Januvia, launched in October 2006, is the newest product among these three brands. With a market share of 15%, Avandia has the lowest brand share among the three. It is also the oldest of the three, having been launched in May 2001, two months before Actos was introduced to the market.

Table 1 also provides summary statistics about the prescription rate across physicians for the three drugs. The mean monthly new prescription rates for the three brands are quite small (fewer than two). However, the standard deviation in the prescription rate across the physician-months is quite high. With respect to the summary statistics of detailing visits, on average, the brand with the highest share (Actos) receives the highest average number of detailing visits, and the brand with the lowest market share (Avandia) receives the lowest average number of detailing visits. This suggests the possibility of strategic detailing allocation on the part of pharmaceutical firms—those physicians who respond more to detailing may receive more detailing calls (e.g., Manchanda et al. 2004, Dong et al. 2009)—which could result in potential endogeneity of the detailing visits variable. Our model formulation specifically addresses this issue; we elaborate on the issue in the next section. Finally, the last column in Table 1 lists the average and standard deviations for the number of surveys each physician receives in a month. The average number of surveys (across all physicians) lies between 0.08 and 0.15, which is about one-quarter of the average number of detailing visits for each brand.

A few points about the survey generation process deserve special mention. The surveys are generated by the marketing research firm ImpactRx, which also collected the prescription and detailing data from the physician panel that is analyzed in this study. To avoid the possibility that physicians are annoyed by too many surveys, the marketing research firm administered a survey after approximately every fourth drug detailing. We emphasize that this "fourth detailing visit" rule is within a brand but *across* physicians (i.e., any time a drug is detailed for the fourth time, a survey specific to the drug will be automatically generated by the third-party research firm and sent to the physician who received the fourth detailing call). We note that the pharmaceutical firms are not involved in any part of the survey administration, and there is no coordination between the pharmaceutical firms and the marketing research firm. Since the pharmaceutical firms allocate detailing visits, the marketing research firm does not know which physician is likely to receive the fourth detailing call and therefore would not know which physician is likely to participate in a survey at a given point in time. Even with respect to the timing of survey administration, the pharmaceutical firms do not know when a particular physician is being surveyed (for competing brands and for their own brand). Since physicians can always refuse to meet with salespersons, the firms do not have control over whether a salesperson indeed met with a physician at a particular point to use the survey as a strategic tool. Given all of the above features, surveying is not employed as a strategic tool by the pharmaceutical firms, although detailing is allocated strategically across physicians.

To the extent that physicians who receive more detailing calls are likely to receive more surveys, the number of brand-specific surveys is correlated with the number of brand-specific detailing calls at the physician level. To take into account the relationship between detailing visits and survey participation, we estimate a model that accounts for the possibility that physicians can receive targeted detailing, and thus, their opportunities to participate in surveys can vary.[8]

Table 1 Summary Statistics of Physicians' Monthly New Prescription Rate, Number of Detailing Visits, and Number of Surveys Taken

		New prescriptions		Detailing visits		No. of surveys	
Brand	Market share (%)	Mean	S.D.	Mean	S.D.	Mean	S.D.
Actos	52	1.02	(1.82)	0.66	(1.07)	0.15	(0.41)
Avandia	15	0.29	(0.84)	0.39	(0.79)	0.08	(0.31)
Januvia	33	0.62	(1.19)	0.67	(1.04)	0.14	(0.40)

[7] The 100% participation rate in the surveys is part of the contractual obligation for physicians who serve on the panel. This participation rate helps us obviate self-selection issues in measuring the survey participation effect. In our context, the panel's decision to participate in the study is not determined by its predisposition to prescribing a brand. We note that the limited set of studies (e.g., Chandon et al. 2004, Borle et al. 2007) that examines survey participation effect using behavioral data does not account for self-selection issues (i.e., for the possibility that customers who have favorable judgments toward a retailer are more likely to participate in firm-initiated surveys and are likely to have a stronger behavioral association with the firm). We also note that the panel was a nationally representative sample of primary care physicians selected by ImpactRx.

[8] In other words, once we account for the endogeneity of detailing levels at the individual physician level, there are no other variables that determine survey participation at the individual physician level. We conducted various empirical tests to provide evidence that the

We discuss this further in the following section, where we explain in detail our proposed model formulation and the model estimation steps.

4. Model Formulation and Estimation

Our proposed econometric model comprises two parts. The first part captures the physician's prescription behavior; the second part is related to firms' decision on allocation of detailing visits at the individual physician level. Our unit of analysis is at the monthly level. We describe each of these two models below.

4.1. Physicians' Prescription Behavior

The number of new prescriptions written by each physician for each brand in each month is a count variable with a small mean; a normal or log-normal distribution, which would treat the variable as a continuous variable, would not be appropriate. In addition, as demonstrated in Table 1, the variances in monthly new prescription counts for a particular drug are much higher than the means, and therefore a standard Poisson model would not be appropriate either. Given that physicians are writing prescriptions across three drugs, the number of prescriptions at the individual physician level could be correlated across these drugs. To accommodate these issues in the data, we adopt the multivariate Poisson-lognormal model (Aitchison and Ho 1989), which we explain in detail below.

Let rx_{ijt} represent the number of new prescriptions written by physician i ($i = 1, \ldots, I$) for brand j ($j = 1, \ldots, J$) in month t ($t = 1, \ldots, T$). Let λ_{ijt} denote the mean number of new prescriptions written by physician i for brand j in time t. Given a Poisson distribution with parameter λ_{ijt}, the probability that rx_{ijt} takes the value n is given by (Cameron and Trivedi 1998):

$$P(rx_{ijt} = n \mid \lambda_{ijt}) = \frac{\lambda_{ijt}^{n} \exp(-\lambda_{ijt})}{n!}.$$

We use the log-link function to account for the effect of covariates on the mean prescription rate λ_{ijt} as follows:

$$\lambda_{ijt} = \exp(u_{ijt}). \tag{1}$$

We specify u_{ijt} as a function of both observed and unobserved factors that impact physicians' prescription decisions as follows:

$$\begin{aligned} u_{ijt} = {} & \beta_{ij0} + \beta_{ijd} \ln(dtl\_stock_{ijt} + 1) + \beta_{ijdc} \ln(cdtl\_stock_{ijt} + 1) \\ & + \beta_{ijs}\, srv\_stock_{ijt} + \beta_{ijsc}\, csrv\_stock_{ijt} \\ & + \beta_{ijds}(dtl\_stock_{ijt} \times srv\_stock_{ijt}) \\ & + \beta_{ijl} \ln(rx_{ij,\,t-1} + 1) + \epsilon_{ijt}. \end{aligned} \tag{2}$$

The intercept β_{ij0} represents the physician-brand–specific effect and accounts for physician i's intrinsic prescription rate of the focal drug j. In addition, physicians' prescription behavior is affected by four sets of observed factors. The first set accounts for the influence of marketing communication efforts, as represented by the number of detailing visits for both the focal brand and its competitors. Following the literature (e.g., Gönül et al. 2001, Manchanda et al. 2008), we adopt a stock formulation of detailing. The detailing stock variable at a time period t is calculated as

$$dtl\_stock_{ijt} = \delta \times dtl\_stock_{ij,\,t-1} + dtl_{ijt},$$

where δ denotes the monthly carryover rate and dtl_{ijt} denotes the number of detailing visits received by physician i from brand j's company in time t. The initial value for the stock variable (i.e., $dtl\_stock_{ij,\,t=0}$) is calculated using the detailing visits in 2007 (i.e., from a period that precedes our calibration data period); β_{ijd} captures physician i's response to the detailing stock variable of the focal brand j. Following the recent studies in pharmaceutical marketing (e.g., Donohue and Berndt 2004, Fischer and Albers 2010, Fischer et al. 2010, Montoya et al. 2010, Camacho et al. 2011), we adopt a natural log transformation of the detailing stock to accommodate for possible diminishing returns of detailing. The competitive detailing stock variable is denoted by $cdtl\_stock_{ijt}$, which is calculated as the sum of all the competitors' detailing stock targeted at the same physician i by time t, and β_{ijdc} captures physician i's response to the detailing stock of the competing brands. Since detailing stock can take the value of zero, we add 1 to it before taking the natural log transformation. In sum, we account for the cumulative and recency effects of detailing and for its diminishing returns; we take such an approach so that the survey participation effects, if detected in our empirical setting, are not confounded with the dynamic effects of detailing.

The second set of variables accounts for the survey participation effects in a competitive setting, which include the stock variables calculated based on the number of own and competing surveys completed by physician i up to time t (denoted as srv_stock_{ijt} and $csrv\_stock_{ijt}$, respectively).[9] Although the surveys measure physicians' intention to prescribe as well as their opinions about the sales representatives they met and the messages delivered, given the high correlation

number of surveys a physician receives is not endogenous to the physician's prescription decisions (or to the physician's response to prior surveys), conditional on the number of detailing calls received. More information on these tests is available from the authors upon request.

[9] We note that we allow the decay parameters associated with the detailing stock and the survey stock to be different. We conducted a grid search and found the value of $\delta = 0.7$ for the detailing stock and 0.8 for the survey stock to provide the best model fit, i.e., the highest marginal log-likelihood. Note that the 0.7-monthly carryover rate that we set for detailing stock is consistent with the values used in prior studies (e.g., Manchanda et al. 2008).

between these items at the individual physician level, we do not work with the different items separately. Instead, we work with the number of times a physician participated in (brand-specific) surveys. Parameters β_{ijs} and β_{ijsc} capture the survey participation effects associated with the focal brand and the competing brands, respectively, at the individual physician level.

The third set of variables accounts for the interaction effect between survey participation and marketing communication. This is captured by the product of the detailing stock and survey stock received by physician i in the same time period t; i.e., $dtl\_stock_{ijt} \times srv\_stock_{ijt}$. Parameter β_{ijds} captures the interaction effect at the individual physician level.

Past literature has documented that physicians exhibit persistence in their prescription behavior (Janakiraman et al. 2008). Accordingly, the fourth set of variables accounts for the state dependence of physician's prescription decisions. We operationalize state dependence by the number of prescriptions written by physician i for the same brand j in the last time period $t-1$, i.e., $rx_{ij,\,t-1}$. Following precedence (Dong et al. 2009), we use the natural log transformation of the lagged prescription variable. Since lagged prescriptions can take the value of 0, we add to it 1 before taking the natural log transformation. The last component in the model specification is ϵ_{ijt}, an additive random error term. This term, albeit unobserved to the econometrician, helps account for other physician-brand-time–specific factors that could impact a physician's prescription decision. We assume that ϵ_{ijt} follows a mean zero multivariate normal distribution across physicians and time. Having a multivariate normal random error term within the log-link function of the Poisson parameter makes our model a multivariate Poisson-lognormal model (Aitchison and Ho 1989, Munkin and Trivedi 1999, Riphahn et al. 2003). This model is more flexible than a standard Poisson model in the following three important ways (Cameron and Trivedi 1998). First, it allows for overdispersion, thus making it less restrictive than a standard Poisson model. Second, it helps account for an overproportion of zeros, as is the case with our data. Finally, it allows for the number of new prescriptions by a physician in a time period to be correlated across brands, which is captured through the correlations of the multivariate error term ϵ_{ijt} across brands. It is possible that such unobserved factors can simultaneously influence a physician's prescription decisions for all brands. For example, if physician i happens to be out of the office for a few days during time t, the prescription rates of all brands will be influenced as a result of this unobserved incidence.

As mentioned above, we obtain seven individual physician-level response parameters for each brand (see Equation (2)). This set includes the intercept, responses to marketing communication for the focal drug and the competing drugs, the survey participation effects associated with the focal drug and the competing drugs, the interaction between survey participation and marketing communication, and physicians' inertia. The response parameters could vary across physicians depending on their prior experience. For example, in the context of the mere-measurement effect, Morwitz et al. (1993) found that at the aggregate level, consumers with product experience are less susceptible to the mere-measurement effect than those lacking experience. We take this into consideration and allow all of the seven individual-level response parameters ($\beta_{ij} = \{\beta_{ij0}, \beta_{ijd}, \beta_{ijdc}, \beta_{ijs}, \beta_{ijsc}, \beta_{ijds}, \beta_{ijl}\}$) to be influenced by physicians' experience with brand j. This is accomplished using a hierarchical Bayesian framework. We note that unlike prior studies that are concerned with the relationship between survey participation and consumers' prior experience at the category level (e.g., Fitzsimons and Morwitz 1996), we are able to relate the survey participation effects to consumers' experience with individual brands in a competitive context. We do this after accounting for the effects of marketing communication on consumers' choice behavior. In the hierarchical model, all the individual-physician-level response parameters are assumed to follow a multivariate normal distribution, with the mean of the prior distribution being influenced by the physician's experience with a particular brand j (denoted as W_{ij}). As there are seven parameters (denoted by $\{g = 1, \ldots, 7\}$) for each brand in our proposed model (see Equation (2)), the hierarchical model represents a multivariate regression with dimension $7 * J$ (where J represents the number of brands in the model). We denote the parameters from this hierarchical model as $\{\theta_{gj0}, \theta_{gj1}, j = 1, \ldots, J, g = 1, \ldots, 7\}$, where θ_{gj0} refers to the intercept for each of the seven parameters for brand j and θ_{gj1} refers to the set of coefficients associated with variable W_{ij}. Let us define $\theta_{0j} = \{\theta_{gj0}, g = 1, \ldots, 7\}$ and $\theta_{1j} = \{\theta_{gj1}, g = 1, \ldots, 7\}$. Given the above setup, the hierarchical model can be noted as follows:

$$\beta_{ij} = \theta_{0j} + \theta_{1j} W_{ij} + \mu_{ij}, \tag{3}$$

where β_{ij} (dimension 7 for each i) denotes the individual-level parameter vector associated with brand j (specified in Equation (2)), and θ_{0j} and θ_{1j} are the parameters to be estimated in the hierarchical framework, which are specific to each brand j. The experience variable W_{ij} is obtained by calculating the total number of new prescriptions written by each physician i for each brand j during the one-year period between January 2007 and December 2007. (Note that this period precedes the data period used in the model estimation.) We assume that the random error term μ_{ij} is distributed multivariate normal with zero mean and covariance matrix Σ_μ.

4.2. Firms' Detailing Allocation Behavior

As discussed in §3, pharmaceutical firms strategically set detailing levels at the individual physician level. To account for the possible endogeneity of detailing, we adopt the "heuristic" approach suggested by Manchanda et al. (2004) and applied by Montoya et al. (2010) and Dong et al. (2011). Following these studies, we specify a detailing model that we simultaneously estimate with the prescription model presented earlier. Similar to the prescription model, we use a multivariate Poisson-lognormal model to model the number of detailing calls that a physician receives. Accordingly, we assume the observed number of detailing visits for physician i and brand j in month t follows a Poisson model with mean parameter $\exp(\gamma_{ijt})$, which is specified as a log-link function as below:

$$\gamma_{ijt} = \alpha_{j0} + \alpha_{j1}\beta_{ij0} + \alpha_{j2}\beta_{ijd} + \alpha_{jz}Z_i + \xi_{ijt}, \qquad (4)$$

where α_{j0} represents the average detailing level of brand j. Besides α_{j0}, we specify γ_{ijt} as a linear function of two sets of physician-specific explanatory variables. The first set is based on the heuristic approach suggested in Manchanda et al. to capture pharmaceutical firms' nonrandom detailing allocation across physicians. It consists of individual-level parameters from the prescription model. We use the prescription model intercepts (β_{ij0}, capturing the individual-level intrinsic prescription rate) and the own detailing parameters (β_{ijd}, capturing individual-level responses to detailing stock). The second set of variables is the set of exclusion variables (Z_i) that help us identify the detailing model. Ideally, these exclusion variables would affect a firm's detailing effort targeted at individual physicians and not influence the prescription behavior of the physicians. For example, some reasonable cost shifters that vary the marginal cost of detailing across physicians may satisfy these requirements. In our data, a key piece of physician information that we have is the geographic location of their practices; the 503 physicians in our estimation data are in 468 different zip codes. We attempt to leverage the spatial variation in physicians' practices by complementing this information with several publicly available secondary data to construct such exclusion variables. Following the literature (such as Dong et al. 2009), we work with variables from the following five data sources at the zip code level: (1) demographic information from the census data (available at http://www.census.gov), which includes age, income, and ways of commuting among employed people in each zip code; (2) the number of hospitals and beds in each hospital in a zip code from the American Hospital Directory (available at http://www.ahd.com); (3) the physician's directory from IMS Health, from which we calculate the number of each type of physician in each zip code (such as primary care physicians and specialists in each field); (4) the average salary information for a professional salesperson for the different states available from the Bureau of Labor Statistics (http://www.bls.gov); and (5) the average monthly gasoline prices for each region (defined as a group of neighboring states) from the U.S. Energy Information Administration website (available at http://www.eia.gov). Among all these variables, we found that only two are useful in yielding statistically significant estimates: the number of physicians in the same zip code, constructed from the physician's directory, and the percentage of commuters who walk to work, obtained from the census data. From a pharmaceutical firm's perspective, it would be cheaper to target a physician whose practice is closer to many other physicians in the same zip code compared with a physician whose practice is relatively isolated. In this case, we expect a positive sign for the parameter associated with the total number of physicians in the physician-level detailing model. In a similar vein, if a physician's practice is located in a crowded downtown area, the cost of regularly visiting these physicians may be higher (after accounting for the number of physicians who practice in the same zip code) compared with visiting those physicians who practice in less crowded areas (for reasons such as transportation, congestion, and difficulty in parking). In our context, we use the percentage of commuters who walk to work as a proxy for the density of the zip codes that we work with. If this argument is correct, we would expect to see a negative sign for the parameter associated with this variable in the detailing model.

In Equation (4), ξ_{ijt} denotes other unobserved (to the econometricians) factors that might influence the outcome of the number of detailing visits a physician received from a pharmaceutical company during a month. We allow the error terms from both the prescription and the detailing models to be correlated.[10] In particular, we allow them to be distributed as a multivariate normal distribution with zero mean; i.e.,

$$\begin{bmatrix} \{\epsilon_{it}\} \\ \{\xi_{it}\} \end{bmatrix} \sim \mathrm{N}(0, \Sigma_{\epsilon\xi}).$$

The covariance matrix $\Sigma_{\epsilon\xi}$ has dimension $2J$, where J is the total number of brands in the model. In our empirical context, $J = 3$.

In estimating the model, we adopt a full information likelihood approach to obtain all the model parameters simultaneously from both prescription and detailing

[10] Given the survey administration rule in our context (where there is a survey of a drug after every fourth detailing of the drug, with the every fourth detailing rule applying across physicians), any factor that is potentially correlated with a survey that would lead to survey endogeneity is already captured by the correlation between the residuals of the prescription and the detailing allocation models.

models, and thus achieve efficiency in the parameter estimates. The full information likelihood function of all the parameters is given as follows:

$$\begin{aligned} &L(\{\beta_{ij}\}, \Sigma_\beta, \{\theta_0, \theta_1\}, \Sigma_\mu, \{\alpha_j\}, \Sigma_{\epsilon\xi} \mid rx_{ijt}, dtl_{ijt}, W_i, Z_i) \\ &\quad = \prod_{i,j,t} f(rx_{ijt} \mid rx_{ij,t-1}, \{dtl_{ijt}\}, \{\beta_{ij}\}, \epsilon_{ijt}) \\ &\qquad \times f(dtl_{ijt} \mid \beta_{ij0}, \beta_{ijd}, Z_i, \{\alpha_j\}, \xi_{ijt}) \\ &\qquad \times f(\{\beta_{ij}\} \mid \{\theta_{0j}, \theta_{1j}\}, W_{ij}, \Sigma_\mu) \times f(\{\epsilon_{ijt}, \xi_{ijt}\} \mid \Sigma_{\epsilon\xi}). \quad (5) \end{aligned}$$

The likelihood function contains four parts, as shown on the right-hand side of Equation (5). The first is from the (conditional) prescription model, which is a Poisson distribution conditional on the random error term $\{\epsilon_{ijt}\}$ and observed detailing and detailing stock variables, as specified in Equation (2). The second part is from the detailing model, which follows a Poisson distribution conditional on the random error term $\{\xi_{ijt}\}$, as specified in Equation (4). The third part is the multivariate normal likelihood, obtained from the hierarchical regression as specified in Equation (3). Finally, the fourth part is the multivariate normal likelihood for the joint distribution of the random error terms from both the prescription and detailing models; that is, $\{\epsilon_{ijt}, \xi_{ijt}\} \sim \mathrm{N}(0, \Sigma_{\epsilon\xi})$.

We use the Markov chain Monte Carlo (MCMC) method to estimate all the parameters ($\{\beta_{ij}\}$, $\{\alpha_j\}$, $\{\theta_0, \theta_1\}$, Σ_β, Σ_μ, $\Sigma_{\epsilon\xi}$) simultaneously. Diffuse but proper priors are employed in the estimation. In particular, the prior distribution for the parameters in the hierarchical model is $\{\theta_0, \theta_1\} \sim \mathrm{N}(0, 100I)$, where I is an identity matrix. This is similar to the prior distribution for the parameters in the detailing model, which is $\{\alpha\} \sim \mathrm{N}(0, 100I)$. The prior for the covariance matrix of the error terms is $\Sigma_{\epsilon\xi} \sim \mathrm{InvWishart}(8I, 8)$. We present and discuss the estimation results in the following section.

5. Results

We use the MCMC method to obtain all the individual-level parameters in the prescription model, as well as the parameters in the hierarchical model and the detailing model simultaneously. The MCMC chain was simulated for 50,000 draws, and the last 20,000 draws were used to obtain the parameter estimates. To ensure convergence, we tested our model with different starting values and inspected the time-series plots; we find that the MCMC chains are stable and converged to the same values. This suggests that our estimates do not suffer the "multimodality" problem that may arise when using a heuristic approach (Manchanda et al. 2004). Furthermore, we also performed a parameter recovery test, in which we simulated data according to our model and successfully recovered the model parameters. Below, we first present the results of our proposed model, followed by a discussion of comparisons between our proposed model and the various alternative models.

Table 2 **Mean Estimates of the Individual-Level Parameters for the Poisson Model**

Variable	Actos	Avandia	Januvia
Intercept	−1.3761	−2.9439	−1.9904
	(−1.49, −1.26)	(−3.08, −2.79)	(−2.09, −1.87)
ln(*dtl_stock*)	0.151	0.4386	0.395
	(0.08, 0.21)	(0.35, 0.52)	(0.34, 0.46)
ln(*cdtl_stock*)	0.0237	−0.1043	−0.0251
	(−0.01, 0.05)	(−0.13, −0.08)	(−0.05, −0.01)
srv_stock	0.2022	0.3046	0.2978
	(0.12, 0.26)	(0.19, 0.46)	(0.20, 0.40)
csrv_stock	−0.0495	−0.0628	−0.1361
	(−0.12, −0.00)	(−0.13, 0.05)	(−0.21, −0.08)
dtl_stock × *srv_stock*	−0.0832	−0.3796	−0.1431
	(−0.10, −0.06)	(−0.43, −0.28)	(−0.17, −0.11)
ln(*lagRx*)	0.2445	0.1313	0.2088
	(0.18, 0.31)	(0.05, 0.22)	(0.09, 0.31)

Note. The 95% posterior confidence intervals are shown in parentheses.

5.1. Results of the Prescription Model

The results for the mean estimates across individuals for our prescription model (β_i) are presented in Table 2, along with the 95% posterior confidence intervals for each parameter. The results show that, consistent with the extant literature, a focal brand's detailing effort positively influences physicians' prescription rates for all three brands. The effect sizes across the three drugs are slightly different, with physicians being most and least responsive (on average across all the physicians) to Avandia's and Actos' detailing efforts, respectively. The parameters of the response to competitive detailing are negative and statistically significant for two of the three drugs (insignificant for Actos).

The parameters related to the survey participation effects are positive and statistically significant for all three brands, which confirms the existence of the survey participation effect in a setting where consumers participate repeatedly in different brand-specific surveys. At this juncture, a point about the interpretation of the survey participation effect in our context needs special mention. Given the survey administration rule in our empirical setting—surveys are always preceded by detailing visits—one can also interpret the parameters associated with the survey participation effect as the amplification of detailing due to the survey participation; i.e., detailing that is followed by a survey has a greater effect than detailing meetings that are not followed by surveys.[11] With respect to the effects of participation in surveys of competing brands, we find that competitive effects are negative for all three

[11] We thank the associate editor for helping us with the interpretation.

brands, although they are only statistically significant for Actos and Januvia. This suggests that participation in competing brands' surveys can have a negative effect or, at best, no effect on physicians' choice of a focal drug. The presence of a negative spillover effect of the survey participation on competing brands is similar to a brand switching effect of the marketing-mix variables in a competitive setting.

The parameters for the interaction variable of detailing and surveys are negative and statistically significant for all three brands. We interpret this as follows: at higher levels of detailing, the effectiveness of the survey participation effect decreases (note that we already account for the cumulative and diminishing returns of detailing). This suggests a "backlash effect" between marketing communication efforts and survey participation. Williams et al. (2004) argued that when consumers sense a persuasive intent behind surveys, measuring intentions can backfire. In our context, although we do not directly manipulate the level of persuasive intent, repeated engagement in the form of detailing calls, coupled with survey participation, can lead to a perception of persuasion on the part of physicians. The negative interaction effect is also in conformance with the information overload explanation as a result of repeated exposure to the focal brand at multiple consumer touch points. When decision makers have no real opportunity to learn, repeated engagement could lead to boredom (Anand and Sternthal 1990) or irritation due to repeated reminders (van Diepen et al. 2009), which in turn can lead to a response against the repeated messages (Campbell and Keller 2003). Extant research has stressed the importance of understanding and planning for marketing-mix strategies in the presence of interaction effects in a general context (e.g., Naik et al. 2005) and in a pharmaceutical marketing context (e.g., Narayanan et al. 2004). Given the nature of our data, although we are unable to explicitly uncover the mechanism behind the negative interaction effect, our study is one of the first to establish the interaction effect between marketing communication and survey participation. The positive direct effects of marketing communication and survey participation, and the negative interaction effects, have theoretical and practical implications for resource allocation coordination. Ai and Norton (2003) cautioned that in the case of the nonlinear models, the interaction effect is not equal to the coefficient of the interaction term. We follow the suggestions put forth by Ai and Norton (2003) and Mallapragada et al. (2012) and discuss interpretation of the interaction effect, both at the aggregate level and at the individual physician level in §7. We refer readers to the Web appendix (available as supplemental material at http://dx.doi.org/10.1287/mksc.2014.0852) for visual analysis of the interaction effect (at the aggregate level). Finally, consistent with the current literature, we find that physicians' current prescription behaviors are positively influenced by their past prescription behaviors, as indicated by the parameters for the lagged prescriptions.

Table 3 **Hierarchical Regression Results on the Brand Experience Variable**

Variable	Actos	Avandia	Januvia
Intercept	0.1628 (0.11, 0.22)	0.0218 (−0.00, 0.05)	0.0866 (0.02, 0.16)
ln(*dtl_stock*)	0.4402 (0.34, 0.55)	0.012 (−0.02, 0.05)	0.0368 (0.02, 0.05)
ln(*cdtl_stock*)	0.2924 (0.16, 0.43)	0.0109 (−0.04, 0.07)	0.0711 (0.04, 0.11)
srv_stock	−0.0320 (−0.06, −0.00)	−0.0986 (−0.17, −0.03)	0.0107 (−0.03, 0.05)
csrv_stock	0.0471 (0.00, 0.10)	0.0131 (−0.08, 0.11)	0.0335 (0.00, 0.07)
dtl_stock × *srv_stock*	0.0854 (0.02, 0.15)	0.0365 (0.01, 0.06)	0.039 (−0.02, 0.10)
ln(*lagRx*)	0.0013 (−0.01, 0.02)	0.0483 (−0.02, 0.10)	0.0645 (−0.01, 0.14)

Note. The 95% posterior confidence intervals are shown in parentheses.

The estimates for the experience variable in the hierarchical model are presented in Table 3. The main takeaway is that physicians' brand experience has a significant negative effect on their responsiveness to survey participation for Actos and Avandia. In other words, physicians with more brand-level experience are less susceptible to the survey participation effect. This is similar to one of the findings by Fitzsimons and Morwitz (1996), who reported that current users of a brand are less susceptible to the survey participation effect (category-level surveys) when compared with nonusers. Finally, the correlation matrix associated with the additive error terms in the prescription model is shown in Table 4. The correlations among the three brands are all positive and statistically significant. The values are in the range of 0.59–0.73.

5.2. Results of the Detailing Allocation Model

The estimates for the detailing model are presented in Table 5. All the model parameters associated with the parameters from the prescription model are positive

Table 4 **Estimated Correlation Matrix for the Additive Error Term**

	Actos	Avandia	Januvia
Actos	1	0.59 (0.48, 0.67)	0.73 (0.66, 0.78)
Avandia		1	0.67 (0.60, 0.73)
Januvia			1

Table 5 **Estimation Results for the Detailing Model**

Variable	Actos	Avandia	Januvia
Detailing model intercept	−0.789 (−0.90, −0.65)	−1.1924 (−1.33, −1.08)	−0.9552 (−1.03, −0.89)
Prescription model intercept β_{0ij}	0.3166 (0.26, 0.39)	0.4034 (0.35, 0.45)	0.1763 (0.10, 0.26)
Own detailing effect in the prescription model β_{ijd}	1.9454 (1.89, 2.03)	1.6416 (1.59, 1.70)	1.4702 (1.39, 1.57)
ln(*Number of physicians in the same zip code*)	0.0281 (−0.02, 0.08)	0.0368 (0.01, 0.07)	−0.0066 (−0.04, 0.04)
Percentage of commuters who walk to work in each zip code	−0.0305 (−0.04, −0.02)	−0.0193 (−0.03, −0.00)	−0.0182 (−0.04, −0.00)

Note. The 95% posterior confidence intervals are shown in parentheses.

Table 6 **Model Fit**

Model no. and description	In-sample LML	In-sample log Bayes factors (cf. Model 5)	Holdout sample LL
1: Survey participation effect-only model	−11,859		−1,861
2: Detailing effect-only model	−11,773	−514	−1,855
3: Survey participation effect and detailing model	−11,672	−413	−1,868
4: Detailing and survey participation effect model with interaction effect	−11,570	−311	−1,564
5: Proposed model: Detailing and survey participation effect model with interaction effect and competitive detailing effects and competitive survey participation effects	−11,259	—	−1,562

Note. LL, log likelihood.

and statistically significant. For example, we find that physicians who have a higher intrinsic prescription rate for each brand (as captured by the brand-specific intercepts β_{0ij} in the prescription model) tend to receive more detailing visits from that brand's company. We also find that physicians who are more responsive to a particular brand's detailing visits (as captured by brand-specific detailing response parameters β_{ijd}) receive more detailing calls from that brand's company. These results hold for all three brands, and the findings are consistent with those reported by Manchanda et al. (2004). For the two exclusion variables, we find that the parameters for the natural log transformation of the number of physicians in the same zip code are positive for both Actos and Avandia but statistically significant only for Avandia. Finally, as expected, the parameters associated with crowdedness of physicians' practice (as captured by the percentage of commuters who walk to work in the zip code) are negative and statistically significant for all three brands.

5.3. Model Comparison

To benchmark the fit of our proposed model, and to assess the existence and magnitude of the survey participation effect in conjunction with marketing communication, we estimated five alternative models. Model 1, which we refer to as the "Survey participation effect-only model," accounts for the effect of survey participation pertinent to a focal brand and the lagged effects on physicians' prescription behavior. Model 2, which we refer to as the "Detailing effect-only model," accounts for the effect of detailing efforts of the focal brand and the lagged effects on physicians' prescription behavior. Model 3, which we refer to as the "Survey participation effect and detailing model," accounts for the main effects of survey participation and detailing efforts of the focal brand and the lagged effects on physicians' prescription behavior. Model 4, which we refer to as the "Survey participation effect and detailing with interaction model," accounts for the main effects of survey participation and detailing efforts of the focal brand, the interaction effect between these two activities, and the lagged effects of prescription behavior. Finally, Model 5 refers to our proposed model that accounts for all the factors included in Model 4, as well as the effects of physicians' survey participation in all the competing brands and the detailing efforts of all the competing brands. All the models account for physicians' unobserved heterogeneity. Models 2, 3, and 4 also account for detailing endogeneity similar to our proposed model, Model 5. We compare the model performances using both in-sample comparisons and holdout sample tests. The results are listed in Table 6.

The first column in Table 6 provides the in-sample log marginal likelihoods (LMLs) of all the models,[12] computed using the Newton–Raftery method (Rossi et al. 2006). Comparisons of the LMLs of Model 3 with Model 1 or Model 2 suggest that Model 3 has a better fit; this underlines the importance of accounting for the effects of both survey participation and marketing communication on the behavior of decision makers. This, in turn, lends empirical evidence to the idea that the underlying data generation process of physicians' prescription behavior is influenced by detailing and survey participation effects. Comparison of the LMLs of Models 3 and 4 suggests that Model 4 provides a better fit; this highlights the importance of accounting for the

[12] Models 2–5 account for the endogeneity of the detailing visits, and therefore, the corresponding likelihood values relate to both the prescription and detailing model components. When we compare the fit of these models to that of Model 1 (which does not account for detailing, and thus, there is no detailing model component), we calculate the LML using only the likelihood value from the prescription model component.

interaction effect between marketing communication and survey participation. The second column in Table 6 presents the in-sample natural log transformation of the Bayes factors for each alternative model when compared with the proposed model, Model 5. All the values in the column are negative, which suggests that the proposed model has the best fit when compared with the four alternative models; this highlights the importance of taking into account the effects of physicians' participation in surveys of the competing brands and the effect of competitive marketing activities.

We also compared the fit of our proposed model against the four alternative models using a holdout sample. We used the observations that pertain to the last two months of the data period for each physician as the holdout sample. Using the individual-level parameters obtained from the estimation sample, we calculated the log-likelihood of the observed number of prescriptions for the holdout sample. The model with the best fit is expected to provide a higher log-likelihood value for the holdout sample. These values are reported in the last column in Table 6. We find that, consistent with the in-sample model comparison result, the proposed model provides the best fit in the holdout sample among all models.[13]

6. Robustness Checks

We performed several tests to make sure that our results are robust to various model specifications and alternative explanations. First, with respect to the functional form of the detailing variables, we estimated a model that incorporates both a linear and a quadratic term for the detailing stock variable. The estimation result is substantively similar to our proposed model, in that we also find statistically significant positive main effects of survey participation and negative interaction effects between survey participation and detailing for all three brands. However, prior literature suggests that polynomial transformation of marketing-mix variables should be avoided as it exhibits supersaturation and can lead to disturbing optimization results (Saunders 1987, Doyle and Saunders 1990). Similar arguments were made by Bass et al. (2007) in their study of the wearout effects of different advertising themes. Following these studies, in our main model, we retain the natural log transformation as opposed to the polynomial terms to account for the diminishing returns of detailing.

Second, with respect to our core finding of a negative interaction effect between survey participation and detailing, one could argue that given that detailing calls always precede surveys (as a result of the fourth detailing rule), the negative interaction effect could be due to a "salesperson evaluation effect," whereby physicians are reminded of an "annoying" sales representative they encountered in the most recent detailing meeting, which could decrease their prescription rate for the focal brand.[14] We first note that our sample of physicians, in general, does not find sales representatives annoying; the panel of physicians rated the salespersons to be at the top or middle tier in the majority of the surveys (97%). To rule out the salesperson evaluation effect explanation, we split the data based on the valence of the survey questions on the salesperson and reestimated the model only with the set of physicians who rated their sales representatives in the top tier.[15] If the negative interaction effect results from low evaluations of the sales representatives, then we would not expect the estimates for the high-tier sales representatives to be negative. However, we find that the significant negative interaction effect between the detailing stock and the survey stock persists for these physicians. This exercise presents evidence that the negative interaction effect between the survey participation effect and detailing is not driven solely by annoyance with the sales representative but is also due to the backlash effect that we propose between detailing and survey participation.

Third, in our hierarchical regression model (as presented in Equation (3)), instead of using brand-specific experience, we estimated our proposed model with category-specific experience and found that our results are substantively similar. We then compared the goodness of fit between these two models using the method of Newton and Raftery (1994). We find that the model with brand-specific experience fits the data better than the model with category-specific experience in both the in-sample and holdout-sample tests.

Fourth, previous research in the area of mere-measurement effect has shown that repeated intent measurement leads to behavior polarization (Morwitz

[13] Ebbes et al. (2011) showed that holdout sample validation favors regression estimates that are not corrected for endogeneity over estimates that are corrected for endogeneity. Following this finding, we correct for endogeneity of detailing in Models 2–5 so that our model comparison results are not subject to the concerns raised in their study.

[14] We thank an anonymous reviewer for raising this issue.

[15] For the completeness of the analysis, we also reestimated the model with two other groups of physicians: those who rated their sales representatives in the middle tier and those who rated their sales reps in the bottom tier. We do this for each brand separately to obtain the largest sample possible for each physician group. Therefore, the data are divided into nine subsets (i.e., three brands crossed with three levels of the sales rep evaluation). We find that the interaction parameter is negative for all nine subsets—they are all statistically significant, except for two cases for Actos (when the sales representatives are rated to be in the middle or bottom tier). The number of physician-month observations is very small in these two cases—53 and 740, respectively—compared with the 12,072 total observations in the estimation sample.

et al. 1993). The authors argue that survey participation related to a product can have a positive effect on subjects who have a positive preference for the product and a negative effect on those who do not like the product. To test whether this hypothesis holds in our data, we chose physicians who had at least one negative (i.e., rated below 4 on a 1–7 Likert scale) response to the intention question and reestimated the model with this group of physicians, 70 of whom are included in our sample. We find a negative effect of survey participation for two of the three brands, although the effect is not statistically significant.[16] We also reestimated the model with the remaining physicians in our sample (those who always had a positive response to the intention question) and found a statistically significant positive effect of survey participation for all three brands. These findings are consistent with the behavioral polarization hypothesis that repeated measures of intent leads to an increased purchase rate (prescription rate in our context) for those with high level of intent and decreased purchase rate for those with low levels of intent.

Finally, it is a common practice in the pharmaceutical marketing literature to add "1" before taking the natural log transformation of the detailing variable. This practice can be traced back to Snedecor and Cochran (1967, p. 329), where they stated that "if some 0 values of x occur, $\log(x+1)$ is often used." However, to ensure that our results are robust, we reestimated our model using $+0.10$ before taking the natural log transformation. The results of the model are substantively similar to that of the proposed model.

7. Managerial Implications

Given our results of a statistically significant effect for both detailing and survey participation on physicians' prescription behavior, it is important for us to further examine the size of the effects of the two. To do this, we conducted a series of computation and counterfactual exercises. First, we compute the short-term elasticity of the detailing visits and the survey participation stocks. By *short-term elasticity* we refer to the percentage change in the current-month prescription rates for every percentage change in the detailing visits or the survey participation stocks. We calculate the elasticities using the individual-level parameters at each of the MCMC steps, with all of the necessary variables (such as the level of detailing and the survey stock variables) taking the values in the data. The individual-level elasticities are then averaged across individuals for each MCMC draw. The posterior means of these average elasticities are presented in Table 7, together with the 95% posterior confidence intervals listed in parentheses. This approach, which is well accepted in the literature (see, e.g., Rossi et al. 1996), captures the full stochasticity in the model parameters for each individual by leveraging the outputs from the MCMC simulation. Inspection of the elasticities reveals that the detailing elasticities are in the same range as reported in the previous literature (e.g., Narayanan et al. 2005, Albers et al. 2010). The survey participation elasticities are smaller than the detailing elasticities, and the values are in the range of 33%–54% of detailing elasticity for the same brand. To the best of our knowledge, our paper is the first to quantify the elasticity of the survey participation effect and compare it to the elasticity of marketing communication.

Table 7 Elasticity of Survey Participation and Detailing

	Actos	Avandia	Januvia
Detailing elasticity	0.0532	0.2068	0.1229
	(0.01, 0.10)	(0.17, 0.26)	(0.07, 0.19)
Survey elasticity	0.0285	0.0873	0.0403
	(0.00, 0.06)	(0.06, 0.11)	(0.01, 0.07)

Note. The 95% posterior confidence intervals are shown in parentheses.

Second, the positive main effects of marketing communication and survey participation indicate that increasing either activity would lead to a higher prescription rate for a brand. However, this is not always true in our empirical context because of the negative interaction effect between these two activities. The negative interaction effect implies that excessive surveying (detailing) can counteract the positive main effects of detailing efforts (survey participation) and therefore lead to a decrease in a physician's prescription rate. More specifically, when the levels of detailing and survey participation stocks are both low, the positive main effects of these two activities (i.e., β_{ijd} *and* $\beta_{ijs} > 0$) dominate; thus an additional detailing visit or survey participation would lead to an increase in the prescription rate. When either the detailing or the survey participation stock value is too high, the negative interaction effect ($\beta_{ijds} < 0$) comes into play. In such a case, any additional detailing visits or survey participations would lead to a lower prescription rate. We find that the interaction effect is negative for 70%–80% of the observations across the three brands in our study (Actos: 70.3%, Avandia: 80.3%, and Januvia: 71.2%). We refer readers to the Web appendix for details on how we calculate the interaction effect at the individual physician level.

To demonstrate this, we derive the equation to calculate the elasticity of the detailing stock variable on the expected number of new prescriptions for a given month at the individual physician level. We then

[16] One of the reasons that we did not find a significant negative survey participation effect for physicians with low levels of intent could be due to the small sample size (the number of physicians with low levels of intent).

compute the threshold detailing level at which the return on marginal detailing changes from positive to negative. The elasticity of detailing stock on expected prescriptions is given by

$$\frac{\partial E(rx_{ijt})/E(rx_{ijt})}{\partial(dtl\_stock_{ijt})/dtl\_stock_{ijt}} = \frac{\partial \ln(E(rx_{ijt}))}{\partial \ln(dtl\_stock_{ijt})} = \beta_{ijd} + \beta_{ijds} srv\_stock_{ijt} \times dtl\_stock_{ijt}, \quad (6)$$

where $E(rx_{ijt})$ is the expected number of prescriptions, and the other parameters and variables are the same as defined in Equation (2). According to this equation, if $srv\_stock_{ijt} = 0$, the elasticity of detailing stock reduces to β_{ijd}, which is the same as in a model not considering the survey participation effects. The effect of srv_stock_{ijt} on the elasticity of dtl_stock_{ijt} only shows up when both of these variables are nonzero. In this case, the elasticity could be positive or negative, depending on the values of dtl_stock_{ijt} and srv_stock_{ijt}, and the two model parameters β_{ijd} and β_{ijds}. Setting the right-hand side of Equation (6) to 0, when $dtl\_stock_{ijt} > 0$ and $srv\_stock_{ijt} > 0$, we can get the *critical* value of the detailing stock $dtl\_stock^*_{ijt}$:

$$dtl\_stock^*_{ijt} = \frac{-\beta_{ijd}}{\beta_{ijds} \times srv\_stock_{ijt}}, \quad dtl\_stock_{ijt} > 0 \text{ and } srv\_stock_{ijt} > 0. \quad (7)$$

The value of $dtl\_stock^*_{ijt}$ is the threshold level of the detailing stock variable above which conducting any additional detailing would lead to *negative* returns due to the presence of the backlash effect. Note that this threshold level depends on the ratio of the size of the detailing effect, the size of the interaction effect, and the value of the survey stock variable. Given that these measures are all individual physician specific, and the survey stock variable also varies across time periods, $dtl\_stock^*_{ijt}$ is specific to each physician-month observation.

Similarly, we can also calculate the elasticity of the survey stock on the expected prescriptions as follows:

$$\frac{\partial E(rx_{ijt})/E(rx_{ijt})}{\partial(srv\_stock_{ijt})/srv\_stock_{ijt}} = \frac{\partial \ln(E(rx_{ijt}))}{\partial \ln(srv\_stock_{ijt})} = (\beta_{ijs} + \beta_{ijds} dtl\_stock_{ijt}) \times srv\_stock_{ijt}. \quad (8)$$

Based on Equation (8), if physician i has not yet participated in any survey related to brand j up to month t (i.e., $srv\_stock_{ijt} = 0$), then the survey elasticity for the expected prescription is 0. However, if the physician has already taken at least one survey from the brand (i.e., $srv\_stock_{ijt} > 0$), then the survey elasticity for an individual physician could be either positive or negative, depending on how many *detailing* visits the physician has received so far (i.e., the value of dtl_stock_{ijt}). Setting the right-hand side of Equation (8) to 0, given that the population-level mean estimate of β_{ijds} is negative, we obtain

$$\frac{\partial \ln(E(rx_{ijt}))}{\partial \ln(srv\_stock_{ijt})} < 0 \quad \text{when } dtl\_stock_{ijt} > \frac{-\beta_{ijs}}{\beta_{ijds}}, \ \beta_{ijds} < 0, \text{ and } srv\_stock_{ijt} > 0. \quad (9)$$

Combining Equations (7) and (9), we obtain the following four conditions:

(i) $\beta_{ijds} < 0$.
(ii) $srv\_stock_{ijt} > 0$.
(iii) $dtl\_stock_{ijt} > -\beta_{ijd}/(\beta_{ijds} srv\_stock_{ijt})$.
(iv) $dtl\_stock_{ijt} > -\beta_{ijs}/\beta_{ijds}$.

When all conditions are satisfied, an additional detailing visit or survey participation would lead to a lower level of new prescriptions written for the current month. If only the first three conditions are satisfied, then an additional detailing visit would decrease, but an additional survey would still increase, the expected new prescription rate for the current month. Similarly, if only conditions (i), (ii), and (iv) are satisfied, then an additional survey would decrease, but an additional detailing visit would increase the expected new prescription rate for the current month.

Using the posterior draws of the individual-level parameters β_{ijd} and β_{ijds}, and the observed values of srv_stock_{ijt}, we determined which of the four conditions is satisfied for each of the physician-months. We then calculated the percentage of observations that satisfy all four conditions; only conditions (i), (ii), and (iii); and only conditions (i), (ii), and (iv) in Table 8. The first row reports the percentage of physician-month observations where the detailing and survey elasticity are both negative. These physicians should not receive

Table 8 Percentage of Observations that Satisfies the Conditions in Equations (7) and (9)

	Actos (%)	Avandia (%)	Januvia (%)
Both detailing and survey elasticities are negative (satisfies conditions i–iv)	14.8	8.5	16.3
Only detailing has negative elasticity (satisfies conditions i, ii, and iii only)	12.1	8.0	13.2
Only survey has negative elasticity (satisfies conditions i, ii, and iv only)	16.8	17.1	19.8

any detailing visits or surveys during these selected months. The second row reports the percentage of observations for which only the detailing elasticity is negative, and the third row reports the percentage of observations for which only the survey elasticity is negative.

Adding the first two rows of Table 8 would give us the percentage of observations for which the detailing elasticity is negative: 26.9% for Actos, 16.5% for Avandia, and 29.5% for Januvia. Therefore, these physicians should not receive any additional detailing visits from the respective brand company during the selected months. If these physicians were to receive one additional detailing visit from the respective brand company during the selected months, then their new prescription rate for the current month would decrease by 21.4% for Actos, 21.3% for Avandia, and 18.8% for Januvia. Similarly, by combining the first and the third rows, we obtain the percentage of observations for which the survey elasticity is negative: 31.6% for Actos, 25.6% for Avandia, and 36.1% for Januvia. To avoid the negative consequence of the backlash effect, these physicians should not participate in any more surveys for the respective brand during the selected months. Having these physicians participate in one additional survey for the respective brand would lead to a 28.8%, 34.7%, and 29.2% reduction in the current month's new prescription rate for Actos, Avandia, and Januvia, respectively. This exercise highlights the importance of coordinating marketing communication efforts with the administration of surveys when targeting individual physicians.[17] This insight is particularly important given that marketing communication activities and customer surveys are traditionally viewed as two entirely separate and independent functions, and as a result, they are handled by different divisions in most companies.

8. Conclusion, Limitations, and Directions for Future Research

Our study complements the limited set of studies that use market data to examine the survey participation effect. Unlike most of the extant studies that have focused on the effect of survey participation in the context of nonroutine behaviors, our study examines the presence of the survey participation effect in a stable context, where habitual responses on the part of decision makers have been established (Wood et al. 2005). We estimate the effect of survey participation after accounting for the effect of the individually targeted marketing communication efforts for all the competing brands. Using a panel data set of physicians' prescriptions of competing drugs in a therapeutic category, individual physician-level marketing communication data, and physicians' participation in brand-specific surveys for all the competing branded drugs, we demonstrated the existence of a negative interaction between survey participation and marketing communication in a field setting.

Given the positive main effects of survey participation and marketing communication, and the negative interaction effect between the two, we then performed simulations using our individual-level estimates of physicians' response to detailing and survey participation to obtain the detailing threshold levels at which conducting any additional surveys or detailing visits would lead to negative returns. Operating outside the regime of these threshold levels can only hurt the brand. Given our context, where targeted marketing communication policies are often followed, our results and simulation exercises help highlight the importance of coordination between marketing communication and market research activities. Our study also helps confirm the mechanism by which the survey participation effect operates. As shown in the behavioral literature, survey participation helps consumers easily access the brand-level or product-level cognition that they would not otherwise access. In a separate stream of research that relates to product-related cognition (Alba et al. 1991), it was documented that access to cognition related to a product can make it more difficult for consumers to access cognition related to competing products. Taken together, these arguments would suggest that, in the context of the survey participation effect, participation in competing surveys can lead to brand switching. Our results that two of the three parameters related to competing brands' survey participation are negative suggest evidence of brand switching as a result of participation in competing brands' surveys, which in turn supports the argument that the mere-measurement effect stems from easier access to brand-related cognition. Our study documents this, albeit indirectly, by using actual behavioral data.

Our study makes the above contributions, but it is certainly not without its limitations. Although we demonstrate the existence of the survey participation effect in a field setting where consumers participate in surveys from competing brands, we are unable to identify the specific survey questions that drive the survey participation effect. This limitation is common with other studies that use market data to examine the survey participation effect. Future research can manipulate the design of survey questions to examine the drivers of the survey participation effect. Given

[17] We acknowledge that in our empirical context, detailing visits and surveying are performed by two different parties; pharmaceutical firms set the detailing levels, whereas the marketing research company conducts surveys. Therefore, the coordination requires joint efforts from both parties. However, in many other cases, firms can easily coordinate these two activities.

that surveys are always preceded by marketing communication in our empirical setting, we are unable to disentangle the extent to which the detailing enhancement effect contributes to the main effect of survey participation. Future studies could explicitly examine the nuances of the effect of marketing communication messages and specific responses to a given survey on consumers' subsequent behavior. Firms often practice an umbrella branding strategy in which products carry the same brand name across different product categories. Future research can look into the spillover effects of survey participation across different products of a particular brand. In our data, we had a 100% participation rate due to contractual obligations on the part of the decision makers. We argued that since the participation decision in our context is not related to brand preferences, self-selection is not an issue. Future studies could explicitly study the decisions of consumers to participate in surveys administered by manufacturers/retailers and measure the effect of survey participation using a controlled field experiment or study the effect of survey participation accounting for the self-selection issue. Despite these limitations, we hope that our study offers new insights and spurs more research on this interesting behavioral phenomenon.

Supplemental Material

Supplemental material to this paper is available at http://dx.doi.org/10.1287/mksc.2014.0852.

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