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Are Physicians "Easy Marks"? Quantifying the Effects of Detailing and Sampling on New Prescriptions

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Much public attention and considerable controversy surround pharmaceutical marketing practices and their impact on physicians. However, views on the matter have largely been shaped by anecdotal evidence or results from analyses with insufficient controls. Making use of a dynamic fixed-effects distributed lag regression model, we empirically assess the role that two central components of pharmaceutical marketing practices (namely, detailing and sampling) have on physician prescribing behavior. Key differentiating features of our model include its ability to (i) capture persistence in the prescribing process and decompose it into own-growth and competitive-stealing effects, (ii) estimate an unrestricted decay structure of the promotional effects over time, and (iii) control for physician-specific effects that, if not taken into account, induce biased coefficient estimates of detailing and sampling effects. Based on pooled time series cross-sectional data involving three drugs, 24 monthly observations, and 74,075 individual physicians (more than 2 million observations in total), we find that detailing and free drug samples have positive and statistically significant effects on the number of new prescriptions issued by a physician. However, we find that the magnitudes of the effects are modest.

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Introduction

As the cost of prescription drugs continues to escalate, increased public attention is being focused on the marketing practices of the pharmaceutical firms as one source of the problem. Direct-to-physician activities account for the bulk of U.S. pharmaceutical firm promotional spending. IMS Health (2003) estimates that over $5.8 billion was spent in 2002 on detailing, i.e., pharmaceutical sales representatives (PSRs) visiting physicians to promote their firm's drugs. In addition, the retail value of the free drug samples distributed during these visits is estimated at $11.5 billion.

A detailing visit typically lasts two to five minutes during which time a PSR discusses one to three of the company's drugs. Information (and, at times, misinformation) about a drug's composition, therapeutic value, proper dosage, and potential side effects is communicated (Zigler et al. 1995). Often, PSRs will also dispense samples and possibly offer small gifts to the physician. At issue is whether these interactions with PSRs compromise physician integrity and affect their prescribing behavior. More precisely, the key public policy issue is the extent to which the industry's promotional tactics lead to an increase in appropriate versus inappropriate use of drugs in a cost-effective manner.

Concern that pharmaceutical marketing practices have exacerbated increases in public health costs has prompted government actions at the federal and state levels. For example, in 2002 the federal government issued a warning to the drug industry to curtail some of their marketing practices (*Washington Post* 2002). H.R. 2356, which calls for ongoing annual funding of $75 million to conduct comparative cost-effectiveness drug studies, was introduced in Congress in June 2003. A primary intent of this legislation is to provide objective scientific evidence to "reduce doctors' reliance on marketing information from the pharmaceutical industry" (Pear 2003). Given the fact that one of every five dollars spent on pharmaceutical drugs in the United States is paid for by a state program, state governments have also taken steps to counter PSR influence. Most notably, several states have undertaken counterdetailing initiatives (Gold 2001). State employees visit physicians in hopes of persuading them to switch from prescribing branded drugs to prescribing lower-cost generic drugs.

Prescription drug expenditures are projected to remain the fastest-growing sector of health care expenditures. They are expected to account for 14.5% of $3.1 trillion health care expenditures by 2012 (compared with approximately 10% in 2001). With recent legislation providing a Medicare drug benefit expected to cost the federal government $534 billion over the next decade, it is no wonder that the impact of pharmaceutical industry marketing practices is of keen interest to policymakers, the business community, and the general public.

Two competing views have dominated discussion on the matter. The prevailing view contends that PSRs significantly influence physicians' prescribing behavior and that this influence has negative effect on patients' welfare, in that PSRs encourage physicians to prescribe more expensive branded drugs. Many public policy organizations and consumer advocacy groups adhere to this view (see, for example, www.nofreelunch.org). The prominent alternative view argues that PSRs do influence physicians' prescribing behavior, but that this influence is positive in that PSRs provide physicians with valuable information. As a result, physicians are better informed and make better choices for their patients. Pharmaceutical companies and industry groups advocate this second view.

Despite the substantial resources that pharmaceutical companies invest in promoting their products and the controversy associated with pharmaceutical marketing practices, surprisingly little is known about the magnitude of the impact that PSR visits and free drug samples have on physician prescribing behavior. Narayanan et al. (2003) report a pharmaceutical executive as stating, "No one is really sure if sending the legions of reps to doctors' offices really works. Everyone is afraid to stop it, because they don't know what difference it's making" (p. 4).

In point of fact, much of the evidence on PSR effectiveness is anecdotal. The empirical studies investigating the issue have been subject to data or methodological limitations that restricted their ability to control for potential biases and have come to contradictory conclusions regarding even the central issues: the effects of detailing on prescriptions (e.g., Parsons and Abeele 1981 versus Gonul et al. 2001), of detailing on price elasticity (e.g., Rizzo 1999 versus Gonul et al. 2001), and even of price on sales (e.g., Rizzo 1999 versus Gonul et al. 2001).

We have obtained access to a unique database that allows us to undertake econometric analysis that overcomes a number of fundamental limitations existing in past research. In particular, making use of a dynamic fixed-effects distributed lag model that accounts for physician-specific effects likely to induce bias if left uncontrolled, we assess the effect of detailing and sampling on physician prescribing behavior. The large number of observations in the database (it involves a total of more than 2 million observations) allows us to accurately pinpoint the impact that interactions with PSRs have on the number of new prescriptions issued by physicians.

We find that, although detailing and free drug samples have a positive and statistically significant association with the number of new prescriptions issued by a physician, the magnitudes of the effects are modest. As such, our results challenge the two dominant views and support the contention that, rather than being easy marks, physicians are tough sells. This realization is important because the public policy debate continues over how best to address the high cost of prescription drugs.

PSR Influence on Physicians

Most discussions of PSRs have focused on the factors facilitating their influence. Unquestionably, PSRs provide physicians with information about new drugs, new indications, dosages, and interactions for existing medicines. Azoulay (2002) finds evidence that detailing diffuses product information. Avorn et al. (1982) report that 20% of surveyed physicians view information provided by PSRs as "very important" in influencing their prescribing behavior. Furthermore, PSRs are trained in persuading physicians. Detailing takes the form of presenting facts and, as has been documented (Zigler et al. 1995), misrepresenting facts about the drug in an effective manner. Finally, mere exposure or salience effects might lead to a temporary increase in prescribing following a PSR visit. Numerous studies have reported high physician responsiveness to PSR activity attributed it to PSR persuasiveness (Avorn et al. 1982, Powers 1998).

Less attention has been paid to the factors limiting PSR effectiveness. The key consideration here is that PSRs are not the only or even the primary source of information about drugs for physicians. Scientific papers, advice from colleagues, and a physician's own training and experience also influence prescribing practices. Indeed, most physicians view these other influences as far more important than that of PSRs (Peay and Peay 1990).

PSR influence is limited by the fact that many physicians have skeptical or negative attitudes toward PSRs (Lichstein et al. 1992, McKinley et al. 1990). Attribution theory suggests that with low source credibility, which is determined by factors such as a source's trustworthiness and expertise (Dholakia and Sternthal 1977), arguments in a message will be discounted (Eagley and Chaiken 1975). Physicians recognize that PSRs are neither experts nor com-

pletely trustworthy. They realize that information presented is biased toward the promoted drug and is unlikely to be objective or even accurate (Connelly et al. 1990). Thus, physicians will discount information received from a PSR.

Some additional characteristics of physicians would seem to make them particularly tough sells. Friestad and Wright's (1994) persuasion knowledge model suggests that targets of persuasion use their knowledge about the persuasion agent and can effectively cope with and even achieve their own goals during a persuasion attempt, e.g., obtaining free drug samples that can be later distributed to patients. Campbell and Kirmani's (2000) tests of the persuasion knowledge model reveal that busy targets with accessible agent motivation (a profile that would fit most physicians) are particularly effective in resisting persuasion.

When cast within the workings of other sources of influence, we would expect the ability of PSRs to influence physician behavior to be relatively small. As such, we hypothesize a relatively small effect of PSR activity on physician prescribing behavior.

Previous Empirical Research

The various studies assessing the effect of PSR activity on physician prescribing behavior have generated conflicting results. Indeed, on some of the most central issues—ranging from the effects of detailing on prescriptions, of detailing on price elasticity, and even of price on sales—studies have come to diametrically opposite conclusions. Data and methodological limitations, however, raise concerns about the inferences drawn from these analyses.

A few quasi-experimental studies of the issue originate in the medical community. These studies compare physicians who did not see PSRs or were visited less frequently by PSRs to physicians who saw PSRs or were visited more frequently by PSRs (Chren and Landefeld 1994, Powers 1998). The limitation of these studies is that they are not randomized: PSRs do not determine which physicians to visit on a random basis. Rather, PSRs tend to see physicians who are more likely to utilize the drug or who prescribe in higher volume. This consideration invalidates these attempts to assess the effect of PSRs independent of controls accounting for motivation influencing PSR behavior.

The ability to potentially control for other influences is an advantage of regression-based analysis. Past research has made use of different regression techniques to assess PSR influence. Unfortunately, it has been inadequate in controlling for physician-specific effects. Parsons and Abeele (1981) use data for 24 months and 14 territories to model the number of prescriptions sold in a given territory for a given month as a function of sales calls. Interestingly, the estimated main effect of detailing was negative. The most dominant explanatory factor in the model is sales lagged one period, which would reflect persistence in behaviors and carryover effects magnifying the influence of detailing. Alternatively, lagged sales could be reflective of territory-specific effects that are not modeled and, as such, could lead to biased estimates. Wotruba (1982), for example, raises this possibility of territory-specific effects to question the reported effects of detailing.

Rizzo (1999) uses annual data for the period 1988–1993 and for 46 drugs to estimate a brand-level model linking prescriptions for a drug for a given year to pharmaceutical company marketing activities. He finds that price is negatively related to sales and that detailing is anticompetitive in that it decreases price sensitivity. Detailing is also found to have a direct positive effect on sales. Surprisingly, no consideration is given to the dynamic properties of sales. The classic spurious regression characteristics, i.e., very high R^2 in the presence of substantial unmodeled autocorrelation, appear to be present. As such, questions exist about the validity of both the point estimates and standard errors reported in the analysis.

Gonul et al. (2001) use data involving 1,785 patient visits to estimate a multinomial logit model assessing factors influencing physician prescribing behavior. Exactly opposite to the findings of Rizzo (1999), they report that price has a positive effect on prescription probabilities and that detailing increases price sensitivity. They find positive effects of detailing and sampling, but do not discuss the implications of their magnitudes. These magnitudes, calculated based on descriptive statistics, imply elasticities that are surprisingly large.[1] The elasticity estimates for the seven drugs studied, evaluated at the mean level of detailing and sampling, average 41% for detailing and 48% for sampling. Particularly for samples, which have a negligible marginal cost, their estimated coefficients imply enormous returns to enhanced PSR activity. In point of fact, these substantial effects could arise, not from the influence of PSR activity, but rather as an outgrowth of a joint correlation with an omitted factor from the model, e.g., larger practices prescribe more and receive more free samples.

A concern, which Gonul et al. (2001) explicitly acknowledge, is over the role of physician-specific effects that can induce a bias in the estimated coefficients. They state (p. 84),

> prescription behavior patterns might be strongly influenced by factors other than the explanatory variables we include in our model. Examples are physi-

[1] The elasticity of prescription probability P_j to covariate x_{jk} in a conditional logit model is calculated as $(\partial P_j/P_j)/(\partial x_{jk}/x_{jk}) = \beta_k * x_{jk} * (1 - P_j)$.

> cians' unobservable personal characteristics . . . . Ignoring these factors might bias the coefficients of the included explanatory variables.

The extent to which their estimates are biased by the failure to control for unobservable factors remains unanswered, but this is one consideration that might account for the large estimated effects.

Empirical Analysis

A key benefit of utilizing pooled time series cross-sectional (panel) data is the ability to test for and control the effect of unobserved fixed factors. These unobserved factors, if left uncontrolled, can induce bias in the coefficient estimates of the explanatory factors included in the model. Past research has either not used panel data or not made full use of the benefits of panel data analysis. We make use of pooled time series cross-section observations (24 months of observations across 74,075 physicians) and panel data statistical methods (i.e., a dynamic fixed-effects distributed lag regression model) to assess the effect of detailing and sampling on physician prescribing behavior.

Data

Access to the data was gained from a U.S. pharmaceutical manufacturer with the only condition of ensuring the anonymity of the firm and the drugs in the study. Two different sets of data were merged to form the database. One data set pertains to the number of new prescriptions for the studied drugs and their competitors issued by physicians during a month. The *new prescription* measure reflects both new and repeat usage, but does not reflect refills accompanying the prescriptions. These data cover a 24-month period for three widely prescribed drugs. The second data set pertains to detailing and sampling activity by PSRs for the same three drugs. The two data sets were merged into one database containing prescribing and promotional activity information by month and physician.

To reduce the possible influence of extreme values (outliers) that would arise from, for example, data entry errors and the common practice of one physician signing for all samples that later get distributed to a group of physicians attending a conference, we excluded the top 0.5% of observations for the number of details, samples, and new prescriptions. We later undertook sensitivity analysis on alternative definitions of outliers (e.g., 0%, 1%, 5%) and found results in close correspondence across these alternative samples.

Table 1 presents basic background information and descriptive statistics for the drugs included in our study. The drugs differ on a variety of dimensions: They have been on the market from less than 1 year to 11 years; annual sales range from under \$0.5 billion to more than \$1 billion; they come from different therapeutic areas. Although the effect of detailing can vary across drugs, analysis of these three drugs offers some generalizable insights, not only because they provide a cross-section of drugs in the marketplace, but because they represent more than 4 million PSR interactions with physicians.

Model

We employ the following dynamic fixed-effects distributed lag regression model to assess the effect of detailing and sampling on new prescriptions:

$$\begin{aligned} Prescribe_{it} &= \alpha_i + \sum_{j=0}^{6} \beta_j * Details_{it-j} + \sum_{j=0}^{6} \gamma_j * Samples_{it-j} \\ &\quad + \sum_{j=0}^{6} \lambda_j * Competitor_{it-j} + \sum_{j=1}^{6} \phi_j * Prescribe_{it-j} \\ &\quad + \sum_{\tau=1}^{T} \delta_\tau * Time(\tau) + \sum_{s=1}^{11} \kappa_S * Specialty(s) * Trend_t \\ &\quad + \varepsilon_{it}, \end{aligned} \tag{1}$$

where $Prescribe_{it}$, $Details_{it}$, $Samples_{it}$, and $Competitor_{it}$ are, respectively, the number of new prescriptions issued, the number of PSR visits, the number of free drug samples received, and the number of new prescriptions issued for competitive drugs by physician i at time period t. $Time(\tau)$ is an indicator function that takes on the value 0 prior to the time period τ and 1 from the time period τ on, $Specialty(s)$ is an indicator function that takes on the value 1 when the specialty area of the physician is s, 0 otherwise (i.e., separate dummy variables for each of the 11 specialty areas), and Trend is the observation number for a given month and year. Because it includes both current-term and lagged variables in the model, Equation (1) allows for a wide range of possible effects and influences, e.g., serial correlation (current-effects) and state-dependent (persisting) dynamic relationships.

A key characteristic of Equation (1) is that it allows for a physician-specific effect, i.e., the intercept α_i is allowed to vary by physician. This consideration acknowledges that physician behavior patterns are influenced by unobserved or unobservable factors, e.g., physician characteristics. To the extent that these unobserved factors are correlated with detailing and sampling, analysis not controlling for their effects will result in biased estimated effects for the marketing phenomena. Although a Hausman (1978) specification test can empirically assess the role played by fixed effects, we have a priori reason to believe that these unobserved factors will in fact be correlated with marketing activity. For instance, larger practices will generate more prescriptions and will also attract more

Table 1 Drug Profiles

	Drug A	Drug B	Drug C
Sales range (US$)	0.5 to 1 billion	over 1 billion	under 0.5 billion
Time on the market at the beginning of the study period	3 years	11 years	6 months
Estimated number of competitors in the respective therapeutic area	12	18	11
Mean number of details per physician per month	1.73 (1.75)	1.98 (1.70)	1.73 (1.44)
Mean number of free drug samples per physician per month	4.34 (9.76)	7.79 (13.71)	4.02 (7.73)
Mean number of monthly new prescriptions per physician	13.18 (14.81)	8.82 (10.43)	2.27 (3.58)
Average number of refills following one new prescription	2–3	2–3	2–3
Recommended duration of therapy	3 months, and as maintenance with periodic patient reexamination	3 months and up to 9 months	Not yet established
Cost of therapy relative to other drugs in the therapeutic area	Average	Above average	Average
Mean number of monthly new prescriptions per physician in the respective therapeutic area	42.91 (43.63)	48.80 (53.73)	22.46 (19.03)
Therapeutic area is	Relatively new	Well established	Well established
Specialty area of top prescribers	Psychiatry	Primary care‡‡	Primary care‡‡
Number of physicians	10,516	55,896	30,005
Number of data points	252,384	1,341,504	720,120

Note. Standard errors are in parentheses. The sum of the number of physicians in each of the three data sets is greater than the total number of physicians in the study (74,075) because some physicians are in the upper 60 prescribing percentile for more than one of the drugs in our study.

‡‡The primary care specialty area includes family practice, general practice, internal medicine, and osteopathy.

detailing. As such, a spurious positive correlation, i.e., unrelated to any potential effects of detailing, will exist between detailing and prescriptions due to a joint correlation with practice size.

Because the effects of PSR activity are unlikely to be limited to the month when the visit occurred, we allow for current and lagged effects for both detailing and sampling. The number of observations available in our data sample allows us to directly estimate separate effects for lagged terms. As such, we do not need to impose a specific decay pattern as a necessity for preserving degrees of freedom. We do, however, need to specify the length of time that a PSR visit might influence physician behavior. We select a six-months lag length under the view that the effect of a visit will dissipate substantially over this period, but we also test for longer-term lagged effects. Still, we expect the effects of detailing to be largest in the months directly following the visit. The cumulative direct effect of detailing and sampling can be obtained simply by summing the coefficients, i.e., $\sum_{j=0}^{6}\beta_j$ and $\sum_{j=0}^{6}\gamma_j$.

Lagged values for new prescriptions ($Prescribe_{it-j}$) capture autocorrelation in the series that arises through inertia and persistence in physician behavior. These autoregressive effects play a key role in that they magnify the effects of detailing and sampling. That is, the total effects of detailing and sampling involve both a direct effect on prescriptions and an indirect effect that arises through persistence in physician behavior. The total effect of detailing and sampling can be calculated as

$$\sum_{j=0}^{6}\beta_j\Big/\left[1-\sum_{j=1}^{6}\phi_j\right] \quad \text{and} \quad \sum_{j=0}^{6}\gamma_j\Big/\left[1-\sum_{j=1}^{6}\phi_j\right],$$

respectively.

We include both lagged own prescriptions and lagged competitors' prescriptions, thus the model separates lagged total demand dynamics into two key components: competitive substitution and own demand growth. Lagged own prescriptions will have a positive effect on prescriptions. Lagged competitors' prescriptions will have a negative impact on prescriptions because they capture the substitution effects that physicians make between competing drugs. The current competitive prescriptions, however, will capture two different phenomena. They will reflect not only substitution effects, but also changes in total demand due to market expansion or contraction. In this regard, current-term competitive effects will act as a proxy for two different phenomena with opposite effects, i.e., negative substitution effects and positive market demand effects. As such, the current-term coefficient (λ_0) will depend on the relative magnitude of the two conflicting effects and, therefore, the sign of the effect cannot be postulated a priori.

The other variables in Equation (1) are time-period-specific indicators and specialty-specific trends. The time-period-specific indicators (the coefficients δ_τ) allow for the fact that the number of prescriptions can shift across time periods. These intercepts capture not only seasonal effects but all brandwide influences that shift prescribing behavior across all physicians (e.g., price changes, changes in the set of alternative medications available, changes in advertising campaigns, etc.). That is, the inclusion of the time-specific indicator variables will capture *all* effects common across physicians, which would include the diffusion pattern for the drug, research reports in scientific journals, any negative or positive publicity for the brand or its competitors, etc. The 11 specialty-specific trends κ_s capture influences that shift prescribing behavior across all physicians in a particular specialty. After we take first differences, the time-period-specific indicators and the specialty-specific trends are transformed into time-period-specific intercepts and specialty-specific intercepts.

To remove the influence of physician specific effects (i.e., α_i), we take first differences of Equation (1) to obtain

$$\begin{aligned}\Delta Prescribe_{it} &= \sum_{j=0}^{6} \beta_j * \Delta Details_{it-j} + \sum_{j=0}^{6} \gamma_j * \Delta Samples_{it-j} \\ &\quad + \sum_{j=0}^{6} \lambda_j * \Delta Competitor_{it-j} + \sum_{j=1}^{6} \phi_j * \Delta Prescribe_{it-j} \\ &\quad + \sum_{\tau=1}^{T} \delta_\tau * \Delta Time(\tau) + \sum_{s=1}^{11} \kappa_S * Specialty(s) + \eta_{it}. \quad (2)\end{aligned}$$

Of notable absence from our model, due to lack of available data, is competitive marketing effort. However, several considerations suggest that the potential bias in the estimated effects of firm detailing and sampling stemming from this omission will be minor. First, the inclusion of competitor sales will capture some of the effects of competitive marketing activities and thus reduce potential omitted variable bias. In essence, the competitor sales variable in the model will act as a proxy variable (Wickens 1972) for competitor marketing expenditures and, as such, reduce potential omitted variable bias. As a result, we expect the correlation between the change in firm detailing and the change in competitor detailing for a given physician (after controlling for changes in competitor sales) to be minor, which would lead to minimal omitted variable bias. Pharmaceutical firms do not coordinate detailing activity with competitors, nor do they have access to information about competitive detailing at the individual physician level. To get some perspective as to the magnitude of correlation between firm and competitive detailing, we undertook a separate analysis based on changes in monthly brand-level data for antiulcer drugs.[2] We found that, after making use of competitive sales as a proxy variable, monthly changes in brand detailing exhibited little correlation (0.01) with monthly changes in competitor detailing. Given the absence of correlation at the brand level, the correlation at the physician level can also be expected to be similarly small and, indeed, even smaller. As such, the magnitude of this bias in the estimated effect of detailing caused by the exclusion of competitive detailing from the analysis can be expected to be minimal, or even completely absent.

We estimate Equation (2) using instrumental variable estimation as ordinary least squares will generate biased estimates of the coefficients for $\Delta Prescribe_{it-1}$ and $\Delta Competitor_{it}$. By construction, $\Delta Prescribe_{it-1}$ will be correlated with the differenced error term η_{it} and, just as substitution effects cause competitor prescriptions to influence own prescriptions, own prescriptions will influence the amount of competitor prescriptions. Following Anderson and Hsiao (1982), we use lagged values of the levels of the series (values at time period $t-2$ and earlier) to generate instrumental variable estimates for $\Delta Prescribe_{it-1}$ and $\Delta Competitor_{it}$. This procedure generates consistent (i.e., asymptotically unbiased) estimates of the parameters and their standard errors.[3]

Results

For each of the three drugs in our study, we estimated the Equation (2) regression model. Table 2 reports the estimated coefficients. Figures 1 and 2 graphically depict the estimated direct effects of detailing and sampling, respectively.

Persistence in Prescribing Behavior. For each of the three drugs in the study we observe significant persistence in physicians' prescribing behavior. Although the first-order autocorrelation is the most substantial for all three drugs, significant higher-order effects are present as well. For Drug A the estimated coefficients for months 1 through 6 of 0.357, 0.205,

[2] We thank Ernst R. Berndt for granting us access to these data. See Berndt et al. (2003) for a complete description of the data. Consistent with lack of correlation between firm and competitor detailing, we also found little difference in the estimated effect of detailing on sales generated from a model that included competitive detailing versus a model that excluded competitive detailing (i.e., the difference was less than 3%).

[3] The consistency of the lagged-variable-based instrument hinges on the assumption that the disturbances are serially uncorrelated; see, for example, Arellano and Bond (1991) for a discussion. We included higher-order lagged endogenous variables in the model to ensure this would be the case and we find no evidence to the contrary. For example, for Drug A the second-order autocorrelation of the residual is −0.0029, with a t-stat of −0.96; the 3rd order autocorrelation is 0.00035, with a t-statistic of 0.12. Indeed, our estimated residual correlation matrix is in near exact correspondence with that postulated by a fixed effects, autoregressive model for each of our three drugs (Arellano 2003).

Table 2 The Effects of PSR Detailing and Drug Samples on Physician New Prescription Issuings: Equation (2) Estimates

	Dependent variable: $\Delta Prescribe_{it}$		
	Drug A	Drug B	Drug C
$\Delta Details_{it}$	0.120 (0.015)*	0.054 (0.005)*	0.021 (0.004)*
$\Delta Details_{it-1}$	0.103 (0.019)*	0.033 (0.006)*	0.028 (0.005)*
$\Delta Details_{it-2}$	0.062 (0.020)*	0.026 (0.006)*	0.024 (0.005)*
$\Delta Details_{it-3}$	0.065 (0.020)*	0.023 (0.007)*	0.021 (0.006)*
$\Delta Details_{it-4}$	0.047 (0.020)*	0.014 (0.006)*	0.012 (0.005)*
$\Delta Details_{it-5}$	0.003 (0.019)	0.002 (0.006)	0.011 (0.005)*
$\Delta Details_{it-6}$	0.016 (0.015)	−0.001 (0.005)	0.010 (0.004)*
$\Delta Samples_{it}$	0.018 (0.003)*	0.006 (0.0006)*	0.007 (0.0007)*
$\Delta Samples_{it-1}$	0.002 (0.004)	0.003 (0.0008)*	0.003 (0.0009)*
$\Delta Samples_{it-2}$	0.006 (0.004)	0.002 (0.0009)*	0.001 (0.001)
$\Delta Samples_{it-3}$	0.006 (0.004)	0.002 (0.0009)*	0.0005 (0.001)
$\Delta Samples_{it-4}$	0.004 (0.004)	0.002 (0.0009)*	−0.0003 (0.001)
$\Delta Samples_{it-5}$	0.007 (0.004)	0.002 (0.0008)*	0.00001 (0.001)
$\Delta Samples_{it-6}$	−0.003 (0.003)	0.001 (0.0006)*	0.0001 (0.0008)
$\Delta Prescribe^{\ddagger\ddagger}_{it-1}$	0.357 (0.022)*	0.208 (0.008)*	0.078 (0.010)*
$\Delta Prescribe_{it-2}$	0.205 (0.013)*	0.143 (0.006)*	0.033 (0.008)*
$\Delta Prescribe_{it-3}$	0.111 (0.008)*	0.099 (0.004)*	0.026 (0.006)*
$\Delta Prescribe_{it-4}$	0.040 (0.006)*	0.060 (0.003)*	0.012 (0.005)*
$\Delta Prescribe_{it-5}$	0.004 (0.004)	0.012 (0.002)*	−0.006 (0.003)
$\Delta Prescribe_{it-6}$	0.017 (0.003)*	0.007 (0.001)*	0.012 (0.002)*
$\Delta Competitor^{\ddagger\ddagger}_{it}$	0.25 (0.050)*	0.738 (0.050)*	0.030 (0.040)
$\Delta Competitor_{it-1}$	−0.041 (0.0017)*	−0.022 (0.0005)*	−0.004 (0.0006)*
$\Delta Competitor_{it-2}$	−0.038 (0.002)*	−0.014 (0.0007)*	−0.004 (0.001)*
$\Delta Competitor_{it-3}$	−0.032 (0.002)*	−0.014 (0.0006)*	−0.002 (0.001)
$\Delta Competitor_{it-4}$	−0.012 (0.003)*	0.0014 (0.0011)	−0.002 (0.001)
$\Delta Competitor_{it-5}$	−0.002 (0.002)	0.005 (0.0009)*	−0.001 (0.001)
$\Delta Competitor_{it-6}$	−0.006 (0.002)*	−0.001 (0.0005)*	−0.0005 (0.0006)
F-statistic	$F(52, 149{,}413) = 45.03$*	$F(52, 851{,}244) = 169.34$*	$F(52, 455{,}873) = 35.44$*

‡Standard errors in parentheses. The model also includes time-period-specific and specialty-specific intercepts. The number of observations differs from that in Table 1 due to the taking of first differences, the inclusion of six lagged terms, and removing outliers.

‡‡Instrumental variable estimate utilized.

*P value < 0.05.

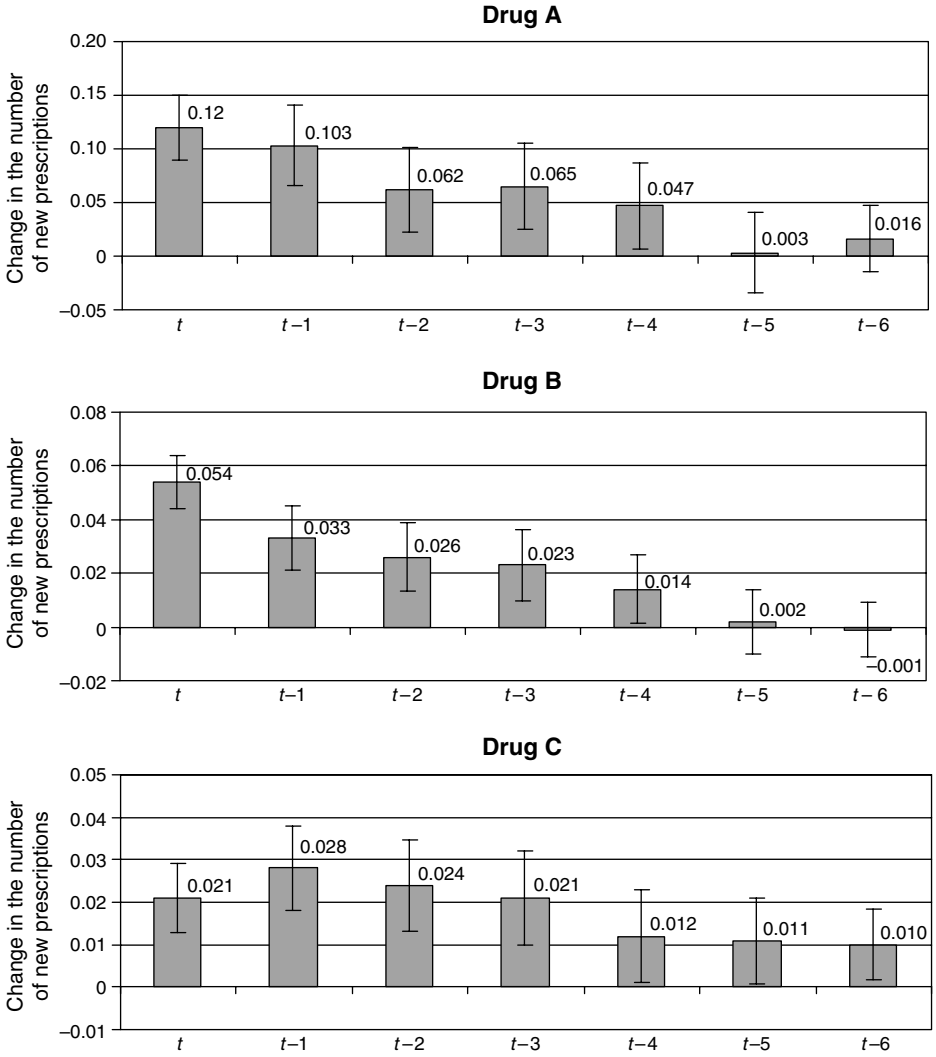
0.111, 0.04, 0.004, and 0.017 imply a total persistence of 0.734 (std. = 0.049). For Drug B we observe lower levels of persistence with the estimates decreasing from 0.208 in month 1, to 0.143, to 0.099, to 0.06, to 0.012, and to 0.007 in month 6. Total persistence for Drug B is 0.529 (std. = 0.022). Drug C prescriptions exhibit even lower persistence, with estimates of 0.078, 0.033, 0.026, 0.012, −0.006, and 0.012 for months 0 through 6. The total persistence for Drug C is only 0.156 (std. = 0.031).

Detailing. For each of the three drugs in the study we observe statistically significant positive, albeit modest, effects of detailing on prescriptions. Both current-term and carryover effects exist. For Drug A, statistically significant positive effects are present contemporaneously and for the subsequent four months (0.120, 0.103, 0.062, 0.065, 0.047, respectively). The effects for months 5 and 6, i.e., 0.003 and 0.016, are statistically insignificant. The cumulative direct effect that an additional PSR visit has on the number of new prescriptions (i.e., the sum of the estimated coefficients) is 0.415 (std. = 0.089). The total effect that detailing has on prescriptions depends jointly on this direct effect and on the indirect effect that arises through the persistence of physician behavior. Accounting for both these effects (calculated as $\sum_{j=0}^{6}\beta_j/[1-\sum_{j=1}^{6}\phi_j]$) yields an estimated total effect of 1.56, with a 95% confidence interval of [0.80; 2.23].[4] That is, on average, a PSR visit generates approximately one and one-half new prescriptions of Drug A.

The estimated response to a change in PSR visits for Drug B is similar to Drug A in that we observe a statistically significant response the month of the visit that diminishes over the subsequent six months. The magnitude of the effect, however, is smaller. The

[4] The estimated total effect involves the ratio of normal variables, thus its distribution will be nonnormal. We make use of simulation methods based on the Model (2) coefficient estimates, their variance-covariance matrix, and 10,000 draws to construct confidence intervals (Krinsky and Robb 1986).

Figure 1 Direct Effects of Detailing on the Prescribing Behavior of Physicians



Note. Coefficient estimates from Equation (2). Numbers represent the additional number of new prescriptions a physician will issue in a given month following a one PSR visit increase in promotional effort occurring in the current month. Error bars represent 95% confidence intervals.

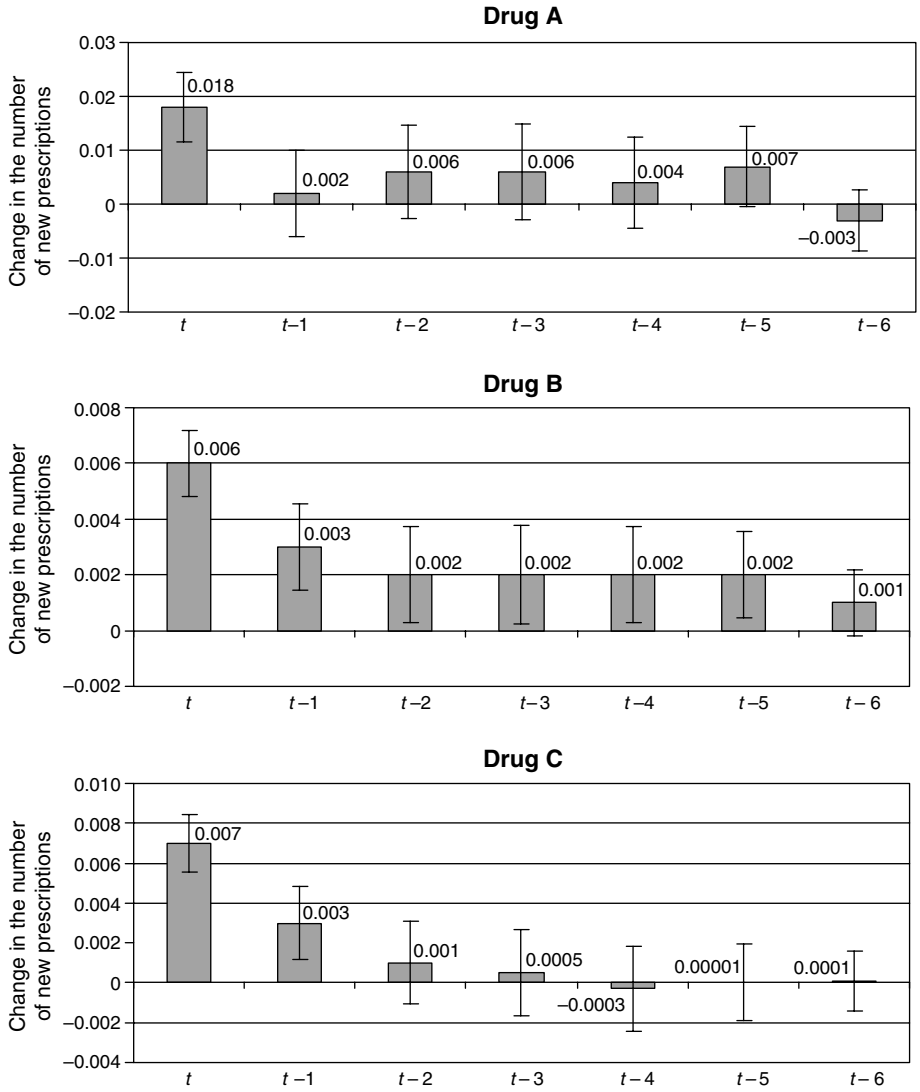
estimated effects for months 0 through 6 are 0.054, 0.033, 0.026, 0.023, 0.014, 0.002, and −0.001. These sum to a cumulative direct effect of 0.151 (std. = 0.029). Once we consider the persistence in the prescribing process, the total effect of one detailing visit for Drug B is estimated at 0.32, with a 95% confidence interval of [0.219; 0.428]. In other words, on average it takes an additional 3.11 PSR visits to generate an additional new prescription for Drug B.

For Drug C we again observe similar results in that the estimated effect of a PSR visit is statistically significant, but small in magnitude. The estimated effects for months 0 through 6 are 0.021, 0.028, 0.024, 0.021, 0.012, 0.011, and 0.010, respectively. All estimates are statistically different from zero. The estimated cumulative direct effect of 0.129 (std. = 0.024) is the smallest of the three drugs studied. Furthermore, because Drug C prescriptions exhibit the lowest persistence, the total effect of one detailing visit for Drug C is also the smallest at 0.153, with a 95% confidence interval of [0.105; 0.201]. On average, it would take an additional 6.54 PSR visits to induce one additional new prescription of Drug C.

Sampling. We also observe statistically significant but small effects for sampling. Sampling for Drug A has a positive and statistically significant contemporaneous effect (0.018), but statistically insignificant effects for months 1 through 6 (0.002, 0.006, 0.006, 0.004, 0.007, and −0.003, respectively). The estimated cumulative direct effect across the six months is 0.041 (std. = 0.02). The total (direct and indirect) effect of one free sample of Drug A is 0.155 with a 95% confidence interval of [0.032; 0.310]. As such, on average an additional 6.44 samples are needed to induce one additional new prescription of Drug A.

We see a smaller response for sampling for Drug B than for Drug A. A change in sampling has statistically

Figure 2 Direct Effects of Sampling on the Prescribing Behavior of Physicians



Note. Coefficient estimates from Equation (2). Numbers represent the additional number of new prescriptions a physician will issue in a given month following a one free drug sample increase in promotional effort occurring in the current month. Error bars represent 95% confidence intervals.

significant positive, albeit declining, effects for each month (0.006, 0.003, 0.002, 0.002, 0.002, 0.002, and 0.001 for months 0 through 6, respectively). The estimated cumulative direct effect across the six months is 0.019 (std. = 0.004). The total effect of one free sample of Drug B is 0.039 with a 95% confidence interval of [0.025; 0.054]. It would take 25.39 additional samples to generate one additional new prescription for Drug B.

The estimated response to sampling is smallest for Drug C. The estimated effects are 0.007, 0.003, 0.001, 0.0005, −0.0003, 0.00001, and 0.0001, with only the contemporaneous and one-month lag effects being statistically significant. The estimated cumulative direct effect is 0.012 (std. = 0.005). Because the persistence level is very low for Drug C, the total effect of one free drug sample is only slightly higher at 0.014, with a 95% confidence interval of [0.0042; 0.0232]. This means that it takes 73.04 additional samples of Drug C to generate one new prescription.

Competitive Prescriptions. As expected, consistent with brand switching we observe negative effects for lagged competitive prescriptions. The inclusion of these competitive effects in the model is important, not only in helping explain new prescriptions, but also in allowing us to isolate the persistence in physician behaviors. That is, because competitive prescriptions are correlated with own prescriptions, failure to model these competitive effects would result in biased estimates of the autocorrelation coefficients and, as a result, biased estimates of total detailing and sampling effects.

As discussed earlier, the instrumental variable for the contemporaneous competitive prescriptions captures two distinct phenomena with two distinct

effects, i.e., a positive effect of growth in total demand and a negative effect of brand switching.[5] The positive contemporaneous effect observed for Drugs A and B is consistent with the growth in demand effects dominating brand switching. For Drug C, the statistically insignificant estimate implies that, contemporaneously, the market-growth effects essentially cancel out the brand-switching effects.

Other Influences. The model also includes dummy variables capturing time-period-specific effects (our model involves first differences of the data and six lags, thus Time Period 1 is the eighth month of data in our 24-months sample) and differences among specialties. The time-varying intercepts capture all the unobserved influences that drive prescriptions across all the physicians in a given month (e.g., diffusion, rate of the drug addition to the formularies, published scientific studies, new indications, etc.), but are not explicitly in the model. Although we find some specialty-area effects and time-period-specific differences, little correlation exists between the change in detailing or sampling and these control variables. As such, their inclusion in the model has little impact on the estimated direct effects of detailing or sampling. The estimates of total persistence, however, are substantially affected. If the time-period-specific intercepts are omitted from the model the persistence estimates are significantly biased downward. Because both direct effects and persistence determine the total effects of marketing effort, omitting the time-period-specific intercepts would lead to erroneous inferences about marketing effectiveness.

Model Validation and Sensitivity Analysis

We undertook a number of sensitivity tests to assess the validity of the model and stability of our results. For example, we tested for the possibility of longer lagged effects of detailing and sampling, for feedback effects (i.e., simultaneity), and nonlinearity in the relationships. In all cases, we found no evidence that calls into question the results we report. Although they are consistent, the estimates we report would not be efficient if in fact physician-specific effects were not present. A Hausman (1978) specification test, however, documents the presence of significant physician-specific fixed effects and strongly rejects the no-fixed-effects model specification.

We investigated the possibility that data reporting problems (i.e., measurement error) might be biasing our estimates of the detailing effect downward. Both random measurement and systematic error (e.g., PSRs might be motivated to overreport either the number of drugs they promoted to a physician on a particular visit or the number of visits that occurred) might lead to a bias toward zero in the estimated effect of detailing. To assess this possibility we estimated our model including only those details that were also accompanied by drug samples. We can be reasonably certain that these details did in fact occur, because the sampling data is recorded by a third party from the receipt slips signed by the physician receiving the free samples. The estimates of the detailing effect obtained from estimating Equation (2) on this restricted data sample are indistinguishable from those we reported in Table 2. As such, we have no reason to believe that our results are driven by measurement error bias.

To investigate the generalizability of our results we obtained data on a fourth drug, one that differed significantly from the three drugs in the study in that it was new to the market and achieved commercial success during the period of study. It also was in a new therapeutic class and had only one direct competitor. Despite these differences, estimation of Equation (2) for this fourth drug generated similar results to those obtained for the other drugs on our study. The estimated cumulative direct effects were 0.091 for detailing and 0.008 for sampling. With an estimated persistence of 0.19, this gave rise to total effects for detailing and sampling of 0.112 and 0.009, respectively. As such, we find no evidence to question the generality of our findings based on unique features of the three drugs in our study. Analysis of this fourth drug generates findings similar to those that we observe for Drug C.

Although our sensitivity analysis failed to challenge the Table 2 results, additional investigation is still warranted. Several directions are of potential promise for future research. One direction would involve assessing the characteristics of drugs that induce variation in responsiveness to PSRs. That is, what can explain the differences in responsiveness that we observe among the drugs studied? A variety of factors, ranging from time on the market to the efficacy of the drug, might induce differences in responsiveness. Furthermore, analysis assessing which physicians are most responsive to PSR influence and why they are responsive would be of considerable value both from a public policy standpoint and from the standpoint of increasing marketing's understanding of factors influencing responsiveness to sales force efforts. Although our results provide aggregate effects, differences in responsiveness could exist across physicians. Physician characteristics as varied as, for example, specialty area, years of experience, size of practice, gender, and age might account for interphysician differences in responsiveness.

[5] Excluding this variable from the estimation model does not alter the estimated effects of detailing or sampling.

Discussion

The focus of our study was to assess the magnitude of physician responsiveness to two main pharmaceutical marketing practices while controlling for other possible influences on prescribing. Our results show that physicians are tough sells, in that the effect of sales force activity on prescribing behavior is modest. For the three drugs in our study the estimated total effects on new prescriptions are 1.56, 0.32, and 0.153 for detailing and 0.155, 0.039, and 0.014 for sampling. In other words, for the three drugs in our study it would take an additional 0.64, 3.13, and 6.54 PSR visits, respectively, to induce one additional new prescription for the drugs. It would take 6.44, 25.64, and 73.04 additional free drug samples to induce one additional new prescription.

The high statistical significance of the estimates indicates that these marketing activities have an effect on the new prescriptions, but the magnitude of the effect indicates that PSR activities have only a modest impact. The large sample size provides small standard errors (allowing us to distinguish between a very small effect versus a statistically insignificant effect) so that we are able to accurately pinpoint the magnitude of the effect.

To give these results some additional context, we can consider the effect of an additional detail or sample over a one-year time frame (by which time the effect of the PSR visit is mostly, or in some cases completely, dissipated). For Drug A, as can be calculated from Table 1, an additional detail represents a 4.8% annual increase. Based on the dynamic pattern of the coefficients reported in Table 2, we find that, over a one-year period, this additional detail will result on the average in a 1.32 increase in new prescriptions in the subsequent 12 months (i.e., 84% of the total effect of the detailing (1.32/1.56) will occur in the first year following the visit). As such, we see that the 4.8% detailing increase is associated with a 0.83% increase in new prescriptions over a 12-month period. This indicates a 12-months detailing elasticity estimate of 0.17. The same type of calculations give rise to 12-months detailing elasticity estimates of 0.07 for Drug B (where approximately 96% of the effect occurs within 12 months) and 0.115 for Drug C (where approximately 100% of the effect occurs within 12 months). For sampling, our estimated results imply 12-months sampling elasticity estimates of 0.042, 0.032, and 0.027 for Drugs A, B, and C, respectively. These estimates are substantially smaller than those speculated based on anecdotal evidence or those reported in some prior research.

Given the modest response to PSR activity, the question is no longer "Are physicians easy marks?" but rather "Why do drug companies make such extensive use of PSRs, given their limited effectiveness?" It appears that drug company profits might be enhanced (or drug prices reduced) through cost savings achieved through a reduction in PSR numbers. Although this might be true, some additional issues need to be considered. First, it should be remembered that our estimates reflect the effect of a visit on the sales of a single drug. A PSR might discuss more than one drug during a visit, so the impact of a given visit will be greater than its effect on a single drug. Second, the reported estimates relate to new prescriptions issued. Sales of the drug, however, will also be based on the refills accompanying the prescription, which average between two and three for the drugs in our study. Both these considerations magnify the financial implications of a detailing visit. Furthermore, the margin to the pharmaceutical firm on a drug can be considerable. Based on these considerations, we believe that the returns to detailing of Drug A are positive, which stems both from its larger margin and the larger estimated physician response to detailing, whereas returns to detailing of Drug B and of Drug C are negative.

Why would the firms persist at engaging in a practice that has negative returns? Indeed, the number of PSRs continues to increase and is now over 85,000 (IMS Health 2003); it has almost doubled in the past five years, although the number of physicians has remained constant (*Wall Street Journal* 2002). Our results suggest that for some drugs (e.g., those with lower margins) the current detailing system is suboptimal. This situation might be a result of the intensive PSR "arms race" the pharmaceutical industry has undertaken. A recent McKinsey report (Elling et al. 2002) questions the effectiveness of the current PSR system and advocates that pharmaceutical companies transform their sales model.

The limited effectiveness that we find for PSRs is consistent with some conclusions reached by others. For example, Pennsylvania stopped its counterdetailing program based on the conclusion that, although it had some effect, "it was labor intensive, cost a lot of money and it didn't have any staying power" Gold (2001, p. B8).

Conclusion

We do not wish to make any value judgments about the magnitude of physician's responsiveness to PSR visits and the reasons behind it. Responsiveness could mean that physicians are better internalizing information about a drug and the result is better patient outcomes or care at a lower cost. Alternatively, responsiveness could simply reflect brand switching among drugs that provide similar benefits. Although drug company revenues would be affected, patient

care or costs would not. However, responsiveness to PSRs could result in inferior patient care or in higher costs when physicians prescribe, for example, branded drugs that are no more effective than a generic equivalent but are priced higher. Whatever the relative costs and benefits, the bottom line remains that the average effect of PSR activity on physician prescribing behavior is modest.

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