Asymmetric Social Interactions in Physician Prescription Behavior: The Role of Opinion Leaders

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The authors quantify the impact of social interactions and peer effects in the context of physicians' prescription choices. Using detailed individual-level prescription data, along with self-reported social network information, the authors document that physician prescription behavior is significantly influenced by the behavior of research-active specialists, or "opinion leaders," in the physician's reference group. The authors leverage a natural experiment in the category: New guidelines released about the therapeutic nature of the focal drug generated conditions in which physicians were more likely to be influenced by the behavior of specialist physicians in their network. The authors (1) find important, statistically significant peer effects that are robust across model specifications; (2) document asymmetries in response to marketing activity across nominators and opinion leaders; (3) measure the incremental value to firms of directing targeted sales force activity to these opinion leaders; and (4) present estimates of the social multiplier of detailing in this category.

Keywords: social interactions, peer effects, social multiplier, contagion, physician prescription behavior, pharmaceutical industry

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Marketers, sociologists, and economists have traditionally been interested in the role of interpersonal communication (i.e., communication outside the firm's control) in consumer choice and consumption behavior. These interactions have been variously labeled as "peer effects," "contagion," and "word-of-mouth effects." In this article, we test and provide empirical support for asymmetric peer effects. These effects arise when some consumers exert a stronger influence on the attitudes and behavior of other consumers than vice versa. Such consumers have typically been labeled "opinion leaders" in the literature (Rogers 2003, chap. 8). However, little research in marketing has tested for the existence of these asymmetric peer effects.

The context of our analysis is physicians' prescription drug choice. An asymmetric social interaction or "peer effect" arises in this setting because nonspecialist physicians may rely on prominent physicians, the "opinion leaders," to help reduce the uncertainty about their prescription choices. The role of opinion leaders becomes most salient when changes occur in the therapeutic environment because these typically lead to increased uncertainty about drug efficacy among the nonspecialist physicians. The pharmaceutical industry believes in the existence of such opinion leaders and has invested in targeting marketing activities at opinion leaders (Cutting Edge Information 2004). However, to date, there has been little empirical evidence that opinion leaders "matter" (i.e., significantly influence the opinions

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and behavior of other physicians). Coleman, Katz, and Menzel (1966) find no asymmetries in peer effects between nominators and their opinion leaders' adoption patterns for a new drug. More recent work using the same data as that study finds no peer effects at all (Van den Bulte and Lillien 2001). Finally, using computational models of network tipping, Watts and Dodds (2007) also find little or no role for opinion leaders.

Asymmetric social interactions have important implications for firms' allocation of marketing effort. If such social interactions are present, they will increase the return on investment to marketing activity targeted at agents who have a stronger influence. In the pharmaceutical context, if opinion leaders' actions have a true causal effect on other physicians' prescription behavior, marketing effort directed at these opinion leaders will generate a multiplier effect. The multiplier arises because an incremental sales call to an opinion leader increases the opinion leader's prescriptions, which in turn induces the physicians he or she influences to prescribe more. The extent to which net prescriptions are higher as a result of these cross-physician spillovers is the "social multiplier" (Becker and Murphy 2000). Given that pharmaceutical firms in the United States currently tend to set physician-level detailing efforts according to past prescription volume (Manchanda, Rossi, and Chintagunta 2004), the presence of significant social multipliers would imply that the return on investment of detailing to opinion leaders may be much higher than is suggested by just the opinion leader's prescription volume. We use our estimates to measure the social multiplier of detailing in the data.

The identification and management of opinion leaders is an important component of marketing in the pharmaceutical industry. Marketing to opinion leaders is typically managed through direct contact with physicians through detailing and, in several instances, using special teams consisting of higher-caliber detailers (called "medical scientific liaisons" [MSLs]). About half of the large pharmaceutical companies are reported to have MSLs, with an average team size of approximately 45 (Cutting Edge Information 2004). Opinion leaders are typically physicians who have an academic title with the department of a medical school and have contributed peer-reviewed publications (Tan 2003). Industry reports suggest that the pharmaceutical industry spends an estimated 24% of its new product commercialization budget on opinion leader activities (Cutting Edge Information 2004). Opinion leader activity is also stepped up when environmental changes occur, for example, during the launch or withdrawal of drugs or the issue of new guidelines by the Department of Health and Human Service and/or the National Institutes of Health (NIH). Although firms try to manage their relationships with these opinion leaders through marketing, anecdotal evidence suggests that the identification of opinion leaders and the extent to which they affect other physicians are issues the industry grapples with, especially because there is little published work quantifying the return on investment from targeting these opinion leaders.

To test for these effects, we leverage a novel data set that is based on a combination of primary (survey) and secondary (behavioral) data. Broadly speaking, five major challenges arise in measuring these effects. First, opinion leaders who constitute the reference group for a given physician need to be identified. Second, after these opinion leaders are identified, some change in the environment needs to take place in a manner that affects the attitudes and/or behavior of the opinion leader. Third, these changes need to be transmitted to the agents whose opinions and/or behavior is affected by the opinion leader's behavior. Fourth, a resultant change in the behavior of these consumers should occur. Fifth, we need to be able to distinguish between correlation and causation in the observed behavior of physicians and their opinion leaders. As we discuss subsequently, correlation in behavior can arise from three possible sources—endogenous group formation, correlated unobservables, and simultaneity—and we need to be able to control for these explanations. As the literature points out (Manski 1993; Moffitt 2001), solving this latter identification problem is not easy. Our data enable us to formulate empirical strategies that address most, if not all, of these issues. We believe that our identification strategy is novel to the literature and is relevant across a broad range of situations involving the analysis of data arising from social interactions.

The key contributions of this research are as follows: First, we test for and find the existence of asymmetric peer effects among a specific social network (prescribing physicians). We find that opinion leaders' behavior significantly affects physician behavior after an exogenous change in the market that resulted in a change in the therapeutic environment. The empirical results also reveal that peer effects in this category are asymmetric in the sense that opinion leaders' prescriptions are not statistically significantly affected by the prescription pattern of the physicians they influence. These effects are robust to functional form and alternative specifications. Furthermore, we document the finding that peer influence can significantly affect behavior even in stable, mature categories. These are novel findings that add to the literature on peer effects in the presence of marketing, especially in the pharmaceutical industry.

Second, we document asymmetries in response to marketing activity (detailing) across opinion leaders and nominators. We find that nominators tend to be less responsive than opinion leaders. Third, we measure empirically the effect size of these peer effects for both physicians and opinion leaders. We then combine the results on the response to marketing activity and peer effects to derive implications for marketing resource allocation for firms, and we present estimates of the social multiplier effects of detailing. We find that for the average opinion leader, who influences 1.56 physicians, social interactions alone provide an additional 5% increase in prescription revenue—that is, a social multiplier of detailing of approximately 1.05. For the top opinion leader, the social multiplier is 1.35. This large difference underscores the importance of estimating both the average multiplier and the identification of top influencers to make optimal resource allocation decisions.

Fourth, we discuss and clarify how the identification issues that arise in measuring and testing for causal peer effects may be overcome for data-rich settings such as ours. The increasing salience of (online) social networks in the real world makes our methodology and findings particularly relevant for practicing managers and academics who are interested in understanding the return on investment of marketing activity to opinion leaders (e.g., Godes and Mayzlin 2009).

This article is related to the sociology, economics, and marketing literature on testing for social interactions using microlevel data. A subset of this work has focused on asymmetric peer effects, modeling, for example, the role of ties in referrals (Reingen and Kernan 1986) or the characteristics of opinion leaders (e.g., Summers 1970) but without quantifying their effect on outcomes. A few other studies from the medical and public health literature have used surveys and/or field experiments to test for opinion leader effects (e.g., Celentano et al. 2000; Lomas et al. 1991) or peer effects in general (Christakis and Fowler 2007). In economics, researchers have investigated social interaction effects more (see, e.g., Conley and Udry 2010; Duflo and Saez 2003; Sorensen 2006). A small but growing number of recent papers in the marketing literature have also investigated the potential role of peer effects in new product adoption (e.g., on prescription drugs, see Iyengar, Valente, and Van den Bulte 2008; Manchanda, Xie, and Youn 2008; Van den Bulte and Lilien 2001; on Internet grocers, see Bell and Song 2007; on video-on-demand technology, see Nam, Manchanda, and Chintagunta 2006). We refer readers to the work of Hartmann and colleagues (2008) for a recent and broad overview of the social interactions literature, which also discusses approaches from several related fields.

Broadly speaking, compared with this previous literature, our approach has several distinguishing characteristics. These include documenting the asymmetric nature of peer interactions, distinguishing causal peer effects rather than correlated outcomes that do not rely on peer effects, and determining peer effects in mature product categories (i.e., using postadoption behavior). In terms of the causal effect determination, we believe that this article is one of the first to comprehensively outline and address the identification issues related to endogenous network formation, correlated unobservables, and simultaneity and is one of the first to include specific controls for targeted marketing activity in the analysis of social interactions in the presence of marketing.

We organize the rest of the article as follows: Next, we present the model and describe the data. Then, we present the results from estimation. In the final section, we offer some conclusions and discuss the limitations.

MODEL

We now discuss the model framework and empirical strategy. The empirical framework is a descriptive linear model of prescription behavior, which we interpret as the reduced form of the behavioral process generating prescriptions for physicians and their opinion leaders (for structural approaches, see Brock and Durlauf 2001; Hartmann 2010). In the "Robustness" section, we discuss some extensions of this linear model that accommodate alternative specifications of the effect of peers and relax the linearity assumption (through the use of a count model). We index physicians by i, i's opinion leader by j(i), and time by t. Let D denote detailing, and let y and x denote continuous variables representing new prescriptions for physicians and opinion leaders, respectively. The starting point of the empirical specification for physician prescriptions is a linear regression:

$$y_{it} = \beta D_{it} + \delta x_{j(i),t} + \upsilon_{it}. \quad (1)$$

Here, υ_{it} denotes unobserved factors that shift prescriptions of physician i over time. Although we would ideally like to include the actual opinions of the opinion leader as a covariate to capture the social interaction, these are unavailable in the data. Here, we consider the prescriptions of the opinion leader a proxy for these opinions (subsequently, we present sensitivity checks to different proxies for leader opinions). Formally, the test for peer effects in prescription behavior is whether δ is statistically significantly different from zero. An alternative model that uses the share rather than the levels of prescriptions is equivalent to Equation 1 because the overall volume of prescriptions written for the disease condition remained roughly constant across the months in the data. Identification of peer effects in this model requires us to resolve the five issues described previously. We discuss these subsequently.

Reference Group/Peer Determination

We need to identify the proper reference group or reference peer for each agent. Manski (1993) discusses in detail the need for exogenously defined social network information to identify peer effects from behavioral data. Intuitively, behavior itself cannot be used to define reference groups if the goal is to obtain the effect of a reference group's behavior on an agent's actions. By grouping agents with *ex post* similar actions together, a researcher attempting this approach essentially produces an upward bias in any peer effects unearthed through subsequent analysis. Moreover, geographic or location-specific proxies for reference groups cannot sort between peer effects and common unobservables that affect the actions of all agents in the location similarly. We overcome these challenges by using a new data set that contains detailed social network information obtained through a "sociometric" approach (e.g., Christakis and Fowler 2007; Coleman, Katz, and Menzel 1966; Conley and Udry 2010; Valente and Pumpuang 2007). Here, people are directly surveyed to obtain information about others who exert a peer effect on their behavior.[1] Each physician in the survey self-reports the doctor whose opinions he or she incorporates in prescription decisions, thus identifying the doctor's social network. This provides us with an exogenous measure of the physician's reference group or peer, circumventing the need to rely on behavior-, location-, or geography-based proxies. Here, the term "opinion leader" should be interpreted as doctors who physicians in this survey nominate (we describe this in greater detail in a subsequent section). In the absence of such data, other researchers have often defined networks in terms of geographical location (Bell and Song 2007; Manchanda, Xie, and Youn 2008), dorm/work location (Duflo and Saez 2003; Sacerdote 2001; Sorensen 2006), and ethnic/cultural proximity (Bertrand, Luttmer, and Mullainathan 2000).

Change in the External Environment

In stable drug categories, general practitioners may have little uncertainty about the usage and efficacy of the drugs they prescribe. Peer effects may be difficult to uncover in such settings. Changes in the environment add exogenous

[1] In contrast, some studies follow the "key informant" approach, in which a few respondents are polled to determine the identity of people with social influence (e.g., Celentano et al. 2000). Iyengar, Valente, and Van den Bulte (2008) find that self-reported opinion leaders are different from those identified through a sociometric approach.

variation that assists in unearthing the peer effect. An advantage here is that the data cover a period during which there was a significant change in the recommended usage of drugs in the therapeutic category. For the therapeutic category we study, this environmental change pertains to new NIH-issued treatment guidelines regarding appropriate treatment for specific disease indications (we describe these new guidelines subsequently). This environmental change occurs around the midpoint of the data and is exogenous to behavior because it arises from the behavior of a third party that is not affected by the actions of physicians and their opinion leaders, which aids identification. Thus, our analysis exploits how changes in prescription behavior of opinion leaders (x in Equation 1) before and after the issuance of the guidelines generate variation in changes in prescription behavior of their nominating physicians (y). In the survey, physicians also report their mode of interactions with their opinion leaders. Thus, the data also enable us to provide insights into the mechanism through which the opinion leader effect manifests.

Distinguishing Causality from Correlation

As we mentioned previously, peer effects imply that the behavior of agents in the same reference group tends to be correlated. However, correlation in the behavior of agents per se does not imply that any one agent's action has a causal effect on the actions of others in the group. In addition to peer effects, such correlation in behavior could arise from three other factors: endogenous group formation, correlated unobservables, and simultaneity (for a discussion, see Moffitt 2001). Only a causal peer effect implies a social multiplier; thus, it is important to sort out causal effects from each of these sources of correlation. In our application, another factor that could lead to correlation is targeted marketing.

Endogenous group formation. Endogenous group formation arises in our context if physicians choose doctors with similar "tastes" for prescriptions as their opinion leaders (i.e., homophily). For example, physicians who face patient bases that require treatments using a specific class of therapeutic drugs may meet experts in that therapeutic category at conferences organized by drug companies. If physicians choose these experts as opinion leaders, it is likely that such physician–opinion leader pairs tend to prescribe more in the therapeutic category than average. In this case, the observed correlation in the behavior of the physician and his or her opinion leader could arise from omitted individual characteristics that are correlated within the group. In Equation 1, such endogenous group formation implies that physician i's unobserved tastes (υ_{it}) and opinion leader j's tastes for prescriptions could be correlated; if opinion leader j's tastes also drive his or her prescriptions, x_{jt}, this generates a correlation between x_{jt} and υ_{it}, leading to a upward bias in the estimates of δ.

The solution to the group formation problem is facilitated by the availability of panel data (as Manski [1993] notes, the prospect for identification of peer effects in cross-sectional data is poor). Panel data enable us to include physician-specific fixed effects in the regression (Equation 1). In terms of our model, we write the following:

$$\upsilon_{it} = \alpha_i + \eta_{it}, \quad (2)$$

where α_i is a fixed effect specific to physician i, which controls for unobserved (to the econometrician) time-invariant tastes for prescriptions. By controlling for physician i's tastes, we control for the portion of υ_{it} that is correlated with x_{jt} through correlation with opinion leader j's tastes, thus accommodating the endogenous group formation problem. The identifying assumption is that group selection is fixed over time and that physician group formation is not influenced by changes in the external environment.

Correlated unobservables. Another concern is whether correlated unobservables exist that drive prescriptions of both the physician and the opinion leader similarly. If uncorrected, these manifest as peer effects. An obvious source of correlation is sales force activity (i.e., detailing) directed at physicians and opinion leaders by drug companies. This source of correlation can be partly controlled for by including time fixed effects that pick up common trends in marketing activity to physicians (e.g., Van den Bulte and Lilien 2001). In our setting, we fully control for such marketing activity by obtaining direct data on detailing to physicians, which we include as explanatory variables in the regression. A potential concern with using this variation arises because detailing may be targeted to physicians. As the recent literature documents (e.g., Manchanda, Rossi, and Chintagunta 2004), many pharmaceutical companies in the United States, including the firm we use in this research, decide detailing allocations on the basis of a volume-based rule. Under this rule, physicians are allocated detailing levels that correspond to their position in deciles of past prescription volume in the focal category (we find evidence for this detailing pattern in the data). This volume-based detailing rule implies that D may be correlated with υ_{it}.

We derive our control for this potential endogeneity from the nature of the targeting rule. In effect, because of stable patient bases, physicians rarely move across deciles (we find this in the data as well). Thus, the inclusion of physician fixed effects controls for the across-physician variation in detailing and accomodates the endogeneity concern. Thus, we use only within-physician, across-time variation in detailing for identification. Fixed effects do not fully absorb all detailing variation, however, because in practice, actual detailing levels are centered on, but not exactly equal to, top-down allocated levels as a result of several unanticipated factors that affect visits. These include shocks to physicians' schedules (e.g., the physician is not in his or her office during a detailing visit) or unanticipated detailer time constraints (a patient is taking a long time, requiring postponement of the visit). This deviation from preallocated levels is orthogonal to physician unobservables, and we use it for identification. Thus, the underlying identifying assumption is that after we control for α_i, within-physician detailing is independent of other physician and time-specific unobservables, η_{it}.

We also consider the possibility that there are additional correlated unobservables that generate comovement in prescriptions. Candidates for such unobservables include trends in overall prescriptions across all physicians in the category and any spatially correlated region-/location-specific shocks to prescription behavior that are captured by

η_{it}. We address these as follows: First, we include a full set of time fixed effects. These control for any time trends common across all physicians and opinion leaders. Second, recall that we include physician fixed effects. Because none of the surveyed physicians in the data share a zip code, physician fixed effects are equivalent to including a full set of zip code fixed effects. Thus, we also fully control for time-invariant spatially correlated unobservables. A final issue is whether there are unobservables that are correlated at the level of the zip code *and* time.

To consider this issue, we discuss a potential difference-in-difference approach.[2] We have access to the prescription behavior of all physicians in the country. We use these data to compute the mean prescription of all other physicians in physician i's zip code, denoted by z_{-it}, which we include as a covariate in the regression. Essentially, z_{-it} proxies for all unobserved time- and location-specific shocks to prescriptions that are common to all physicians in i's location.[3] By including these in the regression, we essentially use the prescription behavior of other physicians in i's location as a control. Thus, we further decompose Equation 2 as follows:

$$\eta_{it} = \gamma z_{-it} + \varepsilon_{it}, \tag{3}$$

where ε_{it} is a mean zero error term. Note that this strategy is subject to the implicit caveat that a given physician's opinion leader does not influence other physicians in his or her zip code. Unfortunately, data on the social networks of the universe of physicians are not available to test this. Thus, our approach is to present extensive sensitivity checks in which z_{-it} is included or excluded from the regression. Tests for correlated unobservables (presented subsequently) suggest that most of the spurious correlation is along the temporal dimension, which is fully picked up by time fixed effects. Thus, the effect of z_{-it} on the results is small.

Simultaneity. Finally, we are careful in considering potential simultaneity. Simultaneity implies that physician i's actions and opinion leader j(i)'s actions may be contemporaneously interdependent. If peer effects exist, the notion that opinion leaders affect physicians while physicians simultaneously affect them leads to an upward bias in the estimation of the interactions. In the context of our model, if physician i and opinion leader j's prescriptions are simultaneously determined, high values of ε_{it} would tend to induce high values of $x_{j(i),t}$, thus leading to an upward bias.

We control for the simultaneity problem with exclusion restrictions. In our context, detailing to the opinion leader, $D_{j(i),t}$, and the mean prescriptions of all other physicians in the opinion leader's zip code, $z_{-j(i),t}$, form excluded variables that affect the prescriptions of the opinion leader (the endogenous variable) and can be excluded from the prescription equation for physician i. Both $D_{j(i),t}$ and $z_{-j(i),t}$ affect the opinion leader's prescriptions and thus are correlated with the endogenous variable $x_{j(i),t}$ but are uncorrelated with ε_{it}. As such, they serve as instruments for $x_{j(i),t}$, thus addressing the potential simultaneity concern. An alternative approach would be to assume that only past opinion leader prescriptions affect the physician's current prescriptions (i.e., there is no contemporaneous linkage in behavior). We explore model sensitivity to such specifications in the "Robustness" section.

Note that if we assumed that because of their "expert" status (we provide details in the "Data Description" section) opinion leaders were not affected by physicians, there would be no simultaneity problem by construction (e.g., Sorensen 2006). Rather than assume away simultaneity concerns a priori in this manner, we use the data to check whether peer effects are truly asymmetric. We run the analogous regressions for opinion leader j(i) (i.e., $x_{j(i),t}$ regressed on $\alpha_{j(i)}$, $D_{j(i),t}$, y_{it}, and $z_{-j(i),t}$) to check whether physician prescriptions have a significant effect on the opinion leaders' prescription behavior. Analogously, we exclude the D_{it} and z_{-it} variables for the opinion leader's prescription equation; they serve as instruments for y_{it} in the opinion leader's prescription equation.

Final specification. In line with the preceding discussion, the final specification is as follows:

$$y_{it} = \alpha_i + \gamma_t + \beta D_{it} + \delta x_{j(i),t} + \gamma z_{-it} + \varepsilon_{it}, \quad i = 1, ..., N; t = 1, ..., T. \tag{4}$$

The corresponding specification for physician i's opinion leader is as follows:

$$x_{j(i),t} = \alpha_{j(i)} + \tau_t + \varpi D_{j(i),t} + \varsigma y_{it} + \zeta z_{-j(i),t}, \; t = 1, ..., T. \tag{5}$$

We estimate both specifications with fixed-effects panel data linear instrumental variables (IV) regression. Next, we describe the data.

DATA DESCRIPTION

The data pertain to physician prescription behavior in a large therapeutic class (we do not reveal the name of this class because of confidentiality concerns). The drugs in this class address a serious chronic disease condition that affects about a quarter of all adults in the United States. We consider a combination-drug subcategory for the treatment of this chronic disease.

The data set we use is a combination of primary and secondary data. The primary data come from a market survey carried out in January–February 2004 of 1500 physicians chosen randomly from a set of 56,000 regularly prescribing physicians across the United States in this therapeutic category. A large pharmaceutical company commissioned the survey, and a market research firm conducted the survey, with the pharmaceutical company bearing all costs (confidentiality reasons preclude us from naming the companies). The main objective of the survey was to obtain names of doctors whose actions influence the nominating physician's approach to the treatment of the chronic disease using combination drugs. Nominating physicians were encouraged to name doctors who were known to them (by reputation or otherwise) and then were queried about the mechanisms by which they were able to obtain information about the opin-

[2] Christakis and Fowler (2007) use the *ex ante* symmetry in the network to rule out correlated unobservables. In other words, if both parties in a dyad are exposed to a common unobservable and it leads to a peer effect, the only reason the effect is asymmetric is because the link between the two parties is asymmetric. Although this is a valid identification strategy, the difference-in-differences approach is more general because it does not depend on the *ex ante* knowledge of asymmetry in the network.

[3] Less than 10% of opinion leaders and nominators are in the same zip code in the data. The results do not change if we exclude these physicians from our sample.

ion leader's beliefs and actions.[4] Note that the pharmaceutical company deemed the answers to the questions detailed in the survey (as described in n. 4) to be sufficient to identify opinion leaders. Although this direct elicitation method is somewhat different from methods followed in the literature (e.g., self-reports, key informants, or sociometric measures, such as "in-degree"), the results suggest that the survey has indeed identified relevant opinion leaders. Under the null of biased elicitation, we can explain only the correlation between prescriptions of the nominator and the nominee (through correlated unobservables) but not the robust asymmetry we uncover. From this survey, we have access to information on 290 physician–opinion leader pairs. The opinion leaders identified reflect each nominating physician's subjective opinion regarding who in the field he or she considers an expert and whose opinion he or she incorporates when making prescription decisions in this therapeutic category. We believe that this individual-specific measure is appropriate for identifying peer effects in such settings.

We then supplemented these data with secondary data—also collected with the help of the pharmaceutical company—on the prescription behavior and the marketing activity directed at both the physicians and the opinion leaders. These data span a period of 24 months (from April 2002 to March 2004 inclusive) and contain the prescription counts for the combination-drug category and the count of details received each month for the universe of physicians for the company's drug. Notably, for this drug, the pharmaceutical firm decided to rely almost entirely on regular detailing to manage relationships with the opinion leaders. Interviews with the managers of the firm indicated that though the firm would have liked to consider other forms of marketing, such as setting up an MSL team, it was not doing so during the period of data collection.[5] We also learned from the firm that only approximately 50% of the doctors identified as opinion leaders in the survey were on the firm's own list of opinion leaders.

[4]The specific questions asked during the survey were (1) "Whose opinions do you value most regarding the treatment and/or management of [disease condition] among [disease condition] patients?" and (2) "How do you obtain information from that influencer about the treatment/management of [disease condition]?" The wording of this question might "encourage" respondents to name peers; however, this would only attenuate the effects we calibrate using (independently) obtained behavioral data, making the results conservative.

[5]The data do not contain information on sampling. However, a sample cannot be dropped unless the sales person calls on the physician. This usually results in a high positive correlation between detailing and sampling, and the exclusion of this data is unlikely to change the results, especially those that pertain to peer effects.

Table 1
DISTRIBUTION OF NOMINATIONS

Number of Nominations	*Number of Nominators*
1	245
2	21
3	1
Total	267

Notes: To be read as follows: There were 245 physicians who nominated 1 opinion leader, 21 who nominated 2 opinion leaders, and 1 who nominated 3 opinion leaders.

Descriptives: Primary Data

We have demographic and location information for 290 opinion leader–physician pairs (including primary affiliation, zip code, and specialty). There are 267 unique nominating physicians and 182 unique nominated physicians (i.e., opinion leaders). We present the distribution of nominations in the survey in Tables 1 and 2. Notably, more than 91% of physicians reported being influenced by only one opinion leader. Approximately 38% of the nominated doctors were named as opinion leaders by more than one physician. We do not observe any overlap between opinion leaders and nominating physicians.[6]

The typical opinion leader is a research-active specialist physician in the therapeutic category and is associated with a university-based hospital. Of the opinion leaders, 97.4% are specialists. More than 90% of the opinion leaders in the sample are associated with hospitals, and approximately 30% are affiliated with university hospitals. The average opinion leader has published approximately 7.2 peer-reviewed articles (SD = 9.68, minimum 0, maximum 40) in this therapeutic class, confirming his or her "expert" status. The survey also queried the nominating physicians about their mode of interaction with the opinion leader. The dominant mechanism of information transfer, as reported by the physicians, was direct contact—approximately 94.5% of nominating physician mentioned using the direct contact method. This provides some support for our model formulation (Equation 1) in which the nominating physician is assumed to respond to the prescriptions of the corresponding opinion leader. Other mechanisms of interaction included symposia/conferences (78%), clinical and/or hospital settings (67%), and scientific articles the opinion leader published (32%).

Prescription/Detailing Data

The secondary data set contains information on 24 months of new prescriptions for the combination-drug category for the entire universe of physicians in the therapeutic class of the disease. The data also contain information on the focal firm's monthly physician-level detailing activity in the category. Unfortunately, detailing activity for the other competing drugs in the category is not available at the indi-

[6]The pharmaceutical company asked the market research firm for a list of only those physicians who nominated an opinion leader. Thus, we can only measure the "treatment on the treated," in which the "treatment" is the effect of an opinion leader.

Table 2
DISTRIBUTION OF OPINION LEADER NOMINATIONS

Number of Nominations	*Number of Opinion Leaders*
1	112
2	56
3	7
4	5
8	1
17	1
Total	182

Notes: To be read as follows: There are 112 doctors who were nominated as opinion leaders by exactly 1 physician, 56 doctors who were nominated as opinion leaders by exactly 2 physicians, and so forth.

vidual physician level. We focus on the two largest drugs—Drug 2, the focal drug, and Drug 1, the main competitor (Drugs 3 and 4 have small market share). We supplemented the data with monthly national aggregate detailing for Drug 1 (detailing for Drugs 3 and 4 was negligible).

We provide descriptive statistics for the sample in Table 3. The table shows that nominating physicians typically write a larger number of prescriptions (almost twice that of the opinion leaders) and also receive a higher level of detailing (approximately 50% higher). This is consistent with anecdotal evidence that opinion leaders tend to be focussed more on medical research and academic publication than on practice. As Table 3 shows, opinion leaders are detailed less than the nominating physicians. This is likely to be a function of the category volume rule followed in the industry.

In computing z_{-it} in Table 3, we needed to decide whether to average all physicians in a zip code or only "active prescribers" in this disease category. We adopted the former approach because it is possible that the guidelines affected all doctors in the zip code, not just active prescribers, and because we did not have access to the database of physicians from which the company picked the random sample, we would otherwise need to make arbitrary guesses about who to include when computing z_{-it}. Given this, we present several robustness checks in which we drop z_{-it} from the regressions.

Change in Drug Usage Guidelines

An important aspect of the data is that they cover a period during which there was a significant change in the guidelines for the usage of drugs in this therapeutic category. This change in the treatment environment is important because it is during such times when family and general practitioners are most likely to seek and value the opinion of specialists in the category. In the Web Appendix (see http://www.marketingpower.com/jmroct10), we provide excerpts from published sources and the summary findings from a survey of physicians attesting that, in general, a change in guidelines usually increases the uncertainty in terms of physician prescription decisions. In our context, an exogenous change in the market occurred in May 2003 in the form of an announcement by the NIH releasing new treatment guidelines for the disease. Thus, we have behavioral data for 13 months before the guidelines were released and 11 months after. The guidelines suggested that against the prevailing norm, the initiation of treatment for severe cases of this condition should comprise at least two agents (or molecules). These guidelines tended to favor the so-called combination drugs in this category. A combination drug typically had the two agents in the same pill and results in "polytherapy." This had the obvious advantage of increasing compliance among patients and had higher efficacy. Thus, we expect all combination drugs, including Drug 2, to show an increase in prescriptions after the issuance of these guidelines. Before the issuance of the new guidelines, combination drugs were generally considered "aggressive" therapy.[7]

[7]The firm also showed us survey data collected from 319 physicians after the guidelines were released. These physicians noted that as a result of the guidelines, they would expect polytherapy to become more prevalent, leading to an increase in the prescription levels of combination drugs.

Table 3
SAMPLE DESCRIPTIVES

Variable	*M*	*SD*	*Minimum*	*Maximum*
Physician prescriptions	4.16	4.40	0	39
Opinion leader prescriptions	2.23	4.85	0	54
Physician details (Drug 2)	.75	1.35	0	11
Opinion leader details (Drug 2)	.52	1.15	0	10
Z_{-it}	.75	.94	0	13.7
$Z_{OPL,t}$	.40	.38	0	3.3

Notes: Number of observations in sample = 6960. Z_{-it} refers to the mean prescriptions of all other physicians in nominator i's zip code, and $Z_{OPL,t}$ refers to the mean prescriptions of all other physicians in the zip code of the physician's opinion leader.

We now document the changes in prescription and detailing behavior before and after the release of the guidelines. Figure 1 presents the distribution of mean monthly prescriptions in the combination-drug category for nominating physicians and opinion leaders before and after May 2003 (when the new treatment guidelines were introduced). For each physician, we compute the mean monthly new prescriptions before and after and present them in a box plot. As Figure 1 shows, both sets of physicians prescribe more of the combination-drug category. The mean increase in new prescriptions across both groups is approximately 10%.

We now turn our attention to the distribution of monthly detailing for Drug 2 across all physicians and months (Figure 2). Notably, the firm seems to deviate from the aforementioned detailing allocation rule just after May 2003. Figure 2 shows that the firm details more to opinion leaders after the change even though they write fewer prescriptions in the combination-drug category (see Figure 1). At the same time, detailing to the "regular" physicians remains relatively unchanged before and after the issuance of the guidelines (the difference is not statistically significant). This suggests that the firm has some knowledge of the opinion leader status of these specialists. As we noted previ-

Figure 1
DISTRIBUTION OF CATEGORY PRESCRIPTIONS BEFORE AND AFTER ISSUANCE OF NEW NIH GUIDELINES

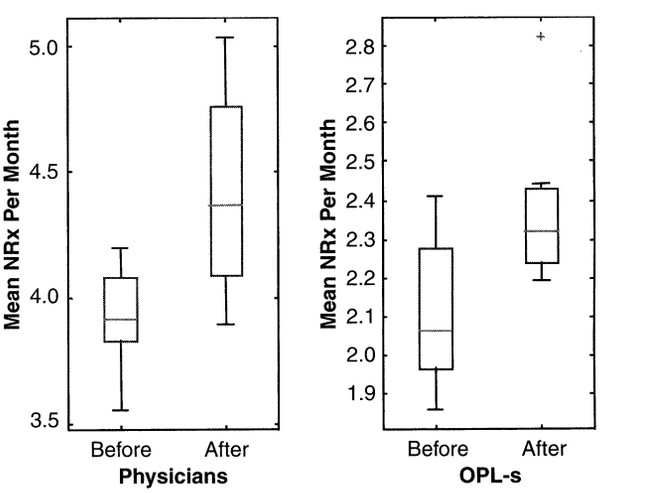
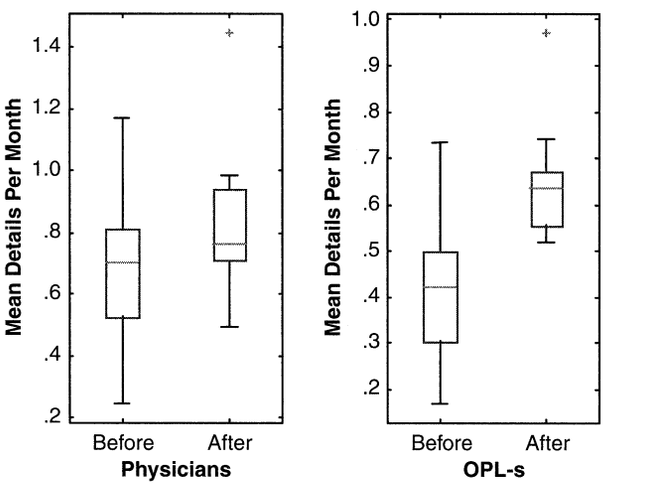


Figure 2
DISTRIBUTION OF DETAILING BEFORE AND AFTER ISSUANCE OF NEW NIH GUIDELINES



ously, the firm characterized about half the nominated opinion leaders as such before the survey.

To summarize, we have a unique data set that combines information from primary and secondary sources. We find that the NIH guidelines affect the prescription behavior of both nominating physicians and opinion leaders; on average, the prescription quantity goes up by 10%. We also find that the firm that markets Drug 2 changed the allocation of its detailing resources after the release of the guidelines. Specifically, before the release of the new guidelines, the firm devoted less detailing to the opinion leaders. However, this pattern reversed after the guidelines were released. This is consistent with feedback that detailing is the main instrument the firm uses in this therapeutic category in terms of managing its relationship with opinion leaders.

RESULTS

We now present the results from the data analysis. We first discuss some model-free evidence for the presence of correlated unobservables in prescriptions between the physicians and the opinion leaders. We then present results from various ordinary least squares (OLS) and fixed-effects IV specifications that control for the identification issues we discussed previously. We then discuss robustness of our main results to alternative model specifications. We conclude by using our estimates to measure the incremental value of targeted sales force activity to the opinion leaders in the data, which we use to estimate the social multiplier of detailing.

We use the mean prescriptions of other physicians in the focal physician's zip code and in the opinion leader's zip code to analyze the extent of correlated unobservables. We test whether unobservables are correlated more temporally (e.g., common time trends in prescription) or spatially (e.g., regional preferences for prescription). Our extensive analysis (available on request, or in a working-paper version of this article) reveals that much of the spurious correlation is along the temporal dimension, which we control for through time fixed effects; we find little evidence for spatially correlated shocks.

PARAMETER ESTIMATES

In the first subsection, we present results from our primary specification (Equation 4), in which we test whether opinion leaders' prescription behavior significantly affects physician prescriptions. We then present results for the reverse regressions (Equation 5) in which we test whether nominating physicians' prescription behavior significantly affects opinion leader prescriptions.

Effect of Opinion Leader Behavior on Physician Prescriptions

OLS estimation results. We begin by discussing the estimates from OLS and fixed-effects specifications of the model. The dependant variable in all regressions is the nominating physician's total new prescriptions in the category. The results from OLS linear regression appear in Table 4. Here, we show that the effect of the opinion leader's prescriptions is positive and significant. Furthermore, the magnitude of the effect is higher after month 13 (May 2003), when the NIH guidelines were introduced. This is consistent with a basic pattern of correlation between the physicians' and the opinion leaders' prescriptions. The OLS estimate of the detailing coefficient is also large and strongly statistically significant. However, this specification does not take into account that firms target heavier detailing toward higher-volume physicians. Thus, the OLS estimate is likely to be biased upward.

Fixed-effects estimation results. We present the results from the fixed-effects linear regression in Table 5. Recall that

Table 4
OLS REGRESSIONS OF PHYSICIAN PRESCRIPTIONS ON OPINION LEADER'S PRESCRIPTIONS

	Parameter	*t-Statistic*	*Parameter*	*t-Statistic*	*Parameter*	*t-Statistic*
Constant	4.079	8.03	4.039	7.93	4.173	8.19
Drug 2 detailing	.825	21.80	.826	21.82	.821	21.68
Drug 1 detailing (aggregate)	–.012	–1.38	–.012	–1.39	–.017	–1.91
Opinion leader's prescriptions	.071	6.77	.070	6.59	.048	.88
Z_{-it}			.059	1.09	.050	3.36
I(t ≥ May 2003)					.298	2.63
(Opinion leader's prescriptions) × I(t ≥ May 2003)					.039	1.86 (5.91)[a]
F	174.91		131.48		90.76	
R^2	.0701		.0703		.0726	

[a]t-statistic for total effect of the opinion leader's prescriptions after May 2003.

Table 5
FIXED-EFFECT REGRESSIONS OF PHYSICIAN PRESCRIPTIONS ON OPINION LEADER'S PRESCRIPTIONS[a]

	Parameter	*t-Statistic*	*Parameter*	*t-Statistic*	*Parameter*	*t-Statistic*
Constant	4.243	12.61	4.104	12.07	4.297	12.65
Drug 2 detailing	.014	.40	.012	.35	.001	.02
Drug 1 detailing (aggregate)	−.002	−.29	−.002	−.30	−.007	−1.27
Opinion leader's prescriptions	.002	.21	.002	.20	−.022	−1.79
Z_{-it}			.190	2.73	.138	1.99
$I(t \geq$ May 2003)					.369	4.92
(Opinion leader's prescriptions) × $I(t \geq$ May 2003)					.043	3.09 (2.63)[b]
F	46.95		47.02		47.47	
R^2	.6050		.6054		.6087	

[a]We included fixed effects for nominating physicians but do not report these values. Number of observations = 6960.
[b]t-statistic for total effect of the opinion leader's prescriptions after May 2003.

fixed effects control for both potential endogenous group formation and the targeting of detailing to high-volume prescribers. Not surprisingly, the magnitude of the detailing coefficient drops under fixed effects. The F-statistics from the regression strongly reject the null that all the fixed effects are zero. Though not reported, the hypothesis that the fixed effects are uncorrelated with the included variables is also strongly rejected in all specifications. The variable z_{-it} controls for both temporally and spatially correlated shocks and is statistically significant. The regressions in Columns 3 and 4 in Table 5 indicate that after we control for physician fixed effects and z_{-it}, the effect of opinion leader prescriptions after May 2003 is positive and strongly significant (t = 2.63). The results indicate that the effect of the opinion leader's prescriptions is significant only after the release of the guidelines. In particular, the opinion leader has little effect on prescriptions before May 2003, when the category was relatively stable. Given the timing of the survey (January–February 2004), these results cannot be explained by a mere measurement effect (i.e., opinion leaders and their behavior becoming more salient because the survey evoked the relationship between the nominating physician and the opinion leader). Notably, after we control for fixed effects, detailing is not significant in explaining physicians' prescriptions over time. This implies that targeting opinion leaders to leverage a social effect may be a better strategy for firms that want to increase category volume among physicians in this market. We explore this strategy toward the end of this section.

Fixed-effects IV results. We now discuss the results from the fixed-effects IV regressions. Recall that we use IVs only to accommodate potential biases arising from concerns related to simultaneity. The endogenous variable we instrument for is $x_{j(i),t}$. We use the detailing to the opinion leader, $D_{j(i),t}$, as well as the mean prescriptions of all other physicians in the opinion leader's zip code, $z_{-j(i),t,}$ along with squared terms of both, as instruments. The results from the first-stage regressions of $x_{j(i),t}$ on the instruments appear in Table 6. Columns 1 and 2 present the results from the regressions of the endogenous variable on only the excluded instruments, and Columns 3 and 4 present the results from the regressions of the endogenous variable on the entire first-stage matrix. The F-statistics from both regressions strongly reject the null that the exogenous instruments have no explanatory power. The first stage explains approximately 59% of the variation in the opinion leader's prescriptions. Thus, it is clear that we do not have a weak-instruments problem. The signs of the parameters also make sense intuitively. Thus, the instruments are working correctly.

The results for the fixed-effects IV regressions appear in Table 7. The IV estimates parallel the results from Table 5. As Columns 3 and 4 in Table 7 show, we find that opinion leaders' prescriptions have a statistically significant effect on the nominating physicians' behavior after May 2003 (t = 2.02), when the NIH guidelines were released. In Table 8, we repeat the IV regressions, including a full set of month fixed effects, and continue to find a significant effect for the opinion leaders' prescriptions after guidelines were issued. Note that the direction of change of the IV estimate incorporates both simultaneity concerns and the nature of the (unknown) correlation between the opinion leaders' prescriptions and the error term. Table 8 also presents sensitivity to the inclusion of z_{-it} in the regression. We present robustness to z_{-it} in the following way: From our previous analysis, we find that the z_{-it} are capturing unobservables that are correlated mostly along the temporal, rather than

Table 6
FIRST-STAGE REGRESSIONS OF OPINION LEADER'S PRESCRIPTIONS ON INSTRUMENTS

	On Only Excluded Instruments		*On First-Stage Instrument Matrix*[a]	
	Parameter	*t-Statistic*	*Parameter*	*t-Statistic*
Drug 2 detailing to nominating physician			−.041	−1.02
Aggregate Drug 1 detailing (1000s)			−.014	−2.11
$I(t \geq$ May 2003)			.002	.03
Z_{-it}			−.057	−.72
Drug 2 detailing to opinion leader	2.311	20.89	1.296	14.37
(Drug 2 detailing to opinion leader)^2	−.167	−7.29	−.110	−6.30
$Z_{-OPL,t}$	2.173	6.72	1.765	4.04
$(Z_{-OPL,t})^2$	−.993	−5.40	−.243	−1.06
Constant	.724	7.42	−.816	−1.00
F	343.88		47.97	
R^2	.1651		.5904	
N	6960			

[a]We include fixed effects for nominating physicians but do not report these values. Number of observations = 6960.

Table 7

FIXED-EFFECT IV REGRESSIONS OF PHYSICIAN PRESCRIPTIONS ON OPINION LEADER'S PRESCRIPTIONS[a]

	Parameter	*t-Statistic*	*Parameter*	*t-Statistic*	*Parameter*	*t-Statistic*
Drug 2 detailing	.078	1.95	.073	1.83	.000	.01
Drug 1 detailing (aggregate)	.011	.32	.010	.27	−.007	−1.21
Opinion leader's prescriptions	−.001	−.18	−.001	−.19	.011	.28
Z_{-it}			.188	2.70	.139	1.99
I(t ≥ May 2003)					.386	3.80
(Opinion leader's prescriptions) × I(t ≥ May 2003)					.032	2.06 (2.02)[b]
Sargen J-statistic (R^2)	10.572 (.789)		10.354 (.790)		9.476 (.793)	
χ^2 *p*-value (d.f.)	.0143 (3)		.0158 (3)		.1485 (6)	

[a]We included fixed effects for nominating physicians but do not reported these values. Number of observations = 6960.
[b]t-statistic for total effect of the opinion leader's prescriptions after May 2003.

Table 8

REGRESSIONS OF PHYSICIAN PRESCRIPTIONS ON THE OPINION LEADER'S PRESCRIPTIONS WITH FULL SET OF PHYSICIAN AND PERIOD FIXED EFFECTS[a]

	Fixed Effects				*Fixed-Effects IV*			
	Parameter	*t-Statistic*	*Parameter*	*t-Statistic*	*Parameter*	*t-Statistic*	*Parameter*	*t-Statistic*
Constant	1.678	2.17	1.627	2.10				
Drug 2 detailing	−.009	−.25	−.010	−.27	−.009	−.25	−.010	−.26
Drug 1 detailing (aggregate)	.042	3.26	.042	3.22	.139	3.95	.137	3.88
Opinion leader's prescriptions	−.023	−1.83	−.023	−1.82	.001	.02	.000	−.01
Z_{-it}			.119	1.71			.120	1.72
(Opinion leader's prescriptions) × I(t ≥ May 2003)	.042	3.02 (2.52)[b]	.042	3.01 (2.52)[b]	.031	2.06 (2.01)[b]	.031	2.05 (2.00)[b]
Period fixed effects	Yes		Yes		Yes		Yes	
F	47.710		47.730					
R^2	.6125		.6127		.795		.795	
Sargen J-statistic					7.299		7.302	
χ^2 *p*-value (d.f.)					.2941 (6)		.2938 (6)	

[a]Number of observations = 6960.
[b]t-statistic for total effect of the opinion leader's prescriptions after May 2003.

spatial, dimension. Thus, these effects may be captured through time fixed effects. We also explored month fixed effects that are specific to each zip code, but we find that the data are too thin to support this specification. In Table 8, we present specifications with a full set of month fixed effects, with and without including z_{-it}, and we find that the results continue to be robust. We interpret these results as evidence that there are peer effects in prescription behavior in these data. In subsequent sections, we examine whether the peer effects have economically significant consequences for the firm's marketing efforts in this industry.

Effect of Physician Behavior on Opinion Leader Prescriptions

We now check whether peer effects in this category are asymmetric by testing whether the prescription behavior of nominating physicians has statistically significant effects on the actions of their opinion leaders. Rather than assume that each nominated doctor has a disproportionate impact on the physician–doctor dyad, we want to verify whether the data support the notion that the nominated doctors are experts and are less affected by the behavior of the nominators. The specifications we estimate correspond to Equation 5. For brevity, we present the fixed-effect IV regressions that incorporate a full set of fixed effects for the opinion leaders and use the detailing to the nominating physician and the mean prescriptions of all other doctors in the nominating physician's zip code (along with squared terms) as instruments for the nominating physician's prescriptions. We present specifications with and without the mean prescription behavior of all other doctors in the opinion leader's zip code as controls for unobservables and with and without a full set of month fixed effects. The results appear in Table 9. We find that the nominating physician's behavior does not have a statistically significant effect on the opinion leader's prescriptions. These results hold after we allow for a post-guideline interaction effect (Columns 3 and 4). Detailing to the opinion leader has a strong significant effect on prescriptions. We take the results in Table 9 as evidence that peer effects tend to be asymmetric, at least in this particular context.

ROBUSTNESS

We now present several specification checks to ascertain the robustness of the findings. In this section, we verify that the qualitative nature of the findings is robust to alternative specifications of the way the opinion leader's behavior affects the nominating physician. We explore five different specifications that vary in the timing and nature of the effect of the opinion leader's behavior on the nominating physi-

Table 9

FIXED-EFFECTS IV REGRESSIONS OF OPINION LEADERS' PRESCRIPTIONS ON NOMINATING PHYSICIANS' PRESCRIPTIONS[a]

	Parameter	*t-Statistic*	*Parameter*	*t-Statistic*	*Parameter*	*t-Statistic*	*Parameter*	*t-Statistic*
Nominator prescriptions	.001	.02	−.037	−.48	−.058	−.76	−.041	−.46
(Nominator prescriptions) × I(t ≥ May 2003)			.044	.90 (.13)[b]	.063	1.31	.058	1.13 (.25)[b]
Drug 2 detailing	.136	4.35	.115	3.61	.110	3.41	.115	3.59
Drug 1 detailing (aggregate: 1000s)	−.009	−2.06	−.012	−2.70	.000	.01	.008	.21
Z_{-it}	.752	3.22	.538	2.28	.480	2.03		
I(t ≥ May 2003)			.066	.34				
All month fixed effects?	No		No		Yes		Yes	
J-statistic (Sargen)	6.81		5.23		4.85		2.14	
χ^2 *p*-value (d.f.)	.0782 (3)		.2645 (4)		.3027 (4)		.3437 (2)	

[a]New treatment guidelines issued in May 2003. We estimated fixed effects for each opinion leader but do not report these values. We report robust t-statistics.
[b]t-statistic for total effect of the opinion leader's prescriptions after May 2003.

cian. Let $f_{OPL}(t)$ denote the variable summarizing the opinion leader's behavior on the physician's prescriptions in month t. The various specifications of $f_{OPL}(t)$ we consider are as follows:

- $f_{OPL}(t) = x_{j(i),t-1} \equiv$ lagged opinion leader prescriptions.
- $f_{OPL}(t) = I(x_{j(i),t}) \equiv$ indicator for whether the opinion leader prescribed this month.
- $f_{OPL}(t) = I(x_{j(i),t-1}) \equiv$ lagged indicator for whether the opinion leader prescribed.
- $f_{OPL}(t) = \Sigma_{\tau=1}^{t} x_{j(i),\tau} \equiv$ cumulative prescriptions by the opinion leader as of this month.
- $f_{OPL}(t) = \Sigma_{\tau=1}^{t-1} x_{j(i),\tau} \equiv$ lagged cumulative prescriptions.

Table 10 presents the results from linear fixed-effects IV regressions in which each of the five $f_{OPL}(t)$ functions are included as regressors. Again, we use $D_{j(i),t}$, as well as the mean prescriptions of all other physicians in the opinion leader's zip code, $z_{-j(i),t}$, along with squared terms of both, transformed analogously to $f_{OPL}(t)$ as instruments (for precise definitions, see the last two rows of Table 10). Table 10 shows that the main message from our early results remains robust to these alternative definitions: Opinion leaders have a statistically significant effect on prescriptions after the NIH guidelines were issued. Table 10 also suggests some evidence of recency effects in social influence in that only recent actions of opinion leaders matter (total cumulative prescriptions do not have an effect). We also checked robustness to the assumption of linearity by estimating a fixed-effects negative binomial regression model (Hausman, Hall, and Griliches 1984). This model accounts for the count nature of prescription data, allows for overdispersion, and also accommodates potential nonlinearities in the prescription response function. The results from the negative binomial regression (available in the Web Appendix at http://www.marketingpower.com/jmroct10) support the basic find-

Table 10

ROBUSTNESS CHECKS: FIXED-EFFECTS IV REGRESSIONS OF PHYSICIAN PRESCRIPTIONS ON FUNCTIONS OF OPINION LEADER'S PRESCRIPTIONS

	1		*2*		*3*		*4*		*5*	
$f_{OPL}(t)$	$x_{j(i),t-1}$ Opinion leader prescriptions last month		$I(x_{j(i),t})$ Did the opinion leader prescribe?		$I(x_{j(i),t-1})$ Did the opinion leader prescribe last month?		$\Sigma_{\tau=1}^{t} x_{j(i),\tau}$ How much has the opinion leader prescribed as of this month?		$\Sigma_{\tau=1}^{t-1} x_{j(i),\tau}$ How much has the opinion leader prescribed as of last month?	
	Parameter	*t-Statistic*	*Parameter*	*t-Statistic*	*Parameter*	*t-Statistic*	*Parameter*	*t-Statistic*	*Parameter*	*t-Statistic*
Drug 2 detailing	−.021	−.56	−.018	−.47	−.023	−.61	−.019	−.50	−.019	−.51
Drug 1 detailing (aggregate: 1000s)	.412	3.37	.347	2.20	.315	1.80	.526	3.95	.525	3.95
$f_{OPL}(t)$	−.018	−.43	.582	.72	1.339	1.33	−.007	−1.91	−.007	−1.19
$f_{OPL}(t) \times I(t \geq$ May 2003)	.036	3.04 (2.60)[a]	.303	3.64 (2.82)[a]	.802	3.30 (2.75)[a]	.005	1.94 (−1.33)[a]	.005	1.93 (−1.58)[a]
Z_{-it}	.112	1.57	.104	1.45	.133	1.77	.126	1.75	.126	1.75
Month fixed effects included?	Yes		Yes		Yes		Yes		Yes	
Instruments[2]	$\mathbf{w}_{j(i),t-1}$		$\mathbf{w}_{j(i),t}$		$\mathbf{w}_{j(i),t-1}$		$\Sigma_{\tau=1}^{t} \mathbf{w}_{j(i),\tau}$		$\Sigma_{\tau=1}^{t} \mathbf{w}_{j(i),\tau-1}$	
R^2	.794		.792		.794		.791		.791	
χ^2 *p*-value (d.f.)	.262 (4)		.316 (4)		.1197 (4)		.7642 (4)		.7523 (4)	

[a]t-statistic for total effect of the opinion leader's prescriptions after May 2003. Dependant variable in all IV regressions is physician prescriptions = y_{it}.

Notes: Each column presents results from fixed-effects IV regressions of a physician's prescriptions on a function of the opinion leader's prescriptions. For example, Column 1 presents results from fixed-effects IV regressions of a physician's prescriptions on a lagged values of his or her opinion leader's prescriptions; the effect of lagged opinion leader prescriptions preguidelines is −.018 and postguidelines is −.018 + .036 = .018. We estimated physician and month fixed effects in all specifications but do not report them. The tern **w** denotes the vector of available IVs: $\mathbf{w}_{j(i),t} = (x_{j(i),t}, x^2_{j(i),t}, z_{-j(i),t}, z^2_{-j(i),t})$.

ing that opinion leader prescriptions have a positive, significant effect on physician prescriptions after the NIH guidelines.

Finally, to check whether the peer effects varied by physician and opinion leader characteristics, we also estimated versions of the linear fixed-effects model (though we do not report the results here) in which we interacted opinion leader prescriptions with opinion leader and physician characteristics (specialty and number of published articles) as well as the channel of social interaction. These interactions were not statistically significant.

TARGETING SALES FORCE ACTIVITY TO OPINION LEADERS

We now use the results to explore the implications of targeting detailing at opinion leaders. In particular, we want to estimate the social multiplier associated with detailing in this industry. An incremental detail to an opinion leader has two effects: It increases the opinion leader's prescriptions, which in turn increases the corresponding physician's prescriptions. We use our model estimates to measure both effects.

We first estimate a prescription response function for the opinion leaders (given the results from the previous section, we do not include the nominating physician's prescriptions as a covariate). Table 11 presents the results from both the OLS and the fixed-effects specifications. As we noted previously, the effect of detailing is strongly significant for opinion leaders. As we expected, the coefficient of detailing drops in absolute magnitude when moving from the OLS to the fixed-effects model. However, the marginal effect of detailing of Drug 2 is .136; this is significantly larger than the corresponding effect for nominating physicians, possibly because firms use better detailers to target opinion leaders.

We use these estimates to compute the incremental revenue of an additional detail of both Drugs 1 and 2 to an opinion leader. Note that the total marginal effect of Drug 1 and Drug 2 detailing is .127 (.136 – .009). In the aggregate data for the category, we find that, on average, one new prescription generates two renewals (obtained by subtracting 1 [the new prescription] from the number of total prescriptions generated per prescriptions for our therapeutic category). Furthermore, the revenue from one additional dose of a combination drug is approximately $50.00. Thus, the incremental revenue from an additional prescription is approximately $150.00, and the incremental revenue from one additional detail in the category is $19.05 ($150.00 × .127). We then compute the incremental revenue arising from the increase in opinion leader prescriptions for a nominating physician for each month after the guideline change. We use the results from the main regression in the last column in Table 8. After the issuance of the NIH guidelines, the additional revenue from a nominating physician as a result of the increase in opinion leader prescriptions is .127 × .032 × $150 = $.61. From Table 2, the average opinion leader in our sample influences 1.56 physicians. Thus, the total effect arising from a social interaction is 1.56 × $.61 = $.95. For the average opinion leader, this accounts for approximately 4.76% [.95/(19.05 + .95)] of the total revenue effect of detailing, implying a social multiplier of detailing in the category of 1.05. For the top opinion leader, who influences 17 physicians, we obtain a social multiplier of approximately 1.35%. (Figure 3 provides the distribution of social multipliers for the physicians in our data.) Note that these numbers are likely to be the lower bound on the peer effect because z_{-it} could contain some prescriptions generated through the opinion leader effect. Even so, we find that peer effects alone provide a 5%–35% lift in the return on investment from targeting marketing at opinion leaders. A takeaway from the analysis is that opinion leader identification is of key importance to the company.

Table 11

RESPONSIVENESS OF OPINION LEADER PRESCRIPTIONS TO DETAILING

	OLS		*Fixed Effects*[a]	
Variable	*Parameter*	*t-Statistic*	*Parameter*	*t-Statistic*
Constant	2.167	4.05	2.358	9.02
Drug 2 detailing	1.614	34.09	.136	4.35
Drug 1 detailing (aggregate)	–.018	–1.98	–.009	–2.06
$Z_{-OPL,t}$	.679	4.75	.753	3.44
F	426.33		12.79	
R^2	.155		.826	
N	6670			

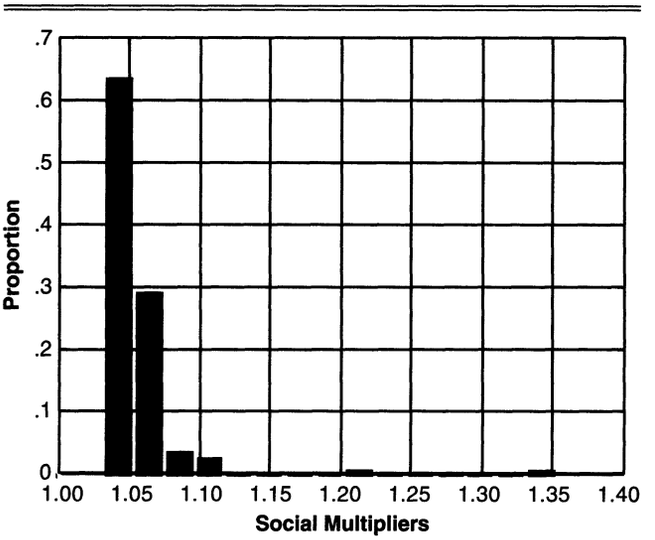
[a]We estimated fixed effects for each opinion leader but do not report these values. We report robust t-statistics.

CONCLUSIONS

This article adds to the small but growing literature that documents peer effects using individual consumer-level data. The unique and novel features of our application enable us to make the following contributions: First, we document the existence of important, statistically significant asymmetric peer effects that are robust across model specifications. Second, we document asymmetries in response to marketing activity across nominators and opinion leaders. Third, we combine the estimates of peer effects and marketing response to quantify the value to firms of directing targeted sales force activity to these opinion leaders through

Figure 3

DISTRIBUTION OF SOCIAL MULTIPLIERS IN DATA



the social multiplier. This social multiplier of detailing is economically significant and therefore has the potential to inform the firm's resource allocation policies.

Our analysis has some limitations. First, we had no control over the survey the firm conducted. As a result, the network in our approach is "thin" relative to that in previous research. In other words, our sample and the resultant network may be more idiosyncratic than a typical network of prescribing physicians. Although the literature has not established what a typical physician prescription network looks like, it is clear that richer networks could allow researchers to investigate the effect of network structure on the peer effect. Second, given that we only have the treatment effect "on the treated," we cannot compute the social multiplier across all types of physicians. Conversations with the firm suggest that most general practitioner doctors in this category are subject to some form of peer influence; thus, treatment on the treated is likely to be the right policy-relevant treatment effect. Third, we do not have direct measurements on the information or opinions the opinion leader shared with the nominating physician. Although this is a limitation, the results are robust to many functions of the opinion leader's prescription activity. As in much of the literature, we do not have data on the identities of the sales force and patients of the physicians in our sample. We also do not have access to marketing activity for all drugs in this category at the individual physician level (e.g., we have only aggregate detailing for Drug 1 [the market leader]). Finally, the data do not enable us to accommodate unobserved heterogeneity in peer effects; conversely, given that we found limited evidence for observed heterogeneity in peer effects, this may not be unreasonable for these data. We hope that with access to richer data, these limitations will be addressed in future work.

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