Financial Conflicts of Interest in Medicine*

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Abstract: We use the geographic distance between a doctor's office and drug company headquarters to instrument for the likelihood of pecuniary transfers, such as meals or speaking fees. Doctors tilt prescriptions in favor of the paying firm's drugs, shifting away from both branded and generic substitutes. Larger transfers cause larger shifts in prescriptions. We explore two potential explanations: 1) persuasion and/or information flow, and 2) rent seeking. Payments increase prescriptions of branded drugs over *generic equivalents*, situations where information cannot play a large role. However, doctors residing in states known to be corrupt in other ways (e.g., electoral fraud) are much more sensitive to payments from the drug industry.

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I. Introduction

When an informed buyer enlists the help of an expert, the potential for conflicts of interest often arises. Take for example a wine sommelier working at a fine restaurant. Is the recommended Pinot Noir the optimal choice for a given meal, or has the restaurant encouraged the sommelier to push a particular brand, perhaps trying to rid itself of a few extra bottles?

While such rent-seeking behavior might not surprise many – who hasn't questioned the necessity of an auto repair – that financial conflicts of interest could influence their physician's advice might be both less expected and more worrisome. For one, doctors are already highly paid, with most falling in the top 5% of the income distribution within the US (U.S. Bureau of Labor Statistics, 2010; U.S. Census Bureau, 2010). Moreover, intrinsic motivation is thought to be important in medicine, with the goal of optimizing patient health being a paramount objective (Heyes, 2005; Rebitzer and Taylor, 2011).

On the other hand, this need not coincide with the objective of pharmaceutical firms, who have strong incentives to maximize prescriptions. Consequently, when drug companies have financial relationships with physicians, the potential for biased decision-making arises.

This possibility has not gone unnoticed by legislators. Beginning in 2014, the Physician Payments Sunshine Act will take effect, when drug and medical device manufacturers will be required to publicly report payments to physicians and teaching hospitals (Centers for Medicare & Medicaid Services, 2013). A presumption underlying this legislature is that transfers from the medical industry create conflicts of interest for providers that, in turn, influence their behavior. This paper evaluates that presumption.

We have three specific goals. First, using micro-level data on payments to individual physicians and their prescriptions, we explore whether a positive association exists at all. Second, we seek to understand whether payments exert a causal influence on physician behavior, or whether omitted doctor or firm attributes create spurious correlation. Finally, provided that a causal relation does appear to exist, we dig deeper into the mechanism. For example, do drug companies use marketing dollars to educate physicians, thereby allowing them to make more informed decisions? Or, is rent-seeking a better explanation, where doctors increase prescriptions in exchange for financial benefit?

To address these questions, we collect data on payments to physicians provided on the *Dollars for Docs* website, hosted by independent journalist consortium ProPublica (ProPublica, 2013a). *Dollars for Docs* is a searchable web interface allowing a user to observe transfers from pharmaceutical firms to specific physicians. In 2011, twelve companies reported payments, including most of the major firms, e.g., Pfizer, Merck, GlaxoSmithKline, AstraZeneca, and Johnson & Johnson. Although reporting is not standardized (yet, see above), most firms break down payments by dollar amount and type, such as gifts, meals, speaking, travel, consulting, and on occasion, proprietary research.

To this dataset we merge prescription information for each doctor as reported from Medicare (Part D) reimbursements, also provided by ProPublica on its *Prescriber Checkup* website (ProPublica, 2013b). This combination allows us to conduct cross-sectional regressions comparing prescribing patterns of doctors who differ in whether, or how much, they are paid by a given drug company.

Our sample is comprised of 334,000 doctors, roughly half of all physicians licensed in the U.S. Pairing each doctor *i* in our sample to each of the twelve

pharmaceutical firms *j*, we ask whether *j* to *i* transfers are associated with more prescriptions for company *j*'s drugs written by doctor *i*. The data indicate a positive association that scales with transfer size. Small payments (e.g., under $1000 for the year) increase prescriptions by about nineteen on average. This increases to almost sixty when the yearly transfer exceeds $1000. This payment-prescription pattern survives both doctor and company fixed effects, meaning that neither unobserved attributes of doctors nor drug companies can drive the relation.

It remains possible, however, for omitted heterogeneity at the doctor-firm level to generate spurious correlations between payments and prescriptions. To see how, consider a physician who specializes in a condition for which relatively few treatments are available, such as Alzheimer's disease. When a new drug is brought to market – take Novartis's introduction of Exelon for dementia in 2007 as an example – such specialist physicians are likely valuable sources of information about side effects, patient compliance, and so on. In these cases, payments from drug companies say, in the form of meals or consulting, need not have any causal impact on prescription behavior, and accordingly, would not qualify as a conflict of interest.

Ruling out such unobserved doctor-firm heterogeneity requires exogenous payments to physicians, which are uncorrelated with their counterfactual prescription patterns absent transfers from drug companies. We approximate this ideal scenario using the geographical distance between each doctor's office and the headquarters of each drug company. Intuitively, the idea is that doctors located closer to a firm's headquarters are more likely to be in contact with its sales force – doctors near Indianapolis probably encounter more Eli Lilly drug representatives than their peers in St. Louis – but due to proximity, rather than unobserved determinants of prescribing behavior.

When we instrument for payments using distance from headquarters for each doctor-firm pairing, we find an even stronger prescription-payment effect relative to the benchmark OLS estimates. This analysis is useful not only for identifying causation, but also for inferring the objectives of drug firms, which is otherwise not observable. Comparing the OLS and IV estimates, the fact that the former are biased *downward* indicates that drug companies focus their marketing efforts on the extensive margin of prescribers.

Our remaining analysis takes as given a causal relation between drug company payments and physicians' prescription choices, and attempts better identify the mechanism. There are three possibilities. First, doctors may become better informed via interactions with drug companies, which may alter prescribing behavior. Second, doctors may simply *think* they have become better informed, but in reality, have not. We refer to these possibilities, respectively, as "informative persuasion" and "non-informative persuasion." The final possibility is pure rent seeking, whereby doctors tilt prescriptions toward firms from which they derive pecuniary benefits, either present or expected.

To evaluate the persuasion hypotheses, we measure the payment-prescription effect among subsamples where information flow should play a far reduced role relative to the entire sample. Our first such comparison is between Astrazeneca's Crestor and Pfizer's Lipitor, twin cholesterol-reducing blockbusters in the "statin" class. Both drugs are widely prescribed to large cross-sections of the American population, making it unlikely that unobserved differences in patient attributes could generate meaningful differences between doctors. Yet, in head-to-head comparisons, we find that payments from Pfizer tilt the balance in favor of Lipitor (with larger payments having a bigger

effect), with transfers from AstraZeneca being associated with more prescriptions of Crestor.

The second subsample we analyze eliminates entirely the ability for information flow (or its perception) to generate correlation between payments and prescriptions. We compare a number of branded drugs to their generic equivalents, i.e., not simply drugs in the same drug class. That we find a positive payment-prescription relation here is virtually impossible to square with information flow from drug companies, even for the most uninformed (hypothesis 1 above) or naïve (hypothesis 2) doctors.

We conclude by providing more direct evidence of the rent seeking hypothesis. As we did for the other alternatives, we examine subsets of the data where the effects of corruption, if true, should be strongest. Using data on federal convictions of corruption-related crime (Glaeser and Saks, 2006) to proxy for the corruption rate of each U.S. state, we compare the least corrupt U.S. states (e.g., Minnesota, Oregon, Nebraska) to the most corrupt (e.g., Louisiana, Mississippi, Illinois), finding a prescription-payment magnitude nearly twice as large in the latter group. This is particularly striking given that the most corrupt states are among the poorest (many in the Southeast), and yet the ratio of branded-to-generic drugs is highest in precisely this region.

Our findings provide an empirical benchmark for assessing the impact of the upcoming Sunshine Act of 2014, given that our sample predates its implementation by four years and even most discussion by two years. Although only a portion of our analysis can distinguish between persuasion and rent-seeking behavior, these mechanisms need not be separated in order for the Sunshine Act to be warranted. Specifically, if either welfare-reducing phenomenon exists then its effects may be ameliorated by the legislated transparency of pharmaceutical firm payments.

The idea that physicians face potential conflicts of interest is not new (American College of Physicians, 1990; Medicare Payment Advisory Committee, 2009). For example, well after ethical standards describing appropriate relationships between pharmaceutical firms and physicians were developed, there was substantial concern that payments affect or reward clinical behavior (Coyle, 2002). Controversy remains in part because existing empirical evidence characterizing pharmaceutical industry and physician relationships relies exclusively on opinion surveys (Madhavan et al., 1997; Wazana, 2000; Katz et al., 2010), rather than on directly observed clinical behavior. The critical component of our study is the availability of prescription-level data making it possible to directly test for a relation between payments from the pharmaceutical industry and physician prescribing behavior.

The remainder of the paper is organized as follows. In Section II, we describe our payment and prescription data and provide summary statistics. Section III provides evidence of a positive relationship between payments and prescriptions, while Section IV explores potential mechanisms for the patterns we observe. We conclude in Section V.

II. Data

We draw on several data sources to study the relationship between industry payments and physician prescribing behavior. First, we construct a listing of payments from pharmaceutical firms to doctors using ProPublica's *Dollars for Docs* database(ProPublica, 2013a). ProPublica is an investigative journalism newsroom that makes data available on industry payments and prescribing patterns. *Dollars for Docs* is an online, searchable database of payments that were made publicly available by pharmaceutical firms either voluntarily or due to legal settlements. The data begin in 2009, and we download all data between 2009 and 2011 for our study. Data for twelve

pharmaceutical firms were available during this time period. Each observation in the dataset is from a named pharmaceutical firm to a named provider and includes time period (year), payment type, and specific or categorical dollar amount.

Table 1 lists several summary statistics of reported payments to providers separated by pharmaceutical firm in Panel A and meal category in Panel B. Reporting is voluntary or arising from legal settlements and is therefore idiosyncratic. Casual inspection of Table 1 suggests these idiosyncrasies explain much of the variation across pharmaceutical firms in the number of providers that receive payment. For example, Merck only reported payments made for speaking over the 2009 to 2011 period. Because payments for speaking are less common, we identify approximately 2,000 providers that Merck made payments to annually.

On the other hand, AstraZeneca began reporting only speaking fees in 2010 but increased its scope for reporting to include meals, gifts, consulting, research and travel in 2011. This expanded disclosure by AstraZeneca increased the number of providers receiving payment from 2,381 in 2010 to 116,643 in 2011. Looking at the last row of Panel A it is clear that the total dollar amount of payments made by pharmaceutical firms increased substantially, from $188.86 million in 2009 to $773.05 million in 2011, but this increase was driven primarily by expanded disclosure (as in the AstraZeneca example) rather than a dramatic increase in actual payments made.

The average dollar amount and prevalence of payments also varies considerably by type of payment. For example, most reported research payments were greater than $10,000, but they were relatively infrequent. Reported consulting, speaking, and travel payments were also large, with many payments in those categories in the thousands. In contrast, the median reported payments for gifts and meals were $72 and $37,

respectively, and reported meal payments were by far the most frequent, comprising more than three-quarters of all reported payments.

ProPublica also provides a database of prescribing patterns called *Prescriber Checkup* (ProPublica, 2013b). *Prescriber Checkup* is a searchable database of health care providers and the number of Medicare Part D prescriptions (including refills) they wrote for specific drugs in 2010 when that provider's number of such prescriptions exceeded 50 (to protect patient confidentiality). Importantly, these data comprise the universe of such provider-prescription information for the U.S. in 2010. ProPublica aggregated these data from 2010 Medicare Part D insurance claims that were obtained from the Centers for Medicare and Medicaid Services under a Freedom of Information Act request. The unit of observation in the *Prescriber Checkup* database is (Doctor, Drug), so for each doctor we know how many Medicare Part D prescriptions she wrote for each drug (provided she wrote at least 50). We use drug names to match drugs to their appropriate pharmaceutical firm (e.g., Lipitor matches with Pfizer). Of the 1,685 drugs in the *Prescriber Checkup* database, 239 match to one of our twelve pharmaceutical firms.

The *Prescriber Checkup* database also includes summary information by doctor including the total number of Medicare Part D claims, the total number of patients receiving at least one claim and identifying information such as name, city, state and medical specialty. We downloaded the *Prescriber Checkup* database and used the identifying information to match providers from the *Prescriber Checkup* database to the *Dollars for Docs* database. Table 2 provides some summary statistics from the matched sample. Of the 334,086 doctors in the *Prescriber Checkup* database we identify 192,484 (58%) as having received at least one payment from our twelve pharmaceutical firms between 2009 and 2011. Panel A of Table 2 also suggests that doctors who are paid by pharmaceutical firms are more active than those who are not. For example, the average

doctor in our sample generated 2,980 Medicare Part D claims in 2010 from 217 patients (13.7 claims per patient). However, doctors who received payments from pharmaceutical firms generated 3,566 claims and saw 243 patients (14.7 claims per patient). Prescription rates are also higher for paid physicians among branded claims, i.e. prescriptions for drugs made by our twelve pharmaceutical firms. Panel A indicates that the average doctor generates 192 branded claims (0.88 per patient) but a paid doctor generates 258 branded claims (1.06 per patient).

Panel B of Table 2 provides summary statistics for (Doctor, Firm) pairs, which is the unit of observation in our main analysis. We choose (Doctor, Firm) rather than (Doctor, Drug) as the appropriate unit because we are unable to observe whether a payment was made to a doctor in connection with a specific drug; rather, we only observe total payments by each drug company to each doctor. Panel B indicates that when (Doctor, Firm) is the unit of observation we observe payments to doctors 11% of time among the over 4 million observations. When a payment is observed, the average size is $1,766 with a standard deviation of $21,403. Given the median payment is $57, the mean and standard deviation are strongly influenced by a handful of extremely large payments for research, speaking and consulting.

III. Drug company payments and physician behavior

This section documents a positive cross-sectional relation between payments from drug companies and prescription choices by physicians. Two types of evidence are presented. First, in subsection A, we aggregate all pharmaceutical firms into a single unit, and show that total payments from the *overall drug industry* are associated with higher ratios of branded-to-generic prescriptions. We then progress toward a finer unit of observation in subsection B, where we consider each doctor-firm pairing. The results

of this analysis suggest that payments from *specific companies* translate to higher prescription rates for those companies' drugs.

A. Variation across doctors

In this section, we consider the distribution of prescription rates and payments by doctor. Recall that we observe for each doctor the specific number of prescriptions for each drug manufactured by our twelve pharmaceutical firms, subject to at least fifty units being prescribed. We also observe for each doctor the total number of Medicare claims and patients. The opportunity to observe both types of prescriptions -- i.e., the brand name drugs manufactured by the twelve drug companies as well as non-brand name drugs – by doctor is useful when making inferences about the effect of payments on prescribing behavior.

To see why, consider the following comparison. In our sample of 334,086 doctors, slightly fewer than half (154,654) did not receive payment from any pharmaceutical firm in our sample. For this group, the rate at which brand name drugs, from any of our twelve firms, were prescribed was 0.48 claims per patient. At the other end of the spectrum, the third- and second-to-highest decile of paid physicians (with payment amounts totaling several hundred dollars) prescribe brand name drugs at a much higher rate, respectively, at 0.80 and 0.96 claims per patient. Doctors in the top payment decile, with gifts, meals, speaking fees and other transfers exceeding thousands of dollars on average, prescribe brand name drugs at a rate of 1.20 per patient.

While these differences are large, causal inferences are made difficult by the fact that payments are endogenous to both doctor and patient characteristics. One example is that some specialties (e.g., internists) are more likely to prescribe drugs than others (e.g., radiologists). Consequently, if drug companies disproportionately target specialties

with high prescription rates, we would expect to find a correlation between payments and prescription rates, even if such targeting were completely ineffective.

The comparisons shown in Figure 1 allow us to evaluate this hypothesis. To capture cross-specialty differences, we first place doctors into deciles ranked by average prescription rates using only generic drugs. Each decile is represented by a different shaded line, with the darkest line corresponding to the 10% most heavily prescribing doctors (about 30 non-branded claims per patient), and the lightest line to the 10% least prescribing doctors (about 2 non-branded claims per patient).

Then, within each of these deciles, we sort doctors based on the amount they receive from any of the pharmaceutical firms in our sample, from the least (none) on the far left, to the most on the far right. Starting with the darkest contour, we see an increase of about 50%, from roughly two brand-name claims per patient for doctors in the least-paid decile, to about three in the most. Moreover, most of the increase is in the last two deciles, which also corresponds to the steepest increase in payment amounts, both in percentages and dollars.

Moving down the figure we observe even larger increases in successive contours, with percentage differences between the unpaid and highest paid deciles of 129%, 150%, 184%, 184%, 213%, 175%, 79%, 49%, and 106%. Averaged across all groups, doctors in the top 20% of the payment distribution prescribe approximately twice the rate of brand name drugs compared to doctors in the bottom 20%.

The bottom panel (B) of Figure 1 shows the results of the same exercise, except that we now plot the prescription rates for generic drugs. While initially this may seem redundant given that contours are generated using generic prescription rates, the remaining concern is that sorting into ten groups may not be precise enough. If, for example, we found increasing non-branded prescription rates within each contours,

there would be concern that Panel A simply reflected further differences in average prescription rates not captured by decile sorts.

However, this does not appear worthy of concern. In virtually every decile, generic prescription rates *decrease* with payment, most so between the 9th and 10th decile. Rather than prescription rates for brand-name drugs simply reflecting heterogeneity in baseline prescription frequencies, there is apparent substitution from generics to brand name drugs, and at a rate increasing drug industry payments.

Table 3 formalizes these comparisons in linear regression coefficient estimates. We estimate:

$$\left(\frac{branded claims}{total\ patients}\right)_i = \beta \cdot payment + controls + \varepsilon_i, \qquad (1)$$

where *branded claims* is all Medicare reimbursements for drugs prescribed by doctor *i* in year 2010, summed across all pharmaceutical firms *j* in our data set. Likewise, *payment* is the sum of all payments received by doctor *i* by any pharmaceutical firm (i.e., summed across all firms *j*), in any year from 2009-2011. *Controls* include specialty fixed effects, state fixed effects, and the rate of non-branded prescriptions written by physician *i*.

In the first three columns, the sample is restricted to doctors with at least one payment from a pharmaceutical firm in our sample. With no doctor or location controls, the coefficient is a highly significant 0.087 ($p<0.001$). The interquartile range for the logarithm of total payments is 3.85-5.88=2.03, implying an increase in per-patient branded prescriptions of about 0.17, or roughly one-quarter of its mean value (0.66).

The second column adds controls for each of the 412 specialties listed by ProPublica and accounts for average differences in brand-name prescription rates across practice types. Although this adds considerable explanatory power to the regression, increasing the R^2 from 0.32 to 0.43, the coefficient on payments remains similar

(0.0773, $p<0.001$). Likewise, state fixed effects give some account, though admittedly coarse, for differences in patient characteristics, which may be correlated with both brand-name prescription rates and pharmaceutical payments. However, the coefficient of interest remains significant, both economically and statistically.

The fourth, fifth, and sixth columns represent the closest analog to Figure 1. Here, we estimate equation (1) using indicator variables for each payment decile of payment, and a separate dummy variable for the group receiving no payments whatsoever. Decile construction is identical to the method described above. The omitted category is the fifth group, capturing the 40th to 50th percentiles of doctors ranked by payment.

Without exception, progressive payment deciles are associated with higher levels of branded prescriptions, and with roughly equivalent magnitude between specifications. With the middle quintile as the benchmark, doctors in the highest quintile write 40-50% more brand name prescriptions, while doctors in the lowest quintile write about 15% fewer. Thus, comparing the top and bottom quintiles gives close to the same 2-1 average ratio as that implied by the contours in Panel A of Figure 1.

B. Variation within doctors

While the previous section suggests that doctors write more branded prescriptions when they receive transfers from the drug industry, it does not exploit perhaps the most important variation in our data: within doctors. Rather than ask whether a doctor who is paid by *any* of our twelve pharmaceutical firms is likely to prescribe *any* of their 239 drugs (as we did in the previous section), we can ask whether a doctor who is paid by a *specific* pharmaceutical firm is more likely to prescribe *that* pharmaceutical firm's drugs. Examining variation within doctors removes any plausible

explanations for payment-prescription sensitivity based on omitted doctor or firm characteristics, leaving only doctor-firm attributes as potential sources of endogeniety.

We begin by forming (Doctor, Firm) pairs, or approximately 334,086 doctors x 12 firms = 4 million total observations. With this unit of observation, we estimate:

$$Claims_{i,j} = \beta \cdot Payments_{i,j} + Controls + \varepsilon_{i,j}, \qquad (2)$$

where *Claims* is a measure of the number of Medicare-reimbursed prescriptions written by physician *i*, for drugs marketed by pharmaceutical firm *j*. *Payments* measures the dollar value of transfers from pharmaceutical firm *j* to physician *i*, in the form of gifts, meals, travel, consulting, research, and speaking fees. *Payments* are observed in years 2009 through 2011, and *Claims* in 2010.

It is important to note that, while we have three years of *Payments*, the conditional probabilities that physician *i* receives a payment from firm *j* are quite persistent over time. Table 4 calculates, for each pharmaceutical firm, the probability of payment in year *t+1* as a function of payment in year *t*. For example, the probability of payment by Merck for a doctor in 2010 is 80.3% if she was also paid in 2009 and 0.1% if she was not. Without exception, this relationship holds for every drug company in every year in which the calculation can be made. For this reason, it makes little difference in the regression analysis whether we define *Payments* for a specific year or as the sum across all three years.

Table 5 shows the results. In the top panel (A), we use a discrete specification, whereby *Claims* takes a value of one if physician *i* prescribes one or more of pharmaceutical firm *j*'s drugs at least fifty times in 2010, and zero otherwise. By focusing on a relatively low threshold,[1] this approach is most useful for inferring the effects of pharmaceutical payments on the extensive margin of prescriptions. In

[1] ProPublica only lists specific drugs that a doctor prescribes at least fifty times or more.

contrast, Panel (B) measures *Claims* continuously, and thus attempts to explain the variation in prescriptions among doctors actively prescribing a given pharmaceutical firm's drugs. Effects here inform us mostly about the intensive margin.

Consider first the results in Panel A. The estimated coefficient of 0.0274 (p<0.001) in the first column indicates that, roughly speaking, doubling the amount a drug company pays to a doctor increases by about 2.7% the likelihood that at least one of its drugs are prescribed (again, at least 50 times). Alternatively, in the second column, we see that that doctors who were paid *any* amount by a pharmaceutical firm in 2009 are over 22% more likely to prescribe. Given an average value for the dependent variable of 0.13 in 2009, this suggests a very strong association between firm-specific transfers to physicians and prescribing behavior.

The next pair of columns report the results of similar tests, the only difference being that *Payments* are measured in 2010, the same year that we observe prescription data. Comparing the fourth column to the second, the magnitude is a bit smaller (0.156, p<0.001), but still indicative of large effects. A doctor receiving payments from a pharmaceutical firm is over twice as likely to actively prescribe its drugs, compared to doctors not receiving any transfers. When we measure 2010 *Payments* continuously in column 3, we observe a nearly identical coefficient (0.0281, p<0.001) to that observed for 2009 *Payments*.

In columns five and six, we attempt to explain the cross-section of prescribing behavior in 2010 using data on pharmaceutical payments in 2011. While at first it may seem counterintuitive to link current prescriptions to future payments, recall from Table 4 the high degree of persistence in payments within doctor-firm pairs. In our context, what this means is that payments in 2011 may simply proxy for payments in prior years. Since the pharmaceutical firms successively increased reporting of payments in each year, the advantage of restricting attention to 2011 payments is that more companies are

included in the analysis. Columns five and six indicate, respectively, effects for the continuous and discrete specification comparable to those observed in the first four columns.

In columns seven and eight, we combine payments from all years 2009-2011. Compared to the previous columns, these aggregated tests indicate similar magnitudes for both the discrete and continuous *Payment* variables. In column nine, we split *Payments* in any year from 2009-2011 into *Big* (>$1000) and *Small* ($\leq$ $1000), allowing us to directly visualize the effects of payments differing in dollar amount. Those in excess of $1000 are associated with an effect on prescriptions roughly twice as large, 0.207 (p<0.001) versus 0.0946 (p<0.001).

The final two columns present results when we incorporate a fixed effect for each of the over 334,086 physicians in our dataset. Here, differences in prescribing tendencies across physicians are removed by the fixed effects, so that the effects of industry payments are identified within individual providers. More specifically, given that there are multiple observations for each of physician, the coefficients on *Big Payments* and *Small Payments* are estimated by comparing a given doctor's tendency to prescribe drug company A's drugs versus those of drug company B, provided that one pays and one does not. As seen, not only are the coefficients nearly identical in magnitude, but the adjusted R^2 remains almost unchanged, suggesting that conditional on specialty, doctor-specific variation is not important.

In Panel B, where we re-estimate Equation (2) with *Claims* measured continuously. Recall that ProPublica does not list specific drugs for which less than fifty were prescribed for each doctor, meaning that *Claims* is biased downward. However, the fact that prescription drugs sales are skewed to the right, with a handful of blockbusters

being responsible for most of a firm's sales in a given year, implies that this bias will be small, if not negligible.[2]

The columns are organized identically to Panel A, with 2009, 2010, 2011, and 2009-2011 aggregated shown in the first, second, third, and fourth pairs of columns. Roughly speaking, a 100% increase in the amount a drug company pays a physician increases by 7-8 the number of prescriptions of that company's drugs. Alternatively, the discrete *Payment* variable indicates that in cases when a physician has any financial relationship with a drug company at all, about 28 additional prescriptions are observed. If the total amount is over $1000, sixty additional prescriptions are observed on average, about three times the amount if the cumulative payment is less than $1000.

As in Panel A, the last column shows the analysis when including doctor fixed effects. The estimates for *Big Payments* and *Small Payments* are nearly identical, suggesting that after controlling for doctor specialty (which all columns do), differences between individual providers is not an important source of bias.

IV. Omitted heterogeneity at the doctor-company level

Because the last column in each of Panels A and B feature doctor fixed effects, alternative explanations for a causal effect of payments on prescriptions cannot appeal to generic attributes of physicians such as age, location, specialty, time in practice, or income. The same applies to drug companies. Consequently, any plausible omitted variable must operate at a more granular level, varying (at least) across doctor-firm pairs.

[2] In the appendix, we repeat the analysis for only physician-firm pairs for which *Claims* is strictly greater than zero. The coefficients are virtually identical to the results shown in Panel B of Table 5.

This can occur in cases where the doctor already prescribes a given company's drugs, and therefore may possess valuable information about, e.g., compliance, side effects, interactions with other drugs, for which the firm is willing to pay. Dinners, consulting arrangements, or speaking fees (in order to disseminate this information to other physicians) may follow accordingly. In such cases, the estimates in Table 5 could be, at least in part, reconciled via reverse causality, with payments being the effect, and prescriptions the cause.

In this section, we conduct additional analysis intended to rule out such omitted heterogeneity at the doctor-firm pair. The first set of tests takes seriously the specific story just described – i.e., the idea that "expert doctors" attract pharmaceutical dollars because of their experience prescribing specific drugs. Our approach is simple: exclude for each doctor his or her most frequently prescribed drugs, and see if a positive payment-prescription relation remains. We present the results of this exercise in subsection A. The second test is more general, and accordingly, remedies generic omitted variable stories operating at the doctor-firm level. In subsection B, we use distance between a doctor's office and drug company headquarters to generate quasi-exogenous variation in payments, and relate this to variation in prescription rates.

A. Expert doctors

Doctors likely represent a source of important information for pharmaceutical firms. In some cases, physicians are particularly knowledgeable about certain diseases or conditions, and therefore, may be in a position to lend expertise. In others, even when a physician isn't an expert, his or her experience prescribing one of the firm's drugs may nevertheless be valuable, e.g., reporting side effects or patient compliance. Importantly,

payments from pharmaceutical firms in such cases need not alter the physician's subsequent prescribing behavior.

This is undoubtedly part of the story, particularly for physicians compensated for research activities. However, these are exceptional cases, comprising only 1.3% of the payment observations reported by ProPublica. In this short section, we focus on situations where a doctor's expertise is unlikely to be the primary motive for a pharmaceutical firm and doctor interaction.

Our first test uses each doctor's observed prescription choices to infer his or her area of expertise, *within a given specialty*. We begin by identifying for each doctor his or her most frequently prescribed drugs. For example, suppose Dr. X is an ophthalmologist specializing in glaucoma, often writing prescriptions for Allergan's Lumigan and Pfizer's Xalatan, eye drops appropriate for this condition. Then, we re-estimate equation (2), but exclude these frequently prescribed drugs when calculating the left hand side variable. In other words, when aggregating Dr. X's prescriptions for Allergan, we ignore those for Lumigan and Xalatan when making the same calculation for Pfizer. This methodology means that any association between Dr. X's prescriptions and the payments of a given drug company are identified from drugs outside his area of expertise – dermatologists prescribing blood pressure medication, gastroenterologists prescribing antidepressants, and so on.

Table 6 shows the results of the continuous *Claims* specification, and thus, should be compared to Table 5, Panel B. When excluding each provider's top five most prescribed drugs, payments in excess of $1000 (*Big Payments*) are seen to increase prescriptions by 37, about one-third less than the benchmark test in Table 5, but nevertheless economically and statistically significant. Likewise, the impact of *Small Payments* is 15 additional prescriptions, similar to, but also a bit smaller compared to when all a provider's prescriptions are included. The second column extends this

exercise to exclude each provider's ten most prescribed drugs. The estimated coefficients of interest are reduced by about one-fifth relative to the first column, but remain highly significant.

Another way to gain some insight into the nature of a doctor's relationship with a drug company is to examine the specific types of activities in which they engage. Presumably, expert and/or highly informed doctors will be disproportionately compensated for *research, consulting, speaking,* and *travel.* Accordingly, we through out any doctor-firm pair that lists any of these specific activities, and thus, identify the effects of payments off more traditional "detailing" such as meals. The estimated coefficient in such cases is just under 23 prescriptions, about 20% smaller than the effect of the *Any Payment* in Table 5, but as in the previous two columns, economically meaningful.

B. Quasi-exogenous variation in payments

The tests in Table 6 are designed to refute a fairly specific story, in which doctors informed about specific drugs and/or conditions are targeted for marketing by pharmaceutical firms. Though this seems to us the most plausible alternative, in this section we present tests intended to identify the causal effect of payments in the presence of generic omitted doctor-firm heterogeneity.

Ideally, we would like to isolate exogenous variation in payments, i.e., transfers to doctors not correlated with other determinants of prescribing behavior. While most payments in our sample do not fit this criterion, the geographical distance separating a doctor's office and drug company headquarters arguably is a source of such variation. Intuitively, the idea is that although most drug companies have sales representatives nation wide, the area surrounding headquarters is likely to be particularly concentrated.

And because drug representatives are the agents through which most payments occur, doctors surrounding a firm's headquarters, we hypothesize, should be subject to transfers at a higher rate than those more distant.

At the same time, it seems exceedingly unlikely that a doctors give any consideration whatsoever to the location of a particular drug company's headquarters when selecting where to set up their private practices. Under these assumptions, we can use the distance between a doctor's office and drug company headquarters to obtain quasi-exogenous variation in payments which, when related to prescriptions, provides fairly direct evidence of a causal relation.

The first step in this analysis is to calculate the distance between each doctor's office and headquarters of each U.S.-based pharmaceutical company in our sample. This domestic restriction limits the sample to Allergan (Irvine, CA), Cephalon (Frazier, PA), Eli Lilly (Indianapolis, IN), EMD Serono (Rockland, MA), Johnson and Johnson (Brunswick, NJ), Merck (Whitehouse Station, New Jersey), and Pfizer (New York, New York). For each of these seven companies, we identify all doctors within a 500 km radius, using physician addresses listed on ProPublica's website, and headquarter locations from company websites. Then, for each firm, we form five concentric donut-shaped regions 100 km thick, with doctors progressively further away in each group. In selecting the sizes of these areas, our goal is to create regions close enough to all be reachable from headquarters within a single day, and yet, long enough to generate meaningful differences in travel costs.

Figure 2 shows the average payment amounts for doctors in each category. For the seven regions within 100 km of company headquarters (one for each firm), the average doctor is paid slightly less than $400, which decreases to $275 for doctors in the 100-200 km range. Physicians in the next ring are paid about $210 on average, then

dropping to $160, and finally to $140 in the outermost ring. These differences serve as the source of quasi-exogenous payments we use in instrumental variable regressions.

We re-estimate Equation (2), but instrument for *Payments* using the distance (in km) from company headquarters. The estimates are shown in Table 7. In columns 1, 3, and 5, the endogenous covariate is *Any Payment* (compare to Table 5B, column 8), *Any Payment – Big* (compare to Table 5B, column 9), and *Any Payment – Small* (compare to Table 5B, column 10). As in Table 5B, these models control for each doctor *i*'s specialty, average prescription intensity (as before, excluding prescriptions for firm *j*), and state of practice. We also include pharmaceutical firm fixed effects. Note that when compared to Table 5B, the smaller number of observations reflects the joint restrictions of: 1) considering only U.S.-based drug firms, and 2) doctors located within 500 km of these firms' headquarters.

Relative to the corresponding columns in Table 5B, the estimates in Table 7 are much larger in magnitude, though as expected, estimated less precisely due to the errors-in-variables problem introduced by the first stage. Small payments are associated with almost 200 additional prescriptions, with large payments conferring almost five times that amount.

Columns 2, 4, and 6 include doctor fixed effects, and represent our most powerful evidence for causation. Here, we the set of 79,073 doctors located within 500 km of at least *two* firms' headquarters, and in effect, ask whether relative distances between them predict differences in prescription patterns.[3] Importantly, models with doctor fixed

[3] There are 52,114 physicians located within 500 km of five firms, 20,821 within 500 km of four, 4,185 within 500 km of three, and 1,953 within 500 km of two. Given that only firms located on the northern part of the Eastern seaboard are close enough to jointly permit the 500 km restriction for multiple doctors, the relevant sample here is comprised mostly from New York, Massachusetts, and Pennsylvania. The 62,028 doctors near either Ely Lilly (Indianapolis) or Allergan (Irvine, CA) will not enter into the estimation with doctor fixed effects, thus explaining the reduced number of observations between columns 1 and 2, 3 and 4, and 5 and 6.

effects account for such characteristics such as patient demographics, income, location (e.g., rural versus urban), or other physician-specific attributes that might influence prescription decisions.

The estimated magnitude of the *Payment* indicators is cut by approximately one half when doctor fixed effects are included. *Small* transfers appear to boost prescriptions by about one hundred, with payments exceeding $1000 having an effect roughly seven times as large. Taking the final column as the estimate most indicative of the underlying behavior, *Any Payment*, as instrumented using geographical distance, appears to increase prescriptions by 106, with a *t*-statistic equal to 9.81.

Relative to the OLS estimates shown in the final column in Table 5B (which also includes physician fixed effects), the IV estimates reported in Table 6 are about 80% larger. There are two reasons why this might occur. First, if drug companies target their marketing efforts toward the extensive margin of prescribers, *Payments* will be negatively correlated with the error term. In this case, the OLS coefficient is biased downward, rendering the estimates in Table 5B conservative. A second possibility is that for doctors close to headquarters, payments are accompanied by factors that enhance their efficacy. For example, suppose that a drug company's best sales representatives work out of headquarters, and thus, are disproportionately to interact with doctors located nearby. Here, distance, or more correctly its inverse, amplifies the impact of payments.

While this ambiguity is not problematic for inferring a causal relation between payments and prescriptions (the exclusion restriction is still satisfied), it does make estimating the present value of marketing dollars challenging. Ultimately the calculation should account for both the types of payments in the ProPublica dataset – i.e., how much was spent on dinner – as well as the labor (or other) costs associated with each particular

event. Without this additional information, it is not possible to assess by how much, or even whether, revenue from increased prescriptions exceeds the dollars spent to cause them.

V. Why do drug industry payments change physician behavior?

For the remainder of the paper, we take as given that payments to physicians have a causal impact on physician's prescription choices, and attempt to better understand the reasons why. We explore three hypotheses, the first two of which are closely related. First, drug companies may spend money to educate doctors, providing information that allows them to make better medical decisions. A second possibility is that drug companies convince physicians that certain drugs are better than others, when in reality they are not. We refer to these, respectively, as informed and uninformed persuasion. In both alternatives, physicians believe they are becoming informed through interactions with drug companies. This is not the case with an explanation based on rent-seeking: payments from drug companies are valued strictly for their pecuniary benefit, apart from any information or persuasion effects.

In this section, we provide evidence intended to help distinguish between these mechanisms. In subsection A, we consider a number of head-to-head drug comparisons where information flow is expected to be low. Specifically, we look at highly prescribed, chemically similar compounds that have been on the market for several years. Even in these cases, the effect of payments on prescriptions is clear. Of particular interest are situations where a branded drug and its *generic equivalent* are being compared; in these cases, it is hard to imagine information exchange playing any role whatsoever, and thus rule out even uninformed persuasion.

We conclude the paper by presenting some direct evidence suggestive of rent-seeking. Once again using each doctor's address, we compare the payment-prescription sensitivities between doctors practicing in traditionally corrupt states like Mississippi and in less corrupt areas like Oregon. As we will see, state-level corruption indices are strongly related to payment-prescription sensitivities, but also to raw expenditures by pharmaceutical firms, suggesting a collective awareness regarding where their dollars go the farthest.

A. Persuasion

Part of why drug companies interact with physicians is likely to provide them with information about current or future therapies. Further, if a doctor becomes better informed about the firm's products, he or she may be more likely to prescribe them to patients. Of course, it is not strictly necessary for advertising to contain genuine information to be effective, as long as a doctor believes it does.

In this section, we attempt to better understand whether the positive cross-sectional correlation between payments and prescriptions reflects information flow from drug companies. Our empirical strategy is to examine specific situations where information asymmetry between firms and doctors, or at least physicians' perception of this deficit, should be very small. One of these comparisons involves close substitutes, and three of them perfect substitutes between branded drugs and their generic equivalents. In all cases, the relevant drugs had been available for several years. Together, these factors should level the information playing field between doctors and physicians, making information flow from firms to doctors an unlikely explanation.

Our first comparison involves cholesterol-reducing drugs in the "statin" class. High cholesterol is one of the most commonly treated medical conditions among

Medicare patients in the U.S. Accordingly, statins were the single most widely prescribed class of medications in 2010, with over 255 million prescriptions, involving both branded and generic alternatives. The two largest branded statins in 2010, by far, were Pfizer's Lipitor (atorvastatin) and AstraZeneca's Crestor (rosuvastatin), with combined sales over $11 billion. Lipitor is the highest selling prescription drug of all time, with sales exceeding $7 billion in 2010 alone. Crestor's sales accounted for almost $4 billion that year, sufficient to make it the eighth highest selling branded drug (in dollars). Among generics, simvastatin (formerly Merck's Zocor) is the most frequently prescribed drug in our Medicare dataset, with over 38 million prescriptions in 2010.

In addition to their ubiquity, two features of statin-class drugs are convenient for our purpose. First, although not identical, all statins share the same mechanism of action, and consequently, have comparable efficacy. Statins lower serum cholesterol levels, an important risk factor for coronary artery disease, by inhibiting HMG-CoA reductase, a catalyst in the biosynthesis of cholesterol (Istvan and Deisenhofer, 2001).

Second, by 2010, statins were a well-established drug class.[4] Mevastatin, the first of the statins to be isolated, was studied and developed beginning in the early 1970s, and lovastatin (formerly Mevacor) was the first statin to be approved by the FDA, in 1987 (Endo, 2004). Although some evidence suggests that rosuvastatin (Crestor) is somewhat more efficacious at reducing low-density lipoprotein cholesterol than atorvastatin (Lipitor) or simvastatin for equal doses (Jones et al., 2003), meta-analyses also suggest that the efficacy of each drug increases similarly with higher doses (Nicholls et al., 2010).

Given the chemical similarity and the extensive experience doctors had with statins, we proceed under the idea that payments from particular manufacturers are unlikely to represent (at least significant) opportunities to educate doctors about these

[4] A PubMed (http://www.ncbi.nlm.nih.gov/pubmed) search for the keyword "statin" yields 24,981 publications through the end of 2010.

drugs. We first compare prescriptions between Crestor and Lipitor, and then consider the implications for prescriptions of the generic alternative simvastatin.

The first two columns of Table 7 show the results of the Crestor-Lipitor comparison. About 10% of doctors in our sample (roughly 33,000) prescribed both drugs at least fifty times in 2010, a requirement for us to conduct a head-to-head analysis.[5] We estimate the following regression:

$$\left(\frac{Cres - Lip}{Total}\right)_i = \beta_1 \cdot Astra\_payment_i + \beta_2 \cdot Pfizer\_payment_i + controls + \varepsilon_i, \quad (3)$$

where *Cres* is the number of prescriptions written by doctor *i* for Crestor, *Lip* for Lipitor. The coefficient $\beta 1$ ($\beta 2$) tells us whether the Crestor-Lipitor difference, scaled by *Total* (the number of total claims for doctor *i*) is influenced by payments from AstraZeneca (Pfizer).

As shown in the first column of Table 7, we estimate significant effects for both coefficients. The AstraZeneca coefficient is 0.00180 ($p<0.001$), indicating that a payment increases the fraction of Crestor prescribed, while the Pfizer coefficient is -0.00053 ($p<0.001$), resulting in comparatively more prescriptions for Lipitor. In the second column, we break these payments, as we have done in previous tables, into large (>$1000) and small. In both cases, the signs are preserved, and we continue to observe statistical significance. A big payment by AstraZeneca increases the scaled Crestor-Lipitor difference by 0.0143 ($p<0.001$), whereas a small payment matters about one-tenth as much. Likewise, large transfers from Pfizer matter approximately four times as much as smaller ones, although both are statistically significant at better than the one percent level. The fact that payments from *both* firms yield statistically significant effects indicates that regardless of which statin is preferred under the available evidence

[5] Recall that ProPublica lists for each doctor drugs prescribed at least fifty times.

in 2010, the observed associations cannot be entirely explained by informative advertising.

Although the first two columns indicate that payments from pharmaceutical firms appear to induce substitution between brand names, the same effect might be observed between brand names and generics. In the third column, we explore whether combined payments from AstraZeneca and Pfizer influence the relative ratio of branded statins (i.e., Crestor plus Lipitor) versus the generic alternative simvastatin. To test for this effect, we estimate:

$$\left(\frac{Cres + Lip - Sim}{Total}\right)_i = \beta_1 \cdot Astra\_payment_i + \beta_2 \cdot Pfizer\_payment_i + controls + \varepsilon_i, \quad (4)$$

where the only change is that the dependent variable is the difference between summed prescriptions of Lipitor and Crestor and simvastatin (*Sim*). As in previous tables, we include state and specialty fixed effects. The third column indicates that payment from either AstraZeneca or Pfizer increases the scaled difference between branded and generic statins. In the fourth and fifth columns, we break this up by firm, both of which are shown to have a positive effect. In each case, large payments matter considerably more than small payments.

To get a sense for the magnitude of wealth transfers in Table 7, we can use the table's coefficient estimates and the retail cost of statin drugs to estimate prescription behavior with and without payment. This approach is conservative because it attributes all of AstraZeneca's and Pfizer's payments to just these two drugs and only considers doctors who wrote 50 or more prescriptions for the drugs. Nevertheless, in 2010 the average retail cost of simvastatin (40 mg) was $68 while the cost for Crestor (40 mg) and Lipitor (40 mg) were $162 and$165, respectively (Consumer Reports Health, 2010).[6] Thus, the per-prescription cost difference between brand-names (taking the simple

[6] Average costs for 20 mg doses were very similar, at $70 (simvastatin), $164 (Crestor), and $161 (Lipitor).

average of Crestor and Lipitor costs) and generic simvastatin (assuming all 30-day prescriptions and their equivalence to monthly costs) was $95.50. Eliminating payments from Pfizer and AstraZeneca, i.e. setting the firms' *Payment Indicators* to zero in the fourth column of Table 7, would have shifted approximately 10 prescriptions per doctor, and 886,239 prescriptions in total, from Crestor and Lipitor to simvastatin. According to this back-of-the-envelope exercise, therefore, eliminating payment-induced brand-name prescriptions would have reduced per-doctor expenditures by $955 and total expenditures by $84.64 million (changes in firm revenue net of production costs would have likely been even higher). In 2011, reported payments from AstraZeneca and Pfizer to providers totaled $308.48 million, so a sizeable portion of total provider payments would have been returned from shifts in prescriptions for just these two drugs among our sample of Medicare doctors. The per-doctor expenditure shift is also worth several large meals or gifts.

Although drugs within the class of statins are plausible substitutes, they are not chemically identical. Thus the possibility remains that the positive correlation between payments and prescriptions for statins is driven by beliefs – rather than incentives – of physicians. Put differently, although genuine information is unlikely to explain the patterns observed in Table 7, doctors may nevertheless be persuaded by pharmaceutical firms. The analysis in this section, because it considers identical chemicals, rules out even uninformed persuasion.

We consider the case of drugs whose name-brand and generic equivalent are both heavily prescribed in our 2010 sample. To be sure, this is a unique phenomenon. We observe very few Medicare claims for off-patent name-brand drugs because insurance companies rarely cover name-brand drugs which have available generic equivalents. In fact, we find only five cases in which both a name-brand drug and its generic equivalent had at least 50 claims by at least 1,000 providers. Those drugs (and their generic

equivalents) are AstraZeneca's Arimidex (anastrozole), Merck's Cozaar (losartan potassium), Pfizer's Dilantin (phenytoin), GlaxoSmithKline's Lanoxin (digoxin) and Pfizer's Protonix (pantoprazole). We remove Dilantin and Lanoxin from the analysis because of concerns that the generic and name-brand are not chemically identical.[7]

We observe heavy volume for each of the three remaining drugs because of changes in the drug's exclusivity during 2010. Merck's patent for high blood pressure drug Cozaar expired in April (Doherty, 2010), and AstraZeneca's patent for cancer drug Arimidex expired in June (Connolly, 2010). In the case of Pfizer's Protonix, generic manufacturers were ordered by a US federal court in April to stop selling their generic version of Pfizer's drug due to patent infringement (Pearson et al., 2010). Patent expiration and court orders are plausibly unrelated to a doctor's belief about a particular drug's efficacy. For this reason, these three drugs provide a natural setting for identifying the incentive effects of payment behavior apart from beliefs.

While it is possible that a doctor might believe rosuvastatin (Crestor) to be more effective than simvastatin, it seems unlikely that a doctor would believe any of our three drugs are more effective than their generic twins. Thus any correlation we observe between payments and the likelihood of prescribing the name brand in favor of the generic is likely an effect of incentives, rather than beliefs.

We begin by considering the subset of doctors who prescribed either the name-brand or the generic equivalent. For example, there were a total of 2,361 doctors who prescribed the cancer drug Arimidex or its generic equivalent, anastrozole. For each of these 2,361 doctors we create a binary variable called *Name-Brand Indicator* which takes the value of one if a doctor prescribed the name brand drug in favor of the generic

[7] Dilantin is an epilepsy drug whose users have reported increases in seizures after switching to generic versions (http://www.webmd.com/epilepsy/news/20041025/generic-epilepsy-drugs-not-same), while Lanoxin had well-publicized recalls of its generic equivalent between 2008 and 2010 (http://www.fda.gov/Safety/Recalls/ArchiveRecalls/ucm150734.htm and https://www.mediguard.org/alerts/alert/940.html).

equivalent (in the case where he prescribes both, a value of 1 is assigned to the drug with the most prescriptions). Then we regress *Name-Brand Indicator* on *Big Payment Indicator* and *Small Payment Indicator* in the first column of Table 8.

The positive coefficients on both *Big Payment Indicator* and *Small Payment Indicator* demonstrate a positive relationship between payments from AstraZeneca and prescriptions of Arimidex. Unconditionally, there is a 79% probability that name- brand Arimidex is prescribed more frequently than its generic equivalent. However, this probability increases to 81% if a doctor received a small payment from AstraZeneca and to 98% if a doctor received a big payment from AstraZeneca. While the coefficient on *Small Payment Indicator* is insignificant, the coefficient on *Big Payment Indicator* is significant at the 1% level.

Columns 2 and 3 repeat the analysis for Merck's Cozaar and Pfizer's Protonix. In the case of Cozaar we can only estimate a coefficient on *Big Payment Indicator* because Merck reported only speaking fees (and not the less-lucrative meals and gifts) between 2009 and 2011 (see Table 1). The coefficient is positive but indistinguishable from zero. In the case of Pfizer's Protonix the coefficient of 0.116 on *Big Payment Indicator* suggests that the probability of prescribing the name brand in favor of the generic increases from 42.2% to 53.6% if a doctor received a big payment from Pfizer.

Column 4 combines the observations from the first three columns and finds that the average increase in the probability of prescribing the name brand is 10.6% (p-value < 0.01) when *Big Payment Indicator* = 1. We find no effect for *Small Payment Indicator*. The final column includes state and specialty fixed effects with little change in the variables of interest.

In this special case, at least, it is worth emphasizing that the specialization mechanism involves a realized financial conflict of interest, at worst, and a potentially welfare-reducing oddity, at best: doctors with an idiosyncratic but demonstrably

incorrect belief in the branded drug are not only rewarded for their idiosyncrasy, but their continued efforts at educating others about the branded drug could induce at least some other physicians to make inappropriate prescriptions, however unlikely the possibility.

B. Rent-seeking

The final explanation involves physicians altering their behavior in exchange for current, or expected, financial benefits from pharmaceutical firms. Unlike the previous alternatives involving information flow, this possibility is less capable of improving decision making, and indeed, may worsen outcomes for patients. For example, financial conflicts of interest may lead doctors to substitute a slightly inferior drug for another, or, as seen in the last section, increase costs via reluctance to prescribe generic alternatives.

In this section, we follow Glaeser and Saks' (2006), study of corruption across U.S. states, and use conviction rates for corruption-related crimes, such as obstruction of justice, fraud, and election irregularities to proxy for state-level rates of corruption. Our idea is that doctors living in more corrupt regions may, themselves, be more sensitive to the payments of drug companies when making prescription decisions.

In Figure 3, we plot the payment-prescription coefficient for each state on the y-axis, as a function of Glaeser and Saks' measure of political corruption on the x-axis, shown as percentiles. States with low levels of corruption are shown toward the left, and include Oregon (50th highest or 2nd percentile), Vermont (6th percentile), and Minnesota (8th percentile). At the other end are high-corruption states: Illinois (88th percentile), Louisiana (96% percentile), and Mississippi (98th percentile).

For each state, we run regression (2), using the same control variables (e.g., doctor specialty, pharmaceutical firm fixed effects, etc.) from Table 5, Panel B. The coefficient of interest is on the *Any_payment* dummy variable, interpreted as the

additional prescriptions the typical doctor prescribes for a given drug company's products, conditional on him receiving a payment from that company. Because states vary so widely in the number of doctors, we scale each point estimate by the standard error of the estimated coefficient, so that a circle with twice the diameter of another is estimated twice as precisely.

Visual inspection reveals an upward sloping relation between prescription sensitivities to payments across states and convictions for corruption related crimes. Of the ten least corrupt states, eight have estimated sensitivities below 20, with only three states below the median corruption level exceeding 25. On the other hand, almost two-thirds of states above the median are associated with coefficients above 25, with seven exceeding 35. Interestingly, the one notable outlier, Alaska, is associated with the highest per-capita conviction rate, and also the only negative estimated prescription-payment sensitivity. However, with only 253 Alaskan doctors entering the estimation, this is not statistically significant.

In Table 9, we formalize these comparisons in regressions. The first three rows show the results of estimating Equation (2) by corruption tercile, progressing from least to more corrupt. Confirming the graphical evidence shown in Figure 3, the first column indicates a point estimate of 20.2 prescriptions ($p<0.001$) for the least corrupt third of U.S. states. The coefficient increases by almost half in the second column to 28.3 ($p<0.001$), and yet again for the most corrupt states (30.91, $p<0.001$). The fourth column aggregates all states together, and interacts the numerical value of the Glaeser-Saks corruption index percentiles, the same numbers displayed the x-axis of Figure 3. The *t*-statistic on the interaction is negative seven, indicating a steeply declining impact for drug company payments in less corrupt states.

In light of these findings, it is worth examining the heat maps shown in Figure 4. Note that both payments and prescription rate of branded drugs are heavily

concentrated in the greater southeast region of the U.S. Focusing on Panel B, note that gulf coast states Texas, Louisiana, Mississippi, Alabama, and Florida, as well as neighbors Georgia and South Carolina – all above median rates of corruption – have significantly elevated prescription rates of branded drugs. States with high branded rates in different regions include New York (7th most corrupt state), New Jersey (17th), and Alaska (1st).

Combining all three heat maps, a coherent picture emerges: doctors in corrupt states are most sensitive to payments (Figure 3), pharmaceuticals disproportionately target these regions (Figure 1A), and the distribution of branded drugs reflects the combination of these effects (Figure 1B).

What these graphical patterns cannot tell us, however, is *why* – i.e., what is it about certain regions that fosters corrupt activity across very different areas, ranging from corrupt elected officials to rent seeking physicians? Manski's (1993) discussion of the "reflection problem" in social effects provides a useful context. *Endogenous* effects refer to classic "peer effects," such as a teenager going to the beach because (and only because) her friends are also going. *Exogenous* effects refer to common characteristics that lead groups to behave similarly, e.g., a group of fair-skinned avoiding the beach together for common fear of sunburn. *Correlated* effects refer to operating under a common environment, such as news of a shark attack inducing a "correlated" response by those living nearby.

Any of these seem plausible on our setting, and we see little way to convincingly distinguish between them. For example, there are considerable demographic differences between states, some of which reflect exogenous attributes, and others of which reflect common environmental influences. Poverty and education rates also differ considerably between states, both of which are positively related to corruption (Berkowitz and Clay, 2004). There is also the possibility that corruption reflects social norms, being more

tolerated in some regions than in others. This latter possibility corresponds to an endogenous effect, and is capable of explaining how corruption in two different arenas – i.e., politics and medicine – could be so strongly correlated within regions.

The only mechanism that probably can be excluded is cross-state differences in enforcement, a contextual factor often making causal inferences in corruption studies difficult. Two features of our setting make this less problematic. The first is a feature of Glaeser and Saks' measure of corruption itself. As the authors note, all convictions were prosecuted by the Federal Department of Justice, rather than local jurisdictions. Second, even were this not the case, receiving payments from drug companies is, in the vast majority of circumstances, not illegal, making its enforcement (or lack of enforcement) largely irrelevant.

VI. Conclusion

Using data from twelve drug companies, more than 300,000 physicians and nearly one billion prescriptions, we find that when a drug company pays a doctor he is more likely to prescribe that company's drug. A payment from a pharmaceutical company corresponds to, on average, an additional 29 Medicare prescriptions per year, and this number rises to nearly 100 prescriptions if the payment is at least $1,000.

Our specifications are stringent, accounting for pharmaceutical firm, state, specialty, and even physician fixed effects. At least some of the evidence reflects rent-seeking behavior on the part of doctors. For example, we find that pay matters for prescribing behavior even among drugs with identical, generic alternatives. Moreover, the pay-for-prescription sensitivity is greater for doctors among high-corruption states and for male doctors.

These findings seem to have clear policy implications, particularly insofar as the effectiveness of the (now implemented) 2014 Physician Payment Sunshine Act. If

payment behavior simply reflects a doctor's already-held opinion, then mandatory disclosure of physician payments would impose unnecessary costs, with little to no offsetting benefit. On the other hand, given that payments appear important even when information transfer is unlikely to play an important role, disclosure may curb rent-seeking incentives, and presumably resulting in less biased medical decisions.

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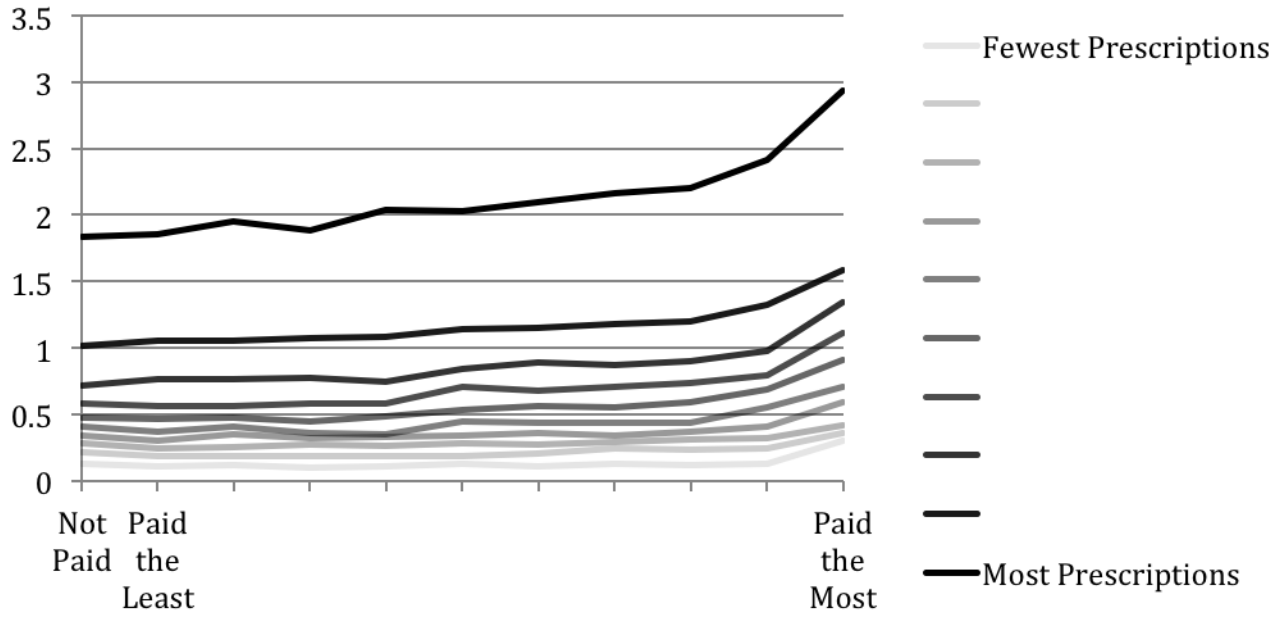
Figure 1: Payments and Prescriptions per Patient

The top panel plots prescriptions per patient for drugs of our twelve pharmaceutical firms. The bottom panel plots prescriptions per patient for drugs not from our twelve pharmaceutical firms. In both panels, doctors are first sorted into decile bins according to total prescriptions and then into decile bins according to total payments from our twelve pharmaceutical firms.

PANEL A: Prescriptions per Patient for Pharmaceutical Firm Drugs



PANEL B: Prescriptions per Patient for non-Pharmaceutical Firm Drugs

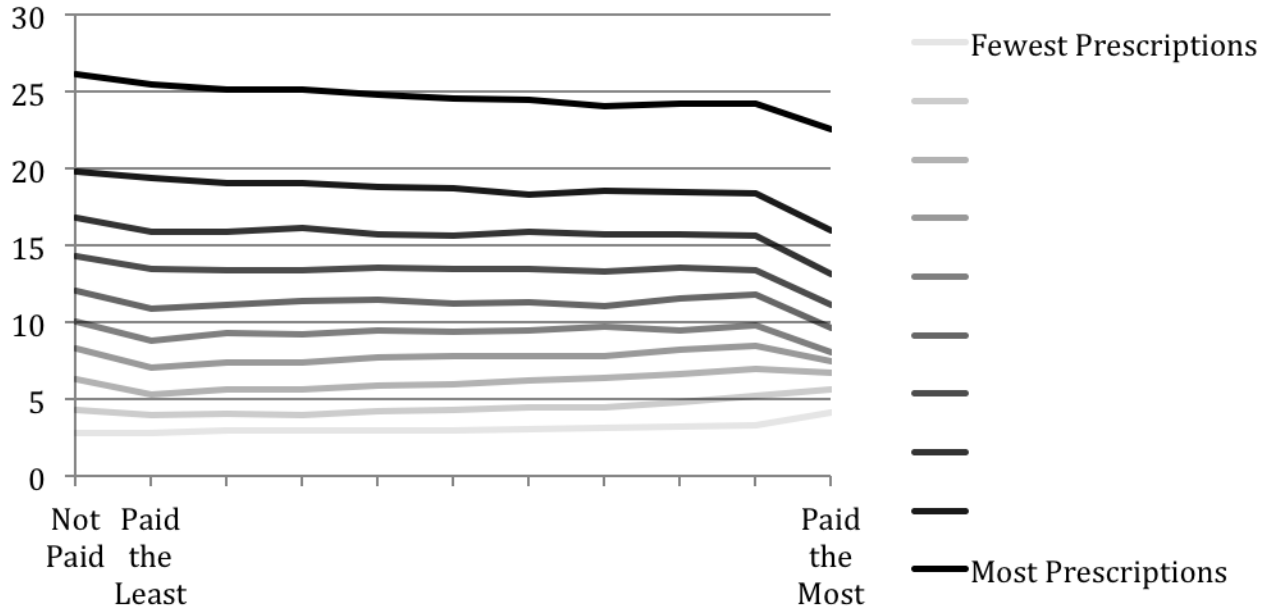


Figure 2: Payments and Headquarter Geography

The graph plots the mean payment from pharmaceutical firms to physicians by location. Closest (furthest) physicians are those within 100 (500) kilometers of pharmaceutical headquarters.

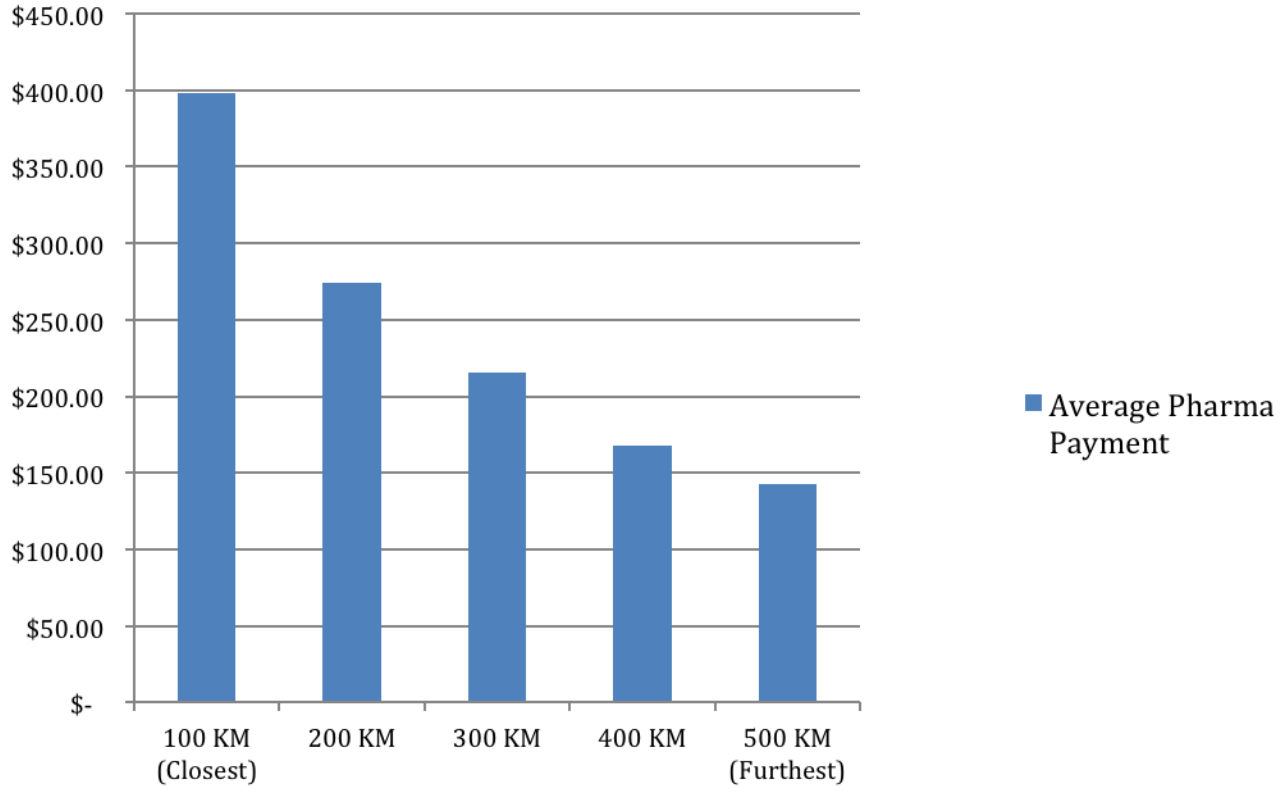


Figure 3: Payment/Prescription Sensitivity and State-Level Corruption

Each bubble in the plot corresponds to an individual state. On the x-axis is the state's per-capita measure of political corruption according to Glaeser and Saks (2006). On the y-axis is the state's coefficient from a regression of total prescriptions on payments. The size of each bubble represents the size of the standard error from these regression, with larger bubbles indicating more precise estimates.

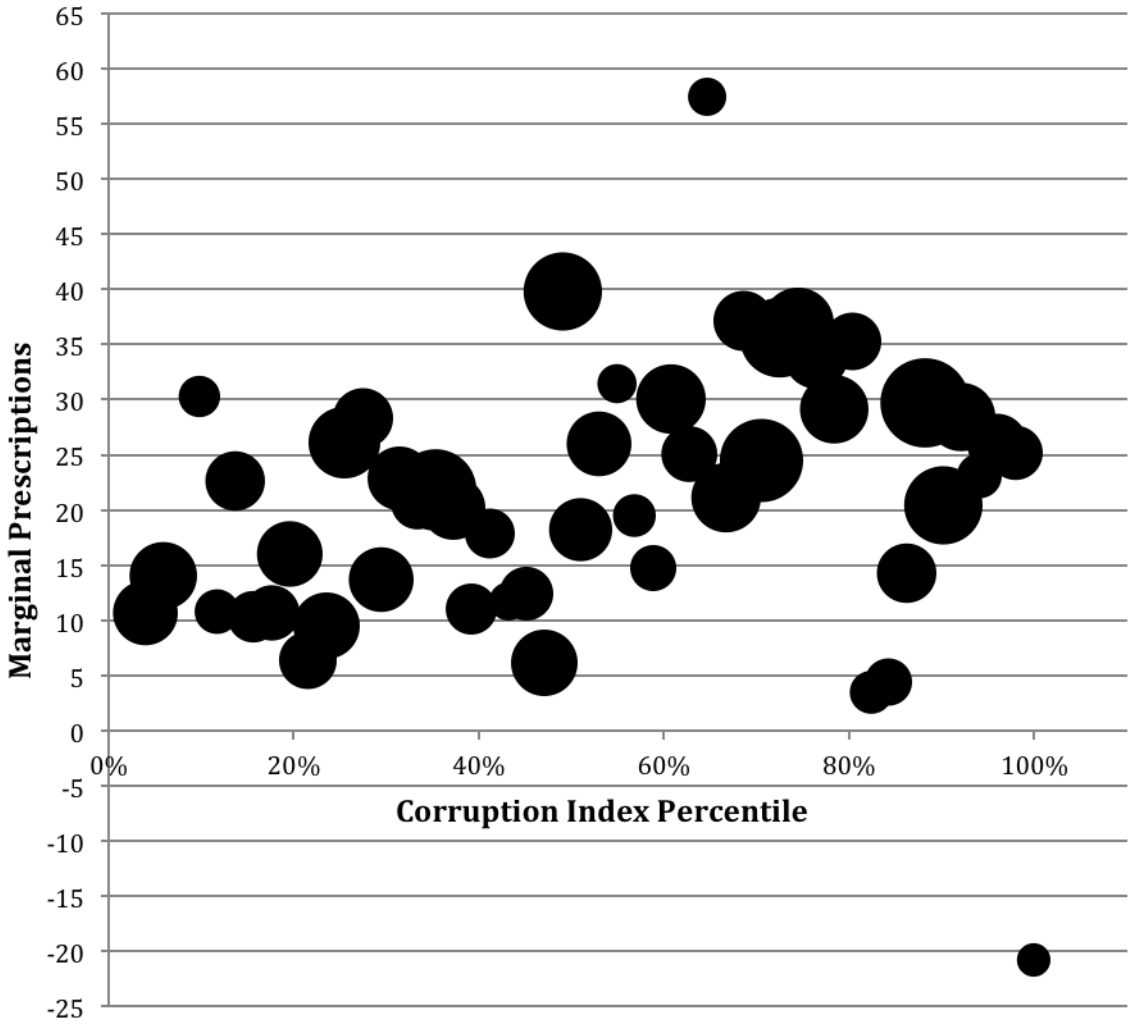
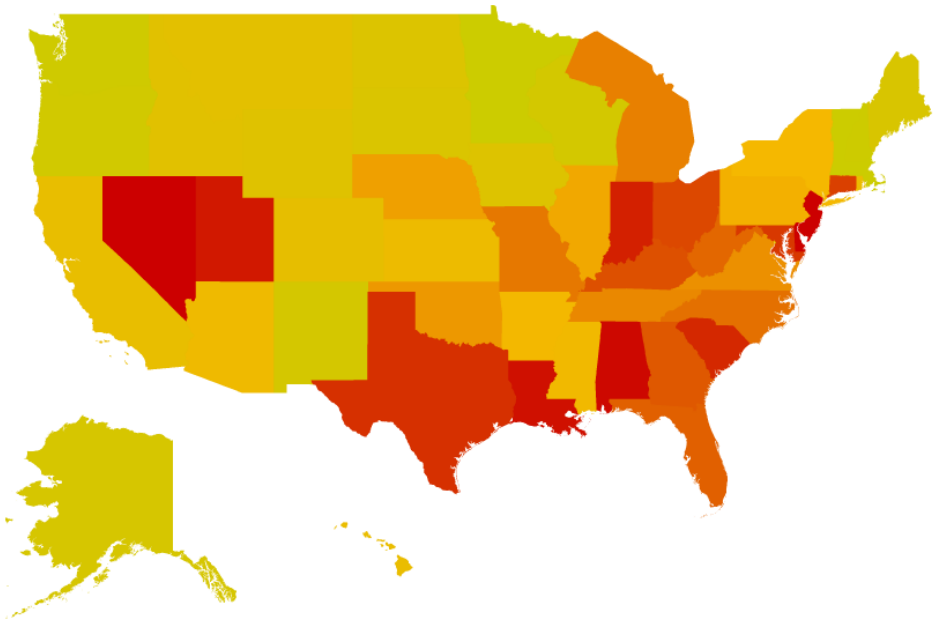


Figure 4: Payments and Prescriptions by State

Each panel provides a heat map by state where intensity runs from low (light green) to high (dark red). The top panel plots the percentage of doctors who receive a payment from any of the twelve pharmaceutical firms in our sample. The bottom panel plots the percentage of total prescriptions that were for drugs sold by our twelve pharmaceutical firms.

PANEL A: Percentage of Doctors Receiving Pharmaceutical Firm Payments



PANEL B: Percentage of Prescriptions for Pharmaceutical Firm Drugs

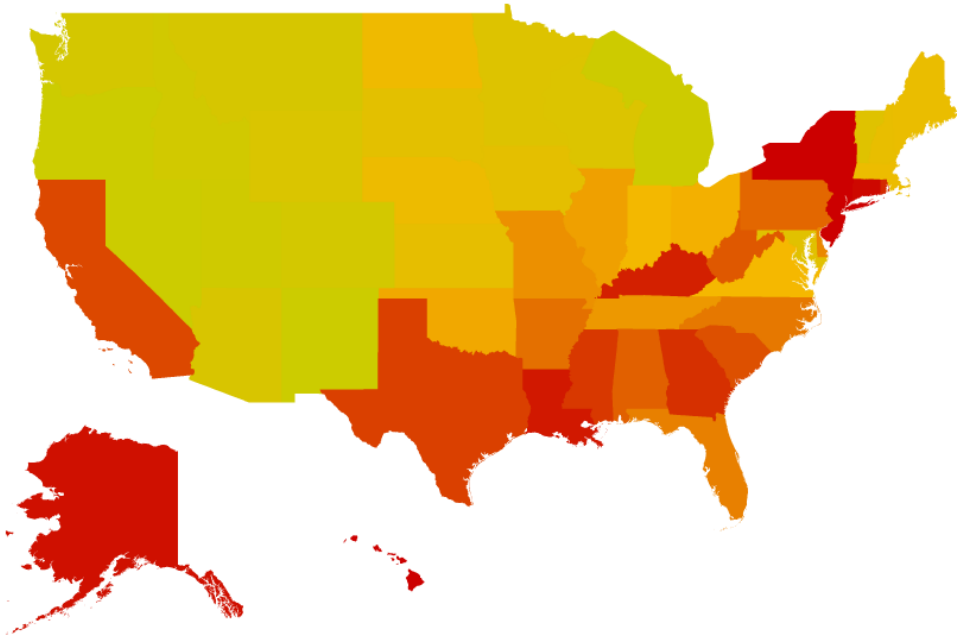


Table 1: Payments and Reporting Practices by Pharmaceutical Firms

The top panel describes the reporting practice for payments to healthcare providers by each of twelve pharmaceutical firms during the period 2009 - 2011. Data are taken from ProPublica's *Dollars for Docs* database (ProPublica 2013a). Reporting varies by year and categories reported. The top panel also includes the number of unique providers to whom payment was made as well as total dollars paid by year. The bottom panel provides summary statistics for the various payment categories.

PANEL A: Pharmaceutical Firms and Reporting Practices

	2009			2010			2011		
	Categories	Providers	Total $ Reported	Categories	Providers	Total $ Reported	Categories	Providers	Total $ Reported
Allergan	-	-	-	None Identified	41,528	-	Research, Gifts, Meals, Royalties, Speaking, Travel	42,572	-
AstraZeneca	-	-	-	Speaking	2,381	$31.47M	Consulting, Gifts, Meals, Research, Speaking, Travel	116,643	$114.21M
Cephalon	None Identified	935	$9.25M	Consulting, Gifts, Meals, Research, Speaking, Travel	45,575	$21.00M	Consulting, Gifts, Meals, Research, Speaking, Travel	36,157	$31.17M
Eli Lilly	Consulting, Speaking, Other	4,963	$82.09M	Consulting, Speaking, Travel, Other	4,875	$77.75M	Consulting, Meals, Research, Speaking, Travel, Other	101,898	$226.40M
EMD Serono	-	-	-	-	-	-	Consulting, Gifts, Meals, Speaking, Travel, Other	11,112	$1.85M
GlaxoSmithKline	Consulting, Speaking	5,716	$50.60M	Consulting, Speaking	5,249	$56.76M	Consulting, Research, Speaking	4,909	$120.82M
Johnson & Johnson	-	-	-	Combination, Consulting, Meals, Speaking, Travel, Other	2,166	$17.94M	Consulting, Meals, Speaking, Travel, Other	80,704	$22.96M
Merck	Speaking	1,640	$9.29M	Speaking	2,019	$20.00M	Speaking	2,454	$26.50M
Novartis	-	-	-	-	-	-	Speaking	3,259	$24.58M
Pfizer	Consulting, Gifts, Meals, Research, Speaking, Travel	4,738	$37.63M	Consulting, Gifts, Meals, Research, Speaking, Travel	196,453	$176.70M	Consulting, Gifts, Meals, Research, Speaking, Travel	161,025	$194.27M
Valeant	-	-	-	Consulting, Gifts, Meals, Other	6,136	$306.69K	Consulting, Expenses, Gifts, Meals, Speaking, Travel, Other	15,855	$1.50M
Viiv	-	-	-	Consulting, Research, Speaking	435	$7.84M	Consulting, Research, Speaking	524	$8.79M
All Pharmas		16,096	$188.86M		264,137	$409.78M		388,451	$773.05M

PANEL B: Payment Size by Type

	Observations	Mean	Standard Deviation	5th Percentile	25th Percentile	Median	75th Percentile	95th Percentile
Consulting	20,940	$4,205	$10,300	$75	$700	$2,000	$4,000	$13,647
Gifts/Items	102,423	$80	$580	$9	$45	$72	$99	$169
Meals	1,295,221	$74	$125	$11	$16	$37	$93	$239
Research	20,961	$51,262	$226,724	$675	$4,650	$14,631	$44,257	$183,550
Speaking	65,238	$9,969	$16,634	$700	$2,000	$4,500	$10,651	$41,900
Travel	34,849	$1,312	$2,647	$20	$104	$565	$1,294	$5,499
Other	90,991	$313	$2,461	$10	$12	$23	$58	$257

Table 2: Sample Summary Statistics

The table provides summary statistics by doctor in Panel A and by (Doctor, Firm) pair in Panel B. The set of doctors and Medicare Part D claims are taken from the ProPublica *Prescriber Checkup* database (ProPublica 2013b). Total payments are the sum of all payments between 2009 and 2011 from the ProPublica *Dollars for Docs* database (ProPublica 2013a). "Branded" claims are insurance claims for drugs marketed by our twelve pharmaceutical firms.

PANEL A: By Doctor

	Observations	Mean	Standard Deviation	5th Percentile	25th Percentile	Median	75th Percentile	95th Percentile
Total Payments	334,086	$2,108	$25,870	$0	$0	$14	$146	$1,701
Payment Indicator	334,086	0.58	0.49	0	0	1	1	1
Total Patients	334,086	217	177.18	51	105	174	280	519
Total Medicare Claims	334,086	2980	4,061	213	637	1,527	3,710	10,508
Total Branded Medicare Claims	334,086	192	439	0	0	55	203	851
Total Patients | Payment Indicator = 1	192,484	243	175	61	123	200	314	564
Total Medicare Claims | Payment Indicator = 1	192,484	3566	4,521	302	850	1,954	4,552	12,099
Total Branded Medicare Claims | Payment Indicator = 1	192,484	258	519	0	0	84	299	1,059

PANEL B: By (Doctor, Firm) Pair

	Observations	Mean	Standard Deviation	5th Percentile	25th Percentile	Median	75th Percentile	95th Percentile
Payment Indicator	4,009,032	0.11	0.31	0	0	0	0	1
Payment Size | Payment Indicator = 1	398,772	$1,766	$21,403	$11	$23	$57	$143	$3,378
Prescription Indicator	4,009,032	0.10	0.30	0	0	0	0	1
Prescriptions | Prescription Indicator = 1	398,515	161	183	52	65	102	185	451

Table 3: Payments and Prescription Rates for Physicians

The dependent variable is prescriptions per patient for drugs of our twelve pharmaceutical firms. *Log(Total Payments)* is the natural logarithm of total payments between 2009 and 2011 from our twelve pharmaceutical firms. *Residual Firm Prescriptions per Patient* are the prescriptions per patient for drugs not from our twelve pharmaceutical firms. *Paid Zero Indicator* is a binary variable which takes the value of one if a doctor was not paid. *Paid Decile = X Indicator* is a binary variable which takes the value of one is a doctor is in decile X of the payment distribution. Robust standard errors are in parentheses. *, **, and *** represent significance at the 10%, 5% and 1% levels, respectively.

	Dependent Variable: Pharmaceutical Firm Prescriptions per Patient					
Log(Total Payments)	0.0865***	0.0733***	0.0702***			
	(0.00137)	(0.00139)	(0.00137)			
Residual Prescriptions per Patient	0.0722***	0.0919***	0.0918***			
	(0.000529)	(0.000797)	(0.000799)			
Paid Zero Indicator				-0.151***	-0.156***	-0.150***
				(0.00705)	(0.00653)	(0.00653)
Paid Decile = 1 Indicator				-0.112***	-0.0810***	-0.0722***
				(0.00876)	(0.00799)	(0.00792)
Paid Decile = 2 Indicator				-0.0917***	-0.0690***	-0.0645***
				(0.00888)	(0.00813)	(0.00806)
Paid Decile = 3 Indicator				-0.0699***	-0.0464***	-0.0482***
				(0.00925)	(0.00848)	(0.00842)
Paid Decile = 4 Indicator				-0.0710***	-0.0441***	-0.0436***
				(0.00898)	(0.00825)	(0.00817)
Paid Decile = 6 Indicator				0.0412***	0.0415***	0.0395***
				(0.00980)	(0.00898)	(0.00889)
Paid Decile = 7 Indicator				0.0798***	0.0829***	0.0798***
				(0.00987)	(0.00910)	(0.00900)
Paid Decile = 8 Indicator				0.168***	0.169***	0.163***
				(0.0104)	(0.00964)	(0.00955)
Paid Decile = 9 Indicator				0.300***	0.295***	0.280***
				(0.0113)	(0.0106)	(0.0106)
Paid Decile = 10 Indicator				0.516***	0.412***	0.407***
				(0.0123)	(0.0119)	(0.0118)
Specialty Fixed Effects	NO	YES	YES	NO	YES	YES
State Fixed Effects	NO	NO	YES	NO	NO	YES
Non-Pharma 12 Decile Fixed Effects	NO	NO	NO	YES	YES	YES
Observations	179,432	179,432	179,432	334,086	334,086	334,086
Adjusted R^2	0.319	0.425	0.437	0.275	0.381	0.390

Table 4: Payments and Persistence

The table reports the likelihood of a doctor in our sample receiving a payment in a year as a function of the prior year for each pharmaceutical firm. The first two columns report the probability of a doctor receiving a payment in 2010 as a function of whether the doctor received a payment in 2009 (column 1) or whether the doctor received no payment in 2009 (column 2). The second two columns report the probability of a doctor receiving a payment in 2011 as a function of whether the doctor received a payment in 2010 (column 3) or whether the doctor received no payment in 2010 (column 4). Missing cells are for pharmaceutical firms that did not report in the prior year.

	Probability of 2010 Payment		Probability of 2011 Payment	
	Given 2009 Payment	Given No 2009 Payment	Given 2010 Payment	Given No 2010 Payment
Allergan	-	-	68.3%	2.0%
AstraZeneca	-	-	82.8%	24.6%
Cephalon	81.7%	8.7%	51.8%	2.7%
Eli Lilly	76.5%	0.1%	86.9%	21.6%
EMD Serono	-	-	-	-
GlaxoSmithKline	65.1%	0.3%	57.5%	0.3%
Johnson & Johnson	-	-	74.6%	15.1%
Merck	80.3%	0.1%	67.7%	0.2%
Novartis	-	-	61.1%	0.2%
Pfizer	90.7%	32.4%	62.6%	9.0%
Valeant	-	-	37.0%	0.9%
Viiv	-	-	69.4%	0.0%

Table 5: Payments and Prescription Behavior for (Doctor, Firm) Pairs

The table relates payments made by pharmaceutical firms to prescribing behavior. The unit of observation is a (Doctor, Firm) pair. The dependent variable in the top panel, *Prescription Indicator*, is binary and equals one if the doctor prescribes any of the pharmaceutical firm's drugs at least 50 times. The dependent variable in the bottom panel is total prescriptions. Robust standard errors are in parentheses. *, **, and *** represent significance at the 10%, 5% and 1% levels, respectively.

PANEL A

	Dependent Variable: Prescription Indicator									
Log Payments 2009	0.0274***									
	(0.000500)									
Payment 2009 Indicator		0.227***								
		(0.00433)								
Log Payments 2010			0.0281***							
			(0.000249)							
Payment 2010 Indicator				0.156***						
				(0.00112)						
Log Payments 2011					0.0269***					
					(0.000177)					
Payment 2011 Indicator						0.129***				
						(0.000725)				
Log Total Payments							0.0270***			
							(0.000156)			
Any Payment Indicator								0.126***		
								(0.000674)		
Any Payment - Big									0.207***	0.207***
									(0.00277)	(0.00277)
Any Payment - Small									0.0946***	0.0893***
									(0.000697)	(0.000712)
Total Prescriptions	3.16e-05***	3.16e-05***	2.74e-05***	2.50e-05***	2.45e-05***	2.25e-05***	2.24e-05***	2.25e-05***	2.45e-05***	
	(1.60e-07)	(1.60e-07)	(1.04e-07)	(9.11e-08)	(9.13e-08)	(8.11e-08)	(8.11e-08)	(8.10e-08)	(9.14e-08)	
State Fixed Effects	YES	YES	YES	YES	YES	YES	YES	YES	YES	-
Specialty Fixed Effects	YES	YES	YES	YES	YES	YES	YES	YES	YES	-
Firm Fixed Effects	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Doctor Fixed Effects	NO	NO	NO	NO	NO	NO	NO	NO	NO	YES
Observations	1,670,430	1,670,430	3,340,860	3,674,946	3,674,945	4,009,032	4,009,032	4,009,032	3,674,946	3,674,946
Adjusted R^2	0.321	0.320	0.300	0.287	0.299	0.287	0.288	0.288	0.299	0.308

PANEL B

	Dependent Variable: Total Prescriptions									
Log Payments 2009	6.947***									
	(0.196)									
Payment 2009 Dummy		58.30***								
		(1.708)								
Log Payments 2010			8.327***							
			(0.100)							
Payment 2010 Dummy				43.47***						
				(0.424)						
Log Payments 2011					6.201***					
					(0.0602)					
Payment 2011 Dummy						28.89***				
						(0.236)				
Log Total Payments							6.375***			
							(0.0542)			
Any Payment Dummy								27.86***		
								(0.211)		
Any Payment - Big									59.69***	58.50***
									(1.223)	(1.1160)
Any Payment - Small									18.84***	21.00***
									(0.194)	-0.196
Total Prescriptions	0.0112***	0.0112***	0.00932***	0.00852***	0.00838***	0.00773***	0.00770***	0.00772***	0.00840***	-1.981***
	(0.000113)	(0.000113)	(6.99e-05)	(6.49e-05)	(6.50e-05)	(6.06e-05)	(6.04e-05)	(6.06e-05)	(6.51e-05)	(0.597)
State Fixed Effects	YES	YES	YES	YES	YES	YES	YES	YES	YES	-
Specialty Fixed Effects	YES	YES	YES	YES	YES	YES	YES	YES	YES	-
Pharma Fixed Effects	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Doctor Fixed Effects	NO	NO	NO	NO	NO	NO	NO	NO	NO	YES
Observations	1,670,430	1,670,430	3,340,860	3,674,946	3,674,945	4,009,032	4,009,032	4,009,032	3,674,946	3,674,946
Adjusted R^2	0.324	0.324	0.297	0.263	0.282	0.251	0.252	0.251	0.281	0.3670

Table 6: Quasi-exogenous transfers to physicians inferred from geography

The table shows the same specification from Table 5B, except that payments are instrumented using the distance (in km) from each doctor's office to the relevant pharmaceutical firm. The unit of observation is a (Doctor, Firm) pair. Sample is limited to doctor-firm pairs within 500 km. The dependent variable is the total number of prescriptions written by doctor *i* for drugs marketed by firm *j*. Robust standard errors are in parentheses. *, **, and *** represent significance at the 10%, 5% and 1% levels, respectively.

	Dependent Variable: Total Prescriptions					
Any Payment – Small (instrumented)	189.5***	99.72***				
	(24.54)	(10.94)				
Any Payment – Big (instrumented)			991.6***	729.1***		
			(160.9)	(98.80)		
Any Payment (instrumented)					176.5***	106.4***
					(21.22)	(10.80)
Total Prescriptions	0.00639***		0.00868***		0.00612***	
	(0.000336)		(6.81e-05)		(0.000264)	
State Fixed Effects	YES	-	YES	-	YES	-
Specialty Fixed Effects	YES	-	YES	-	YES	-
Pharma Fixed Effects	YES	YES	YES	YES	YES	YES
Doctor Fixed Effects	NO	YES	NO	YES	NO	YES
Number of Observations	422,343	360,315	422,343	360,315	446,675	360,315

Table 7: Specialization

Only Meal Payment Indicator takes the value of one for a (Doctor, Firm) pair if a doctor only received a meal as payment from a pharmaceutical firm. Columns 1 and 2 repeat the analysis of Panel A of Table 5 (Column 10) but restrict attention to only the drugs outside a doctor's top five and top ten most prescribed drugs. Robust standard errors are in parentheses. *, **, and *** represent significance at the 10%, 5% and 1% levels, respectively.

	Dependent Variable: Total Prescriptions		
	Outside Top 5	Outside Top 10	
Any Payment Indicator - Big	37.28***	29.23***	
	(1.126)	(1.144)	
Any Payment Indicator - Small	15.23***	11.92***	
	(0.232)	(0.247)	
Only Meal Payment Indicator			22.86***
			(0.205)
Pharma Fixed Effects	YES	YES	YES
Doctor Fixed Effects	NO	NO	NO
Observations	1,940,444	1,393,227	2,279,173
Adjusted R^2	0.343	0.355	0.256

Table 8: Statins

This table considers the case of two branded statin drugs and a within-class generic competitor: Crestor (rosuvastatin), Lipitor (atorvastatin), and simvastatin (formerly marketed as Zocor). The dependent variable is the difference in the number of prescriptions between Crestor (both branded drugs) and Lipitor (simvastatin), scaled by each doctor's total Medicare claims. Columns 1-2 consider only those doctors observed to have prescribed both Crestor and Lipitor while columns 3-5 consider only those doctors observed to have prescribed Crestor or Lipitor, and simvastatin. Robust standard errors are in parentheses. *, **, and *** represent significance at the 10%, 5% and 1% levels, respectively.

	Dependent Variable:				
	Crestor - Lipitor	Crestor - Lipitor	Crestor/Lipitor - simvastatin	Crestor/Lipitor - simvastatin	Crestor/Lipitor - simvastatin
Pfizer Payment Indicator	-0.000526***			0.00243***	
	(0.000181)			(0.000204)	
AstraZeneca Payment Indicator	0.00180***			0.000854***	
	(0.000177)			(0.000207)	
Pfizer Payment Indicator - Small		-0.000508***			0.00216***
		(0.000179)			(0.000204)
Pfizer Payment Indicator - Big		-0.00228***			0.0105***
		(0.000675)			(0.000840)
AstraZeneca Payment Indicator - Small		0.00152***			0.000553***
		(0.000175)			(0.000207)
AstraZeneca Payment Indicator - Big		0.0143***			0.0171***
		(0.00115)			(0.00139)
Astra or PfizerPayment Indicator			0.00230***		
			(0.000198)		
Specialty Fixed Effects	YES	YES	YES	YES	YES
State Fixed Effects	YES	YES	YES	YES	YES
Observations	32,860	32,860	90,559	90,559	90,559
Adjusted R^2	0.072	0.083	0.108	0.109	0.114

Table 9: Name-Brand vs. Generic Drugs

This table considers the case of three name-brand drugs and their generic equivalents: Arimidex (anastrozole), Cozaar (losartan potassium) and Protonix (pantoprazole). The dependent variable, *Name-Brand Indicator*, is a binary variable that takes the value of 1 if a doctor prescribes the name-brand instead of the generic (in the case where she prescribes both, a value of 1 is assigned to the drug with the most prescriptions). Column 1 (2, 3) considers only the set of doctors who prescribed Arimidex (Cozaar, Protonix) or its generic equivalent. Columns 4 and 5 combine all of the observations in the first three columns. Robust standard errors are in parentheses. *, **, and *** represent significance at the 10%, 5% and 1% levels, respectively.

	Dependent Variable: Name-Brand Indicator				
	Arimidex	Cozaar	Protonix	All	All
Payment Indicator - Big	0.190***	0.0273	0.116***	0.107***	0.0853***
	(0.00847)	(0.0307)	(0.0283)	(0.0233)	(0.0207)
Payment Indicator - Small	0.0217		-0.00757	-0.00345	0.00809
	(0.0173)		(0.00847)	(0.00767)	(0.00698)
Firm Fixed Effects	NO	NO	NO	YES	YES
Specialty Fixed Effects	NO	NO	NO	NO	YES
State Fixed Effects	NO	NO	NO	NO	YES
Observations	2,361	12,707	12,477	27,545	27,545
Adjusted R^2	0.002	0.000	0.001	0.294	0.400

Table 10: Rent-Seeking

The dependent variable is the total number of prescriptions in the (Doctor, Firm) pair. Columns 1 (2, 3) consider the subset of states in the bottom (middle, top) tercile of the Glaeser and Saks (2006) corruption index. Columns 4 considers all states. Robust standard errors are in parentheses. *, **, and *** represent significance at the 10%, 5% and 1% levels, respectively.

	Dependent Variable: Total Prescriptions | Prescription Indicator = 1			
	Low Corruption States	Medium Corruption States	High Corruption States	All
Any Payment Indicator	20.35***	29.06***	32.13***	21.28***
	(1.147)	(1.151)	(0.909)	(1.232)
Any Payment Indicator * Corruption Index				0.136***
				(0.0193)
Total Prescriptions	0.0147***	0.0201***	0.0158***	0.0169***
	(0.000609)	(0.000349)	(0.000198)	(0.000188)
Specialty Fixed Effects	YES	YES	YES	YES
State Fixed Effects	YES	YES	YES	YES
Pharma Fixed Effects	YES	YES	YES	YES
Observations	83,737	132,883	181,895	397,894
Adjusted R^2	0.345	0.341	0.318	0.326

Table A.1: Payments and Prescription Behavior for (Doctor, Firm) Pairs

The table relates payments made by pharmaceutical firms to prescribing behavior. The unit of observation is a (Doctor, Firm) pair. *Prescription Indicator*, is binary and equals one if the doctor prescribes any of the pharmaceutical firm's drugs at least 50 times. The dependent variable is the total number of prescriptions in the (Doctor, Firm) pair given that *Prescription Indicator* = 1. Robust standard errors are in parentheses. *, **, and *** represent significance at the 10%, 5% and 1% levels, respectively.

	Dependent Variable: Total Prescriptions | Prescription Indicator = 1									
Log Payments 2009	9.882***									
	(0.356)									
Payment 2009 Indicator		86.45***								
		(3.257)								
Log Payments 2010			8.996***							
			(0.192)							
Payment 2010 Indicator				45.89***						
				(0.919)						
Log Payments 2011					6.989***					
					(0.151)					
Payment 2011 Indicator						28.62***				
						(0.646)				
Log Total Payments							7.176***			
							(0.137)			
Any Payment Indicator								28.93***		
								(0.621)		
Any Payment - Big									89.86***	95.05***
									(2.421)	(4.272)
Any Payment - Small									15.61***	10.63***
									(0.577)	(1.136)
Total Prescriptions	0.0184***	0.0184***	0.0174***	0.0170***	0.0173***	0.0169***	0.0169***	0.0169***	0.0173***	
	(0.000264)	(0.000264)	(0.000181)	(0.000186)	(0.000183)	(0.000188)	(0.000188)	(0.000188)	(0.000183)	
State Fixed Effects	YES	YES	YES	YES	YES	YES	YES	YES	YES	-
Specialty Fixed Effects	YES	YES	YES	YES	YES	YES	YES	YES	YES	-
Firm Fixed Effects	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Doctor Fixed Effects	NO	NO	NO	NO	NO	NO	NO	NO	NO	YES
Observations	231,374	231,374	387,958	398,371	388,101	398,515	398,515	398,515	388,102	388,102
Adjusted R^2	0.367	0.367	0.361	0.329	0.358	0.326	0.329	0.326	0.360	0.304