Regulating Conflicts of Interest through Public Disclosure and Social Image Incentives: Evidence from a Physician Payments Sunshine Law

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Abstract:

This paper evaluates whether public disclosure can effectively regulate conflicts-of-interest arising from direct-to-physician pharmaceutical marketing. In 2009, Massachusetts began requiring that pharmaceutical industry gifts, meals, and consulting payments to physicians be published in a publicly searchable database. Using four years of monthly prescriptions data for 5312 physicians and eight drug classes, we demonstrate that disclosure decreased average marketed drug prescriptions by Massachusetts physicians relative to similar physicians in states without disclosure. However, physicians who accepted (and disclosed) the largest industry payments post-law showed no change in prescriptions due to disclosure, suggesting that the observed effect could be driven by the subset of physicians who reduced or abstained from payments to avoid appearing unethical. In addition, disclosure requirements did not disproportionately affect new-to-the-market drugs; this runs counter to industry claims that free gifts and meals only influence prescriptions because they come bundled with information about new drugs. Instead, our results suggest that pharmaceutical gifts may influence physicians via non-informational mechanisms, such as reciprocity, and that mandated disclosure can reduce these influences by invoking public image concerns in physicians.

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I. Introduction

Mandatory disclosure of incentives is a commonly used strategy for addressing potential conflicts of interest. U.S. Congressmen, financial advisors, and academic researchers are often required to publicly disclose financial sources of income that may conflict with their professional responsibilities. Models of rational advisees theorize that disclosure allows individuals to properly discount advice or actions from agents that have conflicts of interest. However, laboratory experiments suggest that advisees do not behave in this manner; instead, many ignore conflicts of interest or otherwise insufficiently adjust beliefs to account for them (Cain, Loewenstein, and Moore 2005; Hampson et al 2006; Loewenstein, Sah, and Cain 2012). Even if advisees adjust their beliefs, they may nevertheless feel pressure to comply with conflicted advice to avoid offending the advisor (Loewenstein, Cain, and Sah 2011). Nevertheless, disclosure may still impact outcomes by influencing advisors; experimental evidence demonstrates that if advisors care about their social image in front of advisees, they may abstain from incentives that create conflicts of interest, leading to changes in their advice (Sah and Loewenstein 2014). This paper tests these hypotheses in a non-laboratory setting by examining the effects of a mandatory public disclosure policy. This policy was implemented in 2009 to address potential conflicts of interest from financial relationships between physicians and the pharmaceutical industry.

The U.S. pharmaceutical industry employs tens of thousands of sales representatives to market their products directly to physicians (Rockoff 2012). These sales reps visit their assigned physicians on a regular (often bi-weekly) basis, and in many cases, they serve as a conduit for free meals and consulting payments to these physicians and their staffs (Fugh-Berman and Ahari 2008). Many medical professionals across the country have voiced concern over the conflicts of interest these incentives may impose (Dana and Loewenstein 2003; Steinbrook 2009). In response, Massachusetts became one of the first states to implement a physician payments sunshine law requiring that all pharmaceutical companies annually provide a list of meals and consulting payments provided to Massachusetts-licensed physicians.[1] The state

[1] Specifically, the law mandated disclosure of all such payments valued $50 or more (see https://www.mass.gov/service-details/learn-about-the-pharmaceutical-code-of-conduct for a summary). Sales reps

collated this data into a publicly searchable database for consumers; this represented the first state law that provided consumers with a publicly searchable database of physician payments.[2] This study uses a difference-in-differences approach to evaluate the impact of this law. It does so by examining monthly prescriptions (in the two years before and after the law) for 254 drugs by 2,719 physicians in Massachusetts and comparing to monthly prescriptions by 2,593 similar physicians in four comparable states.

The paper estimates that, in aggregate, the disclosure law decreased prescriptions of marketed drugs by over 135,000 scripts in Massachusetts in the two years following the policy, for just these specific Massachusetts physicians and drugs. Disclosure had no statistically significant effect on generic drugs, suggesting that disclosure led to a *decrease* in total prescribing volume (specifically, an approximately 4% decrease in total scripts). Importantly, these effects occurred even for the subset of physicians that never showed up in the list of payments submitted to the state during this time period, suggesting that the effects were not simply because patients responded to disclosed payments by requesting different drugs or by switching physicians. Thus, these results do not conflict with laboratory evidence suggesting that advisees (i.e. patients) do not sufficiently adjust beliefs or behavior in response to disclosed conflicts of interest (Cain, Loewenstein, and Moore 2005; Loewenstein, Cain, and Sah 2011; Sah, Loewenstein, and Moore 2013).

Instead, laboratory evidence suggests that the observed effect of disclosure is most likely due to changes in the behavior of advisors (i.e. physicians). Specifically, advisors may opt out of conflicts of interest under conditions of disclosure in order to avoid appearing biased (Sah and Loewenstein 2014). This

generally provide meals for a physician's entire staff, leading to meals expenses well above $50 per doctor visit (Silverman 2015); consulting services also are typically much higher than $50 per incidence (as evidenced by the actual figures reported in the Massachusetts sunshine database). In addition, although the Massachusetts policy placed restrictions on non-educational gifts and off-campus meals, practice-related gifts (e.g. drug samples, branded medical devices such as surgical shears, etc.) and in-office and in-hospital meals were all still allowed, and these have comprised a vast majority of pharmaceutical gifts since 2002, when the industry self-regulated against non-educational gifts (Sillup et al 2010).

[2] Minnesota, Washington D.C., and Vermont implemented public disclosure policies prior to Massachusetts, but their disclosures information was not collated into a publicly accessible electronic dataset. Similar differences hold for now-defunct policies from West Virginia and Maine. Thus, social image is not likely to be a prominent incentive in these alternative states; this difference may explain the mostly negligible disclosure effects on prescriptions observed in other studies based on these other state policies (Pham-Kanter et al 2012).

implies that physicians are responding to a social image or reputation mechanism, similar to results seen in laboratory experiments on social image incentives (Andreoni and Bernheim 2009; Tadelis 2011; Sah and Loewenstein 2014). This mechanism is also consistent with our results; physicians that did not have payments to disclose, and those that had the lowest payments to disclose (i.e. the bottom quartile in meals or consulting payments), showed the strongest magnitude effects in response to the sunshine law. These physicians may belong to these categories because they have opted out of some meals and payments in response to disclosure. This interpretation is also consistent with evidence in the literature that these types of meals and consulting payments cause physicians to prescribe more marketed drugs (Larkin et al 2017); therefore, those that began refusing gifts and meals in response to disclosure would be predicted to decrease their prescriptions of marketed drugs.

Conversely, physicians that had the most payments to disclose (i.e. the top quartile of physicians in meals or consulting payments) showed no response to the sunshine law. These physicians may have ended up in the top quartile because they did not opt out of any industry meals or payments; if so, then they remained similarly conflicted in their interests post-law, leading to no observed changes in prescription patterns. This is despite the fact these top quartile physicians also prescribed more on average and thus had more capability to show larger magnitude effects in response to disclosure.

Pharmaceutical companies argue that if disclosure influences prescribing through physician avoidance of meals and payments, it is only because these meals and payments typically come packaged with relevant medical information about new drugs, such as clinical trials results (Saul 2006; Huang, Shum, and Tan 2012). By avoiding payments and thus the medical information it comes packaged with, physicians lose out on information that could positively influence prescription choices. If this is true, then this would imply an informational mechanism independent of any reputation mechanism, and this would carry important welfare implications for disclosure. To test this, we evaluate whether the sunshine law differentially impacted new-to-the-market drugs (i.e. drugs that have been on the market for one year or less) versus older marketed drugs. If the industry mechanism is correct, then new-to-the-market drugs should be more impacted by the sunshine law, since there is more for physicians to learn from new clinical trials for these drugs. However, new-to-the-market showed little or no response to disclosure, and aggregate results were driven primarily by older marketed drugs. This held true even after controlling for the possibility that newer drugs may have lower marketshare. This therefore runs counter to industry's claim that gifts and payments influence physicians only because they pave the way for salespeople to provide information about new drugs to physicians. Instead, the results are consistent with non-informational channels, such as simple reciprocity to meals and payments, which can still affect older marketed drugs that physicians are already knowledgeable about.

Finally, the social image effect implied by the results is relatively independent from any financial incentives related to physician reputation. The physicians in this study were all full-time attending physicians at academic medical centers; these physicians are typically salaried with no private practice of their own. As a result, their patients come largely from referrals, and their patient volume, number of procedures, and most importantly their income are not directly tied to their reputation the way that it might be for private practice physicians.

Altogether, this paper demonstrates the potential effect of social image incentives in an important healthcare policy context. Although social image incentives have been studied closely in lab settings (Andreoni and Bernheim 2009; Tadelis 2011) and even in field studies on charitable giving (Butera and Horn 2017), few observational studies have implicated social image incentives in more complex policy settings such as conflicts of interest. This paper does so while providing arguments against other related effects, such as reputation-based financial incentives or advisee-driven (i.e. patient-driven) rational updating.

On a practical level, these results carry important implications for understanding the effects of more recent disclosure policies implemented elsewhere. In 2013, the U.S. implemented similar disclosure requirements on the national level via a provision within the Affordable Care Act.[3] The results of this paper suggest that this nation-wide disclosure may help reduce the number of expensive, marketed drugs

[3] This provision required all pharmaceutical companies to publicly disclose all forms of compensation to U.S. physicians starting in 2013.

prescribed by physicians due to non-informational reasons such as reciprocity to salespeople. However, the results also suggest that physicians who continue to accept payments anyways may not be affected by these types of policies.

The rest of this paper is organized as follows. Section II reviews the literature on conflicts of interest, mandated disclosure, and pharmaceutical marketing. Section III summarizes the data. Section IV presents the main results on policy effects. Section V implements various robustness checks of the main results. Section VI identifies policy effects on subsets of Massachusetts physicians that had payments to disclose post-policy. Section VII discusses the mechanisms that are likely driving the observed effects, and also addresses potential confounds. Section VIII evaluates disclosure effects on new versus older marketed drugs. Section IX concludes.

II. Relevant Literature

Biases from Industry-Physician Conflicts of Interest

Laboratory studies have demonstrated how conflicts of interest, such as those that can arise between industry and physicians, can lead to biased recommendations by agents (e.g. Cain, Loewenstein, and Moore 2005; Sah and Loewenstein 2014). This is especially true when there is ambiguity in the environment (Haisley and Weber 2010). Malmendier and Schmidt (2017) further show that uncertainty (instead of ambiguity) is enough to cause an agent to be biased by a conflict-of-interest.

Motivated in part by the above research, many have hypothesized that physicians with ties to industry are biased in their decision-making (Dana and Loewenstein 2003). Within the medical literature, enough has been published to require three successive meta-analyses examining different aspects of industry-physician relationships (Lexchin 1993; Wazana 2000; Spurling et al 2010). The majority of these studies find correlational evidence that marketing activities of varying types can alter prescriptions or other aspects of patient care.

The marketing literature provides larger-scale correlational evidence by examining the relationship between physician-level prescriptions over time and physician-level measures of marketing exposure over time (e.g. Gonul et al 2001; Mackowiak and Gagnon 1985; Manchanda and Chintagunta 2004; Mizik and Jacobson 2004; Rizzo 1999; Engelberg, Parsons, and Tefft 2014). Importantly, these analyses may not be causal in nature, since sales reps track physician prescriptions on a bi-weekly basis and change their marketing tactics in response to a physician's prescription patterns (Fugh-Berman and Ahari 2007). As a result, reverse causality is a concern. Nevertheless, this literature provides additional correlational evidence of the association between these incentives and prescribing patterns.

Finally, Larkin et al (2017) use a quasi-experimental methodology to more causally estimate the impact of gifts and meals on prescribing. The authors treat changes in academic medical center policies that governed marketing activities as exogenous to physician-industry interactions and find that restrictions on these marketing activities correspond to a decrease in the prescription of marketed drugs. Given this and the previously cited evidence, we conclude that if disclosure causes physicians to avoid or opt out of meals or payments, then this will likely lead to a similar effect on prescriptions.

Disclosure Policies

Laboratory studies suggest that mandated disclosure can have a wide range of effects on biases from conflicts-of-interest. Cain, Loewenstein and Moore (2005) use a principal-agent setup to demonstrate that disclosure policies can cause those that disclose to feel morally licensed to be biased (Cain, Loewenstein and Moore 2005), since conflicts of interest are now common knowledge across all parties. In a physician prescriptions context, this implies disclosure of industry payments may allow some physicians to feel morally licensed to prescribe a marketed drug more often. On the other hand, Sah and Loewenstein (2014) use a similar principal-agent game and find that mandated disclosure can cause some agents to opt out of conflicts-of-interests, whenever opting out is possible (Sah and Loewenstein 2014). In a prescriptions context, this suggests physicians may opt out of industry payments in response to disclosure.[4] Indeed, correlational evidence suggests that physicians in states with disclosure laws have lower acceptance rates of gifts and payments than physicians in states without such laws (Chen et al 2013). Since gifts and meals increase marketed drug prescriptions (Larkin et al 2017), this mechanism would cause disclosure to decrease marketed drug prescriptions. Altogether, these laboratory results make disparate predictions on whether disclosure will increase or decrease marketed drug prescriptions.

Pham-Kanter et al (2012) use the 2004 Maine and West Virginia sunshine laws to more causally measure the effect of disclosure on physician prescriptions. They examine selective serotonin reuptake inhibitors (SSRIs), a class of antidepressants, as well as statins, which treat high cholesterol. They find that these disclosure policies did not cause any changes to the rates of marketed drugs prescribed relative to neighboring states. However, these states' disclosure policies did not require public disclosure of payments; instead, disclosure was only to a state government department. Thus, physicians knew that their patients had no information on physician-industry interactions, and the effects measured by this study have no relation to *public* disclosure. These results did not invoke physician concerns over their reputation and image, and thus may not generalize to the effects of the Massachusetts or federal disclosure laws, both of which require disclosure via a publicly searchable database.

In a working paper, Guo, Sriram and Manchanda (2017) use a smaller dataset to measure the effect of the Massachusetts sunshine law on prescriptions. They limit their sample to three drug classes (statins, antidepressants, and antipsychotics) and use physicians operating on the geographic borders of Massachusetts, Connecticut, and New York. Similar to our paper, they find that disclosure decreased the prescription of marketed drugs. However, while geographic borders often represent a plausible identification strategy, in this case it ignores the fact that their physicians are largely private practice and thus may face long-term financial repercussions from negative social image resulting from disclosure of

[4] Given the negative connotations to conflicts-of-interest, physicians may be motivated to avoid having conflicts to disclose even if they are convinced that they are not influenced by industry marketing. In fact, some physicians have stated this sentiment directly in response to other efforts at disclosure of conflicts of interest (Wen 2014).

payments.[5] As a result, they cannot isolate whether their results are driven by financial or social incentives. Our paper sidesteps these issues by only comparing full-time attendings at academic medical centers, whose incomes likely are not tied to patient or procedure volume. Our paper also differs from Guo et al (2017) by comparing disclosure effects on those that accepted high payments post-disclosure to those that accepted no such payments; this tests whether disclosure only impacts those who are likely opting out of conflicts of interest due to the policy.[6]

Informational vs. Non-Informational Influence

Supporters of industry-physician payments, including pharmaceutical companies, often argue that these interactions yield positive effects for patients (LaMattina 2016). They claim that gifts, meals, and other small payments are necessary for gaining access to physicians so that industry reps can spread product efficacy information on new drugs to practitioners (PhRMA 2002). There is in fact empirical evidence for this rationale; Huang, Shum and Tan (2012) find that important new contraindications information for the drug Crestor spread faster to physicians with ties to industry than those with no ties.

This study will examine the effects of disclosure on older versus newer marketed drugs to evaluate whether changes in marketing exposure (caused by disclosure) differentially impact newer versus older marketed drugs. This analysis is based on the assumption that on average, there will be more information on newer drugs to pass on to physicians than older drugs that have existed for many years and are generally well understood. If newer drugs are not differentially impacted by disclosure, this would suggest that a non-informational mechanism of influence may account for the effect of disclosure on prescriptions. This test represents a third way in which our paper extends beyond the results from Guo et al (2017).

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[5] In addition, their identification ignores state-level differences in laws (e.g. taxes) that might influence whether a physician chooses to establish a private practice on one side of a geographic border or another.
[6] Our paper also differs by using a much larger sample of eight drug classes and four counterfactual states.

III. Data

The dataset consists of four components: (1) physician–hospital affiliations over time for all physicians affiliated with any academic medical center (AMC) in one of six metropolitan regions (MSAs), one of which is in Massachusetts; (2) monthly prescriptions by each physician from January 2006 to December 2012 for all drugs within eight drug classes; (3) drug characteristics including whether a drug is branded and when a generic version for each branded drug became available; and (4) annual lists of industry payments to specific Massachusetts physicians between July 2009 and December 2012. Each of these components is listed in more detail in Figure 1.

(1) Physicians and Affiliations Data

The data consists only of physicians who are affiliated full-time with an AMC. We make this choice for several reasons. First, this ensures that physicians in the control and treatment groups are comparable in terms of training, expertise, and background; all of these AMC physicians are clinicians full-time affiliated with some of the most respected medical centers in the world. Second, full-time attending physicians at AMCs are generally salaried clinical faculty members whose patients are derived through referrals from primary care physicians. Thus, both income and patient volume for these physicians should be mostly independent from their actual reputation or public image. If results suggest these physicians are averse to disclosing industry payments, we can better isolate the reasons why these particular physicians might care (or not care) about their reputation.

We chose to focus on physicians employed by AMCs in six MSAs (listed in Figure 1), one of which is in Massachusetts. These MSAs were chosen because they contain the most AMCs, and also because they include similar quality AMCs that should employ similar quality physicians.

Next, we obtained a list of all physicians that were full-time affiliated with any of these AMCs, provided they prescribed any of the drugs selected for this study (detailed in the next section). Affiliations data were obtained from IMS Health, a leading pharmaceutical market research firm. IMS tracks all hospitals that are owned or governed by AMCs. IMS also surveys hospitals quarterly to obtain physician-hospital affiliations data. IMS identified all attending-level (FTE) physicians affiliated with any of these hospitals for at least one quarter between January 2006 and June 2009, and who regularly prescribed drugs from at least one of the drug classes we selected. In total, 9,998 physicians initially met these criteria and were initially included in this dataset. Physicians did not switch affiliations frequently in the data (only 12% switch during this 3.5 year period); for affiliations data corresponding to July 2009 through December 2012, we carry forward the affiliations data from June 2009.[7]

(2) Prescriptions Data

IMS Health also provided physician prescriptions data. IMS compiles monthly physician-level prescriptions data by purchasing data directly from retail pharmacies. In total, IMS purchases data on approximately 75% of the retail market and projects the rest using geographic and demographic variables. This is the most comprehensive dataset on the market, and it is widely considered the best source of prescriptions data by pharmaceutical companies and academic researchers alike.[8] IMS provided total prescriptions filled for every physician-drug-month in the dataset.

The dataset includes monthly prescriptions data for 377 drugs in eight drug classes: statins, antihyperglycemics, proton pump inhibitors, antihypertensives, antidepressants, sedatives (anxiolytics and sleep aids), antihyperactives and antipsychotics. These drug classes were chosen based on physician feedback identifying each of these classes as a composition of drugs that are heavily marketed and not marketed at all. For each of these drug classes, all drugs within the class (according to IMS classifications) were included except for drugs that were rarely, if ever, prescribed during this time span by these physicians.

Prescriptions data were purchased in two batches. The first, purchased in 2010, covered 218 drugs from January 2006 – June 2009 and was initially intended for use in a different study that required the same conditions (Larkin et al 2017). The second batch, purchased in summer 2013, covered July 2009 –

[7] Affiliations for 2010-2012 were not available to us. Physicians newly affiliated with these AMCs after July 2009 are not included; since physician turnover from 2006-2009 was not high, this should not exclude too many physicians.
[8] Since the data represents only the retail market, prescriptions filled at hospital pharmacies or by mail are not included. Additionally, prescriptions that are written but not filled are not observed in the data.

December 2012 and included all original 218 drugs that were still commonly prescribed (some were no longer produced or commonly prescribed by June 2009); in addition, this second batch added new drugs from these drug classes that entered the market around or after June 2009 and were commonly prescribed.

(3) Additional Drug Data

Additional details about each drug were obtained through the FDA's website on FDA-approved drugs.[9] This data identifies whether a drug is branded, and if so, whether a generic version was or is available on the market. To proxy for whether a drug is marketed in a particular month, we identify whether the drug is branded and whether a generic version was on the market in that month. Almost all branded drugs with no generic alternative will be marketed; according to IMS Health data from 2006-2009, more than 95% of such drugs had salespeople assigned to market the drug. Since actual sales force data was not available to use, we use this proxy instead. To the extent that this captures a few drugs that are not marketed, this would bias against finding effects of disclosure on marketed prescriptions.

(4) Massachusetts Disclosures

Industry gifts, meals, and payments to Massachusetts physicians for July 2009 through December 2012 are publicly available and were downloaded from the Massachusetts Department of Health and Human Services website.[10] This dataset includes the name of each physician and the value and type of compensation they received (i.e. consulting fees or meals). This data was merged to the IMS dataset using physician first name, last name, and in some cases middle initial or middle name. Address and type of degree (e.g. M.D., O.D., R.N.) were also used to improve matches in the rare cases (<0.5%) where matching by name led to duplicate matches.

[9] Available at https://www.accessdata.fda.gov/scripts/cder/drugsatfda/
[10] Available at http://www.mass.gov/eohhs/gov/departments/dph/programs/hcq/healthcare-quality/pharm-code-of-conduct/data/data-download.html

Table 1 summarizes the disclosures data. In total, there are 4,278 physicians in our data that were affiliated with a Massachusetts AMC after the sunshine law was implemented, although only 2,719 remain after data exclusions (see the subsequent sub-section titled “Data Exclusions”). Of these, 413 physicians accepted a meal of $50 or greater at least once from 2009-2012 and 345 physicians accepted consulting payments at least once.

There are two caveats to this data. First, in 2009, only 6 months of data are included, since the policy took effect in July 2009. Second, in 2012, there are fewer entries; while this may partly be because physicians are accepting fewer meals and consulting payments over time, it is also due to a change in reporting requirements. For 2012 and onwards, the Massachusetts state legislature specified that payments already scheduled to be reported to the federal government (due to the new federal sunshine law, which was supposed to begin tracking payments in 2012) did not have to be reported to Massachusetts. Since the federal dataset did not end up including 2012, these other payments are not available in either dataset.

Data Exclusions

When analyzing the data, we exclude several types of observations from the data. This is done to avoid a specific set of confounds described below.

First, we drop physicians who, according to IMS Health designations, have attending affiliations to multiple AMCs for any one quarter in the sample. These physicians may split their time between many hospitals, including even between states. In addition, these physicians may be different from the others in this dataset, since they are not a full-time employee for a single AMC; for instance, they may be erroneously classified as attendings by the IMS survey and thus may have a private practice. Second, we drop all physicians who switch hospitals or states at some point in the dataset. This rules out the possibility that physicians switched affiliations in response to the policy. These two exclusions remove approximately 16% of physicians from the original list of physicians provided by IMS.

Next, we focus all analysis on just the 24 months before and after the sunshine law was implemented, and drop all observations outside of this 48-month period. This reduces the likelihood of other events impacting the fixed differences assumptions that we make. In particular, health policy experts expressed concern that changes in Massachusetts insurance laws (namely “Romneycare”) implemented in 2006 could impact prescribing patterns in 2006; to avoid confounds from this, we exclude all prescriptions from 2006. The earliest prescriptions data we use is July 2007, corresponding to exactly two years before the sunshine law was implemented and thirteen months before the sunshine law was signed by lawmakers (and publicized). We choose to balance the dataset by also allowing exactly two years of post-disclosure prescriptions data; this conveniently excludes 2012 prescriptions data corresponding to the year Massachusetts amended the disclosures law. In addition, by the end of this 48-month time period, only a single batch of payments data (corresponding to July09-Dec09) had been released by the state, and thus the measured disclosure effects do not include any possible effects related to the release of 2010 or 2011 payments data. This helps us better isolate the mechanism behind the observed effects, as explained further in the results section.

Third, we drop all branded drugs where a generic version was introduced during this 48-month period.[11] Since our sample of physicians have not been stratified by specialty, physicians in one state can respond more drastically than physicians in another state when a drug comes off patent; for instance, 75% of Massachusetts physicians in our sample have prescribed a psychiatric drug at least once, while only 67% of control physicians in our sample have done so. Thus, marketed prescriptions in Massachusetts will respond more sharply to the introduction of a generic version of Prozac than marketed prescriptions in control states, leading to violations of the fixed differences assumption for marketed drugs.[12] For similar reasons, we exclude generic drugs that were introduced to the market during our time sample.

Finally, we drop *physician:drug-class* pairings that are almost always zero. For instance, the psychiatrists in the dataset will not prescribe many drugs from the statins class. Likewise, cardiologists do

[11] New branded drugs that came on the market in the middle of the time period *are* included as long as no generic version became available during this same time period.

[12] If a doctor prescribes a branded drug when a biochemically equivalent generic exists, insurance companies require the pharmacist to directly substitute the generic; these substitutions are not of interest in our analysis. The introduction of a generic version of a branded drug is unlikely to have nearly as significant an impact on non-biochemically equivalent branded drugs and thus these exclusions help maintain the fixed differences assumption.

not generally prescribe antidepressants. We drop all physician drug-class pairings where the physician prescribed fewer than 48 scripts of all of the drugs in a drug class over the 48-month time period. A physician would have to prescribe no more than a single script per month[13] of an entire drug class to fall under this threshold. This eliminates pairings such as psychiatrist-statins and cardiologist-antidepressants.

For every *physician:drug-class* pairing that remains, every physician-drug-month observation is included, even if the physician never prescribes that drug. For instance, if a physician regularly prescribes Cymbalta and Prozac, but no other anti-depressant, that physician would still have observations (with prescriptions values of zero) corresponding to every other anti-depressant included in the dataset, for every month.[14] These zeroes represent the possibility that the physician could have (but chose not to) prescribe these drugs for patients in need of an antidepressant; including them naturally biases estimated policy effects towards zero, but excluding them could bias results in the opposite direction.

Table 2 displays summary statistics of the final physician-drug-month dataset. Table 2 emphasizes the high percentage of observations that are zero (~90%). This is not surprising considering that physicians who regularly prescribe a drug class (such as statins) are still unlikely to prescribe every drug in that class every month; this is especially true for larger drug classes, such as anti-depressants. Naturally, this impacts the average prescriptions across all physician-drug-month observations; averages reported in Table 2 vary from 0.40 to 1.13 scripts per physician-drug month. However, these figures make sense when we consider just how many drugs this applies to for each physician-month. For instance, a physician that prescribes only antidepressants and averages 0.4 prescriptions per physician-drug-month across both generic and marketed drugs would still be writing 20 scripts per month, since there are up to 45 products in the antidepressant drug category in a given month. Physicians that average 0.4 prescriptions per physician-drug-month but who prescribe more than one drug class would be writing 35+ scripts per month. This interpretation will be

[13] These were calculated after the exclusion of drugs listed earlier in the data exclusions section.
[14] Drugs that were only included in the data for 24 months (i.e. drugs that were only included in one of the two batches of prescriptions data we purchased – see Data section) are naturally only included for those 24 months; likewise, branded drugs that entered the market in the middle of the data also have less than 48 total observations per physician.

relevant for analyzing later regression results that report the average effect of the sunshine law on each physician-drug-month observation.

Importantly, regression results are always robust to not making these data exclusions. However, for the reasons stated above, our main results use the restricted sample of data, which provides the best identification.

Parallel Trends

Figure 2 displays the raw data for marketed drug prescriptions for the two years before and after the sunshine law was implemented. Note that *marketed* drugs refer only to branded drugs that do *not* have a biochemically equivalent generic version on the market for a given month.[15] The two lines represent the treatment and control groups. The *MA* group consists of physicians that are always affiliated with a Massachusetts AMC and are thus subject to the disclosure law as of July 2009. The *Other* group consists of physicians always affiliated with AMCs in Pennsylvania, Illinois, New York, and California; based on their hospital affiliations data, none of these physicians were ever subject to any state disclosure requirements during this time. Since we have limited the data to physicians that never changed affiliations, the number of physicians in each group is constant; we therefore simply plot the total number of marketed prescriptions that each group prescribed in each month. The figure also includes a bar plot of the exact differences in each month; from August 2007 through June 2009, differences appear relatively fixed.

If there is a policy effect on marketed prescriptions, Figure 2 should show a change in *slope* around the time the policy was signed and/or implemented, but not a discontinuous jump in marketed prescriptions. This is in part because the prescriptions data includes prescription refills, which constitute the vast majority of total prescriptions filled at retail pharmacies. Patients already on medication that works for them will continue to stay on that medication regardless of changes to disclosure requirements (especially for the

[15] Branded drugs that have a generic version already on the market are classified with the generics, since they are virtually never marketed and are also rarely prescribed. Even if a physician prescribed such a drug, the pharmacist would be required by insurance companies to replace it with the generic version, and the filled-prescriptions data from IMS would only reflect a prescription for the generic.

chronic conditions treated by the drugs in this data, e.g. hypertension, depression, high cholesterol, etc.). Thus, refills are not likely to be affected by disclosure. Instead, disclosure will primarily affect first-time prescriptions, leading to a change in slope to marketed prescriptions as new prescriptions are mixed into the data over time. Figure 2 displays exactly such a gradual change in slope in response to disclosure.

Figure 3 provides the same graph as Figure 2, except it examines prescriptions of generic drugs instead of marketed drugs. Since generics show a fairly fixed difference both pre- and post- sunshine law, the raw data suggests that mandated disclosure is more likely to affect how often marketed drugs are prescribed rather than how often generic drugs are prescribed.

Note that we plot both the time the policy was signed (August 2008; see Massachusetts Session Laws for 2008, Chapter 305[16]) and the time it took effect (July 2009), since physicians could respond to either milestone by changing their prescriptions. For instance, when the law is signed and announced, physicians may hear about it through sources such as Blue Cross Blue Shield[17] and decide to change their interactions with industry representatives immediately in anticipation of the impending implementation. On the other hand, physicians may pay closer attention to the actual implementation date and only change just before the policy actually takes effect. In the next regression session, we will analyze effects using both the month the policy was signed and the month the policy took effect.

IV. Aggregate Policy Effects

Statistical Method

In this section, we use ordinary least squares to implement a difference-in-differences regression model. We choose a linear model in part for ease of interpretability, but also because it can more easily handle the many physician-drug-month observations with zero prescriptions. Non-linear models will struggle with the variance matrix of this data, which can be highly singular due to the many zeroes.

[16] Documented at https://malegislature.gov/Laws/SessionLaws/Acts/2008/Chapter305
[17] For instance, this publication from September 2008: https://bluecrossmafoundation.org/chapter-305-acts-2008

The primary dependent variable (DV) of interest is the number of scripts of a specific drug prescribed by a particular physician in a given month. The main independent variable of interest is whether a sunshine-law is in place for each physician-drug-month observation. In some specifications, we will instead use an independent variable for whether the sunshine-law has been signed/announced, regardless of whether it has been implemented yet. The regression model is represented as:

$$R_{ijt} = \beta_0 + \beta_1 * sunshine_{st} + \beta_2 * marketed_{jt} + \beta_3 * (marketed_{jt} * sunshine_{st}) + \lambda_1 * X_{ijt} \quad (1)$$

where i represents the physician, j represents the drug, t represents the month, s represents the state, and X_t represents a vector of control variables. R_{ijt} represents prescriptions for that physician-drug-month.

The $sunshine_{st}$ variable is an indicator for the sunshine-law. It takes a value of 1 if a sunshine law was in effect for that physician-month. For physicians in Massachusetts, $sunshine_{st}$ takes a value of 0 in all months prior to July 2009, and 1 for all months from July 2009 onwards. For physicians not in Massachusetts, it always takes on a value of 0. In alternative specifications, this instead measures whether the sunshine law has been signed; it therefore takes a value of 1 for all months from August 2008 and onwards, but only for Massachusetts physicians.

The $marketed_{jt}$ variable represents whether a drug is marketed by its manufacturer for a given drug-month. For previously discussed reasons, we use a binary indicator that represents whether the drug is branded with no generic version available in that month.

The $marketed_{jt} * sunshine_{st}$ interaction separates the effect of the sunshine law on marketed drugs from its effect on non-marketed drugs. The coefficient for $sunshine_{st}$ (β_1) measures the effect of the law on non-marketed drugs, and the coefficient for $marketed_{jt} * sunshine_{st}$ (β_3) measures the additional effect that the law has on marketed drugs. To properly interpret how the sunshine law affects a marketed drug, we will evaluate the linear combination of the coefficients for $sunshine_{st}$ and $marketed_{jt} * sunshine_{st}$.

The remaining variables are a set of controls. This includes month fixed effects as well as physician*drug fixed effects. State and AMC fixed effects are not included, since the sample is limited to physicians that never switch affiliations, so physician is collinear to state and AMC. Since including both fixed effects and a lagged dependent variable can complicate identification (Angrist and Pischke 2009), the base specifications use fixed effects but no lag.

Standard errors are clustered at the AMC level. Although the disclosure policy and thus treatment is at the state level, we only have five states in the data and clustering over so few states could lead to bias (Cameron and Miller 2014). Instead, we choose to cluster at the AMC level, which can also partially account for institution-level correlations in prescriptions (e.g., philosophical practices, administrative influences, patient demographics, etc.). However, all results are robust to clustering at the physician or state level instead.

In a follow-up regression (Model 3), we run the same specification but replace the DV with a drug's marketshare. For each physician-drug-month observation, we divide the total prescriptions by the total number of drugs prescribed by that physician in that month, and set R_{ijt} equal to this marketshare measure. This marketshare analysis adjusts the policy effect to account for differences in total prescribing volume across physician-months. However, since some physicians prescribed no drugs in these drug categories in a given month, this specification requires adding a binary indicator to capture zero-prescription months.

Results

Table 3 displays the fixed effects panel OLS results from the above specifications. Model (1) represents the main result of this paper. It indicates that the sunshine law decreased marketed prescriptions by on average 0.103 scripts per physician-drug-month for *every* marketed drug the physician prescribes. The model shows no statistically significant effect of the policy on generic drugs. The model estimates that disclosure led to 135,440 fewer prescriptions of marketed drugs for just the Massachusetts physicians and drugs in this dataset, representing a nearly 4% decrease in prescriptions.[18] Since there was no corresponding statistically significant effect on generic drug prescriptions, this indicates that the disclosure policy caused physicians in Massachusetts to prescribe *fewer* drugs overall. In other words, meals and payments from

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[18] This is because the 0.103 coefficient applies to 1,314,951 physician-drug-month observations that occur in Massachusetts post-disclosure.

pharmaceutical companies may cause physicians to prescribe marketed drugs to patients they otherwise would not have prescribed *anything* to.

Model (2) estimates the same specification but using the date the sunshine law was signed as the treatment event, as opposed to the date the law went into effect. This captures the possibility that physicians, upon hearing the disclosure law, might change their willingness to accept meals and payments from sales reps even before the policy takes effect. For instance, physicians may not have paid close attention to the details of the policy, or they may decide that they should stop accepting earlier to be "safe" or to begin adjusting their habits and routines in anticipation of the change. Results show largely similar effects as Model (1), although point estimates are slightly smaller, as might be expected given that not all physicians would likely respond to just the announcement.

Model (3) estimates the same specification as Model (1), but using drug marketshare as the DV. This model estimates that disclosure reduced each marketed drug's marketshare by 0.2%, although the policy effects on generic and marketed drugs are only marginally significantly different. Results in this model likely differ from Model (1) in part because marketshare is a zero-sum measure; a decrease in marketed drug marketshare necessarily leads to an increase in generic drug marketshare, so results are more statistically significant for generic drugs and less statistically significant for marketed drugs than in Model (1). In other words, Model (3) by nature implements more of a substitution story than Model (1).

V. Robustness Checks

To further test the assumptions of the difference-in-differences model, we run a lag and leads model that separately estimates difference-in-differences coefficients for all 48 months. In Figure 4, we plot these coefficients by month, omitting the month prior to the policy being signed. We choose this month since months between signing and implementation could theoretically show some effect of the policy. Figure 4 demonstrates that in the 12 months prior to the policy being signed, there is no significant trend between groups; however, after the policy is signed, branded prescriptions for Massachusetts physicians drops, especially in the months following implementation. These trends in general support the fixed differences assumption of the identification strategy.

VI. Policy Effects on Massachusetts Physicians, Categorized by Industry Ties

Procedure

The Massachusetts payments data (see Table 1) identifies which physicians accepted meals or consulting fees after the sunshine law was passed.[19] Unfortunately, the data only identifies payments at the year-level; in addition, the 2009 and 2012 data are incomplete (see previous Data section). It is therefore difficult to use the timing of these payments to evaluate the effect of payments on monthly prescriptions. We instead use this data to classify physicians as a *consulting physician* if they ever appear in the data for accepting consulting fees from 2009-2012. We then further categorize these *consulting physicians* into quartiles according to the total value of consulting payments they accepted from 2009-2012. We similarly classify physicians as a *meals physician* if they appear in the data for having accepted meals, and categorize these physicians into quartiles according to the total value of meals they accepted from 2009-2012. Table 4 displays the dollar value cutoffs for these quartiles. These groups are contrasted with *non-consulting* and *non-meals physicians* – those in Massachusetts who had no such payments to report post-sunshine law. Table 5 displays regressions that separately estimate disclosure effects for these groups of physicians.

In addition, Table 6 presents the same analysis as the main regression, Model (1), but excluding all doctors that showed up in the meals or consulting payments data for 2009. This 2009 payments data was released in late November 2010 (Sullivan 2011), and thus it is possible that the release of this data could have influenced prescriptions behavior for these doctors in the last year of this dataset (e.g. if patients found the data and requested changes in prescriptions). Excluding these doctors limits the regression to those whose names never showed up in the meals and consulting payments data during the time spanned by this dataset (the 2010 payments data was not released until November 2011, four months after the last month in

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[19]As noted previously, only payments of $50+ were required to be reported, although some pharmaceutical companies still disclosed some smaller payments, leading to some data points below the $50 threshold.

this dataset). Model (6) therefore tests for disclosure effects on doctors that could not have been influenced by patient, media, or physician response to seeing the physician's name in the payments data.

Regression Results and Interpretation

In Table 5, we run the same regression as in Model (1), but we include indicators and interaction terms that account for whether a physician is a meals physician or a consulting physician, and if so, which quartile of each category the physician belongs to. Standard errors are clustered at the AMC level, as usual.

Model (8) shows the regression for meals physicians. This model demonstrates that physicians in the top quartile of meals showed no statistically significant response to disclosure, with a point estimate that is positive. Physicians in the 2nd, 3rd, and 4th quartiles showed progressively larger (and more statistically significant) negative effects of disclosure on marketed drug prescriptions, with the bottom quartile exhibiting a large -0.25 script decrease in marketed drug scripts per physician-drug-month. This is despite the fact the top quartile are higher overall volume prescribers (and of marketed drugs specifically) and thus have more opportunities to decrease prescriptions than any other quartile. These results suggest that physicians who accepted large amounts of meals post-law are simply unresponsive to disclosure; they may have accepted meals pre-law and did not care to change their exposure to industry meals post-law, thus leading to their top-quartile status as well as no change in prescription behavior. However, they also did not increase their prescriptions of marketed drugs and therefore did not exhibit moral licensing, at least according to this specification. Physicians in the lower quartiles (and some of the non-meals physicians) may have decreased their exposure to industry meals in response to disclosure, perhaps in hopes they would not appear in the disclosures data or to at least lower the dollar amount associated with them in the dataset. Consistent with prior research, this change in exposure to industry meals would lead to decreased prescriptions in marketed drugs (Larkin et al 2017).

Model (9) tells a similar story for consulting physicians. Non-consulting physicians and consulting physicians in the bottom three quartiles all decreased prescriptions of marketed drugs in response to disclosure, while those in the top quartile of consulting payments showed no change in prescriptions in response to disclosure (with a point estimate close to 0). Note that consulting payments could include payments for research trials conducted in conjunction with industry; these research-oriented clinicians may vary in how frequently they see patients and prescribe. As a result, the story with consulting doctors is less straight-forward than meals doctors, although the general trend appears similar.

Both regressions also demonstrate that non-meals and non-consulting physicians decreased marketed prescriptions in response to the policy even though some of these physicians likely never interacted with industry even pre-law. For instance, survey results from other studies suggest that about 70% of physicians accepted free meals around 2008 (Campbell et al. 2010); thus, 30% of physicians in the non-meals group may have never accepted meals or payments even pre-disclosure.[20] These physicians likely experienced no change in prescriptions due to disclosure, and these physicians would therefore reduce the estimated average effect of the disclosure law on the entire group of non-meals Massachusetts physicians. It may therefore be difficult to compare effect sizes between those that accepted payments post-disclosure versus those that didn't, although it appears safe to say that all physicians except the top quartile groups decreased marketed drug prescriptions on average in response to disclosure.

Model (10) in Table 6 demonstrates that the main results from Model (1) are robust to excluding doctors whose names showed up in the 2009 payments data. This is not surprising given how few physicians showed up in the 2009 payments data. Since these doctors never showed up in the meals or consulting payments data during the time span of this dataset, the observed effect of disclosure cannot be driven by patient, media, or doctor response to seeing specific doctor names in the published data.

VII. Interpretation and Possible Mechanisms

[20] Chen et al (2013) estimate that 20% of physicians in Massachusetts accepted payments post-disclosure. Using the 70% figure from Campbell et al (2010) to estimate the fraction of physicians accepting payments pre-disclosure, this suggests that 63% of physicians in the non-meals group accepted payments pre-disclosure, but stopped doing so post-disclosure. If we assume that physicians who never accepted payments pre-disclosure were unaffected by the policy, then we can assume the average effect on those that used to accept payments but stop doing so post-disclosure is 1.63 x 0.10 = 0.163 scripts per marketed drug per month. Naturally, this back-of-the-envelope calculation should be viewed with skepticism, but it is included in this footnote as a sanity check of sorts.

In this section we discuss whether these observed changes in prescriptions could be caused by changes in physician behavior, in industry marketing behavior, or in patient behavior.

First, our evidence suggests these effects are not likely to be patient driven. Only the first set of physician payments data (for payments made in July 2009 – December 2009) was published by the last month of prescriptions in this dataset, and Model (5) demonstrates that disclosure effects are robust to excluding physicians that showed up in that first batch of payments data. Therefore, the observed changes are not attributable to actual usage of the data by patients (or colleagues, hospitals, or the media).

Instead, our evidence is consistent with a physician social image mechanism, where physicians opt out of meals or payments to avoid being perceived as biased or unethical in the eyes of patients, colleagues, or the public. This reduction in meals and payments would lead to the observed changes in prescriptions, as demonstrated by previous literature (Larkin et al 2017). Consistent with this, we find that physicians in the top quartile of meals and consulting payments post-disclosure were *not* affected by the policy; this lack of effect may be because these physicians did not reduce meals or payments in response to disclosure, leading to their top-quartile status. This mechanism is also consistent with correlational evidence from other literature showing that the percent of physicians that accept gifts from industry in Massachusetts post-disclosure is less than physicians in states without disclosure (Chen et al 2013), as well as with laboratory evidence suggesting that many agents will opt out of conflicts-of-interest in the face of disclosure (Sah and Loewenstein 2014). Finally, our results differ from previous studies that showed no effect of mandated disclosure to state governments (Pham-Kanter et al 2012), suggesting that the public nature of disclosure in Massachusetts may be crucial to our observed effects.

These physician-level image concerns may be separate from physician-level concerns over how their reputation might affect their long-term income. Since the physicians in this dataset are full-time attendings at an academic medical center, they are salaried employees whose incomes should be largely independent of the number of procedures or tests they perform and the number of patients that they see. In addition, their patients come largely from referrals, so their patient volume would not be affected by their reputation in the way private practitioners' might be. Finally, physicians in general seem to care about (and display emotional responses to) how their patients perceive them ethically, independently from any impact on earnings, as evidenced by physician message board responses to other disclosure efforts (Wen 2014).

Finally, there is no evidence that pharmaceutical companies initiated any reductions in meals or payments to Massachusetts physicians in response to disclosure requirements. First, as the disclosures data illustrates, pharmaceutical firms continued to provide significant numbers of meals and consulting payments to Massachusetts physicians post-law.[21] Second, no industry source has ever publicly suggested implementing a state-level change in marketing (such as a state-specific change in emphasis on marketing activities, or a state-specific change in sales force size) in response to the Massachusetts sunshine law. Instead, news interviews with industry appear to imply that salesperson marketing strategies are largely national and not region-specific, and moreover that recent national-level changes in the nature of physician-industry interactions are attributed to changing physician attitudes towards salespeople and not to any state-specific disclosure laws (Rockoff 2012). Recent national-level decreases in various marketing activities have also been publicly attributed to loss of exclusivity (i.e. patent expiration) of important drugs, as well as an increase in online, "virtual" meetings between industry and physicians (Sullivan 2013); they have never, however, been attributed to disclosure policies at either the state or federal level.

Other Identification Concerns

There are two Massachusetts-specific changes in healthcare provision around this time period that we must account for when interpreting our results.

1. *Romney Care*

The most well-known Massachusetts-specific healthcare initiative in this time period is what is known as Romney Care, named after the Massachusetts governor who signed the law. This bill was signed

[21] Note that the sunshine law required pharmaceutical firms to track and report these payments, which entails a mostly fixed cost of establishing a tracking system; once this fixed cost is paid, there is little additional cost of tracking to increasing the number of meals or payments made.

in April 2006 and required that nearly all Massachusetts residents obtain a minimum level of health insurance coverage.[22] To help implement this, the bill created an independent public authority, the Commonwealth Health Insurance Connector, to act as an insurance broker and offer subsidized private insurance plans to residents. This bill was driven in part due to rising costs of insurance, as well as concern over free-riders who did not have insurance but would use emergency room services for non-emergency medical care. In 2010 the state also began restricting residents to an open enrollment period for purchasing insurance through the Connector.

Importantly, if Romney Care affected prescriptions, it would likely be in the first years of the bill (in 2006-2007). This would be when Romney Care would likely have the largest impact on the number of insured individuals in the state; by July 2009, when the disclosure policy was implemented, many of the previously uninsured would already have enrolled in health insurance.[23] Moreover, an increase in health insurance coverage should theoretically *increase* marketed drug prescriptions because increased insurance coverage makes the expensive, marketed drugs more affordable to patients. Similarly, the increase in insurance coverage could also increase patient visits to physicians, thus increasing drug prescription volume. These all suggest against Romney Care being the driver of any observed decrease in marketed prescriptions in response to the disclosure policy.

2. Alternative Quality Contracts

In 2009, Blue Cross Blue Shield of Massachusetts, a health insurance company, launched a new payment arrangement, known as the Alternative Quality Contract (AQC). These contracts stipulated fixed payments for the care of a patient over a specified time period, and they connect payments to quality goals and a five-year budget (Chernew et al 2011). In particular, providers could receive quality bonuses for staying under budget. In 2009, seven provider organizations in Massachusetts entered into these contracts,

[22] The bill also called for the state to provide free health insurance to those earning less than 150% of the federal poverty level, and it also required employers with 10+ full-time employees to provide insurance to employees.
[23] Since we excluded 2006 prescriptions data from this analysis, any such effects of RomneyCare on 2006 prescriptions would not affect our regression estimates.

and another four joined in 2010. However, even after 2010, this covered only 1600 primary care physicians and 3200 specialists (Chernew et al 2011), which represent a small fraction of the physicians in the state, and likely also a small fraction of the physicians in our dataset.

In addition, these changes are unlikely to lead to changes in prescriptions. Afendulis et al (2014) demonstrate that AQCs in Massachusetts did not have any impact on the use of either marketed or generic drugs. They used a difference-in-differences approach comparing drug prescription usage by Massachusetts physicians belonging to providers that enrolled versus did not enroll in an AQC in 2009. They find that AQCs had no effect on prescriptions between these two groups of Massachusetts physicians, and therefore this insurance event is unlikely to have had any impact on the observed results.

VIII. Informational versus Non-Informational Influence

There is significant debate over whether meals and other forms of industry payments are informational or non-informational sources of influence on physicians. Anti-marketing groups naturally claim that gifts or meals have no informational content and are thus a source of non-informational persuasion. Industry argues that these meals or similar payments are just a ticket for getting a salesperson into the door, and any influence from them is a result of the information that salespeople subsequently dispense to physicians, such as new clinical trials results for new-to-the-market drugs (Carlat 2007). If the latter is true, this would imply that changes in marketed drug prescriptions in response to marketing could be welfare-increasing for patients.

We take advantage of drug-level differences to evaluate whether these marketing effects are consistent with informational or non-informational sources of influence. In particular, we assume that new-to-the-market drugs require more information dissemination from manufacturers to physicians, since physicians are less familiar with the idiosyncrasies and details of these drugs. There is likely also a higher volume of clinical trials results being released for these newer drugs, leading to more information for salespeople to disseminate. If disclosure influences new-to-the-market marketed drugs differently than older marketed drugs, this can shed insight on the degree to which this marketing influence could be informational or non-informational in nature. As a result, this helps evaluate whether disclosure is reducing influence that may benefit patients, or whether it is reducing influence that is not based on information.

Old vs. New Marketed Drugs

Our data includes 62 branded drugs that were introduced to the market in the middle of this 48-month time period. We test whether disclosure differentially affected these new drugs relative to older marketed drugs. We define a new marketed drug as one that has been on the market for one year or less (and which has no generic alternative), although alternate cutoffs (e.g., 18 months, 24 months) yield similar results.

Table 7 runs linear models that are similar to Model (1), except we include an additional indicator for whether a drug is new to the market for the month corresponding to each observation. We also include an interaction term between this new-drug indicator and the sunshine-law indicator.

Model (11) suggests that newer marketed drugs are *less* affected by the sunshine law than older marketed drugs, and Model (12) shows similar results when using marketshare as the DV. This effect is not driven by differences in baseline prescription rates between newer marketed and older marketed drugs; in this dataset, new-marketed drugs average 0.387 scripts per physician-drug-month, which is comparable to the 0.418 scripts for older-marketed drugs. Altogether, these results are consistent with gifts and consulting payments serving as *non-informational* sources of influence.

Importantly, these results do not rule out the possibility that salespeople can play an informational role for physicians. Instead, they suggest that gifts, meals, and consulting payments may not be crucial for allowing information about new drugs to be passed on to physicians via salespeople.

IX. Discussion

This paper uses a state-level policy change to evaluate whether mandated public disclosure of industry-related conflicts of interest can alter how physicians prescribe. The results suggest that public disclosure reduced the prescriptions of marketed drugs, yielding a net *decrease* in prescription volume.

These changes likely occurred because disclosure requirements invoke social image concerns in physicians. Physicians are trained in the spirit of the Hippocratic Oath (i.e. "first, do no harm"), and this norm helps enforce a desire in physicians to avoid appearing biased or unethical in the eyes of patients, colleagues, and the public. Mandated disclosure may interact with this norm and encourage physicians to abstain from meals and payments to avoid having to disclose such conflicts-of-interest, even if they do not believe they are influenced by these meals and payments. Since these free meals and payments have been shown to influence how physicians prescribe (Larkin et al 2017), abstaining from these activities likely leads to the changes in prescriptions observed in this paper. Consistent with this, we find that physicians who accepted high payments post-disclosure were the only group *not* affected by the law. In addition, physicians that did not show up in the disclosures data were strongly affected by the law, suggesting that the effects were not because patients saw their physician in the dataset and demanded changes in their treatment.

In addition, physicians' concerns over their public image are not necessarily tied to concerns over the effects of reputation on long-term income. The physicians in this sample are salaried employees whose income largely should not depend on the number of patients they see or the number of procedures they perform. Thus, their concern for their image could be tied closer to psychological incentives than to economic ones.

These social image effects are consistent with behavioral economics literature on public image and norm adherence. Lab experiments have shown that social image can cause individuals to conform to norms of fairness (Andreoni and Bernheim 2009) and trust (Tadelis 2011). It stands to reason that social image concerns may also cause physicians to publicly adhere to ethical norms that were emphasized to them during their training. It remains to be seen whether disclosure can reduce biases from conflicts-of-interest where ethical norms may not be as firmly entrenched through rigorous training (such as financial advising or political contexts). Indeed, some studies suggest that medical versus non-medical contexts can be relevant when measuring the effect of disclosure on biases (Loewenstein, Cain, and Sah 2011).

In addition, the effects were not more pronounced in new-to-the-market drugs. This is counter to arguments by industry that these meals or payments influence prescriptions only through information on new drugs that come packaged with these interactions. Instead, the results are more consistent with a non-informational mechanism, such as reciprocity to these meals or payments, which would likely impact both new and old drugs similarly. Moreover, meals doctors and consulting doctors both showed similar responses to disclosure, despite the large difference in dollar values between these types of interactions. This result is also consistent with a reciprocity mechanism; results in psychology demonstrate that feelings of appreciation for a gift (and subsequent reciprocity to that gift) are only minimally correlated with the perceived value of the gift (Flynn and Adams 2009).

These results provide policymakers with a starting point for understanding possible effects of disclosure across many contexts (including the disclosure provision in the Affordable Care Act). Although results are promising, it’s important to recall that disclosure did not appear to affect those willing to accept high payments from industry even post-disclosure. Although these physicians did not show evidence of moral licensing (i.e. they did not prescribe *more* marketed drugs because of disclosure), that they were immune to disclosure suggests more heavy-handed measures, such as outright bans, may be necessary if policymakers aim to completely eliminate these conflicts-of-interest. In addition, this study focused on physicians affiliated with academic medical centers in order to use comparable physicians across states; private practitioners may respond differently to disclosure due to either differences in physician type or differences in the incentives they face.

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Figure 1. Data Summary

Data Type	Final Data Included	Data Selection Based on:	Data Excluded
Geographic Regions	1) Chi, IL 2) Bos, MA 3) Phi, PA and Pit, PA 4) NYC, NY 5) Northern CA 6) Southern CA	1. Total Academic Medical Center (AMC) counts in each MSA in the US	1. All other U.S. regions
Academic Medical Centers	25 AMCs in these Metropolitan Regions	1. IMAP Policy Database 2. IMS AMC-Hospital Affiliations Survey	1. AMCs outside of these regions 2. AMCs that do not own/operate hospitals of their own
Drugs	254 drugs in eight drug classes	1. IMS drug classifications 2. 2006-2012 IMS prescriptions	1. Other drug classes 2. Drugs in these classes rarely prescribed in '06 – '12 3. Branded drugs with generics available pre-July 2011 4. Generic drugs introduced after June 2007
Physicians	5,312 full-time attending physicians at AMC-owned hospitals for at least one quarter in January 2006 – June 2009	1. Quarterly IMS survey data (1/06 – 6/09)	1. Physicians with multiple affiliations 2. Physicians that switched affiliations in 2006 – 2009 3. Not a full-time attending at a selected AMC 4. Physicians that don't prescribe the drugs in this dataset
Monthly Prescriptions	Monthly prescriptions from July 2007 – July 2011 for each physician-month in the dataset	1. 24 months before policy through 24 months after policy	1. Prescriptions data outside this 48-month period
Measures of Marketing	Whether a drug is branded with no generic on the market (for a given month)	1. FDA drug databases	1.Other measures of marketing expenditures
Payments to Physicians	All payments/gifts of $50+ to MA-licensed physicians from July 2009 – December 2012	1. Disclosures collected via the Massachusetts Sunshine Law	1. Payments to non-MA licensed physicians 2. Payments before July 2009 3. Payments under $50 not submitted to the state

Table 1: Payments Data Summary (Massachusetts Physicians)

	Accepted Meals		**Accepted Consulting**	
	Total Physicians* (out of 2719)	**Mean and Median Dollars per Physician (SD)**	**Total Physicians* (out of 2719)**	**Mean and Median Dollars per Physician (SD)**
2009	210	$210 Avg $129 Med (206) SD	183	$10,034 Avg $4549 Med (14,788) SD
2010	200	$249 Avg $125 Med (337) SD	226	$12,603 Avg $5,113 Med (24,110) SD
2011	155	$255 Avg $125 Med (342) SD	205	$13,073 Avg $4,256 Med (22,122) SD
2012	26	$289 Avg $140 Med (273) SD	26	$16,378 Avg $9784 Med (18,411) SD
2009-2012 (sum per physician)^	413 Physicians (591 obs)	$342 Avg $150 Med (538) SD	345 (640 obs)	$22,581 Avg $6,582 Med (45,271) SD

*****There were 4278 physicians (out of the initial 9998) in this data affiliated with a Massachusetts AMC post-disclosure. 2719 of these remain after the data exclusions specified in the Data section.
^This reports total per physician, *not* per physician-year. Hence averages are higher than the individual year averages because some physicians accepted meals or consulting for multiple years.

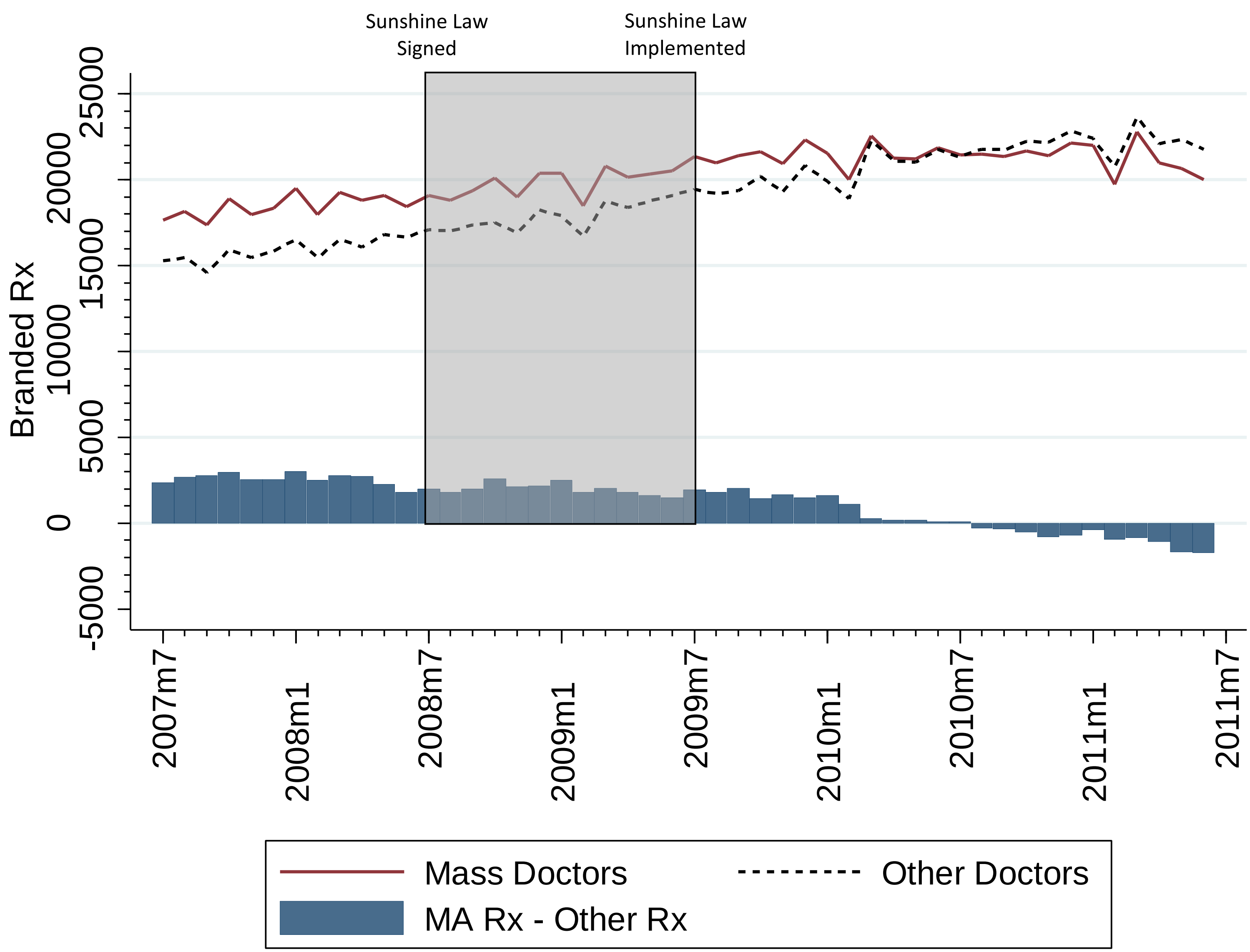
Table 2: Summary Statistics (Physician-Month-Drug)

	Total Observations (physician-drug-month)	Jul 2007 – Jun 2009 (pre-law)	Total Observations (physician-drug-month)	Jul 2009 – Jul 2011 (post-law)
Marketed Rx, Massachusetts	1,107,361 (90% zeroes)	0.41 (2.30)	1,314,969 (91% zeroes)	0.39 (2.23)
Marketed Rx, Control States	1,012,482 (91% zeroes)	0.40 (2.44)	1,177,654 (91% zeroes)	0.43 (2.64)
Generic Rx, Massachusetts	3,495,405 (88% zeroes)	0.74 (4.19)	2,631,154 (82% zeroes)	1.13 (5.16)
Generic Rx, Control States	3,090,316 (91% zeroes)	0.41 (3.04)	2,183,940 (86% zeroes)	0.72 (4.03)
Marketed Rx Marketshare, Massachusetts*	1,035,252 (82% zeroes)	0.83% (5.38)	1,257,293 (90% zeroes)	0.61% (4.10)
Marketed Rx Marketshare, Control States*	880,666 (90% zeroes)	1.21% (7.22)	1,087,995 (90% zeroes)	0.98% (6.00)
Generic Rx Marketshare, Massachusetts*	3,236,121 (87% zeroes)	1.51% (7.87)	2,507,504 (81% zeroes)	2.08% (9.18)
Generic Rx Marketshare, Control States*	2,648,893 (90% zeroes)	1.47% (8.03)	2,014,917 (85% zeroes)	2.16% (9.72)

^ Rx data were purchased in two batches, and the drug compositions in those two batches differed. The dataset includes some drugs with only 24 months of data (drugs with Rx data from Jan06 – Jun09 that were removed from the Jul09 – Jun12 batch due to low prescriptions, and drugs with Rx data from only the Jul09 – Jun12 batch because they were introduced in that time period or because they became more widely prescribed during that time period). As a result, direct comparisons of the before and after averages should be done with this caveat in mind.

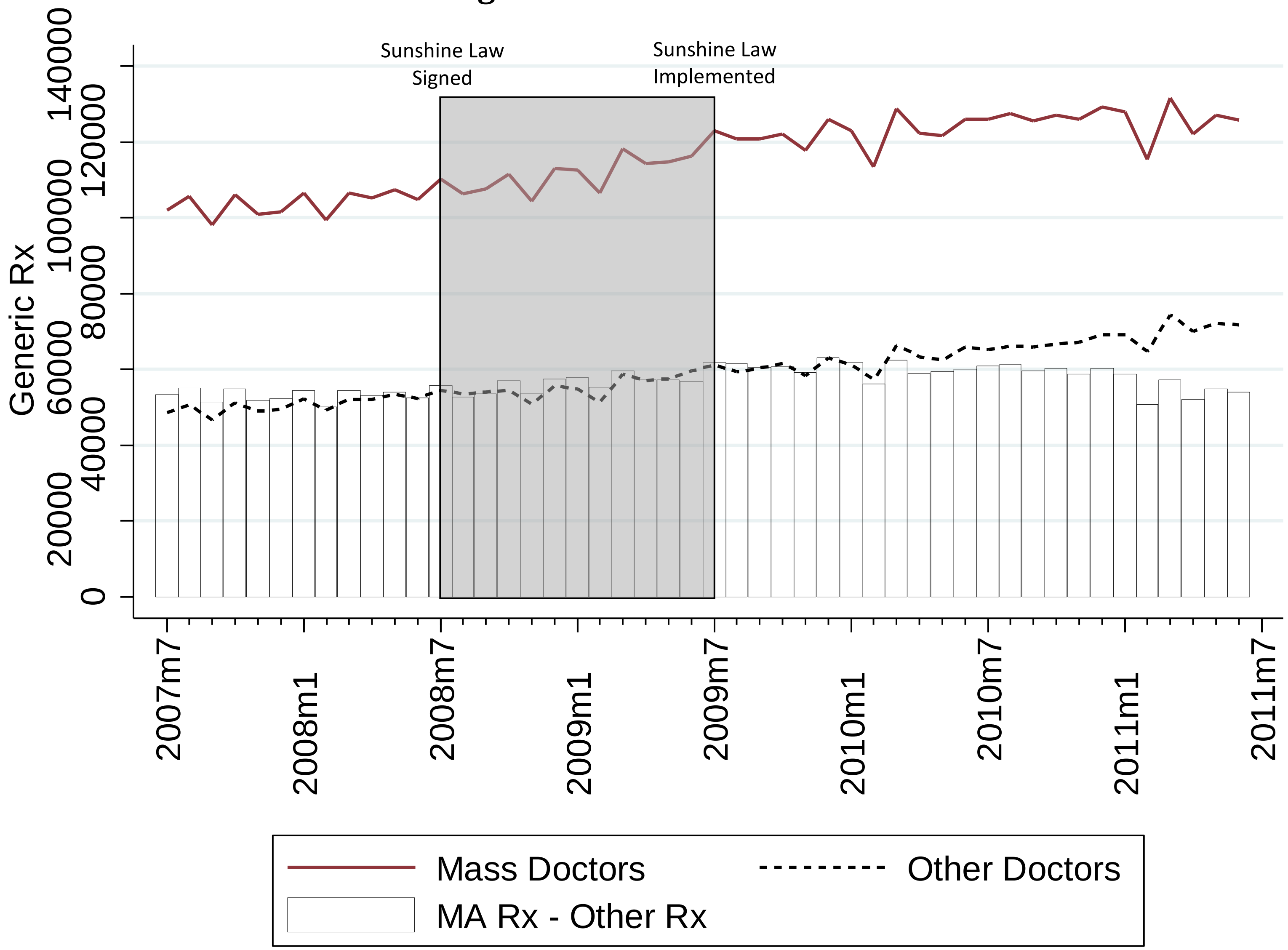
*Marketshare represents the total Rx for that physician-drug-month divided by all Rx for that physician-month. In 33,789 physician-months (2,817 physicians over 12,771,652 physician-drug-months), physicians prescribed a total of 0 scripts of the drugs in the dataset, resulting in a missing value for marketshare.

Figure 2: Total Branded Rx



The "MA" group includes all 2719 physicians that are affiliated with a MA AMC when the sunshine law was implemented.
The "Other" group includes the 2593 physicians that are not affiliated with a MA AMC when the sunshine law was implemented.

Figure 3: Total Generic Rx



The “MA” group includes all 2719 physicians that are affiliated with a MA AMC when the sunshine law was implemented.
The “Other” group includes the 2593 physicians that are not affiliated with a MA AMC when the sunshine law was implemented.

Table 3: Sunshine Effects, OLS (Physician-Drug-Month)

	(1)	(2)	(3)
DV	**Rx**	**Rx**	**Marketshare**
N (physician-drug-month)	**16,013,281**	**16,013,281**	**16,013,281**
Physicians	**5,312**	**5,312**	**5,312**
Drugs	**All**	**All**	**All**
Adj-R^2	**0.876**	**0.876**	**0.558**
Policies			
1. Sunshine (Implementation)	-0.007		-0.10***
	(0.037)		(.01)
2. Sunshine (Signed/Announced)		0.007	
		(0.020)	
Interactions			
3. Sunshine*Marketed	-0.097***	-0.064***	-0.10*
	(0.020)	(0.015)	(0.05)
Controls			
Marketshare Missing Indicator	NO	NO	YES
Month FE	YES	YES	YES
Physician*Drug FE	YES	YES	YES
Linear Combinations of Coefficients			
a.) 1 + 2	-0.103***		-0.200***
	(0.031)		(0.001)
b.) 1 + 3		-0.056***	
		(0.015)	

*p<0.10; ** p<0.05; *** p<0.01
SEs are robust and clustered by AMC.

The *Marketed* indicator is always dropped for being collinear with physician-drug FEs.
Model (3) treats marketshare as a percentage. The coefficient on Sunshine*Marketed represents a 0.1% effect on marketed drugs that is significant at the p < 0.074 level.

The marketshare-missing indicator takes on a value of 1 for all physician-months where the physician prescribed no drugs in this dataset in that month, and it takes a value of 0 otherwise. In these months, marketshare value is set to 0 to prevent these observations from being dropped.

Linear combinations (a) and (b) represent the net effect of *sunshine* on *marketed drugs* in those specifications.

Table 4: Quartile Cutoffs

	1st Quartile	**2nd Quartile**	**3rd Quartile**	**4th Quartile**
Meals (413 physicians)	($12, $86]	($86, $150.15]	($150.15, $332.38]	($332.38, $4,287.57]
Consulting (345 physicians)	($56, $1500]	($1500, $6581.72]	($6,581.72, $23,672.69]	($23,672.69, $377,159)

Figure 4: Difference-in-Differences Coefficients by Month

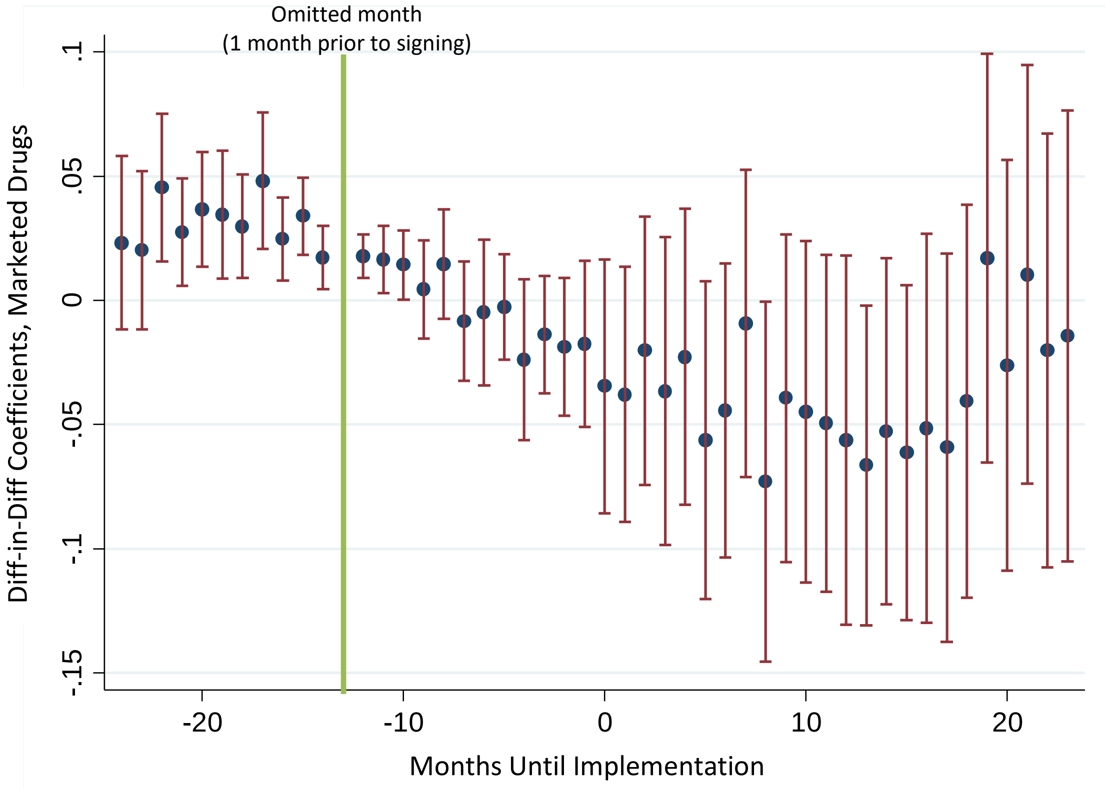


Table 5: Meals/Consulting Quartile Splits

	(8)	(9)
DV	**Rx**	**Rx**
N (physician-drug-month)	**16,013,281**	**16,013,281**
Physicians	**5,312**	**5,312**
Payment Category for Quartiles	**Meals**	**Consulting**
Adj-R^2	**0.876**	**0.876**
Main Policy Effects		
1. Sunshine	-0.006	0.013
	(0.043)	(0.037)
2. Sunshine*Marketed	-0.092***	-0.113***
	(0.022)	(0.017)
*Policy * Quartile*		
3. Sunshine * 1stQuartile (top 25)	0.182	0.023
	(0.196)	(0.040)
4. Sunshine * 2ndQuartile	0.131	-0.272***
	(0.153)	(0.088)
5. Sunshine * 3rdQuartile	-0.240	-0.185
	(0.173)	(0.154)
6. Sunshine * 4thQuartile (bottom 25)	-0.130**	-0.193**
	(0.053)	(0.077)
*Policy * Quartile * Marketed*		
7. Sunshine * 1stQuartile * Marketed	-0.070	0.057
	(0.073)	(0.056)
8. Sunshine * 2ndQuartile * Marketed	-0.115	0.153***
	(0.127)	(0.053)
9. Sunshine * 3rdQuartile * Marketed	0.127	0.109
	(0.160)	(0.110)
10.Sunshine * 4thQuartile * Marketed	-0.023	0.193***
	(0.073)	(0.055)
Controls		
Month FE	YES	YES
Physician*Drug FE	YES	YES
Linear Combinations of Coefficients		
1 + 2	-0.098***	-0.100***
	(0.035)	(0.033)
1 + 2 + 3 + 7 (Top Q)	0.015	-0.021
	(0.124)	(0.049)
1 + 2 + 4 + 8 (2^{nd} Q)	-0.082*	-0.219***
	(0.042)	(0.052)
1 + 2 + 5 + 9 (3^{rd} Q)	-0.212***	-0.175***
	(0.037)	(0.052)
1 + 2 + 6 + 10 (4^{th} Q)	-0.251***	-0.100*
	(0.039)	(0.052)

*p<0.10; ** p<0.05; *** p<0.01. SEs are robust and clustered by AMC.
Omitted for collinearity with physician*drug: main effect of quartiles, marketed, and quartile*marketed.
Linear combinations represent:
a.) net effect of *sunshine* on *marketed drugs* for *non-meals/non-consulting physicians*;
b - e.) net effect of *sunshine* on *marketed drugs* for each quartile of *meals/consulting physicians.*

Table 6: Sunshine Law Effects Excluding Doctors that Accepted Payments in 2009

	(10)
DV	**Rx**
N (physician-drug-month)	**14,906,525**
Physicians	**4,987**
Adj. R^2	**0.869**
Policies	
1. Sunshine	-0.003
	(0.042)
Interactions	
2. Sunshine*Marketed	-0.100***
	(0.021)
Controls	
Physician*Drug, Month FE	YES
Linear combinations of coeff	
a.) 1 + 2	-0.103***
	(0.034)

*p<0.10, **p<0.05, ***p<0.01
SEs are robust and clustered by AMC.
Linear combinations represent:
a.) Net effect of *sunshine* on *marketed drugs*.

Table 7: Sunshine Law Effects on New Marketed Drugs

	(11)	(12)
DV	**Rx**	**Marketshare**
N (physician-drug-month)	**16,013,281**	**16,013,281**
Physicians	**5,312**	**5,312**
Adj. R^2	**0.876**	**0.558**
Policies		
1. Sunshine	-0.015	-0.058**
	(0.039)	(0.022)
Drug Type		
2. New Marketed Drug	-0.050***	-0.067***
	(0.012)	(0.020)
Interactions		
3. Sunshine*Marketed	-0.125***	-0.126***
	(0.021)	(0.032)
4. Sunshine*New Marketed	0.096***	0.126***
	(0.014)	(0.032)
Controls		
Physician*Drug, Month FE	YES	YES
Linear combinations of coeff		
a.) 1 + 3	-0.140***	-0.195***
	(0.035)	(0.066)
b.) 1 + 3 + 4	-0.044	-0.069
	(0.026)	(0.047)

*p<0.10, **p<0.05, ***p<0.01
SEs are robust and clustered by AMC.
Linear combinations represent:
b.) Net effect of *sunshine* on *marketed drugs* that are not new-to-the-market;
c.) Net effect of *sunshine* on *marketed drugs* that are new-to-the-market.