Essays on Information Disclosure, Healthcare Marketing & Consumption

by

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Dedication

To my mother, Dongmei Li, and my father, Wei Guo.

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This thesis is dedicated to all of those with whom I shared my sweat, tears and joys during the PhD grind.

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ABSTRACT

Recent regulatory changes have introduced more transparency to healthcare practice and marketing. The intention of these regulatory changes is to help consumers make more informed decisions, to reduce healthcare costs, and to resolve conflict-of-interest issues. My work in this area aims to understand if and to what extent such regulations achieve the desired goals, and in what ways firms and physicians are impacted. In addition, my work also investigates whether there are unintended consequences of such regulation.

My dissertation studies the disclosure of a specific form of information: marketing payments to physicians from pharmaceutical firms and their rivals. In two essays, I investigate how making this information public changes physician prescriptions and firm payments, as well as whether there are unintended consequences of such regulation.

In the first essay of my dissertation, "Let the Sun Shine In: The Impact of Industry Payment Disclosure on Physician Prescription Behavior", I provide evidence on the effectiveness of increased transparency of physicians' industry financial ties in reducing physician prescriptions. Specifically, I use individual-level claims data from a major provider of health insurance in the U.S. and employ a difference-in-difference research design to study the effect of the payment disclosure law introduced in Massachusetts in June 2009. The research design exploits the fact that while physicians operating in Massachusetts were impacted by the legislation, their counterparts in the neighboring states of Connecticut and New York were not. In order to keep the groups of physicians comparable, I restrict my analysis to the physicians in the counties that are on the border of these states. I find that the Massachusetts disclosure law resulted in a

decline in prescriptions in all three drug classes studied: statins, antidepressants, and antipsychotics. My findings are robust under alternative controls, time periods, and variable transformations. I show that the effect is highly heterogeneous across brands and physician groups, and that the decrease in prescription is unlikely due to changes in financial incentives.

In the second essay, "The Effect of Information Disclosure on Industry Payments to Physicians", I seek answer to the following question: does disclosing industry payment information influence subsequent payments to physicians? I quantify the impact of information disclosure during 2014-2015 (after ACA Physician Open Payment Act) on direct-to-physician payments. In essence, I use a quasi-experimental difference-in-difference research design to find control "clones" for every physician-product pair in the states with and without prior disclosure laws, facilitated by recent advances in machine learning methods. The novel algorithm (Wager and Athey, 2017) is computationally efficient and robust to model mis-specifications, while preserving consistency and asymptotic normality. Using a 29-month national panel covering $100 million in payments between 16 anti-diabetics brands and 50,000 physicians, I find that the monthly payments declined by 2% on average due to disclosure. However, there is considerable heterogeneity in the treatment effects with 14% of the drug-physician pairs showing a significant increase in their monthly payment. Moreover, the decline in payment is smaller among drugs with larger marketing expenditure, and among physicians who were paid more heavily pre-disclosure and prescribed more heavily. Thus, while information disclosure did lead to reduction in payments on average (as intended by policy makers), the effect is limited on big drugs and popular physicians. I further explore potential mechanisms that are consistent with the data pattern.

CHAPTER 1

Introduction

Recent regulatory changes have introduced more transparency to healthcare practice and marketing. The intention of these regulatory changes is to help consumers make more informed decisions, to reduce healthcare costs, and to resolve conflict-of-interest issues. My work in this area aims to understand if and to what extent such regulations achieve the desired goals, and in what ways firms and physicians are impacted. In addition, my work also investigates whether there are unintended consequences of such regulation.

My dissertation studies the disclosure of a specific form of information: marketing payments to physicians from pharmaceutical firms and their rivals. In two essays, I investigate how making this information public changes physician prescriptions and firm payments, as well as whether there are unintended consequences of such regulation.

In the first essay of my dissertation, "Let the Sun Shine In: The Impact of Industry Payment Disclosure on Physician Prescription Behavior", I provide evidence on the effectiveness of increased transparency of physicians industry financial ties in reducing physician prescriptions. Using data from a large, national US health insurer (OptumInsight De-identified Clinformatics^{TM} Data Mart), I exploit the natural experiment occurred in Massachusetts in 2009, where physicians operating in Massachusetts were required to disclose marketing payments from firms, but their counterparts in the neighboring states of Connecticut and New York were not. To address concerns about the differential trend between MA and the control states, I use the generalized synthetic control method to match physicians in MA with a convex combination of physicians in CT and NY based on their pre-period prescription behavior. I find that information disclosure has led to a decline in branded prescriptions by 40%-59% for statins, antidepressants, and antipsychotics, with heavier prescribers and more popular brands being influenced the most. Interestingly, the prescriptions of generic drugs in the three classes declined as well, although the magnitude of decline is smaller than that of branded prescriptions. Since manufacturers of generic drugs do not typically make

payments to physicians, this result suggests that the change in prescription behavior is unlikely to be related to changes in payment structure as a result of the disclosure of these payments. Rather, the decrease in prescription is possibly a consequence of increased self-monitoring among physicians to curb over-diagnosis.

In the second essay, "The Effect of Information Disclosure on Industry Payments to Physicians", I seek answer to the following question: does disclosing industry payment information influence subsequent payments to physicians? I quantify the impact of information disclosure during 2014-2015 (after ACA Physician Open Payment Act) on direct-to-physician payments, using machine learning technique with quasi-experimental research design. The technique effectively matches the treated and the control units while circumventing the curse of dimensionality in traditional parametric matching methods. This allows me to obtain individual-level estimates with good asymptotic properties that are robust to irrelevant features. Using a 29-month national panel covering $100 million in payments between 16 anti-diabetics brands and 50,000 physicians, I find that the monthly payments declined by 2% on average due to disclosure. However, there is considerable heterogeneity in the treatment effects with 14% of the drug-physician pairs showing a significant increase in their monthly payment. Moreover, the decline in payment is smaller among drugs with larger marketing expenditure, and among physicians who were paid more heavily pre-disclosure and prescribed more heavily. Thus, while information disclosure did lead to reduction in payments on average (as intended by policy makers), the effect is limited on big drugs and popular physicians. I present a data pattern consistent with the idea that firms respond to information about competitive payments by trying to differentiate themselves.

My dissertation contributes to our knowledge of the consequences from the well-publicized disclosure laws. In my first essay, I provide evidence that disclosure laws decrease physician prescriptions in both a statistically and economically significant manner. In my second essay, I provide evidence that while public disclosure of payment information led to a reduction in overall payments, the effect is muted for heavily prescribing physicians and heavily marketed drugs. My dissertation offers two take-aways for regulators. First, regulators may want to re-evaluate whether the benefit from the intervention justifies its costs. Second, firm differentiation possibly plays a big role in explaining the impact of disclosing marketing expenditure in other industries, too. As FTC is pushing for sponsorship disclosure in social influencer marketing, findings in my dissertation warn regulators about the possible increased dominance of high-paying brands among Opinion Leader voices due to differentiation.

CHAPTER 2

"Let the Sun Shine In": The Impact of Industry Payment Disclosure on Physician Prescription Behavior

2.1 Introduction

The U.S. pharmaceutical industry spent more than $6 billion as marketing payments to physicians during 2013 - 2015.[1] These direct payments (consulting/speaking fees, conference/meal reimbursements) are pervasive with 75% of U.S. physicians receiving at least one payment from a company in a year.[2] Extant academic research has documented a relationship between prescribed drugs and payments (Yeh et al., 2016; DeJong et al., 2016), calling into question the unbiasedness of physician decision-making (Campbell et al., 2007; Agrawal et al., 2013; Kesselheim et al., 2013; Perry et al., 2014). Concerned about higher healthcare costs and lower patient welfare due to conflict of interest (Manchanda and Honka, 2013; Carey et al., 2015; Engelberg et al., 2014; Grochowski Jones and Ornstein, 2016), policy-makers have been pushing for full disclosure of these payments. Several states introduced disclosure laws ("the Sunshine law") that require companies to report physician payments to the state government (Chimonas et al., 2010; Pham-Kanter et al., 2012)[3] followed by the federal government in 2013 (as part of the Affordable Care Act). The idea behind these laws is that increased public scrutiny as a result of the disclosure might persuade firms to decrease these payments or render physicians less willing to accept them (Chen et al., 2016). To the extent that payments are related to a greater propensity to prescribe branded drugs, the potential

[1] From OpenPayment data from CMS. `https://openpaymentsdata.cms.gov/summary`

[2] See https://www.propublica.org/article/doctors-who-take-company-cash-tend-to-prescribe-more-brand-name-drugs

[3] These states include: Maine (2004), West Virginia (2004), Minnesota (1993), Massachusetts (2008), Vermont (2001), and District of Columbia (2003).

reduction in payments as a result of disclosure could lead to lower healthcare costs (e.g., by motivating physicians to switch from prescribing branded to generic drugs). On the other hand, critics of disclosure argue that it will relieve physicians of the guilt around biased prescriptions by providing a "moral license" (assuming a bias exists) (Cain et al., 2005, 2011). At the same time, disclosure can lead to unintended negative consequences for patient welfare due to the overall decrease in physician willingness to prescribe any drug (Sade, 2011; Santhakumar and Adashi, 2015). This is because physicians may prescribe more conservatively to avoid the inference the public may draw from the disclosed financial relationship with firms, even when there is no change in how much they are paid. Taken together, these diverse viewpoints make it hard to predict the impact of disclosure.

In this paper, I evaluate the effect of *enforced payment disclosure* on physician prescription behavior. To this end, I exploit the introduction of the Massachusetts Open Payment law that went into effect in July 2009 to study how physician prescription behavior changed as a result of enforced payment disclosure. The data used in my analysis come from one of the largest health insurance companies in the U.S. I use outpatient prescription information at the claim level during a four-year period between January 2008 and December 2011. This allows me to track the number of new prescriptions and refills written by each physician for various drugs over time for all the patients affiliated with the insurance provider. I study prescription behavior in three therapeutic classes that receive the highest levels of marketing spending - statins, antidepressants and antipsychotics (Campbell, 2009). Note that I do not study the effect of the change in payments on physician prescription behavior as I do not have access to payment data before the disclosure took effect.

My identification of the effect of disclosure legislation relies on the change in new prescriptions by physicians located in Massachusetts (MA) after the policy intervention, relative to their counterparts from "control" states where no such law existed at the same time period. To ensure that the physicians in these "control" states are comparable, I focus on physicians located in the border counties of Massachusetts and Connecticut (CT). The idea is that the physicians in these border counties should have patient pools with similar need for treatment, but show differential impact in response to the legislation depending on the side of the border they are located. My empirical strategy is to examine the change in behavior using pre- and post-comparisons via a panel data specification that includes physician and time fixed effects. I use three different temporal points to characterize the change from pre to post as compliance with the disclosure law occurred in a phased manner. These three points are: (1) July

1, 2009 - when firms began to prepare their internal data for submission under the law, (2) July 1, 2010 - when the firms first reported their payment information to the government, and (3) November 22, 2010 - when the data were made available to the public. The use of these three time points also acts as a "temporal robustness check." Finally, in order to ensure that my findings are robust to my definition of the control group, I carry out two additional checks. In the first robustness check, I use the border counties of another neighboring state, New York (NY), as a control. In the second, I use the generalized synthetic control method to create a control group that is as close pre-treatment to the treatment group as possible.

My results reveal that, on average, the disclosure law resulted in a decline in the prescription of branded drugs in Massachusetts. Specifically, the intervention led to a 48%-59% decrease for branded statins, a 46%-54% decrease for branded antidepressants, and a 40%-45% decrease for branded antipsychotics when I consider physicians in the MA-CT border counties.[4] The result is robust to my definition of the control group: I replicate virtually all my findings when I use physicians in the NY border counties or against the constructed synthetic control group. The results are also robust to alternative definitions of the policy change (the three temporal breaks) and choice of model specification. Interestingly, we find that the prescriptions of generic drugs in all three drug classes also declined as a result of the disclosure - statins by 38%-46%, antidepressants by 32%-41% and generic antipsychotics by 38%-40%. In addition, my results suggest that heavy prescribers in each drug class exhibited a greater tendency to shift their prescriptions away from branded drugs (relative to generics) as a result of the disclosure. When I consider differences across brands within each therapeutic class, I find that the relative magnitude of the drop in prescriptions was larger among higher market share brands. Overall, these results suggest that the disclosure law was effective in reducing the total number of prescriptions and possibly in driving physicians to substitute away from branded drugs to generics. These results are among the first to provide empirical evidence that disclosure laws had an impact on physician prescription behavior, both in a statistical and economic sense.

As noted earlier, given that I do not observe direct payments before disclosure went into effect, I cannot draw any definitive conclusions on whether the decrease in prescriptions is related to *changes in payments* made by pharmaceutical companies. However, as noted above, a surprising finding is that, in addition to the branded drugs,

[4]While the extent of the drop seems large, it is consistent with previous research on changes in physician prescription behavior as a function of other environmental changes e.g., King and Bearman, 2013. I discuss this in detail in 2.4.5.

prescriptions of generic drugs in the three therapeutic classes also declined as a result of disclosure. Since manufacturers of generic drugs do not usually make payments to physicians, this result suggests that the change in prescription behavior is unlikely to be related to changes in payment structure arising from disclosure. I further test whether the decline in prescriptions is associated with payments by exploring whether physicians in locations (ZIP codes) that receive more payments from pharmaceutical companies are also more prone to decreasing their prescriptions of branded drugs. The premise is that if (a) prescriptions are tied to payments and (b) payments change as a result of the legislation, the effect of the legislation on prescriptions is likely to be larger among physicians that receive higher payments. I find that this relationship is statistically insignificant, suggesting that the change in prescription behavior might not be related to adjustments in payment structure as a result of the legislation. Rather, the results support the notion that the change in prescription behavior was driven by self-monitoring among physicians to curb "over-diagnosis," rather than a change in how firms deliver payments. While on the one hand, this may be seen as a "good" outcome i.e., lower prescriptions especially of branded drugs are likely to reduce health care costs, there could be "bad" aspects in that self-monitoring may shift physicians from "over-diagnosis" to "under prescribing," leading perhaps to worse health outcomes. Thus, the contribution of this paper is in establishing *what* happened and proposing some explanations for *why* it happened, setting the stage for further investigation by researchers and policy makers into the benefits and costs of the legislation.

The rest of the essay proceeds as follows. Section 2.2 introduces the institutional background of the policy intervention in Massachusetts and describes the data. Section 2.3 explains the identification strategy and empirical specification. Section 2.4 reports and discusses the findings. Section 2.5 concludes and suggests directions for future research.

2.2 Institutional Setting and Data

2.2.1 Background

The Pharmaceutical and Medical Device Manufacturer Code of Conduct, or Massachusetts Marketing Code of Conduct, was created in 2008 to promote "cost containment, transparency and efficiency in the delivery of quality health care."[5] It incorporated requirements from the voluntary code of conduct of the Pharmaceutical Research

[5] http://www.ncsl.org/Portals/1/documents/magazine/ma_s2863.pdf

and Manufacturers of America (PhRMA) and the Advanced Medical Technology Association (AdvaMed). Effective from July 1, 2009, it required "all pharmaceutical and medical device manufacturers that employ or contract with any person to sell or market prescription drugs or medical devices in Massachusetts" to collect and report certain financial transactions related to marketing activities with Massachusetts health care providers. The policy came into effect over a series of steps between July 2009 and November 2010:

1. Starting on July 1st, 2009, the companies were required to establish compliance and training programs for their sales and marketing agents regarding the Massachusetts Marketing Code of Conduct.

2. On July 1st, 2010, the companies reported the first wave of "the value, nature, purpose and particular recipient of any fee, payment, subsidy or other economic benefit with a value of at least $50" with Massachusetts-licensed health care providers.[6] Payments in conjunction with genuine research and clinical trials, prescription drugs for use by patients exclusively, demonstration units, items for charity care, royalties and licensing fees based on intellectual property agreements, and price concessions such as discounts and rebates, are exempt from disclosure.[7] For July 1, 2010, transactions for the period July 1, 2009 through December 31, 2009 were reported. In each year thereafter, the disclosures will cover a full calendar year of transactions.

3. On Nov 22, 2010, Massachusetts Office of Health and Human Services set up the online query website to public that allows consumers to look at prepared reports, carry out customized searches by company, physician, year, payment category and amount, or keywords, and/or download the whole dataset. At that point in time, Massachusetts was the first state to open up an online database of firm payments to physicians publicly. Figure A.2 shows a snapshot of the customizable search engine.

[6] http://www.mass.gov/eohhs/gov/departments/dph/programs/hcq/healthcare-quality/pharm-code-of-conduct/information-for-consumers.html

[7] http://www.mass.gov/eohhs/docs/dph/quality/healthcare/pharm-medical-device-conduct-faq.pdf

2.2.2 Data

2.2.2.1 Prescription Data

My prescription data come from De-identified Clinformatics™ Data Mart Database (provided by OptumInsight Inc.) from a large, national US health insurer.[8] The data contain all outpatient prescription claims made on behalf of the beneficiaries enrolled with the insurer in the United States during 2006-2011. For each claim, I observe four sets of data: a) system-encrypted physician unique ID ; b) drug information, including names, therapeutic class, National Drug Code, indicator for whether the drug is branded or a generic; c) prescription information, including fill date, indicator for whether this is a new prescription, quantity dispensed, days of supply, and maximum number of refills; d) standardized cost information. In addition, I observe some patient characteristics such as their age (capped at 90), gender, zipcode of residence, starting date of the membership, the insurance coverage type (e.g. HMO, PPO, etc.) they are enrolled in and all the prescription claims filed on their behalf. I further pair my data with FDA National Drug Code database to obtain manufacturer information (`http://www.fda.gov/Drugs/InformationOnDrugs/ucm142438.htm`). This information, along with data from FDA Orange Book, allow me to identify whether a drug is branded or a generic equivalent.

2.2.2.2 Sample Preparation and Summary Statistics

As discussed in the Introduction, I consider physicians located at the border of Massachusetts and another New England state, Connecticut. Specifically, I choose four counties from Connecticut (Litchfield, Hartford, Tolland, Windham) and three counties from Massachusetts (Hampden, Berkshire and Worcester). Two of the counties in MA, Berkshire and Worcester extend all the way up to the state's northern border with Vermont and New Hampshire. For these two counties, I only include physicians operating in ZIP codes within 30 miles of the MA-CT border (see Figure 2.1 for a map).

The idea behind restricting my analysis to physicians in the border counties is that they will have similar characteristics and face patient pools with comparable needs for prescription drugs in different classes. I check the population in these border counties in terms of their demographic and socio-economic characteristics, health insurance coverage, and educational attainment in Table 2.1. These data suggest that the popu-

[8]Due to the presence of a non-disclosure agreement, I am unable to reveal the name or the exact market share of the insurer.

lation on either side of the MA-CT border are comparable in terms of demographic and socio-economic characteristics, health insurance coverage, and educational attainment. Therefore, the premise that the physicians operating in these areas should face patient pools with similar need for prescription medication appears to have face validity. In my empirical analysis, I also include the number of patients that each physician receives over time as a covariate in order to control for changes in their need to prescribe drugs.

As noted earlier, I do not observe the Zip codes where each physician operates in my data. Therefore, I infer this from the location information of each physician's patient pool from Massachusetts and Connecticut, before the policy intervention comes into effect. I first identified all physicians who have prescribed for Massachusetts and Connecticut patients during 2006-2009. I then assigned each physician to the modal zipcode where most of her patients resided. I exclude 12% of physicians that draw a significant number of patients from both states. This leaves me with 7,504 physicians in Connecticut and 5,918 in Massachusetts.

I investigate the impact of disclosure on new prescriptions for three chronic drug classes: statins, antidepressants, and antipsychotics. I choose these drug classes for the following reasons. First, antihyperlipidemic agents (to which statins belong) and antidepressants are among the top three most-prescribed categories of drugs in the U.S. during the 2008-2011 time period. These categories account for 5.3% and 4.1% of the total prescriptions in my data, respectively. These numbers are substantial given that the average market share of a drug category in my data is about 0.3%. I include antipsychotics as the third category because it saw a significant increase in prescriptions between 2008 and 2011. Two of the popular drugs in this class, Abilify and Seroquel, ranked among the top five best selling drugs during the 2008-2011 period (`https://www.drugs.com/top200_2009.html`). Second, there are no OTC drugs in these three drug classes, which implies that they can only be obtained against a physician prescription. Thus, I can characterize drug usage in these classes based on the insurance claims data alone. Moreover, the impact of marketing in these three drug classes has received some attention in the literature (e.g. Shapiro, 2016).

Within each drug class, I focus on new prescriptions and renewals written by the physicians in my sample. Unlike refills within existing prescriptions, new prescriptions and renewals represent active decisions by physicians because they usually involve evaluation of a patient's condition and their responsiveness to the treatment. Admittedly, there might be greater inertia with renewals compared to new prescriptions, suggesting that the impact of the legislation is likely to be muted for renewals compared to new prescriptions. To rule out the effect of inertia, one can restrict the analysis to the first

prescription that a patient receives in that particular drug class. However, my data do not allow me to infer the date when a patient starts taking prescription drugs in a given therapeutic class. Nevertheless, if I do find that physicians changed their prescription behavior subsequent to the legislation despite combining new prescriptions and renewals, the effect would have been stronger if I had only considered the former.

In my empirical analysis, I keep the border physicians who have prescribed any of these three drug classes during January 2008 - December 2011, and aggregate new prescriptions and renewals by brand to the monthly level for each physician. I also record the monthly total patients receiving a new prescription (from all locations and all drug classes) for each physician in order to infer the size of the physician's monthly patient pool. Table 2.2 summarizes the number of physicians, drugs, branded drugs, and zipcodes in my sample, while Table 2.3 reports summary statistics on monthly total new prescriptions per class and monthly total patients per physician. Between January 2008 and December 2011, an average physician from MA border prescribes 0.09 branded statins (0.19 for CT physicians), 0.03 branded antidepressants (0.11 for CT physicians), and 0.18 branded antipsychotics (0.22 for CT physicians) per month. The data in Table 2.3 also reveal that the physicians vary significantly in terms of the size of their patient pool.

I treat January 2008 -June 2009, i.e., before the pharmaceutical started collecting payment data for disclosure as the pre-treatment period. Table 2.4 shows that during the pre-period, MA and the control states follow similar time trends in average monthly prescriptions across statins and antidepressants. The only exception is antipsychotics. I discuss further robustness checks for this concern in section 2.3.2.1.[9] The raw data patterns of monthly prescriptions per physician in Figure 2.2 suggest that the level difference between Massachusetts and Connecticut prescriptions enlarges from pre-period to post-period, in all three drug classes. Specifically, in the post period, the number of generic and branded prescriptions per physician increases considerably in Connecticut. On the other hand, when I consider Massachusetts, the number of branded prescriptions per physician either remained constant or decreased post legislation. Although generic prescriptions increased marginally in MA, the magnitude was much smaller compared to CT. Together, these data suggest that the disclosure law might have had a negative impact on prescriptions.

There are two possible alternative explanations for this pattern: (a) there was an

[9] As a robustness check, I perform additional analyses based on the synthetic control method that matches physicians in MA with a convex combination of physicians in CT and NY based on their pre-period prescription behavior. This analysis should address concerns about this differential trend between MA and the control states for antipsychotics.

exodus of patients from the insurance company that provided me the data in MA and/or (b) growth in the number of patients enrolled with the insurance provider in CT. In my empirical analysis, I control for the size of the patient pool for each physician in order to address these concerns.

2.3 Research Design

In order to understand the effect of their introduction on prescriptions written by physicians, I exploit the idea that while some states like MA instituted payment disclosure laws, many of other states did not. Specifically, I consider how physicians in MA changed the number and composition of prescriptions they wrote in three different drug classes - statins, antidepressants, and antipsychotics - subsequent to the legislation. However, this difference would also include any change in prescriptions that might have occurred even in the absence of the policy intervention. Therefore, I use a difference-in-difference (DID) specification by comparing the changes among physicians in MA (i.e., the treated state) vs. those in a neighboring state, i.e., CT (the control state). As noted earlier, I focus on physicians in counties on the border of MA and CT in order to ensure that the two physician groups are comparable. This approach of using contiguous border areas, has been used to investigate the impact of interventions in multiple domains (Holmes, 1998; Tucker et al., 2013; Card and Krueger, 1994, 2000; Dube et al., 2010; Shapiro, 2015). The identifying assumption is that the physicians located along the border have similar characteristics and face patient populations with similar need for prescriptions in these three drug classes. Recall that the data in Table 2.1 support the notion that the border counties are comparable in terms of demographic and socio-economic characteristics as well as private health insurance enrollment. Therefore, any differential trends in physician prescriptions for the three drug classes between the two sides of the state border result only from differences in the policy change. In addition, I also verified that during the time period under study, there was no reported drug supply chain disruptions (e.g., shortages), public health condition shocks or entry of new brands that vary across counties in Massachusetts and Connecticut.

I consider the number of prescriptions written by each physician during a 48-month period between January 2008 and December 2011. Recall that the MA disclosure law came into effect in discrete steps over time. This creates some ambiguity in terms of how I should define treatment, i.e., the introduction of the disclosure law in MA. As is suggested in Goldfarb and Tucker, 2014, I check sensitivity of the results to three

different definitions of post treatment periods based on the various temporal cutoffs related to the introduction of disclosure. Thus, the pre-period and three sets of post-periods are as follows: *pre-treatment period* ($T_0 \leq 18$): January 1, 2008 - June 30, 2009; *Post-treatment period* 1 ($19 \leq T_1 \leq 48$): July 1, 2009 - Dec 31, 2011, i.e., after the law came into effect; *Post-treatment period* 2 ($31 \leq T_2 \leq 48$), i.e., after the pharmaceutical companies reported the first set of payment data: July 1, 2010 - Dec 31, 2011; *Post-treatment period* 3 ($36 \leq T_3 \leq 48$): Nov 22, 2010 - Dec 31, 2011, i.e., after the payment information was made available to the public. This enables me to assess the robustness of my results to multiple measures of policy intervention. In similar vein, studying the effect of disclosure laws on prescriptions in three different drug classes helps me assess whether the key results exhibit a generalizable pattern. In addition, I verify robustness in two different ways: (a) using NY as an alternative control state and (b) using synthetic controls to identify a convex combination of units within the control state (i.e., CT) that are similar in terms of pre-intervention prescription trends to the corresponding units in the control state (i.e., MA).

2.3.1 Physician Panel Regression

As mentioned above, I consider prescriptions written by each physician over a 48-month period. In the first set of analyses, I aggregate prescriptions into two broad groups: branded and generic. Let i index physicians, s index state, b index the type of drug (branded or generic), and t index month 1 to 48. Let Rx_{ibt} indicate the average monthly new prescriptions written by physician i from state s for branded/generic drugs in a given class in month t. I first estimate the following specification:

$$Rx_{ibt} = \alpha_b I_s I_t + \lambda_{bt} + I_{ib} + X_{it}\beta + \epsilon_{ibt}, \tag{2.1}$$

where I_s are state-specific fixed effects and I_t is a post-treatment indicator variable that equals 1 if $t \geq 18$ and 0 otherwise. The coefficients $\alpha_{branded}$ and $\alpha_{generic}$ capture the causal impact of policy intervention on the prescriptions of branded and generic drugs by Massachusetts physicians. A negative $\alpha_{branded}$ indicates that the policy intervention discourages physicians from prescribing more (expensive) branded drugs. I include physician-brand fixed effects, I_{ib} to control for systematically different demand levels across physicians for branded versus generic drugs. Similarly, I control for temporal trends in prescriptions that are common across the treated and control states but different across brands, by including λ_{bt}, a series of brand-specific month dummies. Moreover, recall that my data pertain to prescription claims made on behalf of patients

that are enrolled in insurance plans offered by the focal firm. Therefore, I may observe changes in prescription claims as the number of enrollees changes over time. I control for changes in the number of enrollees over time by including the total number of patients receiving any new prescriptions (beyond the three therapeutic classes being studied) from each physician in month t as a covariate, X_{it}. I cluster errors by physicians.

I expand the above analysis in two ways. First, I consider whether the effect of the disclosure law varied across brands within each drug class. To this end, I consider prescriptions at the individual brand level rather than aggregating all the branded drugs into one group as in my earlier analysis. With each therapeutic class, I consider the top 2 or 3 brands and lump the remaining brands into an omnibus brand of "other." Specifically, in antidepressants and antipsychotics, the top three brands contribute to 90% of total branded prescriptions. Therefore, I have four brands after I consider these three brands and the composite "other." On the other hand, in statins, I only consider top 2 brands (Lipitor and Crestor) as they contribute to more than 96% of all branded statin prescriptions. All other brands of statins are combined into the composite brand category. As in the first analysis, I treat all generics as the final "brand".

My conjecture is that larger brands are likely to have higher marketing budgets, including resources allocated towards physician payments. If disclosure laws render firms reluctant to make payments or curtail physicians' willingness to accept payments, I would expect larger firms to experience a greater adverse effect. Especially, this is likely to be the case when payments are related to prescriptions by physicians. Even if payments are not directly related to prescriptions, if prescriptions decline as a result of the disclosure law, larger brands, by virtue of their relative size, are likely to contribute more to the decline.

Second, I explore the heterogeneous effects of the policy intervention on light, medium and heavy prescribers. As noted earlier, I divide physicians in each state into three equal sized groups on each side of the state border based on their total prescriptions within the corresponding class during January 2006 - Jun 2009 (i.e., before any treatment occurs).[10] The premise behind this analysis is that heavier prescribers are more likely to be the target of payments by firms. Therefore, if the disclosure law resulted in lower payments, these physicians are likely to change their prescription behavior. Moreover, heavier prescribers are likely to be more risk-averse and tend to overestimate the need for medication. Therefore, I expect them to be the most responsive to the policy intervention by cutting down the excessive prescriptions. In order to

[10]I also check robustness by using an alternative definition of groups based on fixed number of prescriptions on both sides of the state border.

capture this difference in responsiveness to the policy intervention, I estimate different treatment effects (one for branded and one for generic) for each physician group g separately:

$$Rx_{ict} = \alpha_g^b I_s I_t + \lambda_{bt} + I_{ib} + X_{it}\beta + \epsilon_{ibt}. \tag{2.2}$$

Thus, I investigate the relative magnitude of the policy influence on the tendency to prescribe any branded or generic drugs across the three physician groups ($\alpha_g^{branded}$, $\alpha_g^{generic}$), for $g \in \{light, medium, high\}$. Note that in this specification, the state main effects have been absorbed by physician-drug dummies (I_{ib}), and the post-treatment main effect has been absorbed by drug-month dummies(λ_{bt}).

2.3.2 Robustness Checks

I expand the basic research design outlined above by performing a series of robustness checks following Goldfarb and Tucker, 2014. First, in order to rule out the possibility that any treatment effect recovered in the above analysis is idiosyncratic to the choice of CT as the control state, I replicate the analyses with NY as an alternative control state. As in the case of MA-CT, I consider physicians that are located in the counties along the MA-NY border. The second robustness check was motivated by changes introduced by some insurers in MA. Specifically, starting in January 2009, Blue Cross Blue Shield of MA introduced Alternative Quality Contract in Massachusetts, which sought to compensate physicians based on health outcomes rather than treatments. Since my data are from a different insurance company that did not institute a similar change, my analysis should not be influenced by changes made by Blue Cross Blue Shield. Nevertheless, I check robustness of my findings by using a pre-period that excludes data prior to the introduction of the alternative quality contract, i.e., I use Jan-Jun 2009 as an alternative pre-period. By considering data only after the introduction of the alternative quality contract, I rule out its influence in driving any changes in prescriptions in MA.

Third, since I am considering prescriptions at the individual physician level, there are many months with zero prescriptions. The problem is more acute when I consider prescriptions at the individual brand level. The large number of zero entries and a long tail of large positive prescriptions for some physicians raise the concern that the distribution of the monthly prescriptions in my data is heavily right skewed, and the estimation results could be sensitive to the long tail. To address this issue, I perform two forms of log transformation of the dependent variable: 1) $Y = \log(Rx_{ibt} + 1)$, and 2) $Y = \log(Rx_{ibt} + \sqrt{Rx_{ibt}^2 + 1})$, keeping the zero observations. Form one is a Box-

Cox transformation frequently used in marketing and biomedical studies. Form two is a scale invariant Inverse-Hyperbolic-Sinh transformation frequently used in labor and wealth studies (see MacKinnon and Magee, 1990). Both forms of transformation work to approximate normality of the residuals with a large number of zero outcomes (e.g. MacKinnon and Magee, 1990).

2.3.2.1 Robustness Check Using Generalized Synthetic Control Method

In the panel regression, I attempt to control for any differences across the treatment and control states by (a) focusing on the contiguous border areas and (b) absorbing the brand-physician differences. Moreover, I control for common time-varying unobservables with fixed effects. I further check the sensitivity of the conclusion using three different drug classes, different pre- and post-periods, and an alternative control state. However, there could still be concern that the physicians in the treated and control units may be different in important ways. For example, prior to the policy intervention, physicians in MA, CT, and NY might have followed different temporal trends in drug prescriptions. Although I find that the pre-treatment trends are comparable in MA and the control states of CT and NY (please refer to Table 2.4), there were some discrepancies in the antipsychotics category, especially in the MA-NY border. While it is hard to find a "perfect clone" from existing states, researchers have proposed constructing a "clone" for each unit in the treated group by using a convex combination of units in the control group. This synthetic control method (Abadie et al., 2010, 2015) is gaining popularity in marketing studies with quasi-experimental designs (e.g. Tirunillai and Tellis, 2016). The idea behind the method is that the synthetic control unit will closely represent the unit in the treated state along dimensions that the researcher deems important. In my case, I construct a "synthetic clone" unit in the control state that mimics the pre-period prescription trend for each unit in MA. Since my analysis involves applying the idea of synthetic controls to a large number of treated units (i.e., physicians), I use the generalized synthetic control (GSC) method (Xu, 2017).

Intuitively, the generalized synthetic control (GSC) method first projects the treated units onto the multi-dimensional latent space spanned by the control units such that they are matched on key characteristics, which, in my case, are their pre-period outcomes. In this way, the treated units and the control units are made "comparable" by adjusting the loadings they have on each of the dimensions (factors). Using the loadings and the latent factors, GSC method obtains the projected post-period outcomes for treated units if they had not been treated. By comparing the projected outcomes to the actual values, I can obtain an estimate of the average treatment ef-

fect. To evaluate the statistical significance of the estimate, GSC method constructs bootstrapped distribution of the estimate in a way similar to the placebo test in the traditional synthetic control method. When there is sufficiently long pre-period data (i.e., $T_0 > 10$), GSC is computationally faster, less sensitive to the idiosyncrasies of a small number of observations, and produces more interpretable uncertainty estimates such as the standard errors and confidence intervals (Xu, 2017).

However, GSC, as well as the original synthetic control method, does not perform well if the outcome is highly discrete and sparse (i.e., with a lot of zeros, which is likely to be the case when I consider prescriptions at the individual physician level). Thus, I aggregate the prescription data to the ZIP code level and use zipcodes as my unit of analysis. Let Rx_{zt} indicate the average monthly new prescriptions written per physician in zipcode z and month t, ($t = 1, 2, 3, ..., 48$). I use the following specification:

$$Rx_{zt} = D_{zt}\delta_z + X_{zt}\beta + F_t\Lambda_z + \epsilon_{zt}, \tag{2.3}$$

where the treatment indicator D_{zt} equals 1 if zipcode z is from Massachusetts and t is after month 18, and equals 0 otherwise. δ_z captures the treatment effect for an average physician in zipcode z that is treated. X_{zt} includes a constant term and the number of patients receiving any new prescriptions (beyond the three categories being considered) from physicians in that zipcode. Recall that the number of patients in a zipcode accounts for changes in the number of patients enrolled in insurance plans offered by the data provider over time. $F_t = [f_{1t}, ..., f_{rt}]'$ consists of r unobserved orthogonal factors (each have T values, T=48), with $\Lambda_z = [\lambda_{z1}, ..., \lambda_{zr}]'$ the (r x 1) unknown factor loadings. Note that the treated and control units are influenced by the same set of factors. The number of factors, r, is fixed throughout month 1 to 48, while each zipcode can have a different set of loadings on r factors. In practice, the number of factors is determined in a data-driven way using cross-validation.

Note that zipcode fixed effects and time fixed effects can be considered as two special cases of the unobserved factors by setting $f_t = 1$ (for zipcode fixed effects) and $\lambda_z = 1$ (for time fixed effects). When bringing the model to data, I explicitly impose it as a model restriction so that I always have the two-way fixed effects. ϵ_{zt} is the zero mean idiosyncratic error for zipcode z and month t. I discuss model estimation and inference details in appendix A.1.

2.4 Results

I present the results from my analysis for the three drug classes in Table 2.6. Recall that I control for the post-treatment main effects and physician-brand specific fixed effects. The results reveal that the treatment effects are negative and statistically significant for all three drug classes and across alternative definitions of treatment. This implies that the prescriptions of branded drugs in the three drug classes declined as a result of the disclosure law. The results are robust when I control for total number of patients receiving new prescriptions and renewals from each physician each month, although the magnitude of the effects is lower by 2%-8%. Moreover, as expected, the sign of the coefficient for the size of the patient pool is positive. These findings suggest that some of the change in prescriptions was driven by changes in the size of the patient pool that a physician treats every month. Specifically, the estimated decline is 48%-59% for branded statins, 46%-54% for branded antidepressants, and 40%-45% for branded antipsychotics, after controlling for monthly patient group sizes per physician. The size of the effect is the lower for treatment 1 (i.e., when the law came into effect) compared to alternative definitions of treatment that consider later time periods. This suggests a cumulative impact from the policy intervention over time.

In addition, I find that the prescriptions for generic drugs decreased as a result of disclosure, although the decline was not as pronounced as in the case of branded drugs. Specifically, the decline is between 38%-46% for generic statins; 32%-41% for generic antidepressants; and 38%-40% for generic antipsychotics. The percentage difference between changes in generics and the branded drugs ranges from 7%-10% in case of statins, 13%-14% in case of antidepressants, and 2%-5% in case of antipsychotics.

2.4.1 Robustness Checks

Below, I investigate the robustness of my key findings that both branded and generic prescriptions declined in MA subsequent to the introduction of the disclosure law.

2.4.1.1 Alternative Control State

In order to verify that the results are not idiosyncratic to the choice of CT as the control state, I replicate the analysis by considering physicians on the MA-NY border. The idea is that if I obtain similar results with two different control states, they are unlikely to be driven by an idiosyncratic trend.

I present the results using prescription data from the MA-NY border in Table A.3

of Appendix B. These results are smaller, and broadly consistent with those from the MA-CT border. Specifically, the results from the MA-NY border suggest that the prescriptions of branded drugs in the three therapeutic classes declined as a result of the policy intervention. Moreover, I find the prescriptions declined by 25%-31% for branded statins (compared to 48%-59% on MA-CT border), by 31%-42% for branded antidepressants (compared to 46%-54% on MA-CT border), and by 34%-42% for branded antipsychotics (compared to 40%-45% on MA-CT border). However, the decline of branded antidepressants is only statistically significant for the first treatment in July, 2009, i.e., when the law was passed (Table A.3).

The negative effect on generics is relatively smaller when I consider the MA-NY border. Specifically, the generic statins decline insignificantly. Nevertheless, the number of generic prescriptions in the antidepressant and antipsychotic categories declined in a statistically significant manner. The results in Table A.3 imply that generic antidepressants declined by 17.3%-17.6%, much smaller compared to the 32%-41% decline estimated on MA-CT border. On the other hand, generic antipsychotics declined by 45%-51%, which is slightly larger than those from MA-CT border (38%-40%). Consistent with findings on MA-CT border, the effect from the policy intervention is stronger for branded drugs than for generics on MA-NY border in two out of three classes, with the decline gradually increases from treatment 1 to 3. This indicates that physicians are potentially substituting branded drugs with generics on MA-NY border.

2.4.1.2 Robustness with Generalized Synthetic Control Method

Recall that the premise behind my analysis is that the physicians in the control state will help in projecting the counterfactual prescriptions that would have been written by physicians in MA had the disclosure law not been instituted. This is accomplished by creating a combination of units in the control state (CT or NY) that would match each unit in the treated state (MA) based on some characteristics. In my application, I match the control and treated zipcodes based on the average monthly prescriptions per physician in month 1 to 18 (i.e., the pre-treatment period). This helps me project the counterfactuals for the treated zipcodes if the treatment had not occurred. I present the estimated ATE from this analysis in Table 2.7.

Overall, GSC method replicates the significant negative effects found in panel regression (Table 2.7, with bootstrapped standard errors from a placebo test). The only exception is branded antidepressants on MA-CT border, for which GSC estimation does not achieve usual significance level. However, the mean estimates are very similar to the panel regression results. Specifically, the number of new prescriptions and re-

newals for branded statins declined by 49% - 64% on MA-CT border, and by 33% - 45% on MA-NY border. The decline for branded antidepressants is between 50%-56% on MA-CT border, and 47%-54% on MA-NY border. These numbers for branded antipsychotics are between 36%-46% on MA-CT border, and between 40%-50% on MA-NY border. For generics, GSC estimation uncover significant decline across all three classes on MA-CT border (34%-59% for statins, 20%-40% for antidepressants, and 52%-75% for antipsychotics), as well as for statins (26%-30%) and antidepressants (24%-27%) on MA-NY border. Table 2.8 compares the size of the estimated ATE across the two estimation methods, by state borders and drug classes. The GSC method generates relatively larger estimates than panel regression. Again, the results from GSC method still indicate that branded prescriptions decline more than their generic counterparts (Table 2.7) for two out of three classes. The size of the decline grows over time across all three drug classes.

2.4.1.3 Alternative Pre Period

As discussed earlier, in January 2009, Blue Cross Blue Shield introduced Alternative Quality Contract (AQC) in MA. Although my prescription data correspond to patients enrolled in plans offered by a different insurance company, I wanted to verify that my results are not contaminated by AQC. To this end, I check robustness of my findings using Jan 2009-June 2009 as the pre-period. This would eliminate prescriptions that were written prior to AQC and would therefore give me the average treatment effect using data after AQC was introduced. I present the results from this analysis in appendix A.3. These results suggest that using Jan-Jun 2009 as the new pre-period does not change the conclusion of the findings, although the estimated ATE are smaller (Table A.6c). Specifically, prescriptions declined by 43%-54% for branded statins , by 28%-31% for branded antidepressants, and by 21%-27% for branded antipsychotics. With the new pre-period, the magnitude of decline is between 38%-45% for generic statins, 28%-38% for generic antidepressants, and 23%-29% for generic antipsychotics. Thus, branded statins declined by 5%-9% more than generic statins, while the size of decline is pretty comparable across branded and generics for antidepressants and antipsychotics. Consistent with my previous results, the effect size of the policy intervention gradually increased over time, indicating a cumulative impact from the policy intervention. Therefore, the broad results that (a) prescriptions of branded and generic drugs declined as a result of the disclosure law, (b) branded drugs declined no less than their generic counterparts, and (c) the effect of the policy intervention becomes stronger if I consider later time periods do not appear to be an artifact of AQC.

2.4.1.4 Transformed Dependent Variable

Recall that the large number of zero monthly prescriptions at the individual physician level raises the concern that the result can be highly sensitive to the long tail of large positive prescriptions. I perform two forms of log transformation of the dependent variable: 1) $Y = \log(Rx_{ibt} + 1)$, and 2) $Y = \log(Rx_{ibt} + \sqrt{Rx_{ibt}^2 + 1})$ to address this issue. I present the results from this analysis in Appendix A.4. The findings from log-transformations are more robust to outliers and consistent across specifications. The estimates from the two log transformations are very similar to each other and are consistent with the key results reported earlier. For the model with transformed variables, the suggested decline ranges from 38%-49% for branded statins, 31%-40% for branded antidepressants, and 16%-21% for branded antipsychotics. Generics show a smaller size of decline than branded in two out of three classes. On average, generic statins declined by 9% less than branded, and generic antidepressants declined by 12% less than branded. Generic antipsychotics slightly deviates from this pattern, where generic antipsychotics declined by 5% more than branded on average. Overall, log-transforming the dependent variables again show that a) both branded and generic prescriptions decline after policy intervention, with the magnitude and significance varying across three drug classes; b) branded prescriptions decline more than generics for two out of three classes; and c) the effect of the intervention increases over time.

The robust findings across the physician panel regression with alternative control state, alternative pre-period, generalized synthetic control estimation, as well as the log-transformed outcome measures, establish the existence of the effect from the policy intervention, which is both statistically and economically significant.

2.4.2 The Brand Level Impact of Disclosure

I further explore whether there are systematic differences across brands, especially in terms of their market share. I present the brand-specific estimation results in Tables 2.9a-2.9c. Note that I only report the estimated average treatment effect in percentages and the significance level, but not the regression coefficients. There are two main findings. First, the effects of policy intervention on the top brands across three drug classes (Lipitor, Crestor, Lexapro, and Seroquel) are statistically significant and robust in terms of their magnitude across alternative specifications. However, when I consider the second, third, and other smaller branded antipsychotics and antidepressants, the decline is not statistically significant after controlling for the total number of patients. Second, market leaders show a much larger percentage decline post intervention com-

pared to their immediate followers and generics. For example, prescriptions for the top antidepressant, Lexapro, declined by 33%-52%, whereas the second player Cymbalta experienced a smaller decline of around 25%. Similarly, prescriptions for the top branded antipsychotics, Seroquel, declined by 38%-43%, which is twice as large as the decline experienced by the second player, Abilify (19%-24%). Statin brands exhibit some deviation from this trend. Specifically, the decline of the top 2 statins, Lipitor and Crestor, are nearly equally large. A possible explanation is that Lipitor and Crestor were equally strong on the market. Note that the top brands (Lipitor, Crestor, Lexapro, and Seroquel) also show 2%-14% larger decline than their generic counterparts. Overall, these findings indicate that the disclosure law impacts the market leader brands more than the follower brands, as well as generics.

2.4.3 The Role of Physician Heterogeneity

Recall that I classify physicians into three different groups - light, medium, or heavy - in each of the three drug classes based on the total number of prescriptions written by them during January 2006-June 2009 i.e., before the policy change.[11] I report the average number of branded and generic prescriptions written by each month by physicians in the three groups as well as the average number of patients in Table 2.5. In general, light prescribers only write 1 prescription a year for the three drug classes. Medium prescribers write 1-2 prescriptions in every 10 months, while heavy prescribers write at least 1 prescription per month. On average, physicians prescribe less branded statins and antidepressants than their generic counterparts, and prescribe more branded antipsychotics than generics. The discrepancy between branded and generic prescriptions is especially large for heavy prescribers: they give twice as much generic statins as branded statins, 2-6 times more generic antidepressants than branded antidepressants, and twice as much branded antipsychotics as generic antipsychotics. Meanwhile, the number of patients visiting heavy prescribers each month doubles the number visiting light prescribers, and is about 1.5 times the number visiting medium prescribers.

I check whether the policy has heteogeneous effects on different physician groups following eq.2.2 (Table 2.10a-2.10c for MA-CT results, Table A.5a-A.5c for MA-NY results). Due to space limit, I only report the estimated average treatment effect in percentages instead of regression coefficients. While both heavy and light prescribers show significant decline in branded statins and in generics of all three classes, only the

[11] The conclusion does not change if I define groups based on fixed number of prescriptions.

heavy prescribers show robust significant decline in branded antipsychotics. Among branded antidepressants, the decline for different physician groups is not robust when controlling for total monthly patients. Interestingly, for both branded and generic medication, light prescribers show a larger percentage decline than the heavy prescribers, although the absolute change is bigger for the latter. A possible explanation is that light prescribers have low average levels of prescriptions (as seen in Table 2.5). Therefore, a small change in the number of prescriptions will result in a large change in percentage terms. Comparing the change in branded medication with those for generics might be more meaningful to evaluate the impact of the policy intervention across physician groups. Focusing on the difference in the decline between the branded and generic drugs within the same physician group, the biggest impact is for the heavy prescribers (on average 12% on MA-CT border, 6% on MA-NY border) compared to medium prescribers (on average 4% on MA-CT border, 3.6% on MA-NY border) and light (on average -6% on MA-CT border, -5.7% on MA-NY border). This indicates that the policy intervention led to greater decline in branded prescriptions compared to generics. This suggests that the legislation is somewhat effective in shifting heavy prescribers from branded to generics drugs.

2.4.4 Relationship between Changes in Prescription and Firm Payments

As discussed earlier in the paper, one of the rationales behind the disclosure law was that it would either discourage firms from making payments to physicians or render it unattractive for physicians to accept them. The resulting decrease in payments might reduce the number of branded prescriptions in favor of generic equivalents. Therefore, from a policy-maker point of view, it might be interesting to understand if the decline in branded prescriptions is in some way tied to changes in payments.

Across the various analyses discussed above, I consistently find that generic prescriptions declined after the policy intervention. Since manufacturers of generic drugs do not typically make payments to physicians, this result suggests that the change in prescription behavior documented above is unlikely to be solely a result of changes in payment structure as a result of the legislation. To further investigate this, I marry the prescription data with the payment data made available in MA as a result of the disclosure law. I use the physician payment data from Massachusetts Health and Human Services for the period July 2009-Dec 2011. These data are available at the firm-physician-year level. Specifically, each record includes the names and location

information for both the firm and the payee, the payee's state license type (e.g. physician, nurse, pharmacist) and license number, the payment amount in dollars, and the nature of the payment (e.g. food, meeting, training, grants, etc.). During 2009-2011, 68% of the payments in the data are directed at 10,918 physicians from 363 manufacturers. Of these payments, the largest proportion is for meals (44%), followed by compensation for Bona Fide services[12] (26.7%), grants/educational gifts (13.8%), and education/training (10.8%). The remaining 5% are for other purposes including meetings, continuing medical education (CME), marketing studies and charitable donations. In terms of the dollar amount, 87.9% ($66 million) was on account of compensation for Bona Fide services, 4.5% goes to education/training, 3% to food and the remaining 4.5% to other purposes.

However, given that the identity of physicians is masked in the prescription data, I cannot match individual physicians with payments that they receive. Therefore, I aggregate prescriptions to the ZIP code level and consider the total payments paid per physician in that ZIP code to create a measure of per-capita payments. An additional limitation of these data is that I only observe payments subsequent to the law. Therefore, I do not observe how and to what extent payments changed as a result of the disclosure law.

In view of these data limitations, my empirical strategy is to exploit the cross-sectional variation in payments in each Massachusetts zipcode in the disclosed data and test whether the level of prescription decline is associated with how much the local area is paid in general. The rationale is that, should firms cut down payments after the legislation or physicians become more reluctant to accept them, the resulting decline in payment would be larger in zipcodes that receive larger payments. Thus, if the decline in prescriptions that I observe is due to change in payments after the legislation, I expect to see such decline to be the largest among the most heavily paid zipcodes. The cross-sectional nature of my analysis cannot pinpoint the (causal) impact of payments directly. Nevertheless, by examining the relationship between payments and branded prescription post treatment (equation 2.4), I expect to collect suggestive evidence that sheds light on one potential mediator of the impact from mandated disclosure on prescriptions.

Since the payment data do not include variation across different products from the same firm, I take all payments from a firm as the proxy for the dollar influence over

[12]Bona Fide services include but are not limited to consulting and participation in speaker's bureaus, joint research projects, clinical trials, and advising on disease treatments. See Kesselheim et al., 2013, and Forbes report: http://www.forbes.com/sites/johnlamattina/2013/06/11/nejm-sheds-light-on-payments-to-doctors/#6e8891f46c49.

each single brand under study. Payments for Connecticut physicians and for generic drugs are set to zero. I divide the zipcode-level payments by total number of physicians from the zipcode in the prescription data to obtain a proxy for payment per capita (in 000s): $PayPerCapita_z^f = \frac{\sum_{y=2009}^{2011} AnnualPayment_y^f}{1000*\sum_i 1(i \in z)}$. The mean payment per capita-brand is highest for statins ($56), followed by antipsychotics ($47), and antidepressants ($26) (Table 2.3).

I investigate the direct relationship between firm payments and changes in prescriptions of each brand with the following specification:

$$Rx_{ibt} = \alpha_b I_b I_s I_t + \delta_b I_b I_s I_t * PayPerCapita_z^{f|b \in f} + \lambda_{bt} + I_{ib} + X_{it/}\beta + \epsilon_{ict} \tag{2.4}$$

For brands other than the top 3, I lump payments from all other firms together and consider it as a shifter for all small brands. Note that the main effect of $PayPerCapita_z^f$ is absorbed by brand-physician fixed effects (I_{ib}). A negative δ_b suggests that prescriptions of brand b declines more when b is more heavily marketed through physician payments, on top of the average decline of brand b across all zipcodes. As can be seen from Table 2.11-2.13, on top of the decline that occurred for all Massachusetts physicians after the disclosure intervention, the additional influence from firm payments is statistically indistinguishable from zero. This result is consistent for all brands across the three drug classes, with Cymbalta (top 2 brand in antidepressants) being the only exception. The insignificance could be due to the data limitation, as I do not have access to physician-brand level payment data and instead use aggregates at zipcode and firm level. On the other hand, the lack of support for the direct relationship between the decline in branded prescriptions and the level of firm payments for branded drugs seems to echo the implication from the decline of generic prescriptions. That is, both suggest that the change in prescription behavior is unlikely to be solely related to changes in payment structure as a result of the legislation.

2.4.5 Discussion

The pattern of my results clearly suggests the following. First, disclosure changes prescription behavior. Specifically it lowers prescriptions for branded and generic drugs. This finding is robust across different estimation models, different drug classes, different control groups, and to different specifications.[13] I therefore rule out that a firm-side change in payments post disclosure caused this outcome as, in that case, I should have

[13]For a full set of robustness checks, see table A.1 and the appendix.

seen no change in generic prescriptions. Second, the impact of the disclosure is higher on branded drugs (relative to generics) and within branded drugs, higher on the larger brands. Third, the policy is more effective in shifting heavier prescribers from branded to generics. Finally, using post disclosure payments data, I am unable to find a statistically significant relationship between payments and prescriptions (at the zipcode level). The explanation that is most consistent with these findings is that physicians increase self-monitoring, leading to lower overall prescriptions. This is also reinforced by the second and third findings described above. As pharmaceutical companies typically allocate marketing resources based on prescription volume (Manchanda and Chintagunta, 2004), the extent of self-monitoring is likely to be the most for branded drugs among the heaviest prescribers.

Another noteworthy aspect of my results is that the extent of decline seems to be economically "big." I investigate this by replicating my results using border counties along the border with another state (New York) as the control group (details are in Appendix A.2). I replicate my results and find the drop in branded statins to be 28% on average (55% in CT), branded antidepressants to be 42% (51% in CT) and branded antipsychotics to be 39% (42% in CT). In a similar vein, the drop in generic statins is 6% on average (43% in CT), generic antidepressants to be 18% (38% in CT) and generic antipsychotics to be 48% (39% in CT). In fact, my results are of similar magnitude to those reported in King and Bearman, 2013's, where the prescriptions of four newly marketed mental health medications drop by 39% to 83% in states that prohibit pharmaceutical gifts to doctors. Thus, the effect sizes I find do not seem to be idiosyncratic to my initial choice of control (state).

The decline in prescriptions could be due to two reasons - higher "under-prescription" or lower "over-diagnosis." In the former, physicians are reluctant to conclude that the patient's condition warrants medication (e.g., they may advocate weight loss, dietary control and lifestyle change rather than a statin to prevent cardiovascular disease[14] while in the latter, physicians may overestimate the need for medication in order to err on the safe side. Given that under-prescribing can lead to worse health outcomes (see Carey et al., 2015), the welfare implications of my results is not clear.

[14]See, for example, Stampfer et al., 2000 and Hu et al., 2001

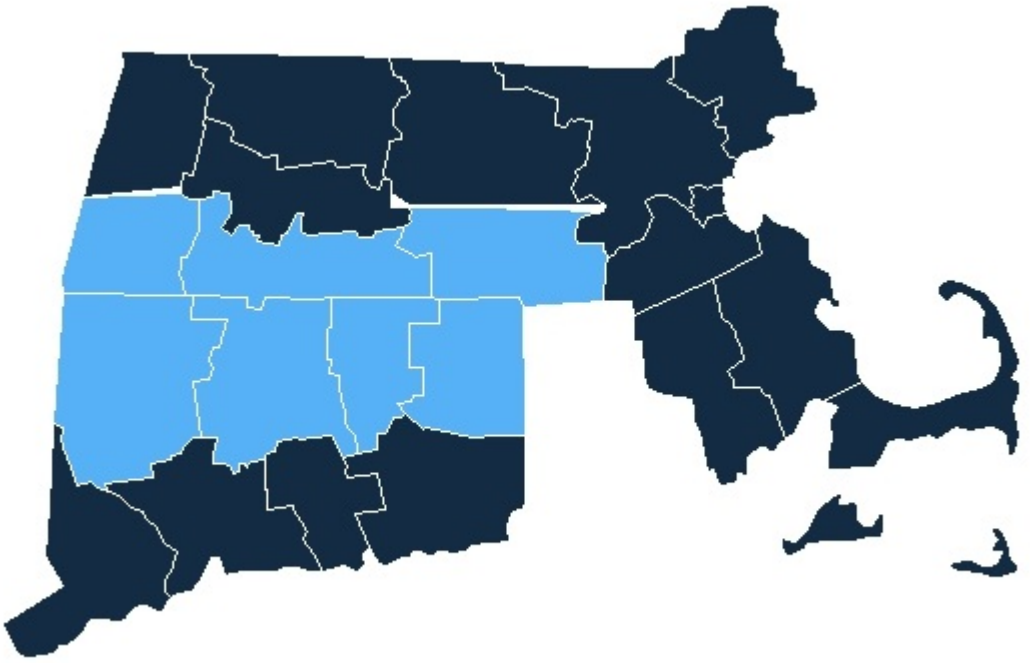
2.5 Conclusion

This essay adds to the growing literature on the impact of mandated transparency on healthcare, with specific focus on how marketing regulations influence physician behavior. Specifically, using very high quality behavioral data and a rich set of controls, this essay provides evidence that disclosure laws impact physician prescription behavior in both a statistically and economically significant manner. The results show that, across a series of policy interventions in Massachusetts from 2009 to 2011, the number of prescriptions of both branded and generic drugs drops with the relative magnitude of the drop being higher for branded drugs. The pattern of the results suggests that the main reason for the change in behavior is self-monitoring by physicians rather than a change in the direct payment regime on the firm side. I show the robustness of my results using prescriptions in three different drug classes across three temporal change points representing the policy intervention, via the use of a different estimation method, the use of an alternative control group, the use of a different pre-period (January 1, 2009 to June 30, 2009) as well as different data transformations. The full set of robustness checks is summarized in TableA.1, with the results presented in Appendix A.2-A.4. Across all these checks, my overall finding is that there is negative and significant impact on prescriptions as a result of the introduction of the disclosure law.

My establishing *what* happened and (perhaps) *why* it happened as a result of the disclosure law opens up multiple avenues for future research. The first avenue deals with the physician behavior - self-monitoring - that I suggest is at play here. Will self-monitoring (and the consequent drop in prescriptions) remain an important force over the long-term, especially once the disclosure of payment information becomes the norm for all physicians across the nation? Is it possible to manage both over-diagnosis and under-prescription, especially as there are some concerns about the potential efficacy decline of drugs prescribed by physicians under disclosure (Carey et al., 2015)? The second avenue is focused around firm and patient response to public payment information. Will firms respond strategically to the revealed payment information from rivals? Will patients examine the payments data and if they do, will that affect their healthcare decisions? Finally, what are the implications for social welfare for creating and implementing these laws? Many of these payments are made to physicians in recognition of their involvement in the innovation process, such as industry-driven research, that brings new treatment options to the market. Could the public disclosure of physician payment hinder physician participation in bringing new treatment options to market (e.g., prescribing drugs that are in the clinical trial phase)? This

can be detrimental to advancement in treatment options that are available to patients in the future (Santhakumar and Adashi, 2015). Moreover, it is unclear whether the potential savings in healthcare costs (from reduced prescriptions) justify the expenditure throughout the process of data collection, preparation, interpretation and public dissemination.

Figure 2.1: MA-CT border counties in the regression sample.



Litcheld, Hartford, Tolland, Windham (CT); Hampden, Berkshire and Worcester (MA).Berkshire and Worcester zipcodes are included up to the northern border of Hampden (lat <42.4N).

Figure 2.2: Average monthly prescriptions per physician, by class and state.

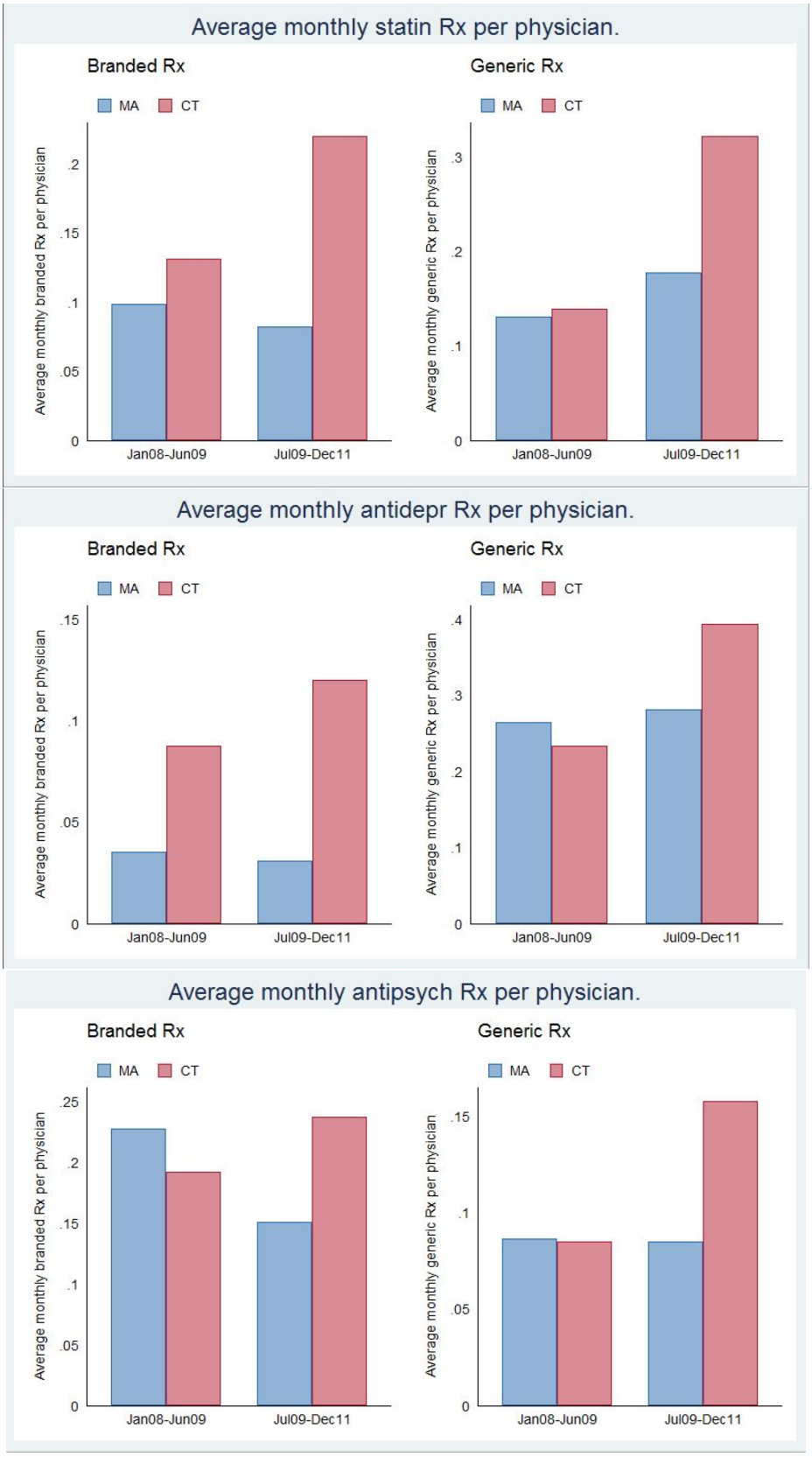


Table 2.1: Socio-Economic Conditions of the MA-CT Border Counties.

	Massachusetts			Connecticut			
	Berkshire	Hampden	Worcester	Hartford	Litchfield	Tolland	Windham
DEMOGRAPHICS							
Total Population	130,866	464,072	799,277	893,504	189,277	152,137	118,046
%Female	51.90%	51.90%	50.70%	51.70%	50.80%	49.50%	50.40%
Median Age	44.6	38.8	39.2	39.9	44.4	38.3	39.4
%18+	80.60%	76.40%	76.60%	77.30%	78.60%	80.00%	77.80%
%65+	18.80%	14.30%	12.90%	14.60%	16.10%	12.20%	13.00%
SOCIAL-ECONOMIC							
Total Households	55,612	177,756	299,350	349,158	76,149	54,499	43,909
%Family HHld	58.50%	65.30%	67.30%	65.20%	67.80%	69.40%	67.90%
%HHld with 1+ under 18	25.50%	33.50%	34.30%	32.40%	30.40%	33.70%	32.10%
%HHld with 1+ over 65	31.90%	26.70%	24.20%	26.40%	28.50%	23.90%	24.20%
%Unemployed	9.20%	10.40%	8.70%	9.60%	7.60%	6.60%	10.60%
%income below poverty level¶	12.40%	17.10%	10.30%	11.50%	6.20%	7.00%	11.70%
Median HHld Income§	47,513	49,729	65,968	64,752	71,345	80,887	58,489
Mean HHld Income§	66,048	64,999	82,415	85,910	91,518	94,117	71,286
Per Capita Income§	28,939	25,646	31,609	34,356	37,410	34,468	27,456
HEALTH INSURANCE COVARAGE							
%private insurance	72.80%	66.00%	78.00%	73.60%	79.10%	84.80%	70.80%
%public insurance	41.20%	43.20%	30.50%	30.50%	27.20%	21.60%	31.70%
%Uninsured	3.70%	4.40%	3.50%	8.00%	7.00%	5.10%	9.30%
EDUCATIONAL ATTAINMENT							
Population 25 years and over	93,083	305,343	535,755	610,975	135,759	96,665	79,249
%high school or higher	91.00%	83.70%	89.10%	87.70%	91.30%	92.60%	85.90%
%bachelor's or higher	29.80%	24.20%	33.80%	34.40%	32.60%	36.70%	21.90%
%graduate or professional	12.60%	9.10%	13.00%	14.70%	13.10%	16.80%	9.00%

Source: U.S. Census Bureau, 2008-2012 American Community Survey.

¶in the past 12 months.

§all income measures are in 2012 inflation-adjusted dollars.

Table 2.2: Number of physicians, drugs, and brand-name drugs in the sample.

	#physicians		#drugs[§]		#brandnames		#zips	
	MA	CT	MA	CT	MA	CT	MA	CT
Statins	1,014	1,326	13	16	9	11	100	95
Antidepressants	1,322	1,752	42	57	14	22	102	103
Antipsychotics	393	703	29	34	15	19	81	90

§A drug is uniquely defined by its trademark name or generic name.

Table 2.3: Physician panel summary statistics, January 2008 - December 2011.

(a) Statins.

Variable		N	Mean	sd	Max
Branded Rx, per physician-month	MA	48,672	0.09	0.39	12
	CT	63,648	0.19	0.59	12
Generic Rx, per physician-month	MA	48,672	0.16	0.53	18
	CT	63,648	0.25	0.88	24
Total patients, per physician-month	MA	48,672	4	9	118
	CT	63,648	5	13	323[¶]
Payment per capita ($), per physician-brand[§]	MA	4,056	56.04	210.15	2,159.91

(b) Antidepressants.

Variable		N	Mean	sd	Max
Branded Rx, per physician-month	MA	63,456	0.03	0.24	10
	CT	84,096	0.11	0.54	16
Generic Rx, per physician-month	MA	63,456	0.28	1.07	38
	CT	84,096	0.33	1.29	39
Total patients, per physician-month	MA	63,456	4	12	376[¶]
	CT	84,096	5	11	256
Payment per capita ($), per physician-brand	MA	7,932	26.09	121.68	1,427.89

(c) Antipsychotics.

Variable		N	Mean	sd	Max
Branded Rx, per physician-month	MA	18,864	0.18	0.79	27
	CT	33,744	0.22	0.86	18
Generic Rx, per physician-month	MA	18,864	0.09	0.46	14
	CT	33,744	0.13	0.65	28
Total patients, per physician-month	MA	18,864	4	10	161
	CT	33,744	4	8	170[¶]
Payment per capita ($), per physician-brand[§]	MA	1,965	47.45	187.26	1,622.03

§Brands other than the top 3 (top 2 for statins) are lumped together. Generics are excluded. [¶] In 2008, a physician sees 23 patients per day on average. This translates into 460 patients in 1 month (4 weeks, 5day/week). Source: The Physicians Foundation.

Table 2.4: Average monthly growth rate in total prescriptions during Jan 2008 - June 2009, in the border samples.

Border	State	Statins	Antidepressants	Antipsychotics
MA-CT	MA	3.34%	2.89%	1.44%
	CT	4.24%	4.26%	5.61%
MA-NY	MA	6.86%	7.44%	5.18%
	NY	6.47%	6.39%	12.61%

Table 2.5: Average prescription level and monthly patients by physician group, Jan2008-Dec2011.

(a) Statins.

	CT			MA		
Average per physician-month	Light	Medium	Heavy	Light	Medium	Heavy
Branded Rx	0.05	0.18	0.61	0.03	0.08	0.29
Generic Rx	0.07	0.21	0.84	0.05	0.14	0.49
Total patients	4	7	9	3	5	7

(b) Antidepressants.

	CT			MA		
Average per physician-month	Light	Medium	Heavy	Light	Medium	Heavy
Branded Rx	0.02	0.08	0.40	0.01	0.02	0.12
Generic Rx	0.08	0.24	1.21	0.08	0.21	0.96
Total patients	4	5	7	3	4	6

(c) Antipsychotics.

	CT			MA		
Average per physician-month	Light	Medium	Heavy	Light	Medium	Heavy
Branded Rx	0.07	0.17	0.89	0.05	0.13	0.68
Generic Rx	0.03	0.10	0.53	0.02	0.05	0.33
Total patients	4	5	7	3	2	8

Table 2.6: Panel Regression Results.

(a) Statins.

Treatment Measure§	1	1	2	2	3	3
ATE, Branded Rx	-0.104***	-0.078***	-0.143***	-0.107***	-0.166***	-0.123***
	(0.010)	(0.009)	(0.013)	(0.012)	(0.015)	(0.013)
ATE, Generic Rx	-0.135***	-0.109***	-0.190***	-0.154***	-0.221***	-0.177***
	(0.015)	(0.014)	(0.022)	(0.020)	(0.025)	(0.023)
Monthly total patients		0.015***		0.016***		0.016***
		(0.003)		(0.003)		(0.003)
Brand-year-month FE			YES			
Physician-brand FE			YES			
ATE%, branded	-56%	-48.6%	-63.8%	-56.8%	-66.3%	-59.3%
ATE%, generics	-43.2%	-38%	-49.6%	-44.3%	-51.4%	-46%
N	224,640	224,640	168,480	168,480	149,760	149,760

(b) Antidepressants.

Treatment Measure§	1	1	2	2	3	3
ATE, Branded Rx	-0.037***	-0.027***	-0.048***	-0.033***	-0.054***	-0.037***
	(0.008)	(0.009)	(0.011)	(0.012)	(0.011)	(0.013)
ATE, Generic Rx	-0.143***	-0.132***	-0.201***	-0.187***	-0.222***	-0.204***
	(0.025)	(0.024)	(0.030)	(0.029)	(0.033)	(0.031)
Monthly total patients		0.012***		0.012***		0.012***
		(0.002)		(0.003)		(0.003)
Brand-year-month FE			YES			
Physician-brand FE			YES			
ATE%, branded	-54.5%	-46.5%	-60.8%	-51.9%	-62.9%	-53.6%
ATE%, generics	-33.6%	-31.9%	-41.6%	-39.8%	-43.1%	-41.2%
N	295,104	295,104	221,328	221,328	196,736	196,736

(c) Antipsychotics.

Treatment Measure§	1	1	2	2	3	3
ATE, Branded Rx	-0.121***	-0.100**	-0.143***	-0.113**	-0.138***	-0.104**
	(0.044)	(0.042)	(0.048)	(0.045)	(0.049)	(0.046)
ATE, Generic Rx	-0.074***	-0.053**	-0.089***	-0.059**	-0.093***	-0.058**
	(0.021)	(0.022)	(0.024)	(0.025)	(0.025)	(0.026)
Monthly total patients		0.017***		0.016***		0.015***
		(0.004)		(0.004)		(0.004)
Brand-year-month FE			YES			
Physician-brand FE			YES			
ATE%, branded	-44.6%	-40%	-50.5%	-44.6%	-48.5%	-41.4%
ATE%, generics	-46.5%	-38.3%	-49.7%	-39.5%	-50.1%	-38.8%
R-squared	0.006	0.03	0.008	0.03	0.008	0.03
N	105,216	105,216	78,912	78,912	70,144	70,144

*** p<0.01. ** p<0.05. * p<0.1. Standard Errors clustered at physician level. ATE% calculated at the mean. §Treatments are measured by 3 different temporal points: (1) when the data collection began on July 1, 2009, (2) when the firms first reported their payment to the government (July 1, 2010), and (3) when the data were made available to the public (Nov 22, 2010).

Table 2.7: Average Treatment Effect as percentage change of post counterfactuals, using Generalized Synthetic Control method.

	MA-CT border			MA-NY border		
	Statins	Antidep.	Antipsych.	Statins	Antidep.	Antipsych.
Branded Rx per physician/zip						
Post 1	-48.8%***	-49.7%	-35.6%***	-33.3%*	-52.1%*	-39.8%*
Post 2	-59.9%***	-55.8%	-45.8%***	-36.9%	-54.3%	-49.5%**
Post 3	-63.9%***	-54.1%	-44.6%***	-44.8%*	-46.9%	-50.2%*
Generic Rx per physician/zip						
Post 1	-50.3%***	-24.3%***	-66.2%*	-30.5%**	-26.8%***	-68.5%
Post 2	-58.8%***	-40.1%***	-74.6%*	-28.6%**	-24.0%**	-67.7%
Post 3	-34.7%***	-20.2%***	-51.9%*	-25.6%**	-26.0%***	-56.2%
Nzip, control	95	103	90	62	67	47
Nzip, treat	100	102	81	76	76	49

*** p<0.01. ** p<0.05. * p<0.1. Standard errors are from a placebo test and are bootstrapped for 1,000 times. Monthly patients control and two-way fixed effects included. ATE% evaluated at the mean counterfactual.

Table 2.8: Comparison of estimated decline from Panel Regression and Generalized Synthetic Control.

			Statins	Antidep.	Antipsych.
MA-CT border	Branded Rx	Panel Reg.	48.6%-59.3%	46.5%-53.6%	40%-45%
		GSC	48.8%-64.0%	49.7%-55.8%	35.6%-45.8%
	Generic Rx	Panel Reg.	38%-46%	32%-41%	38%-39%
		GSC	34.7%-58.8%	20.2%-40.1%	51.9%-74.6%
MA-NY border	Branded Rx	Panel Reg.	25%-31.5%	30.9%-42.2%	33.9%-42.1%
		GSC	33.3%-44.8%	46.9%-54.3%	39.8%-50.2%
	Generic Rx	Panel Reg.	3.5%-8.6%	17.3%-17.6%	44.9%-51.5%
		GSC	25.6%-30.5%	24.0%-26.8%	56.2%-68.5%

Monthly patients control and two-way fixed effects included in both methods.
ATE% evaluated at the mean counterfactual.

Table 2.9: Brand-Specific Effects.

(a) Statins.

Treatment Measure§	1	1	2	2	3	3
ATE, Top 1 (Lipitor)	-53.3%***	-48.1%***	-61.7%***	-56.5%***	-64.8%***	-59.7%***
ATE, Top 2 (Crestor)	-63.4%***	-51.2%***	-69.4%***	-58.5%***	-70.3%***	-58.9%***
ATE, Generic Rx	-43.2%***	-38%***	-49.6%***	-44.3%***	-51.4%***	-46%***
Monthly total patients	NO	YES	NO	YES	NO	YES
Brand-year-month FE			YES			
Physician-brand FE			YES			
N	449,280	449,280	336,960	336,960	299,520	299,520

(b) Antidepressants.

Treatment Measure§	1	1	2	2	3	3
ATE, Top 1 (Lexapro)	-44.3%***	-32.8%**	-59.1%***	-46.5%***	-66.0%***	-51.7%***
ATE, Top 2 (Cymbalta)	-39.0%***	-24.7%**	-41.2%***	-23.8%	-43.3%***	-23.7%
ATE, Top 3 (Pristiq)	-71.3%***	7.4%	-71.1%***	21.4%	-65.9%***	27.6%
ATE, Generic Rx	-32.3%***	-31.5%***	-40.0%***	-39.2%***	-42.0%***	-41.1%***
Monthly total patients	NO	YES	NO	YES	NO	YES
Brand-year-month FE			YES			
Physician-brand FE			YES			
N	737,760	737,760	553,320	553,320	491,840	491,840

(c) Antipsychotics.

Treatment Measure§	1	1	2	2	3	3
ATE, Top 1 (Seroquel)	-44.3%***	-40.7%**	-47.5%***	-42.7%**	-43.5%**	-37.6%*
ATE, Top 2 (Abilify)	-34.8%*	-22.8%	-40.1%**	-24.1%	-37.6%*	-18.9%
ATE, Top 3 (Zyprexa)	-39.5%*	-28.8%	-50.0%**	-36.5%	-49.7%*	-33.8%
ATE, Generic Rx	-46.5%***	-38.3%***	-49.7%***	-39.5%***	-50.1%***	-38.8%***
Monthly total patients	NO	YES	NO	YES	NO	YES
Brand-year-month FE			YES			
Physician-brand FE			YES			
N	263,040	263,040	197,280	197,280	175,360	175,360

*** p<0.01. ** p<0.05. * p<0.1. Standard Errors clustered at physician level. ATE% calculated at the mean.§Treatments are measured by 3 different temporal points: (1) when the data collection began on July 1, 2009, (2) when the firms first reported their payment to the government (July 1, 2010), and (3) when the data were made available to the public (November 22, 2010).

Table 2.10: Heterogeneous effects across physician groups.

(a) Statins.

Treatment Measure§	1	1	2	2	3	3
ATE, Branded Rx, Light	-67.0%***	-35.7%**	-74.1%***	-45.4%**	-80.5%***	-51.1%***
ATE, Branded Rx, Medium	-51.0%***	-25.7%**	-61.4%***	-38.8%***	-66.8%***	-42.8%***
ATE, Branded Rx, Heavy	-45.9%***	-36.4%***	-53.3%***	-42.6%***	-54.9%***	-43.3%***
ATE, Generic Rx, Light	-66.0%***	-63.8%***	-71.8%***	-69.6%***	-78.0%***	-75.3%***
ATE, Generic Rx, Medium	-48.5%***	-44.9%***	-52.5%***	-49.4%***	-56.5%***	-53.3%***
ATE, Generic Rx, Heavy	-30.3%***	-28.0%***	-37.6%***	-35.1%***	-38.0%***	-35.3%***
Monthly total patients	NO	YES	NO	YES	NO	YES
Brand-year-month FE			YES			
Physician-brand FE			YES			
N	291,264	291,264	218,448	218,448	194,176	194,176

(b) Antidepressants.

Treatment Measure§	1	1	2	2	3	3
ATE, Branded Rx, Light	-63.2%***	40.1%	-69.7%***	55.0%	-75.9%***	60.8%
ATE, Branded Rx, Medium	-52.6%***	13.7%	-61.0%***	22.9%	-63.0%***	28.3%
ATE, Branded Rx, Heavy	-41.2%***	-15.0%	-47.2%***	-15.6%	-49.1%***	-15.1%
ATE, Generic Rx, Light	-58.9%***	-56.5%***	-65.2%***	-63.3%***	-67.5%***	-65.4%***
ATE, Generic Rx, Medium	-35.0%***	-33.0%***	-41.4%***	-39.2%***	-43.9%***	-41.7%***
ATE, Generic Rx, Heavy	-31.6%***	-31.0%***	-38.8%***	-38.0%***	-39.4%***	-38.6%***
Monthly total patients	NO	YES	NO	YES	NO	YES
Brand-year-month FE			YES			
Physician-brand FE			YES			
N	582,912	582,912	437,184	437,184	388,608	388,608

(c) Antipsychotics.

Treatment Measure§	1	1	2	2	3	3
ATE, Branded Rx, Light	-38.2%	27.5%	-37.9%	58.6%	-33.7%	82.7%
ATE, Branded Rx, Medium	-33.0%*	-7.9%	-48.6%**	-13.5%	-46.2%*	-1.2%
ATE, Branded Rx, Heavy	-52.9%***	-47.1%**	-54.5%***	-48.1%**	-52.2%***	-45.5%**
ATE, Generic Rx, Light	-65.4%***	-60.5%***	-62.9%***	-56.7%**	-62.3%***	-55.6%**
ATE, Generic Rx, Medium	-51.3%***	-47.8%***	-56.6%***	-52.3%***	-58.3%***	-53.7%***
ATE, Generic Rx, Heavy	-41.9%**	-39.8%**	-42.9%***	-40.5%**	-41.9%***	-39.5%**
Monthly total patients	NO	YES	NO	YES	NO	YES
Brand-year-month FE			YES			
Physician-brand FE			YES			
N	209,328	209,328	156,996	156,996	139,552	139,552

*** p<0.01. ** p<0.05. * p<0.1. Standard Errors clustered at physician level. ATE% calculated at the mean. §Treatments are measured by 3 different temporal points: (1) when the data collection began on July 1, 2009, (2) when the firms first reported their payment to the government (July 1, 2010), and (3) when the data were made available to the public (November 22, 2010).

Table 2.11: Relationship between firm payments and statin prescriptions.

Treatment Measure§	1	1	2	2	3	3
Top 1 brand	-0.069***	-0.056***	-0.094***	-0.076***	-0.111***	-0.089***
	(0.007)	(0.007)	(0.010)	(0.009)	(0.011)	(0.010)
Top 1 x paypercapita(000s)	-0.050	-0.041	-0.035	-0.028	-0.023	-0.017
	(0.058)	(0.057)	(0.067)	(0.064)	(0.066)	(0.064)
Top 2 brand	-0.033***	-0.020***	-0.048***	-0.039***	-0.054***	-0.032***
	(0.004)	(0.004)	(0.006)	(0.005)	(0.006)	(0.006)
Top 2 x paypercapita(000s)	-0.010	-0.010	-0.006	-0.008	-0.004	-0.008
	(0.012)	(0.012)	(0.012)	(0.012)	(0.011)	(0.011)
other brands	-0.0002	-0.013***	0.0003	0.019***	-0.00004	0.022***
	(0.001)	(0.003)	(0.001)	(0.004)	(0.001)	(0.005)
others x paypercapita(000s)	-0.0007	-0.0009	-0.0003	-0.001	-0.0005	-0.002
	(0.002)	(0.002)	(0.001)	(0.002)	(0.002)	(0.003)
Total generics Rx	-0.135***	-0.122***	-0.190***	-0.172***	-0.221***	-0.199***
	(0.016)	(0.015)	(0.022)	(0.021)	(0.025)	(0.024)
Monthly total patients		0.007***		0.008***		0.008***
		(0.001)		(0.001)		(0.001)
Brand-year-month FE			YES			
Physician-brand FE			YES			
N	449,280	449,280	336,960	336,960	299,520	299,520

*** p<0.01. ** p<0.05. * p<0.1. Standard Errors clustered at physician level. ATE% calculated at the mean. §Treatments are measured by 3 different temporal points: (1) when the data collection began on July 1, 2009, (2) when the firms first reported their payment to the government (July 1, 2010), and (3) when the data were made available to the public (November 22, 2010).

Table 2.12: Relationship between firm payments and antidepressant prescriptions.

Treatment Measure§	1	1	2	2	3	3
Top 1 brand	-0.017***	-0.013**	-0.027***	-0.021***	-0.032***	-0.025***
	(0.0055)	(0.0058)	(0.0073)	(0.0075)	(0.0077)	(0.008)
Top 1 x paypercapita(000s)	0.010	0.015	0.012	0.018	0.0076	0.013
	(0.016)	(0.017)	(0.016)	(0.017)	(0.013)	(0.015)
Top 2 brand	-0.011***	-0.0078*	-0.013**	-0.0073	-0.014**	-0.006
	(0.004)	(0.0044)	(0.0053)	(0.0057)	(0.0057)	(0.0062)
Top 2 x paypercapita(000s)	-0.0693*	-0.053	-0.076**	-0.062*	-0.074**	-0.062*
	(0.036)	(0.035)	(0.037)	(0.037)	(0.037)	(0.037)
Top 3 brand	-0.0031***	0.00079	-0.0031***	0.0027	-0.0026***	0.0042
	(0.0011)	(0.0024)	(0.0011)	(0.0029)	(0.00095)	(0.00312)
Top 3 x paypercapita(000s)	0.0071	0.055	0.017	0.057	0.013	0.052
	(0.014)	(0.049)	(0.022)	(0.060)	(0.022)	(0.062)
other brands	-0.0041**	-0.00012	-0.0043**	0.0016	-0.0046**	0.0023
	(0.0017)	(0.0027)	(0.0018)	(0.0032)	(0.0018)	(0.0036)
others x paypercapita(000s)	0.0013	0.0020	0.0015	0.0019	0.0015	0.0018
	(0.0034)	(0.0038)	(0.0035)	(0.0040)	(0.0035)	(0.0041)
Total generics Rx	-0.143***	-0.139***	-0.201***	-0.196***	-0.222***	-0.215***
	(0.025)	(0.025)	(0.030)	(0.030)	(0.033)	(0.032)
Monthly total patients		0.0049***		0.0049***		0.0049***
		(0.0009)		(0.001)		(0.001)
Brand-year-month FE			YES			
Physician-brand FE			YES			
N	737,760	737,760	553,320	553,320	491,840	491,840

*** p<0.01. ** p<0.05. * p<0.1. Standard Errors clustered at physician level. ATE% calculated at the mean. §Treatments are measured by 3 different temporal points: (1) when the data collection began on July 1, 2009, (2) when the firms first reported their payment to the government (July 1, 2010), and (3) when the data were made available to the public (November 22, 2010).

Table 2.13: Relationship between firm payments and antipsychotic prescriptions.

Treatment Measure§	1	1	2	2	3	3
Top 1 brand	-0.0550**	-0.0474**	-0.0623***	-0.0510**	-0.0557**	-0.0425*
	(0.0226)	(0.0215)	(0.0240)	(0.0228)	(0.0251)	(0.0239)
Top 1 x paypercapita(000s)	-0.0424	-0.0306	-0.0249	-0.0158	-0.0311	-0.0246
	(0.0616)	(0.0587)	(0.0682)	(0.0655)	(0.0557)	(0.0528)
Top 2 Brand	-0.0168*	-0.00851	-0.0205**	-0.00860	-0.0200*	-0.00642
	(0.00942)	(0.00938)	(0.0104)	(0.0104)	(0.0109)	(0.0109)
Top 2 x paypercapita(000s)	-0.0267	-0.0219	-0.0226	-0.0202	-0.0221	-0.0201
	(0.0203)	(0.0182)	(0.0210)	(0.0184)	(0.0215)	(0.0184)
Top 3 brand	-0.0209*	-0.0138	-0.0279**	-0.0170	-0.0286**	-0.0158
	(0.0113)	(0.0117)	(0.0133)	(0.0138)	(0.0137)	(0.0144)
Top 3 x paypercapita(000s)	-0.00149	0.0585	0.0643	0.113	0.101	0.140
	(0.207)	(0.187)	(0.232)	(0.214)	(0.246)	(0.228)
other brands	-0.0213**	-0.0138	-0.0274**	-0.0162	-0.0293**	-0.0163
	(0.0106)	(0.0112)	(0.0118)	(0.0127)	(0.0120)	(0.0130)
others x paypercapita(000s)	-0.0297	-0.0201	-0.0315	-0.0235	-0.0325	-0.0260
	(0.0420)	(0.0384)	(0.0429)	(0.0398)	(0.0431)	(0.0404)
Total generics Rx	-0.0739***	-0.0654***	-0.0888***	-0.0767***	-0.0925***	-0.0789***
	(0.0206)	(0.0202)	(0.0238)	(0.0233)	(0.0248)	(0.0244)
Monthly total patients		0.00685***		0.00641***		0.00615***
		(0.00178)		(0.00178)		(0.00171)
Brand-year-month FE			YES			
Physician-brand FE			YES			
N	263,040	263,040	197,280	197,280	175,360	175,360

*** p<0.01. ** p<0.05. * p<0.1. Standard Errors clustered at physician level. ATE% calculated at the mean. §Treatments are measured by 3 different temporal points: (1) when the data collection began on July 1, 2009, (2) when the firms first reported their payment to the government (July 1, 2010), and (3) when the data were made available to the public (November 22, 2010).

CHAPTER 3

The Effect of Information Disclosure on Industry Payments to Physicians

3.1 Introduction

U.S. pharmaceutical companies devote a significant amount of resources towards persuading physicians to prescribe their drugs (Manchanda and Honka, 2013, Manchanda and Chintagunta, 2004). A sizable fraction of this marketing budget is in the form of direct payments and gifts such as consulting and speaker fees, conference travel, and meals. In 2015, these direct payments totaled $2.6 billion, raising concerns that such practice can bias treatment decisions (DeJong et al., 2016, Grochowski Jones and Ornstein, 2016, Carey et al., 2015, Engelberg et al., 2014) and lead to higher healthcare costs. As a result, several states have instituted disclosure laws wherein firms are required to publicly declare the payments that they made to physicians (Chimonas et al., 2010).[1] In 2013, this law was rolled out to all 50 states as part of the Affordable Care Act.

The motivation behind these disclosure regulations is that patients and payers (insurance companies and the government) would make negative inferences about physicians and pharmaceutical firms once they understood the extent of the financial relationship between them (Pham-Kanter, 2014, Pham-Kanter et al., 2012, Perry et al., 2014, Agrawal et al., 2013, Carey et al., 2015, Mojir, 2017). This, in turn, might render physicians reluctant to accept payments (Chen et al., 2016) and firms less willing to offer them, leading to a decline in such transactions. However, a consequence of public disclosure is that, in addition to patients and payers, physicians and rival firms can observe which physicians are being targeted by which firms and also learn about the

[1] In similar vein, Sudhir and Talukdar, 2015 discuss how transparency as a result of IT adoption can result in lower corruption in emerging economies

amount of marketing expenditure directed towards each physician. As a result, the effect of disclosure of financial ties on subsequent payments is not clear.

In this paper, I investigate the impact of public disclosure of payment information on subsequent payments. Furthermore, I attempt to parse out the plausible mechanism driving such changes in payments as a result of public disclosure. In order to achieve my research objectives, I study the federal regulatory change instituted in 2013 as part of the Sunshine provision of the Affordable Care Act. The law requires full disclosure of direct-to-physician payments from every pharmaceutical firm in the United States (*Physician Open Payment Data* from Centers for Medicare and Medicaid Services, henceforth CMS). As a result of this law, firms began collecting the payment data starting in August 2013. These data are at a very granular level and contain information on the dollar value of the gift/payment at the physician-drug level as well as the date of the corresponding payment. Firms were required to submit all the data from 2013 to the CMS by March 31, 2014. Physicians and teaching hospitals then had 60 days to review and dispute the payment information related to themselves before the data were made public.[2] In September 2014, the first batch of payment data from 2013 were made public via an online portal. Subsequent releases of the payment data happened on June 30 of every following year.

Due to the review process, any physician-side and firm-side concerns regarding reputation should be reflected in the payments before data disclosure.[3] Therefore, starting in October 2014, the change in payments made by firms should be a result of (a) possible reactions from patients and payers upon seeing the disclosed payment information, (b) comparison of payments on the physician side, and (c) pharmaceutical firms gaining access to information on the physicians that their rivals target. My study tracks the payment data until December 2015. Thus, I can compare the payments for the 14-month period before the first-time disclosure (Aug 2013 - Sep 2014) to those in the 15 months after (Oct 2014 - Dec 2015) to understand how payments changed subsequent to public disclosure.

A potential concern with such a comparison is that payment changes over time could have been driven by other unobservables that are unrelated to disclosure. To control for the baseline time trends that are not explained by the information disclosure, I exploit the fact that certain states (MA, VT, MN) had similar state-level disclosure laws in effect prior to the federal regulation.[4] Therefore, the federal disclosure did not

[2]https://www.cms.gov/OpenPayments/Downloads/Open-Payments-User-Guide.pdf

[3]As I discuss subsequently, I study the effect of disclosure and the enactment of the law per se.

[4]These state disclosure laws were generally more stringent than the federal regulation. For example, in Massachusetts, companies must report payments to *anyone* "who prescribes, dispenses or purchases

provide any additional information to the stakeholders in these states. This variation in the level of information gain from disclosure across states creates a quasi-experimental setting wherein I can compare the observed payment changes pre- and post-disclosure in other states to the corresponding payment changes in these states with prior disclosure laws. This comparison will enable me to understand the causal effect of disclosing competitors' direct-to-physician payment information.

The validity of such a difference-in-differences design hinges on the comparability of physician-drug pairs from the treated and control states (Goldfarb and Tucker, 2014). In essence, I need to find the best control counterpart, a "clone", for every physician-drug pair in the treated states. The conventional approach is to employ propensity score matching (PSM; Hirano et al., 2003, Imbens and Rubin, 2015), in order to balance the control and treated units. This involves modeling the propensity of a unit to be treated using kernel or series estimation with first-order terms and interactions of selected observed characteristics. Such an approach can become very cumbersome as the number of covariates increases, especially when (a) higher-order interactions are also considered and (b) researchers are unwilling to impose any prior assumptions in covariate selection. Although several revisions to the conventional method have been proposed (e.g. McCaffrey et al., 2004), they have been found to be sensitive to the exact implementation (e.g. Khwaja et al., 2011, Athey and Imbens, 2017).

In this paper, I achieve better matching through a machine learning algorithm, Causal Forest (CF; see Wager and Athey, 2017), that is computationally efficient, robust to model mis-specification, and offers consistent and asymptotically normal point estimates for each drug-physician pair. Forest-based models (Breiman, 2001) are some of the most popular supervised machine learning methods in computer science and engineering, known for reliable "out-of-the-box" performance without much model tuning (Athey and Imbens, 2017). While mostly used for prediction tasks (e.g. Berk, 2012; also see Dzyabura and Yoganarasimhan, 2018 for a review of the methods), these methods have recently been adapted to allow for causal inference (Athey and Imbens, 2016, Wager and Athey, 2017, Ascarza, 2016, Wang et al., 2015). CF is especially powerful in solving the curse of dimensionality problem in PSM, while letting the data choose the most relevant features in matching.

prescription drugs or medical devices in the Commonwealth". These include individuals such as pharmacist, nurse practitioner, audiologist, podiatrist or physical therapist, as well as facilities such as nursing home, clinic, hospice program, clinical laboratory, home health agency or pharmacy. These individuals and facilities are not required to report based on federal law. In practice, the more stringent one applies where the two laws overlap. See http://www.mass.gov/dph/pharmamed. Therefore, I argue that the federal law did not provide any new information in these three states that had prior laws.

Intuitively, CF clusters the most similar observations into the same group adaptively: the group is larger along the directions with higher degree of homogeneity and smaller along directions with higher degree of heterogeneity. All observations within the same group are considered to have the same propensity to be treated. This adaptive nature of CF can substantially increase the clustering accuracy and robustness when dealing with large space of covariates. I also include the full sequence of pre-disclosure outcomes as an additional set of features in matching, which further helps me control for differential pre-trends across control and treatment units in the spirit of synthetic control methods (e.g. Tirunillai and Tellis, 2016). Additionally, as CF is the bagging of thousands of trees, each of which takes a subsample of the data and covariates, I can recover both a point estimate and a confidence interval for each treated unit. This allows me to make inferences about the magnitude and the significance of the treatment effect for each drug-physician pair. Inferring the heterogeneous treatment effects at such a granular level would enable me to investigate the likely impact of the disclosure law as well as the potential mechanism driving the impact of payment disclosure.

My empirical analysis is based on payments made by pharmaceutical firms to physicians in the anti-diabetics category. My data include over $100 million in monthly payments for 16 branded anti-diabetics to 50,000 physicians between August 2013 and December 2015. I chose the anti-diabetics category because it is one of the fastest-growing categories in the pharmaceutical industry, receiving top investments in R&D and marketing. As a result, new medications are constantly introduced, thereby adding to competitive intensity. The dynamics of this market makes it a good test field to study the effect of payment information disclosure among competitors on subsequent payments between firms and physicians.

My results reveal that the monthly payments declined by 2%, on average, due to disclosure. However, there is considerable heterogeneity in the treatment effects with 14% of the drug-physician pairs showing a significant increase in their monthly payment. Hence, while payments did decrease subsequent to disclosure as policy makers intended, it would be more effective if the decrease is more pronounced for (a) more expensive drugs, (b) drugs that have a history of making larger payments, and (c) physicians that tend to prescribe more drugs. The idea is that if payments lead to biased treatments that can drive up healthcare costs, greater reduction among these subclasses of drugs and physicians would magnify the positive impact of disclosure. However, my results suggest that the decline in payments as a result of disclosure is lower for more expensive drugs and for drugs that were paying more prior to the disclosure. Moreover, the decline in payment is smaller among physicians who were

paid more heavily pre-disclosure. Interestingly, these physicians with smaller treatment effects (and larger pre-treatment payments) also tend to be heavier prescribers. Thus, while information disclosure did lead to reduction in payments on average (as intended by policy makers), the effect is muted for heavy prescribers.

As noted earlier, the ideal scenario envisioned by the policy makers was that public disclosure of payments would result in increased scrutiny from patients and payers, thereby resulting in a reduction in payments (Pham-Kanter, 2014). However, this mechanism cannot explain the increase in payments among 14% of drug-physician pairs in my sample. Furthermore, if public scrutiny results in differential impact across drugs and physicians, I should expect greater reduction in payments among more expensive drugs and physicians that receive more payments. However, my results are not consistent with this either. Therefore, I argue that increased scrutiny from patients and payers is unlikely to have been the main driver of the effect of disclosure. The second mechanism wherein physicians have increased ability to compare payments received by their peers from the same drug should have resulted in overall decrease in disparity in payments made by a drug across physicians (see the discussion on CEO compensation disclosure in Gipper 2016). Furthermore, I find that the disparity in payments made by the same drug to different physicians operating in a market increased after disclosure. Together, these results do not support the second mechanism of physicians seeking payment parity subsequent to disclosure.

My third plausible mechanism is related to firms responding to information about the marketing efforts of their rivals. In this regard, I find that within-physician payment disparity across brands increases by 4% as a result of the information disclosure, with the low-pay brands reducing payments more than the high-pay brands. This pattern is consistent with the idea that firms respond to information about competitive payments by trying to differentiate themselves, further investigation is needed to pin down the reasons leading to this change.

My essay takes the first step towards shedding light on the role of public disclosure in alleviating conflict-of-interest in the pharmaceutical industry, especially in reducing payments made by pharmaceutical firms to physicians. While public disclosure of payment information led to a reduction in overall payments, the effect is muted for highly paid physicians, who also tend to be heavier prescribers. If these physicians are the intended targets of the disclosure policy, regulators may need to re-evaluate whether the benefit from the intervention justifies its costs. This essay also contributes to understanding the consequences of information disclosure about competitor strategies in other settings. As Federal Trade Commission is pushing for sponsorship disclosure

for digital content (e.g. Sahni and Nair, 2016, Aribarg and Schwartz, 2017, Edelman and Gilchrist, 2012) in social influencer/Key Opinion Leader (KOL) marketing (e.g. among fashion bloggers and TV celebrities; see Hwang and Jeong, 2016, Boerman et al., 2017, Carr and Hayes, 2014, Dekker and van Reijmersdal, 2013), my findings provide a preview of the consequences of such policies.

The rest of the essay proceeds as follows. Section 3.2 introduces the institutional background of the federal policy and describes the data. Section 3.3 discusses the research design, sets up the causal inference framework, and introduces the Causal Tree and Forest estimation. Section 3.4 reports the results. Section 3.5 discusses the plausible mechanisms. In Section 3.6, I present some concluding comments.

3.2 Data and Institutional Background

3.2.1 Physician Open Payments Program

The federal Open Payments Program, also known as the "Sunshine Act," was introduced as part of the Affordable Care Act in 2010. It is a national disclosure program operated by the Center for Medicare and Medicaid Services (CMS) to promote transparency in healthcare marketing practice. Specifically, it requires full disclosure of data on the financial relationships between the healthcare industry (pharmaceutical and medical device companies operating in the US) and healthcare providers (physicians and teaching hospitals) on a publicly accessible website.[5] These financial relationships include any payments and transfers for marketing (food & beverage, speaker fees, consulting, travel & lodging, and gifts & entertainments), physician ownership and investment interests, and research purposes. Per the disclosure law, payments that were $10 or more in value needed to be reported.[6] The published data include the identities of the payment recipients (name, professional degree, specialty, and address) and the paying firms (name and address), date of payment, associated product, payment amount, and nature of payment. Open Payments is the **only** national resource of its type for beneficiaries, consumers, physicians, and industry.[7]

The timeline of the data collection and reporting procedure is as follows:

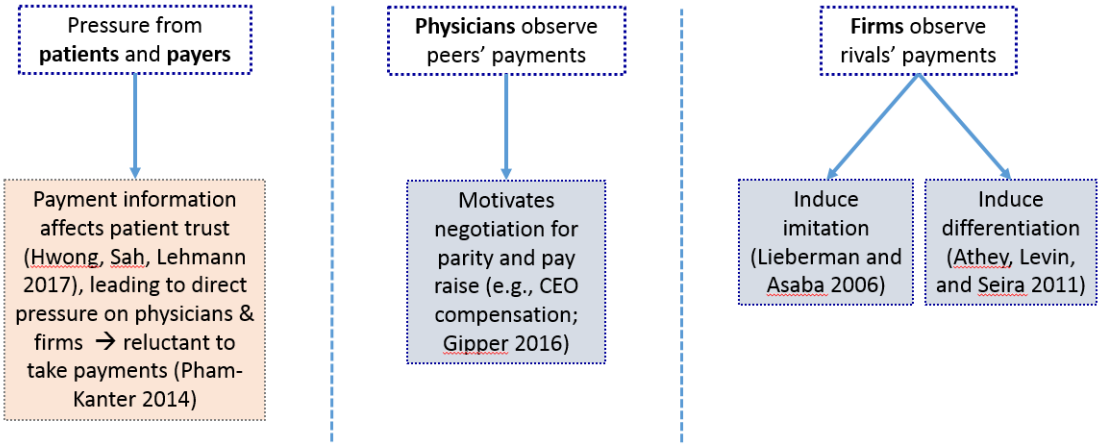
- On August 1, 2013, pharmaceutical firms and device manufacturers started collecting the required data;

[5] https://www.cms.gov/OpenPayments/Downloads/Open-Payments-User-Guide.pdf

[6] When the total annual value of payments or other transfers of value is more than $100, all payments need to be reported irrespective of the value of individual payments.

[7] https://www.cms.gov/openpayments/about/open-payments-data-in-context.html

Figure 3.1: Potential Mechanisms of Disclosure in Changing Payments.



Note: First box is the scenario envisioned by the policy makers.

- By March 31, 2014, manufacturers reported the data between August 1 to December 31, 2013 to CMS. Physicians and teaching hospitals who have covered records were invited to review the data and dispute any questionable records;
- On September 30, 2014, CMS reported the 2013 payment information on its Open Payment Program website for the first time;
- On June 30 of the following years, CMS will report payment information from the previous calendar year on its website.

Prior to the federal regulation, several states had enacted similar disclosure laws.[8] Among them, three states (MA, MN, VT) made these data publicly available before 2013. While the federal regulation preempts any State or local laws requiring reporting of the same types of financial information, the added effect of disclosure under federal regulation is more limited among these three states than in other states without prior disclosure law. Thus, I compare physicians from other states to physicians from these three states to estimate the effect from payment disclosure. I discuss this in detail in Section 3.3.

3.2.2 Effect of Disclosure on Subsequent Payments

There are three main avenues by which disclosure of payment information could alter subsequent payments (please refer to Figure 3.1 for a graphical representation of the three mechanisms). First, the main motivation behind the disclosure law was that increased transparency will be beneficial in reducing questionable financial relationships between the pharmaceutical industry and physicians. The ideal scenario envisioned by the law was that increased public scrutiny by patients and payers (insurance companies and the government) will render physicians reluctant to accept payments and firms less willing to offer them (Pham-Kanter, 2014; Chen et al., 2016). As Pham-Kanter, 2014 notes, such a tacit penalty for financial transactions between firms and physicians would arise if (a) patients and payers are aware of the disclosure law and are sufficiently motivated to seek this information, (b) they are sufficiently put off by such transactions, and (c) they can act on this negative reaction by imposing some costs on physicians and firms. As a result, there should be a decrease in payments.

Although this was the scenario envisioned by lawmakers, it is not clear if it would have been realized. While I lack credible information on the extent to which the general population is aware of the payment information (i.e. (a)), there has been increased media coverage on this issue subsequent to disclosure. While Hwong et al. (2017) find in an experiment with 278 participants that disclosure of industry payments to physicians affected perceptions of individual physician honesty and fidelity, it did not affect trust ratings for the medical profession or the pharmaceutical industry. Finally, for the public scrutiny mechanism to be effective, patients and payers should be able to sanction physicians for participating in questionable financial transactions. On the patient side, they should be able to credibly signal that they can switch away from physicians with questionable transactions. This might be hindered if patients experience considerable search and switching costs (e.g., fewer available physicians within their insurance network). On the payer side, insurers could decide to reimburse only doctors who do not accept payments and drop those that do from their network. Such a threat would be credible only if insurance companies have significantly more bargaining power than the network of physicians.

The second mechanism is based on the idea that healthcare providers can observe payments received by their peers from the same paying brand. This can lead to compar-

[8]These states include: Maine (2004), West Virginia (2004), Minnesota (1993), Massachusetts (2008), Vermont (2001), and District of Columbia (2003). California instituted gift bans to healthcare providers but did not require disclosure of the payments. As discussed subsequently, I exclude these states in my empirical analysis.

ison among peers regarding payments made by the same firm. The CEO compensation disclosure literature sheds some light on the consequence of making such compensation information public. This literature suggests that observing peers' income could trigger negotiations for payment parity (e.g. Gipper 2016), driving up the payments they receive in the future. Borrowing from this logic, I expect payments to increase if the physician comparison is the major consequence of the disclosure. Furthermore, such comparison is likely to result in a decrease in payment disparity across physicians for the same drug.

The third mechanism invokes the idea that disclosure of payments would enable competing firms to observe which physicians are targeted by their rivals as well as the magnitude of such patronage. Note that prior to public disclosure, such competitive information is generally not available to firms. They can respond to such competitive information in two alternative ways. In the first scenario, disclosure can result in a bidding war, wherein firms end up imitating one another (Lieberman and Asaba, 2006). Low-paying firms might be motivated to catch up with rivals to stay competitive, while high-paying firms might realize that they are overpaying certain physicians and reduce their payments to match those of their rivals. Thus, imitation by rival firms can either lead to an increase or decrease in payments depending on the relative strength of these two effects. In the second scenario, firms could choose to differentiate themselves (Athey et al., 2011) by focusing on certain physicians where they have competitive advantage. They can increase (decrease) payments among physician-drug pairs with stronger (weaker) financial relationships. This can result in a net increase or decrease in payments to the physician subsequent to disclosure depending on which force is more dominant.

3.2.3 Data Description

3.2.3.1 Anti-diabetics market

Diabetes has become a prevalent public health concern worldwide. As of 2015, 30.3 million Americans - 9.4% of the US population - had diabetes.[9] As a result, anti-diabetic medications have become one of the fastest-growing categories in the pharmaceutical industry. Consequently, rival pharmaceutical firms have introduced many new drugs into the market, bringing in intensive competition, thereby drawing generous marketing expenditures. The dynamics in this market makes it a good test field to study the

[9]National Diabetes Statistics Report, 2017. See https://www.cdc.gov/diabetes/pdfs/data/statistics/national-diabetes-statistics-report.pdf

effect of payment disclosure on subsequent payments between firms and physicians.

Anti-diabetics is a large category of drugs that treat diabetes mellitus by lowering glucose levels in the blood. Frequently used anti-diabetics include: (1) insulin (a hormone that stimulates glucose uptake and lipid synthesis), (2) agents that increase the sensitivity of target organs to insulin (sensitizers), (3) agents that increase the amount of insulin secreted by the pancreas (secretagogues), and (4) agents that decrease the rate at which glucose is absorbed from the gastrointestinal tract (glycosurics).[10] Depending on the nature of the diabetes, different subclasses of the anti-diabetics are used. For type 1 diabetes caused by the lack of insulin, regular insulin injection is the only valid treatment. For type 2 diabetes (the most common type of diabetes, T2DM) that is caused by cells' resistance to insulin, multiple treatments can be used. Metformin is the first-line medication for the treatment of type 2 diabetes by improving insulin sensitivity (i.e., Sensitizers). It is available as a generic with zero marketing payments and therefore excluded from my sample.

The 16 brands in my data fall into four subclasses of anti-diabetics: insulin, Glucagon-Like Peptide-1 (GLP-1) receptor agonists (secretagogues), Dipeptidyle Peptidase4 (DPP-4) inhibitors (secretagogues), and Sodium-Glucose Co-Transporter-2 (SGLT-2) inhibitors (glycosurics). Both GLP-1 receptor agonists and DPP-4 inhibitors are lower blood glucose by increasing insulin release from the pancreatic β cells. Thus, the four sub-classes of anti-diabetics in my sample include two types of secretagogues: GLP-1 and DPP-4. DPP-4 inhibitors block the action of DPP-4, an enzyme that inactivates incretins (such as GLP-1) that help the body produce more insulin. On the other hand, GLP-1 receptor agonists bind to the membrane GLP-1 receptor, preventing uptake of GLP-1 from the blood. This raises the level of blood GLP-1, stimulating insulin secretion and suppressing glucagon secretion (Drucker and Nauck, 2006). SGLT-2 inhibitors are a new group of oral medications used for treating type 2 diabetes, approved in 2013. They inhibit the sodium-glucose transport proteins (SGLT-2) that help re-absorb glucose into the blood, and pass out the excess glucose as urine (Kalra, 2014).

Except for Metformin (excluded from my data as a zero-payment generic), no single brand is highlighted according to the Standards of Medical Care in Diabetes (American Diabetes Association 2015). Overall, physicians have discretion in the drug they prescribe within a subclass of anti-diabetics. Thus, manufacturers are motivated to influence physician prescription decisions through providing monetary incentives.

[10] https://en.wikipedia.org/wiki/Anti-diabetic_medication

3.2.3.2 Payment Data

I downloaded the payment data for the 29-month period between August 2013 and December 2015 from the Physician Open Payment website in mid-2016. These data contain the dollar value of the gift/payment that transpired between a named physician and a named company, the nature of payment (e.g. meals, travel, consulting, and speaker fees), the associated products for which the payments were made, and the date of payment (see section 3.2.1). Note that my sample does not include any research-related expenditure. For my analysis, I aggregated the data to the level of a month for each physician-drug pair. As mentioned earlier, I focus on the anti-diabetics market.

In order to guarantee that there is enough information prior to disclosure to control for physician-drug idiosyncrasies, I exclude one-off financial ties and focus on the monthly payment sequences between physician-drug pairs who have paid at least twice before disclosure. The 16 anti-diabetics (marketed by nine firms) included in my analysis satisfy three conditions: they have been paying physicians at least 10 times in the observation window, at least two months before the 2014 disclosure and have at least ten physicians from the three states with prior disclosure laws (MA, VT, MN).[11] In addition to the payment data, CMS provides a separate dataset on the profile of all paid physicians, including their full name, physical addresses, licensed states, and specialty. For each physician, I obtained their National Provider Identifier (NPI), gender, medical school graduation year, the size of the associated practice group, and the number of claims-based hospitals from the Physician Compare National Downloadable File.[12]

Table 3.1: Summary Statistics of the Payment Panel.

	N	Mean	Std. Dev.	Min	Max
Payment/physician-drug-month ($)	5,826,216	20.10	356.04	0	97,004.84
29-month total USD/physician ($)	49,999	2,341.89	20,128	0.70	1,103,088
29-month total USD/drug ($)	16	7,318,421	9,981,005	286,325.3	30,657,472

My final sample consists of $118 million monthly payments from 16 branded anti-diabetics to 49,999 physicians between August 2013 and December 2015 who have been actively prescribing during the observation window. The average monthly payment between a drug and a physician is $20, with the highest payment reaching $97,004

[11] This is to guarantee that I have enough control units to match to the treated units. I will discuss it in more details in the research design section.

[12] https://data.medicare.gov/data/physician-compare. I match physicians based on first name, last name and state. The matching rate is 77.4%.

(Table 3.1). From August 2013 to December 2015, an average physician received $2,342 from all paying drugs, while an average drug paid $7.3 million to all physicians in the sample (Table 3.1). In my sample, six major payment purposes are recorded in the nature of payment field: consulting fee, speaker events, food and beverage, education, travel and lodging, and honoraria (Table 3.2). While payments for physicians serving as a speaker cost firms the most money (63% of the total dollar amount), payments for food and beverage covers more than 98% of the physician population in my sample (49,085). Given the sparsity of the payment events other than for food and beverage, I aggregate payments of different nature to physician-drug-month level.

Table 3.2: Share of Payment Nature in 29-Month Window.

Nature of Payment	Dollar Share	Payment Recipients
Speaker Fee*	62.7%	1,124
Food and Beverage	23.6%	49,085
Travel and Lodging	7.24%	1,152
Consulting Fee	5.8%	1,191
Education	0.69%	13,991
Honoraria	0.02%	23

*This includes serving as a faculty or a speaker at a venue other than a continuing education program.

3.2.3.3 Supplemental Data

I supplement the payment data with *annual data on prescriptions for each physician* from the Medicare Part D Prescriber Public Use File (PUF).[13] The dataset records the total number of prescriptions that were dispensed at the physician-drug-year level under the Medicare Part D Prescription Drug Program. Each physician is identified by their National Provider Identifier (NPI) and each drug is identified by its brand name and generic name. The data also contain the total drug cost, consisting of "the ingredient cost of the medication, dispensing fees, sales tax, and any applicable administration fees and is based on the amount paid by the Part D plan, Medicare beneficiary, government subsidies, and any other third-party payers."[14] Note that prescriptions covered under

[13]The Part D Prescriber PUF is based on information from CMS's Chronic Conditions Data Warehouse, which contains Prescription Drug Event records submitted by Medicare Advantage Prescription Drug (MAPD) plans and by stand-alone Prescription Drug Plans (PDP). https://www.cms.gov/Research-Statistics-Data-and-Systems/Statistics-Trends-and-Reports/Medicare-Provider-Charge-Data/PartD2013.html

[14]https://www.cms.gov/Research-Statistics-Data-and-Systems/Statistics-Trends-and-Reports/Medicare-Provider-Charge-Data/PartD2013.html

Medicare Part D represent only a part of all prescriptions written by each physician. In addition, these physician level prescriptions are available only at the annual level. I divide the total drug cost by the total prescriptions of that drug to obtain the unit price in 2013.

Since the disclosure law came into effect in the middle of the calendar year, I cannot cleanly match these data to the pre- and post-regimes even at the annual level (recall that my pre-disclosure payments data start in August 2013 and the treatment occurs mid 2014). However, these data are useful to me in three ways. First, I match physicians based on their pre-disclosure prescription pattern (i.e., total 2013 prescriptions) in each of the subclasses of anti-diabetics (I lump all other prescriptions into an "other" group). Second, I investigate whether the treatment effects differ across physicians in terms of their prescribing patterns. Third, I use these prescription data to perform some robustness checks as detailed in Section 3.4.2.1.

Table 3.3: Summary Statistics of Key Drug and Physician Characteristics.

	N	Mean	Std. Dev.	Min	Max
Drug Age in 2014	16	6	5.65	0	18
2013 unit price/drug ($)	14*	367.69	76.87	263.07	567.47
2013 total Rx/drug (log$)	16	9.31	4.61	0	14.06
Pre-disclosure total payment/drug (log$)	16	14.44	1.29	12.28	16.43
Parental Company on ProPublica	16	0.563	0.512	0	1
Female	49,999	0.292	0.454	0	1
Physician Graduation Year	49,999	1989	11	1933	2015
Size of associated practice group	49,999	101	218	0	5,616
2013 total Rx/physician	49,999	6,610.5	6,592.1	11	150,475
2013 anti-diabetics Rx/physician	49,999	508.6	572.6	0	10,299
#paying drugs pre-disclosure/physician	49,999	4	3	1	16

*There is no 2013 price data for Tanzeum and Farxiga as they entered market in early 2014.

I summarize the key drug and physician characteristics constructed in the sample in Table 3.3. In 2014, the 16 drugs have been on market for an average of six years. Of these, Humalog is the oldest and Farxiga is the newest. During 2013, the average unit price per drug is $367, based on the amount paid by the Part D plan. Nesina is the cheapest drug at $263 per claim, and Victoza is the most expensive among all ($567 per claim). In 2013, an average anti-diabetic in the sample was prescribed 11,048 times in the nation, with Lantus the most frequently used. I include additional information regarding the manufacturer, the drug subclass and the initial approval date of each drug, please refer to appendix Table B.1.

As of 2015, these physicians had mean of 26 years of experience after graduating

from the medical school, although there is considerable heterogeneity in experience (standard deviation of 11 years). On average, these physicians are associated with a group practice containing 100 other physicians, but some physicians do practice alone. In 2013, an average physician filed for 6,610 prescription claims in Medicare Part D, among which 508 (7.7%) were for anti-diabetics. During the 14 months before disclosure, while some physicians were paid by all the 16 drugs that I consider, an average physician was paid by four drugs.

3.2.3.4 Descriptive Analysis

I begin by comparing the four drug classes in terms of their payments made to physicians, number of prescriptions, and price. I report these statistics in Table 3.4. Insulins are the most frequently prescribed anti-diabetics, with the lowest unit price and payments towards the physicians in my sample. DPP4 Inhibitors are the second most prescribed anti-diabetics, spending the least in persuading these physicians. GLP1 receptor agonists, on the contrary, are part of the most expensive subclass. At the same time, they have the largest expenditure towards gifts/payments to physicians. However, they have fewer prescriptions than insulins and DPP4 inhibitors. SGLT2 inhibitors are the newest anti-diabetics initially approved in 2013. They receive the fewest prescriptions but have the highest per-drug expenditure, plausibly mostly due to their newness.

Table 3.4: Subclass Comparison in the Sample.

Sub-Class	29-Month Pay ($)	Pay/drug ($)	2013 Rx	Unit Price 2013 ($)	N drugs
Insulin	17,624,346	3,524,869.2	2,621,476	375	5
DPP4 Inhibitors	14,358,327	2,393,054.5	659,225	316	6
GLP1-R Agonists	50,712,054	16,904,018	184,543	517	3
SGLT2 Inhibitors	34,400,006	17,200,003	9,955	342	2

Building on these preliminary data patterns, I investigate if there is a relationship between pre-disclosure payments and the prices of the drugs. I present a scatter plot of the pre-disclosure payments made by the drugs and their prices in Figure 3.2. These results suggest that more expensive drugs are significantly associated with higher payments to physicians during the 14 pre-disclosure months. Hence, if there is concern that physician payments might influence prescriptions, this data pattern suggest that payments can potentially increase the cost of healthcare by shifting to more expensive treatment options. Further, I wanted to explore if there is a relationship between the total number of prescriptions written by a physician and the payments that she

receives. To this end, I divide physicians into five quintiles based on the total dollars they received from all paying drugs during the 14 pre-disclosure months. As is shown in Figure 3.3, heavier prescribers were paid more (with the payment quintile increasing from 1 to 5). This is consistent with firms allocating more resources towards the heavier prescribers in their direct-to-physician marketing (Manchanda et al., 2004). Together, I find that 1) more expensive drugs tend to spend more on physicians and 2) heavier prescribers tend to be paid more by firms. Therefore, conflict-of-interest, if it exists, is likely to be more profound among these expensive drugs and heavy prescribers, thereby raising concerns about the role of payments in driving up the cost of healthcare. Therefore, if the objective of disclosure is to reduce subsequent payments, it would be desirable to have greater effect for more expensive drugs and among heavier prescribers.

Figure 3.2: Correlation between Pre-Disclosure Payments and 2013 Drug Price.

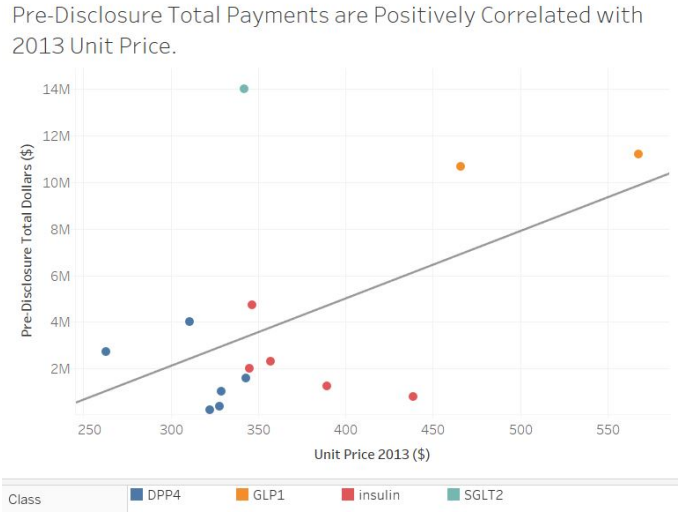
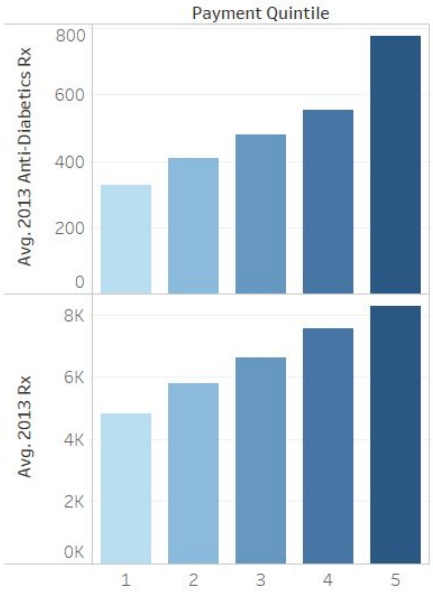


Figure 3.3: Relationship between Physician Payment and Physician Prescriptions.



3.3 Research Design and Methodology

Recall that my objective is to estimate the causal effect of public disclosure of payments on subsequent payments. In order to accomplish this, I need to know how much each physician would have been paid by a drug company if the payment information had not been disclosed. I can then compare this against the realized payments post disclosure to infer the causal effect of the policy intervention. Since I do not observe the same physician-drug pair under both "disclosed" and "non-disclosed" conditions at the same time, I cannot make this comparison directly. Furthermore, given my context, randomized experiments are not feasible. Therefore, I have to approach this causal inference problem using observational data.

As I discuss below, my empirical setting constitutes a quasi-experiment in that I have an exogenous shock due to the federal policy intervention that influences the states differently. Specifically, while most of the states did not institute any payment disclosure laws before the federal intervention in 2014, certain states (MA, VT, MN) did have similar state disclosure laws that made the industry payments received by in-state healthcare providers public. All stakeholders in these states could therefore see how each physician was pursued by rival firms even before the date of federal disclosure. However, in states without prior disclosure regulation, this information was not available until the date of federal disclosure. This variation in the level of information across states creates a quasi-experimental setting to study how subsequent

payments will change due to provision of payment information. In my case, the control group consists of physicians from the states with prior disclosure laws, while the treated group consists of physicians from the states without prior laws. The treatment under investigation is whether the firms and physicians receive any new information from the federal payment disclosure in 2014. Note that although Maine, West Virginia, District of Columbia, and California did not have prior public disclosure laws, they had instituted some form of restrictions on physicians receiving payments from industry sources. Therefore, I exclude these territories from my analysis.

My difference-in-difference research design addresses a common concern with before-after designs that payments might have changed over time as a result of factors other than the payment disclosure, thereby rendering a before-after comparison of payments in the treated states an invalid estimate of the treatment effect. For example, fluctuation in the macro-economic factors could have influenced payments. In addition, some market-specific characteristics could also have contributed to the temporal changes in payment. As discussed in section 3.2.3.1, anti-diabetics is a heavily R&D-active category with new medications being constantly approved. Since new drugs are usually launched nationally and influence the control and treated states at the same time, this effect could be captured by the payment changes in the control states. While these unobserved factors remain the same across all the states, by implementing a diff-in-diff comparison, I exploit the fact that firms, physicians, patients, and payers from the treated states obtain more new information about payments from the federal disclosure than those in control states. Measuring the payment changes in the treated states on top of the changes captured by the control states could give me a "cleaner" and more conservative estimate of the effect from payment disclosure.

As discussed earlier, by the end of 2015, the federal government had collected 29 months of payment data subsequent to the implementation of the disclosure law. Of these, 14 months were prior to the public disclosure of the payment data. Thus, I evaluate the effect of payment disclosure on subsequent payments by comparing the changes in payments between a physician-drug pair from the 14 months pre-disclosure to 15 months post-disclosure, across the control and treated groups. The identifying assumption in such a diff-in-diff design is that, without the payment disclosure, the average monthly payment in the treated and control groups follow a parallel trend before and after disclosure. That is, the federal payment disclosure is the only reason why treated and control groups would show different patterns around the time of this intervention. Therefore, the validity of such a diff-in-diff design hinges on the comparability of physicians from the treated and control states.

In Table 3.5, I compare the average monthly payment per drug-physician pair across the states with and without prior disclosure laws, before and after ACA disclosure. This comparison suggests that, both pre and the post payment levels in the states without prior laws appear to be much lower than those in the states with prior laws. Specifically, the average payment in the former is only about half of those in the latter. This raises the concern about the comparability of MA, VT, MN to all the other states as a whole. As a first step towards matching, I restrict the comparison to their neighbor states: CT (vs. MA), NH (vs. VT) and WI (vs. MN). For each state pair, their historical trends in the age-adjusted percentage rate of adult diabetes are highly similar to each other (See appendix Figure B.1). As can be seen from Table 3.5, row 4, the average payments in these neighbor states are closer to the payment levels in MA, VT and MN.

Table 3.5: Raw Diff-In-Diff Comparison of the Average Monthly Payment per Drug-Physician Pair

	Pre ACA Disclosure	Post ACA Disclosure	Post - Pre
States w/o prior laws	\$21.00	\$18.72	-\$2.28 (10.86%)
States w/ prior laws*	\$49.26	\$35.21	-\$14.05 (28.52%)
NoLaw- HadLaw	-\$28.26	-\$16.49	\$11.77 (41.65%)
Neighbor states w/o prior laws§	\$32.85	\$29.39	-\$3.46 (10.53%)
Neighbor NoLaw- HadLaw	-\$16.41	-\$5.82	\$10.59 (64.53%)

* States with prior disclosure laws were MA, VT, MN.

§The neighbor states to MA, VT, MN considered here are CT, NH, WI.

In months with no payment between a drug-physician pair, the payment is recorded as 0.

No fixed effects or physician-level matching. Percentage reported is relative to pre-disclosure level.

These patterns highlight the importance of matching physicians in the treatment states with appropriate counterparts in the control states while executing my diff-in-diff research design. In what follows, I discuss the general framework and conditions required for causal inference. I then describe various approaches to matching, including the setup of Causal Forest approach. As I discuss subsequently, the causal forest approach helps in estimating the average treatment effect as well as the heterogeneity in this effect across treatment units (physician-drug pairs in my case). Recovering heterogeneous treatment effects would serve two purposes. First, it would help policy makers evaluate the effectiveness of the regulation and identify potential avenues to improve effectiveness. Moreover, recovering heterogeneous treatment effects would help me uncover plausible mechanisms that potentially drove the treatment effect. This, in turn, would help me comment not only on *what* happened as a result of payment disclosure, but also potentially *why* it happened.

3.3.1 Alternative Approaches for Matching

3.3.1.1 Unconfoundedness and Conditional Average Treatment Effect (CATE)

I begin this discussion by considering the case of one drug. Extending this to the case of multiple drugs is relatively straight forward. For a set of *i.i.d.* physicians $i = 1, ..., n.$, I observe a vector of d covariates (features) $X_i \in \mathbb{R}$, a response metric $Y_i \in \mathbb{R}$, and a treatment assignment $W_i \in \{0, 1\}$. In my case, Y_i is the difference between the post-disclosure and pre-disclosure average monthly payment the physician received from the drug. W_i is the indicator for whether the physician is from the states without prior disclosure law. My X_i contains three sets of features: a) the full 14-month pre-disclosure payment sequences the physician received from all 16 drugs; b) the 2013 prescription patterns of the physician for anti-diabetics within each subclass as well as those for all other non-anti-diabetic medications, and c) physician demographics. Following the potential outcomes framework (Rubin, 1974; Imbens and Rubin, 2015), the causal inference problem in observational studies (e.g. the quasi-experiment in out case) can be expressed as estimating the conditional average treatment effect (CATE):

$$\tau(x) = \mathbb{E}\left[Y_i^{(W=1)} - Y_i^{(W=0)} \middle| X_i = x\right], \tag{3.1}$$

if the **unconfoundedness** assumption holds:

$$W_i \perp \left\{Y_i^{(0)}, Y_i^{(1)}\right\} \middle| X_i. \tag{3.2}$$

I can estimate the CATE by considering the nearby observations in the x-space as if these observations come from a randomized experiment. The idea is that these physicians are similar to each other in terms of their observable characteristics and can be considered as "clones", with the only difference being that a subset of them are from the treated states. The most common "clone-finding" method in the literature is the Propensity Score Matching (PSM, e.g. Hirano et al., 2003, Imbens and Rubin, 2015), which proceeds in two stages. The first-stage involves parametric estimation of the treatment propensity for each physician (i.e., belonging to the treated vs. control state) based on user-specified matching variables and the propensity function. In the second-stage, I can estimate the ATE by comparing the outcomes from the treated and the control physicians with the same estimated treatment propensity.

More formally, define $e(x) = \mathbb{E}\left[W_i \mid X_i = x\right]$ as the propensity to receive treatment at x. I am able to directly estimate CATE by inverse propensity weighting if I know

$e(x)$:

$$\tau(x) = \mathbb{E}\left[Y_i \left(\frac{W_i}{e(x)} - \frac{1 - W_i}{1 - e(x)} \right) \middle| X_i = x \right]. \tag{3.3}$$

The conventional practice in PSM is to impose parametric specification in estimating the propensity score, $e(x)$ (e.g. Hirano et al., 2003, Imbens and Rubin, 2015). However, when the space of covariates grows large, the conventional implementations that rely on kernel or series estimation of the propensity score become very cumbersome (Athey and Imbens, 2017). While there have been several revisions proposed to the way of propensity score estimation to deal with large number of covariates (e.g. McCaffrey et al., 2004), these weighting methods have been found to be sensitive to the exact implementation, e.g. using logit versus probit models (see Athey and Imbens, 2017). Moreover, the weights can change substantially for units with extreme values of propensity scores when using different specifications, thereby rendering the estimators less robust than those obtained from non-parametric methods.

3.3.1.2 Causal Tree and Forest

I address the concerns associated with propensity score matching through a forest-based algorithm (Causal Forest, CF) that is computationally efficient, robust to model mis-specifications, and achieves desired consistency and asymptotic normality when estimating the heterogeneous treatment effect at the individual level (Wager and Athey, 2017). Forest-based models (Breiman, 2001) are one of the most popular supervised machine learning methods in computer science and engineering, known for reliable "out-of-the-box" performance without much model tuning (Athey and Imbens, 2017). While mostly used for prediction tasks (e.g. Berk, 2012), these models have recently been adapted for causal inferences in marketing settings (e.g. to understand the effectiveness of online ad campaigns (Wang et al., 2015), and to identify customers at high risk of churning (Ascarza, 2016)).

At a high level, tree- and forest-based models help in finding the most similar observations locally in the covariate space with an adaptive neighborhood metric. This is different from the classical kNN methods, where the algorithm finds k nearest neighbors according to some pre-defined distance metric (i.e., a kernel). Trees (the component units of a forest) cluster the most similar observations into the same leaf (i.e. a terminal node), where the leaf is wider along the directions with higher degree of homogeneity and narrower along directions with higher degree of heterogeneity. All observations within the same leaf are considered to have $i.i.d.$ responses. This adaptive

nature of trees can substantially increase the power of accurate clustering with large space of covariates.

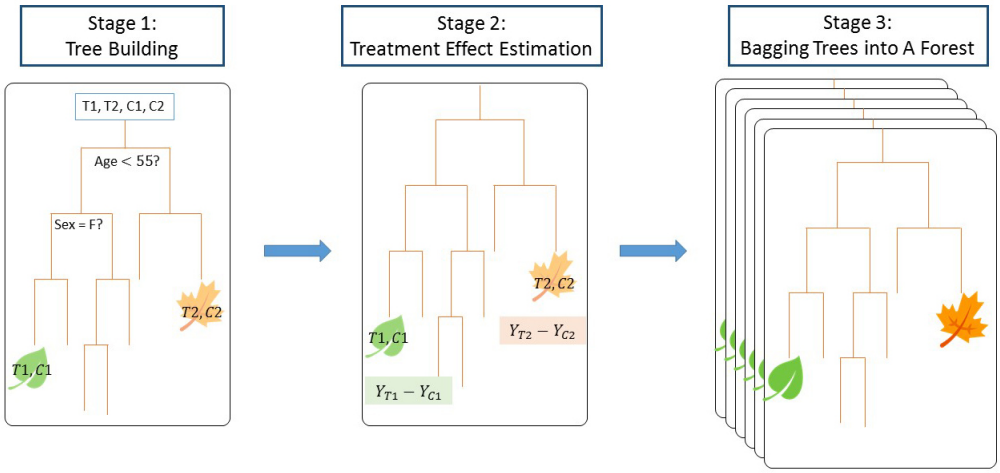
Researchers have developed forest-based methods that use a large number of trees as opposed to relying on a single tree. This is because a single tree might not represent the "best" characterization of the data, especially when the feature set is also large. Forest-based methods were shown to yield significantly better prediction performance in practice (Breiman, 2001). By constructing a "forest" of many independently grown trees[15] where each tree is grown using a random subsample of covariates (and in the case of Causal Forest, a subsample of observations), I can reduce the model variance and smooth sharp decision boundaries (Büchlmann and Yu, 2002).

Athey and Imbens (2016) adapt the classical tree model into a Causal Tree to estimate the within-leaf treatment effect as the difference between the means of $Y_{i\in l}^{(0)}$ and $Y_{i\in l}^{(1)}$ in that leaf l. A key revision in their algorithm is to separately build the tree structure and estimate the treatment effects given the structure using *i.i.d.* training samples. This separation of "tree building" and "effect estimation" steps is essential to obtain a consistent and asymptotically normal estimator for the treatment effects within leaves. They call this "honesty": a tree is "honest" if, for a unit i in the training sample, it only uses the response Y_i to estimate the within-leaf treatment effect $\tau_{i\in l} = \bar{Y}_{i\in l}^{(1)} - \bar{Y}_{i\in l}^{(0)}$, or to decide where to place the splits, but not both (Athey and Imbens, 2017; Wager and Athey, 2017).

In this paper, I estimate a forest version of the Causal Trees adapted for observational data ("propensity trees", see Wager and Athey, 2017). This method achieves honesty by first training classification trees for the treatment assignments W_i without using information in Y_i, and then estimating the within-leaf treatment effects using Y_i given the tree structure. The algorithm contains three stages (Figure 3.4): 1) building a propensity tree; 2) estimating the heterogeneous treatment effects within the tree; and 3) bootstrapping and aggregating ("bagging") thousands of trees into a forest to improve robustness and to obtain the confidence interval around the point estimate for each physician-drug pair.

[15]Causal Forest and Random Forest algorithms grow each tree independently and therefore can be easily paralleled. On the other hand, algorithms like gradient boosting adds new trees/classifiers based on how well the existing trees/classifiers do. In that case, trees are not grown independently.

Figure 3.4: An Illustration of the Three Stages in the Causal Forest Estimation.



Stage 1: Tree Building.

Let$|T|$ denote the number of leaves in a tree T; N_L denote the number of observations in a leaf L; $p_L = \frac{1}{N_L}\sum_{X_i \in L} I(W_i = 1)$ denote the proportion of treated observations in a leaf L; and $Q_L(T) = 2p_L(1-p_L)$ denote the impurity measure for leaf L. Given the data and a set of candidate covariates ("features"), the algorithm grows a classification tree T by repetitively partitioning the data into two subgroups along the covariate (and at the best cutoff if the covariate is continuous) that gives the largest drop in the total impurity of all the leaves (termination groups) produced after this partition. At each leaf, the algorithm will continue to partition the data until it reaches the termination criteria that there is at least k observations of *each* class in a leaf. Formally, at each split the algorithm solves a one-dimensional optimization problem by picking the covariate and the threshold at which to cut the data:

$$\min_{T} \; C(T) = \sum_{L=1}^{|T|} N_L Q_L(T), \tag{3.4}$$

$$s.t. \; |\{i : W_i = 1, X_i \in L\}| \geq k, \; |\{i : W_i = 0, X_i \in L\}| \geq k, \; \forall L \in T.$$

The parameter k can be formally picked by cross-validation.[16] I end up with a tree (i.e. "propensity tree") that partitions observations into subgroups, within which observations have the same propensity, p_L, to be treated. In other words, observations within the same leaf are considered roughly from a random experiment.

[16]In my empirical analysis, I tried different values of k and find that the estimation results are substantively the same.

Stage 2: Heterogeneous Treatment Effects Estimation.
Given the tree partitions, I can estimate the within-leaf treatment effects by taking the difference between the mean outcomes of treated and control units in the same leaf:

$$\hat{\tau}(x) = \frac{1}{|\{i : W_i = 1, X_i \in L\}|} \sum_{\{i:W_i=1, X_i \in L\}} Y_i - \frac{1}{|\{i : W_i = 0, X_i \in L\}|} \sum_{\{i:W_i=0, X_i \in L\}} Y_i. \tag{3.5}$$

Because the tree is grown by repetitively solving the one-dimensional optimization problem locally (thus the algorithm is "greedy"), each tree is grown in a deterministic manner. That is, given the same data input and the same feature set, you always end up with the same tree. This makes a single tree pretty sensitive to the outliers in the data as well as the irrelevant features supplied to the algorithm. In addition, the greediness of the algorithm does not guarantee any single tree structure to be globally optimal (because at each split the algorithm optimizes without incorporating the subsequent consequences of taking this split). To improve the robustness of the final estimation, I bootstrap and aggregate (i.e. "bagging") thousands of independently grown trees into a forest, which is shown to give much better model performance in practice (Breiman, 2001).

Stage 3: Bagging Trees into a Forest.
I build the Causal Forest in observational data by growing the propensity trees B times, each time with a random subsample of covariates and observations. The forest gives the final predictions by averaging over the estimates from B trees:

$$\hat{\tau}(x) = B^{-1} \sum_{b=1}^{B} \hat{\tau}_b(x), \tag{3.6}$$

where $\hat{\tau}_b(x)$ is the treatment effect estimate from the b-th tree. To measure the randomness in $\hat{\tau}(x)$ due to the training sample, I estimate the variance of the Causal Forest in the following way (Efron, 2014; Wager, 2014):

$$\hat{V}(x) = \frac{n-1}{n} \left(\frac{n}{n-s} \right)^2 \sum_{i=1}^{n} Cov \left[\hat{\tau}_b(x), N_{ib} \right]^2, \tag{3.7}$$

where $N_{ib} \in \{0, 1\}$ indicates whether observation i is used for the b-th tree, $\frac{n-1}{n} \left(\frac{n}{n-s} \right)^2$ is a finite-sample correction for forests grown by subsampling without replacement, and the covariance is taken with respect to all B trees in this forest. $\hat{\tau}(x)$ from eq.3.6 is shown to be asymptotically Gaussian and unbiased; and $\hat{V}(x)$ from eq.3.7 is proven

consistent (Wager and Athey, 2017).

Because a physician i can be clustered with different groups of physicians across different trees in the forest, I can construct a point estimate of treatment effect and the confidence interval for each i. A large variance estimate for i indicates a high level of variation in the outcomes from its neighbor observations across the trees, suggesting that the matching of i to its closest neighbors in x-space is less than ideal. With the model variance incorporated in the confidence intervals, I am able to determine which subgroups have significantly adjusted their payments responding to the information disclosure.

In practice, I estimate the individual-level treatment effects for each drug separately. I estimate the Causal Forest model for $Y_{id} = \frac{1}{T_1}\sum_t \log USD_{idt|t\in T_1} - \frac{1}{T_0}\sum_t \log USD_{idt|t\in T_0}$ for a given drug d,[17] which is the change in average monthly log payment between a physician i and the drug d going from pre-disclosure period (T_0) to the post-disclosure period (T_1). Using logs allows me to evaluate differences in payments across physicians and drugs in relative terms. Throughout the estimations for all drugs, I obtain point estimates for individual treatment effects using 5-fold cross prediction. That is, I train a Causal Forest model using four folds (80%) and predict for the fifth fold (20%). I loop over the 5 folds and obtain predictions for the whole sample. This procedure is implemented to avoid overfitting.

The covariate vector X_i contains three parts of features: 1) the 14-month drug-specific payment sequences physician i received from each of the 16 anti-diabetics in the sample; 2) the annual number of 2013 Medicare Part D prescriptions physician i wrote for every subclass of anti-diabetics, as well as all other medications as a whole; 3) physician demographics, including gender, medical school graduation year, the size of the affiliated group practice, and the number of claims-based hospitals associated with the physician. The idea is to find clusters of physicians who have similar payment sequences and prescription patterns prior to disclosure, and are demographically comparable.

3.4 Results and Discussion

I begin by discussing the baseline findings from the panel regression and propensity score matching estimation. I then present the results from Causal Forest estimation.

[17] In practice, the log transformation is done in the form of log(USD+1) to keep all zero payment observations.

3.4.1 Panel Regression and Propensity Score Matching

To obtain the basic idea of the average treatment effect of disclosure on subsequent payments, I regress the log payment received by physician i from drug d in month t onto the interaction between the treatment state dummy D_i and the post-period dummy $POST_t$.

$$logPay_{idt} = \beta_1 D_i * POST_t + I_{id} + I_{dt} + \epsilon_{idt}. \tag{3.8}$$

D_i equals 0 if the physician is from MA, VT, or MN and 1 otherwise. $POST_t$ equals zero if the month is between August 2013 and September 2014, and one otherwise. I control for differences across physicians and drugs in terms of payments by including physician-drug fixed effects (i.e., I_{id}), and allow for a different time trend for each drug by including drug-month fixed effects (i.e., I_{dt}). The main effects of D_i and $POST_t$ are absorbed by the physician-drug fixed effects and the drug-month fixed effects respectively and are thus omitted from the equation.

I report the panel regression results in Table 3.6. The coefficient on the interaction term, β_1, represents the estimate of the Average Treatment Effect (ATE). The results in column 1 are based on the analysis that includes all the treated states. The ATE estimate suggests a positive, yet insignificant effect of the disclosure on subsequent payments. A potential concern with this analysis is that physicians in the treated states may not be strictly comparable to those in the control states. Therefore, I attempt to "match" the two groups of physicians by considering those operating in states that are geographically proximate to the three states with prior payment disclosure laws. Specifically, I only include the neighbor states to MA, VT, and MN: CT, NH and WI. As discussed in appendix Figure B.1, these states demonstrate similar historical trends in the age-adjusted percentage rate of adult diabetes to MA, VT and MN respectively. I report these results in column 2 of Table 3.6. I see that with very basic comparability control (by looking at the similar neighbor states), the sign of the ATE changes to negative, although the effect continues to be statistically insignificant.

Next, I consider a more formal matching approach: propensity score matching (PSM). In the PSM estimation, I aggregate the payment data by physician-drug pair (in log form) and obtain the difference between the average log monthly payments post- and pre-disclosure as my dependent variable. The matching variables I consider in estimating the treatment propensity in the first-stage include: 1) physician demographics, such as gender, medical school graduation year, size of the associated practice group, and number of claims-based hospitals (where physicians practice); 2) drug

Table 3.6: Panel Regression Results.

	(1)	(2)
	All States	Neighbor States[§]
$D_i * POST_t$	0.0096	-0.016
	(0.0139)	(0.0174)
Observations	5,826,361	225,939
physician-drug FE	YES	YES
drug-month FE	YES	YES

*** p<0.01, ** p<0.05, * p<0.1. Robust standard errors clustered by physicians.
§Only the neighbor states (CT, NH, WI) to MA, VT, MN are included as the treated group.
The dependent variable is the log monthly payment per physician-drug pair.

characteristics, such as 2013 total prescription volumes per drug, 2013 unit price, pre-disclosure total payments to physicians (i.e., payments prior to disclosure), subclasses, and approval dates; 3) 2013 prescription patterns per physician, for each subclass of anti-diabetics and all non-antidiabetics (from Medicare Part D data); and 4) the pre-disclosure average monthly payment per physician-drug from each of the 16 drugs, for the last five months in 2013 and the first nine months in 2014 respectively. In addition, one can include the full sequence of the pre-disclosure monthly payments each physician received from all drugs when matching physicians. However, that will introduce 14 months*16 drugs = 224 additional variables into the estimation, making PSM infeasible. Instead, I attempt to account for the pre-disclosure payment trend in a very simple manner by including only two measures of the monthly pre-disclosure payment per physician-drug pair: the average monthly payment during the last 5 months in 2013 and during the first 9 months in 2014.

I report the PSM results using both logit and probit specifications in Table 3.7. The robust standard errors adjust for the fact that the propensity scores are estimated from the first-stage (see Abadie and Imbens, 2016). The results in Table 3.7 reveal that using a logit specification gives a large, positive and statistically significant average treatment effect at about 20%. However, when I change the specification to probit, the estimate becomes negative and insignificant (-9.5%). These estimates differ considerably from each other as well as from those in the panel regression results. These results highlight the concerns raised in the previous literature regarding the sensitivity of PSM estimation results to the implementation approach (see Athey and Imbens, 2017). The lack of robustness of PSM estimation calls for comparison across a variety of methods to assess the overall findings. In particular, a more robust matching algorithm under large covariate space is needed. As discussed earlier, the causal forest

Table 3.7: Propensity Score Matching Results.

	(1)	(2)
	All States, PSM-logit	All States, PSM-probit
ATE	0.190***	-0.095
	(0.0424)	(0.134)
Observations	182,825	181,694

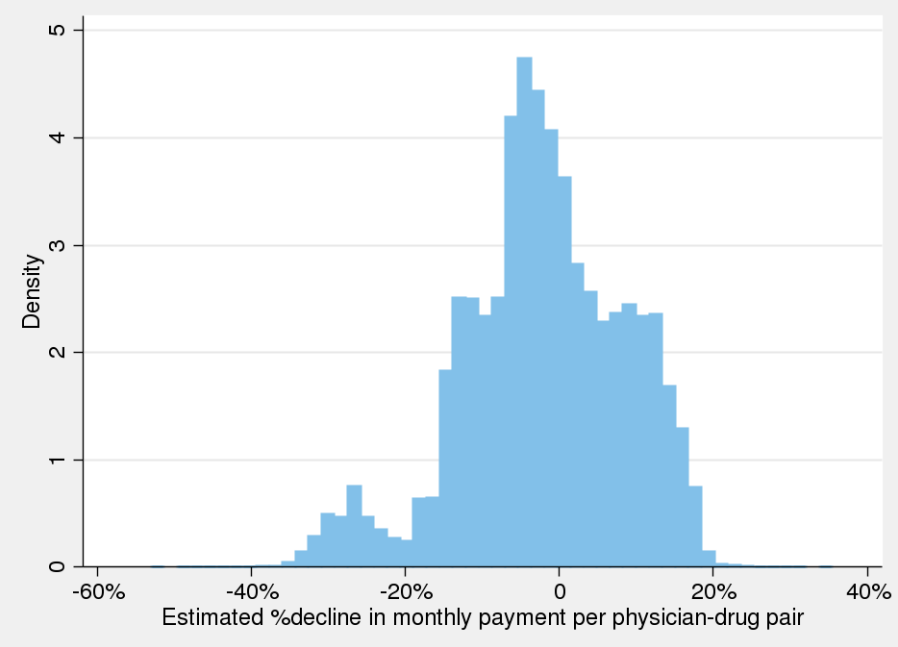
*** p<0.01, ** p<0.05,* p<0.1. Robust standard errors adjusted for the propensity score estimation. Dependent variable is the difference between the post and the pre average monthly payment (in log), per physician-drug pair. No time-serial variation is included in this case. Sample sizes are slightly different after dropping observations that violate the overlap assumption in each specification.

method addresses these issues.

3.4.2 Causal Forest Estimation Results

Recall that the CF estimation helps me uncover different treatment effects for each drug-physician pair. I plot the distribution of the estimated percentage change in the monthly payment for a physician-drug pair in Figure 3.5.

Figure 3.5: Distribution of the Treatment Effect per Physician-Drug Pair.



I also summarize the distribution of the estimated treatment effects at the individual level in Table 3.8. $\hat{\tau}_{id}^{pay}$ stands for the percentage change in monthly payments per drug-

Table 3.8: Summary Statistics of the Treatment Effects by Drug-Physician Pairs.

Monthly Payment Change	N	mean	std	min	max
$\hat{\tau}_{id}^{pay}$	199,085	-0.021	0.106	-0.530	0.354
$\hat{\tau}_{id}^{pay} \vert \hat{\tau}_{id}^{pay} < 0$ at 95% level	42,564	-0.156	0.074	-0.530	-0.022
$\hat{\tau}_{id}^{pay} \vert \hat{\tau}_{id}^{pay} > 0$ at 95% level	28,238	0.116	0.033	0.024	0.354
$\hat{\tau}_{i}^{GINI}$	38,949§	0.043	0.0045	0.026	0.060

§Change in Gini is only measured for physicians paid both pre and post.

physician pair. On average, across all physician-drug pairs, payments declined by 2% as a result of the disclosure. This result is consistent with those from the panel regression with naive matching by considering the neighboring treated and control states (-1.59%). Since the causal forest method enables me to make statistical inference of the treatment effect by each physician-drug pair, I can comment on significance of the treatment effect at this level of disaggregation. Overall, I find that 21% of the drug-physician pairs show a significant decline at 95% level (conditional mean = -15.6%), and 14% of the drug-physician pairs show a significant increase at 95% level (conditional mean = 11.6%).

I report the average payment changes by drug names (sorted by mean payment change) in Table 3.9. These results suggest that there is a large heterogeneity across drugs. For example, payments made by Novolog (insulin manufactured by Novo Nordisk) declined by 27% as a result of disclosure. On the other hand, payments made by Farxiga (new generation SGLT2 anti-diabetics, manufactured by AstraZeneca) increased by 12%. Next, I consider how the average treatment effect varies across drug-classes. I report these results in Table 3.10. These results suggest that GLP1-R Agonists increased their payments while the other subclasses mostly decreased their payments as a result of the disclosure.

Table 3.9: Average Change(%) in Monthly Payment Per Capita by Drugs.

Drug Name	N physicians	mean	std	min	max
NOVOLOG	10546	-0.274	0.035	-0.530	-0.163
OSENI	4409	-0.171	0.026	-0.405	-0.003
TRADJENTA	13722	-0.125	0.041	-0.420	0.023
NESINA	7780	-0.114	0.034	-0.199	0.166
INVOKANA	24356	-0.089	0.044	-0.336	0.355
ONGLYZA	4719	-0.049	0.027	-0.185	0.092
BYDUREON	22102	-0.048	0.023	-0.133	0.176
TANZEUM	2007	-0.039	0.024	-0.187	0.097
JANUVIA	9056	-0.030	0.020	-0.144	0.100
LANTUS	15727	-0.009	0.039	-0.301	0.312
JANUMET	4031	0.003	0.012	-0.133	0.081
APIDRA	5192	0.011	0.035	-0.211	0.116
LEVEMIR	30241	0.055	0.046	-0.238	0.250
VICTOZA	20779	0.071	0.057	-0.155	0.245
HUMALOG	9584	0.108	0.053	-0.039	0.210
FARXIGA	14156	0.119	0.038	-0.114	0.217

Table 3.10: Average Change(%) in Monthly Payment Per Capita by Sub-Classes.

Subclass	Mean
DPP4 Inhibitor	-0.088
SGLT2 inhibitor	-0.013
Insulin	-0.004
GLP1-R Agonists	0.007

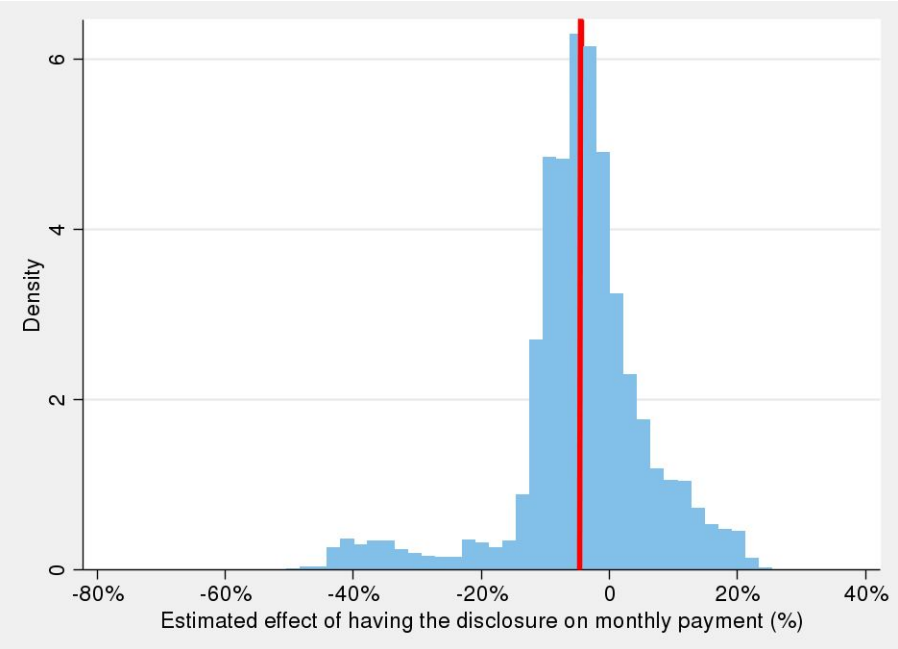
3.4.2.1 Robustness Checks

Matching based on Post-Disclosure Payments In my research design, I consider the states with prior disclosure laws as the control group because the agents in these states did not receive any new information subsequent disclosure. Correspondingly, when I match physicians in the treatment and control states, I use pre-treatment payments as a matching variable. However, a potential concern is that prior to disclosure, physicians in the treated and control states were in different disclosure regimes. Therefore, if disclosure did change payments, physicians that received similar payments

pre-disclosure in the treated states may not be strictly comparable to physicians receiving similar payments in the control states, which already had disclosure laws in place.

In order to address this concern, I match physicians in the treated and the control states based on post-disclosure payments when all physicians were subjected to the same disclosure regime. I then investigate if the payments received by these physicians in the treated states prior to the disclosure differed from those received by their counterparts in the control states during the same time period. The rationale is that subsequent to the ACA disclosure, both groups are under the same disclosure regime and are therefore more similar to each other. This "reverse diff-in-diff" comparison gives me an estimate of the effect from "not having the disclosure." By reversing the sign of this effect, I can recover the effect of "having disclosure." I plot the estimation in Figure 3.6. On average, the effect of disclosure as is estimated from this "reverse diff-in-diff" setting is -4.3%. Among all drug-physician pairs, 31% of the drug-physician pairs show significant changes, with the majority having the same sign/significance as my earlier findings. The substantive conclusions are similar to those presented earlier.

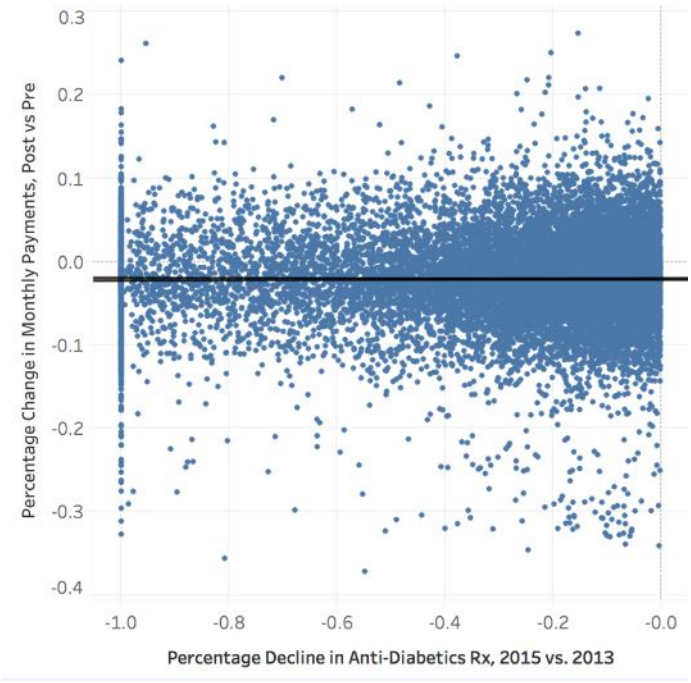
Figure 3.6: Distribution of the Treatment Effect per Physician-Drug Pair, Reversed Diff-In-Diff.



	Mean	Std. Dev.	Min	Max
Payment Change (%)	-0.043	0.108	-0.757	0.338

Relationship between Changes in Prescriptions and Payments A potential concern with my analysis is that the enactment of the disclosure law (rather than just the data disclosure that I intend to study) might have changed the prescription behavior of physicians. This, in turn, might have changed the payments made by pharmaceutical firms. If this change in prescriptions and the corresponding payment rules coincided with disclosure of payments, my estimate of the causal effect of disclosure is bound to be contaminated. In order to verify if changes in prescriptions is a potential confounder of the effect from disclosure, I check to see if there is a relationship between the change in payments and the decline in anti-diabetics prescriptions between 2013 and 2015 in the Medicare Part D data. I present the results from this analysis in Figure 3.7. These results suggest that there is no relationship between the decline in prescriptions and the corresponding changes in payments. Therefore, I do not believe that the treatment effects pertaining to the change in payments as a result of disclosure were contaminated by the concomitant decline in prescriptions.

Figure 3.7: Decline in Anti-Diabetics Prescriptions is Unrelated to the Decline in Payments.



3.4.3 Exploring Differences in Treatment Effects Across Drugs and Physicians

As discussed earlier, if the objective of policy makers while mandating payment disclosure was to deter inappropriate financial relationships between the firms and the physicians (Pham-Kanter, 2014; Santhakumar and Adashi, 2015), the policy would have been effective if it led to steeper decline in payments for (a) expensive drugs, and (b) physicians who wrote more prescriptions. Since I estimate the treatment effects for each physician-drug pair ($\hat{\tau}_{id}^{pay}$), I can characterize the heterogeneity in this effect across drugs and physicians. This can help me comment on the overall effectiveness of the policy. Below, via a series of generalized linear regressions, I investigate the relationship between the estimated treatment effects and drug and physician characteristics. This analysis accounts for the fact that the dependent variables (i.e., the treatment effects at the physician-drug level) are estimated rather than observed and thus have an associated standard error (Hanushek, 1974 and Eichholtz et al., 2010).

3.4.3.1 Relationship between Payment Changes and Drug Characteristics

I explore the relationship between the estimated treatment effect (payment) changes and drug characteristics using the following regression:

$$\begin{aligned}\hat{\tau}_{id}^{pay} = \alpha_1 LogPreUSD_d + \alpha_2 AvgPrePrice_d + \alpha_3 LogPreRx_d + \\ \alpha_4 ProPublicaFirm_d + \alpha_5 ApprovalDate_d + I_{subclass} + I_i + \epsilon_{id}.\end{aligned} \tag{3.9}$$

In essence, I regress $\hat{\tau}_{id}^{pay}$ onto the following set of covariates: total payment (to all physicians) made by the drug during the pre-period in log form ($LogPreUSD_d$), 2013 average drug price ($AvgPrePrice_d$), the pre-period total prescription level of the drug in log form based on the 2013 data from Medicare Part D ($LogPreRx_d$), drug approval dates, and a dummy for whether the parental firm has voluntarily disclosed any aggregate payments in the past on ProPublica, controlling for the subclass dummies $I_{subclass}$ (insulin, SGLT2, GLP1, with DPP4 to be the omitted baseline) and physician characteristics fixed effects I_i.

I report the regression results in Table 3.11. These results reveal that pre-disclosure payments and the average drug price have a positive effect on the treatment effect. Together, these results suggest that drugs with larger pre-disclosure payments to physicians were associated with an increase or smaller decline in payments after disclosure. Similarly, the effect of disclosure was muted among more expensive drugs.

Table 3.11: Relationship between Payment Changes and Drug Characteristics.

DV = $\hat{\tau}_{id}^{pay}$	(1)	(2)
LogPreUSD	0.00588***	0.00500***
	(0.000528)	(0.000665)
AvgPrePrice§	0.00110***	0.00119***
	(6.43e-06)	(8.46e-06)
LogPreRx§	0.00605***	0.00624***
	(9.71e-05)	(0.000127)
ProPublicaFirm	0.0372***	0.0364***
	(0.000505)	(0.000655)
ApprovalDate	3.15e-05***	3.45e-05***
	(3.59e-07)	(4.71e-07)
Intercept	-1.177***	-1.256***
	(0.00763)	(0.0101)
Observations	182,808	182,808
physician FE	NO	YES
drug subclass dummy	YES	YES
R2	0.264	0.388

*** p<0.01, ** p<0.05, * p<0.1. Robust standard errors clustered by physicians. Dependent variable is estimated treatment effect in payment per physician-drug pair. §Average price and pre-disclosure prescriptions measured via 2013 Medicare Part D data. I exclude Farxiga and Tanzeum as they do not have 2013 price data.

3.4.3.2 Relationship between Payment Changes and Physician Characteristics

I explore the heterogeneity across physicians within the same drug brand along three dimensions: a) the extent to which a physician was paid more heavily than other physicians prior to disclosure, b) the extent to which a physician prescribed more anti-diabetics than others prior to disclosure, and c) the extent to which a brand was leading or lagging behind other brands in pursuing a given physician prior to disclosure. Of these, (c) is a physician-drug characteristic in the sense that the same drug can be a leading patron for some physicians, but lag behind its rivals for other physicians. On the other hand, (a) and (b) are physician characteristics. Note that drugs did not have this information about competitor behavior in (c) at physician level prior to disclosure.

For (a), I construct the physician popularity metric (POP_i) as log of the total payments a given physician received from all anti-diabetics brands pre-disclosure. For (b), I use the anti-diabetics prescriptions (log) per physician based on the Medicare data for 2013. For (c), within each physician, I generate the quantile ranks for each paying drug based on their pre-disclosure payments' ranking order, varying between 0 and 1 and the larger the higher ranked.[18] These ranks reflect the extent to which payments from the brand are likely to be salient from the physician's point of view. Since there will be variation in drug ranks only among physicians that was paid by multiple drugs, I generate a "multipayer" dummy that equals 1 if the physician was paid by more than one drug and 0 otherwise. I run the following specification with physicians paid only by a single drug as the omitted baseline:

$$\begin{aligned}\hat{\tau}_{id}^{pay} = \alpha_1 POP_i + \alpha_2 Log2013AntiDiabetics_i + \alpha_3 Multipayer_i + \\ \alpha_4 Multipayer_i * Rank_{id} + I_{specialty} + X_i'\beta + I_d + \xi_{id}\end{aligned} \tag{3.10}$$

where i indexes physician and d indexes drug. Each drug is allowed a different constant term to capture the baseline differences across drugs (i.e. drug fixed effects). X_i are a vector of physician control variables including gender, medical school graduation year, size of the associated practice groups, and number of claims-based hospitals. I also include specialty dummies for Internal Medicine (omitted baseline), Family Medicine, Endocrinologists, other specialties, and dual specialties (Internal + Family). In the first specification, I include control variables for physician characteristics but exclude physician fixed effects to obtain the main effect estimates for the popularity metric,

[18]For example, for 3 drugs paying the same physician, the quantile ranks for them are 100%, 66%, and 33%, respectively.

the level of pre-disclosure prescriptions, and specialty dummies. In the remaining two specifications, I add physician fixed effects, but exclude physician characteristics that do not vary across drugs and check sensitivity of the results to the physician unobservables. I cluster the standard errors by physicians.

I report the results from this analysis in Table 3.12 with drug fixed effects included in all specifications. In column 1, I exclude physician fixed effects to obtain the main effect estimates from popularity metric, physician prescription level, and specialty dummies. The result indicates that more popular physicians (i.e., those receiving larger payments) and heavier prescribers in the anti-diabetics category tend to have larger increase/smaller decline in payments. Relative to physicians paid by a single firm, multi-payer physicians tend to have larger increase/smaller decline in payments. This positive effect increases as the drug moves up in the within-physician ranking (as is measured by its payment to the physician). This effect is robust to including physician fixed effects. In both columns 1 and 2, the positive significant effect on the interaction term between $Multipayer_i$ and $Rank_{id}$ suggests that the low-paying drugs tend to drop their payments more than the high-paying drugs regarding the same physician they pay.[19] When I allow this "rank" effect to vary by the degree of physician popularity (column 3), the result indicates that the "rank" effect is only positive for more popular physicians. In other words, the top paying drugs sustain their payments only to the popular physicians. Among less popular physicians, all drugs tend to reduce their payments.

In appendix B.1, I explore how the treatment effects vary by the nature of payment. I discuss the possible conjecture consistent with the data pattern.

3.5 Discussion on Mechanisms

As discussed earlier, there are three main mechanisms that could result in public disclosure of payments having an effect on subsequent payments. First, public disclosure could inform patients and payers (insurance companies and the government) about the financial ties between their doctors and pharmaceutical companies. This can render physicians less willing to accept payments. Second, physicians can see how much their peers are getting paid by the pharmaceutical company. This comparison could result in physicians seeking parity, which can lead to larger payments and plausibly lower

[19]The larger $Rank_{id}$, the higher rank the drug paid the physician relative to the other paying drugs to the same physician. The positive coefficient associated with $Rank_{id}$ suggests that if a drug ranks higher, it would show smaller decline/larger increase.

Table 3.12: Relationship between Payment Changes, Physician Popularity & Drug Ranking within Physician.

DV = $\hat{\tau}_{id}^{pay}$	(1)	(2)	(3)
Popularity	0.00276***		
	(0.000143)		
Log2013AntiDiabetics	0.000253***		
	(8.00e-05)		
Multipayer	-0.000571		
	(0.000605)		
Multipayer*Rank	0.00839***	0.00894***	-0.0266***
	(0.000365)	(0.000433)	(0.00273)
Multipayer*Rank*Popularity			0.00575***
			(0.000441)
Specialty Dummy§			
Family Med	0.000691***		
	(0.000247)		
Endocrinologists	-0.00957***		
	(0.000547)		
Internal + Family Med	-0.00338***		
	(0.000938)		
Others	0.00143***		
	(0.000320)		
physician control variables:			
Female	-0.000278		
	(0.000240)		
Medical School Graduation Year	0.000120***		
	(1.01e-05)		
Practice Group Size	8.88e-06***		
	(1.06e-06)		
Number of Hospitals	-0.000240***		
	(7.18e-05)		
Intercept	-0.249***	0.00780***	0.00816***
	(0.0202)	(0.000547)	(0.000545)
Observations	199,085	199,085	199,085
physician FE	NO	YES	YES
drug FE	YES	YES	YES
R2	0.856	0.903	0.904

*** p<0.01, ** p<0.05, * p<0.1. Robust standard errors clustered by physician.
§Internal Medicine is the omitted baseline.

disparity in payments across physicians for the same brand. Third, pharmaceutical firms can observe which physicians their rivals are patronizing. This information can either lead to firms trying to match their rivals or differentiating from each other by targeting different sets of physicians.

My data do not enable me to cleanly isolate the role of alternative mechanisms in driving the causal effect of the payment disclosure on subsequent payments. Nevertheless, since the three alternative mechanisms yield different predictions regarding the causal effect of the policy intervention, I attempt to comment on the most plausible mechanism that drove the outcome. In order to do so, I consider the scenario wherein the three mechanisms occur in isolation.

The first mechanism, based on increased public scrutiny, would predict a decrease in payments subsequent to disclosure. While payments indeed declined on average, 14% of physician-drug pairs in my sample show a significant increase in the payments post-disclosure. Moreover, if public scrutiny did lead to differential pressure among drugs and physicians, I would expect more negative effects on high priced drugs, high paying drugs, and highly paid physicians. Recall that my results suggest the opposite pattern. Together, these patterns do not appear to support the idea that public pressure was the dominant driver behind the effect of disclosure on payments.

The second mechanism based on physicians comparing payments received by their peers predicts 1) an increase in payments, and 2) a decline in the payment disparity across physicians in the same social comparison group. For one, this is not consistent with the statistically significant decrease in payments experienced by 21% of physician-drug pairs in my sample. To test this second conjecture, I define my social comparison group at the city level and measure the payment disparity across physicians who received payments from the same drug in the same city, pre- and post-disclosure. Specifically, for drug d paying N_d physicians in city c, I construct the Gini index during the period t for drug d in city c as: $GINI^t_{dc} = \frac{1}{N_d}(N_d + 1 - 2 * \frac{\sum_{i=1}^{N_d}(N_d+1-i)*Y_i}{\sum_{i=1}^{N_d} Y_i})$, where Y_i is the total dollar amount physician i received from drug d during period $t \in \{pre, post\}$. I compare this metric pre- and post-disclosure for the same city-brand. I find that the payment disparity across the physicians in the same city receiving payments from the same brand increased after the disclosure. Both pieces of evidence suggest that physician comparison is unlikely to be the dominant mechanism.

The third mechanism wherein firms can observe information on physicians targeted by their rivals predicts either an increase or decrease in payments as a result of disclosure. As a result, *prima facie*, this offers the most parsimonious description of the observed pattern of the causal effect. Within this mechanism, I further investigate

whether the effect was driven by firms imitating each other or differentiating from each other. Under the former, rival firms would converge to similar payment levels for each physician post disclosure, thereby reducing the variance in payments within each physician. On the other hand, differentiation would have led to an increase in variance of within-physician payments.

In order to understand how within physician payment disparity across drugs changed as a result of disclosure, I fit another Causal Forest using $Y_i = GINI_i^{POST} - GINI_i^{PRE}$ as the outcome variable, which is the change in Gini index[20] for a physician i. The Gini index measures the total disparity in payments that a physician receives across all drugs during the pre and post periods. A low value of the Gini index would suggest that all paying drugs patronize a physician almost equally. The estimate from the Causal Forest, $\hat{\tau}_i^{GINI}$, stands for the percentage change in the Gini index per physician due to the disclosure. This result suggests that the disparity in payments received by a physician across drugs increased for all physicians who were paid by multiple drugs (Table 3.8, last row). As a result, the payments a physician received from all paying drugs becomes 4% more unequal, on average. Therefore, these results suggest that the variance in payments received by each physician (as measured by the Gini index) increased for all physicians in my sample subsequent to disclosure. Therefore, I conjecture that disclosure of payments led to rival firms differentiating from each other by choosing different sets of physicians that they target.

As noted earlier, my data preclude me from ruling out the possibility that a combination of all three mechanisms drove the causal effect of disclosure. Therefore, my discussion presents a preliminary exercise in evaluating the role of the three mechanisms in isolation. Given the data I have, I favor firm differentiation as the main mechanism. I leave the formal test of mechanisms to future research.

3.6 Concluding Remarks

My essay studies the impact of making the industry payments to physicians public on the subsequent payments between firms and physicians, and to what extent it effectively solves the conflict-of-interest concern. I find that, on average, disclosing competitors' marketing information leads to a decline by 2% in the monthly marketing payment per physician-drug pair. Further investigation suggests that payments for heavier marketed

[20]For each physician i paid by N_i brands, her Gini index during period t is: $GINI_i^t = \frac{1}{N_i}(N_i + 1 - 2 * \frac{\sum_{d=1}^{N_i}(N_i+1-d)*Y_d}{\sum_{d=1}^{N_i} Y_d})$, where Y_d is the total dollar amount the physician received from drug d during period t.

drugs, as well as physicians who were more heavily paid by multiple drugs and were prescribing more anti-diabetics, tend to be less influenced by the disclosure. Meanwhile, the average payment disparity across brands within the same physician goes up, with the low paying brands dropping payments more frequently than the high paying brands. This pattern is consistent with the idea that firms respond to information about competitive payments by trying to differentiate themselves. As any reputation concerns that might impose negative pressure on physicians and firms have been incorporated immediately after the law introduction, firms and physicians could have already adjusted the payments downward before the actual data disclosure. Thus, I consider the 2% decline from my study a conservative estimate of the overall effect from the disclosure law.

My study takes the first step in answering an important question about the well-publicized disclosure law. However, data limitations leave some productive avenues for future research. First, given the nature of my data, I stop short of directly linking changes in monthly payments to changes in monthly prescriptions as I do not have access to the latter. This prevents me from evaluating the return on marketing payments towards physicians pre- and post-disclosure in this paper, and from investigating the mechanisms more thoroughly. Second, I do not have access to complete data on the firm side, e.g. data on other marketing expenditures and the cost of disclosure regulation. These data would enable me to comment on whether a change in payments is linked to a change in other marketing expenditure. Finally, I'm also unable to comment on the impact of disclosure on patient welfare, as I do not have access to health outcomes. I hope that future research can address these limitations.

CHAPTER 4

Summary and Outlook

Disclosure regulation has been chosen as a quick cure for Conflict-Of-Interest (COI) by policy makers frequently, thanks to its easy implementation and non-intrusive nature. The idea behind it is that making the COI information accessible to everyone generates the public pressure, pushing agents under the influence away from further questionable practice. However, the effectiveness of such regulation has been debated.

In my dissertation, I studied the disclosure of a specific form of information: marketing payments to physicians from pharmaceutical firms and their rivals. My results suggest that disclosure works, to a limited extent, in alleviating the COI in the industry-physician relationship. On the one hand, the number of prescriptions of both branded and generic drugs in Massachusetts drops with the relative magnitude of the drop being higher for branded drugs (Essay 1). On the other hand, the monthly payment received by a physician only declined by 2% on average, after the disclosure rolled out to all 50 states in the country (Essay 2).

The pattern of the results suggests that public pressure is not the only story behind the changes in behavior. Given that bigger drugs and heavier prescribers are the least influenced by the federal disclosure regulation, and that payments to the same physician become more disperse after disclosure, firms seem to differentiate from each other by allocating more resources towards physicians with whom they've already gained a favorable position. This suggests that players in the market might strategically leverage the information disclosed, which might lead to unexpected consequences from policy makers' perspective.

Is disclosure good or bad? To better answer the question, future research should draw upon additional datasets to better characterize the agents' behavior. To patients, what is the change to the health outcome? To physicians, what is the (quantifiable) penalty for COI practice? To firms, what is the benefit from learning about rivals' behavior? To regulators, what is the cost of disclosure regulation? Will it create privacy issues? Are there alternative strategies? I leave these questions to the readers.

APPENDIX A

Appendix to Chapter 2

In section A.1-A.4, I report additional robustness check results for chapter 2 with the following subsamples.

Table A.1: Summary of robustness checks.

Section	Sample used	Purpose
A.1	MA-CT border counties, zipcode average	corrects for parallel trends in pre-period
A.2	MA-NY border counties	alternative control group
A.3	MA-CT border counties, pre-period is Jan-Jun 2009	alternative pre-period§
A.4	MA-CT border counties, DV is log transformed	alternative data transformations¶

§Blue Cross Blue Shield introduced Alternative Quality Contract in Massachusetts starting 2009. While my data is from a different insurance company and should not be influenced by changes in BCBN, I check robustness of my findings using a pre-period that excludes the temporal point when the changes occurred.¶I apply log transformations to account for potential skewness of the distribution of residuals.

A.1 Generalized Synthetic Control Estimation.

Let Rx_{zt} indicate the average monthly new prescriptions written per physician in zipcode z and month t (1 to 48). I model the data generation process as:

$$Rx_{zt} = D_{zt}\delta_z + X_{zt}\beta + F_t\Lambda_z + \epsilon_{zt}$$

Where the treatment indicator D_{zt} equals 1 if zipcode z is from Massachusetts and t is after month 18, and equals 0 otherwise. δ_z captures the heterogeneous treatment effect for an average physician in zipcode z that is treated. X_{zt} includes a constant term and the average monthly patients per physician receiving any new prescriptions at the zipcode. It captures the time-varying demand shocks such as a local insurance plan

membership expansion. $F_t = [f_{1t}, ..., f_{rt}]'$ is a vector of r unobserved common factors (each have T values, T=48), with $\Lambda_z = [\lambda_{z1}, ..., \lambda_{zr}]'$ the (r x 1) vector of unknown factor loadings. Note that the treated and control units are influenced by the same set of factors, and the number of factors is fixed throughout month 1 to 48. However, each zipcode can have a different set of loadings on r factors. Note that zipcode fixed effects and time fixed effects can be considered as two special cases of the unobserved factors by setting $f_t = 1$ and $\lambda_z = 1$.

A.1.1 Model Estimation

The model is estimated following three steps (Xu, 2017):

Step 1: Obtain the estimated coefficients on X, the factors, and the loadings using only the control group data.

$(\hat{\beta}, \hat{F}, \hat{\Lambda}_{co}) = argmin \sum_{z \in co} (Rx_{zt} - X_{zt}\beta - F_t\Lambda_z)'(Rx_{zt} - X_{zt}\beta - F_t\Lambda_z), \quad z \in Control\ State$

$$s.t. F'F/T = I_r, \Lambda_z'\Lambda_z = diagonal.$$

Step 2: Given the estimated coefficients on X and the factors from step 1, obtain the factor loadings for each treated unit using only the pre-treatment period data.

$$\hat{\lambda}_z = argmin(Rx_{zt_0} - X_{zt_0}\hat{\beta} - \hat{F}_{t_0}\lambda_z)'(Rx_{zt_0} - X_{zt_0}\hat{\beta} - \hat{F}_{t_0}\lambda_z), \quad z \in MA.$$

Step 3: Calculate treated counterfactuals for the post-treatment period, based on $\hat{\beta}, \hat{F}, \hat{\Lambda}_{MA}$.

$$\hat{Rx}_{zt_1}(0) = X_{zt_1}\hat{\beta} + \hat{F}_{t_1}\hat{\Lambda}_z, \quad z \in MA.$$

Finally, I obtain an estimator for Average Treatment Effect on the Treated by averaging the differences in the observed and counterfactual outcomes across all zipcodes in MA during the post-treatment period:$\hat{ATT}_{t_1} = \frac{1}{N_{z \in MA}} \sum_{z \in MA} [Rx_{zt_1}(1) - \hat{Rx}_{zt_1}(0)]$.

The number of factors, r, is determined by a leave-one-out-cross-validation procedure in step 2. Intuitively, the algorithm iteratively goes through all pre-periods, holds back one period's data of all treated units in step 2, estimates the loadings, predicts the outcomes for the holdout sample, and obtain the mean squared prediction error (MSPE) for the treated units given r: $MSPE(r) = \sum_{s=1}^{T_0} \sum_{z \in MA} \frac{Rx_{zs}(0) - \hat{Rx}_{zs}(0)}{T_0}$. After trying a set of values for r, pick $r*$ that minimizes the MSPE for the treated units in the pre-periods.

A.1.2 Model Inference

The inference is done by constructing the variance and the confidence interval for $\hat{ATT}_{t_1}$through bootstrapping. The intuition follows the placebo test in the traditional synthetic control method: construct an empirical distribution of prediction errors for the GSC method, and evaluate if the true $\hat{ATT}_{t_1}$looks different enough from the prediction errors for the effect to be real.

Step 1: Simulate the prediction errors for the treated units.

At each iteration m, one control unit is randomly drawn to be the "pseudo-treated" unit (unit i), and the new control donor pool (of the same size as the original donor pool) for this "pseudo-treated" unit is generated by re-sampling with replacement from the remaining control units. Apply the GSC method and obtain the vector of residuals for this "pseudo-treated" unit: $\hat{\epsilon}^p_{(m)} = Rx_i - \hat{R}x_i(0)$, for iteration round m and pseudo-treated unit i. Do this for B1 times, and collect all B1 vectors of predictions errors: $\hat{\boldsymbol{e}}^{\boldsymbol{p}} = \{\hat{\epsilon}^p_{(1)}, ..., \hat{\epsilon}^p_{(B1)}\}$. This is the constructed sample of prediction errors for the treated units. Note that from the estimation part, I have already obtained the original set of residuals for the control units:$\hat{\boldsymbol{e}} = \{\hat{\epsilon}_1, ..., \hat{\epsilon}_{N_{co}}\}$.

Step 2: Construct bootstrapped samples of untreated outcomes and obtain $\hat{ATT}^{(k)}_{t_1}$.

Start a new bootstrap loop for B2 times. At each round k, construct a bootstrapped sample $S^{(k)}$ of untreated outcomes using $\hat{\beta}, \hat{F}, \hat{\Lambda}$ from the estimation part and the simulated errors from last step:

$$\tilde{Rx}^{(k)}_i(0) = X_i\hat{\beta} + \hat{F}\hat{\lambda}_i + \tilde{\epsilon}_i, \quad i \in Control$$

$$\tilde{Rx}^{(k)}_j(0) = X_j\hat{\beta} + \hat{F}\hat{\lambda}_j + \tilde{\epsilon}^p_j, \quad j \in Treated$$

where $\tilde{\epsilon}_i, \tilde{\epsilon}^p_j$ are randomly drawn from the prediction error sets $\hat{\boldsymbol{e}}$ and $\hat{\boldsymbol{e}}^p$. Apply the GSC method to $S^{(k)}$ and obtain a new $\hat{ATT}^{(k)}_{null}$. Because the bootstrapped treated counterfactuals do not contain the treatment effect, I add back the estimated ATT estimate to obtain the corresponding ATT estimate for this round k: $\hat{ATT}^{(k)}_{t_1} = \hat{ATT}^{(k)}_{null} + \hat{ATT}_{t_1}$.

Step 3: Compute the variance of $\hat{ATT}_{t_1}$.

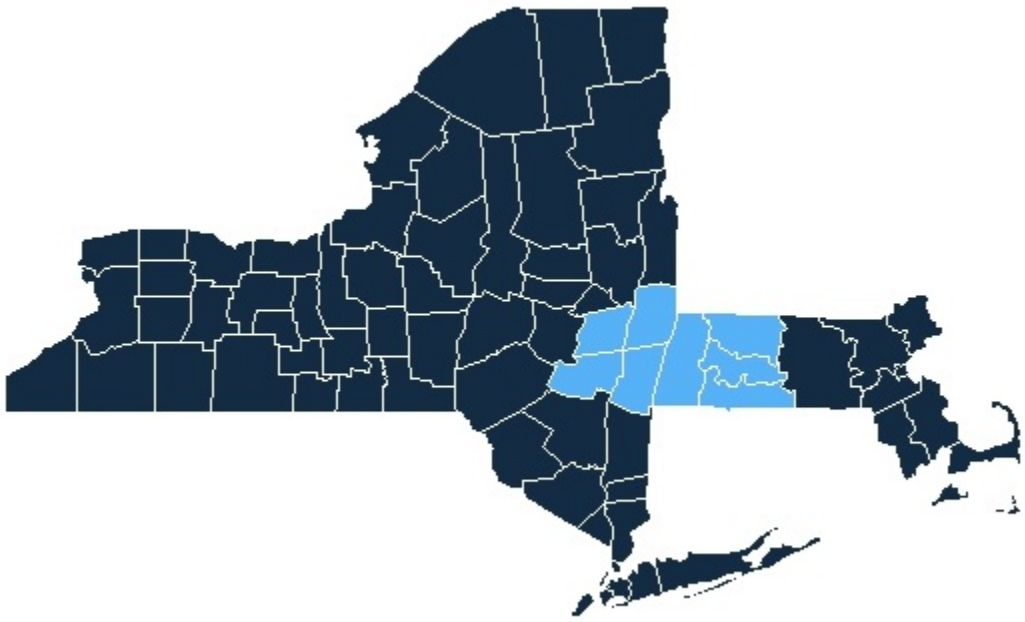
$$Var(\hat{ATT}_t|D, X, \Lambda, F) = \frac{1}{B2}\sum_{k=1}^{B2}(\hat{ATT}^{(k)}_t - \frac{1}{B2}\sum_{j=1}^{B2}\hat{ATT}^{(j)}_t)^2$$

Its confidence interval is obtained using percentile method as in Efron and Tibshirani (1994). Xu (2017) has shown in Monte Carlo exercises that GSC estimator has less bias than the DID estimator in the presence of unobserved, decomposable time-varying

confounders and is more efficient than the original synthetic matching estimator. When the sample is large enough ($T_0 > 10$ and $N_{co} > 40$),[1] the cross-validation procedure recovers the correct number of factors reasonably well. [2]

A.2 Results from MA-NY border counties.

Figure A.1: MA-NY border counties.

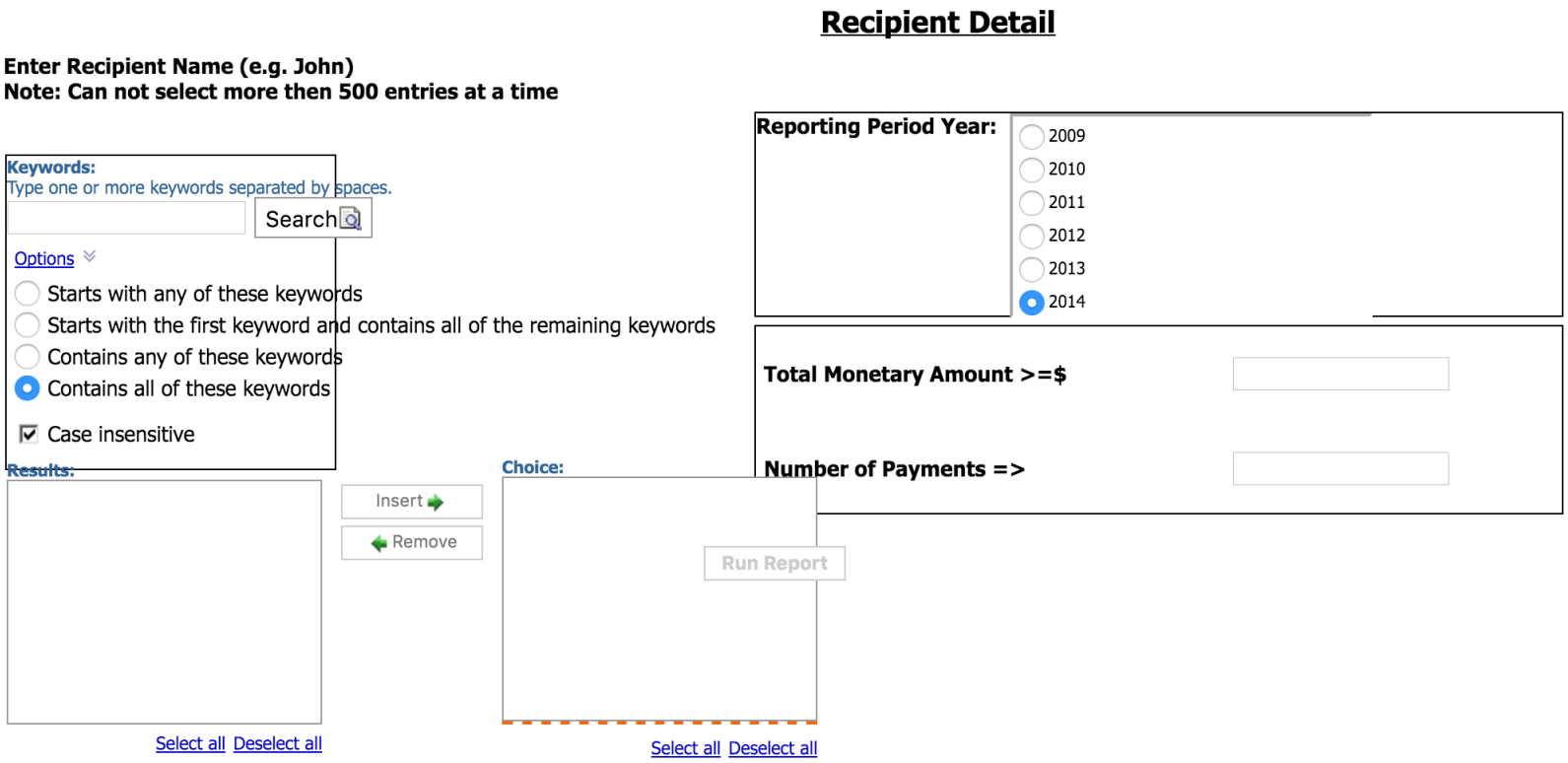


Berkshire, Franklin, Hampshire, Hampden (MA); Columbia, Rensselaer, Albany, Greene (NY).

[1] In my case, I have $T_0 = 18$, and $N_{co}^{statin} = 95, N_{co}^{antidepr} = 103, N_{co}^{antipsych} = 90$, for MA-CT border counties. These numbers for MA-NY border counties are: 62, 67, and 47.

[2] I implement the above estimation and inference procedures in R using -gsynth- package.

Figure A.2: Snapshot of the online search engine of Massachusetts physician payments.



The webpage is fully functional although the layout is slightly disorganized. After entering the keywords, click on "Run Report" to extract the related record details. Each query will return maximum 500 records. The webpage can be accessed through "Payments Made To Recipient Custom Report" at:
http://www.mass.gov/eohhs/gov/departments/dph/programs/hcq/healthcare-quality/pharm-code-of-conduct/data/custom-reports.html#rcpt.

Table A.2: Socio-economic conditions of the MA-NY border counties.

	New York				Massachusetts			
County	Albany	Columbia	Greene	Rensselaer	Hampshire	Franklin	Berkshire	Hampden
DEMOGRAPHICS								
Total Population	304,511	62,881	49,122	159,464	158,850	71,489	130,866	464,072
%Female	51.70%	49.60%	47.60%	50.60%	53.20%	51.10%	51.90%	51.90%
Median Age	38.2	45.3	44.0	39.6	36.2	44.3	44.6	38.8
%18+	80.20%	79.70%	80.90%	78.80%	83.40%	80.40%	80.60%	76.40%
%65+	14.07%	18.20%	17.70%	13.70%	12.80%	15.40%	18.80%	14.30%
SOCIAL-ECONOMIC								
Total Households	122,674	25,476	18,411	63,952	58,932	30,472	55,612	177,756
%Family HHld	57.20%	64.20%	64.20%	62.00%	58.60%	60.90%	58.50%	65.30%
%HHld with 1+ under 18	27.20%	28.30%	27.20%	29.10%	26.40%	28.20%	25.50%	33.50%
%HHld with 1+ over 65	24.50%	30.80%	32.20%	24.00%	25.40%	26.00%	31.90%	26.70%
%Unemployed	7.00%	8.40%	8.10%	7.90%	7.90%	8.00%	9.20%	10.40%
%income below poverty level ¶	13.10%	9.80%	14.60%	11.80%	11.90%	11.90%	12.40%	17.10%
Median HHld Income §	59,359	56,445	47,539	58,959	61,264	53,298	47,513	49,729
Mean HHld Income	76,707	79,555	60,810	72,644	77,266	66,926	66,048	64,999
Per Capita Income	31,924	32,934	23,842	29,647	29,246	28,841	28,939	25,646
HEALTH INSURANCE COVARAGE								
%private insurance	78.70%	74.50%	70.80%	77.60%	84.20%	75.90%	72.80%	66.00%
%public insurance	27.40%	31.90%	34.70%	28.90%	25.00%	35.80%	41.20%	43.20%
%Uninsured	7.20%	9.70%	6.80%	7.00%	3.20%	4.30%	3.70%	4.40%
EDUCATIONAL ATTAINMENT								
Population 25 years and over	202,976	45,264	34,935	107,085	97,486	51,643	93,083	305,343
%high school or higher	91.60%	87.60%	85.30%	89.80%	92.90%	91.80%	91.00%	83.70%
%bachelor's or higher	38.20%	27.90%	19.10%	27.90%	42.40%	32.90%	29.80%	24.20%
%graduate or professional	18.50%	13.20%	8.30%	12.40%	21.30%	14.30%	12.60%	9.10%

Source: U.S. Census Bureau, 2008-2012 American Community Survey.

¶in the past 12 months.

§all income measures are in 2012 inflation-adjusted dollars.

Table A.3: Panel regression results in MA-NY border counties.

(a) Statins.

Treatment Measure§	1	1	2	2	3	3
ATE, Branded Rx	-0.0191***	-0.0173***	-0.0220***	-0.0215***	-0.0248***	-0.0252***
	(0.00655)	(0.00656)	(0.00779)	(0.00778)	(0.00828)	(0.00829)
ATE, Generic Rx	-0.0136	-0.0118	-0.00983	-0.00930	-0.00549	-0.00585
	(0.0103)	(0.0101)	(0.0129)	(0.0127)	(0.0142)	(0.0139)
Monthly total patients		0.00553***		0.00577***		0.00602***
		(0.000918)		(0.000981)		(0.00102)
Brand-year-month FE			YES			
Physician-brand FE			YES			
ATE%, branded	-27.0%	-25.0%	-29.2%	-28.7%	-31.2%	-31.5%
ATE%, generics	-9.8%	-8.6%	-6.3%	-6.0%	-3.3%	-3.5%
N	100,992	100,992	75,744	75,744	67,328	67,328

(b) Antidepressants.

Treatment Measure§	1	1	2	2	3	3
ATE, Branded Rx	-0.0116**	-0.0122**	-0.00760	-0.00929	-0.00560	-0.00806
	(0.00515)	(0.00514)	(0.00609)	(0.00604)	(0.00629)	(0.00625)
ATE, Generic Rx	-0.0332***	-0.0337***	-0.0349**	-0.0366**	-0.0350**	-0.0375**
	(0.0125)	(0.0123)	(0.0161)	(0.0158)	(0.0165)	(0.0162)
Monthly total patients		0.00328***		0.00335***		0.00356***
		(0.00115)		(0.00119)		(0.00116)
Brand-year-month FE			YES			
Physician-brand FE			YES			
ATE%, branded	-41.1%	-42.2%	-31.0%	-35.4%	-23.7%	-30.9%
ATE%, generics	-17.1%	-17.3%	-16.9%	-17.6%	-16.7%	-17.6%
N	129,216	129,216	96,912	96,912	86,144	86,144

(c) Antipsychotics.

Treatment Measure§	1	1	2	2	3	3
ATE, Branded Rx	-0.0487**	-0.0475**	-0.0609**	-0.0606**	-0.0629**	-0.0635**
	(0.0227)	(0.0225)	(0.0273)	(0.0270)	(0.0303)	(0.0300)
ATE, Generic Rx	-0.0372**	-0.0360**	-0.0345*	-0.0343*	-0.0329*	-0.0335*
	(0.0159)	(0.0158)	(0.0184)	(0.0183)	(0.0187)	(0.0187)
Monthly total patients		0.00219		0.00240		0.00252
		(0.00154)		(0.00168)		(0.00160)
Brand-year-month FE			YES			
Physician-brand FE			YES			
ATE%, branded	-34.5%	-33.9%	-41.6%	-41.5%	-41.8%	-42.1%
ATE%, generics	-52.3%	-51.5%	-46.7%	-46.5%	-44.5%	-44.9%
N	33,120	33,120	24,840	24,840	22,080	22,080

*** p<0.01. ** p<0.05. * p<0.1. Standard Errors clustered at physician level. ATE% calculated at the mean. §Treatments are measured by 3 different temporal points: (1) when the data collection began on July 1, 2009, (2) when the firms first reported their payment to the government (July 1, 2010), and (3) when the data were made available to the public (Nov 22, 2010).

Table A.4: Brand-specific effects in MA-NY border counties.

(a) Statins.

Treatment Measure§	1	1	2	2	3	3
ATE, Top 1 brand (Lipitor)	-27.6%***	-26.3%**	-29.3%**	-29.0%**	-30.8%**	-31.0%**
ATE, Top 2 brand (Crestor)	-27.2%	-23.5%	-29.9%*	-29.0%	-32.7%*	-33.2%*
ATE, Generic Rx	-9.8%	-8.6%	-6.3%	-6.0%	-3.3%	-3.5%
Monthly total patients	NO	YES	NO	YES	NO	YES
Brand-year-month FE			YES			
Physician-brand FE			YES			
N	201,984	201,984	151,488	151,488	134,656	134,656

(b) Antidepressants.

Treatment Measure§	1	1	2	2	3	3
ATE, Top 1 brand (Lexapro)	-31.5%	-32.8%	18.4%	6.0%	44.4%	20.6%
ATE, Top 2 brand (Cymbalta)	-36.7%*	-37.6%*	-39.2%	-41.7%*	-33.1%	-37.1%
ATE, Generic Rx	-17.1%***	-17.3%***	-16.9%**	-17.6%**	-16.7%**	-17.6%**
Monthly total patients	NO	YES	NO	YES	NO	YES
Brand-year-month FE			YES			
Physician-brand FE			YES			
N	323,040	323,040	242,280	242,280	215,360	215,360

Pristiq is lumped into "other brands" due to insufficient observations on MA-NY border.

(c) Antipsychotics.

Treatment Measure§	1	1	2	2	3	3
ATE, Top 1 brand (Seroquel)	-27.9%	-27.5%	-31.8%	-31.7%	-31.3%	-31.6%
ATE, Top 2 brand (Abilify)	-47.4%**	-46.6%**	-54.4%**	-54.2%*	-51.7%*	-52.0%*
ATE, Top 3 brand (Zyprexa)	-22.5%	-20.8%	-27.8%	-27.4%	-41.2%	-41.9%
ATE, Generic Rx	-52.3%**	-51.5%**	-46.7%*	-46.5%*	-44.5%*	-44.9%*
Monthly total patients	NO	YES	NO	YES	NO	YES
Brand-year-month FE			YES			
Physician-brand FE			YES			
N	82,800	82,800	62,100	62,100	55,200	55,200

*** p<0.01. ** p<0.05. * p<0.1. Standard Errors clustered at physician level. ATE% calculated at the mean. §Treatments are measured by 3 different temporal points: (1) when the data collection began on July 1, 2009, (2) when the firms first reported their payment to the government (July 1, 2010), and (3) when the data were made available to the public (November 22, 2010).

Table A.5: Heterogeneous effects across physician groups on prescriptions, on MA-NY border.

(a) Statins.

Treatment Measure§	1	1	2	2	3	3
ATE, Branded Rx, Light	-8.3%	-19.7%	-12.0%	-24.2%	-24.1%	-32.8%
ATE, Branded Rx, Medium	-32.4%**	-7.1%	-34.2%**	-8.2%	-36.8%***	-11.5%
ATE, Branded Rx, Heavy	-28.4%***	-27.5%***	-30.3%***	-30.8%***	-28.4%***	-29.8%***
ATE, Generic Rx, Light	-37.5%***	-38.6%***	-39.2%**	-40.5%***	-45.8%***	-46.8%***
ATE, Generic Rx, Medium	-38.6%***	-34.3%***	-40.0%***	-35.6%***	-44.5%***	-40.2%***
ATE, Generic Rx, Heavy	-2.6%	-2.5%	5.1%	4.8%	12.3%	11.7%
Monthly total patients	NO	YES	NO	YES	NO	YES
Brand-year-month FE			YES			
Physician-brand FE			YES			
N	130,368	130,368	97,776	97,776	86,912	86,912

(b) Antidepressants.

Treatment Measure§	1	1	2	2	3	3
ATE, Branded Rx, Light	9.9%	14.9%	47.2%	31.7%	52.4%	33.0%
ATE, Branded Rx, Medium	-44.0%	-32.2%	-22.8%	-21.8%	-18.4%	-16.1%
ATE, Branded Rx, Heavy	-28.0%**	-29.9%***	-28.1%**	-34.0%***	-24.3%*	-32.3%***
ATE, Generic Rx, Light	-12.7%	-11.8%	-22.6%	-22.0%	-20.8%	-20.2%
ATE, Generic Rx, Medium	-20.8%*	-20.9%*	-21.3%	-21.5%	-22.5%	-22.6%
ATE, Generic Rx, Heavy	-26.9%***	-27.1%***	-25.5%***	-25.8%***	-25.8%***	-26.2%***
Monthly total patients	NO	YES	NO	YES	NO	YES
Brand-year-month FE			YES			
Physician-brand FE			YES			
N	257,472	257,472	193,104	193,104	171,648	171,648

(c) Antipsychotics.

Treatment Measure§	1	1	2	2	3	3
ATE, Branded Rx, Light	-28.7%	8.2%	-53.9%	-23.1%	-58.5%	-35.1%
ATE, Branded Rx, Medium	-50.8%*	-53.1%**	-63.2%**	-64.4%**	-63.1%*	-64.3%**
ATE, Branded Rx, Heavy	-44.3%***	-45.5%***	-54.2%***	-54.9%***	-56.9%***	-57.4%***
ATE, Generic Rx, Light	-38.0%	-32.3%	-36.4%	-30.5%	-31.8%	-26.6%
ATE, Generic Rx, Medium	-66.7%**	-67.3%**	-66.9%*	-67.5%*	-64.0%*	-64.6%*
ATE, Generic Rx, Heavy	-57.5%**	-57.7%**	-55.3%**	-55.4%**	-55.8%***	-56.0%***
Monthly total patients	NO	YES	NO	YES	NO	YES
Brand-year-month FE			YES			
Physician-brand FE			YES			
N	61,824	61,824	46,368	46,368	41,216	41,216

*** p<0.01. ** p<0.05. * p<0.1. Standard Errors clustered at physician level. ATE% calculated at the mean. §Treatments are measured by 3 different temporal points: (1) when the data collection began on July 1, 2009, (2) when the firms first reported their payment to the government (July 1, 2010), and (3) when the data were made available to the public (November 22, 2010).

A.3 Results from MA-CT border counties, with January - June 2009 as pre-period.

Table A.6: Panel regression results on MA-CT border, using Jan-Jun2009 as pre-period.

(a) Statins.

Treatment Measure§	1	1	2	2	3	3
ATE, Branded Rx	-0.0831***	-0.0617***	-0.122***	-0.0839***	-0.145***	-0.0969***
	(0.0109)	(0.0116)	(0.0141)	(0.0150)	(0.0156)	(0.0165)
ATE, Generic Rx	-0.130***	-0.108***	-0.185***	-0.147***	-0.215***	-0.167***
	(0.0159)	(0.0153)	(0.0227)	(0.0216)	(0.0259)	(0.0242)
Monthly total patients		0.0234***		0.0256***		0.0257***
		(0.00327)		(0.00337)		(0.00351)
Brand-year-month FE				YES		
Physician-brand FE				YES		
ATE%, branded	-50.3%	-42.9%	-60.0%	-50.8%	-63.2%	-53.5%
ATE%, generics	-42.2%	-37.9%	-48.8%	-43.1%	-50.8%	-44.5%
N	168,480	168,480	112,320	112,320	93,600	93,600

*** p<0.01. ** p<0.05. * p<0.1. Standard Errors clustered at physician level. ATE% calculated at the mean. §Treatments are measured by: (1) when the data collection began on July 1, 2009, (2) when the firms first reported their payment to the government (July 1, 2010), and (3) when the data were made available to the public (Nov 22, 2010).

Table A.6: Panel regression results on MA-CT border, using Jan-Jun2009 as pre-period (Continued).

(b) Antidepressants.

Treatment Measure§	1	1	2	2	3	3
ATE, Branded Rx	-0.0267***	-0.0118	-0.0377***	-0.0141	-0.0441***	-0.0143
	(0.00761)	(0.00842)	(0.0102)	(0.0118)	(0.0109)	(0.0132)
ATE, Generic Rx	-0.127***	-0.112***	-0.186***	-0.162***	-0.206***	-0.177***
	(0.0261)	(0.0250)	(0.0322)	(0.0308)	(0.0347)	(0.0331)
Monthly total patients		0.0190***		0.0204***		0.0218***
		(0.00308)		(0.00325)		(0.00343)
Brand-year-month FE			YES			
Physician-brand FE			YES			
ATE%, branded	-46.5%	-27.7%	-55.0%	-31.4%	-58.0%	-30.9%
ATE%, generics	-31.1%	-28.5%	-39.7%	-36.5%	-41.4%	-37.7%
N	221,328	221,328	147,552	147,552	122,960	122,960

(c) Antipsychotics.

Treatment Measure§	1	1	2	2	3	3
ATE, Branded Rx	-0.0849**	-0.0477	-0.106**	-0.0521	-0.101**	-0.0389
	(0.0383)	(0.0351)	(0.0430)	(0.0399)	(0.0443)	(0.0415)
ATE, Generic Rx	-0.0720***	-0.0348	-0.0868***	-0.0327	-0.0905***	-0.0281
	(0.0238)	(0.0228)	(0.0267)	(0.0267)	(0.0277)	(0.0280)
Monthly total patients		0.0267***		0.0267***		0.0264***
		(0.00460)		(0.00566)		(0.00570)
Brand-year-month FE			YES			
Physician-brand FE			YES			
ATE%, branded	-36.0%	-24.0%	-43.2%	-27.2%	-40.9%	-21.0%
ATE%, generics	-45.9%	-29.1%	-49.2%	-26.7%	-49.6%	-23.4%
N	78,912	78,912	52,608	52,608	43,840	43,840

*** p<0.01. ** p<0.05. * p<0.1. Standard Errors clustered at physician level. ATE% calculated at the mean. §Treatments are measured by: (1) when the data collection began on July 1, 2009, (2) when the firms first reported their payment to the government (July 1, 2010), and (3) when the data were made available to the public (Nov 22, 2010).

A.4 Results from MA-CT border counties with log transformations.

Table A.7: Panel regression results for statins.

(a) $Y = \log(Rx + 1)$

Treatment Measure§	1	1	2	2	3	3
ATE, Branded Rx	-0.0524***	-0.0409***	-0.0706***	-0.0551***	-0.0817***	-0.0634***
	(0.00470)	(0.00439)	(0.00608)	(0.00559)	(0.00677)	(0.00621)
ATE, Generic Rx	-0.0585***	-0.0471***	-0.0814***	-0.0658***	-0.0944***	-0.0761***
	(0.00620)	(0.00572)	(0.00834)	(0.00759)	(0.00942)	(0.00852)
Monthly total patients		0.00642***		0.00660***		0.00666***
		(0.00104)		(0.00116)		(0.00122)
Brand-year-month FE			YES			
Physician-brand FE			YES			
ATE%, branded	-44.1%	-38.1%	-53.4%	-45.7%	-56.8%	-48.6%
ATE%, generics	-33.6%	-29.6%	-40.0%	-34.7%	-42.2%	-36.3%
N	224,640	224,640	168,480	168,480	149,760	149,760

(b) $Y = \log(Rx + \sqrt{Rx^2 + 1})$

Treatment Measure§	1	1	2	2	3	3
ATE, Branded Rx	-0.0678***	-0.0531***	-0.0916***	-0.0715***	-0.106***	-0.0824***
	(0.00607)	(0.00566)	(0.00786)	(0.00723)	(0.00877)	(0.00803)
ATE, Generic Rx	-0.0756***	-0.0608***	-0.105***	-0.0852***	-0.122***	-0.0987***
	(0.00799)	(0.00738)	(0.0107)	(0.00978)	(0.0121)	(0.0110)
Monthly total patients		0.00828***		0.00851***		0.00859***
		(0.00134)		(0.00149)		(0.00158)
Brand-year-month FE			YES			
Physician-brand FE			YES			
ATE%, branded	-44.4%	-38.5%	-53.7%	-46.1%	-57.2%	-49.0%
ATE%, generics	-33.7%	-29.8%	-40.2%	-34.9%	-42.4%	-36.5%
N	224,640	224,640	168,480	168,480	149,760	149,760

*** p<0.01. ** p<0.05. * p<0.1. ATE% calculated at the mean. §Treatments are measured by 3 different temporal points: (1) when the data collection began on July 1, 2009, (2) when the firms first reported their payment to the government (July 1, 2010), and (3) when the data were made available to the public (November 22, 2010).

Table A.8: Panel regression results for antidepressants.

(a) $Y = \log(Rx + 1)$

Treatment Measure§	1	1	2	2	3	3
ATE, Branded Rx	-0.0213***	-0.0175***	-0.0267***	-0.0213***	-0.0307***	-0.0243***
	(0.00316)	(0.00341)	(0.00402)	(0.00429)	(0.00441)	(0.00473)
ATE, Generic Rx	-0.0469***	-0.0431***	-0.0674***	-0.0620***	-0.0768***	-0.0704***
	(0.00681)	(0.00666)	(0.00860)	(0.00840)	(0.00935)	(0.00912)
Monthly total patients		0.00459***		0.00453***		0.00455***
		(0.000768)		(0.000820)		(0.000849)
Brand-year-month FE				YES		
Physician-brand FE				YES		
ATE%, branded	-42.3%	-31.1%	-50.3%	-36.6%	-54.0%	-39.5%
ATE%, generics	-21.2%	-18.8%	-28.6%	-25.6%	-31.1%	-27.7%
N	295,104	295,104	221,328	221,328	196,736	196,736

(b) $Y = \log(Rx + \sqrt{Rx^2 + 1})$

Treatment Measure§	1	1	2	2	3	3
ATE, Branded Rx	-0.0274***	-0.0226***	-0.0345***	-0.0276***	-0.0397***	-0.0315***
	(0.00407)	(0.00438)	(0.00517)	(0.00551)	(0.00568)	(0.00608)
ATE, Generic Rx	-0.0600***	-0.0552***	-0.0864***	-0.0795***	-0.0986***	-0.0904***
	(0.00871)	(0.00853)	(0.0110)	(0.0108)	(0.0120)	(0.0117)
Monthly total patients		0.00588***		0.00580***		0.00583***
		(0.000981)		(0.00105)		(0.00109)
Brand-year-month FE				YES		
Physician-brand FE				YES		
ATE%, branded	-42.6%	-31.5%	-50.6%	-37.1%	-54.3%	-40.0%
ATE%, generics	-21.1%	-18.7%	-28.5%	-25.5%	-31.0%	-27.6%
N	295,104	295,104	221,328	221,328	196,736	196,736

*** p<0.01. ** p<0.05. * p<0.1. ATE% calculated at the mean. §Treatments are measured by 3 different temporal points: (1) when the data collection began on July 1, 2009, (2) when the firms first reported their payment to the government (July 1, 2010), and (3) when the data were made available to the public (November 22, 2010).

Table A.9: Panel regression results for antipsychotics.

(a) $Y = \log(Rx + 1)$

Treatment Measure§	1	1	2	2	3	3
ATE, Branded Rx	-0.0461***	-0.0378***	-0.0566***	-0.0451***	-0.0553***	-0.0421***
	(0.0124)	(0.0120)	(0.0141)	(0.0136)	(0.0147)	(0.0143)
ATE, Generic Rx	-0.0289***	-0.0206**	-0.0355***	-0.0240**	-0.0389***	-0.0257**
	(0.00826)	(0.00871)	(0.0101)	(0.0105)	(0.0107)	(0.0112)
Monthly total patients		0.00670***		0.00611***		0.00595***
		(0.00148)		(0.00144)		(0.00140)
Brand-year-month FE			YES			
Physician-brand FE			YES			
ATE%, branded	-26.6%	-16.1%	-33.7%	-20.7%	-31.8%	-16.2%
ATE%, generics	-36.6%	-22.3%	-39.8%	-22.4%	-41.4%	-21.9%
N	105,216	105,216	78,912	78,912	70,144	70,144

(b) $Y = \log(Rx + \sqrt{Rx^2 + 1})$

Treatment Measure§	1	1	2	2	3	3
ATE, Branded Rx	-0.0590***	-0.0483***	-0.0724***	-0.0577***	-0.0709***	-0.0540***
	(0.0158)	(0.0153)	(0.0180)	(0.0174)	(0.0188)	(0.0183)
ATE, Generic Rx	-0.0371***	-0.0265**	-0.0456***	-0.0310**	-0.0501***	-0.0332**
	(0.0106)	(0.0112)	(0.0130)	(0.0136)	(0.0138)	(0.0144)
Monthly total patients		0.00858***		0.00782***		0.00763***
		(0.00189)		(0.00184)		(0.00179)
Brand-year-month FE			YES			
Physician-brand FE			YES			
ATE%, branded	-26.3%	-15.7%	-33.4%	-20.4%	-31.6%	-16.0%
ATE%, generics	-36.6%	-22.4%	-39.8%	-22.6%	-41.5%	-22.2%
N	105,216	105,216	78,912	78,912	70,144	70,144

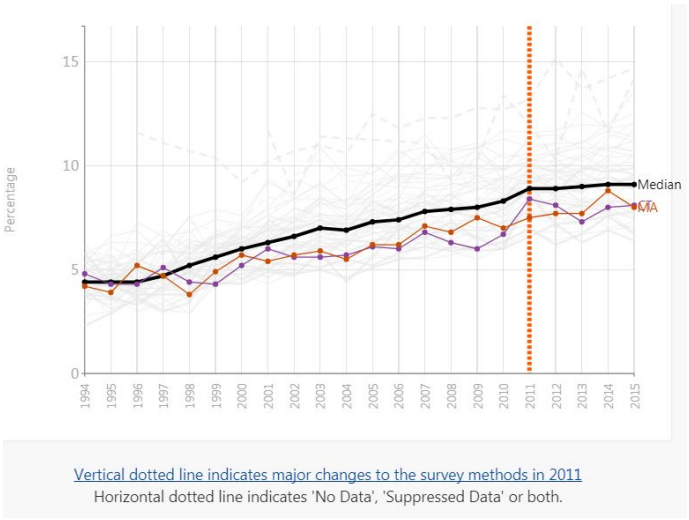
*** p<0.01. ** p<0.05. * p<0.1. ATE% calculated at the mean. §Treatments are measured by 3 different temporal points: (1) when the data collection began on July 1, 2009, (2) when the firms first reported their payment to the government (July 1, 2010), and (3) when the data were made available to the public (November 22, 2010).

APPENDIX B

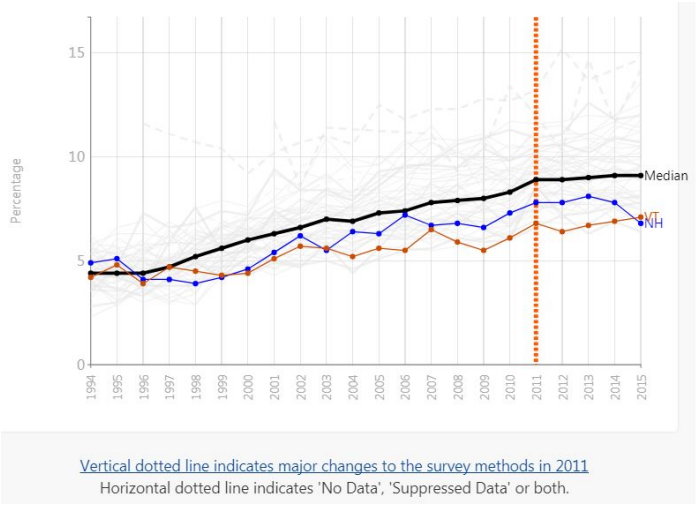
Appendix to Chapter 3

Figure B.1: Age-Adjusted Percentage of Adults with Diabetes, 1994-2015.

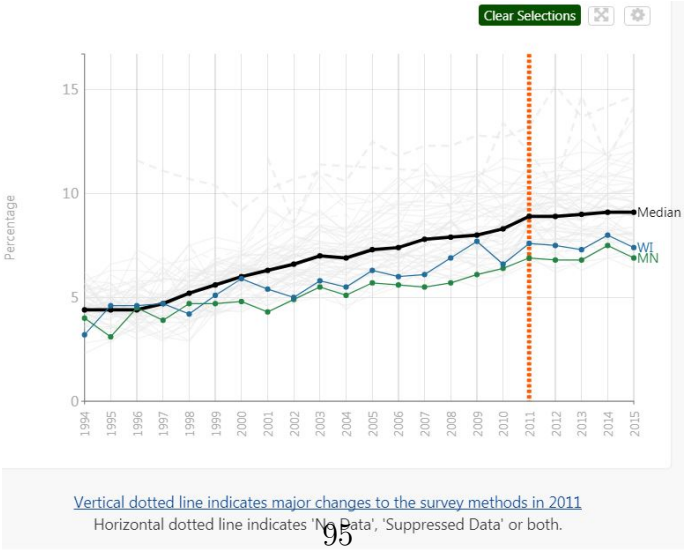
(a) MA and CT.



(b) VT and NH.



(c) MN and WI.



Source: Centers for Disease Control and Prevention. https://gis.cdc.gov/grasp/diabetes/DiabetesAtlas.html

Table B.1: Drug Information.

Brand Name	Manufacturer	Subclass	Initial Approval Date
APIDRA	Sanofi	Insulin	4/16/2004
BYDUREON	AstraZeneca	GLP1 receptor agonist	1/27/2012
FARXIGA	AstraZeneca	SGLT2 inhibitor	1/8/2014
HUMALOG	Eli Lilly	Insulin	6/14/1996
INVOKANA	Janssen	SGLT2 inhibitor	3/29/2013
JANUMET	Merck	DPP4 inhibitor	4/2/2007
JANUVIA	Merck	DPP4 inhibitor	10/17/2006
LANTUS	Sanofi	Insulin	4/20/2000
LEVEMIR	Novo Nordisk	Insulin	6/16/2005
NESINA	Takeda	DPP4 inhibitor	1/25/2013
NOVOLOG	Novo Nordisk	Insulin	6/7/2000
ONGLYZA	AstraZeneca	DPP4 inhibitor	7/31/2009
OSENI	Takeda	DPP4 inhibitor	1/25/2013
TANZEUM	GSK	GLP1 receptor agonist	4/15/2014
TRADJENTA	BI and Eli Lilly	DPP4 inhibitor	5/2/2011
VICTOZA	Novo Nordisk	GLP1 receptor agonist	1/26/2010

B.1 Relationship between Payment Changes and Nature of Payments.

In my sample, six major payment purposes are recorded in the nature of payment field: consulting fee, speaker events, food and beverage, education, travel and lodging, and honoraria (table 3.2). While payments for physicians serving as a speaker cost firms the most money (63% of the total dollar amount), payments for food and beverage covers more than 98% of the physician population in my sample (49,085). Note that my sample does not include any research-related expenditure.

One conjecture is that physicians receiving more controversial payments (i.e. payments suffering more from COI) are more influenced by the disclosure regulation. Physicians' service as a speaker for the industry has long been debated (e.g. Brennan et al., 2006), especially because they tend to be sizable and can influence peer physicians. On the other hand, sponsorship for attending continuing education programs and conferences may be viewed as more legitimate. If this is indeed the case, I will see in the data that physicians predominantly paid for consulting/speaker services are the most responsive to the disclosure than physicians mostly paid under other categories.

I characterize each physician by the shares of different nature of payment she received pre-disclosure from all drugs. That is, for each physician, I compute the share of payments received for a particular reason during the pre-disclosure period. Each person can thus be characterized by a vector of shares based on various reasons she got paid before. I regress the estimated payment change per physician-drug pair onto the vector of shares across different payment reasons to understand which reason makes the physician more sensitive to the disclosure. Specifically, I run the following specification with the share of travel expenditure as the omitted baseline:

$$\begin{aligned}\hat{\tau}_{id}^{pay} = \alpha_1 SpeakerShare_i + \alpha_2 ConsultingShare_i + \alpha_3 HonorariaShare_i + \\ \alpha_4 FoodShare_i + \alpha_5 EducationShare_i + \alpha_6 LogPreUSD_i + X_i'\beta + I_d + \xi_{id}.\end{aligned} \tag{B.1}$$

For each physician, I control for the total dollars received pre-disclosure (in log), the basic physician characteristics (gender, medical school graduation year, size of the associated practice group, and number of claim-based hospitals), and allow for common drug fixed effects. I cluster errors by physicians.

As is reported in table B.2, a unit increase in the share of speaker fees received by a physician is associated with the largest decline in payment percentage. On the contrary,

a unit increase in the share of sponsorship for continuing medical education is associated with the smallest decline/largest increase in payment percentage. This is consistent with the public notion that compensated for serving as a speaker for the industry is the least legitimate while sponsored for attending continuing education programs is the most legitimate. The data pattern also suggests that physicians mainly receiving food and beverage show a smaller decline/larger increase in the post-disclosure payments compared to physicians mainly receiving travel benefits.

I would like to caution readers that there could be explanations for the same data pattern other than the perceived legitimacy. For example, the speaker payments are the largest in dollar amount, while its ROI (return-on-investment) may be less straightforward. Firms might thus decide to cut down these payments. With the current data, I could not and would not prove which explanation is the most correct. I leave this to future research.

Table B.2: Relationship between Payment Changes and Nature of Payments.

DV = $\hat{\tau}_{id}^{pay}$	(1)	(2)
Share of Payment Received by a Physician		
Speaker	-0.0160***	-0.0141***
	(0.00563)	(0.00541)
Consulting	-0.00201	0.000342
	(0.00622)	(0.00599)
Honoraria	0.0140	0.0142
	(0.0160)	(0.0154)
Food&Beverage	0.0195***	0.0220***
	(0.00523)	(0.00502)
Education	0.0222***	0.0253***
	(0.00534)	(0.00513)
physician control variables:		
Total Pre-Disclosure Payment (log$)	0.00547***	0.00565***
	(0.000115)	(0.000116)
Female		-0.000622***
		(0.000231)
Medical School Graduation Year		0.000121***
		(9.77e-06)
Practise Group Size		9.29e-06***
		(1.07e-06)
Number of Hospitals		-0.000200***
		(6.85e-05)
Intercept	-0.0726***	-0.317***
	(0.00532)	(0.0201)
Observations	199,085	199,085
Drug FE	YES	YES
R2	0.856	0.857

*** p<0.01, ** p<0.05, * p<0.1. Robust standard errors clustered by physician. Share of travel expenditure per physician is the omitted baseline.

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