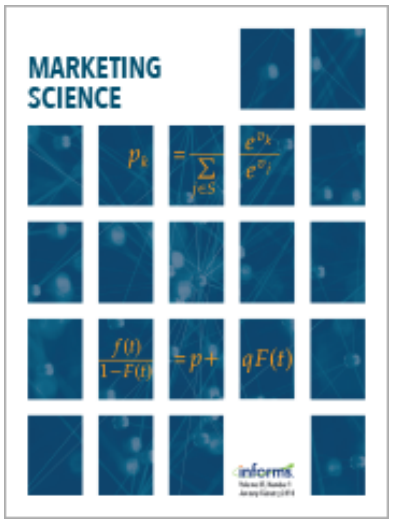

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"Let the Sunshine In": The Impact of Industry Payment Disclosure on Physician Prescription Behavior

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Abstract. U.S. pharmaceutical companies frequently pay doctors to promote their drugs. This has raised concerns about conflict of interest, which policy makers have attempted to address by introducing payment disclosure laws. However, it is unclear if such disclosure has an effect on physician prescription behavior. We use individual-level claims data from a major provider of health insurance in the United States and employ a difference-in-differences research design to study the effect of the payment disclosure law introduced in Massachusetts in June 2009. The research design exploits the fact that, although physicians operating in Massachusetts were impacted by the legislation, their counterparts in the neighboring states of Connecticut, New York, New Hampshire, and Rhode Island were not. In order to keep the groups of physicians comparable, we restrict our analysis to physicians in the counties that are on the border of these states. We find that the Massachusetts disclosure law resulted in a decline in prescriptions in all three drug classes studied: statins, antidepressants, and antipsychotics. Our findings are robust to alternative control groups, time periods and estimation methods. We also show that the effect is highly heterogeneous across physician groups. Finally, we explore potential mechanisms driving these results.

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1. Introduction

The U.S. pharmaceutical industry spent more than $6 billion as marketing payments to physicians between 2013 and 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 (Manchanda and Chintagunta 2004, DeJong et al. 2016, Yeh 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 resulting from conflict of interest (Manchanda and Honka 2009, Engelberg et al. 2014, Carey et al. 2015, Grochowski Jones and Ornstein 2016), policy makers have been pushing for full disclosure of these payments. Several states have introduced disclosure laws ("sunshine laws") that require companies to report physician payments to the state government (Chimonas et al. 2010, Pham-Kanter et al. 2012).[3] In 2013, the federal government made disclosure mandatory in all states as part of the Affordable Care Act.

The idea behind disclosure laws is that increased public scrutiny as a result of the disclosure might persuade firms to decrease these payments (Inderst and Ottaviani 2012)[4] 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 reduction in payments as a result of disclosure could motivate physicians to switch from prescribing branded to generic drugs. Furthermore, physicians may prescribe more conservatively as a result of increased public scrutiny arising from the disclosure of financial relationship with firms even when there is no change in how much they are paid. As a result, disclosure laws can help lower healthcare costs either via switching from branded drugs to less expensive generics or by curtailing prescriptions overall. On the other hand, critics of disclosure argue that it relieves physicians of the guilt around "biased" prescription decisions (assuming a bias exists) via "moral licensing" (Cain et al. 2005, 2011, Loewenstein et al. 2011). Taken together, these diverse viewpoints make it hard to predict the impact of disclosure ex ante.

In this paper, we evaluate the effect of *enforced payment disclosure* on physician prescription behavior.

To this end, we 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 our analysis come from one of the largest health insurance companies in the United States. We use outpatient prescription information at the claim level during a six-year period between January 2006 and December 2011. This allows us 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. We study prescription behavior in three therapeutic classes that receive the highest levels of marketing spending: statins, antidepressants, and antipsychotics (Campbell 2009). Note that we do not study the effect of the change in payments on physician prescription behavior as we do not have access to payment data before the disclosure took effect.

Our 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 in which no such law existed in the same time period. To ensure that the physicians in these control states are comparable, we focus on physicians located in the border counties of MA and its neighboring states that did not have disclosure laws during this period, that is, Connecticut (CT), New York (NY), New Hampshire (NH), and Rhode Island (RI). 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 on which they are located. Our 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. Because the compliance with the disclosure law occurred in a phased manner, it is difficult to clearly define when the treatment occurred. Therefore, we use three different temporal points to characterize the change from pretreatment to posttreatment: (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.

Our results reveal that, on average, the disclosure law resulted in a decline in the prescription of branded drugs in MA. Specifically, the intervention led to a 47%–54% decrease for branded statins, a 50%–56% decrease for branded antidepressants, and a 31%–32% decrease for branded antipsychotics when we consider physicians in MA and the border areas of neighboring states.[5] Interestingly, we find that the prescriptions of generic drugs in all three drug classes also declined as a result of the disclosure: statins by approximately 33%, antidepressants by approximately 25%, and generic antipsychotics by 28%. Therefore, the percentage decline in generic prescriptions was less pronounced than those for branded drugs. Nevertheless, the finding that both branded and generic prescriptions declined suggests that the law led to an overall decline in prescriptions in these three categories. As we discuss, we are able to replicate virtually all our findings across alternative definitions of the control group and estimation approaches. The results are also robust to alternative definitions of the policy change (i.e., the three temporal points that characterize various aspects of the law coming into effect) and choice of model specification. In addition, our 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 MA disclosure law.

In order to assess potential threats to the validity of our research design, we carry out three robustness checks. In the first robustness check, we drop CT from the pool of control states to verify if some of its unique policies might have contaminated the estimated effect of disclosure. Second, we use the generalized synthetic control method to create a control group that closely resembles the treatment group. In the third robustness check, we verify if the result was driven by changes in physician payments as a result of the MA disclosure law. If such payment changes were primarily driven by local pharmaceutical reps reallocating their marketing budgets across physicians operating on either side of state borders, this would render the border identification strategy problematic. To alleviate this concern, we perform a robustness check with physicians from a nonneighboring state, Illinois (IL), as the control group. Overall, these results consistently 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 in both a statistical and economic sense.

As noted earlier, there are three plausible reasons why disclosure laws could alter prescription behavior among physicians: (a) pharmaceutical companies might reduce payments to physicians, (b) physicians might become less open to accepting payments, and (c) physicians might resort to self-monitoring and become reluctant to prescribe medications. However, because we do not directly observe payments before disclosure went into effect, we cannot draw any

definitive conclusions on whether the decrease in prescriptions is related to *changes in payments* (i.e., (a) and (b)) made by pharmaceutical companies. Nevertheless, the results from our empirical analysis do present some useful pointers to the plausible mechanism driving this change. If industry payments to physicians influence prescription behavior, (a) and (b) can explain the decrease in branded prescriptions as a result of the MA disclosure law. However, (a) and (b) cannot explain the reduction in generic prescriptions because generic drug manufacturers do not usually make payments to physicians. Therefore, we conjecture that the reduction in prescriptions was likely driven by self-monitoring among physicians to curb "over-diagnosis" rather than changes in how firms deliver payments. Although one might suspect the decline in generic prescriptions could come from the decline of branded prescriptions that are mandatorily substituted into generics, in Section 4.4, we provide arguments for why self-monitoring is still likely to be the driving force behind the decline in prescriptions. Although, on the one hand, this may be seen as a "good" outcome, that is, lower prescriptions especially of branded drugs are likely to reduce healthcare 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. Nevertheless, our data do not permit us to comment on such welfare implications of the disclosure law. 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.

Our paper builds on extant literature on the effect of disclosure laws on the prescribing behavior of physicians, notably Pham-Kanter et al. (2012) in several ways. First, a notable difference is that Pham-Kanter et al. (2012) study the effect of disclosure laws enacted in Maine and West Virginia. As Pham-Kanter et al. (2012) note, the laws enacted in these states did not disseminate payment information to the public in an accessible way. In fact, MA was one of the first states to make payment information readily accessible to the public. Given that the greater public scrutiny that comes with transparency is usually hypothesized as an important driver of changes in payments and prescribing behavior, we believe that the MA disclosure law sheds qualitatively different light than those presented in Pham-Kanter et al. (2012). Second, we study the effect of both branded and generic prescriptions. The estimated decline in generic prescriptions is a surprising finding, which led us to conjecture that self-monitoring among physicians might have played a role in driving the estimated changes in prescriptions. Third, we report results from a battery of analyses that address various concerns regarding the appropriateness of comparing changes in prescriptions in states that enacted disclosure laws with corresponding changes in states that did not. Specifically, the generalized synthetic control method that we employ helps in addressing a key limitation noted in Pham-Kanter et al. (2012) regarding the appropriateness of their matching approach.

The rest of the paper proceeds as follows. Section 2 introduces the institutional background of the policy intervention in MA and describes the data. Section 3 explains the identification strategy and empirical specification. Section 4 reports and discusses the findings. Section 5 provides concluding remarks and suggests directions for future research.

2. Institutional Setting and Data

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."[6] It incorporated requirements from the voluntary code of conduct of the Pharmaceutical Research 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"[7] to collect and report certain financial transactions related to marketing activities with Massachusetts healthcare providers. The policy came into effect over a series of steps between July 2009 and November 2010:

1. Starting on July 1, 2009, 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 1, 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 healthcare providers.[8] 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 were exempt from disclosure.[9] For July 1, 2010, transactions for the period July 1, 2009, through December 31, 2009, were reported. In each year thereafter, the disclosures covered a full calendar year of transactions.

3. On November 22, 2010, the Massachusetts Office of Health and Human Services set up an online website 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 data set. At that point in time, Massachusetts was the first state to open up an online database of firm payments to physicians publicly. Figure 1 shows a snapshot of the customizable search engine.

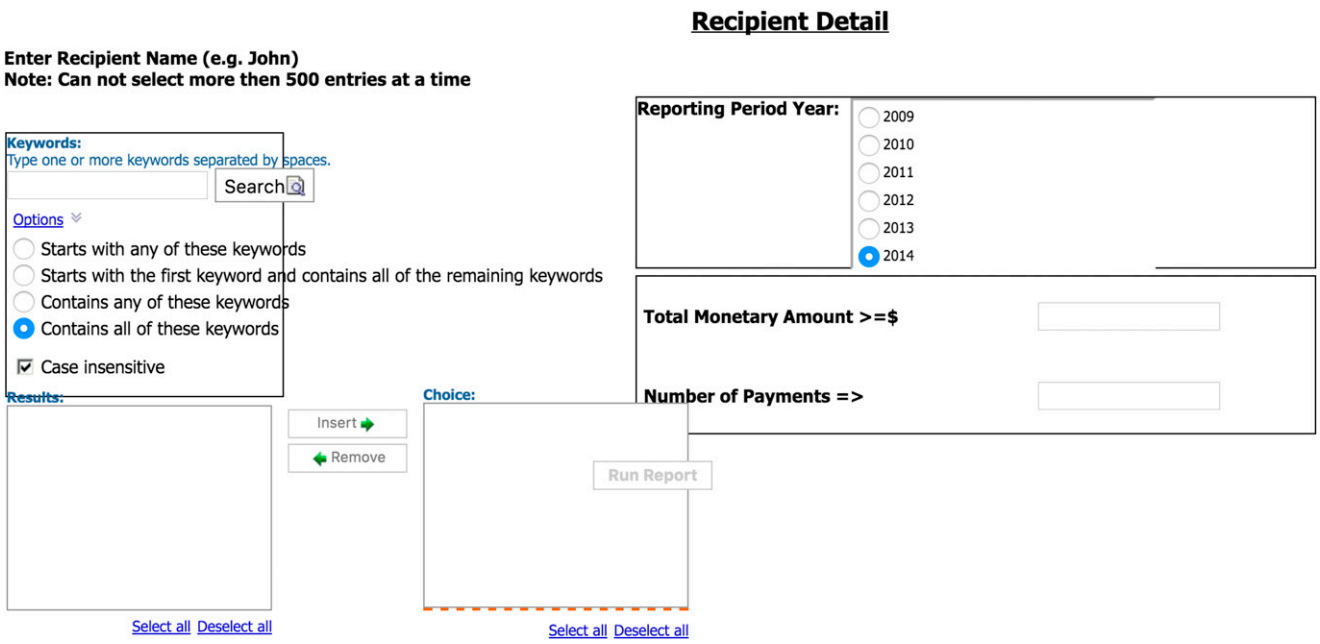
2.2. Data

2.2.1. Prescription Data. Our prescription data come from the De-identified Clinformatics™ Data Mart Database (provided by OptumInsight Inc.) provided by a large, national U.S. health insurer.[10] 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, we observe four types of information: (a) system-encrypted physician unique ID; (b) drug information, including names, therapeutic class, national drug code, and 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; and (d) standardized cost information. In addition, we observe some patient characteristics, such as their age (capped at 90), gender, zip code of residence, starting date of the membership, the insurance coverage type (e.g., HMO, PPO, etc.) in which they are enrolled and all the prescription claims filed on their behalf. We further pair our data with the 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, allowed us to identify whether a drug is branded or a generic equivalent.

2.2.2. Sample Preparation and Summary Statistics. As noted earlier, we consider physicians located at the border of MA and its neighboring states without prior disclosure laws, that is, CT, NY, RI, and NH (see Figure 2). This covers physicians operating in almost all the counties of MA.

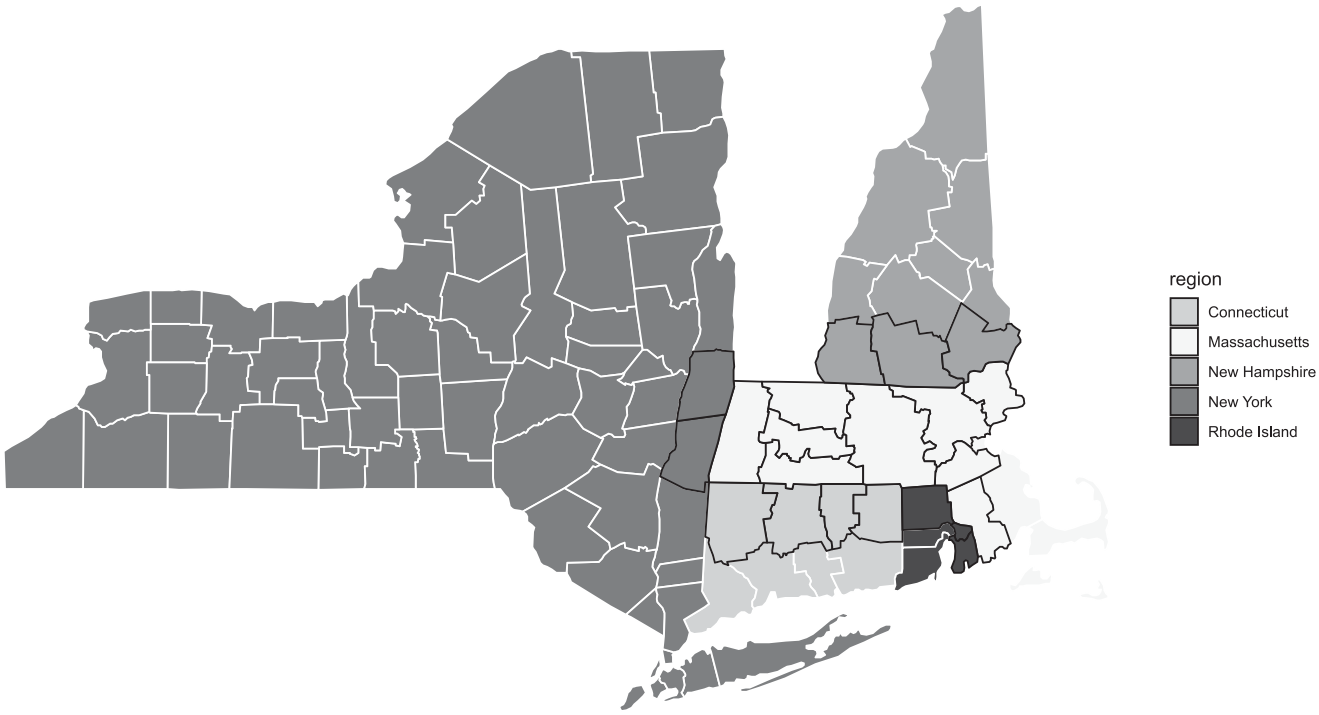
The idea behind restricting our analysis to physicians in the border counties is that they will have similar characteristics and face patient pools with comparable needs for prescription drugs (across different therapeutic classes). We check the population in these border counties in terms of their demographic and socioeconomic characteristics, health insurance coverage, and educational attainment in Table 1. These data suggest that the population on either side of the border of MA and its neighboring states are comparable in terms of demographic and socioeconomic characteristics, private health insurance coverage, and educational attainment. Therefore, the premise that the

Figure 1. (Color online) Snapshot of the Online Search Engine for Massachusetts Physician Payments



Notes. The web page 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 returns maximum 500 records. The web page 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.

Figure 2. MA and Neighboring Border Counties in the Regression Sample



Notes. Control sample: Litchfield, Hartford, Tolland, Windham (CT); Rensselaer, Columbia (NY); Providence, Bristol, Newport (RI); Cheshire, Hillsborough, Rockingham (NH). Treatment sample: all counties in MA except the coastal area (Suffolk, Plymouth, Barnstable, Dukes, Nantucket).

physicians operating in these areas should face patient pools with similar needs for prescription medications appears to have face validity. However, there are still some differences in observable characteristics across state borders, which can potentially impact prescription decisions made by physicians. In our empirical

Table 1. Socioeconomic Conditions of the MA and Neighboring States Border Counties

Border	South		West		North	
State	CT & RI	MA[a]	NY	MA[a]	NH	MA[a]
Demographics						
Total population	2.10E + 06	2.80E + 06	2.22E + 05	1.31E + 05	7.74E + 05	3.30E + 06
Percentage female	51.4	51.6	50.3	51.9	50.6	51.3
Median age	40	40	42	45	41	40
Percentage 18+	78.0	77.6	79.0	80.6	77.3	77.9
Percentage 65+	14.3	14.1	15.0	18.8	12.6	13.6
Socioeconomic						
Total households	8.16E + 05	1.10E + 06	8.94E + 04	5.56E + 04	2.99E + 05	1.20E + 06
Percentage family HHld	64.6	65.5	62.6	58.5	68.3	65.0
Percentage unemployed	6.3	6.0	5.3	5.8	4.9	5.3
Median HHld income[b]	$61,931	$64,497	$58,240	$47,513	$71,852	$72,349
Mean HHld income[b]	$80,700	$84,072	$74,621	$66,048	$89,274	$95,230
Per capita income[b]	$32,074	$32,821	$30,587	$28,939	$34,947	$37,276
Health insurance coverage						
Percentage private insurance	72.8	76.8	76.7	72.8	79.5	79.1
Percentage public insurance	29.9	32.5	29.8	41.2	22.1	29.4
Percentage uninsured	9.4	3.6	7.8	3.7	9.5	3.8
Educational attainment						
Percentage high school or higher	86.3	88.1	89.2	91.0	92.0	90.5
Percentage bachelor's or higher	31.8	34.4	27.9	29.8	35.3	41.9
Percentage graduate or professional	13.3	14.0	12.6	12.6	12.7	18.8

Source: U.S. Census Bureau, 2008–2012 American Community Survey.
[a]Because we only have county-level data, in this table, Berkshire is counted in all three borders; Worcester is counted for both North and South.
[b]All income measures are in 2012 inflation-adjusted dollars. Data calculated using county-level estimates weighted by county population.

analysis, we control for cross-sectional differences across physicians to prescribe drugs via physician–brand fixed effects. In addition, we also include the number of patients in the insurance pool in the zip code in which each physician operates as a covariate. Because the number of patients in the insurance pool varies over time, it enables us to control for changes in each physician's need to prescribe drugs. In our final specification, we also include county-level annual household income (median) and county-level private and public insurance coverage (American Community Survey (ACS) one-year estimates) as controls for key socioeconomic and healthcare variations in the local areas over time.

We do not observe the zip code in which a physician is located in our data. Therefore, we infer this from the location information of each physician's patient pool from MA and the neighboring states *before* the policy intervention comes into effect. We first identified all physicians who have prescribed for patients residing in these states during 2006–2009. We then assigned each physician to the modal zip code in which most of the physician's patients resided. As noted, our sample comprises physicians from the border counties in MA and all neighboring states (except VT). This leaves us with 8,091 unique physicians in MA and 2,852 in the neighboring states.

We investigate the impact of disclosure on new prescriptions for three chronic drug classes: statins, antidepressants, and antipsychotics. We chose these drug classes for the following reasons. First, statins (or antihyperlipidemic agents) and antidepressants are among the top three most prescribed categories of drugs in the United States during the 2006–2011 time period. These categories account for 5.3% and 4.1% of the total prescriptions in our data, respectively. These numbers are substantial given that the average market share of a drug category in our data are about 0.3%. We include antipsychotics as the third category because it saw a significant increase in prescriptions between 2006 and 2011. Two of the popular drugs in this class, Abilify and Seroquel, ranked among the top five best-selling drugs during the 2006–2011 period (https://www.drugs.com/top200_2009.html). Second, there are no over-the-counter drugs in these three drug classes, which implies that they can only be obtained against a physician prescription. Thus, we 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 2018a).

Within each drug class, we focus on new prescriptions and renewals written by the physicians in our 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 the patient's response to the treatment. Admittedly, there might be greater inertia with renewals compared with new prescriptions, suggesting that the impact of the legislation is likely to be muted for renewals compared with 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, our data do not allow us to infer the date when a patient starts taking prescription drugs in a given therapeutic class. Nevertheless, if we 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 we had only considered the former.

In our empirical analysis, we keep the border physicians who have prescribed any of these three drug classes during January 2006–December 2011 and aggregate new prescriptions and renewals by brand to the monthly level for each physician. We also record the monthly total patients registered with the focal insurance provider within the zip code in which each physician operates as a proxy for the size of the physician's monthly patient pool. Table 2 summarizes the number of physicians, drugs, branded drugs, and zip codes in our sample, and Table 3 reports summary statistics on monthly total new prescriptions per class and monthly total patients per physician. Between January 2006 and December 2011, an average physician from the MA border prescribed 0.13 branded statins (0.23 for physicians operating in the border areas of neighboring states), 0.06 branded antidepressants (0.14 for physicians operating in the border areas of neighboring states), and 0.31 branded

Table 2. Number of Physicians, Drugs, and Brand Name Drugs in the Regression Sample

	Number of physicians		Number of drugs[a]		Number of brand names		Number of zips	
	MA	Control	MA	Control	MA	Control	MA	Control
Statins	4,714	1,645	17	17	13	13	280	80
Antidepressants	6,580	2,139	69	66	35	34	295	91
Antipsychotics	1,985	863	39	36	21	20	255	78

[a]A drug is uniquely defined by its trademark name or generic name.

Table 3. Physician Panel Summary Statistics, January 2006–December 2011

Panel A: Statins

Variable		*N*	Mean	Standard deviation	Maximum
Branded prescriptions, per physician-month	Control	118,440	0.232	0.755	13
	MA	339,408	0.133	0.599	23
Generic prescriptions, per physician-month	Control	118,440	0.280	0.944	23
	MA	339,408	0.196	0.709	29
Monthly eligible members from physician's zip code	Control	118,440	722	394	2,410
	MA	339,408	589	356	1,746

Panel B: Antidepressants

Variable		*N*	Mean	Standard deviation	Maximum
Branded prescriptions, per physician-month	Control	154,008	0.143	0.658	17
	MA	473,760	0.060	0.414	28
Generic prescriptions, per physician-month	Control	154,008	0.393	1.486	46
	MA	473,760	0.386	1.863	69
Monthly eligible members from physician's zip code	Control	154,008	747	421	2,410
	MA	473,760	593	356	1,746

Panel C: Antipsychotics

Variable		*N*	Mean	Standard deviation	Maximum
Branded prescriptions, per physician-month	Control	62136	0.255	1.043	19
	MA	142,920	0.308	1.490	42
Generic prescriptions, per physician-month	Control	62,136	0.101	0.570	30
	MA	142,920	0.089	0.534	34
Monthly eligible members from physician's zip code	Control	62,136	741	408	2,410
	MA	142,920	595	357	1,746

antipsychotics (0.26 for physicians operating in the border areas of neighboring states) per month. The standard deviations in Table 3 also reveal that the physicians differed significantly in terms of the size of their patient pool.

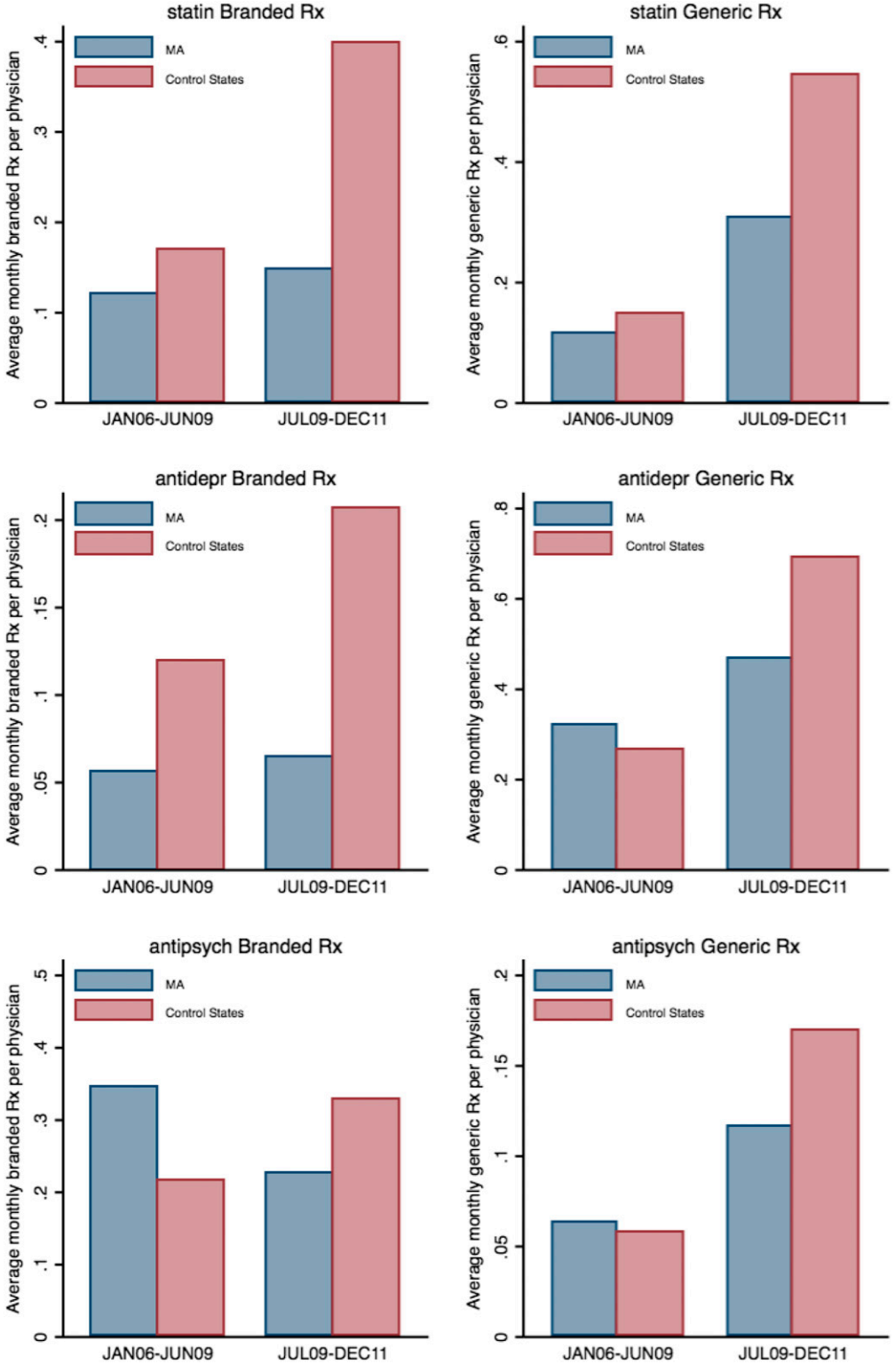
We treat January 2006–June 2009, that is, before the pharmaceutical firms started collecting payment data for disclosure, as the pretreatment period. Table 4 shows that, during the pretreatment period, MA and the control states follow directionally similar time trends in average monthly prescriptions across all three categories. However, there are some notable differences in the growth rates in prescriptions across state borders. Note that these differences are at the state level and might not be replicated at the individual physician level. Nevertheless, we discuss further robustness checks based on the synthetic control method that matches physicians in MA with a convex combination of physicians in the control states to address concerns regarding nonparallel trends in Section 3.2.1. The raw data patterns of monthly prescriptions per physician in Figure 3 suggest that the level difference between prescriptions in MA and the neighboring states increases from the preperiod to the postperiod in all three drug classes. Specifically, in the postperiod, the number of generic and branded prescriptions per physician increases considerably in the neighboring states.[11] On the other hand, the number of branded prescriptions per physician in MA either remains relatively stable or decreases post-legislation. Although generic prescriptions increased marginally in MA, the magnitude was much smaller compared with those in neighboring states. Together, these data suggest that the disclosure law might have had a negative impact on prescriptions, especially relative to control groups.

There are two possible alternative explanations for this pattern: (a) there was an exodus of patients from the insurance company that provided us the data in MA and/or (b) growth in the number of patients enrolled with the insurance provider in the neighboring states. In our empirical analysis, we control for the size of the patient pool for each physician in order to address these concerns.

Table 4. Average Monthly Growth Rate in Total Prescriptions (January 2006–June 2009) in the Regression Sample

State	Statins	Antidepressants	Antipsychotics
Control, %	9.23	6.65	4.98
MA, %	5.43	2.95	1.58

Figure 3. (Color online) Average Monthly Prescriptions per Physician by Class and State



3. Research Design

In order to understand the effect of the introduction of the MA disclosure law on prescriptions written by physicians, we exploit the idea that, although some states such as MA instituted payment disclosure laws, many other states did not. Specifically, we consider how physicians in MA changed the number and composition of prescriptions they wrote in three different drug classes—statins, antidepressents, 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, we use a difference-in-differences (DID) specification by comparing the changes among physicians in MA (i.e., the treated state) versus those in neighboring states, that is, CT, NY, RI, and NH (the control states). As noted earlier, we focus on physicians in counties on the border of MA and the neighboring states in order to ensure that the two physician groups are comparable. This approach of using contiguous border areas has been employed to investigate the impact of interventions in multiple domains (Card and Krueger 1994, 2000; Holmes 1998; Dube et al. 2010; Tucker et al. 2013; Shapiro 2018b). The identifying assumption is that the physicians located along the border have similar characteristics and face patient populations with similar needs for prescriptions in these three drug classes. Recall that the data in Table 1 support the notion that the border counties are comparable in terms of demographic and socioeconomic characteristics as well as private health insurance enrollment. In our regression, we further control for these key time-varying socioeconomic and health characteristics across the counties. In addition, we control for any state-specific characteristics that might induce differences in prescriptions via a rich set of fixed effects. 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, we also verified that, during the time period under study, there were no reported drug supply chain disruptions (e.g., shortages), public health condition shocks, or entry of new brands that vary across counties in MA and the neighboring states.

We consider the number of prescriptions written by each physician during a 72-month period between January 2006 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 we should define treatment, that is, the introduction of the disclosure law in MA. We check the sensitivity of the results to three different definitions of post-treatment periods based on the various temporal cut-offs related to the introduction of disclosure. Thus, the preperiod and three sets of postperiods are as follows: *Pretreatment period* ($T_0 \leq 42$): January 1, 2006–June 30, 2009; *Posttreatment period* 1 ($43 \leq T_1 \leq 72$): July 1, 2009–December 31, 2011, that is, after the law came into effect; *Posttreatment period* 2 ($55 \leq T_2 \leq 72$), that is, after the pharmaceutical companies reported the first set of payment data to the government: July 1, 2010–December 31, 2011; *Posttreatment period* 3 ($60 \leq T_3 \leq 72$): November 22, 2010–December 31, 2011, that is, after the payment information was made available online to the public. This enables us to assess the robustness of our results to multiple measures of policy intervention. In a similar vein, studying the effect of disclosure laws on prescriptions in three different drug classes helps us assess whether the key results exhibit a generalizable pattern. In addition, we verify robustness in three different ways: (a) dropping CT from the set of control states to see if some of its unique policies might have contaminated the results, (b) using synthetic controls to construct a convex combination of units within the control states that are similar in terms of preintervention prescription trends to the corresponding units in the control state (i.e., MA), and (c) performing the analysis with a non-neighboring state (IL) to verify if the results are driven by substitution of payments among physicians operating in MA and the bordering states by local sales teams.

3.1. Physician Panel Regression

As mentioned, we consider prescriptions written by each physician over a 72-month period. In the first set of analyses, we 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 months 1 to 72. The state of MA shares borders with five states, namely CT and RI in the south, NY in the west, and VT and NH in the north. Of these, we excluded VT from the analysis because it had prior disclosure laws. We characterize these three border regions using the index $r, r \in \{North, South, West\}$. Let Rx_{ibt} indicate the average monthly new prescriptions written by physician i from state s for drug $b, b \in \{branded, generic\}$ in month t. We first estimate the following specification:

$$Rx_{ibt} = \alpha_b I_{MA} I_t + I_{ib} + \lambda_{rbt} + X_{it}\beta + \epsilon_{ibt}, \qquad (1)$$

where I_{MA} is an indicator variable for MA and I_t is a posttreatment indicator variable that equals one if $t \geq 42$ (or 54 or 59 based on the definition of treatment) and zero otherwise. The coefficients $\alpha_{branded}$ and $\alpha_{generic}$ capture the causal impact of policy intervention on the prescriptions of branded and generic drugs by MA physicians. A negative $\alpha_{branded}$ would indicate that the policy intervention discouraged physicians from prescribing more (expensive) branded drugs. We include physician-brand fixed effects, I_{ib} to control for systematically different prescription levels across physicians for branded versus generic drugs. Recall that the average number of prescriptions written by physicians in MA was somewhat different from those in neighboring states. The fixed effects control for these cross-sectional differences in the average number of prescriptions. Similarly, recall that the growth rates in Table 4 indicate an overall positive trend in prescriptions in MA and in the border counties of the

Table 5. Average Prescription Level and Monthly Patients by Physician Group, January 2006–December 2011

Panel A: Statins						
	Control			MA		
Average per physician-month	Light	Medium	Heavy	Light	Medium	Heavy
Branded prescriptions	0.029	0.086	0.602	0.016	0.057	0.356
Generic prescriptions	0.039	0.108	0.719	0.035	0.108	0.492
Panel B: Antidepressants						
	Control			MA		
Average per physician-month	Light	Medium	Heavy	Light	Medium	Heavy
Branded prescriptions	0.019	0.036	0.386	0.006	0.018	0.166
Generic prescriptions	0.061	0.108	1.043	0.055	0.130	1.029
Panel C: Antipsychotics						
	Control			MA		
Average per physician-month	Light	Medium	Heavy	Light	Medium	Heavy
Branded prescriptions	0.044	0.063	0.706	0.035	0.056	0.901
Generic prescriptions	0.025	0.029	0.265	0.018	0.025	0.244

control states. We control for temporal trends in prescriptions that are common across the treated and control states but different across brands and border regions by including λ_{rbt}, a series of region and brand-specific month dummies. Note that, in this specification, the state main effects have been absorbed by physician–drug dummies (I_{ib}), and the posttreatment main effect has been absorbed by region–brand–month dummies (λ_{rbt}). Moreover, recall that our data pertain to prescription claims made on behalf of patients who are enrolled in insurance plans offered by the focal firm. Therefore, we may observe changes in prescription claims as the number of enrollees changes over time. We control for changes in the number of enrollees over time by including the total number of patients in the focal provider's insurance pool in the zip code in which the physician operates in month t as a covariate in X_{it}. In addition, we control for county-level private and public health insurance coverage (obtained from the Census Bureau's American Community Survey one-year estimate) in X_{it}. We cluster errors by physicians.

We expand this analysis by exploring the heterogeneous effects of the policy intervention on light, medium, and heavy prescribers. As noted earlier, we 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–June 2009 (i.e., before any treatment occurs).[12] 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, either because firms altered their payments or physicians were less willing to accept them, these physicians are more likely to change their prescription behavior. Moreover, heavier prescribers have a greater scope for reducing their prescriptions compared with their lighter counterparts. Therefore, we expect them to be more responsive to the policy intervention by cutting down on excessive prescriptions. In order to capture this difference in responsiveness to the policy intervention, we estimate different treatment effects (one for branded and one for generic) for each physician group g separately:

$$Rx_{ibt} = \alpha_g^b I_{MA} I_t + I_{ib} + \lambda_{rbt} + X_{it}\beta + \epsilon_{ibt}. \quad (2)$$

Thus, we 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 $g \in \{light, medium, high\}$. Throughout our panel regressions, we log-transformed[13] the dependent variable to correct for skewness.

3.2. Robustness Checks

We expand the basic research design outlined herein by performing a series of robustness checks (Goldfarb and Tucker 2014). First, CT introduced its own set of laws requiring companies to adopt policies consistent with the PhRMA Code or AdvaMed's Code of Ethics on Interactions with Health Care Professionals as of May 2010.[14] These changes might potentially contaminate our estimates of the treatment effect of the MA disclosure law. In order to address this concern,

we perform a robustness check by dropping CT from the set of control states.

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 the alternative quality contract (AQC) in MA, which sought to compensate physicians based on health outcomes rather than treatments. Because our data are from a different insurance company that did not institute a similar change, our analysis should not be influenced by changes made by Blue Cross Blue Shield. Nevertheless, we check robustness of our findings by using a preperiod that excludes data prior to the introduction of the AQC; that is, we use January–June 2009 as an alternative preperiod. By considering data only after the introduction of the alternative quality contract, we rule out its influence in driving any changes in prescriptions in MA.

Third, the introduction of the MA disclosure law might have caused pharmaceutical firms to reassess their payments to physicians operating in that state. As a result, pharmaceutical representatives might have reallocated their payments across physicians operating in different states, albeit within the same border region. If such reallocation of payments indeed occurred, it would contaminate the validity of the research design. Therefore, as a robustness check, we estimate the model with physicians operating in a similar but geographically distant state, that is, IL. This robustness check is inspired by the top-down nature of the marketing budget allocation practice followed in the industry: the company sets a fixed overall (national) budget at the beginning of the calendar year, which is then allocated to geographic regions. The allocation of payments within each region is at the discretion of local sales teams. As each regional team usually takes care of a contiguous territory, it is unlikely that they reallocate funds (as a result of disclosure) *across* regions. Therefore, payment reallocation from MA to IL is less likely to be an issue relative to reallocation from MA to its neighboring states.[15]

We summarize all the robustness checks in Table 6.

3.2.1. Robustness Check Using Generalized Synthetic Control Method. In the panel regression, we attempt to control for any differences across the treatment and control states by (a) focusing on the contiguous border areas and (b) absorbing brand–physician differences in prescribing behavior. Moreover, we control for common time-varying unobservables with region–brand–time fixed effects. That is, we allow branded (vs. generic) specific time trends to vary by region. We further check the sensitivity of our results via the use of three different drug classes, different preperiods and postperiods, 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 and the control states (i.e., CT, NY, NH, and RI) might have followed different temporal trends in drug prescriptions that could not be fully explained by region–brand–time fixed effects. Although 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 2017). The idea behind the method is that the synthetic control unit closely represents the unit in the treated state based on the outcome of interest. In our case, we construct a synthetic clone unit in the control states that mimics the preperiod prescription trend for each unit in MA. Because our analysis involves applying the idea of synthetic controls to a large number of treated units (i.e., physicians), we use the generalized synthetic control (GSC) method (Xu 2017).

The main rationale for identification in the synthetic control methods, including the GSC, is very similar to the idea behind the DID estimator. Specifically, once the treatment and synthetic control units (which are created from units in the control states) are matched in terms of pretreatment outcomes, any deviation that happens posttreatment is caused by the treatment, that is, the MA disclosure. In addition to matching on

Table 6. Robustness Checks

Table	Sample used	Purpose
8	MA-neighbor border counties, excluding CT	Excluding debated state
9	MA versus IL	Alternative control state
10	MA-neighbor border counties, zip code average	Corrects for parallel trends in preperiod
12	MA-neighbor border counties, preperiod is January–June 2009	Alternative preperiod[a]
B.1	MA-neighbor border counties, excluding December 2011	Excluding off-patent period

[a]Blue Cross Blue Shield introduced alternative quality contract in Massachusetts starting 2009. Although our data are from a different insurance company and should not be influenced by changes in BCBN, we check robustness of our findings using a preperiod that excludes the temporal point when the changes occurred.

outcomes in the pretreatment period, the method also permits us to include controls for all observable shifters, such as local income level, private and public health insurance coverage, patient enrollment in our data, and seasonality, that are unrelated to the treatment. The assumption on the unobservables, as is typical for such causal inference methods, is that they are unrelated to the treatment.

The first step in GSC is a factorization of the complete prescription sequences in the control units: it decomposes them into a common set of factors of length t. The factors summarize the effects from unobserved time-varying prescription shifters that are influencing all units in the control states. Assuming that the same set of latent factors also influence the treated units (but to different degrees through a different linear combination), in its second step, GSC estimates the loadings for the treated units only using their pretreatment prescription sequences. Finally, GSC projects the prescriptions of the treated units to the posttreatment periods, using the factors built from the controls and the loadings estimated from the pretreatment prescriptions of the treated. Intuitively, these projected prescriptions represent the counterfactuals in the treated group should there be no disclosure. Thus, by comparing the projected outcomes to the actual prescriptions, we obtain an estimate of the average treatment effect.

There are two issues to note here. First, the degree to which a unit is influenced by a latent driving force can be different. For example, firm strategies in MA and CT might follow the same time trend, but the intensity of the trend in MA may be higher than CT. This is achieved by allowing the loadings (scalars) to be different across the units, and factors ($t \times 1$ vectors) are held constant. In a nutshell, these factors and factor loadings allow for flexible time fixed effects across cross-sectional units. Second, these factors are typically not interpreted.

To evaluate the statistical significance of the estimate, the GSC method constructs a bootstrapped distribution of the estimate in a way similar to the placebo test in the traditional synthetic control method. When there are sufficiently long preperiod 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).

The GSC method, 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 we consider prescriptions at the individual physician level). Thus, we aggregate the prescription data to the zip code level and use zip codes as our unit of analysis. Let Rx_{zt} indicate the average monthly new prescriptions written per physician in zip code z and month t, ($t = 1, 2, 3, \ldots, 72$). We use the following specification:

$$Rx_{zt} = D_{zt}\delta_z + X_{zt}\beta + F_t\Lambda_z + \epsilon_{zt}, \tag{3}$$

where the treatment indicator D_{zt} equals one if zip code z is from MA and t is after month 42 and equals zero otherwise. Here, δ_z captures the treatment effect for an average physician in zip code z that is treated; X_{zt} includes the intercept, the monthly patient pool per zip code, county-level annual household income (median), and county-level annual private and public health insurance coverage (ACS one-year estimate). Recall that the number of patients in a zip code accounts for changes in the number of patients enrolled in insurance plans offered by the data provider over time. The factors $F_t = [f_{1t}, \ldots, f_{kt}]'$ consists of K unobserved orthogonal factors (each have T values, $T = 72$) with $\Lambda_z = [\lambda_{z1}, \ldots, \lambda_{zk}]'$ the $(K \times 1)$ unknown factor loadings. Note that the treated and control units are influenced by the same set of factors. The number of factors, K, is fixed throughout month 1 to 72, and each zip code can have a different set of loadings on K factors. In practice, the number of factors is determined in a data-driven way using cross-validation.

Note that zip code and time fixed effects can be considered as two special cases of the unobserved factors by setting $f_t = 1$ (for zip code fixed effects) and $\lambda_z = 1$ (for time fixed effects). When bringing the model to data, we explicitly impose it as a model restriction so that we always have the two-way fixed effects. Zip code fixed effects absorb all cross-sectional differences that are constant across time. Time fixed effects absorb common intertemporal changes across all zip codes. ϵ_{zt} is the zero mean idiosyncratic error for zip code z and month t. We discuss model estimation and inference details in Appendix A.

4. Results

We present the results from our analysis for the three drug classes in Table 7. Recall that we include physician–brand specific fixed effects (cross-sectional differences in prescription behavior across physicians) and region–brand–time fixed effects to account for regional (i.e., different regions of the MA border) temporal trends in the prescriptions of branded and generic drugs. 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. Specifically, the estimated decline is 47%–54% for branded statins, 50%–56% for branded antidepressants, and 31%–32% for branded

Table 7. Panel Regression Results

Drug class	Statins			Antidepressants			Antipsychotics		
Treatment measure[a]	1	2	3	1	2	3	1	2	3
ATE, total branded prescriptions	−0.0865*** (0.00508)	−0.109*** (0.00635)	−0.125*** (0.00720)	−0.0452*** (0.00343)	−0.0497*** (0.00428)	−0.0555*** (0.00479)	−0.0758*** (0.0112)	−0.0838*** (0.0127)	−0.0820*** (0.0134)
ATE, total generic prescriptions	−0.0665*** (0.00620)	−0.0924*** (0.00786)	−0.109*** (0.00888)	−0.0671*** (0.00703)	−0.0893*** (0.00857)	−0.102*** (0.00934)	−0.0247*** (0.00614)	−0.0313*** (0.00784)	−0.0325*** (0.00875)
Brand–year–month–border fixed effects		Yes			Yes			Yes	
Physician–brand fixed effects		Yes			Yes			Yes	
ATE percentage, branded	−47.29	−51.89	−54.33	−50.24	−53.96	−56.09	−31.04	−32.27	−30.75
ATE percentage, generics	−28.26	−34.29	−37.30	−20.86	−25.37	−27.54	−25.33	−29.44	−29.41
ATE number, branded	−0.10	−0.13	−0.15	−0.05	−0.05	−0.06	−0.10	−0.11	−0.11
ATE number, generics	−0.08	−0.12	−0.14	−0.09	−0.13	−0.15	−0.03	−0.03	−0.04
N	915,696	763,080	712,208	1,255,536	1,046,280	976,528	410,112	341,760	318,976

Notes. Standard errors clustered at physician level. ATE percentage calculated at the mean. Control variables include monthly zip-level patient pool, annual county-level household income (median), private and public health insurance coverage (percentage).

[a]Treatments are measured by three 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).

***$p < 0.01$; **$p < 0.05$; *$p < 0.1$.

antipsychotics after controlling for the time-varying size of the patient pool in a physician's zip code, county-level annual household income (median), and county-level private and public health insurance coverage.

In addition, we find that the prescriptions for generic drugs decreased as a result of disclosure. Specifically, the decline is between 28% and 37% for generic statins, 21%–28% for generic antidepressants, and 25%–29% for generic antipsychotics. These results suggest that, although generic prescriptions declined as a result of the MA disclosure law, the effect was more pronounced for branded drugs. Based on our estimates, the percentage difference between changes in generics and the branded drugs ranges from 17% to 19% in the case of statins, around 29% in the case of antidepressants, and 1%–6% in the case of antipsychotics. In addition to the percentage changes in prescriptions, we also report the change in the implied number of prescriptions per month for an average physician. These results suggest that, when we consider the change in number of prescriptions, generics saw a greater decline than their branded counterparts in the statin and antidepressant categories, and the opposite is true for antipsychotics. These results are somewhat different from the percentage changes reported earlier because generic prescriptions are more common in the statin and antidepressant categories, and branded prescriptions are common among antipsychotics.

Overall, our analysis highlights two key sets of results: (a) the disclosure law led to a decrease in branded and generic prescriptions, and (b) the decrease is more pronounced for branded rather than generic prescriptions. Nevertheless, the fact that both branded and generic prescriptions declined suggests that the MA disclosure law led to an overall decline in prescriptions in these three categories. At the same time, we cannot rule out the possibility that physicians switching from branded to generic drugs also contributed to these effects. In what follows, we investigate the robustness of these key findings to alternative definitions of control units and estimation approaches.

We report the robustness check results following the discussion in Section 3.2: Robustness to Changes in Control States. In our first robustness check, we drop CT from the pool of control states to eliminate the potential contamination from its own set of regulations. We present the results from this analysis in Table 8. These results reveal that (a) the disclosure law led to a decrease in branded and generic prescriptions, and (b) that the decrease is more pronounced for branded rather than generic prescriptions continues to hold. In our second robustness check, we use a distant state (IL) as the control state to alleviate the concern that pharmaceutical representatives might have reallocated their payments across physicians operating in MA and the adjacent states. We present the results in Table 9. Once again, these results suggest that the two main findings still hold. Moreover, the magnitude of the percentage change in prescriptions is consistent with the results presented earlier.

Table 8. Panel Regression Results, Excluding CT

Drug class	Statins			Antidepressants			Antipsychotics		
Treatment measure[a]	1	2	3	1	2	3	1	2	3
ATE, total branded prescriptions	−0.110*** (0.0197)	−0.0997*** (0.0205)	−0.0981*** (0.0222)	−0.0617*** (0.0105)	−0.0408*** (0.0110)	−0.0335*** (0.0114)	−0.0699*** (0.0216)	−0.0793*** (0.0232)	−0.0784*** (0.0253)
ATE, total generic prescriptions	−0.145*** (0.0289)	−0.152*** (0.0329)	−0.150*** (0.0345)	−0.172*** (0.0279)	−0.171*** (0.0315)	−0.164*** (0.0328)	−0.00839 (0.0142)	−0.0196 (0.0203)	−0.0212 (0.0230)
Brand–year–month–border fixed effects		Yes			Yes			Yes	
Physician–brand fixed effects		Yes			Yes			Yes	
ATE percentage, branded	−53.93	−50.02	−48.38	−58.32	−49.21	−43.56	−29.45	−31.25	−30.00
ATE percentage, generics	−47.42	−47.15	−45.93	−41.66	−40.54	−38.91	−10.31	−20.79	−21.44
ATE number, branded	−0.13	−0.12	−0.12	−0.07	−0.04	−0.04	−0.09	−0.10	−0.10
ATE number, generics	−0.19	−0.20	−0.20	−0.25	−0.26	−0.25	−0.01	−0.02	−0.02
N	695,376	579,480	540,848	972,288	810,240	756,224	293,904	244,920	228,592

Notes. Standard errors clustered at physician level. ATE percentage calculated at the mean. Control variables include monthly zip-level patient pool, annual county-level household income (median), private and public health insurance coverage (percentage).
[a]Treatments are measured by three 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).
***$p < 0.01$; **$p < 0.05$; *$p < 0.1$.

4.1. Robustness with Synthetic Controls

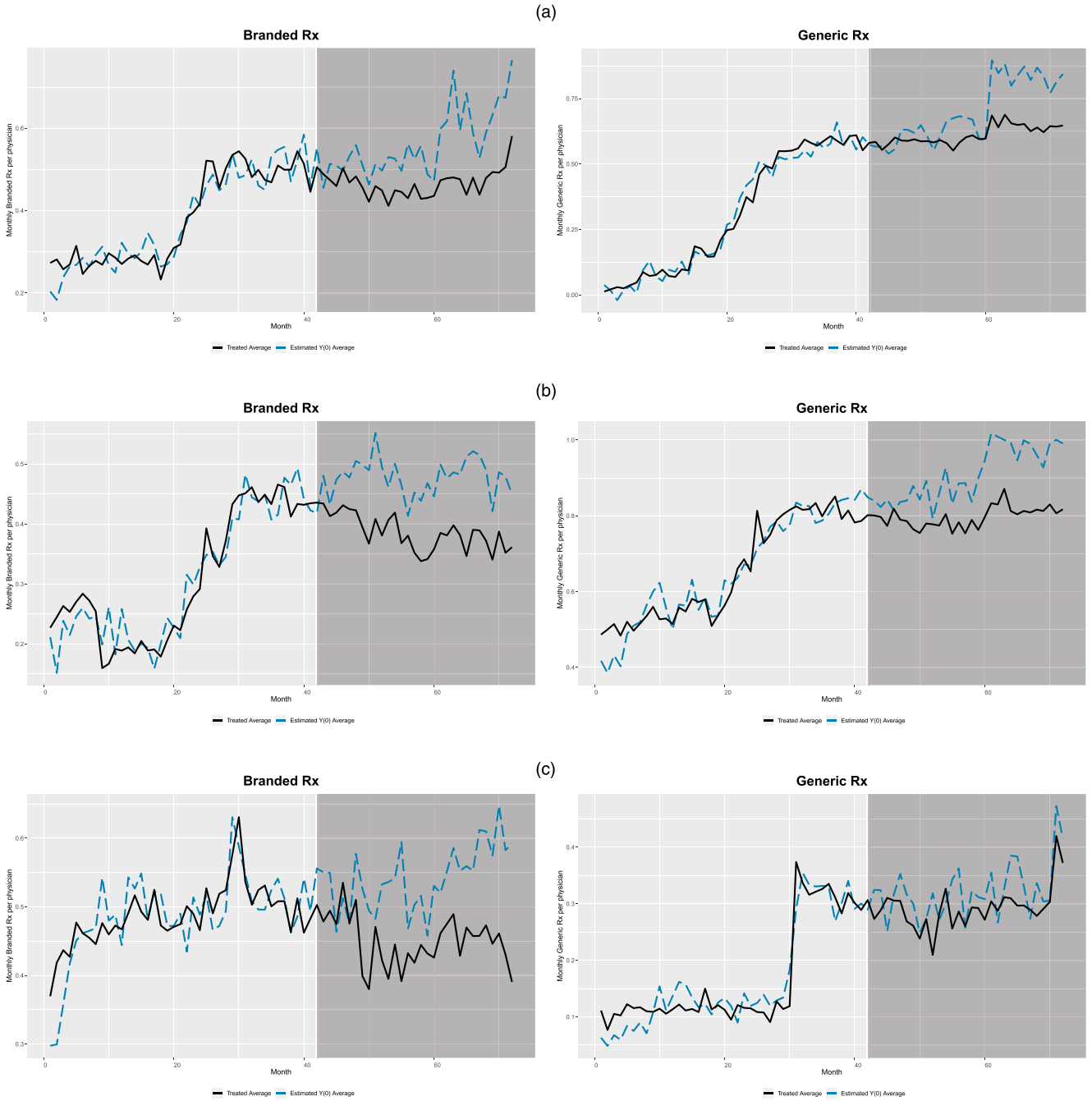
Recall that the premise behind our analysis is that the physicians in the control state help in projecting the counterfactual prescriptions that would have been written by physicians in MA had the disclosure law not been instituted. This is accomplished by creating a combination of units in the control states (i.e., CT, NY, NH, and RI) that would match each unit in the treated state (MA) based on some characteristics. In our application, we match the control and treated zip codes based on the average monthly prescriptions per physician in months 1 to 42 (i.e., the pretreatment period). This helps us project the counterfactuals for the treated zip codes if the treatment had not occurred. In Figure 4, we present the time series of the average monthly prescriptions for the treated group

Table 9. Panel Regression Results, MA vs. IL

Drug class	Statins			Antidepressants			Antipsychotics		
Treatment measure[a]	1	2	3	1	2	3	1	2	3
ATE, total branded prescriptions	−0.0727*** (0.00247)	−0.0786*** (0.00288)	−0.0812*** (0.00308)	−0.0518*** (0.00183)	−0.0556*** (0.00215)	−0.0569*** (0.00227)	−0.0877*** (0.00553)	−0.0955*** (0.00624)	−0.0950*** (0.00651)
ATE, total generic prescriptions	−0.0505*** (0.00294)	−0.0592*** (0.00349)	−0.0587*** (0.00374)	−0.0638*** (0.00336)	−0.0767*** (0.00396)	−0.0794*** (0.00418)	−0.0142*** (0.00303)	−0.0174*** (0.00370)	−0.0175*** (0.00401)
Brand–year–month–border fixed effects		Yes			Yes			Yes	
Physician–brand fixed effects		Yes			Yes			Yes	
ATE percentage, branded	−44.17	−44.87	−44.42	−55.88	−58.86	−58.76	−36.50	−37.46	−36.20
ATE percentage, generics	−23.66	−25.67	−24.81	−21.12	−23.63	−23.78	−18.10	−20.82	−20.28
ATE number, branded	−0.08	−0.09	−0.09	−0.06	−0.06	−0.06	−0.11	−0.12	−0.12
ATE number, generics	−0.06	−0.07	−0.07	−0.09	−0.11	−0.11	−0.02	−0.02	−0.02
N	2,258,784	1,882,320	1,756,832	2,964,384	2,470,320	2,305,632	813,312	677,760	632,576

Notes. Standard errors clustered at physician level. ATE percentage calculated at the mean. Control variables include monthly zip-level patient pool, annual county-level household income (median), private and public health insurance coverage (percentage).
[a]Treatments are measured by three 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).
***$p < 0.01$; **$p < 0.05$; *$p < 0.1$.

Figure 4. (Color online) Time Series of Treated and Synthetic Controls, January 2006–December 2011



Notes. DV is average monthly prescriptions per physician. Month 1 is January 2006 and month 72 is December 2011. MA is represented in the solid line and synthetic control the dotted line. The shaded periods are months after June 2009.

(MA, solid line) and the synthetic control group (border states taken together, dotted line) for the three categories, with month 1 = January 2006 and month 72 = December 2011. The postdisclosure periods are shaded. As can be seen from the figure, the prescription trends across the treatment and (the synthetic) control are very similar predisclosure but diverge after disclosure, thus providing evidence for the treatment effect. We present the estimated average treatment effect (ATE) from this analysis in Table 10.

Overall, the GSC method replicates the significant negative effects found in the panel regression (Table 10 with bootstrapped standard errors from a placebo test). The only exception is branded antidepressants for which the GSC estimates are not statistically significant for some definitions of the treatment.

Table 10. Average Treatment Effect Using Generalized Synthetic Control

	Statins	Antidepressants	Antipsychotics
Branded prescriptions per physician/zip			
Post 1, %	−17.28**	−19.58*	−17.14**
Post 2, %	−21.36**	−21.66	−21.30***
Post 3, %	−22.54**	−27.34	−25.18**
Generic prescriptions per physician/zip			
Post 1, %	−13.32**	−12.40***	−9.27*
Post 2, %	−18.92***	−14.49**	−15.11***
Post 3, %	−11.25***	−11.78*	−8.56*
Nzip, control	80	91	77
Nzip, treat	280	295	255

Notes. Standard errors are from a placebo test and are bootstrapped for 1,000 times. Controlling for monthly patient pool control, county-level annual income, health insurances coverage, and two-way fixed effects. ATE percentage evaluated at the mean counterfactual.
***$p < 0.01$; **$p < 0.05$; *$p < 0.1$.

However, the estimated treatment effects are generally lower compared with those from the panel regression. Specifically, the number of new prescriptions and renewals declined by 17%–23% for statins, 20%–27% for antidepressants, and 17%–25% for antipsychotics. For generics, the GSC estimation uncovers a significant decline across all three classes (11%–19% for statins, 12%–14% for antidepressants, and 9%–15% for antipsychotics). Overall, these results are consistent with the two main findings.

Although the GSC estimates are qualitatively consistent with those from panel regression, the former show relatively smaller average treatment effects than the latter. Recall that, although we performed the panel regression at the physician level, we consider zip code–level average prescriptions per physician in the GSC estimation. Therefore, the disparity in the magnitude of the average treatment effects might be due to a result of differences in data aggregation or the differences in the two methods (panel regression and the GSC). In order to tease out the most likely reason for the difference in the results, we estimate the panel regression at the zip code level (i.e., thus ensuring no difference in method) and present a comparison of the average treatment effects in Table 11. These results suggest that the average treatment effects at the zip code level fall in between the results from the zip code–level GSC estimates and the physician-level panel regression. Therefore, we conclude that the differences in the magnitudes of the average treatment effects between the panel regression and GSC are mostly driven by the difference in the level of data aggregation.

4.2. Robustness to an Alternative Preperiod

We check robustness of our findings using January–June 2009 as the preperiod after Blue Cross Blue Shield introduced the alternative quality contract in MA. We present the results from this analysis in Table 12. Although the estimated ATEs are smaller, they are largely consistent with our main findings. Specifically, prescriptions declined by 22%–32% for branded statins, by 12%–13% for branded antidepressants, and by 23%–27% for branded antipsychotics. With the new preperiod, the magnitude of decline is between 18% and 24% for generic statins, 15%–20% for generic antidepressants, and 19%–24% for generic antipsychotics. Therefore, the broad results that (a) prescriptions of branded and generic drugs declined as a result of the disclosure law and (b) branded drugs declined by a greater percentage compared with their generic counterparts continue to hold (with the exception of antidepressants).

4.3. The Role of Physician Heterogeneity

Recall that we classify physicians into three different groups—light, medium, or heavy—in each of the three drug classes based on the total number of prescriptions relative to others in MA and control states written by them during January 2006–June 2009, that is, before the policy change.[16] Table 5 reports the mean number of branded and generic prescriptions written each month by physicians in the three groups during 2006–2011. In general, light prescribers only write one prescription a year for each of the three drug classes. Medium prescribers write one to two prescriptions every 10 months, and heavy prescribers write at least one prescription per month. On average,

Table 11. Comparison of the Estimated Decline

		Statins	Antidepressants	Antipsychotics
Branded prescriptions	Zip-level panel regression	31.37%–35.56%	32.60%–38.70%	35.08%–36.93%
	Physician-level panel regression	47.29%–54.33%	50.24%–56.09%	30.75%–32.27%
	GSC	17.28%–22.54%	19.58%–27.34%	17.14%–25.18%
Generic prescriptions	Zip-level panel regression	13.94%–22.84%	30.00%-35.43%	30.79%–33.98%
	Physician-level panel regression	28.26%–37.30%	20.86%–27.54%	25.33%–29.44%
	GSC	11.25%–18.92%	11.78%–14.49%	8.56%–15.11%

Notes. Controlling for monthly patient pool control, county-level annual income and health insurances coverage, and two-way fixed effects. ATE percentage evaluated at the mean counterfactual.

Table 12. Panel Regression Results for MA & Border Counties with January–June 2009 as Preperiod

Drug class	Statins			Antidepressants.			Antipsychotics.		
Treatment measure[a]	1	2	3	1	2	3	1	2	3
ATE, total branded prescriptions	−0.0296*** (0.00565)	−0.0447*** (0.00733)	−0.0532*** (0.00842)	−0.00702 (0.00436)	−0.00708 (0.00559)	−0.00650 (0.00629)	−0.0496*** (0.0117)	−0.0600*** (0.0146)	−0.0546*** (0.0157)
ATE, total generic prescriptions	−0.0426*** (0.00626)	−0.0614*** (0.00831)	−0.0697*** (0.00936)	−0.0473*** (0.00709)	−0.0661*** (0.00913)	−0.0726*** (0.00998)	−0.0210** (0.00884)	−0.0296*** (0.0111)	−0.0274** (0.0122)
Brand–year–month–border fixed effects		Yes			Yes			Yes	
Physician–brand fixed effects		Yes			Yes			Yes	
ATE percentage, branded	−22.25	−29.17	−31.93	−12.59	−13.41	−12.20	−23.22	−26.64	−24.02
ATE percentage, generics	−17.94	−22.79	−24.34	−14.68	−18.84	−19.96	−19.15	−24.10	−21.82
ATE number, branded	−0.034	−0.051	−0.062	−0.007	−0.007	−0.007	−0.061	−0.075	−0.068
ATE number, generics	−0.054	−0.081	−0.093	−0.067	−0.097	−0.108	−0.023	−0.033	−0.031
N	186,768	124,512	103,760	585,432	390,288	325,240	186,768	124,512	103,760

Notes. Standard errors clustered at physician level. ATE percentage calculated at the mean. Control variables include monthly zip-level patient pool, annual county-level household income (median), private and public health insurance coverage (percentage).

[a]Treatments are measured by three 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).

***$p < 0.01$; **$p < 0.05$; *$p < 0.1$.

all three groups prescribe fewer branded statins and antidepressants than their generic counterparts but prescribe more branded antipsychotics than generics.

We check whether the policy has heterogeneous effects on different physician groups in Tables 13–15. The results suggest that physicians in all groups

Table 13. Heterogeneous Effects Across Physician Groups on Statin Prescriptions

Treatment measure[a]	1	2	3
ATE, total branded prescriptions, light	−0.0452*** (0.00451)	−0.0555*** (0.00572)	−0.0683*** (0.00657)
ATE, total branded prescriptions, medium	−0.105*** (0.00549)	−0.133*** (0.00688)	−0.154*** (0.00783)
ATE, total branded prescriptions, heavy	−0.137*** (0.00781)	−0.172*** (0.00928)	−0.191*** (0.0101)
ATE, total generic prescriptions, light	−0.0809*** (0.00572)	−0.0986*** (0.00729)	−0.114*** (0.00827)
ATE, total generic prescriptions, medium	−0.110*** (0.00714)	−0.144*** (0.00894)	−0.167*** (0.0100)
ATE, total generic prescriptions, heavy	−0.0184** (0.00881)	−0.0514*** (0.0109)	−0.0647*** (0.0121)
Brand-year-month-border fixed effects		Yes	
Physician-brand fixed effects		Yes	
Average treatment effect, %			
Branded × L	−67.16	−66.98	−69.52
Branded × M	−65.94	−70.04	−72.33
Branded × H	−40.81	−46.42	−48.27
Generics × L	−62.77	−62.75	−64.49
Generics × M	−49.85	−54.59	−57.72
Generics × H	−5.22	−13.18	−15.72
N	915,696	763,080	712,208

Notes. Standard errors clustered at physician level. ATE percentage calculated at the mean. Control variables include: monthly zip-level patient pool, annual county-level household income (median), private and public health insurance coverage (percentage).

[a]Treatments are measured by three 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).

***$p < 0.01$; **$p < 0.05$; *$p < 0.1$.

Table 14. Heterogeneous Effects Across Physician Groups on Antidepressant Prescriptions

Treatment measure[a]	1	2	3
ATE, total branded prescriptions, light	−0.0139***	−0.00805**	−0.0105**
	(0.00314)	(0.00399)	(0.00453)
ATE, total branded prescriptions, medium	−0.0561***	−0.0603***	−0.0674***
	(0.00374)	(0.00465)	(0.00522)
ATE, total branded prescriptions, heavy	−0.0836***	−0.103***	−0.112***
	(0.00507)	(0.00605)	(0.00653)
ATE, total generic prescriptions, light	−0.0699***	−0.0817***	−0.0904***
	(0.00649)	(0.00791)	(0.00866)
ATE, total generic prescriptions, medium	−0.0950***	−0.120***	−0.136***
	(0.00768)	(0.00936)	(0.0102)
ATE, total generic prescriptions, heavy	−0.0481***	−0.0834***	−0.0981***
	(0.0105)	(0.0124)	(0.0132)
Brand–year–month–border fixed effects		Yes	
Physician–brand fixed effects		Yes	
Average treatment effect, %			
Branded × L	−62.21	−46.70	−52.28
Branded × M	−75.72	−77.00	−78.37
Branded × H	−44.65	−52.02	−53.86
Generics × L	−49.82	−49.63	−50.63
Generics × M	−43.95	−48.28	−51.27
Generics × H	−9.39	−15.21	−17.30
N	1,255,536	1,046,280	976,528

Notes. Standard errors clustered at physician level. ATE percentage calculated at the mean. Control variables include: monthly zip-level patient pool, annual county-level household income (median), private and public health insurance coverage (percentage).

[a]Treatments are measured by three 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).

***$p < 0.01$; **$p < 0.05$; *$p < 0.1$.

decreased their prescriptions of both branded and generic drugs with the only exception being light prescribers. Interestingly, for both branded and generic drugs, light and medium prescribers show a larger percentage decline than the heavy prescribers although the absolute change is bigger for the latter. A possible explanation is that lighter prescribers have lower average levels of prescriptions compared with their heavier prescribing counterparts (as seen in Table 5). Therefore, a small change in the absolute number of prescriptions results in a large change in percentage terms. Comparing the change in branded drugs 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, we find that the biggest impact is for the heavy prescribers (29% on average) compared with medium prescribers (10% on average) and light (−10% on average). This indicates that the policy intervention led to greater decline in branded prescriptions compared with generics across almost all physician types in the three categories.

4.4. Discussion

Our empirical analyses highlight three key sets of results. First, disclosure changes physician prescription behavior. Specifically, it lowers prescriptions for both branded and generic drugs. This finding is robust across different estimation methods, different drug classes, and different control groups. Second, the MA disclosure led to a larger drop in branded prescriptions (relative to generics). Third, the gap in the decline of branded versus generic prescriptions was larger among heavier prescribers. Although these results clearly articulate what happened as a result of the MA disclosure law, in what follows, we discuss why these changes might have occurred and present conjectures on the plausible implications.

As noted earlier, there are three plausible reasons why disclosure laws could alter prescription behavior among physicians: (a) pharmaceutical companies might reduce payments to physicians, (b) physicians might become less open to accepting payments, and (c) physicians might resort to self-monitoring and become reluctant to prescribe medications. Because generic drug manufacturers typically do not make

Table 15. Heterogeneous Effects Across Physician Groups on Antipsychotic Prescriptions

Treatment measure[a]	1	2	3
ATE, total branded prescriptions, light	0.00417	0.0108	0.0169
	(0.00954)	(0.0109)	(0.0116)
ATE, total branded prescriptions, medium	−0.0371***	−0.0417***	−0.0422***
	(0.00994)	(0.0115)	(0.0123)
ATE, total branded prescriptions, heavy	−0.231***	−0.259***	−0.258***
	(0.0188)	(0.0206)	(0.0213)
ATE, total generic prescriptions, light	−0.00928	−0.00610	−0.00377
	(0.00572)	(0.00752)	(0.00857)
ATE, total generic prescriptions, medium	−0.0349***	−0.0400***	−0.0427***
	(0.00623)	(0.00796)	(0.00882)
ATE, total generic prescriptions, heavy	−0.0503***	−0.0681***	−0.0702***
	(0.0105)	(0.0127)	(0.0138)
Brand–year–month–border fixed effects		Yes	
Physician–brand fixed effects		Yes	
Average treatment effect, %			
Branded × L	8.81	21.94	34.36
Branded × M	−40.56	−42.80	−42.80
Branded × H	−43.10	−45.93	−45.04
Generics × L	−26.53	−15.41	−9.10
Generics × M	−54.81	−55.37	−56.65
Generics × H	−23.86	−30.54	−30.69
N	410,112	341,760	318,976

Notes. Standard errors clustered at physician level. ATE percentage calculated at the mean. Control variables include: monthly zip-level patient pool, annual county-level household income (median), private and public health insurance coverage (percentage).

[a]Treatments are measured by three 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).

***$p < 0.01$; **$p < 0.05$; *$p < 0.1$.

direct payments to physicians, the decline in generic prescriptions suggests that (c) is likely to be a strong driver of the observed decline in prescriptions as a result of the MA disclosure law. However, because we do not observe payments to these physicians prior to the disclosure law, we cannot rule out the possibility that changes in these payments also contributed to the decline in branded prescriptions.

The results also shed some light on what drove the change in prescriptions as a result of the MA disclosure law: physicians switching from branded to generic drugs or becoming more conservative in prescribing medications overall (i.e., both branded and generic). The finding that both branded and generic prescriptions declined suggests that physicians in MA prescribed fewer medications overall. However, the fact that branded prescriptions declined more than those for generic equivalents suggests that some switching from the former to the latter might also have played a role in driving the results.

One specific feature of the regulatory environment in MA could have played a role in this behavior (switching from a branded to a generic drug prescription). During the window of our study, MA and its neighboring states had (state) laws that required pharmacists to substitute branded prescriptions with their existing generic equivalents unless the prescribing physician explicitly recommends against this substitution.[17] Given this, MA physicians have three choices when prescribing drugs: (1) prescribe a branded drug without generic substitutes or explicitly ask the pharmacist not to substitute with a generic; (2) prescribe a branded drug with generic substitutes, which eventually gets substituted with generics mandatorily; or (3) prescribe a generic drug. Given the mandatory generic substitution law in MA, prescription types (2) and (3) would be observationally equivalent in our data. Therefore, it is possible that the decline in generic prescriptions might have arisen from a reduction in prescriptions of type (2).

Nevertheless, we believe that this does not negate the role of physician self-monitoring in driving the decline in prescriptions as a result of the MA disclosure law for three reasons. First, given that the MA disclosure law was well in place during the period of our analysis, physicians are likely to have internalized the fact that type (2) prescriptions are equivalent to prescribing generics. In other words, physicians know that, when they prescribe a branded drug, it will be substituted to a generic (unless they forbid

it explicitly). This implies that the changes in generic prescriptions arising from corresponding changes in (2) or (3) can be viewed as equivalent, that is, not involving switching between branded and generic drugs. Second, an alternative to the self-monitoring explanation is that the MA disclosure law altered payments from pharmaceutical companies, which resulted in fewer branded prescriptions. Although we agree that this is definitely plausible, we do not believe that this would have contributed to fewer prescriptions of type (2) because firms are likely to reward physicians based on drugs dispensed rather than prescribed.[18] Third, even if type (2) prescriptions drove some of the decline in generic prescriptions, it is unlikely that they contributed to *all* the decline in *all* three drug categories; that is, some of the decline is likely to have arisen from (3) as well.

Another noteworthy aspect of our results is that the extent of decline seems to be economically "big." The individual-level results (which are very robust) suggest that, on average, the drop in branded statins is 51%, branded antidepressants is 53%, and branded antipsychotics is 31%. Similarly, the average drop in generic statins is 33%, generic antidepressants is 25%, and generic antipsychotics is 28%. Looking at the estimates from the GSC analysis carried out at the zip code level, the average drop is 20% for branded statins (15% for generics), 23% for branded antidepressants (13% for generics), and 21% for branded antipsychotics (11% for generics). Consistent with our findings, other researchers have also found drops of similar magnitudes when policies directed at physicians change. For example, King and Bearman (2013) report that the prescriptions of four newly marketed mental health medications drop by 39%–83% in states that prohibited pharmaceutical gifts to doctors. Thus, the effect sizes reported here, although large, do not seem to be idiosyncratic to our analysis.

If the decline is indeed driven by self-monitoring, there are two possible mechanisms that could lead to this. We label these two mechanisms as higher under-prescription or lower over-diagnosis. Under-prescription refers to the mechanism by which physicians are reluctant to conclude that the patient's condition warrants treatment via drug prescription (e.g., they may advocate weight loss, dietary control, and lifestyle change rather than a statin to prevent cardiovascular disease).[19] Under this mechanism, disclosure leads to physicians' increasing the degree of under-prescribing, therefore lowering prescriptions. Over-diagnosis is the mechanism by which physicians overestimate the need for medication in order to err on the safe side. In contrast to the mechanism of under-prescribing, under the over-diagnosis mechanism, disclosure leads to physicians refraining from making positive diagnoses, resulting in lower prescriptions. Although it may be tempting to conclude that lower prescriptions are a positive outcome, there is some evidence that under-prescribing can lead to worse health outcomes (see Carey et al. 2015). Thus, one might need to consider all these nuances when commenting on the overall social, medical, and economic impact of the disclosure.

5. Conclusion

This paper adds to the growing literature on the impact of mandated transparency on healthcare participants and institutions with a specific focus on how marketing regulations influence physician behavior. Specifically, using very high-quality behavioral data and a rich set of controls, this paper 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 MA 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 most likely reason for the change in behavior is self-monitoring by physicians rather than a change in the direct payments regime on the firm side. We show the robustness of our 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 alternative control groups, and the use of a different preperiod (January 1, 2009 to June 30, 2009). Across all these checks, our overall finding is that there is negative and significant impact on prescriptions as a result of the introduction of the disclosure law.

Our 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 we 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 establish the existence of over-diagnosis or under-prescription or both and then to develop the appropriate policies to manage these? 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?[20] 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.

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Appendix A. Generalized Synthetic Control Estimation

Appendix A reports additional robustness check details using generalized synthetic control methods and the sample from MA and border counties from all neighboring states (except Vermont). Let Rx_{zt} indicate the average monthly new prescriptions written per physician in zip code z and month t (1 to 72). We 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 one if zip code z is from MA and t is after month 18 and equals zero otherwise. δ_z captures the heterogeneous treatment effect for an average physician in zip code z that is treated. X_{zt} includes a constant term, the monthly patient pool per zip code, county-level annual household income (median), and county-level annual private and public health insurance coverage. It captures the time-varying demand shocks, such as a local insurance plan membership expansion. $F_t = [f_{1t}, \ldots, f_{rt}]'$ is a vector of r unobserved common factors (each have T values, $T = 72$) with $\Lambda_z = [\lambda_{z1}, \ldots, \lambda_{zr}]'$ the $(r \times 1)$ 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 months 1 to 72. However, each zip code can have a different set of loadings on r factors. Note that zip code and time fixed effects can be considered as two special cases of the unobserved factors by setting $f_t = 1$ and $\lambda_z = 1$. We explicitly impose it as a model restriction so that we always have the two-way fixed effects.

A.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.

$$(\hat{\beta}, \hat{F}, \hat{\Lambda}_{co}) = \arg\min \sum_{z \in co} (Rx_{zt} - X_{zt}\beta - F_t\Lambda_z)' \times (Rx_{zt} - X_{zt}\beta - F_t\Lambda_z), \quad z \in \textit{Control State}$$
$$\text{s.t. } F'F/T = I_r, \Lambda_z'\Lambda_z = \textit{diagonal}.$$

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 pretreatment period data.

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

Step 3: Calculate treated counterfactuals for the posttreatment 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, we obtain an estimator for average treatment effect on the treated by averaging the differences in the observed and counterfactual outcomes across all zip codes in MA during the posttreatment 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 preperiods, holds back one period's data of all treated units in step 2, estimates the loadings, predicts the outcomes for the holdout sample, and obtains 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 preperiods.

A.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 resampling with replacement from the remaining control units. Apply the GSC method and obtain the vector of residuals for this pseudo-treated unit: $\hat{\epsilon}^p_{(m)} = Rx_i - \hat{Rx}_i(0)$ for iteration round m and pseudo-treated unit i. Do this for B1 times and collect all B1 vectors of prediction errors: $\hat{\epsilon}^p = \{\hat{\epsilon}^p_{(1)}, \ldots, \hat{\epsilon}^p_{(B1)}\}$. This is the constructed sample of prediction errors for the

treated units. Note that, from the estimation part, we have already obtained the original set of residuals for the control units: $\hat{e} = \{\hat{\epsilon}_1, \ldots, \hat{\epsilon}_{N_{co}}\}$.

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

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 the last step:

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

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

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

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

Its confidence interval is obtained using the percentile method as in Efron and Tibshirani (1994). Xu (2017) has shown in Monte Carlo exercises that the 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 $N_{co} > 40$),[21] the cross-validation procedure recovers the correct number of factors reasonably well.[22]

In Figure 4, we present the time series of the average monthly prescriptions for the treated group (MA, solid line) and the synthetic control group (bordering states as a whole, dotted line) for three categories with month 1 = January 2006 and month 72 = December 2011. The postdisclosure periods are shaded. The curves of the treated and the synthetic controls overlap nicely predisclosure, and the two groups diverge after the disclosure. This visual inspection suggests decent matching between the treated and the control through the GSC method. The average difference across the treated and the control, post compared with pre, is statistically significant for three categories (Section 4.1).

Appendix B. Robust Check of Lipitor Patent Expiration

Appendix B reports robustness checks by dropping the last month of 2011 when, toward the end of our observation window, one major statin brand (Lipitor) went off patent. Because patent expiration is likely to affect prescriptions in all states, the region–brand–month fixed effects should be able to pick them up. Given that these fixed effects vary by the border region (i.e., north, south, and west), they should be able to account for differential effects of patent expiration on prescriptions in these categories. However, one can argue that patent expiration could have had a different effect on prescriptions in MA versus in the neighboring states. As the patent for Lipitor expired on November 30, 2011, we perform a robustness check by dropping the last month of 2011. As is presented in Table B.1, our results do not change.

Table B.1. Panel Regression Results, Excluding December 2011

Drug class	Statins			Antidepressants			Antipsychotics		
Treatment measure[a]	1	2	3	1	2	3	1	2	3
ATE, total branded prescriptions	−0.0842*** (0.00499)	−0.106*** (0.00622)	−0.122*** (0.00707)	−0.0448*** (0.00342)	−0.0492*** (0.00429)	−0.0552*** (0.00483)	−0.0749*** (0.0112)	−0.0829*** (0.0127)	−0.0808*** (0.0135)
ATE, total generic prescriptions	−0.0648*** (0.00613)	−0.0908*** (0.00780)	−0.108*** (0.00887)	−0.0654*** (0.00698)	−0.0877*** (0.00854)	−0.101*** (0.00933)	−0.0233*** (0.00604)	−0.0294*** (0.00774)	−0.0302*** (0.00867)
Brand–year–month–border fixed effects		Yes			Yes			Yes	
Physician–brand fixed effects		Yes			Yes			Yes	
ATE percentage, branded	−47.19	−52.07	−54.76	−49.91	−53.66	−55.88	−30.62	−31.79	−30.11
ATE percentage, generics	−27.85	−34.01	−37.16	−20.46	−25.04	−27.30	−24.52	−28.64	−28.43
ATE number, branded	−0.10	−0.12	−0.15	−0.05	−0.05	−0.06	−0.09	−0.11	−0.10
ATE number, generics	−0.08	−0.12	−0.14	−0.09	−0.13	−0.15	−0.03	−0.03	−0.03
N	902,978	750,362	699,490	1,238,098	1,028,842	959,090	404,416	336,064	313,280

Notes. Standard errors clustered at physician level. ATE percentage calculated at the mean. Control variables include monthly zip-level patient pool, annual county-level household income (median), private and public health insurance coverage (percentage).

[a]Treatments are measured by three 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).

***$p < 0.01$; **$p < 0.05$; *$p < 0.1$.

Endnotes

[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 the District of Columbia (2003).

[4] The authors present a general model of payments to experts and show that, although mandatory disclosure discourages all payments, it does so more for payments paid by a more cost-efficient firm. We thank an anonymous reviewer for pointing this out.

[5] Although 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, for example, King and Bearman (2013). We discuss this in detail in Section 4.4.

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

[7] See https://www.mass.gov/service-details/background-information-about-the-pharmaceutical-code-of-conduct.

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

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

[10] Because of the presence of a nondisclosure agreement, we are unable to reveal the name or the exact market share of the insurer.

[11] Nationally, the number of prescriptions in the three classes were on the rise. Specifically, statin prescriptions for adults over 40 grew from 17.9% in the early 2000s to 27.8% in 2012 (Salami et al. 2017); antidepressant prescriptions increased from 6.4% in the early 2000s to 10.7% in 2011 (NCHS 2017); antipsychotics increased by 93% in 2011 compared with 2001 according to the marketing research firm IMS Health.

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

[13] $Y = \log(\mathrm{Rx}_{ibt} + 1)$.

[14] We thank an anonymous reviewer for pointing this out.

[15] Nevertheless, we acknowledge that we cannot fully rule out the possibility of payment reallocation between MA and IL and note this as a caveat.

[16] The conclusion from this analysis does not change if we define these three groups based on absolute number of prescriptions rather than a relative number.

[17] We thank an anonymous reviewer for pointing this out.

[18] Typically, firms compute physician drug market shares using pharmacy audit data; these reflect prescriptions filled as opposed to written.

[19] See, for example, Stampfer et al. (2000) and Hu et al. (2001).

[20] In Guo et al. (2017), the authors investigate the impact of the information disclosure as part of the Affordable Care Act on subsequent payments to physicians.

[21] In our case, we have $T_0 = 42$ and $N_{co}^{statin} = 80, N_{co}^{antidepr} = 91, N_{co}^{antipsych} = 77$ for MA and neighbor state border counties.

[22] We implement these estimation and inference procedures in R using -gsynth- package for branded and generic prescriptions for all three drug classes on the MA-adjacent border sample. We choose the number of factors (between 0 and 10) by cross-validation in addition to the two-way fixed effects. We bootstrap 1,000 times in a nonparametric procedure for uncertainty estimates. For technical details, see http://yiqingxu.org/software/gsynth/gsynth_examples.html.

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