Who pays for gifts to physicians?
Heterogeneous effects of industry payments on drug costs

MELISSA NEWHAM∗MARICA VALENTE†

Abstract
This paper estimates the impact of gifts – monetary or in-kind payments – from pharmaceutical firms on physicians’ prescription decisions and drug costs in the US. Using exhaustive micro data on prescriptions for antidiabetic drugs from Medicare Part D, we find that payments cause physicians to prescribe more brand drugs. On average, for every dollar received, payments generate a $6 increase in drug costs. Using causal forests, we show that differences in physician responses are predominantly explained by the insurance coverage of patients: physicians prescribe more brand drugs in response to payments when patients benefit from subsidies that reduce out-of-pocket drug costs. Finally, we estimate that a gift ban would reduce drug costs to treat diabetes by 3%.

JEL: I11, I18, M31
Key words: public health, payments to physicians, gift ban

∗Corresponding author. ETH Zurich and KU Leuven (e-mail: mnewham@ethz.ch).
†University of Innsbruck and DIW Berlin (e-mail: marica.valente@uibk.ac.at). We are very grateful to Elliot Ash, Albert Banal-Estanol, Tomaso Duso, Shan Huang, Paolo Pertile, Julien Sauvagnat, Fiona Scott Morton, Jo Seldeslachts, Otto Toivanen, Hannes Ulrich and Frank Verboven for helpful comments. This paper also benefited from comments in seminars and conferences at the EuHEA Seminar Series, DIW Berlin, the ETH/IZH Seminar in Economics & Data Science, UZH Business Department Seminar, the Zurich Political Economy Seminar Series and SESS Annual Congress. Parts of this research originate from a chapter of Newham’s PhD dissertation. The authors declare that they have no relevant or material financial interests that relate to the research described in this paper.
1 Introduction

Drug firms’ monetary and in-kind payments to physicians in the US totaled $2 billion in 2020.\(^1\) Concerned that payments may influence physicians’ prescribing decisions and lead to unnecessary and expensive prescriptions, a growing number of states have implemented, or considered, “gift bans.”\(^2\) Previous studies find that physicians increase prescribing of drugs for which they receive payments (Datta and Dave, 2017; Fernandez and Zejcirovic, 2018; Carey et al., 2021; Grennan et al., 2021; Agha and Zeltzer, 2022). Yet, the impact of gift bans on drug costs has remained unexplored. Further, if individual physicians differ in their response to payments, the impact of a gift ban may vary across states. Understanding which factors drive physicians’ responses to payments plays an essential role in evaluating the efficacy of gift bans.

In this paper, we estimate heterogeneous causal effects of payments on prescriptions for physicians in the US, explain differences in physicians’ responses, and quantify the aggregate impact of a gift ban on drug costs for certain states. The key problem in determining the effect of payments on prescriptions is physician heterogeneity coupled with the fact that receiving a payment is likely to be non-random. We address these challenges by estimating heterogeneous causal effects of payments using causal forests, a method that allows to flexibly control for a high-dimensional set of outcome determinants and sources of selection (Wager and Athey, 2018, 2019; Athey et al., 2019). We allow physicians to be heterogeneous not only in their observable characteristics but also in their payment (or “treatment”) levels. By undertaking a data-driven search for causal effect heterogeneity, the algorithm allows us to uncover potentially unexpected drivers of physicians’ responses which theory-driven subgroup analysis might have missed.\(^3\)

We estimate that, on average, for every dollar received, payments generate a $6 increase in drug costs. Larger payments cause physicians to prescribe more brand drugs, ceteris paribus, although payment value matters less when a high

\(^{1}\)US payment data is publicly disclosed since mid-2013. Payments include, e.g., meals and consulting fees. Similarly in Europe payments to physicians are frequent, however there is less transparency regarding their value (Fabbri et al., 2018).

\(^{2}\)The state of Vermont implemented a strict gift ban in 2009. California passed a gift ban in 2019 (Sullivan, 2018), and the city of Philadelphia considered a gift ban in 2019 (Brennan, 2019).

\(^{3}\)Prior applications to quasi-experiments include, e.g., Britto et al. (2022) who estimate the effects of job loss on crime, and Valente (2021) who evaluates the impact of waste pricing policies.
share of patients benefit from subsidies that reduce out-of-pocket drug costs. We estimate that a gift ban would reduce drug costs to treat diabetes by 3%. As the largest savings can be expected to come from lower prescriptions of brand drugs to subsidized patients, the public healthcare system (and the taxpayer) will save on drugs cost.

Our analysis leverages rich micro data, linking information from Open Payments, a federal database on the universe of payments to physicians, with data on physicians and their prescriptions from Medicare Part D for the period 2014-2017. We focus on payments and prescriptions related to the treatment of diabetes – a widespread and rapidly growing chronic condition which exacts a high human and financial cost.\(^4\) It was the most costly condition among common health conditions in 2013 (Dieleman et al., 2016), and its treatment costs amount to 20% of the estimated cost for all prescription drugs in the US over 2015-17 (Zhou et al., 2020). Thus, containing the cost of diabetes drugs is of high interest for public policy.

Payments and prescription data are complemented with socioeconomic and health data relevant for explaining antidiabetic prescriptions. By focusing on one therapeutic field we can control for disease-specific covariates (e.g., share of obese adults and adults diagnosed with diabetes) that help to predict prescriptions for this class of drugs, allowing to more cleanly identify causal effects of payments.\(^5\)

We estimate counterfactual scenarios with/without a gift ban using rich information on physicians in the Northern New England states: Maine, New Hampshire, and Vermont, a tri-state area with comparable health statistics concerning medical expenditures, healthcare cost and utilization, hospital statistics, and patient populations.\(^6\) Vermont is the only state in the US with a strict ban in place since 2009. As the ban was implemented before the availability of data on payments, it is not possible to assess the impact of payments using a natural experiment approach.

To overcome the problem of a missing control group within the state of Vermont, we propose a policy evaluation approach which leverages causal forest algorithms for counterfactual prediction. In a first step, we estimate functions mapping ob-

---

\(^4\)Diabetes is the seventh leading cause of death in the US (CDC, 2021).

\(^5\)Focusing on one drug or drug market is the typical approach in the literature. For example, Datta and Dave (2017) focus on the drug Famvir. Agha and Zeltzer (2022) focus on new anticoagulants. Grennan et al. (2021) analyze statins. A notable exception is Carey et al. (2021) who estimate average effects of payments using a mix of different drug classes.

\(^6\)See, e.g., Onpoint Health Data (2010), a tri-state evaluation of health services commissioned by the Vermont Department of Banking, Insurance, Securities & Health Care Administration.
served covariates to prescriptions and propensities to receive payments in New Hampshire and Maine where payments are allowed. We then estimate the distribution of causal responses to payments conditional on a wide array of individual and local characteristics, allowing for an algorithmic specification that uses estimated propensities and prescriptions to be more robust to confounding. Finally, we combine the estimated (causal) changes in brand drug quantities with brand drug prices to quantify the effects of payments on drug costs. In a second step, we use the estimated mapping from payments to responses to simulate brand drug prescribing for physicians in Vermont based on their estimated propensities to receive payments in the absence of a ban.

We find that, on average, a $10 payment increases the quantity of brand drugs prescribed by 0.1 prescription claims. Relative to the mean, each payment is associated with a 2.5% increase in brand claims. We find positive effects despite the fact that payment values are on average small: the median paid physician in our sample receives about $40 per year. On average, for every dollar received, payments generate a $6.3 increase in total drug costs. The average effect of payments estimated using causal forests is stable across estimation methods including standard two-way fixed effect regression (as implemented in, e.g., Datta and Dave, 2017; Carey et al., 2021; Agha and Zeltzer, 2022) and doubly robust machine learning estimators (Chernozhukov et al., 2015, 2018, 2022).

Next, we analyze sources of variation in payment intensity to shed new light on how drug companies may allocate payments. Our results highlight that larger payments are directed towards male physicians treating larger volumes of patients who can be expected to need relatively higher amounts of antidiabetic prescriptions because they are on average sicker or more at risk of diabetes.

We then analyze the selection-adjusted causal effect estimates at the physician level. Our results show that the average effect of payments masks significant heterogeneity across physicians. We find that the most important driver of physician responses – accounting for 40% of the variance in causal effects – is the insurance coverage of their patients. In particular, physicians with more patients who benefit from a low-income subsidy (LIS) prescribe more brand drugs in response to payments. Payments of a higher value trigger stronger responses, although payment value matters less when a high share of patients benefit from a LIS. Low-income subsidies are associated with reduced (or zero) out-of-pocket drug costs
for patients. Thus, combined, these findings suggest that physicians take patients’ finances into consideration when responding to payments. When patients have higher out-of-pocket costs, it appears that higher value payments are needed to encourage physicians to prescribe more brand drugs. However when patients pay very little (or nothing) for drugs, responses are large regardless of payment value.

Finally, we quantify the impact of a gift ban. Our counterfactual analysis focuses on the financial impact of a gift ban, holding market features such as prices and drug choice constant. We find that the gift ban in Vermont has resulted in savings amounting to 1% ($570k) of the total drug cost for diabetes for the period 2014-17. In the neighboring states of New Hampshire and Maine, a gift ban has the potential to decrease total costs by 3% ($740k) owing to the differing characteristics (and hence responses) of physicians and their patients in these states. We find that payments affect drug costs through higher prescriptions of brand drugs on top of prescriptions for generic drugs, as opposed to substituting generics.\footnote{This finding is not surprising given that during our sample period there are several important drug classes without generic substitutes available (see Section 2.3)}

Our work adds to a large and growing literature on industry payments to physicians. Prior studies, each using different samples and approaches to correct for selection, generally estimate the average causal effect of payments (binary treatment) on prescribing (e.g. Datta and Dave, 2017; Fernandez and Zejcirovic, 2018; Carey et al., 2021; Grennan et al., 2021; Agha and Zeltzer, 2022). Compared to these studies, this paper allows for potentially heterogeneous payment effects across physicians both in terms of payment values and observables. Further, it aims to deliver more precise estimates as it exploits the large variation in payments and flexibly accounts for many possible sources of selection.

Beyond the methodological contribution, our approach, which treats payments as a continuous variable, leads to new findings on the role of payment value. In line with prior work, we estimate that each payment is associated with a 2.5% increase in prescription volume (relative to the mean), which lies in-between the 1.6% increase estimated by Carey et al. (2021) and the 5% increase found by Agha and Zeltzer (2022). However, for physicians receiving high payments, we find significantly larger elasticities. This result supports policymakers’ beliefs that large-dollar payments have more of an impact. This is evidenced by some states like Minnesota and Massachusetts which ban payments above a certain dollar amount.
Moreover, we show that physicians respond to other factors, in particular, payment value matters when patients have high out-of-pocket costs, but becomes negligible when patients are heavily insured against drug costs. Finally, this is the first study to perform a drug cost analysis for the large and growing antidiabetics market, showing significant cost savings from the adoption of a gift ban.

The remainder of the paper is structured as follows. Section 2 describes background and data. Section 3 discusses the empirical framework. Section 4 presents the main results and their policy implications. Section 5 undertakes additional analyses and robustness checks. Section 6 concludes.

2 Background and Data

This section introduces the two main datasets used in the empirical analysis; Open Payments and Medicare Part D. Thereafter we provide information on the antidiabetic drug market, introduce our final sample, present descriptive statistics, and discuss the relevant predictors used in our model.

2.1 Open Payments

Payments to physicians typically arise because of a face-to-face marketing encounter, known as a detailing visit.\(^8\) We obtain comprehensive data on payments to physicians from the Open Payments website. Open Payments is a national disclosure program created by the Affordable Care Act and managed by the Centers for Medicare and Medicaid Services (CMS). The creation of Open Payments follows the passing of the Physician Payments Sunshine Act in 2010 which aims to promote transparency and accountability by collecting and publishing information about financial relationships between the healthcare industry and providers.

The Act requires manufacturers to fully-disclose payments of $10 or more in value made to physicians and teaching hospitals. The published data includes the identities of the payment recipients and the paying firms, date of payment, associated product, payment amount, and nature of payment (e.g. gift, meal, speaker fee, travel and lodging). The first full year in which payments were publicly

\(^8\)Payments can also occur without any face-to-face encounter if, for example, a physician did not meet with the sales representative but still accepted a gift.
disclosed is 2014.9

While US law forbids companies from explicitly paying physicians to prescribe drugs or medical devices, gifts linked to the promotion of drugs or fees for consulting or speaking events are largely unrestricted. Only a handful of states ban certain types of payments or limit the value of such transfers.10 One of the most comprehensive statutory gift bans was implemented by Vermont in 2009. Vermont’s gift ban (18 V.S.A §4631a) prohibits most gifts, including free meals, to physicians who regularly practice in Vermont.11

2.2 Medicare Part D

We assess the impact of payments on prescriptions made under Medicare Part D. We obtain data on prescriptions dispensed under the Medicare Part D Program from the Centers for Medicare and Medicaid Services (CMS).12 Medicare is the federal health insurance program in the US for people over the age of 65 and people with disabilities. Medicare Part D provides subsidized private insurance for outpatient prescription drugs for enrollees and represents about 30% of US retail prescription drug expenditure (Kaiser Family Foundation, 2019).

Beneficiaries of Medicare Part D pay a share of drug costs themselves (“out-of-pocket”). This share depends on their plan. Beneficiaries elect either a Medicare Advantage Plan (MAPD) or a stand-alone drug plan (PDP). A MAPD provides more comprehensive cover and caps yearly out-of-pocket spending. Additionally, beneficiaries with sufficiently low income and assets receive a low-income subsidy (LIS).13 Beneficiaries with a LIS benefit from zero or much lower out-of-pocket

---

9The 2013 program year includes only data collected from the second half of the year.
10States with statutory gift bans payments prior to the Sunshine Act include Vermont (2009), Colorado (2007), Minnesota (1994) and Massachusetts (2009) (Gorlach and Pham-Kanter, 2013). Maine enacted a ban of certain types of payments at the end of our sample, in mid-2017, however clear rules only came into effect in June 2020 (Sullivan, 2020). In our data we do not see a decline in the value or number of payments to physicians in Maine in 2017 vs. 2016 and 2015.
11The law is actively enforced and violators have been made to pay penalties in the past. In 2013, Novartis was reported to have paid $36k because of six meals that violated the state’s gift ban (Hams and Wilkinson, 2013).
12The Medicare Part D Detailed Prescriber Public Use File (PUF) provides data on all prescriptions at the physician-drug-year level. A further database, the Medicare Part D Prescriber Summary PUF contains additional information at the physician-year level. In both datasets, physicians can be identified by their National Provider Identifier (NPI) and so the two datasets can be easily combined.
13In 2018, 12 million (29%) people received low-income subsidies (Cubanski et al., 2018).
expenditures for premiums, deductibles and drug costs (Yala et al., 2014).

2.3 Antidiabetics

We focus on payments and prescriptions for drugs used to treat diabetes. Diabetes is on the rise in the US, as well as globally, and costs to treat Medicare beneficiaries with diabetes have grown steadily overtime.\(^{15}\) Based on the full Medicare Part D dataset for the US, total Medicare expenditures for brand and generic treatments for diabetes amounted to $8.6 billion in 2013 and increased to $17.6 billion in 2017. To the extent that payments to physicians may lead to higher drug costs, the potential savings in this market could be large. Moreover, since diabetes disproportionately affects older people, the sample of prescriptions to Medicare beneficiaries is likely to cover a substantial portion of antidiabetic prescriptions.\(^{16}\)

Diabetes has no cure and must be managed by life-long therapy. Treatment is primarily drug based, alongside comprehensive lifestyle modification. There are two main types of diabetes: type 1 and type 2. Type 1 patients are treated primarily with insulin. Type 2 diabetes, which accounts for 90-95% of all cases, is treated in a more complex way. The first line medication is metformin which is used for as long as the body tolerates it, thereafter different drugs are added to the treatment regimen (Draznin et al., 2022). Diabetes drugs can be grouped in several classes based on the drug mechanism of action (e.g. DPP-4 inhibitors or SGLT-2 inhibitors). Drugs within the same drug class are more substitutable than drugs in different classes. However, therapy often combines several drugs from different classes. The treatment guidelines from the American Diabetes Association provides some suggestions of how this step-wise addition of drugs can proceed, however ultimately treatment is complex and depends on patient factors such as comorbidities, cardiovascular risk, preferences for side-effects, tolerance and cost (Draznin et al., 2022). There is no clear best drug class, owing to the different side effects associated with each class, patient factors, as well as the need for combination and/or step-wise therapy. SGLT-2 inhibitors is the newest drug class in our dataset, launched shortly before our sample begins (in 2013).

\(^{14}\)Yala et al. (2014) find that the average out-of-pocket costs for LIS beneficiaries are 74% lower than the out-of-pocket costs for non-LIS beneficiaries with gap coverage ($148 vs. $570).

\(^{15}\)In 2016, 33% of beneficiaries had diabetes, up from 18% in 2000 (Cubanski et al., 2019).

\(^{16}\)Physicians in the sample may prescribe antidiabetics to other patients that are not enrolled in Part D. This information is not publicly available and is not included in the dataset.
There are limited possibilities for physicians to directly substitute brand medications for generic medications when treating diabetes during our sample period. There are only three drug classes out of eight where generic drugs are available during our sample period. The first line treatment, metformin, is available as a generic. For drug classes which are often introduced later, once metformin is no longer effective, such as SGLT-2 inhibitors and DPP-4 inhibitors, there are no generic alternatives. Insulin, vital for type 1 patients, is not available as a generic. In our data, payments are almost exclusively related to the drug classes DPP-4, GLP-1, SGLT-2 and insulins, where generic alternatives are not available. No new generics are launched during the sample period. Appendix A provides a more detailed discussion of the treatment of diabetes and provides an overview of the drugs in our sample and their respective drug class.

2.4 Sample and summary statistics

Pharmaceutical companies make payments to physicians in connection with specific brand drugs. Our aim is to study heterogeneous responses to payments more generally and quantify the effect of banning (all) payments on drug costs. Consequently, we aggregate payments and prescriptions related to antidiabetics to the physician-year level and analyze the effect of all payments on drug prescriptions and costs. By doing so, we ensure that brand drug substitution patterns are accounted for.\textsuperscript{17} Moreover, by focusing on total brand prescriptions we account for changes in both the extensive and intensive margin.

The final dataset is a panel of 1,904 physicians in the states of New Hampshire (37%), Maine (46%) and Vermont (17%), who prescribe antidiabetic medication to Part D beneficiaries, matched with payments data for the years 2014 to 2017. Variables are at the physician-year level unless otherwise stated. Data on physicians, payments and prescriptions is complemented with socioeconomic and health information for the areas (5-digit zip code or county) in which physicians are located. This includes data on household income, education, and population demographics from the US Census Bureau’s American Community Survey and data on diagnosed

\textsuperscript{17}We also test if payments affect generic antidiabetic prescriptions. Given the specifics of the antidiabetic drug market, explained in Section 2.3, we do not expect a strong effect of payments on generic prescriptions, and this is indeed what we will find.
diabetes rates and obesity from the Centers for Disease Control and Prevention.\textsuperscript{18} A detailed description of the dataset construction can be found in Appendix B. The list of all relevant variables in the datasets, descriptions and sources is provided in Table 7 in Appendix B.

In order to obtain credible estimates of the causal effects of payments, we exclude physicians in Maine and New Hampshire that do not satisfy the overlap assumption when estimating propensities to receive payments.\textsuperscript{19} Below, we present the summary statistics of the trimmed dataset in order to allow for a correct interpretation of the results.\textsuperscript{20}

The resulting trimmed sample comprises of 1,701 physicians located in New Hampshire (35\%), Maine (46\%) and Vermont (19\%). About 16\% of physicians in Maine and New Hampshire receive at least one payment, the treatment, for a total of $117,837. Table 1 shows summary statistics for payments related to antidiabetic drugs.

| Table 1: Summary statistics for payments related to antidiabetic drugs for paid physicians in Maine and New Hampshire over 2014-2017. |
|---------------------------------|--------|---------|--------|---------|--------|
| Obs. 459                        | Median | Mean    | Min.   | Sd      | Max.   |
| No. payments per year           | 4.00   | 9.36    | 1.00   | 13.40   | 95.00  |
| No. cash payments per year      | 0.00   | 0.14    | 0.00   | 1.19    | 16.00  |
| No. in-kind payments per year   | 4.00   | 9.22    | 0.00   | 12.96   | 94.00  |
| Value ($) payments per year     | 35.10  | 256.72  | 0.83   | 1817.11 | 28433.31 |
| Value ($) cash payments per year| 0.00   | 149.05  | 0.00   | 1481.66 | 22901.73 |
| Value ($) in-kind payments per year | 34.31   | 107.68  | 0.00   | 355.26  | 5531.58 |

The median paid physician receives four payments yearly for a total of $35. Most payments are below $104. Physicians most often receive in-kind payments (97\%). Typically, physicians receive cash payments in addition to in-kind payments, thus looking at cash and in-kind payments separately would undermine the dollar size of payments. Cash payments are generally higher than in-kind payments, on average $150 versus $108.

Looking at the time dimension, payments are highest in 2014 ($58,646), lowest in 2017 ($13,391) and of similar value in 2015 ($22,659) and 2016 ($23,141).

\textsuperscript{18}Available at: https://gis.cdc.gov/grasp/diabetes/DiabetesAtlas.html
\textsuperscript{19}All physicians must have close comparisons in the opposite treatment group (paid/unpaid) for unbiased estimates of the causal effects without extrapolation (Imbens and Rubin, 2015).
\textsuperscript{20}Descriptives as well results using the untrimmed dataset are available upon request. Without trimming, we find that payments cause higher costs to treat diabetes by 4\% vs. 3\% with trimming.
In terms of distribution, Wilcoxon and Kolmogorov-Smirnov tests (1971; 1945) show that differences in payments across years are mostly insignificant. Moreover, physicians in Maine are generally paid more than physicians in New Hampshire, in median $42 versus $30 and in total $90,472 versus $27,365. Figure 1 maps payment heterogeneity across states at the local level (5-digit zip code). Significant heterogeneity exists also when payments are measured per inhabitant of the corresponding local area (see Figure 9 in Appendix).

![Figure 1: Total payments ($p$, $\$) to physicians aggregated by area (5-digit zip code) across Maine and New Hampshire over 2014-2017.](image-url)
Table 2: Summary statistics for physicians receiving vs. not receiving payments, as well as physicians in Vermont (subject to a payment ban).

| Variable                              | No Payment | Received Payment | Vermont (ban) |
|---------------------------------------|------------|------------------|---------------|
|                                       | Mean       | Sd               | Mean          | Sd          |
|                                       |            |                  | Mean          | Sd          |
|                                       |            |                  | Mean          | Sd          |
| DRUG CLAIM COUNTS                     |            |                  |               |             |
| Generic drugs                         | 183.76     | 111.51           | 231.35        | 119.27      | 167.82       | 104.49       |
| Brand drugs                           | 95.4       | 104.24           | 151.33        | 121.76      | 103.51       | 123.29       |
| TOTAL DRUG COSTS $                    |            |                  |               |             |
| Generic drugs                         | 3233.37    | 3429.46          | 3945.22       | 3385.07     | 2892.58      | 3001.29      |
| Brand drugs                           | 45325.79   | 43458.84         | 77392.8       | 56560.93    | 59780.83     | 92801.63     |
| COVARIATES                            |            |                  |               |             |
| Physician                             |            |                  |               |             |
| Family practitioner (0/1)             | 0.62       | 0.49             | 0.68          | 0.47        | 0.61         | 0.49         |
| Male physician (0/1)                  | 0.67       | 0.47             | 0.86          | 0.34        | 0.62         | 0.49         |
| New practitioner (0/1)                | 0.14       | 0.35             | 0.1           | 0.3         | 0.1          | 0.3          |
| Antidiabetics claim share             | 0.05       | 0.02             | 0.06          | 0.01        | 0.06         | 0.06         |
| Physician’s patients                  |            |                  |               |             |
| Share of beneficiaries > 65           | 0.13       | 0.27             | 0.14          | 0.25        | 0.12         | 0.27         |
| Share of male beneficiaries           | 0.43       | 0.1              | 0.45          | 0.07        | 0.43         | 0.12         |
| MAPD claim share                      | 0.2        | 0.14             | 0.21          | 0.14        | 0.09         | 0.08         |
| LIS claim share                       | 0.49       | 0.22             | 0.53          | 0.22        | 0.46         | 0.17         |
| No. of beneficiaries                  | 337.92     | 143.69           | 356.25        | 142.01      | 321.59       | 124.9        |
| Average age of beneficiaries          | 70.94      | 4.56             | 70.17         | 4.34        | 71.33        | 3.35         |
| Average risk score of beneficiaries   | 1.22       | 0.28             | 1.16          | 0.2         | 1.13         | 0.22         |
| Share of beneficiaries with insulin claims | 0.23 | 0.17 | 0.22 | 0.14 | 0.27 | 0.15 |
| Physician’s practice location         |            |                  |               |             |
| Population                            | 16677.93   | 11592.39         | 14264.17      | 10688.14    | 11205.96     | 7938.3       |
| Population per sq. mile               | 882.91     | 1467.58          | 665.44        | 1133.74     | 643.63       | 1360.64      |
| Median household income $              | 56040.31   | 18989.48         | 52704.98      | 16350.02    | 54799.75     | 12683.44     |
| No. diagnosed with diabetes           | 12741.61   | 8082.76          | 12196.58      | 7308.11     | 4162.6       | 2114.85      |
| Percent diagnosed with diabetes       | 8.16       | 1.08             | 8.46          | 1.09        | 6.88         | 0.88         |
| No. obese                             | 37853.85   | 24304.17         | 35810.59      | 22055.19    | 13216.86     | 7150.19      |
| Percent obese                         | 28.69      | 3.43             | 29.4          | 3.59        | 25.16        | 4.09         |
| Population w/o high school degree < 24| 0.87      | 0.08             | 0.88          | 0.07        | 0.87         | 0.1          |
| Population w. college degree < 24     | 0.55       | 0.14             | 0.52          | 0.16        | 0.52         | 0.18         |
| Population w/o high school degree 25-34| 0.06     | 0.04             | 0.06          | 0.05        | 0.07         | 0.05         |
| Population w. college degree 25-34    | 0.36       | 0.17             | 0.32          | 0.17        | 0.39         | 0.18         |
| Population w/o high school degree 35-44| 0.06   | 0.04             | 0.06          | 0.05        | 0.06         | 0.04         |
| Population w. college degree 35-44    | 0.36       | 0.17             | 0.31          | 0.17        | 0.42         | 0.16         |
| Population w/o high school degree 45-64| 0.07   | 0.04             | 0.08          | 0.04        | 0.07         | 0.04         |
| Population w. college degree 45-64    | 0.31       | 0.14             | 0.27          | 0.13        | 0.37         | 0.12         |
| Population w/o high school degree > 65| 0.15     | 0.07             | 0.17          | 0.08        | 0.14         | 0.07         |
| Population w. college degree > 65     | 0.27       | 0.12             | 0.23          | 0.11        | 0.33         | 0.12         |
| Share of White population             | 0.932      | 0.047            | 0.938         | 0.05        | 0.944        | 0.038        |
| Share of Black population             | 0.016      | 0.021            | 0.013         | 0.018       | 0.013        | 0.015        |
| Share of Asian population             | 0.021      | 0.023            | 0.017         | 0.024       | 0.017        | 0.02         |
| Share of Multi-race population        | 0.001      | 0.003            | 0.001         | 0.002       | 0.001        | 0.001        |
| Physician’s network                   |            |                  |               |             |
| No. of payments other physicians in zip code | 1.08 | 1.6   | 1.73  | 1.87  | 0  | 0  |
| No. of payments other physicians in county | 8.28 | 5.95 | 9.78  | 5.22  | 0  | 0  |
| No. of physicians in zip code         | 12.4       | 10.52            | 9.15          | 8.1         | 7.17         | 6.06         |
| No. of physicians in county           | 78.42      | 46.35            | 73.49         | 44.71       | 30.22        | 18.86        |
| Observations (4,693)                  | 3,215      | 3,215            | 459           | 459         | 1,019        | 1,019        |
Table 2 compares key attributes of untreated (No Payment) and treated (Received Payment) physicians in Maine and New Hampshire as well as physicians in Vermont for 2014-2017. Table 2 shows that physicians who have received a payment, on average, prescribe a higher volume of brand and generic drugs in comparison to physicians who do not receive payments. Drug costs are also higher for paid physicians. Paid physicians are more likely to be male: about 90% of paid physicians are male, whereas as only 70% of unpaid physicians are male. In terms of the characteristics of physicians’ patients and socioeconomic and health data associated with the location where the physician practices, average differences across paid and unpaid physicians are less pronounced. Paid and unpaid physicians have networks of comparable size with on average 9 and 12 peers practicing in the same area (5-digit zip code), respectively. This holds true also for the number of payments to peer physicians: on average, the peers of paid (unpaid) doctors receive 1.7 (1.1) payments per year. Full summary statistics of the included attributes are presented in Tables 8 and 9 in Appendix. All predictors are discussed in the next section.

2.5 Relevant predictors

Many variables can explain differences in prescribing decisions and payments across physicians. In our analysis we control for a large set of observables that potentially affect prescriptions and/or payments at the physician level. Attributes can be grouped into four categories: physician characteristics, characteristics of the physician’s patients, and characteristics of the physician’s practice location and peer network.

Physician characteristics. Prescribing patterns, payments and physicians’ responses to payments may differ depending on their individual characteristics. For example, previous studies find that a physician’s gender is an important predictor of differences in prescribing behavior. In particular, male physicians begin prescribing new drugs faster than female physicians (Tamblyn et al., 2003; Zhang et al., 2019; Méndez et al., 2021). Similar results are found for general practitioners vs. specialists (Tamblyn et al., 2003). In our analysis, we include the following physician characteristics: physician gender, whether he/she is a new practitioner, whether the physician is a general practitioner as opposed to a specialist, and the
share of antidiabetic claims out of all claims which provides a further measure of the physician’s specialization in diabetes.

**Physician patient characteristics.** Patient characteristics are expected to affect physicians prescribing decisions and are likely to influence how drug firms target physicians. For example, drug firms may target physicians who treat a high volume of patients (Fugh-Berman and Ahari, 2007). Patient characteristics such as age, gender, and general health status can affect their risk of diabetes and in turn influence payments and prescribing decisions. To investigate and control for differences in patient population we use information on Medicare beneficiaries, aggregated at the physician-year level. Variables include the total number of beneficiaries that a physician treats, the share of male beneficiaries, the share of beneficiaries over the age of 65, average age of beneficiaries, and the average risk score (HCC) of beneficiaries.\textsuperscript{21} To control for the fact that physicians may see different proportions of patients with type 1 vs. type 2 diabetes we control for the share of beneficiaries with insulin claims as insulin is the only treatment option for type 1 diabetes. Previous studies indicate that physicians take patients’ out-of-pocket expenditures for drugs and into consideration when prescribing drugs (moral hazard, see Lundin, 2000) and their cost sensitivity (Carrera et al., 2018). In our analysis, patients’ out-of-pocket expenditures is proxied by information on patients’ insurance plans including the share of beneficiaries with a MAPD and a LIS.

**Physician practice location.** The geographic area in which a physician practices may influence prescriptions and payments. For example, if a physician practices in an area where the population has a higher risk of diabetes due to age, obesity and/or race, prescriptions and payments related to antidiabetic drugs may be higher.\textsuperscript{22} To control for demographic, socioeconomic and health differences we include information on population, population density, median household income and race in the 5-digit zip code area in which the physician’s practice is located, as well as the number and percentage of adults diagnosed with diabetes, and the

\textsuperscript{21}The beneficiary average Hierarchical Condition Category (HCC) risk score is determined by CMS using demographic information and diagnoses on Medicare fee-for-service claims to measure each beneficiary’s medical risk status, with higher scores going to beneficiaries with more (or more severe) health conditions or demographic risk factors. Thus, risk scores provide a proxy for patients’ health status.

\textsuperscript{22}Risk factors associated with type 2 diabetes, which accounts for 90-95\% of all diabetes cases, include being overweight, being 45 years or older, and being African American, Hispanic/Latino American, American Indian, or Alaska Native (CDC, 2021).
number and percentage of obese adults at the county level. Bronnenberg et al. (2015) find that how informed or expert consumers are affects their choice of brand in health care markets. To account and test for the potential influence of patients’ education we include several variables on educational attainment in the physician’s practice location (5-digit zip code level).

**Physician network characteristics.** Payments may indirectly affect prescribing behaviors of unpaid physicians belonging to the same peer network of targeted doctors Agha and Zeltzer (2022). In other words, payments may have spillover effects leading to an increase in brand drug prescriptions by unpaid physicians in the control group. Not accounting for such possible spillovers would lead to the underestimation of the direct effect of payments on prescribing. As in Agha and Zeltzer (2022), we add the number of payments to other physicians in the same network as an explanatory variable for prescribing. Physicians are considered connected if they practice in the same 5-digit zip code or, alternatively, more broadly in the same county.

Moreover, the geographic proximity of physicians to other physicians has been shown to positively affect quality of care through increases in local competition (Gravelle et al., 2019, 2016). Also, drug firms may target physicians depending on the size of their network. To capture these possible confounding effects, we control for the network size of each physicians defined as the number of other physicians practicing in the same county and in the same 5-digit zip code.

### 3 Empirical Strategy

#### 3.1 Baseline causal effects of payments

We begin our analysis by estimating the average causal effects of payments using a standard OLS regression of brand drug prescription quantities on payments using our sample of treated (paid) and untreated (unpaid) physicians. Our research design relies upon variation in a physician’s prescribing behavior over time and, thus, allows to account for time-invariant characteristics that may drive the targeting of payments to physicians. We also control for common shocks to patient or physician preferences with year and area (5-digit zip code) fixed effects. In addition, physicians’ prescribing may be affected by time-varying factors such as patients’
health risk and insurance coverage levels.\textsuperscript{23} The richness of our dataset allow us to control for a large number of covariates that may help explaining further variation in prescribing behaviors.

However, including the high-dimensional covariate set would cause an especially large bias when variables affecting brand drug prescriptions are correlated with payments (Chernozhukov et al., 2018). On the one side, for instance, patients with low insurance are less likely to have brand versions prescribed than patients with high insurance coverage (Lundin, 2000). On the other side, drug firms may direct larger payments to physicians with patient populations who are more heavily insured. To address this targeting of payments that may be correlated with both outcome and treatment levels, we estimate changes in brand drug prescriptions causal to receiving a payment using a residualized regression approach. For an intuitive illustration of this approach, it is useful to think about causal diagrams (Pearl, 2009) that represent this identification strategy. One possible model summarizing our data generating process is shown Figure 2.

\begin{figure}[h]
\centering
\includegraphics[width=0.5\textwidth]{causal_diagram.png}
\caption{A causal diagram of the effect of payments (P) on physicians’ brand drug prescriptions (Y). Estimation proceeds via residualized regression following steps 1, 2, 3.}
\end{figure}

The causal diagram shows that our outcome variable, brand drug prescriptions (Y), is determined by both pharmaceutical payments (P) and exogenous covariates (X). These include observable characteristics of physicians, their patients, practice location and network (see list in Table 2). As mentioned before, drug firms commonly target physicians with specific characteristics X for payments.\textsuperscript{24} Thus, X may affect both P and Y. In light of this diagram, the next steps are pretty clear. We need to get rid of the variation of P and Y due to X (step 1 and 2 in Figure 2) in

\textsuperscript{23}There is indeed large variation in, e.g., a physician’s share of insured patients over time. Within physicians, the coefficient of variation of the share of patients with a MAPD insurance plan ranges between an average of 0.23 and a maximum of 1.25.

\textsuperscript{24}Note that the covariate set X affecting P and Y do not necessarily fully overlap. In our case, we add lags of P to predict P and the number of payments to peers to predict Y. We motivate this choice in the text below when addressing possible threats to identification.
order to isolate just the variation we need (step 3 in Figure 2).

In practice, we consider inference on the target coefficient $\alpha_0$ in the model (let us omit the true time subscript $t$ for simplicity):

$$y_i = p_i \alpha_0 + x_i \beta_0 + \epsilon_i,$$

under the traditional assumption of conditional exogeneity (or ignorability),
$$\mathbb{E}[\epsilon_i|x_i, p_i] = 0.$$ Here $p_i$ is the target regressor of payments (treatment variable), $x_i$ the exogenous covariates that may determine outcomes and propensities to receive a payment, and $y_i$ the outcome for all physicians $i = 1, \ldots, n$. Importantly, we also apply fixed effects to our model for comparison purposes and for assessing the robustness of the estimated effect against the presence of unobserved confounders. The residualized regression approach recovers an estimate of the effect $\alpha_0$ of $p_i$ on $y_i$ in three steps.

1. Consider the selection equation: $p_i = x'_i \pi^p_0 + \gamma^p_i$, and save the residuals $\gamma^p_i = p_i - \hat{p}_i$ left after partialling out the effect of $x_i$ from $p_i$.

2. Consider the outcome equation: $y_i = x'_i \pi^y_0 + \gamma^y_i$, and save the residuals $\gamma^y_i = y_i - \hat{y}_i$ left after partialling out the effect of $x_i$ from $y_i$.

3. Run the regression model: $\gamma^y_i = \alpha \gamma^p_i + \epsilon_i$ where $\alpha = \alpha_0$ solves the orthogonal population equation: $\mathbb{E}[(\gamma^y_i - \alpha \gamma^p_i) \gamma^p_i] = 0$.

In essence, residualization removes the correlation of covariates with payments and outcomes, rendering the estimator robust to the parametric form in which covariates are included. In addition, residualization makes the causal effect estimator insensitive to small errors in the nuisance components, and avoids the regularization bias occurring in the one-step procedure especially when some covariates are correlated with the treatment (Nie and Wager, 2021; Chernozhukov et al., 2018). This approach is based on Robinson (1988) and the Frisch-Waugh-Lovell theorem. In high-dimensional settings, this approach has been generalized under the name of “double/debiased machine learning” (Chernozhukov et al., 2018). In particular, residualization makes the estimator “doubly robust”, i.e., as long as the estimator for either payments or outcomes is consistent, the resulting estimator for the treatment effect is consistent.
Identification of R-learning models is first studied by Robinson (1988) in low-dimensional settings. Chernozhukov et al. (2018) adjust and identify Robinson’s model for average causal effect estimation via machine learning allowing to handle the large covariate dimension. Threats to identification may still arise with this approach in the case of spillovers across physicians and reverse causality issues.

**Reverse causality.** Physicians likely received some payments before payment data were disclosed. As drug firms may monitor physicians, they could target highly responsive physicians who increased brand drug prescriptions after a payment in the past. In other words, the targeting of payments may be path dependent. We capture a possible path dependence by adding the most recent lag of payments, when available, to the covariate set used to predict physicians’ propensities to receive payments. Its relevance in explaining current payment levels remains, therefore, an empirical question.

**Spillovers/network effects.** Physicians may adjust their prescriptions in response to a peer physician receiving a payment due to the manner in which physicians may interact, share knowledge and influence each other (Agha and Zeltzer, 2022). Neglecting spillover effects from paid to unpaid physicians would understate the direct impact of payments on prescribing. To address this concern, we control for the number of payments made to physician $i$’s peers in year $t$.

### 3.2 Heterogeneous effects of payments via causal forest

We are interested in estimating the unit level causal effect of payments, $\delta(x)$, for any physician $i = 1, \ldots, n$ described by a vector of characteristics $X_{it} = x$ in year $t$. For each $X_{it} = x$, $\delta(x)$ represents the quantity change in brand drugs $Y_{it}$ causal to the payment, $P_{it}$. We want to estimate $\delta(x)$ in a neighborhood of $x$ for which we can assume that the causal effect function is constant, i.e., $\delta(x) = \delta$ for all $x \in X$. How do we estimate such a neighborhood? We implement causal forest algorithms (Wager and Athey, 2018, 2019; Athey et al., 2019) belonging to the class of adaptive (data-driven) $k$-nearest neighbor matching estimators.

Similarly to other successful applications to quasi-experiments (see, e.g., Britto et al., 2022), we follow the implementation in Athey et al. (2019). In particular, we flexibly control for the many potential sources of selection using the random forest instantiation of the residualized regression estimator (or R-learner). This
approach is shown to perform well in both settings with and without confounding, and can better handle the case with both confounding and effect heterogeneity.\textsuperscript{25} Wager and Athey (2019) describe the details of this procedure.

In essence, the causal forest method generalizes the original algorithm of Breiman (2001) by adapting to the problem of both prediction and heterogeneous treatment effect estimation. Random forests rely on data-driven sample splits, thus limiting our discretion when selecting the relevant dimensions of heterogeneity. In addition, they allow us to capture high-dimensional non-linearities while avoiding overfitting through the use of sample splitting into training and estimation samples.

Nie and Wager (2021) study identification of R-learning models for flexible heterogeneous causal effect estimation via machine learning. Essentially, identification of the causal effects of payments to physicians relies on the potential outcome approach (Rubin, 1974). For every physician \( i \), there is a set of potential outcomes \( Y_{it}(p), p \in \mathcal{P} \), each being a random variable mapping a particular potential payment (treatment), \( p \), to a potential outcome such that \( Y_{it} = Y_{it}(p) \). This is also referred to as the unit level dose-response function. For any physician defined by a vector of characteristics \( X_{it} = x \), we wish to estimate the individual treatment effect defined as \( Y_{it}(p) - Y_{it}(0) \). This is, however, unobserved for any unit. Therefore, we will estimate the Conditional Average Treatment Effect (CATE) function \( \Delta(x) = \mathbb{E}[Y_{it}(p) - Y_{it}(0)|X_{it} = x] \) and the Conditional Average Payment Effect (CAPE) function \( \delta(x) = \frac{\partial \mathbb{E}[Y_{it}(p)|X_{it} = x]}{\partial p} \) under the “canonical” assumptions of unconfoundedness and no spillovers (Wager and Athey, 2019). In the case of multivalued treatment, this assumption writes \( Y_{it}(p) \perp \perp P_{it} | X_{it} \forall p \in [0; p_{\text{max}}] \), i.e., requires conditional independence to hold for each value of the treatment. Imbens (2000) referred to this as weak unconfoundedness, since it does not require joint independence of all potential outcomes.\textsuperscript{26}

Threats to identification may still arise in the case that the high-dimensional set of observable covariates does not fulfill the unconfoundedness assumption in Wager and Athey (2018). To relax this assumption, we account for heterogeneity across physicians, networks and time trends in all variables, namely, we remove

\textsuperscript{25}The better performance of the residualized regression estimator is compared against the classical “one-step” estimator based on the inclusion of all variables (treatment and covariates) in the outcome equation (see Athey et al., 2019, for simulation exercises).

\textsuperscript{26}Yet, it is difficult to think of applications where the weaker form would be plausible but the stronger form would not be. Differences between the two are rather conceptual (Imbens, 2000).
from them physician-, area- (5-digit zip code), and year-specific effects. Then, to reassure against the presence of unaccounted-for fixed factors, we run the causal forest over demeaned data. In this way, we can check whether our rich set of observables is able to capture the relevant heterogeneity. For unconfoundedness to hold in our setting, we should obtain similar estimates of the average causal effect regardless of whether or not we remove fixed effects from the data.

4 Results

4.1 Baseline estimates of payment effects

Table 3 shows estimates of the average causal effect of payments across estimation methods and models with/without fixed effects.

Table 3: Estimates of the average causal effect of a 10$ payment to physicians on brand drug claim counts. Standard errors are clustered by physician.

| Method                        | Model | (1)  | (2)  |
|-------------------------------|-------|------|------|
| **OLS**                       |       |      |      |
| without controls              | 0.105 | 0.115|
|                               | (0.028)| (0.029)|
| with controls (full list in Table 2) | 0.126 | 0.128|
|                               | (0.018)| (0.018)|
| **Double machine learning**   |       |      |      |
| with Lasso (Chernozhukov et al., 2015) | 0.105 | 0.115|
|                               | (0.028)| (0.029)|
| with Post-Lasso (Chernozhukov et al., 2015) | 0.105 | 0.115|
|                               | (0.028)| (0.029)|
| with CV Lasso (Friedman et al., 2010) | 0.129 | 0.125|
|                               | (0.017)| (0.016)|
| with CV Elastic Net (Friedman et al., 2010) | 0.127 | 0.126|
|                               | (0.017)| (0.017)|
| with CV Ridge (Friedman et al., 2010) | 0.122 | 0.122|
|                               | (0.019)| (0.018)|
| with best estimator           | 0.105 | 0.115|
|                               | (0.028)| (0.029)|
| **Causal forest** (Athey et al., 2019) | 0.100 | 0.115|
|                               | (0.015)| (0.015)|

| Observations                  | 3,674 | 3,674|
| Fixed effects (physician, year, zip5) | No    | Yes |

*Note: ***p-values<0.01 for all coefficients

We first look at simple OLS regression of payments on brand drug claims without
controls. The point estimate of the effect of a $10 payment is 0.1 with a standard error of 0.03. This suggests that payments significantly increase brand drug prescriptions - if payments increase by $10 relative to a trend then the predicted brand drug claims go up by 0.1, without controlling for observable covariates. Reassuringly, our estimate of the effect of payments remains stable when we account for fixed effects.

We next include our rich set of controls (see full list in Table 2). First, we estimate the model by OLS and then by double machine learning using different dimensionality reduction algorithms for prediction of outcomes and treatment variable. We implement penalized regression methods, namely, Lasso, Post-Lasso, Elastic Net and Ridge estimators.\footnote{Lasso and Post-Lasso rely on a theoretically grounded, data-driven choice of the penalty parameter developed in Belloni and Chernozhukov (2013) and provided in the hdm package for R (Chernozhukov et al., 2015). Cross-validated (CV) estimators use a cross-validated choice of the penalty parameter as provided in the glmnet package for R (Friedman et al., 2010).} In model 1, we find that the best prediction rule (in terms of lowest root mean squared prediction error) is always given by the Post-Lasso estimator. In model 2, the best method for predicting payments is Lasso, and the best method for predicting brand drug claims is Post-Lasso. We report the estimated average effect of payments from the final OLS regression of residualized outcomes on residualized payments. After controlling for covariates, the point estimate of payments remains stable around 0.1 across estimation methods and models with/without fixed effects.\footnote{The $R^2$ increases from less than 1% (without controls) to about 70% (with controls).} This indicates that the amount of confounding affecting both outcome and treatment levels is on average small. In other words, the main drivers of physicians' propensities to receive payments and brand drug prescribing do not fully overlap (see Section 4.2.1 for details).

Next, we apply the causal forest estimator. Comparing results from models 1 and 2 reveals that causal forests return similar estimates of the average causal effect whether or not we remove fixed effects from the data. This indicates that the selection on observables (unconfoundedness) assumption in Wager and Athey (2018) most likely holds.

Finally, comparing results from causal forests vs. double machine learning methods sheds light on the relationship between outcome and covariates. Both methods rely on the residualized regression approach. Differently from causal forests, penalized regression methods assume linearity in covariates. Our results show that
the estimated average effect of payments is almost insensitive to this assumption. Yet, the average effect likely masks important heterogeneities and covariates may play a different role in different subgroups of the data. Allowing for causal effect heterogeneity is at the heart of this paper’s motivation to employ causal forests.

4.2 Heterogeneous estimates of payment effects

4.2.1 Physicians’ propensities to receive payment

We start our analysis by estimating propensities to receive payments for physicians in New Hampshire and Maine conditional on the large set of included covariates and lagged payment values. In order to credibly estimate causal effects of payments, the common support condition for treated and untreated physicians needs to hold. Figure 10 in Appendix provides evidence of such support.

To understand sources of variation in payment intensity, we linearly project the included covariates on payment propensities, and we perform model selection via penalized regression (Lasso). Among the more than 30 included variables, only a subset of 9 predictors survives the shrinkage operation. We report results in column 1 in Table 10 in Appendix. Among all possible explanatory factors, we find that drug firms direct larger payments especially to physicians with sicker patients. In particular, predicted propensities to receive payments increase by 0.26 standard deviations per standard deviation increase in the average risk score of beneficiaries, ceteris paribus. Further, received payments are ceteris paribus higher for male physicians, physicians who treat higher volumes of diabetic patients, and younger patients. Other relevant, though less important, drivers of propensities to receive payments are treating larger shares of patients with a LIS, working in areas with low shares of White people, and high shares of uneducated people.

These results shed new light on how drug companies may allocate payments. Overall, payments seem to be directed towards physicians treating larger volumes of patients who can be expected to need relatively higher amounts of antidiabetic prescriptions because they are on average sicker or more at risk of diabetes.

To gauge the amount of potential confounding, we also estimate the main drivers of our outcome, brand drug prescriptions. We report results in column 2 in Table 10 in Appendix. We find that claims of brand drug prescriptions are best explained by the insurance level of patients as measured by the LIS claim share. In line
with previous research, we see that physicians prescribe more brand drugs when treating patients who have lower out-of-pocket costs (moral hazard, see Lundin, 2000). Among other determinants, brand drug prescribing is ceteris paribus higher for physicians with more patients, higher shares of patients with insulin claims (insulin has no generic substitute), and older patients. The remaining variation is then explained by physicians’ gender and years of practice, with female physicians and new practitioners prescribing lower amounts of brand drugs, ceteris paribus.

Our findings show that the main determinants of payment and outcome levels do not fully overlap, however, certain variables affecting brand drug prescriptions are correlated with payments. For instance, physicians prescribe more brand drugs to patients with low out-of-pocket expenditures, yet, this effect may be confounded by the fact that physicians with more heavily subsidized patients receive higher payments. Further, male physicians may prescribe more brand drugs, ceteris paribus, and be more likely to receive higher payments. In light of this, causal forests use predicted propensity scores to be more robust to confounding when estimating physician level causal effects of payments in any relevant subset of the data.

### 4.2.2 Physicians’ responses to payment

We begin our causal analysis by estimating unit level causal effects of payments on physicians’ prescriptions of brand drugs.\(^29\) We find that 87% of payments (or about $114k in total) have a positive and statistical significant effect on claims of brand drugs (p-values < 0.05), indicating a pervasive effect of payments on prescribing behavior. Causal effects are largely heterogeneous, and the hypothesis of effect homogeneity is strongly rejected (Levene, 1960). Figure 3 presents the distribution of the estimated conditional average causal effects of payments for all physicians in the sample.\(^30\)

---

\(^{29}\)Implementation details can be found in Appendix E.

\(^{30}\)We focus our attention on causal effects over the whole time period because dynamics are not specific to any particular year. Statistical tests (Kolmogorov-Smirnov, Mann-Whitney U) reject the hypothesis that the CAPE distribution differs across years (p-values>0.1). Figure 11 in Appendix plots the CAPE distribution by year.
As shown in Table 3, on average, a $10 payment increases quantity of brand drugs by 0.1 claims. This effect is the same when considering the average treatment effect and the average treatment effect on the treated, which indicates that the estimation accounts well for the effect of selection.

In order to compare our heterogeneous estimates to previous studies, we compute the percent increase in prescription volumes caused by payments relative to the mean. We find that each payment is associated with a 2.5% increase in prescription volume, which lies in-between the 1.6% increase estimated by Carey et al. (2021) and the 5% increase found by Agha and Zeltzer (2022).

Causal effects are similar across states in terms of mean and median. However, physicians in Maine show stronger responses to payments. Figure 4 plots the CAPE distribution by state.
What explains stronger responses to payments in Maine? The main driver may be, e.g., payment levels, as physicians in Maine receive higher payments than physicians in New Hampshire.\textsuperscript{31} However, physicians in Maine treat patients with higher levels of insurance, so they may have greater incentives to prescribe costly drugs.\textsuperscript{32} What drives heterogeneity in responses to payment?

We first compare average characteristics of physicians with CAPE above and below the median (see Table 11 in Appendix). We find that high- and low-responsive physicians significantly differ across many dimensions (as measured by standardized differences above the critical value of 0.2). Above all, the main driver of response heterogeneity is the share of patients with a LIS (as measured by standardized difference above 0.8): payments cause physicians to prescribe more brand drugs especially when their patients have low out-of-pocket expenditures.

To further investigate the drivers of heterogeneity, we linearly project the set of physician and patient characteristics on the estimated causal effects. Due to the high dimensionality of the covariate set, we perform variable selection via Lasso. Table 4 shows the main drivers of effect heterogeneity obtained from running an OLS regression of causal effect estimates on the variables selected via Lasso.

\textsuperscript{31}Physicians responding to payments in Maine receive on average $321 ($41 in median) versus $201 ($30 in median) for physicians in New Hampshire.

\textsuperscript{32}In Maine, on average 61% (27%) of patients benefit from LIS (MAPD) versus 45% (9%) in New Hampshire.
Table 4: Main drivers of physicians’ response heterogeneity. OLS coefficients of payment levels and variables selected by Lasso. Variables are rescaled so that coefficients’ units are the same. Three observations are excluded due to missing MAPD values.

| Dependent variable: | CAPE |   |   |
|---------------------|------|---|---|
| (1) | (2) | (3) |
| Value ($ payments | 0.099*** | 0.187*** | 0.214*** |
|        | (0.034) | (0.041) | (0.042) |
| LIS claim share | 0.413*** | 0.411*** | 0.413*** |
|        | (0.054) | (0.053) | (0.053) |
| MAPD claim share | 0.208*** | 0.204*** | 0.202*** |
|        | (0.052) | (0.052) | (0.051) |
| No. of beneficiaries | −0.323*** | −0.315*** | −0.318*** |
|        | (0.036) | (0.036) | (0.035) |
| Family practitioner | 0.170* | 0.163* | 0.161* |
|        | (0.088) | (0.086) | (0.086) |
| Value ($ payments * LIS claim share | −0.089*** | −0.116*** |
|        | (0.023) | (0.026) |
| Value ($ payments^2 | 0.087** |
|        | (0.036) |
| Observations | 456 | 456 | 456 |
| Fixed effects (year, state) | Yes | Yes | Yes |
| Zip5 level variables | Yes | Yes | Yes |
| Adjusted R^2 | 0.517 | 0.531 | 0.536 |

Note: *p<0.1; **p<0.05; ***p<0.01

Payment levels affect response heterogeneity to a significant and positive extent. In model 1, an increase in payments by one standard deviation leads to 0.1 standard deviations higher effect heterogeneity, ceteris paribus. The importance of payment values in explaining heterogeneity in physician responses to payments is rather small compared to other selected characteristics. The largest driver of effect heterogeneity is the share of patients with a LIS: when the latter increases by one standard deviation, causal effect heterogeneity increases by 0.41 standard deviations. At the mean value, this variable explains about 40% of the variance in causal effects. The rest of the variation in CAPE is largely explained by physicians’ volume of patients and the share of patients covered by MAPD plans. In particular,
responses are higher for physicians with lower volumes of patients, ceteris paribus, and physicians with a larger share of patients covered by MAPD plans. Taken together, MAPD and LIS shares explain about 60% of the variation in physicians’ responses.

Both MAPD plans and low-income subsidies are associated to lower out-of-pocket expenditures for patients. Thus our results strongly suggest that physicians take their patients’ finances into consideration when responding to payments: when patients pay less for drugs, physicians are more likely to respond to payments with increased brand prescriptions.

Figure 5 shows the linearly increasing effect of having higher shares of patients with a low-income subsidy on physicians’ responses to payments for brand drugs. Physicians treating very high (~95%) versus very low (~4%) shares of patients with a LIS almost double their response to payments.

Moreover, ceteris paribus responses to payments are higher for general practitioners rather than specialists. One potential explanation for this is that general practitioners may be less knowledgeable about antidiabetics and thus may be more easily influenced by payments.

To further explore the relevance of payment size in explaining physician responses, we interact the value of payments with the most important driver, LIS claim share.\textsuperscript{33} Model 2 in Table 4 shows the results. At average LIS levels (50%),

\begin{figure}[h]
\centering
\includegraphics[width=\textwidth]{figure5}
\caption{CAPE heterogeneity in LIS shares: Estimates of payment effects (CAPE) on brand drug prescriptions fitted at different percentile values of LIS shares.}
\end{figure}

\textsuperscript{33}One caveat is that the effect of payment values may not be accurately estimated across LIS levels. Reassuringly, performing Wilcoxon and Kolmogorov-Smirnov tests (1945; 1971) shows that the distribution of LIS claim share does not statistically differ across payment levels. See summary statistics in Table 12 in Appendix.
the value of payments explains about 23% of the variation in payment responses (vs. 10% in model 1). Further, the coefficient of the interaction term shows that payment values best explain differences in physician responses at lower LIS levels. For instance, at very low (high) LIS levels, payment values explain 39% (12%) of the variation in physician responses. This means that receiving a high (vs. low) payment causes physicians to increase brand drug prescriptions the most when only a small fraction of patients benefit from a low-income subsidy.

In model 3, we add a quadratic polynomial for the value of payments and we find that physician response heterogeneity at high payments is significantly higher than at low payments. In particular, we estimate that, for payments below the median ($40), responses do not significantly vary across high and low payments (p-value = 0.2). Differently, for payments above the median, responses significantly increase with payment values. Figure 6 plots response (CAPE) heterogeneity in payment values for payments above the median and across LIS levels.

![Figure 6](image_url)

**Figure 6:** CAPE heterogeneity in payment values above the median ($): Estimates of payment effects on brand drug prescriptions across the lowest (<20%) and highest (>70%) LIS shares. CAPE estimates fitted at different percentile values of payments.

At low LIS levels, Figure 6 shows that payment elasticities at large vs. low payment levels are about twice as high. This result contributes to the literature by explaining that large payments are particularly effective when patients have high

---

\(^{34}\)Since the coefficient of LIS claim share is relatively large compared to the coefficient of the interaction term, its importance in explaining physician responses remains strong at any payment value. For instance, LIS still explains 36% of CAPE heterogeneity when a payment equals $1,000.  

\(^{35}\)Below 20% (above 70%).
out-of-pocket costs. At high LIS levels, Figure 6 shows that receiving different payment values drives similar responses. In other words, if patients benefit from low (or zero) out-of-pocket costs via a LIS, both large and small payments have similar, and large, effects. The implication from this finding is that, because some patients have low (or zero) out-of-pocket costs for prescription drugs, effective policy should ban both large- and small-value payments.

One possible interpretation of our finding is that physicians weigh up both their own financial gain and patients’ financial costs when responding to payments. When patients have higher out-of-pocket costs, higher dollar value payments are needed to encourage physicians to prescribe more brand drugs; to offset the additional financial cost to patients (assuming a given health benefit). This interpretation is in line with prior research finding that physicians respond to financial incentives (e.g. Liu et al., 2009; Papanicolas and McGuire, 2015; Mueller et al., 2021) as well as to the cost-sensitivity of patients (Epstein and Ketcham, 2014; Carrera et al., 2018). Further, this explanation is consistent with results from a field experiment by Lu (2014) finding that when doctors in China are provided with an incentive to promote drug sales, prescriptions for insured patients amount to more than those for uninsured patients.

4.3 Drug cost increases due to payments

To determine the increase in drug costs due to payments we compute the increase in brand prescriptions for each physician receiving payment $p$, multiply this by the average unit cost per claim for brand drugs $c$, and take the sum over each physician $i = 1, \ldots, n$ with characteristics $X_i = x$:

$$\text{Change in drug costs due to payments ($\$) = } \sum_{i=1}^{n} \left[ \hat{CAPE}(x)^* p^* c \right]$$

We estimate that payments cause physicians to increase brand drug prescriptions by a total of 1,460 claims and $739,682 over 2014-2017 in the states of Maine and New Hampshire (see Table 5). Given that payments totaled $117,837 in this time period, the impact of $1 in payments received by physicians on drug costs is $6.3. Compared to a counterfactual world without payments, the estimated in-
crease corresponds to about a 3% total cost increase for society to treat diabetes. In other words, banning payments has the potential to decrease total costs to treat diabetes by 3%.

Table 5: Summary statistics for the effects of payments on brand claims and drug costs ($). Estimates at physician level aggregated for Maine (ME) and New Hampshire (NH).

| Outcome        | State | Mean  | Sd   | Total      |
|----------------|-------|-------|------|------------|
| Brand claims ME| 4.25  | 32.17 | 1144.51 |
| NH             | 2.58  | 20.51 | 314.95 |
| Drug costs $   | ME    | 1810.09 | 11363.80 | 486913.32 |
| NH             | 2071.87 | 19161.50 | 252768.54 |

Figure 7 maps drug costs for physicians in Maine and New Hampshire, showing that estimates are heterogeneous within country and generally higher for physicians in Maine (dark green areas).

Figure 7: Drug costs ($) increases due to payments. Physician level estimates aggregated by area (5-digit zip code).

4.4 The effects of a gift ban on drug cost

We now quantify the cost savings in the period 2014-2017 due to the prevailing gift ban in Vermont. The first step entails answering the question: If all physicians in Vermont would be allowed to receive a payment, which amount would they receive?

\[36\]

We obtain this figure considering only statistically significant increases in brand claims due to payments.
We answer this question by predicting propensities to receive payments for physicians in Vermont using the estimated function mapping covariates to predicted propensities in the other states. Then, we match payments of physicians in the other states with physicians in Vermont based on the nearest predicted propensities. Physicians in Vermont would have received a $36 payment in median, for a total of about $132k.

The second step entails answering the question: How would physicians in Vermont respond to such payments? We predict counterfactual responses to payments and corresponding costs for society using the estimated causal forest. We estimate that responses to payments are positive and statistically significant for 91% of physicians in Vermont. Overall, banning payments in Vermont has reduced brand drug prescriptions by 1,280 claims for a total cost saved of about $570k. Figure 8 maps the predicted drug cost savings in Vermont due to the gift ban. Although some physicians do not significantly respond to payments (9%), aggregated drug cost savings at the local level (5-digit zip code) are positive overall.

Figure 8: Predicted drug costs ($) savings due to a ban on payments. Physician level estimates aggregated by area (5-digit zip code).

The cost savings amount to about 1% of the actual costs for drug claims in Vermont. This figure is lower than the estimated share (3%) of costs that New Hampshire and Maine would save with a ban on payments. This difference is driven by the fact that physicians in Vermont display lower responses to payments.
The distribution of treatment effects in Vermont is more left skewed than that for the other states (see Figure 12 in Appendix). Statistical tests confirm that causal effects in Vermont versus in the other states are statistically different in terms of mean and distribution (p-values < 0.01). The reason is that physicians in Vermont have characteristics within the covariate space explaining lower responses to payments, namely, a high fraction of physicians in Vermont treat patients with relatively low shares of LIS and MAPD. Compared to physicians in the other states, physicians in Vermont have lower LIS and MAPD shares by on average 8 pp and 13 pp, respectively. In particular, physicians in Vermont receiving a payment have patients with a LIS of on average 48%. Differences in causal effects between Vermont and the other states disappear if we consider only the subgroup of physicians with a LIS below the average (p-values > 0.1, see Figure 13 in Appendix for a graphical representation). This finding supports the importance of patients’ out-of-pocket expenditures in explaining physician response heterogeneity.

The implication of our analysis for policy is that banning payments has the potential to significantly lower drug costs. In addition to the financial impact of a gift ban, a further consideration for policy is whether and how payments affect the quality of prescriptions. In fact, answering the question of whether paid and unpaid physicians prescribe drugs of a similar efficacy would require detailed health data at the patient level and medical expertise. While previous studies generally find no evidence that payments lead to better health outcomes for patients (Fernandez and Zejcirovic, 2018; Carey et al., 2021; Amaral-Garcia, 2020; Agha and Zeltzer, 2022; Bergman et al., 2021), the health impacts of a gift ban may differ across therapeutic fields. For example, Grennan et al. (2021) find that in the case of statins where there is underprescribing, payments increase prescriptions to the benefit of patients. By contrast Alpert et al. (2019) find that the introduction and marketing of OxyContin accounts for a substantial share of overdose deaths in the last two decades. In the case of diabetes, medical studies find that a substantial proportion of older adults with diabetes are potentially overtreated and that the harms of intensive treatment likely exceed the benefits (Lipska et al., 2015; Maciejewski et al., 2018). This provides some cause for concern.

---

37 We perform t-tests for equality of means, Kolmogorov-Smirnov tests for equality of empirical cumulative distribution functions, Mann-Whitney U tests for equality of mean ranks, and Levene tests for equality of variances.
that payments could harm patients with diabetes both financially and clinically. A gift ban may also cause equilibrium effects potentially leading to changes in drug pricing and direct to consumer advertising which in turn can impact on drug costs (Grennan et al., 2021). A full welfare analysis of a gift ban for antidiabetic drugs is outside of the scope of this paper, but is an important and challenging avenue for future research.

5 Supporting Analyses and Robustness

5.1 Effects on generic drugs

Payments associated with brand drugs may induce physicians to substitute generic drugs with brand drugs. To test this we estimate the causal effects of payments on generic drugs. For drugs where bioequivalent generics are available, payments may cause physicians to directly substitute the brand version for the generic version. However, this is not the only way in which payments may affect brand-generic substitution. Substitution can also occur if, for example, payments cause physicians to switch a patient from the first line treatment, metformin (which is available as a generic), to a brand drug in a different drug class, or add one or more brand drugs to the treatment regime.

Using the same forest-based approach and payment propensities as in the main estimation, we find that on average payments have no statistically significant effects on claims for generic drugs. The average causal effects of a $10 payment amounts to 0.007 with a standard error of 0.02. The magnitude of this coefficient remains very small across different estimation methods. All results are reported in Table 13 in Appendix.

Our findings thus suggest that payments affect drug costs through higher prescriptions of brand drugs on top of prescriptions for generic drugs, as opposed to substituting generics. This result is consistent with the findings of Carey et al. (2021) who show that paid physicians switch patients to generic alternatives (for bioequivalent drugs) just as quickly as unpaid physicians. This result is also not surprising when we think about the specifics of the antidiabetics market during our sample period: There are a number of popular drug classes for antidiabetics which face no generic competition during our sample (e.g. insulin, GLP-1 receptor
agonists, DPP-4 inhibitors and SGLT-2 inhibitors), and thus physicians have less opportunity to replace brand drugs with generics. Payments are also primarily linked to drugs in these classes. Other therapeutic fields may differ in this dimension and thus one should exercise caution when extrapolating this result to other diseases.

5.2 Binary causal effect estimation

Binary causal effect estimation assumes no effect heterogeneity in payments. As we find that responses at the physician level differ across payment values to a significant though relatively small extent, we expect no large differences, on average, between causal effects in the binary vs. continuous treatment case. However, in the binary treatment case, we expect that causal forests will underestimate causal effect heterogeneity: since the algorithm will likely assign physicians with high and low payments to the same subsets of the data (to the same neighbors/weights), the estimated responses for these physicians will be the same. As a result, heterogeneity analysis will likely be biased. The severity of this bias remains, however, an empirical question.

First, we estimate probabilities to receive payments using the usual forest-based approach. We find that determinants of propensity score heterogeneity include most variables that previously explained propensities to receive (continuous) payments, and share the same sign (cfr. Table 10 and Table 14). In particular, larger payments are mostly directed towards male physicians, ceteris paribus.

Second, we estimate heterogeneous causal effects using the estimated propensity scores and our rich set of covariates. As expected, we estimate a similar average causal effect to the continuous treatment case, namely, brand claims increase by 0.11 for a $10 payment.

Third, we analyse causal effect heterogeneity. We estimate statistically significant responses for 97% of physicians. We find that the distributions of physician level responses in the binary versus continuous treatment case differ significantly. As expected, causal effect heterogeneity in the binary case is significantly lower than in the continuous case (standard deviation is 50% lower in the binary case). Compared to the continuous treatment case, Lasso selects a smaller set of variables driving causal effect heterogeneity (see Table 15 in Appendix). Similarly to the
continuous treatment case, the main driver of response heterogeneity is the share of LIS patients (see Figure 14). This variable explains 34% of the overall variance in responses.

In sum, the binary treatment case (i) shows that not accounting for heterogeneity in payment levels in the estimation procedure leads to the underestimation of physician response heterogeneity, and (ii) confirms that patients’ lower out-of-pocket costs, due to the receipt of a low-income subsidy, are the most important driver of physician response heterogeneity.

6 Conclusion

In this paper, we estimate the average and heterogeneous causal effects of drug firms’ payments to physicians on prescriptions and drug costs in the US. We use a random forest instantiation of the residualized regression estimator to flexibly control for a high-dimensional set of prescription determinants and sources of selection, thus correcting for the likely non-random assignment of payments.

Focusing on prescriptions for antidiabetic drugs prescribed under Medicare Part D, we find that, on average, a $10 payment increases the quantity of brand drugs prescribed by 0.1 claims. Relative to the low value of payments (a median of $40 per year), the effects on drug costs to treat diabetes are sizeable: On average, for every dollar received, payments generate a $6 increase in total drug costs.

Our analysis delves deeper and explores heterogeneity in physician responses to payments yielding two key insights: (i) The most important driver of physician responses is the insurance coverage of their patients. Specifically, physicians with a higher share of patients that benefit from a Medicare Part D low-income subsidy prescribe more brand drugs in response to payments. (ii) Payments of a higher value trigger stronger responses, although payment value matters less when a high share of patients benefit from a low-income subsidy.

We apply our model to compute the financial impact of a gift ban. We find that a gift ban in New Hampshire and Maine has the potential to decrease total costs to treat diabetes by 3%. In Vermont, our counterfactual analysis shows that the prevailing gift ban has resulted in savings amounting to 1% of the total drug cost. The figure is lower due to the different characteristics, and hence responses, of physicians in this state.
High and growing drug costs are a concern for policy makers. Given that existing research does not generally link payments with strong health benefits to patients, one upshot of this paper is that gift bans could be an effective way to contribute towards containing healthcare costs in the US. Assessing whether payments affect not only the quantity and costs of prescriptions but also their quality (drug efficacy) presents an exciting research opportunity that we hope to tackle in future work.

References

Agha, L. and D. Zeltzer (2022). Drug diffusion through peer networks: The influence of industry payments. *American Economics Journal: Economic Policy* 14(2), 1–33.

Alpert, A. E., W. N. Evans, E. M. Lieber, and D. Powell (2019). Origins of the opioid crisis and its enduring impacts. *National Bureau of Economic Research Working Paper*.

Amaral-Garcia, S. (2020). Medical device companies and doctors: Do their interactions affect medical treatments? *ECARES working paper 2020-18*.

Athey, S., J. Tibshirani, and S. Wager (2019). Generalized random forests. *The Annals of Statistics* 47(2), 1148–1178.

Belloni, A. and V. Chernozhukov (2013). Least squares after model selection in high-dimensional sparse models. *Bernoulli* 19(2), 521–547.

Bergman, A., M. Grennan, and A. Swanson (2021). Lobbying physicians: Payments from industry and hospital procurement of medical devices. *National Bureau of Economic Research Working Paper*.

Breiman, L. (2001). Random forests. *Machine Learning* 45(1), 5–32.

Brennan, P. J. (2019). Banning gifts from pharma to doctors is a big step toward increasing patient trust. *The Philadelphia Inquirer*. Available: https://www.inquirer.com/opinion/commentary/pharmaceutical-industry-doctor-gifts-phadelphia-city-council-20190129.html (Accessed 26 January 2022).

Britto, D. G. C., P. Pinotti, and B. Sampaio (2022). The effect of job loss and unemployment insurance on crime in Brazil. *Econometrica (forthcoming)*.

Bronnenberg, B. J., J.-P. Dubé, M. Gentzkow, and J. M. Shapiro (2015). Do pharmacists buy bayer? informed shoppers and the brand premium. *The Quarterly Journal of Economics* 130(4), 1669–1726.
Carey, C., E. M. Lieber, and S. Miller (2021). Drug firms’ payments and physicians’ prescribing behavior in Medicare Part D. *Journal of Public Economics 197*, 104402.

Carrera, M., D. P. Goldman, G. Joyce, and N. Sood (2018). Do physicians respond to the costs and cost-sensitivity of their patients? *American Economic Journal: Economic Policy 10*(1), 113–52.

CDC (2021). Diabetes risk factors. Centers for Disease Control and Prevention (CDC). Available: https://www.cdc.gov/diabetes/basics/risk-factors.html (Accessed 26 January 2022).

Chernozhukov, V., D. Chetverikov, M. Demirer, E. Duflo, C. Hansen, W. Newey, and J. Robins (2018). Double/debiased machine learning for treatment and structural parameters. *The Econometrics Journal 21*(1), C1–C68.

Chernozhukov, V., C. Hansen, and M. Spindler (2015). Valid post-selection and post-regularization inference: An elementary, general approach. *Annual Review of Economics 7*(1), 649–688.

Chernozhukov, V., W. Newey, and R. Singh (2022). Automatic debiased machine learning of causal and structural effects. *Econometrica 90*(3), 967–1027.

Conover, W. J. (1971). *Practical Nonparametric Statistics*. New York: John Wiley and Sons.

Cubanski, J., A. Damico, and T. Neuman (2018). Medicare part D in 2018: The latest on enrollment, premiums, and cost sharing. Kaiser Family Foundation. May 17. Online. Available: https://www.kff.org/medicare/issue-brief/medicare-part-d-in-2018-the-latest-on-enrollment-premiums-and-cost-sharing/ (Accessed 11 November 2019).

Cubanski, J., T. Neuman, S. True, and A. Damico (2019). How much does Medicare spend on insulin? Kaiser Family Foundation. April 1. Available: https://www.kff.org/medicare/issue-brief/how-much-does-medicare-spend-on-insulin/ (Accessed 11 November 2019).

Datta, A. and D. Dave (2017). Effects of physician-directed pharmaceutical promotion on prescription behaviors: Longitudinal evidence. *Health Economics 26*(4), 450–468.

Dieleman, J., R. Baral, M. Birger, and al. (2016). US spending on personal health care and public health, 1996-2013. *JAMA internal medicine 316*(24), 2627–2646.

Draznin, B., V. R. Aroda, G. Bakris, G. Benson, F. M. Brown, R. Freeman, J. Green, E. Huang, D. Isaacs, S. Kahan, et al. (2022). 9. pharmacologic approaches to glycemic treatment: Standards of medical care in diabetes-2022. *Diabetes Care 45*(1), S125–S143.

Epstein, A. J. and J. D. Ketcham (2014). Information technology and agency in physicians’ prescribing decisions. *The RAND Journal of Economics 45*(2), 422–448.
Fabbri, A., A. la Santos, S. Mezinska, S. Mulinari, and B. Mintzes (2018). Sunshine policies and murky shadows in Europe: disclosure of pharmaceutical industry payments to health professionals in nine European countries. *International Journal of Health Policy and Management* 7(6), 504–509.

Fernandez, F. and D. Zejcirovic (2018). The role of pharmaceutical promotion to physicians in the opioid epidemic. Working paper.

Friedman, J., T. Hastie, and R. Tibshirani (2010). Regularization paths for generalized linear models via coordinate descent. *Journal of Statistical Software* 33(1), 1–22.

Fugh-Berman, A. and S. Ahari (2007). Following the script: How drug reps make friends and influence doctors. *PLoS Medicine* 4(4), e150.

Gorlach, I. and G. Pham-Kanter (2013). Brightening up: the effect of the physician payment sunshine act on existing regulation of pharmaceutical marketing. *The Journal of Law, Medicine and Ethics* 41(1), 315–322.

Gravelle, H., D. Liu, C. Propper, and R. Santos (2019). Spatial competition and quality: Evidence from the English family doctor market. *Journal of Health Economics* 68, 102249.

Gravelle, H., A. Scott, P. Sivey, and J. Yong (2016). Competition, prices and quality in the market for physician consultations. *The Journal of Industrial Economics* 64(1), 135–169.

Grennan, M., K. Myers, A. Swanson, and A. Chatterji (2021). No free lunch? welfare analysis of firms selling through expert intermediaries. National Bureau of Economic Research Working Paper.

Hams, M. and W. Wilkinson (2013). Vermont shows the way on physician payment sunshine act enforcement. Available: https://communitycatalyst.org/blog/vermont-shows-the-way-on-physician-payment-sunshine-act-ppsa-enforcement (Accessed 11 November 2019).

Imbens, G. (2000). The role of the propensity score in estimating dose-response functions. *Biometrika*.

Imbens, G. and D. Rubin (2015). *Causal Inference for Statistics, Social, and Biomedical Sciences - Chapter 14 Assessing Overlap in Covariate Distributions*, Volume III. Cambridge University Press.

Kaiser Family Foundation (2019). 10 essential facts about Medicare and prescription drug spending. Available: https://www.kff.org/infographic/10-essential-facts-about-medicare-and-prescription-drug-spending/. (Accessed 26 January 2022).

Levene, H. (1960). Robust tests for equality of variances. *Contributions to Probability and Statistics: Essays in Honor of Harold Hotelling*. 

38
Lipska, K. J., J. S. Ross, Y. Miao, N. D. Shah, S. J. Lee, and M. A. Steinman (2015). Potential overtreatment of diabetes mellitus in older adults with tight glycemic control. *JAMA internal medicine* 175(3), 356–362.

Liu, Y.-M., Y.-H. K. Yang, and C.-R. Hsieh (2009). Financial incentives and physicians’ prescription decisions on the choice between brand-name and generic drugs: Evidence from Taiwan. *Journal of Health Economics* 28(2), 341–349.

Lu, F. (2014). Insurance coverage and agency problems in doctor prescriptions: evidence from a field experiment in China. *Journal of Development Economics* 106, 156–167.

Lundin, D. (2000). Moral hazard in physician prescription behavior. *Journal of Health Economics* 19(5), 639–662.

Maciejewski, M. L., X. Mi, J. Sussman, M. Greiner, L. H. Curtis, J. Ng, S. C. Haffer, and E. A. Kerr (2018). Overtreatment and deintensification of diabetic therapy among medicare beneficiaries. *Journal of General Internal Medicine* 33(1), 34–41.

Méndez, S. J., A. Scott, and Y. Zhang (2021). Gender differences in physician decisions to adopt new prescription drugs. *Social Science and Medicine* 277, 113886.

Mueller, T., C. Schmid, and M. Gerfin (2021). Rents for pills: Financial incentives and physician behavior. Available at SSRN 4066408.

Nie, X. and S. Wager (2021). Quasi-oracle estimation of heterogeneous treatment effects. *Biometrika* 108(2), 299–319.

Onpoint Health Data (2010). Tri-state variation in health services utilization and expenditures in northern new england. *Report upon request from the Vermont Department of Banking, Insurance, Securities and Health Care Administration (BISHCA).*

Papanicolas, I. and A. McGuire (2015). Do financial incentives trump clinical guidance? hip replacement in England and Scotland. *Journal of Health Economics* 44, 25–36.

Pearl, J. (2009). Causal inference in statistics: An overview. *Statist. Surv.* 3, 96–146.

Robinson, P. (1988). Root-n-consistent semiparametric regression. *Econometrica* 56(4), 931–954.

Rubin, D. B. (1974). Estimating causal effects of treatments in randomized and nonrandomized studies. *Journal of Educational Psychology* 5(5), 688–701.

Sullivan, T. (2018). California senate passes ban on “gifts” to physicians. *Policy and Medicine.* Available: https://www.policymed.com/2017/05/california-bans-gifts-to-doctors.html (Accessed 26 January 2022).
Sullivan, T. (2020). Maine finalizes its physician “gift ban” rules updates to $500 limit includes exemptions for pharmacists, speaker fees, expenses, accredited education and market research. *Policy and Medicine*. Available: https://www.policymed.com/2020/06/maine-finalizes-its-physician-gift-ban-rules-to-exclude-pharmacists-speaker-fees-expenses-accredited-education-and-market-research.html (Accessed 26 January 2022).

Tamblyn, R., P. McLeod, J. A. Hanley, N. Girard, and J. Hurley (2003). Physician and practice characteristics associated with the early utilization of new prescription drugs. *Medical Care*, 895–908.

Tibshirani, J., S. Athey, S. Wager, R. Friedberg, L. Miner, and M. Wright (2018). grf: Generalized random forests (beta). R package version 0.10.1. Available: http://CRAN.R-project.org/package=grf.

Valente, M. (2021). Policy evaluation of waste pricing programs using heterogeneous causal effect estimation. *arXiv preprint arXiv:2010.01105*.

Wager, S. and S. Athey (2018). Estimation and inference of heterogeneous treatment effects using random forests. *Journal of the American Statistical Association 113*(523), 1228–1242.

Wager, S. and S. Athey (2019). Estimating treatment effects with causal forests: An application. *Observational Studies 5*(2), 37–51.

Wilcoxon, F. (1945). Individual comparisons by ranking methods. *Biometrics Bulletin 1*(6), 80–83.

Yala, S. M., O. K. Duru, S. L. Ettner, N. Turk, C. M. Mangione, and A. F. Brown (2014). Patterns of prescription drug expenditures and medication adherence among Medicare Part D beneficiaries with and without the low-income supplement. *BMC Health Services Research 14*(1), 14–665.

Zhang, Y., S. J. Méndez, and A. Scott (2019). Factors affecting general practitioners’ decisions to adopt new prescription drugs—cohort analyses using australian longitudinal physician survey data. *BMC Health Services Research 19*(1), 1–12.

Zhou, X., S. Shrestha, H. Shao, and P. Zhang (2020). Factors contributing to the rising national cost of glucose-lowering medicines for diabetes during 2005–2007 and 2015–2017. *Diabetes Care 43*(10), 2396–2402.
Online Appendix

A Diabetes treatments

Diabetes has no cure and must be managed by life-long therapy. There are two main types of diabetes: type 1 and type 2. Type 1 patients are treated exclusively with insulin. Typically the treatment involves several insulins simultaneously. A patient can be treated with “rapid”, “long” or “intermediate” insulin. An interview with a diabetologist revealed that physicians may change the type of insulin prescribed to a patient within these groups due to reasons such as side effects, patient intolerance, insurance reimbursement, and due to the introduction of new insulin that is generally perceived as a better product by the physician.

Type 2 diabetes, which accounts for 90-95% of all cases, is treated in a more complex way. The first line treatment is metformin (and comprehensive lifestyle modification) which is used for as long as the body tolerates it, thereafter different drugs are added to the treatment regimen (Draznin et al., 2022). In patients with contraindications or intolerance to metformin, initial therapy should be based on patient factors (Draznin et al., 2022). When monotherapy with metformin is no longer effective, physicians can choose between prescribing drugs from different drug classes for example DPP-4 inhibitors or SGLT-2 inhibitors. Type 2 patients with a severe condition are treated with insulin therapy.

Diabetes drugs can be grouped in several classes: sensitizers, insulins, GLP-1 receptor agonists, DPP-4 inhibitors, SGLT-2 inhibitors, alpha-glucosidase inhibitors, secretagogues and injectable amylin analogues. Drugs within the same drug class are more substitutable than drugs in different classes. However, therapy often combines several drugs from different classes. The guidelines for diabetes treatment from the American Diabetes Association provides some suggestions of how this step-wise addition of drugs can proceed, however ultimately treatment is complex and depends on patient factors such as comorbidities, cardiovascular risk, preferences for side-effects, tolerance and cost (Draznin et al., 2022). Below we provide an overview of the brand drugs and generics available in each drug class. The FDA approval year of each drug and generic entry year (if relevant) is provided in parentheses. The table provides a list of all diabetes treatments that are in the final dataset.

• Sensitizers
  Biguanides/Metformin: Glucophage (1995), generic metformin hydrochloride (2003), Riomet (2003), Fortamet (2004), Glumetza (2005), Actoplus Met (2005; generic 2011)
  TZDs (Thiazolidinediones): Avandia (1999), Actos (1999; generic 2012), Avandamet (2002), Avandaryl (2005), Duetact (2006)

• Insulins
  Rapid and intermediate acting insulins: Humulin (1982), Novolin (1991), Humalog (1996), Novolog (2000), Apidra (2004), Afrezza (2014)
Long acting insulins: Lantus (2000), Levevir (2005), Toujeo (2015), Tresiba (2015), Basaglar (2015), Xultophy (2016), Soliqua (2016)

- **GLP-1 receptor agonists**
  - Byetta (2005), Victoza (2010), Bydureon (2012), Tanzeum (2014), Trulicity (2014), Adlyxin (2016)

- **DPP-4 inhibitors**
  - Januvia (2006), Janumet (2007), Onglyza (2009), Kombiglyze XR (2010), Tradjenta (2011), Jentadueto (2012), Nesina (2013), Oseni (2013), Kazano (2013)

- **SGLT-2 inhibitors**
  - Invokana (2013), Farxiga (2014), Invokamet (2014), Jardiance (2014), Xigduo (2014), Glyxambi (2015), Synjardy (2015)

- **Alpha-glucosidase inhibitors**
  - Precose (1995; generic 2008), Glyset (1996)

- **Secretagogues**
  - Sulfonylureas: Glucotrol (1984, generic 1994), Glynase (1992), Glyburide Micronized (1992; generic 1997), Amaryl (1995; generic 2005), Glucovance (2000, generic 2004), Metaglip (2002; generic 2005)
  - Non-sulfonylurea secretagogues/Meglitinides: Prandin (1997, generic 2013), Starlix (2000, generic 2009), Prandimet (2008)

- **Injectable amylin analogues**
  - Symlin (2005)
## Table 6: Diabetes treatments in the sample

| Firm and drug name          | Drug class               |
|-----------------------------|--------------------------|
| PFIZER GLYSET               | Alpha-glucosidase        |
| GENERIC COMPANY PRECOSE     | Alpha-glucosidase        |
| AstraZeneca SYMLIN          | Amylin agonists          |
| Merck JANUMET               | DPP-4 inhibitors         |
| Boehringer Ingelheim TRAJENTA | DPP-4 inhibitors     |
| Merck JANUVIA               | DPP-4 inhibitors         |
| Takeda KAZANO               | DPP-4 inhibitors         |
| Boehringer Ingelheim JENTADUETO | DPP-4 inhibitors |
| AstraZeneca ONGLYZA         | DPP-4 inhibitors         |
| AstraZeneca KOMBIGLYZE XR   | DPP-4 inhibitors         |
| Takeda OSENI                | DPP-4 inhibitors         |
| Takeda NESINA               | DPP-4 inhibitors         |
| GlaxoSmithKline TANZEUM     | GLP-1 agonists           |
| Novo Nordisk VICTOZA       | GLP-1 agonists           |
| AstraZeneca BYDUREON        | GLP-1 agonists           |
| AstraZeneca BYETTA          | GLP-1 agonists           |
| Eli Lilly TRULICITY         | GLP-1 agonists           |
| Novo Nordisk TRESIBA       | Insulins - Long          |
| Novo Nordisk LEVEMIR       | Insulins - Long          |
| Eli Lilly BASAGLAR          | Insulins - Long          |
| Sanofi TOUJEO SOLOSTAR     | Insulins - Long          |
| Sanofi LANTUS              | Insulins - Long          |
| Eli Lilly HUMULIN          | Insulins - Rapid & intermediate |
| Novo Nordisk NOVOLOG       | Insulins - Rapid & intermediate |
| Mannkind AFREZZA           | Insulins - Rapid & intermediate |
| Eli Lilly HUMALOG          | Insulins - Rapid & intermediate |
| Novo Nordisk NOVOLIN       | Insulins - Rapid & intermediate |
| Sanofi APIIDRA             | Insulins - Rapid & intermediate |
| Boehringer Ingelheim GLYXAMBI | SGLT-2 inhibitors   |
| Janssen (Sub. Pfizer) INVOKEMET | SGLT-2 inhibitors |
| Boehringer Ingelheim SYNJARDY | SGLT-2 inhibitors |
| Boehringer Ingelheim JARDANCE | SGLT-2 inhibitors |
| Janssen (Sub. Pfizer) INVOKANA | SGLT-2 inhibitors |
| AstraZeneca FARXIGA        | SGLT-2 inhibitors         |
| AstraZeneca XIGDUO         | SGLT-2 inhibitors         |
| Novartis STARLIX           | Secretagogues - Non-sulfonylureas |
| Gemini Laboratories PRANDIN | Secretagogues - Non-sulfonylureas |
| Generic Company STARLIX    | Secretagogues - Non-sulfonylureas |
| Generic Company PRANDIN    | Secretagogues - Non-sulfonylureas |
| Generic Company GLYBURIDE (MICRONIZED) | Secretagogues - Sulfonylureas |
| Sanofi AMARYL              | Secretagogues - Sulfonylureas |
| Generic Company GLYBURIDE BRAND | Secretagogues - Sulfonylureas |
| Generic Company GLUCOTROL  | Secretagogues - Sulfonylureas |
| Bristol Myers Squibb GluCOVANCE | Secretagogues - Sulfonylureas |
| Pfizer Glucotrol           | Secretagogues - Sulfonylureas |
| Generic Company METAGLIP   | Secretagogues - Sulfonylureas |
| Generic Company AMARYL     | Secretagogues - Sulfonylureas |
| Generic Company GLUCOVANCE | Secretagogues - Sulfonylureas |
| Santarus Glumetza          | Sensitzers - Metformin   |
| Takeda ACTOPLUS MET        | Sensitzers - Metformin   |
| Andrx Labs FORTAMET        | Sensitzers - Metformin   |
| Sun Pharmaceutical Hiomet  | Sensitzers - Metformin   |
| Generic Company ACTOPLUS MET | Sensitzers - Metformin |
| Generic Company METFORMIN BRAND | Sensitzers - Metformin |
| Bristol Myers Squibb GlucoPHAGE | Sensitzers - Metformin |
| Takeda ACTOS               | Sensitzers - TZDs         |
| Generic Company ACTOS      | Sensitzers - TZDs         |
B Data Appendix

Dataset construction

The construction of the dataset follows four main steps. First, the set of all antidiabetic drugs is established. Next, we isolate prescription data from Medicare Part D on physicians that prescribe antidiabetic drugs in the states of Vermont, New Hampshire or Maine. In a third step, Part D prescriptions are matched with the payments data at the physician-drug-year level. Finally, the dataset is aggregated to the physician-year level. Below, each step is described in greater detail.

The set of all approved treatments for diabetes (both brand and generic) is identified using the FDA Orange Book matched with Anatomical Therapeutic Chemical (ATC) Codes. We select drugs with the ATC “A10 - Drugs used in diabetes”. The complete list of antidiabetic treatments is matched with the drug names in Part D and Open Payments using string-matching algorithms.

Information on the prescriptions of antidiabetic drugs, including the name and address of the prescribing physician, is extracted from the Medicare Part D database. This sample provides the universe of antidiabetic medications prescribed to patients enrolled in Medicare Part D. The sample is restricted to physicians located in Vermont, New Hampshire or Maine.

Part D prescriptions are matched with the Open Payments data contained in the general payments file at the physician-drug-year level for the years 2014 to 2017. Each payment in Open Payments is linked to a specific drug, in cases where multiple drugs are listed the payment value is split equally amongst all listed drugs. The dataset is aggregated to the physician-drug-year level such that payment values reflect the sum of all payments associated with a specific drug in a given year. The data is matched with Part D on the basis of drug name and physician name. Since there is no common physician ID that connects Part D and Open payments, the datasets are matched on the basis of full name and 9-digit zip code. This is complemented by a manual check in cases where physicians in Part D did not directly match to the Open Payments database.

Lastly, the dataset at the physician-drug-year is aggregated to the physician-year level. The final sample is restricted to physicians. Nurses and physician assistants are dropped due to the fact that they never receive payments from pharmaceutical companies. Observations with missing information for any variable are dropped. Certain information in Part D is redacted for cases where a drug is prescribed to 10 or fewer unique beneficiaries, thus physicians who prescribe antidiabetic medication to a total of 10 or fewer beneficiaries per year end up being dropped. Despite the ban, a small fraction (4%) of physicians in Vermont received a positive payment at some point in the time span 2014 to 2017. These observations were excluded from the analysis.

---

38 The Part D data uses each provider’s NPI number as its unique ID. The Open Payments system uses a randomly generated unique ID.

39 These physicians account for only 8% of all claims in the data, thus the final dataset covers the majority of prescriptions by physicians for diabetes treatments.
| Variable | Description | Source |
|----------|-------------|--------|
| **PAYMENTS** | | |
| No. payments | Total number of payments for physician per year (Open Payments) | |
| No. in-kind payments | Total number of in-kind payments for physician per year (Open Payments) | |
| No. cash payments | Total number of cash payments for physician per year (Open Payments) | |
| Value ($) payments | Value ($) of payments for physician per year (Open Payments) | |
| Value ($) in-kind payments | Value ($) of in-kind payments for physician per year (Open Payments) | |
| Value ($) cash payments | Value ($) of cash payments for physician per year (Open Payments) | |
| **DRUG CLAIM COUNTS** | | |
| Generic drugs | Number of Medicare Part D claims for generic antidiabetic drugs prescribed by physician per year (Part D Detailed Data) | |
| Brand drugs | Number of Medicare Part D claims for brand antidiabetic drugs prescribed by physician per year (Part D Detailed Data) | |
| **TOTAL DRUG COSTS** | | |
| Generic drugs | Total yearly drug cost for generic antidiabetic prescribed by physician. The drug cost includes the amount paid by the Part D plan, the beneficiary, government subsidies, and any other third-party payers. (Part D Detailed Data) | |
| Brand drugs | Total yearly drug cost for brand antidiabetics prescribed by physician. The drug cost includes the amount paid by the Part D plan, the beneficiary, government subsidies, and any other third-party payers (Part D Detailed Data) | |
| **COVARIATES** | | |
| Family practitioner (0/1) | Indicator taking the value 1 if speciality is recorded as Family Practice, Family Medicine and/or General Practice (vs. Specialist practitioner) | (Part D Prescriber Summary, Open Payments) |
| Male physician (0/1) | Indicator taking the value 1 if physician gender is male | (Part D Detailed Data) |
| New practitioner (0/1) | Physicians with a provider enumeration year (date of NPI assignment) later than or equal to 2008 (NPPES NPI Registry) | |
| Share of beneficiaries age 65 | Share of beneficiaries age 65 and older with at least one claim for an antidiabetic drug | (Part D Detailed Data) |
| Share of male beneficiaries | Share of male beneficiaries | (Part D Prescriber Summary) |
| MAPD claim share | Share of total claims attributable to beneficiaries covered by MAPD plans | (Part D Prescriber Summary) |
| LIS claim share | Share of total claims attributable to beneficiaries with a Part D low-income subsidy | (Part D Prescriber Summary) |
| No. of beneficiaries | Number of unique beneficiaries | (Part D Detailed Data) |
| Average age of beneficiaries | Average age of beneficiaries (Beneficiary age is calculated at the end of the calendar year or at the time of death) | (Part D Prescriber Summary) |
| Average risk score of beneficiaries | Average Hierarchical Condition Category (HCC) risk score of beneficiaries determined based on beneficiaries’ diagnoses or demographic risk factors | (Part D & Prescriber Summary) |
| Share of beneficiaries with insulin claims | Share of claims for insulin out of all claims for antidiabetic drugs | (Part D Detailed Data) |
| Population | Total population in physician practice location (5-digits zip) (American Community Survey, ACS) | |
| Population per sq. mile | Total population divided by area in square miles for physician practice location (5-digits zip) (ACS) | |
| Median household income | Median household income in the past 12 months in dollars for physician practice location (5-digits zip) (ACS) | |
| No. diagnosed with diabetes | Number of adults (20+) with diagnosed diabetes in physician practice location (county) | (Centers for Disease Control and Prevention, CDC) |
| Percent diagnosed with diabetes | Age-adjusted percentage of adults (20+) with diagnosed diabetes in physician practice location (county) | (CDC) |
| No. obese | Number of adults (20+) with obesity in physician practice location (county) | (CDC) |
| Percent obese | Age-adjusted percentage of adults (20+) with obesity in physician practice location (county) | (CDC) |
| Population w/o high school degree < 24 | Share of population below 24 years with less than high school degree in physician practice location (5-digits zip) | (ACS) |

Continued on next page
| Description                                                                 | Description                                                                 |
|----------------------------------------------------------------------------|----------------------------------------------------------------------------|
| Population w. college degree < 24                                         | Share of population below 24 years with some college or associate’s degree in physician practice location (5-digit zip) (ACS) |
| Population w/o high school degree 25-34                                    | Share of population 25 to 34 years with less than high school degree in physician practice location (5-digit zip) (ACS) |
| Population w. college degree 25-34                                         | Share of population 25 to 34 years with some college or associate’s degree in physician practice location (5-digit zip) (ACS) |
| Population w/o high school degree 35-44                                    | Share of population 35 to 44 years with less than high school degree in physician practice location (5-digit zip) (ACS) |
| Population w. college degree 35-44                                         | Share of population 35 to 44 years with some college or associate’s degree in physician practice location (5-digit zip) (ACS) |
| Population w/o high school degree 46-64                                    | Share of population 46 to 64 years with less than high school degree in physician practice location (5-digit zip) (ACS) |
| Population w. college degree 46-64                                         | Share of population 46 to 64 years with some college or associate’s degree in physician practice location (5-digit zip) (ACS) |
| Population w/o high school degree > 65                                     | Share of population above 65 years with less than high school degree in physician practice location (5-digit zip) (ACS) |
| Population w. college degree > 65                                         | Share of population above 65 years with some college or associate’s degree in physician practice location (5-digit zip) (ACS) |
| Share of White population                                                  | Share of White population in physician practice location (5-digit zip) (ACS) |
| Share of Black population                                                  | Share of Black population in physician practice location (5-digit zip) (ACS) |
| Share of Asian population                                                  | Share of Asian population in physician practice location (5-digit zip) (ACS) |
| Share of Multi-race population                                             | Share of Multi-race population in physician practice location (5-digit zip) (ACS) |
| No. of payments other physicians in zip code                               | Number of total payments to other physicians in sample in same 5-digit zip area and year (Open Payments & Part D Detailed Data) |
| No. of payments other physicians in county                                 | Number of total payments to other physicians in sample in same county and year (Open Payments & Part D Detailed Data) |
| No. of physicians in zip code                                              | Number of other physicians in sample in same 5-digit zip area (Part D Detailed Data) |
| No. of physicians in county                                                | Number of other physicians in sample in same county (Part D Detailed Data) |
C Additional summary statistics

Figure 9: Total payments \( (p, \$) \) per inhabitant aggregated by area (5-digit zip code) across Maine and New Hampshire over 2014-2017.
## Table 8: Summary statistics for physicians receiving vs. not receiving payments in the states of New Hampshire and Maine over 2014-2017.

| Variable                                | No Payment | Received Payment |
|-----------------------------------------|------------|------------------|
|                                        | Mean | Min | Max | Std | Mean | Min | Max | Std |
| **DRUG CLAIM COUNTS**                   |      |     |     |     |      |     |     |     |
| Generic drugs                           | 183.757 | 11  | 819 | 111.51 | 231.353 | 0  | 861 | 119.271 |
| Brand drugs                             | 95.403 | 0   | 1027 | 104.244 | 151.329 | 11 | 948 | 121.76 |
| **TOTAL DRUG COSTS $**                  |      |     |     |     |      |     |     |     |
| Generic drugs                           | 3233.372 | 46.9 | 47291.47 | 3429.464 | 3945.218 | 0  | 36506.27 | 3385.066 |
| Brand drugs                             | 45325.793 | 0 | 353068.6 | 43458.835 | 77392.801 | 2287.03 | 417772.8 | 56560.935 |
| **COVARIATES**                          |      |     |     |     |      |     |     |     |
| Physician                               |      |     |     |     |      |     |     |     |
| General practice (0/1)                  | 0.62  | 0   | 1   | 0.485 | 0.684 | 0   | 1   | 0.465 |
| Male physician (0/1)                    | 0.671 | 0   | 1   | 0.47  | 0.863 | 0   | 1   | 0.344 |
| New practitioner (0/1)                  | 0.142 | 0   | 1   | 0.35  | 0.102 | 0   | 1   | 0.303 |
| Antidiabetics claim share                | 0.053 | 0.008 | 0.275 | 0.017 | 0.057 | 0.016 | 0.1 | 0.014 |
| Physician's patients                    |      |     |     |     |      |     |     |     |
| Share of beneficiaries > 65             | 0.126 | 0   | 1   | 0.265 | 0.142 | 0   | 1   | 0.251 |
| Share of male beneficiaries             | 0.427 | 0.11 | 0.789 | 0.104 | 0.454 | 0.205 | 0.701 | 0.074 |
| MAPD claim share                        | 0.202 | 0   | 0.998 | 0.144 | 0.21  | 0.005 | 0.596 | 0.143 |
| LIS claim share                         | 0.485 | 0.03 | 0.994 | 0.221 | 0.532 | 0.044 | 0.953 | 0.217 |
| No. of beneficiaries                    | 337.924 | 45 | 1082 | 143.693 | 356.251 | 62 | 921 | 142.014 |
| Average age of beneficiaries            | 70.937 | 44 | 87 | 4.558 | 70.17 | 53 | 82 | 4.337 |
| Average risk score of beneficiaries     | 1.219 | 0.693 | 2.901 | 0.277 | 1.159 | 0.8 | 2.776 | 0.197 |
| Share of beneficiaries with insulin claims | 0.227 | 0 | 0.927 | 0.167 | 0.219 | 0 | 1 | 0.137 |
| Physician's practice location           |      |     |     |     |      |     |     |     |
| Population                             | 16677.934 | 41 | 45011 | 11592.392 | 14264.166 | 41 | 37301 | 10688.136 |
| Population per sq. mile                 | 882.913 | 1.028 | 8655.903 | 1467.581 | 665.439 | 3.641 | 8614.026 | 1133.737 |
| Median household income $                | 56040.309 | 25536 | 131490 | 18989.476 | 52704.982 | 25536 | 131490 | 16530.023 |
| No. diagnosed with diabetes             | 12741.611 | 1523 | 29827 | 8082.757 | 12196.582 | 1696 | 29827 | 7308.108 |
| Percent diagnosed with diabetes         | 8.158 | 5.6 | 10.6 | 1.081 | 8.46 | 6.2 | 10.6 | 1.09 |
| No. obese                               | 37853.849 | 4576 | 87652 | 24304.174 | 35810.59 | 4576 | 87652 | 22055.19 |
| Percent obese                           | 28.694 | 22.7 | 36.6 | 3.428 | 29.398 | 22.7 | 35.9 | 3.589 |
| Population w/o high school degree < 24  | 0.873 | 0.283 | 1 | 0.079 | 0.876 | 0.467 | 1 | 0.072 |
| Population w. college degree < 24       | 0.55  | 0   | 1   | 0.143 | 0.517 | 0.091 | 1 | 0.156 |
| Population w/o high school degree 25-34 | 0.063 | 0 | 0.441 | 0.044 | 0.06 | 0 | 0.283 | 0.046 |
| Population w. college degree 25-34      | 0.356 | 0 | 0.947 | 0.166 | 0.317 | 0 | 0.793 | 0.165 |
| Population w/o high school degree 35-44 | 0.056 | 0 | 0.328 | 0.043 | 0.063 | 0 | 0.204 | 0.048 |
| Population w. college degree 35-44      | 0.363 | 0 | 0.943 | 0.171 | 0.307 | 0 | 0.851 | 0.167 |
| Population w/o high school degree 45-64 | 0.072 | 0 | 0.233 | 0.04 | 0.076 | 0 | 0.214 | 0.042 |
| Population w. college degree 45-64      | 0.314 | 0 | 0.813 | 0.137 | 0.267 | 0 | 0.697 | 0.125 |
| Population w/o high school degree > 65  | 0.149 | 0 | 0.42 | 0.071 | 0.17 | 0 | 0.297 | 0.078 |
| Population w. college degree > 65       | 0.274 | 0 | 0.718 | 0.119 | 0.234 | 0 | 0.394 | 0.106 |
| Share of White population                | 0.932 | 0.499 | 1 | 0.047 | 0.938 | 0.498 | 0.996 | 0.05 |
| Share of Black population                | 0.016 | 0 | 0.136 | 0.021 | 0.013 | 0 | 0.124 | 0.018 |
| Share of Asian population                | 0.021 | 0 | 0.317 | 0.023 | 0.017 | 0 | 0.317 | 0.024 |
| Share of Multi-race population           | 0.001 | 0 | 0.023 | 0.003 | 0.001 | 0 | 0.019 | 0.002 |
| Physician's network                     |      |     |     |     |      |     |     |     |
| No. of payments other physicians in zip code | 1.081 | 0 | 9 | 1.601 | 1.732 | 0 | 8 | 1.871 |
| No. of payments other physicians in county | 8.285 | 0 | 23 | 5.955 | 9.776 | 0 | 22 | 5.221 |
| No. of physicians in zip code           | 12.404 | 0 | 46 | 10.52 | 9.146 | 0 | 42 | 8.099 |
| No. of physicians in county             | 78.418 | 1 | 155 | 46.347 | 73.49 | 8 | 155 | 44.709 |
| Observations                            | 4215  | 3215 | 2415 | 3215 | 459 | 459 | 459 | 459 |
Table 9: Summary statistics for physicians in the state of Vermont over 2014-2017. Vermont has a ban on payments since 2009.

| Variable                        | Mean   | Min   | Max   | Sd    |
|---------------------------------|--------|-------|-------|-------|
| **DRUG CLAIM COUNTS**           |        |       |       |       |
| Generic drugs                   | 167.816| 0     | 625   | 104.494|
| Brand drugs                     | 103.508| 0     | 1284  | 123.295|
| **TOTAL DRUG COSTS $**          |        |       |       |       |
| Generic drugs                   | 2892.58| 0     | 38769.25| 3001.293|
| Brand drugs                     | 59786.831| 0     | 1189548| 92801.63|
| **COVARIATES**                  |        |       |       |       |
| **Physician**                   |        |       |       |       |
| Family practitioner (0/1)       | 0.613  | 0     | 1     | 0.487 |
| Male physician (0/1)            | 0.617  | 0     | 1     | 0.486 |
| New practitioner (0/1)          | 0.103  | 0     | 1     | 0.304 |
| Antidiabetics claim share       | 0.058  | 0.013 | 0.603 | 0.061 |
| **Physician’s patients**        |        |       |       |       |
| Share of beneficiaries > 65     | 0.119  | 0     | 1     | 0.271 |
| Share of male beneficiaries     | 0.426  | 0.099 | 0.773 | 0.115 |
| MAPD claim share                | 0.088  | 0.005 | 0.98  | 0.084 |
| LIS claim share                 | 0.46   | 0.27  | 0.998 | 0.174 |
| No. of beneficiaries            | 321.593| 50    | 815   | 124.898|
| Average age of beneficiaries    | 71.335 | 56    | 84    | 3.35  |
| Average risk score of beneficiaries | 1.133 | 0.701 | 2.536 | 0.223 |
| Share of beneficiaries with insulin claims | 0.271 | 0     | 0.791 | 0.148 |
| **Physician’s practice location** |        |       |       |       |
| Population                      | 11205.964| 495   | 28849 | 7938.298|
| Population per sq. mile         | 643.631| 17.309| 5048.319| 1360.636|
| Median household income $        | 54799.75| 25.795| 114861| 12683.442|
| No. diagnosed with diabetes     | 4162.602| 511   | 7823  | 2114.854|
| Percent diagnosed with diabetes | 6.883  | 5.6   | 9.5   | 0.883 |
| No. obese                       | 13216.861| 1419  | 26194 | 7150.188|
| Percent obese                   | 25.165 | 19.4  | 34.3  | 4.086 |
| Population w/o high school degree < 24 | 0.867 | 0.316 | 1     | 0.1  |
| Population w. college degree < 24 | 0.518 | 0     | 1     | 0.184 |
| Population w/o high school degree 25-34 | 0.072 | 0     | 0.289 | 0.054 |
| Population w. college degree 25-34 | 0.392 | 0     | 0.7   | 0.18 |
| Population w/o high school degree 35-44 | 0.063 | 0     | 0.286 | 0.043 |
| Population w. college degree 35-44 | 0.42  | 0.005 | 0.799 | 0.157 |
| Population w/o high school degree 45-64 | 0.069 | 0.005 | 0.257 | 0.043 |
| Population w. college degree 45-64 | 0.371 | 0.066 | 0.704 | 0.119 |
| Population w/o high school degree > 65 | 0.137 | 0     | 0.371 | 0.072 |
| Population w. college degree > 65 | 0.333 | 0.059 | 0.718 | 0.122 |
| Share of White population       | 0.944  | 0.808 | 1     | 0.038 |
| Share of Black population       | 0.013  | 0     | 0.07  | 0.015 |
| Share of Asian population       | 0.017  | 0     | 0.116 | 0.02 |
| Share of Multi-race population  | 0.001  | 0     | 0.008 | 0.001 |
| **Physician’s network**         |        |       |       |       |
| No. of payments other physicians in zip code | 0  | 0     | 0     | 0 |
| No. of payments other physicians in county | 0  | 0     | 0     | 0 |
| No. of physicians in zip code   | 7.166  | 0     | 24    | 6.06  |
| No. of physicians in county     | 30.22  | 0     | 67    | 18.857|
| Observations                    | 1,019  | 1,019 | 1,019 | 1,019 |
D Predicted propensities to receive payments

Our sample of physicians contains observations on covariates and treatment status with only limited overlap in terms of covariates. Thus, we construct a subsample that has a more substantial degree of overlap. We do so by discarding some units in the treatment group and some in the control group. In particular, we exclude units with GPS values outside the 0.05 and 0.95 quantiles of the GPS distribution of treated and control units, respectively. The resulting sample achieves good overlap statistics, namely, all units in each treatment group have a close comparison in the other treatment group.

In order to compare the distributions of GPS of treated and control units, we compute the normalized difference in means, coverage frequencies, and the number of units with a close comparison unit. The normalized difference in means is less than one standard deviation (0.27). 93% (92%) of treated (control) units have covariate values within the 0.025 and 0.975 quantiles of the empirical distribution of the covariate values. Lastly, for all units, there is at least one unit with the other treatment status with a less than 10% difference in GPS. Thus, overlap statistics indicate that we can estimate causal effects credibly for most units without extrapolation (Imbens and Rubin, 2015). Figure 10 below shows evidence of the propensity score overlap between physicians receiving (white) and not receiving (grey) payment.

Figure 10: Common support condition for physicians receiving (treated) and not receiving (untreated) payment.
Table 10: Main drivers of propensities to receive payment (P) and brand drug prescribing (Y).

OLS coefficients of variables selected by Lasso. Coefficients of rescaled variables reflect the variable’s relative importance in explaining the outcome.

| Dependent variable: | (1) Predicted P | (2) Predicted Y |
|---------------------|----------------|----------------|
| No. of beneficiaries | 0.117*** (0.015) | 0.358*** (0.010) |
| Average age of beneficiaries | -0.164*** (0.021) | 0.259*** (0.015) |
| Average risk score of beneficiaries | 0.259*** (0.024) | |
| Male physician | 0.184*** (0.048) | 0.148*** (0.022) |
| New practitioner | -0.172*** (0.030) | |
| Share of beneficiaries with insulin claims | 0.318*** (0.012) | |
| Antidiabetics claim share | 0.189*** (0.015) | 0.241*** (0.010) |
| LIS claim share | 0.144*** (0.027) | 0.414*** (0.017) |
| Share of male beneficiaries | 0.046* (0.023) | |
| Population w/o high school degree > 65 | 0.065*** (0.016) | |
| Share of White population | -0.078*** (0.016) | |
| Observations | 3,673 | 3,674 |
| Fixed effects (year, state) | Yes | Yes |
| Adjusted R^2 | 0.213 | 0.656 |

Note: *p<0.1; **p<0.05; ***p<0.01

Variable selection is robust against the inclusion of all interaction terms summing up to 704 covariates. Outcome and continuous covariates are rescaled so that coefficients are interpretable as changes in standard deviations of the outcome per standard deviation increase in the covariate. In model 1, we lose one observation due to a missing value in the share of male beneficiaries.
E Implementation details

We have explored from 500 to 10,000 trees in the RF, and treatment effect estimates become stable after 15,000 trees, thus, results are obtained using this value. All trees are grown with cross-validated values for the number of randomly subsampled covariates, minimum leaf size, and penalty for imbalanced splits, namely, splits in which the size of parent and child node are very different are penalized. In particular, each node is required to include a minimum number of both treated and control units, i.e., enough information about both factual and counterfactual to estimate the treatment effect reliably. For this reason, a penalization is imposed also to nodes including an unbalanced number of treated and control units. Following Athey et al. (2019), values for such parameters are obtained via cross-validation.\footnote{\textsuperscript{40}We use the software R-4.0.3 and the grf package version 1.2.0 (Tibshirani et al., 2018).}
Other results

Figure 11: Physician level estimates of payment effects (CAPE) on brand drug prescriptions aggregated by year. CAPE are measured as changes in brand drug claims for a $10 increase in payment.
Table 11: Average characteristics of physicians with high vs. low CAPE (Conditional Average Payment Effect). Variables are sorted by the values of the standardized difference in means (3).

|                                | CAPE<sup>high</sup> | CAPE<sup>low</sup> | Std. Diff. | p-value |
|--------------------------------|---------------------|--------------------|------------|---------|
| LIS claim share                | 0.56                | 0.38               | 0.87       | 0.004   |
| Family practitioner            | 0.74                | 0.44               | 0.63       | 0.011   |
| Average age of beneficiaries   | 69.89               | 72.43              | 0.60       | 0.005   |
| Share of White population      | 0.94                | 0.92               | 0.56       | 0.004   |
| Population                     | 14093.94            | 20259.52           | 0.54       | 0.006   |
| Population per sq. mile        | 554.70              | 1368.55            | 0.54       | 0.003   |
| Share of Black population      | 0.01                | 0.02               | 0.51       | 0.008   |
| Population w/o high school degree 45-64 | 0.06               | 0.08               | 0.48       | 0.006   |
| Share of beneficiaries > 65   | 0.08                | 0.21               | 0.47       | 0.012   |
| Share of Asian population      | 0.02                | 0.03               | 0.44       | 0.006   |
| No. of physicians in zip code  | 10.35               | 14.75              | 0.43       | 0.007   |
| Population w/o high school degree 35-44 | 0.05               | 0.07               | 0.43       | 0.007   |
| Share of Multi-race population | 0.00                | 0.00               | 0.43       | 0.007   |
| No. of beneficiaries           | 317.03              | 379.02             | 0.42       | 0.009   |
| Population w/o high school degree 25-34 | 0.06               | 0.07               | 0.40       | 0.009   |
| Population w/o high school degree > 65 | 0.14               | 0.17               | 0.34       | 0.005   |
| Average risk score of beneficiaries | 1.24             | 1.17               | 0.25       | 0.005   |
| Percent obese                  | 29.10               | 28.26              | 0.25       | 0.009   |
| No. diagnosed with diabetes    | 11957.19            | 13872.61           | 0.24       | 0.004   |
| No. obese                      | 35450.92            | 41193.65           | 0.24       | 0.007   |
| No. of physicians in county    | 73.85               | 84.42              | 0.23       | 0.011   |
| Share of beneficiaries with insulin claims | 0.24             | 0.20               | 0.22       | 0.003   |
| No. of payments other physicians in zip code | 1.02           | 1.39               | 0.22       | 0.012   |
| New practitioner               | 0.16                | 0.10               | 0.20       | 0.008   |
| Male physician                 | 0.66                | 0.75               | 0.19       | 0.012   |
| Antidiabetics claim share      | 0.05                | 0.06               | 0.15       | 0.011   |
| Population w. college degree > 65 | 0.28             | 0.26               | 0.14       | 0.010   |
| Share of male beneficiaries    | 0.42                | 0.44               | 0.12       | 0.003   |
| Population w. college degree 25-34 | 0.34             | 0.36               | 0.12       | 0.005   |
| Percent diagnosed with diabetes | 8.24              | 8.12               | 0.11       | 0.010   |
| Population w/o high school degree < 24 | 0.88           | 0.87               | 0.11       | 0.012   |
| Population w. college degree 45-64 | 0.31             | 0.30               | 0.10       | 0.023   |
| MAPD claim share               | 0.21                | 0.20               | 0.09       | 0.013   |
| Median household income $       | 56089.69            | 54816.87           | 0.07       | 0.010   |
| Value ($) payments             | 45.94               | 8.87               | 0.06       | 0.396   |
| Population w. college degree < 24 | 0.54             | 0.55               | 0.05       | 0.237   |
| No. of payments other physicians in county | 8.35             | 8.67               | 0.05       | 0.342   |
| Population w. college degree 35-44 | 0.36             | 0.36               | 0.01       | 0.866   |

Columns 1 and 2 report average characteristics of physicians with CAPE above and below median, respectively. Column 3 shows the magnitude of heterogeneity across groups measured as the standardized mean difference. Values of 0.2, 0.5, and 0.8 represent small, medium, and large heterogeneity between groups. Column 4 reports p-values testing for differences across groups with Holm-Bonferroni correction for multiple hypothesis testing.
Figure 12: Density of CAPE estimates for physicians responding to payments in Vermont (VT) versus New Hampshire (NH) and Maine (ME).

Figure 13: Density of CAPE estimates for physicians responding to payments in VT versus NH and ME, by LIS shares below (left) and above (right) the average in VT.

Table 12: Summary statistics of the share of LIS claims for paid physicians by payment values, namely, between the first and third quartiles (IQR), below the first quartile (<P25) and above the third quartile (>P75).

| Variable       | Value payments | Mean  | Sd   | Min  | P50  | P75  | Max   | Obs. |
|----------------|----------------|-------|------|------|------|------|-------|------|
| LIS claim share | IQR $[14; 104]$ | 0.519 | 0.201 | 0.044 | 0.53  | 0.664 | 0.948  | 229  |
|                | <P25 ($14)     | 0.487 | 0.222 | 0.030 | 0.478 | 0.660 | 0.994  | 3331 |
|                | >P75 ($104)    | 0.556 | 0.236 | 0.074 | 0.589 | 0.751 | 0.953  | 114  |
## G Payment influence on generic drugs

Table 13: Estimates of the average causal effect of a 10$ payment to physicians on generic drug claim counts. Standard errors are clustered by physician.

| Method                        | Model     | (1)  | (2)  |
|-------------------------------|-----------|------|------|
| OLS                           | without controls | -0.013 | -0.007 |
|                               |           | (0.019) | (0.02) |
|                               | with controls | 0.021 | 0.017 | (0.009) | (0.01) |
| Double machine learning       | with Lasso (Chernozhukov et al., 2015) | -0.012 | -0.008 | (0.019) | (0.02) |
|                               | with Post-Lasso (Chernozhukov et al., 2015) | -0.013 | -0.007 | (0.018) | (0.02) |
|                               | with CV Lasso (Friedman et al., 2010) | 0.019 | 0.013 | (0.005) | (0.007) |
|                               | with CV Elastic Net (Friedman et al., 2010) | 0.018 | 0.008 | (0.005) | (0.005) |
|                               | with CV Ridge (Friedman et al., 2010) | 0.012 | 0.006 | (0.005) | (0.007) |
|                               | with best estimator | -0.013 | -0.008 | (0.019) | (0.02) |
| Causal forest (Athey et al., 2019) |           | 0.007 | 0.013 | (0.017) | (0.017) |
| Observations                  |           | 3,674 | 3,674 |
| Fixed effects (physician, year, zip5) | No | Yes |
# Binary causal effect estimation

Table 14: Binary treatment case: Determinants of propensities to receive payment (predicted probabilities or propensity scores). OLS coefficients of variables selected by Lasso. Outcome and continuous covariates are rescaled so that coefficients’ units are the same.

| Dependent variable: | Predicted probabilities |
|---------------------|-------------------------|
| No. of beneficiaries | 0.122***                |
|                     | (0.012)                 |
| Average age of beneficiaries | $-0.077^{***}$                |
|                     | (0.013)                 |
| Male physician      | 0.918***                |
|                     | (0.036)                 |
| No. of physicians in zip code | $-0.134^{***}$                |
|                     | (0.012)                 |
| Percent diagnosed with diabetes | 0.097***                |
|                     | (0.013)                 |
| Antidiabetics claim share | 0.102***                |
|                     | (0.011)                 |
| LIS claim share     | 0.088***                |
|                     | (0.013)                 |
| Share of male beneficiaries | 0.042**                |
|                     | (0.017)                 |
| Population w/o high school degree >65 | 0.118***                |
|                     | (0.016)                 |
| Population w. college degree <24 | $-0.045^{***}$                |
|                     | (0.012)                 |
| Population w. college degree 35-44 | $-0.061^{***}$                |
|                     | (0.022)                 |
| Population w. college degree 45-64 | $-0.322^{***}$                |
|                     | (0.024)                 |

Observations 3,673  
Fixed effects (year, state) Yes  
Adjusted $R^2$ 0.547  

*Note:* $^*$p<0.1; $^{**}$p<0.05; $^{***}$p<0.01
Table 15: Binary treatment case: Main drivers of physicians’ response heterogeneity. OLS coefficients of variables selected by Lasso. Outcome and continuous covariates are rescaled so that coefficients’ units are the same. Three observations are excluded due to missing MAPD values.

| Dependent variable: | CAPE |
|---------------------|------|
| LIS claim share     | 0.343*** (0.042) |
| Percent diagnosed with diabetes | 0.147*** (0.033) |
| MAPD claim share    | 0.068 (0.047) |

Observations: 456
Fixed effects (year, state): Yes
Zip5 level variables: Yes
Adjusted $R^2$: 0.599

Note: *p<0.1; **p<0.05; ***p<0.01

Figure 14: Binary treatment case and CAPE heterogeneity in LIS shares: Estimates of payment effects (CAPE) on brand drug prescriptions fitted at different percentile values of LIS shares.