Supply-Side Determinants of Prescription Opioid Use: Evidence from Primary Care

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Abstract

Primary care is the most frequently utilized health service and is the source of nearly half of all opioids prescribed in the United States. This paper studies the impact of exposure to high prescribing primary care providers (PCP) on opioid use, opioid use disorder, and mental health. Using electronic health records, we exploit within-facility quasi-random assignment of providers, who differ in their opioid prescribing tendency, to 650,000 new patients enrolling in care with the Veterans Health Administration. We find that assignment to a PCP who prescribes opioids at a 2.54 percentage point (pp) higher rate (equivalent to the average difference between a 90th and 10th percentile prescriber within a facility) increases the probability of long-term opioid use by 20% (or 0.43pp) and development of opioid use disorder by 4% (or 0.035pp). Veterans' mental health deteriorates: the three-year likelihood of a depression diagnosis increases by 1.3% (or 0.31pp). We find that PCPs with more cautious prescribing behavior rely more on non-opioid pain management and adhere more to clinical recommendations on naloxone distribution. Finally, in an event study model, we fail to detect statistically significant effects of any of the six major state-level opioid-related policies implemented to date on lenient prescribers' opioid prescribing.

1. Introduction

Opioid use disorder is an adverse health condition that causes harm to affected individuals and carries large negative externalities for families, as well as society as a whole. To develop

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effective policy solutions aimed at curbing the associated epidemic, it is crucial to uncover what causes individuals to develop a dependence on opioids. Prescription opioids prescribed by clinicians for pain relief are a potential suspect, but rigorous empirical research quantifying the role of provider behavior is scant. This is particularly true for primary care, which is the most frequently utilized health service, as well as the source of nearly half of all opioids prescribed in the United States (Levy et al., 2015).

In this paper, we quantify the impact of primary care provider (PCP) type, given by their propensity to prescribe opioids, on long-term opioid use and abuse, mental health, and mortality of more than 650,000 patients. We circumvent key endogeneity concerns related to patient-provider matches by leveraging the unique assignment process in the Veterans Health Administration (VHA): new enrollees do not choose their PCP, but are assigned to one by an administrative clerk in their primary care clinic ("facility") of choice, leading to as-good-as-random assignment within-facility. We measure opioid prescribing type using a leave-out, residualized measure of a patient's PCP's opioid prescribing rate across all other new patients at their index visit, relative to the mean across all other PCPs at the same facility. Since VHA care eligibility is fixed for each veteran, this allows us to follow a diverse patient population over long periods of time without worrying about attrition such as through employment changes.

We find substantial variation in prescribing tendency across PCPs, with providers at the 90th percentile of the distribution prescribing opioids to patients at first visit at a 2.54 percentage point (pp) higher rate compared to PCPs at the 10th percentile, on average, within the same facility (the standard deviation is 1.85pp and inter-quartile range is 1.52pp). The prescribing tendency measure also strongly predicts receipt of a prescription at the index visit: Being assigned to a PCP at the 90th percentile of propensity, as opposed to one at the 10th percentile, increases the likelihood of prescription receipt by 1.21pp (65%).

With quasi-random assignment and our prescribing tendency measure in hand, we establish five main results. First, a patient's long-term prescription opioid use is to a substantial extent determined by their primary care provider: assignment to a provider at the 90th percentile

(relative to one at the 10th percentile) increases long-term prescription opioid use—defined as filling at least 180 days supply of prescription opioids in the first year following initial assignment—by 20% (or 0.43pp). This estimate is an order of magnitude larger than the equivalent ones found in prior research in emergency settings, likely because primary care relationships involve repeated interactions over a long period of time. On average, ca. 38% of all opioids filled in the first year are from the veteran's assigned PCP; this fraction increases by 9pp for every 2.54pp increase in our prescribing tendency measure.

Second, assignment to higher prescribing PCPs increases the likelihood of developing an opioid use disorder (OUD; medically defined as a problematic pattern of opioid use leading to clinically significant impairment or distress), but within-facility variation in PCP's prescribing behavior is unlikely to be the main driver behind the surge in cases of OUD in recent decades. Assignment to a 90th percentile, as opposed to a 10th percentile, prescribing provider-type, leads to a 0.035pp increase in developing an OUD within three years of the first visit, which marks a 4% increase relative to the mean. Combined with our estimate on long-term use, it suggests that about 8% of patients who become long-term users because of their lenient PCP subsequently develop an OUD. Furthermore, we find a noisy null effect on opioid overdose mortality.

Third, primary care providers identified as high prescribers of opioids influence veterans' mental health outcomes at large. Moving from a provider at the 10th percentile to one at the 90th percentile of the prescribing distribution increases the likelihood of a depression diagnosis by 0.31pp (1.3% relative to the mean). We also find suggestive evidence of an increase in suicide attempts. Overall mortality is unaffected, suggesting that these effects are not simply driven by differences in overall provider quality of care.

Fourth, we shed light on the mechanisms driving our results; what behavior on the side of the high-prescribing PCP is causing the adverse outcomes we observe for patients? Is it higher opioid prescribing alone? We find that lenient opioid prescribers are prescribing prescription opioids *in place of* alternate non-opioid therapy such as referrals to pain clinics and complementary integrative health clinics (e.g., chiropractic medicine, acupuncture,

massage therapy, etc.) that the VHA has been encouraging in recent years. High prescribers are also less likely to adhere to clinical recommendations set forth by the VHA to prescribe naloxone alongside prescription opioids. These findings are consistent with high prescribers having poorer overall pain management skills, beginning with appropriate prescribing and considering alternative non-opioid treatments, to adhering to new clinical recommendations. It suggests that educational policies should address the entire set of pain management abilities, beyond just targeting prescribing.

Finally, given the important role primary care providers play for patient opioid use, we investigate whether major policies targeting prescription opioids have curbed prescribing by primary care providers across the prescribing propensity distribution. To do so, we exploit the staggered adoption of policies across states in an event study framework. While we find an overall decline in opioid prescribing over the ten year period of our sample, the event studies suggest that, in the VHA, this decline is unlikely to be the result of state-level opioid regulation: of the six major state-level policies enacted to date (such as prescription drug monitoring programs, prescribing limits, pill mill laws, etc.), none are associated with a statistically significant decline in prescribing among either above- or below-median prescribers in the VHA.

This paper contributes to the literature on the causes of the opioid epidemic, with a focus on the medical supply side. Its primary contribution is an evaluation of the role of primary care in causing opioid dependence. Prior research has focused on emergency departments (Barnett et al., 2017, 2019; Eichmeyer and Zhang, 2021),[1] a setting where short-term, non-refillable opioid prescriptions are commonly prescribed for acute pain. A distinct feature of primary care relative to emergency care is the repeated nature of interactions between patient-provider pairs, which may amplify the importance of provider prescribing behavior for patient outcomes. Furthermore, given the prevalence of primary care and sheer volume of opioid prescriptions originating from this setting, a careful analysis of the consequences of

[1]See Maclean et al. (2020) for a detailed review of the opioid-related literature. Relative to the above-mentioned papers, we also study a larger set of outcomes beyond opioid use disorder diagnoses and mortality, including mental health more generally.

primary care provider behavior in the domain of opioids is of great policy importance.[2,3]

This paper also contributes to the broader literature on the impact of various dimensions of primary care on patient outcomes. This includes research on how primary care access impacts patient outcomes (Martin et al., 2008; Dolton and Pathani, 2016) and on provider education and prescribing behavior (Schnell and Currie, 2018). There is a recent literature showing the influence of PCPs on patients' healthcare utilization and spending by studying PCP moves and retirements (Fadlon and Van Parys 2020, Sabety (2020); Sabety et al. (2021); Schwab (2021); Staiger (2022)). Currie and Zhang (2021) construct a measure of provider effectiveness in reducing avoidable emergency department visits and hospitalizations, and then correlate it with provider behaviors. We contribute to this literature a focus on a parsimonious and policy-relevant provider behavior metric, in a setting with alleviated patient-provider selection concerns.

It is important to note that this paper studies the impact of prescribing propensities that are driven by prescribing decisions requiring clinical judgment rather than through specific VHA policies or differences in adherence to clinical practice guidelines. Opioid prescribing and care delivered at the VA, and studied here, were within clinical guidelines during this period. Furthermore, it is not substandard care but rather practice variance *within practice norms* that provides us with the variation for our research design and results in the outcomes presented in this paper.

The remainder of the paper is structured as follows. The next section describes the institutional setting and data sources. Section 3 details our empirical strategy, including the construction of our measure of the propensity to prescribe opioids. We present the main

[2]Using a movers design methodology and Danish population data, Laird and Nielsen (2016) also study the role of primary care prescribing tendency for opioid-related outcomes. They find that a move-induced switch to a PCP who prescribes at a 10 percentage point higher rate is associated with a 4.5 percentage point increase in a patient's propensity to fill a prescription for opioids in the year following the move, as well as a drop in labor earnings and labor supply.

[3]Other related papers studying supply-side factors influencing opioid use and abuse are Finkelstein et al. (2021) (local opioid abuse rates), Meinhofer (2018); Buchmueller and Carey (2018); Borgschulte et al. (2018); Kaestner and Ziedan (2019); Grecu et al. (2019); Alpert et al. (2020) (prescription drug monitoring programs), Borgschulte and Zejcirovic (2020) (pharmaceutical promotion), opioid prescribing limits Sacks et al. (2021), and naloxone access laws Doleac and Mukherjee (2021); Rees et al. (2019).

results and address potential threats to identification in Section 4. Sections 5 and 6 explore mechanisms and investigate how key reforms to opioid prescribing regulation moderate prescribing behavior, respectively. The last section concludes.

2. Setting, Data, and Sample

2.1 Institutional Details

In the VHA, newly enrolled veterans are assigned to primary care teams based on geographic location, scheduling availability, and panel capacity. Each primary care team consists of a primary care provider (PCP), a nurse care manager, a clinical associate, and an administrative clerk. The PCP can be a physician, a physician assistant, or advanced registered nurse (i.e., nurse practitioner); all of whom have full diagnosing and prescribing authority in the VHA. Since each team consists of only one PCP who can prescribe and diagnose, we use the term PCP throughout this paper to refer to the primary care team.

Assignment to PCPs for new enrollees begin with enrollment in VHA health benefits via Form 1010-EZ (see Figure A.1).[4] A potential new enrollee lists their demographic information, military history, preferred VHA clinic, and whether they would like to be contacted by the VA to set up their first appointment. If the veteran answers yes to this last question, then an administrator contacts the veteran to schedule an appointment based on the veteran's preferred clinic, desired appointment date, and each PCP's availability. This appointment determines the veteran's first assigned PCP.

This patient-provider match process—which quasi-randomly assigns new enrollees in a given year seeking primary care appointments in the same clinic around the same time to different PCPs—is in contrast to primary care outside of the VHA, where patients may

[4] A veteran is eligible for VHA care in two ways. First, veterans who enlisted after September 7, 1980 and served 24 continuous months or the full active duty period with a non-dishonorable discharge. Or second, fit in any of the following categories: have a service-connected disability, receive VA pension, served in Vietnam, Southwest Asia during the Gulf War era, Camp Lejeune, qualify for Medicaid benefits, were a prisoner of war, or received a Purple Heart or a Medal of Honor.

search for a provider, often researching providers ahead of time (Harris, 2003).[5] Under the VHA assignment process, the new patient has little influence over their initial PCP assignment; however, patients can switch providers. Therefore, to minimize selection bias due to patient sorting to PCPs based on unobserved health characteristics, we focus on new enrollees' first PCP assigned in an intent-to-treat framework. We empirically probe the quasi-random assignment assumption in subsection 3.3, and provide additional robustness checks in subsection 4.3.

2.2 Data Sources

Our empirical analysis uses several sources of health data for US veterans. The main source is electronic health record data from the VHA Corporate Data Warehouse (CDW). This includes medical (outpatient and inpatient) and prescription records with standard clinic, patient, provider, and prescriber identifiers, diagnosis and procedure codes, and time of visit. With electronic health records, we also observe provider orders such as lab tests, test results, and referrals to specialists. On the enrollment side, we have data on VHA benefit enrollment forms (Form 1010-EZ), whether they indicate they want to set up their first appointment, and the desired appointment date they give the administrative scheduler. We also have historical records documenting each PCP assignment, along with the start and end of the PCP-patient relationship.

We supplement the CDW data with VA/CMS data: Medicare claims from 2011-2019 and Medicaid claims from 2011-2014 for all veterans. We observe medical claims for both Medicare (Part A and B) and Medicaid, along with prescription claims for patients enrolled in any Medicare Part D plan, and Medicaid prescription claims. While we do not observe any healthcare paid by non-VHA, non-CMS payers, we plausibly have a more complete view of health for veterans who receive primary care in the VHA.

Our final data source comes from VHA Vital Status files and CDC National Death Index

[5]This is one reason why there is a recent literature developing using PCP (Fadlon and Van Parys 2020, Sabety (2020); Sabety et al. (2021); Schwab (2021); Staiger (2022)), which are plausibly exogenous to patient characteristics.

(NDI) Plus files, which provide us with both the date and cause of death for each veteran until the end of 2018.

2.3 Sample and Outcome Construction

Sample construction

We follow Currie and Zhang (2021) and construct a sample of veterans newly enrolled in primary care services with the VHA. Specifically, we focus on male veterans between the ages of 20 and 90 between 2005 and 2017.[6]

We begin with approximately one million 1010-EZ forms for new enrollees seeking primary care appointments. After restricting attention to clinics with at least two PCP per year and PCPs with at least 50 new patients in our study period, we arrive at 743,056 veterans. Finally, we drop veterans with opioid prescriptions in the prior year and thus focus on those who are less likely to shop for providers based on the likelihood of receiving opioids. While these are new enrollees, they may have received healthcare from the VHA previously without being enrolled (13%). We also observe their Medicare and Medicaid prescriptions. Our final sample consists of 656,155 veterans who are assigned to 4,941 PCPs in 721 clinics.

Outcome construction

Our outcome measures fall into three categories: i) opioid-related outcomes; ii) healthcare utilization; and iii) mental health and mortality. We measure them within a 1-3 year horizon after the index visit, and not more long term, in order to maximize power and reduce attrition. All outcomes exclude the initial primary care visit. Using all VHA and CMS prescription records, we construct a measure of long-term opioid use (*Long-Term Use*) following the medical literature (Barnett et al., 2017, 2019; Jena et al., 2016; Dunn et al., 2010; Braden et al., 2010) as an indicator for observing at least 180 days supply of prescription opioids in

[6]Female veterans are excluded because they are often assigned to specific women's health teams. There is often only one such team in a clinic, which makes it impossible to conduct our within-clinic randomization and analyses. Female veterans make up 9% of the new VHA enrollee population.

the 365 days following the index primary care visit. We also study the log of one plus total milligrams of morphine equivalent (*log MME*) to account for the potency of the consumed opioids. Our measures of adverse opioid-related health outcomes are the following: we include an indicator for having a poisoning or accident event (*Poisonings and Accidents*) as a proxy for drug-induced impulsivity and/or sedation (in turn predictive of opioid overdose risk), an indicator for *Opioid Use Disorder* (both from diagnosis codes), and *Overdose Mortality*, an indicator for death from drug overdose. The latter four variables are measured within three years of the initial primary care visit.

Our main measure of healthcare utilization, *Log Total Cost*, is the total cost of all medical encounters for each veteran, which the VHA constructs by distributing average VHA-level cost to individual encounters (Wagner et al., 2003). We further distinguish between outpatient and inpatient spending (*Log Outpat Cost* and *Log Inpat Cost*), along with the number of primary care, mental health, and emergency department (ED) encounter days over the first three years of the initial primary care visit (*PCP Visits, MH Visits, ED Visits*, respectively) .

Health outcomes that we study include indicators for diagnosis of major depressive disorder (*Depression*), for *Suicide Attempts*, and for mortality (*3-Year Mortality*), all within three years. We also construct alternate measures of each health outcome excluding diagnoses by their assigned PCP to deal with endogenous diagnosing.[7]

We also observe prior year medical utilization for all veterans who are treated at VHA facilities without being enrolled in benefits and those who are enrolled in Medicare or Medicaid. Using these data, we construct prior year measures of each outcome (e.g., whether the veteran had an opioid overdose in CMS or VA records in the prior year) and Elixhauser comorbidity index.

[7]We find that the vast majority of diagnoses that enter into our outcome measures are not diagnosed by the patient's PCP. For example, excluding any diagnoses from their assigned PCP, three year opioid use disorder rate drops from 0.91% to 0.88%.

Summary statistics

Table 1 describes relevant summary statistics for our final baseline analysis sample. The average age of individuals in our sample is 54.9; 74% are non-Hispanic White and 13% are Black veterans. The average annual income in 2019 dollars is $44,477 and a third of the sample is also on Medicare or Medicaid. Each veteran has 6.9 visits with their assigned PCP over the first three years. 1.8% are prescribed a prescription opioid at their first primary care appointment and conditional on being prescribed, the median prescription is a 30-day supply of 15 mg of morphine equivalent per day. 7.2% had an VHA emergency department encounter or inpatient admission in the year prior to enrollment.

3. Empirical Strategy

We investigate the causal effect of being assigned to a PCP of a given type—summarized by their propensity to prescribe opioids—on patient outcomes. Therefore, any differences in outcomes we estimate among patients assigned to lenient vs. strict prescribers may be due to differences in prescription opioid exposure, or due to differences in *other* dimensions of care that correlate with prescribing tendency, or due to a combination of the two. Our baseline empirical strategy (and hence, our main results) remains agnostic as to the mechanisms, which we investigate in more detail in Section 5.

3.1 Measuring Provider Propensity to Prescribe

A primary care provider's tendency to prescribe opioids is measured relative to their clinic mean and net of a set of observable patient characteristics. We construct this measure following a leave-out (jackknife) residualized approach as in Kling (2006); Dobbie et al. (2017); Bhuller et al. (2019); Eichmeyer and Zhang (2021) as follows. For each veteran in our sample, we observe their first PCP visit and whether an opioid was prescribed at that encounter: $Prescribed_i$. Next, we regress, at the veteran (initial) encounter-level, $Prescribed_i$ on a set of fixed effects and predetermined veteran covariates:

$$Prescribed_i = \alpha_{clinic} + \alpha_{year \times month} + \alpha_{dayofweek} + \alpha_{desired} + \theta X_i + \mu_i \quad (1)$$

where α_{clinic}, $\alpha_{year \times month}$, $\alpha_{dayofweek}$ and $\alpha_{desired}$ are fixed effects for clinic, calendar year-month, day of week, and bins for the number of days between the veteran's desired first appointment date and actual appointment date. The choice of these fixed effects reflects the fact that new enrollees cohorts who attempt to schedule appointments at the same time in the same clinic are quasi-randomly assigned to PCPs. These are the only controls required for an unbiased estimate of each PCP's propensity to prescribe opioids; however, for statistical power we include baseline controls, X_i: race, five-year age bins, marital status, enrollment priority groups,[8] Medicaid or Medicare beneficiary indicators, exposure to radiation/Agent Orange during service, major diagnosis category of primary diagnosis at first visit, prior year health history (inpatient, ED utilization, homelessness, depression, suicide attempt, opioid use disorder, accidental falls), and prior year Elixhauser comorbidity index based on prior year encounters. These controls are *not* included when we assess our identifying assumption in the next section.

We then construct the prescribing propensity for each patient i in our sample as the average residual over all other patients in the sample seen by patient i's PCP j, excluding patient i's μ in the average:

$$Propensity_i = \frac{1}{N_{-i,j}} \sum_{i' \in \{\mathbb{J} \setminus i\}} \hat{\mu}_{i'} \quad (2)$$

By leaving out the patient's own prescription outcome from his or her PCP's propensity measure, we eliminate the mechanical bias that stems from patient i's own case entering into the prescribing tendency measure. The residualization approach reduces the variation in opioid prescribing to the part that is not due to observable patient characteristics or patient sorting into clinics, thereby reducing selection bias. The resulting PCP propensity measure can be interpreted as the average (leave-out) new patient opioid prescription rate of patient

[8] VHA benefit enrollees are classified into groups based on military history, disability status, and income.

i's provider, relative to other providers in the same clinic and year, controlling for seasonality and veteran characteristics.

3.2 Main Empirical Specification

Our preferred specification to estimate the impact of provider type, given by the propensity to prescribe opioids, on patient outcomes is a simple linear OLS model with the propensity variable as the key right-hand side variable of interest. We also consider an alternative semi-parametric model with dummies of provider propensity percentiles/deciles/quartiles as the right-hand side variables of interest. The key advantages of the linear model are its parsimony (summarizing the effect in a single coefficient of interest) and its increased statistical power; we report the non-parametric model estimates in the robustness section.

We estimate the following regression model:

$$Y_i = \beta Propensity_i + \alpha_{clinic} + \alpha_{year \times month} + \alpha_{dayofweek} + \alpha_{desired} + \theta X_i + \epsilon_i \tag{3}$$

where Y_i is the outcome of interest for patient i (e.g., three-year mortality), and the remaining controls are the same as in Equation 1.

The parameter of interest is β, which represents the change in outcome Y_i when a veteran's PCP "type", given by $Propensity_i$, increases by one (i.e. 100pp). Because propensity values as high as 1 are very rare in the data, and thus do not provide an empirically relevant treatment effect size, we scale the estimate by the average prescribing difference between very lenient and very strict prescribers observed in the data. That is, we use a the scaling factor the average within-facility difference between a 90th percentile and a 10th percentile prescribing PCP, which is 2.54pp (i.e. we multiply $\hat{\beta}$ by 0.0254), and the multiply again by 100 for readability. Therefore, the scaled parameter estimate represents the causal effect of being assigned a PCP-type who prescribes at a 2.54pp higher rate to patients at their index visit (roughly the difference between 90th and 10th percentile PCPs) on the outcome of interest Y_i in percentage points.

Under the identifying assumption that providers are assigned to patients quasi-randomly (conditional on a set of fixed effects), then β can be interpreted as the causal. We empirically probe the quasi-random assignment assumption through a check for balance on observables in subsection 3.3, and provide additional robustness checks in subsection 4.3.

3.3 Assessing the Identifying Assumption

We conduct a test of balance on observables as a check for violation of quasi-random assignment. That is, do lenient and strict providers treat the same case mix of patients conditional on clinic, calendar year-month, day of week, and difference between actual and desired appointment dates? Figure 1 tests this assumption graphically. Panel A represents the impact of provider propensity on the likelihood of being prescribed an opioid at the index visit. Specifically, it plots a histogram of the propensity measure $Propensity_i$ along the x-axis and the left y-axis. A local-linear regression of the fitted probability of being prescribed opioids on provider propensity after residualizing is overlaid and displayed on the right y-axis. The histogram displays substantial variation in the prescription rate among primary care providers working in the same facility, treating similar patients (in terms of observable demographic characteristics) and identical diagnoses. The standard deviation of $Propensity_i$ is 1.42pp.[9] The first column in Table 2 presents the regression table analog of the local linear graph. The association between first encounter opioid prescription receipt and provider propensity is strong. Being assigned to a 2.54 percentage point more lenient PCP opioid prescriber is associated with a 1.21 percentage point increase in the likelihood of being prescribed an opioid at first encounter with the PCP.

Panel B of Figure 1 overlays a local linear regression of prescribed opioid prescription status on 100 bins of $Propensity$ where opioid prescription status is predicted using a comprehensive set of demographic, military, and medical history variables (see Figure A.2 for a complete list). Predicted opioid prescription is not correlated with provider propensity (the coefficient

[9] This is roughly in line with Laird and Nielsen (2016) who find a standard deviation of 1.8pp for physicians in Denmark. They use all primary care visits, whereas we use only first visits.

on the slope parameter is not economically or statistically significant). Figure A.2 further checks for violations of quasi-random assignment by regressing $Prescribed_i$ and $Propensity_i$ on a comprehensive set of patient observables in the left and right panels. While patient observables predict opioid prescription status, they do not jointly predict provider propensity to prescribe opioids. Therefore, we do not find evidence of any violations of quasi-random assignment on patient observables. Furthermore, since we focus on opioid-naïve patients, we further reduce any likelihood of non-random sorting based on prior opioid use.

Finally, in our robustness Section 4.3, we report additional checks supporting the assumption of quasi-random assignment. They include a placebo check that finds no association between provider prescribing propensity and a patient's outcomes in the year *prior* to assignment, as well as results showing that effects increase in the number of interactions between a patient and their PCP.

4. Results

In the next two subsections, we report our causal effect estimates of assignment to PCPs who vary in their opioid prescribing tendency, based on Equation 3. We scale estimates such that they correspond to the average causal effect, in percentage points, of a 2.54pp increase in one's PCP's prescribing tendency, corresponding to the average difference in prescribing tendency between the 90th and 10th percentile of the within-facility prescribing tendency distribution.

4.1 Opioid Outcomes

Table 2 presents our estimates of the impact of assignment to a higher-intensity opioid prescribing PCP on long term-opioid use, dependence, and overdose mortality.

Opioid Use

We find large effects on prescription opioid use, with assignment to a higher prescriber—that is, someone who prescribes at a 2.54pp higher rate at the index visit—increasing the likelihood of becoming a long-term prescription opioid user by 0.43pp (column 2). Such assignment also increases the total *amount* of prescription opioids consumed by 15.5% over three years (column 3).

The magnitude of the effect on long-term use marks a *20% increase* relative to the mean, highlighting that a veteran's long-term opioid use is to a substantial extent determined by their primary care provider. It is an order of magnitude larger than the equivalent estimate found for the case of emergency department physicians in Eichmeyer and Zhang (2021).[10] This is despite the fact that the sample used in this study is likely to be on average healthier than the ED sample in Eichmeyer and Zhang (2021). The much larger impact of primary care providers is explained by one's PCP assignment being more consequential than one's ED physician assignment, due to the repeat nature of interactions: the average veteran-PCP relationship in our sample spans 15 encounters, while patients do not have repeat visits with the ED physician encountered at their index ED visit.

To investigate the source of the increased prescription opioid use, we obtain estimates of the fraction of prescription opioids obtained from one's PCP in Table 3. On average, for veterans who have some opioid use in the first year after initial PCP assignment, 37-38% of their opioid supply— measured either as number of prescriptions, or as total days of pills supplied, or as MME—are from their assigned PCP, highlighting the major role of PCPs as suppliers of prescription opioids.[11] The share increases by 9pp when assigned to a PCP who prescribes at a 2.54pp higher rate. Thus, the increased prescription opioid use arising from assignment to a high-intensity prescribing PCP-type is directly due to more prescriptions originating from that PCP.

[10]The equivalent reduced form estimate in Eichmeyer and Zhang (2021) for a 2.54pp increase in an ED physician's tendency to prescribe at the index ED visit is a 0.05pp increase in long-term use. Moving from the bottom decile to the top decile of ED physician leniency (a much larger jump of 11.6pp) leads to a 0.24pp increase in long-term use, a 4.1% increase relative to the mean.

[11]This proportion is broadly in line with national estimates from Levy et al. (2015).

Opioid Use Disorder and Overdose Mortality

Having established the important role primary care providers play for patients' prescription opioid consumption, we next turn to adverse consequences of intense use: opioid use disorder, poisonings and accidents as proxies for drug-induced impulsivity and sedation, and opioid overdose mortality. We report regression estimates in columns 4-6 of Table 2, and establish three results.

First, while assignment to a higher prescribing PCP-type does significantly increase the likelihood of an OUD diagnosis down the line, within-facility variation in PCP's prescribing behavior is unlikely to be the main driver behind the surge in cases of OUD observed in recent decades. Specifically, we find that assignment to a higher prescribing PCP increases the three-year diagnosis rate of an OUD by 0.035pp on a base of 0.91%—a 4% increase relative to the mean.[12] This estimate is marginally significant at the 10% level, and we can rule out an effect size larger than 0.070pp (or 7.5% relative to the mean) with 90% confidence. Similarly, we find statistically significant, but economically small positive effects on poisonings and accidents—indicators of drug-induced impulsivity and/or sedation that are predictive of opioid overdose risk (column 5).

Second, a back-of-the-envelope calculation suggests that a minority—about 8%—of veterans who become long-term prescription opioid users because of their PCP further develop an OUD (that they would not have developed with a stricter PCP). It is based on the following simple calculation: the effect size for OUD is 8% of the one we estimate for long-term opioid use. Thus, under the assumption that the impact on OUD is experienced by those induced to become long-term users, the above-mentioned estimate obtains.[13]

Third, we do not detect an effect on opioid overdose mortality. The event is very rare in

[12]This estimate is about 2.3 times the size compared to the equivalent one found in the ED in Eichmeyer and Zhang (2021), given by the reduced form effect of a 2.54pp increase in one's ED physician's tendency to prescribe opioids.

[13]Of note, the equivalent estimate for the ED setting studied in Eichmeyer and Zhang (2021) is much higher, at 28%. This difference is likely due to important differences in the two samples: the average ED visitor may be in worse mental health, and thus more vulnerable to substance use disorders, compared to the average primary care patient, and thus may be more likely to develop OUD after exposure to prescription opioids.

the data (we observe a total of 266 such cases), which gives rise to more noisy estimates. We can rule out an effect size smaller than -0.010pp and larger than 0.006pp with 90% confidence, on a base of 0.046%.

Together, these results suggest that while PCPs have an important influence on prescription opioid use of their patients, the majority of patients who consume higher amounts of opioids because of lenient PCPs does not experience severe adverse opioid-related health events as a consequence of the higher consumption.

4.2 Health Care Utilization, Mental Health, and Mortality

Health Care Utilization

Our results for health care utilization (presented in Table 4) suggest that more leniently prescribing PCP types do not put patients on fundamentally different care trajectories relative to those types who are stricter prescribers, on average. Column 1 indicates that assignment to a higher prescribing PCP leads to a statistically significant, but economically small, 2.7% increase in total costs, driven entirely by outpatient care (column 2).[14] Columns 4-6 show a clear null effect of PCP opioid prescribing type on the number of visits to other major types of care (primary care, mental healthcare specialists, and emergency departments, respectively).

Mental Health and Mortality

Recent advances in the neuroscience of pain, addiction and mood disorders highlight an important role of the endogenous opioid system—that is, a system of neurons that produce opioids, and a network of opioid receptors—for pain *and* mood regulation (Peciña et al., 2019; Wilson and Junor, 2008). Consequently, it has been hypothesized that opioid consumption may affect mood disorders, such as depression (Rosoff et al., 2021). It could thus be possible that prescription opioid exposure via one's PCP has downstream consequences for mood disorders. In this section, we provide suggestive evidence for such a link by analyzing how

[14]Note that since the outcome is in log points, the impact on total cost does not mechanically need to be between outpatient and inpatient costs.

PCP opioid prescribing type affects key markers of mental health, as well as mortality. The results are reported in Table 5.

Consistent with the above-mentioned link, we find a statistically significant effect of 0.31pp (or 1.3% relative to the mean) on depression diagnoses (column 1).[15] Given the effect size for long-term prescription opioid use (0.43pp), this effect is sizable. Assuming that the causal effect on major depressive disorder diagnosis only obtains among individuals induced to become long-term prescription opioid users by their lenient PCP, it suggests that 73% of those induced to become long-term prescription opioid users due to their lenient PCP also receive a depression diagnosis that they would have otherwise not received.[16] We also find a small positive, but more noisy effect estimate for suicide attempts (column 2). When investigating the effect of prescribing propensity non-parametrically through the estimation of prescribing propensity quartile fixed effects, we find that the effect is driven by the most lenient prescribers (see Table A.1, column 6).

These results are unlikely to be driven by selection of more likely depressed patients to more lenient PCPs,[17] by endogenous diagnosing on the side of the PCP and/or differential referral patterns across lenient and strict providers leading to differential probabilities to get diagnosed with major depressive disorder,[18] or by differences in overall quality of care between lenient and strict PCPs. Regarding the latter: if the tendency to prescribe opioids is strongly correlated with low provider quality of care overall, we would expect a positive impact on mortality. However, we do not detect a statistically significant effect of PCP prescriber type on mortality (Table 5, column 3). With a 95% confidence interval, the three-year overall mortality effect from going from a 10th percentile to 90th percentile prescriber is no more

[15] As before, we scale effects sizes to represent the difference between a 90th percentile and a 10th percentile prescribing type.

[16] The estimate obtains by dividing the effect estimate on depression by that on long-term use, 0.31/0.43.

[17] Neither suicide attempt nor depression diagnosis in the year prior to assignment had any predictive power with respect to provider propensity (in fact, prior year suicide was negatively correlated with propensity). In addition, a composite measure of prior year mental health including depression, suicide, and other mental health diagnoses such as bipolar disorders, is included as a baseline control.

[18] We show robustness to omitting all diagnoses originating from the patient's assigned PCP from the construction of the mental health outcomes in Table A.2, and to controlling for a PCP's tendency to refer to mental healthcare in Table A.3.

than 0.097pp, corresponding to 1.9% relative to the mean. Thus, we can rule out that within-facility variation in PCP-types—as measured by opioid prescribing tendency—have a large impact on veteran mortality in the short- to medium-term.

To the best of our knowledge, this is one of the first papers to establish a causal effect between primary care provider and patient mental health. The link between lenient prescriber type and poorer mental health could work directly through opioid use and abuse (as alluded to earlier), including worse treatment for substance abuse, or worse treatment of mood disorders. The latter mechanism would have to involve lenient PCPs' worse ability to recognize *who* requires mental healthcare, since we find our results robust to controlling for a PCP's tendency to refer to mental healthcare, *overall*.

4.3 Robustness

Our results are robust to alternative specifications probing the key assumption of quasi-random assignment, as well as alternative sample selection criteria, and alternative ways of constructing prescribing propensity and outcomes. We describe each robustness check in detail in Appendix B, and provide a brief overview here.

First, we probe the assumption of quasi-random assignment of patients to PCPs within facilities. We conduct a placebo check that finds no association between PCP prescribing propensity and a patient's outcomes *in the year before* PCP assignment (Table A.4). We also show that effects increase in the number of interactions between a patient and their PCP (Table A.5), consistent with such interactions driving results, as opposed to selection in the assignment process. Second, we probe our sample selection criterion by restricting the sample to those patients who stay with their initially assigned PCP for at least one year (Table A.6). Third, we show robustness to endogenous diagnosing by restricting our diagnosis code-based outcome measures to diagnosis codes originating from encounters with clinicians *other than* the patient's assigned PCP (Table A.2). Finally, we construct a time-varying prescribing propensity measure using Empirical Bayes shrinkage, finding results closely in line with those from our baseline specification (Table A.7).

5. Investigating Mechanisms

The results presented so far identify the causal effect of a lenient prescriber, but *not* necessarily, or solely, the causal effect of being prescribed opioids. This distinction is key because lenient opioid prescribers may differ along other dimensions that may influence patient outcomes. In other words, if providers were identical along all dimensions and treatment practices except for their opioid prescribing tendency, then our findings are driven entirely by the sum of all prescription opioids prescribed by that particular PCP. However, to the extent that provider practices do differ, other mechanism may be at play.

Prescriber Characteristics We begin by investigating demographic correlates of prescribing behavior. For each PCP, we obtain a single prescribing propensity by averaging across the residuals in Equation 1 without the leave-out step; then we categorize providers as lenient or strict if their propensity measure falls in the top or bottom quartile of their facility, respectively. Correlating this measure with provider characteristics in Table A.8, we document that lenient prescribers are more likely to be male than strict providers (55% versus 46%), while being similar in age. Consistent with findings among Medicare beneficiaries (Muench et al., 2019), lenient prescribers are slightly more likely to be physicians as opposed to physician assistants or nurse practitioners. There are only marginal differences in full-time status and number of patients seen per day between lenient and strict PCPs. In all, demographic characteristics of lenient and strict prescribers are remarkably similar, suggesting that variation in prescribing behavior mainly originates from differences in tastes and/or beliefs *within* demographic groups, as opposed to *across* those groups.

Prescriber Behavior To shed light on a fuller set of mechanisms—besides prescription opioid supply—that may drive the effects we documented for opioid-related outcomes, we study provider behavior in the domain of pain management, risk mitigation and treatment for opioid use disorder. In particular, we study whether high opioid-prescribing PCPs are more or less likely to adhere to VHA guidelines created in 2014 aimed at increasing non-opioid

therapy treatment by referring to pain clinics and community integrative health clinics (e.g., acupuncture, massage therapy), and to implement harm reduction strategies by co-prescribing naloxone with opioid prescriptions.

For each PCP with at least one opioid prescription between 2014 and 2018, we construct a measure of the fraction of their opioid patients who also get referred to non-opioid therapy options and the ratio of their number of naloxone prescriptions to the number of opioid prescriptions. We then estimate whether PCP opioid prescribing propensity is correlated with the two measures in Table 6. PCPs at the 90th percentile of opioid prescribing propensity are 4.0 percentage points less likely to refer their opioid patients to alternate non-opioid therapies on a base of 14.8%. They also prescribe 0.09 fewer naloxone prescriptions (17%) per 100 opioid prescriptions. Thus, these findings suggest that lenient opioid prescribers are less likely to adhere to (newer) VA opioid guidelines. Another important skill among clinicians prescribing opioids is the ability to recognize opioid use disorders and refer them to treatment. In column 3 we check whether high-prescribing PCPs are more or less likely to prescribe medication for opioid use disorder (e.g., buprenorphine, suboxone) or refer their patients to substance use disorder treatment. Despite the fact that high prescribers prescribe more opioids that lead to higher levels of long-term use and a higher likelihood of their patients developing opioid use disorders, we find they are not more likely to start or refer their patients for treatment.

Taken together, our findings suggest that high opioid prescribers are likely worse at overall pain management and opioid-related risk-mitigation. Therefore, increased risk of opioid use disorder and adverse opioid-related outcomes among patients of lenient PCPs may be explained not only by higher prescription opioid consumption, but also by worse pain management and opioid-related oversight.

6. Can Policies Mitigate Lenient Prescribing?

Having established the important role primary care providers play for patients' opioid use, including a small but significant effect on adverse opioid-related events (development of OUD, as well as poisonings and accidents), it is important to investigate the effectiveness of existing policies in curbing lenient prescribing.[19] Our long panel covering multiple "waves" of the crisis and policy changes is particularly suitable for this investigation.

Intuitively, we compare prescribing behavior before vs. after various opioid-related policies were put in place in a given location, distinguishing between the response of lenient and strict prescribers. We consider all major opioid-related regulations enacted over the sample period. Five of those were enacted at the state-level: 1) prescription drug monitoring programs (PDMPs)—we further distinguish between non-mandatory and must-access ones—2) prescribing limits, 3) "Pill mill" laws, 4) "Good Samaritan" laws, and 5) Naloxone distribution policies;[20] and two occurred at the national-level: 6) VHA's Opioid Safety Initiative, and 7) Oxycontin's abuse-deterrent reformulation.[21]

Our empirical strategy follows an event study approach, with provider-level panel data. The outcome of interest is given by the total number of opioid prescriptions over the total number of patients, at the PCP-month level; to keep with the set of quasi-randomly assigned patients, the measure includes only (prescriptions to) patients who were initially assigned to the provider in the previous three years. We restrict the sample to the subset of PCPs with

[19] Our main contribution relative to existing studies on this topic (for reviews, see Ansari et al., 2020; Maclean et al., 2022) is the ability to identify impacts across the prescribing intensity distribution in a setting with quasi-randomly assigned patients. We thus isolate differences in impacts on prescribing behavior of lenient vs. strict providers treating similar sets of patients. Furthermore, to the best of our knowledge, our study is the first to focus on lenient prescribers *and* compare the effectiveness across all major opioid-related policies in a single setting.

[20] We obtain state-level introduction dates for each policy from Lee et al. (2021), who also provide a detailed description of each type of regulation. In short, PDMPs are electronic databases that allow providers to track opioid prescriptions a patient has received across providers; prescribing limits limit the length and/or strength of opioid prescriptions. Pill mill laws restrict opioid prescription dispensing without medical indication by pain management clinics; Good Samaritan laws protect those calling emergency services because of overdose from arrest for possession of illegal substances; Naloxone distribution policies allow pharmacies to dispense Naloxone, which is used to revers opioid overdoses, without prescription.

[21] Note that although not all the above-mentioned regulations directly target extensive margin opioid prescribing, they may still have indirect impacts, for example through changes in demand.

complete panel data for the years 2009-2015, and further to those facilities with at least two PCPs for whom that holds. We split the resulting analysis sample of PCPs at each facility into a below-median and above-median propensity group (i.e. a "lenient" and "strict" group - we use the median split to maximize sample size), based on the sample's distribution of prescribing propensity in this facility in the year 2009, when no strict opioid regulation was in place.[22] The sample period used for the analysis is restricted to the six years covering 2010-2015 (i.e. excluding the year used to calculate leniency).

We start by looking at the raw time series. Figure A.3 shows the average opioid prescribing rate for each month in the sample period, separately for above- (diamonds) vs. below-median (circles) prescribers; the two national-level changes—Oxycontin reformulation and the VHA's Opioid Safety Initiative—are marked by gray dashed lines. Figure A.4 show the same outcome for each state-level policy, by month relative to the introduction date. For the latter figure, we limit the sample in each panel to those prescribers whose facilities are located in states that enacted a given policy in the sample period. The series represent unadjusted, completely non-parametric "event studies". They allow for a direct, visual inspection of pre-trends, as well as of sharp changes in outcomes around the introduction dates.

The raw series reveal two findings: first, differences in prescribing rates among initially below- and above-median prescribers persist over the six-year period; while there is an initial drop in prescribing among above-median prescribers in 2010, prescribing differences stabilize in the years 2011 to 2015, when above-median prescribers prescribe on average 30-45% more opioid prescriptions per capita than their below-median counterparts. Second, none of the eight policies is associated with a sharp, visually detectable drop in prescribing among either low or high prescribers; similarly, none of the policies is associated with a visually detectable, persistent trend break in prescribing among either group. Instead, we observe gradual pre-trends for many policies, consistent with potential endogeneity in the implementation timing or anticipation effects.

[22]To maximize sample size, prescribing propensity is given by the simple average prescription rate per capita in 2009. We show robustness to using a residualized leniency measure, which reduces sample size due to reliance on five years of data (2005-2009) in Table A.11.

In order to more cleanly isolate causal effects of the policies, and to perform formal statistical tests of treatment effects, we implement an event study model. The model exploits variation in introduction dates across states to control for overall time trends and common shocks. That is, for each state-level policy separately, we regress our outcome of interest at the provider-month-level on a set of relative event time indicators, as well as provider fixed effects, and year-month fixed effects. We also control for the presence of all other state-level opioid-related policies in the regressions.[23] We estimate the model separately for above- and below-median prescribers and for each policy, and report the results in Figure 2.[24] They largely confirm the suggestive findings from the inspection of raw trends. We find that none of the six state-level policies had statistically significant effects on prescribing rates in either prescribing group, when measured in our event study framework; magnitudes of the coefficients are small (less than half of a percentage point), with precision varying across policies. Our findings are in line with existing evidence on the aggregate effects, which is fairly mixed for each policy, with results depending on which state is studied; it points towards the importance of stringency in implementation, as well as the extent of substitution across opioid classes, for whether the policies effectively curb prescription opioid use (for reviews, see Ansari et al., 2020; Maclean et al., 2022).[25]

7. Conclusion

With almost half of all opioid prescriptions originating from primary care providers, it is imperative to empirically assess their role in the opioid epidemic. In this paper, we have documented wide variation in providers' tendency to prescribe opioids, consistent with

[23] Our specification is $y_{it} = \mu_i + \gamma_t + \sum_{s=-12}^{-2} \beta_s \mathbb{1}[r_{it} = s] + \sum_{l=0}^{12} \beta_l \mathbb{1}[r_{it} = l] + X_{it} + \epsilon_{it}$; where y_{it} is the outcome for PCP i in year-month t, r_{it} is month relative to policy introduction, and μ_i and γ_t are provider and date fixed effects. X_{it} controls for ACA-expansion status, as well as dummies for presence of each of the other five state-level opioid-related regulations. For states that did not enact a given policy (and for relative event time periods falling 12 or more months before the implementation date), the relative event time is set to -12. For all periods 12 or more months after introduction, relative event time is set to 12.

[24] The corresponding results in table-form are reported in Table A.9; see Table A.10 for treatment effect estimates averaged over the 12 months post-introduction.

[25] A compounding factor specific to the VHA setting may be that state-level policies may be given less priority than VHA-wide ones.

differences in training, beliefs, or tastes. Furthermore, we found that a patient's prescription opioid consumption is in significant part determined by their PCP's "type", with patients assigned to providers in the most lenient prescribing decile consuming 20% more prescription opioids than those assigned to someone in the least lenient decile. However, despite a statistically significant effect on the likelihood to receive an opioid use disorder diagnosis down the line, assignment to a more lenient provider upon entering care at the Veterans Health Administration can only explain a relatively small share of overall cases of opioid use disorder among veterans. Finally, we find that assignment to a more lenient opioid prescribing PCP significantly increases diagnosis of major depressive disorder, consistent with a potential causal link between opioid use and mood disorders. Investigating into the mechanisms, we find that higher opioid prescribers are less likely to adhere to new opioid clinical recommendations. It suggests that our results are driven by a provider's broad attitude and knowledge towards opioids and pain management. Finally, in our event study estimation, we find no statistically significant effects of any of the major state-level opioid-related regulations on opioid prescribing of lenient prescribers in the VHA.

Taken together, our findings suggests that the primary care setting provides a very suitable and important setting for initiatives aimed at reducing patient exposure to prescription opioids: primary care providers have a very large influence on their patients' prescription opioid use. In combination with the substantial variation in prescribing behavior we documented, our results further support existing evidence pointing to the importance of targeted policies that address provider education (Schnell and Currie, 2018; Bounthavong et al., 2017).

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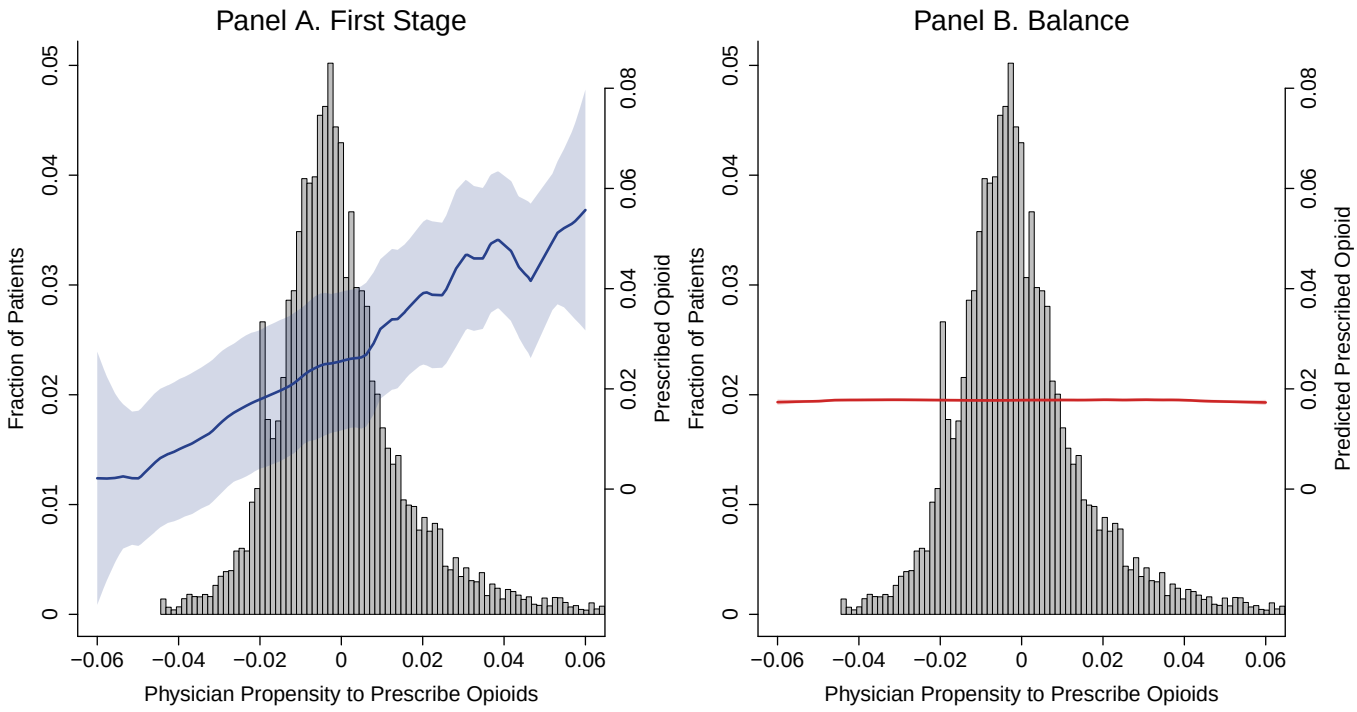
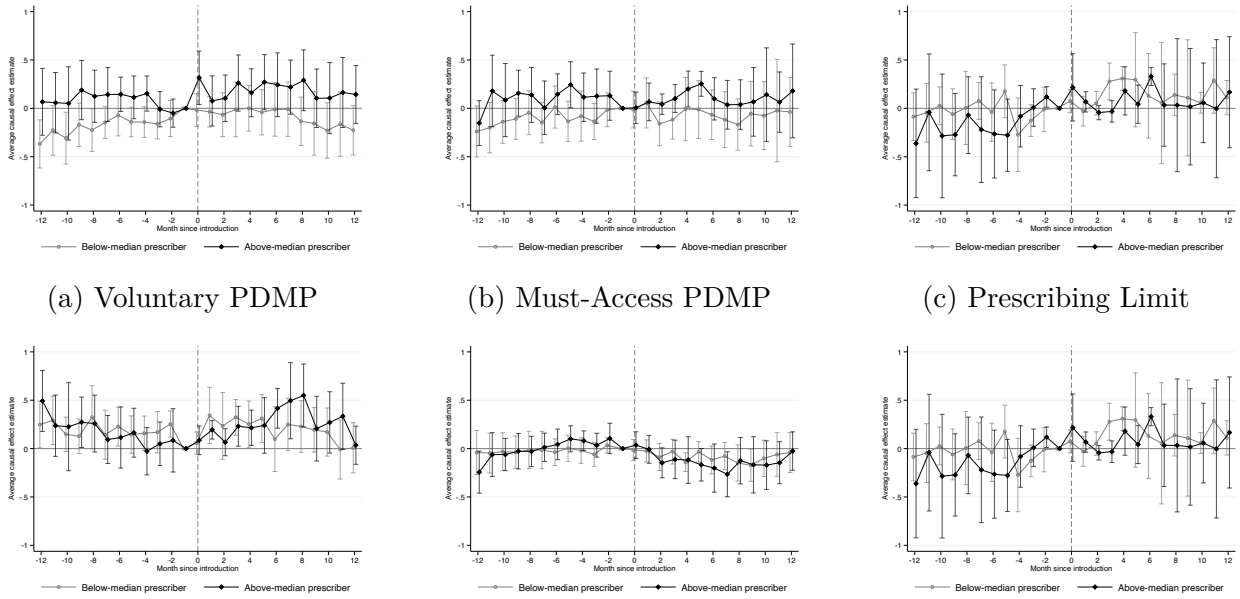
Figures and Tables

Figure 1: First Stage and Balance Test for Quasi-Random Assignment



Notes: This figure plots the distribution of the provider propensity measure and tests for random assignment of providers to patients for our baseline sample. In both panels, a histogram of the provider propensity measure is plotted (left y-axis). Panel A displays the strength of the first stage of how provider propensity impacts likelihood of being prescribed an opioid. We do this by fitting a local linear regression through provider propensity bins and its corresponding mean dependent variable of whether the veteran is prescribed an opioid on their first index visit. Panel B tests for balance by first fitting predicted likelihood of being prescribed an opioid using veteran demographics, military history, and previous medical history (see Figure A.2 for the complete list of covariates), and then fitting a local linear regression using this predicted likelihood of being prescribed. The estimated coefficient (and standard error) of the slope parameter for the two figures are 0.432 (0.026) and 0.00006 (0.00106).

Figure 2: Impact of Opioid-Related Regulation On Prescribing: Event Study Estimates



(a) Voluntary PDMP (b) Must-Access PDMP (c) Prescribing Limit

(d) "Pill Mill" (e) "Good Samaritan" (f) Naloxone Access

Notes: This figure plots the event study results (coefficient estimates and 95% confidence bars) from a regression of the prescriber-month level opioid prescription rate (multiplied by 100 for readability) on relative event time indicators, provider fixed effects, and date fixed effects. The regression includes controls for presence of other opioid-related regulation in the PCP's state in the given month, as well as ACA-expansion status. For states that did not enact a given policy (and for relative event time periods falling 12 or more months before the implementation date), the relative event time is set to -12. Each panel and each series within a panel reports results from a separate regression. Standard errors are clustered at the state-level.

Table 1: Summary Statistics for Baseline Sample

	Q1	Median	Mean	Q3
Asian/Pacific Islander	-	-	0.018	-
Black	-	-	0.131	-
Hispanic	-	-	0.059	-
Native American or Alaska Native	-	-	0.007	-
White Non-Hispanic	-	-	0.738	-
Age	43	59	54.9	66
Currently Married	-	-	0.579	-
Divorced/Separated/Widowed	-	-	0.285	-
Never Married	-	-	0.136	-
Service-Connected Disability?	-	-	0.51	-
Income (2019 dollars)	12,275	33,467	44,477	64,495
Medicaid or Medicare?	-	-	0.338	-
Number of Visits with Assigned PCP (3 Years)	2	5	6.9	9
Prescribed Opioid at First Visit	-	-	0.018	-
Prior Year Medical History:				
Inpatient Admission	-	-	0.072	-
Depression Diagnosis	-	-	0.011	-
Homelessness	-	-	0.001	-
Suicide Attempt	-	-	0.0005	-
Opioid Prescription (Conditional on Prescribed):				
Mg of Morphine Equivalence/Day (2005)	10	15	19.57	20
Mg of Morphine Equivalence/Day (2011)	10	15	19.47	20
Days Supply (2005)	30	30	38.25	30
Days Supply(2011)	30	30	34.38	30
N= 656,155				

Notes: This table displays summary statistics of demographics, prior year medical history, and opioid prescriptions for our baseline sample of veterans newly enrolling in VHA healthcare and requesting primary care appointments between 2005-2017 (as described in the text).

Table 2: Opioid-Related Outcomes

	Dependent variable (Scaled†):					
	Prescribed at Index Visit	Long-Term Use	Log MME	Opioid Use Disorder	Poisonings & Accidents	Overdose Mortality
	(1)	(2)	(3)	(4)	(5)	(6)
Propensity	1.214***	0.426***	15.484***	0.035*	0.049*	−0.002
	(0.066)	(0.055)	(0.893)	(0.020)	(0.026)	(0.005)
Mean (×100)	1.866	2.157	191.11	0.910	1.45	0.046
Observations	649,773	639,341	616,126	616,126	616,126	578,430

†: All coefficients and standard errors are scaled by the average difference between the 90th and 10th percentile lenient opioid prescriber within a facility (2.54pp) and then again by 100.

Notes: This table reports the output from a regression of each opioid-related outcome on provider opioid prescribing propensity based on regression model from Equation 3. Prescribed is an indicator for receiving an opioid prescription at the first index primary care visit with their assigned provider. Long-term use is defined as at least 180 days supply of opioids in the year following a patient's initial primary care visit. Log milligrams of morphine equivalent, and indicators for opioid use disorder, poisonings and accidents, and overdose mortality are calculated based on three-year windows. Poisonings and accidents do not include suicides. Coefficients and standard errors are scaled for interpretability. Mean is the mean of the dependent variable. All regressions include clinic, year-by-month, day of week, bins for days between desired and actual appointment date, race, five-year age bins, marital status, Medicare/Medicaid beneficiary status, prior year medical history, and prior year Elixhauser comorbidity index. Robust standard errors are clustered at the clinic-level. All samples are constrained to be alive during the outcome period. *p<0.1; **p<0.05; ***p<0.01.

Table 3: Share of Prescription Opioids Prescribed by Assigned PCP

	Dependent variable (Scaled†):		
	Share of Total Number of Prescriptions	Share of Total Days Supply	Share of Total MME
	(1)	(2)	(3)
Propensity	8.938***	9.213***	9.186***
	(0.443)	(0.457)	(0.464)
Mean (×100)	36.79	38.21	37.70
Observations	100,169	100,169	100,169

†: All coefficients and standard errors are scaled by the average difference between the 90th and 10th percentile lenient opioid prescriber within a facility (2.54pp) and then again by 100.

Notes: This table reports the output from our main empirical regression model for outcomes relating to share of opioid prescriptions prescribed by the veteran's assigned PCP. All outcome variables are measured in the first year and the sample consists only of veterans who are prescribed at least one opioid prescription in that year. Coefficients and standard errors are scaled for interpretability. Mean is the mean of the dependent variable. All regressions include clinic, year-by-month, day of week, bins for days between desired and actual appointment date, race, five-year age bins, marital status, Medicare/Medicaid beneficiary status, prior year medical history, and prior year Elixhauser comorbidity index. Robust standard errors are clustered at the clinic-level. All samples are constrained to be alive during the outcome period. *p<0.1; **p<0.05; ***p<0.01.

Table 4: Healthcare Utilization

	Dependent variable (Scaled†):						
	Log Total Cost	Log Outpat Cost	Log Inpat Cost	PCP Visits	MH Visits	ED Visits	Relationship Length
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Propensity	2.717***	2.652***	−0.303	−4.045	−1.845	−0.145	71.314
	(0.771)	(0.756)	(0.690)	(5.166)	(2.807)	(0.395)	(397.715)
Mean (×100)	805.65	797.99	94.73	677.01	785.27	53.51	71,137.19
Observations	616,126	616,126	616,126	616,126	616,126	616,126	616,126

†: All coefficients and standard errors are scaled by the average difference between the 90th and 10th percentile lenient opioid prescriber within a facility (2.54pp) and then again by 100.

Notes: This table reports the output from a regression of each outcome listed in the column headers on provider opioid prescribing propensity based on regression model from Equation 3. All outcomes are calculated over a three-year period. Columns 1-3 report the log of 1+ total spending, columns 4-6 report number of day encounters, and column 7 reports the length of the patient-PCP relationship in days. Coefficients and standard errors are scaled for interpretability. Mean is the mean of the dependent variable. All regressions include clinic, year-by-month, day of week, bins for days between desired and actual appointment date, race, five-year age bins, marital status, Medicare/Medicaid beneficiary status, prior year medical history, and prior year Elixhauser comorbidity index. Robust standard errors are clustered at the clinic-level. All samples are constrained to be alive during the outcome period. *p<0.1; **p<0.05; ***p<0.01.

Table 5: Mental Health and Mortality

	Dependent variable (Scaled†):		
	Depression	Suicide Attempts	3-Year Mortality
	(1)	(2)	(3)
Propensity	0.308**	0.018	0.001
	(0.141)	(0.020)	(0.049)
Mean (×100)	23.36	0.49	5.18
Observations	616,126	616,126	590,265

†: All coefficients and standard errors are scaled by the average difference between the 90th and 10th percentile lenient opioid prescriber within a facility (2.54pp) and then again by 100.

Notes: This table reports the output from a regression of each outcome listed in the column headers on provider opioid prescribing propensity based on regression model from Equation 3. All outcomes are calculated over a three-year period. All outcomes except for mortality are measured with diagnosis codes. Coefficients and standard errors are scaled for interpretability. Mean is the mean of the dependent variable. All regressions include clinic, year-by-month, day of week, bins for days between desired and actual appointment date, race, five-year age bins, marital status, Medicare/Medicaid beneficiary status, prior year medical history, and prior year Elixhauser comorbidity index. Robust standard errors are clustered at the clinic-level. All samples are constrained to be alive during the outcome period. *p<0.1; **p<0.05; ***p<0.01.

Table 6: PCP Non-Opioid Pain Management, Harm Reduction and Treatment for Opioid Use Disorders

	Dependent variable (Scaled†):		
	Refer Non-Opioid Therapy	Prescribe Naloxone	Initiate OUD Treatment
	(1)	(2)	(3)
Propensity	−4.027***	−0.091***	−0.0002
	(1.230)	(0.038)	(0.031)
Mean Dep. Var. (×100)	14.78	0.53	0.72
Observations	4,204	4,204	4,941

†: All coefficients and standard errors are scaled by the average difference between the 90th and 10th percentile lenient opioid prescriber within a facility (2.54pp) and then again by 100.

Notes: This table reports the output from PCP-level regressions of various opioid-related behavior outcomes on standardized PCP opioid prescribing propensity. Column 1 reports the fraction of patients receiving opioids, who also get referred to non-opioid therapy options, column 2 reports the number of naloxone prescriptions prescribed by the PCP to the number of opioid prescriptions prescribed, and column 3 reports the fraction of each PCP's patients who are prescribed medication for opioid use disorder or get referred to OUD treatment (by the PCP) within 3 years of their index visit. Analysis samples in columns 1 and 2 are restricted to those who have at least one patient in 2014 (coinciding with non-opioid therapy and naloxone guidelines). All regressions include clinic fixed effects and are weighted by the number of assigned patients in our baseline sample. Robust standard errors are clustered at the clinic-level. *p<0.1; **p<0.05; ***p<0.01.

Table 7: Do certain policies do better at curbing the pandemic?

	Dependent variable (Scaled†):			
	Long-Term Use	Opioid Use Disorder	Log Total Cost	Poisonings & Accidents
	(1)	(2)	(3)	(4)
Propensity	0.846***	0.065*	1.753	0.065
	(0.103)	(0.037)	(1.402)	(0.051)
Propensity × PDMP	−0.160	−0.029	1.708	−0.007
	(0.165)	(0.055)	(2.045)	(0.068)
Propensity × MA-PDMP	0.427**	−0.072	−2.888	−0.317
	(0.212)	(0.122)	(4.604)	(0.236)
Propensity × Prescribing Limit	−0.308	−0.027	−4.975*	−0.123
	(0.290)	(0.125)	(2.662)	(0.162)
Propensity × Pill Mill	−0.425**	−0.165***	−0.300	−0.043
	(0.168)	(0.063)	(2.405)	(0.086)
Propensity × Good Samaritan	−0.277	−0.061	−2.484	−0.050
	(0.191)	(0.089)	(2.493)	(0.113)
Propensity × Naloxone	−0.130	0.123	2.168	0.203**
	(0.156)	(0.078)	(2.005)	(0.101)
Propensity × Medicaid Expansion	−0.311*	−0.110	−4.504	−0.137
	(0.185)	(0.099)	(2.771)	(0.168)
Mean (×100)	2.36	0.90	800.09	1.37
Observations	332,831	320,852	320,852	320,852

†: All coefficients and standard errors are scaled by the average difference between the 90th and 10th percentile lenient opioid prescriber within a facility (2.54pp) and then again by 100.

Notes: This table explores whether various policies were more effectively at curbing the opioid epidemic by interacting a pre-period (2005-2009) propensity with indicator variables for whether the encounter was during a time where the state had enacted a given policy. The smaller sample size reflects the fact that many PCPs are not observed between 2005-2009. Coefficients and standard errors are scaled for interpretability. Mean is the mean of the dependent variable. The main effects (scaled by 100) on PDMP for outcomes in columns 1 to 4 are: -0.059, 0.106, 3.963, -0.093; MA-PDMP: 0.225, -0.096, 4.067, 0.461; Prescribing Limit: -0.243, -0.358, -5.283, -0.152; Pill Mill: -0.564; 0.177; -3.285; 0.010; Good Samaritan: -0.123, 0.060, 2.203, 0.036; Naloxone: 0.053, -0.039, 2.211, 0.197; Medicaid Expansion: 0.065, -0.030, -1.713, 0.232. All regressions include clinic, year-by-month, day of week, bins for days between desired and actual appointment date, race, five-year age bins, marital status, Medicare/Medicaid beneficiary status, prior year medical history, and prior year Elixhauser comorbidity index. Robust standard errors are clustered at the clinic-level. All samples are constrained to be alive during the outcome period. *p<0.1; **p<0.05; ***p<0.01.

Appendix (For Online Publication Only)

A. Additional Figures and Tables

Appendix Figure A.1: Enrollment Form For New VHA Benefits: Form 1010EZ

OMB Control No. 2900-0091
Estimated Burden Avg. 30 min.
Expiration Date 12/31/2020

Department of Veterans Affairs

VA DATE STAMP
(For VHA Use Only)

APPLICATION FOR HEALTH BENEFITS

SECTION I - GENERAL INFORMATION

Federal law provides criminal penalties, including a fine and/or imprisonment for up to 5 years, for concealing a material fact or making a materially false statement. (See 18 U.S.C. 1001)

TYPE OF BENEFIT(S) APPLYING FOR:

☐ **ENROLLMENT** - VA Medical Benefits Package (Veteran meets and agrees to the enrollment eligibility criteria specified at 38 CFR 17.36)

☐ **REGISTRATION** - VA Health Services (Veterans meets the "Enrollment not required" eligibility criteria specified at 38 CFR 17.37)

1A. VETERAN'S NAME *(Last, First, Middle Name)*	1B. PREFERRED NAME	2. MOTHER'S MAIDEN NAME

3A. BIRTH SEX	3B. SELF-IDENTIFIED GENDER IDENTITY	4. ARE YOU SPANISH, HISPANIC,OR LATINO?	5. WHAT IS YOUR RACE? *(You may check more than one. Information is required for statistical purposes only.)*
☐ MALE ☐ FEMALE	☐ MALE ☐ FEMALE ☐ TRANSMALE/TRANSMAN/FEMALE-TO-MALE ☐ TRANSFEMALE/TRANSWOMAN/MALE-TO-FEMALE ☐ CHOOSE NOT TO ANSWER	☐ YES ☐ NO	☐ ASIAN ☐ AMERICAN INDIAN OR ALASKA NATIVE ☐ BLACK OR AFRICAN AMERICAN ☐ WHITE ☐ NATIVE HAWAIIAN OR OTHER PACIFIC ISLANDER ☐ CHOOSE NOT TO ANSWER

6. SOCIAL SECURITY NO.	7A. DATE OF BIRTH *(mm/dd/yyyy)*	7B. PLACE OF BIRTH *(City and State)*	8. RELIGION

9A. MAILING ADDRESS *(Street)*	9B. CITY	9C. STATE	9D. ZIP CODE	9E.COUNTY

9F. HOME TELEPHONE NO. *(optional)* *(Include Area Code)*	9G. MOBILE TELEPHONE NO. *(optional)* *(Include Area Code)*	9H. E-MAIL ADDRESS *(optional)*

10A. HOME ADDRESS *(Street)*	10B. CITY	10C. STATE	10D. ZIP CODE	10E.COUNTY

11. CURRENT MARTIAL STATUS

☐ MARRIED ☐ NEVER MARRIED ☐ SEPARATED ☐ WIDOWED ☐ DIVORCED

12A. NEXT OF KIN NAME	12B. NEXT OF KIN ADDRESS	12C. NEXT OF KIN RELATIONSHIP

12D. NEXT OF KIN TELEPHONE NO. *(Include Area Code)*	12E. NEXT OF KIN WORK TELEPHONE NO. *(Include Area Code)*	13. DESIGNEE - INDIVIDUAL TO RECEIVE POSSESSION OF YOUR PERSONAL PROPERTY LEFT ON PREMISES UNDER VA CONTROL AFTER YOUR DEPARTURE OR AT THE TIME OF DEATH *(Note: This does not constitute a will or transfer of title)*

14. WHICH VA MEDICAL CENTER OR OUTPATIENT CLINIC DO YOU PREFER? *(for listing of facilities visit www.va.gov/find-locations)*	15. WOULD YOU LIKE FOR VA TO CONTACT YOU TO SCHEDULE YOUR FIRST APPOINTMENT? ☐ YES ☐ NO

SECTION II - MILITARY SERVICE INFORMATION

1A. LAST BRANCH OF SERVICE	1B. LAST ENTRY DATE	1C. FUTURE DISCHARGE DATE	1D. LAST DISCHARGE DATE

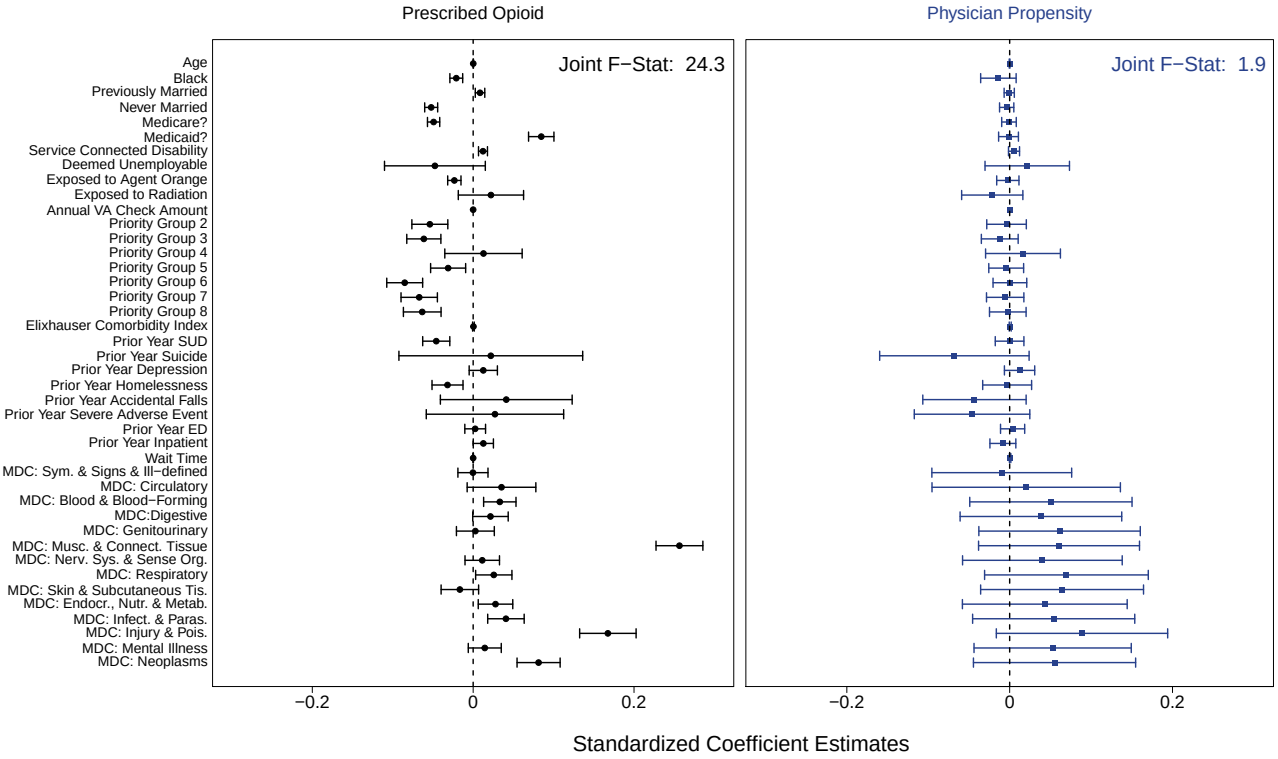
1E. DISCHARGE TYPE	1F. MILITARY SERVICE NUMBER

2. MILITARY HISTORY *(Check yes or no)*	YES	NO		YES	NO
A. ARE YOU A PURPLE HEART AWARD RECIPIENT?	☐	☐	G. DO YOU HAVE A VA SERVICE-CONNECTED RATING?	☐	☐
B. ARE YOU A FORMER PRISONER OF WAR?	☐	☐	IF "YES", WHAT IS YOUR RATED PERCENTAGE _____ %		
C. DID YOU SERVE IN A COMBAT THEATER OF OPERATIONS AFTER 11/11/1998?	☐	☐	H. DID YOU SERVE IN VIETNAM BETWEEN JANUARY 9, 1962 AND MAY 7, 1975?	☐	☐
D. WERE YOU DISCHARGED OR RETIRED FROM MILITARY FOR A DISABILITY INCURRED IN THE LINE OF DUTY?	☐	☐	I. WERE YOU EXPOSED TO RADIATION WHILE IN THE MILITARY?	☐	☐
E. ARE YOU RECEIVING DISABILITY RETIREMENT PAY INSTEAD OF VA COMPENSATION?	☐	☐	J. DID YOU RECEIVE NOSE AND THROAT RADIUM TREATMENTS WHILE IN THE MILITARY?	☐	☐
F. DID YOU SERVE IN SW ASIA DURING THE GULF WAR BETWEEN AUGUST 2, 1990 AND NOVEMBER 11, 1998?	☐	☐	K. DID YOU SERVE ON ACTIVE DUTY AT LEAST 30 DAYS AT CAMP LEJEUNE FROM AUGUST 1, 1953 THROUGH DECEMBER 31, 1987?	☐	☐

VA FORM 10-10EZ, FEB 2021 PREVIOUS EDITIONS OF THIS FORM ARE NOT TO BE USED PAGE 3 OF 5

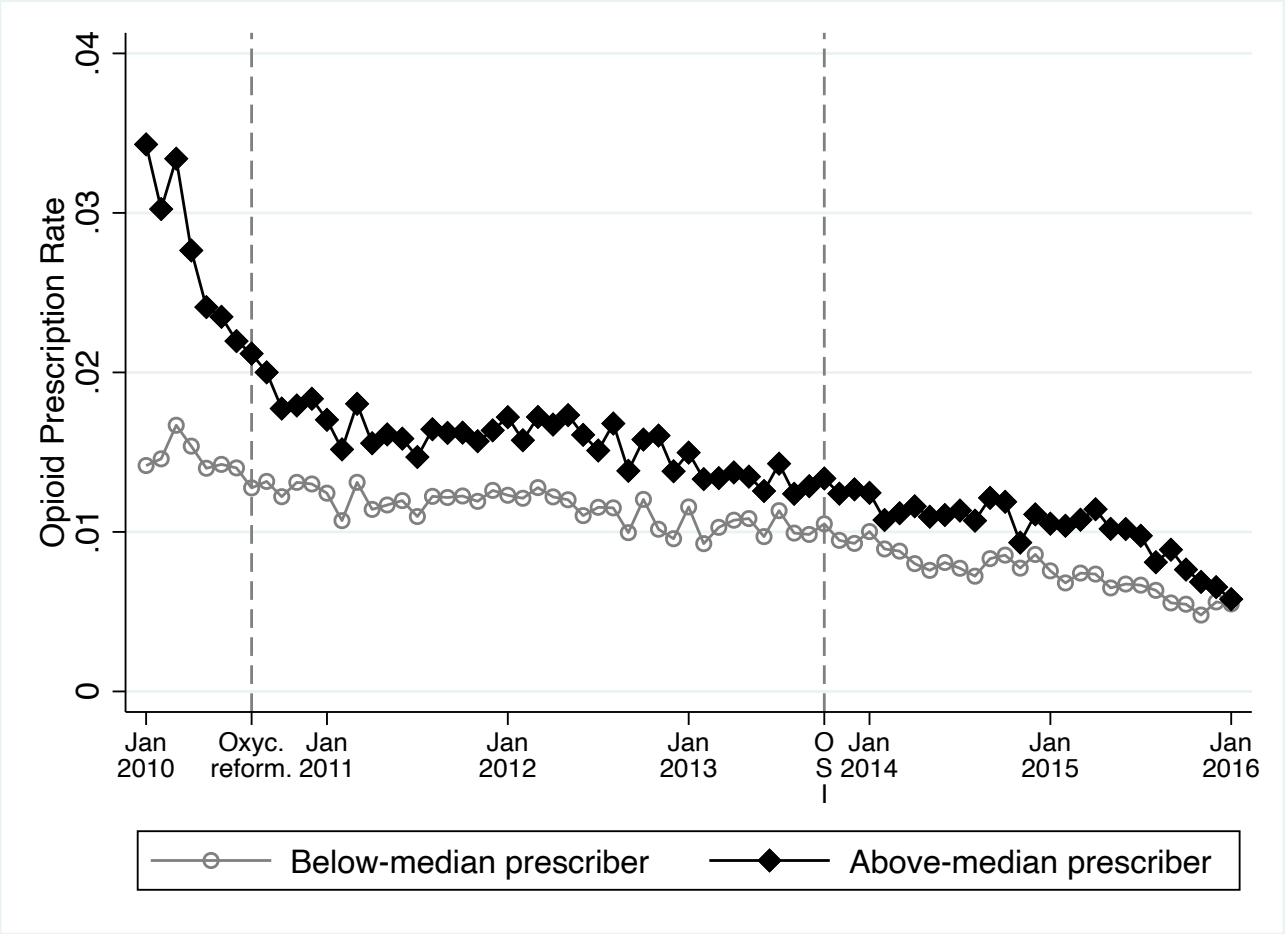
Notes: Form 1010-EZ for veterans enrolling in VHA health benefits. Veterans fill out demographic information, military service history, and whether they would like to be contacted to schedule an appointment (in box 15).

Appendix Figure A.2: Balance Test for Quasi-Random Assignments



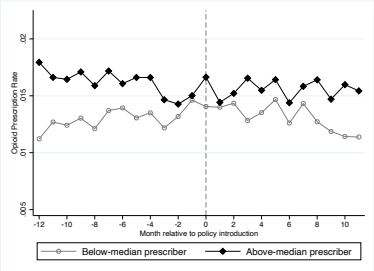
Notes: This figure tests for random assignment of providers to patients for our baseline sample. The left panel regresses prescribed opioid indicator on all observables, jointly (patient demographics and previous medical history), and the right panel is the same regression but with the provider propensity as the dependent variable. Both dependent variables are standardized. Regression coefficients and its accompanying 95% confidence intervals are plotted. Construction of the propensity measure is described in the text. Residualization fixed effects include hospital-year, year-month, day of week, diagnosis, and gender. The joint F-statistics are reported. The number of observations is 846, 101 for both regressions. Robust standard errors are clustered at the provider level.

Appendix Figure A.3: Time Series of Opioid Prescription Rates

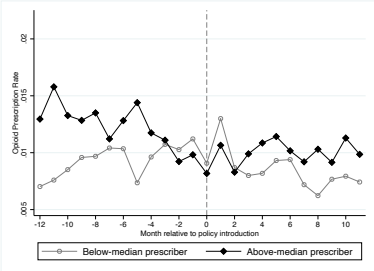


Notes: This figure shows mean opioid prescription rates of above- vs. below median primary care providers at the monthly-level. The median is calculated within-facility, based on data from the year 2009. The sample includes all PCPs with complete panel data for the years 2009-2015 who work in facilities with at least one other PCP who has complete panel data ($N = 2,036$). Prescription rates are calculated as the fraction of patients receiving at least one opioid prescription in a given month, among all patients originally assigned to the PCP in the preceding three years.

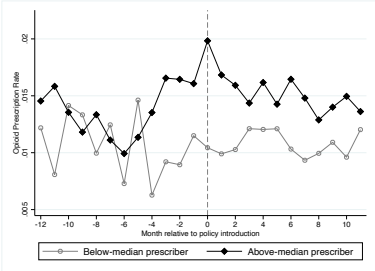
Appendix Figure A.4: Trends in Opioid Prescribing Following Implementation of Various Opioid Policies



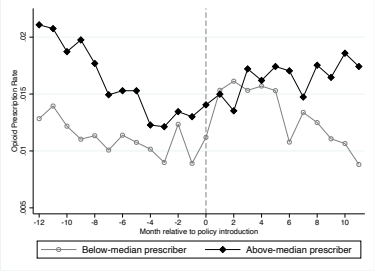
(a) Voluntary PDMP



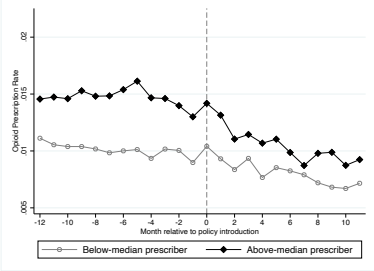
(b) Must-Access PDMP



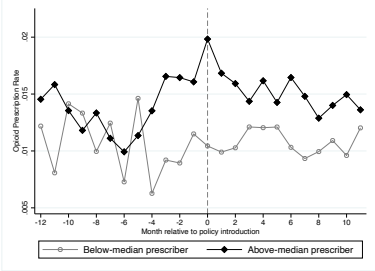
(c) Prescribing Limit



(d) "Pill Mill"



(e) "Good Samaritan"



(f) Naloxone Access

Notes: This figure plots the time series of average number of opioid prescriptions per patient among high and low prescribers relative to the implementation timing of different state-level opioid policies. High and low prescribers are defined as above and below median opioid prescribing within a facility in 2009, and fixed over the time period. Based on a balanced panel of providers with complete data for the years 2009-2015, working at facilities with at least two PCPs satisfying this criterion. Each panel is restricted to subset of PCPs from facilities in states in which the respective policy was implemented at some point in the sample period (2010-2015). The policy implementation dates are at the state-month level and taken from Lee et al. (2021).

Appendix Table A.1: Main Outcomes with Provider Propensity Quartiles

	Dependent variable (×100):							
	Long-Term Use	Opioid Use Disorder	Poisonings & Accidents	Overdose Mortality	Log Spending	Depression	Attempted Suicide	Mortality
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Propensity: Q2	−0.013	−0.019	0.007	−0.008	3.062***	−0.134	0.008	−0.108
	(0.059)	(0.035)	(0.047)	(0.007)	(1.146)	(0.170)	(0.026)	(0.080)
Propensity: Q3	0.152***	0.011	0.048	0.001	3.296***	−0.173	0.010	−0.021
	(0.059)	(0.036)	(0.045)	(0.008)	(1.177)	(0.178)	(0.026)	(0.080)
Propensity: Q4	0.199***	0.002	0.080*	0.001	7.272***	0.367*	0.017	0.090
	(0.068)	(0.037)	(0.044)	(0.008)	(1.169)	(0.198)	(0.026)	(0.078)
Mean (×100)	2.16	0.91	1.45	0.05	805.65	23.36	0.49	5.18
Observations	639,341	616,126	616,126	578,430	616,126	616,126	616,126	590,265

Notes: This table reports the output from a regression of each outcome listed in the column headers on (within-facility) provider propensity quartile. The omitted reference group is the bottom quartile of PCP propensity. Coefficients and standard errors are scaled by 100 for interpretability. Mean is the mean of the dependent variable. All regressions include clinic, year-by-month, day of week, bins for days between desired and actual appointment date, race, five-year age bins, marital status, Medicare/Medicaid beneficiary status, prior year medical history, and prior year Elixhauser comorbidity index. Robust standard errors are clustered at the clinic-level. All samples are constrained to be alive during the outcome period. *p<0.1; **p<0.05; ***p<0.01.

Appendix Table A.2: Outcomes Constructed By Excluding Any Diagnosis From Assigned PCP

	Dependent variable (Scaled†):			
	Opioid Use Disorder	Poisonings & Accidents	Depression	Suicide Attempts
	(1)	(2)	(3)	(4)
Propensity	0.032	0.071**	0.222*	0.012
	(0.020)	(0.034)	(0.123)	(0.018)
Mean (×100)	0.88	2.73	21.2	0.44
Observations	616,126	616,126	616,126	616,126

†: All coefficients and standard errors are scaled by the average difference between the 90th and 10th percentile lenient opioid prescriber within a facility (2.54pp) and then again by 100.

Notes: This table reports the output from a regression of each outcome listed in the column headers on provider opioid prescribing propensity using diagnosis outcomes constructed *excluding* any diagnosis from their assigned PCP to deal with endogeneous diagnosing. Outcomes constructed from non-diagnosis specific data are excluded. Coefficients and standard errors are scaled for interpretability. Mean is the mean of the dependent variable. All regressions include clinic, year-by-month, day of week, bins for days between desired and actual appointment date, race, five-year age bins, marital status, Medicare/Medicaid beneficiary status, prior year medical history, and prior year Elixhauser comorbidity index. Robust standard errors are clustered at the clinic-level. All samples are constrained to be alive during the outcome period. *p<0.1; **p<0.05; ***p<0.01.

Appendix Table A.3: Results Controlling for Mental Health Referral Propensity

	Dependent variable (Scaled†):							
	Long-Term Use	Opioid Use Disorder	Poisonings & Accidents	Overdose Mortality	Log Spending	Depression	Attempted Suicide	Mortality
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Propensity (EB)	0.428***	0.031	0.049*	−0.001	2.595***	0.282***	0.018	−0.0001
	(0.055)	(0.021)	(0.026)	(0.005)	(0.763)	(0.136)	(0.020)	(0.049)
Mean (×100)	2.16	0.91	1.45	0.05	805.65	23.36	0.49	5.18
Observations	639,341	616,126	616,126	578,430	616,126	616,126	616,126	590,265

†: All coefficients and standard errors are scaled by the average difference between the 90th and 10th percentile lenient opioid prescriber within a facility (2.54pp) and then again by 100.

Notes: This table reports the output of our baseline regression of each dependent variable on PCP opioid prescribing propensity, controlling for their propensity to refer patients to mental health specialists. This latter (leave-out) propensity is calculated analogously using initial visits. Coefficients and standard errors are scaled for interpretability. Mean is the mean of the dependent variable. All regressions include clinic, year-by-month, day of week, bins for days between desired and actual appointment date, race, five-year age bins, marital status, Medicare/Medicaid beneficiary status, prior year medical history, and prior year Elixhauser comorbidity index. Robust standard errors are clustered at the clinic-level. All samples are constrained to be alive during the outcome period. *p<0.1; **p<0.05; ***p<0.01.

Appendix Table A.4: Placebo Prior Year Outcomes

	Dependent variable (Scaled†)		
	Depression	Suicide Attempts	Poisonings & Accidents
	(1)	(2)	(3)
Propensity	0.004	0.001	−0.008
	(0.046)	(0.004)	(0.006)
Mean (×100)	2.92	0.04	0.08
Observations	663,912	663,912	663,912

†: All coefficients and standard errors are scaled by the average difference between the 90th and 10th percentile lenient opioid prescriber within a facility (2.54pp) and then again by 100.

Notes: This table reports the output of regressions of prior year ("placebo") outcomes on provider opioid prescribing propensity based on regression model from Equation 3. We observe outcomes in the prior year for veterans who utilize VHA care prior to being enrolled or utilize Medicare and Medicaid. There are no opioid-related prior outcomes because the sample consists of opioid-naïve patients. All outcomes are measured with diagnosis codes; poisonings and accidents do not include suicides. Coefficients and standard errors are scaled for interpretability. Mean is the mean of the dependent variable. All regressions include clinic, year-by-month, day of week, bins for days between desired and actual appointment date, race, five-year age bins, marital status, Medicare/Medicaid beneficiary status, and prior year Elixhauser comorbidity index. Robust standard errors are clustered at the clinic-level. $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$.

Appendix Table A.5: Effects by Number of PCP Encounters

	Dependent variable (Scaled†):							
	Long-Term Use	Opioid Use Disorder	Poisonings & Accidents	Overdose Mortality	Log Spending	Depression	Attempted Suicide	Mortality
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Propensity	0.046	0.007	0.102**	0.006	−0.242	0.245	0.066**	0.319***
	(0.053)	(0.041)	(0.042)	(0.011)	(1.779)	(0.199)	(0.026)	(0.099)
Propensity ×	0.189**	−0.023	−0.121**	−0.009	3.024*	−0.068	−0.072**	−0.504***
# Y1 Encounters: 2-4	(0.077)	(0.051)	(0.059)	(0.013)	(1.641)	(0.211)	(0.035)	(0.116)
Propensity ×	1.100***	0.130**	0.005	−0.011	1.629	0.285	−0.054	−0.261*
# Y1 Encounters: 5+	(0.140)	(0.059)	(0.072)	(0.014)	(1.874)	(0.251)	(0.039)	(0.139)
Mean (×100)	2.16	0.91	1.45	0.05	805.65	23.36	0.49	5.18
Observations	639,341	616,126	616,126	578,430	616,126	616,126	616,126	590,265

†: All coefficients and standard errors are scaled by the average difference between the 90th and 10th percentile lenient opioid prescriber within a facility (2.54pp) and then again by 100.

Notes: This table reports the output from a regression of each outcome listed in the column headers on provider opioid prescribing propensity, interacted with bins of the number of encounters with their PCP in the first year. Long-term use is defined as 180 days of opioid supply in the first year; the other three outcomes are over a three-year period. Coefficients and standard errors are scaled for interpretability. Mean is the mean of the dependent variable. All regressions include clinic, year-by-month, day of week, bins for days between desired and actual appointment date, race, five-year age bins, marital status, Medicare/Medicaid beneficiary status, prior year medical history, and prior year comorbidity index. Robust standard errors are clustered at the clinic-level. All samples are constrained to be alive during the outcome period. *p<0.1; **p<0.05; ***p<0.01.

Appendix Table A.6: Results for Patients Who Stay with PCP for at Least One Year

	Dependent variable (Scaled†):							
	Long-Term Use	Opioid Use Disorder	Poisonings & Accidents	Overdose Mortality	Log Spending	Depression	Attempted Suicide	Mortality
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Propensity	0.507***	0.023	0.030	0.002	2.857***	0.266*	−0.002	−0.0002
	(0.066)	(0.024)	(0.031)	(0.005)	(1.065)	(0.161)	(0.022)	(0.053)
Mean (×100)	1.91	0.82	1.41	0.03	806.3	22.39	0.44	3.91
Observations	469,473	453,242	453,242	408,393	453,242	453,242	453,242	414,516

†: All coefficients and standard errors are scaled by the average difference between the 90th and 10th percentile lenient opioid prescriber within a facility (2.54pp) and then again by 100.

Notes: This table reports the output from a regression of each outcome listed in the column headers on provider opioid prescribing propensity for veterans who stay with their assigned PCP for at least one year. Long-term use is defined as 180 days of opioid supply in the first year; the other outcomes are over a three-year period. Coefficients and standard errors are scaled for interpretability. Mean is the mean of the dependent variable. All regressions include clinic, year-by-month, day of week, bins for days between desired and actual appointment date, race, five-year age bins, marital status, Medicare/Medicaid beneficiary status, prior year medical history, and prior year Elixhauser comorbidity index. Robust standard errors are clustered at the clinic-level. All samples are constrained to be alive during the outcome period. *p<0.1; **p<0.05; ***p<0.01.

Appendix Table A.7: Results with Year-Varying Empirical Bayes Provider Propensity Measure

	Dependent variable (Scaled†):							
	Long-Term Use	Opioid Use Disorder	Poisonings & Accidents	Overdose Mortality	Log Spending	Depression	Attempted Suicide	Mortality
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Propensity (EB)	0.666***	0.047***	0.014	−0.001	2.977***	0.414***	0.014*	0.054**
	(0.029)	(0.010)	(0.013)	(0.002)	(0.289)	(0.048)	(0.008)	(0.022)
Mean (×100)	2.16	0.91	1.45	0.05	805.55	23.36	0.49	5.18
Observations	637,182	614,046	614,046	576,783	614,046	614,046	614,046	588,586

†: All coefficients and standard errors are scaled by the average difference between the 90th and 10th percentile lenient opioid prescriber within a facility (2.54pp) and then again by 100.

Notes: This table reports the output from a regression of each outcome listed in the column headers on a year-varying Empirical Bayes (EB) provider propensity measure as described in subsection B.4. Coefficients and standard errors are scaled for interpretability. Mean is the mean of the dependent variable. All regressions include clinic, year-by-month, day of week, bins for days between desired and actual appointment date, race, five-year age bins, marital status, Medicare/Medicaid beneficiary status, prior year medical history, and Elixhauser comorbidity index. Robust standard errors are clustered at the clinic-level. All samples are constrained to be alive during the outcome period. *p<0.1; **p<0.05; ***p<0.01.

Appendix Table A.8: Average Characteristics of Providers in the Top and Bottom Quartile of Propensity

	Quartile: Top ("Lenient")	Bottom ("Strict")
Female	0.463	0.550
Age	50.6	51.0
Age: <35	0.060	0.042
Age: 35-44	0.206	0.227
Age: 45-54	0.305	0.279
Age: 54+	0.392	0.424
Physician	0.582	0.522
Full-time	0.647	0.663
Patients per day	12.0	11.8

Notes: This table summarizes primary care providers based on their observable characteristics for providers in the top ("lenient") and bottom ("strict") quartile of prescribing propensity within a facility. Prescribing propensity is calculated as the sum of all residuals from Equation 1. Primary care providers in the VA can be physicians, nurse practitioners, or physician assistants. Age is a weighted average of the provider's age at time of each new patient assignment. Patients per day is the number of unique patients seen by the provider each calendar day we observe them on-shift.

Appendix Table A.9: Impact of Opioid-Related Regulation on Prescribing I

	PDMP		MA-PDMP		Prescr. Limit		Pill Mill		Good Samar.		Nalox. Access	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Low	High	Low	High	Low	High	Low	High	Low	High	Low	High
Month 3	-0.01	0.26*	-0.12	0.10	0.28***	-0.03	0.32***	0.23**	-0.03	-0.11	-0.05	-0.04
	(0.14)	(0.15)	(0.10)	(0.07)	(0.10)	(0.06)	(0.09)	(0.11)	(0.07)	(0.10)	(0.06)	(0.08)
Month 6	-0.01	0.24	-0.07	0.10	0.13	0.33***	0.10	0.42***	-0.12	-0.20	-0.03	0.01
	(0.14)	(0.17)	(0.13)	(0.11)	(0.22)	(0.05)	(0.17)	(0.10)	(0.07)	(0.13)	(0.11)	(0.13)
Month 9	-0.16	0.10	-0.06	0.07	0.11	0.02	0.19	0.21	-0.17**	-0.17	-0.07	-0.03
	(0.17)	(0.15)	(0.17)	(0.18)	(0.31)	(0.31)	(0.12)	(0.17)	(0.08)	(0.15)	(0.11)	(0.12)
Mean	1.70	1.76	0.95	1.29	2.52	1.68	1.08	1.70	0.96	1.35	0.97	1.18
Obs	67817	80811	67817	80811	67817	80811	67817	80811	67817	80811	67817	80811

Notes: This table reports event study estimates of the impact of various opioid-related policies for each month relative to the introduction date (i.e. the table-version of Figure 2). Each column reports estimates from a separate regression, focusing on a specific policy regulation (listed in the header), and the sample of below-median ("Low") vs. above-median ("High") prescribers (defined within-facility), respectively. We report treatment effect estimates at 3, 6, and 9 months post-introduction ($\beta_3, \beta_6, \beta_9$) from the following event study model: $y_{it} = \mu_i + \gamma_t + \sum_{s=-12}^{-2} \beta_s \mathbb{1}[r_{it} = s] + \sum_{l=0}^{12} \beta_l \mathbb{1}[r_{it} = l] + X_{it} + \epsilon_{it}$; where y_{it} is the opioid prescription rate (multiplied by 100 for readability) of PCP i in year-month t, r_{it} is month relative to policy introduction, and μ_i and γ_t are provider and date fixed effects. X_{it} controls for ACA-expansion status, as well as whether any of the other state-level opioid-related regulations (i.e. those listed in the column headers) are in place. For states that did not enact a given policy (and for relative event time periods falling 12 or more months before the implementation date), the relative event time is set to -12. For all periods 12 or more months after introduction, relative event time is set to 12. "Mean" is the mean of the dependent variable in the month before policy introduction (corresponding to the omitted relative event time period). Standard errors clustered at the state-level are shown in parentheses.

Appendix Table A.10: Impact of Opioid-Related Regulation on Prescribing II

	PDMP		MA-PDMP		Prescr. Limit		Pill Mill		Good Samar.		Nalox. Access	
	(1) Low	(2) High	(3) Low	(4) High	(5) Low	(6) High	(7) Low	(8) High	(9) Low	(10) High	(11) Low	(12) High
Post	0.08	0.11	0.03	-0.03	0.17	0.21**	0.02	0.15	-0.06	-0.14*	0.00	-0.01
	(0.10)	(0.10)	(0.06)	(0.11)	(0.13)	(0.09)	(0.10)	(0.10)	(0.05)	(0.08)	(0.06)	(0.06)
Mean	1.30	1.66	0.90	1.43	1.18	1.13	1.18	1.90	1.05	1.55	1.13	1.28
Obs	67817	80811	67817	80811	67817	80811	67817	80811	67817	80811	67817	80811

Notes: This table reports event study estimates of the average impact of various opioid-related policies over the full year post-introduction. It shows the coefficient estimate of *Post* from the following event study model: $y_{it} = \mu_i + \gamma_t + \eta \mathbb{1}[r_{it} \leq -12] + Post \mathbb{1}[0 \leq r_{it} < 12] + \nu \mathbb{1}[r_{it} \geq 12] + X_{it} + \epsilon_{it}$; where y_{it} is the opioid prescription rate (multiplied by 100 for readability) of PCP i in year-month t, r_{it} is month relative to policy introduction (for never-adopting states we set $r_{it} = -12$), and μ_i and γ_t are provider and date fixed effects. X_{it} controls for ACA-expansion status, as well as whether any of the other state-level opioid-related regulations (i.e. those listed in the column headers) are in place. Each column reports estimates from a separate regression, focusing on a specific policy regulation (listed in the header), and the sample of below-median ("Low") vs. above-median ("High") prescribers (defined within-facility), respectively. "Mean" is the mean of the dependent variable in the year before policy introduction (corresponding to the omitted relative event time period). Standard errors clustered at the state-level are shown in parentheses.

Appendix Table A.11: Impact of Opioid-Related Regulation on Prescribing III

	PDMP		MA-PDMP		Prescr. Limit		Pill Mill		Good Samar.		Nalox. Access	
	(1) Low	(2) High	(3) Low	(4) High	(5) Low	(6) High	(7) Low	(8) High	(9) Low	(10) High	(11) Low	(12) High
Post	-0.14	0.18	-0.07	0.03	0.31**	0.31**	0.27	-0.08	-0.04	-0.10	-0.07	0.11
	(0.17)	(0.12)	(0.08)	(0.17)	(0.15)	(0.15)	(0.20)	(0.09)	(0.08)	(0.08)	(0.08)	(0.12)
Mean	1.30	1.50	0.89	1.09	0.98	1.08	1.21	1.63	1.22	1.57	1.10	1.21
Obs	33434	40223	33434	40223	33434	40223	33434	40223	33434	40223	33434	40223

Notes: This table reports results from the same specification as in Table A.10, and the same table notes apply. However, instead of using mean prescription data of the year 2009 to construct leniency (used to split the sample into within-facility above- and below-median prescribers), this table is based on a residualized leniency measure using prescribing data of the years 2005-2009 and constructed as described in subsection 3.1.

B. Robustness

B.1 Quasi-Random Assignment: Placebo Checks

The key identifying assumption to interpret our estimates as the causal effect of being assigned a high prescribing PCP is that providers are quasi-randomly assigned to patients (the conditional independence assumption). While we are leveraging a unique primary care assignment process where new opioid-naïve patients are enrolling in VHA benefits and entering new primary care teams, as well as having demonstrated balance checks along patient observables, this assumption may still be violated. Namely, by patient-provider selection along unobservable margins. At its core, quasi-random assignment along unobservables is inherently untestable, but we will provide some robustness checks and empirical exercises that lend credibility to this assumption.

One way selection along unobservables may impact our findings is through the fact that while veterans are new beneficiaries, some have prior health records with the VHA (through care such as emergency visits while being not formally enrolled) and Medicare or Medicaid. To test whether this is a concern, we can conduct "placebo" tests by checking whether provider propensity impacts prior year measures of our main outcomes. The regression results are reported in Table A.4. Note that since we are focusing on opioid-naïve veterans, we cannot conducted placebo tests on opioid-related outcomes, nor can we test mortality. The coefficients are not statistically significant or economically meaningful.

If our findings are entirely driven by selection and not PCP propensity to prescribe opioids, then the correlation between provider propensity and patient outcomes should be independent of how many times the patient sees their PCP (i.e., the intensity of treatment). However, if instead the findings are driven by provider propensity, then the effects should be increasing with the number of encounters. To test this relationship, we estimate our main regression specification including a categorical variable for number of encounters between the veteran and the PCP in the first year, interacted with $Propensity_i$. Although the number of primary care visits is not exogenous, in subsection 4.2 we find that number of visits is not correlated with propensity, and this exercise can still yield some descriptive insight. Table A.5 reports the result of this exercise which exhibits a common pattern. Across all outcomes, the main

propensity effect shrinks towards zero: For veterans who only see their PCP once, the PCP's propensity has little to no impact on patient outcomes. While we lose some statistical power by studying this heterogeneity margin, the effect of propensity on outcomes load on veterans who have more encounters with their PCP.

B.2 Sample Selection Checks

Results for sub-sample who stays with assigned provider Even if initial PCP assignment is quasi-random, veterans can choose to switch providers. We find in our baseline sample that 94% of initial patient-PCP relationships last at least two months and 73% last at least one year (this includes cases where the relationship ends not due to initiation by the veteran, but due to a PCP retiring or leaving the VHA). There is no relationship between each patients' relationship length with their assigned PCP and their PCP's opioid prescribing tendency. Column 7 of Table 4 shows that the difference between 90th and 10th percentile in opioid prescribing propensity only increases their relationship length by a statistically insignificant 0.71 days (and roughly 0.04 visits, column 4). We conduct our main analyses for patients who remain with their initially assigned PCP for at least one year, keeping in mind that veterans who experience the worst outcomes may choose a new PCP. The results are reported in Table A.6 and are generally qualitatively similar to our main results, albeit the standard errors are larger.

B.3 Ruling out Bias from Endogenous Diagnosing

As alluded to throughout the paper, except for mortality, all of our outcome measures are based on diagnosis codes. This may be a concern for two reasons. Endogenous diagnoses are an issue if the assigned PCP contributes both to the elevated opioid-prescribing risk and directly to our outcomes by their diagnosing.[26] Then our results may not necessarily be picking up worse outcomes, but rather they may reflect the likelihood conditions *are diagnosed*. Note that to an extent, this concern is present in all research with outcomes constructed using clinician diagnosing, and is not unique to our setting. Nevertheless, to

[26] In addition, even if the PCP is not the one directly diagnosing the conditions, they may be indirectly contributing to the outomes via their referral patterns.

address this concern, we run our main specification with only diagnoses from other clinicians (excluding their assigned PCP's). The regression coefficients are reported in Table A.2 and are consistent with our main results.

B.4 Alternative Prescribing Propensity Measure: Time-Varying Empirical Bayes Estimator

In our main analysis we construct a time-invariant measures of provider propensity for veteran i treated by provider j, $Propensity_{ij}$. If providers change their prescribing behavior—relative to other providers in the same facility—then it may be more fitting to allow propensity to vary over time, say by year t: $Propensity_{ijt}$. A simple way to allow for this is to average the residuals (from the residualization regression Equation 1) at the provider j, year t level, leaving out the own residual, as in Eichmeyer and Zhang (2021). However, in the case of primary care, this approach runs into issues with statistical power (in a quarter of the provider-years, there are fewer than 17 new patient cases), meaning that the propensity measure becomes noisier.

To remedy this issue, we borrow a technique from the teacher value-added literature and allow for a provider's propensity in a given year to depend on his/her propensity in other years. In the context of teachers, a teacher can learn or improve from their previous teaching experiences (Chetty et al., 2014). The relationship across yearly propensities is estimated non-parametrically, and the weights also depend on the number of observations in a given year. Specifically, for each year t, we run the following regressions:

$$Prescribed_{ijt} = \sum_{t'=2005}^{2017} \sum_{k} \beta_{kt'}^{t} \mathbb{1}\{N_{jt'} = k\} \times Propensity_{ijt'} + \epsilon_{ijt}, \tag{4}$$

where $N_{jt'}$ denote the number of new patient cases for provider j in year t'. We create four bins: 0 - 9, 10 - 24, 25 - 49, 50+. The yearly propensities are interacted with bins for number of cases, giving more weight to more precisely estimated propensities, and shrink the noisier propensities towards the facility mean, zero. This is an Empirical Bayes estimator (Kane and Staiger, 2008). Note that this is a separate regression for each year t, and the coefficients $\beta_{kt'}^{t}$ differ by year. This means a provider's propensity in 2008 can affect the 2009 propensity

value differentially from how 2008 affects 2010 or how 2009 affects 2010-this is important if there were sudden policy changes.

Finally, the value of our new time-varying provider propensity measure is simply the predicted value, $\widehat{Prescribed}_{ijt}$. Then we regress our main results on this year varying empirical Bayes propensity and report the findings in Table A.7. The findings are qualitatively similar, and in many cases more precisely estimated.