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Primary care providers' influence on opioid use and its adverse consequences ☆

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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 0.43 pp) and development of opioid use disorder by 4% (or 0.035 pp). Veterans' mental health deteriorates: the three-year likelihood of a depression diagnosis increases by 1.3% (or 0.31 pp). 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.

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 (Florence et al., 2021; Degenhardt and Hall, 2012; Lynch et al., 2018; Romanowicz et al., 2019). To develop 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 1.85 pp and inter-quartile range is 1.52 pp). 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 1.21 pp (65%).

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With quasi-random assignment and our prescribing tendency measure in hand, we establish four 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 0.43 pp). 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 9 pp for every 2.54 pp 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 0.035 pp 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 0.31 pp (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.

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; Barnett et al., 2019; Eichmeyer and Zhang, 2022),[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 patients and providers, 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 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 (2022); Sabety et al. (2021); Schwab (2021); Staiger (2022)). Currie and Zhang (2022) construct a measure of provider effectiveness in reducing 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 results and address potential threats to identification in Section 4. Section 5 explores mechanisms. 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.

[1] See Maclean et al. (2021) 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.

[2] Using a movers design methodology and Danish population data, Laird and Jessica (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. (2022) (local opioid abuse rates), Meinhofer (2018); Buchmueller and Carey (2018); Borgschulte et al. (2018); Kaestner and Ziedan (2020); Grecu et al. (2019); Alpert et al. (2022) (prescription drug monitoring programs), Borgschulte and Mark (2019) (pharmaceutical promotion), opioid prescribing limits Sacks et al. (2021), and naloxone access laws Doleac and Mukherjee (2021); Rees et al. (2019).

Assignment to PCPs for new enrollees begins with enrollment in VHA health benefits via Form 1010-EZ (see Fig. 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, scheduling availability, and each PCP's existing caseload and unused capacity (typically between 1,000–1,400 patients based on VHA rules). The administrator observes each PCP's caseload and capacity to coordinate assignments; in other words, an administrator (and not the PCP) makes assignments based on panel availability. Providers have little input on whether to take on new patients. 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 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 (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

2.3.1. Sample construction

We follow Currie and Zhang (2022) 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.

2.3.2. 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 longer 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; Barnett et al., 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 the 365 days following the index primary care visit, excluding the initial 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).

Non-opioid 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

[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.

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

[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.

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.

2.3.3. 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

Intuitively, we construct a primary care provider's tendency to prescribe opioids as their overall prescription rate net of their clinic's mean and a set of observable patient characteristics. Our preferred measure closely follows Eichmeyer and Zhang (2022) in that it is not time-varying, and solely considers a provider's prescription decision at the index visit. It is chosen to be time-invariant, because, empirically, we observe large auto-correlation in provider propensity across time and prefer a parsimonious measure. We focus solely on the index visit in order for our prescribing propensity measure to isolate as much as possible a provider's prescribing tastes—if we measured prescribing tendency over a longer time horizon, other practice behaviors, such as which type of patient to call in for additional visits, or the quality of care provided, would in turn feed into the propensity measure.

However, we also provide robustness checks using three alternative measures of PCP prescribing propensity: one allows the physician-level measure to vary across years, thereby more accurately capturing providers who substantially change their prescribing behavior. It addresses associated concerns related to statistical power (i.e., not observing many cases per physician-year) by use of an Empirical Bayes estimation procedure. We describe this measure in more detail in this section. The other two measures—described in Appendix subsection B.5—measure physician prescribing behavior over a longer horizon than just the index visit, and also consider the strengths of prescriptions.[8]

Main Propensity Measure We construct our preferred prescribing propensity measure following a leave-out (jackknife) residualized approach as in Kling (2006); Dobbie et al. (2017); Bhuller et al. (2020); Eichmeyer and Zhang (2022). Specifically, 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:

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

where $\alpha_{clinic}, \alpha_{year\times month}, \alpha_{dayofweek}$ 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 enrollee 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,[9] 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 $j, \mathbb{J}$, excluding patient i's μ in the average:

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

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

Time-Varying Propensity Measure 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 $t : Propensity_{it}$. A simple way to allow for this is to average the residuals (from the residualization regression Eq. 1) at the provider j, year t level, leaving out patient i's own residual, as in Eichmeyer and Zhang (2022). However, in the case of primary

[7] For the diagnosis-based outcomes of *Opioid Use Disorder*, *Poisonings and Accidents*, *Depression* and *Suicide Attempts*, it is important to take into account potential differences across PCPs in their tendency to *diagnose* these health conditions. We address this point through robustness checks that include only diagnoses not originating from a patient's index PCP in the construction of the outcomes. Furthermore, we find that in practice, for the particular health conditions studied in this paper, the vast majority of diagnoses that enter into our outcome measures are diagnosed by providers other than the patient's index PCP. For example, excluding any diagnoses from their assigned PCP, the three year opioid use disorder rate observed in our sample drops only marginally from 0.91% to 0.88%.

[8] Results for all three robustness checks are provided in the robustness subsection 4.3.

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

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):				
Milligrams of Morphine Equivalence/Day (2005)	10	15	19.57	20
Milligrams 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).

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; Kane and Staiger, 2008). 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:

$$Prescribed_{it} = \sum_{t'=2005}^{2017} \sum_{k} \beta^{t}_{kt'} \mathbb{1}\{N_{j(i)t'} = k\} \times Propensity_{it'} + \epsilon_{it}, \tag{3}$$

where $N_{j(i)t'}$ denotes the number of new patient cases for i's 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. Note that this is a separate regression for each year t, and the coefficients $\beta^{t}_{kt'}$ 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, $\widehat{Prescribed}_{it}$.

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:

$$Y_i = \beta Propensity_i + \alpha_{clinic} + \alpha_{year\times month} + \alpha_{dayofweek} + \alpha_{desired} + \theta X_i + \epsilon_i \tag{4}$$

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 Eq. 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., 100 pp). 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 2.54 pp (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 2.54 pp 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? Fig. 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 1.42 pp.[10] 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.

In Panel B of Fig. 1, we report results from a test of balance on observables. The panel overlays a local linear regression of *predicted* 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 Fig. A.2 for a complete list). We find that patients' predicted opioid prescription receipt is not correlated with provider propensity: the coefficient on the slope parameter is neither economically nor statistically significant (coefficient estimate of 0.00006, standard error of 0.00106).[11]

Fig. 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 with 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 our main empirical specification from Eq. 4. We scale estimates such that they correspond to the average causal effect, in percentage points, of a 2.54 pp 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.

4.1.1. Opioid use

We find large effects on prescription opioid use, with assignment to a higher prescriber—that is, someone who prescribes at a 2.54 pp higher rate at the index visit—increasing the likelihood of becoming a long-term prescription opioid user by 0.43 pp (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.[12]

The effect of assignment to a physician who prescribes at a 2.54 pp higher rate at the index visit is an order of magnitude larger than the equivalent estimate found for the case of emergency department physicians in Eichmeyer and Zhang (2022).[13] When comparing effect sizes for a move from the bottom decile to the top decile of prescribing among PCP vs. ED providers, we find that the effect in the PCP setting, in absolute terms, is about twice the size of that in the ED; relative to the baseline mean of long-term use in the respective setting, the PCP effect is about five times the size of that found in the ED setting.[14] 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 (2022). 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.[15] The share increases by 9 pp when assigned to a PCP who prescribes at a 2.54 pp 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] This is roughly in line with Laird and Jessica (2016) who find a standard deviation of 1.8 pp for physicians in Denmark. They use all primary care visits, whereas we use only first visits.

[11] While this pattern is consistent with balance of observable patient characteristics that predict opioid receipt, it could also be explained by the relatively modest predictive power of a patient's demographics and military and medical history for opioid prescription receipt (we find a R^2 of 0.024 and a standard deviation of predicted opioid receipt of 0.7%). Therefore, we also report results from a balance test of predicted three-year mortality (for which we find a larger R^2 of 0.08, using the same set of predictors as before) in Fig. A.3. We find strong balance with this variable as well, in support of the assumption of quasi-random assignment.

[12] An alternative way to gauge the significance of primary care providers for patients' prescription opioid use is to calculate the counterfactual rates of prescription opioid use that would obtain if all providers in a facility prescribed at a certain, lower rate. Using our main regression model and coefficient estimates, we can predict, for each individual in the sample, what their outcome would look like if we gave their PCP a specific prescribing leniency and compare the predicted outcome with their actual observed outcome. We find that if we moved all PCPs to the 25th percentile of prescribing leniency (within their facility), there would be 9,236 opioid prescriptions from first visits (a 24% reduction relative to the observed number) and 12,719 long-term users (a 8% reduction). If all PCPs were moved to the 10th percentile, there would be 7,859 opioid prescriptions from first visits (a 35% reduction) and 12,244 long-term users (a 11% reduction).

[13] The equivalent reduced form estimate in Eichmeyer and Zhang (2022) for a 2.54 pp increase in an ED physician's tendency to prescribe at the index ED visit is a 0.05 pp increase in long-term use.

[14] Moving from the bottom decile to the top decile of ED physician leniency (a jump which is at 11.6 pp much larger than the equivalent 2.54 pp jump in the PCP setting) leads to a 0.24 pp increase in long-term use, a 4.1% increase relative to the mean.

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

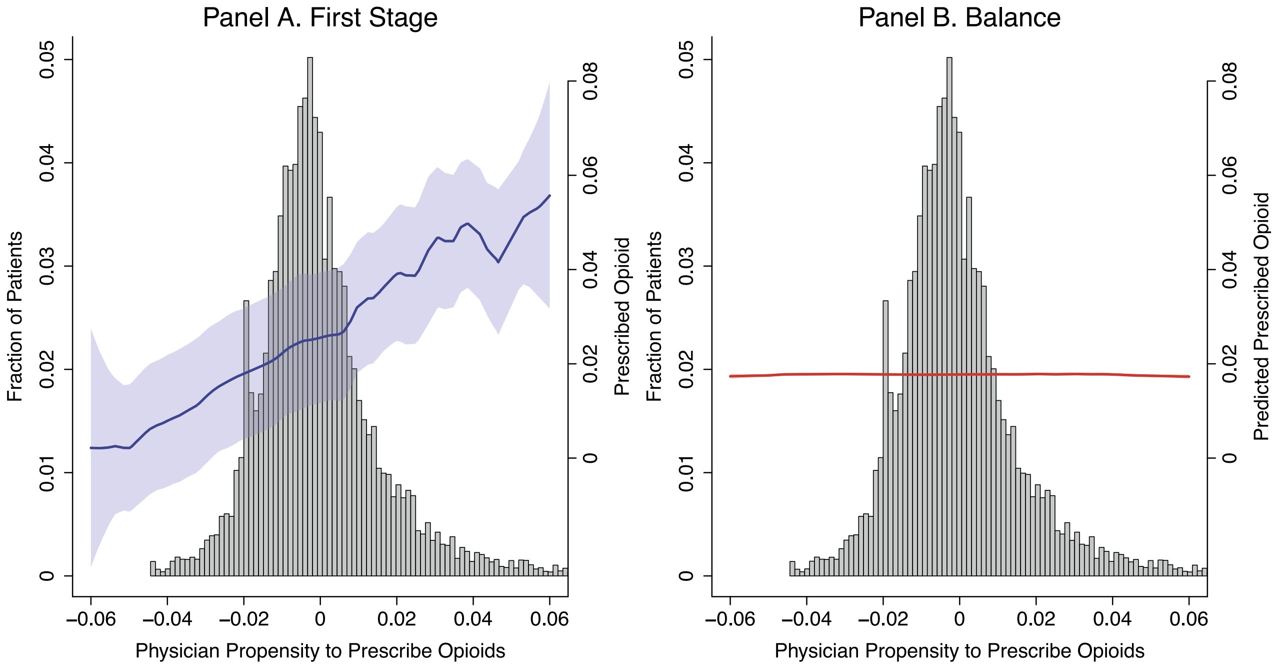

Fig. 1. First Stage and Balance Test for Quasi-Random Assignment. *Notes*: This figure plots the distribution of the provider propensity measure and tests for random assignment of providers to patients for our baseline sample. In both panels, a histogram of the provider propensity measure is plotted (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 their corresponding mean dependent variable of whether the veteran is prescribed an opioid on their first index visit. Panel B tests for balance by first estimating the predicted likelihood of being prescribed an opioid based on veteran demographics, military history, and previous medical history (see Fig. A.2 for the complete list of covariates), and then fitting a local linear regression through provider propensity bins and their corresponding mean predicted likelihood of being prescribed. The estimated coefficient (and standard error) of the slope parameter of a linear regression for the two figures are 0.432 (0.026) and 0.00006 (0.00106).

Table 2
Opioid-related outcomes.

	Dependent variable (Scaled†):					
	Prescribed at Index Visit (1)	Long-Term Use (2)	Log MME (3)	Opioid Use Disorder (4)	Poisonings & Accidents (5)	Overdose Mortality (6)
Propensity	1.214***	0.426***	15.484***	0.035*	0.058*	−0.002
	(0.066)	(0.055)	(0.893)	(0.020)	(0.032)	(0.005)
Mean (×100)	1.866	2.157	191.11	0.910	3.21	0.046
Observations	649,773	639,341	616,126	616,126	616,126	578,430

Notes: This table reports the output from a regression of each opioid-related outcome on provider opioid prescribing propensity based on regression model from Eq. 4. 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.
† All coefficients and standard errors are scaled by the average difference between the 90th and 10th percentile lenient opioid prescriber within a facility (2.54 pp) and then again by 100.

4.1.2. 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 0.035 pp on a base of 0.91%—a 4% increase relative to the mean.[16] This estimate is marginally significant at the 10% level, and we can rule out an effect size larger than 0.070 pp (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

[16] This estimate is about 2.3 times the size compared to the equivalent one found in the ED in Eichmeyer and Zhang (2022), given by the reduced form effect of a 2.54 pp increase in one's ED physician's tendency to prescribe opioids.

Table 3
Share of prescription opioids prescribed by assigned PCP.

	Dependent variable (Scaled†):		
	Share of Total Number of Prescriptions (1)	Share of Total Days Supply (2)	Share of Total MME (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

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

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.[17]

Third, we do not detect an effect on opioid overdose mortality. The event is very rare in 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 −0.010 pp and larger than 0.006 pp 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.[18]

4.2. Health care utilization, mental health, and mortality

4.2.1. 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).[19] 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).

4.2.2. 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 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 0.31 pp (or 1.3% relative to the mean) on depression diagnoses (column 1).[20] Given the effect size for long-term prescription opioid use (0.43 pp), 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.[21] 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,[22] 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,[23] or by differences in overall quality of care between lenient and strict PCPs. Regarding the latter: if the tendency to prescribe opioids was 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 than 0.097 pp, corresponding to 1.9% relative to the mean. Consequently, 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

[17] Of note, the equivalent estimate for the ED setting studied in Eichmeyer and Zhang (2022) is much higher, at 28%. This difference is likely due to important differences in the two samples: the average ED visitor may suffer from more severe pain-related health conditions, and may be in worse mental health, compared to the average primary care patient, which could influence the propensity to develop OUD through long-term exposure to prescription opioids.

[18] At the same time, neither do we find evidence of improvements to pain-related outcomes—the main reason opioids are prescribed for. Table A.11 shows a null effect of prescriber leniency on a patient's propensity to have pain-related ED visits, as well as a positive effect on self-reported pain scores—that is, patients assigned to leniently prescribing providers report on average *higher* pain in the years following their PCP assignment.

[19] 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.

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

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

[22] 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.

[23] 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.

Table 4
Healthcare utilization.

	Dependent variable (Scaled†):						
	Log Total Cost (1)	Log Outpat Cost (2)	Log Inpat Cost (3)	PCP Visits (4)	MH Visits (5)	ED Visits (6)	Relationship Length (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

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 Eq. 4. 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.

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

Table 5
Mental health and mortality.

	Dependent variable (Scaled†):		
	Depression (1)	Suicide Attempts (2)	3-Year Mortality (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

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 Eq. 4. 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.

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

(as alluded to earlier), including worse treatment of OUD, or it could work through 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*.[24]

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, regression specification checks, 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). Fourth, we show robustness to using year-varying (non-Empirical Bayes) propensity including facility-year fixed effects to account for facility-specific time trends (Table A.8). Finally, we construct three alternative measures of prescribing propensity—a time-varying one using Empirical Bayes shrinkage, as well as measures covering a longer time horizon than just the index visit, and incorporating the strengths of prescriptions—finding results closely in line with those from our baseline specification (Table A.7 and Table A.12).

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 Eq. 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.10, 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

[24] We also investigate the *timing* of effects across different outcomes (Fig. A.4), finding that impacts on depression diagnoses commence early, already ca. 2–3 quarters after provider assignment, and 1–2 quarters before we observe effects on OUD diagnosis. These dynamics could be consistent with persistent opioid use leading to a relatively quick deterioration in mood, and a slightly slower progression into (or diagnosis of) OUD.

Table 6
Other physician behaviors that correlate with prescribing tendency.

	Dependent variable (Scaled†):				
	Refer Non-Opioid Therapy (1)	Prescribe Naloxone (2)	Initiate OUD Treatment (3)	Prescribe Oxycontin (4)	MME > 90 (5)
Propensity	−4.027***	−0.091***	−0.0002	0.177	−0.027
	(1.230)	(0.038)	(0.031)	(0.185)	(0.067)
Mean Dep. Var. (×100)	14.78	0.53	0.72	12.57	2.39
Observations	4,204	4,204	4,941	4,941	4,941

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. Columns 4 and 5 reports the fraction of each PCP's opioid prescriptions in the first year that are Oxycontin (oxycodone) and have a daily milligrams of morphine of at least 90, respectively. 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.
† All coefficients and standard errors are scaled by the average difference between the 90th and 10th percentile lenient opioid prescriber within a facility (2.54 pp) and then again by 100.

from differences in tastes and/or beliefs *within* demographic groups, as opposed to *across* those groups.[25]

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 four dimensions of care: i) adherence to VHA guidelines created in 2014 aimed at increasing non-opioid therapy treatment for pain management by referring to pain clinics and community integrative health clinics (e.g., acupuncture, massage therapy); ii) use of harm reduction strategies by co-prescribing naloxone with opioid prescriptions; iii) prescription of medication for treatment of OUD and referral to OUD treatment; iv) particularly risky prescription of opioids (i.e., particularly strong opioids or particularly abuse-prone opioids).

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. Finally, in columns (4) and (5), we investigate the correlation of prescribing propensity with the likelihood of supplying particularly risky opioids, conditional on prescribing an opioid. Here, we do not find evidence that particularly leniently prescribing providers also prescribe particularly strong or abuse-prone opioids.

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. 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 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.

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).

[25] In line with overall differences in provider tastes and/or beliefs driving prescribing differences, we also find monotonicity of prescribing propensity across patient types. Following Bhuller et al. (2020), we test a key implication of monotonicity which asserts that leniency constructed by leaving out a particular subsample has predictive power over that same left-out subsample. Table A.9 shows results of the impact of provider prescribing tendency on prescription opioid receipt for different demographic sub-groups (such as above median age), when prescribing propensity is estimated off of different, non-overlapping sub-groups (such as below median age). We find that prescribing propensity estimated off of a left-out sample has significant predictive power over (in the sense of a strong impact on) opioid receipt among the remaining sample.

Declaration of Competing Interest

The authors received funding during their PhD from the Department of Veterans Affairs (VA). Zhang is a federal contractor with the VA during the time of this work.

Appendix A. Supplementary material

Supplementary data associated with this article can be found, in the online version, at https://doi.org/10.1016/j.jpubeco.2022.104784.

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