Prescription Opioid Use, and Physical and Mental Health: Evidence from Primary Care

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Abstract

Primary care is the most frequently utilized health service and is the source for 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 abuse, and physical and mental health among veterans. Using over two decades of electronic health records, we exploit variation in opioid prescribing tendency across providers in the same facility, in conjunction with quasi-random assignment of providers to new patients. We find that assignment to a PCP who prescribes opioids at a 3 percentage point (pp) higher rate (equivalent to the difference between a 90th and 10th percentile prescriber within a facility) is associated with an increase in the probability of long-term opioid use by 0.72pp, development of an opioid use disorder by 0.12pp, and five-year opioid overdose mortality by 0.008pp. Veterans' mental health deteriorates; the three-year likelihood of attempted suicide or self-harm increases by 0.023pp and depression diagnosis increases by 0.18pp. Investigating into the mechanisms, we find evidence consistent with high opioid prescribers being less likely to refer patients to alternative pain management, adhere to clinical recommendations on naloxone distribution, or refer patients to substance use disorder treatment.

1. Introduction

The opioid epidemic is one of the worst drug epidemics in US history, taking the lives of over half a million Americans since 1999 (CDC, 2018). In response, health organizations and

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health practitioners are debating what levels of opioid prescribing can meet patient needs by managing pain without compromising patient health and dependence (Lowenstein et al., 2018; Ross et al., 2011). In this debate, empirical evidence on the long-term consequences of opioid prescribing is crucial, but scarce. Our paper aims to reduce this knowledge gap.

We study the association between a patient's primary care provider's (PCP) propensity to prescribe opioids and the patient's long-term opioid use, misuse and abuse, physical and mental morbidities, and mortality. Concentrating on the primary care setting is particularly important because it accounts for nearly half of all prescription opioids dispensed in the United States (Levy et al., 2015). Other research has found that even short-term exposure to opioids in emergency departments (EDs) can lead to long-term opioid use, abuse, and overdoses (Barnett et al., 2017, 2019; Eichmeyer and Zhang, 2020). This long-term impact stemming from short-term exposure requires a consistent source of prescription opioids, which is most easily facilitated via a patient's PCP since these relationships are longer lasting and the interactions are more frequent.[1] This also means that a PCP's opioid prescribing behavior is likely to be consequential and may have distinct dynamic impacts on patient outcomes (Fadlon and Parys, 2020; Finkelstein et al., 2018).

Precisely because of the long-lasting and frequent nature of patient-PCP relationships, patients may "shop" for their physician. For example, a patient may prefer their physician to have experience treating their pre-existing conditions or be of similar race or gender (Bornstein et al., 2001); or perhaps patient experiencing chronic pain may search over multiple doctors ("doctor shop") until they find a physician who readily prescribes large quantities of prescription opioids (Peirce et al., 2012; Finkelstein et al., 2018). In settings with such non-random assignment, it becomes difficult to identify the impact of PCPs on long-term patient outcomes. We leverage the unique assignment process in the Veterans Health Administration (VHA), where new veterans do not choose their PCP, but are assigned to one by an administrative clerk, to minimize such selection issues. Empirically, we find that

[1] According to the US National Ambulatory Medical Care Survey and the US National Hospital Ambulatory Medical Care Survey for the US, adults visit their PCP (the ED) an average of 1.5 (0.46) times a year (CDC, 2016).

conditional on seasonality (time of year, day of week), initial primary care diagnosis, and gender, certain VHA facilities assign veterans to PCPs quasi-randomly (464 of 678 facilities). Moreover, we focus on opioid-naïve patients—veterans who have not filled opioid prescriptions in the previous year—who are less likely to choose physicians based on likelihood of receiving an opioid prescription.

Using the universe of VHA electronic health records from 1999-2020, supplemented with veterans' Medicare and/or Medicaid claims covering years 2011-2016, we follow the health trajectories of all US veterans who newly entered the VHA since 1999, starting from their first quasi-randomly assigned VHA primary care visit. Equipped with quasi-random assignment of PCPs to patients within the same facility, we measure each provider's propensity to prescribe opioids. This propensity to prescribe opioids is interpreted as the probability of prescribing prescription opioids for the average new veteran, relative to other PCPs in the same facility, working at the same time, treating the same condition.[2] Our empirical strategy allows us to estimate the causal effect of being assigned a physician with a higher propensity to prescribe opioids on the veterans's long-term (three year) outcomes falling into four categories: i) long-term opioid use; ii) opioid misuse and abuse; iii) medical care utilization; and iv) physical and mental health, including mortality. It is important to note that PCPs differ along many dimensions, some of which may be correlated with opioid prescribing propensity and even influence the above outcomes. We focus on opioid prescribing propensity because it is measurable, salient, and of first-order importance on opioid abuse outcomes, among others.

We establish four main results. First, a PCP's propensity to prescribe opioids impacts both the likelihood that the veteran is initially prescribed an opioid at their first visit, and the likelihood of becoming a long-term prescription opioid user. Being assigned to PCP who prescribes opioids at a 3 percentage point (pp) higher rate than other PCPs in the same facility (equivalent to the difference between PCPs at the 90th and 10th percentile of

[2]This propensity to prescribe opioids is similar in spirit to the judges instrument in applications related to judges and incarceration, and is used as an instrumental variable in Eichmeyer and Zhang (2020). Note that we *do not* use this propensity as an instrument in this paper; recognizing the myriad ways through which PCPs can influence the health trajectories of their patients in years-long, multiple interactions, above and beyond opioid prescribing, we chose to instead analyze the "reduced form" only.

propensity; the standard deviation is 1.85pp and inter-quartile range is 1.52pp), is associated with a 2.15pp higher probability of being prescribed an opioid at the first visit, and a 0.72pp higher probability of filling at least 180 days supply of prescription opioids in the first year following initial assignment. This increased likelihood of long-term prescription opioid use is roughly an increase by a third, on a base rate of 2.25%. It is an order of magnitude larger than the long-term use estimates found in prior research in emergency settings, likely because primary care relationships involve repeated interactions over a long period of time. Ca. 41% of all opioids filled in the first year are from the veteran's assigned PCP; this fraction increases by 11pp as the provider prescribes 3pp more.

Second, veterans assigned to higher prescribing PCPs are more likely to misuse and abuse prescription opioids and develop opioid use disorders (OUD; colloquially referred to as opioid addiction). A 3pp higher prescribing physician is associated with a 0.037pp increase in developing an OUD, on a base of 0.62pp (6%). This estimate implies that about 5% of patients who become long-term users through their lenient PCP develop an opioid use disorder within three years. We also find weak evidence of opioid overdose events increasing by about 0.006pp for every 3pp increase in prescribing propensity. Additional proxies for opioid sedation tell a similar story: diagnoses of accidental falls and vehicle accidents, which are proxies for impulsivity and sedation and predictive of opioid overdose risk, increases by roughly 0.03-0.04pp.

Third, physicians identified as high prescribers of opioids influence veterans' physical and mental health outcomes at large. Likelihood for an emergency department visit and inpatient hospitalization increase by 0.30pp and 0.15pp respectively, when a patient is assigned a 3pp higher prescribing PCP. We go on to study the link between exposure to prescription opioids and mental health, an area where the research has lacked strong evidence. We find that veterans who are assigned to a PCP who prescribes 3pp more relative to their peers, have a 0.023pp higher probability of attempting suicide or self-harm and 0.18pp higher probability of being diagnosed with major depressive disorder within three years. Both opioid abuse and mental health disorders are issues that are particularly prevalent among veterans, making this

finding especially distressing given that PCPs are the most frequently visited practitioners and serve as both prescribers and referrers to therapists and counselors. Five-year opioid overdose mortality increases by 0.008pp, roughly a 25% increase (with 40% of this mortality effect coming from heroin or synthetic opioids such as fentanyl); for overall mortality, we cannot reject an effect size of zero.

Finally, 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 clinic 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. Moreover, even though high prescribers are more likely to put veterans on a path to opioid misuse, they are worse at recognizing opioid dependence (via fewer diagnoses per prescription) and less likely to refer patients to substance abuse treatment and mental health clinics. This last set of findings is consistent with high prescribers having poorer *overall* pain management skills, beginning with appropriate prescribing and considering alternative non-opioid treatments, to recognizing and finally treating opioid abuse, and even 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 is closely related to Eichmeyer and Zhang (2020), Barnett et al. (2017) and Barnett et al. (2019), in that it exploits within-facility, across-provider variation in opioid prescribing tendency (within clinical guidelines) as a source of variation in opioid exposure across patients.[3] It employs the same methodology as Eichmeyer and Zhang (2020) to construct a residualized measure of propensity to prescribe opioids that is relative to the primary care facility mean. Different from the aforementioned papers which focus on short-term exposure to opioid prescribing risk, this paper studies the primary care setting where

[3]See Eichmeyer and Zhang (2020) for a detailed review of the opioid-related literature.

any elevated prescribing risk is likely to be persistent over time. Furthermore, Eichmeyer and Zhang (2020) study the causal effect of a single *opioid prescription*, vis à vis this paper studies the causal effect of being assigned a *high prescribing provider*. While the latter directly increases the likelihood of being prescribed opioids, it may be correlated with other physician practice behavior that influence patient outcomes (in other words, the exclusion restriction). This reduced form interpretation makes our paper more similar to Barnett et al. (2017) and Barnett et al. (2019), except we focus on the primary care setting and also study a larger set of opioid abuse, physical and mental health, and mortality. Given the prevalence of primary care and amount of opioids prescribed from it, the question of how lenient opioid prescribers affect patient outcomes is of policy importance in and of itself.

Other related research that study opioids in primary care settings are Laird and Nielsen (2016) on the impact of prescription opioid dissemination through primary care for the case of Denmark, and Finkelstein et al. (2018) on the impact of place-based factors as measured by local rates of opioid abuse for the sample of disabled Medicare recipients. Both papers exploit moves of patients across municipalities for their research design. Laird and Nielsen (2016) find that moves to municipalities where local PCPs prescribe at a 10 percentage point higher rate are 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. Finkelstein et al. (2018) find that moving to a county with a 20 percent higher rate of opioid abuse increases rates of opioid abuse substantially, by 6 percent. Like the two papers, we also exploit quasi-experimental supply-side opioid prescribing variation, however, we study physical and mental health outcomes, and also study the mechanisms beyond differences in opioid prescribing rates.

Linking drug abuse and mental health, Borgschulte et al. (2018) also study how prescription drug monitoring programs that potentially restrict the supply of prescription opioids can impact suicide. They find that when addicted individuals have the resources for substance abuse treatment, they're less likely to commit suicide. Our paper builds on their work on how supply-side factors can influence suicide; namely, we focus on a patient's PCP and argue

that those physicians who put their patients on a path to opioid addiction, are also worse at recognizing addiction and referring patients to treatment.

Finally, this paper also fits in the broader literature on the impact of primary care on patient outcomes. For instance, there is a literature on the effects of primary care spending or access (usually via a larger government budget) on health outcomes and utilization. Martin et al. (2008) find that increased primary care spending improved cancer and circulatory disease outcomes; Dolton and Pathani (2016) find that increasing access to primary care hours led to fewer ambulance use and hospitalizations. Focusing on a specific PCP quality measure, Dusheiko et al. (2011) find that PCPs who are better at managing stroke care generate lower health care costs. Hsiao and Boult (2008) and Friedberg et al. (2010) summarize the literature tying primary care quality to outcomes and find that greater continuity of care is associated in lower hospital costs, and patients who claim primary care as their main source of care have better mortality results. Using ranking of medical school as a proxy for physician education quality, Schnell and Currie (2018) find that physicians who received better training are less likely to prescribe opioids, suggesting that high opioid prescribing behavior, all else equal, correlates with lower physician quality overall. One of the key challenges in studying the impacts of primary care on patient outcomes is having a definitive quality metric (Young et al., 2017). Existing quality measures and ratings may be confounded with interpersonal skills and patient satisfaction (Chen, 2018). We build on this literature by alleviating patient-physician selection, focusing on a single easily interpretable physician metric, and studying a multitude of patient outcomes over a panel of three years.

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 assignment process of providers to patients in the VHA as well as the data. Our methodology to construct each PCP's propensity to prescribe opioids is described in section 3. It is followed by a description of our empirical strategy in section 4. We present the main results and address potential threats to identification in section 5. In section 6, we review the interpretation and explore mechanisms behind our findings. The last section concludes by discussing implications for policy.

2. Setting, Data, and Sample

2.1 Institutional Background on PCP assignment

In the VHA, veterans are assigned to facilities based on geographical distance. Within facilities, each veteran is assigned to one primary care provider, who is responsible for all of the veterans' primary care.[4] This provider is usually a physician, but can also be a physician's assistant, or advanced registered nurse (i.e., nurse practitioner). In any case, the assigned PCP has the license to prescribe in the VHA. When a veteran seeks primary care, they first visit a nearby medical facility, and upon first contact, an administrative clerk assigns the veteran a PCP based on availability.[5,6] This "assignment" process is in contrast to primary care outside of the VHA, where patients may search for a general practitioner according to their own preferences, sometimes researching providers ahead of time (Harris, 2003). Under the VHA assignment process, the new patient has little influence over their initial PCP assignment, overall; however, patients can switch teams and subsequent assignments may be based on the physician's clinical expertise and the patient's medical history. Therefore, to minimize

[4]In October 2009, the VHA implemented a patient-driven team-based care model called Patient Aligned Care Teams (PACT) which consists of a primary care provider, a nurse care manager, a clinical associate, and an administrative clerk. This change affected the care veterans received, but not the assignment process or who the prescription drug prescriber is.

[5]Assignment is not at first VHA enrollment, but rather the first time a veteran seeks out primary care. Common reasons for a first visit include screens for hypertension, diabetes, and general examinations.

[6]This procedure has been anecdotally verified by various VHA researchers and administrators, and Petzel (2014). We do empirically check it in subsection 3.2.

selection bias due to patient sorting to PCPs based on unobserved health characteristics, we focus on each veteran's *first* assigned PCP.

2.2 Description of 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) starting from 1999 to April 2020. It includes standard outpatient, inpatient, pharmacy, and enrollment data. For each inpatient and outpatient medical encounters, we observe standard variables such as hospital, patient, and physician identifiers, diagnosis, procedures performed, and time of visit. For the pharmaceutical data we see the prescriber and patient identifiers, origin of prescription, issue and release dates. The enrollment tables provide us with the date each veteran first received VHA benefits, their enrollment priority status (there are eight priority groups based on veteran income, disability, and war decorations), and their service-connected disability rating; we also have a set of standard patient demographics such as race, gender, marital status, and income. Since we have access to electronic health record data, we also observe physician orders and referrals to specialists, which we utilize in section 6 when we investigate referral to alternate pain treatments and substance abuse clinics. Finally, every PCP assignment is documented since 1999 for every veteran, including information on when a relationship starts and ends (which can happen if a patient switches providers or if a provider leaves the VHA).

For a more complete view of veterans' health history, we supplement the CDW data with VA/CMS data: Medicare claims from 2011-2016 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. Finally, we also observe some non-VHA, non-CMS care. VHA benefits cover some community non-VHA care that the VHA does not have the capacity to completely cover. Examples include emergency care, nursing homes, childbirth at private hospitals, among others. This care then gets reimbursed by the VHA provided that the veteran file a

reimbursement claim. We observe all claims a veteran attempts to reimburse, regardless of final VHA approval status. In 2014, non-VHA access expanded greatly under the Veterans Access, Choice, and Accountability Act ("Choice Act"). The main provision gave veterans who are unable to schedule an appointment or live too far from a VA medical clinic the ability to access community care covered by their VHA benefits. Medical care from the Choice program are automatically updated in the VHA system nightly from vendor claims.[7]

Our final data source comes from VHA Vital Status files and Center for Disease Control and Prevention (CDC) National Death Index (NDI) Plus files which provides us with both the date and cause of death for each veteran. We observe all dates of death until early 2020, and all causes of death until the end of 2017. This data is sourced from the Social Security Administration Death Master File, Medicare Vital Status File, and interval VA records, which include hospital,family, and National Cemetery Administration records. Eichmeyer and Zhang (2020) describe all four VHA data sources in greater detail.

2.3 Sample Construction

Our sample construction process can be summarized as first empirically identifying the VHA facilities that quasi-randomly assign PCPs to patients, then concentrating on the *first* PCP assignment to opioid-naïve non-terminal cancer/end-of-life veterans between 1999 and April 2017. The cohort cutoff at 2017 is to ensure we have three years of outcomes to study.

We begin with 2.94 million veterans between the ages of 20 and 90 who were assigned and treated by a PCP[8] between 1999 and April 2017 and have no VHA primary care visits prior to assignment. We restrict to people who newly entered the VHA in/after 1999. For each veteran, we keep only the first PCP assignment/visit. We exclude ca. 10,000 veterans who are on end-of-life hospice care or have terminal cancer—prescription opioids are routinely

[7]Specifically, the data comes from Fee Basis Claims System (FBCS) and Program Integrity Tool (PIT) in the Corporate Data Warehouse.

[8]We exclude special population PCP and PACTs from Petzel (2014): geriatric, home-based, homeless, infectious diseases, post-deployment, renal dialysis, serious mental illness, spinal cord injuries and disorders, women's health. We also remove PACT team names that are clearly "specialty" primary care such as psychiatric care and substance treatment.

prescribed under such conditions—prior to or within three years of initial PCP assignment (our outcome period spans three years). Finally, we restrict attention to facilities that are sufficiently large: ones with at least 500 new patients during the study period and at least two PCPs per year. This procedure leaves us with 2.69 million veterans (and first PCP visits).

Next, to alleviate patient-physician selection concerns that plague standard primary care settings, we identify the facilities with evidence of quasi-random assignment.[9] We use a F-test approach à la Chan et al. (2020), and Feng and Jaravel (2020), following the steps listed below:

1. Ca. one million of the 2.69 million veteran visits have at least six months of VHA benefits prior to the initial PCP visit (and hence medical history). Using these visits, we regress a prior opioid use dummy (from pharmaceutical data or diagnosis codes for opioid dependence, abuse, or overdose)[10] on a rich set of patient demographics, prior medical history, patient risk, and initial primary care variables:[11]

$$OpioidUse_{i,t-1} = \theta X_{i,t-1} + \alpha_{hosp\times year} + \alpha_{year\times month} + \alpha_{dayofweek} + \alpha_{diag} + Gender_i + \epsilon_{i,t}. \tag{1}$$

   The resulting coefficient estimates are listed in Table B.1.

2. We obtain predicted prior opioid use, $\widehat{OpioidUse}_{i,t-1}$,—which has all the variation from the basis spanning the rich set of observables embedded—and regress it on a full set of PCP fixed effects separately for each facility f in the set of all facilities $\mathcal{F}$:

$$\widehat{OpioidUse}_{i,t-1} = \sum_{j\in f} \beta_j + \alpha_{year\times month} + \alpha_{dayofweek} + \alpha_{diag} + Gender_i + \eta_{i,t} \qquad \forall f \in \mathcal{F} \tag{2}$$

   where β_j is a fixed effect for each physician j that works in facility j.

[9] A facility is a VHA medical center outpatient clinic, or a VHA-affiliated community based outpatient clinic.

[10] Approximately 8.05% of the one million veterans used an opioid in the previous year.

[11] The full set of independent variables include age, race, gender, log income, marital status, enrollment priority group, 3 digit zip code, Elixhauser comorbidity score, prior opioid overdose, opioid use disorder, alcohol abuse, mental health, attempted suicide and self-harm, homeless, vehicle accident, accidental fall, inpatient hospitalization, emergency visit, benefit length, Medicare/Medicaid flag, service connected disability percentage, annual VA check amount, year-month, facility-year, day of week and hour fixed effects.

3. We then obtain the p-value associated with the joint F-test on the set of physician fixed effects for each facility. Figure 1 graphs the histogram of the resulting p-values. There is a distinct set of facilities that do not randomize. Facilities with a p-value greater than 0.1 are classified as facilities that show evidence of quasi-random assignment of providers to patients.

Out of the 678 facilities, 464 display evidence of quasi-random assignment, conditional on year by month, day of week, initial primary care visit diagnosis, and patient gender. These facilities are geographically scattered across the country. All subsequent analysis focuses only on patients who are assigned and treated at these facilities. To summarize, we identify the set of facilities that appear to quasi-randomly assign patients based on a rule-based empirical test that checks for evidence of non-random assignment of providers to patients. In other words, at the 464 facilities identified through this procedure, the variation in the rich set of characteristics (which include demographics, prior medical history and initial primary care characteristics) that predicts prior opioid use does not appear to differ across physicians within the same facility. Comparing facilities that do and do not quasi-randomly assign patients, on average, the quasi-random facilities have fewer physicians and therefore fewer patients than the non-random facilities; however, the number of new patients per physician is equal across the two types of facilities. One explanation could be that larger facilities have more administrators and resources, and a larger set of physicians to allocate to based on patient preferences. Patients treated at quasi-random facilities are in slightly poorer health. Table B.2 summarizes the differences between the two types of facilities.

Finally, with the 1.21 million veterans that are assigned at the quasi-random facilities, we restrict attention to opioid-naïve veterans who are treated by a physician with at least 100 cases over the entire sample period (this final cut is needed to build our physician propensity measure). Opioid-naïve veterans are selected because they are less likely to "doctor shop" to receive an opioid; we also remove the physicians that are in the top and bottom 0.5% of prescribing propensity (defined in section 3) to remove outliers and potential "pill mills". Note that make no restrictions on the reason or diagnosis condition at first visit. Our final sample

consists of 959,088 veterans who are assigned to 3,255 PCPs for the first time. We take the first assignment because first assignments are plausibly more random than subsequent PCP switches. Furthermore, even if patients were choosing their first PCP, it is unlikely that opioid-naïve patients are doing so to seek opioids.

2.4 Variable Construction

The main variable used to construct physician prescribing propensity is $Prescribed_i$, an indicator for whether patient i is prescribed *any* prescription opioid at their first primary care visit. Opioid prescription status is obtained based on a precise match between prescriber and PCP identifiers, prescription issue date and primary care visit date, along with patient identifier matches.

Our outcome measures fall into four categories: i) long-term opioid use; ii) measures of opioid misuse and abuse; iii) medical care utilization; and iv) physical and mental health, including mortality. All measures are dummy-variables that equal one if a given event occurred within the relevant time frame. With the exception of long-term opioid use, the time frame for all outcomes encompasses the three years following the date of the initial primary care visit. For long-term opioid use, the time frame is the year following the index visit. All outcomes, including long-term prescription opioid use, exclude the initial PCP visit.

Long-term opioid use is measured by an indicator for observing at least 180 (or 90) days supply of prescription opioids (from any source within the VHA or CMS) in the following 365 days for a given patient. This definition of long-term opioid use is common in the medical literature (Barnett et al., 2017, 2019; Jena et al., 2016; Dunn et al., 2010; Braden et al., 2010). The initial opioid prescription filled from the index primary care encounter is not counted towards this measure; however, subsequent refills are. We also construct the fraction of opioid prescriptions filled by each veteran (measured in prescription length or dosage) that is prescribed by their assigned PCP in the first year.

We have four proxies for opioid misuse and abuse: The two most direct measures of opioid misuse are an indicator for diagnosis of an opioid use disorder (OUD; colloquially

opioid addiction, and related to opioid dependence)[12] and whether the patient experienced an opioid overdose event. These measures are constructed from VHA, CMS, and some non-VHA medical diagnosis codes,[13] meaning that overdose events treated at non-VHA hospitals not covered by Medicaid or Medicare with no reimbursement filed by the veteran, and events treated at home or outside of VHA settings with naloxone are not captured in our data. Overdose events include both prescription, synthetic, and illicit opioid (i.e., heroin, fentanyl, and tramadol) events. The fourth and fifth measures of opioid misuse are proxies for impulsivity and sedation, which are predictive of opioid overdose risk (Oliva et al., 2017a): Indicators for having had a vehicle accident, and for having had an accidental fall diagnosis following the initial primary care visit. These two proxies also serve as physical health outcomes.

We study primary care, emergency department, and inpatient hospitalization for our medical care utilization outcomes. Primary care utilization is measured by the number of primary care visits with the patient's initially assigned provider in the first year. Emergency and inpatient utilization are measured by simple indicator variables for whether a veteran has had any encounter of that type in the three year period following their index PCP encounter.

Health and economic well-being is measured using the following five indicators: Mental health diagnosis (for major depressive disorder), attempted suicide and self harm (excluding suicide ideation), diagnosis of alcohol abuse, homelessness event, and mortality. The first three measures are based on medical diagnosis codes. Homelessness is measured using a variety of sources, including medical diagnosis codes for lack of housing/inadequate housing and outreach to VA homeless and/or shelter programs.[14] All outcomes exclude diagnoses recorded at the index primary care encounter.

Finally, for mortality, we build indicators for whether a veteran has died of a particular

[12]Opioid use disorder is defined in the Diagnostic and Statistical Manual of Mental Disorders (DSM-5) as a "problematic pattern of opioid use leading to clinically significant impairment or distress.".

[13]Namely, ICD-9 codes for OUD: 304.0x, 304.5x, and 304.7x. ICD-9 codes for opioid overdoses: 965.x, E850.0-E850.2, E935.0-E935.2, and E980.0.

[14]We follow VHA homelessness definitions from Peterson et al. (2015), which uses diagnosis codes, VA homeless service codes (Department of Housing and Urban Development VA Shared Housing, telephone and health care for homeless veterans, community outreach to homeless veterans), and VA inpatient treatment such as domiciliary care for homeless veterans.

cause within one-, three-, and five-years of their initial visit.

Many of our patient outcomes are based on clinician diagnoses, and diagnosing is endogenous. Therefore, we perform a robustness check in which we construct all diagnosis-based outcome measures *excluding* encounters from the assigned PCP; 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, the base 3-year OUD rate drops from 0.62% to 0.61% and attempted suicides drop from 0.36% to 0.33%. The results are largely unchanged, albeit with larger standard errors, and reported in Table B.3.

2.5 Summary Statistics

Table 1 describes relevant summary statistics for our final sample of veterans (non-cancer, opioid-naïve, first primary care visit with their PCP assigned at a facility that quasi-randomly assigns providers to patients). The median veteran is a middle aged white male: 95% are male, 90% are white and average age is 59. Average annual income is approximately $30,000, 61% are married, 21% have a service-connected disability (and hence are eligible for VA disability compensation), and 46% are on Medicare or Medicaid. Compared to the sample of veterans visiting emergency departments in Eichmeyer and Zhang (2020), primary care users are older, earn more, and are much healthier in terms of prior health morbidities, reflecting the fact that primary care sees a much more representative sample of patients than care in emergency departments.[15] For these first primary care visits, 3.1% of veterans are prescribed an opioid. Conditional on being prescribed an opioid, the daily milligrams of morphine equivalent (the standard unit of measurement for opioid intensity) is 23.4mg in 2000 and 20.4 in 2011. Relative to opioids prescribed during ED visits, those prescribed in primary care are similar in intensity, but cover much longer periods: In 2000, over three quarters of opioid prescriptions originating from primary care were over 25 days long, and 70% were refillable (often multiple times). In 2011, prescription lengths increased, with over three quarters

[15] Other reasons include the following: the sample in Eichmeyer and Zhang (2020) excludes ED visits for health conditions that are never prescribed opioids; our primary care sample drops prior opioid users; some patients in our baseline sample have limited prior medical history.

being over 30 days long, and 64% of them being refillable. In contrast, opioid prescriptions originating from ED visits—which tend to be prescribed for acute conditions—have a median prescription length of just five days, and virtually none are refillable.

3. PCP Propensity to Prescribe Opioids Measure

3.1 Constructing Physician Propensity

A primary care provider's tendency to prescribe opioids is measured relative to the facility mean and net of a set of observable patient characteristics. We construct this measure following a very similar approach as in Eichmeyer and Zhang (2020) (which in turn is based on Kling (2006); Bhuller et al. (2019); Dobbie et al. (2017)), as follows. Recall that 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 encounter-level prescription status on the set of fixed effects from Equation 1 and baseline controls:

$$Prescribed_i = \theta X_i + \alpha_{hosp \times year} + \alpha_{year \times month} + \alpha_{dayofweek} + \alpha_{diag} + Gender_i + \mu_i. \quad (3)$$

The fixed effects are essential because, as discussed in subsection 2.3, we assume quasi-random assignment conditional on facility-year, year-month, day of week and time of day, diagnosis, and gender. Age, race, and prior Elixhauser comorbidity index are included as controls, X_i, to improve precision. 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, excluding patient i's μ in the average: $Propensity_i = \sum_{k \neq i} \hat{\mu}_k$.

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 characteristic or patient sorting to facilities, 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 physician, relative to other physicians in the same outpatient facility and year, controlling for seasonality, time of day, patient age, race, gender and diagnosis at first encounter.

The prescriber tendency $Propensity_i$ is chosen to be time-invariant, because, empirically, we observe large auto-correlation in physician propensity across time and prefer a parsimonious measure. However, to accommodate cases in which providers substantially change their prescribing behavior, we also construct year-varying provider propensities with "drift", letting the data inform us on how the propensity in a given year depends on other years.[16] The construction of this year-varying measure is described in Appendix A and the results are reported in Table B.5.[17]

3.2 Assessing Quasi-Random Assignment and Propensity

With our physician propensity measure constructed, the natural next step is to test whether our quasi-random assignment holds at the patient-physician assignment level. That is, do lenient and strict physicians treat the same case mix of patients conditional on facility-year, year-month, day of week and time of day, diagnosis, and gender? Figure 2 tests this assumption graphically. The left figure plots the regression output for a single regression of $Prescribed_i$ on patient demographics and prior year medical history. As expected, these covariates jointly explain prescription status. However, as seen in the right figure, these covariates do not explain physician propensity. In fact, the joint F-statistic drops about 40-fold from 59.8 to 1.4. In sum, we selected veterans that are assigned PCPs at facilities that appear to randomize with respect to *prior* opioid use, predicted by a set of rich patient covariates. These same covariates also do not explain physician propensity to prescribe opioids. Therefore, we do not find evidence of any violations of quasi-random assignment on observables for both opioid status prior to and at the index primary care visit.

[16] Similar to the teachers value-added literature with empirical Bayes shrinkage: Chetty et al. (2014); Kane and Staiger (2008).

[17] To summarize, the main outcomes are qualitatively and quantitatively robust to using a year-varying propensity measure. In fact, some outcomes are actually measured with more precision and are larger in magnitude, such as OUDs, opioid overdoses, accidental falls, and depression.

Figure 3 graphs the 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 physician propensity after residualizing is overlaid and displayed on the right y-axis. The histogram displays substantial variation in the prescription rate among physicians working in the same facility, treating similar patients (in terms of observable demographic characteristics) and identical diagnoses. The standard deviation of $Propensity_i$ is 1.85pp.[18] The difference between the average physician at the 90th percentile of prescribing propensity and the 10th percentile—within the same facility—is 3.03pp. The difference between 75th and 25th percentiles is 1.53pp. While the overall mean opioid prescription rate across all first encounters is 3.1%, approximately 19% of all first encounters are associated with a prescriber whose propensity measure is at least two percentage points above or below the residualized facility mean. The first column in Table 2 presents the regression table analog of the local linear graph. The association between first encounter opioid prescription and physician propensity is strong. Being assigned to a 3 percentage point more lenient PCP (relative to the average in that facility-year, year-month, day of week, diagnosis, and gender) is associated with a 2.15 percentage point increase in the likelihood of being prescribed an opioid at first encounter with the PCP. The average veteran in our sample visits their assigned PCP 15 times throughout the sample period (see Table 1), and prescribing propensity also affects the likelihood of being prescribed an opioid for subsequent visits. This finding suggests that a physician's propensity to prescribe opioids may have prolonged effects on patient health. It is the basis for studying long-term patient outcomes in section 5.

3.3 How Are Lenient Opioid Prescribers Different?

With our quasi-random assignment assumption, we can 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

[18] 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.

influence patient outcomes. In other words, if physicians 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 physician practices do differ, other mechanism may be at play.

In section 6 we dive into the mechanisms, but here we begin with a simple descriptive exercise on lenient versus strict physicians. We obtain a single propensity for each provider by averaging across the residuals in Equation 3 without the leave-out step, then we categorize providers as lenient or strict if their propensity measure falls in the top or bottom quartile, respectively. Correlating this measure with provider characteristics, we document that lenient prescribers are much more likely to be male than strict physicians (49.1% versus 37.6%) and similar in age (see Table B.4). Consistent with findings among Medicare beneficiaries (Muench et al., 2019), lenient prescribers are 19 percentage points more likely to be physicians (M.D. or D.O.), while strict prescribers are equally more likely to be nurse practitioners or physicians assistants. Lenient physicians tend to work two fewer days per year than strict physicians, yet treat more patients. They see one additional patient per day, a 6.4% increase.

4. Empirical Specification

We study the effect of physician propensity on patient outcomes by estimating the following linear equation at the patient level:

$$Y_i = \beta Propensity_i + \theta X_i + \alpha_{hosp \times year} + \alpha_{year \times month} + \alpha_{dayofweek} + \alpha_{diag} + Gender_i + \epsilon_i, \quad (4)$$

where Y_i is the outcome of interest for patient i, which is either a one-year outcome or three-year outcome following the date of the index visit. $Propensity_i$ is the opioid prescribing tendency of the patient's PCP, and X_i are our baseline controls which include age, marital status, log income, Medicare/Medicaid flag, prior Elixhauser comorbidity index, and prior mental health diagnosis. The fixed effects α represent the level at which patients are randomly assigned to physicians (as verified in the previous section). For parsimony, we also report

regression outputs without the baseline controls in each of our regression output tables.

The parameter of interest is β, which represents the change in outcome Y_i when a veteran's PCP's propensity to prescribe $Propensity_i$ increases by one (or 100pp). Under the identifying assumption that physicians are assigned to patients quasi-randomly, conditional on the fixed effects in Equation 4, then β can be interpreted as the causal effect of physician propensity on veteran outcomes. We consider potential threats to this assumption in subsection 5.5. Note that the interpretation is *not* necessarily the causal effect of receiving opioid prescriptions itself.

Except for when the outcome is mortality, all regression samples are restricted to veterans who are alive for the full outcome period of study. This should not introduce any biases because later we will show that we do not find any significant effects on overall veteran mortality. Robust standard errors are clustered at the physician level. All regression coefficients are scaled by a factor of $0.03 \times 100\text{pp}=3\text{pp}$, so the interpretation is as follows: If a veteran is assigned a PCP with a 3pp higher propensity to prescribe opioids—this means they prescribe on average 3pp more than their facility average, treating the same patient (VHA-wide first-visit prescription average is 3.1%)—they are $3 \times \hat{\beta}$ percentage points more likely to exhibit outcome Y_i. The difference between the 90th and 10th percentile providers within the same facility is 3.03pp.

5. Results

5.1 Prescription Opioid Use

Table 2 presents our prescription opioid use results. Columns 1 and 2 report regression coefficients for prescription status at the index visit (this is the first stage or validity of the instrument if we were using an instrumental variable approach), 3 and 4 for long-term opioid use defined as 90 days supply over the first year, and 5 and 6 for 180 days supply. Being assigned a 3pp higher prescriber (equivalent to the difference in prescribing rates between PCPs in the 90th versus 10th percentiles) leads to an increased probability of being prescribed

opioids at first visit by 2.15pp, 90 days supply of long-term use (excluding the initial visit) by 1.33pp and 180 days supply of long-term use by 0.72pp. To be consistent with Barnett et al. (2017, 2019); Eichmeyer and Zhang (2020), henceforth, long-term prescription opioid use will refer to the more stringent 180 day definition. The coefficients are not sensitive to including the set of baseline controls.

To put these magnitudes in perspective: The 0.72pp difference in long-term prescription opioid use stemming from a 3pp difference in prescribing propensity, is a 32% increase on the base long-term rate of 2.25%. This effect size is large in magnitude and likely associated with a large increase in own consumption of prescripiton opioids (as opposed to accounted for mainly by increased prescription opioid diversion (Schnell, 2017)), because findings in Section 5.2 show increased misuse, abuse, and worse mental health. A veteran's long-term opioid use is thus to a substantial extent determined by their primary care provider. This is consistent with findings in Barnett et al. (2017); Finkelstein et al. (2018); Laird and Nielsen (2016); Eichmeyer and Zhang (2020), where place-based factors play a substantial role. Policies targeting supply-side factors could thus have a substantial influence on opioid-use trajectories of opioid-naive patients. We will return to this at the end of this paper.

How does having a high prescribing PCP impact the source of a veteran's opioids? In Table 3 we investigate the share of all opioids filled in the first year (again, excluding the initial visit) that are prescribed by the veteran's assigned PCP. On average, for veterans who have some opioid use in the first year, 41% of their opioid supply, measured in both days supply or milligrams of morphine equivalence, are from their assigned PCP. This proportion is broadly in line with national estimates from Levy et al. (2015). The share increases by 11pp for a 3pp increase in PCP propensity. A high prescribing PCP not only increases the total amount of long-term opioid use, they are also responsible for a greater share.

The association between a PCP's prescribing propensity and long-term prescription opioid use is an order of magnitude *larger* than the equivalent one found for the case of ED

physicians in Eichmeyer and Zhang (2020).[19] This is not surprising given the impat measured in the ED-based paper is that of a single short-term prescription originating from one ED visit, whereas the average veteran-PCP relationship in our sample spans 15 encounters. Therefore, any elevated exposure to opioid risk in primary care due to a high prescribing PCP is compounded relative to short-term exposure due to a high prescribing physician in an emergency department. Our estimate is broadly in line with the magnitudes found in Laird and Nielsen (2016) for primary care settings in Denmark: They find a 4.5pp increase in the propensity to use *any* prescription opioids in a given year in association with a 10pp increase in a patient's PCP's opioid prescribing rate.

5.2 Opioid Dependence, Misuse, and Abuse

Long-term use does not necessarily indicate dependence, misuse, or abuse. For instance, a veteran could perfectly follow the instructions of their physician and pharmacist and take prescription opioids over a long period of time, but in modest amounts. Table 4 reports the regression table for the set of misuse and abuse outcomes. A PCP with a 3pp higher propensity to prescribe opioids increases the three-year diagnosis rate of an opioid use disorder by 0.037pp on a base of 0.62% (a 6% increase). It means that ca. 5% of veterans who classify as long-term users—based on their filled prescriptions—because of their primary care provider, also develop an opioid addiction. To the extent that opioid use disorders are under-diagnosed relative to the degree of under-counting of long-term prescription opioid use (e.g., more patients are hiding their addiction from their doctors than the amount of unobserved pharmacy prescriptions), this fraction is an underestimate of the true probability of developing an OUD conditional on using opioids long-term. In fact, when we restrict our attention to OUDs diagnosed by other clinicians—excluding their assigned PCP—the coefficient drops ever so slightly to 0.031pp (Table B.3).

[19] The 2SLS and first-stage estimates in Eichmeyer and Zhang (2020) are 1.17 and 1.71 respectively. The reduced form estimate is 2.0; thus, in the ED setting, a 3pp increase in an ED physician's tendency to prescribe (for certain diagnosis groups in that sample) is associated with a 0.06 pp increase in long-term use. Moving from the top decile to bottom decile of ED physician leniency (a much larger jump) is associated with a 0.23pp increase in long-term use, a 4.1% increase.

While opioid use disorder is defined as a "problematic pattern of opioid use leading to clinically significant impairment or distress", it is unclear whether a veteran's physical health is impacted. In columns 3-8, we find that a 3pp increase in PCP propensity leads to an increased probability of opioid overdose by 0.006pp, accidental falls by 0.04pp, and vehicle accidents by 0.03pp. The latter two outcomes are adverse events that are more likely to occur when one is sedated and has been found to be predictive of opioid overdose risk. The regression coefficients corresponding to opioid overdoses and accidental falls are not statistically significant. For overdoses, this is presumably because overdoses occur less frequently than opioid use disorders and are also often more life-threatening. This means opioid overdoses may be observed less frequently in the VHA data if veterans go to a non-VHA emergency department and fail to file a reimbursement claim within 30 days. In Table B.5, when we use a year-varying physician propensity, the effect on opioid overdoses is statistically significant at the 10% level and increases to 0.011pp per 3pp increase in propensity. Nevertheless, together these results show that veterans are not simply becoming long-term users of prescription opioids, but some are placed onto troubling paths of prescription opioid use and misuse that can lead to adverse events such as overdoses, falls, and accidents.

5.3 Utilization, Health, and Economic Outcomes

Table 5 displays the regression results on health care utilization in primary care, emergency department, and inpatient settings. First, there is no discernible difference in the number of primary care encounters in the first year; in fact, the coefficient is slightly negative. Next, in our preferred specification, being assigned to a 3pp higher propensity PCP leads to a 0.3pp higher likelihood of utilizing a VHA emergency department and 0.15pp of being hospitalized within three years. Some of this effect can be directly related to opioid related adverse events but, but the total effect size exceeds that on opioid overdoses, accidental falls, and vehicle accidents combined. There are many potential reasons why having a lenient opioid prescriber can lead to increased non-primary care utilization, one of which may be worse overall health.

Towards this end, we investigate how PCP propensity affects health and economic

outcomes and find that exposure to prescription opioids negatively affects health, especially mental health (Table 6). Continuing to frame the effects in terms of a 3pp increase in a PCP's propensity to prescribe opioids, suicide attempts and self-harm episodes increase by 0.023pp and diagnosis of major depressive disorder increases by 0.18pp. Recall that neither suicide nor depression in the year prior to assignment had any predictive power with respect to physician 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. The effect on attempted suicide is on a base of 0.36, so approximately a 7% increase.

To the best of our knowledge, this is one of the first papers to establish a causal effect between exposure to prescription opioids via a PCP's propensity and its detrimental effects on patients' mental health. The link between lenient prescribing and poorer mental health may be through opioid use and abuse, or worse treatment for substance abuse. In section 6, we explore into the mechanisms and find evidence of lenient PCPs referring to substance abuse treatment at lower rates. These results are especially important because amidst two decades of increases in suicides in the United States, suicide rates among veterans are one and a half times greater than non-veterans, prompting President Donald Trump to sign an executive order in 2019 to form a veteran suicide prevention task force (Executive Office of the President, 2019). Moreover, pain, addiction, and mental despair are connected themes that plague working class Americans (Case and Deaton, 2020).

There also appears to be some substitution between opioid use/abuse and alcohol abuse. Diagnosis of alcohol use disorders decrease by 0.18pp; this is in contrast to the 0.72pp increase in long-term use and 0.037pp increase in opioid dependence.

5.4 Mortality

Finally, we turn our attention to veteran mortality. Each day 7.5 veterans die from opioid overdoses and 20 from suicide (Peltzman et al. 2020; Executive Office of the President, 2019). Here we study whether physician prescribing tendency is associated with veteran deaths.

Table 7 displays the outcome for various causes of one-, three-. and five-year mortality.

Overall veteran mortality, perhaps reassuringly, is not correlated with a primary care provider's opioid-prescribing propensity. With a 95% confidence interval, the five-year overall mortality effect from going from a 10th percentile to 90th percentile prescriber is no more than 1.2% of the mean dependent variable. However, when we focus on deaths from drug overdoses, which make up a very small fraction of all deaths, the story completely changes. There is a 0.0044pp, 0.0088, and 0.0129pp increase in one-year, three-year, and five-year mortality from drug overdoses associated with a 3pp increase in primary care prescribing tendency. This is an increase in about a third over the mean three and five-year drug mortality rates. In fact, the effect sizes on drug overdose mortality are roughly the same magnitude as all-cause mortality.

Much of this effect on drug mortality is concentrated in an effect through opioid overdoses. Opioid overdose deaths within five years increase by 0.008pp, on a base of 0.033, a 25% increase; one-year and three-year opioid overdose mortality are positive and roughly what one would expect given the five-year estimate but not statistically significant. By specifically focusing on heroin and synthetic opioids, we can have a better understanding on whether prescription opioids can lead to illicit opioid use. Albeit not significant, the effect on deaths from heroin or synthetic opioids are roughly 40% the overall opioid overdose effect, suggesting that there is some transition to illicit use (consistent with Eichmeyer and Zhang (2020)).

We do not find a statistically significant increase in suicide mortality. This finding contrasts with our finding of a significant increase in diagnoses for attempted suicide. There are two main explanations for this pattern: i) Statistical power-issues, associated with suicide mortality being a very low probability event; ii) opioid abusers are more likely to die of drug overdoses and the majority of suicide attempts fail or overdose deaths *by suicide* are difficult to ascertain as a cause of death. Looking at other common sources of veteran deaths including chronic liver disease,[20], heart disease, chronic lower respiratory disease, and cerebrovascular

[20]Combining drug overdoses, suicide, and chronic liver disease into a "deaths of despair" category (Case and Deaton, 2015) does not yield a statistically or economically significant coefficient. This is perhaps consistent with their hypothesis that it is a plethora of factors contributing to the decline in the well-being of middle-aged, non-hispanic whites and not simply the opioids via PCPs.

disease, it does not appear that high prescribing PCPs are influencing non-drug related deaths (in the case of heart disease which is statistically significant, the effect is only 2% of overall mortality rate from heart diseases). This is reassuring because to the first-order, our findings are picking up an association that operates through opioid prescriptions, and not selection or overall physician abilities.

5.5 Addressing Threats to Identification

The key identifying assumption to interpret our estimates as the causal effect of being assigned a high prescribing PCP on the outcomes of interest is that providers are quasi-randomly assigned to patients. While we have empirically selected facilities that display evidence of quasi-random assignment, focused on opioid-naïve patients who are less likely to be "doctor shopping" for opioids, and shown balance along patient observables, this assumption may still be violated. Namely, by patient-physician 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 some veterans are new beneficiaries, and hence we do not fully observe their prior morbidities. To test whether this is a concern, we take advantage of the fact that roughly 300,000 veterans have been VHA beneficiaries for at least 12 months prior to the initial PCP assignment or have prior year Medicare or Medicaid claims. For these veterans, we can conduct "placebo" tests by checking whether physician propensity impacts prior year measures of our main outcomes. The regression results are reported in Table 8. The majority of the prior year outcomes are not significant, prior year suicide attempts is significant, but actually in the opposite direction. We have also controlled for prior mental health in all our results presented earlier. It is also worth noting that provided a veteran has VHA benefit history, their prior opioid use status is very likely to be perfectly observed. This is because VHA drug benefits are very generous and the VHA is seen as a national model for affordable drug prices (Good

and Valentino, 2007; Gaffney et al., 2020).[21]

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. However, if instead the findings were at least partially driven by provider propensity, then the effect sizes should be increasing with the number of encounters. To test this relationship, we conduct our main regressions as in Equation 4 but now include 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 (e.g., one may need to return if care provided was inadequate), in subsection 5.3 we find that number of visits is not correlated with propensity, and this exercise can still yield some descriptive insight. Table 9 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. With the exception of emergency visits, and 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.[22]

Even if initial PCP assignment is quasi-random, veterans can choose to switch physicians. For example, if a veteran is seeking opioids and their assigned PCP refuses to prescribe them any, the veteran may switch to a more lenient doctor. Note that such endogenous switching would bias our estimates *towards zero*. Moreover, in our sample, 90% of initial patient-PCP relationships last at least two months and 75% 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). Veterans assigned to 3pp higher prescribing PCP tend to stay with their PCP for three additional weeks. There does not seem to be a relationship with respect to number of switches. Table 10 displays the outcome of a regression of relationship length in

[21] There is no premium and no coinsurance for prescription drugs in the VHA, only a copayment for certain priority groups. The most expensive copayment for a 30-day supply of brand name prescription drugs for the lowest priority group was capped at $2 prior to 2002, $7 prior to 2006, $8 prior to 2010, $9 prior to 2017, and $11 after 2018 (Stroupe et al. (2007), VHA Health Benefits, 2018).

[22] It is worth noting that more primary care encounters may mechanically lead to more diagnoses for some outcomes such as depression, but not for opioid overdoses which are primarily treated in EDs.

days on physician propensity, and number of switches on physician propensity. 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 11 and are generally qualitatively similar to our main results, albeit the standard errors are much larger.

Finally, if the concern is our selected set of facilities are unique in some way that introduces biases, Table B.6 presents the main outcomes for the set of all facilities. The estimated effects on long-term use, ED visit, accidental falls, depression, and suicide attempts are qualitatively consistent with our main results while OUD, overdoses, and overdose mortality are positive but much noisier.

6. Interpretation and Mechanisms

We find that veterans who are quasi-randomly assigned to higher prescribing PCPs and hence exposed to higher opioid prescribing risk—likely presistent—are more likely to experience a host of detrimental outcomes. Our findings on long-term prescription opioid use are roughly in line with Laird and Nielsen (2016) and Finkelstein et al. (2018) who also find increased opioid use and abuse stemming from prescribing variation. Our long-term use estimates are about one order of magnitude larger than the ones found in Eichmeyer and Zhang (2020). This is driven by the fact that the latter estimates are for a single ED visit, whereas exposure to elevated prescribing risk in primary care is persistent over multiple visits.

To shed light on the causal mechanisms driving our findings, we study physician behavior with other pain and opioid-related decisions. Namely, are higher opioid-prescribing providers more or less likely to refer to alternate forms of pain treatment including pain clinics and community integrative health clinics (e.g., acupuncture, massage therapy);[23] what about prescribe naloxone? These two also serve as proxies for adherence to clinical recommendations

[23]Pain clinics are multidisciplinary outpatient facilities that specialize in treating pain, usually in a team-based approach. It usually includes generalists and specialists including occupational therapist, physical therapist, rehabilitation specialists, psychologist/psychiatrist and often includes acupuncturists or yoga specialists.

because by fiscal year 2014, the VHA started recommending complementary non-opioid treatment for pain, and implemented an Overdose Education and Naloxone Distribution (OEND) program (GAO, 2018; Oliva et al., 2017b). Since we do not observe whether non-opioid treatment or naloxone prescriptions are appropriate at the encounter level, we construct ratios of referrals to non-opioid treatment or naloxone prescriptions to the total number of opioid prescriptions prescribed by a given physician, each year. Note that our earlier findings indicate that high prescribers will treat more opioid dependent patients, and in turn prescribe more naloxone mechanically; selection on sicker patients reinforce this effect. We run regressions of the ratio of number of non-opioid treatment referrals to total number of opioid prescriptions and number of naloxone prescriptions relative to the total number of opioid prescriptions at the physician-year level on physician propensity to prescribe opioids, and find the opposite (Table 12): Higher opioid prescribers prescribe opioids as a *substitute* for non-opioid therapy and higher prescribers are *less* likely to prescribe naloxone. For every 100 opioid prescriptions prescribed, the average physician refers to non-opioid treatment 6.8 times. This number drops by almost 0.9 for every 3pp increase in physician propensity. Naloxone prescriptions tell a similar, albeit less precisely estimated story. For every 100 opioids prescriptions (prescription over 100 milligrams of morphine equivalence daily-MEDD[24]), the average physician prescribes 0.15 (3.26) naloxone kits. Again, for every 10pp increase in physician propensity, the number of kits distributed drops by 0.005 (0.204). Although this exercise is purely descriptive, it is consistent with evidence that high prescribers are less likely to consider alternate pain management strategies and adhere to VHA clinical guidelines.

Patients of high prescribers are more likely to misuse opioids and develop dependence. This raises the question of how PCPs differ in recognizing opioid misuse and abuse, and who is diagnosing these patients with OUDs? Is it the original prescriber herself? Of the almost 5,400 veterans in our sample who develop an OUD within three years, only 3.5% have

[24]Milligrams of morphine equivalence is the standard unit to measure opioid dosage intensity. The cutoffs of 20, 50, and 100 are taken from a screening risk index (RIOSORD) that the VHA developed as a tool to help physicians screen for opioid overdose risk and aid in the naloxone prescription decision.

the condition diagnosed by their assigned PCP. The vast majority are through non-VHA settings, inpatient settings (e.g., admission for overdose), and substance abuse and mental health clinics (e.g., either through referral from another physician or self-admissions).[25] Although the vast majority of OUDs are not diagnosed by the PCP, it could be the case that the PCP recognizes an issue and refers the patient to a psychiatrist or specialist that ultimately diagnoses them. To study PCP ability in recognizing opioid abuse, we construct ratios of OUD diagnosis and referrals to substance abuse screen/treatment (e.g., outpatient, inpatient residential/domiciliary, social work screens, etc.) relative to the total number of opioid prescriptions at the physician-year level. We then run the same regressions as for pain management choices of PCPs and report the results in Table 13. For every 100 opioid prescriptions the average PCP writes, they diagnose 0.29 OUDs and make a 7 substance abuse referrals. Providers who prescribe opioids at a 3pp higher rate, on average diagnose 0.085 fewer OUDs and make 1.31 fewer referrals to substance abuse treatment per 100 opioids they prescribe. This result is shocking because higher prescribers should see more patients with opioid use disorder, because these PCPs are more likely to cause patients to become dependent.

7. Conclusion

In this paper, we study the effect of exposure to a leniently opioid-prescribing PCP on measures of patient outcomes falling into categories of patient long-term opioid use, misuse, physical and mental health, and mortality. We leverage quasi-random assignment of patients to primary care providers in the Veterans Health Administration, in conjunction with variation in prescribing tendency—within clinical guidelines—across primary care physicians within the same outpatient facility. Consistent with Barnett et al. (2017, 2019), and Eichmeyer and Zhang (2020), we find a positive association between exposure to higher prescribing providers and long-term opioid use and abuse, and mortality (with the latter paper). While

[25] Two thirds of OUD diagnoses for our sample are non-VHA facilities or VHA inpatient facilities. Of the VHA outpatient facilities, substance abuse and mental health clinics make up the majority share.

their research studies opioids prescribed in emergency departments, our paper departs from their short-term setting by studying the frequent, long-term relationships between clinicians and patients in primary care.

In recent work, Case and Deaton (2017, 2020) draw attention to not only the physical pain that the working class cope with, but also the "distress, and social dysfunction" that can also lead to deteriorating mental health. Along with Finkelstein et al. (2018) and Laird and Nielsen (2016), we have shown that primary care providers play an instrumental role in a patient's opioid use and abuse; moreover, we also study how opioid prescribing risk can influence veteran's mental health. We find that the patients of lenient opioid prescribers have higher rates of attempted suicide and self-harm, and diagnosis of major depressive disorder. This finding particularly underscores the importance of primary care providers. They are not only a frequent point of contact for individuals suffering through pain, but they are often also a gatekeeper for mental health and social work services. This suggests that team-based models of patient care that do not heavily rely on one provider, in conjunction with evaluating pain and physical and mental health as a comprehensive, connected system, may achieve better outcomes (Gatchel et al., 2007; Jackson et al., 2013).

Investigating into the mechanisms that are responsible for the link between opioid prescribing and adverse patient outcomes, we find that higher opioid prescribers not only induce higher rates of opioid abuse among their patients, but are also less likely to adhere to new opioid clinical recommendations (e.g., in recent years the VHA has been encouraging non-opioid pain therapy and naloxone distribution) and are less likely to recognize, and thus treat opioid dependence. This suggests that our results are driven by a physician's broad attitude and knowledge towards opioids and pain management. Addressing the opioid epidemic begins with policies targeting prescribing, but it does not end there. It is paramount that physicians prescribe more appropriately, consider alternate treatments, recognize when a patient is at risk, and adhere to changing opioid policies. It is encouraging that physician behavior is malleable and policies addressing physician education (Schnell and Currie, 2018), which have already been shown to be successful (Bounthavong et al. (2017) find that academic detailing

resulted in increased naloxone prescribing), may be an effective policy tool to curb the opioid epidemic.

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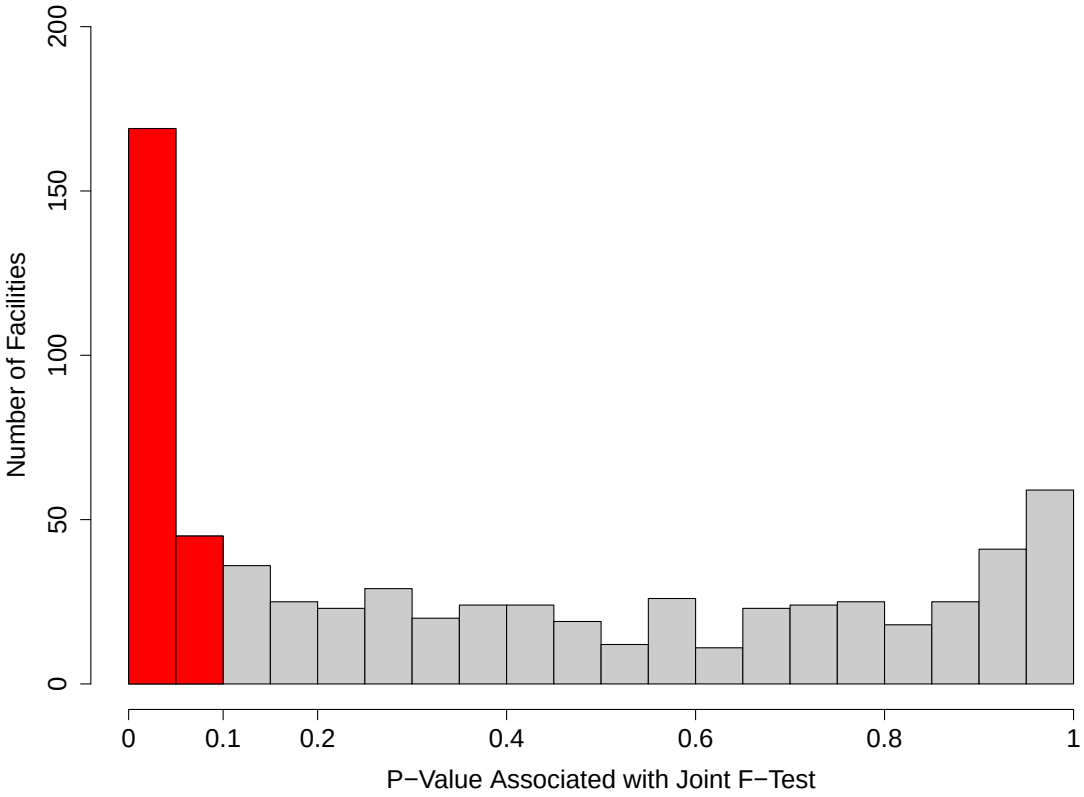
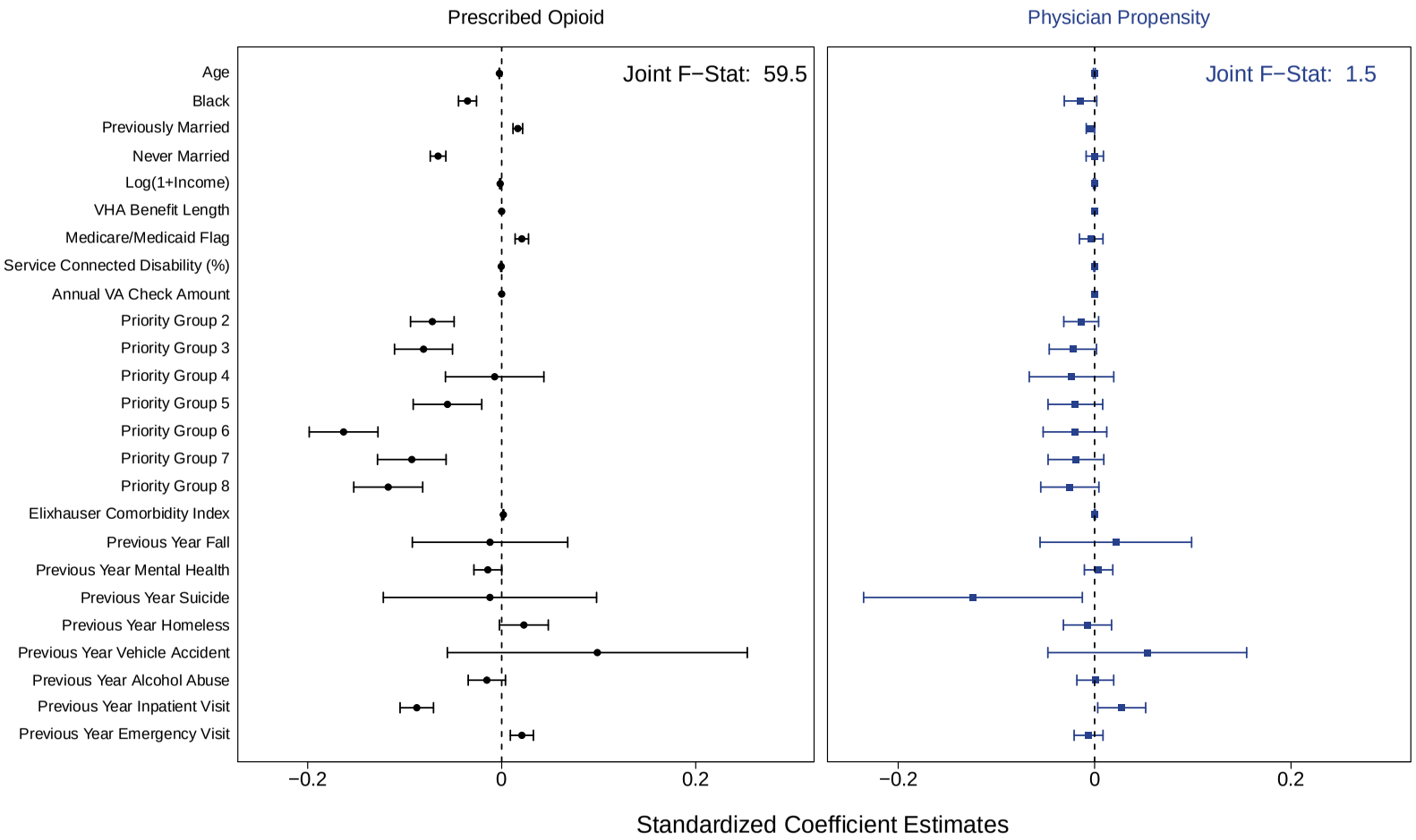
Figures

Figure 1: Outpatient Facilities that Quasi-Randomly Assign Physician to Patients



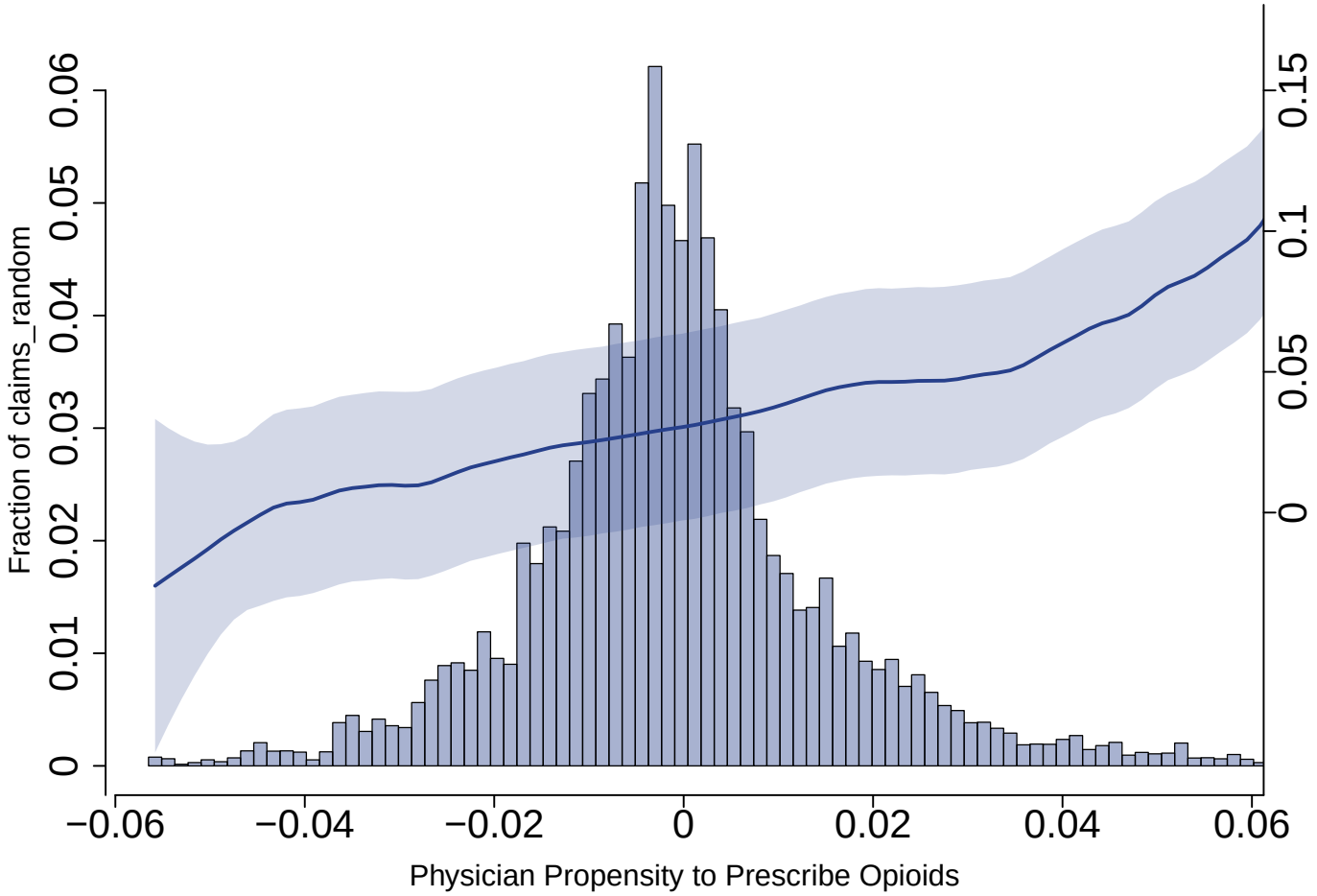
Notes: This figure plots the histogram of the p-values associated with a joint F-test of all PCP fixed effects explaining predicted prior opioid use, separately for each facility. See subsection 2.3 for more details. The bars shaded red represent facilities that fail this joint F-test (at the 0.10 significance level); these facilities non-randomly assign PCPs to patients (214 facilities). The remaining facilities display empirical evidence of quasi-random assignment (464 facilities).

Figure 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 physician 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 physician level.

Figure 3: Distribution of Physician Propensity to Prescribe Opioids and Mean (Residualized) Prescription Rate



Notes: This figure plots the histogram of PCP propensity to prescribe opioids along the x-axis and the left y-axis. A local-linear regression of the fitted probability of being prescribed opioids on the PCP propensity after residualizing (precise fixed effects and controls are described in the text) is overlaid and displayed on the right y-axis. 95% confidence bands are also shown.

Tables

Table 1: Summary Statistics for Baseline Sample

	Q1	Median	Mean	Q3
Female	-	-	0.052	-
Black	-	-	0.10	-
Age	50	61	59	71
Married	-	-	0.61	-
Service-Connected Disability?	-	-	20.9	-
Income	3,106	20,000	29,651	42,000
Medicaid or Medicare?	-	-	0.455	-
Number of Dependents	0	1	0.724	1
Prescribed Opioid at First Visit	-	-	0.031	-
Number of Visits with Assigned PCP	4	12	15.1	35
Prior Year Medical History:				
Hospitalization	-	-	0.011	-
Emergency Department	-	-	0.049	-
Mental Health	-	-	0.026	-
Homeless	-	-	0.009	-
Alcohol Abuse	-	-	0.013	-
Opioid Prescription (Conditional on Prescribed):				
Mg of Morphine Equivalence/Day (2000)	9	15	23.4	25
Mg of Morphine Equivalence/Day (2011)	10	15	20.4	22.5
Days Supply (2000)	25	30	26	30
Days Supply(2011)	30	30	33.8	30
Refillable? (2000)	-	-	0.704	-
Refillable? (2011)	-	-	0.640	-

Notes: This table displays summary statistics for demographics, prior year medical history, and opioid prescriptions for our baseline sample of veterans as described in the text. All variables with only the mean reported are indicator variables. .

Table 2: Opioid Use Outcomes

	Dependent variable (×3):					
	Prescribed at Index Visit		Long-Term Prescription Opioid Use 90 Days		180 Days	
	(1)	(2)	(3)	(4)	(5)	(6)
Propensity	2.149***	2.150***	1.328***	1.326***	0.717***	0.715***
	(0.079)	(0.078)	(0.060)	(0.061)	(0.042)	(0.043)
Residualization FEs?	Yes	Yes	Yes	Yes	Yes	Yes
Baseline Controls?	No	Yes	No	Yes	No	Yes
Mean Dep. Var. (×100)	3.06		4.02		2.25	
Observations	959,088		939,912		939,912	

Notes: This table reports the output of various regressions of opioid use outcomes on physician opioid prescribing propensity based on regression model from Equation 4. Prescribed is an indicator for receiving an opioid prescription at the first index primary care visit with their assigned physician. Long-term use is defined as of the year following a patient's initial primary care visit. Coefficients and standard errors are scaled by $3 = 0.03 \times 100$, to represent the difference between the 90th and 10th percentile prescribers: 3pp. All regressions include the residualization fixed effects and even columns include baseline controls; both are described in the text. Standard errors are clustered at the provider level and reported in parentheses. 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 (×3):			
	Share Total Days Supply		Share Total MME	
	(1)	(2)	(3)	(4)
Propensity	10.6***	10.5***	10.7***	10.7***
	(0.4)	(0.4)	(0.4)	(0.4)
Residualization FEs?	Yes	Yes	Yes	Yes
Baseline Controls?	No	Yes	No	Yes
Mean Dep. Var. (×100)	41.1		40.8	
Observations	115,989		115,989	

Notes: This table reports the output of share of total days supply (or total milligrams of morphine equivalent) that is prescribed by their assigned PCP in the first year, conditional on any opioid use that year, on physician opioid prescribing propensity based on regression model from Equation 4. Coefficients and standard errors are scaled by $3 = 0.03 \times 100$, to represent the difference between the 90th and 10th percentile prescribers: 3pp. All regressions include the residualization fixed effects and even columns include baseline controls; both are described in the text. Standard errors are clustered at the provider level and reported in parentheses. All samples are constrained to be alive during the outcome period. *p<0.1; **p<0.05; ***p<0.01.

Table 4: Opioid Dependence, Misuse, and Abuse Outcomes

	Dependent variable (×3): 3-Year Outcomes							
	Opioid Use Disorder		Opioid Overdose		Accidental Falls		Vehicle Accident	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Propensity	0.038**	0.037**	0.006	0.006	0.040	0.038	0.030*	0.030*
	(0.018)	(0.016)	(0.006)	(0.006)	(0.026)	(0.025)	(0.018)	(0.018)
Residualization FEs?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Baseline Controls?	No	Yes	No	Yes	No	Yes	No	Yes
Mean Dep. Var. (×100)	0.62		0.078		1.646		0.654	
Observations	893,739		893,739		893,739		893,739	

Notes: This table reports the output of various regressions of opioid dependence, misuse, and abuse outcomes on physician opioid prescribing propensity based on regression model from Equation 4. All four outcome variables are indicators based off of 3-year post-visit diagnoses described in the text. Coefficients and standard errors are scaled by $3 = 0.03 \times 100$, to represent the difference between the 90th and 10th percentile prescribers: 3pp. All regressions include the residualization fixed effects and even columns include baseline controls; both are described in the text. Standard errors are clustered at the provider level and reported in parentheses. All samples are constrained to be alive during the outcome period. *p<0.1; **p<0.05; ***p<0.01.

Table 5: Health Care Utilization and Setting

	Dependent variable:					
	PCP Visit Count (1 Yr)		Emergency (3 Yrs)		Inpatient (3 Yrs)	
	×0.03		×3		×3	
	(1)	(2)	(3)	(4)	(5)	(6)
Propensity	−0.02	−0.02	0.30***	0.30***	0.16**	0.15**
	(0.02)	(0.02)	(0.10)	(0.09)	(0.07)	(0.06)
Residualization FEs?	Yes	Yes	Yes	Yes	Yes	Yes
Baseline Controls?	No	Yes	No	Yes	No	Yes
Mean Dep. Var. (×100)	3.8		17.1		9.7	
Observations	939,911		893,739		893,739	

Notes: This table reports the output of various regressions of number of primary care encounters, emergency department and inpatient hospitalization utilization on physician opioid prescribing propensity based on regression model from Equation 4. PCP visit count is the number of visits the veteran has with their initially assigned PCP in the first year. Emergency and inpatient are indicators for any ED visit or inpatient hospitalization in the three years following PCP assignment. Coefficients and standard errors in columns 3-6 are scaled by $3 = 0.03 \times 100$, to represent the difference between the 90th and 10th percentile prescribers: 3pp. All regressions include the residualization fixed effects and even columns include baseline controls; both are described in the text. Standard errors are clustered at the provider level and reported in parentheses. All samples are constrained to be alive during the outcome period. *p<0.1; **p<0.05; ***p<0.01.

Table 6: Health and Economic Outcomes

	Dependent variable (×3): 3-Year Outcomes							
	Suicide Attempt/ Self-Harm		Depression		Homelessness		Alcohol Abuse	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Propensity	0.024**	0.023**	0.185	0.179*	−0.078**	−0.071*	−0.175**	−0.175**
	(0.012)	(0.012)	(0.124)	(0.101)	(0.040)	(0.039)	(0.074)	(0.069)
Residualization FEs?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Baseline Controls?	No	Yes	No	Yes	No	Yes	No	Yes
Mean Dep. Var. (×100)	0.36		19.3		3.1		8.1	
Observations	893,739		893,739		893,739		893,739	

Notes: This table reports the output of various regressions of attempted suicide (excluding ideation), major depressive disorder, proxy for homelessness, and alcohol abuse on physician opioid prescribing propensity based on regression model from Equation 4. Construction of outcome variables are described in subsection 2.4. Coefficients and standard errors are scaled by $3 = 0.03 \times 100$, to represent the difference between the 90th and 10th percentile prescribers: 3pp. All regressions include the residualization fixed effects and even columns include baseline controls; both are described in the text. Standard errors are clustered at the provider level and reported in parentheses. All samples are constrained to be alive during the outcome period except for mortality. *p<0.1; **p<0.05; ***p<0.01.

Table 7: Mortality and Cause of Death

	Dependent variable (×3):					
	One Year		Three Year		Five Year	
	Mean Dep. Var. (×100)	Estimate (×3)	Mean Dep. Var. (×100)	Estimate (×3)	Mean Dep. Var. (×100)	Estimate (×3)
Cause of Death:	(1)	(2)	(3)	(4)	(5)	(6)
All Mortality	2.196	0.0158 (0.0254)	7.014	−0.0062 (0.0493)	12.17	0.0119 (0.0719)
Drug Overdose	0.012	0.0044** (0.0022)	0.042	0.0088** (0.0044)	0.071	0.0129** (0.0053)
Opioid Overdose	0.004	0.0010 (0.0013)	0.018	0.0035 (0.0030)	0.033	0.0080** (0.0038)
Heroin or Synthetic Opioid	0.001	−0.00001 (0.0007)	0.007	0.0014 (0.0018)	0.013	0.0036 0.0023
Suicide	0.030	−0.0025 (0.0034)	0.092	0.0003 (0.0061)	0.150	0.0030 (0.0075)
Chronic Liver Disease	0.026	−0.0008 (0.0028)	0.090	−0.0076 (0.0052)	0.148	−0.0011 (0.0069)
Heart Disease	0.491	0.0163 (0.0110)	1.768	0.0175 (0.0220)	3.130	0.0634** (0.0309)
Chronic Lower Respiratory Disease	0.105	0.0058 (0.0054)	0.422	−0.0172* (0.0103)	0.782	−0.0238 (0.0154)
Cerebrovascular Disease	0.085	−0.0014 (0.0049)	0.302	−0.0069 (0.0092)	0.543	0.0043 (0.0138)

Notes: This table reports the output of various regressions of types of (one year, three year, and five year) mortality on physician opioid prescribing propensity based on regression model from Equation 4. Coefficients and standard errors are scaled by $3 = 0.03 \times 100$, to represent the difference between the 90th and 10th percentile prescribers: 3pp and the mean dependent variable is scaled by 100. Samples are constrained such that we fully observe whether a veteran has passed away within one, three, or five years. All regressions include the residualization fixed effects and even columns include baseline controls; both are described in the text. Standard errors are clustered at the provider level and reported in parentheses. *p<0.1; **p<0.05; ***p<0.01.

Table 8: Prior Year "Placebo Outcomes" for Veterans Continuously Enrolled One Year Prior or with Prior CMS Enrollment

	Dependent variable (×3):						
	Falls	Vehicle	ED	Suicide	Depression	Homeless	Alcohol
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Propensity	0.006	0.005	0.019	−0.016**	0.007	−0.035	0.024
	(0.012)	(0.007)	(0.092)	(0.006)	(0.061)	(0.035)	(0.044)
Mean Dep. Var. (×100)	0.1	0.04	5.91	0.03	3.07	1.04	1.85
Observations	298,694						

Notes: This table reports the output of regressions of prior year main "outcomes" on $Propensity_i$ for veterans who were VHA beneficiaries for at least one year prior to their index PCP visit or veterans with prior year Medicare or Medicaid claims (a placebo test). Coefficients and standard errors are scaled by $3 = 0.03 \times 100$, to represent the difference between the 90th and 10th percentile prescribers: 3pp. All regressions include the residualization fixed effects and even columns include baseline controls; both are described in the text. Standard errors are clustered at the provider level and reported in parentheses. *p<0.1; **p<0.05; ***p<0.01.

Table 9: Main Outcomes by Number of PCP Encounters

	Dependent variable (×3):							
	Long-Term Use	OUD	Overdose	Falls	ED	Suicide	Depression	5Y Opioid Mortality
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Propensity	0.008	−0.034	−0.010	0.005	0.478	0.024	−0.050	−0.003
	(0.091)	(0.046)	(0.010)	(0.058)	(0.324)	(0.034)	(0.215)	(0.010)
Propensity ×	0.485***	0.043	0.006	0.030	−0.309	−0.016	−0.005	0.002
𝟙{2-4 1st yr visits}	(0.086)	(0.051)	(0.012)	(0.068)	(0.353)	(0.042)	(0.252)	(0.012)
Propensity ×	1.635***	0.132**	0.026	0.098	−0.056	0.006	0.163	0.017
𝟙{5+ 1st yr visits}	(0.198)	(0.058)	(0.020)	(0.094)	(0.502)	(0.053)	(0.314)	(0.013)
Mean Dep Var (×100)	2.0	0.55	0.07	1.55	16.4	0.32	18.2	0.03
Observations	629,273	595,758	595,758	595,758	595,758	595,758	595,758	634,298

Notes: This table reports the output of regressions of our main outcomes on $Propensity_i$, number of primary care encounters with their assigned PCP in the first year, and an interaction of the two. Number of visits is binned into three categories: one visit (only the initial), two to four visits, and five or more in the first year. The first value is omitted. Coefficients and standard errors are scaled by $3 = 0.03 \times 100$, to represent the difference between the 90th and 10th percentile prescribers: 3pp. The samples are constrained to veterans who stay with their initial assignment for at least one year and are alive for during the outcome period (except for the mortality column). All regressions include the residualization fixed effects and even columns include baseline controls; both are described in the text. Standard errors are clustered at the provider level and reported in parentheses. *p<0.1; **p<0.05; ***p<0.01.

Table 10: Initial PCP Relationship Length and Number of PCP Switches

	Dependent variable: (×0.03)	
	Initial Relationship Length	Number of Switches
	(1)	(2)
Propensity	21.33**	−0.004
	(9.99)	(0.016)
Residualization FEs?	Yes	Yes
Baseline Controls?	Yes	Yes
Mean Dep. Var.	911.9	1.42
Observations	869,720	959,088

Notes: This table reports the output of length of relationship (in days) with the veterans initially assigned PCP and the number of PCP switches over a three year period. Initial relationships that are still current at the time this paper was written are dropped from the first regression (column 1). Coefficients and standard errors are scaled by 0.03. All regressions include the residualization fixed effects and even columns include baseline controls; both are described in the text. Standard errors are clustered at the provider level and reported in parentheses. *p<0.1; **p<0.05; ***p<0.01.

Table 11: Main Outcomes For Veterans Who Remain With Assigned PCP For At Least a Year

	Dependent variable (×3):							
	Long-Term Use	OUD	Overdose	Falls	ED	Suicide	Depression	5Y Opioid Mortality
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Propensity	0.769***	0.029	0.001	0.046	0.262**	0.017	−0.032	0.0031
	(0.052)	(0.020)	(0.007)	(0.032)	(0.103)	(0.015)	(0.117)	(0.0042)
Mean Dep Var (×100)	2.04	0.55	0.07	1.55	16.39	0.32	18.22	0.03
Observations	629,274	595,759	595,759	595,759	595,759	595,759	595,759	634,299

Notes: This table reports the output of regressions of our main outcomes based on Equation 4 for veterans who remain with their assigned PCP for at least one year (not shopping for doctors in the first year). Coefficients and standard errors are scaled by $3 = 0.03 \times 100$, to represent the difference between the 90th and 10th percentile prescribers: 3pp. The samples are constrained to veterans who are alive for during the outcome period (except for the mortality column). All regressions include the residualization fixed effects and even columns include baseline controls; both are described in the text. Standard errors are clustered at the provider level and reported in parentheses. *p<0.1; **p<0.05; ***p<0.01.

Table 12: Do Lenient Prescribers Recommend Non-Opioid Treatment and Adhere to Clinical Recommendations? Alternate Pain Treatment Referrals and Naloxone Prescriptions

	Dependent variable (×3):				
	Non-opioid referral / Opioids	Naloxone / Opioids	Naloxone / $MEDD \geq 20$	Naloxone / $MEDD \geq 50$	Naloxone / $MEDD \geq 100$
	(1)	(2)	(3)	(4)	(5)
Propensity	−0.862***	−0.005	−0.011	−0.156	−0.204
	(0.294)	(0.005)	(0.010)	(0.121)	(0.162)
Mean Dep. Var. (×100)	6.42	0.15	0.25	1.2	3.26
Observations (PCP-Yr):	4,744	4,744	4,728	4,454	4,122

Notes: This table reports the output of regressions of decisions PCPs can make (i.e., refer patient to alternate non-opioid treatment or prescribe naloxone) on physician propensity to prescribe at the physician-year level (after August 2013). The dependent variable is a ratio of referrals or naloxone prescriptions per opioid or opioid of a particular strength prescribed by the PCP in a given year. The dependent variable in column 1 is alternate non-opioid treatment include pain management clinics and complementary and integrative health (CIH) clinics such as acupuncture, massage therapy, relaxation therapy, etc. per opioid prescribed. Column 2 is number of naloxone prescribed per opioid prescribed. In columns 3-5, the denominator changes to opioids of particular strengths: milligrams of morphine equivalence daily (MEDD) above 20, 50, or 100mg. Propensity is the average across all residuals in Equation 3 at the physician level without leave-out. Coefficients and standard errors are scaled by $3 = 0.03 \times 100$, to represent the difference between the 90th and 10th percentile prescribers: 3pp. All regressions include facility and year fixed effects. The regressions are weighted by the number of cases the provider treats each year. Standard errors are clustered at the provider level and reported in parentheses. *p<0.1; **p<0.05; ***p<0.01.

Table 13: Do Lenient Prescribers Recognize Substance Abuse: Diagnosis of OUD and Referral to Substance Abuse Treatment

	Dependent variable (×3):	
	Opioid use disorder diagnosis	Substance abuse referral
	Opioids	Opioids
	(1)	(2)
Propensity	-0.085^{*}	-1.312^{***}
	(0.046)	(0.356)
Mean Dep. Var. (×100)	0.29	6.74
Observations (PCP-Yr):	23,411	23,411

Notes: This table reports the output of regressions of proxies for recognizing and treating opioid use disorder on physician propensity to prescribe at the physician-year level. The dependent variable in column 1 is the number of OUDs diagnosed by the PCP divided by the total number of opioid prescriptions they've written. Column 2 is the total number of referrals to substance abuse treatment (outpatient, inpatient, residential, social work, psychiatry screens, etc.) divided by the total number of opioid prescriptions they've written. Propensity is the average across all residuals in described in Equation 3 at the physician level without leave-out. Coefficients and standard errors are scaled by $3 = 0.03 \times 100$, to represent the difference between the 90th and 10th percentile prescribers: 3pp. All regressions include facility and year fixed effects. The regressions are weighted by the number of cases the provider treats each year. Standard errors are clustered at the provider level and reported in parentheses. *p<0.1; **p<0.05; ***p<0.01.

Appendices

A. Empirical Bayes Shrinkage

In our main analysis we construct a time-invariant measures of physician 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 3) at the physician j, year t level, leaving out the own residual, as in Eichmeyer and Zhang (2020). However, our primary care setting with new patients, this approach runs into issues with statistical power (in a quarter of the provider-years, there are fewer than 33 new patient cases). This means $Propensity_{ijt}$ will be measured with noise.

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. Chetty et al. (2014) calls this "drift" because a teacher can learn or improve from their previous teaching experiences. This is done non-parametrically where the data determines the relationship across yearly propensities, and the weights also depend on the number of observations in a given year. Specifically, for each year t, we run the following regressions:

$$Prescribed_{ijt} = \sum_{t'=1999}^{2017} \sum_{k} \beta_{kt'}^{t} \mathbb{1}\{N_{jt'} = k\} \times Propensity_{ijt'} + \epsilon_{ijt}, \tag{5}$$

where $N_{jt'}$ denote the number of new patient cases for physician j in year t'. We create four bins: 0 - 50, 50 - 100, 100 - 150, 150+. The yearly propensities are interacted with bins for number of cases, willing more weight to more precisely estimated propensities, and shrink the noisier propensities towards the facility mean, zero. This is the empirical Bayes shrinkage (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 output in Table B.5.

B. Tables

Table B.1: Regression Output of Prior Opioid Use on Veteran Observables

	Dependent variable (×100):
	Prior Opioid Use
Age	−0.138***
	(0.003)
Black	−0.913***
	(0.085)
Female	−0.217*
	(0.127)
Elixhauser Comorbidity Index	0.151***
	(0.004)
Log Income	−0.087***
	(0.007)
Prior Year Opioid Overdose	38.535***
	(2.814)
Prior Year OUD	81.076***
	(0.488)
Prior Year Fall	22.493***
	(0.565)
Prior Year Mental Health	−0.782***
	(0.132)
Prior Year Suicide	−5.997***
	(1.116)
Prior Year Homeless	−0.298
	(0.239)
Prior Year Vehicle Accident	22.736***
	(0.915)
Prior Year Alcohol Abuse	−3.710***
	(0.188)
Benefit Length	0.0002***
	(0.00003)
Medicaid or Medicare Flag	1.393***
	(0.072)
Service Connected Disability (%)	0.034***
	(0.004)
Annual VA Check Amount	0.0002***
	(0.00001)
Prior Year Inpatient Hospitalization	17.121***
	(0.166)
Prior Year Emergency Department Visit	13.701***
	(0.100)
Observations	1,003,910
Adjusted R^2	0.164

Notes: This table reports the results of regression Equation 1 where veteran observables are used to predict prior opioid use status. The following fixed effects are also included in the above regression but left out from the table: diagnosis, marital status, hour of day, day of week, PCP assignment year, visit year-month, facility-year, 3-digit patient ZIP code, and enrollment priority group. Standard errors are clustered at the provider level and reported in parentheses. *p<0.1; **p<0.05; ***p<0.01.

Table B.2: Facility and Veteran Characteristics by Facility Quasi-Random Assignment Status

	Does facility quasi-randomly assign?	
	Yes	No
Panel A: Facility Characteristics		
New patients per year	178	410
Physicians per year	5.6	12.0
Region:		
Northeast	0.18	0.21
Midwest	0.26	0.19
South	0.35	0.42
West	0.20	0.17
N=	464	214
Panel B: Veteran Characteristics		
Female	0.059	0.059
Black	0.103	0.114
Age	58.3	58.7
Married	0.595	0.598
Fraction with a Service Connected Disability	0.230	0.242
Income	29,389	27,640
Prior Year Mental Health	0.029	0.031
Elixhauser Comorbidity Index	1.90	2.08
Medicaid or Medicare	0.44	0.46
Prior Year Hospitalization	0.016	0.022
Prior Year Emergency Department	0.016	0.022
N=	1,210,857	1,461,571

Notes: This table summarizes key facility (Panel A) and veteran (Panel B) characteristics for facilities that do and do not quasi-randomly assign PCPs to patients. The empirical exercise that determines whether a facility quasi-randomly assigns is described in subsection 2.3. Veteran characteristics in Panel B refer to veterans that have their first index visit at a random or non-random facility and are assigned a PCP there.

Table B.3: Main Outcomes Excluding Any Diagnosis From Assigned PCP

	Dependent variable (×3):					
	OUD	Overdose	Falls	Vehicle Accident	Suicide	Depression
	(1)	(2)	(3)	(4)	(5)	(6)
Propensity	0.031*	0.007	0.032	0.028*	0.015	0.144
	(0.016)	(0.007)	(0.024)	(0.017)	(0.012)	(0.098)
Mean Dep. Var. (×100)	0.61	0.08	1.52	0.61	0.33	18.6
Observations	893,739	893,739	893,739	893,739	893,739	893,739

Notes: This table reports the output of regressions of our main diagnosis outcomes based on Equation 4, excluding any diagnosis from their assigned PCP to deal with endogeneous diagnosing. Coefficients and standard errors are scaled by $3 = 0.03 \times 100$, to represent the difference between the 90th and 10th percentile prescribers: 3pp. The samples are constrained to veterans who are alive for during the outcome period. All regressions include the residualization fixed effects and even columns include baseline controls; both are described in the text. Standard errors are clustered at the provider level and reported in parentheses. *p<0.1; **p<0.05; ***p<0.01.

Table B.4: Average Characteristics of Physicians in the Top and Bottom Quartile of Propensity

	Lenient	Strict
Male	0.491	0.376
Age	53.2	53.7
Physician (MD or DO)?	0.808	0.619
Physician Assistant?	0.053	0.085
Nurse Practitioner?	0.134	0.292
Cases per year	4238	4028
Days worked per year	246	248
Patients per day	16.7	15.7

Notes: This table displays the simple mean of each variable for physician classified as lenient or strict. Lenient and strict are based on the top and bottom quartile of propensity which is the average across all residuals in described in Equation 3 at the physician level without leave-out. Age is calculated at the last year the PCP is observed in the data and cases, days worked, and patients per day are calculated in 2011. The results are qualitatively robust to calculating at each physician's busiest year.

Table B.5: Main Outcomes with Year-Varying Empirical Bayes Provider Propensity Measure

	Dependent variable (×3):							
	Long-Term Use	OUD	Overdose	Falls	ED	Suicide	Depression	5Y Opioid Mortality
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\widehat{Prescribed}_{ijt}$	0.706***	0.046***	0.011*	0.029	0.140*	0.024*	0.227**	0.0119***
	(0.041)	(0.016)	(0.006)	(0.023)	(0.083)	(0.012)	(0.097)	(0.0038)
Mean Dep Var (×100)	2.25	0.62	0.08	1.65	17.1	0.36	19.3	0.03
Observations	938,801	892,677	892,677	892,677	892,677	892,677	892,677	926,638

Notes: This table reports the output of regressions of our main outcomes on a year-varying empirical Bayes provider propensity measure as described in Appendix A. Coefficients and standard errors are scaled by $3 = 0.03 \times 100$, to represent the difference between the 90th and 10th percentile prescribers: 3pp. The samples are constrained to veterans who are alive for during the outcome period (except for the mortality column). All regressions include the residualization fixed effects and baseline controls; both are described in the text. Standard errors are clustered at the provider level and reported in parentheses. *p<0.1; **p<0.05; ***p<0.01.

Table B.6: Main Outcomes with All Facilities

	Dependent variable (×3):							
	Long-Term Use	OUD	Overdose	Falls	ED	Suicide	Depression	5Y Opioid Mortality
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\widehat{Prescribed}_{ijt}$	0.733***	0.012	0.002	0.038**	0.209***	0.017*	0.212***	0.003
	(0.028)	(0.011)	(0.004)	(0.017)	(0.075)	(0.009)	(0.074)	(0.002)
Mean Dep Var (×100)	2.07	0.58	0.08	1.67	18.3	0.36	19.2	0.03
Observations	2,160,203	2,050,812	2,050,812	2,050,812	2,050,812	2,050,812	2,050,812	2,141,435

Notes: This table reports the output of regressions of our main outcomes for all facilities, not just the ones displaying evidence of quasi-random assignment. Coefficients and standard errors are scaled by $3 = 0.03 \times 100$, to represent the difference between the 90th and 10th percentile prescribers: 3pp. The samples are constrained to veterans who are alive for during the outcome period (except for the mortality column). All regressions include the residualization fixed effects and baseline controls; both are described in the text. Standard errors are clustered at the provider level and reported in parentheses. *p<0.1; **p<0.05; ***p<0.01.