Opioid Use and Employment Outcomes: Evidence from the U.S. Military[†]

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Abstract

There is significant interest in understanding labor market consequences of the opioid epidemic, but little is known about how opioid use affects on-the-job performance. We analyze the impact of opioid initiation on job performance using linked medical and personnel data for active-duty military members. Exploiting quasi-random assignment of patients to physicians in the emergency department, we find that military members assigned to high-intensity opioid prescribing physicians have a higher likelihood of long-term opioid use, are less likely to receive promotions, and are more likely to receive disciplinary actions and leave their jobs. Our results demonstrate productivity costs of opioid use.

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1. Introduction

The United States is in the midst of an unprecedented opioid epidemic. Since 1999, opioid prescriptions have quadrupled and there have been more than 500,000 overdose deaths involving opioids.[1] Most of these deaths have been among the working age population.[2] While much of the evidence has focused on the mortality and health consequences of this epidemic (Maclean et al. (2021)), the impacts on the U.S. labor market and economy are not well understood. Krueger (2017) conjectured that the massive decline in the labor force participation rate since 2000 could be driven in part by the opioid epidemic, which began around the same time as this decline.[3] Some have also speculated that rising opioid use during the coronavirus pandemic contributed to recent labor shortages (Rockeman (2021)).

Prior research on the labor market consequences of the opioid epidemic has focused on aggregate and extensive margin measures of labor supply—primarily employment—finding that opioid use leads to job loss (e.g., Hollingsworth et al. (2017), Aliprantis et al. (2019), Harris et al. (2020), Park and Powell (2021)). However, 63% of adults who report misusing opioids are currently employed (NSDUH, 2020).[4] How opioid use impacts workers' performance on the job is largely unknown, yet it could have significant impacts on productivity. In this paper, we provide the first evidence on how opioid use affects on-the-job performance using individual-level data. We study how receiving an initial opioid prescription affects an individual's subsequent work performance and the mechanisms underlying these effects, including how opioid use impacts physical work capacity, behavioral problems, and criminal behavior.

[1] See https://www.cdc.gov/drugoverdose/epidemic/index.html.
[2] In 2018, 96% of deaths involving opioids were among individuals under age 65 (see Table 1 in Wilson et al. 2020).
[3] CDC dates the opioid epidemic beginning in the 1990s (https://www.cdc.gov/drugoverdose/epidemic/index.html).
[4] https://www.samhsa.gov/data/report/2019-nsduh-detailed-tables (see Table 1.60A). This figure represents the proportion of adults ages 18+ reporting misuse of opioids who are employed full-time or part-time.

To do this, we use administrative data from the U.S. military, the largest employer in the country. We link medical and personnel records for the universe of active duty soldiers from 2008 to 2017. The medical records include medical and prescription drug claims and random drug screenings for active duty military members. Personnel files contain employment and performance measures, such as promotions and demotions, disciplinary actions, physical fitness metrics, and job separations (including detailed reasons for separation). These detailed measures provide an unusually rich characterization of job performance and workforce capability that goes beyond the aggregate labor supply outcomes previously studied in the literature.

The military setting is ideal for studying the impacts of opioid use on workforce outcomes for several reasons. First, the Military Health System (MHS) provides a data linkage between longitudinal healthcare and personnel workforce records that is not typically available in the U.S. labor market. This allows us to observe how job performance evolves after an initial opioid prescription. Second, we are able to continuously follow a large proportion of members over time due to multi-year enlistment contracts which limit turnover in the military. Third, job performance is frequently evaluated and observed through records of promotions and demotions, and direct performance measures (e.g., physical fitness tests) that are assessed at least annually. These outcomes are rarely observed and collected in the civilian labor market. Fourth, military members are highly exposed to opioids (Peters et al. (2019)) and are at risk for negative long-term consequences of opioid use. Finally, opioid use driven productivity consequences have important policy implications for the military's readiness for future missions.

Our empirical approach leverages the quasi-random assignment of patients to physicians when they visit the emergency department (ED) in the military health system. We show that there is wide variation in opioid prescribing behavior across emergency medicine physicians, even within the same hospital and for patients with the same diagnosis. We use the physician's opioid prescribing propensity as an instrument for whether the patient receives an opioid prescription in the ED. We then estimate the impact of receiving an opioid prescription on long-term opioid use and workforce outcomes. We estimate these effects for opioid-naïve individuals to measure the impact of initial opioid exposure. We focus on the ED because patients do not have the ability to request a specific physician in this setting and opioids are frequently prescribed in the ED—about one-quarter of visits in our sample resulted in an opioid prescription. Prior work has used this strategy to show that receiving an opioid prescription in the ED increases the probability of long-term opioid use, opioid use disorder, and overdose death (Barnett et al. (2017), Barnett et al. (2019), Eichmeyer and Zhang (2022)). However, this strategy has not been used to study labor market outcomes.

Conditioning on hospital, diagnosis, and other patient characteristics, we find that patients assigned to a physician with a 10 percentage point higher prescribing propensity are 8.6 percentage points more likely to receive an opioid prescription in the ED. In line with the prior literature, our instrumental variable estimates show that receiving an opioid prescription in the ED increases the probability of long-term opioid use (filling more than 180 days of supply of opioids in the following year) and doctor shopping—two indicators of potential opioid misuse. We further find that individuals who receive an opioid in the ED are 34% more likely to have an opioid-positive random drug screening test in the year following the visit.

While the pain relief benefits of opioids could have positive effects on productivity after an acute injury or condition, we find that opioid initiation has, on average, large downstream negative impacts on workforce measures that reduce workers' productivity. Three main findings lead to this conclusion. First, job performance, as measured by promotions and demotions, declines following opioid initiation. Our instrumental variable estimates show that the probability of receiving a job promotion decreases by 5% in the year after obtaining a first opioid prescription (relative to a baseline promotion rate of 28%). Job demotions, which occur more infrequently, increase by about 1%, although this estimate is not statistically significant. These negative job performance effects persist through our two years of follow-up.

Second, we document behavioral problems that contribute to poor job performance and separations. We find a 12% increase in the likelihood that a military member receives a disciplinary action, i.e., misconduct that could result in discipline or separation, including tardiness, unexcused absences, poor attitude, or not performing assigned duties satisfactorily. In contrast, we do not find evidence of an increase in more serious criminal activities. Job separations increase by 9% following opioid initiation. Discipline-related separations increase by 26%, explaining almost half of the increase in job separations while the remainder of the increase is due to voluntary separations—non-renewal of contracts and retirement. These voluntary separations could partially reflect changes in preferences to continue employment due to cognitive and behavioral changes or a response to non-promotion or anticipated disciplinary investigations. These effects are unlikely to be due to stigma related to opioid use, since medical records pertaining to prescription drug use are private and not revealed to commanding officers. We also do not find any increase in job separations due to medical disability or death, suggesting that opioid misuse is the mechanism for the workforce effects and not the underlying medical condition itself. Overall, using a back-of-the-envelope calculation, we show that reducing the physician opioid prescribing rate to the 25th percentile (15%) would decrease separations by 0.6% and discipline-related separations by 1.8% at the population level.

Third, we do not find evidence that a member's physical job performance is limited after opioid initiation. Physical fitness test scores and test passing rates are unchanged following receipt of an opioid prescription. The ability to manage pain may offset the negative effects of opioids in this case. However, an important caveat is that our physical fitness test results exclude the most severely injured who can be excused from testing with a physician's note.

In summary, we find deterioration in workforce outcomes along multiple dimensions following opioid use. These negative effects on work performance are likely to impact employers' productivity and generate higher recruitment and employee retention costs. These effects are largely driven by behavioral issues rather than limitations to physical work capacity. Moreover, behavioral infractions are more common than criminal misconduct among opioid users. Although our 2SLS estimates can be interpreted as the local average treatment effect (LATE) of compliers—i.e., patients who would receive an opioid prescription from a high intensity provider but not from a low intensity provider—our complier population represents a large proportion of the sample (approximately 37%) and closely mirrors the demographic characteristics of the full sample. Thus, our results provide a high degree of external validity.

Our heterogeneity analyses show that the likelihood that members transition from opioid initiation to negative employment outcomes is related to socio-demographic characteristics. For example, having less than a college education or pre-existing mental health conditions predicts a greater likelihood of negative workforce outcomes in response to opioid initiation.

This study contributes to several lines of research. First, we contribute to the literature on the impact of opioid use on labor market outcomes by using individual-level data to study a rich set of job performance outcomes within the firm. The prior literature finds negative effects of opioid use on aggregate employment outcomes using data at the state or county level. Aliprantis et al. (2019) and Harris et al. (2020) find that areas with higher prescription opioid access have lower labor force participation rates.[5] Other studies use policy variation from the introduction and reformulation of OxyContin (Park and Powell (2021), Powell (2021), Cho et al. (2021)) and rescheduling of hydrocodone (Beheshti (2022)) to show that labor force participation rates decline following large national or state-level shocks to opioid supply.[6] However, none of these prior studies provide data on workplace performance. Furthermore, there is limited evidence using individual-level data due to the difficulty of linking prescription records with workforce outcomes. Two exceptions are studies using Danish administrative data (Laird and Nielsen (2017), Thingholm (2020)).[7] Our paper advances this literature by introducing quasi-random assignment of physicians to address endogenous physician selection concerns. Furthermore, unlike the high level workforce measures used in these studies, we examine granular workforce measures, including physical work capacity, disciplinary actions, and criminal behavior to understand the mechanisms that ultimately impact job performance and labor force participation.

Second, we contribute to the nascent research on behavioral issues and criminal activity in the workplace. Long-term opioid use has been associated with a wide variety of cognitive and mental changes including mood alteration, difficulty fulfilling obligations, and less attentiveness that can affect workplace behavior and lead to criminal activity (Winkelman (2018)). However, there is no causal evidence documenting how workplace behavior changes after opioid initiation. Evidence exists on the impact of opioid use on criminal activity, particularly heroin sale and

[5] To address the potential for reverse causality, Harris et al. (2020) instrument county-level opioid prescription rates with the concentration of high-volume Medicare prescribers. Aliprantis et al. (2019) study geographic variation in opioid prescription rate growth and instrument for prescriptions using triplicate prescription programs.
[6] Park and Powell (2021) and Powell (2021) also find negative effects of opioid supply on earnings. In the military setting, wages have tight bands along ranks that make it a less informative outcome. Additionally, non-wage benefits, including housing allowances, represent a majority share of compensation.
[7] Laird and Nielsen (2017) leverage patient movers in Denmark to show that patients moving to high-intensity opioid prescribing physicians have reduced labor force participation and earnings, but no changes in the receipt of sick pay and disability insurance. Thingholm (2020) also uses Danish data and studies similar outcomes.

possession (Meinhofer (2016), Mallat (2018), Mallat (2020), Dave et al. (2021), Deiana et al. (2021)). The military data offers a more comprehensive dataset by capturing all ongoing and prosecuted criminal activities for Army members. Relative to the literature, we find small effects of opioid use on criminal behavior, but large effects on non-criminal behavioral problems.[8]

Third, we add to the broader literature studying the relationship between medical innovations and labor supply. Access to non-opioid pain medications has been shown to improve labor market outcomes (Garthwaite (2012), Bütikofer and Skira (2018)), although these drugs do not have the same addictive properties as opioids. Generally, prior studies have focused on innovations which improve both health and economic well-being (Currie and Madrian (1999)), however, we show that medical innovations, such as those in pain management, can create a tradeoff between health benefits and risks that can have detrimental labor supply effects.

Finally, we show how opioid use has negatively impacted military productivity for active duty service members. Prior research has focused largely on veterans and on the health consequences of opioid use. Opioid abuse among veterans is a growing concern, as opioid-related mortality among veterans increased by 50% from 2000 to 2016 (Lin et al. 2019). Eichmeyer and Zhang (2022) find that opioid initiation in the ED among veterans increases long-term opioid use by 21%, and opioid overdose mortality by 45% within 3 years.[9] Cesur et al. (2019) find that combat induced opioid abuse leads to annual healthcare costs of $1.04 billion.

2. Background on the Military Health System

[8] Differences in the accessibility of prescription opioids in the military relative to other settings could potentially explain why we do not find significant increases in crime. Prior studies find the largest effects for heroin-related crimes. These studies examine the effects of reduced opioid supply through policy channels, such as PDMPs or the OxyContin reformulation, which lead to substitution to heroin. If opioid prescriptions are more accessible in the military, then we would not expect as much substitution to heroin, and hence lower heroin-related crime rates.

[9] Barnett et al. (2019) show that veterans' opioid initiation in the ED increases long-term opioid use. Eichmeyer and Zhang (2023) find similar results on long-term opioid use for veterans exposed to a high prescribing primary care physician, suggesting that the effects may generalize across healthcare service settings.

The setting for this paper is the Military Health System (MHS), a distinct entity from the Veterans Health Administration, which is both a payer and provider of care for active duty military service-members, military retirees, and their families. The MHS is a two part system. It provides care in a "direct-care" system that includes 51 military hospitals on military bases and over 400 outpatient clinics. The MHS also includes the Tricare insurance benefit that pays for medical services both in the direct-care system and in the civilian market ("purchased care"). Active duty military beneficiaries must enroll in "Tricare Prime," an HMO plan that has near-zero out-of-pocket costs and requires that members receive most care in Military facilities. Active duty members can obtain primary care in the civilian market, but only if they live more than 1 hour away from the nearest military clinic. Additionally, active duty members must get a referral before seeking urgent care outside of the MHS and are expected to go to the MHS for emergency room care if it is the closest facility. For these reasons, most active duty members will receive care at the military base where they reside.[10]

We focus on military emergency departments in this study. MHS EDs are run in a similar fashion as civilian EDs. The main difference is that they have a mix of active duty and civilian physicians and see few non-military patients (Frakes and Gruber 2019; Frakes et al. 2023). The emergency care physician assignment to a patient in a military hospital is quasi-random. A patient that enters the ED is first triaged by a nurse and then placed in a queue for the next available ED physician. The patient cannot request a specific physician.

3. Data

[10] For our sample of opioid-naïve active duty military members, the majority (96.4%) of ED visits occur in a military hospital while the remainder occur in civilian EDs. We exclude civilian ED visits from our study.

We use administrative medical data for the universe of active duty soldiers from the Military Health System Data Repository (MDR) covering 2008-2017. The MDR data include medical claims for inpatient and outpatient services as well as pharmaceutical records. These records include all claims for military members regardless of the site of care, including claims from civilian medical providers. We use data from all military services (Army, Navy, Air Force, Marine Corps) for most of our analyses, although we focus on the Army in some specifications where the outcome measure is unavailable for other services.

We link these medical records with a rich dataset of workforce measures that come from multiple military personnel systems including the Defense Manpower Data Center (DMDC) and Integrated Total Army Personnel Database (ITAPD). These data include information on opioid use as measured by drug testing and workforce outcomes such as promotions, demotions, physical fitness, disciplinary actions, and job separations. Demographic variables include age, race, gender, marital status, education, military rank, job title, and military tenure.

3.1 Sample Construction

We construct a sample of emergency department (ED) visits in hospitals located on U.S. military bases. We allocate prescription opioids filled within 7 days of the ED visit to the ED provider.[11,12] As described in more detail in Section 4, we compute a leave-out, residualized opioid prescribing rate for each provider as our instrument for receiving an opioid prescription. We restrict our sample to providers that treat more than 10 patients in the ED in a year. We also

[11] 7 days is commonly used in the literature (e.g., Barnett et al. 2017). Using smaller windows changes the number of prescriptions included only slightly. Most opioid prescriptions are filled on the same day (72%) or within 2 days (91%) of the visit. While rare, we treat multiple ED visits by the same person as separate index visits, but exclude any ED visits occurring within 7 days of the initial visit to ensure accurate attribution of the opioid prescription.
[12] We identified ER doctors based on a three-digit specialty code ("BIA" refers to ED) and we also have the treating provider's NPI. ED providers can be physicians or physician extenders (i.e. nurse practitioners and physician associates). If both are listed on the ED claim, then we assign the physician. Physician extenders are assigned when no physician is present on the claim.

limit our sample to ED visits for patients who are opioid-naïve (i.e., patients who did not fill an opioid prescription within the 6 months prior to the index ED visit (Barnett et al. 2017)), which accounts for about 71% of all ED visits. This allows us to measure outcomes associated with opioid initiation. Our final sample includes 1,447,758 ED visits from 2008-2017.[13]

3.2 Outcome Measures

3.2.1 Opioid Prescriptions and Misuse

We use multiple measures to track opioid use in the year following the index ED visit. We examine the number of opioid prescriptions filled and whether the patient received opioid prescriptions from 7 or more different providers—an indicator of "doctor shopping".[14] Additionally, we measure long-term opioid use, which is defined as filling more than 180 days supply of opioids within the year following the ED visit, excluding the initial prescription. This measure, which has been used in prior work (Barnett et al. 2017), is an indicator of potential opioid dependency or misuse since clinical guidelines recommend a much shorter course of treatment for acute medical conditions.

As a complementary measure of opioid misuse, we use data from random drug screening tests. The military randomly tests 10% of service members each month and tests 100% of members once per year. We measure whether the patient failed a drug test within one year following the ED visit by type of drug (opioid, heroin, marijuana, benzodiazepines, and all other

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[13] In addition to limiting our sample to opioid-naïve patients and providers that treat more than 10 patients in a given year, our other sample restrictions include dropping members with frequent ED visits (i.e., multiple visits on the same day, more than one visit within 7 days, or more than 10 total ED visits), and ED visits that have missing provider information. We do not select our sample based on a specific set of diagnosis codes that would be treated by opioids because there are a wide range of conditions that receive opioid prescriptions in the ED for both appropriate and inappropriate reasons (Ukert and Polsky (2023), Alpert et al. (2024)).

[14] While prior work often uses 4 or more (e.g., Chua et al. (2019); Baumblatt et al. (2014)) or 5 or more (e.g., Buchmueller and Carey (2018)) opioid prescribers in the year to measure doctor shopping, we use a cutoff of 7 or more opioid prescribers because military members generally have a higher number of visits with different providers. We test the robustness of this cutoff and find similar results with other commonly used cutoffs.

drugs). We also observe whether the member had an "excused reason" for test failure (i.e., they had been prescribed an opioid). The drug screening data allow us to capture illicit opioid and other drug use not captured in the prescription data.

3.2.2 Job Performance

First, we study promotions and demotions as an observable measure of job performance. Promotions occur frequently in the military. Promotions for the lowest ranks (Private through Private First Class) are largely automatic based on time in service, but there is some discretion in promotion decisions at the margin. Promotions for mid-level enlisted ranks (Specialist to Staff Sergeant), however, are largely merit based. We exclude promotions for officers because they are rare in our data and made through a centralized decision process. We also study demotions, which occur less frequently (about 4% of members are demoted each year). Demotions occur when a service member violates the Uniformed Code of Military Justice (UCMJ) and can include relatively minor infractions such as showing up late for work or disrespecting a more senior non-commissioned officer as well as larger infractions that could result in a court-martial.[15]

Second, we study disciplinary actions, formally known as "Suspension of Favorable Personnel Actions (SFPA) Flags." These records are available for the Army only. We evaluate the type of SFPA recorded: discipline flag, criminal investigation flag, and flag for drug or alcohol abuse. Receiving a flag is fairly common, with 22% of individuals receiving at least one in a given year. The "discipline flag" includes minor infractions, such as being late to work and unexcused absences, but excludes drug and alcohol offenses and all other categories such as security violations and domestic violence. The drug or alcohol abuse flag would be used if the

[15] We measure promotions by evaluating whether a service member's rank is higher than the rank at the time of the ED visit. Likewise, we evaluate a demotion if the rank is lower than the rank at the time of the ED visit.

member had a positive drug test (without an excused reason) or possession of an illicit drug or failed to complete alcohol or drug abuse rehabilitation programs (see Army Regulation 600-85 and Army Regulation 635-200). Members would typically not be flagged for drug abuse if they tested positive for opioids in a random screening but had a legal prescription.

Third, we study the probability of being denied a security clearance or having it revoked, which is an indicator of performance issues. Security clearances are essential for performing the tasks of most military jobs and are needed for career advancement (in our sample, 78% hold a security clearance). Obtaining a security clearance involves an extensive background check that may involve interviews with colleagues, family, and friends. A member could be denied a security clearance for reasons including drug/alcohol involvement, criminal conduct, personal conduct (e.g., questionable judgement and dishonesty), and financial considerations (e.g., running up debt). However, members would typically not be denied a security clearance for having a legal opioid prescription. Thus, finding an increase in security clearance denials following an opioid prescription would likely indicate changes in a person's character and trustworthiness induced by opioid use. Although quite rare, having a security clearance revoked is considered a serious violation that would end most military careers.

3.2.3 Work Capacity

As a direct measure of work capacity, we use data from physical fitness tests to measure an individual's physical readiness for the job. The Army Physical Fitness Test (APFT) is administered to each Army service member at least once per year, although most service members will take 2 tests per year. The APFT is designed to test physical strength, endurance, and cardio-respiratory fitness. Individuals who are injured are exempt from taking the test if they have a formal medical provider's note. We study whether individuals take the APFT, pass the APFT, and their test score (which is standardized to the Z-score).

We also evaluate the physical capabilities of Army members with scores from the Physical Capability Grading System. These scores are derived from the Periodic Health Assessment (PHA) that Army members receive annually. In this assessment, a physician conducts a physical exam to assess a member's current health status and identifies any medical conditions. They also conduct a behavioral health screening.[16]

3.2.4 *Employment Separation*

Finally, we study job separations as a consequence of poor performance or work capacity. Approximately 18% of service members in our data leave the military within a year of the ED visit. Job separations occur for voluntary and non-voluntary reasons. Voluntary separations generally occur when the contract expires and the service member makes the choice to not sign a new contract. Non-voluntary exits occur for disciplinary or medical reasons.

Military separations are classified in two distinct ways in our data. First, a "reason code" is entered for the separation. There are more than one-hundred highly descriptive reason codes. The most common reason is "expiration of term of service," or the end of the contract. Other codes include reasons such as: "pattern of minor disciplinary infractions", "civil court conviction" and "desertion." We analyze the universe of discharges and create 7 categories to classify the reason codes as follows: non-renewal of contract or retirement, discipline, failure to meet physical standards, substance abuse, other non-discipline, medical, and uncharacterized.[17]

[16] The APFT ranges from 0 to 300 and a score of 180 or more is a passing score. The majority of members in our sample (93%) receive a passing score. The PHA physical scores ranges from 1 to 4 and a score from 1 to 2 implies an acceptable physical score. A score of 3 or 4 often leads to discharge. A score of 4 is quite rare. For more information on the APFT see: https://usarmybasic.com/army-physical-fitness/apft-standards/. For the PHA see: https://health.mil/Military-Health-Topics/Health-Readiness/Reserve-Health-Readiness-Program/Our-Services/PHA.
[17] "Uncharacterized discharges" are defined as: a) occurring within 180 days of enlistment, b) member does not understand enlistment, c) member is away without official leave for more than 30 days.

Second, service member exits are separately given a character of separation code that impacts their military benefits: “Honorable”, “General under Honorable conditions”, “Other than Honorable,” “Bad Conduct”, and “Dishonorable.” An important note is that while the reason code is not punitive in nature, a ”General under Honorable conditions” discharge, for instance, makes the service member ineligible to reenter the military and precludes use of GI bill education benefits. Any discharge below “Honorable” requires legal justification and is administratively burdensome. This means that a service member may exit with a negative reason code but still receive an Honorable discharge. Thus, Honorable discharges can be given for both voluntary (e.g., contract end date, transfer, and retirement) and involuntary (e.g., disciplinary actions) separations if the infractions are considered fairly minor. In our analysis, we categorize separations as either “Honorable” or “Non-Honorable” (“Non-Honorable” includes all discharge types listed above excluding “Honorable”).

4. Empirical Strategy

We estimate the causal effect of receiving an opioid prescription during an ED visit on long-term opioid use and job performance outcomes. Receiving an opioid prescription is correlated with injury severity or pain and these traits themselves predict negative workforce outcomes. To address this endogeneity issue, we exploit variation in physicians’ propensities to prescribe opioids. We instrument the receipt of an opioid prescription in the ED with physician-level residualized (leave-one-out) opioid prescribing propensities. We focus on the ED setting where patients do not have the ability to request a specific physician during their visit, creating quasi-random assignment of patients to physicians. Our identification strategy relies on idiosyncratic differences in patients’ probability of receiving an opioid prescription stemming only from differences in physician practice styles.

Following a strategy used in prior work (Eichmeyer and Zhang (2022); Barnett et al. (2017)), we construct an instrument that measures physician opioid prescribing intensity. This strategy is similar to other "judges IV" research designs (e.g., Maestas et al. (2013); Dobbie et al. (2018); Agan et al. (2023)). In the first step, we estimate residuals from the regression:

$$Prescription_{ijt} = \alpha_{ht} + \sigma_{hw} + \theta_d + X_{it}'\beta + \epsilon_{ijt} \quad (1)$$

where $Prescription_{ijt}$ is a binary indicator that equals 1 if patient i received an opioid prescription within 7 days of their ED visit with physician j in month-year t. We control for hospital-month-year fixed effects α_{ht} and hospital-day of week fixed effects σ_{hw} to account for differences in prescribing rates across hospitals and time.[18] We include diagnosis fixed effects θ_d to account for the possibility that some ED physicians may specialize in treating certain diagnoses or higher severity cases and would have a higher tendency to prescribe opioids.[19] We also control for patient demographic characteristics X_{it}, including indicators for age group, White, female, married, college, Armed Forces Qualification Test (AFQT) score, military rank, military tenure, and military service-by-occupation fixed effects. These demographic controls are not needed for identification given random assignment, but they improve the precision of the estimates. After conditioning on these fixed effects, the residual variation in the prescribing rate ϵ_{ijt} represents idiosyncratic factors affecting physician prescribing decisions.

For each patient, we then construct our residualized, leave-out instrument of physician opioid prescribing intensity, $Intensity_{ijy}$, as the mean of the physician j's residuals across the calendar year y from equation (1), leaving out the residual for patient i (denoted by $-i$):

[18] Month-year takes on unique values for each month-year combination, while day of week takes on seven values. Time of day is not available in our extract of the MHS data. While we are unable to control for the time of day of the visit, we conducted a complementary analysis to assess the extent to which physicians specialize in specific time of day shifts. We find that the majority of physicians are exposed to patients of different types and severity that would be correlated with time of day or day of week due to their significant rotation across shifts (see Appendix Figure 1).
[19] We define diagnoses by the first 3-digits of the primary ICD-9 or ICD-10 code on the ED claim.

$$Intensity_{ijy} = \frac{1}{N_{-ijy}} \sum_{i=1}^{N_{-ijy}} \hat{\epsilon}_{-ijy} \qquad (2)$$

where N_{-ijy} is the number of ED encounters for physician j in year y, excluding patient i. We leave out patient i to avoid bias from including the error term in both the instrument and outcome variables. This instrument allows us to measure differences in the opioid prescribing rate across physicians within the same hospital who are treating the same diagnosis. We also construct an alternative binary instrument used in some specifications where we define physicians as having a prescribing $Intensity_{ijy}$ in the first versus fourth quartile of the distribution (henceforth, "low intensity" and "high intensity" opioid prescribers).[20]

Our first stage relationship is estimated with the following equation:

$$Prescription_{ijt} = \delta Intensity_{ijy} + \alpha_{ht} + \sigma_{hw} + \theta_d + X'_{it}\beta + \epsilon_{ijt} \qquad (3)$$

We then estimate our second stage using 2SLS, taking the general form:

$$Y_{ijt} = \gamma \widehat{Prescription}_{ijt} + \alpha_{ht} + \sigma_{hw} + \theta_d + X'_{it}\beta + \epsilon_{ijt} \qquad (4)$$

where Y_{ijt} measures long-term opioid use or workforce outcomes in the 1-2 years following the initial ED visit. In both equations, we include the full set of controls from equation (1). Standard errors are clustered by physician. We interpret differences in workforce outcomes between patients assigned to higher or lower intensity opioid prescribing physicians as the effect of a change in the probability of receiving an opioid prescription. We note that we do not identify the effect of opioid misuse on workforce outcomes.[21] Identification relies on the assumptions that physician assignment is as good as random (i.e., conditional independence) and that our

[20] In the 1st versus 4th quartile specification, we compute our $Intensity_{ijy}$ instrument including patient i. This allows for physicians to be consistently defined as either high or low intensity prescribers across all patients within a year.
[21] Whether someone receives an opioid prescription is an observable outcome, unlike opioid misuse, for which we only have proxies of this outcome. For this reason, we focus on receiving an opioid prescription as our main endogenous variable. However, our results are suggestive that opioid misuse is likely the mechanism through which an opioid prescription impacts workforce outcomes.

instrument does not operate through channels other than opioid prescribing (i.e., exclusion restriction). Additionally, we assume monotonicity in opioid prescribing intensity across patients. Although these assumptions are fundamentally untestable, we provide supporting evidence for each of these assumptions in Sections 5.2 and 5.5. If these assumptions hold, then we can interpret our 2SLS estimates as the local average treatment effect (LATE) of compliers, i.e., patients who would receive an opioid prescription from a high intensity provider but not from a low intensity provider. This population of marginal patients is especially policy relevant given that there is significant gray area in which conditions are deemed appropriate for prescribing opioids and substantial heterogeneity across providers in how they prescribe opioids.

5. Results

5.1 Descriptive Statistics

Figure 1 shows the distribution of the raw mean opioid prescribing rate and the residualized instrument for each physician-year. There is significant variation in both of these measures across ED physicians. The opioid prescribing rate ranges from 14.1% to 30.1% when comparing the mean rate for the bottom and top quartiles of physicians. This range is similar to Eichmeyer and Zhang (2022). A potential reason for the wide variation in prescribing rates is that there is little consensus on whether diagnoses are either appropriate or inappropriate for opioid treatment (Ukert and Polsky 2023; Alpert et al. 2024). After controlling for hospital, time, diagnosis, and patient characteristics, we find that moving from the bottom to top quartile physician increases the mean prescribing rate by 19 percentage points.[22]

In Column 1 of Appendix Table 1, we provide descriptive statistics for the sample of ED patients. The average military ED patient is 26 years old, male (79%), White (65%), married

[22] The 5th, 25th, 75th and 95th percentiles of the raw opioid prescribing rate distribution are: 0.08, 0.15, 0.28, 0.41. The same percentiles of the residualized distribution are: -0.10, -0.04, 0.03, 0.13.

(53%), does not have a college degree (95%), has junior rank (62%), AFQT score of 60, and has a military tenure of 5 years. Relative to the commercially insured and Medicare populations, the military ED patient is younger, more likely to be male, and exhibits a lower likelihood of college education, however, the ED opioid prescribing rate of 22% is quite similar to other settings (e.g., Barnett et al. (2017), Ukert and Polsky (2023), Alpert et al. (2024)).

5.2 *Validity of the Instrument*

We provide tests of exogeneity, monotonicity, and relevance of the opioid prescribing propensity instrument. We provide evidence supporting the exclusion restriction in Section 5.5.

5.2.1 Exogeneity of Physician Assignment: Balance Tests

First, we provide balance tests showing the relationship between patient characteristics or pre-treatment outcomes and our instrument, supporting the conditional independence assumption, in Appendix Table 1 and Appendix Tables 2-4. In Appendix Table 1, we regress each patient characteristic or pre-treatment outcome on our instrument for physician prescribing intensity. Each cell is from a separate regression. Column 2 shows coefficients on the continuous instrument of prescribing intensity and Column 5 shows coefficients on the binary (top vs. bottom quartile intensity) instrument. If patients are randomly assigned to ED physicians, we would expect that these patient characteristics would be uncorrelated with prescribing intensity. Indeed, we do not observe statistically significant differences across patients seen by a higher or lower opioid prescribing physician for any of the demographic characteristics. We also do not observe a meaningful relationship between our outcome variables measured 6 months *prior* to the ED visit and the physician's prescribing intensity. In an alternative version of the balance test, we regress the physician prescribing intensity instrument on all patient characteristics jointly. These results are displayed in Appendix Table 2.

Column 1 shows a regression of a binary indicator for receiving an opioid prescription. As expected, this is correlated with patient characteristics. Column 2 shows the balance test. Consistent with random assignment, patient characteristics do not predict the physician opioid prescribing intensity. Almost all coefficients are close to zero and the joint F-statistic is 0.49. In Column 3, we show another variant of the balance test where we construct the opioid prescribing intensity instrument using only hospital-month-year, hospital-day of week, and diagnosis fixed effects. The results are almost identical.[23]

Finally, in Appendix Table 4 we regress whether the patient has a given ED diagnosis on the physician's opioid prescribing intensity. We show results for diagnoses that are coded as "definitive emergencies" or "definitive non-emergencies" based on the widely used New York University ED Algorithm (Johnston et al. 2017).[24] We also show results for the top 10 diagnoses in our ED sample which account for about one-third of visits. If some physicians specialize in treating more severe conditions and have a higher tendency to prescribe opioids, then we could find a positive correlation for more severe conditions. However, we do not find a statistically significant relationship among the emergency and non-emergency diagnoses and the physician's opioid propensity. Furthermore, among the top 10 diagnoses, only two (acute pharyngitis and respiratory system) have a statistically significant relationship with the physician's opioid prescribing intensity and there is no correlation for the conditions that have the highest opioid prescribing rates (e.g., disorders of the back, sprains and strains of back or ankle and foot).

[23] When we replicate Appendix Table 1 using only hospital-month-year, hospital-day of week, and diagnosis fixed effects to construct the instruments, we also find similar coefficients and p-values (see Appendix Table 3). Thus, the balance test results are not sensitive to including sociodemographic controls in the residualization.

[24] The NYU algorithm probabilistically assigns each ED visit into emergent and non-emergent categories based on the discharge diagnosis. Most diagnoses fall into multiple categories. We define definitive emergency care cases as those who have a total assigned probability of 100% across all emergent categories and definitive non-emergent as those with a probability of 100% in the non-emergent category.

Overall, the results from the balance tests support the assumption that assignment of patients to physicians is as good as random.

5.2.2 Monotonicity of the Instrument

Another assumption of our 2SLS strategy is that the relationship between physician prescribing propensities and the likelihood of receiving an opioid is monotonic. In other words, a patient that receives an opioid from a low propensity physician would also receive one from a high propensity physician. If the monotonicity assumption is violated, then we are unable to interpret our estimates as local average treatment effects (LATE).[25] We show results from tests of the monotonicity assumption discussed in Frandsen et al. (2023). We conduct tests of the "average monotonicity" assumption which demonstrates that our 2SLS strategy uncovers a weighted average of individual treatment effects. Average monotonicity implies that each patient's likelihood of receiving an opioid from their physician is positively correlated with the physician's overall propensity to prescribe. Frandsen et al. (2023) notes that this assumption can be tested by showing that the first stage is positive for all demographic subsamples. This test is also commonly used in the "judges IV" literature (e.g., Dobbie et al. (2018); Eichmeyer and Zhang (2022); Agan et al. (2023)). In Column 1 of Appendix Table 5, we estimate the first stage regression for age, gender, marital status, race, education, AFQT, and depression subsamples. This leads to 14 distinct subsamples in which we can observe whether there is a positive first-stage relationship. Across all subsamples we find a statistically significant positive relationship

[25] Chan, Gentzkow and Yu (2022) posit that the monotonicity assumption may be violated if physicians differ in both their diagnostic skills and their preferences for prescribing opioids. In our context, this could arise if some low propensity prescribing physicians are better at diagnosing patients than a higher propensity physician, leading to differential treatment decisions along the distribution of physicians' prescribing rates. This is less likely to be a concern in our setting given that there is little medical consensus on which conditions should or should not receive an opioid, which may minimize the distinction between skills relative to preferences (Ukert and Polsky 2023; Alpert et al. 2024). Additionally, in the next section, we conduct a test for differential skill in physician treatment by controlling for differences in overall treatment intensity. Our results are robust to these tests.

between the instrument and the probability of receiving an opioid, consistent with the average monotonicity assumption. In a related test, following Bhuller et al. (2018), we leave out each subsample in constructing the instrument and then run the first stage on the left-out subsample (see Column 2). Again, we find large and positive estimates across the subsamples.[26]

5.2.3 Relevance of Instrument: First Stage

Finally, we estimate the first stage relationship between the physician prescribing intensity and the probability of receiving an opioid prescription following the ED visit. Panel A of Table 1 shows the results for the continuous prescribing intensity instrument and Panel B shows the results for the binary (top vs. bottom quartile) instrument. Consistent with the graphical representation of the first stage in Figure 1, both instruments have a strong association with a patient's probability of receiving an opioid prescription. The first stage F-statistic is well above conventional thresholds.[27] Panel A shows that patients assigned to a physician with a 10 percentage point higher prescribing intensity are 8.6 percentage points more likely to receive an opioid prescription, while Panel B shows that assignment to a physician in the top quartile of prescribing intensity increases the probability of receiving an opioid prescription by 18.5 percentage points compared to the bottom quartile.

5.3 2SLS Results

5.3.1 Opioid Misuse Outcomes

[26] In a secondary test, we follow Frandsen et al. (2023) who proposes a strict monotonicity test which jointly tests the monotonicity and exclusion restriction assumptions. In Appendix Table 6, we implement the test in each of the ten largest hospitals in our data and fail to reject the null hypothesis that monotonicity and exclusion hold in nine of the hospitals, thereby providing additional suggestive evidence that the monotonicity assumption holds. Although the test proposed in Frandsen et al. (2023) is designed to be conducted for the full sample, this is computationally challenging with our data. We follow the implementation of this test in Agan et al. (2023) which uses separable covariates and stratifies the sample by court (or, in our case, hospital).

[27] The first stage F-statistics for the continuous intensity instrument are 10,213 and 3,139 for the binary instrument.

The existing literature has shown that assignment to a high intensity physician increases the probability of long-term opioid use for veterans and Medicare beneficiaries (Barnett et al (2017), Eichmeyer and Zhang (2022)). In Table 1, we replicate these findings using our sample of active-duty military members. Column 2 shows the number of opioid prescriptions filled during the first year of follow-up; Column 3 shows an indicator of doctor shopping (having 7 or more prescribers);[28] and Column 4 shows an indicator for long-term opioid use (180 days supply within one year). The latter two measures indicate potential opioid misuse. The results are similar for both instruments and we focus on the continuous instrument, which uses the full sample, in our discussion.[29] For the continuous instrument, the results show that after receiving an opioid prescription in the ED, an individual has 11% more opioid prescription fills (baseline mean fill rate of 0.36), is 60% more likely to exhibit doctor shopping behavior (baseline mean of 0.30%), and is 62% more likely to have long-term opioid use (baseline mean of 0.29%).[30]

Table 2 presents complementary evidence of long-term opioid use from random drug screening tests in the year following the ED visit. Unlike the prescription data, drug screenings capture opioid use from both medical and illicit sources, which prior studies have not been able to examine. Column 1 shows that the probability of failing a drug test increases by 1 percentage point (34% increase) after receiving an opioid prescription. Column 2 shows that the increase in drug test failures is predominantly driven by opioid drug test failures that are accompanied by a legal prescription (0.8 percentage point, or 112%, increase). There is no statistically significant

[28] Other commonly utilized doctor shopping thresholds, such as having 4 or more, 5 or more, or 6 or more unique opioid prescribers provides similar and somewhat stronger effects (see Appendix Table 7).
[29] We also estimate the reduced form relationship between the prescribing outcomes (number of prescriptions, doctor shopping, and long-term use) and the intensity instrument. We find that these estimates are statistically significant and display effects of 0.033, 0.002, and 0.002, respectively.
[30] We also created a binary indictor for individuals with an Opioid Use Disorder (OUD) diagnosis within the first or second year. OUD diagnoses are very rare in this sample (about 0.06% in the first year) and we find no effect in a 2SLS regression on the probability of receiving an OUD diagnosis.

increase in drug test failures from opioids without a prescription (Column 3). In Columns 4-7 we look at other drug test failures that could be substitutes (heroin and marijuana) or complements (benzodiazepines) for prescription opioids, and other drugs. We find that the probability of a heroin drug test failure decreases by 0.05 percentage points, potentially stemming from patients' expanded access to prescription opioids following an initial prescription. On the other hand, we find no spillovers of opioid use on marijuana, benzodiazepines, or any other illicit drugs.

The results in this section are broadly consistent with the prior findings in the literature—individuals who encounter a high intensity prescriber in the ED are significantly more likely to initiate long-term opioid use. However, an important question remains: how does opioid initiation (which leads to long-term opioid use) impact job performance and work capacity? We bring to bear our linked healthcare and workforce data to examine this question.

5.3.2 Workforce Outcomes

In this section, we examine the effects of opioid initiation on job performance, work capacity, and job separations. We then analyze how these outcomes vary across subgroups defined by socio-economic characteristics and pre-existing mental health conditions.

A. Job Performance

In Table 3, we examine promotions and demotions among enlisted service members as an observable measure of job performance. Promotions are a high frequency outcome in the military with about 28% of enlisted members receiving a promotion within one year of the ED visit and 39% within two years. Demotions for poor job performance, on the other hand, are quite rare (about 4%). Using the continuous instrument in Panel A, we find that the probability of receiving a promotion decreases by 1.4 percentage points within one year of receiving an opioid in the ED and by 1.2 percentage points within two years. These findings imply 5% and

3% fewer promotions, respectively, relative to the baseline means. Results from the binary high intensity instrument are similar. Not surprisingly, the estimates for demotions are noisier given their infrequent occurrence. The probability of being demoted increases by 1.4% after one year (relative to the baseline mean of 3.5%) and 4% in the second year (baseline mean of 4.7%), although these estimates are not statistically different from zero. The larger effects for demotions in the second year could reflect the longer time it takes for demotions to pass through legal and administrative channels, while promotions are more quickly implemented by superiors.

B. Physical Work Capacity

Our results on promotions show a sizeable decline in job performance following opioid initiation. We use our detailed workforce data to explore the potential mechanisms driving these results. We first examine how opioids affect physical performance. Similar to many other physically-demanding civilian jobs such as construction, manufacturing, and mining, it is essential for military members to maintain a high level of physical fitness for performing the core functions of their jobs. Opioids, even when taken as prescribed, have known physiological effects on physical performance, such as slowed breathing and heart rate and delayed reaction times, which reduce endurance and make exercise more difficult. Moreover, opioids can have negative effects on mental functioning and could lead to depression, which may lead military members to put less effort into their training (Mazereeuw et al. (2018)). On the other hand, opioid use could potentially improve physical performance by reducing the pain from an acute injury or condition. Whether these conflating effects translate into meaningful reductions in work capacity is unknown. The military is a useful setting for studying this question because members are required to take physical fitness tests that have an explicit passing threshold reflecting the physical demands of the job. Thus, we can observe whether opioid initiation impacts workers' ability to meet these physical demands.

Table 4 displays the effects of opioid use on physical performance from the Army Physical Fitness Test (APFT) in the year after the ED visit. In Column 1, we do not find evidence of a decline in the probability of taking this test after receiving an opioid prescription, even though members could receive permission to delay testing due to the injury or medical condition. The coefficients are small and only marginally statistically significant for the binary instrument. Conditional on taking the APFT, the probability of passing the test decreases by about 0.7%, although this is not statistically significant. The Z-score standardized physical fitness test score decreases by a statistically insignificant 0.03 standard deviations (SD=44 points). Overall, we find little evidence that opioid initiation affects physical fitness scores.

C. Behavioral Problems and Criminal Activity

We also consider the effects of opioid use on behavioral problems in the workplace. In cases of long-term use, opioids have been associated with a wide variety of mental health changes including mood alteration, difficulty fulfilling obligations, lower attention span and less attentiveness (Meyer (2019), Richards et al. (2018)). More broadly, opioids can increase absenteeism (CDC, 2019), and opioid use is associated with criminal justice system involvement (Winkelman (2018)). All of these factors could lead to problematic workplace behaviors that limit performance. However, there is little causal evidence for most of these relationships.[31]

In Table 5 we evaluate whether receiving an opioid prescription changes the probability of receiving a disciplinary action (known as a "Suspension of Favorable Personnel Actions

[31] The existing evidence links prescription opioids and crime, focusing on heroin sale and possession, though the literature is mixed (Meinhofer (2016), Mallat (2018) & (2020), Dave et al. (2021), Deiana et al. (2021)), and some have suggested that this link may not be as strong as it is for illicit drugs (Maclean et al. (2021)).

(SFPA) Flag"). There are 19 distinct groups for which an individual may receive a flag for problematic behaviors. We look at three specific flag groups where we would expect to see the largest effects of opioids: a flag for discipline, a flag for criminal investigation, and a flag that combines drug and alcohol abuse.[32] The discipline category includes minor infractions such as being late to work and unexcused absences, but excludes drug and alcohol offenses and all other categories such as security violations, and domestic violence.

We find evidence of a 1.1 percentage point increase (12% relative to the baseline mean of 8.7%) in the probability of receiving a discipline flag within one year of receiving an opioid prescription which is statistically significant at the 5% level. Estimates in year two are slightly smaller and not statistically significant. The less precise estimates in the second year likely reflects that some members receiving disciplinary flags in year 1 would be discharged for disciplinary reasons by year 2. Column 2 shows results for flags for criminal investigation. These effects are small and statistically insignificant. We also do not find effects of opioid initiation on drug and alcohol flags (Column 3). While this might seem counterintuitive, it is consistent with previous results in Table 2, where we showed that most opioid-positive drug tests are accompanied by a legal prescription. Therefore, these drug tests would not flag individuals for drug abuse, even though some proportion is likely misusing opioids as suggested by our results on doctor shopping and long-term use. This highlights the difficulty of detecting problematic opioid use in the workplace through drug screening.

Another outcome which may indicate behavioral problems is the probability of receiving a denial of a security clearance or having a security clearance revoked. Obtaining a security clearance involves an extensive background check and denials could reflect changes in a military

[32] There is no separate flag or data specifically recording absenteeism or missed days of work.

member's behavior related to their judgement, reliability, and trustworthiness. In Appendix Table 8, we find that opioid use after an ED visit leads to a higher probability of receiving a denial of a security clearance by 0.5 percentage points after one year (28% increase) which is statistically significant, but has no effect on the more serious infraction of having one's security clearance revoked. It is not surprising that there is no effect on revocations given that this is an extremely rare outcome. The reason for the increase in security clearance denials is likely due to behavioral issues induced by opioid use rather than an increased likelihood of being flagged for drug abuse given the prior results in Table 5.

These findings are consistent with the overall picture of worsening job performance among those who initiated opioid use. From this set of results, it appears that the negative effects of opioids on performance are largely driven by behavioral issues rather than physical limitations. Moreover, we find that minor behavioral infractions are more common than criminal misconduct among opioid users.

D. Job Separations

The lower rate of promotions along with the higher rate of disciplinary actions and security clearance denials may eventually lead to involuntary discharges from the military for those initiating opioid use. Voluntary separations—such as contract non-renewal, transfer to the national guard or military reserve, or retirement—may also occur if opioid use changes an individual's behavioral or cognitive capacity to work. In some cases, a voluntary separation such as contract non-renewal or transfer could be done to pre-empt a future discipline-related discharge or as a response to non-promotion.

In Column 1 of Table 6, we show that the probability of being discharged from the military for any reason increases by 1.6 percentage points within one year of the ED visit. The magnitude of this effect is substantial and represents an increase in separations of 9% relative to the baseline mean of 18%. We find similar effect sizes two years after opioid initiation.

In Columns 2-8, we categorize discharges based on the military's separation reason code. We find that the largest proportional increase in discharges is for discipline-related reasons, which increased by 26% relative to the baseline mean.[33] The number of discipline discharges remained elevated (16% increase) and statistically significant in the second year after the ED visit. Voluntary discharges, which occur when a member does not renew their contract or retires, increased by 17% within the first year, but the estimates are not statistically significant in the second year. Although we observe a faster rate of increase for discipline-related discharges relative to voluntary discharges, they are a smaller share of overall discharges and account for about 42% of the overall discharge effect. Voluntary discharges due to non-renewal of contracts and retirement comprise the remaining increase in discharges.

For the other discharge types, the estimates are not statistically distinguishable from zero. Finding no effect for discharges due to failure to meet weight and body fat standards is consistent with our prior results which showed that opioid use did not impact physical fitness test scores. Additionally, we do not find an increase in discharges due to substance abuse. This is consistent with the drug screening test results showing that the majority of opioid-positive drug tests are accompanied by a legal prescription, and therefore, we would expect few discharges due to opioid misuse to be coded as substance abuse. However, given the significant increase in doctor shopping we observe, it is likely that some of these individuals are misusing opioids. It is

[33] Discipline discharges are given for the following reasons: civil court conviction, security, court martial, AWOL/Desertion, Good of the Service (discharge in lieu of court martial), misconduct, pattern of minor disciplinary infractions, Commission of a Serious Offense, failure to meet minimum qualifications for retention, unsatisfactory performance, unfitness or unacceptable conduct, discreditable incidents, imprisonment/desertion, failure of course of instruction, failure of selection for promotion, motivational problems (apathy).

difficult for the military to discharge individuals for opioid abuse based on drug screenings because of the presence of legal prescriptions. Instead, it is more likely that opioid-related discharges are coded with a reason related to job performance.

The remaining three categories of discharges in Columns 6-8 serve as placebo tests. "Other non-discipline discharges" contain other reasons for discharge that should be unrelated to opioid use—for example, early release to attend school, pregnancy or parenthood, and errors made by the military in the enlistment process (e.g., underage enlistment). Indeed, we do not find any impact of receiving an opioid on these outcomes. We also examine medical-related discharges which include disability and death. Deaths are a rare event, with less than 0.1% dying within one year. We did not find a change in the probability of death within one or two years after opioid receipt. The effects on other medical discharges are also close to zero and statistically insignificant. The results for medical discharges supports our identifying assumption that patients' assignment to ED doctors is unrelated to the severity of their health condition.

Finally, in Columns 9-10 we examine discharges using the broader "Character of Separation" codes which determine veteran benefits. We report discharges as falling into two separate categories: "Honorable" or "Non-Honorable." Most service members receive honorable discharges, meaning their service was not marred by anything negative. Given the lengthy administrative process involved in obtaining a non-honorable discharge, many members discharged for minor disciplinary reasons will actually be classified as an honorable discharge. Only serious offenses such as illicit drug possession, assault, and criminal misconduct will generally trigger non-honorable discharges. We find that the increase in discharges are driven by honorable discharges in the first year. Column 10 shows that honorable discharges increase by about 1.2 percentage point within the first year after the ED visit (9% increase), but there is no effect for non-honorable discharges. After two years, the effect for honorable discharges becomes slightly smaller, but we find a large and statistically significant increase in non-honorable discharges of 0.8 percentage points (13% increase). It is not surprising that we find delayed effects for non-honorable discharges because it takes time for formal investigations to conclude, while honorable discharges can be implemented more expeditiously. While both categories include discipline-related discharges, the non-honorable discharges will contain more serious infractions while honorable discharges will be a mix of minor infractions and discharges unrelated to misconduct (e.g., for medical reasons, pregnancy, end of service term). Overall, the results in this section show large increases in discipline-related discharges from opioid use.

5.4 Heterogeneity

Next, we consider how the IV estimates vary across different subgroups. We consider subgroups based on demographic characteristics and pre-existing diagnoses of depression or anxiety.[34] Appendix Figure 2 shows the coefficient estimates and 95% confidence intervals from estimating the IV regression in equation (4) separately for each demographic subgroup.

The first two panels display the results for long-term opioid use (180 days supply) and doctor shopping. Receiving an opioid prescription in the ED predicts an increase in long-term opioid use and doctor shopping for almost all subgroups, although not all estimates are statistically significant. We find larger effects for White military members and those without a college education for doctor shopping, which is the strongest indicator of opioid misuse. These results are consistent with Case and Deaton (2015) which showed that White non-Hispanic individuals with less than a college education have been most impacted by the opioid crisis.

[34] At baseline, 16% are receiving medication for depression or anxiety. Depression/Anxiety diagnoses are defined based on prescription fills in the year prior to the index ED visit for one of the following therapeutic classes: Benzodiazepines, Antidepressants, or Antipsychotic agents.

In the remaining panels of Appendix Figure 2, we examine heterogeneity in workforce outcomes. We focus on promotions, discipline flags, discipline discharges and non-honorable discharges for which we found the largest effects in the full sample. For promotions, although effects are roughly similar across subgroups, we find larger negative point estimates for military members who are White, have a below median AFQT score, and for those who have depression or anxiety. The most pronounced differences occur by race. White members experience a much larger negative effect of opioid use on the likelihood of a promotion, while the effect for non-White members is not statistically distinguishable from zero. When examining the other outcomes we do not find meaningful differential effects by race. Instead, the characteristics that most strongly predict a higher likelihood of disciplinary action or discharge are: younger than age 30, single, no college education, below median AFQT score, and having depression or anxiety. These results show that certain demographic factors predict a greater likelihood that initial opioid exposure results in negative performance and employment consequences. In subsequent analysis, we conduct a complier analysis. The results are described in Appendix A2.

5.5 Alternative Explanations: Exclusion Restriction

Interpreting our 2SLS estimates as the causal effect of receiving an opioid prescription assumes that ED physicians are not influencing employment outcomes through other channels besides opioid prescribing (i.e., exclusion restriction). This assumption cannot be directly tested, however we believe that the exclusion restriction is likely to hold in our setting. First, unlike other physician-patient relationships, patients typically have a one-time interaction with the ED physician so there is limited scope for them to have long run effects on patient outcomes. An exception to this is if the physician prescribes an opioid that is used long term after it is initiated, making opioid prescribing a plausible channel for long run effects. Second, although other medical services rendered during the ED visit could potentially have long run impacts on health, these impacts are more likely to be positive (i.e., those other medical interventions would *improve* labor market outcomes), whereas opioids are more likely to be negative. Hence, this would bias our estimates towards zero and give us a lower bound on workforce effects. Notably, we did not observe any improvements in health outcomes and work performance due to receiving an opioid prescription (as shown in the previous section) making this less of a concern. Nevertheless, we test for this potential violation of the exclusion restriction. Specifically, we test for whether the physician's opioid prescribing propensity is correlated with their propensity to provide other medical services during the ED visit which also impacts downstream outcomes.

To test for this, we construct a leave-out residualized instrument analogous to equation (2) for four measures of medical treatment intensity for each physician across the calendar year: Berenson-Eggers Type of Service (BETOS) codes, BETOS and procedures, log work relative value units (wRVUs), and the ED prescribing propensity for anxiety or depression medication.[35] We include the intensity measures to account for non-opioid prescribing or procedure intensity that could have lasting effects beyond the acute condition.[36] We then include each predicted medical treatment intensity measure in the original 2SLS equation (4) as a control. Appendix Table 9 shows that our main results are generally robust to controlling for each measure of medical treatment intensity suggesting that receiving an opioid prescription predicts negative workforce outcomes independent of other medical services rendered during the ED visit.

5.6. Additional Robustness Tests and Complier Analysis

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[35] BETOS codes are categories of HCPCS procedure and service codes. Specifically, we include the number of BETOS codes for procedures, diagnostic tests and imaging. We exclude Evaluation & Management (E&M) codes since nearly every visit includes one claim per patient, as well as durable medical equipment, and unclassified codes. We take the log of wRVUs given considerable skewness in this variable.

[36] For the young adult military population, anxiety and/or depression drugs are among the most commonly used chronic medications and are often prescribed in the ED.

We conduct several tests of robustness and alternative explanations, described in detail in Appendix A2. We show that our results are not sensitive to the inclusion of control variables (Appendix Table 10) and cannot be explained by unobserved injury severity and health status (Appendix Table 11) or sample attrition (Appendix Table 12). Additionally, we analyze the share of compliers and their characteristics (Appendix Table 13), further supporting the external validity of our results, as we find a large and representative population of compliers.

6. Conclusion

This paper studies the impact of opioid use on job performance and workforce outcomes. We focus our analysis on the U.S. military because of the unique ability to link medical records with detailed job performance outcomes. Relative to the existing literature, we provide the first evidence, to our knowledge, on the broader implications of opioid initiation on measures of job performance and work capability. In particular, we examine granular outcomes that impact on-the-job performance such as physical work capacity, behavioral issues, and criminal conduct; measures that could not be observed in most civilian employer databases. We investigate the relationship between opioid use and employment outcomes by employing a 2SLS identification strategy that relies on the quasi-random assignment of patients to physicians in the ED and the physician's historic opioid prescribing propensity at the time of the patient's ED visit.

Our results have three main take-aways. First, we show that opioid initiation reduces job performance. We observe fewer promotions and increases in demotions. Most of these negative outcomes emerge within one year after opioid initiation. Second, we show that opioid initiation leads to behavioral problems that increase the likelihood of disciplinary action and discharges. These findings suggest lower job productivity that can lead to increased recruitment and employee retention costs. Effects on disciplinary actions are concentrated among minor infractions, such as being late to work and unexcused absences. We do not find increases in serious criminal activities or disciplinary actions for drug abuse. The latter result is consistent with the findings that the majority of opioid use is tied to legal prescriptions and that there is no observed effect of opioid use on the abuse of other illicit drugs. Third, while we observe negative workforce outcomes related to behavioral issues, we do not find reductions in physical capability. However, our physical performance outcomes are conditional on testing, thus leaving open the possibility that some individuals with severe conditions were able to avoid testing.

We use our results to perform a back-of-the-envelope calculation to estimate how opioid misuse and workforce outcomes would change at the population level if the physician opioid prescribing rate were reduced from the mean rate (22%) to the rate of the 25th percentile physician (15%) through policy levers.[37] This extrapolation exercise suggests there would be a 4% decline in doctor shopping behavior and long term opioid use, a 0.3% increase in promotions and a 0.9% decrease in disciplinary flags among active-duty military members. Overall, discharges would decrease by 0.6% and discipline-related discharges would decrease by 1.8%.

In summary, these findings show the significant impact of the opioid epidemic on labor market productivity and military readiness. Limiting opioid prescribing and expanding substance abuse treatment could reduce job loss and increase productivity. While employer spending on substance abuse treatment has increased—rising from $828 million in 2010 to $2.6 billion in 2016 (KFF, 2018)—further expansions may be warranted. Identifying individuals in need of opioid treatment in the workplace, however, is challenging. We show that the behavioral

[37] This extrapolation scales our IV estimates by 0.07, the difference from moving between the mean rate (0.22) to the 25th percentile (0.15). Alternatively, we could extrapolate from an (out-of-sample) prescribing rate of zero to the mean rate (0.22), to represent how the introduction of the opioid epidemic may have affected workforce outcomes. In this case, we would find an increase in total discharges by 2% and discipline-related discharges by 6%. As a point of reference, the U.S. employment rate fell by 6% from 1999 to 2018 (Abraham and Kearney (2020)).

consequences of opioid misuse are more likely to trigger disciplinary actions and separations than drug and medical screenings.

Our paper has several limitations. First, we estimate the effects of opioid initiation for active duty military members receiving care on U.S. military bases. This may not generalize to military members serving in combat or to veterans, though previous work has shown similar opioid use patterns for veterans (Barnett et al. (2019); Zhang (2021)). However, our sample more closely resembles civilian workers relative to other military populations, thus broadening its applicability. Second, our results may reflect lower bound estimates of negative workforce outcomes as disciplinary actions requires a formal administrative process and some commanding officers may not report minor infractions. Thus, we may not capture all behavioral problems. Finally, our 2SLS strategy relies on the assumption that our instrument does not operate through channels other than opioid prescribing in the ED. In particular, a physician who is a high-intensity opioid prescriber may also provide other types of care to the patient more (or less) intensively. Although we cannot control for unobserved components of patient care, we find that opioid prescribing independently predicts long-term opioid use and negative workforce outcomes when controlling for observable measures of medical care intensity.

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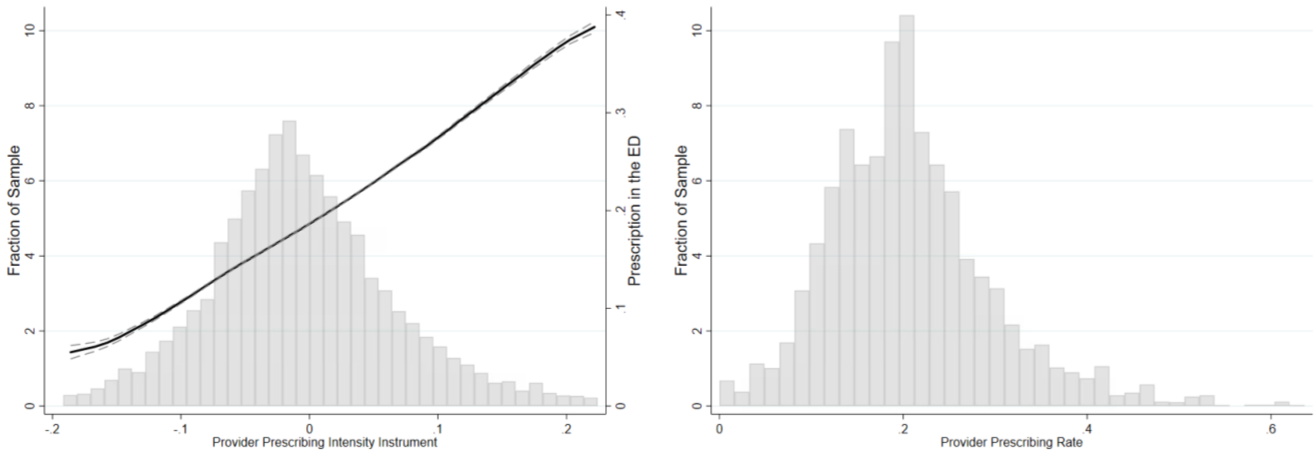
Figure 1: Distribution of Physician Opioid Prescribing Rate and First Stage



Notes: The left figure shows the residualized prescribing rate for each physician-year, which reflects idiosyncratic factors affecting prescribing decisions that are unrelated to hospital, time, diagnosis, and patient sociodemographic characteristics. The solid line is a local linear regression of the first stage, displaying the relationship between the prescribing intensity instrument and the probability of receiving an opioid prescription along with the 95% confidence interval. The local linear regression is estimated from the 1st to 99th percentiles. The right figure displays the raw distribution of physicians' mean opioid prescribing rate for each year. The figures include prescribing rates from 2,551 unique providers.

Table 1: Probability of Opioid Prescription in ED and Long-Term Opioid Use

	First Stage	2SLS - Opioid Use Within 1 Year		
	Prescription in ED	Number of Prescriptions	Doctor Shopping	Long Term Use (180 Days Supply)
	(1)	(2)	(3)	(4)
Panel A: Prescribing Rate Instrument				
Prescribing Intensity	0.8644***			
	(0.009)			
Prescription in ED		0.0384**	0.0018**	0.0018**
		(0.0165)	(0.0008)	(0.0008)
Panel B: High vs. Low Instrument				
High Prescribing Intensity	0.1847***			
	(0.0033)			
Prescription in ED		0.0613***	0.0018**	0.0022***
		(0.0152)	(0.0007)	(0.0007)
Full Sample Mean	0.22	0.3603	0.0030	0.0029
Total Observations	1,447,758	1,447,758	1,447,758	1,447,758

Notes: This table presents coefficients obtained from the first stage (Column 1) and the second stage (Columns 2-4) of the instrumental variable regressions on the impact of an opioid prescription on long-term opioid use after the ED visit. Panel A uses the residualized continuous prescribing rate as an instrument while Panel B uses the binary version of the same variable (equal to one if the residualized prescribing rate is in the top quartile and zero if it is in the bottom quartile) for the endogenous variable of whether a patient filled an opioid prescription within 7 days of the ED visit. All regressions include the full set of fixed effects (hospital-month-year, hospital-day of week, diagnosis, and military service-by-occupational specialty) and the sociodemographic control variables. Doctor shopping is defined as having 7 or more different prescribers in 1 year. Number of prescriptions and long term use exclude the first prescription filled within 7 days of ED visit. Full sample mean and total observations are for the full sample used in Panel A. The number of observations in Panel B is 646,979. *,**, *** display statistical significance at the 10%, 5%, and 1% level respectively.

Table 2: IV Results for Probability of Drug Test Failure

	Drug Test Failure	Drug Test Failure Involving: Opioid with Prescription	Opioid without Prescription	Heroin	Marijuana	Benzos	Other Drugs
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Prescribing Rate Instrument							
Prescription in ED	0.0100***	0.0076***	0.0003	-0.0005**	0.0001	0.0000	0.001
	(0.0033)	(0.0018)	(0.0010)	(0.0002)	(0.0018)	(0.0007)	(0.0016)
Panel B: High vs. Low Instrument							
Prescription in ED	0.0104***	0.0088***	0.0009	-0.0003	0.0002	-0.0003	-0.0003
	(0.0028)	(0.0014)	(0.0009)	(0.0002)	(0.0017)	(0.0006)	(0.0015)
Full Sample Mean	0.029	0.0068	0.0032	0.00024	0.012	0.0016	0.0086
Total Observations	1,045,277	1,045,277	1,045,277	1,045,277	1,045,277	1,045,277	1,045,277

Notes: This table presents coefficients obtained from the second stage of the instrumental variable regressions on the impact of an opioid prescription on the probability of drug test failure in the year after the ED visit. Panel A uses the residualized continuous prescribing rate as an instrument while Panel B uses the binary version of the same variable (equal to one if the residualized prescribing rate is in the top quartile and zero if it is in the bottom quartile) for the endogenous variable of whether a patient filled an opioid prescription within 7 days of the ED visit. All regressions include the full set of fixed effects (hospital-month-year, hospital-day of week, diagnosis, and military service-by-occupational specialty) and the sociodemographic control variables. Full sample mean and total observations are for the full sample used in Panel A. The sample in Panel B is 472,499. *,**, *** display statistical significance at the 10%, 5%, and 1% level respectively.

Table 3: IV Results for Probability of Promotion and Demotion

	Promotion (1 Yr)	Promotion (2 Yr)	Demotion (1 Yr)	Demotion (2 Yr)
	(1)	(2)	(3)	(4)
Panel A: Prescribing Rate Instrument				
Prescription in ED	-0.0135**	-0.0120*	0.0005	0.0019
	(0.0057)	(0.0062)	(0.0027)	(0.0031)
Panel B: High vs. Low Instrument				
Prescription in ED	-0.0137**	-0.0112*	0.0008	0.0027
	(0.0054)	(0.0059)	(0.0026)	(0.0030)
Full Sample Mean	0.28	0.39	0.035	0.047
Total Observations	1,447,750	1,447,750	1,447,750	1,447,750

Notes: This table presents coefficients obtained from the second stage of the instrumental variable regressions on the impact of an opioid prescription on the probability of promotion and demotion within 1 and 2 years after the ED visit. Panel A uses the residualized continuous prescribing rate as an instrument while Panel B uses the binary version of the same variable (equal to one if the residualized prescribing rate is in the top quartile and zero if it is in the bottom quartile) for the endogenous variable of whether a patient filled an opioid prescription within 7 days of the ED visit. All regressions include the full set of fixed effects (hospital-month-year, hospital-day of week, diagnosis, and military service-by-occupational specialty) and the sociodemographic control variables. Full sample mean and total observations are for the full sample used in Panel A. The sample in Panel B is 646,979. *,**, *** display statistical significance at the 10%, 5%, and 1% level respectively.

Table 4: IV Results for Army Physical Fitness Test (APFT) - Army Only

	Take an APFT	Pass an APFT	Z-Score APFT
	(1)	(2)	(3)
Panel A: Prescribing Rate Instrument			
Prescription in ED	-0.0092	-0.0068	-0.0267
	(0.0089)	(0.0062)	(0.0227)
Panel B: High vs. Low Instrument			
Prescription in ED	-0.0149*	-0.0048	-0.0353
	(0.0083)	(0.0057)	(0.0223)
Full Sample Mean	0.57	0.93	-
Total Observations	718,973	410,033	409,946

Notes: This table presents coefficients obtained from the second stage of the instrumental variable regressions on the impact of an opioid prescription on Army Physical Fitness test outcomes in the year after the ED visit. Panel A uses the residualized continuous prescribing rate as an instrument while Panel B uses the binary version of the same variable (equal to one if the residualized prescribing rate is in the top quartile and zero if it is in the bottom quartile) for the endogenous variable of whether a patient filled an opioid prescription within 7 days of the ED visit. All regressions include the full set of fixed effects (hospital-month-year, hospital-day of week, diagnosis, and military service-by-occupational specialty) and the sociodemographic control variables. Full sample mean and total observations are for the full sample used in Panel A. The samples in Panel B are 340,868, 196,274, and 196,225 respectively. *,**, *** display statistical significance at the 10%, 5%, and 1% level respectively.

Table 5: IV Results for Disciplinary Actions - Army Only

	Discipline Flag (1)	Criminal Investigation Flag (2)	Drug or Alcohol Flag (3)
<u>Within 1 Year Following ED Visit:</u>			
Panel A: Prescribing Rate Instrument			
Prescription in ED	0.0108**	0.0008	0.0005
	(0.0052)	(0.0011)	(0.0018)
Panel B: High vs. Low Instrument			
Prescription in ED	0.0110**	0.0012	0.0005
	(0.0051)	(0.0012)	(0.0015)
Full Sample Mean	0.087	0.0058	0.0082
<u>Within 2 Years Following ED Visit:</u>			
Panel A: Prescribing Rate Instrument			
Prescription in ED	0.0084	0.0017	0.0012
	(0.0052)	(0.0015)	(0.0021)
Panel B: High vs. Low Instrument			
Prescription in ED	0.0086	0.0019	0.0008
	(0.0056)	(0.0016)	(0.0019)
Full Sample Mean	0.12	0.0096	0.012
Total Observations	718,973	718,973	718,973

Notes: This table presents coefficients obtained from the second stage of the instrumental variable regressions on the impact of an opioid prescription on the probability of an Army "Suspension of Favorable Personnel Actions (SFPA) Flag" within 1 and 2 years after the ED visit. Panel A uses the residualized continuous prescribing rate as an instrument while Panel B uses the binary version of the same variable (equal to one if the residualized prescribing rate is in the top quartile and zero if it is in the bottom quartile) for the endogenous variable of whether a patient filled an opioid prescription within 7 days of the ED visit. All regressions include the full set of fixed effects (hospital-month-year, hospital-day of week, diagnosis, and military service-by-occupational specialty) and the sociodemographic control variables. Full sample mean and total observations are for the full sample used in Panel A. The sample in Panel B is 340,868. *,**, *** display statistical significance at the 10%, 5%, and 1% level respectively.

Table 6: IV Results for Military Discharges

	Reason for Discharge:								Character of Separation:		
	Any Discharge	Non-Renewal of Contract or Retirement	Discipline	Failure to Meet Weight or Body Fat Standards	Substance Abuse	Other Non-Discipline	Medical	Death	Uncharacterized	Honorable	Non-Honorable
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Within 1 Year Following ED Visit:											
Panel A: Prescribing Rate Instrument											
Prescription in ED	0.0159**	0.0113***	0.0067***	0.0010	-0.0008	0.0007	-0.0011	0.0000	-0.0019	0.0117**	0.0052
	(0.0065)	(0.0036)	(0.0022)	(0.0011)	(0.0017)	(0.0019)	(0.0028)	(0.0004)	(0.0024)	(0.0055)	(0.0033)
Panel B: High vs. Low Instrument											
Prescription in ED	0.0126**	0.0091***	0.0066***	0.0009	-0.0024	0.0003	-0.0006	0.0000	-0.0013	0.0086*	0.0049*
	(0.0055)	(0.0034)	(0.0020)	(0.0010)	(0.0016)	(0.0017)	(0.0025)	(0.0004)	(0.0021)	(0.0049)	(0.0029)
Full Sample Mean	0.180	0.068	0.026	0.0045	0.014	0.017	0.026	0.001	0.022	0.130	0.043
Within 2 Years Following ED Visit:											
Panel A: Prescribing Rate Instrument											
Prescription in ED	0.0126*	0.0071	0.0069**	0.0005	-0.001	0.0022	-0.0016	0.0004	-0.0015	0.0058	0.0084**
	(0.0072)	(0.0047)	(0.0028)	(0.0014)	(0.0021)	(0.0027)	(0.0036)	(0.0006)	(0.0026)	(0.0066)	(0.0038)
Panel B: High vs. Low Instrument											
Prescription in ED	0.0158**	0.0075*	0.0080***	0.0009	(0.0019)	0.0013	0.0008	0.0003	-0.0006	0.0084	0.0092***
	(0.0064)	(0.0045)	(0.0026)	(0.0013)	(0.0020)	(0.0023)	(0.0032)	(0.0005)	(0.0024)	(0.0059)	(0.0034)
Full Sample Mean	0.320	0.140	0.042	0.0077	0.020	0.031	0.054	0.002	0.030	0.250	0.064
Total Observations	1,447,758	1,447,758	1,447,758	1,447,758	1,447,758	1,447,758	1,447,758	1,447,758	1,447,758	1,447,758	1,447,758

Notes: This table presents coefficients obtained from the second stage of the instrumental variable regressions on the impact of an opioid prescription on the probability of military discharge within 1 and 2 years after the ED visit, reason for discharge and "character of separation." Panel A uses the residualized continuous prescribing rate as an instrument while Panel B uses the binary version of the same variable (equal to one if the residualized prescribing rate is in the top quartile and zero if it is in the bottom quartile) for the endogenous variable of whether a patient filled an opioid prescription within 7 days of the ED visit. All regressions include the full set of fixed effects (hospital-month-year, hospital-day of week, diagnosis, and military service-by-occupational specialty) and the sociodemographic control variables. Honorable + non-honorable discharge do not add up to any discharge because there are some discharges that are unclassified. Full sample mean and total observations are for the full sample used in Panel A. The sample in Panel B is 646,979. *,**, *** display statistical significance at the 10%, 5%, and 1% level respectively.

FOR ONLINE PUBLICATION

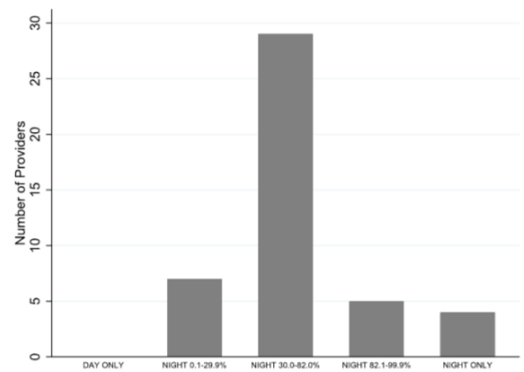
Appendix A1 – Additional Tables and Figures

Appendix Figure 1: Shifts by Time of Day and Weekend for Example Emergency Department

Panel A: Proportion of Shifts by Time of Day and for Weekend, by Provider

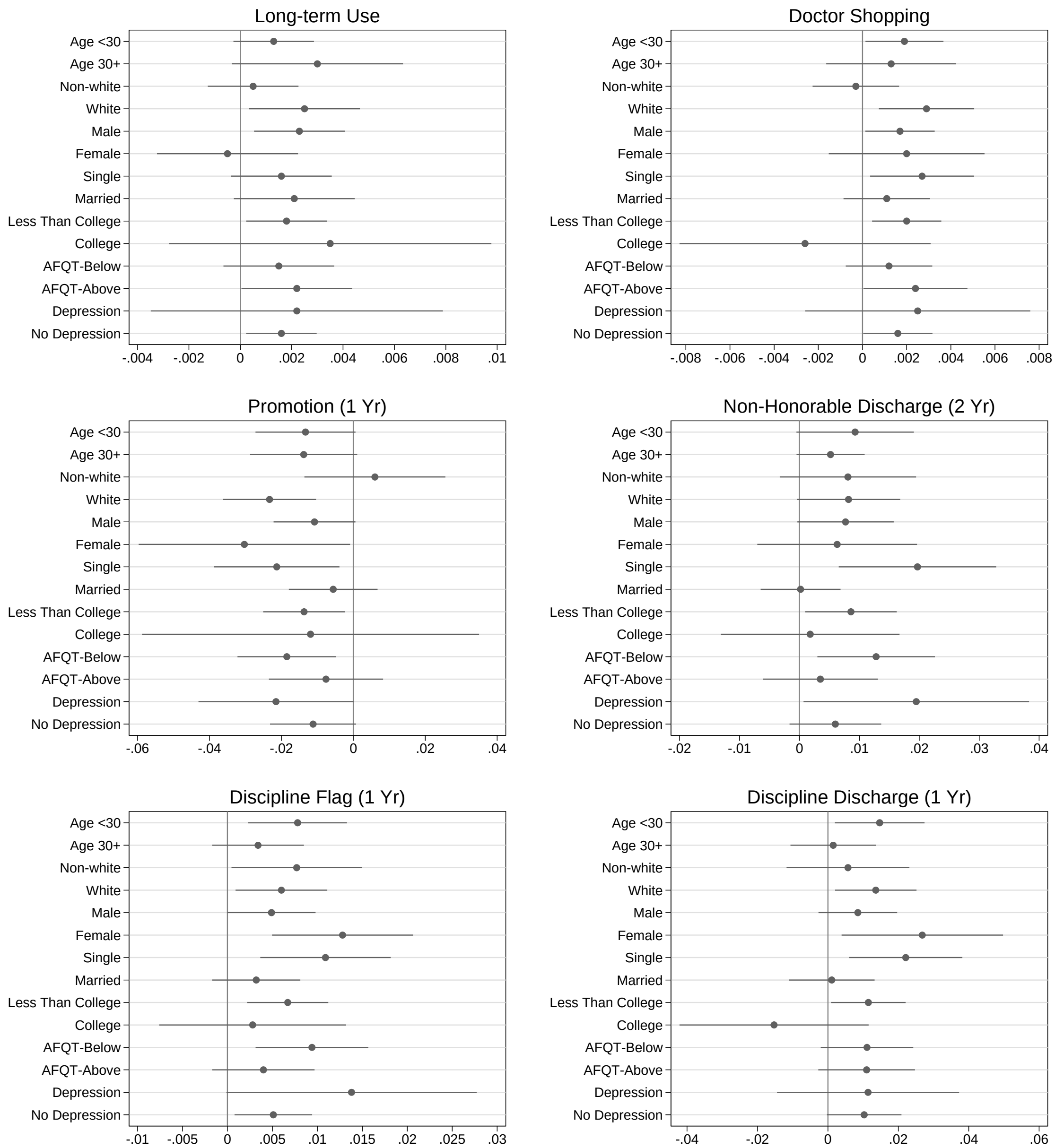
Provider ID	Proportion of Shifts by Time of Day: 7am-3pm	9am-5pm	3pm-11pm	5pm-1am	11pm-7am	Proportion of Shifts on Weekend
1	0.22	0.22	0.44	0.00	0.11	0.33
2	0.33	0.25	0.25	0.08	0.08	0.25
3	0.14	0.36	0.29	0.21	0.00	0.64
4	0.21	0.18	0.32	0.11	0.18	0.21
5	0.11	0.47	0.37	0.05	0.00	0.32
6	0.07	0.29	0.50	0.14	0.00	0.36
7	0.29	0.24	0.33	0.14	0.00	0.24
8	0.50	0.33	0.00	0.00	0.17	0.17
9	0.22	0.28	0.28	0.17	0.06	0.28
10	0.14	0.14	0.41	0.05	0.27	0.32
11	0.19	0.29	0.26	0.16	0.10	0.26
12	0.10	0.29	0.43	0.10	0.10	0.24
13	0.20	0.00	0.75	0.05	0.00	0.30
14	0.09	0.00	0.04	0.00	0.87	0.48
15	0.12	0.16	0.36	0.32	0.04	0.28
16	0.27	0.40	0.00	0.20	0.13	0.13
17	0.00	0.00	0.00	0.00	1.00	0.11
18	0.29	0.21	0.21	0.14	0.14	0.21
19	0.40	0.30	0.20	0.00	0.10	0.10
20	0.19	0.14	0.19	0.43	0.05	0.33
21	0.17	0.25	0.08	0.00	0.50	0.25
22	0.46	0.00	0.33	0.00	0.21	0.17
23	0.53	0.06	0.41	0.00	0.00	0.35
24	0.25	0.38	0.31	0.06	0.00	0.50
25	0.84	0.00	0.16	0.00	0.00	0.32
26	0.25	0.25	0.40	0.10	0.00	0.30
27	0.00	0.00	0.89	0.00	0.11	0.22
28	0.17	0.50	0.17	0.17	0.00	0.33
29	0.08	0.00	0.77	0.00	0.15	0.46
30	0.06	0.00	0.89	0.06	0.00	0.00
31	0.00	0.08	0.00	0.08	0.85	0.15
32	0.32	0.26	0.21	0.11	0.11	0.16
33	0.40	0.30	0.20	0.10	0.00	0.20
34	0.10	0.00	0.00	0.00	0.90	0.30
35	0.79	0.00	0.21	0.00	0.00	0.07
36	0.75	0.08	0.17	0.00	0.00	0.17
37	0.91	0.00	0.09	0.00	0.00	0.55
38	0.27	0.45	0.09	0.18	0.00	0.36
39	0.42	0.08	0.25	0.25	0.00	0.17
40	0.13	0.44	0.25	0.19	0.00	0.25
41	0.15	0.25	0.25	0.20	0.15	0.35
42	0.86	0.03	0.00	0.00	0.11	0.22
43	0.40	0.00	0.40	0.10	0.10	0.30
44	0.00	0.00	0.00	0.00	1.00	0.00
45	0.00	0.00	0.41	0.00	0.59	0.00
Total:	0.27	0.17	0.29	0.09	0.18	0.26

Panel B: Proportion of Shifts During Night Hours



Notes: We collected work schedules for all ED physicians from the largest U.S. military hospital from February-April 2023 to assess how often physicians work during the evening or weekend. Panel A shows the proportion of total shifts worked during each time of day or on the weekend during these 3 months for each physician. We exclude pediatric and "on call" shifts. We exclude physicians who worked fewer than 10 shifts. There are 45 physicians working in this ED over 3 months (on average, 9 per day). It is notable that ED physicians tend to work during almost all shifts during the 3-month period, suggesting a high degree of rotation across shifts as opposed to specialization. Panel B shows the frequency with which physicians work night shifts (i.e., shifts extending beyond 5pm) over these 3 months. Each bar shows the number of providers that work a given proportion of night shifts. Only 4 out of the 45 physicians work only during night shifts and all of the physicians work some night shifts. The distribution of the share of a physician's shifts that occur during night is centered around 0.56, which is the rate expected if physicians were randomly assigned to a day or night shift (this is because night shifts occur 56% of the time). We also show that almost all physicians work weekend shifts close to the rate of 0.26 which is what would occur randomly, which is reassuring that there is rotation across weekend and weekday shifts. Thus, it appears that the majority of physicians are exposed to patients of different types and severity that would be correlated with time of day or day of week due to their significant rotation across shifts. We will further show in our balance tests in Section 5.1 that patient characteristics and presenting diagnoses are uncorrelated with physician opioid prescribing propensities, which provides further evidence that there is limited temporal specialization in the ED.

Appendix Figure 2: Heterogeneity of IV Results



Notes: Figure displays subsample analysis of the instrumental variable results using the prescribing rate instrument for six main outcomes. Non-Honorable Discharge (2 Yr) includes all non-honorable discharges within 2 years of the ED visit ("General", "Other than Honorable," "Bad Conduct", and "Dishonorable").

Appendix Table 1: Descriptive Statistics and Balance Test

		Prescribing Intensity			High Prescribing Intensity		
	Full Sample Mean	Coefficient	Std. Error	p-value	Coefficient	Std. Error	p-value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Patient Characteristics: at time of ED visit							
Age	26.17	-0.116	0.448	0.795	-0.034	0.086	0.687
Female	0.21	-0.030	0.025	0.235	0.001	0.005	0.754
Race - White	0.65	0.007	0.031	0.808	0.001	0.006	0.810
Rank - Junior	0.62	-0.003	0.033	0.932	-0.001	0.006	0.827
College	0.05	-0.006	0.009	0.526	-0.002	0.002	0.271
Marital Status - Married	0.53	0.003	0.023	0.895	0.000	0.004	0.939
Military Tenure (Yrs)	5.44	-0.011	0.357	0.976	0.008	0.069	0.911
AFQT	59.82	-0.047	1.628	0.977	0.190	0.283	0.501
Depression Diagnosis	0.109	-0.006	0.014	0.678	0.004	0.003	0.185
Pre-treatment Outcomes: in 6 months prior to ED visit							
Promotion	0.282	-0.001	0.043	0.976	-0.004	0.008	0.635
Demotion	0.010	-0.002	0.004	0.654	0.000	0.001	0.718
APFT Z-Score	0.000	-0.030	0.087	0.730	-0.014	0.016	0.394
Discipline Flag	0.055	0.003	0.008	0.673	0.003	0.002	0.034
Criminal Investigation Flag	0.003	0.000	0.001	0.842	0.000	0.000	0.790
Drug or Alcohol Flag	0.004	0.002	0.002	0.325	0.000	0.000	0.226
Denied a Security Clearance	0.012	0.003	0.003	0.197	0.001	0.001	0.080
Security Clearance Revoked	0.006	0.001	0.002	0.659	0.000	0.000	0.264
Physical Z-Score	0.000	-0.002	0.034	0.962	-0.001	0.007	0.882
Psych Z-Score	0.000	0.027	0.032	0.397	0.014	0.006	0.025
Observations	1,447,758	1,447,758	1,447,758	1,447,758	646,979	646,979	646,979

Notes: Means in Column 1 are calculated based on the patient's characteristics at the time of the ED visit or in the 6 months prior to the ED visit. Coefficients, standard errors, and p-values stem from a regression with the demographic characteristic as the dependent variable and the residualized instrument (continuous Prescribing Intensity instrument in Columns 2-4 and binary indicator for High Prescribing Intensity in Columns 5-7) as the independent variable. Each cell represents results from a separate regression and standard errors are clustered by physician.

Appendix Table 2: Joint Balance Test of Patient Characteristics and Physician Opioid Prescribing Intensity

	(1)	(2)	(3)
	ED Prescription	Residualized Instrument	Residualized Instrument (incl. time, hospital, Diag. FE only)
Age	0.0028***	0.0000	0.0000
	(0.0002)	(0.0001)	(0.0001)
Race - White	0.0345***	0.0000	0.0000
	(0.0014)	(0.0007)	(0.0007)
Female	-0.0442***	-0.0009	-0.0008
	(0.0017)	(0.0007)	(0.0007)
Rank - Junior	-0.0221***	-0.0002	-0.0002
	(0.0017)	(0.0007)	(0.0007)
Education - College Degree	-0.0290***	-0.0003	-0.0003
	(0.0020)	(0.0007)	(0.0007)
Marital Status - Married	0.0114***	0.0001	0.0001
	(0.0010)	(0.0004)	(0.0004)
Military Tenure (Yrs)	0.0012***	0.0000	0.0000
	(0.0002)	(0.0001)	(0.0001)
Depression	0.0116***	0.0004	-0.0001
	(0.0025)	(0.0006)	(0.0006)
Armed Forces Qualificaiton Test Percen	-0.0004***	0.0000	0.0000
	(0.0000)	(0.0000)	(0.0000)
Observations	1,447,758	1,447,758	1,447,758
F-Statistic	548.81	0.49	0.28

Notes: Column 1 shows a regression of a binary indicator for receiving an opioid prescription on patient characteristics. Column 2 shows the balance test where we regress the continuous physician prescribing rate instrument on patient characteristics. Column 3 is the same as Column 2 except that we construct the instrument using only hospital-month-year, and hospital-day of week fixed effects (i.e., sociodemographic and diagnosis controls are excluded from the instrument). Consistent with random assignment, patient characteristics do not predict the physician opioid prescribing intensity. *,**, *** display statistical significance at the 10%, 5%, and 1% level respectively.

Appendix Table 3: Descriptive Statistics and Balance Test (No Controls)

		Prescribing Intensity			High Prescribing Intensity		
	Full Sample Mean	Coefficient	Std. Error	p-value	Coefficient	Std. Error	p-value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Patient Characteristics: at time of ED visit							
Age	26.17	-0.060	0.447	0.893	-0.009	0.086	0.915
Female	0.21	-0.030	0.025	0.237	0.001	0.005	0.800
Race - White	0.65	0.007	0.031	0.829	0.002	0.006	0.793
Rank - Junior	0.62	-0.006	0.033	0.846	-0.003	0.006	0.645
College	0.05	-0.005	0.009	0.558	-0.002	0.002	0.296
Marital Status - Married	0.53	0.006	0.023	0.796	0.001	0.004	0.801
Military Tenure (Yrs)	5.44	0.040	0.356	0.911	0.031	0.004	0.645
AFQT	59.82	-0.004	0.014	0.753	0.184	0.286	0.521
Depression Diagnosis	0.109	-0.004	0.014	0.753	0.005	0.003	0.121
Pre-treatment Outcomes: in 6 months prior to ED visit							
Promotion	0.282	-0.004	0.042	0.916	-0.006	0.008	0.465
Demotion	0.010	-0.002	0.004	0.637	-0.001	0.001	0.434
APFT Z-Score	0.000	-0.031	0.087	0.719	-0.014	0.016	0.394
Discipline Flag	0.055	0.004	0.008	0.585	0.004	0.002	0.024
Criminal Investigation Flag	0.003	0.000	0.001	0.824	0.000	0.000	0.918
Drug or Alcohol Flag	0.004	0.002	0.002	0.311	0.000	0.000	0.246
Denied a Security Clearance	0.012	0.003	0.002	0.185	0.001	0.001	0.072
Security Clearance Revoked	0.006	0.001	0.002	0.650	0.000	0.000	0.211
Physical Z-Score	0.000	0.001	0.034	0.986	-0.001	0.007	0.886
Psych Z-Score	0.000	0.029	0.032	0.369	0.014	0.007	0.034
Observations	1,447,758	1,447,758	1,447,758	1,447,758	636,358	636,358	636,358

Notes: This table shows coefficients on the continuous Prescribing Intensity variable (Column 2) and the indicator for High Prescribing Intensity (Column 5) from a regression with the demographic characteristic as the dependent variable. The table replicates Appendix Table 1, but does not include any demographic controls when constructing the prescribing intensity instruments. Each cell represents a separate regression with the demographic characteristic as the dependent variable. Standard errors clustered by physician.

Appendix Table 4: Descriptive Statistics and Balance Test for Presenting Diagnosis at the Time of the ED Visit

			Balance Test: Prescribing Intensity		
	Full Sample Mean	Opioid Prescribing Rate	Coefficient	Std. Error	p-value
ED Visit Diagnosis	(1)	(2)	(3)	(4)	(5)
NYU Algorithm Classification					
Definitive Emergency	0.041	0.24	0.005	0.013	0.70
Definitive Non-Emergency	0.033	0.19	-0.001	0.005	0.80
Top 10 Diagnosis Groups					
Acute pharyngitis	0.040	0.15	-0.029	0.010	0.01
Upper respiratory infections	0.036	0.06	-0.013	0.010	0.20
Sprains and strains of ankle and foot	0.032	0.37	-0.014	0.011	0.21
Other symptoms involving abdomen and pelvis	0.030	0.27	-0.014	0.007	0.07
Symptoms involving respiratory system	0.029	0.11	-0.024	0.011	0.03
Symptoms involving digestive system	0.029	0.04	-0.002	0.007	0.82
General symptoms of ill-defined conditions	0.028	0.06	0.013	0.010	0.18
Encounters for administrative purposes	0.028	0.12	0.005	0.013	0.71
Disorders of the back	0.025	0.52	-0.006	0.007	0.40
Sprains and strains of back	0.022	0.48	0.005	0.007	0.46
Observations	1,447,758	1,447,758	1,447,758	1,447,758	1,447,758

Notes: This table displays summary statistics on opioid prescribing for ED visits classified as emergent, non-emergent, and for the top 10 diagnosis groups. Column 1 displays the proportion of ED visits with the diagnosis code. Column 2 shows the opioid prescribing rate for each diagnosis code. Columns 3-5 displays coefficients, standard errors, and p-values from a balance test where we regress whether the patient has the given diagnosis on the continuous physician prescribing rate instrument.

Appendix Table 5: Monotonicity Tests

	Subsample First Stage (1)	Reverse-Sample Instrument (2)
Age < 30	0.8430***	0.4647***
	(0.0094)	(0.0146)
Age 30+	0.9306***	0.8557***
	(0.0123)	(0.0192)
White	0.8735***	0.6500***
	(0.0091)	(0.0177)
Non-White	0.8497***	0.7237***
	(0.0125)	(0.0212)
Female	0.7774***	0.7067***
	(0.0197)	(0.0256)
Male	0.8851***	0.5063***
	(0.0084)	(0.0167)
Single	0.8178***	0.6556***
	(0.0113)	(0.0180)
Married	0.9060***	0.7498***
	(0.0091)	(0.0193)
College	0.8258***	0.8137***
	(0.0252)	-0.0351
Less than College	0.8662***	0.1578***
	(0.0085)	-0.0088
AFQT Below Median Score	0.8813***	0.7307***
	(0.0088)	(0.0160)
AFQT Above Median Score	0.8457***	0.6882***
	(0.0109)	(0.0171)
Depression	0.8785***	0.8024***
	(0.0212)	(0.0271)
No Depression	0.8601***	0.3813***
	(0.0095)	(0.0175)
Full Sample	0.8644***	-
	(0.0086)	

Notes: This table displays results from two versions of the monotonicity test. In Column 1 we estimate the first stage by regressing the probability of receiving an opioid within 7 days of the ED visit on the continuous residualized prescribing rate instrument separately for each of the socio-demographic subsamples. In column 2 we leave out each respective subsample in constructing the residualized prescribing rate instrument and then run the first stage regression on the left-out subsample. *,**, *** display statistical significance at the 10%, 5%, and 1% level respectively.

Appendix Table 6: Frandsen et al. (2023) Test of Joint Monotonicity and Exclusion, by Hospital

Hospital ID	Observations	FLL p-value
41	71,167	0.152
5	55,364	0.647
37	58,404	0.962
29	45,812	0.355
4	47,804	0.132
30	51,364	0.164
20	39,840	0.504
16	35,425	0.043
7	26,083	1.000
14	31,109	0.536

Notes: This table presents results from the Frandsen et al. (2023) test of the joint null hypothesis that the monotonicity and exclusion restrictions hold using the STATA package testjfe. We test the null hypothesis within hospitals for the top 10 hospitals in our data using hospital-month-year and hospital-day of week fixed effects.

Appendix Table 7: IV Results for Doctor Shopping – Robustness of Threshold

Doctor Shopping Threshold:	≥ 4 Providers	≥ 5 Providers	≥ 6 Providers	≥ 7 Providers
	(1)	(2)	(3)	(4)
Panel A: Prescribing Rate Instrument				
Prescription in ED	0.0029	0.0030*	0.0021*	0.0018**
	(0.0022)	(0.0016)	(0.0011)	(0.0008)
Panel B: High vs. Low Instrument				
Prescription in ED	0.0089***	0.0045***	0.0028***	0.0018**
	(0.0020)	(0.0014)	(0.0010)	(0.0007)
Full Sample Mean	0.0233	0.0110	0.0056	0.0029
Total Observations	1,447,758	1,447,758	1,447,758	1,447,758

Notes: This table presents coefficients obtained from the second stage of the instrumental variable regressions on the impact of an opioid prescription on doctor shopping within one year after the ED visit. Panel A uses the residualized continuous prescribing rate as an instrument while Panel B uses the binary version of the same variable (equal to one if the residualized prescribing rate is in the top quartile and zero if it is in the bottom quartile) for the endogenous variable of whether a patient filled an opioid prescription within 7 days of the ED visit. All regressions include the full set of fixed effects (hospital-month-year, hospital-day of week, diagnosis, and military service-by-occupational specialty) and the sociodemographic control variables. Doctor shopping is defined as having 4 or more different prescribers in 1 year in Column 1. Columns 2-4 define doctor shopping as having 5 or more, 6 or more, and 7 or more different prescribers, respectively. Full sample mean and total observations are for the full sample used in Panel A. The number of observations in Panel B is 646,979. *,**, *** display statistical significance at the 10%, 5%, and 1% level respectively.

Appendix Table 8: IV Results for Probability of Obtaining a Security Clearance – Army Only

	Within 1 Year following ED Visit		Within 2 Years following ED Visit	
	Denied a Security Clearance	Security Clearance Revoked	Denied a Security Clearance	Security Clearance Revoked
	(1)	(2)	(3)	(4)
Panel A: Prescribing Rate Instrument				
Prescription in ED	0.0047**	0.001	0.0041*	-0.0011
	(0.0021)	(0.0021)	(0.0024)	(0.0026)
Panel B: High vs. Low Instrument				
Prescription in ED	0.0045**	0.0001	0.0040*	-0.0005
	(0.0021)	(0.0020)	(0.0024)	(0.0023)
Full Sample Mean	0.017	0.012	0.021	0.018
Total Observations	718,973	718,973	718,973	718,973

Notes: This table presents coefficients obtained from the second stage of the instrumental variable regressions on the impact of an opioid prescription on the probability of security clearance outcomes within 1 and 2 years after the ED visit. Panel A uses the residualized continuous prescribing rate as an instrument while Panel B uses the binary version of the same variable (equal to one if the residualized prescribing rate is in the top quartile and zero if it is in the bottom quartile) for the endogenous variable of whether a patient filled an opioid prescription within 7 days of the ED visit. All regressions include the full set of fixed effects (hospital-month-year, hospital-day of week, diagnosis, and military service-by-occupational specialty) and the sociodemographic control variables. Full sample mean and total observations are for the full sample used in Panel A. The sample in Panel B is 340,868.*,**, *** display statistical significance at the 10%, 5%, and 1% level respectively.

Appendix Table 9: Controlling for Intensity of Medical Treatment during ED visit

	Main Results	Controlling for BETOS Number of Tests and Images Propensity	Controlling for BETOS Number of Tests, Images, and Procedures Propensity	Controlling for Log of Work RVU Propensity	Controlling for Medication Propensity
	(1)	(2)	(3)	(4)	(5)
Prescriptions in 1 Year	0.0384**	0.0368**	0.0324**	0.0323*	0.0363**
	(0.0165)	(0.0162)	(0.0163)	(0.0169)	(0.0165)
7 Providers in 1 Year	0.0018**	0.0017**	0.0017**	0.0015*	0.0018**
	(0.0008)	(0.0008)	(0.0008)	(0.0008)	(0.0008)
180 Days of Supply in 1 Year	0.0018**	0.0018**	0.0017**	0.0018**	0.0018**
	(0.0008)	(0.0008)	(0.0008)	(0.0008)	(0.0008)
Promotion in 1 Year	-0.0135**	-0.0128**	-0.0090*	-0.0087	-0.0116**
	(0.0057)	(0.0056)	(0.0054)	(0.0055)	(0.0056)
Discipline Flag in 1 Year (Army Only)	0.0108**	0.0103**	0.0081	0.0102*	0.0104**
	(0.0052)	(0.0052)	(0.0053)	(0.0052)	(0.0052)

Notes: This table presents in the first column the baseline coefficients obtained from the second stage of the instrumental variable regressions on our outcomes of interest: long-term opioid use, promotions and disciplinary flags. Column 1 displays the results from the previous baseline results. Columns 2 and 3 present 2SLS results that include the residualized BETOS control variable that measures the intensity of treatment with the performed number of tests and images during the ED visit and the number of performed tests, images, and procedures during the ED visit, respectively. Column 4 displays results controlling for the residualized log work RVU measure. The last column presents results from controlling for the residualized ED prescribing propensity for anxiety/depression medication. All regressions include the full set of fixed effects (hospital-month-year, hospital-day of week, diagnosis, and military service-by-occupational specialty) and the sociodemographic control variables. *,**, *** display statistical significance at the 10%, 5%, and 1% level respectively.

Appendix A2 – Additional Robustness Tests and Complier Analysis

A2.1 Sensitivity to Control Variables

In Appendix Table 10, we conduct a number of robustness checks to evaluate whether our main IV results hold up to varying the model specification. In Column 1, we present the results excluding all controls except for hospital-year-month and hospital-day of week fixed effects. In Column 2, we add diagnosis fixed effects. In Columns 3-6 we add basic demographic control variables, education, and military occupational variables. The results are generally robust to the set of controls included. The majority of coefficients do not change as control variables are added, suggesting coefficient stability, and some standard errors decrease. Almost all results remain statistically significant at the 5% level. Thus, the control variables may improve precision but do not materially impact our main coefficients, which is expected given random assignment.

A2.2 Unobserved Injury Severity

Our instrumental variable analysis finds worse workforce outcomes for those who received an opioid in the ED. One alternative explanation for these findings may be that these individuals have a (unobservably) more serious or lasting injury that would independently lower job performance. If this was correlated with being assigned to a high propensity opioid prescriber, then this could bias our estimates upward in magnitude. The balance tests presented previously provide strong support that assignment to a high propensity prescriber is unrelated to patient characteristics. However, we additionally test this alternative hypothesis more directly by examining whether there are differences in health status within one year following the ED visit among patients who received or did not receive an opioid prescription in the ED. While receiving an opioid prescription could have a direct causal impact on health status on its own, finding a null effect would suggest both that 1) there is no impact of the opioid itself on health status and 2) patient assignment to physicians is uncorrelated with the severity of the medical condition, supporting the conditional independence assumption. We conduct this test using the Physical Capability Grading System score, which is derived from annual health assessments of Army members. In this assessment, a medical provider conducts a physical exam to assess a member's current health status and to identify any medical conditions. They also conduct a behavioral health screening. In Appendix Table 11, our IV estimates suggest that there is no effect of receiving an opioid prescription on the Z-scores for the physical and behavioral assessments in the year following the ED visit. Thus, members who received an opioid prescription are as physically capable as those who did not receive an opioid prescription, suggesting that the severity of the medical condition cannot explain the lower job performance outcomes we observe. These results are consistent with Table 6, which also showed that opioid receipt did not predict a higher likelihood of being discharged from the military for a medical reason.[38] Taken together, these results suggest that it is the receipt of an opioid prescription rather than the injury or medical condition itself that leads to negative workforce outcomes.

A2.3 Sample Attrition

We also consider how attrition from the military affects our main estimates. About 18% of military members exit the sample within a year of the ED visit due to job separation. We explore how these individuals differ from the sample that does not attrit and we estimate the extent to which these discharges impact our main results. To do this, we re-estimate our results conditioning on members who stay in the military. Given that we find large effects of opioid receipt on job separations, those who leave the military are more likely to be misusing opioids

[38] While it may seem surprising that opioid use would not impact health status, we note that OUD and non-fatal overdoses, which would trigger the largest changes in health care utilization, are rare within the first few years after an ED prescription (Eichmeyer and Zhang (2022)). These null results are also in line with studies in Denmark that showed no increase in sick leave and disability claiming related to opioid prescribing (Laird and Nielsen (2017)).

and have worse job performance. Thus, excluding attriters from our sample would likely understate the effects of receiving an opioid prescription on job performance outcomes. Specifically, we would understate the decrease in promotions and the increase in discipline flags.

Panel A of Appendix Table 12 displays our main results excluding attriters. Column 1 shows the full sample. Column 2 shows results excluding those who exit the military within 6 months after the ED visit and Column 3 excludes those who exit between 6 months and 1 year. Column 2 results are similar to the full sample. Thus, it appears that members who leave the military within six months after the ED visit do so for reasons unrelated to opioid use or adverse performance. This is not surprising given that it would take time for a person to transition from an initial opioid prescription to misuse and negative work behaviors. The results in Column 3 decrease slightly for opioid use and promotions, and there is a somewhat larger decrease for disciplinary flags. These results are consistent with our main results on job separations. They show that members with negative performance outcomes (especially a discipline flag) following an opioid prescription are most likely to be discharged. Thus, excluding these members would slightly understate the negative effects of opioid use on job performance outcomes. However, since these differences are small, our main results are relatively insensitive to whether or not we exclude attriters. Thus, the negative performance effects— reduction in promotions and increase in disciplinary actions—are not driven by the increase in discharges

A2.4 Complier Analysis

Following the approach of previous work (e.g., Dobbie et al. (2018); Eichmeyer and Zhang (2022); Agan et al. (2023), we calculate the share of compliers, never-takers, and always-takers. Never-takers and always-takers are patients who would never (always) receive an opioid prescription regardless of the assigned physician's prescribing intensity. Our 2SLS estimates are relevant for compliers, who are defined as those who receive an opioid because they see a high intensity prescribing physician but would not have received an opioid had they seen a low prescribing physician. The fraction of compliers can be calculated as $\delta(\bar{z} - \underline{z})$ where $\bar{z}$ is the highest prescribing intensity and $\underline{z}$ is the lowest prescribing intensity and δ is the estimated first stage coefficient.[39] Panel A of Appendix Table 13 shows that approximately 37% of patients are compliers, suggesting that the 2SLS estimates are representative of a large proportion of the sample. Always-takers and never-takers make up 5.7% and 57.7%, respectively. These rates are comparable to those reported by Eichmeyer and Zhang (2022). In Panel B we show characteristics of the compliers. The table displays the share of the sample with a given demographic characteristic for 14 subgroups (Column 1), the share in a given subgroup conditional on being a complier (Column 2), and the relative likelihood of the share of the subgroup in the complier sample vs. the full sample (Column 3). Compliers are 10% less likely to have a college degree or a depression diagnosis and 7% less likely to be female, but are otherwise generally similar to the average ED patient, suggesting that compliers are not concentrated in a specific demographic subgroup. In sum, these findings provide additional evidence of external validity given our large and representative complier population.

[39] We define the lowest prescribing intensity as the 1, 1.5, and 2nd percentiles of the residualized prescribing intensity distribution and the highest intensity as the 98, 98.5, and 99th percentiles. We calculate the share of never-takers and always-takers using other moments of the first stage following Agan et al. (2023).

Appendix Table 10: Sensitivity to Control Variable Selection

Specifications: Prescribing Rate Instrument Results	(1)	(2)	(3)	(4)	(5)	(6)
Prescriptions in 1 Year	0.0323*	0.0357**	0.0371**	0.0370**	0.0371**	0.0384**
	(0.0165)	(0.0168)	(0.0166)	(0.0166)	(0.0166)	(0.0165)
7 Providers in 1 Year	0.0017**	0.0018**	0.0018**	0.0018**	0.0018**	0.0018**
	(0.0008)	(0.0008)	(0.0008)	(0.0008)	(0.0008)	(0.0008)
180 Days of Supply in 1 Year	0.0017**	0.0018**	0.0018**	0.0018**	0.0018**	0.0018**
	(0.0008)	(0.0008)	(0.0008)	(0.0008)	(0.0008)	(0.0008)
Promotion in 1 Year	-0.0100	-0.0155*	-0.0142**	-0.0143**	-0.0102*	-0.0135**
	(0.0090)	(0.0079)	(0.0064)	(0.0064)	(0.0058)	(0.0057)
Disciplinary Flag in 1 Year (Army Enlisted Only)	0.0156**	0.0122**	0.0109**	0.0114**	0.0107**	0.0108**
	(0.0063)	(0.0054)	(0.0053)	(0.0053)	(0.0052)	(0.0052)
Fixed Effects						
Hospital-Year-Month	Yes	Yes	Yes	Yes	Yes	Yes
Hospital-Day of Week	Yes	Yes	Yes	Yes	Yes	Yes
Diagnosis	No	Yes	Yes	Yes	Yes	Yes
Military Occupational Specialty (MOS)	No	No	No	No	No	Yes
Controls						
Age	No	No	Yes	Yes	Yes	Yes
Race - White	No	No	Yes	Yes	Yes	Yes
Female	No	No	Yes	Yes	Yes	Yes
Marital Status - Married	No	No	Yes	Yes	Yes	Yes
College	No	No	No	Yes	Yes	Yes
Rank - Junior	No	No	No	No	Yes	Yes
Military Tenure (Yrs)	No	No	No	No	Yes	Yes
AFQT Qualification Test Percentile	No	No	No	No	Yes	Yes

Notes: This table presents coefficients obtained from the second stage of the instrumental variable regressions (using the residualized continuous prescribing rate as the instrument). Each cell represents a separate regression. The outcome variables are number of prescriptions, doctor shopping, long term opioid use, promotions and disciplinary action in the year after the ED visit. The first column includes only hospital-year-month, hospital-day of week fixed effects and each subsequent column adds control variables and/or fixed effects.*,**, *** display statistical significance at the 10%, 5%, and 1% level respectively.

Appendix Table 11: IV Results for Physical Capability – Army Only

	(1)	(2)
	Physical Z-Score	Psych Z-Score
Panel A: Prescribing Rate Instrument		
Prescription in ED	-0.0083	-0.0087
	(0.0208)	(0.0220)
Panel B: High vs. Low Instrument		
Prescription in ED	-0.0083	-0.0088
	(0.0185)	(0.0195)
Total Observations	718,973	718,973

Notes: This table presents coefficients obtained from the second stage of the instrumental variable regressions on the impact of an opioid prescription on physical and psychiatric capability (as measured by the Physical Capability Grading System score) within the year after the ED visit. Panel A uses the residualized continuous prescribing rate as an instrument while Panel B uses the binary version of the same variable (equal to one if the residualized prescribing rate is in the top quartile and zero if it is in the bottom quartile) for the endogenous variable of whether a patient filled an opioid prescription within 7 days of the ED visit. All regressions include the full set of fixed effects (hospital-month-year, hospital-day of week, diagnosis, and military service-by-occupational specialty) and the sociodemographic control variables. The raw physical and psych scale ranges from 1 to 5 with lower scores implying higher capability. Total observations are for the full sample used in Panel A. The sample in Panel B is 340,868.*,**, *** display statistical significance at the 10%, 5%, and 1% level respectively.

Appendix Table 12: Attrition Tests

Panel A: IV Results	Full Sample (1)	Excl. Attrition <6 months (2)	Excl. Attrition 6-12 months (3)
Prescriptions in 1 year	0.0384**	0.0447**	0.0365**
	(0.0165)	(0.0177)	(0.0174)
7 Providers in 1 year	0.0018**	0.0019**	0.0018**
	(0.0008)	(0.0008)	(0.0008)
180 days of supply in 1 year	0.0018**	0.0021**	0.0019**
	(0.0008)	(0.0009)	(0.0008)
Promotion in 1 year	-0.0135**	-0.0136**	-0.0105*
	(0.0057)	(0.0059)	(0.0058)
Discipline flag in 1 year (Army only)	0.0108**	0.0120**	0.0049
	(0.0052)	(0.0055)	(0.0051)

Panel B: Means for Members who Attrit	Full Sample Mean (1)	Attrit <6 months (2)	Attrit 6-12 months (3)
Prescriptions in 1 year	0.3603	0.0233	0.2796
7 Providers in 1 year	0.0030	0.0007	0.0040
180 days of supply in 1 year	0.0028	0.0007	0.0044
Promotion in 1 year	0.2796	0.0233	0.1044
Discipline flag in 1 year (Army only)	0.0867	0.0792	0.1901
Total Observations	1,447,758	121,526	136,944

Notes: This table presents in Panel A coefficients obtained from the second stage of the instrumental variable regressions on the impact of an opioid prescription on long-term opioid use, promotions and disciplinary action in the year after the ED visit. Column 1 displays the results from the previous baseline results for the full sample, while Columns 2 and 3 exclude those who exit the military within 6 months and 6 months to 1 year after the ED visit, respectively. Panel A uses the residualized continuous prescribing rate as an instrument. All regressions include the full set of fixed effects (hospital-month-year, hospital-day of week, diagnosis, and military service-by-occupational specialty) and the sociodemographic control variables. *,**, *** display statistical significance at the 10%, 5%, and 1% level respectively. Panel B displays mean outcomes for the full sample, and those that attrit within 6 months and 6 months to 1 year, respectively.

Appendix Table 13: Characteristics of Compliers

Panel A: Complier Share

	Lowest Propensity Prescriber Percentile		
	1%	1.5%	2%
Compliers	36.6%	34.2%	33.2%
Always Takers	5.7%	5.3%	6.3%
Never Takers	57.7%	60.4%	60.5%

Panel B: Characteristics of Compliers

Characteristics	Pr[X=x]	Pr[X = x|Complier]	Relative Likelihood
Age >= 30	0.248	0.244	0.983
Age < 30	0.752	0.730	0.970
Non-white	0.346	0.333	0.961
White	0.654	0.648	0.990
Male	0.790	0.795	1.007
Female	0.210	0.195	0.928
Single	0.471	0.455	0.967
Married	0.529	0.543	1.027
High School Degree	0.953	0.949	0.996
College Degree	0.047	0.042	0.899
High AFQT Score	0.485	0.480	0.990
Low AFQT Score	0.515	0.515	1.000
No Depression Diagnosis	0.891	0.902	1.013
Depression Diagnosis	0.109	0.097	0.890

Notes: This table presents in Panel A the share of compliers, always takers, and never takers using different prescriber intensity percentiles. Panel B presents for each demographic characteristic the unconditional share, the conditional probability given they are a complier, and the relative likelihood. The method of calculation follows previous work in Eichmeyer and Zhang (2022) and Dobbie et al. (2018).