Provider Opioid Prescribing Behaviors and Opioid Use in Medicaid

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Abstract

Liberal prescription of opioids is widely believed to have contributed to the ongoing epidemic of opioid misuse and related harms within the United States and elsewhere. Policies aimed at curbing the epidemic have focused on encouraging providers to adopt stricter opioid prescribing behaviors. However, the extent to which the association between providers' opioid prescribing behaviors and their patients' opioid use reflects a causal influence of behavior versus patient-provider sorting is unclear. Using Medicaid claims data for three states from 2016-2021, we use provider exits from Medicaid to evaluate how enrollees with chronic pain are affected by a switch to a lower- or higher-prescribing provider. We find that among patients with prior opioid use, switching to lower intensity physicians leads to as much as a 40% decrease in opioid use, with evidence of increased hospitalizations. While we observe a 15% increase in opioid use among opioid-naive enrollees who switch to more intensely prescribing providers, the health effects are less clear. Our findings are similar when using an instrumental variables approach to correct for the potential endogeneity of the destination provider's prescribing intensity.

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1 Introduction

Liberal prescribing of opioids in the early 2000's is by now largely considered to be a key contributor to the early stages of the ongoing epidemic of opioid misuse and associated harms. In an effort to address the soaring rates of overdose and addiction that have accelerated since the 2010's, policymakers have sought to curtail prescribing behaviors. Punitive policies towards high-intensity prescribers and professional guidance discouraging opioid use contributed to prescription rates falling from a peak of 81.3 per 100 people in 2012 to 43.3 per 100 people in 2020.[1] However, overdose from commonly prescribed opioids remained relatively unchanged during this time period, increasing slightly from 4.5 per 100,000 in 2012, to 4.9 per 100,000 in 2020.[2]

The puzzling lack of a relationship between reduced prescribing intensity and opioid overdose calls into question the underlying assumption that provider prescribing behavior drives patient opioid use. While there is a mechanical relationship between whether a provider writes a patient an opioid prescription and whether the patient subsequently has access to prescription opioids, it's much less clear how much provider prescribing style affects the probability that an opioid-naive patient initiates opioid therapy, and how much patients follow provider prescribing on the intensive margin, in terms of dosage and refill frequency. A key challenge in characterizing the nature of this relationship is in disentangling the causal influence of prescribing behavior on patient opioid use from a selection effect that arises from patients sorting to providers whose prescribing behaviors align with their own opioid use preferences.

In this paper, we seek to isolate the causal effect of provider prescribing style on patients' take-up and use of opioids by evaluating how a patient's opioid use changes after switching between providers with different prescribing intensities. We focus on individuals with chronic pain who are enrolled in Medicaid, a program whose members have the highest rates of

[1]Prescription rate data obtained from: https://www.cdc.gov/drugoverdose/rxrate-maps/index.html

[2]Overdose mortality data differentiates between deaths from different classes of drugs, such as synthetic opioids (such as fentanyl), heroin, and commonly prescribed opioids that can only be obtained via prescriptions. Data is obtained from: https://www.cdc.gov/drugoverdose/data/OD-death-data.html

opioid misuse and dependence relative to other insured populations (MACPAC, 2017).[3] We use Medicaid administrative claims data from three states from 2016-2021, leveraging the relatively frequent churn of providers out of Medicaid as a plausibly exogenous shock that compels patients to switch between providers. We define (origin) provider exits as the key event in an event study analysis, and characterize the prescribing intensity of both origin and post-switch (destination) providers by the average daily dose (measured as the morphine equivalent dose, or MED) of the prescriptions filled by the patients that these providers treat over a pre-switch period. We evaluate the extent to which a patient's opioid use (also characterized by average daily MED) changes in the direction of their destination provider's prescribing intensity following the exit-induced switch, relative to contemporaneous trends in opioid use among patients who experience changes in prescribing intensity of a different size. We include non-switchers in our regressions to improve the precision with which we estimate secular trends in opioid use. Using this same strategy, we then evaluate changes in the likelihood of adverse health outcomes, such as overdose and hospitalizations or emergency department (ED) visits, following a change in provider prescribing intensity. Additionally, our patient population has various levels of prior opioid use, and we separately explore how changes in prescribing behavior differentially affect opioid-naive users with little to no previous use, and non-naive prior opioid use patients.

We find that switching to a destination provider with twice the opioid prescribing intensity of the origin provider (approximately a one standard-deviation increase in intensity) shifts a patient's average daily MED by approximately 20% in the direction of their destination provider. The effect is adaptive in nature, such that patients increasingly shift their MED in the direction of their new provider's prescribing intensity until plateauing several months following their origin provider's exit. Further, it is asymmetric, such that the average effect of switching to a more intensive provider is insignificant on a patient's opioid use, while switching to a less intensive provider dramatically and significantly decreases a patient's average daily MED. A switch towards a more intensive provider is much larger for

[3]This reflects a fundamental transformation in the landscape of the opioid epidemic as death rates among urban and racial minorities (whom are disproportionately represented in Medicaid) have begun to outpace those within rural and white populations, representing a significant demographic shift in the most at-risk communities (Altekruse et al., 2020; Hedegaard et al., 2019; Kariisa et al., 2022; Warraich, 2022).

opioid naive patients who have no record of an opioid prescription in the pre-switch period.

An important concern in our baseline results is that they do no account for potentially unobserved time-varying patient preferences for opioid use that influence their choice of a destination provider. To investigate the importance of this concern, we implement a two-stage least squares (TSLS) approach adapted from Abaluck et al. (2021), which uses the origin provider's prescribing intensity to instrument for the potentially endogenous difference in prescribing intensity between destination and origin provider. This approach leverages the idea that provider intensity is likely to regress to the mean following a patient's switch, such that a patient's destination provider is likely to have prescribing tendencies that are closer to the average of the market. Using this strategy as a test of the robustness for our main results, we find that the effect of a switch to a less intensive provider among chronic pain patients is twice as large in magnitude as in our primary event study specification, while a switch to a more intensive provider remains insignificant.

We then explore the implications of a change in provider prescribing behaviors for other downstream health events, such as ED visits, inpatient hospitalizations, and overdoses. While policy and professional guidance against the use of opioids in treating chronic pain patients led to a reduction in opioid prescribing rates and a shift towards non-opioid pain management therapies, providers and advocates have raised concerns that restricting patients' access to prescription opioids would lead to patient harm, particularly among patients with prior use (Bohnert et al., 2018; Dowell et al., 2016; Fenton et al., 2019; Goldstick et al., 2021, 2022; Guy et al., 2017). Anecdotal reports of increases in uncontrolled pain, suicidality, and overdose among patients whose opioid prescriptions were rapidly decreased or suddenly discontinued have reinforced these misgivings (Hoffman, 2018; Kertesz and Varley, 2022; U.S. Food and Drug Administration, 2019).

Using a pooled difference-in-differences version of our event study model to improve statistical power, we find suggestive evidence that enrollees who switch to a provider with relatively lower prescribing intensity are more likely to have a hospitalization. We find additional evidence that patients who switch to providers with a higher opioid prescribing intensity are relatively less likely to have an overdose, and that this is specifically driven by a relative decrease in the likelihood of an overdose due to commonly prescribed opioids.

Additionally, patients who switch to a less intense destination provider are more likely to be diagnosed with an opioid use disorder (OUD), which may reflect both an increased, objective likelihood of OUD, or the subjectivity of such a diagnosis dependent on the provider's access to patient information as well as their own beliefs. Estimates from our TSLS approach generate results of a similar direction and magnitude, though are less precisely estimated.

We view our paper as making three main contributions to the existing literature. First, our study contributes to the understanding of the role of the provider in influencing patient opioid use, particularly by examining this relationship among the most at-risk population. Previous studies have evaluated the impact on opioid-naive patients of being randomly assigned to a provider with high- or low-prescribing intensity, generally finding that patients of high-intensity prescribers have worse outcomes.[4] Barnett et al. (2017) find that opioid-naive Medicare beneficiaries assigned to ED physicians in the top quartile of prescribing intensity are more likely to be observed with long-term opioid use than patients assigned to physicians in the bottom quartile, and that they are also more likely to have an opioid-related hospital encounter due to opioid overdose or a fall or fracture. Eichmeyer and Zhang (2021) and Eichmeyer and Zhang (2022) document a significant increase in the risk of long-term prescription opioid use and opioid use disorder among US veterans with limited to no prior opioid use who are treated by high-prescribing ED and primary care physicians, as well as an increase in opioid overdose mortality and a diagnosis of depression in the ED and primary care settings, respectively.

In contrast, we find suggestive evidence that patients with prior opioid use who switch to low-intensity prescribers tend to have a higher risk of adverse health events, which may reflect a difference in the underlying demographics (Medicaid) or clinical profile (chronic pain patients with varying prior levels of opioid use) between our patient population and the

[4]Most similar in spirit to our own empirical strategy, Laird and Nielsen (2016) exploit patients' moves across municipalities in Denmark to evaluate how patient prescription drug use changes after switching between providers with different prescribing intensities. In addition to observing that switching to a general practitioner with a 10 percentage point higher opioid prescription rate increases a patient's own likely of filling an opioid prescription by 4.5 percentage points, the authors find a significant negative effect on labor income rank and labor force participation. Notably, while Laird and Nielsen (2016) exploit changes in physician prescribing intensity via cross-municipality moves to identify their effect, we seek to hold all else about the patient's environment constant in order to more convincingly argue that the effects we uncover are due to the change in provider prescribing behavior, and not some other change in environment.

opioid-naive populations evaluated in these studies. Further, this dissimilarity may stem from differences in provider access and type. Relative to other insured cohorts, Medicaid enrollees generally have more difficulty in gaining access to providers and face more regular care disruptions due to provider churn that can lead to adverse health events not necessarily found in other populations, such as Medicare (Staiger, 2022). To the extent that the availability and composition of providers differs in Medicaid from other settings, insights generated from patients of other payors may not generalize to Medicaid. This leaves a significant gap in our understanding of drivers of opioid use among the highest-risk population, which we seek to fill.

Second, our study contributes to a growing body of literature that seeks to document and decompose drivers of variation in opioid use and access across the US. Morden et al. (2014) map substantial national-level variation in the proportion of under-65 disabled Medicare beneficiaries who fill six or more opioid prescriptions (ranging from 0.1 to 0.42). They find that chronic users, whose average daily morphine equivalents range from 31 mg to 168 mg, drive the rise in prescription opioid consumption observed across their study period (2007 to 2011). Other studies have found similarly large degrees of variation in opioid prescriptions and use, even among clinically similar patients (Barnett et al., 2017; Eid et al., 2018; Hill et al., 2017; Mikosz et al., 2020). Exploiting state-level variation in rates of opioid abuse, Finkelstein et al. (2021) follow Medicare beneficiaries enrolled in the Social Security Disability Insurance (SSDI) program who move across states to decompose drivers of opioid abuse into place-specific factors, such as provider prescriber behavior and the availability of "pill mills"; and person-specific factors, such as age, mental health status, and prior substance abuse. They find that moving to a state with a 3.5 percentage point higher rate of abuse among SSDI recipients immediately increases the movers own likelihood of abuse by approximately one percentage point, and by an additional 0.3 percentage points in subsequent years. Using a similar strategy of individuals moving across geographic areas with different opioid-related characteristics (commonly referred to as a "movers analysis"), Ericson et al. (2022) evaluate the causal role of healthcare provider and organization fragmentation on risky opioid use, finding a somewhat counterintuitive lack of a causal relationship.

While substantial variation in opioid prescription intensity has been documented, it is not

entirely clear why physicians vary in their prescribing behavior. Schnell and Currie (2018) attribute intensity of prescribing behavior to the quality of medical education a physician receives, finding that physicians who attend higher-ranked medical schools prescribe opioids at lower rates. We seek to add to the understanding of drivers of variation in prescribing behavior with our empirical strategy, which allows us to decompose variation in provider prescribing behavior into patient-side and provider-side components.

Third, our paper contributes to mounting evidence that restricting access to prescription opioids can result in unintended adverse health consequences. Alpert et al. (2018) and Evans et al. (2019) show that the 2010 introduction of an abuse-deterrent version of one commonly-abused prescription opioid, OxyContin, resulted in patients substituting away from prescription opioids towards illicit drugs, increasing deaths attributable to heroin.[5] Similarly, we observe an inverse relationship between access to opioids via one's provider and adverse health events, such as overdose. However, we find that risk of overdose related to commonly-prescribed opioids is most sensitive to changes in access. Overdose due to heroin or synthetic opioids is much less common within our population in general, and changes in overdose risk due to these sources are very small and not significant.

Relatedly, our paper offers a causal insight regarding the relationship between abrupt or rapid discontinuation of opioids and patient well-being. Following a 2016 recommendation by the CDC against the use of long-term opioid therapy for chronic pain patients (Dowell et al., 2016), a large set of studies have sought to evaluate the relationship between opioid dose tapering or discontinuation and adverse health outcomes. Longer-term, higher-dose opioid use was found to be associated with a higher likelihood of undergoing dose-tapering following the CDC publication, as well as an association with an increased risk of overdose, withdrawal, and mental health crises following tapering events (Agnoli et al., 2021; Fenton et al., 2019, 2022; Nataraj et al., 2022; Neprash et al., 2021; Oliva et al., 2020). Our findings provide supportive evidence that these associations are causal.

The paper proceeds as follows. Section 2 provides a background of opioid use and misuse among patients with chronic pain and among Medicaid enrollees. Section 3 outlines our

[5]Schnell (2021) uses the reformulation of OxyContin to characterize physicians by their prescribing behaviors, finding that counties with a higher share of providers who prescribed more OxyContin following the reformulation are associated with increased overdose deaths.

empirical strategy. We discuss the construction of our sample and primary measures in Section 4, and present our results in Section 5. Section 6 discusses alternative specifications and robustness checks. Section 7 concludes.

2 Opioid Use and Misuse Within Medicaid and Among Chronic Pain Patients

Provider prescribing behavior generally features prominently in discussions of the origins of the current opioid crisis. Around the turn of the century, and despite prior misgivings about their addictive potential, providers faced increasing pressure to prescribe opioids to treat pain. This has been largely attributed to i) the publication of (relatively dubious) evidence in the 1980's that opioids were not as addictive as previously believed; ii) the 1996 launch of OxyContin and its aggressive marketing to providers; and iii) the 2001 classification of pain as the fifth vital sign by the Joint Commission[6] (Maclean et al., 2020; Meldrum, 2016).

Liberal prescribing behavior is believed to have played a central role in the "first wave" of opioid deaths that have been increasing since the late 1990's (Centers for Disease Control and Prevention; Maclean et al., 2020). This first wave (mid-1990's-2010) was characterized by a surge in deaths attributable to prescription opioids in particular. In the second wave (2010-2013), deaths due to prescription opioids began to plateau as mortality attributable to heroin began to increase. The third wave (starting in 2013 and ongoing at the time of writing) has seen a surge in deaths related to synthetic opioids, such as fentanyl, that are prescribed or illicitly manufactured, though prescription opioids are still a leading cause of deaths from overdose during this time. Further, the transition from prescription opioid mortality to deaths from synthetic opioids has coincided with a shifting of the epicenter of the epidemic from predominantly rural, white, male populations to low-income, urban, non-white men and women (Altekruse et al., 2020; Lippold et al.; Maclean et al., 2020; McGranahan and Parker, 2021). Fatality is concentrated predominantly among young and

[6]The Joint Commission provides accreditation to healthcare organizations and programs in the US and internationally as part of an effort to improve patient care quality. This accreditation is used by many states as a pre-requisite for reimbursement in the treatment of Medicare and Medicaid patients.

middle-aged adults aged 18 to 59 (Maclean et al., 2020).

While the third wave has seen declining opioid prescribing rates, many patients continue to rely on opioids to manage their pain. In 2019, approximately one in five U.S. adults had chronic pain, and roughly one in five individuals with chronic pain used prescription opioids to manage their acute or chronic symptoms (Dahlhamer et al., 2021; Groenewald et al., 2022). Though there is limited evidence of the efficacy of opioids in improving pain among this population, estimates of misuse among chronic pain patients—defined as the use of opioids that deviates from the use as directed by the prescriber—range between approximately 20% to 30%, while addiction ranges from 8% to 12% (Agency for Healthcare Research and Quality, 2019; Vowles et al., 2015).

Also contemporaneous with the third wave, Medicaid enrollees are now both more likely to receive prescription pain relievers and to experience adverse health events (such as overdose or opioid misuse) from prescription and illegal opioids than any other insured population (MACPAC, 2017).[7] In 2017, 40% of the two million nonelderly US adults with an opioid use disorder (OUD) were Medicaid enrollees (Office of Inspector General, 2020; Orgera and Tolbert, 2019). Moreover, in 2016, approximately 5% of adult Medicaid enrollees in states hardest hit by the opioid epidemic had a diagnosed substance use disorder (Donohue et al., 2019; Substance Abuse and Mental Health Services Administration, 2018).[8] In response to this outsized challenge, Medicaid enrollees with OUD are approximately twice as likely to receive treatment than those who are privately insured, and Medicaid spends the most of any health payer for substance use disorder (SUD) and recovery services (Donohue et al., 2020; Orgera and Tolbert, 2019).[9]

[7]Medicaid is a state-run healthcare payor that is funded jointly by federal and state money, paying for the healthcare of nearly one in five Americans (Keisler-Starkey and Bunch, 2021). While basis of eligibility can vary across states, enrollees tend to be low-income children and adults or medically and/or financially needy seniors and disabled individuals.

[8]In 2018, 10% of Medicaid beneficiaries in six Appalachian states received a prescription opioid (Donohue et al., 2020).

[9]Historically, state Medicaid agencies varied considerably in their approach to OUD treatment coverage. While all states as of 2018 provided reimbursement for (some form of) medication used in Medication-Assisted Treatment (MAT), they varied in their use of certain constraints such as quantity or dosing limits and the requirement of concurrent psychosocial treatment (Substance Abuse and Mental Health Services Administration, 2018). In 2018, nearly 60% of enrollees in 11 states with OUD were receiving medication treatment (MODRN). In that same year, the Substance Use-Disorder Prevention that Promotes Opioid Recovery and Treatment (SUPPORT) for Patients and Communities Act required by law that all states include all forms of drugs approved by the Food and Drug Administration (FDA) to treat OUD, including

Many states have adopted policies to combat OUD by reducing the availability of opioids to Medicaid enrollees, with mixed results. Prescription drug monitoring programs (PDMPs) track opioid and controlled substance prescriptions, limiting dosages and days supplied and requiring prior authorization for providers to prescribe opioids to Medicaid enrollees (Donohue et al., 2019).[10] Mixed evidence exists on the effects of these policies on opioid use and abuse,[11] though policies that mandate registration with and access of these programs (as opposed to allowing voluntary participation) have been linked to meaningful reductions in both outcomes (Grecu et al., 2019; Meara et al., 2016; Neumark and Savych, 2021; Sacks et al., 2021; Wen et al., 2017). Patient review and restriction, or "lock-in," programs restrict Medicaid enrollees to a single prescriber and/or pharmacy if certain behaviors (such as having multiple prescribers of controlled substances) are flagged in an enrollee's claims. However, some evidence suggests that enrollees simply switch to accessing opioids outside of Medicaid, and the effectiveness of such programs on reducing adverse health events, such as opioid-related hospitalizations and overdose deaths, is as of yet unknown (MACPAC, 2020; Roberts et al., 2016). Further, because access to buprenorphine—a well-established treatment for OUD—can also be restricted due to this program, some practitioners argue that lock-in policies reduce patient access to life-saving treatment and may exacerbate race-based health disparities (Incze et al., 2022).

Federally, the Office of Inspector General (OIG) maintains an "exclusions database" that lists providers who are banned from participating in Federally funded healthcare programs, such as Medicare and Medicaid, due to activities such as felony convictions related to the unlawful distribution or prescription of controlled substances, such as opioids (of Inspector General). The number of excluded physicians in this category increased 21% per year on average between 2007 and 2017 (Chen et al., 2018).

In general, outside of obvious offenders (such as "pill mill" operators, or clinics whose providers prescribe opioids for patients regardless of clinical appropriateness), the extent

methadone, which previously had only been reimbursed by 42 states (Centers for Medicare & Medicaid, 2020; Substance Abuse and Mental Health Services Administration, 2018).

[10]Such programs have also been used to flag clinics and providers with "excessive prescribing practices" (Donohue et al., 2019).

[11]In fact, Mallatt (2020) documents a substitution towards illicit opioid markets driven by PDMPs and other similar policies.

to which providers "cause" Medicaid enrollees' opioid use is unclear. Rather, providers prescribing behavior may reflect the opioid use and demands of their patients. Further, there is evidence that such policies—in addition to emerging stigma surrounding opioid use—have a chilling effect on opioid access among patients who may benefit from such therapies (Sinha et al., 2019; Szalavitz, 2022; Zhang et al., 2020). Establishing a better understanding of the influence of providers on patient opioid use (and related outcomes) may provide key insights into crafting more effective policies that promote patient well-being, potentially by facilitating the continued use of opioids among certain patient populations.

3 Empirical Framework

Our goal is to identify the influence of a provider's opioid prescribing intensity on opioid use and downstream health outcomes among Medicaid enrollees with chronic pain. To do so, we evaluate the degree to which a patient's opioid use and health outcomes change after the patient switches between providers with different prescribing intensities, attempting to hold all else constant. We minimize the likelihood that the patient switched for reasons related to their opioid use by specifically examining patients who are compelled to switch providers after their original provider exits Medicaid, and comparing trends in their opioid use following a change in provider prescribing intensity to trends among patients whose provider switches resulted in a different change in intensity. We seek to hold all else (such as patient location) constant in order to isolate the effect of the change in provider prescribing profile on patient opioid use, and to avoid entangling this effect with other factors that have been shown to influence an enrollee's opoid use, such as geography (Finkelstein et al., 2021).

We use a quasi-experimental design that exploits the relatively frequent exits of providers out of Medicaid to estimate the change in patient i's use of opioids after switching from an origin provider o with opioid prescribing intensity ρ_o, to a destination provider d with opioid prescribing intensity ρ_d. For each patient i, we define the opioid-prescribing intensity of their provider p as a leave-one-out-mean of the average daily strength of opioid prescriptions among their other patients with chronic pain in a pre-period. Specifically, our primary measure of provider opioid prescribing behavior is the average daily morphine equivalent

dose (MED) associated with provider j's patients, excluding patient i, in a "pre-period" defined as the 12 to 7 months prior to the provider's exit.

$$MED_{p(i)} = \frac{\sum_{i'=1,i'\neq i}^{N_p} MED_{i'}}{\sum_{i'=1,i'\neq i}^{N_p} i'}$$

We index MED using $p(i)$ to clearly illustrate that this provider-level measure is calculated for each specific patient i. However, for ease of exposition, we will simply use the index p below.

Following Staiger (2022), our origin and destination providers of interest are the "key providers" (medical doctors [MD] or doctors of osteopathy [DO]; or nurse practitioners [NP] or physician assistants [PA]) of any specialty who are listed as the servicing provider on the plurality of the patient's claims during a given time period. Specifically, we identify a patient's origin provider as the provider responsible for the plurality of office visit claims during the 12- to 7-months prior to that origin provider's exit (below, we explain how our observation of provider behavior prior to their exit informed the selection of this 6-month period). Similarly, we identify a patient's destination provider as the clinician responsible for the plurality of office visit claims in the 12-month post-exit-period. Figure A.2 plots the share of claims associated with the origin and destination provider, relative to the origin provider's exit.[12] The decrease in the share of claims associated with the origin provider starting at approximately 6 months prior to exit and continuing to the month of exit is consistent with our observation of the tapering trend in the number of patients an exiting provider is observed treating relative to their exit (Figure A.1).

Figures A.2 and A.1 inform our characterization of the relevant time periods in our study. We define the 12 to 7 months prior to the origin provider's exit as the pre-period, during which time the patient is primarily treated by the origin provider relative to the destination provider. We define the 6 to 0 months prior to the exit as the transition period, during which time the patient transitions to receiving relatively more care from the destination provider. Finally, we define the 12 months following the exit (exclusive of month 0) as the post-period, during which time the patient receives no more care from their origin provider, and during

[12]Note that these shares include months in which the patient has no visits.

which we observe that the plurality of their claims are attributed to the destination provider. In additional analyses described below, we explore the robustness of our results to defining the main event as the last visit a patient has with their origin provider, which can be several months prior to that provider's exit.

To build intuition around our main empirical strategy, we start by introducing a simple event study model. Consider the volume of prescription opioids, in terms of milligram (mg) equivalents, patient i has access to via prescription fills in time t, y_{it}, as a function of time-invariant patient attributes, α_i; time-varying characteristics of the patient, X_{it};[13] calendar time fixed effects, τ_t; and fixed effects for time relative to the provider's exit in the event study, which occurs at $t' = 0$:

$$y_{it} = \alpha_i + \sum_{q=-\mathcal{T}, q\neq -1}^{\mathcal{T}} \delta_q \mathbb{1}\{t' = q\} + \beta_n X_{it} + \tau_t + \varepsilon_{int}$$

As long as there is no omitted variable that is correlated with both the timing of the provider's exit and the probability of opioid use, our estimates of δ_q will be causal. In other words, as is standard in two-way fixed effects models, we assume that but-for the exit of the provider, trends in patient i's opioid use would have been parallel to the trends of other patients whose provider does not exit. We can then interpret δ_q as the average causal effect of switching between providers on a patient's opioid use, though without necessarily commenting specifically on how the change in prescribing intensity affects y_{it}. However, this simple event study is not ideal when our sample includes patients that switch from both high-intensity to low-intensity providers and vice versa, as the effects of the switch on differences in patients' opioid use may cancel each other out, leading us to estimate a null δ_q that masks the actual effect of provider prescribing intensity. Panel (a) of Figure A.3 supports this concern, reporting the null results obtained from a pooled estimation.

We can instead estimate the model separately on patients who switch to more versus less intense providers. Panels (b) and (c) of Figure A.3 illustrate that when estimated as such, we observe that switching to less or more intense providers decreases and increases opioid use, respectively. Note that a patient switching from a low-intensity provider to a high-

[13] *This draft does not control for any time-varying characteristics.*

intensity provider (for example) may increase their own opioid use for two reasons. First, the increased intensity of their destination provider may cause them to increase their own use of opioids. Second, they may have a preferred level of opioid use that was higher than their origin provider's prescribing intensity, and, by switching to a higher-intensity provider, they are now able to use opioids closer to their preferred level. Regardless of which story drives their change in opioid use, we interpret an observation of a non-zero change in y_{it}, in combination with the parallel trends assumption being met, as a evidence of a causal relationship between provider prescribing intensity and patient opioid use.

The magnitude of the change in provider intensity likely matters in terms of the effect size. To explore whether this is true, Figure A.4 reports the δ_q's estimated on a sample of patients who switch from below-median to below-median intensity providers (panel a); above-median to above-median intensity providers (panel b); above-median to below-median intensity providers (panel c); and below-median to above-median intensity providers (panel d). Panels (a) and (b) show little to no evidence of a change in opioid use, which follows intuitively from switching between providers of similar intensity.[14] Panels (c) and (d) illustrate a significant decrease and increase in opioid use when patients switch to below- and above-median intensity providers, respectively.

While this approach provides additional insight into large-magnitude changes in prescriber intensity (panels c and d) versus smaller changes (panels a and b), it still does not use all of the variation in changing prescriber intensity that is available to us. Instead, we can more flexibly relate the change in provider intensity between origin and destination provider to the change in a patient's opioid use with the following empirical design.

[14]Note that patients in panel (a) slightly increase their opioid use over time, while patients in panel (b) slightly decrease their opioid use over time. We speculate that this is due to switching to a provider who is on average more and less intense, respectively. Specifically, as we exploit in our two-stage least squares approach below, there is a "regression to the mean" feature of these markets, such that patients whose origin providers are below-median intensity are likely to switch to more average providers (i.e. with greater intensity, though still below median by definition), and vice versa for patients whose origin providers are above median.

3.1 Event Study and Change in Provider Intensity

This empirical strategy (often referred to as a "movers analysis") identifies the effect of a provider's prescribing intensity on a patient's opioid use by observing how a patient's opioid use changes following the change in their provider's prescribing intensity (which is induced by the origin provider's exit). Let patient i switch from origin provider p to destination provider p' at time of exit, $t' = 0$. The difference between p and p''s prescribing intensity is:

$$\Delta_p = MED_{p'} - MED_p$$

where both $MED_{p'}$ and MED_p are calculated in the pre-period prior to the origin provider's exit.[15]

As in Finkelstein et al. (2016), we model opioid use y of patient i treated by provider p in time t as:

$$y_{ipt} = \tilde{\alpha_i} + \alpha_p + \tau_t + \sum \gamma_q \mathbb{1}\{Q_{pt} = q\} + \varepsilon_{ipt} \tag{1}$$

where opioid use is a function of patient fixed effects, $\tilde{\alpha_i}$; provider fixed effects, α_p (which include prescribing intensity, ρ_p, in a specific way that we discuss below); calendar time fixed effects, τ_t; and relative time fixed effects, where γ_qs are the coefficients associated with relative time periods Q_{pt} for patients whose provider exits in $q = 0$. Notably, the patient fixed effects allow patients whose providers exit to be systematically different from patients whose providers don't exit in time-invariant ways (such as preference for opioid use). Additionally, the relative time fixed effects allow for the exit itself to affect opioid use.

Following Abaluck et al. (2021), we can decompose α_p—the true effect of provider p on patient i's opioid use (i.e. what we would estimate if we randomly assigned patients to providers)—into two parts: i) the effect of observed provider prescribing intensity, ρ_p; ii) and some other provider-specific effect not related to prescribing intensity, η_p:

$$\alpha_p = \theta\rho_p + \eta_p \tag{2}$$

[15] This can also be done where MED_p is calculated after the switch, and $MED_{p'}$ is calculated before the switch.

where, by definition, $Cov(\rho_p, \eta_p) = 0$.[16] Abaluck et al. (2021) refer to θ as the "forecast coefficient" and η as the "forecast residual." The forecast residual can be interpreted as explaining why we might observe an effect on opioid use that is different than what we would expect given ρ. This may occur because we estimated ρ based on an atypical set of patients that a provider was treating at the time they were included in a treated patient's set of potential destination providers. For example, if the set of potential destination providers available to high-opioid users following the exit of their origin providers had generally lower ρs because they were treating lower-acuity patients who required (or requested) fewer opioids, then the bias associated with observed ρ is atypical of the normal bias associated with our measure of provider prescribing intensity (due to atypical patient composition).

Our primary goal is to evaluate the effect of a provider's prescribing intensity on patient opioid use and downstream health events. To illustrate the identification strategy used in service of this goal, Finkelstein et al. (2016) reconsider Equation 1 in a simple pre-post setting in which all calendar and relative time fixed effects are set to 0:

$$\begin{aligned} y_{io,pre} &= \tilde{\alpha_i} + \alpha_o + \varepsilon_{io,pre} \\ y_{id,pre} &= \tilde{\alpha_i} + \alpha_d + \varepsilon_{id,post} \\ \Delta_y &= \Delta_o^d + \Delta_\varepsilon \end{aligned}$$

where $\Delta_o^d = \alpha_d - \alpha_o$. Specifically, after differencing out time-invariant characteristics of patient i (α_i), the change in patient i's opioid use after switching from origin provider o_p to destination provider d_p (Δ_y) identifies the difference in origin and destination providers' affect on patient i's opioid use (Δ_o^d). With a sufficiently large number of patients switching between o_p and d_p, Δ_y is a consistent estimator for Δ_o^d. In words, holding constant patient fixed effects, the observable change in opioid use can be attributable to provider effects,[17] of

[16] Note that observed provider prescribing intensity ρ_p is a function of a provider's preferred prescribing behavior, and a patient's preferred opioid use. In Appendix D, we tie this more explicitly to the decomposition used in Finkelstein et al. (2016) to illustrate why θ can also be interpreted as the share of the variation in provider prescribing intensity attributable to provider-side components.

[17] Note that it is necessary to observe moves between multiple origin and destination providers in order to identify Δ_o^d, since allowing for the exit itself to have a direct affect on opioid use (i.e. $\theta_{r(i,t)} \neq 0$) would result in Δ_y including that effect as well. Thus, we require that patients switch from p' to p and from p to p'.

which prescribing intensity is a component. To determine the magnitude of this component, we use the decomposition given in Equation 2:

$$\Delta_y = \theta\Delta_\rho + \Delta_\varepsilon + \Delta_\eta \tag{3}$$

We can rewrite Equation 1 as an event study in the following way:

$$y_{it} = \tilde{\alpha_i} + \theta\rho_{it} + \tau_t + \sum \gamma_q \mathbb{1}\{Q_{pt} = q\} + \varepsilon_{it} + \eta_{it}$$

where ρ_{it} is the realization of potential ρ_p when patient i is treated by provider p in time t (and similarly for η_{it}). In a pre-post framework, again setting all calendar and relative time fixed effects to 0, we can write:

$$\begin{aligned} y_{io,pre} =& \tilde{\alpha_i} + \theta\rho_o + \varepsilon_{it} + \eta_{it} \\ y_{id,post} =& \tilde{\alpha_i} + \theta\rho_d + \varepsilon_{it} + \eta_{it} \\ =& \tilde{\alpha_i} + \theta\rho_d + \{\theta\rho_o - \theta\rho_o\} + \varepsilon_{it} + \eta_{it} \\ =& \tilde{\alpha_i} + \theta\Delta_\rho + \theta\rho_o + \varepsilon_{it} + \eta_{it} \end{aligned}$$

Redefining patient fixed effects as $\alpha_i = \tilde{\alpha_i} + \theta\rho_o$, we can equivalently write our main estimating equation:

$$y_{it} = \alpha_i + \sum \theta_q \mathbb{1}\{Q_{pt} = q\} * \Delta_\rho + \sum \gamma_q \mathbb{1}\{Q_{pt} = q\} + \tau_t + \varepsilon_{it} + \eta_{it} \tag{4}$$

where $\Delta_\rho = \hat{\rho}_d - \hat{\rho}_o$ (and $\hat{\rho}_p$ is the sample analog of ρ_p); γ_q are the coefficients associated with indicators for time relative to exit (where $q = 0$ denotes the time of the exit); and θ_qs are the coefficients associated with relative time indicators interacted with Δ_ρ. The key coefficients of interest are the θ_qs, which describe how changes in a patient's opioid use before and after their provider's exit relate to changes in the intensity of their provider's prescribing behavior, relative to other patients who experience changes in prescribing intensity of different magnitudes. In our analysis, we log transform patient MED and provider MED to interpret coefficients in terms of percent changes. The γ coefficients are informative of common trends

in MED experienced by all treated patients relative to the provider's exit, for example due to disrupted care, regardless of the change in provider risky prescribing.[18]

To build intuition around the interpretation of the θ_qs, consider the following "extreme" examples. Say that we observe no pre-trends in the pre-period, i.e. all $\theta_{q\leq 0} = 0$. If we observe that all θ_qs in the post-period are also 0, then providers have no influence on their patient's use of opioids; patient opioid use is determined 100% by the patient. If we instead observe that all θ_qs in the post-period are equal to 1, then providers have complete influence over their patient's opioid use; patient opioid use is determined 100% by the provider (and 0% by the patient). Observing post-period θ_qs that are between 0 and 1 suggests that both patients and providers have some role in determining patient opioid use, where the magnitude of the coefficient describes the degree to which providers influence patient opioid use. The slope of the line post-exit is also informative, and tells us whether the provider's effect is sharp and consistent (i.e. a jump on a switch, and then a flat line across time), or whether it is gradual (i.e. a line that gradually slopes up or down). As noted above, Appendix D discusses an alternative interpretation of $\theta_{q>0}$—i.e. the jump in opioid use following patient i's switch to destination provider d_p—as the share of the observed variation in provider opioid prescribing behavior that is attributable to providers (with $1 - \theta_{q>0}$ being the share attributable to patients). This interpretation is useful in understanding the relative magnitudes of the contributions of provider- and patient-specific components in regional variations of opioid prescribing rates.

The θs can be interpreted as causal only if Δ_ρ interacted with the relative quarter indicators are uncorrelated with any unobserved patient or provider characteristics (such as those in the error terms). To illustrate this, consider the difference-in-differences version of Equation 4:

$$y_{it} = \alpha_i + \theta Post_q * \Delta_\rho + \gamma Post_q + \tau_t + \omega_{it} \tag{5}$$

where $\omega_{it} = \varepsilon_{it} + \eta_{it}$. Then, θ is causal only if $Cov(Post_q * \Delta_\rho, \omega_{it}) = 0$. While this is not

[18]Staiger (2022) provides evidence that provider exits in this setting have a direct effect on patient utilization. Specifically, the author shows that following their primary provider's exit from their managed care network, Medicaid enrollees experience a reduction in primary care visits and an increase in hospitalizations that is exacerbated by the presence of chronic conditions. Thus, we control for the disruption caused by a provider's exit in order to avoid conflating disruption effects with the effect from the change in prescribing intensity.

directly testable, we can use a standard parallel trends test to determine whether opioid use in the pre-switch period for a patient who experienced specific Δ_ρ differed in a systematic way.

It is worth noting that the assumptions we make here are subtly different than in a standard two-way fixed effects setting. Consider a difference-in-differences model that does not scale relative time by Δ_ρ:

$$y_{it} = \alpha_i + \gamma Post_q + \tau_t + \varepsilon_{it} + \eta_{it} \tag{6}$$

To interpret γ_qs as causal, we assume that, if not for the treatment, patient i's opioid use would have trended similarly to opioid use among untreated patients. In a model such as Equation 5 with relative time scaled by Δ_ρ, the assumption is slightly different. Specifically, the counterfactual we seek to test is that if patient i who experienced a change in provider intensity of magnitude $\Delta_{p,1}$ had instead experienced a change of $\Delta_{p,2}$, their opioid use would have trended similarly. Hull (2018) formalizes this, and other assumptions necessary to interpret θ as causal. Notably, this statement does not make an assumption with regards to comparable trends among patients whose providers don't exit.

Observing that $\theta_{q<0}$ is (approximately) 0 provides support for the interpretation of $\theta_{q>0}$ as causal. However, consider that patients may not be randomly assigned to providers (origin and destination). Assume that patients have information on the prescribing intensity of a provider, and that at least some patients select providers whose prescribing intensity aligns with their own preferences. If these preferences are time-invariant, then they will be reflected in level (opioid use) differences accounted for in patient fixed effects α_i, and will not bias our estimates of θ_q. If these preferences are altered due to the exit itself, then in a standard event study specification without $\theta_q\Delta_\rho$, $\gamma_{q>0}$ will be biased by this selection, even if we don't observe any evidence of pre-trends. In our preferred specification (with $\theta_q\Delta_\rho$), we simply require that changes in preference (or anything that influences a patient's selection of a destination provider) cannot systematically vary with specific changes in prescribing intensity.

Further, Δ_ε may reflect a change in (implicit) preference for providers of a particular

prescribing intensity. For example, consider that patients who had high-intensity origin providers running a "pill mill" may be systematically assigned by their managed care organization (or the state Medicaid agency) to providers who prescribe very few opioids to their patients, in an effort to decrease their use.[19] In this instance, our observed θ_qs in the post-period would reflect both the causal effect of the provider, plus some selection effect due to potentially at-risk patients being assigned to these providers.

To state these concerns more formally, we can consider the covariance of Δ_ρ with ε_{it} and η_{it} separately. To do so, we use the intuition provided in Equation 3 to generate the following assumptions:

1. $Cov(\Delta_\rho, \Delta_\varepsilon) = 0$. This assumption states that a particular change in provider prescribing intensity cannot be systematically correlated with a change in some unobservable patient attribute (such as addiction or acuity of illness). This assumption would be violated, for example, if patients who experience large decreases in opioid use were also systematically more likely to begin increasing their opioid use after becoming addicted in the pre-period. We test this assumption by examining whether any trends exist prior to the provider's exit. We discuss addiction in more detail in Appendix B.

2. $Cov(\Delta_\rho, \Delta_\eta) = 0$. This assumption states that a particular change in provider prescribing intensity cannot be systematically correlated with a change in how patients select a destination provider in a way that is unrelated to provider prescribing behavior (ρ). In other words, patients who experience a given Δ_{p1} cannot systematically choose a destination provider differently than patients who experience a given Δ_{p2}.[20]

To summarize, conditional on the absence of pre-trends, we interpret the θ_qs estimated from Equation 4 as informative of how a change in provider prescribing behavior due to

[19] A similar intervention occurs within lock-in programs, described above.

[20] We can use the definitions of Δ_ρ and Δ_η to be more precise about what we are testing:

$$\begin{aligned} Cov(\rho_d - \rho_o, \eta_d - \eta_o) =& Cov(\rho_d, \eta_d) - Cov(\rho_d, \eta_o) - Cov(\rho_o, \eta_d) + Cov(\rho_o, \eta_0) \\ =& 0 - Cov(\rho_d, \eta_o) - Cov(\rho_o, \eta_d) + 0 \\ =& -1\Big(Cov(\rho_d, \eta_o) + Cov(\rho_o, \eta_d)\Big) \end{aligned}$$

Note that if $Cov(\rho_o, \eta_d) = -Cov(\rho_d, \eta_o)$, then $Cov(\Delta_\rho, \Delta_\eta) = 0$ and there is no bias from Δ_η in our estimate of θ_q.

patient selection or re-assignment to providers (causally) affects a change in opioid use. If $Cov(\Delta_\rho, \Delta_\eta) \neq 0$) or $Cov(\Delta_\rho, \Delta_\varepsilon) \neq 0$ (i.e. the pre-trends assumption holds), then this estimate represents the sum of two components: i) the causal effect of the provider's prescribing intensity on patient opioid use; and ii) the effect of a selection; namely, a change in preference among patients who experience certain types of changes in prescribing intensity. To isolate the part of Δ_ρ that is causal with respect to providers' prescribing intensity, we turn to the two-stage least squares approach proposed by Abaluck et al. (2021).

3.2 Two-Stage Least Squares

In this section, we adapt the approach described in Abaluck et al. (2021)—which uses cross-sectional data—to the panel data setting of our study. Our goal is to isolate the causal effect of provider prescribing intensity on patient opioid use, using a specific feature of the relationship between a patient's origin and destination provider intensity (discussed in more detail below) to isolate the target causal relationship. This is similar to the approach used by Agha et al. (2020).

Specifically, we instrument for potentially endogenous Δ_ρ by exploiting the "regression to the mean" relationship between origin provider intensity, ρ_o, and destination provider intensity, ρ_d. This phenomena predicts that patients who experience an origin provider's exit will have a destination provider with a prescribing intensity that is more typical of the average prescribing intensity of the local market. Thus, we expect to observe a negative relationship between ρ_o and ρ_d and, since Δ_ρ is a function of ρ_d, we expect to observe a negative relationship between ρ_o and Δ_ρ.

We define our instrument as $\rho_o \times Post_{it}$. Per Abaluck et al. (2021), the following three assumptions must hold for the IV approach to be valid in terms of returning an unbiased λ (note that these are different assumptions than in standard TSLS). We discuss these in the context of the simplified two-period difference-in-differences relationship given in Equation 3.

1. **First Stage Assumption**, $Cov(\tilde{\rho_o}, \Delta_\rho) \neq 0$

$$\Delta_\rho = \mu^{FS} \rho_{o(i)} + \Delta_u \qquad \text{(First Stage)}$$

In terms of the difference-in-differences version of our main estimating equation given in Equation 5, we can rewrite this first stage as:

$$\rho_{d(i)} \times Post_{it} = \alpha_i^{FS} + \theta^{FS}\rho_{o(i)} * Post_q + \gamma^{FS} Post_q + \tau_t^{FS} + u_{it}$$

The first stage assumption requires that a relationship exists such that $\theta^{FS} \neq 0$. Furthermore, as in Abaluck et al. (2021), we require that patients who are forced to switch providers because of an exit switch to a destination provider who is more typical of the local market average. Thus, for switchers, we expect $\theta^{FS} < 0$. We also expect that patients whose providers do not exit will remain with their providers due to inertia.

2. **Balance Assumption**, $Cov(\tilde{\rho_{o(i)}}, \Delta_\epsilon) = 0$. In words, this assumption requires that origin provider intensity for patients whose providers exit be (conditionally) uncorrelated with unobserved shocks to opioid use. Our patient fixed effects allow for patients to select origin providers based on unobserved preferences, as long as those unobserved preferences are time invariant.

   In our preferred empirical specification, the reduced form is:

$$y_{it} = \alpha_i^{RF} + \theta^{RF}\rho_{o(i)} \times Post_{it} + \gamma^{RF} Post_{it} + \tau_t^{RF} + \varepsilon_{it}^{RF} + \eta_{it}^{RF} \tag{7}$$

   In the difference-in-differences framework, we can rewrite this as:

$$\begin{aligned} y_{i,pre} =& \alpha_i^{RF} + \tau_{pre}^{RF} + \varepsilon_{i,pre}^{RF} + \eta_{i,pre}^{RF} \\ y_{i,post} =& \alpha_i^{RF} + \theta^{RF}\rho_{o(i)} + \gamma^{RF} + \tau_{post}^{RF} + \varepsilon_{i,post}^{RF} + \eta_{i,post}^{RF} \end{aligned}$$

   Setting calendar and relative time trends to zero, and subtracting post from pre, we get:

$$\Delta_y = \theta^{RF}\rho_{o(i)} + \Delta_\varepsilon^{RF} + \Delta_\eta^{RF} \qquad \text{(Reduced Form)}$$

   Further, as in Abaluck et al. (2021), we can illustrate this assumption in the following

hypothetical (infeasible) difference-in-differences specification:

$$\varepsilon_{it} = \Phi_i + \Phi_Z \rho_{o(i)} \times Post_{it} + \Phi_{post} Post_{it} + \Phi_t + v_{it} \tag{8}$$

Then, $Cov(\tilde{\rho_{o(i)}}, \Delta_\varepsilon) = 0$ only if $\Phi_Z = 0$. We allow for provider exits to have direct disruption effects that might be correlated with unobserved shocks to opioid use, i.e. Φ_{post} may not be 0, as long as the effect is not systematically related to observed provider prescribing intensity (i.e. Φ_Z must be 0).

While it's not feasible to estimate Equation 8, we can use the fact that $\Delta_\eta = \eta_{d(i)} - \eta_{o(i)}$ and $Cov(\rho_{o(i)}, \eta_{o(i)}) = 0$ by definition, to evaluate $Cov(\rho_{o(i)}, \Delta_\varepsilon^{RF})$ in the pre-period. In other words, we can implement the standard parallel trends test for the reduced form as a way to check whether there are any systematic differences in trends in opioid use that might be correlated with a particular value of $\rho_{o(i)}$. In this context, absent the origin provider with particular ρ_1 they actually had, that patient would have experienced the same trends in y_{it} had they had an origin provider with ρ_n, where $n \in [0, N-1]$ (and $N-1$ are the total values of ρ that exist). Figure A.8 reports a lack of statistically significant pre-trends in the reduced form specification, which we interpret as evidence in support of our identification strategy.

3. **Fallback Condition**, $Cov(\tilde{\rho_{o(i)}}, \Delta_\eta) = 0$. Of note, since $\Delta_\eta = \eta_{d(i)} - \eta_{o(i)}$, we can re-write this condition as:

$$\begin{aligned} Cov(\tilde{\rho_{o(i)}}, \eta_{d(i)} - \eta_{o(i)}) =& Cov(\tilde{\rho_{o(i)}}, \eta_{d(i)}) - Cov(\tilde{\rho_{o(i)}}, \eta_{o(i)}) \\ =& Cov(\tilde{\rho_{o(i)}}, \eta_{d(i)}) \end{aligned}$$

since $Cov(\tilde{\rho_{o(i)}}, \eta_{o(i)}) = 0$ by definition.

In words, this condition requires that patients with a particular origin provider intensity whose provider exits do not systematically select destination providers differently from patients with an origin provider of a different intensity (other treated patients),

conditional on controls (including patient fixed effects). More formally:

$$\eta_{it} = \phi_i + \phi_Z \rho_{o(i)} \times Post_{it} + \phi_{post} Post_{it} + \phi_t + n_{it}$$

Similar to above, for this condition to hold, ϕ must be 0; having an origin provider with a particular intensity does not induce a systematic change in preferences for unrelated provider characteristics (that are acted upon in the choice of destination provider), relative to having an origin provider with any other propensity. As above, we do not impose any assumptions on whether the exit itself can induce a change in preference, i.e. $\phi_{post} \neq 0$. To test this assumption, we follow the approach outlined in Abaluck et al. (2021), which seeks to test whether observable provider characteristics (such as gender or specialty) are predicted by the origin provider's intensity. Specifically, we regress ρ_p on a set of observable provider characteristics (gender, specialty, degree), calculating the fitted values from this regression for each patient. We then project these fitted values onto ρ_p and interpret the residuals as an observable proxy for η_{it}. We subsequently re-estimate our first stage with the residuals on the left-hand side. A coefficient of or near 0 suggests the fallback condition is met. Table A.3 demonstrates that this is the case in our setting.[21]

3.3 Inference

As standard in estimates of two-way fixed effects models, we compute two-way clustered standard errors at the patient and provider levels, allowing us to incorporate variation within and between these agents into our estimates. Further, because the Δ_p terms are generated regressors, we report bootstrapped standard errors in an Appendix figure that are not subject to the same type of measurement error associated with Δ_p as in typical standard errors. Additionally, because we have staggered event timing and potentially non-homogenous average treatment effects across groups and over time, we implement the alternative two-stage differences-in-differences estimation framework proposed in Gardner (2021) to check

[21] *Note that this version of the test only includes the switching cohort; future drafts will run this test on the full cohort of switchers and non-switchers.*

the robustness of the results we obtain using standard two-way fixed effects approaches.[22]

3.4 Threats to Identification

As noted above, a key assumption in standard two-way fixed effects models is that but for the event (the origin provider's exit), the trends in outcomes among individuals whose key provider exits would have been parallel to trends in outcomes among individuals whose key provider doesn't exit. In our model, in which our main coefficient of interest is the parameter associated with an interaction term (namely, the difference in prescribing intensity interacted with relative time), the relevant counterfactual is less straightforward. Specifically, the key assumption we are making is that the size of the change in provider prescribing intensity is exogenous to how patient opioid use evolved. In other words, patients whom experience Δ_1 do not exhibit opioid use change that is systematically different in evolution than patients whom experience Δ_2 or Δ_3. An example of a violation of this assumption would be if patients switching to low-intensity providers were already intentionally tapering their opioid use, and seeking out providers who would help them achieve cessation. We can check for the presence of differences in pre-exit trends in the coefficients estimated from Equation 4.

An additional assumption that is somewhat harder to test, though equally as important in the interpretation of our coefficients as the causal effect of provider prescribing intensity on patient opioid use, is that no other event occurs at the time of the exit that might also be correlated with the outcome. For example, if high-intensity providers were being systematically dropped from networks and their patients enrolled in substance abuse treatment (SAT) therapies, then we would conflate a decrease in opioid use following their exit to the influence of the newer, less-intense destination provider with the additional influence of SAT.[23] In additional analyses, we evaluate the robustness of our results to the consideration of different characteristics of patients and providers, including origin provider's treatment intensity.

We reiterate that we do not interpret a patient's selection to a destination provider of a particular prescribing intensity as a threat to identification, but rather a re-definition of our

[22] *These checks will be provided in a future draft.*

[23] For this case specifically, we estimate our model using SAT as the outcome of interest; an immediate and sharp increase in SAT at the time of the switch may be evidence of such an intervention.

results as either the causal or permissive effect of switching to a more/less intensive provider on opioid use. However, we implement the TSLS approach described in Section 3 in an effort to isolate the causal effect from the permissive effect.

The data we use span from 2016 to 2021. This time frame includes claims and encounter records from the COVID-19 pandemic, during the first few months of which overall healthcare utilization plunged precipitously, with some return to pre-pandemic utilization levels later in the year (Birkmeyer et al., 2020; Mehrotra et al., 2021). A reasonable concern is that the pandemic's affect on utilization might challenge the interpretation of our results. Specifically, if a provider's exit coincides with the overall decreased utilization during the pandemic, we may incorrectly attribute any reduction in utilization outcomes to that provider's exit. To determine the extent to which this represents an issue in our analysis, we plot average values of our outcomes across all year-months. Within our cohort, we do not observe a significant decrease in prescription fills that mirrors changes in utilization trends during the pandemic. Furthermore, to the extent that the chilling effect on utilization affects both treated and control beneficiaries alike, our year-month fixed effects will account for these secular changes in our outcomes. However, based on the observation that primary care visits in Medicaid decrease for several quarters following a key provider's exit (Staiger, 2022), it may be more likely that a patient who loses their provider also reduces their utilization. Thus, we check the robustness of our estimated effects by estimating our model excluding any enrollees whose provider exited from **MM** to **MM** in 2020 (i.e. the months during which utilization was particularly affected by the pandemic).[24]

Notably, our definition of "exiters" (which we describe in more detail in Section 4 below) can flexibly accommodate the decreased utilization associated with the pandemic; as long as the provider returns in some capacity to treat patients in the months following the initial shut-downs, we will not classify them as exiters. More generally, our definition of an exiter is a provider who is no longer actively participating in Medicaid by treating patients.

[24] *This check will be included in a future draft.*

4 Data and Sample Description

4.1 Data

Through a partnership with Gainwell Technologies, a major recovery audit contractor, we obtained Medicaid administrative claims and encounter record data for nine states from 2015 to June 2021. There are two primary types of Medicaid coverage that are represented in Medicaid administrative data: fee-for-service (FFS) claims generated by the traditional state-run Medicaid programs; and managed care encounter records generated by the managed care organizations that contract with states to assume financial responsibility for the healthcare spending of approximately 83% of enrollees (as of 2019) in exchange for capitated payments.[25] In our data, three states submitted claims from traditional fee-for-service (FFS) Medicaid enrollees only, three states submitted encounter records from one (major) Medicaid managed care (MMC) organization operating in the states, and three states submitted all claims and encounter records from both FFS and MMC enrollees. To more convincingly measure provider prescription intensity based on observations of *all* Medicaid enrollees with chronic pain, we include only those three states with claims and encounter records for all Medicaid enrollees (FFS and MMC). Per our data use agreement, we do not reveal the identities of the states.

4.2 Measures and Outcomes

Adapting from Staiger (2022), we flag a provider as exiting from Medicaid in a given year-month if the following criteria are met in the months leading up to that candidate exit month:

1. The provider treated a rolling average of at least two Medicaid enrollees (with or without chronic pain) in an office setting prior to the candidate exit month. The rolling average is constructed by taking the average of the three months prior to a given index month.

[25] See: MACPAC website

2. The provider treated patients for at least two months prior to the candidate exit month.

3. The candidate exit date must be at least two months prior to the last month for which we have data

We use this approach to identify exiters since a provider's exit from Medicaid is not recorded in our data (nor typically in any similar administrative data). Moreover, unlike Staiger (2022), we do not observe the managed care plan an individual is enrolled in, and thus we are unable to identify plan-level exits (as opposed to more general Medicaid exits). This approach may generate a different set of exiting providers than the set of providers whose names are removed from Medicaid managed care or fee-for-service rosters, which was previously used by Ndumele et al. (2018) to explore provider turnover in Medicaid. However, we believe that this approach is more suitable to our setting than a strategy in which we compare provider rosters across years. First, not all providers who are listed on a Medicaid roster actually treat Medicaid enrollees. Other studies have documented that approximately half of providers listed as participating in MMC networks were actually treating Medicaid enrollees (Office of Inspector General, 2014; Wallace et al., 2020; Zhu et al., 2022). Our strategy identifies providers who transition from actively treating Medicaid enrollees to no longer actively treating enrollees. Second, unlike in an examination of provider rosters which provide intermittent snapshots of participating providers, our approach lets us identify the final month that a provider is actively participating in Medicaid.

Our study's providers of interest are the physicians (MDs or DOs) or advanced practice nurses (NPs or PAs) of any specialty responsible for the plurality of physician office visits during either the twelve to seven months prior to the provider's exit (origin providers) or the twelve months after the origin provider's exit (destination providers). This is similar to the approach taken in both Kwok (2019) and Staiger (2022). As noted above in Section 3, the time periods in which we identify the origin providers are informed by the trends in the number of patients they treat in the months prior to exit. Including months in which patients have no visits, origin providers are associated with approximately 20-25% of claims in the 12 to 7 months prior to their exit, with a sharp decline (to approximately 15% of claims) in the sixth month prior to exit. From relative month -6 to 0, which we refer to generally

as the "transition period," the share of claims associated with the origin provider decreases from approximately 15% to 10%, and becomes exactly 0, by construct, from relative month 1 to 12. Notably, a patient's last visit with her origin provider may be prior to the month of exit. Only slightly over 10% of patients in our sample have their last visit with their origin provider in the month we identify as the exit month, with the mean (median) month of a patient's last visit with their origin provider being 5.7 (7) months before the month of exit. Below, we test the robustness of our results to defining the event of interest to be the last visit a patient has with their origin provider.

Destination providers account for less than 5% of claims in the twelve to seven months prior to exit, and begin increasing their share of claims during the transition period (up to approximately 10%). In the month after exit, there is a sharp increase in the share of claims associated with the destination provider to approximately 20%, and this share remains between 15% and 20% throughout the rest of the 12-month post-exit period.

As noted above, our primary measure of provider opioid prescribing intensity is the log of the provider's average MED per patient-month in the pre-period. The morphine equivalent dose is a standardized measure of the average amount of morphine, in milligrams, a patient has access to per day, and is the sum of all MME's associated with the opioid prescriptions a patient has over a given time period, divided by the number of days in that period (in our case, a month). We discuss our calculation of MED in Appendix C.

While other similar research settings have used prescription rates to characterize provider intensity,[26] we prefer using the log of average MED for two reasons. First, we consider average provider MED to be a more flexible intensive margin measure of a provider's prescribing intensity. Specifically, we seek to differentiate not only between providers that prescribe opioids for few versus many patients, but also to further differentiate between providers who prescribe many of their patients opioids, for example, and those who prescribe relatively small versus relatively large levels of opioids (as captured via MED). In calculating our average, we include *all* of a provider's chronic pain patients, regardless of whether they received an opioid prescription. Thus, a provider who prescribes very few patients a large amount of opioids may have the same prescribing intensity as a provider who prescribes all their

[26]See e.g. Eichmeyer and Zhang (2021, 2022); Laird and Nielsen (2016)

patients a very small amount of opioids. However, we believe this to be more informative of a provider's overall prescribing intensity than calculating average MED conditional on opioid prescriptions, since we do not consider providers who prescribe very few patients a large amount of opioids to have the same prescribing intensity as providers who prescribe many patients a large amount of opioids. That said, we check the robustness of our results to the opioid-conditional version of prescribing intensity, and report how those results differ from our primary measure of prescribing intensity below.[27]

Second, we consider an m-MED difference between providers with relatively low overall average MED per patient-month to be indicative of a more important difference in prescribing behavior than an equally-sized m-MED difference between providers with relatively high overall average MED per patient-month. We take the log transformation of average MED per patient-month to overweight differences between smaller MME levels and underweight differences between larger MED levels.

We examine the effect of provider prescribing intensity on three classes of outcomes: opioid use, healthcare utilization and health events, and treatment of opioid-related disorders. Our primary opioid use outcome of interest is a patient's log daily MED in a given year-month. Notably, this MED includes opioids from all prescriptions a patient fills during that period, regardless of whether the prescribing provider was the origin/destination provider. Thus, our measure of a patient's opioid use reflects supply from the individual key provider of interest as well as other members of the care team. We will more explicitly test for influence of the the prescribing profile of the care team in additional robustness analyses, as well as testing more explicitly for the influence of the key provider's prescribing intensity on a patient's provider-specific opioid use by focusing on the MME associated with the prescription from their origin and destination provider.[28] We take the log transformation of this variable to overweight (same-size) changes in MED between relatively low levels of MED, and underweight (same-size) changes in MED between relatively high levels of MED.

We include three additional measures of opioid use. First, similar to related papers discussed above, we explore changes in the probability that an enrollee has an opioid pre-

[27] *Not included in this draft.*
[28] *Not included in this draft.*

scription (a binary outcome) in a given year-month. Second, we calculate the share of days covered in a given month by an opioid prescription as a slightly more nuanced version of the binary probability of a prescription. Third, we evaluate changes in the likelihood that a patient has a daily MED of 120 mg or greater, which is equivalent to taking 16 tablets of 5 mg Percocet per day (Office of Inspector General, 2020). This MED threshold has been used in prior research to flag opioid abuse, given that there is a significant correlation between filling prescriptions that result in a daily MED of at least 120 mg and abuse (Finkelstein et al., 2021; Meara et al., 2016). We prefer this measure as a more objective flag of opioid abuse than a diagnosis of OUD, which we will discuss further below.

Our second class of outcomes includes several measures of health events and healthcare utilization. We focus on two health events in particular: evidence of opioid use disorder (OUD) and an opioid-related overdose. Opioid use disorder is characterized as a "problematic pattern" of opioid use, and is diagnosed based on patient responses to screening tools (as opposed to lab tests or other clinical indicators).[29] The diagnosis itself is identified in claims data by a specific set of diagnosis codes. However, it is potentially subject to endogeneity concerns; namely, it's possible that a provider's propensity to diagnose OUD is correlated with their propensity to prescribe opioids in a way that would bias our estimated effects. Thus, we include an alternative measure that is arguably more objective (though potentially less precise). Specifically, we flag OUD based on the observation of an enrollee filling opioid prescriptions from at least four providers in a given time period, as defined by Meara et al. (2016) and commonly used in related literature as a proxy for OUD (e.g. Finkelstein et al. (2021)). We observe an OUD diagnosis in 2.8% of all chronic pain patient-months, and evidence of multiple opioid prescribers in less than 1% of all chronic pain patient-months (Table A.4), though both rates are higher for treated enrollees during the pre-exit period (3.9% and 3.9%, respectively).

The second health event of interest is a fatal or non-fatal overdose. This event is characterized by the patient being poisoned by an excessive amount of drug, and can be observed

[29]For example, a provider may diagnose a patient with OUD if the patient reports that they have experienced disruptions in the social or professional sphere, or have unsuccessfully tried to decrease or control opioid use.

via specific diagnosis codes for "poisoning" in administrative claims.[30] In these diagnosis codes, we can further differentiate between different sources of overdose. Specifically, we will evaluate changes in the likelihood of an overdose due to common prescription opioids, such as oxycodone or codeine;[31] heroin; and synthetic opioids, such as fentanyl.

A notable limitation of our approach is that we only observe overdoses (or any other events) for which a claim was generated; it is possible that an enrollee who died from an overdose did not generate a Medicaid claim, particularly if the overdose was not treated by a provider who would file the claim. Thus, our count of overdoses will likely be an undercount. For context, we observe an overdose in 0.1% of chronic pain patient-months, driven seemingly entirely by overdose from commonly prescribed opioids (Table A.4).

While several studies have linked greater direct and indirect access to prescription opioids to an increased risk of OUD and overdose, others have documented the perverse effect of a decrease in access driving some patients to illegal opioid use, such as heroin or fentanyl, which ultimately leads to even higher risks of OUD and/or overdose (Alpert et al., 2018; Edlund et al., 2014; Evans et al., 2019; Hser et al., 2019; Mallatt, 2020; Oliva et al., 2020; Powell et al., 2020; Sacks et al., 2021; Schnell, 2021). Our study will provide additional insight as to the nature of the relationship between access and adverse health events.

Our second group of health-event outcomes includes healthcare utilization measures.[32] Ex ante, it is not necessarily obvious how utilization may change following a switch in provider intensity. Ignoring the potential correlation between provider prescribing intensity and more general intensiveness of utilization (which we might observe particularly in office visits, which may be more at the provider's discretion relative to inpatient hospitalizations and ED visits), an increase in opioid use driven by an increase in provider prescribing intensity may induce additional visits due to adverse health events, such as overdose, stemming from the increased opioid access. Alternatively, as noted above, a reduction in the legal supply of opioids available to the patient may increase the risk of OUD or overdose via illegal opioid use,

[30] For more common definitions related to opioid use, see: https://www.cdc.gov/opioids/basics/terms.html

[31] The CDC defines three classes of prescription opioid overdoses: 1) natural opioids (e.g. morphine, codeine); 2) semi-synthetic opioids (e.g. oxycodone, hydrocodone); and 3) methadone, which is also commonly used in the treatment of OUD. See: https://www.cdc.gov/drugoverdose/deaths/prescription/maps.html

[32] Additional details on how we define these (and all other outcome) measures are given in Appendix C.

in which case we might expect to observe an increase in utilization related to these health conditions/events, such as opioid-related hospitalizations.

Our third class of outcomes includes the treatment of opioid-related disorders. Specifically, we explore whether a change in provider prescribing intensity leads to changes in facility-based treatment or in the use of medication (methadone, buprenorphine, and naltexone) to treat OUD. As noted above, Medicaid enrollees are more likely than privately insured individuals to receive OUD treatment.

4.3 Sample Construction

Our analysis focuses on provider-induced changes in opioid use and related outcomes among adult Medicaid enrollees with a chronic pain diagnosis. Across the three states in our sample, we observe 205,714 enrollees that have a diagnosis of chronic pain per the definition used by the Chronic Condition Warehouse (CCW).[33] Individuals can churn in and out of Medicaid due to a variety of factors (such as failing to re-enroll or becoming ineligible). We identify continuous enrollment periods for each beneficiary based on the start and end dates of each of their enrollment periods. Finally, we restrict our sample to the 163,846 individuals who were at least 18 during their first chronic pain diagnosis period and at most 64 during their last chronic pain diagnosis period.

Of the 197,434 individuals in our starting sample, we subsequently identify 8,575 with a key provider that exits Medicaid at some point during the study period, and 188,859 who are never treated by an exiting provider; this later group comprises our control cohort. Among this cohort, 3,151 have an identifiable destination provider, which is necessary for our analytical strategy. We further define "enrollee-episodes" based on the occurrence of a key provider's exit; for example, an enrollee who has two key provider's that exit at some point during their enrollment will have two enrollee episodes. Notably, there can be multiple enrollee-episodes in one enrollment period. We provide details on these episodes (and other relevant information) in Section 4. These 3,151 enrollees (3,197 enrollee-episodes) comprise our final treated cohort. We observe 496 origin providers and 1,779 destination providers treating the treated cohort.

[33] We use the CCW algorithm for "Fibromyalgia and Chronic Pain and Fatigue," revised February 2021.

4.4 Summary Statistics

Table 1 describes selected characteristics of the sample. We compare characteristics of our control cohort to those of our treated cohort overall, and further disaggregate our treated cohort into two groups: the 1,702 enrollees whose destination provider is less intensive than their origin provider ($\Delta_{ln(MED)} < 0$); and the 1,471 enrollees whose destination provider is more intensive than their origin provider ($\Delta_{ln(MED)} > 0$).

Panel A compares patient characteristics across these cohorts. A slightly larger share of control patients are male, relative to the general and disaggregated treated cohorts, and average age is relatively similar across all cohorts. Just over half of control patients are opioid naive (meaning, we do not observe them filling a prescription opioid in the data), which is on average less than among our treated cohorts, though this may be mechanical given that we observe them for more months on average than our treated cohort (whom we observe for 25 months exactly, by definition). Patients who switch to less intense providers are slightly less likely to be opioid naive (58%) than patients who switch to more intense providers (63%), which supports the intuitive and expected relationship between provider prescribing intensity and patient opioid use if, on average, patients switching to less intense destination providers had more intense origin origin providers (and vice versa). Building the same intuition, it may be unsurprising to note that average daily MED is slightly higher for patients switching to less intense providers (18 mg) relative to patients switching to more intense providers (14 mg). On average, control patients have a significantly lower daily MED (3 mg) than the treated cohort (16 mg), though again, this may be mechanical given that we observe control patients for nearly three times the months that we observe treated patients.

Panel B compares the characteristics of the 7,224 and 496 origin providers treating the control and treated cohorts, respectively. Origin providers for both control and treated cohorts tend to be male (65%–68%), and treated patients are slightly more likely than control patients to have an origin provider who is a physician (75% versus 82%, respectively) than an advanced practice nurse. Approximately 40% of the providers treating both cohorts have a specialty that is associated with primary care[34]. Patients who switch to less intensive

[34]We characterize providers as primary care practitioners if they have a specialty of family practice, general practice, internal medicine, obstetrics and gynecology, or pediatrics.

providers tend to be treated by origin providers who are slightly more intense (log MED of 3.1) than patients who switch to more intense providers (log MED of 2.27), which provides support to the intuition above, as well as to the regression to the mean feature of the sample we will exploit in the two-stage least squares approach. Moreover, origin providers of patients who switch to less intense providers tend to prescribe opioids to a larger share of their patients (0.08) than origin providers of patients who switch to more intense providers (0.04). This observation acts as a sense check on our intensive margin measure; by characterizing provider intensity as the leave-one-out average MED across all patients (unconditional on opioid receipt), we are still in trend with the extensive margin measure of prescription rates.

Panel C similarly compares the characteristics of the 1,799 destination providers treating the treated cohort (by definition, control patients do not have a destination provider). Treated cohort providers are primarily male (73%) and physicians (81%), and 36% have a primary care specialty. Destination providers tend to have a slightly lower prescribing intensity (log MED 2.65) relative to origin providers, though these differences are larger when we disaggregate by direction of switch. As expected, destination providers that are less intensive than origin providers tend to be less intense (log MED of 2.34) and prescribe opioids at a lower rate (0.03) than origin providers. Destination providers who are more intensive than origin providers have a higher average prescribing intensity (log MED of 3.02) and prescribe opioids at a higher rate (0.07).

To illustrate how prescribing intensity varies across providers, Figure 1 plots the distribution of both overall (origin and destination) provider intensity among the treated cohort (panel a), and the difference in provider intensity between origin and destination providers (panel b). The average log MED of origin and destination providers treating the treated cohort is 2.684, and the standard deviation is nearly 100 log points (0.922), suggesting a large degree of variation in provider prescribing intensity. The change in treated enrollee's provider prescribing intensity is closely centered around zero, and there is also a significant amount of variation in the differences between origin and destination providers; as in the distribution of provider intensity, a one-standard deviation in $\Delta_{ln(MED)}$ is approximately 100 log points.

Figure A.9 plots trends of opioid use over time, as captured by our four main measures

(including level daily MED), disaggregated into treated and control cohorts. In Panels (a) and (b), opioid use decreases from 2016 to 2021. Among individuals in the treated cohort, the percent of enrollees filling an opioid prescription in a given year-month fell by half, from nearly 40% of patients in 2016 to approximately 20% in 2020, while the average share of days in a given month covered by an opioid prescription fell from 35% in 2016 to less than 20% by 2020.

Meanwhile, conditional on receiving an opioid prescription, the daily MED a patient had access to increased over this period (panels (c) and (d)). This may reflect that patients who continue to receive opioids despite the overall decrease in their use are those patients with higher levels of pain, or that providers who continue to prescribe opioids are those providers who tend to prescribe larger amounts, or a combination of both, an ambiguity which we hope to comment on via our empirical strategy. In panel (e), we observe relatively no change in the share of patients with a daily MED of greater than 120 mg.

Notably, enrollees in the treated cohort have overall higher levels of opioid use across all measures than in the treated cohort. Our main specification accounts for time-invariant differences between enrollees, so as long as these level differences reflect differences in enrollee characteristics among treated cohorts (experiencing different prescribing intensity changes) that do not change over time, they will not introduce bias into our estimated effects. Additionally, there appear to be seasonal trends in opioid use across these measures; our calendar time controls (which we interact with state) will account for these trends.

4.5 Descriptive Change in Opioid Use After Switch

We present further descriptive evidence that patients who switch to more or less intense destination providers following an origin provider's exit adjust their opioid use in the direction of their destination providers prescribing behavior. Panel (c) of Figure 1 plots average patient log MED against average provider log MED, binned into vigintile buckets. Leaving out any controls, we find that increasing a provider's prescribing intensity by 100 log points (or, one standard deviation) increases a patient's log daily MED by 56 log points in a relatively linear relationship. Without further adjustment, these results might lead us to conclude that providers are responsible for a significant degree (over half) of their patient's behavior

surrounding opioid use. Our empirical strategy seeks to control for any endogeneity reflected in this observed effect.

5 Results

5.1 Provider Prescribing Intensity and Patient Opioid Use

Our first goal is to determine whether there is a causal relationship between provider prescribing intensity and patient opioid use. Figure 2 reports the coefficients obtained from Estimating 4 with patient opioid use (as log MED) as the dependent variable. In the pre-period during which the patient is being treated by their origin provider, the coefficients are approximately zero and relatively flat, providing support for our empirical strategy. During the transition period, as the patient starts tapering off visits with their origin provider and shifting visits to their destination provider, the coefficients begin trending in the direction of the destination provider's prescribing behavior in a manner that suggests dynamic adjustment of opioid dosage. At the month of the origin provider's exit, the patient has increased (decreased) their daily MED by approximately 10% in the direction of a destination provider who is 100 log points more (less) intense than the origin provider.

In the 12-month post-period, the patient continues to scale their opioid use in the direction of their destination provider until approximately the sixth month post-exit, at which point they plateau around 0.20. Interpreting this in terms of a one standard-deviation, this suggests that patients that switch to a destination provider that is one standard-deviation more (less) intense than their origin provider increase (decrease) their daily MED by approximately 20 log points (approximately 28%), following a transitional period during which they adjust their MED in the direction of their destination provider's intensity.

Panel (d) of Figure 1 illustrates this relationship in a slightly different way. We plot the average change in a patient's log MED against the change in their provider's log MED ($\Delta_{ln(MED)}$), binned into vigintiles, with no additional adjustment. The coefficient is 0.19, which is slightly larger than the approximate average of all coefficients in Figure 2 over the post-period months. While this coefficient reports the approximate relationship between a

change of 100 log points in provider intensity and the change in a patient's own opioid use (in log points), it also suggests that approximately 20% of observed provider variation in opioid prescribing intensity is driven by provider-side components, such as beliefs around the dangers of opioid use, and approximately 80% is driven by patient-side components, such as patient acuity of pain or preferences with regards to opioid use.

Column (1) of Table 2 reports the results of estimating Equation 5, i.e. the difference-in-differences model, in which all periods—pre (the reference period), transition, and post—are collapsed to a single period (and set to 0 for all controls), while observations are still at the month level. As reflected in Figure 2, patients begin to modify their daily MED in the direction of their destination provider by approximately 6 log points during the transition period. Over all months in the post period, on average, patients modify their MED by approximately 17 log points in the direction of their destination provider.

The results presented thus far have discussed effects estimated on a pooled sample in which enrollees switch to both more and less intensive destination providers. In Figure 3, we separately present effects by the direction of the switch, generated by estimating the model on enrollees whose destination provider's prescribing intensity is (a) less than their origin provider ($\Delta_{ln(MED)} < 0$); and (b) more intense than their origin provider ($\Delta_{ln(MED)} > 0$). In all specifications, we include all non-switching control enrollees to increase the precision with which we estimate secular trends in MED. Further, for ease of visualization, we have multiplied coefficients estimated on the $\Delta_{ln(MED)} < 0$ sample by -1, since a positive coefficient represents a decrease in an outcome (i.e. a positive relationship with a negative-direction switch), and a negative coefficient corresponds to an increase. These plots illustrate an asymmetric effect of provider prescribing intensity on patient opioid use. Enrollees switching to less intense providers (panel (a)) tend to decrease their own daily MED by 30 to 40 log points in the final six months of the post-period, while enrollees switching to more intense providers (panel (b)) tend to increase their own daily MED by at most 10 log points (not significant).

Table 3 summarizes these effects in one pooled post-period estimate, reporting that while a one-standard deviation decrease in provider intensity decreases MED by approximately 30

log points, an increase in provider intensity does not significantly increase MED.[35] Notably, the effects in Figure 3 illustrate how standard changes in provider intensity affect changes in opioid use. Figure A.4 shows substantially larger and more significant effects for patients switching from below-median to above-median prescribing providers, and similarly sized (though less precisely estimated) effects for patients switching from above- to below-median prescribing providers. We explore heterogeneity in effect size by size of change further in our robustness checks below.

To build additional intuition, we explore for heterogeneity in effects based on prior opioid use. Specifically, we estimate our main event study model on a sample of "opioid naive" patients, i.e. patients without evidence of prior opioids use (as measured in the pre-period), and a sample of "prior opioid use" patients who have a pre-exit record of filling an opioid prescription. In our sample, 1,846 treated beneficiaries and 9,756 control beneficiaries are opioid naive; 1,313 treated beneficiaries and 9,196 control beneficiaries are prior opioid use patients. Panels (a) and (b) of Figure A.5 plot the coefficients estimated from our main event study model on opioid naive patients who switch to less intensive and more intensive providers, respectively.[36] While opioid use does not change among opioid naive patients switching to less intense providers (which follows intuitively), opioid naive beneficiaries switching to more intense providers increase their opioid use between 10 to 20 log points in the post-period. Panels (c) and (d) of Figure A.5 plot the coefficients estimated on a sample of prior opioid use patients. In the final six months of the post-period, prior opioid use patients who switch to less intensive providers reduce their opioid use by approximately 75 log points, while we observe no significant change in opioid use for prior opioid use patients switching to more intense providers. Together, these results suggest prior use is an important determinant of the impact a (new) provider's prescribing behavior will have on patient's opioid use.

Figure A.6 reports the effect of a change in provider prescribing intensity on additional measures of opioid use (Table 2 reports the associated difference-in-differences estimates).

[35] *Results for* $\Delta_{ln(MED)} < 0$ *are presented without multiplying the coefficient by -1, as in the event studies. Future drafts will multiply coefficients by -1 for ease of interpretation.*

[36] The results are robust to including all non-switchers are controls (not just opioid naive), though we prefer the specification including only opioid naive controls in order to more precisely estimate secular trends of opioid use in this population of enrollees, to the extent that they differ in time-varying ways from enrollees who use opioids.

Panel (a) shows that the probability of filling an opioid prescription in a given year-month changes across the transition and post-period in the direction of the destination provider's prescribing intensity, plateauing at just less than 5 percentage points. On a pre-exit mean of approximately 30% (column (2) of Table 2), the average 4.2 percentage point change per year-month in the probability of a prescription opioid fill represents an increase (decrease) of approximately 15% after an enrollee switches to a more (less) intense provider. The magnitude of the effect on the share of days in a given year-month covered by an opioid prescription is similar (a change of approximately five percentage points, or nearly 20%; column (3)).

In terms of implications for opioid misuse, panel (c) suggests that as the intensity of the destination provider increases (decreases) relative to the origin provider, the probability of a patient having access to more than 120 mg in a given day increases (decreases) by between 0.5 and 1.5 percentage points in the second half of the post-period. On a base of 3.5% (column (4) of Table 2), this average change of 0.5 percentage points represents an increase (decrease) of approximately 14% in the overall probability of having a daily MED of over 120 mg. As MED above this threshold is generally regarded as risky, this observation suggests that the probability of risky opioid use increases (decreases) in the long term as provider prescribing behavior becomes more (less) intense. Notably, we determine that this effect is largely driven by a decrease in the probability of MED greater than 120 mg among patients that switch to less intense providers (Figure A.7).

Moreover, Table 2 indicates that the effect of the exit itself (given by the coefficients associated with the $Post_{it}$ parameter) indicate that patients' access to opioids decreases following their providers exit, and that the probability of risky opioid use slightly increases. These findings are consistent with Staiger (2022), who finds that patients of providers that exit Medicaid have fewer primary care visits after the exit (a loss of access to care), and, for patients with certain chronic conditions, the likelihood of adverse health events is higher (specifically, in the likelihood of hospitalizations).

5.1.1 Addressing Endogeneity of Destination Provider Selection

As noted above, it's possible that a change in provider intensity experienced by a patient is also correlated with a change in preferences or a health shock that affects how the patient chooses their destination provider, introducing bias to our estimated effects. To test the robustness of our main results to such considerations, we implement our two-stage least squares approach. Table 4 reports the TSLS results, by stage, with column (1) reporting the estimates from Equation 5 for reference.[37] The first stage (column 2), which is negative and highly significant,[38] suggests that increasing (decreasing) the intensity of one's origin provider by approximately 100 log points decreases (increases) the intensity of one's destination provider by about 59 log points. The reduced form (column 3) suggests that having a more intense origin provider in the pre-period reduces opioid use in the post-period, consistent with having a destination provider with a lower prescribing intensity.

Column (4) reports the second stage coefficient, which is the local average treatment effect of the change in provider prescribing intensity on a patient's log MED relative to the pre-period, relative to patients who experience other changes in intensity. In particular, this estimate represents the average treatment effect among patients who are induced to select to more typical destination providers following the exit of their origin provider ("compliers"). The magnitude of the effect (23.8 log points) is slightly larger than in the difference-in-differences specification (17.3 log points), suggesting that selection somewhat understates the causal effect of a provider on patient opioid use, in aggregate.

When we evaluate this affect in terms of symmetry, we find a substantial and significant effect of provider intensity on reducing opioid use when patients switch to less intensive providers (80 log points), and a null, somewhat imprecisely estimated effect of provider intensity on increasing opioid use when patients switch to more intensive providers (4.9 log points). The instrumented effect on opioid use for patients treated by less intense destination providers is approximately twice the size of the effect estimated in the event study speci-

[37] Because we drop the transition period in our TSLS estimates to avoid having multiple instruments, the number of observations—and composition of not-yet-treated or already-treated beneficiary-month controls that are dropped with the exclusion of the transition period—changes from the sample that comprises Table 2.

[38] The F-statistic is approximately 573.

fication, and suggests that patients who switch to less intensive providers are more likely to maintain a higher level of opioid use than the intensity of their provider would predict. Notably, with no evidence of pre-trends, the event study estimates can still be interpreted as causal; rather, the TSLS estimates seek to isolate the direct effect of the provider's prescribing behavior on patient opioid use from other unobserved factors that may change around the time of the switch.

5.2 Health Outcomes and Utilization

Our second goal is to explore whether a causal relationship exists between provider prescribing intensity and health outcomes and utilization. If provider prescribing behavior induces some change in opioid use, it's also plausible that these changes may have may have downstream health consequences, such as adverse health events that lead to ED visits, hospitalizations, or, in some extreme cases, overdose. We search for these effects using both our difference-in-differences and two stage least squares approaches to determine whether, in addition to influencing patient opioid use, provider prescribing intensity has implications for patient well-being.

Tables 7 and 8 report the results from estimating Equation 5 on the cohort that switches to less intensive and more intensive providers, respectively. The dependent variables are defined as binary outcomes, representing the probability of an event occurring, the coefficients can be interpreted as percentage point changes. Note that a negative coefficient in the less-intensive switching cohort (i.e. Table 7) should be interpreted as indicative of an inverse relationship between decreasing provider intensity and the likelihood of that outcome. Specifically, negative coefficients correspond with an increase in the likelihood of the outcome, while positive coefficients correspond with a decrease in the likelihood of the outcome.

Among patients switching to less intense providers, there is no change in the probability of having an overdose or an ED visit. However, the probability of an all-cause hospitalization significantly increases by 0.8 percentage points (21%). This suggests that patients are more likely to experience adverse health events following a switch to a provider who prescribes a lower average volume of opioids, providing support for concerns that reducing opioid access harms patients. Tables A.5 and A.6, which explore heterogeneity in the effects among the

cohort of patients switching to less intensive providers, illustrate that this effect is driven by the patient population with prior opioid use, who experience a 1.3 percentage point (30%) increase in the probability of a hospitalization following their provider switch.[39]

The probability of having any office visit significantly decreases by approximately 3.3 percentage points (6%),[40] which may be somewhat mechanical given that receiving an opioid prescription often requires an in-person office visit. In other words, it is therefore unsurprising to observe that a reduction in opioid use resulting from a switch to a less intensive provider (who may also prescribe at a lower rate) also results in fewer office visits, if the patient's prior volume of office visits was primarily driven by seeking to refill prescriptions. The observation in Tables A.5 and A.6 that this effect is driven by prior opioid users provides additional support to this hypothesis.

Notably, the likelihood of an OUD diagnosis doubles (increasing by 4.1 percentage points) among patients that switch to less intensive providers. Given the increase in hospitalizations, it is possible that patients who switch to less intense providers subsequently develop OUD as they substitute their opioid use towards other, illicit options, which can also result in hospitalizations and/or ED visits. Alternatively, this observation could reflect the arguably subjective nature of OUD diagnoses themselves. For example, different providers may have different propensities to diagnosis an OUD, which may be correlated with their comfort and/or preferences for prescribing opioids. Thus, patients switching to less intensive providers may be more likely to receive an OUD diagnosis because of a change in provider perspective. This has important implications for using OUD diagnoses in research that aims to quantify opioid misuse. Specifically, using OUD diagnoses in medical data to flag opioid misuse may not capture the true prevalence of opioid misuse in a population, as it is in part a function of the provider who records the diagnosis. Alternative observable measures, such as overdose or multiple prescribers, may more accurately capture prevalence of harmful opioid use.[41]

[39] Notably, the disruption of the switch itself has a direct impact of approximately equal magnitude on the likelihood of hospitalization, as observed in Staiger (2022), as well as an increased probability of an ED visit.

[40] The pre-exit average likelihood for each outcome (calculated across all treated patients switching in both directions) are given in the bottom row of the table

[41] Figure A.10 suggests that there is no change in the likelihood that a patients has multiple opioid prescribers after switching to either a more intense ("increasing") or less intense ("decreasing") provider.

Among patients switching to more intense providers (Table 8), there are no observable changes in the likelihood of office visits, ED visits, hospitalizations, or OUD diagnoses. However, the likelihood of an overdose decreases by approximately 0.2 percentage points (100%) following this switch. Tables A.5 and A.6 show that this effect is driven by a 0.5 percentage point decrease in the likelihood of overdose among prior opioid users who switch to more intense providers.[42]

Figure A.11 provides additional detail on these overdose events. Specifically, the significant effect we observe among patients who switch to more intense providers appears to be driven by a decrease in overdose attributable to prescription opioids. Notably, overdose due to synthetic opioids or heroin are relatively rare in this population. While overdose due to prescription opioids accounts for approximaly 60% of all overdose claims, synthetic opioids and heroin account for less than 10% each (Table A.9). Though this does not comport with observations of secular trends in synthetic opioid overdoses, there are two possible explanations for this observation. First, overdose due to synthetic opioids (or heroin) may be more likely to result in patient death and/or other types of outcomes that do not generate claims. Second, this population may have better access to and/or experience using prescription opioids.

We explore the dynamic nature of these effects in Figure A.12 by estimating Equation 4 for the probability of overdose among prior users who switch to more intensive providers (panel a), hospitalizations among prior users who switch to less intensive providers (panel b), and OUD diagnoses among all patients switching to less intense providers. While our estimates for overdose and hospitalization are not precise, general patterns in panels (a) and (b) show the decreasing and increasing relative trends, respectively, that we observe in the difference-in-differences estimates. There are no clear changes in effect across time. In contrast, panel (c) clearly shows an increase in the probability of OUD diagnosis as the patient shifts to their destination provider. The approximately five percentage point increase in the probability of an OUD diagnosis in the post-period is relatively stable across

[42]Notably, a limitation of the claims data is that we only observe overdose events that generate a claim. Thus, any overdose that did not generate a claim (such as in a death outside of an institutional setting) will not be recorded. The bias this introduces to our estimates is ambiguous, and thus a conservative interpretation of our findings is that switching to a provider with higher (lower) prescribing intensity decreases (increases) the probability of an overdose for which a claim is generated.

the post-exit months.

Tables A.10 and A.11 report the two-stage least squares estimates for patients who switch to less intensive and more intensive providers, respectively. The coefficients can be interpreted as the average treatment effect among patients who are induced to switch to a more "typical" (less extreme) destination provider by their origin provider's exit (i.e. the "compliers"). The estimated coefficients may differ from those estimated in the difference-in-differences results discussed above for two reasons: i) effects of a switch among compliers may be different than effects among the larger treated population; ii) the sample of compliers may not provide sufficient power to estimate effects that were only marginally significant in the full sample.

Effects on overdose and hospitalizations are not precisely estimated, though the magnitudes and direction of the effect among patients switching to less intensive providers are similar to the event study specification. Among patients who switch to less intensive providers, the two-stage least squares approach estimates a large and significant 27.4 percentage point (51%) reduction in office visits, and a 6 percentage point (150%) increase in the likelihood of being diagnosed with an OUD.

6 Robustness

6.1 Time Relative to Last Origin Provider Visit

While the origin provider's exit represents the last month we observe that provider treating patients in an office setting, many patients have their last visit with their origin provider before the exit month. As an alternative to evaluating changes in opioid use relative to the origin provider's exit, we estimate our model where the event of interest is the last month in which the patient has a visit with their origin provider. Panel (a) of Figure A.14 reports the share of beneficiaries who have their last visit with their origin provider in a given month relative to that provider's exit. Note that there are a considerable number of beneficiaries whose last visit with their origin provider occurs in the -12 to -7 months prior the origin provider's exit. To create a balanced panel in the six-month pre-last-visit pre-period, we drop

these beneficiaries from any analysis with the last visit as the event, leaving 1,558 treated beneficiaries.[43]

There are trade-offs in using the last visit with an origin provider (instead of provider exit) as the event of interest. On the one hand, for the specific patient, the most relevant time in which to evaluate the impact of a switch in provider prescribing behavior is arguably at the point the patient transitions from their origin to their destination provider. In contrast to Figure A.2, which suggests that there is a transition period in the six months prior to the origin provider's exit during which a patient is receiving treatment both from their origin and destination provider, panel (b) of Figure A.14 suggests that, on average, patients have a relatively stable and low share of claims (around 0.10) associated with their destination provider prior to the last visit with their origin provider, and then somewhat abruptly shift their visits to their destination provider after the last visit. Specifically, the share of claims associated with their destination provider jumps to approximately 0.2 by the third month after the last visit.

On the other hand, the timing of the last visit a patient has with their origin provider might be less exogenous than the timing of the exit itself. Namely, the timing of the patient's decision to switch providers (or their origin provider's decision to stop treating them) may be correlated with unobserved, time-varying components of the error terms that affect trends in their opioid use. As before, we can check for the existence of parallel pre-switch trends, though this does not address all forms of bias that may enter into our estimates.

Panel (a) of Figure A.15 reports the effect of a change in provider prescribing intensity on patient opioid use on the pooled sample. All coefficients are zero in the pre-period, though there is a slight suggestion of (non-significant) pre-trends in the direction of the destination provider's intensity, in the months leading to the switch. Patients adapt their opioid use towards the intensity of their provider during the first six months following their last visit with their origin provider, and, ultimately, change their opioid use by between 20 to 30 log points in the direction of their destination providers prescribing intensity.

[43]Additionally, in creating relative-month indicator variables used in our regressions, we set all relative months $t < -6$ equal to -7, and all relative months $t > 12$ equal to 13. This allows us to use the information available in these observations to inform our estimation of both secular and relative time trends, while not letting relative months that have few beneficiaries represented have an outsized influence on our estimation of these time trends.

Panels (b) and (c) illustrate that these effects are asymmetric (though, due to smaller sample size, less precisely identified than both the pooled effect and the effects estimated in Figure 3), consistent with the effects we estimate when using the provider's exit month as the event of interest. After switching to less intense providers, patients reduce their opioid usage gradually over a six month period, until opioid use is reduced by between 30 to 40 log points in the seven to thirteen months following their last visit (panel (b)). Conversely, after switching to more intense providers, patients do not alter their opioid use until much later, with panel (c) suggesting an increase in use of slightly less than twenty log points approximately eleven months following the last visit with their origin provider.

Tables A.15 and A.16 report the coefficients estimated from the difference-in-differences regressions with additional health outcomes and utilization measures as the dependent variables for patients switching to less intensive and more intensive providers, respectively. The pre-period includes the six months prior to the last visit, and the post-period includes the 12 months following the last visit. With a smaller sample size, most effects are no longer significant. However, patients who switch to less intensive destination providers are less likely to have an office visit, and more likely to be diagnosed with an OUD following their last visit with their origin provider, consistent with our findings above.

6.2 Additional Robustness Checks

To explore the robustness of our results to heterogeneity in patient and provider composition, we estimate our difference-in-differences model on different quantiles of patient and provider characteristics. Table A.12 estimates the effect of a change in provider intensity on a patient's opioid use (as measured by log MED), by quintile of opioid use prior to the exit. We observe that effects are increasing across quintile of pre-exit use. However, this trend is likely driven by the inverse relationship between pre-switch opioid use and $\Delta_{ln(MED)}$; namely, larger pre-switch opioid use is associated with greater origin provider intensity, which is associated with a negative $\Delta_{ln(MED)}$ (which provides evidence of the regression to the mean). Because our effects are asymmetric with respect to the direction of $\Delta_{ln(MED)}$, it is not entirely surprising to observe that the coefficient on $Post \times \Delta_{ln(MED)}$ is larger in quantiles with greater pre-switch opioid use.

Similarly, we estimate our model on quintiles of origin and destination provider intensity (Tables A.13 and A.13, respectively). We find that a provider's influence on patient opioid use increases as origin provider intensity increases, but that there is not as clear of a pattern with respect to destination provider quintile. Furthermore, our observation of increasing effect across provider quintiles of intensity may also be driven by the mechanical relationship described above (i.e. with respect to the regression to the mean).

7 Conclusion

In November 2022, the CDC published updated guidance regarding the use of opioids in treating pain (Dowell et al., 2022). Referring to their 2016 publication which had been broadly interpreted as a recommendation against long-term opioid use among chronic pain patients (Dowell et al., 2016), they responded to the "misapplication" of their prior guidance, noting that the "inflexible application of recommended dosage and duration thresholds" had resulted in patient harms. In the updated guidance, they promote a "flexible, individualized, patient-centered" approach to care and opioid use. However, as in the 2016 publication, the empirical basis for this guidance with regards to how prescribing behaviors and changes in these behaviors affect patient opioid use and health outcomes is incomplete.

Our study contributes to understanding how a change in opioid prescribing behavior affects patient opioid use and downstream outcomes. We focus in particular on patients with chronic pain who have various levels of prior opioid use. We find that patients who switch to a provider with a different prescribing intensity change their own opioid use in the direction of their new provider's intensity, demonstrating that provider prescribing behavior is influential over patient opioid use, though this effect is asymmetric and much larger in the negative direction. Further, we find that this effect is moderated by a patient's prior opioid use, such that switching to a more intense provider increases opioid use only among opioid-naive patients, while switching to a less intense provider decreases opoid use only among prior opioid users. Additionally, our results allow us to decompose observed variation in opioid prescribing behavior (at least, across these three states) into approximately 20% provider-side factors and 80% patient-side factors. This suggests that the majority of observed variation

on prescribing intensity is due to patient demand of opioids (whether through preference or acuity of illness).

We further observe that among chronic pain patients who switch to lower-prescribing providers, their likelihood of adverse health events increases. Specifically, we find suggestive evidence of an increase in the likelihood of all-cause hospitalizations. Moreover, we observe a decrease in the likelihood of overdose (driven in particular by overdose from prescription opioids) among chronic pain patients who switch to relatively higher prescribers. While these effects are precisely estimated in the difference-in-differences model with a pooled pre and post period, they are less precisely estimated in our primary event study specification, as well as in our two-stage least squares estimations. This is at least in part reflective of the fact that such events are relatively rare.

Our findings build on prior research examining the effect of provider opioid prescribing behavior on patient opioid-related behaviors, which find that providers who prescribe opioids at high rates induce patients to use more opioids and lead to adverse health outcomes (Barnett et al., 2017; Eichmeyer and Zhang, 2021, 2022). While we do observe that opioid-naive patients increase their opioid use as they switch to more intensive providers, we observe no similar effect among patients with prior use, nor do we observe an increase in the likelihood of adverse health outcomes among patients switching to more intense providers. The difference in findings could reflect broader socioeconomic (Medicaid versus Medicare or VHA patients) or clinical (chronic pain versus general, varying levels of prior use versus opioid naive) differences in the study populations. Whereas these studies suggest that assigning opioid-naive patients to high prescribers leads to worse patient outcomes, our findings support concerns that sudden reductions in access to prescription opioids among patients with chronic pain (and varying levels of prior use) can lead to harm among these patients.

Our paper has several limitations, in addition to those discussed above. First, because we can't observe the plan that a patient is enrolled in, we cannot be certain that patients whose origin provider exits (or, patients who we classify as *not* experiencing an exit) remain in the same plan throughout their episode. However, we identify exiters in such a way that we at least exclude those patients who switch plans within Medicaid (the most likely event) to maintain their relationship with their provider. Second, we can only observe

prescription opioid use that generates a claim in Medicaid. Other research has demonstrated that patients who lose access to opioids through Medicaid coverage simply buy opioids out-of-pocket (Roberts et al., 2016). Thus, patients who have low-intensity providers may be accessing opioids outside of Medicaid in ways that we cannot capture. This would, however, provide additional intuition around our findings: if patients can access opioids through their providers, and use them under their guidance, then their risk of misuse and/or overdose might decrease. Third, as mentioned above, we do not observe any health events, such as overdose, that do not generate claims. If synthetic opioids were more likely to fall into this non-claim-generating category, then we might find similar patterns as in other populations were we able to observe all overdoses. Absent this, we are only able to comment on changes in the likelihood of overdoses that generate claims, which, in our sample, are driven primarily through overdose due to prescription drugs.

Despite these limitations, we believe our findings provide clear insights regarding the tradeoffs involved in restricting opioid access (via provider prescribing behavior) to combat opioid misuse, on the one hand; and harming certain patient populations who rely on opioids to manage their pain, on the other. While it's clear that sustained and/or high-dosage use of opioids can be harmful to many patients, leading to misuse and even death, it's also true that for at least some patient populations who use opioids to manage chronic pain, rapid declines in access to these drugs can have adverse consequences that should also be accounted for in policy and practice decisions. Policies or practice guidance that bluntly seek to reduce opioid prescriptions across all patients (and that create difficulties for providers in justifying prescribing opioids) may inadvertently increase the risk of adverse health events, such as overdose, ED visits, or hospitalizations among certain patient populations. Adopting a more patient-centric approach to opioid-related guidelines, or a more cautious approach to tapering patients off of opioids, may mitigate some of these risks.

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8 Tables and Figures

Note: all non-regression tables are created in R using Stargazer (Hlavac, 2018).

Table 1: Patient and Provider Characteristics

	Control	Treated	$\Delta_{Ln(MED)} < 0$	$\Delta_{Ln(Med)} > 0$
Panel A. Patient Characteristics				
Share Male	0.37	0.3	0.29	0.31
Mean Age (SD)	44 (12)	45 (11)	45 (11)	44 (11)
Share Opioid Naive	0.51	0.58	0.54	0.63
Mean Daily MED (SD)	3 (14)	16 (41)	18 (46)	14 (35)
Mean Months Observed (SD)	71 (17)	25 (0)	25 (0)	25 (0)
Num. Patients	18952	3151	1702	1471
Num. Patient-Episodes	18952	3197	1717	1480
Panel B. Origin Provider Characteristics				
Share Male	0.66	0.67	0.65	0.68
Share MDDO	0.75	0.82	0.77	0.87
Share PCP	0.41	0.41	0.43	0.4
Mean prescribing intensity (SD)		2.72 (0.94)	3.1 (0.82)	2.27 (0.87)
Mean prescribing rate (SD)		0.06 (0.08)	0.08 (0.1)	0.04 (0.06)
Num. Origin Providers	7224	496	372	371
Panel C. Destination Provider Characteristics				
Share Male		0.73	0.73	0.74
Share MDDO		0.81	0.79	0.83
Share PCP		0.36	0.34	0.38
Mean prescribing intensity (SD)		2.65 (0.9)	2.34 (0.86)	3.02 (0.81)
Mean prescribing rate (SD)		0.05 (0.08)	0.03 (0.06)	0.07 (0.1)
Num. Destination Providers		1799	1079	961

This table reports characteristics of the sample by cohort. Controls are patients whose provider does not exit Medicaid. Treated beneficiaries are those whose provider exits Medicaid. $\Delta_{Ln(MED)} < 0$ and $\Delta_{Ln(MED)} > 0$ are treated beneficiaries whose destination providers prescribe less and more opioids, respectively, than their origin providers. Because the same provider can treat patients who experience an increase and decrease in provider prescribing intensity, direction-specific treated cohort provider counts will not sum to total treated cohort counts. Share MDDO reports the share of providers that are physicians instead of advanced practice nurses. Share of PCPS are providers that have a specialty associated with primary care (family practice, general practice, internal medicine, obstetrics and gynecology, and pediatrics). Provider prescribing intensity is the average log MED across all patients a provider treats in the pre-exit period, excluding the index patient. Prescribing rate is the share of patients for whom a provider prescribes opioids in the pre-exit period, excludign the index patient. When included, standard errors are in parentheses.

Table 2: Change in Enrollee Opioid Use Relative to Provider Exit, Pre-Post, $\Delta_{ln(Med)}$

	Ln(MED) (1)	Pr(Opioid Rx) (2)	Share(Days Covered) (3)	Pr(MED > 120) (4)
Transition × $\Delta_{ln(Med)}$	0.063***	0.021***	0.018***	0.001
	(0.019)	(0.006)	(0.004)	(0.003)
Post × $\Delta_{ln(Med)}$	0.173***	0.042***	0.044***	0.005*
	(0.041)	(0.008)	(0.009)	(0.003)
Transition	0.016	-0.003	0.0001	-0.0001
	(0.019)	(0.006)	(0.005)	(0.003)
Post	-0.031	-0.034***	-0.025***	0.006*
	(0.037)	(0.008)	(0.008)	(0.003)
Observations	1,421,347	1,421,347	1,421,347	1,421,347
Adjusted R^2	0.49253	0.40678	0.50845	0.41617
Pre-Exit Mean, Treated	0.903	0.292	0.243	0.035

Standard errors are two-way clustered at the enrollee-episode and origin provider level and are reported in parentheses. Coefficients are obtained from estimating Equation 5. The transition period refers to the seven-month period prior to the exit ($t' \in [-6, 0]$). The post-period refers to the 12-month period following the exit ($t' \in [1, 12]$). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 3: Change in Enrollee Opioid Use Relative to Provider Exit, Pre-Post, $\Delta_{ln(Med)}$ Symmetry of Effects

Sample	(1) $\Delta_{ln(Med)} < 0$	(2) $\Delta_{ln(Med)} > 0$
Transition × $\Delta_{ln(Med)}$	0.094**	-0.010
	(0.044)	(0.032)
Post × $\Delta_{ln(Med)}$	0.301***	0.049
	(0.078)	(0.048)
Transition	0.037	0.076**
	(0.032)	(0.037)
Post	0.072	0.055
	(0.050)	(0.048)
Observations	1,384,347	1,378,422
Adjusted R^2	0.46757	0.46287
Pre-Exit Mean, Treated	1.00	0.785

Standard errors are two-way clustered at the enrollee-episode and origin provider level and are reported in parentheses. Coefficients are obtained from estimating Equation 5. The transition period refers to the seven-month period prior to the exit ($t' \in [-6, 0]$). The post-period refers to the 12-month period following the exit ($t' \in [1, 12]$). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 4: Two Stage Least Squares by Stage, Ln(MED)

Specification	(1) Pre-Post	(2) First Stage	(3) Reduced Form	(4) Second Stage
Post $\times$ $\Delta_{ln(Med)}$	0.169***			0.238***
	(0.041)			(0.073)
Post	-0.030	1.61***	0.353***	-0.030
	(0.037)	(0.071)	(0.100)	(0.037)
Post $\times$ Origin $ln(Med)$		-0.593***	-0.141***	
		(0.025)	(0.045)	
Observations	1,398,968	1,398,968	1,398,968	1,398,968
Adjusted R^2	0.47686	0.76261	0.47675	0.47681

Standard errors are two-way clustered at the enrollee-episode and origin provider level and are reported in parentheses. The reference period is the pre-period, as defined above. The transition period is dropped from the data used in estimation. The post-period refers to the 12-month period following the exit, as defined above. The first stage F-statistic is 573. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 5: Two Stage Least Squares $\Delta_{ln(Med)}$ Symmetry of Effects

Sample	(1) $\Delta_{ln(Med)} < 0$	(2) $\Delta_{ln(Med)} > 0$
Post $\times$ $\Delta_{ln(Med)}$	0.807***	0.049
	(0.279)	(0.107)
Post	0.424***	0.056
	(0.161)	(0.095)
Observations	1,372,328	1,368,062
Adjusted R^2	0.45716	0.45390
Pre-Exit Mean, Treated	1.00	0.785

Standard errors are two-way clustered at the enrollee-episode and origin provider level and are reported in parentheses. Coefficients are obtained from estimating Equation 5. The transition period refers to the seven-month period prior to the exit ($t' \in [-6, 0]$). The post-period refers to the 12-month period following the exit ($t' \in [1, 12]$). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 6: Change in Enrollee Opioid Use Relative to Provider Exit, Two Stage Least Squares, $\Delta_{ln(Med)}$

	Ln(MED) (1)	Pr(Opioid Rx) (2)	Share(Days Covered) (3)	Pr(MED > 120) (4)
Post × $\Delta_{ln(Med)}$	0.238***	0.066***	0.070***	-0.005
	(0.073)	(0.015)	(0.015)	(0.006)
Post	-0.030	-0.034***	-0.025***	0.006*
	(0.037)	(0.008)	(0.008)	(0.003)
Observations	1,398,968	1,398,968	1,398,968	1,398,968
Adjusted R^2	0.47681	0.39433	0.49331	0.40371
Pre-Exit Mean, Treated	0.903	0.292	0.243	0.035

Standard errors are two-way clustered at the enrollee-episode and origin provider level and are reported in parentheses. The reference period is the pre-period, as defined above. The transition period is dropped from the data used in estimation. The post-period refers to the 12-month period following the exit, as defined above. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 7: Change in The Probability of Health Events Relative to Provider Exit, $\Delta_{ln(MED)} < 0$

	Overdose (1)	Office Visit (2)	ED Visit (3)	Hospitalization (4)	OUD Dx (5)
Post × $\Delta_{ln(Med)}$	-0.0006	0.033*	-0.009	-0.008*	-0.041***
	(0.0006)	(0.019)	(0.007)	(0.004)	(0.009)
Post	-0.0008	-0.031*	-0.022***	-0.006	-0.008
	(0.0008)	(0.018)	(0.007)	(0.004)	(0.006)
Observations	1,384,347	1,384,347	1,384,347	1,384,347	1,384,347
Adjusted R^2	0.04019	0.32853	0.11787	0.09862	0.31215
Pre-Exit Mean, Treated	0.002	0.537	0.137	0.039	0.039

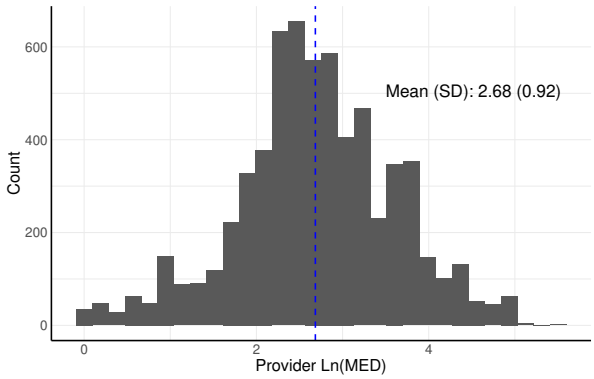
Standard errors are two-way clustered at the enrollee-episode and origin provider level and are reported in parentheses. Coefficients are obtained from estimating Equation 5. The transition period has been omitted for conciseness. The post-period refers to the 12-month period following the exit ($t' \in [1, 12]$). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 8: Change in The Probability of Health Events Relative to Provider Exit, $\Delta_{ln(MED)} > 0$

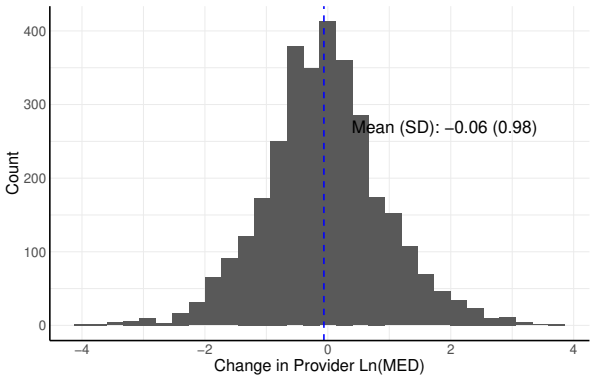
	Overdose (1)	Office Visit (2)	ED Visit (3)	Hospitalization (4)	OUD Dx (5)
Post $\times$ $\Delta_{ln(Med)}$	-0.002**	0.017	0.002	0.0003	-0.008
	(0.0010)	(0.014)	(0.006)	(0.005)	(0.010)
Post	0.001**	-0.013	-0.017**	-0.004	0.013*
	(0.0006)	(0.014)	(0.008)	(0.005)	(0.007)
Observations	1,378,422	1,378,422	1,378,422	1,378,422	1,378,422
Adjusted R^2	0.04022	0.32684	0.11753	0.09792	0.32085
Pre-Exit Mean, Treated	0.002	0.537	0.137	0.039	0.039

Standard errors are two-way clustered at the enrollee-episode and origin provider level and are reported in parentheses. Coefficients are obtained from estimating Equation 5. The transition period has been omitted for conciseness. The post-period refers to the 12-month period following the exit ($t' \in [1, 12]$). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

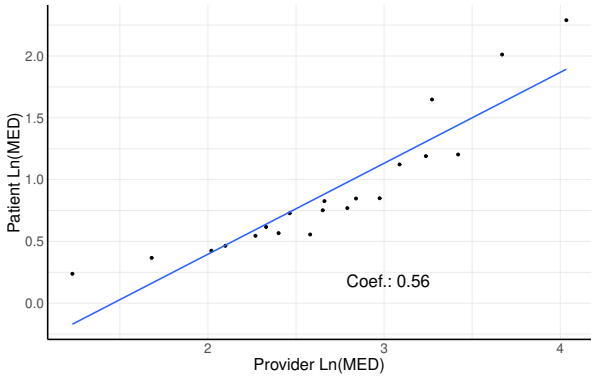
Figure 1: Provider Prescribing Behavior and Patient Opioid Use



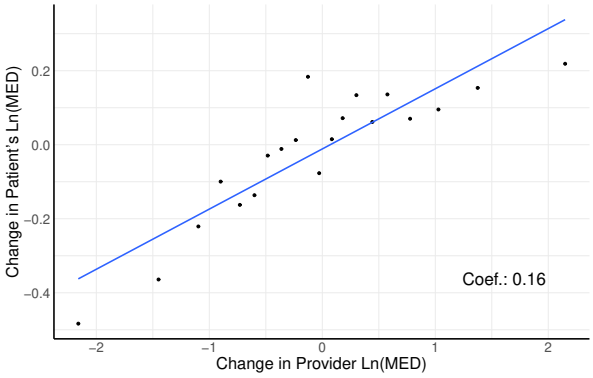
(a) Distribution in Provider Intensity



(b) Distribution in Change in Provider Intensity



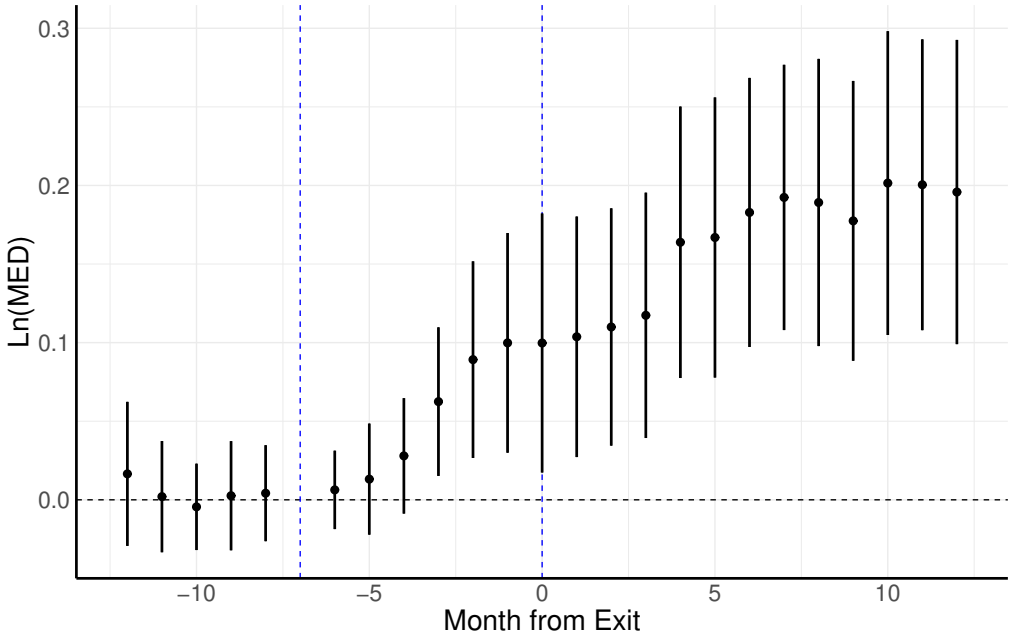
(c) Patient Opioid Use v. Provider Intensity



(d) Change in Patient Opioid Use v. Change in Provider Intensity

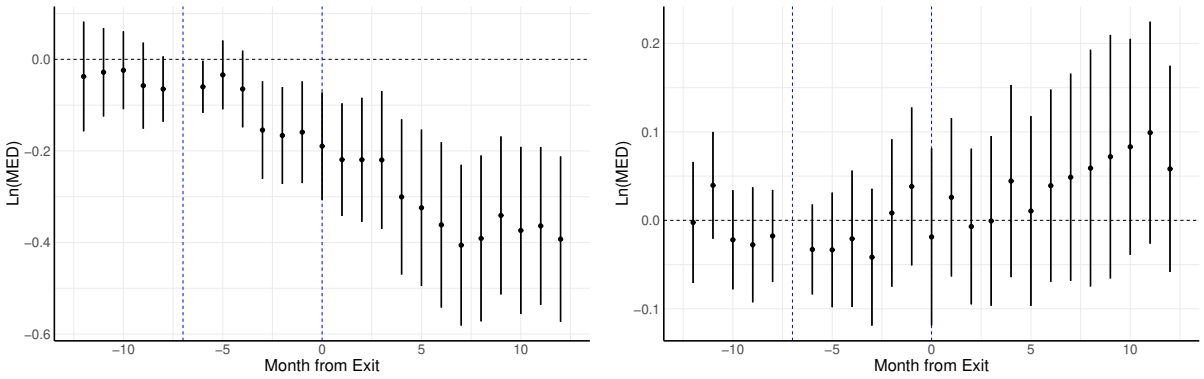
This figure describes trends in provider opioid prescribing behavior (as captured by the log of the average MED of their patients in the pre-period) and the relationship between prescribing behavior and a patient's monthly opioid use (as captured by log of daily MED). Panel (a) plots the distribution of each provider's average log MED in the pre-period. Panel (b) plots the distribution of the raw (non-demeaned) $\Delta_{ln(MED)}$. In panel (c), we calculate the vigintiles of provider opioid prescribing behavior, and plot the mean of the patient-level outcome and provider-prescribing behavior in each bin. Coefficients are estimated by regressing a patient's opioid behavior in a given year-month on the opioid-prescribing profile of the provider a patient has at a given time (in the pre- and transition-period, their origin provider; in the post-period, their destination provider), with no additional controls. In panel (d), we calculate the vigintiles of $\Delta_{ln(MED)}$ behavior, and then plot the mean $\Delta_{i,ln(MED)}$ and $\Delta_{p,ln(MED)}$ in each bin. Coefficients are estimated by regressing patient-level $\Delta_{ln(MED)}$ on $\Delta_{ln(MED)}$, with no additional controls.

Figure 2: Change in Patient's Ln(MED) Relative to Provider's Exit, Scaled by $\Delta_{ln(MED)}$



This figure plots the θ_qs from Equation 4.

Figure 3: Ln(MED) Before and After Switch, Scaled by $\Delta_{ln(MED)}$, by Direction of $\Delta_{ln(MED)}$



(a) $\Delta_{ln(MED)} < 0$ (b) $\Delta_{ln(MED)} > 0$

This figure plots the θ_qs from estimating Equation 4 on the sample of patients who switch to (a) less intensive destination providers; (b) more intensive destination providers.

Appendix A Additional Tables and Figures

Table A.1: Number of Providers by Exit Criteria

PLACEHOLDER

Table A.2: Patient Cohort Construction

PLACEHOLDER

Table A.3: Fallback condition

	(1)
Dep. var	Predicted forecast residual
Post × Origin $ln(Med)$	-0.005
	(0.004)
Post	0.014
	(0.010)
Observations	49,194
Adjusted R^2	0.62265

Standard errors are two-way clustered at the enrollee-episode and origin provider level and are reported in parentheses. The reference period is the pre-period, as defined above. The transition period is dropped from the data used in estimation. The post-period refers to the 12-month period following the exit, as defined above. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A.4: Frequency of Health Events by Month

Health Event	Treated and Control Enrollees	Treated Enrollees, Pre-Exit
ED Visit	0.058 (0.234)	0.137 (0.344)
IP Hosp	0.02 (0.139)	0.039 (0.195)
Office Visit	0.289 (0.453)	0.537 (0.499)
Opioid ED Visit	0.002 (0.047)	0.004 (0.067)
Opioid IP Hosp	0.001 (0.035)	0.003 (0.057)
Opioid Office Visit	0.017 (0.129)	0.025 (0.156)
Poisoning	0.001 (0.03)	0.002 (0.046)
Poisoning, Synthetic Opioids	0 (0.006)	0 (0)
Poisoning, Rx Opioids	0.001 (0.023)	0.001 (0.034)
Poisoning, Heroin	0 (0.009)	0 (0.007)
Disorders	0.028 (0.164)	0.039 (0.192)
Multiple opioid prescribers	0.007 (0.083)	0.039 (0.194)
SAT	0.002 (0.043)	0.004 (0.059)
Rehab	0 (0.021)	0.001 (0.03)
SAT or Rehab	0.002 (0.048)	0.004 (0.066)
Total Enrollee Year-Months	1421347	19182

This table reports the patient-level frequencies and standard deviations (in parentheses) for additional health events, at the month level. Column one reports these events overall, for treated and control enrollees. Column two reports these events among treated enrollees in the pre-exit period.

Table A.5: Change in The Probability of Health Events Relative to Provider Exit, $\Delta_{ln(MED)} < 0$, Opioid Naive

	Overdose (1)	Office Visit (2)	ED Visit (3)	Hospitalization (4)	OUD Dx (5)
Post $\times$ $\Delta_{ln(Med)}$	0.0002	0.013	-0.007	-0.004	-0.022**
	(0.0009)	(0.022)	(0.009)	(0.006)	(0.009)
Post	-0.0002	0.026	-0.010	0.003	-0.002
	(0.0010)	(0.019)	(0.008)	(0.005)	(0.005)
Observations	698,872	698,872	698,872	698,872	698,872
Adjusted R^2	0.04736	0.34173	0.13557	0.11033	0.33647
Pre-Exit Mean, Treated	0.001	0.466	0.114	0.037	0.034

Standard errors are two-way clustered at the enrollee-episode and origin provider level and are reported in parentheses. Coefficients are obtained from estimating Equation 5. The transition period has been omitted for conciseness. The post-period refers to the 12-month period following the exit ($t' \in [1, 12]$). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A.6: Change in The Probability of Health Events Relative to Provider Exit, $\Delta_{ln(MED)} < 0$, Prior Opioid Use

	Overdose (1)	Office Visit (2)	ED Visit (3)	Hospitalization (4)	OUD Dx (5)
Post $\times$ $\Delta_{ln(Med)}$	-0.001	0.050*	-0.011	-0.013*	-0.061***
	(0.002)	(0.026)	(0.011)	(0.007)	(0.018)
Post	-0.002	-0.105***	-0.037***	-0.017**	-0.017
	(0.002)	(0.026)	(0.010)	(0.007)	(0.013)
Observations	685,475	685,475	685,475	685,475	685,475
Adjusted R^2	0.03231	0.32022	0.10096	0.08553	0.29073
Pre-Exit Mean, Treated	0.003	0.636	0.169	0.043	0.045

Standard errors are two-way clustered at the enrollee-episode and origin provider level and are reported in parentheses. Coefficients are obtained from estimating Equation 5. The transition period has been omitted for conciseness. The post-period refers to the 12-month period following the exit ($t' \in [1, 12]$). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A.7: Change in The Probability of Health Events Relative to Provider Exit, $\Delta_{ln(MED)} > 0$, Opioid Naive

	Overdose (1)	Office Visit (2)	ED Visit (3)	Hospitalization (4)	OUD Dx (5)
Post $\times$ $\Delta_{ln(Med)}$	-0.0007	0.010	-0.006	0.0006	-0.006
	(0.0008)	(0.016)	(0.007)	(0.005)	(0.006)
Post	0.0004	0.005	0.009	-0.002	0.011*
	(0.0007)	(0.017)	(0.010)	(0.007)	(0.006)
Observations	698,797	698,797	698,797	698,797	698,797
Adjusted R^2	0.04634	0.34044	0.13494	0.10860	0.34486
Pre-Exit Mean, Treated	0.001	0.466	0.114	0.037	0.034

Standard errors are two-way clustered at the enrollee-episode and origin provider level and are reported in parentheses. Coefficients are obtained from estimating Equation 5. The transition period has been omitted for conciseness. The post-period refers to the 12-month period following the exit ($t' \in [1, 12]$). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A.8: Change in The Probability of Health Events Relative to Provider Exit, $\Delta_{ln(MED)} > 0$, Prior Opioid Use

	Overdose (1)	Office Visit (2)	ED Visit (3)	Hospitalization (4)	OUD Dx (5)
Post $\times$ $\Delta_{ln(Med)}$	-0.005*	0.035	0.011	-0.0003	-0.009
	(0.003)	(0.025)	(0.013)	(0.009)	(0.020)
Post	0.003*	-0.046*	-0.054***	-0.008	0.014
	(0.002)	(0.024)	(0.013)	(0.008)	(0.017)
Observations	679,625	679,625	679,625	679,625	679,625
Adjusted R^2	0.03322	0.31780	0.10061	0.08588	0.29957
Pre-Exit Mean, Treated	0.003	0.636	0.169	0.043	0.045

Standard errors are two-way clustered at the enrollee-episode and origin provider level and are reported in parentheses. Coefficients are obtained from estimating Equation 5. The transition period has been omitted for conciseness. The post-period refers to the 12-month period following the exit ($t' \in [1, 12]$). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A.9: Frequency and Type of Poisoning Events, Treated

Poisoning Event Details	Count	Share
Person-year-months with at least one event		
All	1269	
Synthetic Opioids	52	0.04
Prescription Opioids	767	0.6
Heroin	119	0.09
Sum of all events in person-year-months		
All	1563	
Synthetic Opioids	57	0.04
Prescription Opioids	890	0.57
Heroin	133	0.09

This table reports the frequency and specific type of poisoning events for all treated patients.

Table A.10: Change in The Probability of Health Events Relative to Provider Exit, TSLS, $\Delta_{ln(MED)} < 0$

	Overdose (1)	ED Visit (2)	Hospitalization (3)	Office Visit (4)	OUD Dx (5)
Post $\times$ $\Delta_{ln(Med)}$	-0.004	-0.028	-0.017	0.274***	-0.060**
	(0.003)	(0.021)	(0.017)	(0.088)	(0.024)
Post	-0.003	-0.035**	-0.012	0.138**	-0.022
	(0.002)	(0.016)	(0.012)	(0.056)	(0.014)
Observations	1,372,328	1,372,328	1,372,328	1,372,328	1,372,328
Adjusted R^2	0.04045	0.11638	0.09833	0.32629	0.31081
Pre-Exit Mean, Treated	0.002	0.137	0.039	0.537	0.039

Standard errors are two-way clustered at the enrollee-episode and origin provider level and are reported in parentheses. Coefficients are obtained from estimating the two-stage least squares version of Equation 5. The post-period refers to the 12-month period following the exit ($t' \in [1, 12]$). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A.11: Change in The Probability of Health Events Relative to Provider Exit, TSLS, $\Delta_{ln(MED)} > 0$

	Overdose (1)	ED Visit (2)	Hospitalization (3)	Office Visit (4)	OUD Dx (5)
Post $\times$ $\Delta_{ln(Med)}$	-0.001	0.022	-0.003	-0.037	-0.029
	(0.001)	(0.013)	(0.011)	(0.037)	(0.022)
Post	0.0007	-0.033***	-0.002	0.031	0.030
	(0.001)	(0.013)	(0.009)	(0.031)	(0.020)
Observations	1,368,062	1,368,062	1,368,062	1,368,062	1,368,062
Adjusted R^2	0.04062	0.11598	0.09735	0.32584	0.31615
Pre-Exit Mean, Treated	0.002	0.137	0.039	0.537	0.039

Standard errors are two-way clustered at the enrollee-episode and origin provider level and are reported in parentheses. Coefficients are obtained from estimating the two-stage least squares version of Equation 5. The post-period refers to the 12-month period following the exit ($t' \in [1, 12]$). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A.12: Change in Patient Ln(MED) Relative to Provider Exit, Pre-Post, $\Delta_{ln(Med)}$, By Quantile of Pre-Exit Opioid Use

	(1)	(2)	(3)	(4)	(5)
Sample	0 – 0.15	0.28 – 1.25	1.26 – 3.01	3.02 – 3.96	3.96 – 6.93
Transition × $\Delta_{ln(Med)}$	0.034**	0.114*	0.098*	0.165***	0.134
	(0.015)	(0.064)	(0.053)	(0.063)	(0.082)
Post × $\Delta_{ln(Med)}$	0.097***	0.211***	0.181***	0.328***	0.480***
	(0.028)	(0.078)	(0.068)	(0.081)	(0.103)
Transition	0.094***	0.072	-0.148***	-0.173***	-0.328***
	(0.017)	(0.054)	(0.041)	(0.057)	(0.081)
Post	0.148***	0.045	-0.279***	-0.621***	-0.938***
	(0.029)	(0.061)	(0.059)	(0.086)	(0.112)
Observations	1,408,422	1,348,322	1,377,847	1,377,622	1,378,047
Adjusted R^2	0.44207	0.43227	0.46238	0.52482	0.58528
Pre-Exit Mean, Treated	0.329	0.684	1.67	3.12	4.32
Mean $\Delta_{ln(MED)}$	0.030	-0.084	1.67	-0.128	-0.269

Standard errors are two-way clustered at the enrollee-episode and origin provider level and are reported in parentheses. Coefficients are obtained from estimating Equation 5. The transition period refers to the seven-month period prior to the exit ($t' \in [-6, 0]$). The post-period refers to the 12-month period following the exit ($t' \in [1, 12]$). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A.13: Change in Patient Ln(MED) Relative to Provider Exit, Pre-Post, $\Delta_{ln(Med)}$, By Quantile of Origin Provider Intensity

	(1)	(2)	(3)	(4)	(5)
Sample	0 – 2.12	2.12 – 2.43	2.43 – 2.9	2.91 – 3.61	3.61 – 5.19
Transition × $\Delta_{ln(Med)}$	0.029	0.071**	0.013	0.086	0.090
	(0.024)	(0.030)	(0.042)	(0.086)	(0.056)
Post × $\Delta_{ln(Med)}$	0.029	0.129**	0.069	0.269*	0.291***
	(0.044)	(0.061)	(0.058)	(0.140)	(0.088)
Transition	0.010	0.078***	0.060*	-0.030	0.017
	(0.035)	(0.027)	(0.035)	(0.056)	(0.059)
Post	0.055	0.106**	0.065	-0.143	0.003
	(0.054)	(0.042)	(0.041)	(0.091)	(0.114)
Observations	1,357,422	1,357,397	1,357,422	1,357,772	1,357,022
Adjusted R^2	0.43433	0.43623	0.44331	0.44935	0.46348
Pre-Exit Mean, Treated	0.275	0.451	0.739	1.20	1.86
Mean $\Delta_{ln(MED)}$	0.824	0.162	0.739	-0.225	-0.774

Standard errors are two-way clustered at the enrollee-episode and origin provider level and are reported in parentheses. Coefficients are obtained from estimating Equation 5. The transition period refers to the seven-month period prior to the exit ($t' \in [-6, 0]$). The post-period refers to the 12-month period following the exit ($t' \in [1, 12]$). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A.14: Change in Patient Ln(MED) Relative to Provider Exit, Pre-Post, $\Delta_{ln(Med)}$, By Quantile of Destination Provider Intensity

	(1)	(2)	(3)	(4)	(5)
Sample	0 – 1.98	1.98 – 2.45	2.45 – 2.85	2.85 – 3.37	3.37 – 5.5
Transition × $\Delta_{ln(Med)}$	0.014	0.068*	0.159***	0.131***	0.012
	(0.030)	(0.037)	(0.046)	(0.048)	(0.041)
Post × $\Delta_{ln(Med)}$	0.148**	0.107	0.315***	0.355***	0.091
	(0.060)	(0.075)	(0.115)	(0.066)	(0.073)
Transition	0.022	0.021	-0.030	0.028	0.029
	(0.034)	(0.029)	(0.028)	(0.042)	(0.055)
Post	0.019	0.028	-0.105	-0.097*	-0.011
	(0.050)	(0.045)	(0.070)	(0.053)	(0.103)
Observations	1,357,497	1,357,322	1,357,422	1,357,397	1,357,397
Adjusted R^2	0.43375	0.43987	0.43896	0.44836	0.46579
Pre-Exit Mean, Treated	0.380	0.651	0.722	1.04	1.72
Mean $\Delta_{ln(MED)}$	-0.703	-0.189	0.722	0.222	0.676

Standard errors are two-way clustered at the enrollee-episode and origin provider level and are reported in parentheses. Coefficients are obtained from estimating Equation 5. The transition period refers to the seven-month period prior to the exit ($t' \in [-6, 0]$). The post-period refers to the 12-month period following the exit ($t' \in [1, 12]$). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A.15: Change in The Probability of Health Events Relative to Month from Last Origin Visit, $\Delta_{ln(MED)} < 0$

	Overdose	ED Visit	Hospitalization	Office Visit	OUD Dx
	(1)	(2)	(3)	(4)	(5)
Post × $\Delta_{ln(Med)}$	6.06×10^{-5}	0.005	0.004	0.044**	-0.031**
	(0.001)	(0.007)	(0.005)	(0.018)	(0.013)
Post	1.98×10^{-5}	-0.001	0.001	-0.101***	-0.008
	(0.001)	(0.007)	(0.005)	(0.028)	(0.009)
Observations	1,359,054	1,359,054	1,359,054	1,359,054	1,359,054
Adjusted R^2	0.04001	0.11450	0.09777	0.32826	0.30825
Pre-Exit Mean, Treated	0.002	0.120	0.040	0.703	0.057

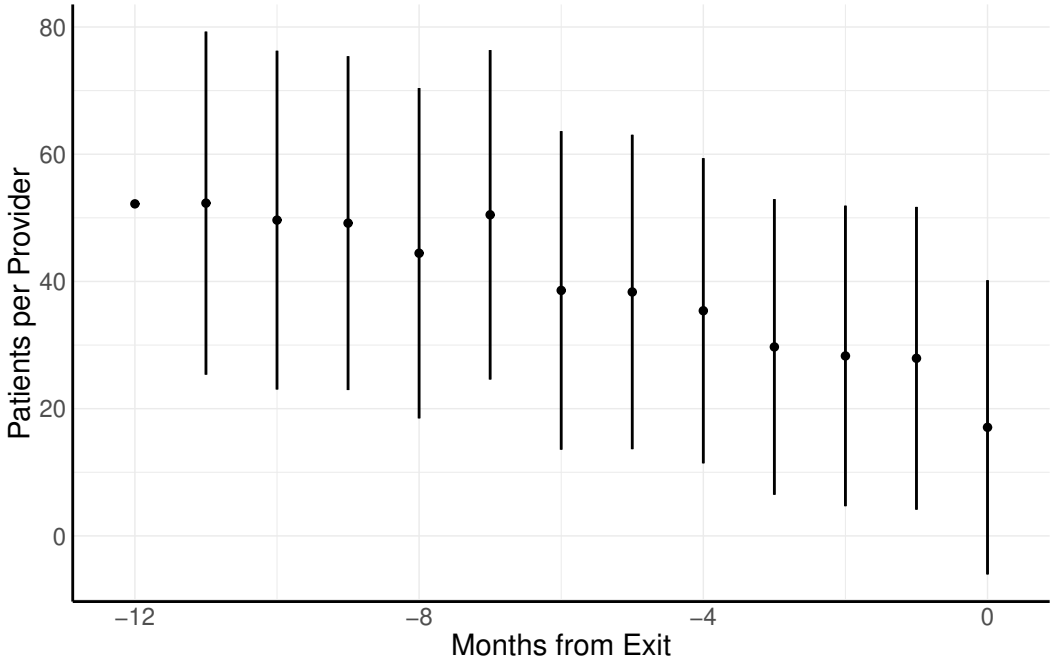
Standard errors are two-way clustered at the enrollee-episode and origin provider level and are reported in parentheses. Coefficients are obtained from estimating Equation 5. The transition period has been omitted for conciseness. The post-period refers to the 12-month period following the exit ($t' \in [1, 12]$). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A.16: Change in The Probability of Health Events Relative to Month from Last Origin Visit, $\Delta_{ln(MED)} > 0$

	Overdose (1)	ED Visit (2)	Hospitalization (3)	Office Visit (4)	OUD Dx (5)
Post $\times$ $\Delta_{ln(Med)}$	-0.003*	-0.016	-0.008	0.004	-0.011
	(0.002)	(0.010)	(0.007)	(0.018)	(0.013)
Post	0.002	0.006	0.008	-0.078***	0.0006
	(0.001)	(0.010)	(0.006)	(0.021)	(0.017)
Observations	1,353,392	1,353,392	1,353,392	1,353,392	1,353,392
Adjusted R^2	0.04004	0.11421	0.09688	0.32656	0.31339
Pre-Exit Mean, Treated	0.002	0.120	0.040	0.703	0.057

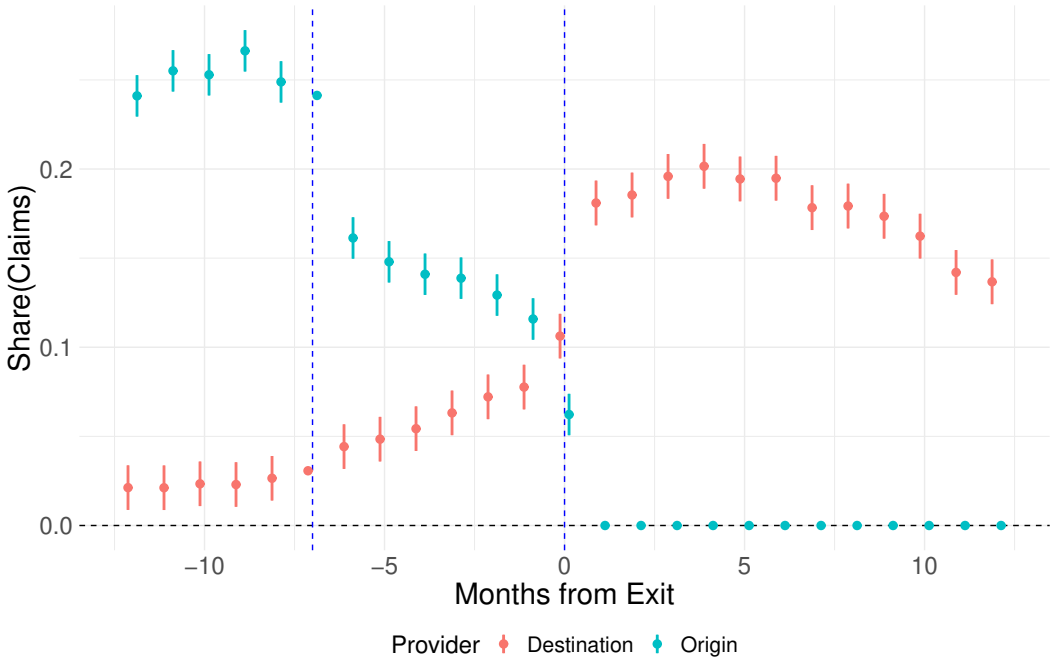
Standard errors are two-way clustered at the enrollee-episode and origin provider level and are reported in parentheses. Coefficients are obtained from estimating Equation 5. The transition period has been omitted for conciseness. The post-period refers to the 12-month period following the exit ($t' \in [1, 12]$). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Figure A.1: Patients per Month Relative to Exit



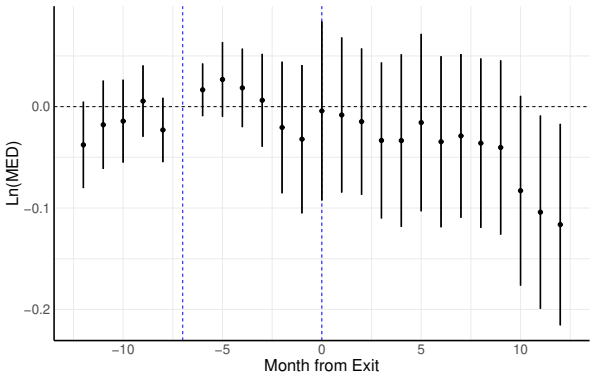
This figure plots the average number of patients a provider is observed treating in an office visit per month prior to their exit in month 0.

Figure A.2: Share of Claims Associated with Origin and Destination Key Providers

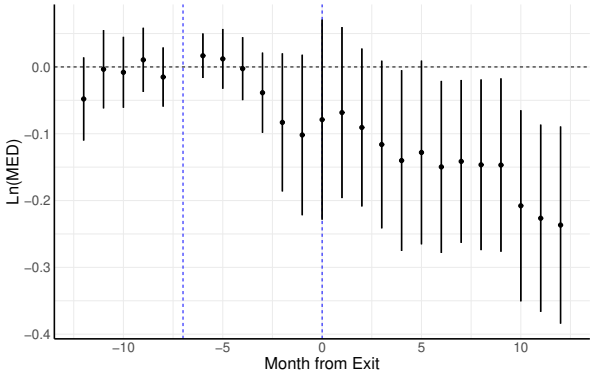


This figure plots the share of physician's office claims, by month relative to origin provider exit, associated with the origin and destination key providers. The point estimates are obtained from regressing the share of claims associated with a particular provider on indicators for relative time, with no additional controls. The estimates include months in which the patient had no claims (i.e. includes 0's). The month of reference is relative month -6.

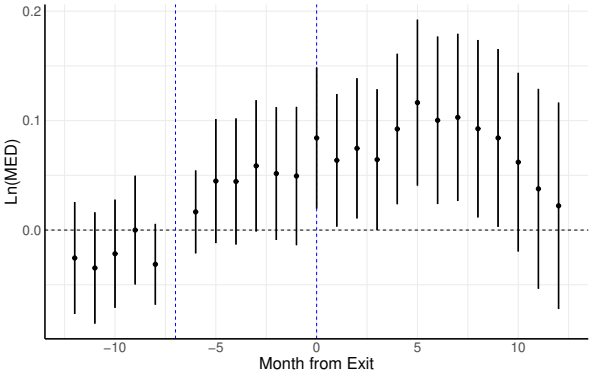
Figure A.3: Ln(MED) Relative to Provider's Exit, by Change in Relative Intensity



(a) All Switches



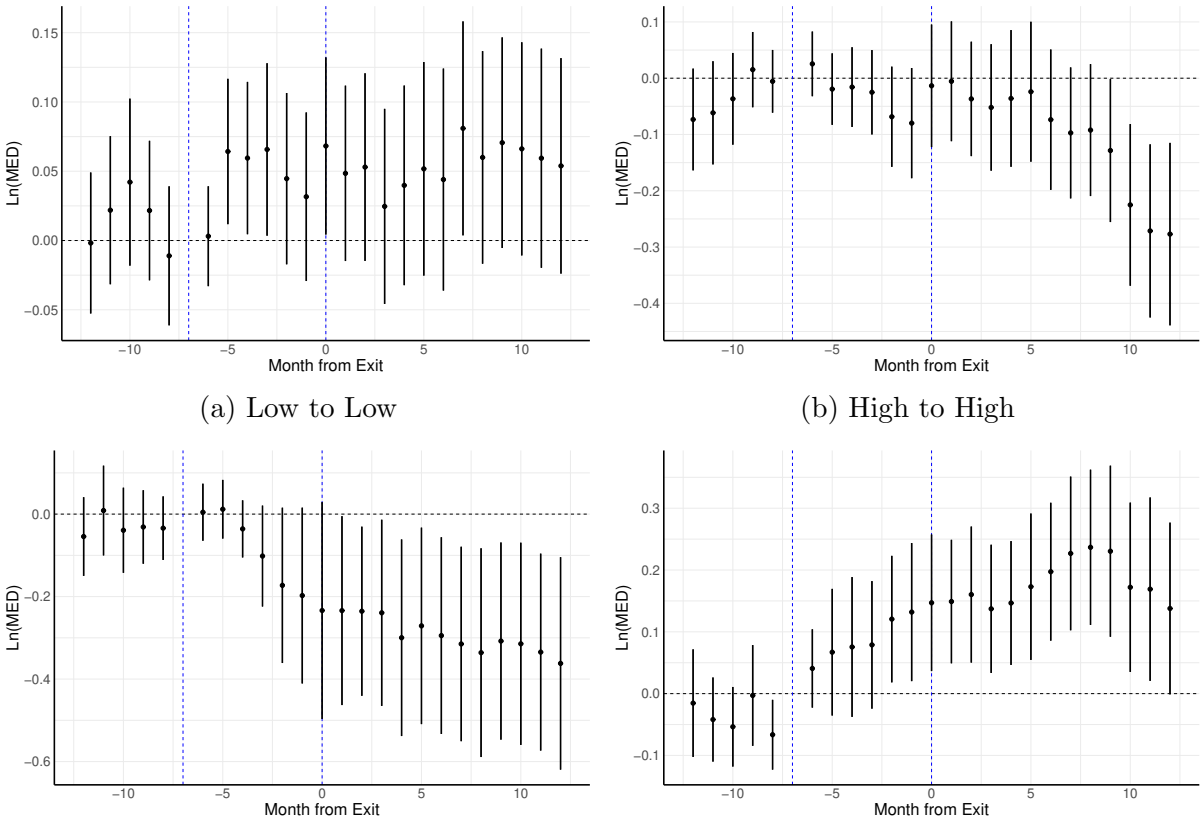
(b) Intensity$_o$ > Intensity$_d$



(c) Intensity$_o$ < Intensity$_d$

This figure plots the coefficients from estimating the simple event study given in Equation 4 separately on a) patients moving to less intensive providers ($\Delta_{ln(MED)} \leq 0$); and b) patients moving to more intensive providers ($\Delta_{ln(MED)} > 0$.

Figure A.4: Ln(MED) Relative to Provider's Exit



(a) Low to Low

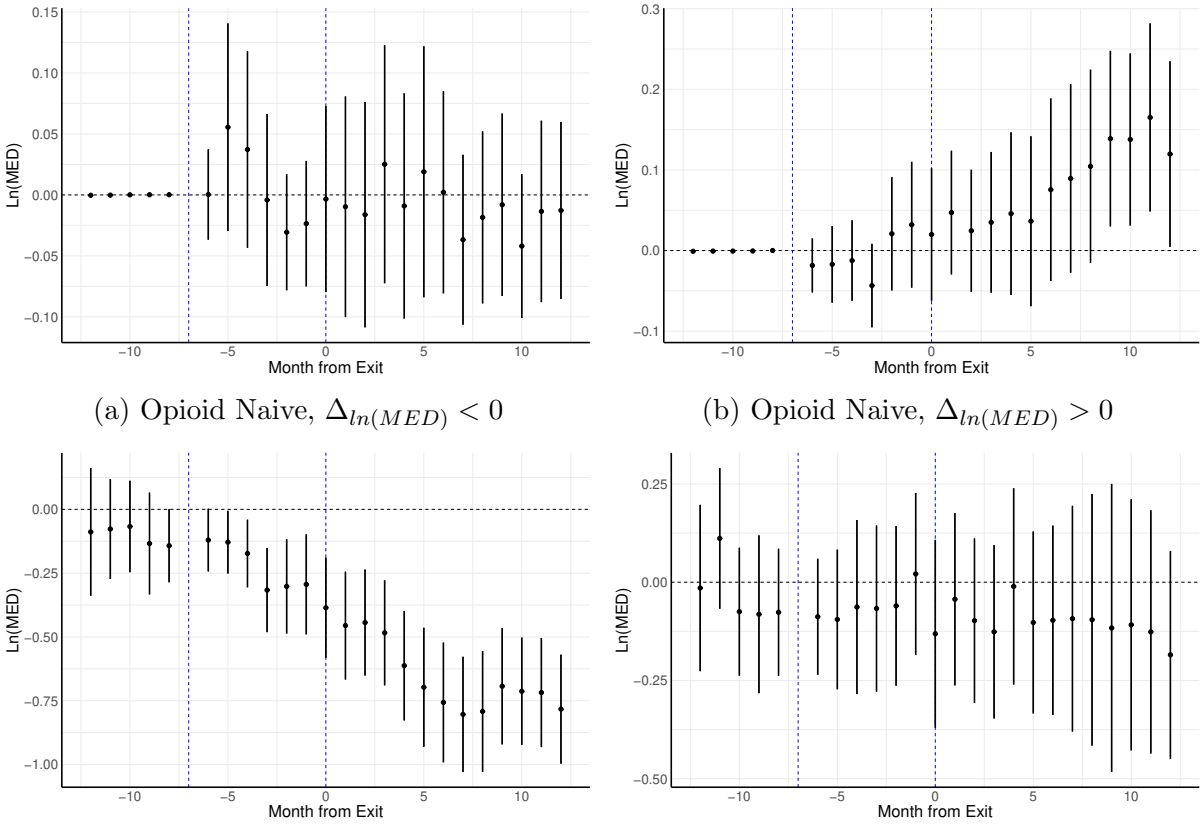
(b) High to High

(c) High to Low

(d) Low to High

This figure plots the coefficients from estimating the simple event study given in Equation 4 separately on patients who switch from: a) a below-median intensity provider to a below-median intensity provider ("low to low"; 1,031 switching patients); b) an above-median intensity provider to an above-median intensity provider ("high to high"; 1,028 switching patients); c) an above-median intensity provider to a below-median intensity provider ("high to low"; 612 switching patients); and d) a below-median intensity provider to an above-median intensity provider ("low to high"; 526 switching patients). All specifications include non-switching patients as the control cohort (18,952 non-switching patients).

Figure A.5: Heterogeneity In Effects by Prior Opioid Use



(a) Opioid Naive, $\Delta_{ln(MED)} < 0$

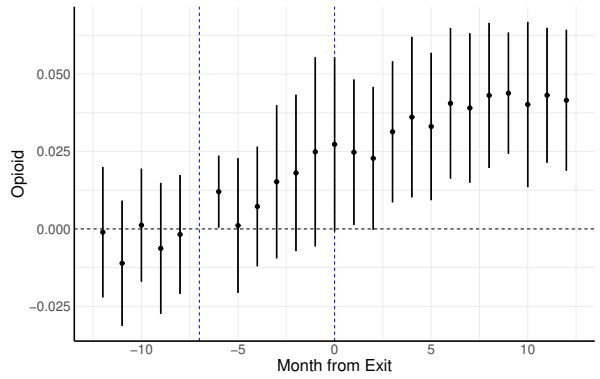
(b) Opioid Naive, $\Delta_{ln(MED)} > 0$

(c) Prior Opioid Use, $\Delta_{ln(MED)} < 0$

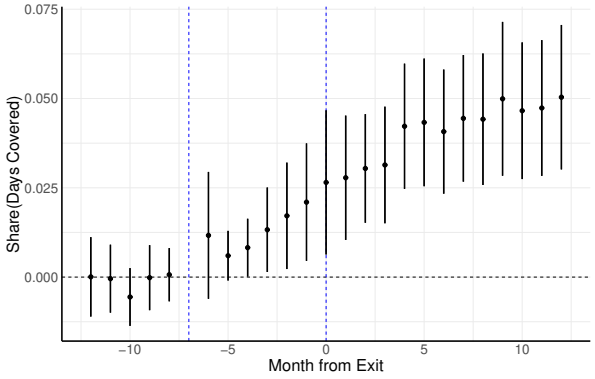
(d) Prior Opioid Use, $\Delta_{ln(MED)} > 0$

This figure plots trends in opioid use and provider intensity for opioid naive patients. Panels (a) and (b) plot the θ_qs obtained from estimating Equation 4 on the sample of opioid naive treated (and control) enrollees whose destination provider is less and more intense than their origin provider, respectively. Panels (c) and (d) plot the θ_qs obtained from estimating Equation 4 on the sample of prior opioid using treated (and control) enrollees whose destination provider is less and more intense than their origin provider, respectively

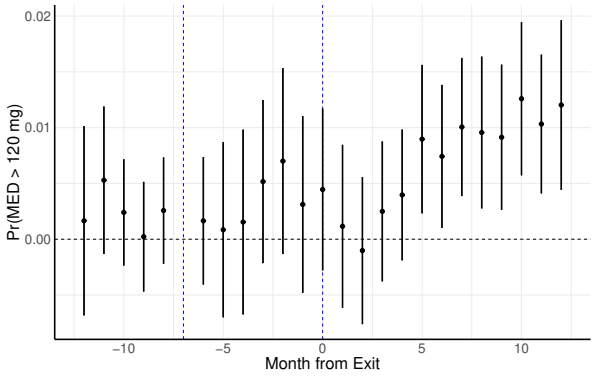
Figure A.6: Change in Additional Patient Opioid Use Measures Relative to Provider's Exit, Scaled by $\Delta_{ln(MED)}$



(a) Probability of Opioid Prescription



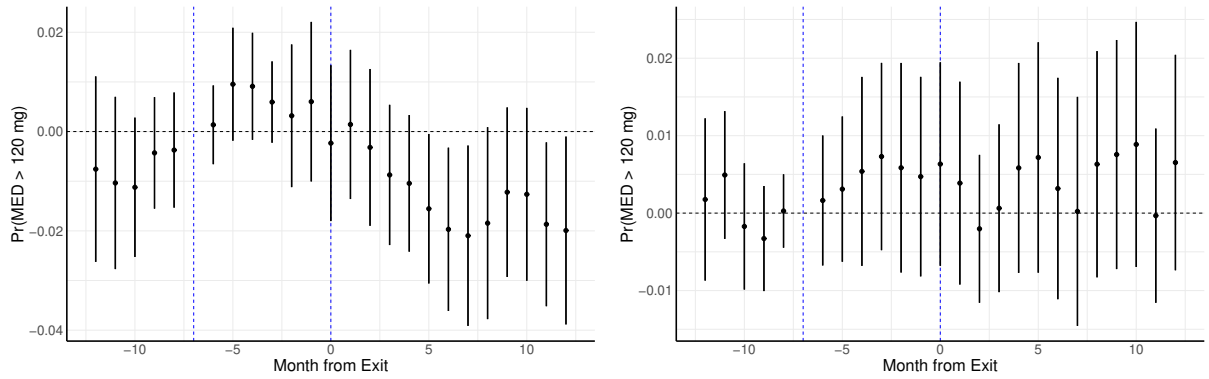
(b) Share of Days Covered



(c) Probability of MED $>$ 120

This figure plots the θ_qs from Equation 4.

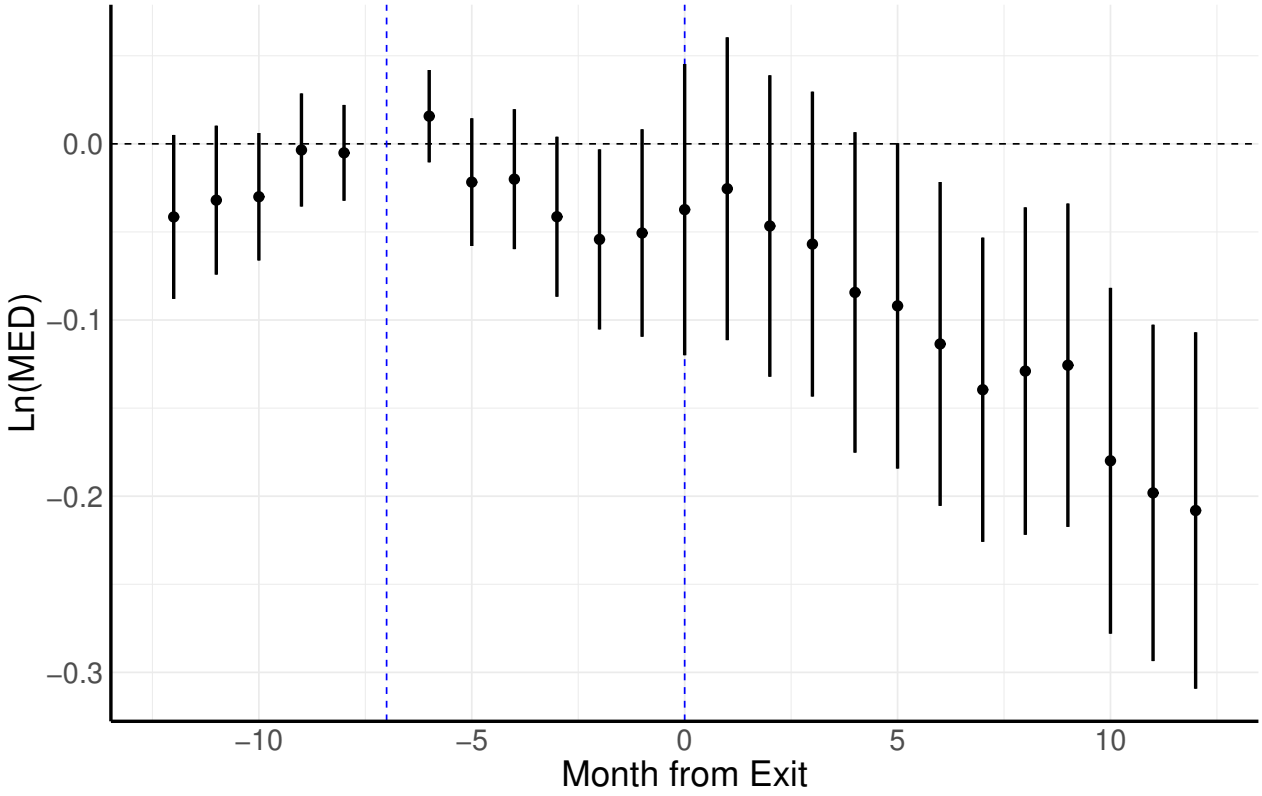
Figure A.7: Probability of MED > 120 Before and After Switch, Scaled by $\Delta_{ln(MED)}$, by Direction of $\Delta_{ln(MED)}$



(a) $\Delta_{ln(MED)} < 0$ (b) $\Delta_{ln(MED)} > 0$

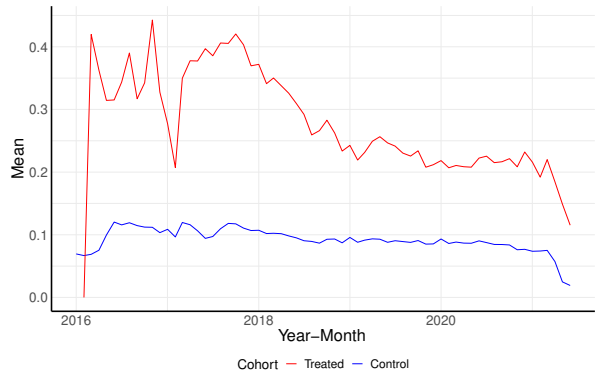
This figure plots the θ_qs from estimating Equation 4 on the change in the likelihood in any given month of having MED > 120 mg on the sample of patients who switch to (a) less intensive destination providers; (b) more intensive destination providers.

Figure A.8: Two-Stage Least Squares Balance Test

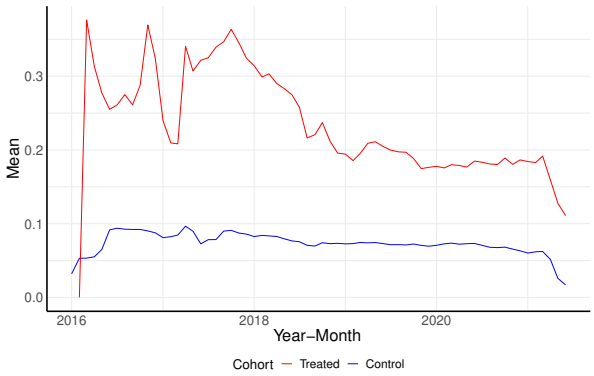


This figure plots the coefficients obtained from estimating the reduced form equation in our two-stage least squares approach.

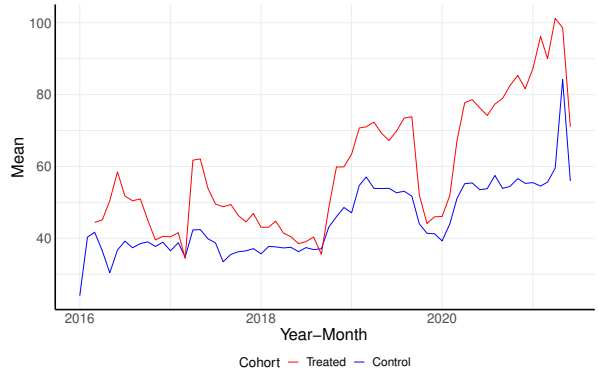
Figure A.9: Trends in Opioid Use Over Time, by Treated and Control Cohorts



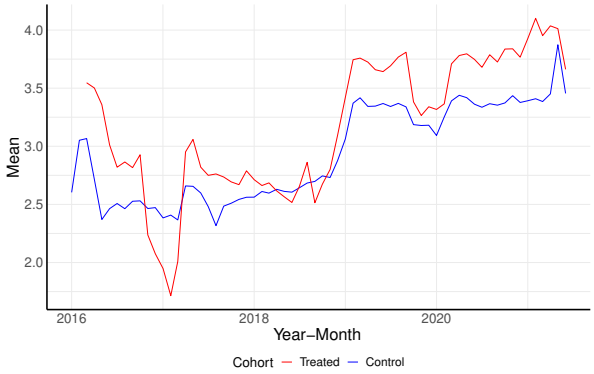
(a) Share of Patients With Opioid Prescription



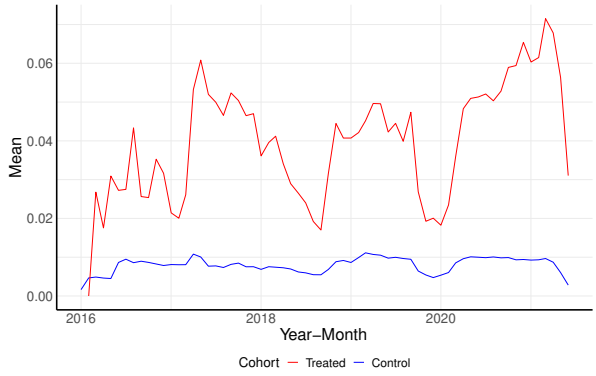
(b) Share of Days Covered



(c) Level MED, Conditional on Opioid Rx



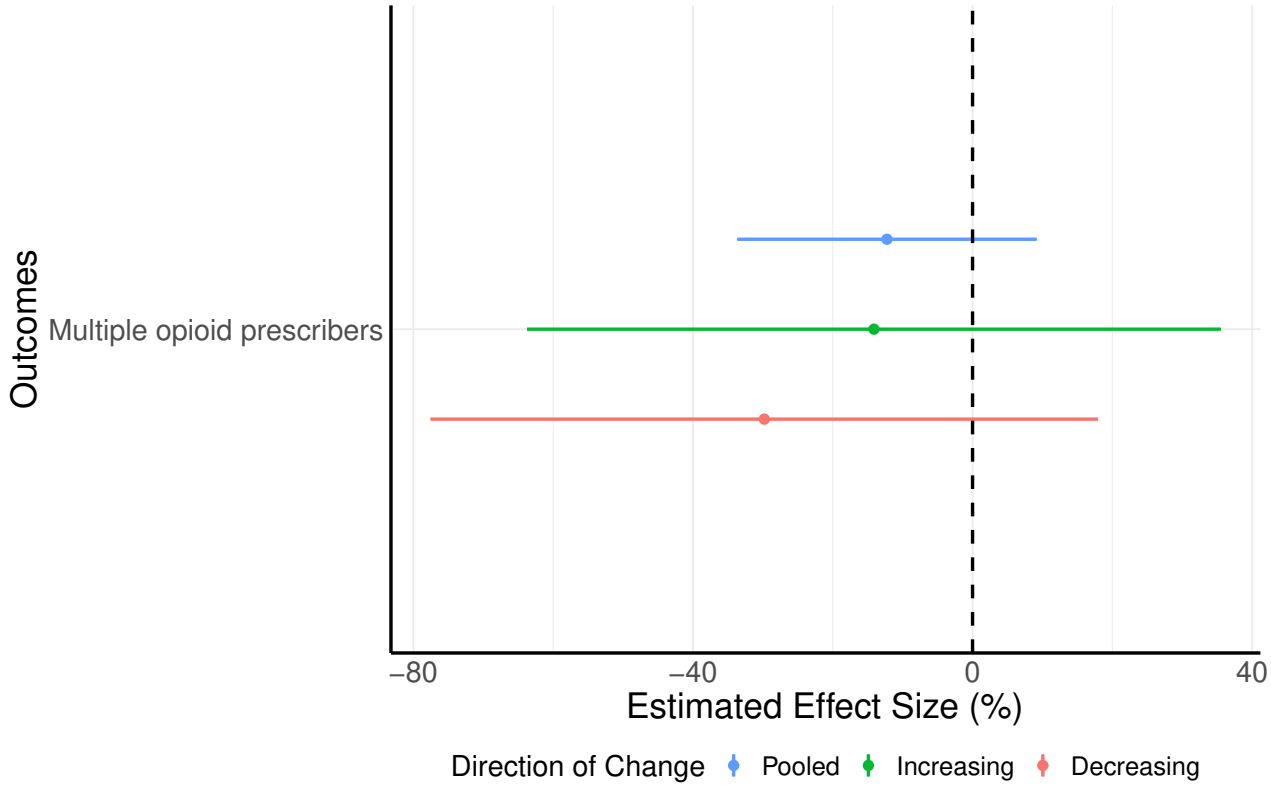
(d) Ln(MED), Conditional on Opioid Rx



(e) Share of Patients With MED $>$ 120 mg

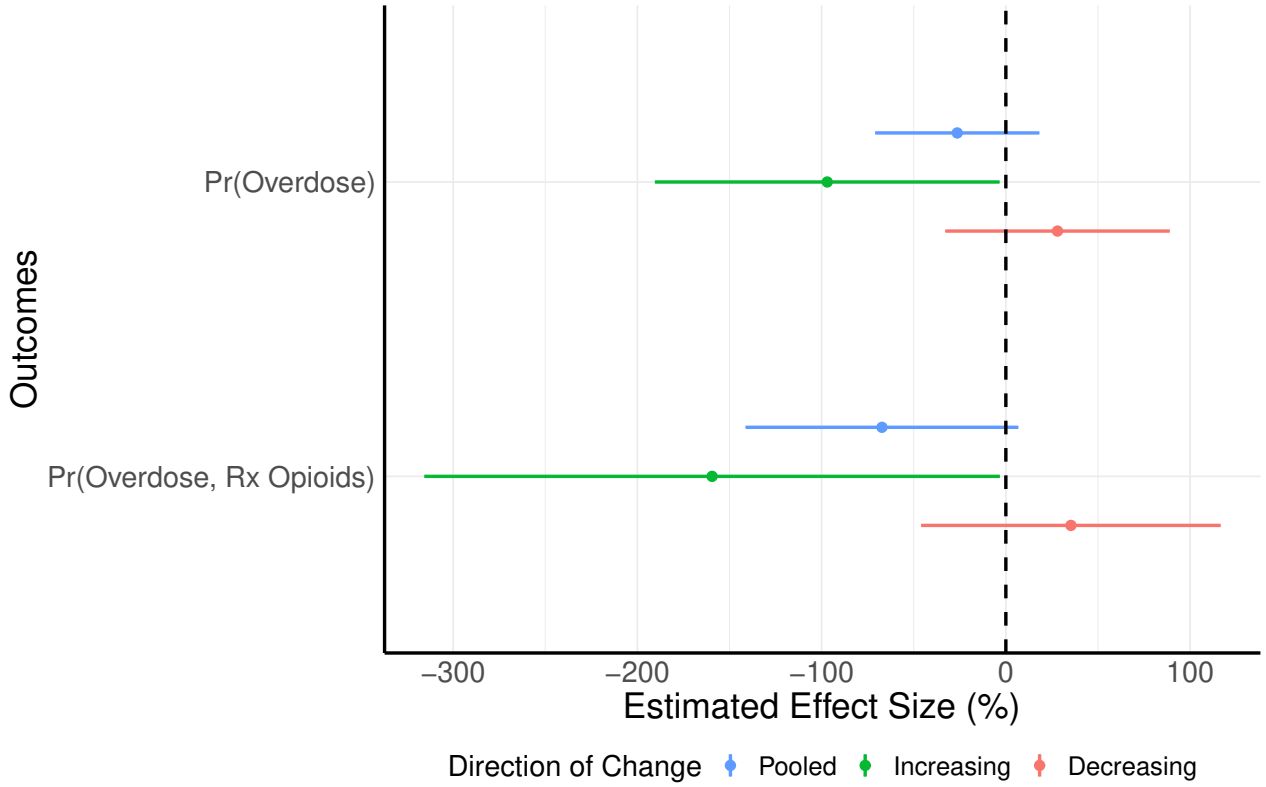
This figure plots unadjusted daily MED over patients that filled an opioid prescription in that year-month, ln(MED) over patients that filled an opioid prescription in that year-month, share of patients with an opioid prescription, share of days covered by an opioid prescription, and share of patients with a daily MED of greater than 120 mg per calendar year-month for treated and control cohorts.

Figure A.10: Changes in the Probability of Multiple Opioid Prescribers, Relative to Provider's Exit, Scaled by $\Delta_{ln(MED)}$



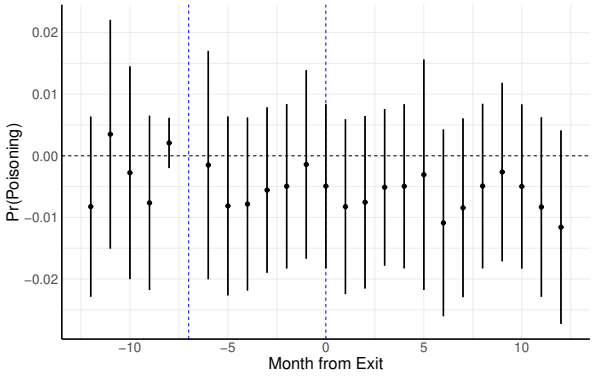
This figure plots the θ from Equation 5 for the probability of a patient having more than one prescriber. Given the very low frequency of overdose due to synthetic opioids and heroin, we omit estimated effects for these events. For ease of interpretation, all effects are represented as percents (we calculate this by dividing the coefficient by the pre-exit mean among treated patients).

Figure A.11: Change in the Probability of Overdose, Relative to Provider's Exit, Scaled by $\Delta_{ln(MED)}$

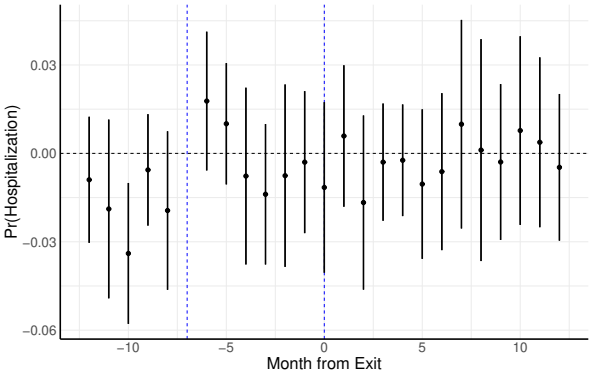


This figure plots the θ from Equation 5 for overdose, overall and by specific type. Given the very low frequency of overdose due to synthetic opioids and heroin, we omit estimated effects for these events. For ease of interpretation, all effects are represented as percents (we calculate this by dividing the coefficient by the pre-exit mean among treated patients).

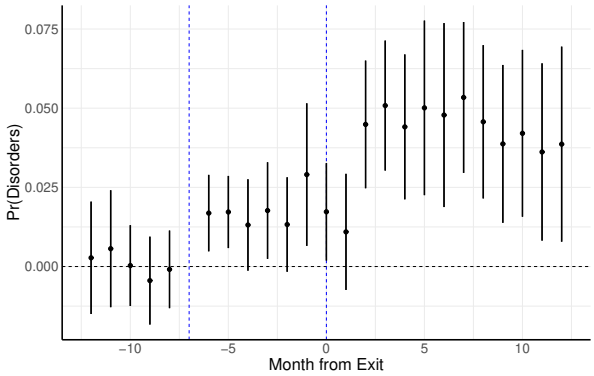
Figure A.12: Change in the Probability of Events, Relative to Provider Exit, Scaled by $\Delta_{ln(MED)}$



(a) Overdose, $\Delta_{ln(MED)} > 0$, Prior Use



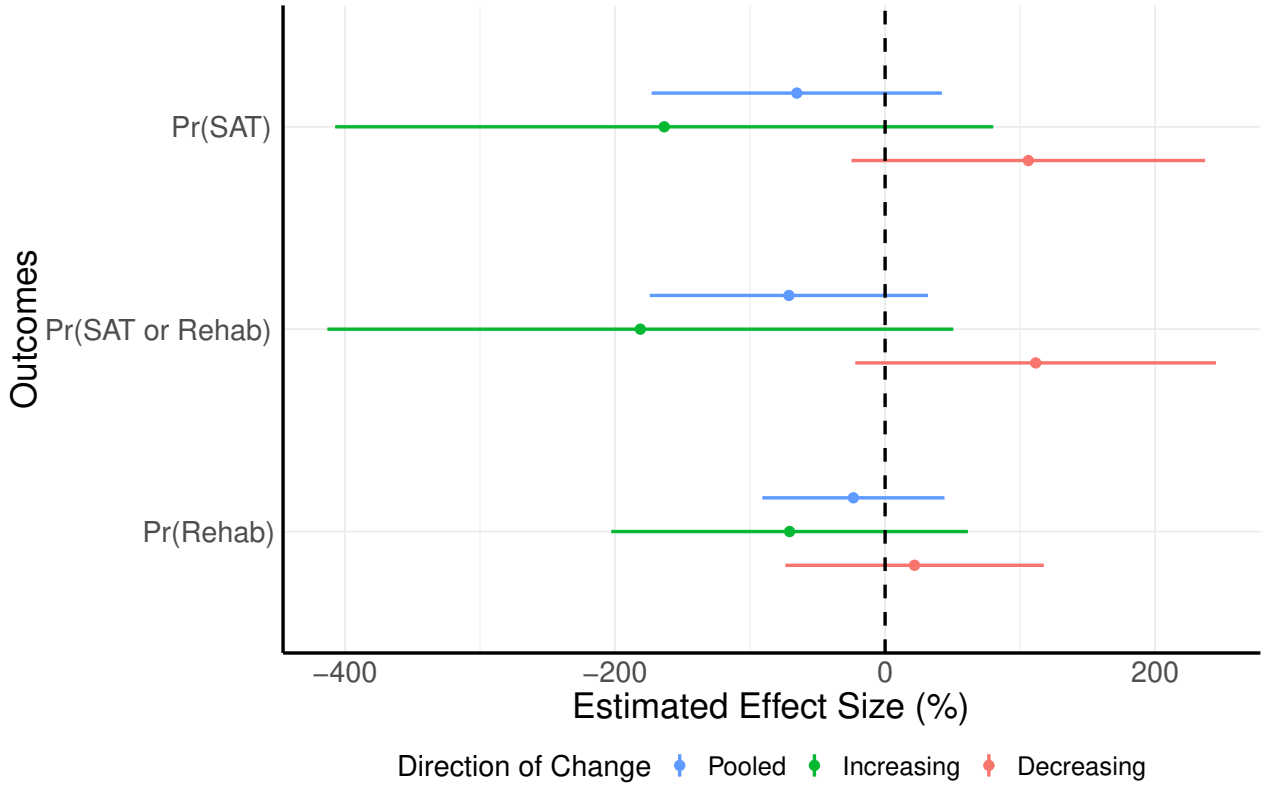
(b) Hospitalization, $\Delta_{ln(MED)} < 0$, Prior Use



(c) OUD Diagnosis, $\Delta_{ln(MED)} < 0$

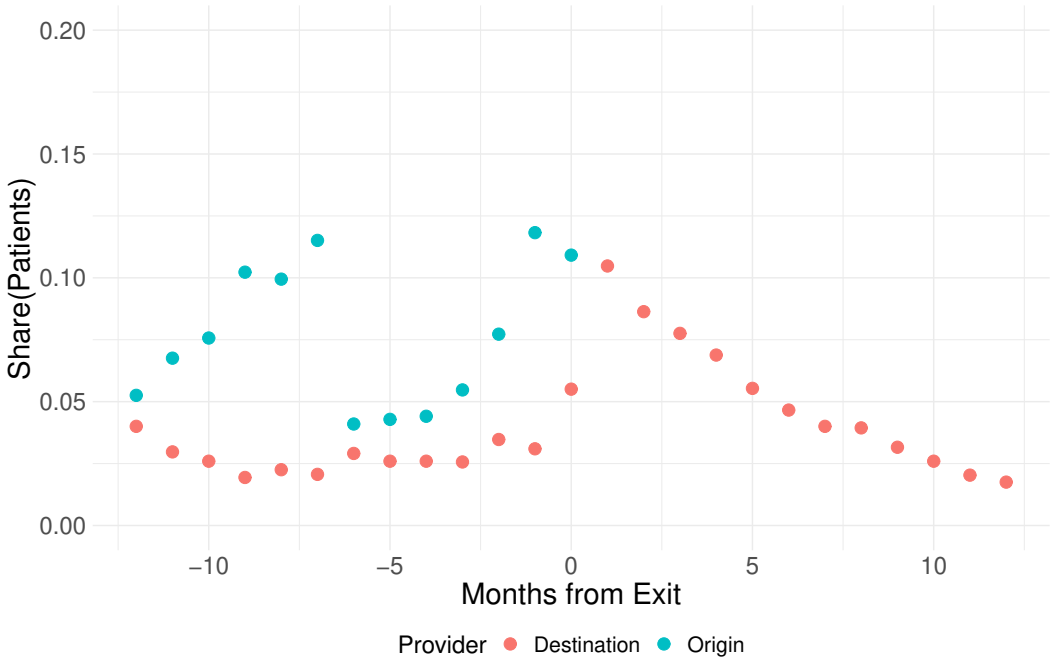
This figure plots the θ_qs estimated from Equation 4 for additional outcomes of interest. Panel (a) plots the relative change in likelihood of an overdose for patients with prior opioid use who switch to more intensive destination providers. Panel (b) plots the relative change in likelihood of a hospitalization for patients with prior opioid use who switch to less intensive destination providers. Panel (c) plots the relative change in likelihood of being diagnosed with an OUD for patients with prior opioid use and opioid-naive patients who switch to a less intensive provider.

Figure A.13: Change in the Probability of SAT or Rehabilitation Center Use, Relative to Provider's Exit, Scaled by $\Delta_{ln(MED)}$

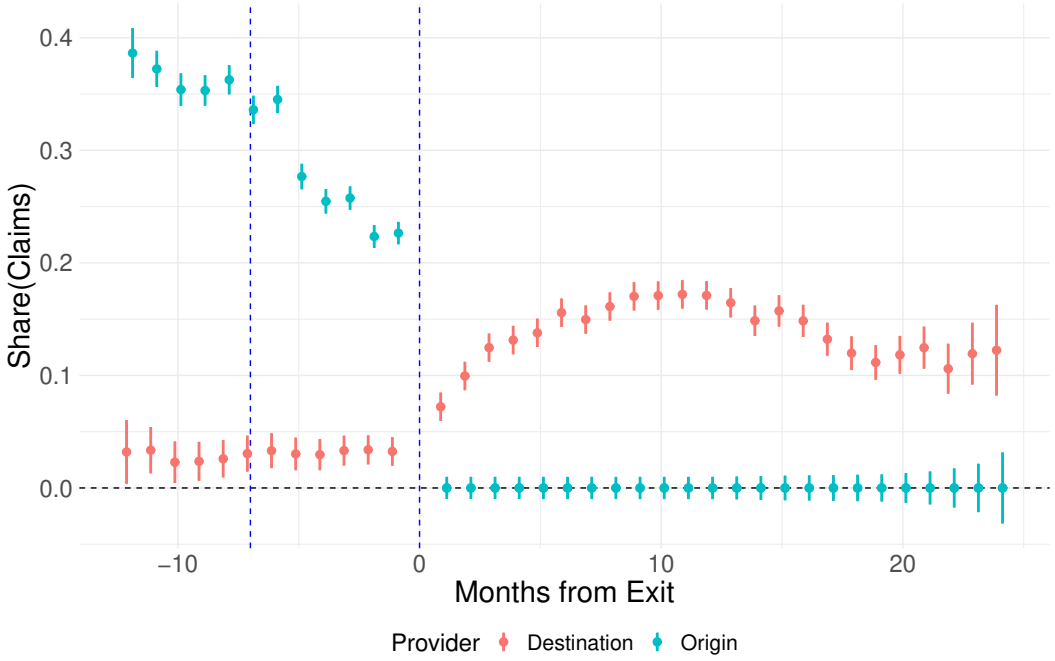


This figure plots the θ_qs estimated from Equation 5 for substance abuse treatment or rehabilitation center use. For ease of interpretation, all effects are represented as percents (we calculate this by dividing the coefficient by the pre-exit mean among treated patients).

Figure A.14: Timing and Claims Associated with Last Visit with Origin Provider



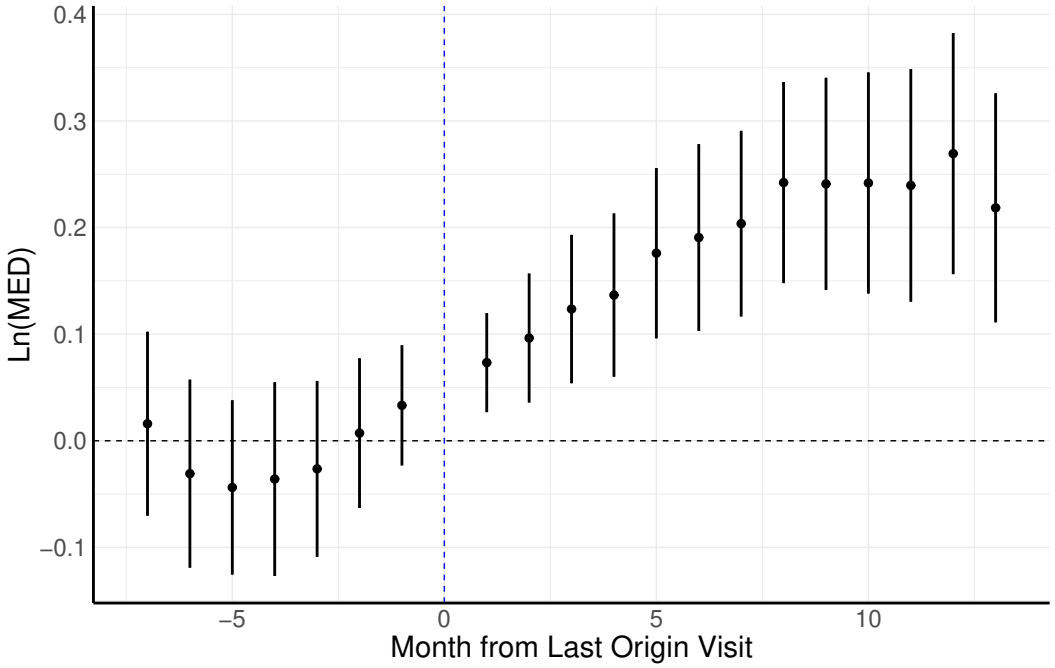
(a) Share of First and Last Visit with Destination and Origin Provider, Respectively



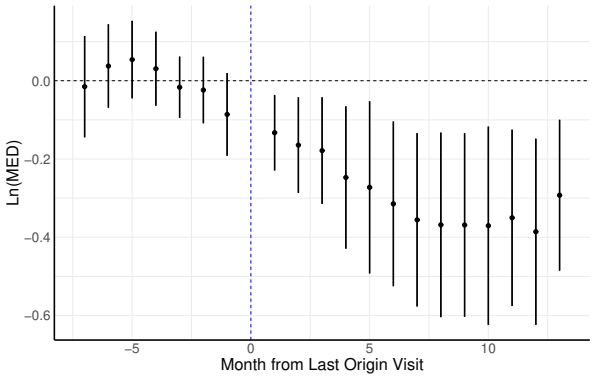
(b) Share Claims Relative to Last Visit with Origin Provider

This figure plots trends in timing of and claims associated with the last visit a patient has with their origin provider. Panel (a) plots the share of patients whose last and first visit with the origin and destination provider, respectively, occur in a given month relative to origin provider exit. Panel (b) plots plots the share of claims in the months relative to the patient's last visit with their origin provider. Months in which the patient has no visits are included.

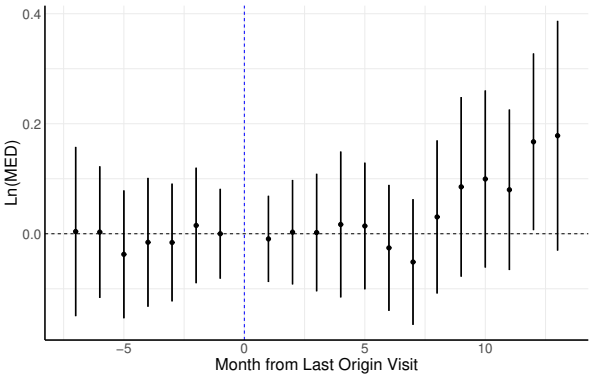
Figure A.15: Change in Patient's Ln(MED) Relative to Last Visit with Origin Provider, Scaled by $\Delta_{ln(MED)}$



(a) Pooled



(b) $\Delta_{ln(MED)} < 0$



(c) $\Delta_{ln(MED)} > 0$

This figure plots the θ_qs from Equation 4, where relative time is defined with respect to the patient's last visit with their origin provider. Only beneficiaries whose last visit with their origin provider occurred at the earliest six months prior to the exit are included in the treated cohort (**XX** beneficiaries).

Appendix B Addiction

IN PROGRESS

Appendix C Definitions

IN PROGRESS

Appendix D Decomposition

The decomposition given in Equation 2 is analogous to that of Finkelstein et al. (2016), in which the authors decompose observed intensity of patient care in a given geographic place j, $\bar{y}_j$ (analogous to ρ_p), into an optimal (expected) patient-level intensity for that area, $\bar{y}_j^*$ (which doesn't have an analogy); and a physician-specific effect, γ_j (analogous to α_p), that includes a physician's preference for delivering care of a particular intensity. Note that only $\bar{y}_j$ is observable (analogously, that only ρ_p is observable). The authors then illustrate that differences in observed place intensity between the patient's origin and destination place (analogously, observed prescribing intensity between the origin and destination provider) can be decomposed into the sum of the differences of place and patient components (analogously, the true effects of the provider, α_p, and the idiosyncratic term η_p, respectively):

$$\bar{y}_j - \bar{y}_{j'} = (\gamma_j - \gamma_{j'}) + (\bar{y}_j^* - \bar{y}_{j'}^*)$$

The authors use this definition to define the share of the difference between geographic areas j and j' that can be attributable to the place as:

$$S_{place}(j, j') = \frac{\gamma_j - \gamma_{j'}}{\bar{y}_j - \bar{y}_{j'}}$$

with an analogous definition for the patient-specific share. Note that we can derive a similar definition from Equation 2 in a difference-in-differences model reflecting a two-period setting for a patient whose origin provider o_p exits, and who is treated by destination provider d_p in the post-exit period:

$$\begin{aligned}
\alpha_o &= \theta\rho_o + \eta_o \\
\alpha_d &= \theta\rho_d + \eta_d \\
\Delta_\alpha &= \theta\Delta_\rho + \Delta_\eta
\end{aligned}$$

In expectation (and assuming $E[\Delta_\eta] = 0$):

$$\begin{aligned}
E[\Delta_\alpha] &= \theta E[\Delta_\rho] \\
\theta &= E[\frac{\Delta_\alpha}{\Delta_\rho}]
\end{aligned}$$

which is analogous to S_{place} in Finkelstein et al. (2016). Thus, another interpretation of θ is the share of the difference in prescribing intensity between origin provider o_p and destination provider d_p that is attributable to the provider (as opposed to the patient). Examples of provider-specific components include beliefs regarding the appropriateness of opioid use for some patients. Examples of patient-specific components include underlying patient pain and opioid addiction.