The Effects of Competition on Physician Prescribing*

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Abstract

This study investigates how competition influences the prescribing practices of physicians. U.S. state law changes granting nurse practitioners (NPs) the ability to prescribe controlled substances without physician oversight generate exogenous variation in competition. In response, we find that general practice physicians (GPs) prescribe significantly more opioids and controlled anti-anxiety medications. GPs also increase their co-prescribing of opioids and benzodiazepines, a practice that is against prescribing guidelines. These effects are more pronounced in areas with more NPs per GP at baseline, are concentrated in physician specialties that compete most directly with NPs, are not observed for many non-controlled drug classes, and lead to sizable increases in fatal drug overdoses. Our findings are consistent with a simple model of physician behavior in which competition for patients leads physicians to move toward the preferences of marginal patients. These results demonstrate that more competition will not always lead to improvements in patient care and can instead lead to excessive service provision.

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I Introduction

Provider competition is a salient feature of health care markets around the world and is often encouraged by policy makers.[1] While policies that foster competition are frequently implemented with the aim of promoting high-quality, cost-efficient care, increased competition need not improve welfare given the many imperfections in health care markets (Gaynor et al., 2015). Increased competition could, for example, lead other providers to exert their market power or increase demand inducement, thereby increasing the provision of costly or inappropriate care (McGuire, 2000).

Most empirical research into the effects of competition in health care has focused on large players, such as insurers and hospitals. There has been relatively little investigation of competition at the level of individual physicians, even though a number of recent policies affect the competitive landscapes facing physicians and physicians ultimately make most decisions about patient care. This lack of research may be due in part to constraints on the availability of physician-level data and the endogeneity of provider concentration, both in the cross-section and in the time-series, which have made empirical analyses of the effects of competition on physician behavior difficult.

This paper asks how the prescribing practices of physicians in general practice (GPs) change following sharp increases in competition being experienced in many U.S. markets.[2] Between 2006 and 2018, nearly one-third of states changed their scope-of-practice laws to allow nurse practitioners (NPs) to independently prescribe controlled substances, an authority that was previously only granted to physicians.[3] Controlled (or "scheduled") drugs are federally regulated in the United States under the Controlled Substances Act because they

[1] While competition is a hallmark of the U.S. health care system, competition is also pronounced in German, Dutch, and French health care markets (Siciliani et al., 2022). Moreover, other European countries—including England, Italy, and Norway—have taken steps in recent years to increase competition. Propper (2018) notes that much of the work on policies intended to increase competition focuses on England, whereas reforms in other countries have received less scrutiny.

[2] We consider doctors in family, general, and internal medicine to be GPs; all of our results are robust to including only physicians in family or general practice.

[3] An NP is a nurse who has obtained at least a master's degree in nursing and who has completed local licensure and national certification requirements. States have the authority to define what NPs are allowed to do and frequently update associated legislation, leading to wide variation in scope of practice for NPs both across states and within states over time.

are generally addictive and carry a risk of fatal overdose.[4] We analyze comprehensive data from IQVIA covering the prescriptions written by individual providers across the United States and find that GPs begin to prescribe more opioids and scheduled anti-anxiety medications when they are subject to increased competition from NPs. GPs also increase their co-prescribing of opioids and benzodiazepines to the same patient on the same day, a behavior that the Centers for Disease Control and Prevention (CDC) prescribing guidelines indicate that clinicians should avoid "whenever possible" (CDC, 2016). Moreover, there is suggestive evidence that the increases in prescribing are accompanied by increases in fatal drug overdoses involving prescription opioids and benzodiazepines. Taken together, the findings are reminiscent of the "medical arms race" literature in that they suggest that more competition will not always lead to improvements in patient care and can instead lead to excessive service provision.

Our findings are consistent with a theoretical model of physician behavior in which competition leads providers to shift toward the preferences of marginal patients. When patient and provider preferences are in opposition, providers set the level of service provision to balance the responsiveness of the number of patients that they attract against their utility per patient seen. As the market becomes more competitive, the number of patients that the provider attracts becomes more responsive to the level of service provision. In response, providers cater more to patient demand, thereby sacrificing the utility that they receive per patient to avoid losing too many patients. Whether competition increases or decreases service provision therefore depends on whether patients want more or less of the service in question. As clinicians frequently report that patient satisfaction suffers when they refuse to prescribe controlled substances—with patients often desiring fast and easy relief from their symptoms even when the risks of the medication might not outweigh the benefits (Frantsve and Kerns, 2007; Zgierska et al., 2012; NYTimes, 2016)—additional competition should lead physicians to write more controlled substance prescriptions.

Three additional sets of analyses leverage variation in competitive pressures induced by the law changes and support the hypothesis that our findings are driven by increased com-

[4] The United States has relatively lax oversight of opioids, allowing any doctor or dentist to prescribe strong opioids with few restrictions. In contrast, other countries enforce stricter rules, such as special training for doctors, dose limits, patient registration, or pre-authorization requirements (Ho, 2019).

petition. First, the observed increases in physician prescribing are higher in areas with a greater number of NPs per GP at baseline. That is, GPs respond more in areas in which they are subject to greater competition from NPs when NPs are allowed to prescribe independently. Second, changes in prescribing are concentrated among physicians practicing in the specialties that compete most directly with NPs rather than in specialties that face little competitive pressure from NPs. Finally, using data on the prescribing of a number of unscheduled drugs from both IQVIA and public use Medicare Part D prescription files, we find little effect on the prescribing of drug classes that are not directly affected by the law changes.

We also show that our findings are not driven by other changes in medical practices that might occur as a result of law changes allowing NPs to independently prescribe controlled substances. Using data from public use Medicare Part B files covering outpatient visits, we show that allowing NPs to independently prescribe controlled substances leads to a reduction in the number of office visits per GP. Combined with null effects on the prescribing of non-controlled substances, these findings suggest that GPs are neither seeing more patients nor spending more time with each patient as a result of the law changes.[5] Moreover, we run balancing regressions to examine whether the law changes are associated with changes in the types of patients seen by different provider types. We find no consistent effects of the law changes on the patient gender, age, and insurance type profiles of prescriptions written by GPs in the IQVIA data or on average risk scores of patients seen by GPs in the Medicare Part B files. Hence, our results are unlikely to be driven by changes in the types of patients seen by GPs following the law changes. We also find similar changes in prescribing within individual physicians.

The results on opioid prescribing are particularly important considering the ongoing opioid crisis in the United States. While not yet at U.S. levels, deaths due to opioids have also been increasing in other counties including Australia, Canada, Sweden, and the United Kingdom (Ho, 2019). To shed additional light on how competition affects opioid prescribing,

[5]If physicians were spending more time with patients following the law changes, then they might identify additional conditions that warrant medication. However, there is little reason to believe that additional time would lead only to the discovery of conditions that require treatment with controlled substances. Using snapshots of the exact practice addresses of providers in two years, we further show that the law changes do not affect the share of GPs practicing in clinics with NPs or the number of NPs per GP practice.

we conduct two additional analyses. First, focusing on patients who did not receive an opioid prescription in the past six months, we find that competition-induced increases in opioid prescribing are driven by prescriptions to "opioid-naïve" patients. Moreover, examining changes in average morphine milligram equivalents (MMEs) per prescription shows that competition leads GPs to write prescriptions with higher dosages, both for patients who are and are not opioid naïve. These results highlight the important role played by physicians in initiating opioid use and contribute to work documenting that the opioid crisis is driven in large part by supply-side factors (Currie and Schwandt, 2021).

Our paper relates to four branches of literature. First, many studies examine the effects of competition among insurers and hospitals.[6] Seminal work by Dafny (2010) and Dafny et al. (2012) documents high levels of concentration in markets for health insurance and finds that insurers charge higher premiums in more-concentrated insurance markets. However, Ho and Lee (2017) and Barrette et al. (2020) highlight that hospitals also have market power; thus, increased concentration in insurance markets could enable insurers to negotiate lower prices from hospitals, possibly increasing consumer welfare. Although studies focused on care provision often find that competition between hospitals leads to improvements in patient outcomes, this is not always the case (Gowrisankaran and Town, 2003; Propper et al., 2008; Gaynor et al., 2013; Bloom et al., 2015; Kunz et al., 2020). Moreover, depending on the market conditions, competition between hospitals can trigger a medical arms race in which more costly and unnecessary care is supplied (Kessler and McClellan, 2000).[7] We complement this work by showing that increased competition among clinicians can likewise have perverse effects, leading to increases in prescribing that are likely to be welfare reducing.

Second, this paper adds to a smaller literature examining the effects of competition among physicians and its impact on physician-induced demand.[8] Given limited variation in

[6] Recent work on retail pharmacies by Janssen and Zhang (2023) shows that competitive pressures can help explain why independent pharmacies are more likely to dispense prescription opioids—both for legitimate and non-medical uses—than chain pharmacies.

[7] A related literature examines the impacts of hospital mergers on prices, quality, and patient outcomes (e.g., Dafny (2009); Gaynor et al. (2015); Gowrisankaran et al. (2015)).

[8] See McGuire (2000) for an overview of the literature on physician-induced demand. Early work by Fuchs (1978) and Cromwell and Mitchell (1986) shows that rates of surgery are higher in locations with more surgeons, a finding that the authors attribute to demand inducement. However, follow-up work by Dranove and Wehner (1994) shows that similar findings also hold for obstetricians and childbirth, a service for which induced demand is likely minimal. These findings highlight the difficulties with designs that

concentration within markets over time, many investigations of competition at the physician level have been cross-sectional (e.g., Dunn and Shapiro, 2014, 2018; Scott et al., 2022). Looking within locations, Brekke et al. (2019) show that Norwegian physicians are more likely to certify sick leave for patients when they are practicing in institutions with stronger incentives to attract patients, whereas Gravelle et al. (2019) show that increases in the number of GPs in local areas in England lead to increases in patient satisfaction and small improvements in some measures of clinical quality. Focusing on prescribing practices, a number of papers have shown that prescription levels are positively associated with the concentration of providers in various countries, including Norway (Kann et al., 2010; Zykova, 2020), Belgium (Shaumans, 2015), and Taiwan (Bennett et al., 2015). We contribute to this literature by using a novel shock to competition to overcome endogeneity concerns and document how increased competition can lead to a deterioration in clinical quality when patients desire services that can harm them from a medical perspective.

Third, this paper relates to the large literature examining factors that drive physician decision-making. Studies have documented pronounced heterogeneity in the intensity of health care provision across locations (e.g., Fisher et al., 2003; Finkelstein et al., 2016) and individual providers (e.g., Parys, 2016; Currie et al., 2016; Gowrisankaran et al., 2023; Currie and Zhang, 2023; Ginja et al., 2024).[9] These findings have motivated work aimed at identifying factors that can explain such differences, including investigations into the roles played by financial incentives (Clemens and Gottlieb, 2014; Alexander and Schnell, 2024), physician skill (Currie and MacLeod, 2017, 2020; Chan et al., 2022), and provider beliefs (Cutler et al., 2019). Particularly relevant for our study, recent work focusing on supply-side drivers of the opioid crisis has examined how opioid prescribing is affected by training (Schnell and Currie, 2018; Zhang, 2023), beliefs about risks (Doctor et al., 2018), pharmaceutical marketing (Alpert et al., 2022; Arteaga and Barone, 2022), and provider altruism (Schnell, 2017). We add to this literature by considering a novel driver of variation in physician behavior—exposure to competition—and show that the competitive landscape

rely predominately on cross-sectional variation in provider supply. Another approach is to conduct a lab experiment, as in Brosig-Koch et al. (2017).

[9]Recent work by Chan and Chen (2022) documents wide variation in productivity among physicians and NPs in the emergency department. Our paper complements this work by showing that competition between these two classes of professionals can alter physician practice styles.

affects physicians' prescribing of controlled substances.

Finally, our paper relates to a growing literature on the impacts of changes in scope-of-practice legislation for NPs on patient care. As outlined in a recent overview by McMichael and Markowitz (2023), much of this literature has focused on the impacts of expanded scope of practice on patient access and health using either aggregate or patient-level data.[10] For example, Traczynski and Udalova (2018) document that allowing NPs to both practice and prescribe independently leads to increases in utilization of primary care services, while Alexander and Schnell (2019) show that allowing NPs to independently prescribe unscheduled drugs (including most antidepressants) leads to improvements in mental health. We therefore add to this prior work by examining how changes in competition induced by changes in scope-of-practice legislation for NPs affect the behavior of physicians.

The rest of the paper proceeds as follows. Section II provides a theoretical framework that highlights how increased competition can lead physicians to increase unnecessary, and potentially harmful, service provision. Section III provides an overview of the data. Section IV introduces our methods and presents our main empirical findings. The role that competition versus alternative mechanisms play in driving our results is considered in Section V, and Section VI provides a discussion and concludes.

II Theoretical framework

This section offers a theoretical framework that outlines how competition can influence the intensity of services provided by physicians. The framework highlights the idea that the effects of competition will depend on the type of service being rendered. In particular, the model predicts that increased competition should put *downward pressure* on the provision of services like C-sections that physicians might like to do more of (e.g., because they are time efficient and highly remunerated) but that marginal patients may not want (e.g., because they are unnecessary and cause complications). In contrast, increased competition should put *upward pressure* on the provision of services like prescription opioids that some marginal

[10] In a law review article, McMichael (2020) argues that law changes granting NPs full practice authority reduced opioid prescribing by physicians over the period 2011–2018. As outlined in Appendix D, there are a number of differences between our analysis and his that fully account for the differences in findings.

patients want (e.g., because of addiction, resale value, or the possibility of immediate pain relief) but physicians may not want to provide more of (because their utility of prescribing to marginal patients is negative).[11] In both cases, physician behavior shifts toward the preferences of the marginal patient when competition increases.[12] Whether increased competition leads to more or less service provision therefore depends on whether physicians are over- or underproviding care from the perspective of the marginal patient at baseline.

In order to capture these ideas, let x denote the intensity of service provision. This x can either be thought of as an extensive margin measure of the share of patients receiving a given service (e.g., the share of patients receiving an opioid or anti-anxiety prescription) or an intensive margin measure that further captures the intensity of treatment conditional on its provision (e.g., average daily MME per opioid prescription).[13] For a given intensity of service provision, the physician sees $N(x)$ patients and receives utility $u(x)$ per patient. $N(x)$ captures patient preferences and will be increasing (decreasing) in x if patients find additional x beneficial (harmful). Analogously, $u(x)$ captures the physician's preferences and financial incentives regarding treatment for a given patient and will be increasing (decreasing) in x if physicians believe additional x to be beneficial (harmful) to their own utility.[14] For simplicity, we assume that $N_{xx} = u_{xx} = 0$.

The physician chooses her optimal level of service intensity to maximize her total utility.

[11] Note that this does not imply that physicians are necessarily altruistic and trying to protect patients from the dangers of addictive medications. As outlined in Schnell (2017), a physician's optimal prescription decision can be modeled as a threshold rule in which the provider chooses a level of patient pain above which they prescribe. This threshold is set such that the physician's marginal utility of prescribing to the threshold patient is zero. If a provider cares both about their impact on patient health and their revenue, this is the point at which the harm caused by the medication just offsets the monetary reimbursement that the provider receives per office visit. In this context, the provider (1) harms their threshold patient from a medical perspective (i.e., they overprescribe) but (2) does not want to prescribe more at the margin (i.e., they do not want to reduce their threshold). Nevertheless, some marginal patients—for example, those with low pain but high tastes for opioids—will want additional prescriptions.

[12] This logic is consistent with evidence showing that increased competition leads to improvements in patient satisfaction (Gravelle et al., 2019).

[13] If all patients are identical, x as an extensive margin measure represents the fraction of these identical patients who receive a given service. If patients differ and are ordered by their appropriateness for the treatment, then a higher value of x indicates that additional patients for whom the treatment is less appropriate receive the service in question.

[14] For our purposes it is not necessary to specify a precise functional form for $u(x)$, but it is typically presumed that a physician derives utility both from the impact their service provision has on patient health and from their revenue (McGuire, 2000).

The physician's problem can therefore be written as:

$$\max_{x} \quad N(x) \cdot u(x).$$

Taking the derivative with respect to x and setting it equal to zero yields the following first-order condition:

$$N_x \cdot u(x^*) + N(x^*) \cdot u_x = 0$$
$$\Rightarrow \frac{N_x}{N(x^*)} = -\frac{u_x}{u(x^*)}. \tag{1}$$

This first-order condition shows that the physician decides on the intensity of service provision by balancing the elasticities with respect to service intensity of the number of patients that she attracts and the utility that she receives per patient.

There are four cases to consider. If both patients and physicians benefit from additional service intensity (i.e., if $N_x > 0$ and $u_x > 0$), then there is no trade-off between per patient utility and the number of patients seen, and the physician sets x^* at the highest possible level. Analogously, the physician sets x^* at the lowest possible level if both patients and physicians are harmed by additional service delivery (i.e., if $N_x < 0$ and $u_x < 0$). The interesting cases therefore occur when the incentives of patients and physicians are misaligned. This will occur whenever: (1) physicians receive higher per-patient utility by increasing service intensity, but additional service intensity loses them patients (i.e., if $N_x < 0$ and $u_x > 0$), or (2) patients desire additional service intensity that physicians do not want to provide for a given patient (i.e., if $N_x > 0$ and $u_x < 0$).

How will increasing competition affect the optimal intensity of service provision chosen by the physician? As the market becomes more competitive, each patient's decision about which provider to see becomes more sensitive to the level of service intensity because the patient has more outside options. In turn, N becomes more sensitive to the intensity of service provision, and thus $\mid N_x \mid$ is increasing in competition. Since an increase in $\mid N_x \mid$ increases the magnitude of the left-hand side of equation (1), either $N(x^*)$ must increase or $u(x^*)$ must decrease for the first-order condition to stay in balance.[15] That is, when there is

[15] In addition to changing the slope of the demand curve, increasing competition will lead the physician-specific demand curve to shift inwards. This shift serves to amplify the effects of the rotation by requiring

a tension between the preferences of patients and physicians, competition leads the physician to sacrifice per-patient utility to try to maintain the number of patients.

Suppose first that $N_x < 0$ and $u_x > 0$. In this case, an increase in competition leads to a reduction in x^*. That is, for services that marginal patients do not want (e.g., because the costs outweigh the potential benefits), but that physicians would like to do more of (e.g., because they are highly remunerated), increased competition should reduce the intensity of service provision. We can therefore use this model to explain the results in Markowitz et al. (2017), who find that C-section rates decreased when scope-of-practice laws for certified nurse-midwives were relaxed, thereby increasing competition facing obstetricians.

Now suppose that $N_x > 0$ and $u_x < 0$. In this case, an increase in competition should instead lead to an increase in x^*. That is, for services that providers do not want to provide more of (e.g., because they are unnecessary or harming marginal patients), but that some marginal patients want (e.g., because of desired pain relief, addiction, or non-health benefits like resale value), increased competition should increase the intensity of service provision. As long as some patients want medications that they are not currently prescribed (or larger prescriptions than they are currently prescribed), this logic will likely govern the impacts of competition on the intensity of services like opioid and benzodiazepine prescribing (Frantsve and Kerns, 2007; Zgierska et al., 2012).[16] Moreover, as patients likely want clinicians to certify their sick leave, even if it is against the clinician's better judgement, this case can be used to explain Brekke et al. (2019)'s findings that competitive pressures increase the issuance of sickness certificates.

Alternative models of physician behavior can also be used to micro-found the finding that increased competition leads to increases in the prescribing of certain medications. For example, as shown in Appendix B, a model of demand inducement can likewise deliver this result (Gruber and Owings, 1996; McGuire, 2000). In a demand-inducement framework, the effect operates through an income effect: When competition increases, physicians lose patients,

physicians to further adjust x to account for the reduction in $N(x)$ at the previous optimum.

[16]The presence of secondary markets for many addictive and abusable medications suggests that there is no shortage of such patients. In the U.S. context, an additional reason for patients to demand controlled substances is that they may be influenced by direct-to-consumer advertising (Ventola, 2011). The United States is one of only three countries that allow such advertising, although some states place restrictions on the advertising of controlled substances.

thereby reducing their income. Given diminishing marginal utility of income, physician utility is more responsive to changes in income at lower levels of income, and thus inducing demand—which is assumed to have a constant marginal cost—is now more appealing. This mechanism will lead to an increase in the intensity of service provision, like unnecessary opioid and benzodiazepine prescribing, that physicians might find more profitable than alternative treatment options.[17] Perverse effects of competition on physician behavior are therefore consistent with a range of theoretical underpinnings, although the fact that we find only minimal impacts of the law changes on the number of patients that physicians see suggests that demand inducement may not be the main mechanism.

III Data

We use two main data sources to examine how changes in competition affect the prescribing practices of physicians. As outlined below, our primary provider-level data on prescriptions come from the IQVIA LRx database, and information on state-level changes in scope-of-practice legislation for NPs come from McMichael and Markowitz (2023). We supplement these data with information from three additional sources. First, to construct measures of prescriptions and providers per capita, we use population counts at the county-year level from the five-year American Community Surveys (ACS).[18] Second, provider-level data from the annual public use Medicare Part B and D files are used to examine impacts on the number of office visits and on the prescribing of additional drug classes, respectively. Finally, information from the National Vital Statistics System (NVSS) is used to investigate whether competition-induced changes in prescribing affect fatal drug overdoses at the county-year level.

[17] Prescribing opioids or other addictive medications may be more profitable than alternative treatment options for four reasons. First, patients who are prescribed controlled substances may need to return for ongoing medical management and monitoring (CDC, 2016), leading to more billing for office visits. Second, even absent the need for medication management, patients may be more likely to return to clinicians who they deem to be more accommodating to their demands (Frantsve and Kerns, 2007). Relatedly, pay is directly linked to patient satisfaction in some cases, which in turn might be linked to successful management of pain or anxiety (Van Zee, 2009; Zgierska et al., 2012). Finally, physicians who are managing medications may be able to bill a higher amount—for example, by billing CPT code 99214 instead of 99213—because managing the patient is more complex and involves a higher level of medical decision-making (AMA, 2023).

[18] The data for 2007–2018 are available here: https://www.socialexplorer.com/explore-tables. We use a linear extrapolation to impute population for 2006.

III.A Prescription data

The primary prescription data that we use come from IQVIA, a public company specializing in pharmaceutical market intelligence. These data include detailed information on most opioid, anti-anxiety, and antidepressant prescriptions written in the United States from 2006 to 2018.[19]

Three features of these data are important for our analyses. First, the data contain a provider identifier and information on each provider from the American Medical Association (AMA). We use the provider identifiers to track prescriptions written by a given provider over time, which allows us to consider outcomes such as the number of new prescribers and the number of prescriptions per prescribing provider. We further use information on each provider's specialty to examine impacts on the prescribing of NPs as well as GPs and other physician types that are differentially exposed to competition from NPs.

Second, the data have an (anonymized) patient identifier and basic patient information such as location and age. The patient identifiers allow us to track the prescriptions for a given patient over time. This in turn allows us to identify patients who are starting new medications ("naïve" patients) and to measure instances of co-prescribing of medications to the same patient. Moreover, as outlined in Appendix C, prescription-specific information on each patient's zip code is used to construct a provider-year–level panel of practice locations over our sample period.[20] Information on patient characteristics such as age, gender, and

[19]IQVIA directly surveys most retail pharmacies, long-term care homes, and mail-order drug suppliers and then uses a patented projection methodology to impute any remaining prescriptions to match industry totals. While IQVIA therefore tracks most retail prescribing in the United States, the LRx data contain the subset of these prescriptions that are written for patients who can be tracked over time. We estimate that the LRx data cover over 75 percent of U.S. retail prescriptions over our sample period for the drug classes that we use, with nearly 90 percent coverage by 2018. The IQVIA data are available for purchase by qualified researchers; for further information, contact Allen.Campbell@iqvia.com.

[20]The IQVIA data include snapshots of provider practice addresses in 2014 and 2018, whereas we aim to know provider locations in each year from 2006 to 2018. As outlined in Appendix C, we use information on the zip codes of patients who fill the prescriptions written by each provider in each year to assign providers to their likely county of practice annually. This location-assignment algorithm identifies the same county (state) in 2018 as IQVIA for 66.6 (89.7) percent of providers and 76.4 (94.8) percent of prescriptions; statistics are similar when comparing our inferred locations to those in IQVIA's 2014 snapshot. We further compare our constructed location panel to locations provided in the AMA Masterfile, the National Plan and Provider Enumeration System, and the Centers for Medicare and Medicaid Services' "Physician Compare" database in Appendix C. These comparisons highlight a number of problems with these alternative data sources—including outdated location information and poor provider coverage—that motivate our use of a data-driven location assignment algorithm.

insurance type is further used to examine the effects of the law changes on the composition of patients that receive prescriptions.

Finally, the data have detailed information on the prescription being dispensed, including the National Drug Code (NDC) of the product, the strength of the medication, and the number of pills. We use the Food and Drug Administration's (FDA's) NDC data to determine which products are controlled substances.[21] Information on the size and strength of prescriptions is used to examine intensive margin measures such as average daily MME per opioid prescription.

Because the law changes that we consider only concern the ability of NPs to independently prescribe controlled substances, we expect the law changes to have the largest impacts on the prescribing of controlled substances. Hence, our primary analyses focus on the prescribing of opioids and scheduled anti-anxiety medications like benzodiazepines.[22] We also consider instances in which the same patient receives both an opioid prescription and a benzodiazepine prescription from the same provider on the same day ("co-prescribing"), a practice that the CDC recommends against because it leads to a heightened risk of respiratory failure (CDC, 2016). To consider impacts on the prescribing of drugs that were not directly affected by the law changes, we further examine the prescribing of two types of unscheduled medications that are available in our extract of the IQVIA data (non-controlled anti-anxiety medications and antidepressants) as well as additional unscheduled medication classes that are available in the public use Medicare Part D data (see Section III.C) in supplementary analyses.[23]

Table 1 provides an overview of the number of unique providers (column (1)) and the total number of prescriptions across controlled drug types (columns (2)–(4)) observed in our data. These statistics are provided over the entire sample period (panel (a)) and separately for the first and last year of the sample (panels (b) and (c), respectively). The over 1.5

[21] The FDA's NDC data is available through the NBER at https://data.nber.org/data/national-drug-code-data-ndc.html.

[22] IQVIA separates opioids into those used primarily for pain relief and those used predominantly to treat opioid use disorder. We have access to information on the prescribing of the first group (medications for pain relief); this class includes buprenorphine and methadone prescriptions in formulations that are used mainly for pain and are filled through retail pharmacies (rather than clinics). We show in Figure A6 that our results are not sensitive to dropping methadone and buprenorphine prescriptions from our data.

[23] All antidepressant medications except for chlordiazepoxide products are unscheduled. As chlordiazepoxide products account for less than 0.5 percent of all antidepressant prescriptions, we exclude them from the list of antidepressants and consider only the prescribing of non-controlled antidepressants.

million unique prescribers observed in the data wrote 2.06 billion opioid prescriptions and 750 million prescriptions for controlled anti-anxiety medications from 2006 to 2018. Controlled anti-anxiety medications such as benzodiazepines accounted for over 80 percent of all anti-anxiety prescribing over the sample period, and over 100 million benzodiazepine prescriptions were co-prescribed with an opioid prescription. Prescriptions for controlled anti-anxiety medications increased substantially from 2006 to 2018; in contrast, prescriptions for opioids increased nationally from 2006 to around 2010 and have since been trending downward.

Columns (2)–(4) of Table 1 further report the shares of each type of controlled substance prescription written by physicians in different specialties and by NPs. Across all drug types considered, GPs account for the most prescriptions of any specialty. This is both because there are many GPs and because they often rank near the top in terms of prescriptions per provider across specialties. Despite being unable to prescribe independently in many state-years over our sample period, NPs also account for a large share of total prescriptions. As shown in panels (a) and (c), respectively, NPs accounted for the third highest share of opioid prescriptions from 2006 to 2018 (behind GPs and orthopedic surgeons) and the second highest share in 2018 (behind only GPs). NPs also accounted for the third highest share of controlled anti-anxiety prescriptions over our sample period (behind GPs and psychiatrists/neurologists). This prominence is due in large part to the high number of NPs: as shown in column (1), the number of NPs observed prescribing these drug classes nearly quadrupled from 2006 to 2018, making them the second largest provider category (behind only GPs) by the end of the sample period.

III.B Scope-of-practice legislation

In Section IV, we exploit changes in scope-of-practice legislation regulating whether NPs can independently prescribe controlled substances as a shock to the competition facing GPs. These law changes come from McMichael and Markowitz (2023) and capture whether NPs could prescribe controlled substances without the supervision or collaboration of a physician in each year of the sample.[24] This legal change often removes the final barrier to NPs

[24] We use the years of the law changes from McMichael and Markowitz (2023) with two exceptions. First, although the relevant statute in Rhode Island was not formally updated until 2013, McMichael and

practicing fully without any required physician oversight.

As shown in Figure 1, 11 states allowed NPs to independently prescribe controlled substances as of 2005. Over our study period (2006–2018), 16 states relaxed their scope-of-practice restrictions and granted NPs the ability to prescribe these medications independently. The geographic distribution of these states is diverse, with four states in each of the four U.S. Census Regions granting NPs independent prescriptive authority for controlled substances over the period.

Before NPs are granted independent prescriptive for controlled substances, NPs can typically prescribe the medications with physician collaboration or oversight. As of 2005, 45 states allowed NPs to prescribe controlled substances *non*-independently, with the remaining six states updating their scope-of-practice legislation to allow NPs to prescribe controlled substances with physician supervision or collaboration between 2006 and 2018. Although law changes granting NPs the ability to prescribe controlled substances with physician oversight may offer additional variation in competitive landscapes, it is less obvious how physicians should respond to an expansion of the services that NPs can provide with their support. We therefore focus on law changes allowing NPs to independently prescribe controlled substances in our main analysis while controlling for changes in non-independent prescriptive authority over the sample period.

Table 2 provides an overview of prescribing patterns among GPs (panel (a)) and NPs (panel (b)) across states. We provide averages separately in the 11 states in which NPs had independent prescriptive authority for controlled substances since 2005 ("always takers"; columns (1)–(3)), the 24 states in which NPs could not independently prescribe controlled substances as of 2018 ("never takers"; columns (4)–(6)), and the 16 states that granted NPs independent prescriptive authority for controlled substances between 2006 and 2018 ("treatment states"; columns (7)–(9)). For controlled substance prescriptions of each type written by either GPs or NPs in each group of states, we consider the number of prescriptions

Markowitz (2023) note that "[r]egulations arguably granting full practice authority were promulgated in January/February 2012." We therefore use 2012 as the year of the law change for the state. Moreover, while McMichael and Markowitz (2023) categorize Nevada as having granted NPs independent prescriptive authority in 2013, they outline that NPs in the state cannot prescribe Schedule II drugs unless the provider has two years/2,000 hours of clinical experience or the medications are prescribed pursuant to a protocol approved by a collaborating physician. As we include Schedule II drugs in our analysis below, we do not consider NPs as having independent prescriptive authority for controlled substances in Nevada.

per 1,000 people, the number of prescribing providers per 1,000 people, and the average number of prescriptions per prescribing provider at the county-year level. As in Table 1, we provide statistics over the entire sample period and separately for the first and last year of the sample.

The number of prescriptions per 1,000 people written by NPs were generally higher in treatment states than in never-taker states over the sample period, with prescriptions by NPs being highest in the always-taker states. Similar patterns are observed for the number of prescribing NPs per 1,000 people and the average number of prescriptions per prescribing NP, with the highest rates generally observed in the always-taker states, the lowest rates observed in the never-taker states, and the rates in treatment states generally falling in-between. In contrast, prescriptions per 1,000 people written by GPs were generally higher in both never-taker and always-taker states than in treatment states over the sample period. Moreover, while the concentration of prescribing GPs was relatively similar in treatment and control states, the average number of prescriptions per prescribing GP was generally lowest in treatment states throughout the sample period. These observations suggest that simple cross-state comparisons between treatment and control states could be misleading.

An important question is whether changes in scope-of-practice legislation granting NPs the ability to prescribe controlled substances independently are correlated with other changes that might also influence prescribing patterns. To examine whether our identifying variation is orthogonal to changes in local socio-demographics such as the age, racial, and educational structure, we estimate balancing regressions that use these candidate controls as dependent variables (Pei et al., 2019).[25] Reassuringly, as shown in Figure A1, there is no evidence that changes in scope-of-practice legislation are correlated with changes in local socio-demographics. As such, we will find nearly identical results when we include or exclude socio-demographic controls in our analyses.

[25] In particular, we estimate analogues of equation (3) introduced in Section IV.B both with and without controls for county-specific linear time trends.

III.C Medicare data

Additional outcomes come from two data sets covering services provided to patients covered by Medicare, the public health insurance program in the United States that primarily provides coverage for the elderly. First, to examine impacts on additional drug classes that are not included in our extract of the IQVIA data, we use data on prescriptions paid for by Medicare Part D at the provider-year level. These data cover the period 2012–2018 and are made publicly available by the Centers for Medicare and Medicaid Services (CMS).[26] We consider prescriptions for eight drug classes in the Medicare Part D files: opioids, controlled anti-anxiety medications, non-controlled anti-anxiety medications, antidepressants, antihypertensives, cholesterol medications, antibiotics, and anti-diuretics. The first four drug classes are used to validate our findings in the IQVIA data using an alternative data source, whereas the last four drug classes are used to extend the analysis to the prescribing of additional types of non-controlled substances. We combine these data with information on the number of individuals aged 65 and older at the county-year level from the ACS to construct measures of prescriptions per capita among the Medicare population.

Second, we examine impacts on the frequency of office visits and the severity of patients seen by GPs in the Medicare Part B files. These data are also publicly available from CMS and again cover the period 2012–2018.[27] To measure effects on office visits, we use data on the number of new and existing patient evaluation and management services (CPT codes 99201–99205 and 99211–99215) paid for by Medicare Part B at the provider-year level. As with Part D prescriptions, we combine the number of office visits with relevant population counts from the ACS to construct office visits per capita among the Medicare population. Finally, to measure effects on patient severity, we use information on the average annual risk scores of beneficiaries seen by each provider in the Part B files. We consider both the unweighted

[26] CMS currently maintains the files from 2013 onward on their website here: https://data.cms.gov/provider-summary-by-type-of-service/medicare-part-d-prescribers/medicare-part-d-prescribers-by-provider-and-drug. Although historical files are periodically removed as new years are added, ProPublica maintains a version for 2012 here: https://www.propublica.org/datastore/dataset/medicare-part-d-prescribing-data-2012.

[27] As with the publicly available Part D data, CMS currently maintains the Part B files for 2013 onward on their website here: https://data.cms.gov/provider-summary-by-type-of-service/medicare-physician-other-practitioners/medicare-physician-other-practitioners-by-provider-and-service. ProPublica unfortunately does not maintain historical versions of the Part B files, although we had downloaded a version of the 2012 data from CMS before it was removed.

and the beneficiary-weighted average across GPs in a county to measure the average severity per GP and the average patient severity at the county-year level, respectively.

III.D Mortality data

Data on drug-related mortality come from the NVSS. The NVSS data that we use cover 2006–2018 and contain information on the date, location, and cause for all deaths in the United States. We follow previous work and define fatal drug overdoses as deaths with International Classification of Disease Version 10 (ICD-10) underlying cause of death codes X40–44, X60–X64, X85, and Y10–Y14. Multiple cause of death codes are used to identify fatal drug overdoses that involved any opioid (T40.0–T40.4 and T40.6), prescription opioids (T40.2 and T40.3), and benzodiazepines (T42.2). As with the prescription data, we combine mortality at the county-year level with population data from the ACS to measure fatal drug overdoses per capita.

IV Effects of law changes on prescribing practices

To examine the effects of competition on the prescribing practices of physicians, we leverage changes in scope-of-practice legislation granting NPs the ability to prescribe controlled substances independently as a shock to the competitive landscape. This section presents our main analyses examining effects on the prescribing of controlled substances by NPs and GPs. Section V then considers impacts on a number of supplementary outcomes—including prescribing among physicians in other specialties, non-controlled substance prescribing, co-practice patterns, the number of office visits, and patient composition—to examine mechanisms driving these main results.

IV.A Graphical evidence

Figure 2 provides an initial look at the impacts of competition by examining the relationship between the number of prescribers of controlled substances and prescribing patterns. In the figure, the number of NPs is set to zero until NPs are allowed to independently prescribe controlled substances. For each medication type, we consider the number of prescriptions per 1,000 people written by GPs and NPs (left subfigures) and the average number of prescriptions written by each prescribing GP (right subfigures) at the county-year level. These county-year observations are residualized from county and year fixed effects and grouped into deciles based on the number of GPs plus the number of NPs per 1,000 people.

The subfigures show a positive relationship between within-county changes in the number of prescribers per capita and the number of opioid prescriptions (panel (a)), controlled anti-anxiety prescriptions (panel (b)), and opioid and benzodiazepine co-prescriptions (panel (c)) per capita and per prescribing GP. While the positive association between the number of prescribers and prescriptions per capita may reflect the impact of better health care access when there are more providers, the positive association between the number of prescribers per capita and the average number of prescriptions written by each prescribing GP is notable. Holding demand fixed, each prescribing GP should need to write fewer—rather than more—prescriptions when there is a greater concentration of other providers available to prescribe. However, as other factors might correlate with changes in the concentration of providers over time, these figures do not directly investigate the role of competition in driving increases in prescribing.

To examine the impacts of law changes that shift the competitive landscape, we begin by estimating event-study specifications. In estimating these event studies, we focus on a balanced panel to ensure that a consistent sample of states is used to identify the event-time coefficients of interest. In particular, we consider law changes for which at least three years of prescription data are available before and after the event. Since the IQVIA data cover the period 2006–2018, this restriction leads us to consider the 11 law changes granting NPs the ability to independently prescribe controlled substances between 2009 and 2015. As discussed below, the results are robust to either including or excluding states with law changes between 2006–2008 and 2016–2018 from the set of control states.

Let Rx^{p}_{cst} denote a prescription outcome for providers of type p in county c of state s in year t. We consider county-year prescription outcomes among all providers and by NPs and GPs separately (i.e., $p \in \{all, \text{NPs}, \text{GPs}\}$). Letting t^*_s denote the year of the law change in state s, we estimate event-study specifications of the form:

$$\begin{aligned} Rx^p_{cst} \ = & \sum_{n\in\{(-4)+,-3,\ldots,3,4+\}} \alpha_n \cdot B_s \cdot 1\left\{t^*_s + n = t\right\} \\ & + \theta \cdot X_{st} + \delta \cdot X_{ct} + \gamma_c + \gamma_t + \gamma_c \cdot t + \epsilon_{cst}, \end{aligned} \tag{2}$$

where $1\left\{t^*_s + n = t\right\}$ is an indicator denoting whether year t for state s is n years from the law change; B_s is an indicator denoting whether state s is part of the balanced panel; X_{st} are time-varying, state-level controls for changes in independent prescriptive authority outside of the balanced panel window and changes in non-independent prescriptive authority for controlled substances; X_{ct} are the time-varying, county-level controls listed in Figure A1; γ_c and γ_t are county and year fixed effects, respectively; and $\gamma_c \cdot t$ are county-specific linear time trends.[28] The year before the law change ($n = -1$) is the omitted category, and standard errors are clustered by state. Because of the balanced panel restriction, the coefficients $[\alpha_{-3}, \alpha_3]$ are identified by a consistent sample of states.

We begin by considering the impacts of the law changes on the county-year number of controlled substance prescriptions written by providers of any type per 1,000 people. Results from estimation of equation (2) are presented in Figure 3. Panels (a) and (b) show that there were no significant differences in trends in opioid and controlled anti-anxiety prescribing per 1,000 people between treatment and control counties in the years before the law changes. However, the prescribing of opioids and controlled anti-anxiety medications jumped when NPs were granted the authority to independently prescribe controlled substances and steadily increased over the next three years. As shown in panel (c), co-prescribing of opioids and benzodiazepines per 1,000 people likewise increased when NPs were granted independent prescriptive authority. While there is some suggestion of a pre-trend for co-prescribing, there is nevertheless a clear jump in the year of the law change that persists for at least three years.

[28] We include county-specific trends in our primary specification for prescription outcomes, as event studies show pre-trends in prescribing among NPs in the absence of such controls (see Figure A2). While unit-specific time trends help account for differential pre-trends across locations, they over-control for time-varying treatment effects (Neumark et al., 2014; Goodman-Bacon, 2021). As discussed further below, our results are robust to including county-specific time trends that are predicted using only pre-period data and to including state-specific rather than county-specific linear time trends.

Figure 4 presents event studies that are analogous to those presented in Figure 3 except that they show the prescribing of controlled substances separately by NPs (left subfigures) and GPs (right subfigures).[29] The left subfigures show that prescribing by NPs per 1,000 people of opioids (panel (a)), controlled anti-anxiety medications (panel (b)), and co-prescribing of opioids and benzodiazepines (panel (c)) generally rose once they were granted the ability to prescribe these medications independently. These findings are not particularly surprising given that such increases were arguably the intent of the law changes. Strikingly, however, the right subfigures show that the prescribing of these medications by GPs also jumped when NPs were allowed to prescribe independently. If patients had merely switched from GPs to NPs following the law changes, prescribing among GPs should have fallen in tandem with the rise in NP prescribing. Hence, the simultaneous increases among NPs and GPs suggest a behavioral response on the part of GPs facing increased competition after the law changes.

IV.B Primary estimates

To summarize the effects in the years following the law changes, we estimate specifications that pool the post-period coefficients from equation (2):

$$\begin{aligned} Rx^{p}_{cst} &= \beta_1 \cdot B_s \cdot 1\{t - t^*_s \in [0,3]\} + \beta_2 \cdot B_s \cdot 1\{t - t^*_s \geq 4\} \\ &\quad + \theta \cdot X_{st} + \delta \cdot X_{ct} + \gamma_c + \gamma_t + \gamma_c \cdot t + \epsilon_{cst}, \end{aligned} \tag{3}$$

where $1\{t - t^*_s \in [0,3]\}$ is an indicator denoting the year of and the three years following the law change in state s (balanced post-period), $1\{t - t^*_s \geq 4\}$ is an indicator denoting years that are at least four years after the law change in state s, and all other variables are defined as in equation (2). Standard errors are again clustered by state. The coefficient of interest is β_1, which measures the average county-level change in a given prescription outcome in the three years following a change in state-level scope-of-practice laws granting NPs the ability to prescribe controlled substances independently. Because of the balanced

[29] Figure A2 presents event-study results for all providers and by provider type with and without trend controls. As shown in Figure A2(c), the inclusion of time trends has little effect on the estimates for GPs. However, the inclusion of county-specific linear time trends—estimated over the entire sample period or predicted using only pre-period data—helps correct for negative pre-trends in the outcomes among NPs (Figure A2(b)).

panel restriction, all treatment states used to identify β_1 are observed for the entirety of this three-year post-period.

Results from estimation of equation (3) are shown in Table 3. As in Figures 3 and 4, panel (a) considers the number of prescriptions per county-year written by all providers (columns (1)–(3)), NPs (columns (4)–(6)), and GPs (columns (7)–(9)) per 1,000 people for opioids, controlled anti-anxiety medications, and opioid and benzodiazepine co-prescribing. In line with Figure 3, panel (a) shows that there are positive effects of allowing NPs to independently prescribe controlled substances on the number of prescriptions. As shown in columns (1)–(3), allowing NPs to independently prescribe controlled substances leads the number of prescriptions per 1,000 people at the county-year level to increase by 38.3 for opioid prescriptions (7.6 percent relative to the baseline mean; p-value $= 0.053$), 10.4 for controlled anti-anxiety prescriptions (5.9 percent; p-value $= 0.046$), and 4.2 for co-prescriptions of opioids and benzodiazepines (15.7 percent; p-value < 0.001). It is notable that the effect on co-prescribing, an unambiguously dangerous practice, is so large.

Looking to the results by provider type, we see in columns (4)–(6) of Table 3 that the estimated effects on NP prescribing are positive, as expected. However, as shown in columns (7)-(9), the estimates for GPs are much larger in levels, and the impacts on all three prescription outcomes are statistically significant. Comparing the estimates for GPs to those for all providers indicates that more than half of the total increases in prescribing come from increases among GPs. The estimates in panel (a) show that granting NPs independent prescriptive authority for controlled substances leads the number of prescriptions written by GPs per 1,000 people at the county-year level to increase by 20.7 opioid prescriptions (8.9 percent relative to the GP-specific baseline mean; p-value $= 0.099$), 6.5 controlled anti-anxiety prescriptions (6.0 percent; p-value $= 0.057$), and 2.3 opioid and benzodiazepine co-prescriptions (14.4 percent; p-value $= 0.009$).

The increases in prescribing observed in panel (a) of Table 3 could come either from additional providers starting to prescribe a certain drug type (extensive margin adjustments) or from existing prescribers increasing their prescription levels (intensive margin adjustments). To shed light on these mechanisms, we examine effects on the number of providers of a given type (i.e., all, NPs, or GPs) who are observed prescribing a medication of a given type per

1,000 people at the county-year level as well as the average annual number of prescriptions per prescribing provider for each provider and drug type.

Perhaps surprisingly, the results in panel (b) of Table 3 show that the law changes do not draw new providers into prescribing controlled substances.[30] This is because many NPs were already prescribing controlled substances—albeit with physician supervision or collaboration—before the law changes (see Table 2). Rather, increases in prescribing come from increases in the number of prescriptions per prescribing provider (panel (c)).[31] Among prescribing GPs, allowing NPs to independently prescribe controlled substances leads to an average of 24.2 more opioid prescriptions (p-value = 0.046), 10.1 more controlled anti-anxiety prescriptions (p-value = 0.009), and 3.9 more opioid and benzodiazepine co-prescriptions (p-value = 0.008) per year. Compared to the respective baseline means of 281.6, 145.3, and 30.6, these estimates reflect increases of 8.6, 6.9, and 12.7 percent. While the point estimates are about half as large among prescribing NPs, the percent effects are even more pronounced given substantially lower baseline means among these providers.

IV.C Additional analyses

Opioid prescribing To probe how competition affects opioid prescribing in particular, we conduct two additional sets of analyses. First, a distinction is often made in the literature between opioid-naïve and non–opioid-naïve patients. If physicians respond to increased competition by writing opioid prescriptions for naïve patients, then competition could have important implications for the initiation of opioid use and possible future opioid abuse.

[30] We require providers to write at least one opioid prescription or controlled anti-anxiety prescription in each month of a given year to be considered a prescriber of the medication. In contrast, we only require providers to co-prescribe opioids and benzodiazepines at least once in a given year since co-prescribing is a relatively rare outcome. Although the law changes do not lead to an increase in the overall number of prescribers, they do lead to an increase in the number of prescribers for whom a given type of prescribing has become a relevant part of their clinical practice. Figure A3 shows impacts on the number of "frequent" prescribers, where a clinician's prescribing is considered frequent if they both (1) write a given type of prescription in each month (or year for co-prescribing of opioids and benzodiazepines) and (2) are above the xth percentile among all GPs who satisfy criterion (1). Across all prescription types, impacts on the number of "frequent" prescribers generally become more pronounced as higher thresholds are used.

[31] Event-study results for the number of prescribers per 1,000 people and the average annual number of prescriptions per prescriber are shown in Figure A4 and A5, respectively. All providers are included when calculating the average number of prescriptions per provider; results are even more pronounced when the samples in Figure A5(a) and (b) are limited to providers who wrote at least one opioid or controlled anti-anxiety prescription in each month of a given year, respectively.

To examine effects by patient type, we divide prescriptions based on whether they were written for a patient who did not have an opioid prescription from any provider in the past six months ("opioid naïve") and patients who did have a prescription ("non-opioid naïve"). Second, since larger opioid prescriptions carry additional risk of physical dependence and misuse (CDC, 2016), we examine effects on the average days supplied and the average daily MME per prescription. We also consider the number of opioid prescriptions with greater than 120 MME daily per 1,000 people given work documenting that prescriptions of this size are strongly correlated with adverse patient outcomes (Sullivan et al., 2010; Bohnert et al., 2011).

Results from these analyses are shown in Table 4 for NPs (columns (1)–(3)) and GPs (columns (4)–(6)). Panel (a) shows that the increases in opioid prescribing are mainly driven by prescriptions for opioid-naïve patients. Allowing NPs to prescribe controlled substances independently leads GPs to write 20.4 more opioid prescriptions for opioid-naïve patients per 1,000 people at the county-year level (10.2 percent relative to the baseline mean; p-value = 0.089) versus only 0.33 additional opioid prescriptions for non-naïve patients (1.0 percent; p-value = 0.685). This suggests that competition-induced increases in opioid prescribing put additional patients at risk of developing opioid use disorder.

The rest of Table 4 shows that the law changes do not affect the length of prescriptions, either for opioid-naïve or non-naïve patients (panel (b)). However, there are sizable increases in the average MME per day supplied for both opioid-naïve and non-naïve patients, with the increases being almost 50 percent larger for non-naïve patients (panel (c)). The number of prescriptions with over 120 MME per day written by GPs also increases among opioid-naïve patients (panel (d)). Given that the CDC recommends that providers start patients on the lowest effective dose and that they "avoid" or "carefully justify" increasing dosage to greater than 90 MME per day (CDC, 2016), this result is especially striking. As competition increases both the number of prescriptions for opioid-naïve patients and the strength of prescriptions for naïve and non-naïve patients, these results suggest that competitive landscapes are important components of both the addiction and availability channels of place-based factors identified by Finkelstein et al. (2022).

Mortality Finally, to ask how granting NPs the ability to independently prescribe controlled substances affects drug overdose deaths, we estimate analogues of equation (2) using the county-year number of fatal drug overdoses per million people as the outcome.[32] Figure 5 reports results for fatal drug overdoses involving prescription opioids (subfigure (a)), benzodiazepines (subfigure (b)), and prescriptions opioids in combination with benzodiazepines (subfigure (c)).

Deaths involving prescription opioids began to rise in treatment counties in the year after the law changes (Figure 5(a)). As shown in column (2) of Table A1, pooling the effects in years 1–3 shows that the law changes lead to 10.3 more prescription opioid fatalities per million people (21.8 percent relative to the baseline mean; p-value = 0.017). Moreover, the increase in all opioid mortality (column (1)) is accounted for by the rise in prescription opioid mortality, suggesting that any mortality effects are driven by changes in prescribing rather than general changes in population drug use. As shown in Figure 5(b) and (c), deaths involving benzodiazepines and the combination of prescription opioids and benzodiazepines may also have risen in the years following the law changes. While sizable, the effects on fatal drug overdoses involving benzodiazepines and opioids with benzodiazepines are less precise (p-values of 0.141 and 0.127, respectively; see Table A1).

These results provide evidence that increases in controlled substance prescribing induced by the law changes lead to increases in fatal overdoses involving prescription opioids and may also lead to increases in deaths involving benzodiazepines. Nevertheless, it is possible that the effects of increased prescribing are mitigated by increases in access to treatment for drug addiction. This would be consistent with results from Grecu and Spector (2019), who find that relaxing scope-of-practice laws increases access to treatment for opioid use disorders. It is also possible that new prescribing takes time to lead to drug abuse and increases in overdose deaths, and thus our difference-in-difference framework may be less well suited to examine impacts on mortality than on prescribing.

[32] We exclude county-specific linear time trends from these analyses because there is little evidence of differential pre-trends between treatment and control counties in specifications without trend controls.

IV.D Robustness

The results of several robustness checks are summarized in Figure A6, which shows that the results are remarkably consistent. Recall from Section IV.B that our primary specification for prescription outcomes includes county-specific linear time trends. We include these time trends because there is some evidence of differential pre-trends in prescribing by NPs between treatment and control counties in the absence of trend controls (see Figure A2). However, as there is no evidence of differential pre-trends among GPs, the results for GPs remain very similar regardless of whether or how time trends are taken into account. As shown in Figure A6, the estimates for GPs are very similar when we use state-specific rather than county-specific linear time trends. Moreover, the effects on all of the considered prescription outcomes are, if anything, more pronounced when county-specific time trends are only estimated using pre-period data (Goodman-Bacon, 2021).

Recent developments in the applied econometrics literature highlight the importance of correcting for potential biases in two-way fixed effects models with staggered treatment adoption and heterogeneous treatment effects. In addition to isolating effects for a balanced panel of states in all of our analyses, Figure A6 provides estimates using the estimator proposed by Sun and Abraham (2021). Results using this alternative estimator are very similar in magnitude but are more precise than our baseline findings, suggesting that our primary focus on results from a two-way fixed effects estimator without the proposed corrections is conservative. The figure further shows results excluding states with law changes over our sample period but outside of the balanced panel window and results using only "never-takers" as controls. The results are nearly identical when alternative sets of states are included in the control group.

Recall that our primary specification includes time-varying controls for socio-demographics and state-level changes in non-independent prescriptive authority for NPs over the sample period (see equation (3)). As there is little evidence that law changes granting NPs independent prescriptive authority for controlled substances are correlated with changes in local socio-demographics (Figure A1), the results are unsurprisingly unaffected by the exclusion of socio-demographic controls. Moreover, controlling for law changes allowing NPs to prescribe controlled substances with physician collaboration or supervision has no impact on the findings. The results are further unaffected by controlling for the state-level adoption of must-access prescription drug monitoring programs (PDMPs) and state-level Medicaid expansions as potential confounders.[33] In fact, Figure A7 shows that the results are very similar if one considers impacts on prescriptions paid for by payers other than Medicaid, further emphasizing that the findings are not driven by state-level Medicaid expansions or changes in socio-demographics that could affect Medicaid enrollment.

Figure A6 shows the results from two additional robustness exercises. The penultimate row in each subfigure considers results excluding methadone and buprenorphine from our definition of "opioids." The results are nearly identical when these medications are excluded, which demonstrates that our findings are not driven by changes in the provision of medications that can be used for the treatment of opioid use disorder rather than pain management. Lastly, the final row in each subfigure shows results from specifications that include provider fixed effects in a provider-level analogue of our primary specification. The results are nearly identical and even somewhat more precise in this alternative specification.

Finally, Figure A8 asks whether our results are driven by counties in a particular treatment state. In our baseline specifications, we weight county-year observations by population since there is likely more noise in the prescription outcomes of less populous counties. As such specifications are by construction more reflective of treatment effects in more populous counties, we begin by presenting results from estimation of equation (3) excluding population weights. Comparing the top two rows in each subfigure of Figure A8, we see that excluding population weights generally leads to larger standard errors, as expected. However, the point estimates for most outcomes are closely aligned whether observations are weighted by population, highlighting that our effects are not only reflective of impacts in large counties. The remaining rows in Figure A8 subsequently drop each treatment state one at a time from this unweighted specification. The findings are similar regardless of which state is excluded,

[33]Data on the state-level enactment dates of must-access PDMPs come from the PDMP Training and Technical Assistance Center (see here: https://www.pdmpassist.org/State) and information on state-level Medicaid expansions come from the Kaiser Family Foundation (see here: https://www.kff.org/affordable-care-act/state-indicator/state-activity-around-expanding-medicaid-under-the-affordable-care-act). Balancing regressions show that our identifying variation is orthogonal to state-level opioid legislation such as the adoption of must-access PDMPs as well as state-level Medicaid expansions. It is therefore unsurprising that our results are unaffected by the inclusion of such controls.

indicating that our results are not driven by counties in a single state.

V Mechanisms

We interpret the results in Section IV as being driven by changes in competition induced by changes in state-level scope-of-practice laws allowing NPs to independently prescribe controlled substances. We provide evidence in support of this interpretation below. In Section V.A, we examine the role of competition directly by examining the pattern of effects across areas, specialties, and drug classes that experienced differential changes in competitive pressures as a result of the law changes. In Section V.B, we turn to alternative mechanisms and examine whether other changes in physician practices that might have occurred as a result of the law changes can explain our findings.

V.A Role of competition

We conduct three sets of tests to probe whether it is indeed competition from NPs that is driving the increases in prescribing among GPs. First, we ask whether the effects are more pronounced in areas in which GPs face greater competition from NPs. In particular, counties are divided into two groups based on whether they had an above- or below-median number of NPs per GP among treatment states at the start of the sample period. We then estimate an augmented version of equation (3) that includes an interaction between the treatment indicator and an indicator denoting whether the county had an above-median number of NPs per GP in 2006. We anticipate that allowing NPs to independently prescribe controlled substances will have greater effects on the prescribing behaviors of GPs practicing in areas with a greater concentration of NPs at baseline.

Results from this analysis are presented in Table 5. The estimates bear out the hypothesis that GPs respond more strongly to the law changes in counties in which NPs are more of a competitive threat to GPs: the estimated effects for opioids (column (1)) and controlled anti-anxiety medications (column (2)) among GPs are 47.4 and 75.0 percent higher, respectively, in counties with an above- versus below-median number of NPs per GP in 2006. Moreover, all of the impacts in the above-median counties are strongly statistically significant, whereas the estimates for opioids and controlled anti-anxiety medications have p-values of 0.131 and 0.118, respectively, in the below-median counties.

Second, we ask whether the effects differ across physicians in different specialties. Since approximately 90 percent of NPs are certified in primary care, NPs are likely to compete most directly with GPs (AANP, 2022). However, NPs also practice in a range of specialties, with nearly 8 percent certified in acute care medicine, 5 percent certified in psychiatry/mental health, and 3 percent certified in women's health. We therefore consider the effects of allowing NPs to independently prescribe controlled substances on the prescribing behaviors of physicians in emergency medicine, psychiatry and neurology, and obstetrics and gynecology. We also consider the effects of the law changes on prescribing practices among two types of surgeons: orthopedic surgeons and general surgeons. While NPs do not provide surgeries, NPs with independent prescriptive authority can offer services such as pain management that are alternatives to some orthopedic surgeries (Blom et al., 2021), thereby competing indirectly with orthopedic surgeons. On the other hand, independent prescriptive authority for NPs should not substantively change the competitive landscape for general surgeons. Constructing the average number of opioid prescriptions, controlled anti-anxiety prescriptions, and co-prescriptions of opioids and benzodiazepines by physicians in each of these five additional specialties at the county-year level, we then estimate equation (3) separately for these physician types.[34]

Table 6 tests the hypothesis that physicians who face more direct competition from NPs will respond more strongly to the law changes. For reference, column (1) repeats the estimates for GPs from panel (c) of Table 3. As shown in columns (2)–(4) of panel (a), physicians in emergency medicine, psychiatry/neurology, and obstetrics/gynecology respond to increased competition from NPs by writing more opioid prescriptions. Physicians in psychiatry/neurology also increase their prescribing of controlled anti-anxiety prescriptions, while physicians in obstetrics/gynecology write more co-prescriptions for opioids and benzodiazepines (panel (c)). These findings are consistent with the fact that many NPs are

[34] As the number of physicians differs greatly across the considered specialties, we use the average number of prescriptions per prescribing provider as the outcome rather than the total number of prescriptions per capita for this analysis. This makes the results easier to compare across physician types, although the take-aways are very similar if we instead consider prescriptions by physician type per capita as the outcome.

certified in related specialties (AANP, 2022). However, given that more NPs are certified in primary care, the results for GPs are often more precise and generally larger—both in levels and relative to the group-specific baseline means—than in these other specialties.

The remainder of Table 6 focuses on surgeons. As shown in column (5), orthopedic surgeons do not significantly increase their prescribing when NPs are allowed to prescribe controlled substances independently. However, the results are marginally significant (e.g., the increase in controlled anti-anxiety prescribing has a p-value of 0.110), suggesting that orthopedic surgeons may adjust their prescribing in response to increased alternatives to their services from NPs. As predicted, there are no statistically significant effects for general surgeons (column (6)), a class of physicians who likely face little competitive pressure from NPs.

Finally, we ask whether the effects on prescribing are concentrated among controlled substances. While the prescribing of non-controlled substances like antibiotics might also be responsive to competitive pressures, the law changes that we consider most directly influence the competitive landscape for controlled substances. We therefore anticipate that the impacts of the law changes will be larger for controlled substance prescribing. To examine effects on the prescribing of non-controlled substances, we use both the IQVIA data and the public use Medicare Part D files. As outlined in Section III, we have information on the prescribing of non-controlled anti-anxiety medications and antidepressants from IQVIA and information on the prescribing of non-controlled anti-anxiety medications, antidepressants, antihypertensives, cholesterol medications, antibiotics, and anti-diuretics from the public use Medicare Part D files. Given the limited time frame available in the Medicare data (2012–2018), we consider the effects of the law changes from two years before to two years after among the balanced panel of five states that granted NPs the ability to independently prescribe controlled substances between 2014 and 2016 in these analyses.[35] To make the samples more comparable in the IQVIA and Medicare data, we further focus on prescriptions to those aged 65 and older in the IQVIA data in these analyses, although we verify

[35] The shorter sample window makes it difficult to estimate stable unit-specific time trends. We therefore exclude county-specific linear time trends when using data for 2012–2018 and focus on results for GPs, as these results were shown in Figures A2 and A6 to be insensitive to the inclusion of various time trend controls.

that the results for non-controlled substance prescribing in the IQVIA data are robust to using the same sample of years and patients as in our primary analysis.

We begin by confirming that we observe positive impacts of the law changes on controlled substance prescribing by GPs when focusing on prescriptions to the Medicare population and using the truncated sample period. In particular, Figure 6(a) provides event-study results from estimation of equation (2) using either the county-year number of opioid prescriptions (left subfigure) or controlled anti-anxiety prescriptions (right subfigure) written by GPs for those aged 65 and older in the IQVIA data (light dots and bars) and paid for by Medicare Part D in the public use Medicare files (dark dots and bars) per 1,000 people aged 65 and older from 2012 to 2018. While we unsurprisingly lose some precision when using these more limited samples, we nevertheless see clear evidence of increases in opioid and controlled anti-anxiety prescribing by GPs following the law changes. Moreover, while the point estimates are generally larger than those observed when considering prescriptions for all patients from 2006 to 2018 in Figure 4, the effect sizes relative to the respective baseline means are similar. For example, the effects on opioid prescribing among GPs shown in the left subfigure of Figure 6(a) reflect increases of 6–8 percent relative to the respective baseline means after two years, whereas we observed a 9 percent increase in opioid prescribing by GPs in our baseline specification in Table 3.

Figure 6(b) shows analogous results for the prescribing of non-controlled substances. To allow for a more direct comparison with our estimates for controlled substance prescribing in Figures 4 and 6(a), in which the y-axes extend to at least one-third of the baseline mean, we scale the y-axes in Figure 6(b) to range from -33 to +33 percent of the baseline mean of each outcome. As anticipated, the effects of the law changes on non-controlled substance prescribing are much less pronounced than the effects on controlled substance prescribing. While there is some evidence that the prescribing of non-controlled anti-anxiety medications may have gradually fallen following the law changes, which would be consistent with the replacement of some non-controlled anti-anxiety medications with controlled alternatives such as benzodiazepines, there are no measurable effects on most of the non-controlled medication classes considered.[36]

[36]Figure A9 replicates our primary analysis, in which we consider prescriptions for all patients in the

V.B Ruling out alternative mechanisms

We next examine whether other changes in physician practices that might occur as a result of law changes allowing NPs to independently prescribe controlled substances play a role in driving the findings. First, we ask whether the results can be explained by changes in physician workloads that might result from reductions in administrative or supervisory duties required of GPs. If GPs who were previously collaborating with or supervising NPs have additional time to devote to patient care once NPs can prescribe controlled substances independently, then an increase in controlled substance prescribing might reflect either an increase in the time spent with each patient (which might allow the provider to identify additional ailments requiring treatment) or an increase in the number of patients seen. The null results for non-controlled substance prescribing shown in Figure 6 already provide strong evidence against these possibilities: if GPs are spending more time with each patient or taking on additional patients following the law changes, then their prescribing of non-controlled substances should likewise increase.

To further examine whether GPs see additional patients following the law changes, we examine effects on the number of office visits using the public use Medicare Part B files. As with the public use Medicare Part D files, these data are available for 2012–2018. Given this shorter sample window, we again consider the effects of the law changes from two years before to two years after among the balanced panel of five states that granted NPs the ability to independently prescribe controlled substances from 2014 to 2016. Figure 7(a) presents event-study estimates from estimation of an analogue of equation (2) using two outcomes at the county-year level: (1) the number of Part B office visits with GPs per 1,000 people aged 65 and over and (2) the average number of Part B office visits per GP. In line with the findings for non-controlled substance prescribing, the left subfigure in Figure 7(a) shows that there is no evidence that the number of GP office visits per capita increased as a result of the law changes. As shown in the right subfigure, the law changes instead led to a reduction

IQVIA data from 2006 to 2018, for non-controlled anti-anxiety medications (left subfigures) and antidepressants (right subfigures). There is suggestive evidence that the prescribing of non-controlled anti-anxiety medications fell slightly, particularly among NPs (panel (b)). There is also suggestive evidence that the prescribing of antidepressants may have risen slightly after the law changes among GPs, although we do not observe an increase in antidepressant prescribing in the public use Medicare Part D data (Figure 6(b)). These results are thus less robust than our main findings.

of around four office visits per GP, a 1.9 percent reduction relative to the baseline mean of approximately 190 visits with Medicare beneficiaries annually.

A related concern is that the results could be driven by increases in physician workloads resulting from NPs leaving their joint practices. While such changes in workloads should also be reflected in non-controlled substance prescribing and in the number of office visits, we can ask whether NPs who were practicing with a physician leave the physician's office to practice elsewhere (e.g., open their own practice) when they can prescribe controlled substances independently. Recall that in our main analyses, we use information on patient zip codes to infer the practice counties of prescribing providers in each year of the sample (see Section III.A and Appendix C). To examine whether independent prescriptive authority affects co-practice patterns among GPs and NPs, we instead use the two snapshots of *exact* practice addresses in 2014 and 2018 provided by IQVIA. Calculating the share of GPs who had at least one NP practicing at their practice address and the average number of NPs per GP practice in each county in these two years, we compare how co-practice patterns changed from 2014 to 2018 in the eight states with law changes between 2015 and 2018 ("treatment"), the 19 states with law changes in or before 2014 ("always takers"), and the states that did not allow NPs to independently prescribe controlled substances by 2018 ("never takers").

As shown in Figure A10(a), around 62 percent of GPs were practicing at the same address as at least one NP in 2014. The share of GPs co-practicing with NPs increased by 2018 in all state groupings to an average of 67 percent across the United States. Although the growth in co-practicing over this period was slightly less pronounced in states that granted NPs the ability to prescribe controlled substances between 2015 and 2018, the number of NPs per GP practice was, if anything, higher in these treatment states. As shown in Figure A10(b), GPs in treatment states on average worked in practices with 13.3 NPs in 2018, an increase of 2.9 NPs from 2014. In contrast, GPs in never-taker and always-taker states on average worked in practices with 11.0 and 10.3 NPs in 2018, respectively, reflecting increases of 2.9 and 2.5 NPs from 2014. These findings provide additional evidence against the possibility that the observed increases in prescribing among GPs are driven by changes in workloads following the law changes.

We also ask whether the observed increases in controlled substance prescribing might be driven by changes in the types of patients seen by GPs. Even if there are no changes in aggregate workloads, the law changes might lead more severe patients to sort away from NPs and toward GPs for their care. While such sorting should lead to reductions rather than the observed increases in controlled substance prescribing among NPs, we can nevertheless examine whether the law changes are associated with changes in the types of patients receiving care from GPs.[37] To do so, we estimate balancing analogues of equation (3) in which we consider the impacts of the law changes on the average risk scores of Medicare patients seen by GPs from 2012 to 2018. We also consider the effects of the law changes on the average patient gender, age, and insurance type profiles of patients receiving controlled substance prescriptions from each provider type in the IQVIA data from 2006 to 2018.

As shown in Figure 7(b), there is no evidence that GPs began seeing patients with higher risk scores following the law changes. The 95 percent confidence intervals demonstrate that average risk scores of Medicare patients seen by GPs did not decrease by more than 0.019 (1.3 percent) or increase by more than 0.015 (0.97 percent) after NPs were allowed to independently prescribe controlled substances. As shown in Figure A11, there is also no consistent evidence that allowing NPs to independently prescribe controlled substances affects the types of patients receiving controlled substance prescriptions from all providers (subfigure (a)), NPs (subfigure (b)), or GPs (subfigure (c)). While an occasional estimate is statistically significant, which might reflect spurious associations given the number of outcomes being examined, the characteristics of patients receiving controlled substance prescriptions from GPs are quite stable despite the large increases in prescribing. It is therefore unlikely that our results are driven by changes in the types of patients seen by GPs following the law changes.

Finally, we consider whether increases in prescribing among NPs might lead GPs to increase their prescribing for medically justified reasons. For example, if NPs start new patients on opioids following the law changes, then GPs might increase their prescribing for these same patients on subsequent visits to avoid disrupting the patients' treatment.

[37] Simultaneous increases in prescribing among GPs and NPs could be observed if both (1) less severe patients sort toward NPs following the law changes and (2) NPs are more lenient in their prescribing. However, recent work by Chan and Chen (2022) shows that NPs are significantly *less* likely to prescribe opioids than physicians.

However, as previously shown in Table 4, the increases in opioid prescribing among GPs come almost entirely from prescriptions for opioid-naïve patients, and thus GPs are not simply continuing pain management treatment initiated by NPs. Relatedly, if NPs get additional patients addicted to opioids, then GPs might increase their prescribing of opioids used for medication-assisted treatment of opioid use disorders. As previously shown in Figure A6, the results are nearly identical when prescriptions for methadone and buprenorphine are excluded, and thus the prescribing increases among GPs are not coming from the initiation of treatment for opioid use disorder.[38]

VI Conclusion

We document the ways in which the prescribing practices of GPs change following increases in competition precipitated by changes in state-level scope-of-practice laws granting NPs the ability to prescribe controlled substances without physician oversight. We find that GPs respond to such legislation by increasing their prescribing of opioids and controlled anti-anxiety medications such as benzodiazepines. GPs also increase their co-prescribing of opioids and benzodiazepines to the same patient on the same day, a behavior that the CDC strongly cautions against because it can lead to respiratory failure (CDC, 2016). Notably, over half of the additional controlled substance prescriptions following the law changes are due to increased prescribing by GPs. While NPs experience a larger percentage increase relative to their baseline prescribing compared to GPs, the greater number of GPs and their significantly higher baseline levels result in GPs contributing more to the overall increase in prescriptions.

Three additional tests support the hypothesis that the increases in controlled substance prescribing among GPs following the law changes are driven by increased competition from NPs. First, the observed increases in GP prescribing are larger in areas with a greater number

[38] Recent work highlights the role played by pharmaceutical companies in contributing to the opioid crisis and affecting prescribing practices more generally (Carey et al., 2021; Alpert et al., 2022; Arteaga and Barone, 2022). If pharmaceutical companies adjust their marketing to clinicians following law changes granting NPs independent prescriptive authority, then these changes might affect how prescribing evolves in the law changes' aftermath. Using publicly available data from Open Payments covering all monetary and in-kind payments made to physicians from pharmaceutical companies from 2013–2018, we find that payments made to GPs were stable in the years surrounding the law changes.

of NPs per GP at baseline. Second, changes in prescribing are concentrated in the specialties that compete most directly with NPs. Third, the law changes do not affect the prescribing of many commonly prescribed non-controlled substances, such as antihypertensives and antibiotics.

Additional evidence indicates that the increases in controlled substance prescribing are unlikely to be driven by other changes to GPs' practices that might occur as a result of the law changes. First, the law changes lead to slight reductions in the number of office visits for Medicare beneficiaries among GPs, which should lead prescribing to decrease all else equal. Moreover, we find no evidence that the law changes lead to reductions in the share of GPs practicing in the same clinics as NPs or in the number of NPs per GP practice. Taken together, these two findings suggest that our results are not driven by increases in workloads among physicians resulting either from GPs spending more time on patient care or from newly independent NPs leaving their joint practices. Finally, we show that the law changes do not affect the gender, age, and payment type profiles of patients receiving controlled substance prescriptions from GPs or the risk scores of Medicare patients seen by GPs.

Examining the increases in opioid prescribing in greater depth shows that GPs increase the strength of opioid prescriptions and the number of very high-strength prescriptions in response to increased competition. Moreover, competition-induced increases in the number of opioid prescriptions are due predominately to increases among opioid-naïve patients, suggesting that competition among providers puts additional patients at risk of developing opioid use disorder. Consistent with these increases in prescribing, we find that the law changes lead to increases in fatal drug overdoses involving prescription opioids. Our work focusing on the role of competition therefore adds another consideration to recent research showing that physician prescribing of opioids is driven in part by training (Schnell and Currie, 2018), beliefs about risks (Doctor et al., 2018), pharmaceutical marketing (Alpert et al., 2022; Arteaga and Barone, 2022), and provider altruism coupled with the existence of secondary markets (Schnell, 2017).

This paper begins to fill an important gap in the literature on the effects of competition in health care markets by examining the impacts of exogenous changes in competition at the individual provider level rather than at the level of the hospital or insurer. The results are consistent with the cautions of authors such as Gaynor et al. (2015) and McGuire (2000), who suggest that more competition will not always lead to improvements in patient care and can instead lead to excessive and even harmful service provision.

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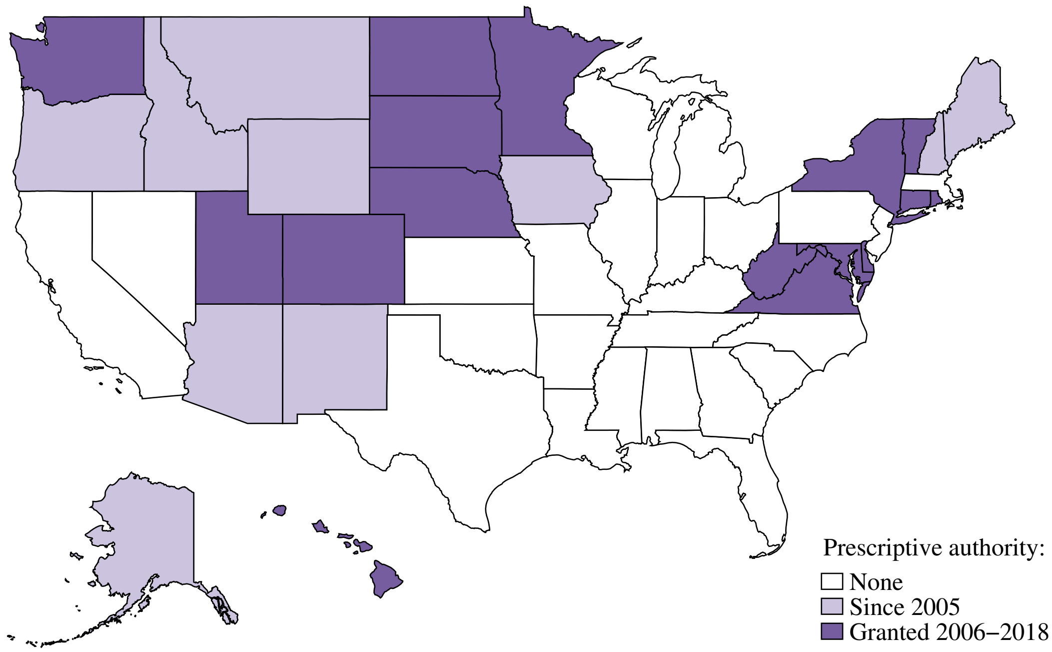
VII Figures

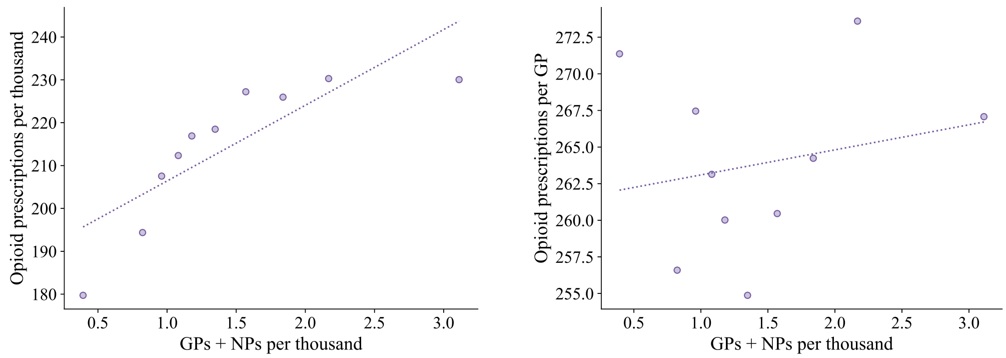
Figure 1: NP independent prescriptive authority for controlled substances: 2006–2018



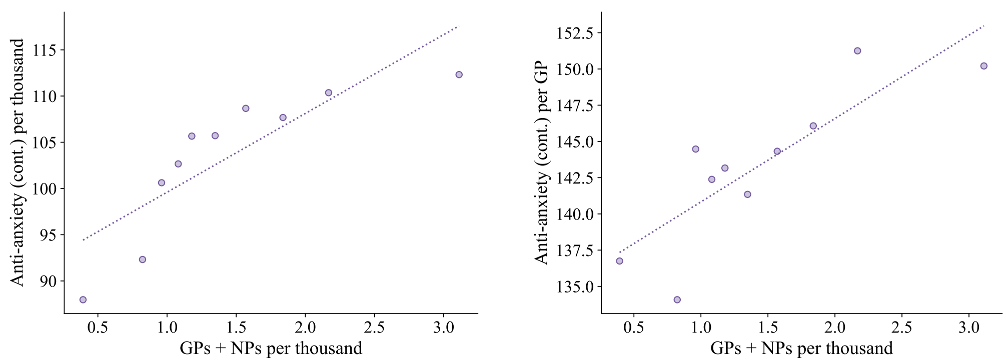
Notes: We consider states as having independent prescriptive authority if nurse practitioners (NPs) registered in the state have the statutory authority to prescribe controlled substances without physician collaboration or supervision. Years in which states granted NPs independent prescriptive authority come from McMichael and Markowitz (2023); see footnote 24 for additional details.

Figure 2: Changes in the number of prescribers and controlled substance prescribing

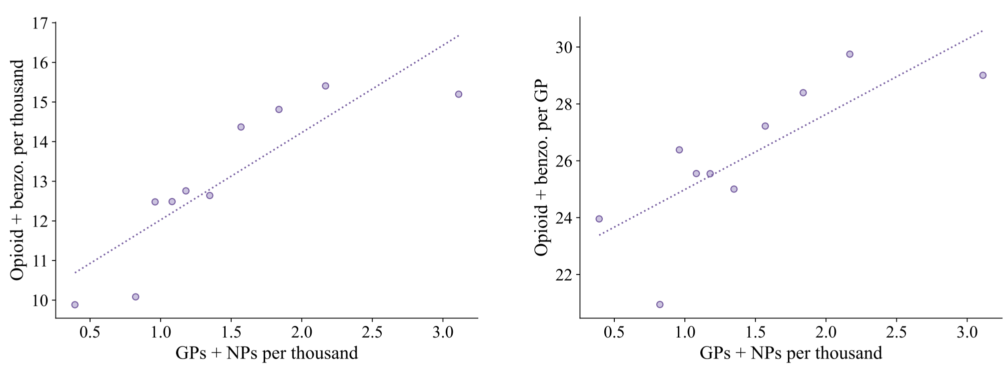
(a) Opioid prescriptions



(b) Controlled anti-anxiety prescriptions



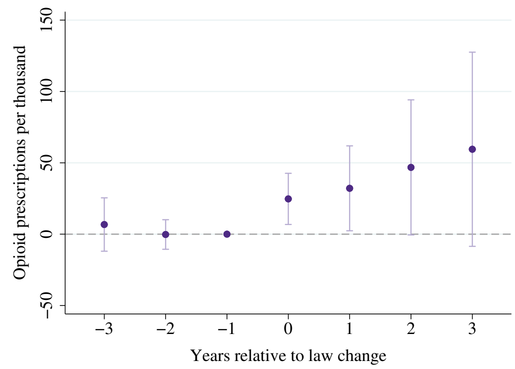
(c) Opioid + benzo. co-prescriptions



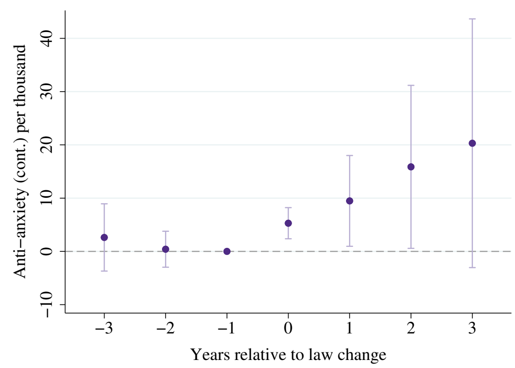
Notes: The above figures show the relationship between changes in the number of general practice physicians (GPs) and nurse practitioners (NPs) per 1,000 people and changes in measures of opioid prescribing (subfigure (a)), anti-anxiety controlled substance prescribing (subfigure (b)), and opioid and benzodiazepine co-prescribing (subfigure (c)) at the county-year level from 2006 to 2018. All relationships are conditional on county and year fixed effects. The left subfigure in each subplot considers the amount of a given prescribing behavior by GPs and NPs per 1,000 people; the right subfigure considers the average amount of a given behavior per prescribing GP. The number of NPs is set to zero until NPs are allowed to prescribe controlled substances independently in a given state. We exclude the six states that granted NPs the ability to prescribe controlled substances non-independently between 2006 and 2018 from these figures. Counties are grouped into deciles accounting for approximately equal shares of the population based on the number of GPs and NPs per 1,000 people. The dotted line is the fitted line across deciles. Data come from the IQVIA LRx database.

Figure 3: Effects of NP independent prescriptive authority on aggregate controlled substance prescribing

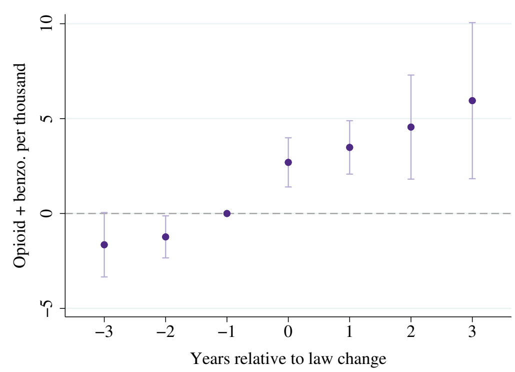
(a) Opioid prescriptions



(b) Controlled anti-anxiety prescriptions



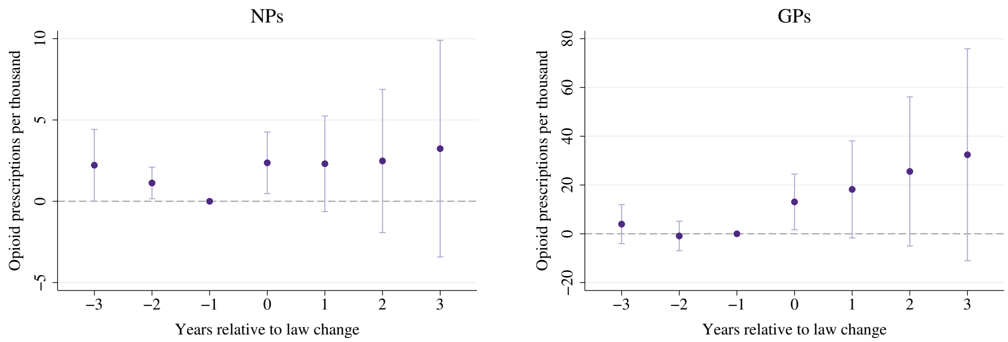
(c) Opioid + benzo. co-prescriptions



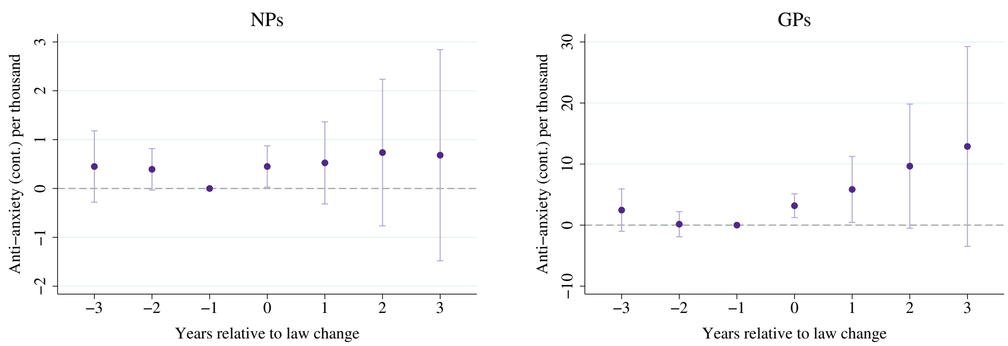
Notes: The above figures present coefficients and 95% confidence intervals from estimation of equation (2) using county-year–level data for 2006–2018. Outcomes are the number of opioid prescriptions per 1,000 people (subfigure (a)), the number of anti-anxiety controlled substance prescriptions per 1,000 people (subfigure (b)), and the number of instances in which an opioid and benzodiazepine prescription were written for the same patient by the same provider on the same day ("co-prescriptions") per 1,000 people (subfigure (c)). To allow for a balanced panel, these figures consider effects in the 11 states with law changes between 2009–2015. The regressions include county and year fixed effects; county-specific linear time trends; time-varying, state-level controls for changes in independent prescriptive authority for controlled substances outside of the balanced panel window and changes in non-independent prescriptive authority for controlled substances; and all time-varying, county-level controls listed in Figure A1. Standard errors are clustered by state. Outcome data come from the IQVIA LRx database.

Figure 4: Effects of NP independent prescriptive authority on controlled substance prescribing by NPs and GPs

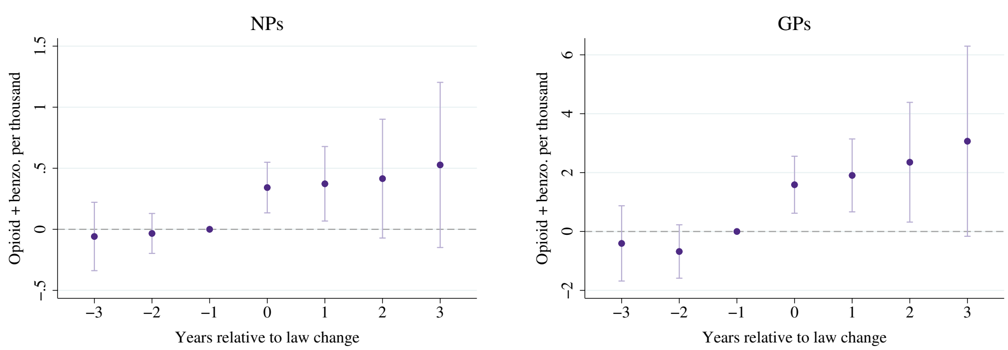
(a) Opioid prescriptions



(b) Controlled anti-anxiety prescriptions



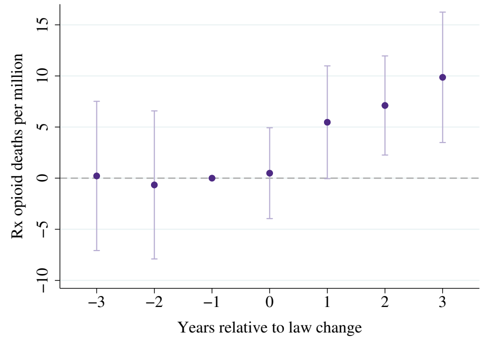
(c) Opioid + benzo. co-prescriptions



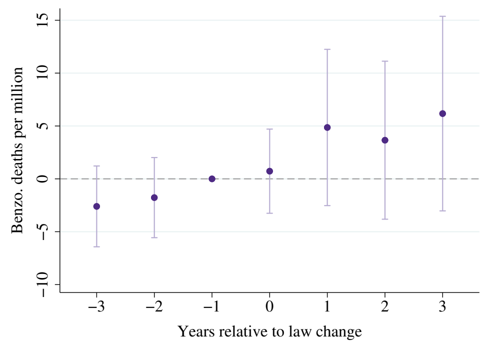
Notes: The above figures present coefficients and 95% confidence intervals from estimation of equation (2) using county-year–level data for 2006–2018. The left (right) subfigure in each subplot only considers prescriptions written by nurse practitioners [NPs] (physicians in general practice [GPs]). Outcomes are the number of opioid prescriptions per 1,000 people (subfigure (a)), the number of anti-anxiety controlled substance prescriptions per 1,000 people (subfigure (b)), and the number of instances in which an opioid and benzodiazepine prescription were written for the same patient by the same provider on the same day ("co-prescriptions") per 1,000 people (subfigure (c)). To allow for a balanced panel, these figures consider effects in the 11 states with law changes between 2009–2015. The regressions include county and year fixed effects; county-specific linear time trends; time-varying, state-level controls for changes in independent prescriptive authority for controlled substances outside of the balanced panel window and changes in non-independent prescriptive authority for controlled substances; and all time-varying, county-level controls listed in Figure A1. Standard errors are clustered by state. Outcome data come from the IQVIA LRx database.

Figure 5: Effects of NP independent prescriptive authority on fatal drug overdoses

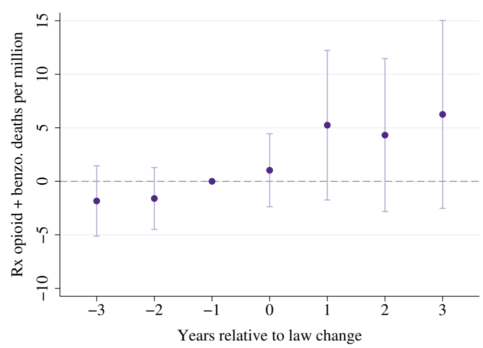
(a) Prescription opioid deaths



(b) Benzodiazepine deaths



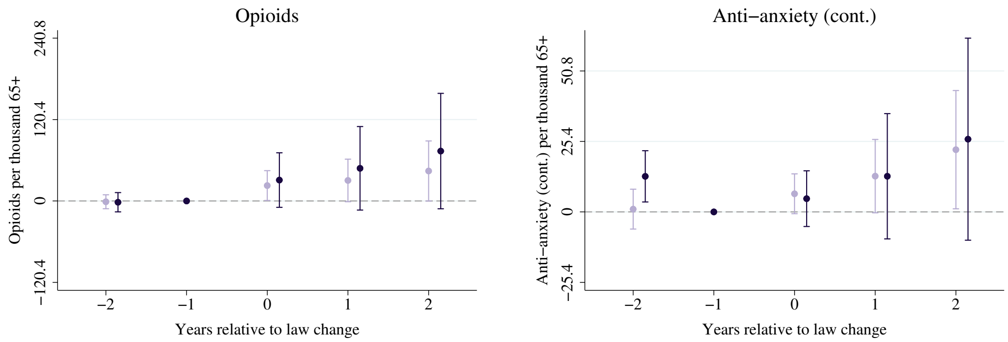
(c) Prescription opioid + benzo. deaths



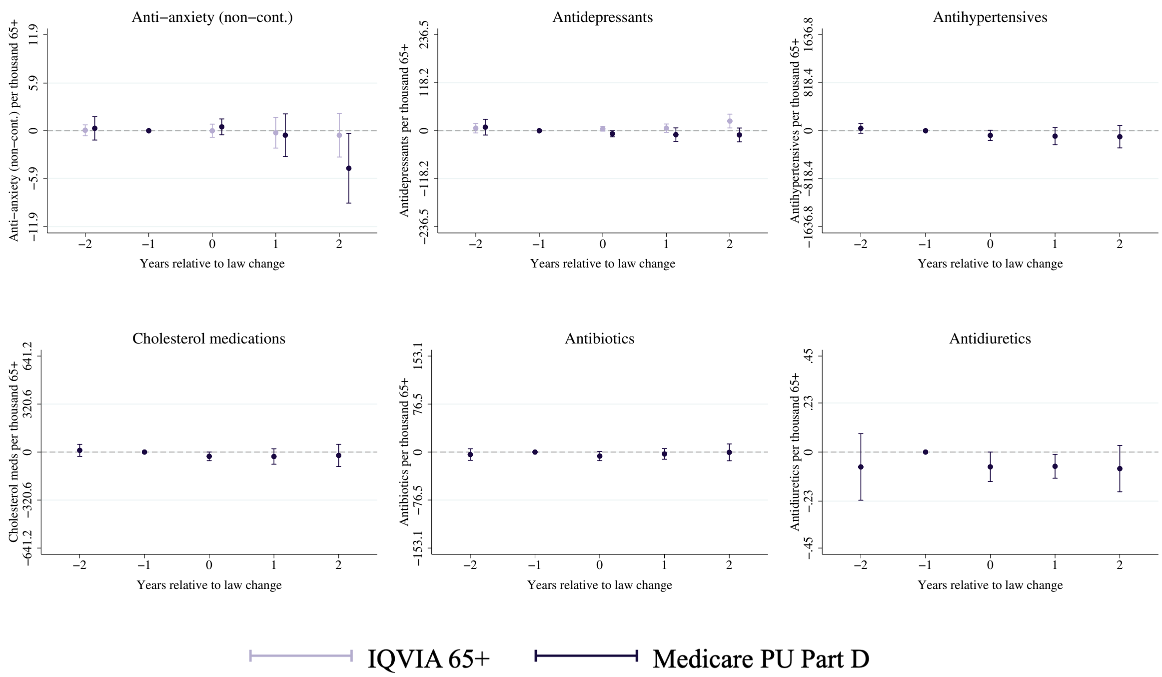
Notes: The above figures present coefficients and 95% confidence intervals from estimation of an analogue of equation (2) using county-year–level data for 2006–2018. Outcomes are the number of fatal drug overdoses per 1,000,000 people involving prescription opioids (subfigure (a)), benzodiazepines (subfigure (b)), and prescription opioids in combination with benzodiazepines (subfigure (c)). To allow for a balanced panel, these figures consider effects in the 11 states with law changes between 2009–2015. The regressions include county and year fixed effects; time-varying, state-level controls for changes in independent prescriptive authority for controlled substances outside of the balanced panel window and changes in non-independent prescriptive authority for controlled substances; and all time-varying, county-level controls listed in Figure A1. Standard errors are clustered by state. Outcome data come from the NVSS database.

Figure 6: Effects of NP independent prescriptive authority on controlled and non-controlled substance prescribing by GPs (IQVIA and Medicare data, 2012–2018)

(a) Controlled substances



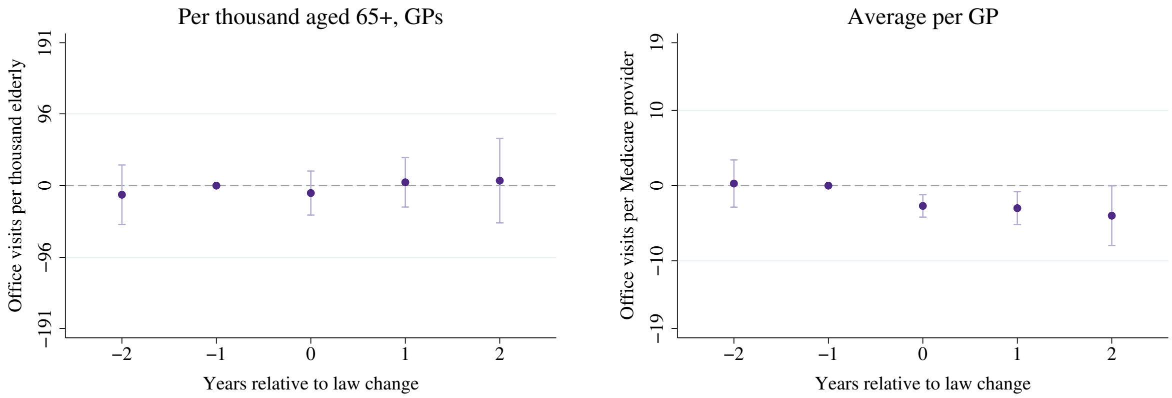
(b) Non-controlled substances



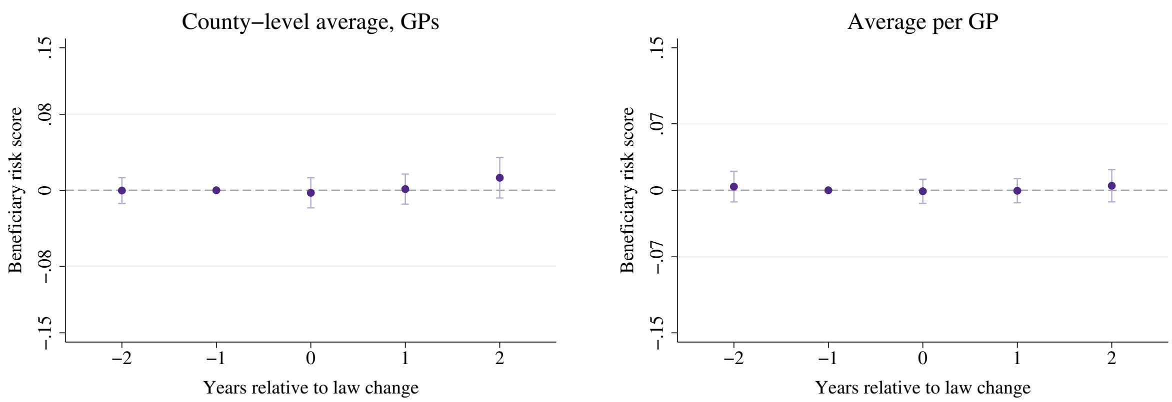
Notes: The above figures present coefficients and 95% confidence intervals from estimation of an analogue of equation (2) using county-year–level data for 2012–2018. Outcomes are the number of prescriptions written by physicians in general practice (GPs) for patients aged 65+ in the IQVIA data (light dots and bars) or paid for by Medicare Part D in the public use Medicare files (dark dots and bars) per 1,000 people aged 65+ for controlled (subfigure (a)) and non-controlled (subfigure (b)) medication classes. To make effect sizes more comparable across medication classes, the y-axes in subfigure (b) are scaled to range from −33 to +33 percent of the baseline mean of each outcome; in subfigure (a), the axes extend to +33 percent of the baseline mean for opioids and +200 percent of the baseline mean for controlled anti-anxiety medications. To allow for a balanced panel, these figures consider effects in the five states with law changes between 2014–2016. The regressions include county and year fixed effects; time-varying, state-level controls for changes in independent prescriptive authority for controlled substances outside of the balanced panel window and changes in non-independent prescriptive authority for controlled substances; and all time-varying, county-level controls listed in Figure A1. Standard errors are clustered by state. See Figure A9 for analogous figures for non-controlled substance prescribing in the full IQVIA data for 2006–2018.

Figure 7: Effects of NP independent prescriptive authority on office visits and patient risk scores among GPs (Medicare data, 2012–2018)

(a) Office visits with GPs



(b) Patient risk scores among GPs



Notes: The above figures present coefficients and 95% confidence intervals from estimation of an analogue of equation (2) using county-year–level data for 2012–2018. The outcomes in subfigure (a) are the number of office visits with physicians in general practice (GPs) paid for by Medicare Part B per 1,000 people aged 65+ (left panel) and per GP in the Part B files (right panel). The outcomes in subfigure (b) are the average risk score among patients seen by GPs in the Medicare Part B files per county (left panel) and per GP (right panel). The y-axes are scaled to range from −10 to +10 percent of the baseline mean of each outcome. To allow for a balanced panel, these figures consider effects in the five states with law changes between 2014–2016. The regressions include county and year fixed effects; time-varying, state-level controls for changes in independent prescriptive authority for controlled substances outside of the balanced panel window and changes in non-independent prescriptive authority for controlled substances; and all time-varying, county-level controls listed in Figure A1. Standard errors are clustered by state. Outcome data come from the public use Medicare Part B files.

VIII Tables

Table 1: Number of prescribers and prescription shares by provider type

		Controlled substance prescription shares		
	Unique providers	Opioids	Anti-anxiety	Opioid + benzo.
	(1)	(2)	(3)	(4)
a. 2006–2018				
Select physician specialties				
General practice	401,916	0.443	0.596	0.609
Emergency medicine	60,035	0.063	0.017	0.036
Psych. & neurology	95,655	0.018	0.162	0.024
Obstetrics & gyn.	62,200	0.026	0.015	0.014
General surgery	71,344	0.055	0.008	0.019
Orthopedic surgery	38,413	0.075	0.007	0.020
Nurse practitioners	269,015	0.068	0.075	0.064
Total providers	1,569,881	1.000	1.000	1.000
Total pres. (billions)		2.060	0.752	0.100
b. 2006				
Select physician specialties				
General practice	241,131	0.477	0.643	0.649
Emergency medicine	32,567	0.074	0.018	0.039
Psych. & neurology	59,902	0.022	0.163	0.034
Obstetrics & gyn.	40,759	0.033	0.018	0.014
General surgery	42,268	0.064	0.010	0.021
Orthopedic surgery	24,856	0.095	0.008	0.023
Nurse practitioners	56,608	0.028	0.030	0.026
Total providers	763,278	1.000	1.000	1.000
Total pres. (millons)		132.3	44.63	5.711
c. 2018				
Select physician specialties				
General practice	305,295	0.382	0.543	0.588
Emergency medicine	51,117	0.042	0.012	0.022
Psych. & neurology	71,910	0.014	0.166	0.017
Obstetrics & gyn.	45,325	0.020	0.011	0.012
General surgery	49,527	0.052	0.007	0.020
Orthopedic surgery	29,476	0.058	0.005	0.017
Nurse practitioners	201,764	0.119	0.132	0.102
Total providers	1,111,232	1.000	1.000	1.000
Total pres. (millons)		131.9	56.30	5.023

Notes: Observations are at the provider-year level. Total prescriptions reflect the total number of prescriptions written by providers of all types (including specialties not reported in the table) in the reported time period; prescription shares are calculated relative to these totals. "Opioid + benzo." denotes instances of co-prescribing of an opioid and a benzodiazepine to the same patient by the same provider on the same day. Data come from the IQVIA LRx database.

Table 2: Average county-level prescription outcomes by treatment status

	Always takers			Never takers			Treatment states		
	'06–'18 (1)	2006 (2)	2018 (3)	'06–'18 (4)	2006 (5)	2018 (6)	'06–'18 (7)	2006 (8)	2018 (9)
Number of states	11			24			16		
a. General practice physicians									
Prescriptions per thousand									
Opioids	233.5	227.4	150.1	236.5	220.9	165.1	177.5	169.6	117.2
Anti-anxiety (cont.)	89.31	80.88	69.32	120.4	104.8	102.7	82.32	70.27	70.38
Opioid + benzo.	10.56	9.541	5.862	16.89	13.96	10.27	9.604	7.890	5.904
Prescribing providers per thousand									
Opioids	0.868	0.776	0.846	0.801	0.741	0.782	0.869	0.806	0.823
Anti-anxiety (cont.)	0.770	0.703	0.746	0.710	0.669	0.692	0.750	0.698	0.713
Opioid + benzo.	0.539	0.523	0.436	0.489	0.481	0.406	0.478	0.455	0.381
Average prescriptions per prescribing provider									
Opioids	267.9	289.3	175.6	295.4	295.8	211.4	204.4	209.9	142.7
Anti-anxiety (cont.)	114.7	112.7	91.74	168.3	154.4	147.4	107.5	97.61	96.84
Opioid + benzo.	19.03	17.57	13.20	33.24	28.06	24.38	19.03	15.90	14.85
Unique providers	40,964	17,729	23,154	298,790	168,120	213,203	116,080	55,282	68,938
b. Nurse practitioners									
Prescriptions per thousand									
Opioids	66.67	31.80	84.92	29.86	8.664	44.23	37.22	17.93	47.30
Anti-anxiety (cont.)	26.24	12.06	34.03	12.13	3.226	21.40	15.17	6.042	22.94
Opioid + benzo.	2.429	1.144	2.056	1.502	0.412	1.553	1.524	0.586	1.423
Prescribing providers per thousand									
Opioids	0.414	0.272	0.544	0.255	0.115	0.389	0.345	0.226	0.443
Anti-anxiety (cont.)	0.379	0.235	0.537	0.200	0.082	0.344	0.296	0.174	0.417
Opioid + benzo.	0.182	0.107	0.218	0.093	0.034	0.136	0.128	0.070	0.151
Average prescriptions per prescribing provider									
Opioids	153.4	111.9	150.9	85.58	48.95	92.77	94.58	67.90	95.45
Anti-anxiety (cont.)	65.18	49.35	59.12	43.64	24.35	54.75	44.21	30.42	49.97
Opioid + benzo.	12.29	9.528	8.843	10.41	5.974	8.506	10.15	6.957	8.136
Unique providers	27,722	7,046	17,773	192,223	31,645	139,440	69,260	17,917	44,551

Notes: Observations are at the county-year level, and averages are weighted by population. "Always takers" refers to states in which NPs has independent prescriptive authority for controlled substances since 2006, "never takers" refers to states in which NPs did not have independent prescriptive authority for controlled substances as of 2018, and "treatment states" refers to states in which NPs were granted the ability to independently prescribe controlled substances between 2006 and 2018. "Opioid + benzo." denotes instances of co-prescribing of an opioid and a benzodiazepine to the same patient by the same provider on the same day. Data come from the IQVIA LRx database.

Table 3: Effects of NP independent prescriptive authority on controlled substance prescribing by provider type

	All providers			Nurse practitioners			General practice physicians		
	Opioids	Anti-anxiety	Opioid + benzo.	Opioids	Anti-anxiety	Opioid + benzo.	Opioids	Anti-anxiety	Opioid + benzo.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
a. Prescriptions per thousand									
Post law change, 0–3 years	38.342	10.418	4.195	2.217	0.450	0.411	20.728	6.465	2.341
	(19.337)	(5.083)	(1.078)	(1.807)	(0.459)	(0.223)	(12.321)	(3.321)	(0.858)
	[0.053]	[0.046]	[<0.001]	[0.226]	[0.331]	[0.071]	[0.099]	[0.057]	[0.009]
Baseline mean	504.4	175.5	26.75	25.45	9.473	1.322	232.1	107.4	16.28
Relative to mean	0.076	0.059	0.157	0.087	0.047	0.311	0.089	0.060	0.144
b. Frequent providers per thousand									
Post law change, 0–3 years	0.002	0.007	0.002	0.000	–0.001	–0.003	0.004	0.000	0.002
	(0.013)	(0.007)	(0.016)	(0.002)	(0.001)	(0.002)	(0.006)	(0.002)	(0.007)
	[0.899]	[0.366]	[0.922]	[0.918]	[0.449]	[0.207]	[0.528]	[0.942]	[0.756]
Baseline mean	1.202	0.711	0.895	0.076	0.049	0.062	0.497	0.383	0.435
Relative to mean	0.001	0.009	0.002	0.003	–0.021	–0.047	0.007	–0.000	0.005
c. Average prescriptions per prescribing provider									
Post law change, 0–3 years	9.955	5.662	2.946	12.099	4.859	2.638	24.192	10.070	3.877
	(5.814)	(2.248)	(0.791)	(6.475)	(2.648)	(0.983)	(11.799)	(3.695)	(1.402)
	[0.093]	[0.015]	[<0.001]	[0.068]	[0.072]	[0.010]	[0.046]	[0.009]	[0.008]
Baseline mean	194.1	84.35	22.98	83.86	39.72	11.20	281.6	145.3	30.57
Relative to mean	0.051	0.067	0.128	0.144	0.122	0.235	0.086	0.069	0.127
Observations	40,911	40,911	40,911	40,911	40,911	40,911	40,911	40,911	40,911

Notes: The above table reports coefficients, standard errors (in parentheses), and p-values [in brackets] from estimation of equation (3) using county-year–level data for 2006–2018. Outcomes are the number of prescriptions of a given type written by providers of a given type per 1,000 people (panel (a)), the number of providers of a given type who are observed writing prescriptions of a given type in each month (or year for opioid-benzo. co-prescribing) per 1,000 people (panel (b)), and the average annual number of prescriptions of a given type written by providers of a given type (panel (c)). "Opioid + benzo." denotes instances of co-prescribing of an opioid and a benzodiazepine to the same patient by the same provider on the same day. Columns (1)–(3) consider all providers, columns (4)–(6) consider nurse practitioners, and columns (7)–(9) consider physicians in general practice. The baseline mean is measured as the average across all counties in 2010. Standard errors are clustered by state. Outcome data come from the IQVIA LRx database.

Table 4: Effects of NP independent prescriptive authority on opioid prescribing by patient type

	Nurse practitioners			General practice physicians		
	Overall	Opioid naive	Non-opioid naive	Overall	Opioid naive	Non-opioid naive
	(1)	(2)	(3)	(4)	(5)	(6)
a. Prescriptions per thousand						
Post law change, 0–3 years	2.217	1.880	0.336	20.728	20.398	0.331
	(1.807)	(1.474)	(0.430)	(12.321)	(11.772)	(0.811)
	[0.226]	[0.208]	[0.438]	[0.099]	[0.089]	[0.685]
Baseline mean	25.45	20.44	5.013	232.1	199.2	32.94
Relative to mean	0.087	0.092	0.067	0.089	0.102	0.010
b. Average days supplied per prescription						
Post law change, 0–3 years	–0.025	–0.041	–0.016	–0.110	–0.122	–0.143
	(0.211)	(0.211)	(0.137)	(0.133)	(0.139)	(0.151)
	[0.907]	[0.848]	[0.908]	[0.414]	[0.385]	[0.348]
Baseline mean	3.250	3.255	2.081	10.46	10.94	6.882
Relative to mean	–0.008	–0.013	–0.008	–0.010	–0.011	–0.021
c. Average MME per day supplied						
Post law change, 0–3 years	21.029	16.382	21.773	26.274	22.688	32.374
	(11.387)	(10.158)	(12.674)	(8.638)	(8.379)	(10.185)
	[0.071]	[0.113]	[0.092]	[0.004]	[0.009]	[0.003]
Baseline mean	189.3	156.3	198.3	388.0	339.1	479.8
Relative to mean	0.111	0.105	0.110	0.068	0.067	0.067
d. Prescriptions with $>$ 120mg MME daily per thousand						
Post law change, 0–3 years	0.881	0.752	0.129	6.085	5.908	0.177
	(0.760)	(0.552)	(0.271)	(2.821)	(2.702)	(0.613)
	[0.252]	[0.179]	[0.637]	[0.036]	[0.033]	[0.774]
Baseline mean	8.644	6.278	2.366	75.94	61.63	14.31
Relative to mean	0.102	0.120	0.054	0.080	0.096	0.012
Observations	40,911	40,911	40,911	40,911	40,911	40,911

Notes: The above table reports coefficients, standard errors (in parentheses), and p-values [in brackets] from estimation of equation (3) using county-year–level data for 2006–2018. For each patient and provider type, outcomes are the number of opioid prescriptions per 1,000 people (panel (a)), the average number of days supplied per opioid prescription (panel (b)), the average daily morphine milligram equivalents (MMEs) per opioid prescription (panel (c)), and the number of opioid prescriptions with greater than 120 MME daily per 1,000 people (panel (d)). Columns (1)–(3) consider prescriptions written by nurse practitioners, and columns (4)–(6) consider prescriptions written by physicians in general practice. "Opioid naive" refers to patients who did not fill an opioid prescription in the past six months. To allow for a balanced panel, this table considers the effects 0–3 years after the law change in the 11 states with law changes between 2009–2015. The regressions include county and year fixed effects; county-specific linear time trends; time-varying, state-level controls for changes in independent prescriptive authority for controlled substances outside of the balanced panel window and changes in non-independent prescriptive authority for controlled substances; and all time-varying, county-level controls listed in Figure A1. The baseline mean is measured as the average across all counties in 2010. Standard errors are clustered by state. Outcome data come from the IQVIA LRx database.

Table 5: Effects of NP independent prescriptive authority on GP controlled substance prescribing by exposure to NPs

	General practice physicians		
Prescriptions per 1,000:	Opioids (1)	Anti-anxiety (2)	Opioid + benzo. (3)
Post law change, 0–3 years (β_1)	18.526	5.445	2.285
	(12.071)	(3.428)	(0.911)
	[0.131]	[0.118]	[0.015]
× Above median (β_2)	8.775	4.086	0.219
	(3.017)	(1.362)	(0.280)
	[0.005]	[0.004]	[0.437]
$\beta_1 + \beta_2$	27.301	9.531	2.504
	(13.086)	(3.076)	(0.712)
	[0.042]	[0.003]	[<0.001]
Baseline mean (below median)	215.4	101.7	14.22
Baseline mean (above median)	267.9	119.6	20.70
Relative to mean (below median)	0.086	0.054	0.161
Relative to mean (above median)	0.102	0.080	0.121
Observations	40,911	40,911	40,911

Notes: The above table reports coefficients, standard errors (in parentheses), and p-values [in brackets] from estimation of an augmented version of equation (3) that includes an interaction between the treatment indicator and an indicator denoting whether the county had an above-median number of nurse practitioners (NPs) per general practice physicians (GPs) among treatment states in 2006 using county-year–level data for 2006–2018. Outcomes are the number of prescriptions of a given type written by GPs per 1,000 people. "Opioid + benzo." denotes instances of co-prescribing of an opioid and a benzodiazepine to the same patient by the same provider on the same day. To allow for a balanced panel, this table considers the effects 0–3 years after the law change in the 11 states with law changes between 2009–2015. The regressions include county and year fixed effects; county-specific linear time trends; time-varying, state-level controls for changes in independent prescriptive authority for controlled substances outside of the balanced panel window and changes in non-independent prescriptive authority for controlled substances; and all time-varying, county-level controls listed in Figure A1. The baseline mean is measured as the average across all counties of a given type in 2010. Standard errors are clustered by state. Outcome data come from the IQVIA LRx database.

Table 6: Effects of NP independent prescriptive authority on controlled substance prescribing across physician specialties

	General practice (1)	Emergency medicine (2)	Psych. & neurology (3)	Obstetrics & gyn. (4)	Orthopedic surgery (5)	General surgery (6)
a. Opioids per prescriber						
Post law change, 0–3 years	24.192	15.079	7.273	5.061	27.142	0.697
	(11.799)	(5.525)	(2.820)	(2.957)	(19.937)	(4.065)
	[0.046]	[0.009]	[0.013]	[0.093]	[0.179]	[0.865]
Baseline mean	281.6	252.4	74.61	99.60	428.2	179.9
Relative to mean	0.086	0.060	0.097	0.051	0.063	0.004
b. Anti-anxiety per prescriber						
Post law change, 0–3 years	10.070	1.188	8.246	1.209	1.916	0.342
	(3.695)	(0.967)	(4.334)	(0.988)	(1.178)	(0.314)
	[0.009]	[0.225]	[0.063]	[0.227]	[0.110]	[0.281]
Baseline mean	145.3	27.37	166.7	24.77	17.48	15.17
Relative to mean	0.069	0.043	0.049	0.049	0.110	0.023
c. Opioid + benzo. per prescriber						
Post law change, 0–3 years	3.877	1.021	0.567	0.874	2.563	0.079
	(1.402)	(0.613)	(0.911)	(0.418)	(1.853)	(0.595)
	[0.008]	[0.102]	[0.536]	[0.042]	[0.173]	[0.895]
Baseline mean	30.57	11.46	13.46	8.074	11.20	9.139
Relative to mean	0.127	0.089	0.042	0.108	0.229	0.009
Observations	40,911	40,911	40,911	40,911	40,911	40,911

Notes: The above table reports coefficients, standard errors (in parentheses), and p-values [in brackets] from estimation of equation (3) using county-year–level data for 2006–2018. Outcomes are the number of prescriptions of a given type written by physicians of a given type per 1,000 people. "Opioid + benzo." denotes instances of co-prescribing of an opioid and a benzodiazepine to the same patient by the same provider on the same day. To allow for a balanced panel, this table considers the effects 0–3 years after the law change in the 11 states with law changes between 2009–2015. The regressions include county and year fixed effects; county-specific linear time trends; time-varying, state-level controls for changes in independent prescriptive authority for controlled substances outside of the balanced panel window and changes in non-independent prescriptive authority for controlled substances; and all time-varying, county-level controls listed in Figure A1. The baseline mean is measured as the average across all counties in 2010. Standard errors are clustered by state. Outcome data come from the IQVIA LRx database.

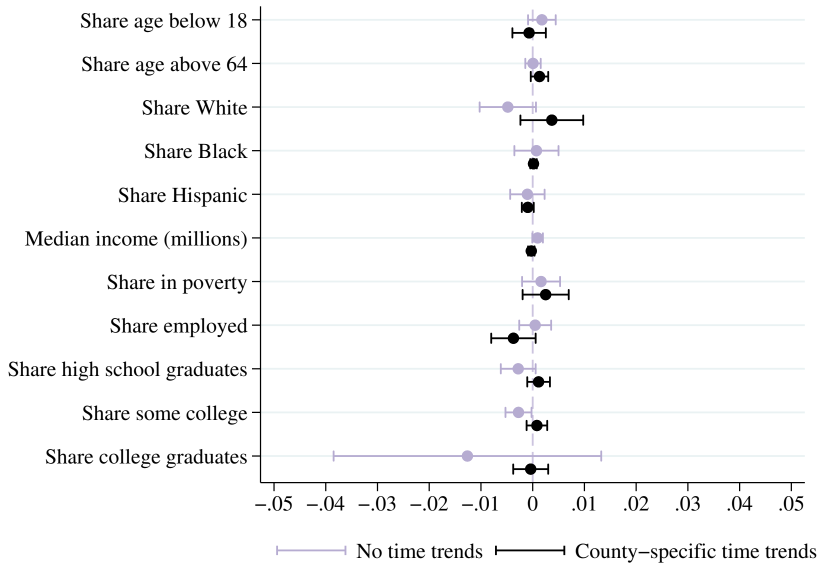
For Online Publication

The Effects of Competition on Physician Prescribing

Currie, Li, and Schnell (2024)

A Supplementary figures and tables

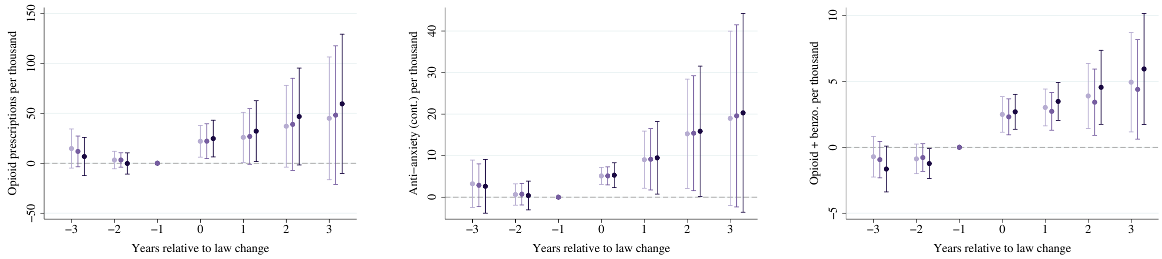
Figure A1: Relationship between changes in NP independent prescriptive authority and potential confounders



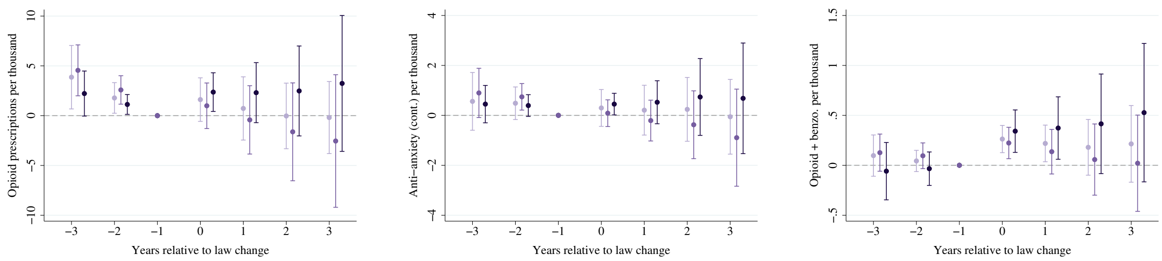
Notes: The above figure presents coefficients and 95% confidence intervals from estimation of balancing analogues of equation (3) using county-year–level data for 2006–2018. Each row presents output from a separate regression in which the potential confounder denoted on the y-axis is the dependent variable. As in our primary analysis, this figure considers the effects 0–3 years after the law change in the 11 states with law changes between 2009–2015. The regressions include county and year fixed effects; time-varying, state-level controls for changes in independent prescriptive authority for controlled substances outside of the balanced panel window and changes in non-independent prescriptive authority for controlled substances; and all time-varying, county-level controls listed in Figure A1. The dark dots and bars (light dots and bars) show results from specifications that include (exclude) county-specific linear time trends. Standard errors are clustered by state. Outcome data come from the ACS.

Figure A2: Effects on controlled substance prescribing: Alternative time trends

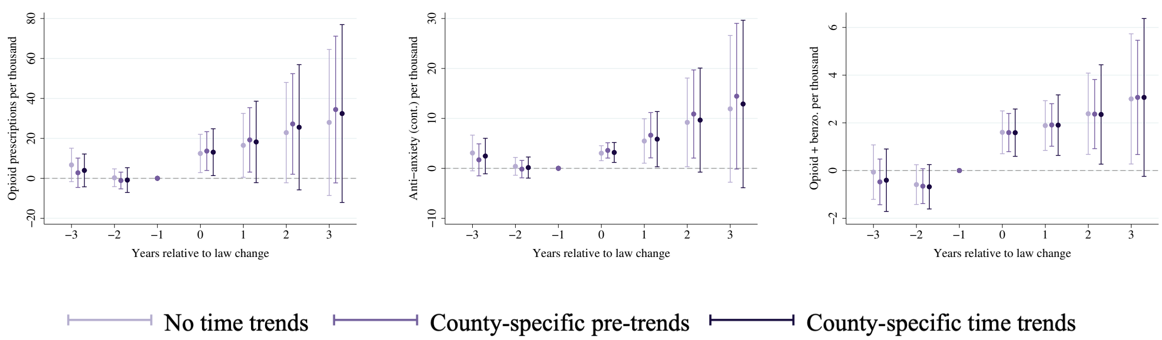
(a) All providers



(b) Nurse practitioners



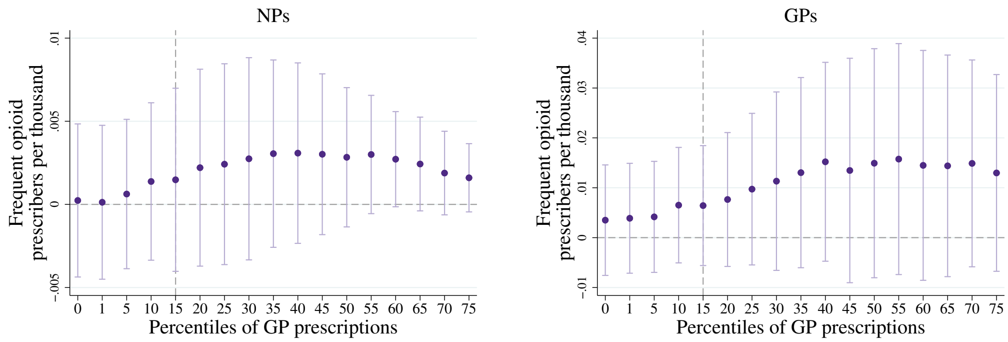
(c) General practice physicians



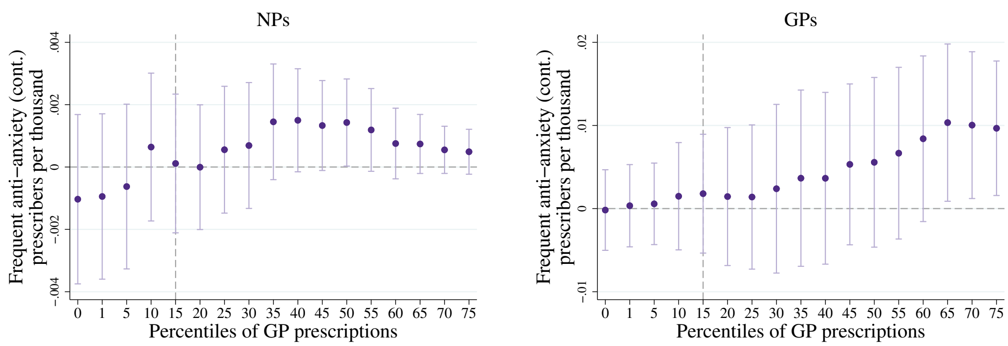
Notes: The above figures present coefficients and 95% confidence intervals from estimation of analogues of equation (2) using county-year–level data for 2006–2018. Outcomes are the number of opioid prescriptions per 1,000 people (left subfigures), the number of anti-anxiety controlled substance prescriptions per 1,000 people (middle subfigures), and the number of instances in which an opioid and benzodiazepine prescription were written for the same patient by the same provider on the same day per 1,000 people (right subfigures) by a given provider type. Subfigure (a) considers prescriptions written by all providers, subfigure (b) considers prescriptions written by nurse practitioners, and subfigure (c) considers prescriptions written by physicians in general practice. To allow for a balanced panel, these figures consider effects in the 11 states with law changes between 2009–2015. The regressions include county and year fixed effects; time-varying, state-level controls for changes in independent prescriptive authority for controlled substances outside of the balanced panel window and changes in non-independent prescriptive authority for controlled substances; and all time-varying, county-level controls listed in Figure A1. The light dots and bars are from specifications without time trends; the medium dots and bars are from specifications that include county-specific linear pre-trends following Goodman-Bacon (2021); and the dark dots and bars are from specifications that include county-specific linear time trends estimated over the entire sample period. Standard errors are clustered by state. Outcome data come from the IQVIA LRx database.

Figure A3: Effects on number of “frequent” prescribers: Alternative definitions

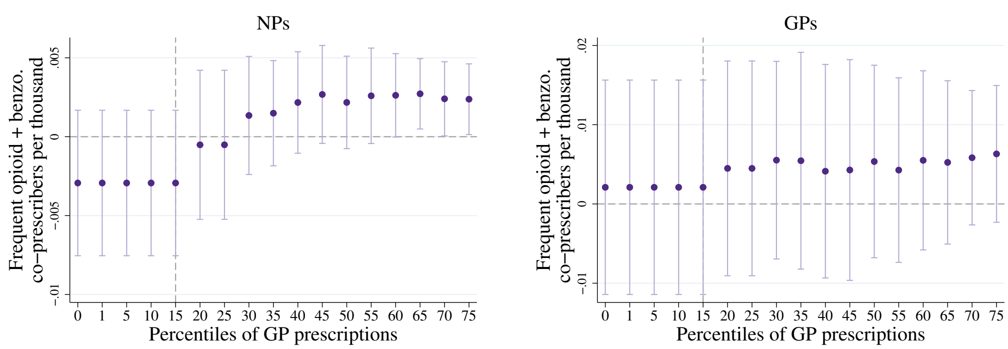
(a) Opioid prescribers



(b) Controlled anti-anxiety prescribers



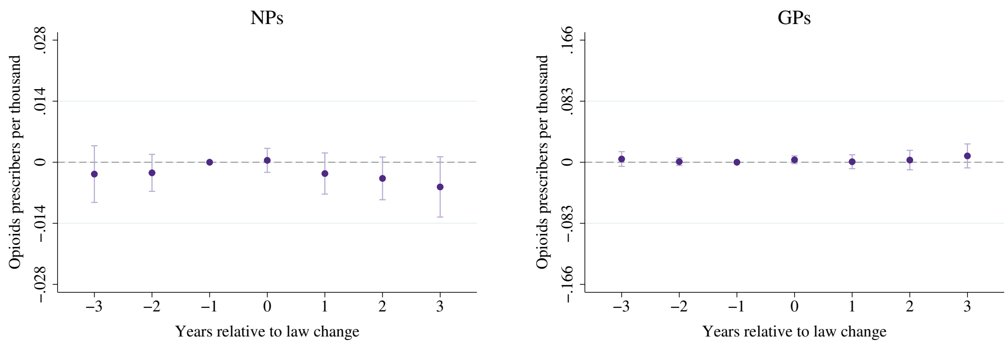
(c) Opioid + benzo. co-prescribers



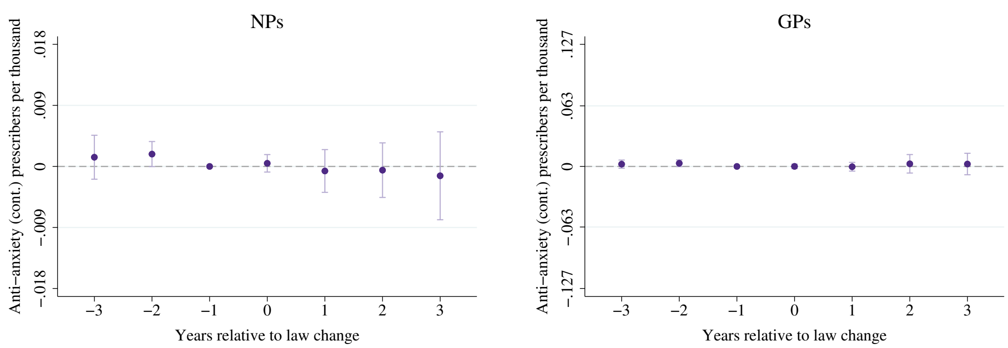
Notes: The above figures present coefficients and 95% confidence intervals from estimation of equation (3) using county-year–level data for 2006–2018. Each coefficient comes from a separate regression in which the outcome is an alternative definition of the number of “frequent” prescribers of a given type per 1,000 people; the left (right) subfigures consider the number of NPs (GPs). “Frequent” is defined as both (1) writing a given type of prescription in each month (or year for opioid-benzo. co-prescribing) and (2) being above the xth percentile of prescribing among all GPs who satisfy criterion (1), where x is defined on the x-axis. As in our primary analysis, these figures consider the effects 0–3 years after the law change in the 11 states with law changes between 2009–2015. The regressions include county and year fixed effects; time-varying, state-level controls for changes in independent prescriptive authority for controlled substances outside of the balanced panel window and changes in non-independent prescriptive authority for controlled substances; and all time-varying, county-level controls listed in Figure A1. Standard errors are clustered by state. Outcome data come from the IQVIA LRx database.

Figure A4: Effects on number of controlled substance prescribers

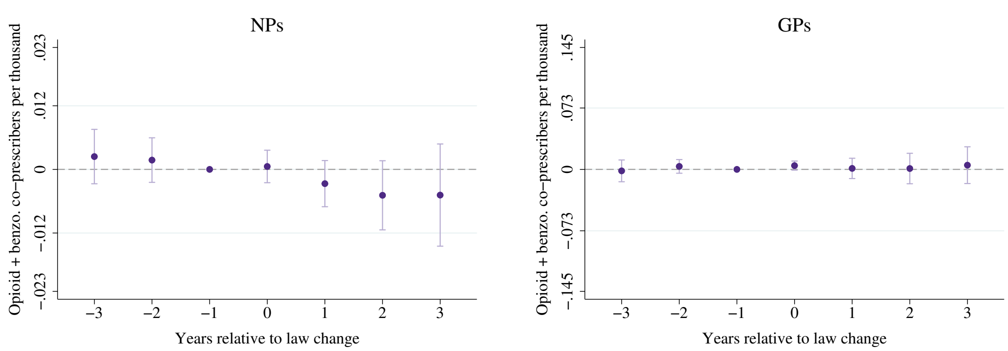
(a) Opioid prescribers



(b) Controlled anti-anxiety prescribers



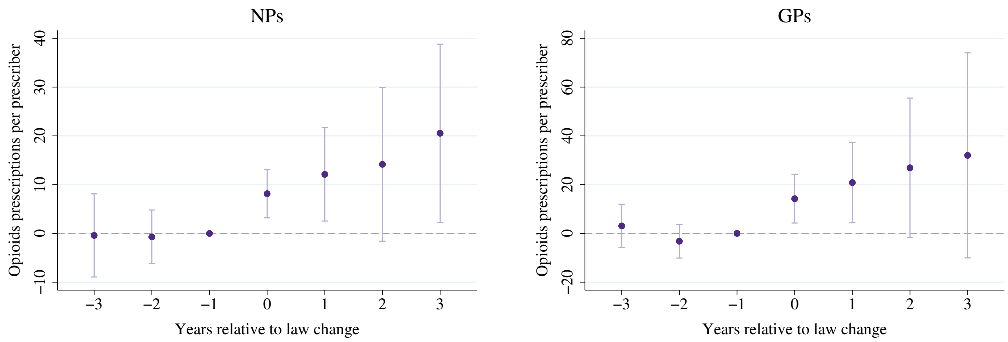
(c) Opioid + benzo. co-prescribers



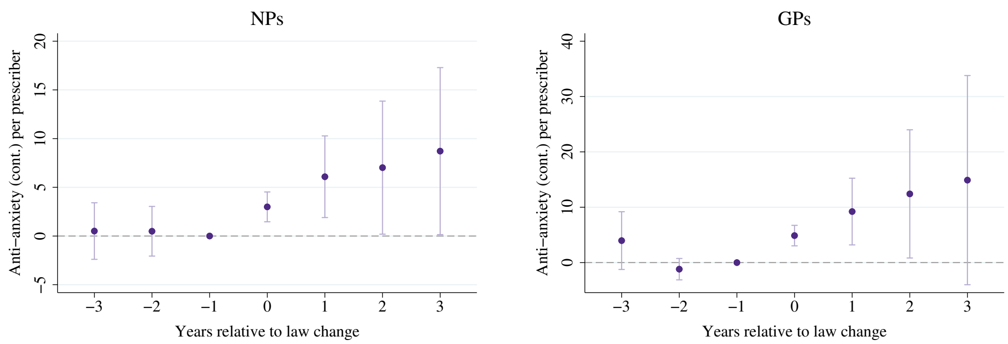
Notes: The above figures present coefficients and 95% confidence intervals from estimation of equation (2) using county-year–level data for 2006–2018. The left (right) subfigure in each subplot considers nurse practitioners [NPs] (physicians in general practice [GPs]). Outcomes are the number of prescribers per 1,000 people of opioids (subfigure (a)), anti-anxiety controlled substances (subfigure (b)), and co-prescriptions of opioids and benzodiazepines (subfigure (c)). In subfigures (a) and (b), prescribers are required to write the given prescription type at least once per month in a given year to be included. To make effect sizes more comparable with other figures, the y-axes are scaled to range from −33 to +33 percent of the baseline mean of each outcome. To allow for a balanced panel, these figures consider effects in the 11 states with law changes between 2009–2015. The regressions include county and year fixed effects; county-specific linear time trends; time-varying, state-level controls for changes in independent prescriptive authority for controlled substances outside of the balanced panel window and changes in non-independent prescriptive authority for controlled substances; and all time-varying, county-level controls listed in Figure A1. Standard errors are clustered by state. Outcome data come from the IQVIA LRx database.

Figure A5: Effects on average annual prescriptions per prescriber

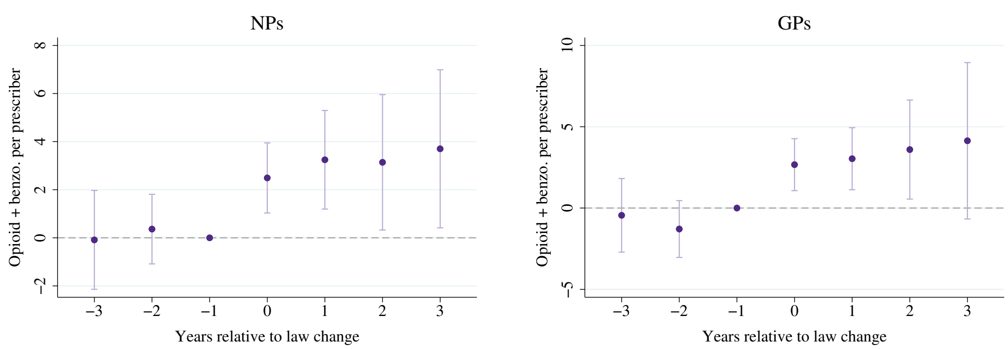
(a) Opioid prescriptions



(b) Controlled anti-anxiety prescriptions



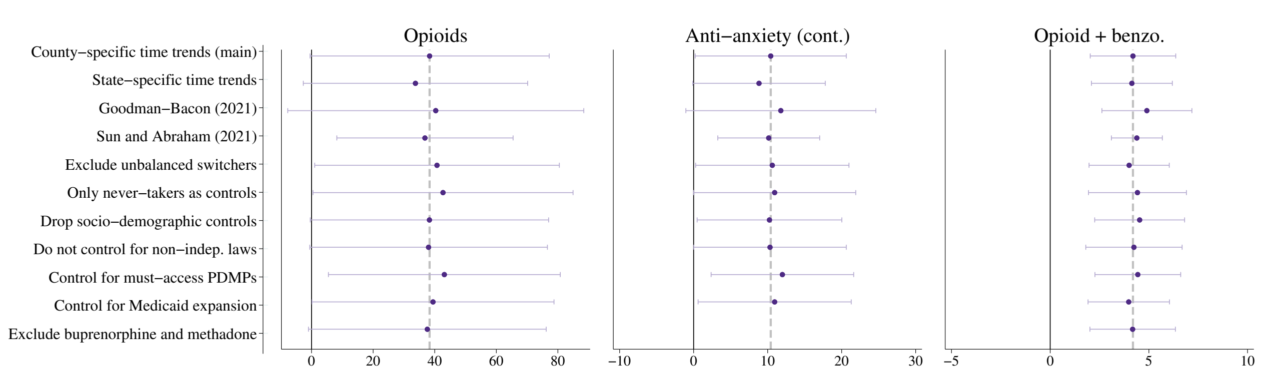
(c) Opioid + benzo. co-prescriptions



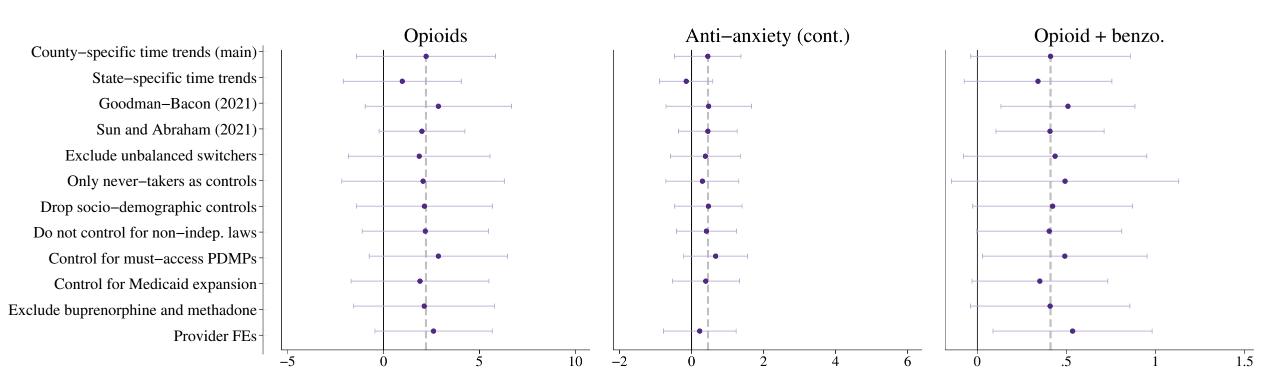
Notes: The above figures present coefficients and 95% confidence intervals from estimation of equation (2) using county-year–level data for 2006–2018. The left (right) subfigure in each subplot considers nurse practitioners [NPs] (physicians in general practice [GPs]). Outcomes are the average annual number of prescriptions per prescriber of opioids (subfigure (a)), anti-anxiety controlled substances (subfigure (b)), and co-prescriptions of opioids and benzodiazepines (subfigure (c)). To allow for a balanced panel, these figures consider effects in the 11 states with law changes between 2009–2015. The regressions include county and year fixed effects; county-specific linear time trends; time-varying, state-level controls for changes in independent prescriptive authority for controlled substances outside of the balanced panel window and changes in non-independent prescriptive authority for controlled substances; and all time-varying, county-level controls listed in Figure A1. Standard errors are clustered by state. Outcome data come from the IQVIA LRx database.

Figure A6: Effects on controlled substance prescribing: Robustness

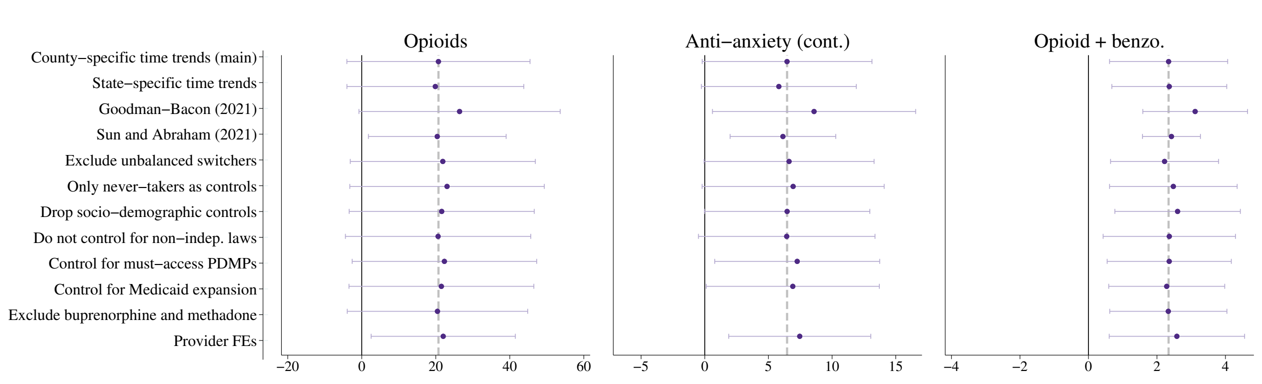
(a) All providers



(b) Nurse practitioners



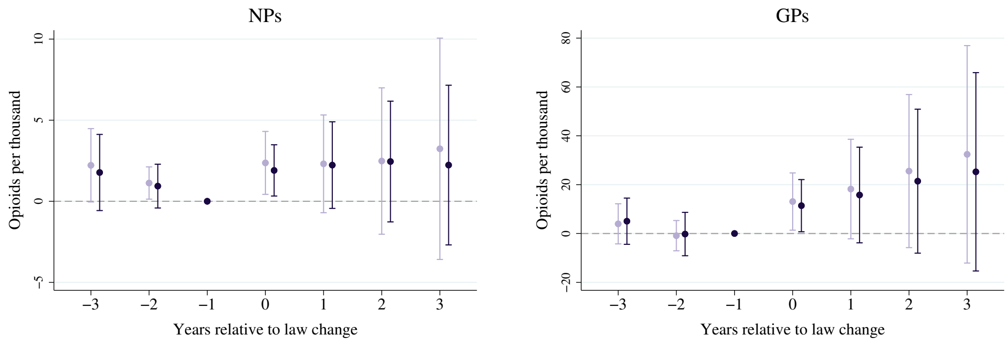
(c) General practice physicians



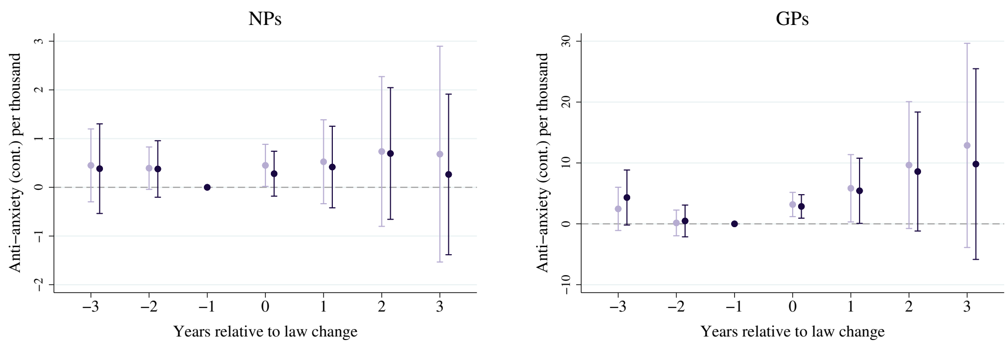
Notes: The above figures present coefficients and 95% confidence intervals from estimation of analogues of equation (3) using county-year–level data for 2006–2018. Each row presents output from a separate regression using the specification denoted on the y-axis. Outcomes are the number of prescriptions of a given type per 1,000 people written by all providers (panel (a)), nurse practitioners (panel (b)), and physicians in general practice (panel (c)). As in our primary analysis, these figures consider the effects 0–3 years after the law change in the 11 states with law changes between 2009–2015. The dashed vertical line in each subfigure displays the coefficient estimate from our baseline specification (as reported in Table 3). Our baseline specification includes county and year fixed effects; county-specific linear time trends; time-varying, state-level controls for changes in independent prescriptive authority for controlled substances outside of the balanced panel window and changes in non-independent prescriptive authority for controlled substances; and all time-varying, county-level controls listed in Figure A1. Standard errors are clustered by state. Outcome data come from the IQVIA LRx database.

Figure A7: Effects of controlled substance prescribing: Excluding Medicaid patients

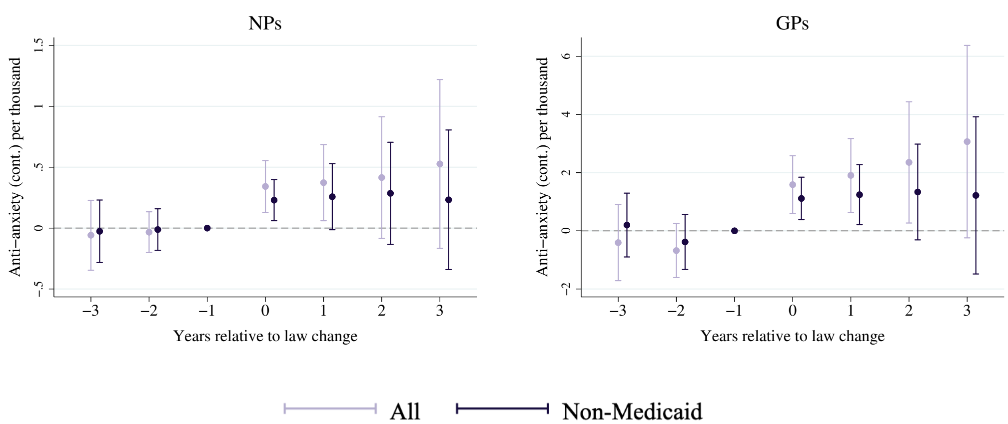
(a) Opioid prescriptions



(b) Controlled anti-anxiety prescriptions



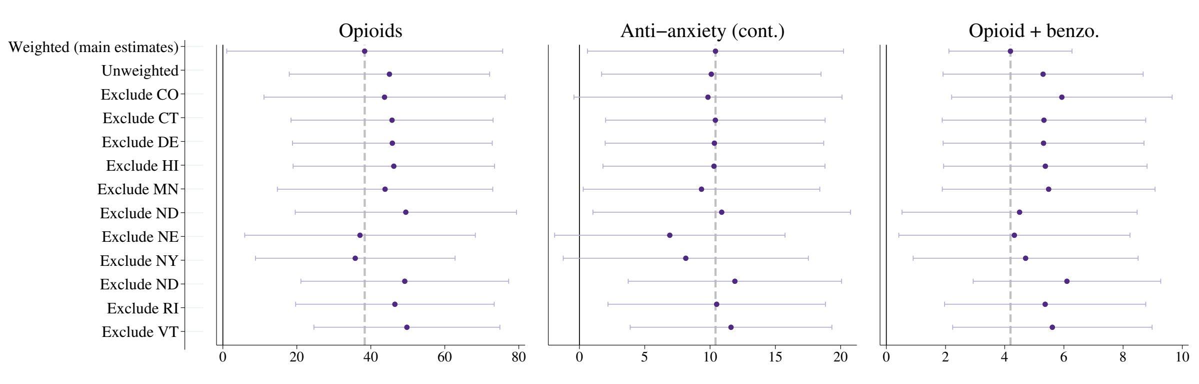
(c) Opioid + benzo. co-prescriptions



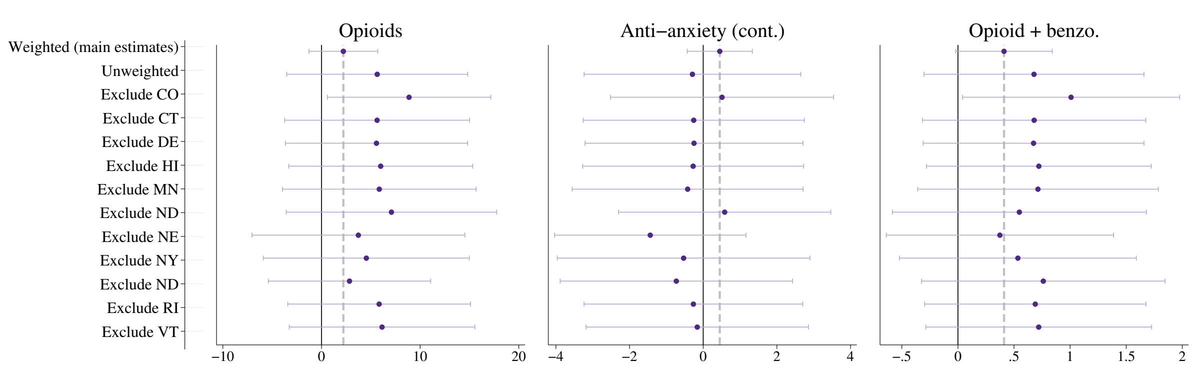
Notes: The above figures present coefficients and 95% confidence intervals from estimation of equation (2) using county-year–level data for 2006–2018. The left (right) subfigure in each subplot only considers prescriptions written by nurse practitioners [NPs] (physicians in general practice [GPs]). Outcomes are the number of opioid prescriptions per 1,000 people (subfigure (a)), the number of anti-anxiety controlled substance prescriptions per 1,000 people (subfigure (b)), and the number of instances in which an opioid and benzodiazepine prescription were written for the same patient by the same provider on the same day ("co-prescriptions") per 1,000 people (subfigure (c)). These outcomes are shown both for all prescriptions (light dots and lines) and for prescriptions paid for by payers other than Medicaid (dark dots and lines). To allow for a balanced panel, these figures consider effects in the 11 states with law changes between 2009–2015. Standard errors are clustered by state. Outcome data come from the IQVIA LRx database.

Figure A8: Effects on controlled substance prescribing: Dropping each treatment state

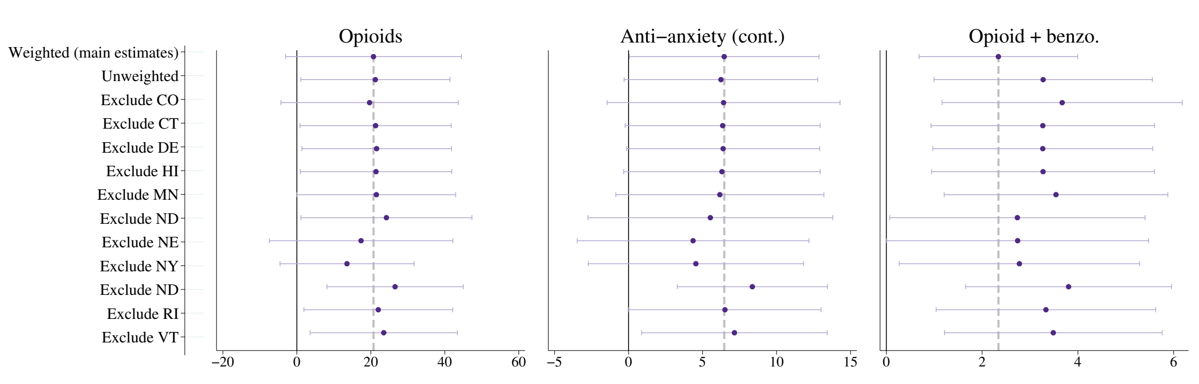
(a) All providers



(b) Nurse practitioners



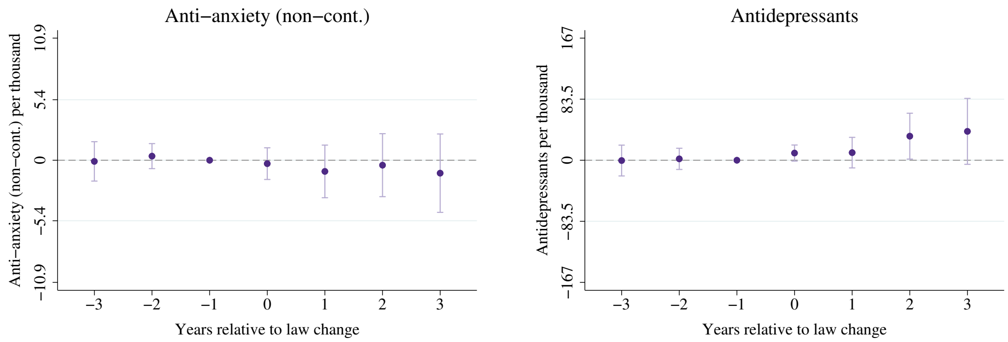
(c) General practice physicians



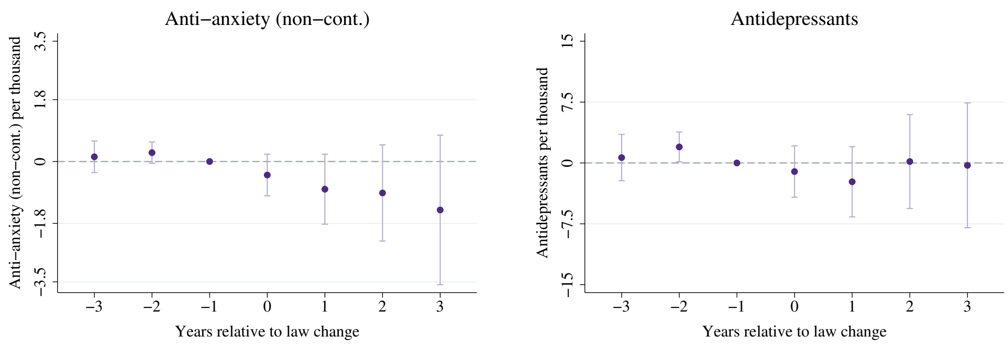
Notes: The above figures present coefficients and 95% confidence intervals from estimation of equation (3) using county-year–level data for 2006–2018. Each row presents output from a separate regression using the specification denoted on the y-axis. Outcomes are the number of prescriptions of a given type per 1,000 people written by all providers (panel (a)), nurse practitioners (panel (b)), and physicians in general practice (panel (c)). The dashed vertical line in each subfigure displays the coefficient estimate from our baseline specification (as reported in Table 3); this specification includes all 11 treatment states in the balanced panel window and weights observations by population. All other specifications in the figure are unweighted. The regressions include county and year fixed effects; county-specific linear time trends; time-varying, state-level controls for changes in independent prescriptive authority for controlled substances outside of the balanced panel window and changes in non-independent prescriptive authority for controlled substances; and all time-varying, county-level controls listed in Figure A1. Standard errors are clustered by state. Outcome data come from the IQVIA LRx database.

Figure A9: Effects on non-controlled substance prescribing (IQVIA data, 2006–2018)

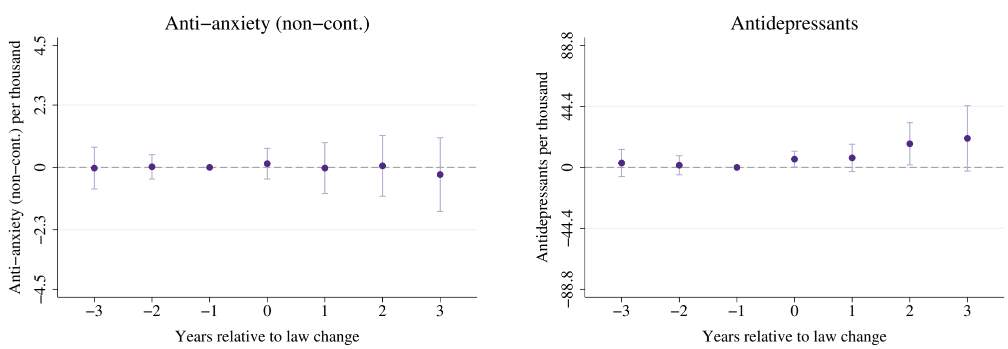
(a) All providers



(b) Nurse practitioners



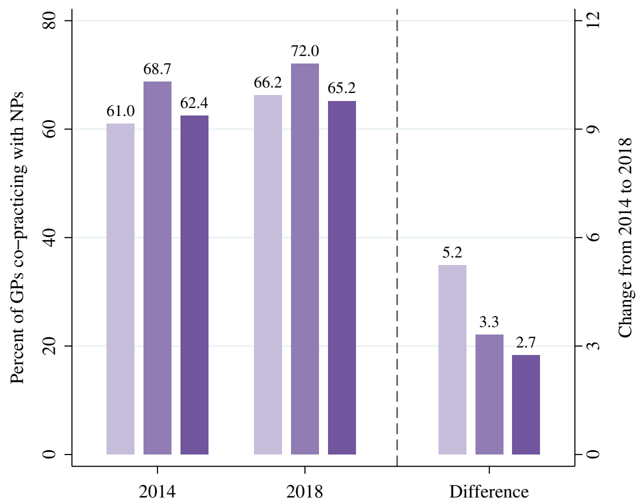
(c) General practice physicians



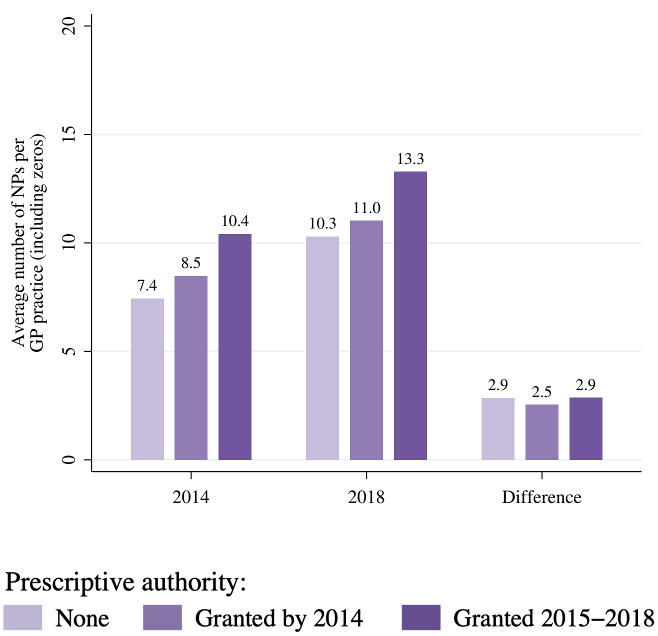
Notes: The above figures present coefficients and 95% confidence intervals from estimation of equation (2) using county-year–level data for 2006–2018. The outcome in the left (right) subfigure in each subplot is the number of prescriptions for non-controlled anti-anxiety medications (antidepressants) per 1,000 people written by all providers (subfigure (a)), nurse practitioners (subfigure (b)), and physicians in general practice (subfigure (c)). To make effect sizes more comparable with Figure 6, the y-axes are scaled to range from −33 to +33 percent of the baseline mean of each outcome; the one exception is non-controlled anti-anxiety prescribing among NPs, for which the y-axis ranges from -100 to +100 percent of the baseline mean. To allow for a balanced panel, these figures consider effects in the 11 states with law changes between 2009–2015. Standard errors are clustered by state. The regressions include county and year fixed effects; county-specific linear time trends; time-varying, state-level controls for changes in independent prescriptive authority for controlled substances outside of the balanced panel window and changes in non-independent prescriptive authority for controlled substances; and all time-varying, county-level controls listed in Figure A1. Outcome data come from the IQVIA LRx database.

Figure A10: Co-practice patterns of NPs and GPs in 2014 and 2018

(a) Percent of GPs co-practicing with NPs

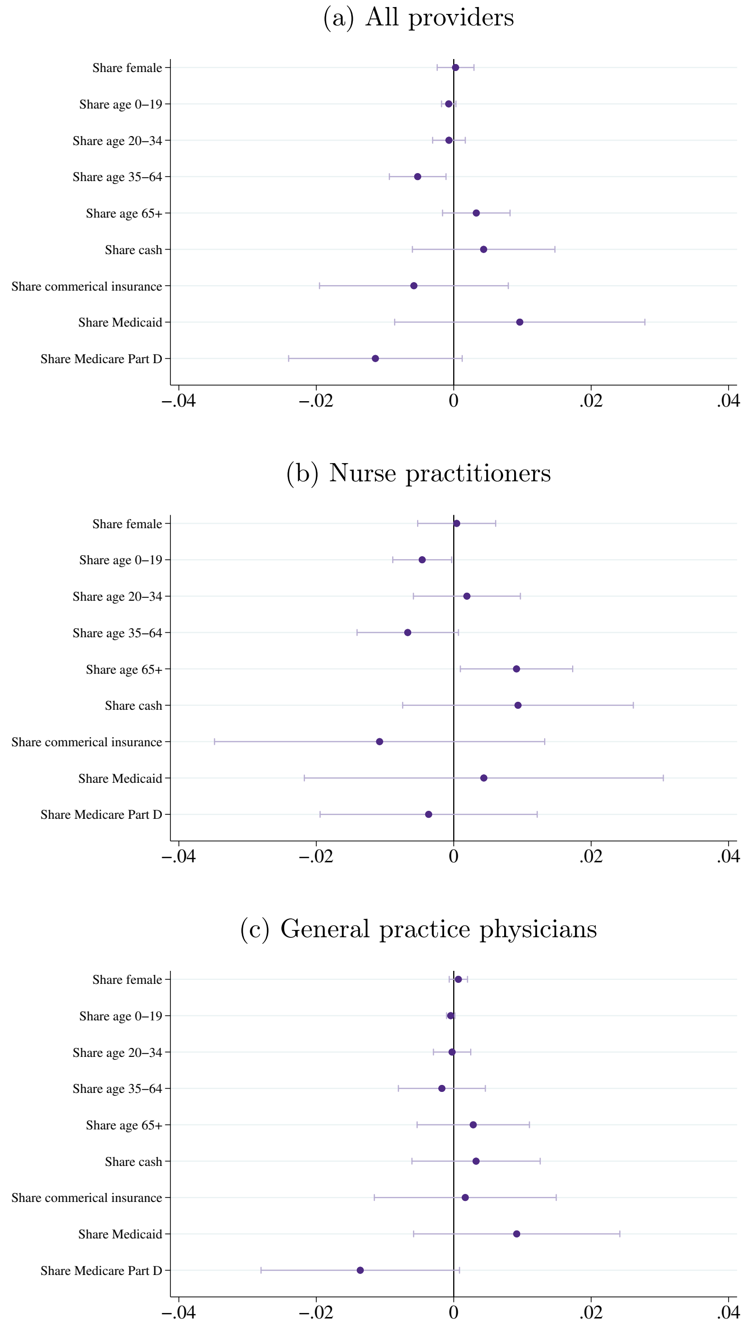


(b) Average number of NPs per GP practice



Notes: The above figures report co-practice patterns among nurse practitioners (NPs) and physicians in general practice (GPs) in states that did not allow NPs to independently prescribe controlled substances by 2018 (light purple), states that granted NPs independent prescriptive authority for controlled substances by 2014 (medium purple), and states that granted NPs the ability to independently prescribe controlled substances between 2015 and 2018 (dark purple). Subfigure (a) shows the population-weighted average of county-year level shares of GPs who were observed practicing in the same clinic as at least one NP; subfigure (b) shows the population-weighted average of the county-year number of NPs who were observed practicing in the same clinic as each GP (including zeros). In both subfigures, the left (middle) panel presents results for 2014 (2018); the right panel shows the difference between the two. We exclude the six states that granted NPs the ability to prescribe controlled substances non-independently between 2006 and 2018 from these figures. Outcome data come from the location snapshots provided by IQVIA and include the exact practice addresses for all providers in the IQVIA data in 2014 and 2018.

Figure A11: Effects on patient composition of controlled substance prescriptions



Notes: The above figures present coefficients and 95% confidence intervals from estimation of analogues of equation (3) using county-year–level data for 2006–2018. Each row presents output from a separate regression using the outcome denoted on the y-axis. As in our primary analysis, this figure considers the effects 0–3 years after the law change in the 11 states with law changes between 2009–2015. The regressions include county and year fixed effects; time-varying, state-level controls for changes in independent prescriptive authority for controlled substances outside of the balanced panel window and changes in non-independent prescriptive authority for controlled substances; and time-varying, county-level controls outside of the outcome domain listed in Figure A1. Standard errors are clustered by state. Outcome data come from the ACS.

Table A1: Effects of NP independent prescriptive authority on fatal drug overdoses

Fatal overdoses per 1,000,000:	Any opioid (1)	Prescription opioids (2)	Benzodiazepines (3)	Prescription opioid + benzo. (4)
Post law change, 1–3 years	13.008	10.303	8.574	8.283
	(17.366)	(4.183)	(5.734)	(5.341)
	[0.457]	[0.017]	[0.141]	[0.127]
Baseline mean	68.21	47.16	21.01	17.84
Relative to mean	0.191	0.218	0.408	0.464
Observations	40,911	40,911	40,911	40,911

Notes: The above table reports coefficients, standard errors (in parentheses), and p-values [in brackets] from estimation of an analogue of equation (3) using county-year–level data for 2006–2018. Outcomes are the number of fatal drug overdoses per 1,000,000 people involving any opioid (column (1)), prescription opioids (column (2)), benzodiazepines (column (3)), and prescription opioids in combination with benzodiazepines (column (4)). To allow for a balanced panel, this table considers effects in the 11 states with law changes between 2009–2015. Because the event studies in Figure 5 suggest that any mortality effects take at least a year following the law changes to surface, we report estimates for years 1–3 rather than years 0–3 as for the prescription outcomes. The regressions include county and year fixed effects; time-varying, state-level controls for changes in independent prescriptive authority for controlled substances outside of the balanced panel window and changes in non-independent prescriptive authority for controlled substances; and all time-varying, county-level controls listed in Figure A1. The baseline mean is measured as the average across all counties in 2010. Standard errors are clustered by state. Outcome data come from the NVSS database.

B Alternative micro-foundation: demand inducement

In Section II, we introduced a model of physician behavior that can rationalize an increase in prescribing among physicians following an increase in competition. This framework formalized the idea that the elasticity of patient demand to service use is increasing in competition; as such, physician behavior shifts toward the preferences of marginal patients in the presence of increased competition to retain demand.

Alternative models of physician behavior can also be used to micro-found our finding that increased competition leads to increases in prescribing of certain medications. Notably, models of demand inducement likewise deliver this result. In these models, the effect operates through an income effect: When competition increases, physicians lose patients, thereby reducing their income. Given diminishing marginal utility of income, physician utility is more responsive to changes in income at lower levels of income, and thus, inducing demand—which is assumed to have a constant marginal cost—is now more appealing. Competition therefore increases optimal demand inducement, putting upward pressure on service provision.

We formalize this intuition below in a standard model of physician-induced demand. In particular, we present a framework that closely follows the one outlined in Gruber and Owings (1996) and McGuire (2000) but that is framed for the case of prescription opioids. We only discuss prescription opioids for simplicity, though the same model holds for addictive anti-anxiety drugs and other controlled substances.

Following the literature on physician-induced demand, suppose that physician utility is given by $U = U(Y, I)$, where Y is income and I is demand inducement. In the case of prescription opioids, I can be thought of as inducing demand for prescription opioids among patients who would be better off with some other treatment. We assume that utility is increasing in income ($U_Y > 0$) at a decreasing rate ($U_{YY} < 0$), while utility is decreasing in demand inducement ($U_I < 0$) at a decreasing rate ($U_{II} < 0$). Let the number of patients that a doctor treats at baseline be given by N, and let $\alpha(I)$ be the fraction of patients who are prescribed opioids. Since prescribing is increasing in demand inducement, we have that $\alpha_I > 0$. We further assume that $\alpha_{II} = 0$, $U_{YI} = 0$, $U_{IY} = 0$.

Let R_{OP} be the full revenue associated with treatment including prescription opioids, and let R_{noOP} be the full revenue associated with treatment that does not include opioids. Since it is often simpler and less time consuming to prescribe opioids to a patient rather than providing some other treatment, we assume that $R_{OP} > R_{noOP}$.[39] Moreover, although we are not explicitly modeling the dynamics, R_{OP} will further exceed R_{noOP} if prescribing opioids increases the probability that patients return for future visits (e.g., for refills).

Physicians choose the level of inducement to maximize their utility subject to a budget constraint. The physician's problem can therefore be written as:

$$\max_{I} U(Y, I) \;\; s.t. \; Y = N \cdot (R_{OP} \cdot \alpha(I) + R_{noOP} \cdot (1 - \alpha(I))) \, .$$

Assuming that utility is separable in income and inducement, taking the derivative with respect to I and setting it equal to zero yields the following the first-order condition:

$$[I] \quad U_Y \cdot N \cdot \alpha_I \cdot (R_{OP} - R_{noOP}) + U_I = 0.$$

This first-order condition shows that the physician decides how much demand to induce by trading off the utility from additional income that prescribing opioids provides against the disutility of inducing demand.

Now, suppose that NPs are granted independent prescriptive authority for controlled substances. Since some patients will now find it preferable to see an NP, N goes down for a given physician. Fully differentiating the first-order condition and rearranging, we obtain:

$$\frac{\partial I}{\partial N} = -\frac{1}{U_{II}} \alpha_I \left(R_{OP} - R_{noOP}\right) U_Y \left(\frac{U_{YY} Y}{U_Y} + 1\right).$$

It is reasonable to assume that the absolute value of the elasticity of marginal utility with respect to income, $\frac{U_{YY} Y}{U_Y}$, is greater than one.[40] In this case, $\frac{U_{YY} Y}{U_Y} + 1 < 0$ and $\frac{\partial I}{\partial N} < 0$. Therefore, as N goes down, physicians induce more demand for prescription opioids. Although

[39] To see this, consider a patient with lower back pain. If the physician decides to prescribe opioids, the provider can quickly write a prescription and move on to the next patient. If the doctor instead decides to focus on non-opioid treatment, an alternative treatment regime might involve counseling the patient to lose weight or coordinating with other providers to incorporate physiotherapy, cognitive behavioral therapy, and other interventions into the patient's treatment program.

[40] For example, Layard et al. (2008) estimate that the elasticity of marginal utility with respect to income ranges from 1.19 to 1.34 using surveys covering over 50 countries between 1972 and 2005.

physicians may dislike prescribing unnecessary opioids (i.e., they experience disutility from inducing demand), a drop in their revenue resulting from increased competition increases the marginal utility of revenue sufficiently to increase such prescribing.

C Provider practice locations

Our extract of the IQVIA data contains an exact practice address for each provider in 2014 and 2018. However, our empirical design requires that we know the county of each prescriber in each year over our 13-year sample (2006–2018). We therefore designed and implemented a location assignment algorithm that uses information on the zip codes of the patients who filled the prescriptions written by each provider in each year to infer the county of each provider annually. The idea behind the algorithm is simple: if, for example, a provider predominately writes prescriptions for patients in Baltimore County, Maryland, but then begins writing prescriptions predominately for patients in Cook County, Illinois, then we assume that the provider moved from Baltimore to Chicago when the locations of her patients changed.

Our location assignment algorithm is implemented as follows. First, for each provider-month, we calculate the share of the provider's total prescriptions across all three of the drug classes included in our data extract (opioids, anti-anxiety medications, antidepressants) that were filled by patients in each zip code. Starting with the zip code with the highest share of prescriptions for that provider, we then add additional zip codes in order of descending prescription shares until we have a set of zip codes covering at least 90 percent of the provider's prescriptions in that month.[41] We call this starting set of zip codes the provider's "monthly practice area."

To determine provider moves, we then compare the monthly practice area in month t to the monthly practice area in month $t-2$.[42] We say that a move potentially occurred between month t and month $t-2$ if there is no overlap between the set of zip codes in the monthly practice areas across these two months. We use a two-period lagged comparison group to

[41] We select zip codes covering 90 percent of prescriptions, rather than only choosing the zip code with the highest share, to avoid having providers "flip-flop" between zip codes across months. For example, suppose that a provider wrote 60 (40) percent of her prescriptions for patients in zip code A (B) in month 1, 40 (60) percent of her prescriptions for patients in zip code A (B) in month 2, and 60 (40) percent of her prescriptions for patients in zip code A (B) in month 3. If we only considered the zip code with the highest share of prescriptions, it would appear as if the provider moved from zip code A to zip code B and then back to zip code A. Rather, the provider was serving a consistent area throughout—a pattern that is accurately captured with our 90 percent threshold.

[42] If a given provider wrote zero prescriptions in month $t-2$, then the monthly practice area in month $t-2$ is not defined. When this occurs, we compare the monthly practice area in month t to the monthly practice area in month $t-x$, where $x > 2$ is the unique x such that (1) the provider wrote zero prescriptions in months $t-x+1$ through $t-2$ and (2) the provider wrote a positive number of prescriptions in month $t-x$.

account for the fact that mid-month moves will result in prescriptions being written to patients in both the origin and destination locations in the month of the move. For example, suppose that a provider wrote 60 (40) percent of her prescriptions for patients in zip code A (B) in month $t-2$, 30 (20) (30) (20) percent of her prescriptions for patients in zip code A (B) (C) (D) in month $t-1$, and 60 (40) percent of her prescriptions for patients in zip code C (D) in month t. If we compared the monthly practice areas in periods t and $t-1$ and periods $t-1$ and $t-2$, we would determine that the provider did not move (since there is always some overlap in the set of zip codes in these adjacent period comparisons). Rather, the provider likely moved from an area with zip codes A and B to an area with zip codes C and D in period $t-1$, a pattern which is accurately captured with our two-period lagged comparison group.

With the months of potential moves identified, we then redefine time spells to be periods between moves rather than months. That is, if a provider was writing prescriptions for patients in overlapping monthly practice areas (as defined above) in months t_1 through t_n, but then began writing prescriptions for patients in a new set of overlapping monthly practice areas in months t_{n+1} through t_N, then we would define months t_1 through t_n as one spell and months t_{n+1} through t_N as another. We call this starting set of spells the provider's "initial spell set."

Below, we assign a specific location to each provider-spell by taking the zip code with the highest share of the provider's prescriptions across that spell. In principle, the most frequent zip code could be the same across two consecutive spells for the same provider. As this is inconsistent with the idea that the provider moved between spells, we iterate on the above procedure until the zip code with the highest share of the provider's prescriptions at the spell level differs across consecutive spells for the same provider.

In particular, after identifying the initial spell set for each provider as outlined above, we determine the set of zip codes needed to cover 90 percent of each provider's prescriptions within each spell. We then compare the practice area in spell t to the practice area in spell $t-1$ and say that a move occurred between these spells if there is no overlap between the set of zip codes in these spell-level practice areas. If a move did not occur between two spells, we merge the spells in question, calculate the practice area for this new spell, and compare the new spell's practice area to the practice area of the spell a period before. We iterate on this procedure—that is, redefining spells, defining spell-level practice areas, and identifying potential moves—until there is no overlap in the practice areas of consecutive spells. This ensures that the zip code with the highest share of prescriptions in each provider-spell changes across identified moves. We use a zip code to county crosswalk provided by the U.S. Department of Housing and Urban Development to assign counties to the most frequent zip code in each provider-spell and use this county as the provider's location for the period covered by the spell.[43]

We can compare the practice counties that we assign to providers in 2014 and 2018 using our algorithm to the practice counties provided by IQVIA in the same years.[44] These snapshots of addresses from IQVIA are the company's best assessment of each provider's location in each of these years based on information from various sources. Reassuringly, our algorithm assigns the same county (state) as IQVIA for 66.6 (89.7) percent of providers in 2018. Unsurprisingly, our algorithm is more accurate for more frequent prescribers, with 76.4 (94.8) percent of prescriptions in 2018 being written by providers whose county (state) we assign in accordance with the IQVIA data. A similar pattern is observed in 2014, with our location assignment algorithm assigning the same county (state) as IQVIA for 53.5 (73.0) percent of providers and 64.8 (81.9) percent of prescriptions.

Comparing our constructed panel of provider locations to one constructed from the National Plan and Provider Enumeration System (NPPES)—a data source that is commonly used to track provider locations over time—suggests that physician moves are significantly underreported in the NPPES.[45] Using our location assignment algorithm, we find that among

[43] The crosswalk is available here: https://www.huduser.gov/portal/datasets/usps_crosswalk.html.

[44] We can further compare the practice counties that we infer in 2018 using our location assignment algorithm to those provided in the 2018 AMA Masterfile, an input into IQVIA's 2018 location snapshot. Physicians are added to the AMA Masterfile when they receive their medical education number; practice locations among physicians who have since moved will therefore be outdated unless the provider choses to update their information with the AMA, and there is little incentive to do so. Our algorithm identifies the same county (state) of practice for 54.2 (84.7) percent of the 84.4 percent of physicians in the IQVIA data who can be linked to the 2018 AMA Masterfile.

[45] Another source of data that is commonly used to identify provider locations is the Centers for Medicare and Medicaid Services' "Physician Compare" database. While these data come from billing records and therefore should in principle have updated address information for providers, it unfortunately only includes a subsample of providers. For example, only 49.3 percent of providers in the IQVIA data in 2018 are also in Physician Compare.

the 94.7 percent of providers in the IQVIA data who can be linked to the NPPES, an average of 13.6 (6.4) percent moved counties (states) annually over the periods 2008–2013 and 2015–2018 (the years for which the NPPES is available through NBER). Among the same set of providers and years in the NPPES, annual cross-county (cross-state) moves are reported for an average of only 4.4 (2.5) percent of providers. This underreporting of provider moves in the NPPES is perhaps not surprising given that providers enter the NPPES when they apply for a National Provider Identifier (NPI) and have little reason to update their location information subsequently. Nevertheless, it highlights the limitations of the NPPES and motivates our use of a data-driven location assignment algorithm.

D Comparison to McMichael (2020)

This section provides a comparison of estimates using our methods to those of McMichael (2020) to investigate the reasons why our conclusions are different than his.

Recall that our work considers the impacts of law changes granting NPs independent prescriptive authority for controlled substances on the prescribing of controlled and non-controlled substances from 2006 to 2018. Information on the law changes that we use comes from McMichael and Markowitz (2023), and our primary prescription data come from IQVIA. When examining effects on opioid prescribing, we consider five primary outcomes at the county-year level: (1) number of opioid prescriptions per 1,000 people (Table 3), (2) number of opioid prescribers per 1,000 people (Table 3), (3) average annual opioid prescriptions per opioid prescribing provider (Table 3), (4) average days supplied per opioid prescription (Table 4), and (5) average MME per day supplied (Table 4). We estimate models with county and year fixed effects and find significant positive effects of the law changes on all outcomes among GPs except for the number of prescribing providers and the average days supplied per prescription (for which we find negative but insignificant effects).

In contrast, McMichael (2020) considers the impacts of law changes granting NPs full practice authority on the prescribing of opioids from 2011 to 2018. He uses self-collected data on the years of the law changes and prescription data from the proprietary Symphony prescription drug database. Like the IQVIA data, the Symphony data cover the near universe of prescriptions filled at retail pharmacies. McMichael considers four opioid-prescribing outcomes at the provider-year level: (1) ln(total MME + 1), (2) ln(total days supplied + 1), (3) ln(opioid patients + 1), and (4) an indicator denoting whether the provider prescribed any opioids. Estimating models with provider, state, and year fixed effects, he finds significant negative effects of the law changes on all outcomes among physicians (Table A1, panel (c)).

Our analysis therefore differs from that of McMichael (2020) in terms of the treatment, sample period, data, outcome measures, and specification. We aim to determine which of these factors help explain the difference between our findings. To do so, column (1) of Table A2 begins by reproducing the estimates reported in McMichael (2020). We focus on his first three outcomes, as those are the outcomes for which there is the greatest difference between his estimates and our own.

Column (2) then replicates McMichael's analysis in the IQVIA data. We consider the three outcomes from column (1) as well as three additional outcomes that are in a similar spirit but are more closely aligned with the outcomes that we use in our analyses.[46] The baseline means for annual opioid patients per provider (panel (b)) and annual days supplied per provider (panel (d)) match McMichael's well, suggesting that the two databases are similar. However, while we find effects in panels (b) and (d) that are negative as in McMichael (2020), the effects using McMichael's law changes and specification in the IQVIA data are substantially smaller and less precise than those reported in McMichael (2020).[47] This discrepancy may stem from the fact that the IQVIA sample contains over two million (approximately 25 percent) more physician-year observations than the Symphony data over the period 2011–2018.

Moreover, we obtain a different signed estimate for MME per provider-year in panel (f), and the baseline means differ by two orders of magnitude between columns (1) and (2). This is likely due to an error in the calculation of MME per provider-year in McMichael (2020): as outlined on page 952 in the Technical Appendix, McMichael calculates "MME per provider-year" by aggregating average MME per day supplied at the prescription level within provider-year cells. Rather, total MME per provider-year requires aggregating total MME per prescription within provider-year cells.[48]

The remaining columns of Table A2 show the impact of additional incremental changes moving between McMichael's analysis and our own. Comparing columns (2) and (3) shows

[46]The estimates reported in panels (c) and (e) of column (10) of Table A2 reproduce those first reported in column (4) of Table 4 of this paper. The estimate reported in panel (a) of column (10) of Table A2 is similar to that reported in column (7) of Table 3, except that we include GPs with zero opioid prescriptions when calculating opioid prescriptions per provider in Table A2 to more closely reflect the measures used in McMichael (2020).

[47]We obtain similar precision to McMichael (2020) when we cluster standard errors at the provider level. As the state is the level of treatment, it is important to allow for correlation in the errors of observations from the same state. We therefore cluster our standard errors by state throughout.

[48]To investigate this issue further, we obtained information on MME shipments at the county level from ARCOS for 2006–2014. These data were unsealed as part of multi-district litigation against opioid manufacturers, wholesalers, and pharmacies and are only available for those years (see https://www.slcg.com/opioid-data). The ARCOS data report that the total MME in 2011 was nearly 350 billion. A mean MME per provider of 0.008 million (as reported by McMichael, 2020) would imply that the number of physician-years in 2011 (i.e., the number of physicians in 2011) was nearly 45 million (350 billion divided by 8,000). Given that there are only approximately a million physicians practicing at any given point in time, this number is not possible.

that when we use law changes granting NPs independent prescriptive authority for controlled substances from McMichael and Markowitz (2023) as in our analysis (rather than full practice authority from McMichael, 2020), the signs of the effects on the first three outcome measures also flip to align with our primary findings. This is perhaps surprising: allowing NPs to independently prescribe controlled substances is typically the final legislative change required to allow NPs to practice without any restrictions, and thus the law changes for independent controlled substance authority and full practice authority should largely overlap. This is confirmed by McMichael and Markowitz (2023), who report years of full practice authority that align with those for independent controlled substance prescribing in most states. Comparing the law changes used in McMichael (2020) to those reported in McMichael and Markowitz (2023) shows that McMichael updated the years of full practice authority after his sole-authored 2020 publication, and thus we believe that the law changes used in our work more accurately capture the legislative environment surrounding NPs' scope of practice.

Columns (4)–(10) show what happens when we make additional changes to the specification. In addition to the set of laws considered, two other changes make a noticeable impact on the findings. First, moving from columns (3) to (4), we see that the effects are typically larger (in percent terms) and more precise when the outcome is specified in levels as in our analysis rather than $ln(x + 1)$ as in McMichael (2020). Work in applied econometrics shows that transforming the outcome by $ln(x + c)$ for some constant c can be problematic because the choice of c is not determined by theory and can have a large influence on the point estimates (Mullahy and Norton, 2022). This difficulty in working with provider-level data with many zeros in part motivated our decision to focus primarily on county-level aggregates, although a comparison of columns (9) and (10) shows that the level of aggregation makes remarkably little difference to the results when the outcome is specified in levels. Second, moving from columns (4) to (5), we see that the effects on all outcomes are larger in percent terms when we focus on GPs (as in our analysis) rather than on all physicians (as in McMichael, 2020). This mirrors the patterns shown in Table 6 and, as outlined in Section V.A, is consistent with the fact that NPs are more of a competitive threat to GPs than to physicians in most other specialties.

The remainder of the columns show that other differences have limited impacts on the findings. Controlling for county fixed effects rather than state fixed effects (column (6) versus column (5)) leads to very similar results. Moreover, considering additional law changes using data for 2006–2018 rather than 2011–2018 (column (7) versus column (6)), using a balanced panel of states (column (8) versus column (7)), and controlling for time trends (column (9) versus column (8)) does not meaningfully change the conclusions. Finally, as noted above, the results are remarkably consistent whether we estimate variants of our primary specification with physician fixed effects in physician-level data or county fixed effects in county-level data (column (9) versus column (10)).

Table A2: Effects on opioid prescribing: Comparison with McMichael (2020)

	McMichael (2020)	*Incremental specification changes*								CLS (2023)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
a. Opioid prescriptions per provider										
Post law change		–0.008	0.022	9.512	17.548	17.535	7.390	8.669	20.902	21.619
		(0.030)	(0.024)	(5.949)	(9.729)	(9.835)	(7.108)	(6.608)	(10.231)	(10.971)
		[0.796]	[0.357]	[0.116]	[0.077]	[0.081]	[0.303]	[0.196]	[0.046]	[0.054]
Baseline mean		175.5	175.5	175.5	186.3	186.3	183.5	183.5	183.5	195.0
Relative to mean		–0.008	0.022	0.054	0.094	0.094	0.040	0.047	0.114	0.111
b. Opioid patients per provider										
Post law change	–0.028	–0.009	0.001	2.799	4.421	4.414	2.432	2.808	3.860	4.274
	(0.004)	(0.029)	(0.020)	(1.562)	(2.138)	(2.173)	(1.651)	(1.482)	(1.837)	(2.120)
	[<0.01]	[0.771]	[0.967]	[0.079]	[0.044]	[0.048]	[0.147]	[0.064]	[0.041]	[0.049]
Baseline mean	59	63.47	63.47	63.47	58.55	58.55	57.29	57.29	57.29	60.91
Relative to mean	–0.028	–0.009	0.001	0.044	0.076	0.075	0.042	0.049	0.067	0.070
c. Average days supplied per prescription										
Post law change		0.009	–0.033	–0.368	–0.472	–0.483	–0.562	–0.652	–0.152	–0.110
		(0.022)	(0.020)	(0.201)	(0.331)	(0.332)	(0.393)	(0.421)	(0.165)	(0.133)
		[0.694]	[0.109]	[0.073]	[0.160]	[0.153]	[0.160]	[0.128]	[0.359]	[0.414]
Baseline mean		10.12	10.12	10.12	10.82	10.82	10.40	10.40	10.40	10.46
Relative to mean		0.009	–0.033	–0.036	–0.044	–0.045	–0.054	–0.063	–0.015	–0.010
d. Days supplied per provider (in thousands)										
Post law change	–0.038	–0.001	–0.011	0.041	0.216	0.215	–0.097	–0.076	0.453	0.448
	(0.006)	(0.000)	(0.000)	(0.090)	(0.156)	(0.157)	(0.131)	(0.110)	(0.195)	(0.202)
	[<0.01]	[0.985]	[0.755]	[0.654]	[0.171]	[0.176]	[0.462]	[0.496]	[0.024]	[0.031]
Baseline mean	2.741	3.223	3.223	3.223	3.738	3.738	3.599	3.599	3.599	10.46
Relative to mean	–0.038	–0.001	–0.011	0.013	0.058	0.058	–0.027	–0.021	0.126	0.043
e. Average MME per day supplied										
Post law change		0.029	0.190	77.493	67.927	67.808	77.600	76.162	28.831	26.274
		(0.079)	(0.103)	(37.329)	(40.574)	(40.482)	(56.414)	(57.899)	(9.087)	(8.638)
		[0.716]	[0.071]	[0.043]	[0.100]	[0.100]	[0.175]	[0.194]	[0.003]	[0.004]
Baseline mean		448.6	448.6	448.6	359.8	359.8	376.6	376.6	376.6	388.0
Relative to mean		0.029	0.190	0.173	0.189	0.188	0.206	0.202	0.077	0.068
f. Total MME per provider (in millions)										
Post law change	–0.060	0.028	0.180	0.280	0.507	0.509	0.552	0.556	0.287	0.254
	(0.007)	(0.000)	(0.000)	(0.100)	(0.203)	(0.204)	(0.217)	(0.228)	(0.109)	(0.096)
	[<0.01]	[0.809]	[0.136]	[0.007]	[0.016]	[0.016]	[0.014]	[0.018]	[0.011]	[0.011]
Baseline mean	0.008	1.354	1.354	1.354	1.571	1.571	1.575	1.575	1.575	388.0
Relative to mean	–0.060	0.028	0.180	0.207	0.323	0.324	0.351	0.353	0.182	0.001
Observations	6,910,111	8,945,508	8,945,508	8,945,508	3,133,596	3,133,575	5,113,536	5,113,536	5,113,536	40,911
Outcome data	Symphony	IQVIA	IQVIA	IQVIA	IQVIA	IQVIA	IQVIA	IQVIA	IQVIA	IQVIA
NP practice authority	Full	Full	Cont. Rx	Cont. Rx	Cont. Rx	Cont. Rx	Cont. Rx	Cont. Rx	Cont. Rx	Cont. Rx
Outcome specification	Log(x+1)	Log(x+1)	Log(x+1)	Level	Level	Level	Level	Level	Level	Level
Physician sample	All	All	All	All	GPs	GPs	GPs	GPs	GPs	GPs
Geographic FEs	State	State	State	State	State	County	County	County	County	County
Physician FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	No
Sample years	2011–18	2011–18	2011–18	2011–18	2011–18	2011–18	2006–18	2006–18	2006–18	2006–18
Balanced panel	No	No	No	No	No	No	No	Yes	Yes	Yes
Linear time trends	No	No	No	No	No	No	No	No	Yes	Yes
Observation level	Phy.-Yr.	Phy.-Yr.	Phy.-Yr.	Phy.-Yr.	Phy.-Yr.	Phy.-Yr.	Phy.-Yr.	Phy.-Yr.	Phy.-Yr.	Cty.-Yr.

Notes: Standard errors (p-values) reported in parentheses (brackets). Column (1) reproduces the estimates reported in McMichael (2020). He considers the impacts of law changes granting NPs full practice authority on opioid prescribing among all physicians at the provider-year level over 2011–2018 in the Symphony data; his model specifies outcomes as ln(x+1) and includes provider, state, and year fixed effects. Column (10) presents output from estimation of our primary specification (equation (3)). We consider the impacts of law changes allowing NPs to independently prescribe controlled substances on opioid prescribing among GPs at the county-year level over 2006–2018 in the IQVIA data; we specify outcomes in levels and include county and year fixed effects. Columns (2)–(9) show the impacts of incremental changes moving between these analyses.