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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 authority to prescribe controlled substances without physician oversight generate exogenous increases in competition by expanding patients' options when seeking care. In response, we find that general practice physicians (GPs)—the physician specialty that competes most directly with NPs—significantly increase their prescribing of opioids and controlled anti-anxiety medications. GPs also increase their co-prescribing of opioids and benzodiazepines, a practice that violates prescribing guidelines. These effects are more pronounced in areas with more NPs per GP at baseline and lead to sizable increases in fatal drug overdoses. In contrast, we observe no changes in prescribing among physician specialties that do not compete with NPs, nor in the prescribing of drug classes not directly affected by the law changes. 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 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. 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 such as opioids, an authority that was previously only granted to physicians.[2] This policy change is often the final step in enabling NPs to practice fully independently of physicians.

Physician groups such as the American Medical Association (AMA) have vociferously opposed these law changes. While the AMA argues that allowing NPs to practice independently endangers patient safety and fails to improve access—since NPs "tend to practice in the same areas that physicians do" (AMA, 2024)—this opposition may also be motivated by a desire to shield physicians from increased competition. Patients often view GPs and NPs as substitute suppliers of primary care, and thus granting NPs the authority to prescribe

[1]Competition is a hallmark of the U.S. health care system and is also pronounced in Germany, the Netherlands, and France (Siciliani et al., 2022). Propper (2018) notes that much of the work on policies intended to increase competition focuses on England, with reforms in other countries receiving less scrutiny.

[2]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 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. Controlled (or "scheduled") drugs are federally regulated in the United States under the Controlled Substances Act because they are generally addictive and carry a risk of fatal overdose.

controlled substances without physician involvement heightens competition for GPs seeking to attract the many patients who seek care for conditions such as pain that can be treated with controlled substances (Case and Deaton, 2017; Cutler and Glaeser, 2021).[3] Although physicians can compete for these patients in various ways, increasing the prescribing of controlled substances is a particularly salient strategy to attract and retain patients seeking immediate—though potentially risky—relief (Zgierska et al., 2012; Ho, 2019).

We analyze comprehensive prescription data from IQVIA 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 facilitates abuse and is strongly advised against by the Centers for Disease Control and Prevention (CDC) prescribing guidelines (CDC, 2016). These results are observed at both the area level and in physician-level analyses, indicating that existing physicians adjust their prescribing in response to the law changes with aggregate implications for the provision of care. Moreover, the increases in prescribing are accompanied by increases in fatal drug overdoses, underscoring the potential harm to patient health from competition-induced changes in physician behavior. 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. As the number of clinicians who can provide a given service increases in a market, each clinician sees fewer patients all else equal (i.e., the provider-specific demand curve shifts inward) and the number of patients that the provider attracts becomes more responsive to the level of service provision (i.e., the provider-specific demand curve becomes more elastic). In response, providers cater more to patient demand to avoid losing too many patients, even when doing so reduces their per-patient utility. Hence, 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.

Whether competition increases or decreases service provision therefore depends on whether patients want more or less of the service in question. Clinicians frequently report that pa-

[3] Studies show that NPs and physicians provide similar quality of care in general practice settings (Mundinger et al., 2000), with patients often reporting equal or higher satisfaction after seeing an NP (Htay and Whitehead, 2021). These facts suggest that GPs may be concerned about losing patients to NPs once they can offer the same services. Consistent with this concern, we find that the number of Medicare patients seen by GPs declines after the law changes.

tient satisfaction suffers when they refuse to prescribe controlled substances, as many patients seek fast and easy relief from their symptoms even when providers believe that the risks of the medication may outweigh the medical benefits (Frantsve and Kerns, 2007; Zgierska et al., 2012; NYTimes, 2016). Hence, we anticipate that 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 the findings are driven by increased competition. First, the observed increases in physician prescribing are higher in areas with more NPs per GP at baseline. That is, when NPs are allowed to prescribe independently, GPs respond more strongly in areas in which they are subject to greater competition from NPs. 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 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, we show that allowing NPs to independently prescribe controlled substances reduces 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 following the law changes.[4] Moreover, balancing regressions show that the law changes have no consistent effects on the patient age, gender, 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. Finally, public-use Open Payments data show that pharmaceutical payments to GPs remained stable around the law changes, indicating that our findings are not driven by changes in pharmaceutical marketing.

The results on opioid prescribing are particularly important considering the ongoing opioid crisis in the United States. We conduct two additional analyses to shed additional light on how competition affects opioid prescribing. First, focusing on patients who did

[4] 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 controlled substances. We further show that the law changes do not affect the share of GPs practicing with NPs or the number of NPs per GP practice.

not receive an opioid prescription in the past six months, we find that competition-induced increases in opioid prescribing are driven by prescriptions to these "opioid-naïve" patients. As many patients seek primary care for pain management (Case and Deaton, 2017; Cutler and Glaeser, 2021), and many non-naïve patients receive refills independent of the competitive environment, this increase among opioid-naïve patients is to be expected if competition leads physicians to become more lenient in their prescribing. 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 clinicians 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.[5] 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. (2022) 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).[6] We complement this work by showing that increased competition among clinicians can likewise have perverse effects, leading to increases in prescribing that are likely welfare reducing.

Second, this paper adds to a smaller literature examining the effects of competition among physicians and its impact on physician-induced demand (McGuire, 2000). Given limited variation in 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

[5]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.

[6]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).

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 correlated 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 both theoretically and empirically how increased competition can lead to a deterioration in clinical quality when patients desire services that can cause medical harm.

Third, this paper contributes to the large literature on clinical decision-making. Numerous studies document substantial heterogeneity in service provision across health care providers (e.g., Currie et al., 2016; Parys, 2016; Currie and Zhang, 2023; Gowrisankaran et al., 2023; Ginja et al., 2024). 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), professional boundaries (Chan and Chen, 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), 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 affects physicians' prescribing of controlled substances.

Finally, our paper relates to a growing literature examining how changes in scope-of-practice legislation for NPs affect patient care. As outlined in a recent overview by McMichael and Markowitz (2023), much of this literature focusses on the impacts on patient access and health using either aggregate or patient-level data.[7] For example, Traczynski and Udalova (2018) document that allowing NPs to both practice and prescribe independently increases utilization of primary care services, while Alexander and Schnell (2019) show that allowing NPs to independently prescribe unscheduled drugs (including most antidepressants) improves mental health. We add to this work by examining how changes in competition induced by changes in scope-of-practice legislation for NPs affect the behavior of physicians.

[7] 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 E, there are a number of differences between our analysis and his that fully account for the differences in findings.

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

II Theoretical framework

This section presents a theoretical framework to examine how increased competition—through the entry of additional providers offering a given service—affects the intensity of services delivered by incumbent physicians. A key feature of this framework is the incentive structure many physicians face: the most common employment contract for primary care physicians includes a salary plus a bonus based on the number of patient encounters and/or the volume of services provided (Singleton and Miller, 2021; AMN Healthcare, 2024). As a result, physicians must maintain patient volume to sustain their compensation. At the same time, prices are typically negotiated in advance—or, in the case of payers like Medicare, set administratively—leaving little room for increased provider supply to affect consumer prices in the short run. We therefore focus on how increased competition influences the quantity of services provided, holding prices fixed.

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 prefer 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 patients demand (e.g., because of addiction, resale value, or the possibility of immediate pain relief) but that physicians may prefer to limit (e.g., because additional prescribing conflicts with their views of medically appropriate care). In both cases, physician behavior shifts toward the preferences of the marginal patient when an increase in the number of suppliers intensifies competition to attract and retain patients. Whether increased competition leads incumbent providers to increase or decrease their service provision therefore depends on whether physicians are over- or under-providing care from the perspective of the marginal patient at baseline.

II.A Baseline model

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).[8] 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.[9] For simplicity, we assume that $N_{xx} = u_{xx} = 0$.

The physician chooses her optimal level of service intensity to maximize her total utility. The physician's problem can therefore be written as:

$$\max_x \; N(x) \cdot u(x).$$

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

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

Equation (1) shows that the physician decides on the optimal level 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 utility per patient 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 involving interior solutions therefore occur when the incentives of patients

[8] If all patients are identical, x 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.

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

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 (i.e., if $N_x > 0$ and $u_x < 0$).

II.B Competitive effects of law changes

How does allowing NPs to independently prescribe controlled substances affect competition for patients? As outlined below, allowing NPs to prescribe without physician involvement affects the physician-specific patient demand curve, $N(x)$, through two channels.[10]

First, the law changes affect the level of demand. Since allowing NPs to independently prescribe controlled substances is often the final step in allowing NPs to practice fully autonomously, and because patients often view NPs and GPs as substitutes for primary care, the law changes effectively increase the number of providers in the market. All else equal, a greater number of providers reduces the number of patients per provider, shifting the demand curve facing each physician inward—that is, decreasing $N(x)$. Although physician behavior will respond in equilibrium, as outlined below, this inward shift in the demand curve weakly decreases the number of patients seen by GPs. We confirm in Section V that the law changes reduce the number of office visits with GPs.

Second, the law changes affect the elasticity of demand. Increasing the number of providers makes the demand curve facing each physician more sensitive to service intensity, as patients can more easily switch to other providers if they do not find their current provider sufficiently accommodating. The law changes thus also rotate the physician-specific patient demand curve upward (i.e., it becomes steeper), increasing $\mid N_x \mid$ all else equal.

Physicians' equilibrium responses Allowing NPs to independently prescribe controlled substances heightens competition among clinicians, reducing the level and increasing the elasticity of the physician-specific demand curve, $N(x)$. Both of these forces serve to increase

[10] These competitive effects will be experienced by physicians who practice independently as well as those who operate in group practices. Since NPs can establish their own practices and compete directly with physicians for patients once they have the statutory authority to practice and prescribe independently, the impacts of the law changes on the level and elasticity of demand facing physicians practicing in clinics composed only of other physicians are clear. However, even when physicians operate in group practices that employ NPs, competition for patients intensifies when NPs become legal substitutes for physicians. When NPs are supervised by physicians, their services are often credited to the collaborating physician under a practice called "indirect billing" (Patel et al., 2022). In contrast, when NPs operate independently, physicians often do not receive credit for these patients, making it more challenging to meet their practice's patient volume expectations. As a result, once NPs become legally substitutable for physicians, competition between clinicians increases, regardless of whether they practice in the same or in separate clinics.

the magnitude of the left-hand side of equation (1). As a result, either $N(x^*)$ must increase or $u(x^*)$ must decrease for the first-order condition to remain satisfied, causing physicians to adjust their optimal service intensity x^* in equilibrium. Below, we consider how physicians' optimal level of service intensity changes in cases with interior solutions.

Suppose first that physicians receive higher per-patient utility by increasing service intensity ($u_x > 0$) but additional service intensity loses them patients ($N_x < 0$). In this case, an increase in competition leads providers to reduce x^*, thereby raising patient demand but lowering the utility that they receive per patient. That is, for services that marginal patients do not want (e.g., because the costs outweigh the 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.[11]

Now suppose that patients desire additional service intensity ($N_x > 0$) that physicians do not want to provide for a given patient ($u_x < 0$). In this case, an increase in competition instead leads providers to increase x^*, raising $N(x^*)$ back toward its previous level at the expense of the utility that the physician receives per patient. 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.[12]

How will physicians adjust their controlled substance prescribing in response to increased competition? If patients demand more controlled substance prescriptions than clinicians are willing to provide at baseline, the framework predicts that physicians will increase their prescribing following the law changes.[13] Whether this occurs is ultimately an empirical question, but existing evidence suggests that there is no shortage of patients who might

[11] This logic is consistent with 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. More recently, Cooper et al. (2025) show that C-section rates rise when hospitals acquire physician practices, increasing consolidation among obstetricians.

[12] As patients likely want clinicians to certify their sick leave, this case can also be used to explain Brekke et al. (2019)'s finding that competition increases the issuance of sickness certificates.

[13] 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 but (2) does not want to prescribe more at the margin. Nevertheless, some marginal patients—for example, those with low pain but high tastes for opioids—will want additional prescriptions.

want a controlled substance prescription. First, many patients seek medical care for pain relief (Case and Deaton, 2017; Cutler and Glaeser, 2021). Although there are a number of ways in which clinicians can ease patients' pain, doctors report that many patients are not interested in alternatives to opioids for pain management and are dissatisfied when denied opioid prescriptions (Frantsve and Kerns, 2007; Zgierska et al., 2012; Onishi et al., 2017). Second, many patients turn to illegal secondary markets to purchase controlled substances (Schnell, 2017; SAMHSA, 2020). Because prices on these markets typically exceed those of legal prescriptions—and purchasing medications on the secondary market carries legal risks—many of these individuals would likely prefer to obtain a cheaper, legal prescription from a clinician if they could. In Section IV, we test and confirm the prediction that controlled substance prescribing by physicians increases in the presence of heightened competition.

Heterogeneity in responses If certain provider groups experience larger inward shifts or upward rotations in their demand curves following the law changes, we would expect them to adjust their practice styles more strongly in response to the changing competitive environment. In Section V.A, we examine heterogeneity in responses across locations, physician types, and medication types for which physician-specific demand curves are expected to be differentially affected. Specifically, allowing NPs to independently prescribe controlled substances is expected to lead to larger changes in the effective number of providers in areas with more NPs at baseline, among patients seeking care in specialties where NPs frequently practice, and among patients seeking treatment for conditions that can be managed with controlled substances. Accordingly, if the findings reflect equilibrium responses to heightened competition, we expect the largest changes in physician behavior to occur in areas with a higher concentration of NPs at baseline, among physicians in specialties that compete closely with NPs, and in the prescribing of controlled substances relative to other services.

Welfare implications An important question is whether competition-induced changes in physician behavior enhance patient welfare. Because competition leads providers to cater more to patient demand, patient surplus should rise. Moreover, if physicians were under-providing necessary care at baseline—for example, by prioritizing revenue-generating procedures over patient-centered services or by favoring leisure over effort—then greater competition should also improve patient health. However, if patients desire treatments that are harmful from a medical perspective, such as excessive medications or procedures, competition could worsen health outcomes and increase unnecessary service use even as patient satisfaction rises. Although a full welfare analysis is beyond this paper's scope, we examine impacts on mortality in Section IV.C to assess implications for patient health.

Alternative mechanisms and frameworks Although the theory outlined above can rationalize both our findings and previous results in the literature, alternative models of physician behavior can also micro-found the prediction that increased competition leads to greater prescribing of controlled substances. Appendix B presents a model of demand inducement that can likewise deliver this result (Gruber and Owings, 1996; McGuire, 2000). If increased competition raises the marginal utility of income by reducing the number of patients per provider, then physicians should respond to increased competition by providing more of all services, regardless of patient preferences. Such a mechanism could rationalize our findings but is inconsistent with findings of higher competition leading to lower service intensity in other work (Markowitz et al., 2017; Cooper et al., 2025).[14]

Moreover, physicians might respond to increased competition by spending more time with patients, thereby allowing them to identify additional ailments requiring treatment. Such a response is consistent with the framework outlined above if patients derive utility from time spent with the clinician. It is also consistent with an alternative framework in which increasing the number of providers relaxes physicians' time constraints. However, as shown in Section V.B, we find no increases in the prescribing of non-controlled substances after the law changes, and additional patient time would be expected to uncover conditions requiring non-controlled as well as controlled substances. Finally, if pharmaceutical marketing to physicians rises following the law changes, and additional marketing raises physicians' utility from prescribing, then prescribing should increase. However, pharmaceutical payments to physicians remained stable around the law changes.[15]

[14] More precisely, in a demand-inducement framework, additional competition leads providers to shift toward services that they find more profitable than alternative treatment options (see Appendix B). Prescribing controlled substances can be more profitable than alternative services for at least four reasons. First, patients on these medications often require ongoing monitoring, which generates more billable visits (CDC, 2016). Second, even absent the need for medication management, patients may be more likely to return to clinicians who accommodate their preferences (Frantsve and Kerns, 2007). Third, physician compensation may be directly tied to patient satisfaction, incentivizing more liberal prescribing (Van Zee, 2009; Zgierska et al., 2012). Finally, managing these medications can justify higher billing—for example, using CPT code 99214 rather than 99213—due to increased patient complexity (AMA, 2023).

[15] In Section V.B, we also examine whether the composition of patients seen by GPs is affected by the law changes, since increased prescribing could reflect greater patient severity. We find no evidence of such sorting and note that such a pattern would be hard to rationalize: allowing NPs to do more should, if anything, lead to a more even distribution of patient severity across providers rather than GPs seeing more severe patients.

III Data

We use four main data sources to assess how changes in competition influence physician prescribing and patient health.[16] The key features of these data are detailed below.

III.A IQVIA data

The primary prescription data 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 2006–2018.[17]

Three features of these data are important for the analyses. First, these data include a provider identifier and information on each provider from the AMA. We use the provider identifiers to construct outcomes such as the number of prescribers and the total number of prescriptions per prescribing provider. We further use information on each provider's specialty to examine impacts separately on the prescribing of NPs, GPs, and other physician types that are differentially exposed to competition from NPs.[18]

Second, these data include an (anonymized) patient identifier and basic patient information such as location and age. We use the patient identifiers to identify patients who are starting new medications ("naïve" patients) and to measure instances of co-prescribing of different 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.[19] Information on patient characteristics such

[16] We supplement these data with population at the county-year level from the five-year American Community Surveys (ACS). The data for 2007–2018 are available here: https://www.socialexplorer.com/explore-tables. We use a linear extrapolation to impute population for 2006.

[17] IQVIA directly surveys most retail pharmacies, long-term care homes, and mail-order drug suppliers and 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.

[18] 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.

[19] 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 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. 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

as age, gender, and insurance type is also used to examine the effects of the law changes on the composition of patients who receive prescriptions.

Finally, these 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.[20] 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 such medications. Hence, our primary analyses focus on the prescribing of opioids and scheduled anti-anxiety medications like benzodiazepines.[21] 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 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.[22]

Table 1 provides an overview of the number of unique providers and the total number of prescriptions across controlled drug types observed in our data. The over 1.5 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. As shown in Table A1, prescriptions for controlled anti-anxiety medications increased from 2006 to 2018; in contrast, prescriptions for opioids

sources that motivate our use of a data-driven location assignment algorithm.

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

[21] 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 A5 that our results are not sensitive to dropping methadone and buprenorphine prescriptions.

[22] 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.

Table 1: Number of prescribers and prescription shares by provider type: 2006–2018

	Unique providers (1)	Controlled substance prescription shares: Opioids (2)	Anti-anxiety (3)	Opioid + benzo. (4)
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

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) from 2006–2018; prescription shares are calculated relative to these totals. Table A1 reports corresponding statistics separately for 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.

increased nationally from 2006 to around 2010 and have since been trending downward.

Table 1 also reports the share 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 largest share of prescriptions among all specialties. This prominence reflects both the large number of GPs and their high prescribing rates per provider relative to other specialties. Despite being unable to prescribe independently in many state-years over our sample period, NPs also account for a substantial share of prescriptions: NPs accounted for the third-highest share of opioid prescriptions from 2006 to 2018, behind GPs and orthopedic surgeons, and the third-highest share of controlled anti-anxiety prescriptions, behind GPs and psychiatrists/neurologists. The sizable role played by NPs is due in large part to the rapid growth in the number of prescribing NPs, with the number of NPs prescribing these drug classes nearly quadrupling from 2006 to 2018 (see Table A1).

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 competitive landscape 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.[23] This legal change often removes the final barrier to NPs 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 the study period (2006–2018), 16 states relaxed their scope-of-practice restrictions and granted NPs the ability to prescribe these medications without physician involvement. 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.[24]

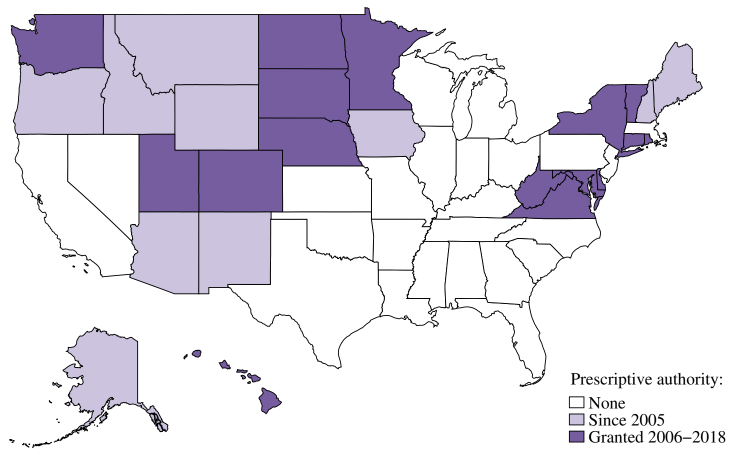
Before NPs are granted independent prescriptive authority for controlled substances, NPs can typically prescribe the medications with physician collaboration or oversight. As of 2005, 45 states allowed NPs to prescribe controlled substances *non*-independently, with the remaining six states updating 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 only 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 A2 provides an overview of prescribing patterns among GPs and NPs across states. The number of prescriptions per 1,000 people written by NPs was 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 higher in both

[23] 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 Markowitz (2023) note that "regulations 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.

[24] Nevertheless, it is worth noting that as of 2018, no state in the West South Central or East South Central Census Divisions allowed NPs to independently prescribe controlled substances.

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 23 for additional details.

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. Because professional nursing organizations and health policy advocates have pushed for scope-of-practice reform nationwide, the success and timing of these policy changes largely reflect idiosyncrasies in state-level political processes rather than differences in local health needs or market conditions. As such, previous work has shown that changes in prescriptive authority for NPs are uncorrelated with changes in local health care needs and market characteristics (Alexander and Schnell, 2019). To further 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. Accordingly, estimation results are nearly identical regardless of whether socio-demographic controls are included.

[25] In particular, we estimate analogs of equation (3) introduced in Section IV.B.

III.C Medicare data

Additional outcomes come from two data sets covering services provided to the nearly 20 percent of the U.S. population enrolled in Medicare, the public health insurance program that primarily serves the elderly.[26] In addition to capturing prescription and practice outcomes not available in our extract of the IQVIA data, a key advantage of the Medicare data is that prices are set centrally and cannot be adjusted in response to changes in the level or slope of physician-specific demand curves.

To examine impacts on additional drug classes that are not included in our 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).[27] 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 diuretics. The first four drug classes are used to validate our findings obtained using the IQVIA data using an alternative source, while the later four are used to extend the analysis to additional non-controlled substances. We combine these data with county-year–level information on the number of individuals aged 65 and older from the ACS to construct measures of prescriptions per capita among the Medicare population.

In addition to prescribing behavior, we also examine impacts on the frequency of office visits and the severity of patients seen by GPs using the Medicare Part B files. These data are also publicly available from CMS and again cover the period 2012–2018.[28] 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 Medicare prescriptions, we combine these counts with relevant population data from the ACS to construct office visits with GPs per capita

[26] Although we do not have access to comparable data for patients with other types of insurance, our primary IQVIA data cover all prescriptions regardless of insurance coverage or type. Moreover, since reports of pain and opioid use are particularly high in the Medicare population, Medicare data have frequently been used to examine the causes and consequences of opioid prescribing (see, e.g., Meara et al., 2016; Barnett et al., 2017; Buchmueller and Carey, 2018; Finkelstein et al., 2025).

[27] CMS 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 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.

[28] 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.

among the Medicare population. Moreover, to measure patient severity, we use information on the average annual risk scores of beneficiaries seen by each provider in the Part B files. These scores are assigned by CMS based on patient demographics and diagnoses and are used for payment adjustments and performance measurement. We consider both the unweighted and the beneficiary-weighted average across GPs in a county to capture 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 for 2006–2018. These data include 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 how competition affects the prescribing practices of physicians, we leverage changes in scope-of-practice legislation granting NPs the ability to prescribe controlled substances independently. By increasing the number of providers in a given area who compete with physicians to attract and retain patients, these law changes serve as a shock to the competitive landscape. As outlined in Section II.B, we expect physicians to respond by becoming more accommodating to patient demand, increasing their prescribing of controlled substances. This section presents our main empirical analyses of the impacts of the law changes on controlled substance prescribing by NPs and GPs. Section V then examines a number of supplementary outcomes—including prescribing by other physician specialties, non-controlled substance prescribing, co-practice patterns, the number of office visits, and patient composition—to shed light on the mechanisms underlying our main results.

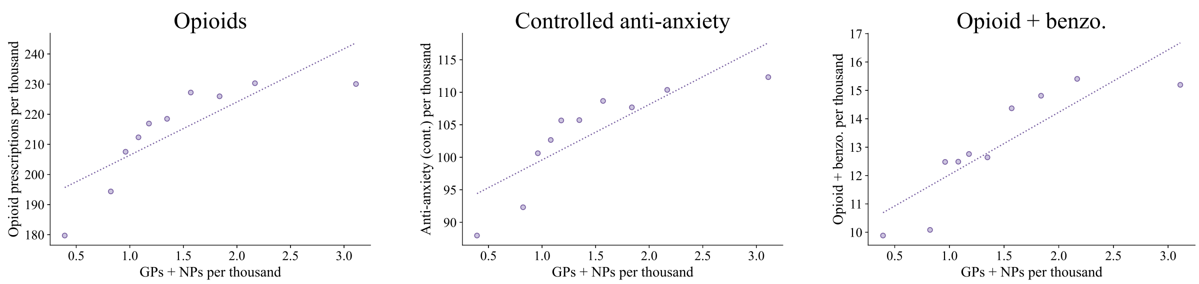
IV.A Graphical evidence

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. For each medication type, we consider the number of prescriptions per 1,000 people written by GPs and NPs and the average number of prescriptions written by each prescribing GP at the county-year level. Observations are residualized from county and year fixed effects and grouped into deciles based on the number of GPs and NPs per 1,000 people, with the number of NPs set to zero until NPs are allowed to independently prescribe controlled substances.

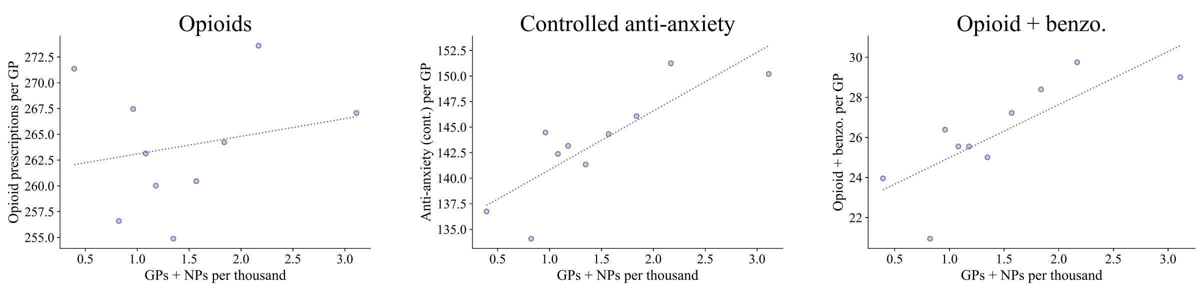
The subfigures show a positive relationship between within-county changes in the number of prescribers per capita and the number of opioid prescriptions, controlled anti-anxiety prescriptions, and opioid and benzodiazepine co-prescriptions per capita and per prescribing

Figure 2: Relationship between number of prescribers and controlled substance prescribing

(a) Prescriptions per 1,000 people



(b) Average prescriptions per GP



Notes: The above figures show the relationship between the number of general practice physicians (GPs) and nurse practitioners (NPs) per 1,000 people and measures of opioid prescribing (left subfigures), anti-anxiety controlled substance prescribing (middle subfigures), and opioid and benzodiazepine co-prescribing (right subfigures) at the county-year level from 2006 to 2018. Subfigure (a) considers the amount of a given behavior by GPs and NPs per 1,000 people; subfigure (b) considers the average amount of a given behavior per prescribing GP. All figures are conditional on county and year fixed effects. The number of NPs is set to zero until NPs are allowed to prescribe controlled substances independently. 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. Data come from the IQVIA LRx database.

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, because other factors may be correlated with changes in provider concentration over time, these figures do not necessarily isolate the role of competition in driving increases in prescribing.

To more formally examine the impacts of law changes that shift the competitive landscape, we estimate 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.

Let Rx^p_{cst} denote a prescription outcome for providers of type p in county c of state s in year t. County-year prescription outcomes are considered for all providers and for NPs and GPs separately (i.e., $p \in \{\text{all}, \text{NPs}, \text{GPs}\}$). Letting t^*_s denote the year of the law change in state s, the event-study specifications take the following form:

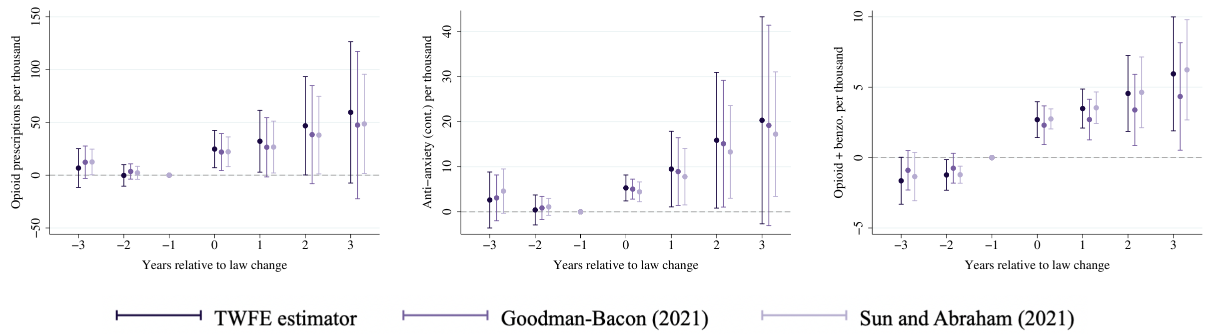
$$\begin{aligned} Rx^p_{cst} \;=& \sum_{n \in \{(-4)+, -3, \ldots, 3, 4+\}} \alpha_n \cdot B_s \cdot 1\{t^*_s + n = t\} \\ &+ \theta \cdot X_{st} + \delta \cdot X_{ct} + \gamma_c + \gamma_t + \gamma_c \cdot t + \epsilon_{cst}, \end{aligned} \tag{2}$$

where $1\{t^*_s + n = t\}$ denotes 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.[29] The year before the law change ($n = -1$) is the omitted category. Standard errors are clustered by state, and

[29] 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 A6). 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 3: Effects on total controlled substance prescribing

(a) Opioid prescriptions (b) Controlled anti-anxiety pres. (c) Opioid + benzo. co-pres.



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)). Results derived from a traditional two-way fixed effects estimator, the estimator proposed by Goodman-Bacon (2021), and the estimator proposed by Sun and Abraham (2021) are shown. 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.

observations are weighted by county population. Because of the balanced panel restriction, the coefficients $[\alpha_{-3}, \alpha_3]$ are identified by a consistent sample of states.

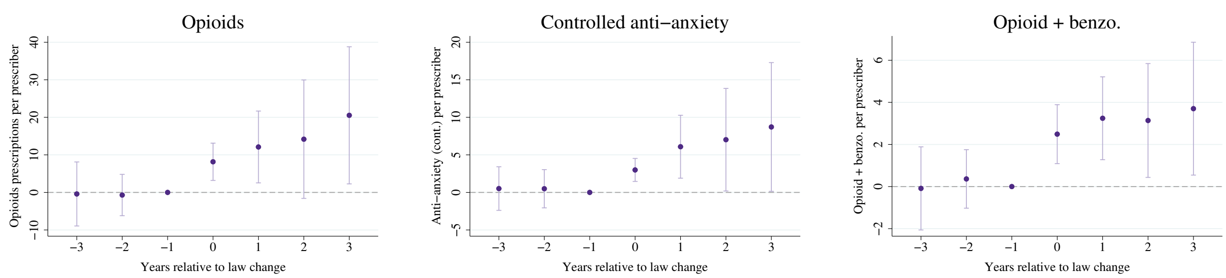
We begin by considering the impacts of the law changes on the county-year number of controlled substance prescriptions written by providers of any type per 1,000 people. Results from estimation of equation (2) are presented in Figure 3. Subfigures (a) and (b) show that there were no significant differences in trends in opioid and controlled anti-anxiety prescribing between treatment and control counties in the years before the law changes. However, prescribing of opioids and controlled anti-anxiety medications jumped when NPs were granted the authority to independently prescribe controlled substances and increased over the next three years. As shown in Figure 3(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.

In addition to presenting results using a traditional two-way fixed effects estimator, we also report results from imputation- and regression-based approaches that correct for treatment effect heterogeneity. Reassuringly, results derived from a two-way fixed effects estimator (dark lines), the estimator proposed by Goodman-Bacon (2021) (medium lines), and the estimator proposed by Sun and Abraham (2021) (light lines) are very similar. We therefore focus on a simple two-way fixed effects estimator in the analyses that follow, while confirming that our findings are robust to heterogeneity-robust methods in Section IV.D.

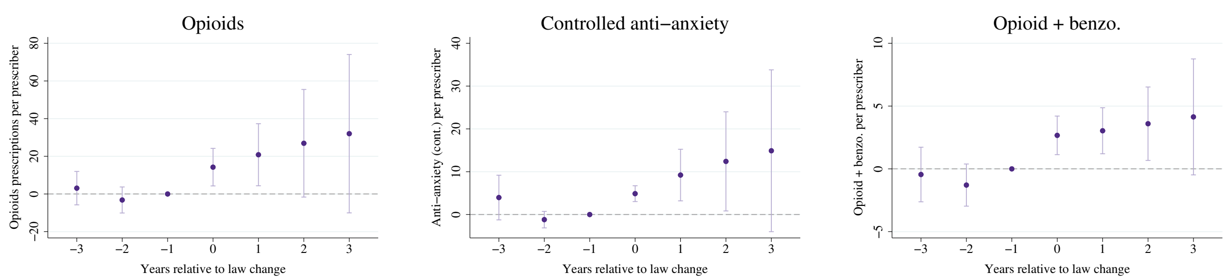
It is important to determine which types of providers are driving the observed increases in prescribing. In particular, we are interested in whether the aggregate rise in controlled substance prescribing reflects a response by physicians to NPs' expanded prescriptive authority or simply increased prescribing by NPs themselves. Figure 4 presents the estimated impacts on the average annual number of opioid prescriptions, anti-anxiety prescriptions, and co-prescriptions for opioids and benzodiazepines per NP (panel (a)) and per GP (panel (b)). Figure 4(a) shows that prescribing by NPs rose after they were granted the authority to prescribe these medications independently. These findings are not surprising given that such increases were arguably the intent of the law changes. Strikingly, however, Figure 4(b) shows that prescribing *by GPs* also jumped when NPs gained independent prescriptive authority. If patients had merely switched from GPs to NPs following the law changes, prescribing by GPs should have fallen in tandem with the rise in NP prescribing. Instead, the simultane-

Figure 4: Effects on average annual prescriptions per prescriber

(a) Nurse practitioners



(b) 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. Subfigure (a) considers prescribing by nurse practitioners, whereas subfigure (b) considers prescribing by physicians in general practice. Outcomes are the average annual number of prescriptions per prescriber of opioids (left subfigures), anti-anxiety controlled substances (middle subfigures), and co-prescriptions of opioids and benzodiazepines (right subfigures). 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.

ous increases among both groups suggest a behavioral response on the part of GPs facing increased competition.[30]

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, and observations are weighted by county population. 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 panel restriction, all treatment states used to identify β_1 are observed for the entirety of this three-year post-period. Given the similarity across estimators demonstrated in our setting in Figure 3, we focus primarily on results from a traditional two-way fixed effects specification, although we confirm that the findings are robust to the use of heterogeneity-robust estimators in Section IV.D below.

Results from estimation of equation (3) are shown in Table 2. Consistent with Figure 3, panel (a) shows that granting NPs independent prescriptive authority for controlled substances leads to increases in the total number of controlled substance prescriptions. The law changes increase the number of prescriptions per 1,000 people at the county-year level by 38.3 for opioids (7.6 percent relative to the baseline mean; p-value $= 0.053$), 10.4 for controlled anti-anxiety medications (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.

The remainder of panel (a) of Table 2 show impacts on the number of controlled substance prescriptions per 1,000 people written separately by NPs and GPs. As expected,

[30]Figure A2 presents event studies analogous to those in Figure 3 except that they show controlled substance prescribing per 1,000 people at the county-year level separately for NPs and GPs. The takeaways are very similar to those observed in the per-prescriber analyses shown in Figure 4.

Table 2: Effects of NP independent prescriptive authority on controlled substance prescribing

	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 (i.e., nurse practitioners [NPs], physicians in general practice [GPs], and physicians in other specialities); columns (4)–(6) consider NPs, and columns (7)–(9) consider GPs. 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.

the estimated effects on NP prescribing are positive. However, 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. Granting NPs independent prescriptive authority for controlled substances increases the number of prescriptions written by GPs per 1,000 people at the county-year level by 20.7 for opioids (8.9 percent relative to the GP-specific baseline mean; p-value $= 0.099$), 6.5 for controlled anti-anxiety medications (6.0 percent; p-value $= 0.057$), and 2.3 for opioid–benzodiazepine co-prescriptions (14.4 percent; p-value $= 0.009$).

The increases in prescribing observed in panel (a) of Table 2 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.

The results in panel (b) of Table 2 show that the law changes do not draw new providers into prescribing controlled substances.[31] This is because many NPs were already prescribing controlled substances—with physician supervision or collaboration—before the law changes (see Table A2).[32] Rather, consistent with Figure 4, increases in prescribing come mainly from increases in the number of prescriptions per prescribing provider (panel (c)). 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, these estimates reflect increases of 8.6, 6.9, and 12.7 percent, respectively. 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.

[31] We require providers to write at least one opioid 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. While the law changes do not lead to an increase in the total 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) and (2) are above the xth percentile among all GPs who satisfy criterion (1). Impacts on the number of "frequent" prescribers generally become more pronounced as higher thresholds are used.

[32] Event-study results for the number of prescribers per 1,000 people are shown in Figure A4.

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 between opioid-naïve and non–opioid-naïve patients. If physicians respond to increased competition by prescribing opioids to naïve patients, competition could have important implications for the initiation of opioid use and the risk of future misuse. To examine effects by patient type, we divide prescriptions based on whether they were written for patients who had not received an opioid prescription from any provider in the past six months ("opioid naïve") or for those who had ("non-opioid naïve"). Second, because larger opioid prescriptions carry a greater risk of physical dependence and misuse (CDC, 2016), we examine effects on average days supplied and average daily MME per prescription, as well as on the number of opioid prescriptions exceeding 120 MME per day per 1,000 people. Prescriptions of this size are strongly correlated with adverse outcomes (Sullivan et al., 2010; Bohnert et al., 2011).

Results from these analyses are shown in Table 3. 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 $= 0.089$) compared to only 0.33 additional prescriptions for non–opioid-naïve patients (1.0 percent; p-value $= 0.685$). These estimates are intuitive: many non–opioid-naïve patients receive refills regardless of the competitive landscape, so adjustments to the extensive margin of prescribing are primarily possible for opioid-naïve patients.[33] Nevertheless, this finding suggests that competition-induced increases in opioid prescribing put additional patients at risk of developing opioid use disorder.

The rest of Table 3 shows that the law changes do not affect prescription length for either opioid-naïve or non–opioid-naïve patients (panel (b)). However, there are sizable increases in the average MME per day supplied for both groups, with the increase being almost 50 percent larger for non-naïve patients (panel (c)). The number of prescriptions exceeding 120 MME per day written by GPs also increases among opioid-naïve patients (panel (d)). Given that the CDC recommends starting patients on the lowest effective dose and advises that doses above 90 MME per day should be "avoided" or "carefully justified," 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

[33] If physicians become more lenient following the law changes (e.g., by lowering the pain threshold required for a patient to receive a prescription), then prescriptions to opioid-naïve patients would increase.

Table 3: Effects 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 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.

the competitive landscape is an important component of both the addiction and availability channels of place-based factors identified by Finkelstein et al. (2025).

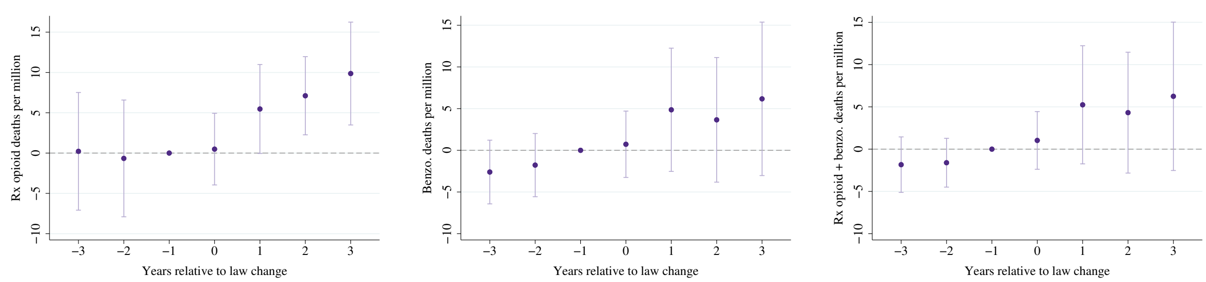
Mortality To ask how granting NPs the ability to independently prescribe controlled substances affects drug overdose deaths, analogs of equation (2) are estimated using the county-year number of fatal drug overdoses per million people as the outcome.[34] Figure 5 reports separate results for fatal drug overdoses involving prescription opioids, benzodiazepines, and prescription opioids in combination with benzodiazepines.

As shown in Figure 5(a), deaths involving prescription opioids begin to rise in treatment counties in the year after the law change. Averaging the effects in years 1–3 shows that the law changes lead to 10.3 more prescription opioid fatalities per million people per year, a 21.8 percent increase relative to the baseline mean (p-value = 0.017; see Table A3). Moreover, the increase in all opioid mortality 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 Figures 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 combined with benzodiazepines are less precisely estimated (p-values of 0.141 and 0.127, respectively; see Table A3).

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. While we do not know if the

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

(a) Prescription opioid deaths (b) Benzodiazepine deaths (c) Rx opioid + benzo. deaths



Notes: The above figures present coefficients and 95% confidence intervals from estimation of an analog 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. Standard errors are clustered by state. Outcome data come from the NVSS database.

[34]County-specific linear time trends are excluded from these analyses because there is little evidence of differential pre-trends between treatment and control counties in specifications without trend controls.

mortality increases stem from changes in prescribing by NPs or physicians, changes in NP prescribing account for only a small share of the total increase following the law changes (see Table 2). Thus, it is likely that much of the rise in mortality is driven by competition-induced changes in prescribing among physicians.[35]

IV.D Robustness

The results of several robustness checks are summarized in Figure A5, which shows that the results are remarkably consistent. Recall from Section IV.B that the primary specification for prescription outcomes includes county-specific linear time trends. These time trends are included 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 A6). 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 incorporated. For example, as shown in Figure A5, the estimates for GPs are very similar when we use state-specific rather than county-specific linear time trends. Moreover, the effects on all prescription outcomes considered are, if anything, more pronounced when county-specific time trends are estimated using only pre-period data (Goodman-Bacon, 2021).

Recent developments in the applied econometrics literature highlight the importance of addressing potential biases in two-way fixed effects designs when treatment effects are heterogenous. In addition to presenting results from the imputation-based correction proposed by Goodman-Bacon (2021), Figure A5 reports estimates using the estimator proposed by Sun and Abraham (2021). As suggested by Figure 3, results derived from these alternative estimators are very similar in magnitude. Notably, estimates based on the Sun and Abraham (2021) approach are more precise than our baseline findings, suggesting that the two-way fixed effects estimator yields conservative estimates. The figure also presents results excluding states with law changes that occurred over the sample period but outside the balanced panel window, as well as results using only "never-takers" as controls. The results remain nearly identical when these alternative sets of states are used in the control group.

Recall that the 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

[35] 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.

socio-demographics (Figure A1), it is not surprising that the results are unaffected by the exclusion of socio-demographic controls. Moreover, controlling for law changes that allowed NPs to prescribe controlled substances with physician collaboration or supervision has no impact on the findings. The results are also unaffected by controlling for the state-level adoption of must-access prescription drug monitoring programs (PDMPs) or for state-level Medicaid expansions as potential confounders.[36] 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 A5 shows the results from two additional robustness exercises. The penultimate row in each subfigure considers results excluding methadone and buprenorphine from the 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 to treat opioid use disorder rather than to manage pain. Lastly, the final row in each subfigure shows results from specifications that include provider fixed effects in a provider-level analog of our primary specification. The results are very similar 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 the baseline specification, county-year observations are weighted by population since there is likely to be more noise in the prescription outcomes of less populous counties. Comparing the top two rows in each subfigure of Figure A8 shows that excluding population weights generally leads to larger standard errors, as expected. However, the point estimates for most outcomes are closely aligned regardless of whether observations are weighted by population, highlighting that the effects are not driven by impacts in large counties. The remaining rows in Figure A8 drop each treatment state one at a time from this unweighted specification. The findings are similar regardless of which state is excluded, indicating that the results are not driven by counties in a single state.

[36]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.

V Mechanisms

We interpret the results in Section IV as being driven by changes in competition induced by changes in scope-of-practice laws allowing NPs to independently prescribe controlled substances. This section provides further evidence in support of this interpretation. Section V.A examines the pattern of effects across groups who experienced differential changes in competitive pressures as a result of the law changes. Section V.B asks whether other changes in physician practices that may result from the law changes can explain our findings.

V.A Heterogeneity by changes in competitive pressure

We conduct three sets of tests to probe whether it is indeed competition from NPs that is driving the increases in prescribing among GPs. If heightened competition is a key mechanism underlying the responses to the law changes observed in Section IV, then provider groups that experienced larger inward shifts or upward rotations in their demand curves following the law changes should have adjusted their practice styles more strongly in response to the changing competitive environment (Section II.B). Below, we examine heterogeneity in responses across locations, physician types, and medication types for which physician-specific demand curves are expected to be differentially affected.

Heterogeneity by baseline concentration of NPs First, we ask whether the effects are more pronounced in areas where GPs face greater increases in competition from NPs following the law changes. 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 for whether the county had an above-median number of NPs per GP in 2006. Allowing NPs to independently prescribe controlled substances should 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 4.[37] As predicted, GPs respond more strongly in counties in which NPs are more of a competitive threat: the estimated effects for opioids and controlled anti-anxiety medications among GPs are 47.4 and 75.0 percent

[37] GPs are more likely to co-practice with NPs when there are more NPs per GP in the market. Thus, the results in Table 4 can also be interpreted as reflecting differences by co-practice patterns. Since clinicians often have patient volume expectations within their practice, the competitive dynamics across practices—with GPs seeking to attract patients by catering to patient demand—are also relevant within practices.

Table 4: Effects 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 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.

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.

Heterogeneity across physician specialties 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 other 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 for controlled substances 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, allowing NPs to independently prescribe controlled substances should not substantively change the competitive landscape facing general surgeons.

Table 5 tests the hypothesis that physicians who face more direct competition from NPs will respond more strongly to the law changes. As shown in 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 (panel (b)), while physicians in obstetrics/gynecology write more co-prescriptions for opioids and benzodiazepines

Table 5: Effects 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 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.

(panel (c)). These findings are consistent with the fact that many NPs are 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 5 focuses on surgeons. 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 such surgeons may adjust their behavior in response to increased alternatives to their services from NPs. As predicted, there are no statistically significant effects for general surgeons, a class of physicians who likely face little competitive pressure from NPs.

Heterogeneity by drug class Finally, we ask whether the effects on prescribing are concentrated among controlled substances. While the prescribing of non-controlled substances 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.

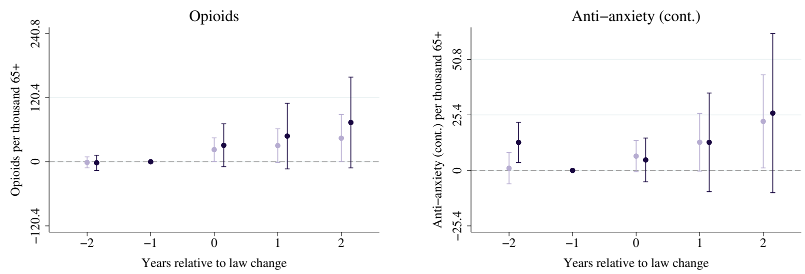
To examine effects on the prescribing of non-controlled substances, we use both the IQVIA data and the public-use Medicare Part D files. As outlined in Section III, the IQVIA data include information on the prescribing of non-controlled anti-anxiety medications and antidepressants, while the public-use Medicare Part D files contain information on the prescribing of non-controlled anti-anxiety medications, antidepressants, antihypertensives, cholesterol medications, antibiotics, and diuretics. Given the limited time frame available in the Medicare data (2012–2018), we examine 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.[38] To make the samples more comparable across the IQVIA and Medicare data, we focus on prescriptions to those aged 65 and older in the IQVIA data, although we verify 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 the primary analysis.

The first step in this analysis is to confirm that the law changes increased GP prescribing of controlled substances in the Medicare population. Figure 6(a) provides event-study results

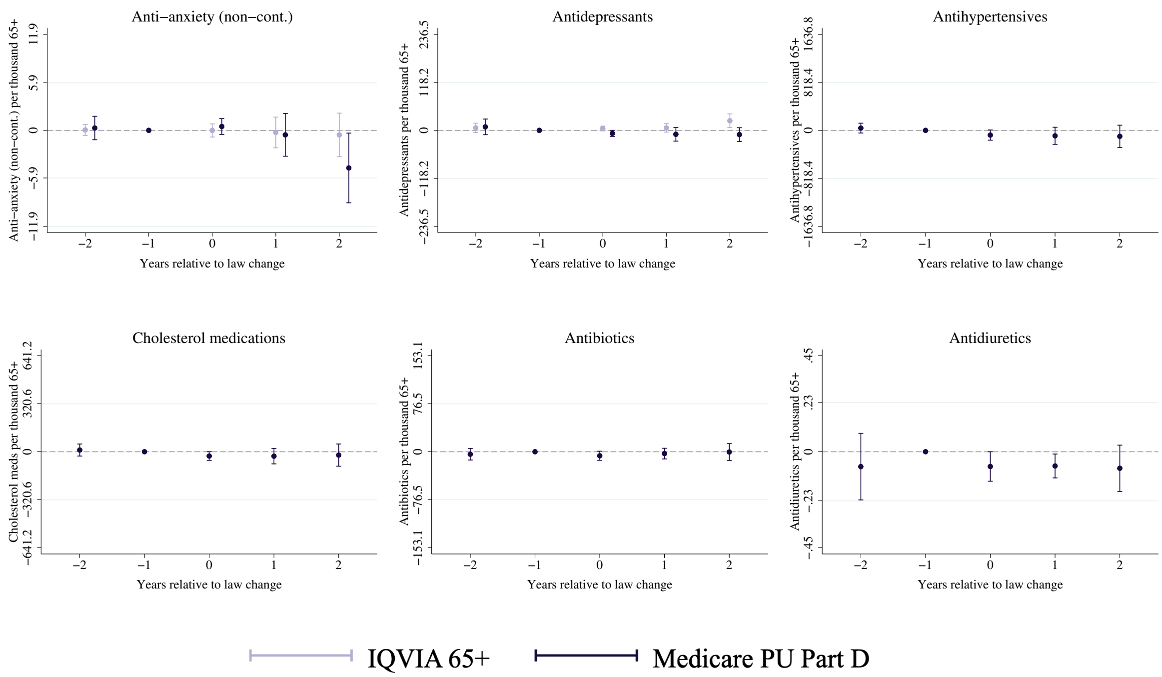
[38]The shorter sample window makes it difficult to estimate stable unit-specific time trends. We therefore exclude county-specific time trends when using data for 2012–2018 and focus on results for GPs, as these results were shown in Figures A6 and A5 to be insensitive to the inclusion of various time trend controls.

Figure 6: Effects on controlled and non-controlled substance prescribing by GPs

(a) Controlled substances



(b) Non-controlled substances



Notes: The above figures present coefficients and 95% confidence intervals from estimation of an analog 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. 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.

from estimation of equation (2) using the county-year number of opioid prescriptions and controlled anti-anxiety prescriptions written by GPs per 1,000 people aged 65 and older from 2012 to 2018. Estimates from the IQVIA data for those aged 65 and older and from the

Medicare Part D data are shown. Although the estimates using these more limited samples are less precise, there is clear evidence of increases in opioid and controlled anti-anxiety prescribing by GPs following the law changes. The point estimates are generally larger than those observed when considering prescriptions for all patients from 2006 to 2018 (see Figure A2), but the effect sizes are similar relative to the respective baseline means. For example, the effects on opioid prescribing by GPs shown in 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 the baseline specification reported in Table 2.

Figure 6(b) shows analogous results for the prescribing of non-controlled substances. To allow for a more direct comparison with the estimates for controlled substance prescribing, in which the y-axes extend to at least one-third of the baseline mean, the y-axes in Figure 6(b) are scaled to range from -33 to +33 percent of the baseline mean of each outcome. 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.[39]

V.B Ruling out alternative mechanisms

This section asks whether other changes in physician practices that may have occurred in response to the law changes contribute to the findings.

Physician workloads It is possible that allowing NPs to independently prescribe controlled substances could affect physician workloads. If GPs who were previously collaborating with or supervising NPs devote additional time to patient care, then an increase in controlled substance prescribing could reflect either more time spent with each patient—which might allow providers 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

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

Figure 6 already provide strong evidence against these possibilities: if GPs were spending more time with each patient or taking on additional patients following the law changes, then their prescribing of non-controlled substances should have increased as well.

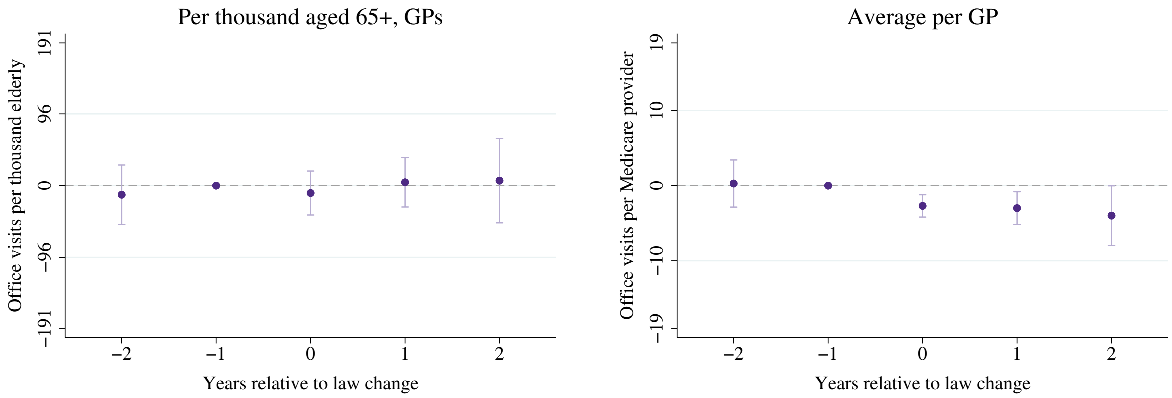
Nevertheless, to ask whether GPs see additional patients following the law changes, we examine effects on the number of office visits using the public-use Medicare Part B files. As with the Part D files, these data are available for 2012–2018. Given this shorter sample window, we again consider the effects 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 between 2014 and 2016. In line with the findings for non-controlled substance prescribing, Figure 7(a) shows that there is no increase in the number of GP office visits per capita after the law changes. The right subfigure shows that the law changes instead led to a reduction 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. Such changes in workloads should also be reflected in non-controlled substance prescribing and in the number of office visits. But we can also ask whether NPs who were practicing with a physician leave that physician's practice to work elsewhere (e.g., open their own practice) when they can prescribe controlled substances independently. As shown in Appendix D, although the growth in co-practicing was slightly less pronounced in treatment states, the number of NPs per GP practice was, if anything, higher in such states. 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.

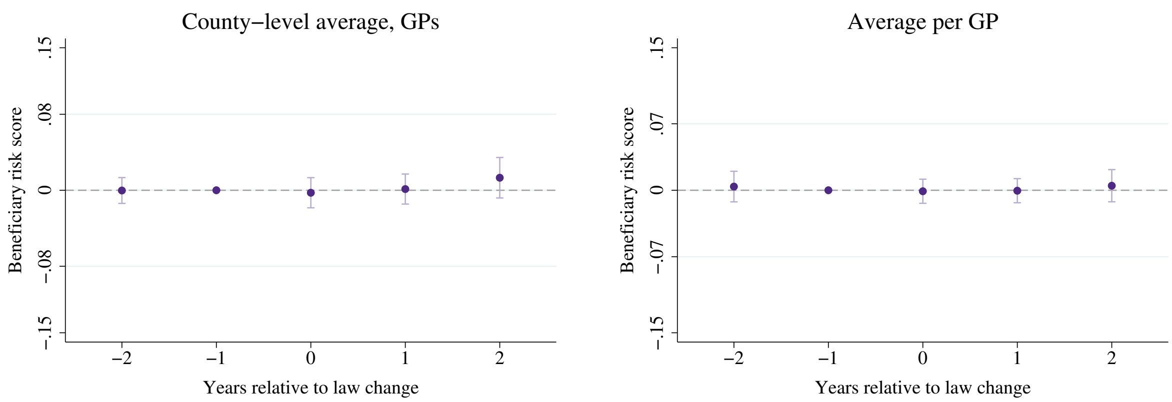
Patient composition It is also possible that increases in controlled substance prescribing by GPs could be driven by changes in the types of patients seen by such providers when NPs are allowed to prescribe controlled substances independently. Even if there are no changes in the number of patients seen by GPs, the law changes might lead more severe patients to sort away from NPs and toward GPs for their care. We emphasize that such sorting is unlikely for two reasons. First, expanding NPs' scope of practice should, if anything, lead them to also treat relatively severe patients, rather than having such patients shift toward GPs. Second, if sicker patients did move away from NPs toward GPs, we would expect NP prescribing of controlled substances to decline and GP patients to become more severe; instead, as shown

Figure 7: Effects on office visits and patient risk scores among GPs

(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 analog 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 (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. Standard errors are clustered by state. Outcome data come from the public-use Medicare Part B files.

in Section IV.B, NP prescribing rises.[40] Nevertheless, we ask whether the law changes are associated with changes in the types of patients treated by GPs. To do so, we estimate balancing equations similar to equation (3) that examine the impacts of the law changes on the average risk scores of the Medicare patients seen by GPs from 2012 to 2018. We also

[40]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, Chan and Chen (2022) show that NPs are significantly *less* likely to prescribe opioids than physicians.

consider the effects of the law changes on the average patient age, gender, and insurance type among 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 begin 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 A10, 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, NPs, or GPs. While an occasional estimate is statistically significant, which might reflect spurious associations given the number of outcomes 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 the 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. 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. However, as shown in Table 3, 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 shown in Figure A5, 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.

Pharmaceutical marketing 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). 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. However, 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.

VI Conclusion

We document changes in the prescribing practices of physicians 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. By increasing the effective number of providers, allowing NPs to independently prescribe controlled substances leads to both an inward shift and an upward rotation in the demand curve facing incumbent physicians. A simple model of physician behavior predicts that physicians will respond to such changes by catering to the preferences of marginal patients.

Consistent with this prediction, we find that GPs increase their prescribing of opioids and controlled anti-anxiety medications such as benzodiazepines when NPs' scope of practice is extended. GPs also increase their co-prescribing of opioids and benzodiazepines to the same patient on the same day—a behavior that facilitates abuse and is advised against by the CDC due to its potential to cause respiratory failure (CDC, 2016). Notably, over half of the additional controlled substance prescriptions following the law changes are driven by increased prescribing by GPs. While NPs experience a larger percentage increase relative to their baseline prescribing, the greater number of GPs and their substantially higher baseline levels of prescribing 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 after the law changes are driven by increased competition from NPs. Each test isolates locations, types of physicians, or services for which we expect the demand curve facing incumbent physicians to be more strongly affected. In line with the predicted responses to increased competition, we find that the observed increases in GP prescribing are larger in areas with a greater number of NPs per GP at baseline; that changes in prescribing are concentrated in physician specialties that compete most directly with NPs; and that the law changes do not affect the prescribing of commonly used non-controlled substances, such as antihypertensives and antibiotics.

Moreover, 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 with GPs among Medicare beneficiaries, 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 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 joint practices. Finally, the law changes do not affect the age, gender, or payment types of patients receiving controlled substance prescriptions from GPs or the risk scores of Medicare patients seen by GPs. These results suggest that the observed increases in prescribing cannot be explained by changes in patient composition.

Examining the increases in opioid prescribing in greater depth shows that in addition to increasing the number of prescriptions, 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. A back-of-the-envelope calculation based on our estimates suggests that relaxed scope-of-practice restrictions contributed to nearly 30,000 fatal overdoses involving prescription opioids between 2006 and 2018, accounting for over five percent of such deaths nationwide and nearly 20 percent of those occurring in treated state-years during this period.[41]

Of course, there are important benefits to relaxing scope-of-practice restrictions. As health care demand continues to exceed supply, allowing NPs to practice and prescribe independently has been shown to be a promising tool for improving access and addressing provider shortages (e.g., Traczynski and Udalova, 2018; Alexander and Schnell, 2019; McMichael and Markowitz, 2023). Furthermore, in settings where service prices are not set administratively, increased access may reduce prices paid by patients over the long run. Thus, our findings do not necessarily imply that allowing NPs to independently prescribe controlled substances is welfare reducing for patients. Rather, our results highlight that physicians are not immune to competitive pressures, and thus the competitive landscape must be considered when examining factors that shape physician practice styles. Our finding that increased competition can lead physicians to increase their provision of powerful and dangerous medications is consistent with the cautions of authors such as Gaynor et al. (2015) and McGuire (2000), who argue that more competition will not always improve patient care and can instead lead to excessive—and even harmful—service provision.

[41] As shown in Table A3, allowing NPs to independently prescribe controlled substances results in an additional 10.3 fatal overdoses involving prescription opioids per million people per year. Multiplying this estimate by the total population across treated state-years (2.8 billion person-years) implies that the law changes led to approximately 28,889 prescription opioid fatalities from 2006 to 2018. For comparison, 565,100 individuals died from a fatal overdose involving prescription opioids nationwide over the same period, with 155,631 of these deaths occurring in treated state-years.

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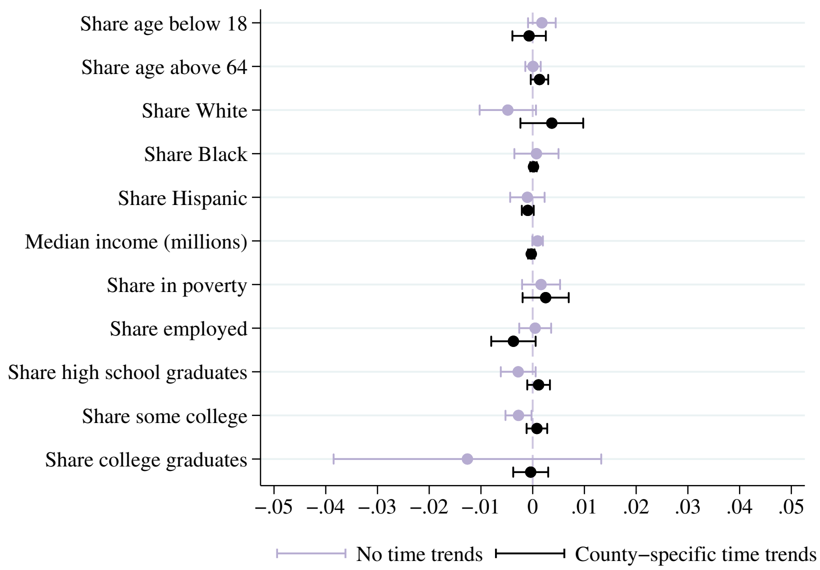
A Supplementary figures and tables

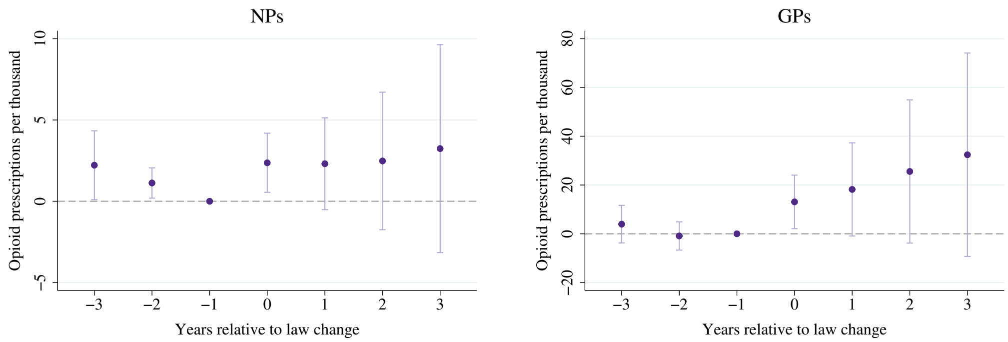
Figure A1: Relationship between changes in NP independent prescriptive authority and potential confounders



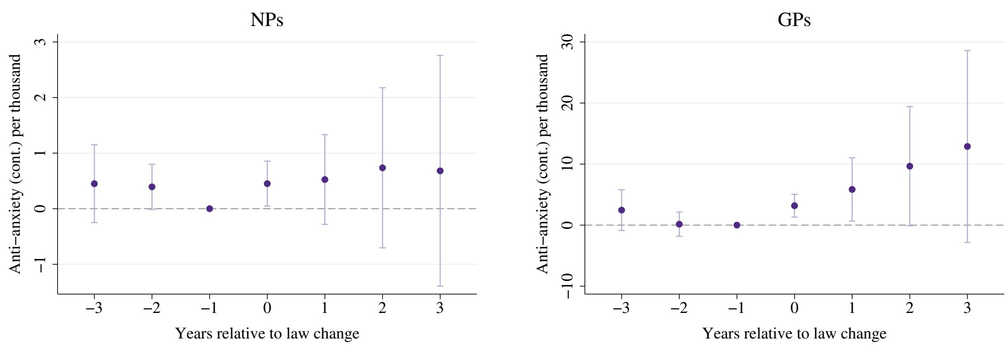
Notes: The above figure presents coefficients and 95% confidence intervals from estimation of balancing analogs of equation (3) using county-year–level data for 2006–2018. Each row presents output from a separate regression in which the potential confounder denoted on the y-axis is the dependent variable. 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 of NP independent prescriptive authority on controlled substance prescribing by NPs and GPs

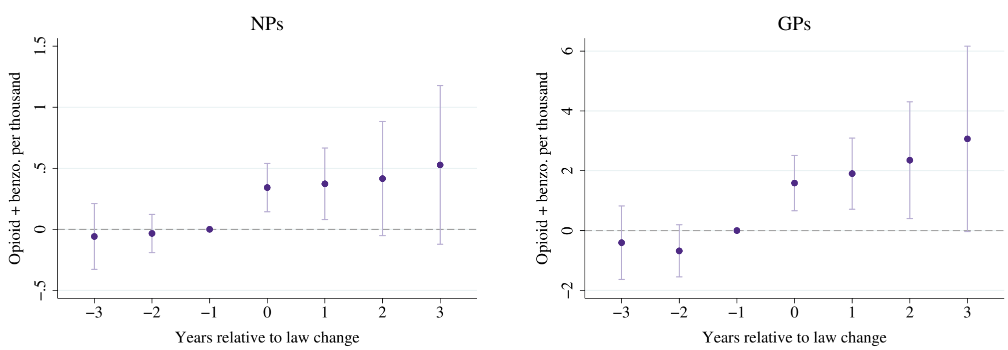
(a) Opioid prescriptions



(b) Controlled anti-anxiety prescriptions



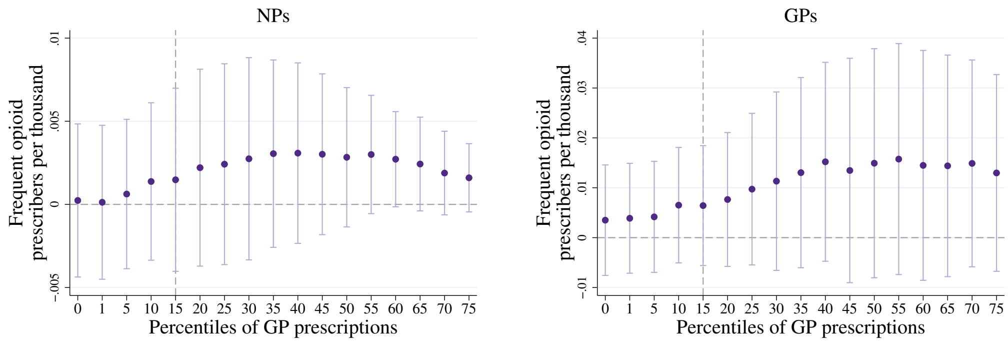
(c) Opioid + benzo. co-prescriptions



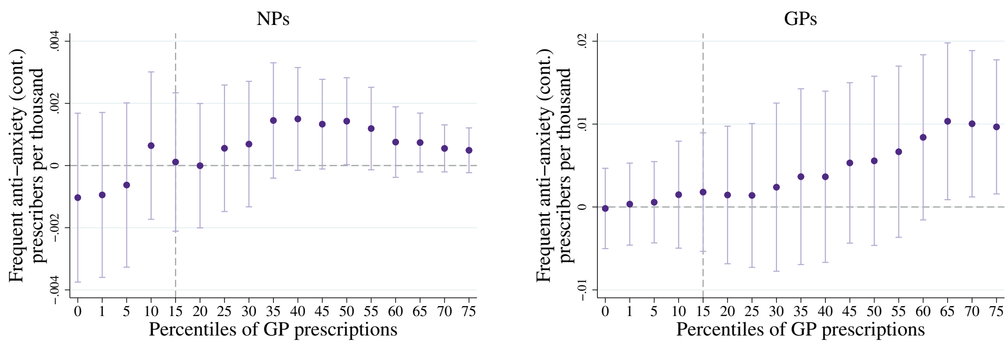
Notes: The above figures present coefficients and 95% confidence intervals from estimation of equation (2) using county-year–level data for 2006–2018. The left (right) subfigure in each subplot only considers prescriptions written by nurse practitioners [NPs] (physicians in general practice [GPs]). Outcomes 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 A3: Effects on number of "frequent" prescribers: Alternative definitions

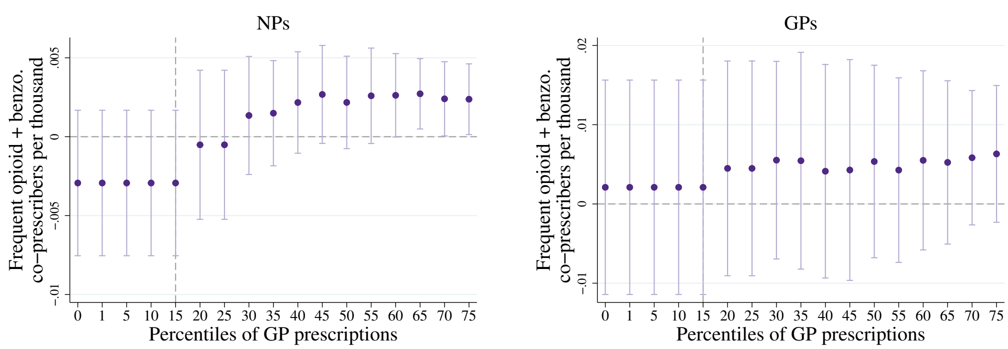
(a) Opioid prescribers



(b) Controlled anti-anxiety prescribers



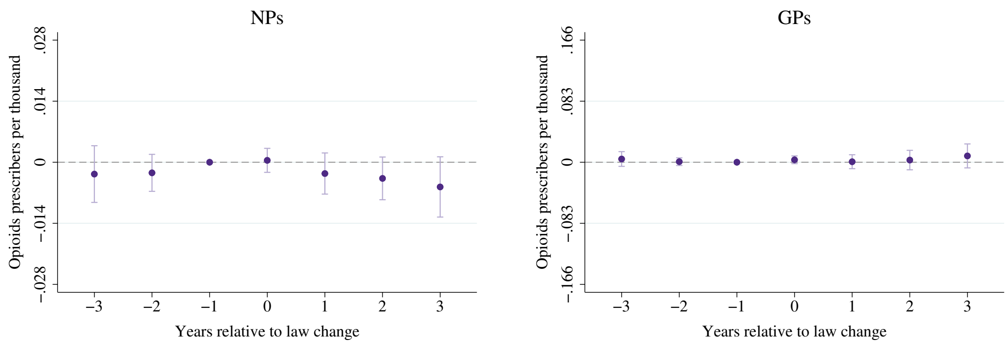
(c) Opioid + benzo. co-prescribers



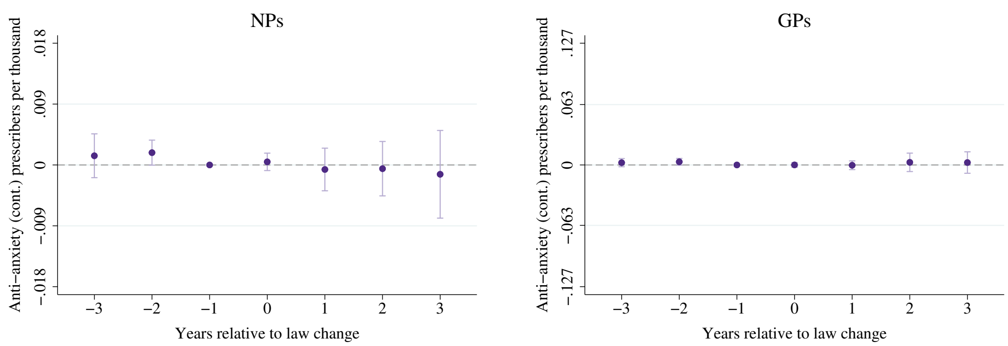
Notes: The above figures present coefficients and 95% confidence intervals from estimation of equation (3) using county-year–level data for 2006–2018. Each coefficient comes from a separate regression in which the outcome is an alternative definition of the number of "frequent" prescribers of a given 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

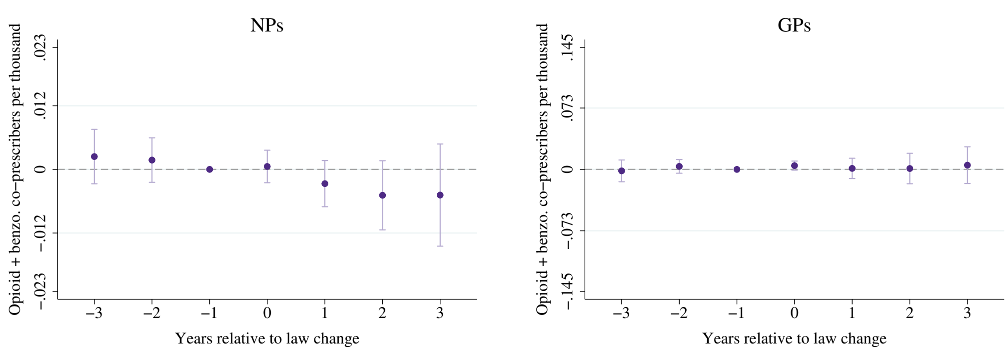
(a) Opioid prescribers



(b) Controlled anti-anxiety prescribers



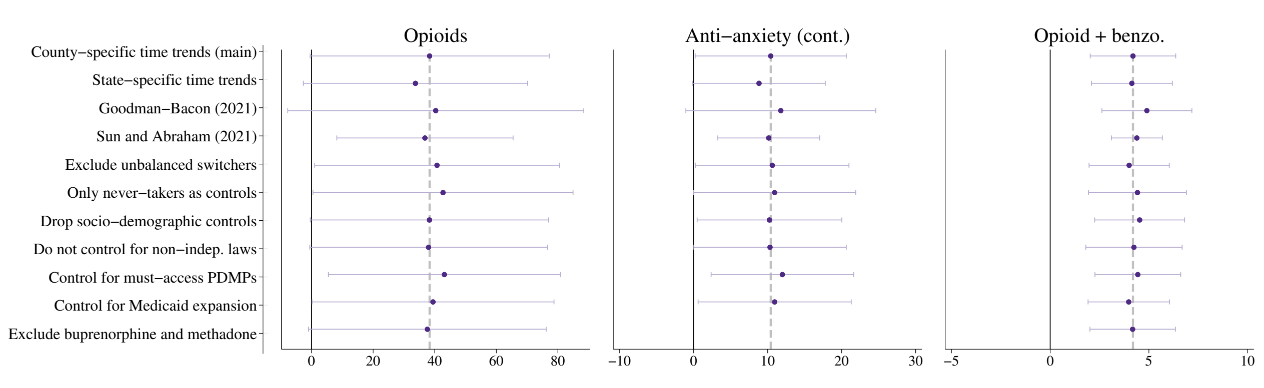
(c) Opioid + benzo. co-prescribers



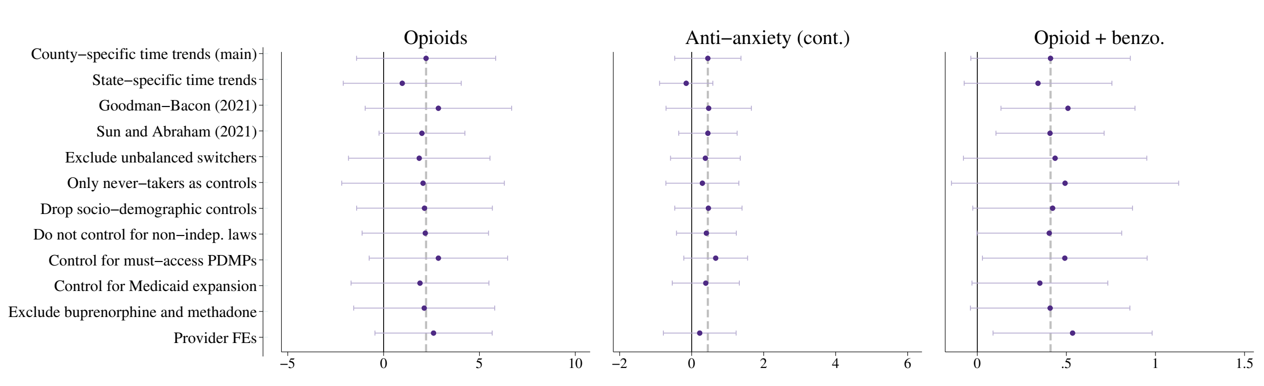
Notes: The above figures present coefficients and 95% confidence intervals from estimation of equation (2) using county-year–level data for 2006–2018. The left (right) subfigure in each subplot considers nurse practitioners [NPs] (physicians in general practice [GPs]). Outcomes are the number of prescribers 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 controlled substance prescribing: Robustness

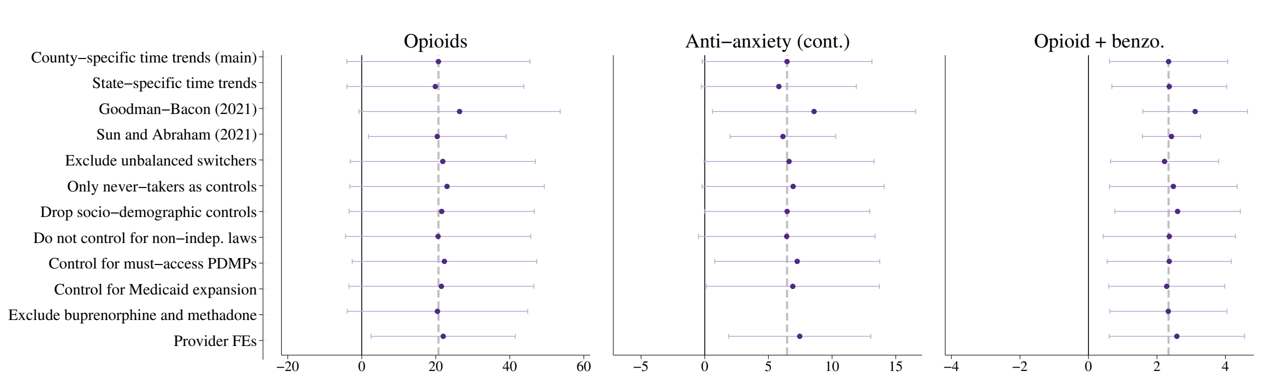
(a) All providers



(b) Nurse practitioners



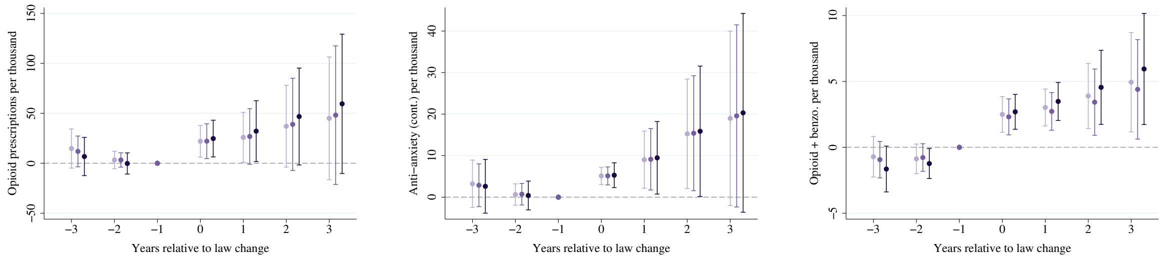
(c) General practice physicians



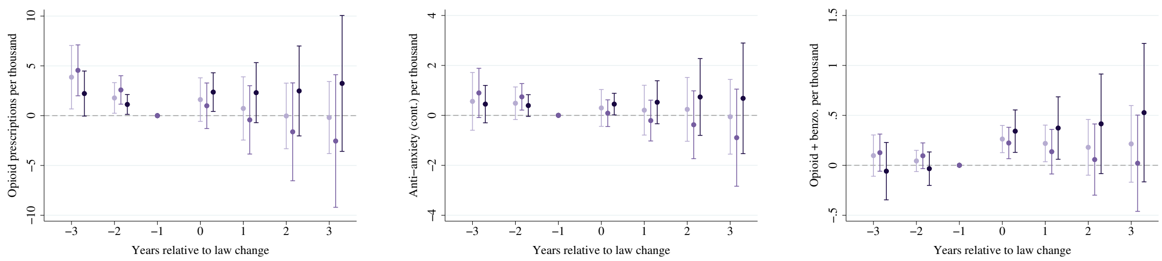
Notes: The above figures present coefficients and 95% confidence intervals from estimation of analogs of equation (3) using county-year–level data for 2006–2018. Each row presents output from a separate regression using the specification denoted on the y-axis. Outcomes are the number of prescriptions 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 2). 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 A6: Effects on controlled substance prescribing: Alternative time trends

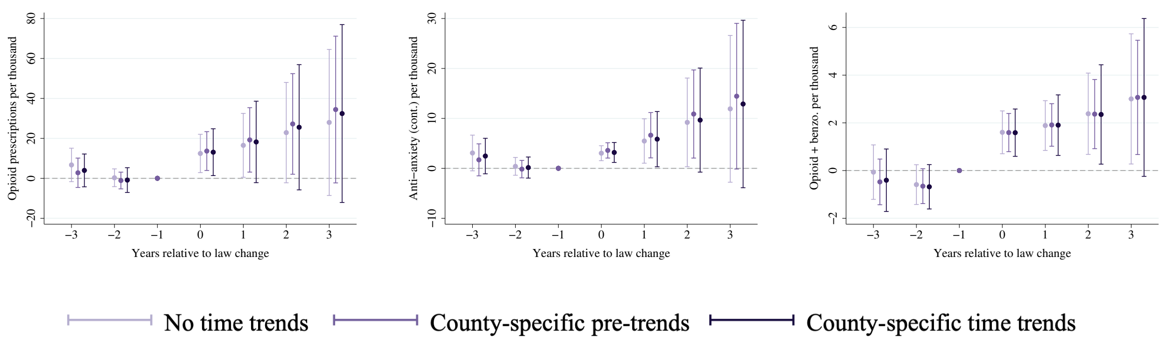
(a) All providers



(b) Nurse practitioners



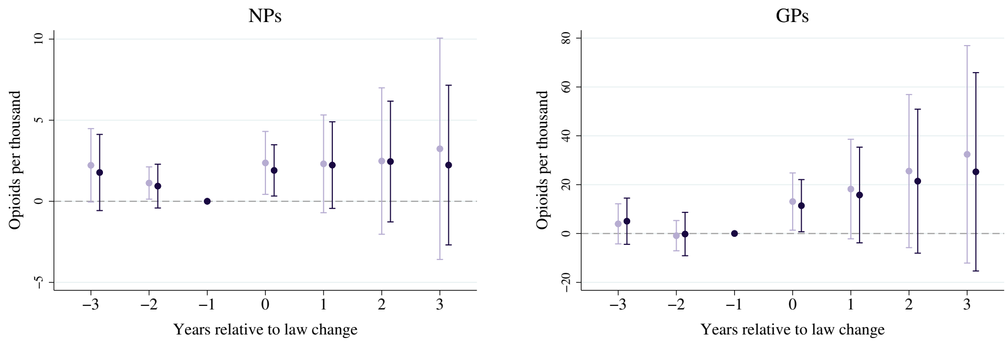
(c) General practice physicians



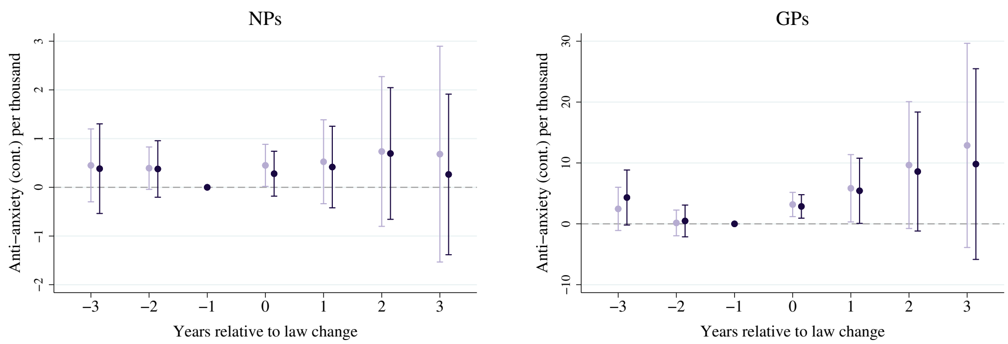
Notes: The above figures present coefficients and 95% confidence intervals from estimation of analogs of equation (2) using county-year–level data for 2006–2018. Outcomes are the number of opioid prescriptions per 1,000 people (left subfigures), the number of anti-anxiety controlled substance prescriptions 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 A7: Effects of controlled substance prescribing: Excluding Medicaid patients

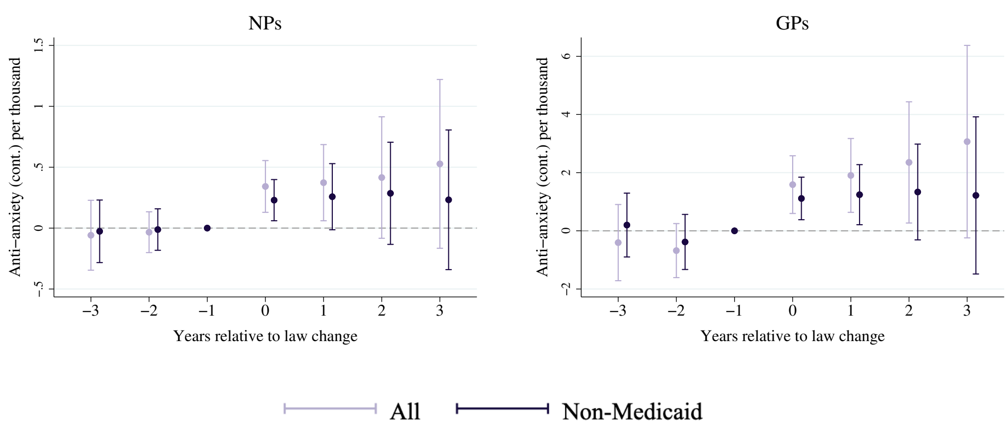
(a) Opioid prescriptions



(b) Controlled anti-anxiety prescriptions



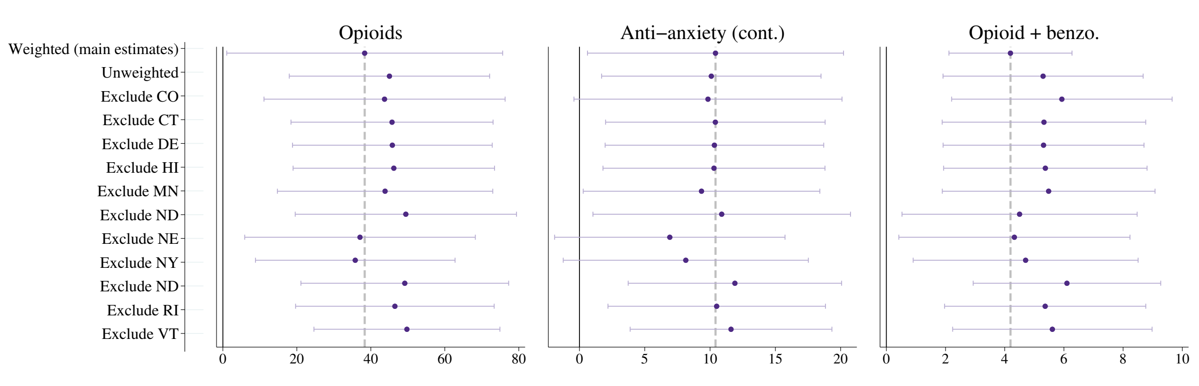
(c) Opioid + benzo. co-prescriptions



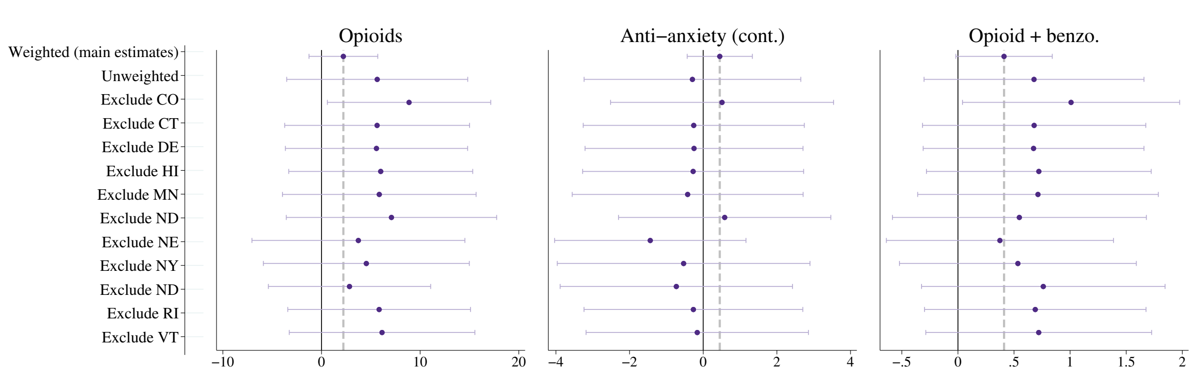
Notes: The above figures present coefficients and 95% confidence intervals from estimation of equation (2) using county-year–level data for 2006–2018. The left (right) subfigure in each subplot only considers prescriptions written by nurse practitioners [NPs] (physicians in general practice [GPs]). Outcomes 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

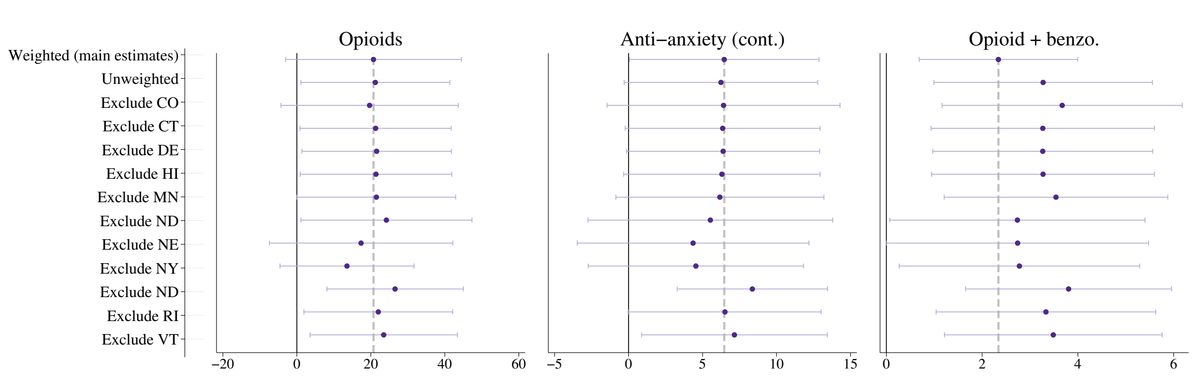
(a) All providers



(b) Nurse practitioners



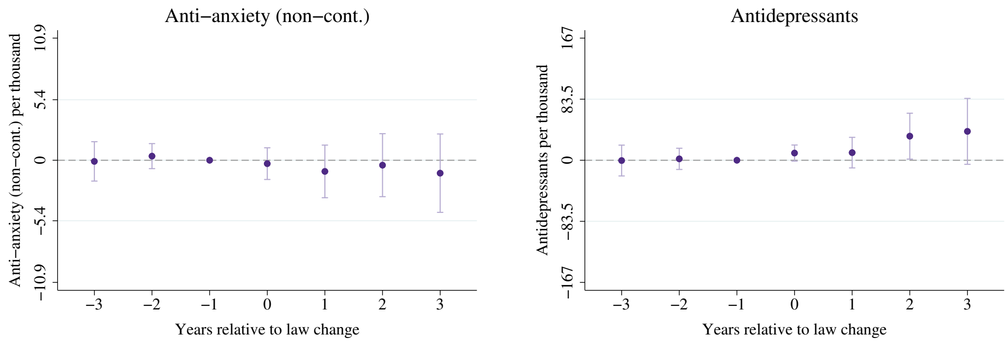
(c) General practice physicians



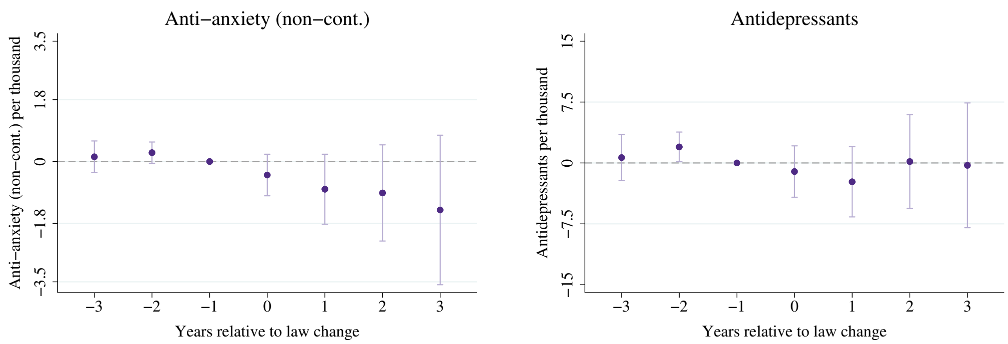
Notes: The above figures present coefficients and 95% confidence intervals from estimation of equation (3) using county-year–level data for 2006–2018. Each row presents output from a separate regression using the specification denoted on the y-axis. Outcomes are the number of prescriptions of a given 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 2); 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)

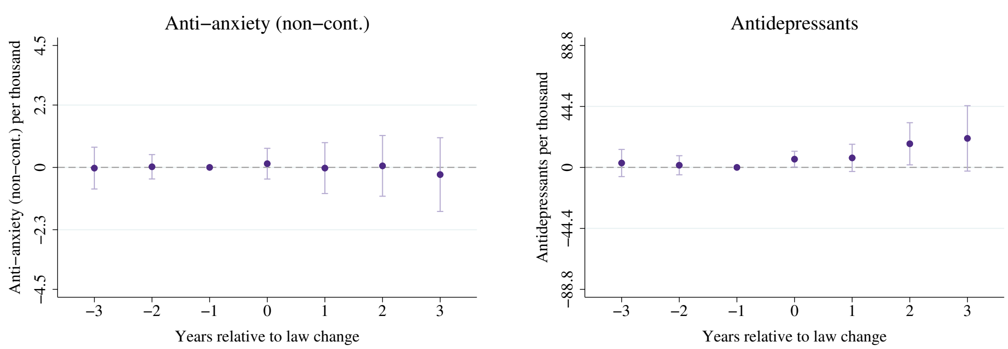
(a) All providers



(b) Nurse practitioners



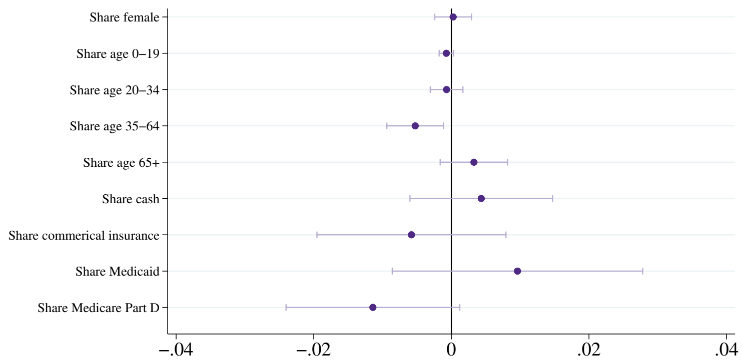
(c) General practice physicians



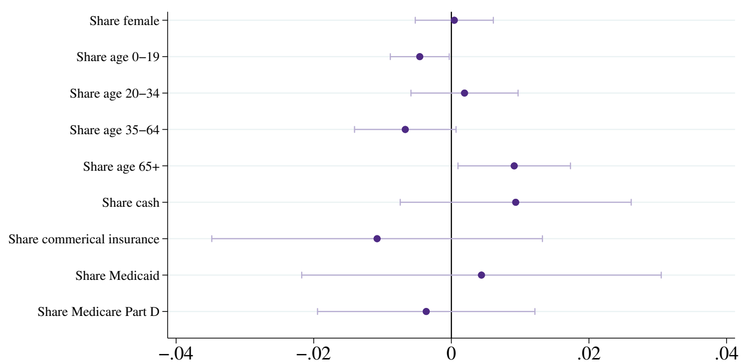
Notes: The above figures present coefficients and 95% confidence intervals from estimation of equation (2) using county-year–level data for 2006–2018. The outcome in the left (right) subfigure in each subplot is the number of prescriptions for non-controlled anti-anxiety medications (antidepressants) 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: Effects on patient composition of controlled substance prescriptions

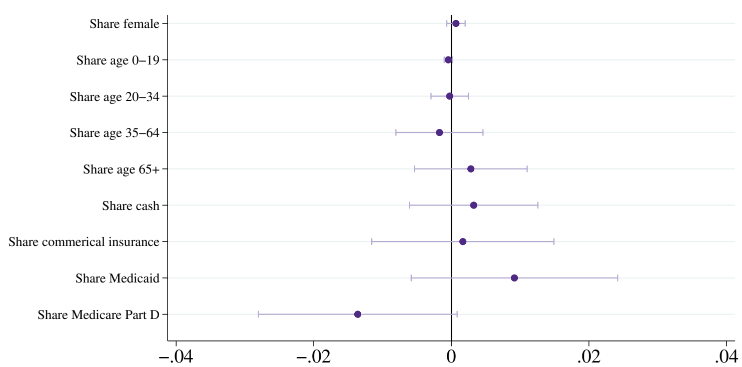
(a) All providers



(b) Nurse practitioners



(c) General practice physicians



Notes: The above figures present coefficients and 95% confidence intervals from estimation of analogs 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: Number of prescribers and prescription shares by provider type

	Unique providers	Controlled substance prescription shares		
		Opioids	Anti-anxiety	Opioid + benzo.
	(1)	(2)	(3)	(4)
a. 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
b. 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. Table 1 reports the corresponding statistics for the period 2006–2018. "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 A2: 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 A3: 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 analog 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 $R_{OP} > R_{noOP}$.[42] Moreover, although

[42]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

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:

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

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.[43] 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 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.

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.

[43]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.

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.[44] 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 $t-2$.[45] 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 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

[44] 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.

[45] 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 $t-x$.

(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.[46]

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.[47] 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.[48] Using our location assignment algorithm, we find that among 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.

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

[47]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.

[48]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.

D Co-practice patterns

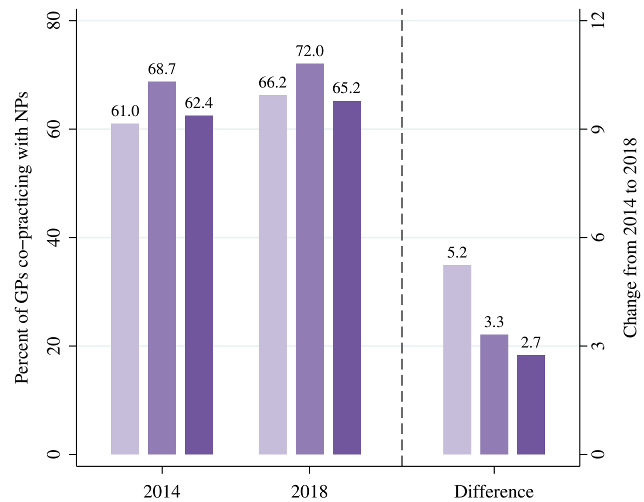
Recall that in our main analyses, we use information on patient zip codes to infer the practice counties of prescribing providers in each year of the sample (see Section III.A and Appendix C). To examine whether independent prescriptive authority affects co-practice patterns among GPs and NPs, we instead 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 A11(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 A11(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.

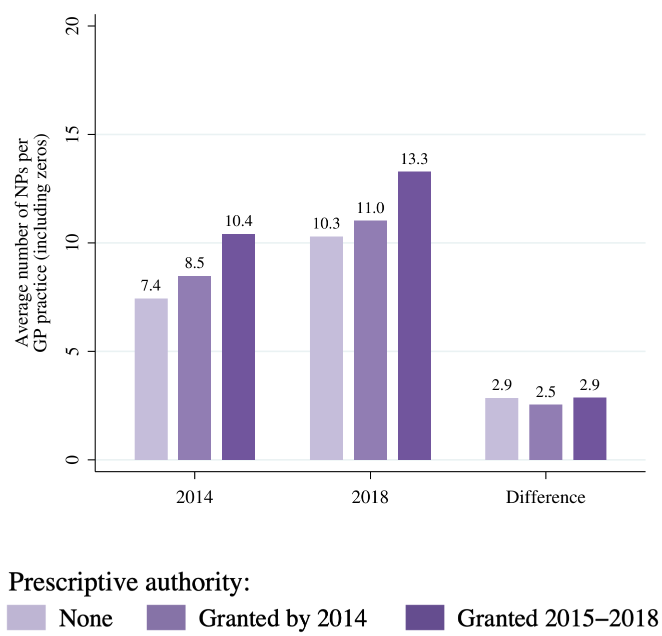
An interesting question is whether the results differ in areas with a higher share of co-practicing physicians. Because GPs are more likely to co-practice with NPs in markets with a higher number of NPs per GP, the heterogeneity in effects shown in Table 4—by baseline NP-to-GP ratios—can also be interpreted as capturing variation in co-practice patterns. Importantly, the same competitive dynamics apply both across and within practices, as clinicians often face patient volume expectations regardless of whether they work alongside or independently from NPs.

Figure A11: 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.

E 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 2), (2) number of opioid prescribers per 1,000 people (Table 2), (3) average annual opioid prescriptions per opioid prescribing provider (Table 2), (4) average days supplied per opioid prescription (Table 3), and (5) average MME per day supplied (Table 3). 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 A4 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.[49] The

[49] The estimates reported in panels (c) and (e) of column (10) of Table A4 reproduce those first reported

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).[50] 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.[51]

The remaining columns of Table A4 show the impact of additional incremental changes moving between McMichael's analysis and our own. Comparing columns (2) and (3) shows 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.

in column (4) of Table 3 of this paper. The estimate reported in panel (a) of column (10) of Table A4 is similar to that reported in column (7) of Table 2, except that we include GPs with zero opioid prescriptions when calculating opioid prescriptions per provider in Table A4 to more closely reflect the measures used in McMichael (2020).

[50] 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.

[51] 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.

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 5 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 A4: 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.