The Effects of Competition on Physician Prescribing*

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Abstract

We ask how competition influences the prescribing practices of physicians. Law changes granting nurse practitioners (NPs) the ability to prescribe controlled substances without physician collaboration or oversight generate exogenous variation in competition. In response, we find that general practice physicians (GPs) significantly increase their prescribing of controlled substances such as opioids and controlled anti-anxiety medications. GPs also increase their co-prescribing of opioids and benzodiazepines, a practice that goes against prescribing guidelines. These effects are more pronounced in areas with more NPs per GP at baseline, are concentrated in physician specialties that compete most directly with NPs, and are not observed for many non-controlled drug classes. 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

Policy makers in the United States have long sought to increase competition in health care markets.[1] Yet, given imperfections in these markets, it is not clear that increased competition will always improve welfare (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 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 limited time-series variation in measures of concentration. Such constraints have made empirical analyses of the effects of competition on physician behavior difficult.

This paper asks how the prescribing practices of general practitioners (GPs) change following sharp increases in competition being experienced in many markets due to changes in state-level scope-of-practice laws granting nurse practitioners (NPs) the ability to independently prescribe controlled substances.[2] Controlled (or "scheduled") drugs are regulated under the Controlled Substances Act because they are generally addictive and carry a risk of fatal overdose. We analyze comprehensive data from IQVIA covering the prescriptions written by individual providers across the United States from 2006 to 2018 and find that GPs begin to prescribe more opioids and scheduled anti-anxiety medications when they are subject to increased competition from NPs. GPs also increase their co-prescribing of opioids and benzodiazepines to the same patient on the same day, a behavior that the Centers for Disease

[1] For example, a joint commission of the Federal Trade Commission and the Department of Justice recommended increasing transparency in pricing and lowering barriers to entry into primary care for allied health professions to increase competition (FTC and DOJ, July 2004).

[2] We consider doctors in family, general, and internal medicine to be GPs; all our results are robust to including only physicians in family or general practice. An NP is a nurse who has obtained at least a master's degree in nursing and who has completed local licensure and national certification requirements. States have the authority to define what NPs are allowed to do and frequently update associated legislation, leading to wide variation in scope of practice for NPs both across states and within states over time. NPs are one type of advanced practice registered nurse (APRN); this broader category also includes certified nurse-midwifes, certified registered nurse anesthetists, and clinical nurse specialists.

Control and Prevention (CDC) prescribing guidelines indicate that clinicians should avoid "whenever possible" (CDC, 2016). These results 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.

Three additional sets of analyses leverage variation in competitive pressures induced by the law changes and support the hypothesis that our findings are driven by increased competition. First, the observed increases in physician prescribing are higher in areas with a greater number of NPs per GP at baseline. That is, GPs respond more in areas in which they are subject to greater competition from NPs when NPs are allowed to prescribe independently. Second, changes in prescribing are concentrated among physicians practicing in the specialties that compete most directly with NPs rather than in specialties that face little competitive pressure from NPs. Finally, using data on the prescribing of a number of unscheduled drugs from both IQVIA and the public use Medicare Part D 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. First, using data from the public use Medicare Part B files, we show that the law changes do not lead to increases in the total number of office visits with GPs. If anything, they lead to slight reductions in the average number of office visits per GP. Using snapshots of the exact practice addresses of providers in two years, we further show that the law changes do not affect the share of GPs practicing in clinics with NPs or the number of NPs per GP practice. Combined with null effects on the prescribing of non-controlled substances, these findings suggest that GPs are neither seeing more patients nor spending more time with each patient as a result of the law changes.[3] Finally, we run balancing regressions to examine whether the law changes are associated with changes in the types of patients receiving prescriptions from different provider types. We find no effects of the law changes on the patient gender, age, and insurance type profiles of prescriptions written by

[3] If physicians were spending more time with each patient following the law changes, then they might be expected to identify additional conditions that could warrant medication. However, there is little reason to believe that additional time would lead only to the discovery of conditions that require treatment with controlled substances. Thus, this argument would suggest that both the prescribing of controlled and non-controlled medication should be affected.

GPs. Hence, our results are unlikely to be driven by changes in the types of patients seen by GPs following the law changes.

The results on opioid prescribing are particularly important considering the ongoing opioid crisis in the United States. To shed additional light on how competition affects opioid prescribing, we conduct two additional analyses. First, focusing on patients who did not receive an opioid prescription in the past six months, we find that competition-induced increases in opioid prescribing are driven by new prescriptions to "opioid-naïve" patients. Moreover, examining changes in average morphine milligram equivalents (MMEs) per prescription shows that competition leads GPs to write prescriptions with higher dosages, both for patients who are and are not opioid naïve. These results highlight the important role played by physicians in initiating opioid use and contribute to work documenting that the opioid crisis is driven in large part by supply-side factors.[4]

Our paper relates to four branches of literature. First, many studies examine the effects of competition among large players, such as 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. (2020) highlight that hospitals also have market power; thus, increased concentration in insurance markets could enable insurers to negotiate lower prices from hospitals, possibly increasing consumer welfare. Focusing on elderly heart attack patients, Kessler and McClellan (2000) find that while competition between hospitals can reduce both costs and mortality under certain market conditions, competition can also lead to a medical arms race in which more costly and unnecessary care is supplied.[6] We complement this work by showing that increased competition among

[4]See Currie and Schwandt (2021) for a recent review of this large literature.

[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]Related work by Gowrisankaran and Town (2003) finds that the effect of competition for patients on hospital quality depends on the type of insurance held by patients. Other work on hospitals finds that measures that increased patient hospital choice in the United Kingdom reduced patient deaths and lengths of stay without increasing costs (Gaynor et al., 2013). Also in the U.K. setting, Bloom et al. (2015) find that decreases in competition due to hospital closures lead to reductions in management quality and increases in deaths among heart attack patients. A related literature at the intersection of health economics and industrial organization examines the impacts of hospital mergers on prices, quality, and patient outcomes. For example, Dafny (2009) shows that hospital mergers result in higher prices among rivals of merged firms;

individual providers can likewise have perverse effects, leading to increases in prescribing that are likely to be welfare reducing.

Second, this paper adds to a smaller literature examining the effects of competition among physicians and its impact on physician-induced demand.[7] Given limited variation in concentration within markets over time, most investigations of competition at the physician level have been cross-sectional. For example, Dunn and Shapiro (2014, 2018) show that areas with higher concentrations of cardiac surgeons have higher prices and higher procedure use, and Scott et al. (2022) find that GPs practicing closer to other GPs provide more unnecessary imaging. An important exception is Gruber and Owings (1996), who show that reductions in the demand for obstetrical services due to declining fertility rates in the 1970s led to increases in the use of (presumably unnecessary) C-sections, which are more highly remunerated than vaginal deliveries. We contribute to this literature by using comprehensive, individual-level panel data and a novel shock to competition to document how increased competition affects the prescribing practices of physicians.

Third, this paper relates to the large literature examining factors that drive physician decision-making. Studies have documented pronounced heterogeneity in the intensity of health care provision across locations (e.g., Fisher et al., 2003; Finkelstein et al., 2016) and individual providers (e.g., Parys, 2016; Currie et al., 2016; Currie and Zhang, forthcoming; Gowrisankaran et al., forthcoming). 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, 2019a), physician skill (Currie and MacLeod, 2017, 2020; Chan et al., 2022), and provider beliefs (Cutler et al., 2019). Particularly relevant for our study, recent work focusing on supply-side drivers of the opioid crisis has examined how opioid prescribing is affected by training (Schnell

see Gowrisankaran et al. (2015) for more recent work on the impacts of hospital mergers on prices and Gaynor et al. (2015) for a recent overview of hospital merger effects.

[7]See McGuire (2000) for an overview of the literature on physician-induced demand. Early work by Fuchs (1978) and Cromwell and Mitchell (1986) shows that rates of surgery are higher in locations with more surgeons, a finding that the authors attribute to demand inducement. However, follow-up work by Dranove and Wehner (1994) shows that similar findings also hold for obstetricians and childbirth, a service for which induced demand is likely minimal. These findings highlight the difficulties with designs that rely predominately on cross-sectional variation in provider supply. Another approach is to conduct a lab experiment as in Brosig-Koch et al. (2017).

and Currie, 2018; Zhang, 2023), beliefs about risks (Doctor et al., 2018), pharmaceutical marketing (Alpert et al., 2022; Arteaga and Barone, 2022), and provider altruism (Schnell, 2017). We add to this literature by considering a novel driver of variation in physician behavior—exposure to competition—and show that the competitive landscape affects physicians' prescribing of controlled substances.[8]

Finally, our paper relates to a growing literature on the impacts of changes in scope-of-practice legislation for NPs on patient care. As outlined in a recent overview by McMichael and Markowitz (2020), much of this literature has focused on the impacts of expanded scope of practice on patient access and health using either aggregate or patient-level data.[9] For example, Traczynski and Udalova (2018) document that allowing NPs to both practice and prescribe independently leads to increases in utilization of primary care services, while Alexander and Schnell (2019b) show that allowing NPs to independently prescribe unscheduled drugs (including most antidepressants) leads to improvements in mental health. In a law review article, McMichael (2020) argues that law changes granting NPs full practice authority reduced opioid prescribing among physicians over the period 2011–2018. As shown in Appendix D, the difference between our findings stems largely from a consideration of alternative law changes that more directly affect the competitive landscape for controlled substance prescribing.[10] We therefore add to this prior work by examining how changes in competition induced by changes in scope-of-practice legislation for NPs affect the behavior of physicians.

The rest of the paper proceeds as follows. Section II provides a theoretical framework that highlights how increased competition can lead physicians to increase unnecessary, and potentially harmful, service provision. Section III provides an overview of the data. Section IV introduces our methods and presents our main empirical findings. The role that compe-

[8]Our findings that competition increases both the number of prescriptions for opioid-naïve patients and the strength of prescriptions for naïve and non-naïve patients suggests that competitive landscapes are important components of both the addiction and availability channels of place-based factors identified by Finkelstein et al. (2022).

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

[10]There are a number of other differences between our analysis and McMichael (2020). Appendix D replicates his findings in our data and shows how other differences impact the results.

tition versus alternative mechanisms play in driving our results is considered in Section V, and Section VI provides a discussion and concludes.

II Theoretical framework

This section offers a theoretical framework that outlines how competition can influence the intensity of services provided by physicians. The framework highlights the idea that the effects of competition will depend on the type of service being rendered.

In particular, the model predicts that increased competition should put *downward pressure* on the provision of services like C-sections that physicians might like to do more of (e.g., because they are time efficient and highly remunerated) but that marginal patients may not want (e.g., because they are unnecessary and cause complications). In contrast, increased competition should put *upward pressure* on the provision of services like prescription opioids that some marginal patients want (e.g., because of addiction, resale value, or the possibility of immediate pain relief) but physicians may not want to provide more of (because their utility of prescribing to marginal patients is negative).[11] In both cases, physician behavior shifts toward the preferences of the marginal patient when competition increases. Whether increased competition leads to more or less service provision therefore depends on whether physicians are over- or underproviding care from the perspective of the marginal patient at baseline.

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 prescription) or an intensive margin measure that further captures the intensity of treatment conditional on its provision (e.g., average daily MME

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

per opioid prescription).[12] 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.[13] 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:

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

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

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

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

There are four cases to consider. If both patients and physicians benefit from additional service intensity (i.e., if $N_x > 0$ and $u_x > 0$), then there is no trade-off between per patient utility and the number of patients seen, and the physician sets x^* at the highest possible level. Analogously, the physician sets x^* at the lowest possible level if both patients and physicians are harmed by additional service delivery (i.e., if $N_x < 0$ and $u_x < 0$). The interesting cases occur when the incentives of patients and physicians are misaligned. This will occur whenever: (1) physicians receive higher per-patient utility by increasing service

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

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

intensity, but additional service intensity loses them patients (i.e., if $N_x < 0$ and $u_x > 0$), or (2) patients desire additional service intensity that physicians do not want to provide for a given patient (i.e., if $N_x > 0$ and $u_x < 0$).

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

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

Now suppose that $N_x > 0$ and $u_x < 0$. In this case, an increase in competition should instead lead to an increase in x^*. That is, for services that providers do not want to provide more of (e.g., because they are harming marginal patients), but that some marginal patients want (e.g., because of desired pain relief, addiction, or non-health benefits like resale value), increased competition should increase the intensity of service provision. As long as some patients want medications that they are not currently prescribed (or larger prescriptions than they are currently prescribed), this logic will likely govern the impacts of competition on the intensity of services like opioid and benzodiazepine prescribing. The presence of secondary markets for many addictive and abusable medications suggests that there is no shortage of such patients.

Of course, alternative models of physician behavior can also be used to micro-found our finding that increased competition leads to increases in the prescribing of certain medications. For example, as shown in Appendix B, a model of demand inducement can likewise deliver this result (Gruber and Owings, 1996; McGuire, 2000). In a demand-inducement framework, the effect operates through an income effect: When competition increases, physicians lose patients, thereby reducing their income. Given diminishing marginal utility of income, physician utility is more responsive to changes in income at lower levels of income, and thus inducing demand—which is assumed to have a constant marginal cost—is now more appealing. This mechanism will lead to an increase in the intensity of service provision, like unnecessary opioid and benzodiazepine prescribing, that physicians might find more profitable than alternative treatment options.[14] Perverse effects of competition on physician behavior are therefore consistent with a range of theoretical underpinnings, although the fact that we find little impact of the law changes on the number of patients that physicians see suggests that demand inducement may not be the main mechanism.

III Data

We use two main data sources to examine how changes in competition affect the prescribing practices of physicians. As outlined below, our primary provider-level data on prescriptions come from the IQVIA LRx database, and information on state-level changes in scope-of-practice legislation for NPs come from McMichael and Markowitz (2020). We supplement these data with information from three additional sources. First, to construct measures of prescriptions and providers per capita, we use population counts at the county-year level from the five-year American Community Survey (ACS).[15] Second, provider-level data from the annual public use Medicare Part B and D files are used to examine impacts on the prescribing of additional drug classes and on the number of office visits. Finally, information from the National Vital Statistics System (NVSS) is used to investigate whether competition-induced changes in prescribing affect fatal drug overdoses at the county-year level.

[14] Although physicians do not directly increase their profits by prescribing opioids as they would, for example, by performing C-sections instead of vaginal deliveries, prescribing opioids takes little time and may make patients more likely to return, leading to more billing for office visits.

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

III.A Prescription data

The primary prescription data that we use come from IQVIA, a public company specializing in pharmaceutical market intelligence. These data include detailed information on most opioid, anti-anxiety, and antidepressant prescriptions written in the United States from 2006 to 2018.[16]

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

Second, the data have an (anonymized) patient identifier and basic patient information such as location and age. The patient identifiers allow us to track the prescriptions for a given patient over time. This in turn allows us to identify patients who are starting new medications ("naïve" patients) and to measure instances of co-prescribing of medications to the same patient. Moreover, as outlined in Appendix C, prescription-specific information on each patient's zip code is used to construct a provider-year–level panel of practice locations over our sample period.[17] Information on patient characteristics such as age, gender, and

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

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

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

Finally, the data have detailed information on the prescription being dispensed, including the National Drug Code (NDC) of the product, the strength of the medication, and the number of pills. We use the Food and Drug Administration's (FDA's) NDC data to determine which products are controlled substances.[18] Information on the size and strength of prescriptions is used to examine intensive margin measures such as average daily MME per opioid prescription.

Because the law changes that we consider only concern the ability of NPs to independently prescribe controlled substances, we expect the law changes to have the largest impacts on the prescribing of controlled substances. Hence, our primary analyses focus on the prescribing of opioids and scheduled anti-anxiety medications like benzodiazepines.[19] We also consider instances in which the same patient receives both an opioid prescription and a benzodiazepine prescription from the same provider on the same day ("co-prescribing"), a practice that the CDC recommends against because it leads to a heightened risk of respiratory failure (CDC, 2016). To consider impacts on the prescribing of drugs that were not directly affected by the law changes, we further examine the prescribing of two types of unscheduled medications that are available in our extract of the IQVIA data (non-controlled anti-anxiety medications and antidepressants) as well as additional unscheduled medication classes that are available in the public use Medicare Part D data (see Section III.C) in supplementary analyses.[20]

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

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

[19] 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 from our data.

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

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

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

III.B Scope-of-practice legislation

In Section IV, we exploit changes in scope-of-practice legislation regulating whether NPs can independently prescribe controlled substances as a shock to the competition facing GPs. These law changes come from McMichael and Markowitz (2020) and capture whether NPs could prescribe controlled substances without the supervision or collaboration of a physician in each year of the sample. This legal change often removes the final barrier to NPs practicing fully without any required physician oversight.

As shown in Figure 1, 18 states allowed NPs to independently prescribe controlled substances as of 2005. Over our study period (2006–2018), 18 states relaxed their scope-of-practice restrictions and granted NPs the ability to prescribe these medications independently. The geographic distribution of these states is diverse, with two states in the West, seven in the South, five in the Midwest, and four in the Northeast granting independent prescriptive authority for controlled substances over the period.

Table 2 provides an overview of prescribing patterns among GPs (panel (a)) and NPs (panel (b)) in the 33 states in which the relevant scope-of-practice laws did not change (columns (1)–(3)) and the 18 states in which the laws did change (columns (4)–(6)) over the sample period.[21] For controlled substance prescriptions of each type written by either GPs or NPs in each group of states, we consider the number of prescriptions per 1,000 people, the number of prescribing providers per 1,000 people, and the average number of prescriptions per prescribing provider at the county-year level. As in Table 1, we provide statistics over the entire sample period (columns (1) and (4)) and separately for the first and last year of the sample (columns (2) and (5) and columns (3) and (6), respectively).

The number of prescriptions per 1,000 people written by GPs and NPs were similar, and if anything slightly higher, in control states than in treatment states over our sample period. This is true even for most drug types in 2018, the point by which NPs were allowed to independently prescribe controlled substances in all 18 treatment states. We observe NPs prescribing in control states because (1) 18 of these states granted NPs independent prescriptive authority before our sample period and (2) NPs were allowed to prescribe controlled substances either in collaboration with or under the supervision of a physician in the 15 other control states over our sample period. Looking to the number of prescribing providers per capita, we see that the concentration of prescribing GPs and NPs was also relatively similar in treatment and control states. However, the average number of prescriptions per prescribing provider was higher in control states. This observation suggests 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

[21] We group "always-taker" and "never-taker" states in Table 2. We show in Section IV.D that our empirical findings are insensitive to whether we use a control group that consists only of never-taker states or includes both never-taker and always-taker states.

that might influence prescribing patterns. To examine whether our identifying variation is orthogonal to changes in local socio-demographics such as the age, racial, and educational structure, we estimate balancing regressions that use these candidate controls as dependent variables (Pei et al., 2019).[22] 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.

III.C Medicare data

Additional outcomes come from two data sets covering services provided to patients covered by Medicare. First, to examine impacts on additional drug classes that are not included in our extract of the IQVIA data, we use data on prescriptions paid for by Medicare Part D at the provider-year level. These data cover the period 2012–2018 and are made publicly available by the Centers for Medicare and Medicaid Services (CMS).[23] 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 antidiuretics. The first four drug classes are used to validate our findings in the IQVIA data using an alternative data source, whereas the last four drug classes are used to extend the analysis to the prescribing of additional types of non-controlled substances. Second, to examine potential impacts on the frequency of 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. These data are also publicly available from CMS and cover the period 2012–2018.[24] We combine these data sources with information on the number of individuals aged 65 and

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

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

[24] 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.

older at the county-year level from the ACS to construct measures of prescriptions and office visit per capita among the Medicare population.

III.D Mortality data

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

IV Effects of law changes on prescribing practices

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

IV.A Graphical evidence

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

The subfigures show a positive relationship between within-county changes in the number of prescribers per capita and the number of opioid prescriptions (panel (a)), controlled anti-anxiety prescriptions (panel (b)), and opioid and benzodiazepine co-prescriptions (panel (c)) per capita and per prescribing GP. While the positive association between the number of prescribers and prescriptions per capita may just reflect the impact of better health care access in areas with 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 in areas in which there is a greater concentration of other providers available to prescribe. However, while suggestive, these figures do not directly investigate the role of competition per se in driving increases in prescribing.

To 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 that took place between 2009 and 2015. As discussed below, the results are robust to including the full set of 18 law changes that took place between 2006 and 2018 (i.e., to not using a balanced panel) and to either including or excluding states with law changes between 2006–2008 and 2016–2018 from the set of control states.

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

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

where $1\{t^*_s + n = t\}$ is an indicator denoting whether year t for state s is n years from the law change; B_s is an indicator denoting whether state s is part of the balanced panel; X_{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.[25] The year before the law change ($n = -1$) is the omitted category, and standard errors are clustered by state. Because of the balanced panel restriction, the coefficients $[\alpha_{-3}, \alpha_3]$ are identified by a consistent sample of states.

We begin by considering the impacts of the law changes on the county-year number of controlled substance prescriptions written by providers of any type per 1,000 people. Results from estimation of equation (2) are presented in Figure 3.[26] Panels (a) and (b) show that there were no significant differences in trends in opioid and controlled anti-anxiety prescribing per 1,000 people between treatment and control counties in the years before the law changes. However, the prescribing of opioids and controlled anti-anxiety medications jumped when NPs were granted the authority to independently prescribe controlled substances. These effects were largely stable in the years following the law changes for opioids, while the effects steadily increased over the next three years for scheduled anti-anxiety medications. As shown in panel (c), co-prescribing of opioids and benzodiazepines per 1,000 people likewise increased when NPs were granted independent prescriptive authority. While there is some suggestion of a pre-trend for co-prescribing, there is nevertheless a clear jump in the year of the law

[25]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 for GPs are robust to including county-specific time trends that are predicted using only pre-period data, to excluding time trends, and to including state-specific rather than county-specific linear time trends.

[26]Results in Figure 3 are conditional on county-specific linear time trends estimated over the entire sample period. Results from specifications that exclude time trends and those for models that include county-specific time trends predicted using only pre-period data (Goodman-Bacon, 2021) are presented in Figure A2(a). The results from these alternative specifications are very similar.

change that persists for at least three years.

Figure 4 presents event studies that are analogous to those presented in Figure 3 except that they show the prescribing of controlled substances separately by NPs (left subfigures) and GPs (right subfigures).[27] The left subfigures show that prescribing per 1,000 people of opioids (panel (a)), controlled anti-anxiety medications (panel (b)), and co-prescribing of opioids and benzodiazepines (panel (c)) by NPs rose once they were granted the ability to prescribe these medications independently. These findings are not particularly surprising given that such increases were arguably the intent of the law changes. Strikingly, however, the right subfigures show that the prescribing of these medications by GPs also jumped when NPs were allowed to prescribe independently. If patients had merely switched from GPs to NPs following the law changes, prescribing among GPs should have fallen in tandem with the rise in NP prescribing. Hence, the simultaneous increases among NPs and GPs suggest a behavioral response on the part of GPs facing increased competition after the law changes.

IV.B Primary estimates

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

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

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

[27] As shown in Figure A2(c), the inclusion of time trends has little effect on the estimates for GPs. However, the inclusion of county-specific linear time trends—estimated over the entire sample period or predicted using only pre-period data—corrects for negative pre-trends in the outcomes among NPs (Figure A2(b)).

used to identify β_1 are observed for the entirety of this three-year post-period.

Results from estimation of equation (3) are shown in Table 3. As in Figures 3 and 4, we consider the number of prescriptions per county-year written by all providers (columns (1)–(3)), NPs (columns (4)–(6)), and GPs (columns (7)–(9)) per 1,000 people for opioids, controlled anti-anxiety medications, and opioid and benzodiazepine co-prescribing. Panel (a) shows estimates using the full sample, and panel (b) shows estimates for the balanced panel of states that are observed for at least three years before and after NPs gained independent prescriptive authority for controlled substances. Panels (c) and (d) also consider this balanced panel but use the estimators proposed by Sun and Abraham (2021) and Borusyak et al. (2022), respectively, to correct for potential biases in two-way fixed effects models with staggered treatment adoption and heterogeneous treatment effects.

All four panels indicate that there are positive effects of allowing NPs to independently prescribe controlled substances on our primary prescription outcomes. However, comparing panels (a) and (b) suggests that it is important to consider a balanced panel—and that doing so yields somewhat larger effects on the prescribing of GPs. Comparing panels (b) and (c) suggests that given this balanced panel, correcting for staggered treatment adoption using the estimator proposed by Sun and Abraham (2021) increases the point estimates while reducing the standard errors. Comparing panels (b) and (d), we see that the estimates for GPs and NPs are very similar when using the estimator proposed by Borusyak et al. (2022), although the aggregate effects are slightly attenuated. Given the robustness of our main effects across estimators, we focus on estimates from the balanced panel without these proposed corrections moving forward.

The estimated effects in Table 3 are large. As shown in columns (1)–(3) of panel (b), allowing NPs to independently prescribe controlled substances leads to 44.3 more opioid prescriptions (8.8 percent relative to the baseline mean), 13.2 more controlled anti-anxiety prescriptions (7.5 percent), and 4.5 more co-prescriptions of opioids and benzodiazepines (16.7 percent) per 1,000 people at the county-year level. It is notable that the effect on co-prescribing, an unambiguously dangerous practice, is so large.

Looking to the results by provider type, we see in columns (4)–(6) that the estimated effects on NP prescribing are positive, as expected. However, as shown in columns (7)-(9), the estimates for GPs are much larger in levels, and the impacts on all three prescription outcomes are statistically significant. Comparing the estimates for GPs to those for all providers indicates that more than half of the total increases in prescribing come from GPs. The estimates in panel (b) show that granting NPs independent prescriptive authority for controlled substances leads the number of prescriptions written by GPs per 1,000 people at the county-year level to increase by 23.3 opioid prescriptions (10.0 percent relative to the GP-specific baseline mean), 8.5 controlled anti-anxiety prescriptions (7.9 percent), and 2.7 opioid and benzodiazepine co-prescriptions (16.3 percent).

IV.C Additional analyses

Intensive vs. extensive margin adjustments The increases in prescribing observed in Table 3 could come either from additional providers starting to prescribe a certain drug type (extensive margin adjustments) or from existing prescribers increasing their prescription levels (intensive margin adjustments). To shed light on these mechanisms, we examine two additional sets of outcomes. First, to examine extensive margin adjustments, we consider 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 (e.g., opioids) per 1,000 people at the county-year level. However, since very small increases in prescribing—for example, a provider moving from zero to one prescription of a given type per year—is unlikely to be relevant for population health and might reflect measurement error in the data, we also consider a measure of "frequent" prescribing. In particular, we consider providers to be frequent prescribers if they both (1) write a given type of prescription in each month (or year for co-prescribing of opioids and benzodiazepines) and (2) are above the 25th percentile among all GPs who satisfy criterion (1).[28] The goal of this measure is to capture the number of providers for whom a given type of prescribing has become a relevant part of their clinical practice. Second, to examine intensive margin adjustments, we consider effects on the average number of prescriptions of a given type written by prescribing providers of a given type at the county-year level.

[28] We only require providers to co-prescribe opioids and benzodiazepines at least once in a given year (rather than monthly) since co-prescribing is a relatively rare outcome. The 25th percentile for criterion (2) is measured across all years in the sample period.

Results from estimation of equation (3) using these additional outcome measures are shown in Table 4 for NPs (columns (1)–(3)) and GPs (columns (4)–(6)). Perhaps surprisingly, the results in panel (a) show that the law changes do not draw new providers of either type into prescribing controlled substances. Moreover, as shown in panel (b), there are no significant increases in the number of "frequent" prescribers.[29] Rather, increases in prescribing come 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 27.8 more opioid prescriptions, 12.1 more controlled anti-anxiety prescriptions, and 4.6 more opioid and benzodiazepine co-prescriptions per year. Compared to the respective baseline means, these estimates reflect increases of 9.9, 8.3, and 15.0 percent. While the point estimates are about half as large among prescribing NPs, the percent effects are even more pronounced given substantially lower baseline means among these providers.

Opioid prescribing and mortality To probe how competition affects opioid prescribing in particular, we conduct three additional sets of analyses. First, a distinction is often made in the literature between opioid-naïve and non–opioid-naïve patients. If physicians respond to increased competition by writing opioid prescriptions for naïve patients, then competition could have important implications for the initiation of opioid use and possible future opioid abuse. To examine effects by patient type, we divide prescriptions based on whether they were written for a patient who did not have an opioid prescription from any provider in the past six months ("opioid naïve") and patients who did have a prescription ("non-opioid naïve"). Second, since larger opioid prescriptions carry additional risk of physical dependence and misuse (CDC, 2016), we examine effects on the average days supplied and the average daily MME per prescription. We also consider the number of opioid prescriptions with greater than 120 MME daily per 1,000 people given work documenting that prescriptions of this size are strongly correlated with adverse patient outcomes (Sullivan et al., 2010; Bohnert et al., 2011).[30]

[29] Figure A3 shows results from specifications that vary the percentile of GP prescribing that serves as the threshold for "frequent" prescribers. For most outcomes, the results are, if anything, more pronounced with a higher threshold.

[30] Prescriptions with more than 120 MME daily have been commonly used in the literature as a measure of risky prescription opioid use (e.g., Finkelstein et al., 2022).

Results from these analyses are shown in Table 5 for NPs (columns (1)–(3)) and GPs (columns (4)–(6)). Panel (a) shows that the increases in opioid prescribing are mainly driven by prescriptions for opioid-naïve patients. Allowing NPs to prescribe controlled substances independently leads GPs to write 22.5 more opioid prescriptions for opioid-naïve patients per 1,000 people at the county-year level (11.3 percent relative to the baseline mean) versus only 0.79 more opioid prescriptions for non-naïve patients (2.4 percent; estimate insignificant at conventional levels). This suggests that competition-induced increases in opioid prescribing put additional patients at risk of developing opioid use disorder.

The rest of Table 5 shows that the law changes do not affect the length of prescriptions, either for opioid-naïve or non-naïve patients (panel (b)). However, there are increases in the average MME per day supplied among both opioid-naïve and non-naïve patients, with the increases being almost 50 percent larger for non-naïve patients (panel (c)). The number of prescriptions with over 120 MME per day also increases among opioid-naïve patients (panel (d)). Given that the CDC recommends that providers start patients on the lowest effective dose and that they "avoid" or "carefully justify" increasing dosage to greater than 90 MME per day (CDC, 2016), this result is especially striking.

Finally, to ask how granting NPs the ability to prescribe controlled substances independently affects drug overdose deaths, we estimate analogues of equation (3) using the county-year number of fatal drug overdoses per million people as the outcome. Because event-study results from estimation of equation (2) suggest that any mortality effects take at least a year following the law changes to surface (see Figure A4), we include a separate indicator for the year of the law change and consider the effects in years 1–3 (rather than years 0–3) when estimating equation (3) using mortality outcomes.

Table 6 reports results from this analysis using fatal drug overdoses per million people involving any drug (column (1)), any opioid (column (2)), and prescription opioids (column (3)) as the outcome. In specifications without time trends (panel (a)), there is a significant increase in fatal drug overdoses involving prescription opioids, with the law changes leading to 8.7 more prescription opioid fatalities per million people (18.4 percent relative to the baseline mean). However, as shown in panels (b) and (c), this effect is attenuated when we add either county-specific pre-trends following Goodman-Bacon (2021) or county-specific time trends estimated over the entire sample period.[31]

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

IV.D Robustness

The results of several robustness checks are summarized in Figure A5, which shows that the results are remarkably consistent. In addition to showing estimates for the unbalanced panel and with the Sun and Abraham (2021) and Borusyak et al. (2022) corrections (as in Table 3), the figure provides estimates omitting demographic controls, controlling for the state-level adoption of must-access prescription drug monitoring programs (PDMPs) as a potential confounder,[32] omitting time trends, adding state-specific rather than county-specific linear time trends, including county-specific linear time trends predicted using only pre-period data (Goodman-Bacon, 2021), excluding states with law changes over our sample period but outside of the balanced panel window from the set of control states, and taking only "never-taker" states as the controls. The final row in each subfigure considers results excluding methadone and buprenorphine from our definition of "opioids." The results are nearly identical when these medications are excluded, which demonstrates that our findings

[31] We include county-specific trends in our primary specification for prescription outcomes, as event studies show pre-trends in prescribing among NPs in the absence of such controls (see Figure A2). In contrast, as shown in Figure A4, event studies for mortality show no pre-trends when time trends are not included (subfigure (a)), whereas including county-specific linear time trends introduces negative pre-trends (subfigure (c)). While the model without time trends (or with county-specific pre-trends) therefore appears to be more valid when examining drug mortality, we conclude that there is only suggestive evidence that laws granting NPs independent prescriptive authority affected drug overdose deaths.

[32] 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). Balancing regressions show that our identifying variation is orthogonal to state-level opioid legislation such as the adoption of must-access PDMPs. It is therefore unsurprising that our results are unaffected by the inclusion of such controls.

are not driven by changes in the provision of medications that can be used for the treatment of opioid use disorder rather than pain management.

The only change that noticeably affects our results is the omission of time trends in the regressions for prescription outcomes among NPs (subfigure (b)). This is not surprising: As shown in Figure A2, there are negative pre-trends among NPs in specifications without location-specific time trends. Since these pre-treatment differences in outcome trends bias the effects downward, the estimates for NPs in specifications without time trends are smaller than in specifications with state- or county-specific time trends (whether trends are predicted using only pre-period data or estimated over the entire sample period). Our primary specification therefore includes county-specific linear time trends, as there are no significant pre-treatment differences between treatment and control states in the prescription outcomes for NPs once we condition on these controls. We note, however, that our primary results focusing on the impacts of the law changes on GPs are not meaningfully affected by the inclusion (or exclusion) of time trends or by any of the other alternative specifications that we consider.

Finally, Figure A6 asks whether our results are driven by counties in a particular treatment state. In our baseline specifications, we weight county-year observations by population since there is likely more noise in the prescription outcomes of less populous counties. As such specifications are by construction more reflective of treatment effects in more populous counties, we begin by presenting results from estimation of equation (3) excluding population weights. Comparing the top two rows in each subfigure of Figure A6, we see that excluding population weights generally leads to larger standard errors, as expected. However, the point estimates are not sensitive to whether observations are weighted by population, highlighting that our effects are not only reflective of impacts in large counties. The remaining rows in Figure A6 subsequently drop each treatment state one at a time from this unweighted specification. The point estimates are very similar regardless of which state is excluded, indicating that our results are not driven by counties in a single state.

V Mechanisms

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

V.A Role of competition

We conduct three sets of tests to probe whether it is indeed competition from NPs that is driving the increases in prescribing among GPs. First, we ask whether the effects are more pronounced in areas in which GPs face greater competition from NPs. In particular, counties are divided into two groups based on whether they had an above- or below-median number of NPs per GP among treatment states at the start of the sample period. We then estimate an augmented version of equation (3) that includes an interaction between the treatment indicator and an indicator denoting whether the county had an above-median number of NPs per GP in 2006. 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 7. The estimates bear out the hypothesis that GPs respond more strongly to the law changes in counties in which NPs are more of a competitive threat to GPs: the estimated effects for opioids (column (1)), controlled anti-anxiety medications (column (2)), and co-prescribing of opioids and benzodiazepines (column (3)) among GPs are 65.2, 118.4, and 68.7 percent higher, respectively, in counties with an above- versus below-median number of NPs per GP in 2006. Moreover, all of the impacts in the above-median counties are strongly statistically significant, whereas the estimates for opioids and controlled anti-anxiety medications are significant only at the 10 percent level in the below-median counties.

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

Table 8 tests the hypothesis that physicians who face more direct competition from NPs will respond more strongly to the law changes. For reference, column (1) repeats the estimates for GPs from panel (b) of Table 3. As shown in columns (3) and (4), physicians in psychiatry/neurology and obstetrics/gynecology respond to increased competition from NPs by writing more opioid prescriptions (panel (a)), controlled anti-anxiety prescriptions (panel (b)), and co-prescriptions of opioids and benzodiazepines (panel (c)). Physicians in emergency medicine (column (2)) also respond by writing more opioid prescriptions. 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 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 8 focuses on surgeons. As shown in column (5), orthopedic surgeons increase their prescribing of opioids when NPs are allowed to prescribe controlled substances independently. This result is expected given that orthopedic surgeons may face some competition in the form of alternatives to their services (e.g., pain management) from NPs. As predicted, there are no statistically significant effects for general surgeons (column (6)), a class of physicians who likely face little competitive pressure from NPs.

Finally, we ask whether the effects on prescribing are concentrated among controlled substances. While the prescribing of non-controlled substances like antibiotics might also be responsive to competitive pressures, the law changes that we consider most directly influence the competitive landscape for controlled substances. We therefore anticipate that the impacts of the law changes will be larger for controlled substance prescribing. To examine effects on the prescribing of non-controlled substances, we use both the IQVIA data and the public use Medicare Part D files. As outlined in Section III, we have information on the prescribing of non-controlled anti-anxiety medications and antidepressants from IQVIA and information on the prescribing of non-controlled anti-anxiety medications, antidepressants, antihypertensives, cholesterol medications, antibiotics, and antidiuretics from the public use Medicare Part D files. Given the limited time frame available in the Medicare data (2012–2018), we consider the effects of the law changes from two years before to two years after among the balanced panel of seven states that granted NPs the ability to independently prescribe controlled substances between 2014 and 2016 in these analyses.[33] To make the samples more comparable in the IQVIA and Medicare data, we further focus on prescriptions to those aged 65 and older in the IQVIA data in these analyses, although we verify that the results for non-controlled substance prescribing in the IQVIA data are robust to using the same sample of years and patients as in our primary analysis.

We begin by confirming that we observe positive impacts of the law changes on controlled substance prescribing among GPs when focusing on prescriptions to the Medicare population and using the truncated sample period. In particular, Figure 5(a) provides event-study results from estimation of equation (2) using either the county-year number of opioid prescriptions (left subfigure) or controlled anti-anxiety prescriptions (right subfigure) written by GPs for those aged 65 and older in the IQVIA data (light dots and bars) and paid for by Medicare Part D in the public use Medicare files (dark dots and bars) per 1,000 people

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

aged 65 and older from 2012 to 2018. While we unsurprisingly lose some precision when using these more limited samples, we nevertheless see clear evidence of increases in opioid and controlled anti-anxiety prescribing by GPs following the law changes. Moreover, while the point estimates are larger than those observed when considering prescriptions for all patients from 2006 to 2018 in Figure 4, the effect sizes relative to the respective baseline means are very similar. For example, the effects on opioid prescribing among GPs shown in the left subfigure of Figure 5(a) reflect increases of around 9 percent relative to the respective baseline means, whereas we observed a 10 percent increase in opioid prescribing by GPs in our baseline specification in Table 3.

Figure 5(b) shows analogous results for the prescribing of non-controlled substances. To allow for a more direct comparison with our estimates for controlled substance prescribing in Figures 4 and 5(a), in which the y-axes extend to at least one-third of the baseline mean, we scale the y-axes in Figure 5(b) to range from -33 to +33 percent of the baseline mean of each outcome. As anticipated, the effects of the law changes on non-controlled substance prescribing are much less pronounced than the effects on controlled substance prescribing. While there is some evidence that the prescribing of non-controlled anti-anxiety medications may have gradually fallen following the law changes, which would be consistent with the replacement of some non-controlled anti-anxiety medications with controlled alternatives such as benzodiazepines, there are no measurable effects on most of the non-controlled medication classes considered.[34]

V.B Ruling out alternative mechanisms

We next examine whether other changes in physician practices that might occur as a result of law changes allowing NPs to independently prescribe controlled substances play a role in driving our findings. First, we ask whether our results can be explained by changes

[34] Figure A7 replicates our primary analyses, in which we consider prescriptions for all patients in the IQVA data from 2006 to 2018, for non-controlled anti-anxiety medications (left subfigures) and antidepressants (right subfigures). Although limited in precision, 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, although most of the post-treatment event-time estimates are statistically insignificant at conventional levels. Moreover, we do not observe an increase in antidepressant prescribing in the public use Medicare Part D data (Figure 5(b)), and thus these results are less robust than our main findings.

in physician workloads that might result from reductions in administrative or supervisory duties required of GPs. If GPs who were previously collaborating with or supervising NPs have additional time to devote to patient care once NPs can prescribe controlled substances independently, then an increase in controlled substance prescribing might reflect either an increase in the time spent with each patient (which might allow the provider to identify additional ailments requiring treatment) or an increase in the number of patients seen. The null results for non-controlled substance prescribing shown in Figure 5 already provide strong evidence against these possibilities: if GPs are spending more time with each patient or taking on additional patients following the law changes, then their prescribing of non-controlled substances should likewise increase.

To further examine whether GPs see additional patients following the law changes, we examine effects on the number of office visits using the public use Medicare Part B files. As with the public use Medicare Part D files, these data are available for 2012–2018. Given this shorter sample window, we again consider the effects of the law changes from two years before to two years after among the balanced panel of seven states that granted NPs the ability to independently prescribe controlled substances from 2014 to 2016. Figure A8 presents event-study estimates from estimation of equation (2) using two outcomes at the county-year level: (1) the number of Part B office visits with GPs per thousand people aged 65 and over and (2) the average number of Part B office visits per GP. In line with the findings for non-controlled substance prescribing, Figure A8(a) shows that there is no evidence that the number of GP office visits per capita increased as a result of the law changes. As shown in Figure A8(b), the law changes actually led to a reduction of around three office visits per GP, a 1.6 percent reduction relative to the baseline mean of approximately 190 visits with Medicare beneficiaries annually.

A related concern is that our results could be driven by increases in physician workloads resulting from NPs leaving their joint practices.[35] While such changes in workloads should

[35] In addition to NPs, GPs often work with physician assistants (PAs). As their name suggests, PAs work directly under the supervision of a physician and do not have independent prescriptive authority. Table A1 shows that PAs write more prescriptions for opioids and controlled anti-anxiety medications and increase their co-prescribing of opioids and benzodiazepines when NPs are granted independent prescriptive authority. These results show that GP practices additionally respond to the law changes by increasing prescribing among non-physicians who are close substitutes to NPs. It further highlights that GPs often work with other providers who might also be able to absorb any excess workload in the event that NPs leave

also be reflected in non-controlled substance prescribing and in the number of office visits, we can ask whether NPs who were practicing with a physician leave the physician's office to practice elsewhere (e.g., open their own practice) when they can prescribe controlled substances independently. Recall that in our main analyses, we use information on patient zip codes to infer the practice counties of prescribing providers in each year of the sample (see Section III.A and Appendix C). To examine whether independent prescriptive authority affects co-practice patterns among GPs and NPs, we instead use the two snapshots of *exact* practice addresses in 2014 and 2018 provided by IQVIA. Calculating the share of GPs who had at least one NP practicing at their practice address and the average number of NPs per GP practice in each county in these two years, we compare how co-practice patterns changed from 2014 to 2018 in the 15 states that did not allow NPs to independently prescribe controlled substances by 2018 ("never-takers"), the 27 states with law changes in or before 2014 ("always-takers"), and the nine states with law changes between 2015 and 2018 ("switchers").

As shown in Figure A9, around 65 percent of NPs (subfigure (a)) and 60 percent of GPs (subfigure (b)) were practicing at the same address as at least one provider of the other type in 2014. This figure declined for NPs by 2018, with more NPs practicing independently or with physicians in specialties outside of general practice. In contrast, the share of GPs co-practicing with NPs increased by 2018, as did the average number of NPs per GP practice (see Figure A10). Importantly, however, these increases in co-practice patterns among GPs were more pronounced in treatment states. As shown in Figure A9(b), the share of GPs co-practicing with at least one NP increased by 5.5 percentage points from 2014 to 2018 (9.3 percent relative to the baseline mean) in treatment states compared to around 5.0 percentage points (8.0 percent) in both never-taker and always-taker states. Moreover, as shown in Figure A10, the average number of NPs per GP practice in treatment states increased by 3.7 among all GPs (subfigure (a)) and by 4.6 among co-practicing GPs (subfigure (b)), changes that are again more pronounced than those observed in never-taker and always-taker 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.

Finally, we ask whether the observed increases in controlled substance prescribing might

their joint practices.

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

As shown in Figure A11, there is no consistent evidence that allowing NPs to independently prescribe controlled substances affects the types of patients receiving controlled substance prescriptions from all providers (subfigure (a)), NPs (subfigure (b)), or GPs (subfigure (c)). While an occasional estimate is statistically significant, which might reflect spurious associations given the number of outcomes being examined, the patterns are if anything consistent with GPs writing a smaller share of their prescriptions (and NPs writing a larger share of their prescriptions) for patients over 65, a population that is likely to have the most severe health conditions. It is therefore unlikely that our results are driven by changes in the types of patients seen by GPs following the law changes.

VI Conclusion

We document the ways in which the prescribing practices of GPs change following increases in competition precipitated by changes in state-level scope-of-practice laws granting NPs the ability to prescribe controlled substances without physician oversight. We find that GPs respond to such legislation by increasing their prescribing of opioids and controlled anti-anxiety medications such as benzodiazepines. GPs also increase their co-prescribing of opioids and benzodiazepines to the same patient on the same day, a behavior that the CDC strongly cautions against because it can lead to respiratory failure (CDC, 2016).

Three additional tests support the hypothesis that the increases in controlled substance prescribing among GPs following the law changes are driven by increased competition from

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

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

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

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

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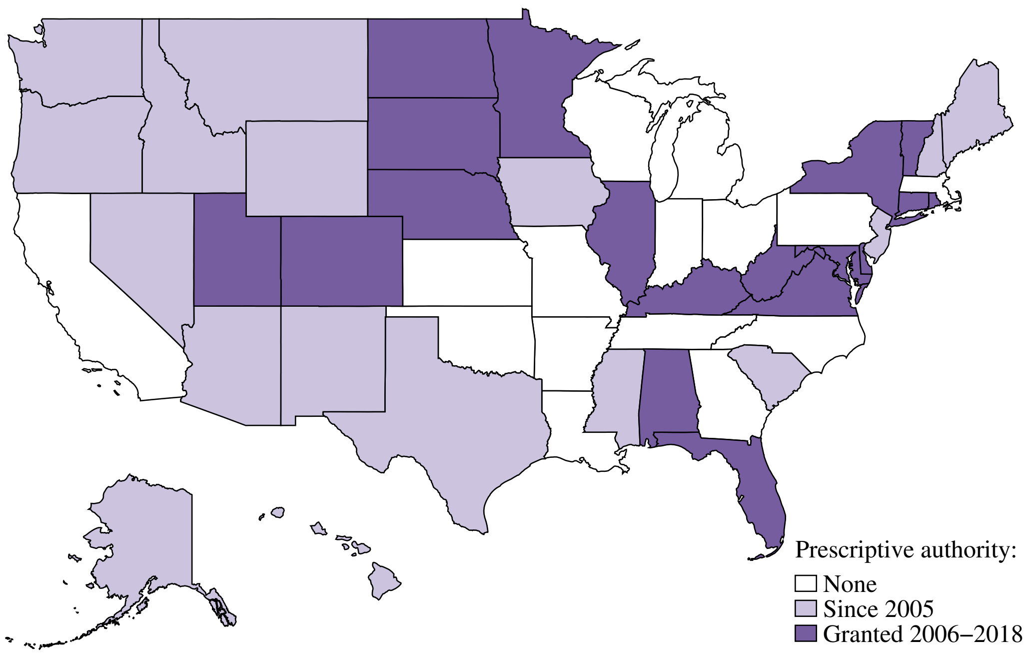
VII Figures

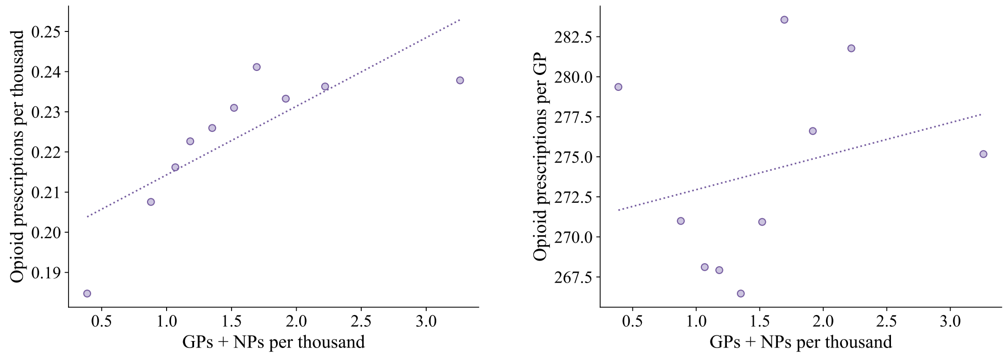
Figure 1: NP independent prescriptive authority for controlled substances: 2006–2018



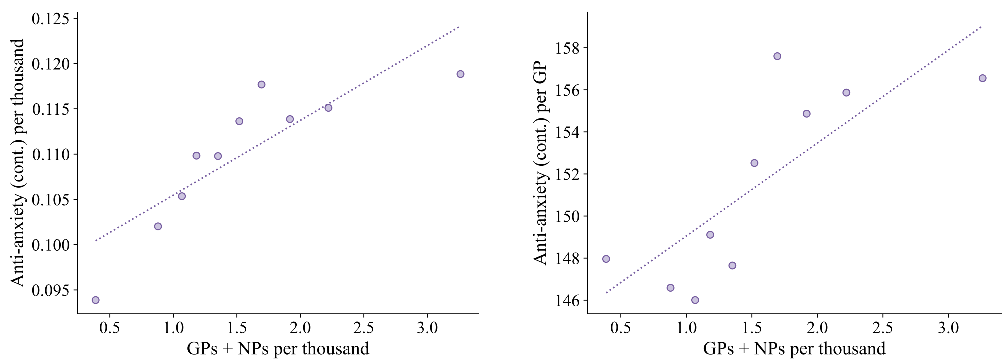
Notes: We consider states as having independent prescriptive authority if nurse practitioners (NPs) registered in the state have the statutory authority to prescribe controlled substances without physician collaboration or supervision. Years in which states granted NPs independent prescriptive authority come from McMichael and Markowitz (2020).

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

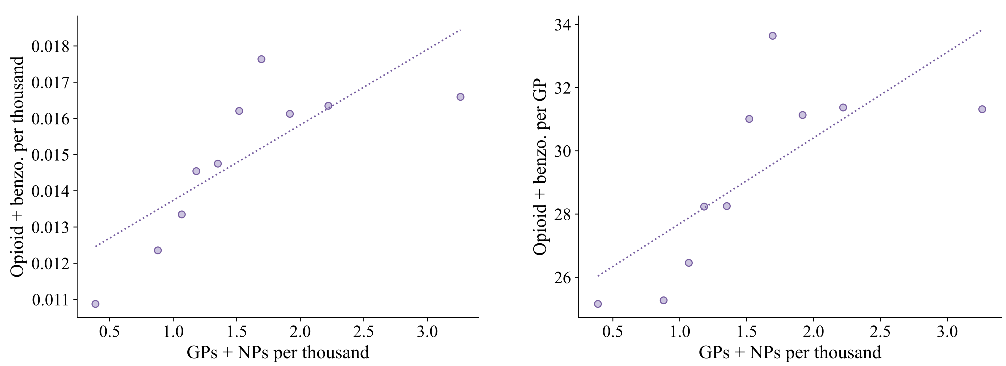
(a) Opioid prescriptions



(b) Controlled anti-anxiety prescriptions



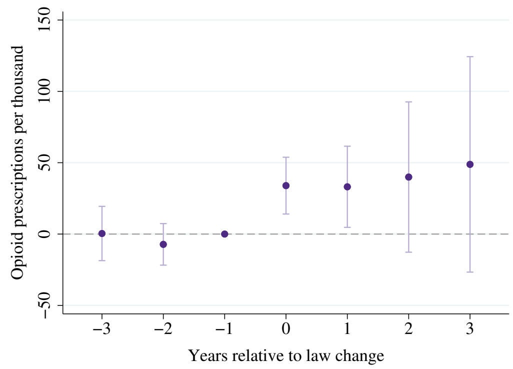
(c) Opioid + benzo. co-prescriptions



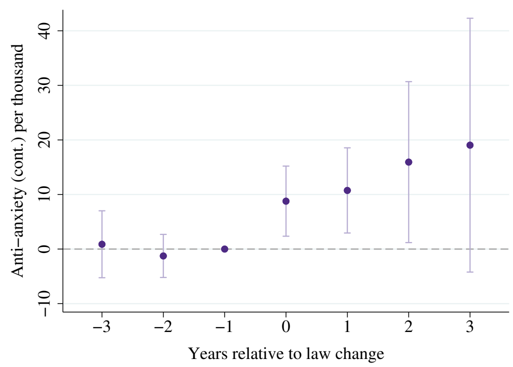
Notes: The above figures show the relationship between changes in the number of general practice physicians (GPs) and nurse practitioners (NPs) per 1,000 people and changes in measures of opioid prescribing (subfigure (a)), anti-anxiety controlled substance prescribing (subfigure (b)), and opioid and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

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

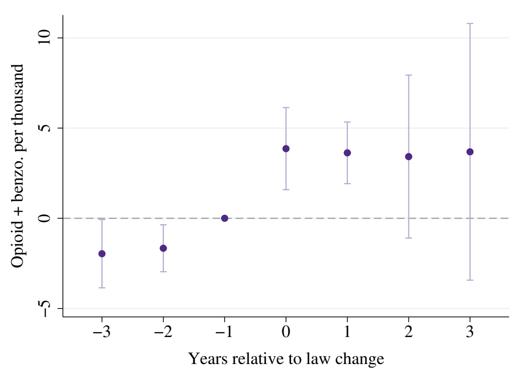
(a) Opioid prescriptions



(b) Controlled anti-anxiety prescriptions



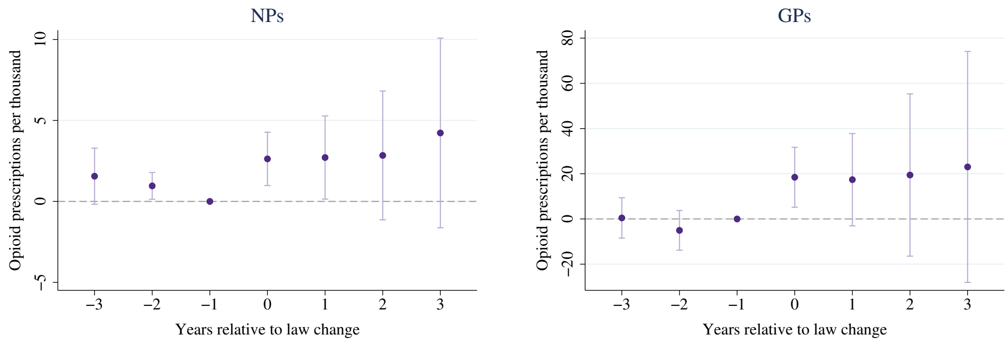
(c) Opioid + benzo. co-prescriptions



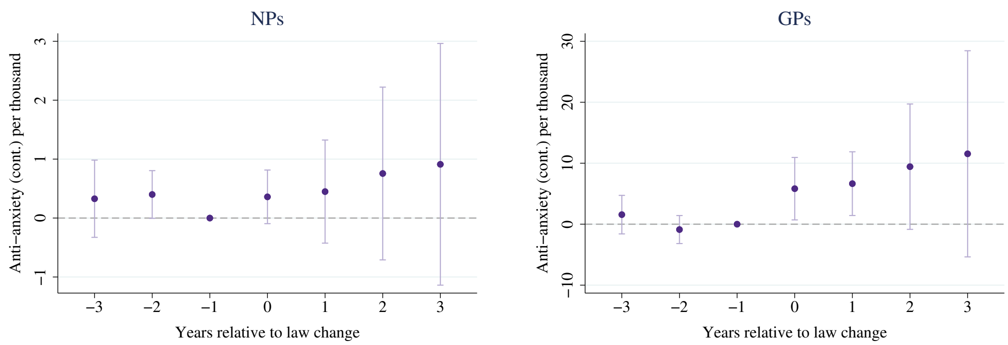
Notes: The above figures present coefficients and 95% confidence intervals from estimation of equation (2) using county-year–level data for 2006–2018. The outcome in subfigure (a) is the number of opioid prescriptions per 1,000 people, the outcome in subfigure (b) is the number of anti-anxiety controlled substance prescriptions per 1,000 people, and the outcome in subfigure (c) is 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. 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, and all time-varying, county-level controls listed in Figure A1. Standard errors are clustered by state. Outcome data come from the IQVIA LRx database.

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

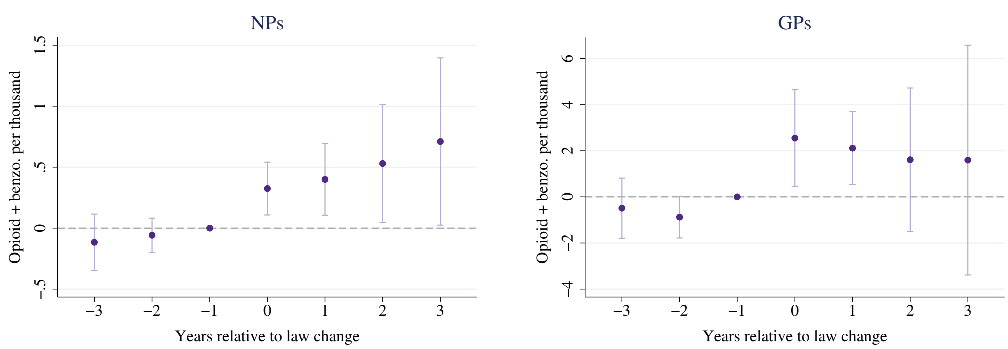
(a) Opioid prescriptions



(b) Controlled anti-anxiety prescriptions



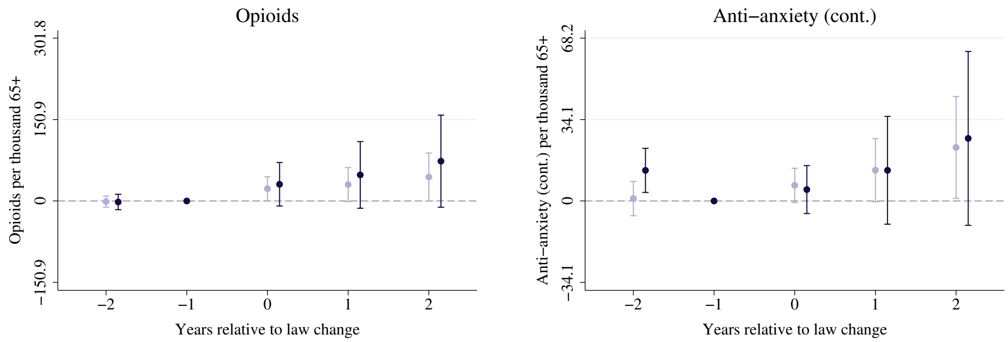
(c) Opioid + benzo. co-prescriptions



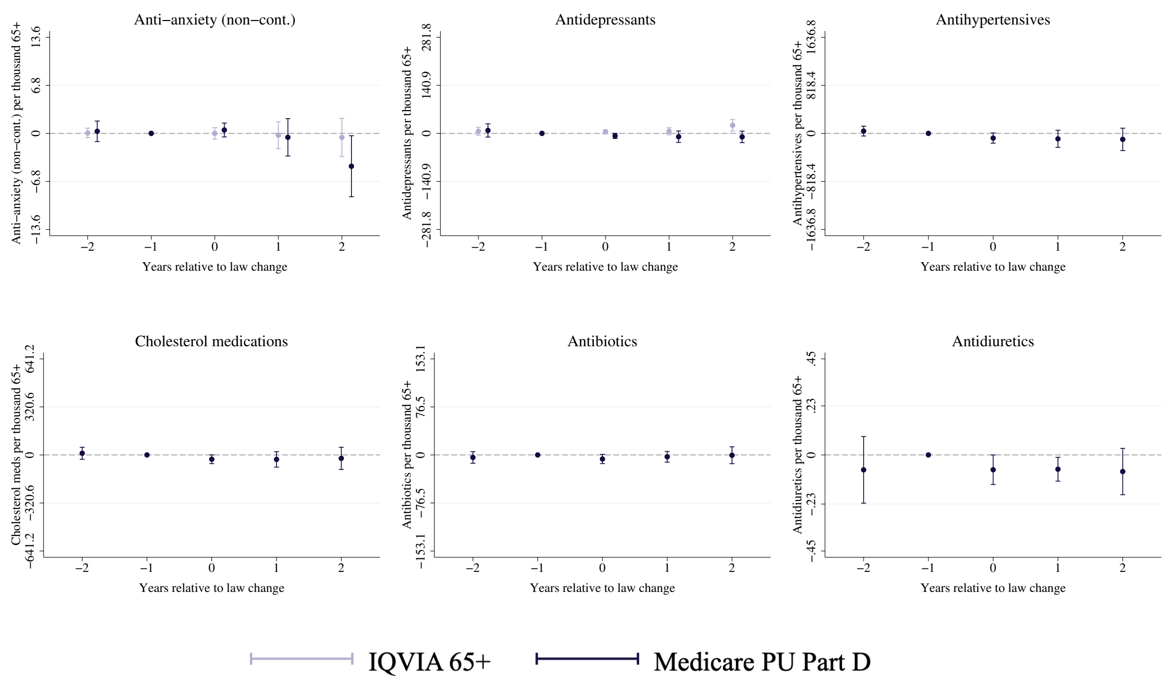
Notes: The above figures present coefficients and 95% confidence intervals from estimation of equation (2) using county-year–level data for 2006–2018. The left (right) subfigure in each subplot only considers prescriptions written by nurse practitioners [NPs] (physicians in general practice [GPs]). The outcome in subfigure (a) is the number of opioid prescriptions per 1,000 people, the outcome in subfigure (b) is the number of anti-anxiety controlled substance prescriptions per 1,000 people, and the outcome in subfigure (c) is 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. 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, and all time-varying, county-level controls listed in Figure A1. Standard errors are clustered by state. Outcome data come from the IQVIA LRx database.

Figure 5: Effects of NP independent prescriptive authority on controlled and non-controlled substance prescribing by GPs (IQVIA and Medicare data, 2012–2018)

(a) Controlled substances



(b) Non-controlled substances



Notes: The above figures present coefficients and 95% confidence intervals from estimation of equation (2) using county-year–level data for 2012–2018. The outcome in each subfigure is 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 7 states with law changes between 2014–2016. The regressions include county and year fixed effects and all time-varying, county-level controls listed in Figure A1. Standard errors are clustered by state. See Figure A7 for analogous figures for non-controlled substance prescribing in the IQVIA data for 2006–2018.

VIII Tables

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

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

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

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

	Control states			States with law changes		
	2006–2018	2006	2018	2006–2018	2006	2018
	(1)	(2)	(3)	(4)	(5)	(6)
Number of states	33			18		
a. General practice physicians						
Prescriptions per thousand						
Opioids	231.2	216.4	159.5	207.7	197.8	141.8
Anti-anxiety (cont.)	109.8	95.45	92.73	110.5	96.26	94.86
Opioid + benzo.	14.89	12.41	9.122	14.86	12.20	8.798
Prescribing providers per thousand						
Opioids	0.809	0.747	0.787	0.845	0.780	0.814
Anti-anxiety (cont.)	0.714	0.671	0.693	0.742	0.691	0.717
Opioid + benzo.	0.492	0.483	0.403	0.488	0.467	0.404
Average prescriptions per prescribing provider						
Opioids	286.5	288.3	203.2	247.6	252.9	174.9
Anti-anxiety (cont.)	153.5	141.1	133.7	147.2	135.4	130.2
Opioid + benzo.	29.56	25.15	22.14	28.39	23.76	20.25
Unique providers	290,476	161,240	204,056	161,930	79,891	101,239
b. Nurse practitioners						
Prescriptions per thousand						
Opioids	37.35	12.86	52.23	27.17	11.16	38.52
Anti-anxiety (cont.)	14.99	4.826	23.63	11.27	3.702	20.56
Opioid + benzo.	1.753	0.577	1.647	1.191	0.343	1.381
Prescribing providers per thousand						
Opioids	0.286	0.142	0.420	0.284	0.167	0.394
Anti-anxiety (cont.)	0.238	0.110	0.379	0.224	0.117	0.362
Opioid + benzo.	0.111	0.048	0.151	0.096	0.045	0.135
Average prescriptions per prescribing provider						
Opioids	101.6	61.74	105.6	72.72	48.67	80.34
Anti-anxiety (cont.)	49.56	29.70	55.35	36.28	22.69	51.28
Opioid + benzo.	11.44	6.846	8.768	8.464	5.582	7.772
Unique providers	186,708	35,791	134,699	101,130	20,817	67,065

Notes: Observations are at the county-year level, and averages are weighted by population. "States with law changes" refers to states that granted NPs the ability to independently prescribe controlled substances between 2006 and 2018; "Control states" refers to all other states. "Opioid + benzo." denotes instances of co-prescribing of an opioid and a benzodiazepine to the same patient by the same provider on the same day. Data come from the IQVIA LRx database.

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

	All providers			Nurse practitioners			General practice physicians		
Prescriptions per 1,000:	Opioids (1)	Anti-anxiety (2)	Opioid + benzo. (3)	Opioids (4)	Anti-anxiety (5)	Opioid + benzo. (6)	Opioids (7)	Anti-anxiety (8)	Opioid + benzo. (9)
a. Unbalanced panel									
Post law change	44.113	11.331	4.088	5.184	2.977	0.651	19.286	5.356	2.243
	(11.951)	(3.810)	(1.436)	(2.096)	(1.733)	(0.267)	(8.337)	(3.570)	(1.235)
	[<0.001]	[0.005]	[0.006]	[0.017]	[0.092]	[0.018]	[0.025]	[0.140]	[0.075]
Relative to mean	0.080	0.063	0.150	0.203	0.313	0.491	0.083	0.050	0.137
b. Balanced panel									
Post law change, 0–3 years	44.279	13.222	4.467	2.643	0.388	0.454	23.296	8.452	2.660
	(16.575)	(4.565)	(1.148)	(1.405)	(0.431)	(0.192)	(11.320)	(3.276)	(1.031)
	[0.010]	[0.006]	[<0.001]	[0.066]	[0.372]	[0.022]	[0.045]	[0.013]	[0.013]
Relative to mean	0.088	0.075	0.167	0.104	0.041	0.343	0.100	0.079	0.163
c. Sun and Abraham (2021)									
Post law change, 0–3 years	46.108	14.086	5.364	2.190	0.285	0.411	25.488	9.232	3.353
	(13.462)	(3.304)	(0.686)	(1.056)	(0.395)	(0.149)	(8.734)	(2.008)	(0.442)
	[0.001]	[<0.001]	[<0.001]	[0.043]	[0.473]	[0.008]	[0.005]	[<0.001]	[<0.001]
Relative to mean	0.091	0.080	0.201	0.086	0.030	0.311	0.110	0.086	0.206
d. Borusyak, Jaravel, and Spiess (2022)									
Post law change, 0–3 years	36.242	10.105	3.755	2.522	1.429	0.642	23.555	6.974	3.055
	(15.719)	(4.218)	(1.982)	(2.401)	(0.916)	(0.474)	(8.687)	(2.922)	(1.190)
	[0.021]	[0.017]	[0.058]	[0.293]	[0.119]	[0.175]	[0.007]	[0.017]	[0.010]
Relative to mean	0.072	0.058	0.140	0.099	0.151	0.486	0.101	0.065	0.188
Baseline mean	504.4	175.5	26.75	25.45	9.473	1.322	232.1	107.4	16.28
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. "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 prescriptions written by all providers, columns (4)–(6) consider prescriptions written by nurse practitioners, and columns (7)–(9) consider prescriptions written by physicians in general practice. Panel (a) considers the effects of all 18 law changes from 2006 to 2018, panel (b) only considers the effects 0–3 years after the law change in the 11 states with law changes from 2009 to 2015, and panels (c) and (d) apply the estimators proposed by Sun and Abraham (2021) and Borusyak et al. (2022) to this balanced panel, respectively. The regressions include county and year fixed effects, county-specific linear time trends, and all time-varying, county-level controls listed in Figure A1. The baseline mean is measured as the average across all counties in 2010. Standard errors are clustered by state. Outcome data come from the IQVIA LRx database.

Table 4: Effects of NP independent prescriptive authority on extensive and intensive margins of controlled substance prescribing

	Nurse practitioners			General practice physicians		
	Opioids	Anti-anxiety	Opioid + benzo.	Opioids	Anti-anxiety	Opioid + benzo.
	(1)	(2)	(3)	(4)	(5)	(6)
a. Prescribing providers per thousand						
Post law change, 0–3 years	0.000	–0.014	–0.003	–0.007	–0.013	–0.001
	(0.010)	(0.006)	(0.003)	(0.012)	(0.011)	(0.008)
	[0.966]	[0.024]	[0.472]	[0.562]	[0.258]	[0.914]
Baseline mean	0.247	0.189	0.087	0.833	0.735	0.517
Relative to mean	0.000	–0.074	–0.034	–0.008	–0.018	–0.002
b. Frequent prescribers per thousand						
Post law change, 0–3 years	0.004	0.001	0.001	0.011	0.002	0.006
	(0.002)	(0.001)	(0.002)	(0.007)	(0.004)	(0.007)
	[0.122]	[0.587]	[0.641]	[0.157]	[0.568]	[0.357]
Baseline mean	0.045	0.027	0.050	0.377	0.283	0.384
Relative to mean	0.090	0.037	0.020	0.029	0.007	0.016
c. Average prescriptions per prescribing provider						
Post law change, 0–3 years	13.227	5.862	2.771	27.793	12.081	4.593
	(5.221)	(1.627)	(0.899)	(11.151)	(3.821)	(1.778)
	[0.014]	[<0.001]	[0.003]	[0.016]	[0.003]	[0.013]
Baseline mean	83.86	39.72	11.20	281.6	145.3	30.57
Relative to mean	0.158	0.148	0.247	0.099	0.083	0.150
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. The outcome in panel (a) is the number of providers of a given type who are observed writing prescriptions of a given type per 1,000 people; the outcome in panel (b) is the number of "frequent" prescribers of a given type per 1,000 people, where "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 25th percentile of prescribing among all GPs who satisfy criterion (1); and the outcome in panel (c) is the average number of prescriptions of a given type written by providers of a given type. "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 nurse practitioners, and columns (4)–(6) consider physicians in general practice. To allow for a balanced panel, this table considers the effects 0–3 years after the law change in the 11 states with law changes between 2009–2015. The regressions include county and year fixed effects, county-specific linear time trends, and all time-varying, county-level controls listed in Figure A1. The baseline mean is measured as the average across all counties in 2010. Standard errors are clustered by state. Outcome data come from the IQVIA LRx database.

Table 5: Effects of NP independent prescriptive authority on 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	2.643	2.224	0.419	23.296	22.507	0.792
	(1.405)	(1.178)	(0.340)	(11.320)	(10.802)	(0.915)
	[0.066]	[0.065]	[0.224]	[0.045]	[0.042]	[0.391]
Baseline mean	25.45	20.44	5.013	232.1	199.2	32.94
Relative to mean	0.104	0.109	0.084	0.100	0.113	0.024
b. Average days supplied per prescription						
Post law change, 0–3 years	–0.005	–0.019	–0.017	–0.091	–0.088	–0.117
	(0.205)	(0.204)	(0.132)	(0.132)	(0.142)	(0.154)
	[0.980]	[0.925]	[0.897]	[0.494]	[0.540]	[0.450]
Baseline mean	3.250	3.255	2.081	10.46	10.94	6.882
Relative to mean	–0.002	–0.006	–0.008	–0.009	–0.008	–0.017
c. Average MME per day supplied						
Post law change, 0–3 years	28.234	22.430	30.582	26.906	23.874	33.260
	(10.883)	(9.631)	(12.267)	(8.765)	(8.553)	(10.391)
	[0.012]	[0.024]	[0.016]	[0.003]	[0.007]	[0.002]
Baseline mean	189.3	156.3	198.3	388.0	339.1	479.8
Relative to mean	0.149	0.144	0.154	0.069	0.070	0.069
d. Prescriptions with >120 MME daily per thousand						
Post law change, 0–3 years	1.342	1.091	0.250	5.814	5.842	–0.028
	(0.719)	(0.540)	(0.252)	(2.707)	(2.551)	(0.546)
	[0.068]	[0.048]	[0.326]	[0.037]	[0.026]	[0.959]
Baseline mean	8.644	6.278	2.366	75.94	61.63	14.31
Relative to mean	0.155	0.174	0.106	0.077	0.095	–0.002
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, the outcome in panel (a) is the number of opioid prescriptions per 1,000 people, the outcome in panel (b) is the average number of days supplied per opioid prescription, the outcome in panel (c) is the average daily morphine milligram equivalents (MMEs) per opioid prescription, and the outcome in panel (d) is the number of opioid prescriptions with greater than 120 MME daily per 1,000 people. Columns (1)–(3) consider prescriptions written by nurse practitioners, and columns (4)–(6) consider prescriptions written by physicians in general practice. Columns (1) and (4) consider prescriptions written for all patients, columns (2) and (4) consider prescriptions written for patients who did not fill an opioid prescription in the past six months ("opioid naive"), and columns (3) and (6) consider prescriptions written for patients who filled an opioid prescription in the past six months ("non–opioid naive"). To allow for a balanced panel, this table considers the effects 0–3 years after the law change in the 11 states with law changes between 2009–2015. The regressions include county and year fixed effects, county-specific linear time trends, and all time-varying, county-level controls listed in Figure A1. The baseline mean is measured as the average across all counties in 2010. Standard errors are clustered by state. Outcome data come from the IQVIA LRx database.

Table 6: Effects of NP independent prescriptive authority on fatal drug overdoses

Fatal overdoses per 1,000,000:	All drugs (1)	All opioids (2)	Prescription opioids (3)
a. No time trends			
Post law change, 1–3 years	7.160	9.339	8.673
	(14.935)	(16.114)	(4.294)
	[0.634]	[0.565]	[0.049]
Relative to mean	0.058	0.137	0.184
b. County-specific pre-trends			
Post law change, 1–3 years	0.119	5.090	4.147
	(8.986)	(9.338)	(2.857)
	[0.990]	[0.588]	[0.153]
Relative to mean	0.001	0.075	0.088
c. County-specific time trends			
Post law change, 1–3 years	–8.347	–5.041	1.944
	(18.590)	(17.607)	(2.936)
	[0.655]	[0.776]	[0.511]
Relative to mean	–0.067	–0.074	0.041
Baseline mean	124.0	68.21	47.16
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 analogue of equation (3) using county-year–level data for 2006–2018. Outcomes are the number of fatal overdoses involving any drug (column (1)), any opioid (column (2)), and prescription opioids (column (3)) per 1,000,000 people. To allow for a balanced panel, this table considers the effects 1–3 years after the law change in the 11 states with law changes between 2009–2015. Because the event studies in Figure A4 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 and all time-varying, county-level controls listed in Figure A1. Panel (b) further includes county-specific linear pre-trends following Goodman-Bacon (2021), and panel (c) includes county-specific linear time trends estimated over the entire sample period. 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.

Table 7: Effects of NP independent prescriptive authority on GP controlled substance prescribing by exposure to NPs

	General practice physicians		
Prescriptions per 1,000:	Opioids (1)	Anti-anxiety (2)	Opioid + benzo. (3)
Post law change, 0–3 years (β_1)	19.763	6.377	2.242
	(11.504)	(3.238)	(1.007)
	[0.092]	[0.054]	[0.031]
× Above median (β_2)	12.890	7.550	1.540
	(6.117)	(4.437)	(1.732)
	[0.040]	[0.095]	[0.378]
$\beta_1 + \beta_2$	32.653	13.927	3.782
	(11.950)	(4.979)	(1.848)
	[0.009]	[0.007]	[0.046]
Baseline mean (below median)	227.6	105.3	15.42
Baseline mean (above median)	273.4	126.9	24.22
Relative to mean (below median)	0.087	0.061	0.145
Relative to mean (above median)	0.119	0.110	0.156
Observations	40,911	40,911	40,911

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

Table 8: Effects of NP independent prescriptive authority on controlled substance prescribing across physician specialties

	General practice (1)	Emergency medicine (2)	Psych. & neurology (3)	Obstetrics & gyn. (4)	Orthopedic surgery (5)	General surgery (6)
a. Opioids per thousand						
Post law change, 0–3 years	23.296	2.321	0.756	0.814	3.083	0.361
	(11.320)	(0.819)	(0.325)	(0.370)	(1.668)	(0.553)
	[0.045]	[0.007]	[0.024]	[0.032]	[0.070]	[0.517]
Baseline mean	232.1	33.95	10.19	14.11	41.18	28.10
Relative to mean	0.100	0.068	0.074	0.058	0.075	0.013
b. Anti-anxiety per thousand						
Post law change, 0–3 years	8.452	0.147	1.846	0.193	0.050	0.068
	(3.276)	(0.097)	(0.945)	(0.114)	(0.060)	(0.068)
	[0.013]	[0.137]	[0.057]	[0.098]	[0.416]	[0.322]
Baseline mean	107.4	3.273	28.61	2.909	1.341	1.553
Relative to mean	0.079	0.045	0.065	0.066	0.037	0.044
c. Opioid + benzo. per thousand						
Post law change, 0–3 years	2.660	0.128	0.025	0.090	0.021	0.094
	(1.031)	(0.054)	(0.053)	(0.033)	(0.050)	(0.047)
	[0.013]	[0.137]	[0.057]	[0.098]	[0.416]	[0.322]
Baseline mean	16.28	1.106	0.719	0.457	0.590	0.524
Relative to mean	0.163	0.116	0.035	0.197	0.036	0.179
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 providers of a given type per 1,000 people. "Opioid + benzo." denotes instances of co-prescribing of an opioid and a benzodiazepine to the same patient by the same provider on the same day. To allow for a balanced panel, this table considers the effects 0–3 years after the law change in the 11 states with law changes between 2009–2015. The regressions include county and year fixed effects, county-specific linear time trends, and all time-varying, county-level controls listed in Figure A1. The baseline mean is measured as the average across all counties in 2010. Standard errors are clustered by state. Outcome data come from the IQVIA LRx database.

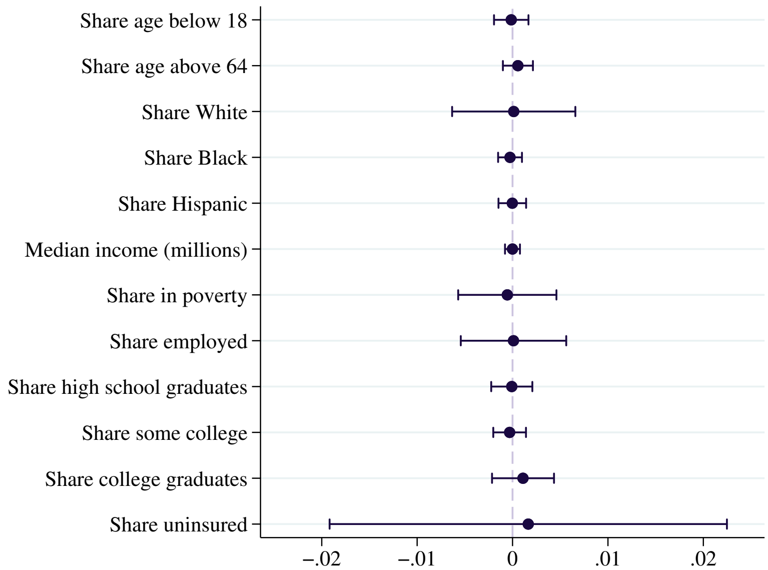
For Online Publication

The Effects of Competition on Physician Prescribing

Currie, Li, and Schnell (2023)

A Supplementary figures and tables

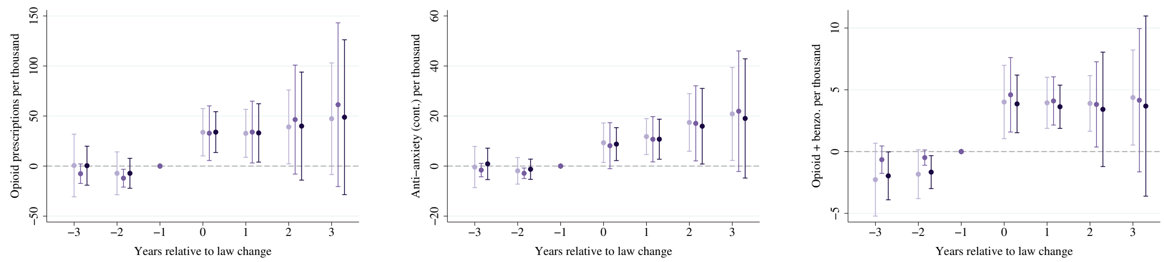
Figure A1: Relationship between changes in NP independent prescriptive authority and potential confounders



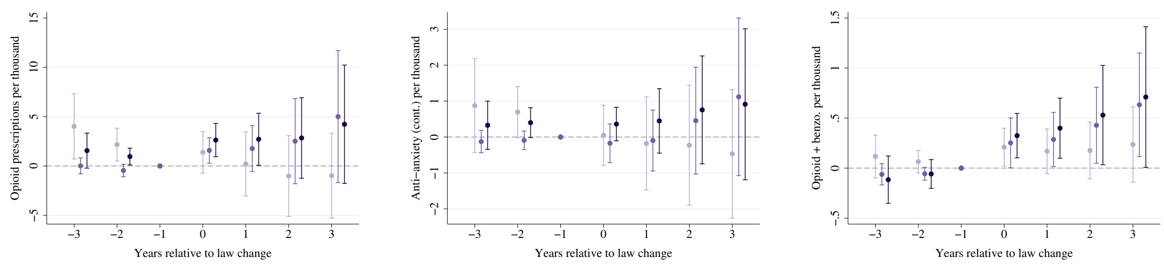
Notes: The above figure presents coefficients and 95% confidence intervals from estimation of balancing analogues of equation (3) using county-year–level data for 2006–2018. Each row presents output from a separate regression in which the potential confounder denoted on the y-axis is the dependent variable. To allow for a balanced panel, 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, county-specific linear time trends, and all time-varying, county-level controls listed in the figure except the potential confounder being used as the outcome. Standard errors are clustered by state. Data on county characteristics come from the ACS, and data on the dates of law changes granting NPs independent prescriptive authority for controlled substances come from McMichael and Markowitz (2020).

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

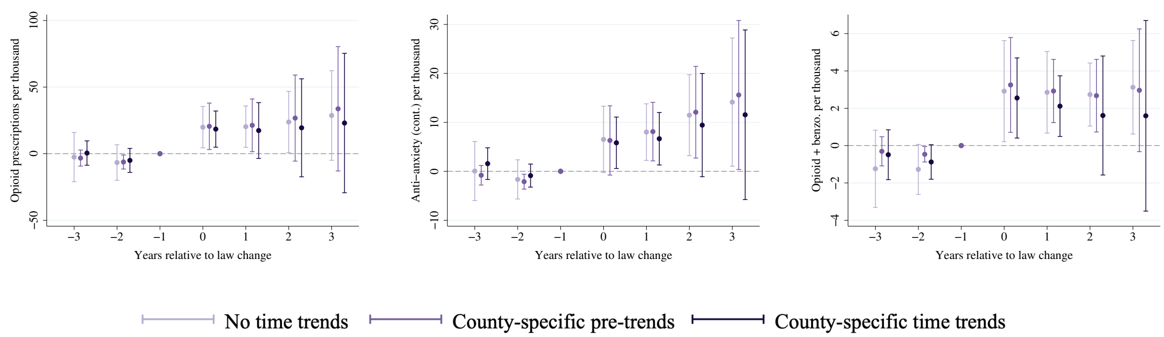
(a) All providers



(b) Nurse practitioners



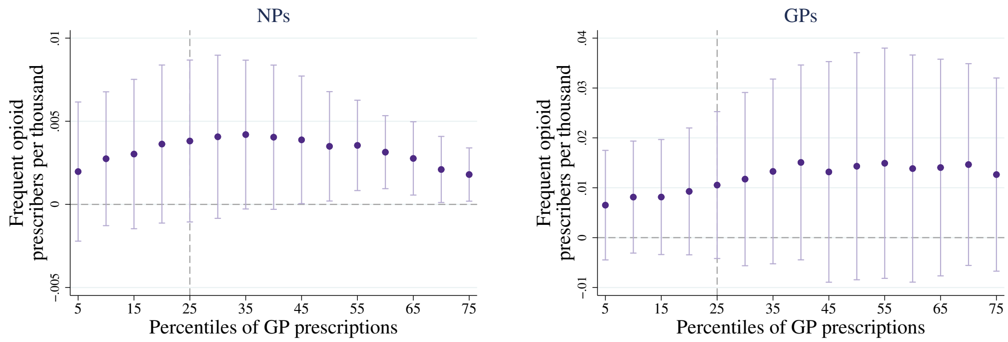
(c) General practice physicians



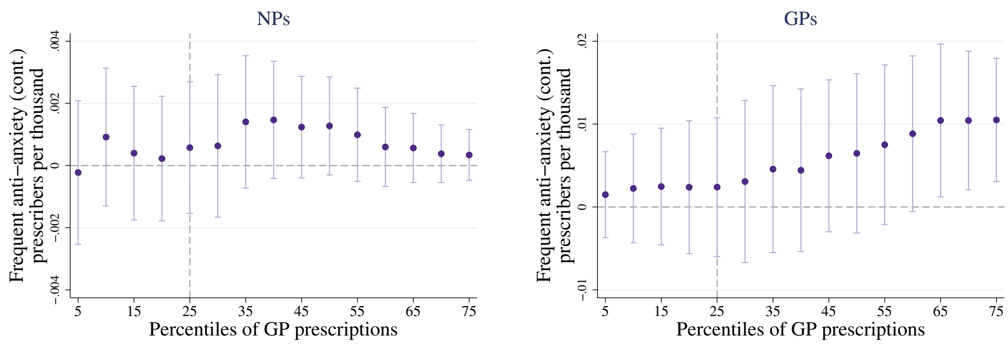
Notes: The above figures present coefficients and 95% confidence intervals from estimation of equation (2) using county-year–level data for 2006–2018. Outcomes are the number of opioid prescriptions per 1,000 people (left subfigures), the number of anti-anxiety controlled substance prescriptions per 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 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); the dark dots and bars are from specifications that include county-specific linear time trends estimated over the entire sample period. Standard errors are clustered by state. Outcome data come from the IQVIA LRx database.

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

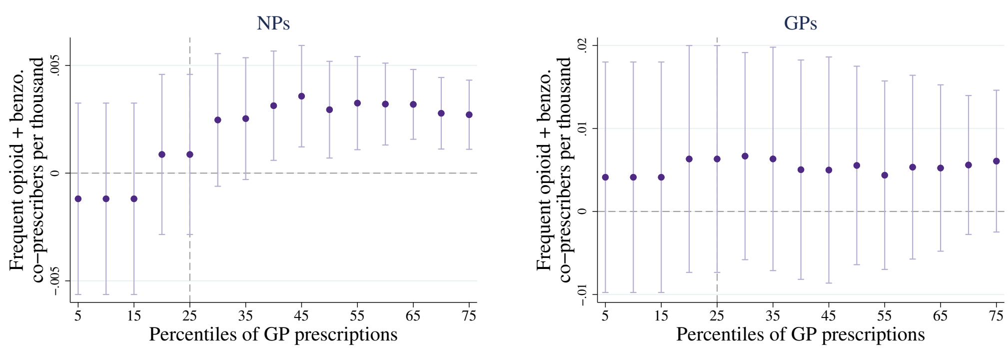
(a) Opioid prescribers



(b) Controlled anti-anxiety prescribers



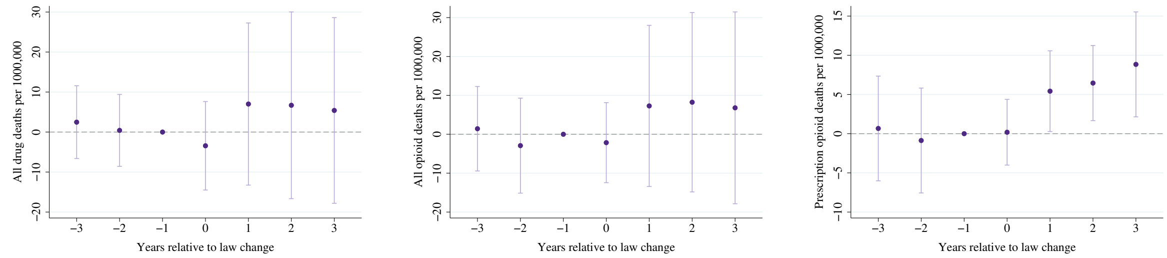
(c) Opioid + benzo. co-prescribers



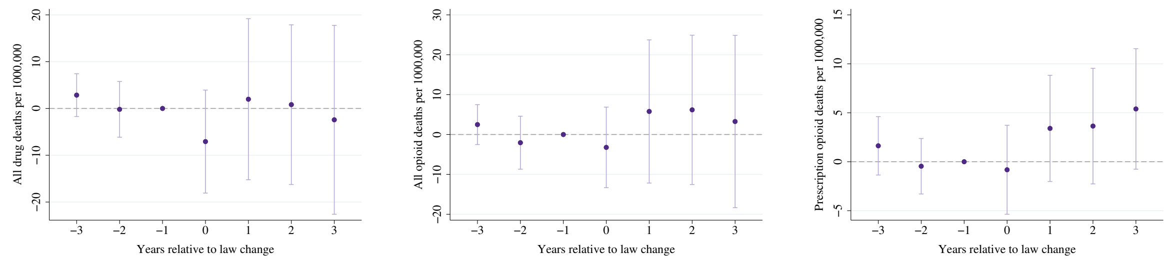
Notes: The above figures present coefficients and 95% confidence intervals from estimation of equation (3) using county-year–level data for 2006–2018. Each coefficient comes from a separate regression in which the outcome is an alternative definition of the number of “frequent” prescribers of a given type per 1,000 people; the left (right) subfigures consider the number of NPs (GPs). “Frequent” is defined as both (1) writing a given type of prescription in each month (or year for opioid-benzo. co-prescribing) and (2) being above the xth percentile of prescribing among all GPs who satisfy criterion (1), where x is defined on the x-axis. To allow for a balanced panel, 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, county-specific linear time trends, 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 of NP independent prescriptive authority on fatal drug overdoses

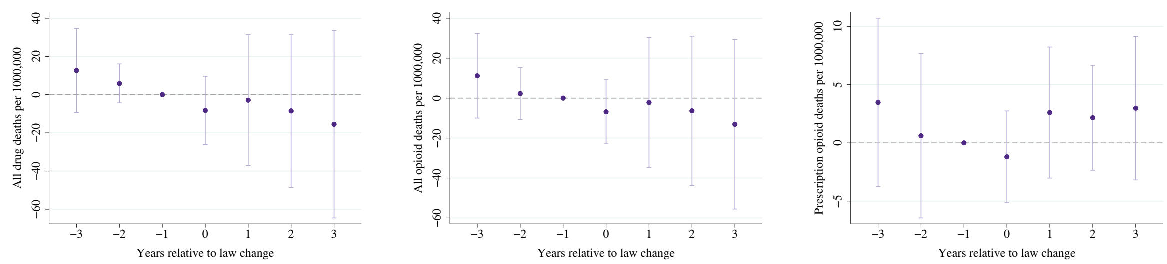
(a) No time trends



(b) County-specific pre-trends



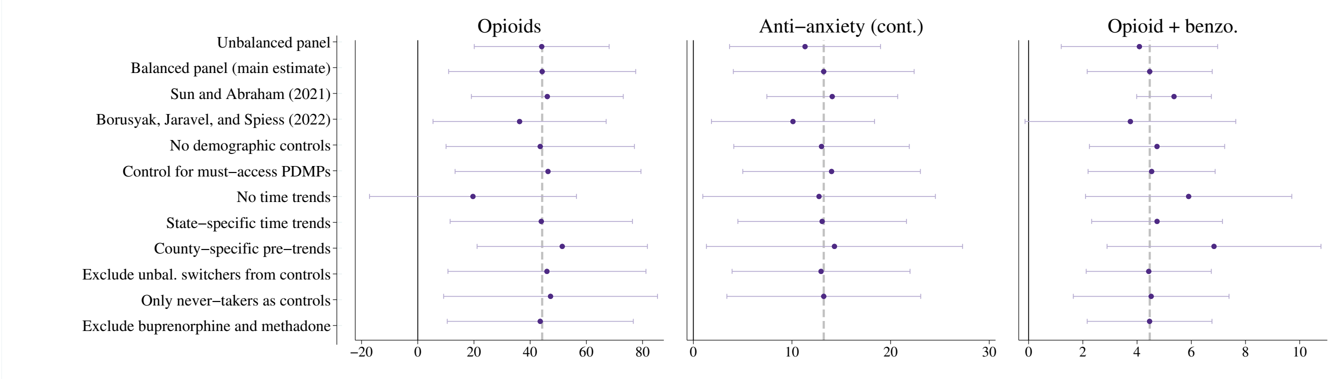
(c) County-specific time trends



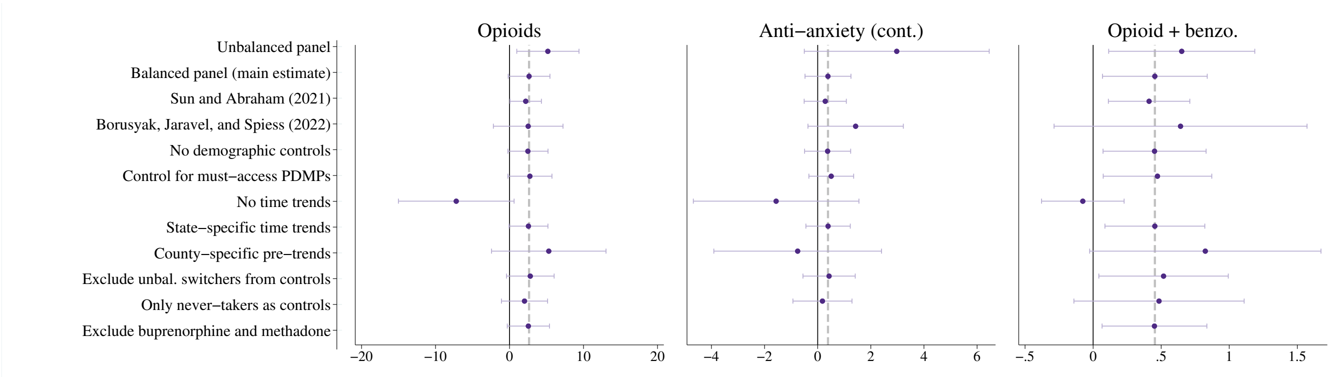
Notes: The above figures present coefficients and 95% confidence intervals from estimation of equation (2) using county-year–level data for 2006–2018. Outcomes are the number of fatal overdoses per 1,000,000 people involving any drug (left subfigures), any opioid (middle subfigures), and prescription opioids (right subfigures). 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 and all time-varying, county-level controls listed in Figure A1. Subfigure (b) further includes county-specific linear pre-trends following Goodman-Bacon (2021), and subfigure (c) includes county-specific linear time trends estimated over the entire sample period. Standard errors are clustered by state. Outcome data come from the NVSS database.

Figure A5: Effects on controlled substance prescribing: Robustness

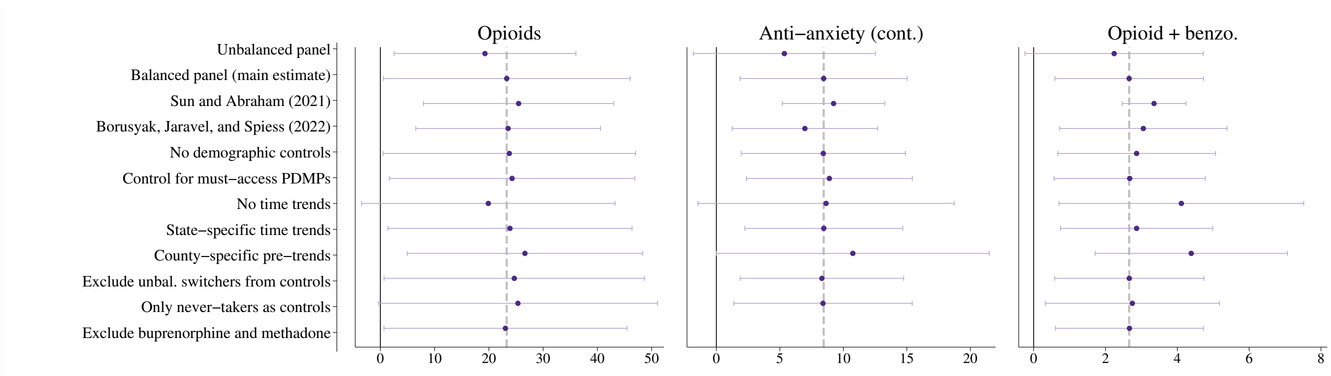
(a) All providers



(b) Nurse practitioners



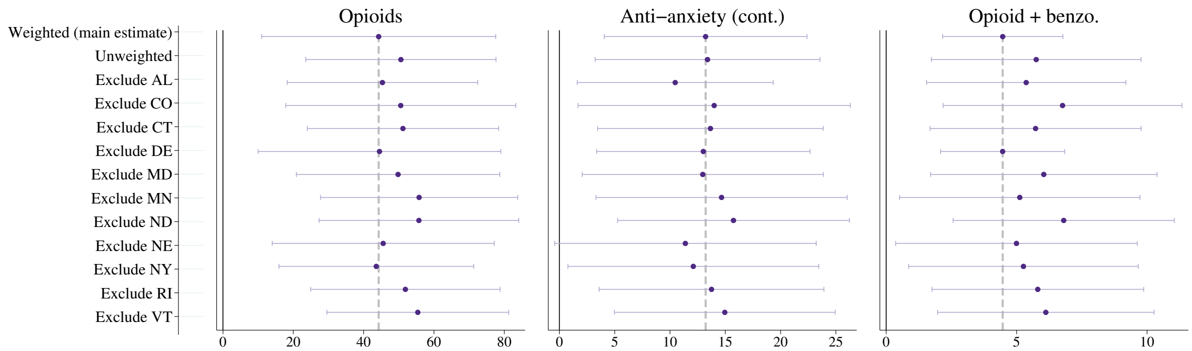
(c) General practice physicians



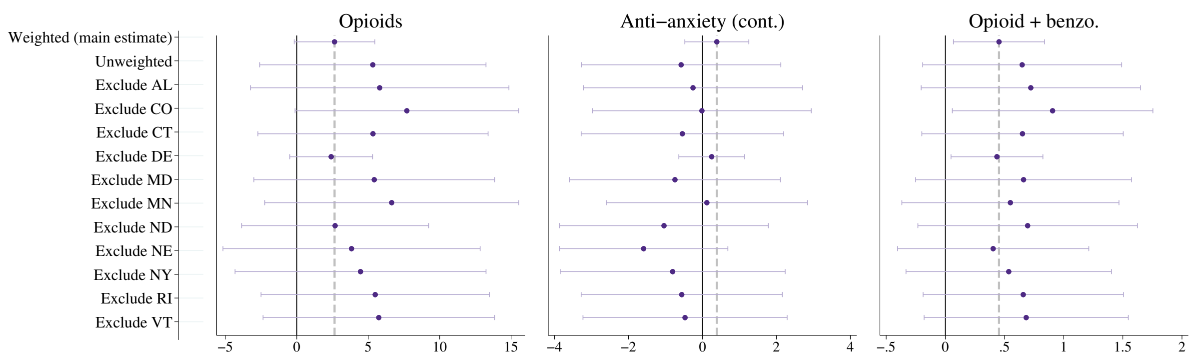
Notes: The above figure presents coefficients and 95% confidence intervals from estimation of equation (3) using county-year–level data for 2006–2018. Each row presents output from a separate regression using the specification denoted on the y-axis. The dashed vertical line in each subfigure displays the coefficient estimate from our baseline specification (as reported in Table 3); this specification considers a balanced panel of the 11 states with law changes between 2009-2015 and includes county and year fixed effects, county-specific linear time trends, and all time-varying, county-level controls listed in Figure A1. Outcomes are the number of prescriptions of a given type written by providers 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. Panel (a) considers prescriptions written by all providers, panel (b) considers prescriptions written by nurse practitioners, and panel (c) considers prescriptions written by physicians in general practice. Standard errors are clustered by state. Outcome data come from the IQVIA LRx database.

Figure A6: Effects on controlled substance prescribing: Dropping each treatment state

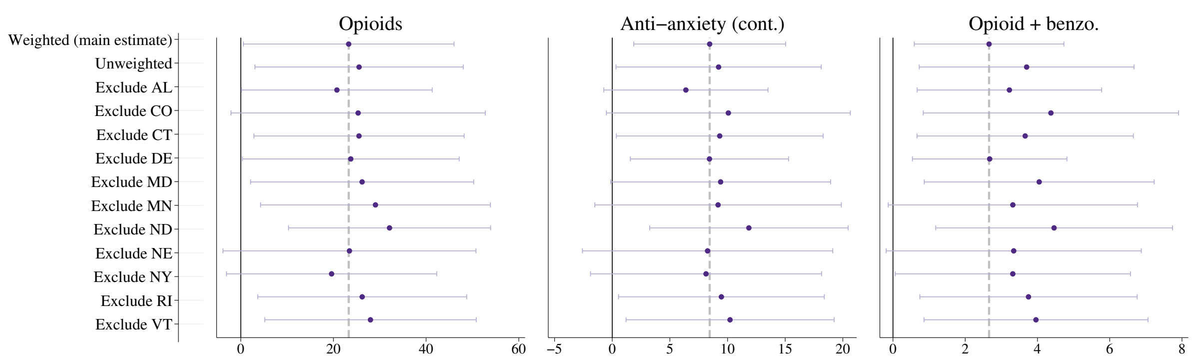
(a) All providers



(b) Nurse practitioners



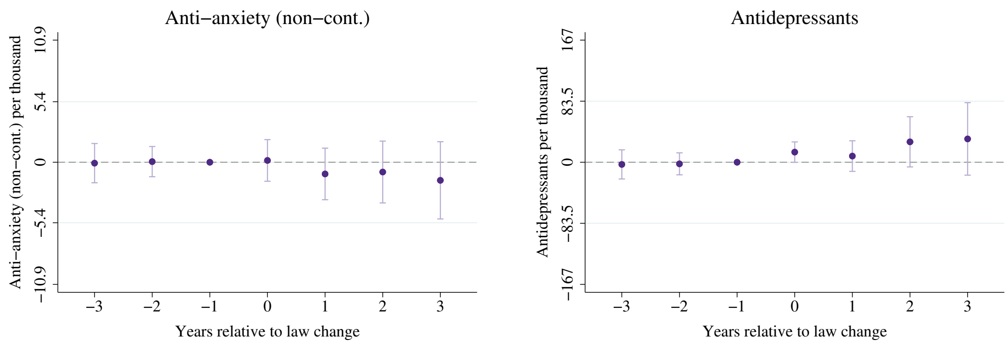
(c) General practice physicians



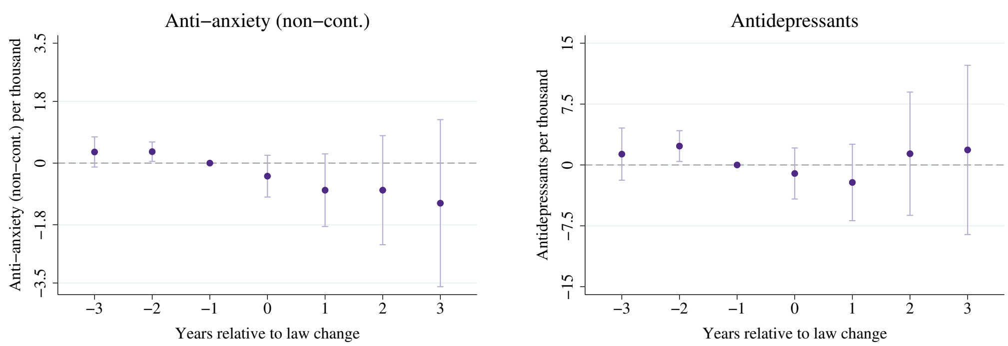
Notes: The above figure presents coefficients and 95% confidence intervals from estimation of equation (3) using county-year–level data for 2006–2018. Each row presents output from a separate regression using the specification denoted on the y-axis. The dashed vertical line in each subfigure displays the coefficient estimate from our baseline specification (as reported in Table 3); this specification includes all treatment states and weights observations by population. All other specifications in the figure are unweighted. Outcomes are the number of prescriptions of a given type written by providers 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. Panel (a) considers prescriptions written by all providers, panel (b) considers prescriptions written by nurse practitioners, and panel (c) considers prescriptions written by physicians in general practice. Standard errors are clustered by state. Outcome data come from the IQVIA LRx database.

Figure A7: Effects on non-controlled substance prescribing (IQVIA data, 2006–2018)

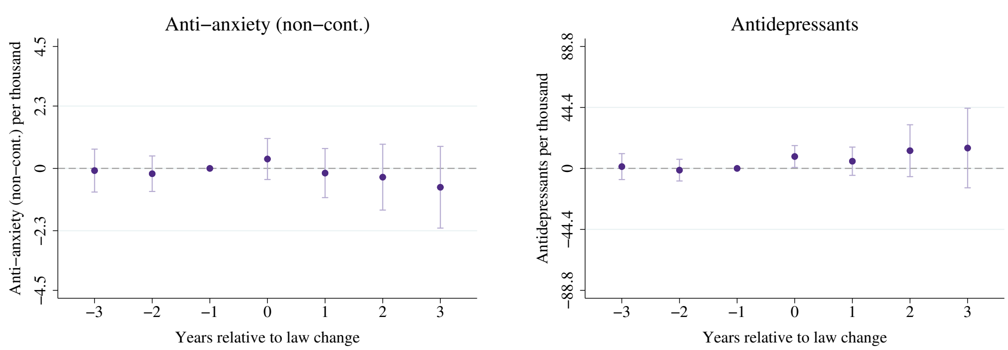
(a) All providers



(b) Nurse practitioners



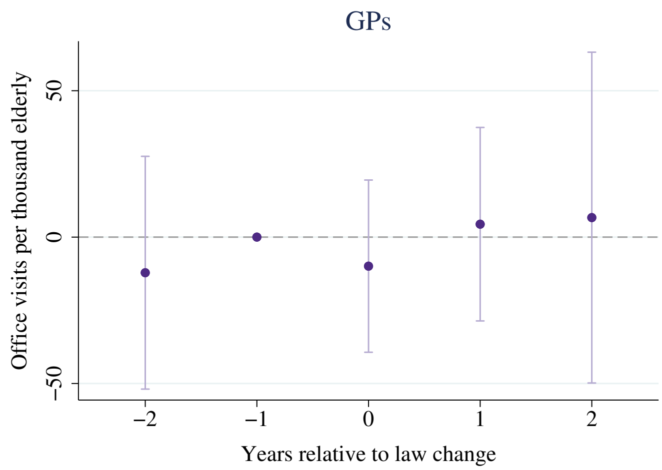
(c) General practice physicians



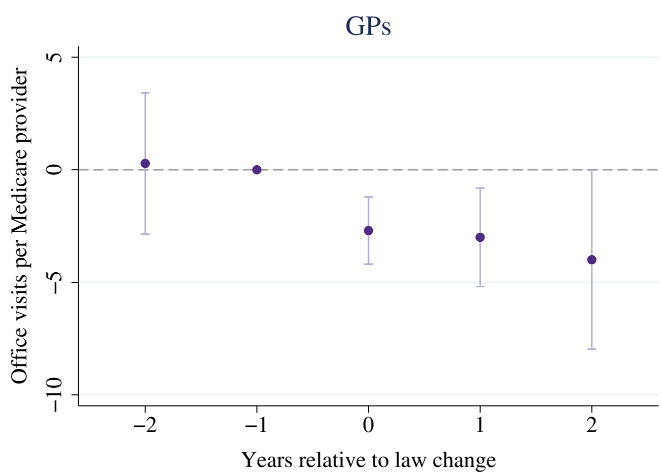
Notes: The above figures present coefficients and 95% confidence intervals from estimation of equation (2) using county-year–level data for 2006–2018. The outcome in the left (right) subfigure in each subplot is the number of prescriptions for non-controlled anti-anxiety medications (antidepressants) written by a given provider type per 1,000 people. Subfigure (a) considers prescriptions from all providers, subfigure (b) considers prescriptions written by nurse practitioners (NPs), and subfigure (c) considers prescriptions written by physicians in general practice (GPs). To make effect sizes more comparable with Figure 5, 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. The regressions include county and year fixed effects, county-specific linear time trends, 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 A8: Effects on number of office visits with GPs

(a) Office visits per thousand aged 65+



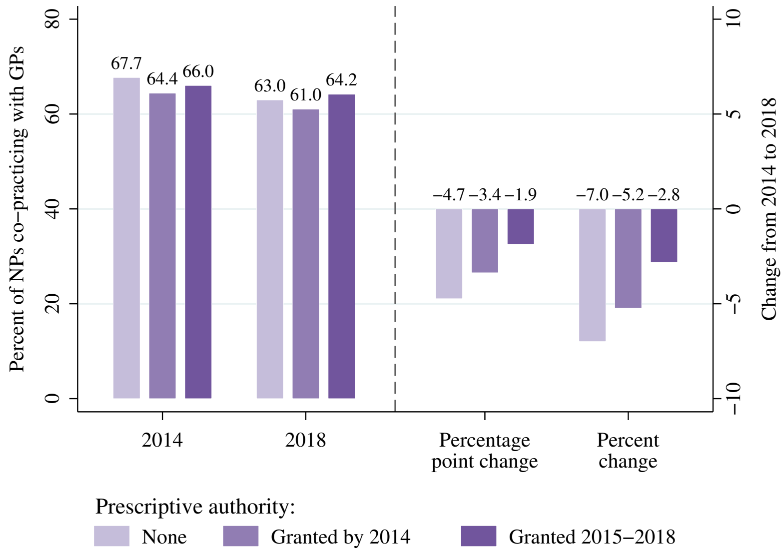
(b) Office visits per GP



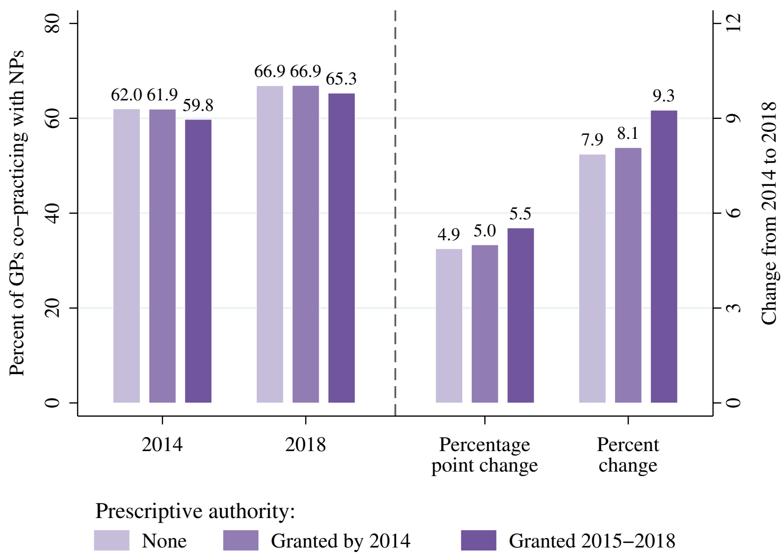
Notes: The above figures present coefficients and 95% confidence intervals from estimation of equation (2) using county-year–level data for 2012–2018. The outcome in each subfigure is the number of office visits with physicians in general practice (GPs) paid for by Medicare Part B per 1,000 people aged 65+ (subfigure (a)) and per GP in the Part B files (subfigure (b)). To allow for a balanced panel, these figures consider effects in the 7 states with law changes between 2014–2016. The regressions include county and year fixed effects and all time-varying, county-level controls listed in Figure A1. Standard errors are clustered by state. Outcome data come from the public use Medicare Part B files.

Figure A9: Co-practice patterns of NPs and GPs in 2014 and 2018

(a) Nurse practitioners



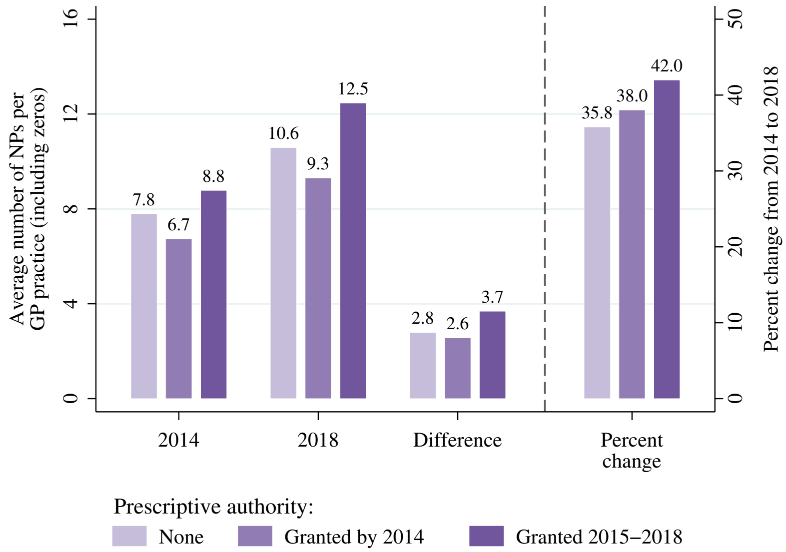
(b) General practice physicians



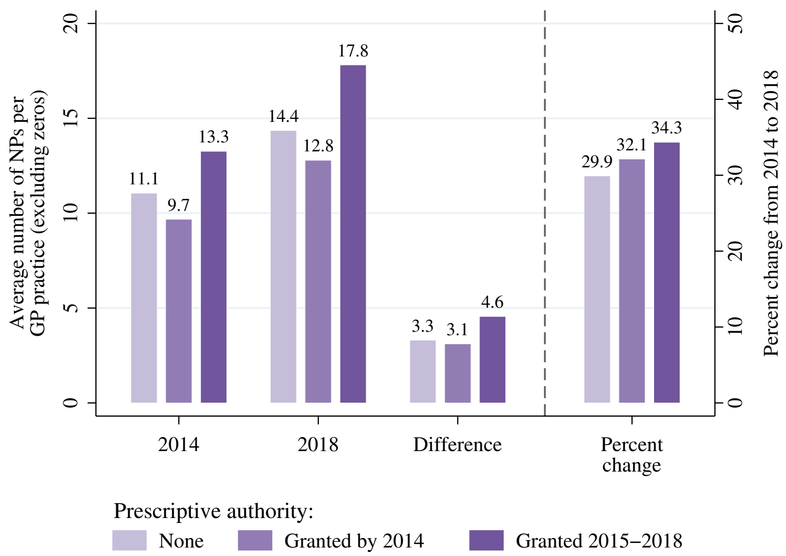
Notes: The above figures report co-practice patterns among nurse practitioners (NPs) and physicians in general practice (GPs) in states that did not allow NPs to independently prescribe controlled substances by 2018 (light purple), states that granted NPs independent prescriptive authority for controlled substances by 2014 (medium purple), and states that granted NPs the ability to independently prescribe controlled substances between 2015 and 2018 (dark purple). The left two panels in each subfigure show the population-weighted average of county-year level percents of a given provider type (NPs in subfigure (a) and GPs in subfigure (b)) who were observed practicing in the same clinic as at least one provider of the other type (GPs in subfigure (a) and NPs in subfigure (b); "co-practicing") in 2014 (first panel) and 2018 (second panel). The right two panels show the population-weighted average percentage point changes (third panel) and percent changes (fourth panel) in these shares from 2014 to 2018. Outcome data come from the location snapshots provided by IQVIA and include the exact practice addresses for all providers in the IQVIA data in 2014 and 2018.

Figure A10: Number of NPs per GP practice in 2014 and 2018

(a) Including zeros



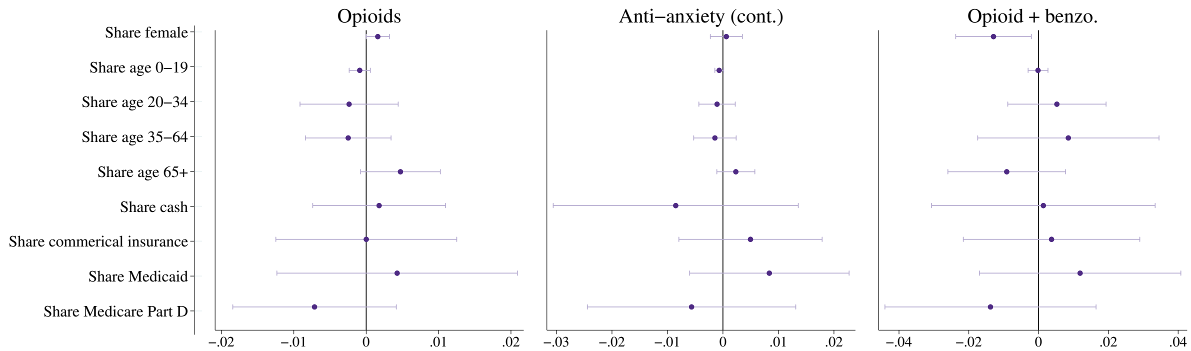
(b) Excluding zeros



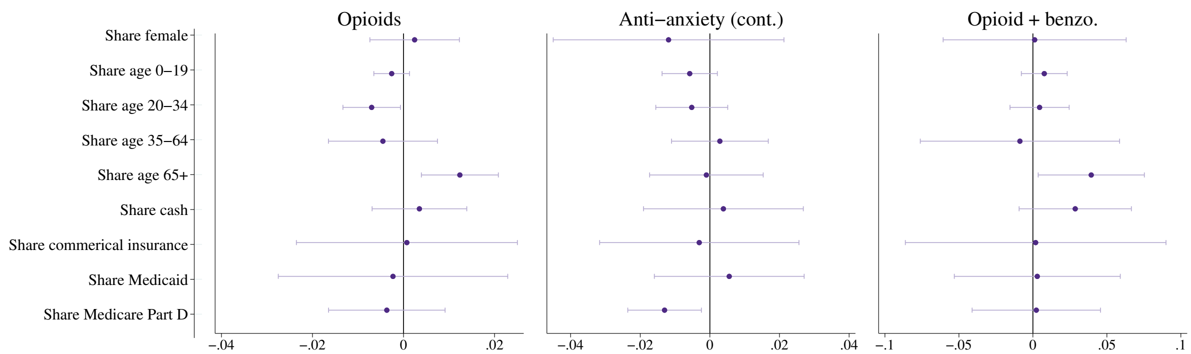
Notes: The above figures report the average number of nurse practitioners (NPs) observed working in the same practice as each physician in general practice (GP) in states that did not allow NPs to independently prescribe controlled substances by 2018 (light purple), states that granted NPs independent 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). The left two panels in each subfigure show the population-weighted average of the county-year number of NPs who were observed practicing in the same clinic as each GP in 2014 (first panel) and 2018 (second panel); subfigure (a) includes GPs with no NPs in their practice in these calculations whereas subfigure (b) excludes such zeros. The right two panels show the population-weighted average level changes (third panel) and percent changes (fourth panel) in these averages from 2014 to 2018. Outcome data come from the location snapshots provided by IQVIA and include the exact practice addresses for all providers in the IQVIA data in 2014 and 2018.

Figure A11: Effects on patient composition by prescription type

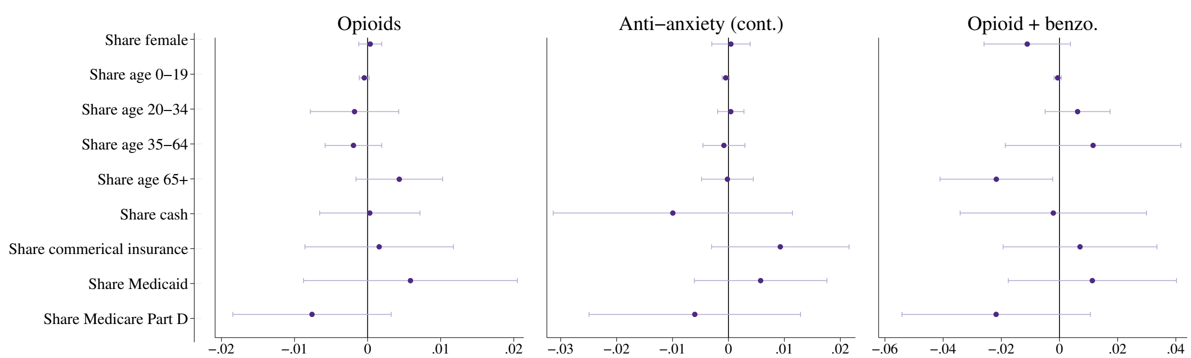
(a) All providers



(b) Nurse practitioners



(c) General practice physicians



Notes: The above figure presents coefficients and 95% confidence intervals from estimation of balancing analogues of equation (3) using county-year–level data for 2006–2018. Each row presents output from a separate regression using the outcome denoted on the y-axis. For each provider type, outcomes are the average provider-level share of opioid prescriptions (left subfigures), anti-anxiety controlled substance prescriptions (middle subfigures), and instances of co-prescribing of an opioid and a benzodiazepine (right subfigures) for each patient type. Subfigure (a) considers all providers, subfigure (b) considers nurse practitioners, and subfigure (c) considers physicians in general practice. To allow for a balanced panel, 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, county-specific linear time trends, 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.

Table A1: Effects of NP independent prescriptive authority on controlled substance prescribing by PAs

	Physician assistants		
	Opioids (1)	Anti-anxiety (2)	Opioid + benzo. (3)
a. Prescriptions per thousand			
Post law change, 0–3 years	4.843	1.060	0.529
	(2.688)	(0.546)	(0.245)
	[0.078]	[0.058]	[0.035]
Baseline mean	29.01	5.385	1.136
Relative to mean	0.167	0.197	0.466
b. Prescribing providers per thousand			
Post law change, 0–3 years	0.011	0.002	0.006
	(0.006)	(0.005)	(0.006)
	[0.067]	[0.638]	[0.297]
Baseline mean	0.180	0.134	0.081
Relative to mean	0.061	0.015	0.074
c. Average prescriptions per prescribing provider			
Post law change, 0–3 years	22.017	4.332	3.008
	(9.457)	(1.834)	(0.693)
	[0.024]	[0.022]	[<0.001]
Baseline mean	135.9	32.53	11.95
Relative to mean	0.162	0.133	0.252
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 equation (3) using county-year–level data for 2006–2018. The outcome in panel (a) is the number of prescriptions of a given type written by physician assistants per 1,000 people, the outcome in panel (b) is the number of physician assistants who are observed writing prescriptions of a given type per 1,000 people, and the outcome in panel (c) is the average number of prescriptions of a given type people written by prescribing physician assistants. To allow for a balanced panel, this table considers the effects 0–3 years after the law change in the 11 states with law changes between 2009–2015. The regressions include county and year fixed effects, county-specific linear time trends, and all time-varying, county-level controls listed in Figure A1. The baseline mean is measured as the average across all counties in 2010. Standard errors are clustered by state. Outcome data come from the IQVIA LRx database.

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 $R_{OP} > R_{noOP}$.[36] Moreover, although we are not explicitly modeling the dynamics, R_{OP} will further exceed R_{noOP} if prescribing opioids increases the probability that patients return for future visits (e.g., for refills).

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

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

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

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

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

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

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

It is reasonable to assume that the absolute value of the elasticity of marginal utility with respect to income, $\frac{U_{YY}Y}{U_Y}$, is greater than one.[37] 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

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

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

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

C Provider practice locations

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

Our location assignment algorithm is implemented as follows. First, for each provider-month, we calculate the share of the provider's total prescriptions across all three of the drug classes included in our data extract (opioids, anti-anxiety medications, antidepressants) that were filled by patients in each zip code. Starting with the zip code with the highest share of prescriptions for that provider, we then add additional zip codes in order of descending prescription shares until we have a set of zip codes covering at least 90 percent of the provider's prescriptions in that month.[38] 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 $t-2$.[39] 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

[38]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.

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

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

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

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

In particular, after identifying the initial spell set for each provider as outlined above, we determine the set of zip codes needed to cover 90 percent of each provider's prescriptions within each spell. We then compare the practice area in spell t to the practice area in spell $t-1$ and say that a move occurred between these spells if there is no overlap between the set of zip codes in these spell-level practice areas. If a move did not occur between two spells, we merge the spells in question, calculate the practice area for this new spell, and compare the new spell's practice area to the practice area of the spell a period before. We iterate on this procedure—that is, redefining spells, defining spell-level practice areas, and identifying potential moves—until there is no overlap in the practice areas of consecutive spells. This ensures that the zip code with the highest share of prescriptions in each provider-spell changes across identified moves. We use a zip code to county crosswalk provided by the U.S. Department of Housing and Urban Development to assign counties to the most frequent zip code in each provider-spell and use this county as the provider's location for the period covered by the spell.[40]

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.[41] 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.[42] Using our location assignment algorithm, we find that among

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

[41]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.

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

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

D Comparison to McMichael (2020)

This section provides a careful 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 a recent working paper by McMichael and Markowitz (2020), and our primary prescription data come from IQVIA. When examining effects on opioid prescribing, we consider five primary outcomes at the county-year level: (1) number of opioid prescriptions per 1,000 people (Table 3), (2) number of opioid prescribers per 1,000 people (Table 4), (3) average annual opioid prescriptions per opioid prescribing provider (Table 4), (4) average days supplied per opioid prescription (Table 5), and (5) average MME per day supplied (Table 5). We estimate models with county and year fixed effects and find significant positive effects of the law changes on all outcomes among GPs except the number of prescribing providers (for which we find negative but insignificant effects).

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

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

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.[43] The baseline means for annual opioid patients per provider (panel (b)) and annual days supplied per provider (panel (d)) match McMichael's well, suggesting that the two databases are similar. However, while we find effects in panels (b) and (d) that are negative as in McMichael (2020), the effects using McMichael's law changes and specification in the IQVIA data are substantially smaller and less precise than those reported in McMichael (2020). 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 total 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.[44]

The remaining columns of Table A2 show the impact of additional incremental changes moving between McMichael's analysis and our own. Comparing columns (2) and (3) shows that when we use law changes granting NPs independent prescriptive authority for controlled substances from McMichael and Markowitz (2020) as in our analysis (rather than full practice

[43]The estimates reported in panels (c) and (e) of column (10) of Table A2 reproduce those first reported in column (4) of Table 5 of this paper. The estimate reported in panel (a) of column (10) of Table A2 is similar to that reported in column (4) of Table 4, except that we include GPs with zero opioid prescriptions when calculating opioid prescriptions per provider in Table A2 to more closely reflect the measures used in McMichael (2020).

[44]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 0.011 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 30 million (350 billion divided by 11,000). Given that there are only approximately a million physicians practicing at any given point in time, this number is not possible.

authority from McMichael, 2020), the signs of the effects on the first four outcome measures also flip to align with our primary findings. This is perhaps surprising: allowing NPs to independently prescribe controlled substances is typically the final legislative change required to allow NPs to practice without any restrictions, and thus the law changes for independent controlled substance authority and full practice authority should largely overlap. This is confirmed by McMichael and Markowitz (2020), who report years of full practice authority that align with those for independent controlled substance prescribing in most states in their Table 1. Comparing the law changes used in McMichael (2020) to those reported in McMichael and Markowitz (2020) 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 percent effects are typically larger 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 8 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)) and using data for 2006–2018 rather than 2011–2018 (column (7) versus column (6)) leads to similar results. Moreover, as first shown in Figure A5 for our primary outcomes, using a balanced panel of states (column (8) versus column (7)) and controlling for time trends (column (9) versus column (8)) does not meaningfully change the conclusions. Finally, as noted above, the results are remarkably consistent whether we estimate variants of our primary specification with physician fixed effects in physician-level data or county fixed effects in county-level data (column (9) versus column (10)).

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

	McMichael (2020)	*Incremental specification changes*								CLS (2023)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
a. Opioid prescriptions per provider										
Post law change		–0.008	0.027	10.028	16.805	16.821	11.590	16.326	22.020	23.791
		(0.030)	(0.023)	(4.368)	(7.651)	(7.698)	(7.976)	(9.108)	(9.702)	(10.211)
		[0.796]	[0.237]	[0.026]	[0.033]	[0.034]	[0.152]	[0.079]	[0.028]	[0.024]
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	0.027	0.057	0.090	0.090	0.063	0.089	0.120	0.122
b. Opioid patients per provider										
Post law change	–0.028	–0.009	0.015	2.518	3.834	3.830	2.679	3.757	3.970	4.467
	(0.004)	(0.029)	(0.021)	(1.416)	(1.824)	(1.840)	(1.599)	(1.612)	(1.750)	(1.958)
	[<0.01]	[0.771]	[0.481]	[0.081]	[0.041]	[0.043]	[0.100]	[0.024]	[0.028]	[0.027]
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	0.015	0.040	0.065	0.065	0.047	0.066	0.069	0.073
c. Average days supplied per prescription										
Post law change		0.009	–0.018	–0.229	–0.321	–0.330	–0.404	–0.559	–0.140	–0.091
		(0.022)	(0.021)	(0.182)	(0.266)	(0.267)	(0.337)	(0.434)	(0.161)	(0.132)
		[0.694]	[0.396]	[0.213]	[0.234]	[0.223]	[0.236]	[0.203]	[0.388]	[0.494]
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	–0.018	–0.023	–0.030	–0.030	–0.039	–0.054	–0.013	–0.009
d. Days supplied per provider (in thousands)										
Post law change	–0.038	–0.001	0.009	0.127	0.273	0.273	0.108	0.189	0.490	0.515
	(0.006)	(0.000)	(0.000)	(0.085)	(0.159)	(0.159)	(0.238)	(0.295)	(0.187)	(0.197)
	[<0.01]	[0.985]	[0.814]	[0.142]	[0.091]	[0.092]	[0.651]	[0.525]	[0.012]	[0.012]
Baseline mean	2.741	3.223	3.223	3.223	3.738	3.738	3.599	3.599	3.599	3.819
Relative to mean	–0.038	–0.001	0.009	0.039	0.073	0.073	0.030	0.052	0.136	0.135
e. Average MME per day supplied										
Post law change		0.029	0.148	62.602	58.050	57.942	80.348	78.887	28.434	26.906
		(0.079)	(0.088)	(31.113)	(30.322)	(30.263)	(45.106)	(54.658)	(8.671)	(8.765)
		[0.716]	[0.098]	[0.050]	[0.061]	[0.061]	[0.081]	[0.155]	[0.002]	[0.003]
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	0.148	0.140	0.161	0.161	0.213	0.209	0.076	0.069
f. Total MME per provider (in millions)										
Post law change	–0.060	0.028	0.158	0.198	0.380	0.381	0.475	0.521	0.266	0.235
	(0.007)	(0.000)	(0.000)	(0.099)	(0.176)	(0.177)	(0.190)	(0.228)	(0.112)	(0.098)
	[<0.01]	[0.809]	[0.129]	[0.051]	[0.035]	[0.036]	[0.016]	[0.027]	[0.022]	[0.020]
Baseline mean	0.008	1.354	1.354	1.354	1.571	1.571	1.575	1.575	1.575	1.667
Relative to mean	–0.060	0.028	0.158	0.146	0.242	0.243	0.301	0.331	0.169	0.141
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.