To require Prior Authorization, or not to require Prior Authorization?

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Abstract

Prior authorization is a utilization management tool used to reduce wasteful utilization and contain health care costs. However, growing evidence suggests that such policies may inadvertently impede access to high-value treatments. Recognizing this, several states have recently repealed prior authorization requirements in Medicaid programs, yet these policies have not been rigorously evaluated. Theoretically, repealing prior authorization should reduce administrative burdens and improve access to care. However, given that prior authorization has long been the norm and given that high-value treatments continue to be underutilized, the impact of repealing prior authorization on access to essential and effective care remains uncertain and warrants further investigation. This study is among the first to examine the impact of repealing prior authorization on access to high-value treatments in the context of Buprenorphine – a highly effective medication for treating opioid use disorder. Using 2015-2019 data from the Transformed Medicaid Statistical Information System (T-MSIS) Analytic Files (TAF), we exploit time variation in repeal of state Medicaid program prior authorization policies to causally measure the impact of repealing prior authorization for prescribing Buprenorphine on access to Buprenorphine and on acute health care utilization among Medicaid beneficiaries with opioid use disorders. We find that repealing prior authorization increases Buprenorphine access and reduces opioid-use related hospitalizations. These effects are larger for beneficiaries in managed care plans and in states with less restrictive prior authorization repeals for Buprenorphine formulations. Our findings imply that although prior authorization has been effective in curbing wasteful utilization and containing rising health care expenditures, its application to high-value treatments may generate barriers in access to care especially for vulnerable populations, and do more harm than good.

Introduction

Prior authorization is a widely-used health plan cost-control process that requires health care providers to obtain advance approval from a health plan before a specific service or drug is delivered to the patient to qualify for payment coverage (American Medical Association 2022). When a service or drug is under prior authorization restriction, in order for the service or drug to be covered, the patient's provider must fill out a form provided by the insurer. The paperwork will usually require the provider to answer some yes-or-no questions regarding why they are choosing to prescribe a restricted drug as well as other drugs used to treat the condition in question, accompanied by documentation to support claims made in the form. After the form is submitted, the provider and patient wait for approval from the insurer which can take between 1 and 5 business days. If authorized, the patient can access the restricted treatment with standard insurance coverage. If authorization is denied, the patient will either pay out-of-pocket or not be able to access treatment unless their provider appeals the decision or submits a new request to the insurer.

Economic theory posits that utilization management tools such as prior authorization limit spending, ensure appropriate service use (Baicker and Robbins 2015) and control the illicit use of potentially dangerous substances (2018-2019 Academy of Managed Care Pharmacy Professional Practice Committee 2019). However, the prior authorization process is onerous and arduous, consuming significant time and resources for providers, which may lead to reduced care provision and access issues (Kyle and Song 2023). Thus, the impact of using prior authorization is a subject of ongoing debate. Evidence supports its role in limiting wasteful use and containing costs but challenges its role in impeding access to essential care. At its best, prior authorization increases guideline-concordant care and reduces unnecessary spending (Anderson et al. 2022). However, at its worst, prior authorization induces administrative burdens for physicians which depresses supply of care to patients and as a consequence, adversely affects access to essential treatments and patient health (Woolhandler et al. 2003; Himmelstein et al. 2020; Dunn et al. 2024; Brot-Goldberg et al. 2023).

Recognizing this trade-off, policymakers have moved to streamlining or repealing prior authorization for select high-value treatments. These efforts have been perceived positively by physicians, patients, and advocacy groups. However, it remains unknown as to whether or not these policy changes have actually improved access to care. Theoretically, repealing prior authorization should reduce hassle costs and improve access to high-value care, however long-standing norms in clinical practice could limit its impact. If providers are accustomed to doing prior authorizations and prescribing alternative medications and therapies due to prior authorization barriers, simply streamlining or repealing prior authorization requirements may not be sufficient to influence prescribing behavior or ensure optimal utilization of high-value treatments. Thus, the broader implications of implementing or eliminating prior authorization unclear. In this study, we seek to fill this gap by empirically examining whether repealing prior authorization for high-value treatments improves access to care and leads to more efficient use of health care services. We answer this question by leveraging a (natural) policy experiment in Medicaid in which some states repealed prior authorization for prescribing Buprenorphine – a highly effective yet underutilized medication used to treat opioid use disorders (Chilcoat et al. 2019). Exploiting geographical and temporal variation in the implementation of prior authorization repeal policies for prescribing Buprenorphine and applying a staggered difference-in-differences framework, we aim to estimate the plausibly causal effects of repealing prior authorization for prescribing Buprenorphine on Buprenorphine access and downstream opioid-use-related health care utilization. Using Buprenorphine as an example, our study more broadly aims to provide evidence on the implications of repealing prior authorization for high value services and inform policy on the types of treatments that should be targeted for utilization management.

Studies have shown that prior authorization has proven effective to limit the utilization of low-value health care services (Colla et al. 2017). Prior authorization has been successful in containing the prevalence of unnecessary medical testing (Sinclair et al. 2004), curtailing the use of outpatient antimicrobial therapy(Conant et al. 2014), and reducing millions of dollars in drug spending (Brot-Goldberg et al. 2023). Insurance companies and policymakers — citing studies that support prior authorization's role in curbing overuse — have largely upheld its use as a cost-control measure. However, physicians, patients and advocacy groups have increasingly lobbied

against prior authorization, arguing that it induces administrative hassles and impedes care access (Kyle and Song 2023; Yang and Yang 2020). Studies have shown that prior authorization leads to substantial time- and resource-costs for physicians and results in delayed — and sometimes — denied access to timely and essential care for patients. Further, a burgeoning literature concerned with administrative burdens (Herd and Moynihan 2021) has documented that ordeals such as prior authorizations have caused unintended harms and blocked delivery of essential high-value services, interventions, and entitlements to vulnerable persons in the greatest need of those services (Lu et al. 2011; Goodman et al. 2016; Landis et al. 2022). Evidence suggests that when efficiently implemented — i.e., when the right types of treatments are targeted — prior authorization can generate cost savings that outweigh administrative burden (Brot-Goldberg et al. 2023). However, when prior authorization is required for high-value, evidence-based treatments, it raises concerns about barriers to accessing timely and appropriate care.

In addition to contributing to the broader economics literature on improving access to high-value care, we contribute to two strands of the literature. First, we contribute to the literature on the adverse consequences of administrative burdens in health care. Prior authorization — as conceptualized by Nichols and Zeckhauser (1982) – serves as an *ordeal* that aims to improve target efficiency — defined broadly as the allocation of resources to the individuals for whom they will do the most good – by ensuring that medications or interventions are prescribed only to those who stand to benefit the most (Nikpay 2022). Ordeals such as prior authorization require providers to signal the appropriateness of a treatment for a patient to payors and efficiently target high-benefit users of a health care service. In our study, using the example of Buprenorphine we demonstrate that requiring an ordeal in the form of prior authorization reduces welfare, as it prevents a number of opioid-using individuals from accessing Buprenorphine treatment potentially leading patients to opt for alternative ineffective treatments or forgoing treatment altogether, thus putting them at risk for overdose and other opioid-related harm. The findings from this study are in line with other research, which has found that ordeals impede access to essential services (Herd and Moynihan 2021).

Second, we contribute directly to the specific literature that explores the effects of repealing prior authorization for prescribing Buprenorphine and other high-value treatments. The literature on the effects of prior authorization on the utilization of medical services is vast but limited in the context of Buprenorphine access for Medicaid beneficiaries. Previous work on the topic has either been limited to Medicaid beneficiaries in fee-for-service plans, or the analysis has been limited to select of state Medicaid programs. One study analyzing data from the State Drug Utilization Data found that repealing prior authorization requirements for Buprenorphine had no effect on the number of Buprenorphine prescriptions (Christine et al. 2023). This study did not account for number of Medicaid enrollees with opioid use disorders, and was not able to distinguish whether a state's Buprenorphine prescriptions rate was a function of care availability or the proportion of the Medicaid population with opioid use disorder. The authors also did not account for differences in Buprenorphine prior authorization policies across states. Further, the dataset used by the authors did not have adequate data on managed care encounters, and their analyses was largely based on fee-for-service enrollees. A second study (Keshwani et al. 2022) using the same dataset focused on 2 states – California and Illinois – and found mixed evidence regarding the impact of prior authorization repeal on Buprenorphine access. Thus, it remains largely unknown as to how repealing prior authorization for Buprenorphine prescribing affects Buprenorphine access. Our study using rich data and a more robust study design makes a case as to why requiring prior authorization for high-value services (like Buprenorphine) might not be optimal to encourage uptake of high-value care.

We find that states that repealed prior authorization for prescribing Buprenorphine displayed higher Buprenorphine access and lower opioid-use related hospitalizations. We find that these results are heterogeneous across insurance plan types and state policies. Our study provides evidence that coverage of high-value treatments without prior authorization can improve access to high-value care (like Buprenorphine) and reduce avoidable hospitalizations and emergency department visits.

The paper proceeds as follows. In section 2 we describe the background and setting. In section 3 we discuss the conceptual model and resulting theoretical predictions for subsequent empirical analysis. We describe the data

in Section 4 and discuss empirical strategy in Section 5. We present the results in Section 6 and robustness checks in Section 7. In Section 8 we conclude.

2. Background and Setting

2.1 Opioid Epidemic in the United States

Drug overdose deaths are one of the leading cause of deaths in the United States. Approximately 900,000 Americans have died as result of opioid overdoses since 1999 (Abraham et al. 2022; Biondi et al. 2022). Despite recent declines in the absolute number of opioid overdose deaths, more than 50,000 Americans died from opioid overdose between May 1, 2024 and April 30, 2025. Owning its origins to the increased prescriptions of OxyContin since the late 1990s, Medicaid beneficiaries have been disproportionately prescribed pain medications which have led to an increase in opioid addiction (Alpert et al. 2022). In order to regulate the OxyContin and pain-medication-induced opioid crisis, policymakers regulated the supply of prescription Oxycontin which led to opioid-using individuals to substitute OxyContin for more harmful illicit opioids such as heroin and synthetic opiates. As a result of the new regulations — prescription drug monitoring programs, Medicaid lock-in programs — use of OxyContin dropped, and use of heroin increased exponentially triggering an opioid epidemic in early 2010s. The crisis worsened as the supply of synthetic opiates — fentanyl and fentanyl-laced substances — circulated increasingly in the market, further triggering a rise in the number of overdose deaths (Alpert et al. 2018). The rising number of opioid-related overdose deaths increased the urgency to link opioid-using patients to effective addiction treatments such as Medications for Opioid Use Disorders (MOUD).

MOUD – Buprenorphine, Naltrexone and Methadone – are Food and Drug Administration (FDA) approved drugs used for treating individuals with opioid use disorders (Clark et al. 2015; Hser et al. 2016; Wakeman et al. 2020; Weiss et al. 2015). MOUD has been shown to be cost-effective, and is considered to be the first-line treatment in assisting the recovery of individuals suffering from opioid use disorders (Madras et al. 2020; Committee on Medication-Assisted Treatment for Opioid Use Disorder; 2019; NIDA. Overview. National Institute on Drug Abuse website. 2021). Studies have shown that any of the three medications are effective in treating opioid use disorders. However, given the differing pharmacological profiles of the drugs, and the differing clinical realities of their administration, opioid-using patients and clinicians have preferences towards a certain MOUD. More specifically, since, Buprenorphine induces fewer side effects compared to Naltrexone (Gonzalez and Brogden 1988), is subject to fewer regulations and constraints on patients than are commonly imposed in Methadone treatment for opioid use disorder(Deck and Carlson 2004; Yarborough et al. 2016). Given its higher overall higher patient acceptability, many clinicians prefer to prescribe Buprenorphine over other MOUD modalities for their opioid-using patients (Yarborough et al. 2016). As a result, numerous interventions intending to improve access to MOUD, try to link patients with outpatient opioid treatment programs that provide Buprenorphine treatment (Gastala et al. 2024).

2.2 Prior Authorization for Buprenorphine

The low cost and high efficacy of Buprenorphine in aiding treatment and recovery among individuals with opioid use disorders makes Buprenorphine a high-value medication and an essential tool for the effective and evidence-based treatment of patients who experience opioid use disorders. However, due to its potential for diversion, trafficking, and misuse, Buprenorphine is classified as a Schedule III controlled substance under the Controlled Substances Act (Drug Enforcement Administration 2025). Despite Buprenorphine's relatively low risk profile and the low incidence of misuse and diversion reported by the Office of Inspector General (HHS, 2023), its prescribing has been subject to stringent regulation and oversight, including prior authorization requirements. While designed to ensure appropriate prescribing and prevent opioid diversion, these stringent regulatory measures have inadvertently exacerbated barriers to Buprenorphine treatment access (Landis, 2022). In addition, access to Buprenorphine has been impeded by several factors. First, there is considerable stigma

amongst physicians and patients in willing to prescribe and use Buprenorphine to treat opioid addictions (Blendon and Benson 2018). Second, a number of physicians lack the education to adequately treat patients with opioid use disorders (Woo et al. 2017; Parish et al. 2025). Third, regulatory barriers like the requirement of X-waivers to prescribe Buprenorphine — a requirement under federal law that required providers to complete additional registration and training to prescribe Buprenorphine — and prior authorization requirements, hindered timely access (Bozinoff et al. 2024) to a high-value treatment for a patient population that already faces significant stigma and other barriers to care (Parish et al. 2025).

Between 2007 and 2013, prior authorization was required for prescribing Buprenorphine in 48 state Medicaid programs (Landis et al. 2022). Studies found that prior authorization requirements for Buprenorphine caused treatment delays (Andraka-Christou and Capone 2018; ASAM 2013) and was associated with lowered odds of Buprenorphine provision, putting patients with opioid use disorder at risk of adverse clinical outcomes and overdose deaths (Andrews et al. 2019). Thus, taking evidence into account that Buprenorphine is safe and effective a number of Medicaid programs started repealing the use of prior authorization for prescribing Buprenorphine in both the fee-for-service and managed care organization (MCO) plans (Christine et al. 2023; Andraka-Christou et al. 2023).

Illinois' Medicaid program was the first to repeal prior authorization for all FDA approved Buprenorphine-Naloxone products for both its fee-for-service and managed care beneficiaries in 2015. Rhode Island's Medicaid program followed suit and repealed prior authorization for all Buprenorphine products in 2015. Similar policies repealing prior authorization for all Buprenorphine products were implemented in Maryland and Pennsylvania in 2017 and 2018 respectively. In Delaware and Nebraska, the Medicaid programs repealed prior authorization for Buprenorphine products that were on the states' preferred drug list. In Indiana, Arizona, Washington and Wisconsin prior authorization was repealed for prescribing Buprenorphine-Naloxone formulations between 2017 and 2018 but prior authorization was still required for prescribing long-acting formulations of Buprenorphine. In North Carolina, the Medicaid program repealed prior authorization for generic films of Buprenorphine that were on the states' preferred drug list, however prior authorization was still required for brand name formulations of Buprenorphine. Eventually, the passage of the Substance Use Disorder Prevention that Promotes Opioid Recovery and Treatment for Patients and Communities (SUPPORT) Act — a comprehensive, bipartisan legislation to address the opioid epidemic — required that state Medicaid programs cover at least one formulation of Buprenorphine without prior authorization in its Medicaid plans (H.R.6 Substance Use-Disorder Prevention That Promotes Opioid Recovery and Treatment for Patients and Communities Act or the SUPPORT for Patients and Communities Act 2018). Owing to the SUPPORT Act, since 2019, all states (except Montana) have covered at least one formulation of Buprenorphine without any prior authorization. However, this provision is set to expire at the end 2025, and Medicaid programs could opt to once again impose prior authorizations for prescribing Buprenorphine. Thus, a rigorous and immediate evaluation of policies repealing prior authorization for prescribing Buprenorphine is warranted.

3. Conceptual Framework

We conceptualize prior authorization for prescribing Buprenorphine to Medicaid beneficiaries with opioid use disorder as a form of ordeal — a non-monetary barrier — that is intended to filter out low-benefit users of Buprenorphine. We use the theoretical framework of ordeal mechanisms (Nichols and Zeckhauser 1982; Finkelstein and Notowidigdo 2019; Brot-Goldberg et al. 2023) to provide insight into how prior authorization for prescribing Buprenorphine acts as a barrier to access.

The ordeals mechanism posits that when prior authorization is required for a treatment, physicians who wish to prescribe a treatment must be willing to overcome administrative and other logistical barriers imposed by the prior authorization process. This creates a treatment access hurdle as the prior authorization process disincentivizes providers from recommending the treatment. Prior authorization imposes a hassle cost but offers

Medicaid an opportunity to save some money. However, the benefit of this hassle cost depends on the value of the treatment in question. If the treatment in question is low-value, inducing ordeals through administrative burdens can be an effective way to curb wasteful utilization and control health care spending, however, in the case of high-value treatments, the prior authorization process can prevent patients from obtaining essential care in a timely manner which can potentially generate downstream health care costs in the form of preventable hospitalizations and emergency department visits due to delayed or forgone care. This is particularly concerning given that many patients are reluctant to seek medical care and already encounter numerous barriers to accessing necessary care.

In the context of prescribing Buprenorphine, the prior authorization process imposes a cost C_P on providers for prescribing Buprenorphine. This would include time, paperwork, administrative effort and other logistics that providers must overcome to ensure that opioid-using individuals Medicaid beneficiaries have access to Buprenorphine treatment. Under prior authorization, if a provider chooses to prescribe Buprenorphine to an opioid-using patient and they have to engage in the prior authorization process and incur the ordeal cost. We consider a principal-agent model in which the principal's decisions — representing the provider —are influenced by both the agent's needs — represented by the patient —and the costs involved in navigating the prior authorization process. Therefore, patient i with opioid use disorder would want to maximize private benefit PB_i which can be expressed as:

$$PB_i = B_i - C_P$$

where B_i is the provider-perceived private benefit that patient i derives from access to Buprenorphine and C_P represents the cost of the ordeal imposed by prior authorization. Thus, only individuals for whom $B_i > C_P$ will have access to treatment, while others may either forgo treatment, opt for alternative treatments or access Buprenorphine in the underground market. This creates a provider-selection process, where only a fraction of Medicaid beneficiaries with opioid use disorders have access to Buprenorphine treatment.

Let $i \in [0,1]$ represent a population of opioid-using Medicaid beneficiaries, each with a private provider-perceived B_i, drawn from a continuous distribution $F(B)$ over $[0, \bar{B}]$. In this model, we assume that providers perceive Buprenorphine to confer a strictly positive benefit to all opioid-using patients, such that $B_i > 0 \; \forall \; i$, but that the Medicaid system does not provide automatic coverage. Instead, prior authorization imposes a hurdle, and demand for Buprenorphine is given by:

$$D(C_P) = \{i : B_i \geq C_P\}$$

The total demand for Buprenorphine treatment is therefore given by:

$$Q(C_P) = 1 - F(C_P)$$

When prior authorization is repealed $C_P = 0$. Individuals who previously could not access Buprenorphine due to the prior authorization process are now able to access Buprenorphine treatment. This increased access to Buprenorphine leads to higher individual welfare (through expanded access to treatment) and greater social welfare (through the reduction of opioid-related harm). The ordeal of prior authorization serves as a filter, and restricts Buprenorphine access to only those patients for whom the provider-perceived benefit of treatment outweighs the ordeal cost for the provider. In the canonical ordeals mechanism this corrects for market inefficiencies and addresses moral hazard (Nichols, Pauly).This result — both theoretically and empirically — holds for low-value treatments such as niche branded drugs where the incremental value of the treatment is low relative to its incremental cost (Brot-Goldberg et al. 2023). However, Buprenorphine has high incremental value for opioid-using individuals and access to Buprenorphine treatment generates substantial social benefits beyond the individual patient. These external benefits include but are not limited to reductions in mortality from

overdoses, decreased crime rates associated with opioid addiction, and reduced transmission of infectious diseases like HIV and hepatitis-C.

Let E_i represent the external benefits associated with treating patient i with Buprenorphine. The total social benefit of treating individual i is:

$$SB_i = B_i + E_i$$

where E_i is a positive externality. In this framework, the socially optimal rule for prescribing Buprenorphine should consider both provider-perceived private benefits and externalities, and should be based on the criterion:

$$SB_i = (B_i + E_i) - C_P$$

However, the private benefit calculation for prior authorization fails to account for these externalities. As a result, the prior authorization process restricts treatment to opioid-using individuals who stand to derive significant benefits from Buprenorphine treatment, thereby leading to under-provision of this high-value treatment. This misalignment between individual and social benefits occurs because prior authorization only allows access to those whose provider-weighted private benefit B_i exceeds the administrative cost C_P, not those for whom $B_i + E_i$ exceeds C_P. Assuming that externality E_i from prescribing Buprenorphine is substantial for many opioid-using individuals with $B_i < C_P$, repealing prior authorization for prescribing Buprenorphine may result in an increase in Buprenorphine access. This increased access to Buprenorphine for opioid-using individuals can reduce the likelihood of opioid-related adverse events, potentially lowering downstream health care costs by decreasing opioid-related hospitalizations and emergency department visits.

The conceptual framework motivates our empirical analysis. We aim to quantify the effects of repealing prior authorization for prescribing Buprenorphine on patient access to Buprenorphine and examine the effects of the policy change on opioid use related health care utilization among Medicaid beneficiaries with opioid use disorders. We hypothesize that the likelihood of being prescribed Buprenorphine will increase as the ordeal of prior authorization for prescribing Buprenorphine is repealed. However, we expect the policy change to have only a modest effect on Buprenorphine access for two reasons. First, prior authorization for Buprenorphine has been the norm since 2007, therefore, providers may have adapted to alternate therapies or medications to treat opioid use. Second, as noted in section 2, Buprenorphine treatment is highly stigmatized amongst both patients and providers. Therefore, repealing prior authorization may not be sufficient on its own to substantially improve access to Buprenorphine treatment. Nevertheless, even modest increases in Buprenorphine access may lead to meaningful improvements in health outcomes and reduce potentially preventable acute health care utilization. If access to Buprenorphine improves as a result of repealing prior authorization, then opioid-using individuals are less likely to access addiction care through hospitalizations and emergency department visits. Therefore, we predict that repealing prior authorization for prescribing Buprenorphine will decrease the number of opioid-use related hospitalizations and the number of opioid-use related emergency department visits.

We anticipate heterogeneity in the policy's impact based on individual-level characteristics and state-specific policy contexts. First, the removal of prior authorization requirements for Buprenorphine prescribing is likely to have a more pronounced effect among Medicaid beneficiaries enrolled in managed care plans. Managed care organizations (MCOs) employ utilization management strategies, including prior authorization, more extensively than most fee-for-service Medicaid programs (Baicker and Robbins 2015). This places greater administrative burdens on providers, who must comply with varying prior authorization protocols across multiple MCOs. Eliminating prior authorization requirements, therefore, may reduce provider burden, lower the incidence of denied Buprenorphine claims, and mitigate barriers to initiating or continuing treatment. As a result, the policy change may facilitate greater provider participation and better improve buprenorphine access for individuals with opioid use disorder enrolled in managed care plans.

Second, as stated in Section 2.2, when prior authorization was repealed across states, different states pursued different policies. In some states, the repeal applied to any and all products containing Buprenorphine, whereas in other states, the repeal only applied to specific formulations of Buprenorphine like Buprenorphine-Naloxone. We hypothesize that states with a more generous (less restrictive) repeal of prior authorization requirements for prescribing Buprenorphine will impose a lesser burden on physicians compared to states with more rigid prior authorization policies. Specifically, we predict that states where prior authorization is limited to a restricted list of Buprenorphine formulations will see a lower likelihood of opioid-using Medicaid beneficiaries receiving Buprenorphine compared to states where the repeal of prior authorization for Buprenorphine applies to a broader range of Buprenorphine products. Therefore, we anticipate that the policy change of repealing prior authorization will have a heterogeneous effect on Buprenorphine access and opioid-use related utilization.

4. Methods

4.1 Data Sources

The data for this study come from the Transformed Medicaid Statistical Information System Analytic Files (TAF) data from 21 U.S. Medicaid programs from 2015-2019, through an agreement with the Centers for Medicare and Medicaid Services. Demographic and enrollment information for beneficiaries come from the demographic and eligibility files. Beneficiaries with opioid use disorders are identified using International Classification of Disease Codes (ICD)-9 and ICD-10 diagnoses codes (See Appendix Table 5) from the inpatient and other services claims files. Data on Buprenorphine use come from the pharmaceutical and other services claims files. Data on hospitalizations and emergency department visits come from the inpatient and other services claims files, respectively.

4.2 Study Sample

The study sample includes a repeated cross-section of non-elderly — aged 18 to 64 years — Medicaid beneficiaries who have a claims-based diagnostic record of opioid use disorder. In 2015 for all 21 states and for Maryland between 2015 and 2017, we used ICD-9 codes to identify beneficiaries with opioid use disorders; for other states after 2016, we use ICD-10 codes to identify to identify beneficiaries with opioid use disorders.

Drug prescription coverage for dual eligible beneficiaries is largely covered by Medicare, and Medicaid only provides partial coverage for Buprenorphine. We therefore exclude beneficiaries who are dual eligible. In one state (Alabama) we cannot exclude duals due to data quality issues. We exclude Alabama from the main sample in a sensitivity analysis to ensure robustness of our findings to the potential inclusion of duals in the analysis. We build on the minimal data quality checks from DQ Atlas for the years 2015-2019 and further exclude states from the analysis in which we cannot reliably ascertain encounters for Buprenorphine prescriptions, inpatient hospitalizations or emergency department visits (Centers for Medicare & Medicaid Services 2025). A detailed description of the inclusion criteria for states is noted in Appendix Tables 1 and 2.

The final study sample includes data from 21 states for 737,379 unique Medicaid enrollees with a diagnostic record of opioid use disorder.

4.3 Study Variables

4.3.1 Treatment Variable: Indicator for Repeal of Prior Authorization for prescribing Buprenorphine

The main explanatory variable of interest is whether an individual with opioid use disorder was in a state Medicaid program in a given time period (quarter) repealed prior authorization for prescribing Buprenorphine. We use a dichotomous indicator that equals 1 if an individual is in a state that repealed the use of prior authorization for prescribing Buprenorphine during the study period (treatment group) and 0 if an individual is in a state that did not repeal the use of prior authorization for prescribing Buprenorphine during the study period (control group). This dichotomization results in 11 states — Arizona, Delaware, Hawaii, Indiana, Illinois,

Maryland, Nebraska, North Carolina, Rhode Island, Washington, Wisconsin — in the treatment group and 10 states in the control group. See Figure 1.

Implementing the inclusion restrictions and accounting for data quality checks, the final study sample consists of 295,521 unique opioid-using Medicaid beneficiaries in states where prior authorization was repealed for prescribing Buprenorphine and 441,858 unique opioid-using Medicaid beneficiaries in states that where prior authorization was required for prescribing Buprenorphine. These 737,379 opioid-using Medicaid beneficiaries contribute to 6,281,448 individual-quarter level observations in the dataset.

4.3.2 Dependent Variables

The study examines the impact of repealing prior authorization for prescribing Buprenorphine on four outcomes: (1) likelihood of receiving Buprenorphine, (2) number of Buprenorphine claims, (3) number of inpatient hospitalizations and (4) number of emergency department visits among Medicaid beneficiaries with opioid use disorders. Buprenorphine claims in the claims data are identified through National Drug Classification (NDC) codes and are dichotomized accordingly to explore the effects of the policy change on the likelihood of receiving Buprenorphine. Number of hospitalizations are identified through an algorithm of federally assigned service category (FASC) noted by TAF (Hula et al. 2021). Emergency department visits are identified through procedure codes and revenue center codes (NCQA HEDIS 2022). The NDC codes and procedure codes used to identify opioid-using individuals, Buprenorphine prescriptions and health care utilization are detailed in Appendix Table 6.

4.3.3 Other Variables

We estimate all models controlling for a beneficiary's age, sex and whether or not they were enrolled in a managed care plan. In addition, we include state fixed effects and time (measured as quarters) fixed effects in all estimations to account for unobserved heterogeneity across states and over time periods. The covariates are summarized in Table 1. Medicaid beneficiaries with opioid use disorders across both states with (control group) and without prior authorization (treatment group) for prescribing Buprenorphine were on average 38 years old. In both the treatment and control groups Medicaid beneficiaries with opioid use disorders were more likely to be male with a higher proportion of men in the treatment group, 64% vs. 52%. Beneficiaries in the treatment group were more likely — 72% versus 58% — to be enrolled in Medicaid managed care plans compared to fee-for-service Medicaid.

5. Empirical Strategy

We use a difference-in-differences regression framework with staggered timing to examine the impact of repealing prior authorization for prescribing Buprenorphine on Buprenorphine access and health care utilization among Medicaid beneficiaries employing multiple estimators for transparency and comparison. The main source of identifying variation is whether a state Medicaid program repealed prior authorization for prescribing Buprenorphine. The identifying assumption in this research design is the parallel-trends assumption. In the context of this study, the assumption warrants that, trends in outcomes across states that repealed the use of prior authorization for prescribing Buprenorphine and states that allowed the use of prior authorization for prescribing Buprenorphine should be similar in absence of the policy change. While inherently untestable after the policy change, a standard way to assess the credibility of this assumption is to examine whether trends are parallel prior to implementation. We additionally test the parallel-trends assumption by plotting the pre-trends in covariates-adjusted outcomes in our preferred event study design using the Callaway and Sant'Anna (2021) estimator described later. The difference-in-differences research design also requires that the stable unit treatment value assumption (SUTVA) and the assumption of common shocks is satisfied (Cliff 2022). To satisfy SUTVA — which requires no interference between treatment and control units — we exclude beneficiaries who move across states from the analysis. To ensure that the common shocks assumption is satisfied we conduct robustness checks in Section 7 by excluding states that in addition to repealing prior

authorization for prescribing Buprenorphine simultaneously implemented other Medicaid policies that aimed to expand Buprenorphine access.

To analyze the impact of repealing prior authorization for prescribing Buprenorphine, we start by estimating the following canonical two-way fixed effects model:

$$\boldsymbol{Y_{ist} = \alpha + \beta_1 \, (PA\ Repeal)_{ist} + X'_{ist}\gamma + \theta_s + \delta_t + u_{ist}}$$

where i indexes the individual, s indexes the states and t indexes the time period.

In the above equation $\boldsymbol{Y_{ist}}$ are patient-level outcomes of interest, $\boldsymbol{X'_{ist}}$ is a vector of time varying controls,$\boldsymbol{PA\ Repeal_{st}}$ equals 1 if individual i is in state s with prohibition on prior authorization for prescribing Buprenorphine in time period t. $\boldsymbol{\theta_s}$ and $\boldsymbol{\delta_t}$ are state fixed effects and quarter fixed effects, respectively. $\boldsymbol{u_{ist}}$ is a mean zero error term clustered at the state level. The coefficient of interest is $\boldsymbol{\beta_1}$, which provides the estimate of how change in outcomes differ across states that did and did not repeal prior authorization for prescribing Buprenorphine. The equation is estimated separately for each outcome.

Recent advances in econometric theory suggest that standard two-way fixed effects models can provide biased estimates when there is variation in treatment timing, as the estimate may capture heterogeneity and variation in the effect rather than the average treatment effect on the treated. More specifically, the use of earlier-treated units as controls for later-treated units — often referred to as a forbidden comparison — will bias the treatment effect if the impact of earlier implementation grows or wanes over time (De Chaisemartin and D'Haultfœuille 2020; Sun and Abraham 2021; Callaway and Sant'Anna 2021; Roth et al. 2023). Since, prior authorization for Buprenorphine prescribing was implemented across different states in different quarters, using a two-way fixed effects model without accounting for variation in treatment timing might violate the parallel trends assumption and provide incorrect estimates (Goodman-Bacon 2021). In order to account for any bias arising from variation in treatment timing across the study period, the models are estimated using methods described in Chaisemartin and D'Haultfœuille (2020) and Callaway and Sant 'Anna (2021). The staggered difference-in-differences models specified using Chaisemartin and D'Haultfœuille (2020) and Callaway and Sant'Anna (2021) ensure that the parallel trends assumption holds when there is variation in treatment timing across units. To formally test the parallel-trends assumption, a dynamic event study of the form is estimated for of all periods using the above-mentioned estimators for each outcome.

We consider the Callaway and Sant'Anna (2021) estimator as the preferred model, since it more effectively avoids problematic 2 × 2 difference-in-differences comparisons that violate the parallel-trends assumption and derives estimates under more general conditions. In doing so, the estimator avoids the forbidden comparison between early and later-treated unit and yields an interpretable average treatment effect on the treated estimate under generalizations of the parallel trends assumption (Roth et al. 2023; Wang et al. 2024). However, given the lack of consensus on which estimator is most appropriate in which cases, for the sake of transparency, results from three estimators — two-way fixed effects, Callaway and Sant'Anna (2021) and Chaisemartin and D'Haultfœuille (2020) — are presented.

6. Results

6.1 Main Results

Estimated effects of repealing prior authorization for prescribing Buprenorphine on the likelihood of receiving Buprenorphine, number of Buprenorphine claims, opioid-use related hospitalizations and opioid-use related emergency department visits are presented in Table 2. Column 1 contains estimated coefficients from the canonical two-way fixed effects models. The estimated coefficients in Column 1 suggest that repealing prior authorization for prescribing Buprenorphine increased the likelihood of receiving Buprenorphine and the number of the Buprenorphine claims. Thus, the estimated coefficients in Column 1 show that repealing prior authorization for prescribing Buprenorphine increased Buprenorphine access and decreased the number of

opioid-related hospitalizations and emergency department visits. However, the estimated effects are not significantly different from zero. The estimates from the canonical two-way fixed effects models are informative but may be subject to problems noted above with two-way fixed-effects models when there is variation in treatment timing.

Table 2, Column 3 shows estimated coefficients from the Callaway and Sant'Anna (2021) estimator. Within this specification, repealing prior authorization for prescribing Buprenorphine is associated with an estimated 3 percent rise in the likelihood of receiving Buprenorphine. The policy change is also associated with an increase of 0.19 Buprenorphine prescriptions, a decrease of 0.03 opioid-use related hospitalizations, and a (statistically insignificant) decrease of 0.003 opioid-use related emergency department visits per quarter. Figure 2 graphically presents the dynamic event study effects of repealing prior authorization, as estimated from the Callaway and Sant'Anna (2021) method. Estimated effects in most of the pre-treatment periods are not significantly different from zero for all four outcomes, suggesting no obvious violation of the parallel-trends assumption. We find similar results implementing the regression model using the Chaisemartin and D'Haultfœuille (2020) method.

In the post-treatment periods, we see that repealing prior authorization for prescribing Buprenorphine slightly increases the likelihood of receiving Buprenorphine, and the same is observed for the number of Buprenorphine claims suggesting that repealing prior authorization for prescribing Buprenorphine has a modest increase on the likelihood of receiving Buprenorphine and the number of Buprenorphine prescriptions over time. The results also suggest that the number of opioid-use related hospitalizations decreases over time in the post-treatment period, especially in the latter half of the post-treatment period. However, opioid related emergency department visits remain largely constant, and the effects found in the post-treatment period are statistically insignificant. We report estimated event study effects of the policy change in all pre- and post-treatment periods in Appendix Tables 7, 8 and 9. The tables show that the estimated dynamic effects tend to be slightly higher and statistically significant for the Callaway and Sant'Anna (2021) and Chaisemartin and D'Haultfœuille (2020) estimators compared to the two-way fixed effects model, likely due to the fact that the Chaisemartian and D'Haultfœuille (2020) and Callaway and Sant'Anna (2021) models account for variation in timing of policy change.

6.1 Heterogeneity

To test for heterogeneous effects of repealing prior authorization for prescribing Buprenorphine, we stratify our main regression equation by several variables using the Callaway and Sant'Anna (2021) estimator. First, we stratify by whether or not an individual was enrolled in a managed care plan. Stratifying the results by managed-care enrollment status underscores strong contrasts in the results. For enrollees in a managed care plan, repealing prior authorization for prescribing Buprenorphine significantly increases the likelihood of receiving Buprenorphine, significantly increases the number of Buprenorphine claims, and significantly decreases the number of opioid-use related hospitalizations. In contrast, we do not find statistically significant effects on these outcomes for enrollees in fee-for-service plans. For both categories of enrollees, we do not find statistically significant effects of the policy change on opioid-use related emergency department visits. We present these estimates in Columns 1 and 2 of Table 3.

Second, we stratify by restrictiveness of the prior authorization repeal for prescribing Buprenorphine. States that repealed prior authorization for all Buprenorphine products — Maryland, Delaware, Nebraska, Hawaii, Rhode Island —were categorized as non-restrictive and states that repealed prior authorization for only Buprenorphine-Naloxone formulations — Arizona, Illinois, North Carolina, Washington, Indiana, Wisconsin—were categorized as states with restrictive prior authorization repeals. We define states with restrictive and non-restrictive prior authorization repeals as separate treatment groups in this analysis. We find that states that repealed prior authorization without restrictions saw a significant 6 percent increase in the likelihood of receiving Buprenorphine. These states also saw a significant increase in the number of Buprenorphine claims, and a significant decrease in both opioid-use related hospitalizations and emergency department visits. For states that repealed prior authorization with restrictions, we found that the said policy change significantly increased the likelihood of receiving Buprenorphine. However, the effect size was smaller compared to states

that repealed prior authorization without restrictions. We also found that for these states, the number of Buprenorphine claims increased, the number of hospitalizations decreased and the effect on the number of emergency department visits was statistically. Estimates are presented in columns 3 and 4 of Table 3.

7. Robustness Checks

We conducted several robustness checks. First, a common issue with analyses of state policies is unobserved confounding, in particular if implementation of prior authorization repeal policies occurred at the same time as other policies that could affect Buprenorphine access and health care utilization. Our estimated results would be biased if there are unobserved state policies that are associated with trends in Buprenorphine access and with health care utilization. To mitigate this concern, we collect information on other Medicaid policies potentially related to Buprenorphine access. One such policy is Delivery System Reform Incentive Program (DSRIP) — a pay-for-performance program aimed at improving medication assisted treatment coverage of Medicaid beneficiaries — which was implemented during the study period in Arizona and Washington (Hinton et al. 2022). We individually and collectively exclude Arizona and Washington from our main analyses to test the robustness of our results to the inclusion of these states in our data. The results are presented in Appendix Table 10. We find that our results are robust to the exclusion of Arizona and Washington from the main sample frame which suggests that our results are not biased by the implementation of the respective policies in Arizona and Washington during the same time period.

Second given that we are not able to exclude duals due to poor data quality of the dual eligibility status variable for Alabama, we conducted a sensitivity analysis by excluding Alabama from the main sample. The results are presented in Column 1 of Appendix Table 11. We find that our results are robust to the exclusion of Alabama from the main sample, which suggests either that there are few opioid-using dual eligible beneficiaries in the state of Alabama or that the potential inclusion of duals in a single state does not significantly influence the study outcomes. In addition, three other states in the dataset — North Carolina, Maine and Rhode Island — exhibit potential issues with their claims volumes and/or enrollment data. Therefore, we conduct a sensitivity check by excluding these states from our main analysis. The results are presented in Columns 2-4 of Appendix Table 11. We find that the exclusion of these states slightly changes the magnitude of the coefficient estimates but the direction of the estimates are consistent with the results of our main analysis.

Third, to ensure that no single state is driving the results in our main analysis beyond the issues described above, we conduct a sensitivity check by sequentially leaving out one state from the main sample using the Callaway and Sant'Anna (2021) method. The results are presented in Appendix Figure 1 and Appendix Table 12. We find that the estimates obtained at each step in this sensitivity analysis are qualitatively similar and the main results using the Callaway and Sant'Anna (2021) estimator (presented in Table 2, Column 3) are not driven by any particular state in the sample.

Finally, we conduct two sensitivity checks on the use of ordinary least squares (OLS) regressions. First in our main specification for the outcome of likelihood of receiving Buprenorphine, we use a linear probability model to estimate the effect of the policy change on the likelihood of receiving Buprenorphine. We do so because the estimator that corrects for mismeasurement in a difference-in-differences framework with staggered treatment timing is built upon OLS regressions, with unclear validity in other models. Addressing potential bias caused by staggered treatment timing was critical in this analysis (Qi et al. 2024). However, it may be more appropriate to estimate this model using a logistic regression, given the binary outcome. We thus test the sensitivity of our results to this choice by comparing the results from a two-way fixed effects linear regression to a two-way fixed effects logistic regression model. Second, in our main specification for the outcomes of Buprenorphine claims, hospitalizations and emergency department visits, we treat the outcomes as continuous variables. However, it may be more appropriate to treat these outcomes as count data since they are nonnegative integers with a skewed distribution. We thus test the sensitivity of our results to this choice using two-way fixed effects negative binomial regressions, which better accommodate count data with a large fraction of zeros. These estimates, shown in Appendix Table 13, have the same direction as the estimates from the two-way fixed effects

OLS regression models. Similar to the two-way fixed effects linear models, the estimates from the two-way fixed effects logistic regression model and the two-way fixed effects negative binomial regression models — controlling for state and time fixed effects, demographic characteristics — are not statistically significant from zero. The size of the estimated effects are smaller for the likelihood of receiving Buprenorphine and number of Buprenorphine claims, and similar for the number of hospitalizations and emergency department visits. The similarity of these estimates suggests that using linear models to estimate the effect of policy change for our outcomes does not result in substantial bias, although it may bias our magnitudes somewhat.

8. Discussion and Conclusion

In this study, we provide empirical estimates of the effects of prior authorization policies on access to high-value care, using the repeal of prior authorization for Buprenorphine in Medicaid programs as an example. Our results contribute to ongoing policy debates about implementing prior authorization policies to contain rising health care costs without restricting access to essential treatments and services for vulnerable populations.

Our results are consistent with the theoretical prediction that repealing prior authorization for Buprenorphine — a treatment which confers positive externalities and social benefits — improves Buprenorphine access for Medicaid beneficiaries with opioid use disorders. A back of the envelope calculation — based on estimates in Table 2 — suggests that on average, the prior authorization repeal for prescribing Buprenorphine resulted in approximately 88,000 more Medicaid enrollees with opioid use disorder getting access to some formulation of Buprenorphine within a year. The results also partially support the theoretical claim that higher Buprenorphine access reduces downstream health care utilization for opioid-using individuals. We show that prior authorization repeal is associated with a 25 percent in decrease in the number of opioid-use related hospitalizations. The effects we find on the estimates for Buprenorphine access (see Table 2, Column 3) are consistent with other studies on the prior authorization repeal for Buprenorphine (Keshwani et al. 2022) and also consistent with other studies of reduced administrative burden — like removing X-waivers — for prescribing Buprenorphine (Christine et al. 2024).

We do not find any statistically significant results for opioid-use related emergency department visits. This might suggest two things. First, it might suggest that the marginal patient with opioid use disorder affected by prior authorization might not be responsive to greater Buprenorphine prescribing. Thus, simply increasing Buprenorphine access — without addressing systemic barriers in access to care — might not be enough to reduce emergency department usage in this population. Second, it might suggest that despite a more conducive policy environment for prescribing Buprenorphine in office-based settings, a significant portion of Buprenorphine treatment continues to be accessed through the emergency department. This may indicate broader issues related to limited acceptance of Medicaid by providers and lack of care coordination between emergency departments and outpatient opioid treatment programs. In this case, the health system should make efforts to optimize linkages between emergency departments and outpatient addiction treatment programs to provide appropriate treatment for opioid-using individuals. In tests of heterogeneity, we found that the estimated effects are stronger for managed care compared to fee-for-service enrollees. We also found slightly greater estimated effects in states where the policy change was implemented without restrictions on which Buprenorphine products or formulations required prior authorization compared to states where prior authorization was repealed for select Buprenorphine products and formulations.

Our study highlights that removing time and resource-intensive ordeals like prior authorization for high-value health care services can improve access to essential health care services for vulnerable populations, this case Medicaid beneficiaries with opioid use disorders. While, the ordeals mechanism offers policymakers an assurance that burdens fulfill a useful social function by optimally targeting scarce resources to those most in need, targetting high-value, life-saving treatments like Buprenorphine may have unintended consequences as impeding acces to essential care for high-risk patients, generate downstream health care costs (Herd et al. 2023). However. Using the example of Buprenorphine prescribing for opioid using patients, our findings demonstrate that easing administrative barriers positively affects access to essential health care services (Dunn et al. 2024)

and fosters a more cost-effective care system by reducing acute health care utilization. Further suggesting that imposing authorization restrictions on low-cost, high-value treatments (like Buprenorphine), tradesoff arduous administrative burden for little to no value and are therefore are likely a deterrent to essential care access (Brot-Goldberg et al. 2023).

The findings from this study should be interpreted in light of several study limitations. First, we only studied one set of patient outcomes, and provided suggestive evidence towards cost-savings. A more nuanced analysis, with better spending data can provide better insight into the tradeoffs at play between bureaucracy, cost-savings and access to care. Second, due to lack of data on claims denials and lack of good quality data on overdose diagnoses codes in the TAF, we were not able to fully examine the impact of prior authorization on provider behavior and health outcomes, both of which are likely to be affected by repealing prior authorization for prescribing Buprenorphine. Future research with better quality data and/or mixed-methods research should shed some light on how prior authorization or the lack thereof for high-value treatments influences provider behavior and other pertinent patient and social outcomes. Third, using Buprenorphine as an example to argue against prior authorization for high-value care may lack external validity. While Buprenorphine is an effective intervention, its unique social and clinical context complicates its generalizability to other high-value treatments. Nevertheless, given demonstrated efficacy of Buprenorphine treatment and its significant underutilization, Buprenorphine serves as a salient example for exploring the broader issues of access to high-value care and the impact of prior authorization policies. Fourth, state TAF records varied in data quality, which may have influenced the results. To address this concern, we excluded some states on the basis of our review of TAF data quality assessments using Medicaid's Data Quality Atlas and outcomes trends for each state (see Appendix Table 2). However, we could not rule out that some data quality issues remained. Lastly, compared to Medicaid data from state agencies, TAF is estimated to underreport opioid use disorder diagnoses by about 11 percent (Chughtai et al. 2025). Thus, the number of individuals with opioid use disorder in the study sample might be undercounted. However, the estimated prevalence of opioid use disorder (3.3%) in our study sample is consistent with estimates of opioid use disorder prevalence in other studies of Medicaid beneficiaries using TAF data (Lindner et al. 2023; 2024).

As prior authorization is more widely adopted in health insurance programs, policymakers must aim to startegically design cost-control measures to target low-value treatments and ensure access to essential care. Our study — using the example of Buprenorphine access among Medicaid beneficiaries — shows that repealing prior authorization improves access, however, opportunities exist to correctly-target Buprenorphine formulations that might constitute as wasteful. An evidence-informed strategy using our example, could be to incentivize the prescription of generic Buprenoprhine formulations by requiring prior authorization for its more-costly, bio-equivalent brand-name formulation. Requiring prior authorization for close but expensive subsitiutes of high-value treatments, creates a manageable burden that can nudge providers towards more cost-effective treatment choices without impeding access to essential care and adversely affecting patient health. Our research highlights that easing administrative barriers for high-value and undertilized treatments can significantly increase patient access to essential evidence-informed treatments. However, to ensure appropriate utilization of services, strategically refining prior authorization requirements offers a more cost-effective solution. In conclusion, with increasing use of prior authorization across health insurance programs, policymakers — by requiring prior authroization for treatments that are truly wasteful and repealing prior authorization for services that are high-value and cost-effective — can strike a balance that enhances and ensures care access while controlling costs in a fiscally strained health care system.

As prior authorization is more widely adopted in health insurance programs, policymakers must aim to startegically design cost-control measures to target low-value treatments and ensure access to essential care. Our study — using the example of Buprenorphine access among Medicaid beneficiaries — shows that repealing prior authorization improves access to care. However, opportunities exist to correctly target Buprenorphine formulations that might be wasteful, like brand-name Buprenorphine. An evidence-informed strategy, using our example, could be to incentivize the prescription of generic Buprenoprhine formulations by requiring prior

authorization for its bio-equivalent brand-name formulation. Requiring prior authorization for close but expensive substitutes for high-value treatments creates a manageable burden that can nudge providers towards more cost-effective treatment choices without impeding access to essential care and adversely affecting patient health. Our research highlights that easing administrative barriers for high-value and undertilized treatments can significantly increase patient access to essential evidence-informed treatments. In conclusion, with increasing penetration of prior authorization across health insurance programs, policymakers — by requiring prior authorization for the treatments that are truly wasteful and repealing prior authorization for services that are high-value and cost-effective — can strike a balance that enhances and ensures access to care while controlling costs in a fiscally strained health care system.

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Table 1: Baseline Characteristics of Medicaid Beneficiaries with Opioid Use Disorders

	Full Sample	States without Prior Authorization for Buprenorphine	States with Prior Authorization for Buprenorphine
	N=737,379	**N=295,521**	**N=441,858**
Age, Mean (SD), Years	38.36 (12.07)	38.01 (12.01)	38.61 (12.09)
Sex, (%)			
Male	56.79	64.19	52.10
Female	43.21	35.81	47.90
Insurance Status, (%)			
Managed Care	58.89	72.06	50.09
Fee-for-Service	41.11	27.94	49.91

Figure 1: States implementing Repeal of Prior Authorization for prescribing Buprenorphine

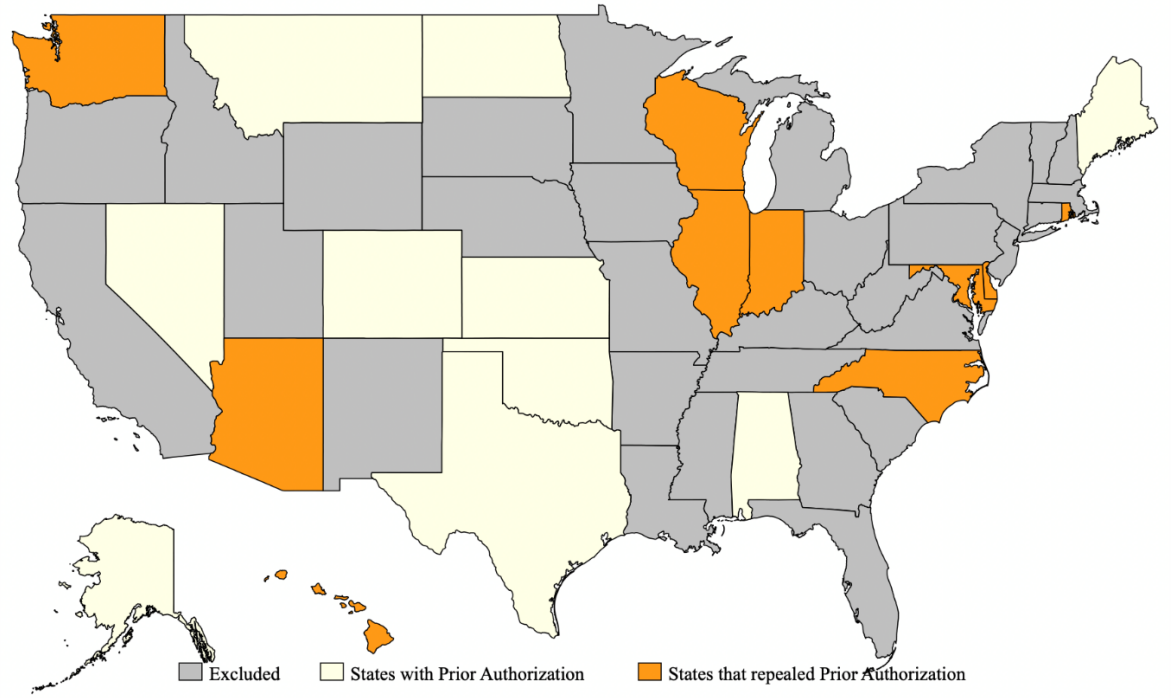


Table 2: Estimates from Difference-in Differences Models

	Two-Way Fixed Effects	**Chaisemartin and D'Haultfoeuille (2024)**	**Callaway and Sant'Anna (2021)**
	Effect of Policy Change (95% CI)	**Effect of Policy Change (95% CI)**	**Effect of Policy Change (95% CI)**
Likelihood of Receiving Buprenorphine	0.017	**0.029*** **	**0.031*** **
Number of Buprenorphine Claims	0.126	**0.153** **	**0.189*** **
Number of OUD-related Hospitalizations	-0.006	**-0.025*** **	**-0.022*** **
Number of OUD-related ED Visits	-0.015	-0.003	-0.008
Fixed Effects			
States	Yes	Yes	Yes
Time (Quarter)	Yes	Yes	Yes
Observations	6,281,448	5,974,562	6,146,150

Note: ***p<0.01; **p<0.05; *p<0.1

Figure 2: Event Study Plots from Differences in Differences Model using Callaway and Sant'Anna (2021)

Table 3: Heterogeneous Effects of Policy using Chaisemartin and D'Haultfoeuille (2020)

	Managed Care	Fee-for-Service	States repeal Prior Authorization without restrictions	States repeal Prior Authorization with restrictions
			DE, HI, MD, RI, NE	**AZ, IL, IN, WA, WI, NC**
Likelihood of Receiving Buprenorphine	**0.05*** **	-0.02	**0.06*** **	**0.02*** **
Number of Buprenorphine Claims	**0.28*** **	-0.09	**0.32*** **	**0.11*** **
Number of OUD-related Hospitalizations	**-0.02*** **	-0.02	**-0.02** **	**-0.04** **
Number of OUD-related ED Visits	-0.004	-0.004	**-0.03** **	0.005
Fixed Effects				
States	Yes	Yes	Yes	Yes
Time (Quarter)	Yes	Yes	Yes	Yes
Observations	3,904,200	2,241,950	2,720,626	3,425,524

Note: ***p<0.01; **p<0.05; *p<0. In columns (3) and (4), we included the states as separate treatment groups and compare them to the states in the control group. The results come from two separate regression, in column (3) the treatment group states are Delaware, Hawaii, Maryland, Nebraska and Rhode Island. In column (4), the treatment group states are Arizona, Illinois, Indiana, North Carolina, Washington and Wisconsin.

Appendix

Appendix Table 1: Cohort Size

	Number
Number of Beneficiaries ages 18-64	26,427,278
Number of Beneficiaries with Opioid Use Disorders	853,364
Excluded for DQ issues (FL,KY)	115,985
Final Sample	**737,379**

Appendix Table 2: State Inclusion based on Key Variables

While the Transformed Medicaid Statistical Information System Analytic Files (TAF) represent an advance in harmonization among Medicaid programs compared with earlier datasets, quality is not uniform across all years and all states. We use the Data Quality Atlas (DQ Atlas), a website maintained by the Centers for Medicare and Medicaid Services, to check each variable we use in our analysis for quality in each state.1 The DQ Atlas is structured by variable, but does not include all variables in the data. For variables it does include, the DQ Atlas rates each state in each year as being low, medium or high concern, or unusable, on each variable or measurement concept (e.g. enrollment count). We use the DQ Atlas as a baseline for evaluating the quality of key variables in each state and year, paying specific attention when the DQ Atlas flags a variable or measurement as unusable or high concern. In keeping with best practices for data use, we additionally check each variable in our specific population. We note where our assessment differs from the DQ Atlas.

For variables without specific DQ Atlas guidance, we run checks in our baseline sample on percent of missingness in each state and either exclude states or include with a sensitivity check as described below.

Key Variable	Potential States Included	States Excluded due to Known Variable Quality Issues	States Included with Caution (Sensitivity Checks)	Notes
Beneficiary ID/TMSIS ID	AL, AK, AZ, CO, DE, DC, FL, HI, IL, IN,KS, KY, ME, MD, MT, NE, NV, NH, NM, NC, ND, OK, RI,TX, WA, WI	None	None	No missingness noted. No DQ Atlas guidance on this specific variable.
Dual Eligibility Code	AL, AK, AZ, CO, DE, DC, FL, HI, IL, IN,KS, KY, ME, MD, MT, NE, NV, NH, NM, NC, ND, OK, RI,TX, WA, WI		AL	DQ Atlas notes that dual eligibles cannot be identified reliably in AL. We include the state with caution.
Diagnosis Codes	AL, AK, AZ, CO, DE, DC, FL, HI, IL, IN,KS, KY, ME, MD, MT, NE, NV, NH, NM, NC, ND, OK, RI,TX,WA, WI	None	None	No DQ Atlas guidance on this specific variable.
Nadtional Drug	AL, AK, AZ, CO, DE, DC, FL, HI, IL, IN,KS, KY, ME, MD, MT, NE, NV, NH, NM, NC, ND, OK, RI,TX, WA, WI	None	None	No DQ Atlas guidance

Classification Codes (NDC)				on this specific variable.
Procedure Codes	AL, AK, AZ, CO, DE, DC, FL, HI, IL, IN, KS, KY, ME, MD, MT, NE, NV, NH, NM, NC, ND, OK, RI,TX, WA, WI	None	ND (OT-2018; 2019), NE (OT; 2015)	For ND and NE, DQ Atlas notes less than 5% percent missingness for ND and NE in the mentioned years.
Type of Bill Code	AL, AK, AZ, CO, DE, DC, FL, HI, IL, IN, KS, KY, ME, MD, MT, NE, NV, NH, NM, NC, ND, OK, RI,TX, WA, WI	None	None	DQ Atlas notes less than 5% missing Bill Type Codes in the IP file for the sample, and 98% percent of the bill type codes are expected.
Managed Care Encounter Data	AL, AK, AZ, CO, DE, DC, FL, HI, IL, IN, KS, KY, ME, MD, MT, NE, NV, NH, NM, NC, ND, OK, RI, TX, WA, WI	FL		Managed Care Data is of poor quality in both the Rx and OT files for Florida.
Claims Volume	AL, AK, AZ, CO, DE, DC, FL, HI, IL, IN, KS, KY, ME, MD, MT, NE, NV, NH, NM, NC, ND, OK, RI, TX, WA, WI	FL, KY	RI, NC	DQ atlas notes that FL and KY have low and high claims volumes respectively in the data
Medicaid Enrollment	AL, AK, AZ, CO, DE, DC, FL, HI, IL, IN, KS, KY, ME, MD, MT, NE, NV, NH, NM, NC, ND, OK, RI, TX, WA, WI	KY	ME, RI	DQ atlas notes that ME and RI have high concern enrollment data

Final State Sample	AL, AK, AZ, CO, DE, HI, IL, IN, KS, ME, MD, MT, NE, NV, NC, ND, OK, RI, TX, WA, WI	FL, KY	RI, NC, ME, AL	

Appendix Table 3: TAF Checklist

Category	Description	Specific to this study
Data		
Files, Years, and Release Versions	• Indicate which TAF files were used in the analysis (e.g., Demographic and Eligibility, Inpatient, Other Services, etc.). • Indicate which years of TAF data were included in the analysis. • Indicate which file versions were included in the analysis (e.g., preliminary, release 1, release 2, etc.). • Indicate whether the study drew from 100% TAF files or pre-specified extracts.	(a) Demographic and Eligibility, Inpatient, Other Services and Rx (b) 2015 -2019 (c) Final release (Release 2 for 2015-2018, Release 1 for 2019) (d) Received 100% TAF files
Analytic Sample		
Eligibility Criteria	• If applicable, describe what eligibility category codes were used to identify the study sample and whether they were used in combination with any other variables (e.g., age, receipt of specific medical services, etc.).	From full TAF files, we extracted enrollees ages 18-64, and identified individuals with a diagnostic record of opioid use disorder using ICD-9 and ICD-10 codes.
Enrollment Span	• If applicable, indicate the minimum period of enrollment required for an enrollee to be included in the study sample and how the enrollment period was defined.	No enrollment span criteria used for main analysis.

Scope of Benefits	• Indicate whether the analysis included enrollees with full scope, comprehensive, or restricted benefits.	No criteria used for main analysis.
Encounter Data	• Indicate whether the analysis excluded either fee-for-service or managed care enrolees. If managed care enrolees were excluded, define the criteria used to do so. • Indicate which types of claims records (e.g., fee-for-service claims, service tracking claims, capitation payments, etc., see variable CLM_TYPE_CD) were included in the analysis.	(a) For the purposes of this analysis, both managed care and FFS enrolees were included. (b) For encounter data, claims types codes 1,3, A and C were analyzed.
Dual Eligibility	• Describe whether individuals dually enrolled in Medicare and Medicaid were included in or excluded from the study sample and, if applicable, how dual eligibility was defined.	Dual eligibles were excluded from the study sample for all states in the sample except Alabama.
State and Territory Exclusions		

Criteria	• Indicate which states and/or territories were included (or excluded) from the analysis on the basis of data quality concerns. • Indicate the criteria by which state exclusions were made, including measures, data sources, and thresholds.	a) We exclude FL and KY b) FL and KY were excluded due to data quality issues related to poor quality data on managed care encounters and high claims volumes, respectively. c) Other states are excluded because they don't report data to TAF in 2015.
State variation table	• Consider including a state-level table (which may appear in an appendix) summarizing the number of observations, means, medians, and missingness for key study measures	See Appendix Table XX
Special Considerations		
Spending	• Indicate which types of claims records (e.g., fee-for-service claims, service tracking claims, capitation payments, etc., see variable CLM_TYPE_CD) were included to measure spending. • If including service-specific spending for managed care encounters, indicate how spending was imputed (payments from plans to providers on encounter records are generally redacted).	Spending was not assessed in this analysis
Using TAF with Predecessor Medicaid Analytic eXtract	• Indicate if the analysis included data from the Medicaid Analytic eXtract (MAX) and, if	No linkages were made with MAX data.

(MAX) Data	so, for what years and which states. • If applicable, include an exhibit examining trends in key measures by state over time and particularly during any transition from MAX to TAF.	

Appendix Table 4: Details of Prior Authorization Repeal Policies

State	Repeal Month-Year	Description of Policy
Arizona	January-2018	PA not required for buprenorphine-naloxone sublingual films. PA still required for long-acting buprenorphine.
Delaware	July-2017	Buprenorphine-naloxone added to PDL without prior authorization, along with other preferred agents for OUD.
Hawaii	June-2017	PA removed for Buprenorphine prescribing.
Illinois	July-2015	Medicaid fee for service and managed care programs cover FDA-approved buprenorphine-naloxone for OUD without a PA
Indiana	December-2017	Repealed PA for buprenorphine-naloxone, PA still required for films.
Maryland	May-2017	PA not required for buprenorphine since May 2017.
Nebraska	November-2017	PA repealed for preferred formulations of buprenorphine
North Carolina	January-2018	PA removed for preferred generic buprenorphine tablet/film. PA still required for non-preferred formulations
Rhode Island	September-2015	PA removed for buprenorphine
Washington	January-2018	PA removed for preferred generic buprenorphine tablet/film. PA still required for non-preferred formulations
Wisconsin	July-2018	PA removed for preferred generic buprenorphine tablet/film. PA still required for non-preferred formulations

Appendix Table 5: Diagnosis Codes used to identify enrollees with Opioid Use Disorders

ICD-9 Code	Description
304.00	Opioid type dependence, unspecified
304.01	Opioid type dependence, continuous
304.02	Opioid type dependence, episodic
304.03	Opioid type dependence, in remission
304.70	Combinations of opioid type drug with any other drug dependence, unspecified
304.71	Combinations of opioid type drug with any other drug dependence, continuous
304.72	Combinations of opioid type drug with any other drug dependence, episodic
304.73	Combinations of opioid type drug with any other drug dependence, in remission
305.50	Opioid abuse, unspecified
305.51	Opioid abuse, continuous
305.52	Opioid abuse, episodic
305.53	Opioid abuse, in remission
304.00	Opioid type dependence, unspecified
304.01	Opioid type dependence, continuous
304.02	Opioid type dependence, episodic
304.03	Opioid type dependence, in remission
304.70	Combinations of opioid type drug with any other drug dependence, unspecified
ICD-10 Code	**Description**
F11.10	Opioid abuse, uncomplicated
F11.120	Opioid abuse with intoxication, uncomplicated
F11.121	Opioid abuse with intoxication, delirium
F11.122	Opioid abuse with intoxication, with perceptual disturbance
F11.129	Opioid abuse with intoxication, unspecified
F11.14	Opioid abuse with opioid-induced mood disorder
F11.150	Opioid abuse with opioid-induced psychotic disorder, with delusions
F11.151	Opioid abuse with opioid-induced psychotic disorder, with hallucinations
F11.159	Opioid abuse with opioid-induced psychotic disorder, unspecified
F11.181	Opioid abuse with opioid-induced sexual dysfunction
F11.182	Opioid abuse with opioid-induced sleep disorder
F11.188	Opioid abuse with other opioid-induced disorder

F11.19	Opioid abuse with unspecified opioid-induced disorder
F11.20	Opioid dependence, uncomplicated
F11.220	Opioid dependence with intoxication, uncomplicated
F11.221	Opioid dependence with intoxication, delirium
F11.222	Opioid dependence with intoxication, with perceptual disturbance
F11.229	Opioid dependence with intoxication, unspecified
F11.23	Opioid dependence with withdrawal
F11.24	Opioid dependence with opioid-induced mood disorder
F11.250	Opioid dependence with opioid-induced psychotic disorder, with delusions
F11.251	Opioid dependence with opioid-induced psychotic disorder, with hallucinations
F11.259	Opioid dependence with opioid-induced psychotic disorder, unspecified
F11.281	Opioid dependence with opioid-induced sexual dysfunction
F11.282	Opioid dependence with opioid-induced sleep disorder
F11.288	Opioid dependence with other opioid-induced disorder
F11.29	Opioid dependence with unspecified opioid-induced disorder
F11.90	Opioid use, unspecified, uncomplicated
F11.920	Opioid use, unspecified with intoxication, uncomplicated
F11.921	Opioid use, unspecified with intoxication delirium
F11.922	Opioid use, unspecified with intoxication, with perceptual disturbance
F11.929	Opioid use, unspecified with intoxication, unspecified
F11.93	Opioid use, unspecified, with withdrawal
F11.94	Opioid use, unspecified, with opioid-induced mood disorder
F11.950	Opioid use, unspecified with opioid-induced psychotic disorder, with delusions
F11.951	Opioid use, unspecified with opioid-induced psychotic disorder, with hallucinations
F11.959	Opioid use, unspecified with opioid-induced psychotic disorder, unspecified
F11.981	Opioid use, unspecified with opioid-induced sexual dysfunction
F11.982	Opioid use, unspecified with opioid-induced sleep disorder
F11.988	Opioid use, unspecified with other opioid-induced disorder
F11.99	Opioid use, unspecified, with unspecified opioid-induced disorder

Appendix Table 6: Codes used to identify Medications for Opioid Use Disorders (MOUD)

	Code Type	Code
Buprenorphine	**NDC**	63481016101, 63481068501, 50383092493, 49999063830, 63481016160, 63481068560, 00093537956, 50383093093, 49999063930, 63481020701, 63481082001, 00228315303, 55700030230, 63874117303, 63481020760, 63481082060, 00228315603, 55700030330, 63481034801, 63481095201, 68308020230, 63481034860, 63481095260, 68308020830, 63481051901, 00054017613, 35356055530, 12496127802, 63481051960, 00054017713, 35356055630, 12496131002
	HCPCS	J0571, J0572, J0573, J0574, J0575
Naltrexone	**NDC**	00406009201, 16729008101, 68084029121, 51224020650, 00406117003, 00555090201, 51285027502, 00406009203, 16729008110, 52152010502, 68094085362, 00185003901, 42291063230, 52152010504, 68115068030, 00185003930, 43063059115, 52152010530, 00056001122, 00406117001, 47335032683, 54868557400, 00056001130, 47335032688, 65694010003, 00056001170, 50436010501, 65694010010, 00056007950, 00555090202, 51224020630, 68084029111, 51285027501
Naltrexone, extended-release injectable	**NDC**	63459030042, 65757030001, 65757030202

Appendix Table 7: Event Study Estimates from Two-Way Fixed Effects Estimator

t	Likelihood of Receiving Buprenorphine			No. Of Buprenorphine Claims			No. of Hospitalizations			No. of ED Visits		
	Coef.	95% CI		Coef.	95% CI		Coef.	95% CI		Coef.	95% CI	
-14	0.057	-0.028	0.142	0.112	-0.370	0.594	-0.024	-0.051	0.003	0.033	-0.013	0.078
-13	0.052	-0.028	0.132	0.088	-0.367	0.543	-0.034	-0.066	-0.003	0.026	-0.017	0.070
-12	0.012	-0.075	0.100	-0.021	-0.475	0.433	-0.025	-0.056	0.006	0.047	-0.002	0.097
-11	0.003	-0.074	0.080	-0.032	-0.451	0.386	-0.032	-0.064	0.000	0.039	-0.010	0.087
-10	0.004	-0.059	0.067	-0.029	-0.377	0.320	-0.025	-0.055	0.004	0.032	-0.013	0.078
-9	0.023	-0.036	0.083	0.049	-0.271	0.369	-0.016	-0.044	0.012	0.029	-0.007	0.064
-8	0.015	-0.041	0.072	0.023	-0.261	0.307	-0.015	-0.040	0.009	0.023	-0.012	0.059
-7	0.026	-0.031	0.083	0.046	-0.226	0.317	-0.011	-0.034	0.011	0.017	-0.015	0.048
-6	0.027	-0.024	0.078	0.074	-0.171	0.319	-0.015	-0.033	0.004	0.010	-0.019	0.039
-5	0.011	-0.014	0.035	0.002	-0.125	0.129	-0.008	-0.023	0.007	0.007	-0.017	0.032
-4	0.000	-0.020	0.020	-0.006	-0.116	0.104	-0.002	-0.015	0.011	0.004	-0.017	0.026
-3	-0.007	-0.022	0.009	-0.040	-0.102	0.022	-0.009	-0.019	0.001	0.003	-0.011	0.018
-2	-0.006	-0.018	0.006	-0.048	-0.102	0.006	-0.001	-0.009	0.008	0.007	-0.003	0.016
0	-0.001	-0.010	0.009	0.003	-0.047	0.053	-0.012	-0.029	0.005	0.000	-0.006	0.006
1	0.003	-0.010	0.015	0.019	-0.043	0.081	-0.011	-0.029	0.008	-0.003	-0.012	0.005
2	0.014	-0.013	0.041	0.065	-0.050	0.180	-0.008	-0.029	0.014	-0.007	-0.021	0.008
3	0.019	-0.014	0.052	0.106	-0.038	0.251	-0.016	-0.046	0.013	-0.008	-0.023	0.006
4	0.020	-0.016	0.055	0.087	-0.083	0.258	-0.014	-0.047	0.019	-0.008	-0.028	0.011
5	0.025	-0.015	0.065	0.138	-0.058	0.335	-0.017	-0.057	0.023	-0.007	-0.031	0.016
6	0.033	-0.012	0.079	0.237	-0.038	0.511	-0.021	-0.068	0.025	-0.012	-0.044	0.020
7	0.036	-0.011	0.083	0.179	-0.082	0.441	-0.028	-0.082	0.025	-0.014	-0.044	0.016
8	0.016	-0.047	0.079	0.150	-0.218	0.518	-0.032	-0.100	0.036	-0.012	-0.042	0.019
9	0.025	-0.046	0.096	0.147	-0.239	0.532	-0.030	-0.092	0.032	-0.020	-0.065	0.025
10	0.035	-0.025	0.095	0.264	-0.092	0.621	-0.039	-0.101	0.024	-0.081	-0.167	0.005
11	0.014	-0.059	0.087	0.086	-0.263	0.435	-0.059	-0.105	-0.013	-0.006	-0.057	0.045
12	0.012	-0.065	0.089	0.070	-0.310	0.449	-0.083	-0.143	-0.022	-0.009	-0.059	0.040
13	0.017	-0.060	0.094	0.092	-0.293	0.477	-0.065	-0.123	-0.008	-0.007	-0.052	0.039
14	0.016	-0.060	0.093	0.069	-0.326	0.463	-0.065	-0.127	-0.003	-0.012	-0.064	0.039
15	0.000	-0.071	0.072	-0.024	-0.433	0.386	-0.086	-0.158	-0.014	-0.011	-0.064	0.043
16	-0.050	-0.140	0.041	-0.235	-0.723	0.252	-0.102	-0.186	-0.018	-0.012	-0.068	0.044
17	-0.059	-0.151	0.033	-0.308	-0.827	0.211	-0.101	-0.191	-0.011	-0.001	-0.061	0.058
18	-0.080	-0.167	0.007	-0.310	-0.821	0.202	-0.104	-0.172	-0.037	-0.003	-0.064	0.058

Appendix Table 8: Event Study Estimates using Chaisemartin and D'Haultfoeuille (2020) Estimator

t	Likelihood of Receiving Buprenorphine			No. Of Buprenorphine Claims			No. of Hospitalizations			No. of ED Visits		
	Coef.	95% CI		Coef.	95% CI		Coef.	95% CI		Coef.	95% CI	
-9	-0.001	-0.087	0.085	0.022	-0.461	0.504	-0.020	-0.054	0.014	0.014	-0.047	0.074
-8	0.030	-0.046	0.105	0.127	-0.278	0.533	-0.006	-0.024	0.012	0.030	-0.013	0.073
-7	0.021	-0.033	0.075	0.070	-0.201	0.342	-0.004	-0.017	0.010	0.028	-0.015	0.071
-6	0.023	-0.030	0.075	0.042	-0.187	0.272	-0.004	-0.012	0.004	0.021	-0.024	0.066
-5	0.019	-0.026	0.065	0.039	-0.162	0.239	-0.008	-0.015	0.000	0.015	-0.029	0.059
-4	0.008	-0.024	0.041	-0.019	-0.162	0.125	0.000	-0.012	0.011	0.011	-0.022	0.043
-3	-0.008	-0.023	0.008	-0.034	-0.130	0.062	0.003	-0.005	0.011	0.001	-0.016	0.019
-2	-0.017	-0.032	-0.003	-0.071	-0.131	-0.010	-0.006	-0.014	0.003	0.000	-0.010	0.009
0	-0.004	-0.011	0.002	-0.035	-0.073	0.002	0.005	-0.001	0.011	0.004	-0.005	0.013
1	0.002	-0.005	0.009	0.022	-0.016	0.059	-0.009	-0.026	0.007	0.002	-0.004	0.007
2	0.005	-0.002	0.012	0.040	-0.004	0.085	-0.008	-0.026	0.010	-0.003	-0.012	0.006
3	0.017	0.007	0.027	0.083	0.007	0.159	-0.004	-0.022	0.014	-0.003	-0.012	0.007
4	0.028	0.008	0.048	0.156	0.066	0.246	-0.011	-0.031	0.009	-0.003	-0.011	0.006
5	0.029	0.008	0.050	0.144	0.028	0.261	-0.012	-0.035	0.012	-0.003	-0.015	0.008
6	0.037	0.021	0.054	0.203	0.097	0.308	-0.014	-0.042	0.014	0.000	-0.012	0.013
7	0.051	0.029	0.073	0.342	0.149	0.535	-0.017	-0.048	0.014	-0.003	-0.022	0.015
8	0.045	0.024	0.066	0.232	0.086	0.378	-0.028	-0.060	0.005	-0.009	-0.029	0.011
9	0.024	-0.004	0.052	0.197	-0.001	0.395	-0.033	-0.078	0.012	-0.007	-0.022	0.007
10	0.049	0.022	0.076	0.268	0.118	0.417	-0.025	-0.036	-0.015	-0.011	-0.024	0.001
11	0.038	0.008	0.067	0.252	0.087	0.417	-0.036	-0.044	-0.028	-0.070	-0.087	-0.054
12	0.043	-0.012	0.098	0.134	-0.136	0.404	-0.088	-0.102	-0.074	0.019	-0.015	0.053
13	0.039	-0.020	0.098	0.107	-0.208	0.422	-0.111	-0.128	-0.094	0.014	-0.020	0.047
14	0.040	-0.032	0.111	0.112	-0.271	0.495	-0.085	-0.103	-0.066	0.021	-0.016	0.057
15	0.063	-0.014	0.140	0.190	-0.216	0.596	-0.088	-0.103	-0.074	0.015	-0.030	0.060
16	0.047	-0.033	0.126	0.104	-0.331	0.539	-0.119	-0.135	-0.102	0.013	-0.033	0.059
17	0.016	-0.078	0.111	-0.023	-0.577	0.531	-0.131	-0.148	-0.115	0.010	-0.033	0.053
18	0.002	-0.097	0.101	-0.085	-0.648	0.478	-0.127	-0.146	-0.109	0.015	-0.025	0.055

Appendix Table 9: Event Study Estimates using Callaway and Sant'Anna (2021) Estimator

t	Likelihood of Receiving Buprenorphine			No. Of Buprenorphine Claims			No. of Hospitalizations			No. of ED Visits		
	Coef.	95% CI		Coef.	95% CI		Coef.	95% CI		Coef.	95% CI	
-12	-0.021	-0.088	0.046	-0.167	-0.453	0.120	-0.013	-0.043	0.017	0.035	0.006	0.064
-11	-0.016	-0.074	0.042	-0.094	-0.390	0.202	-0.034	-0.056	-0.012	0.032	-0.004	0.069
-10	-0.021	-0.063	0.020	-0.122	-0.351	0.108	-0.026	-0.053	0.001	0.025	-0.011	0.061
-9	0.003	-0.037	0.043	-0.013	-0.216	0.189	-0.003	-0.029	0.023	0.019	-0.005	0.042
-8	0.004	-0.035	0.043	-0.010	-0.219	0.200	-0.010	-0.031	0.011	0.016	-0.008	0.040
-7	0.009	-0.037	0.056	-0.016	-0.227	0.196	-0.017	-0.045	0.011	0.009	-0.012	0.030
-6	0.010	-0.033	0.053	0.003	-0.195	0.200	-0.013	-0.035	0.008	0.006	-0.015	0.026
-5	0.000	-0.024	0.024	-0.045	-0.153	0.064	-0.007	-0.028	0.014	0.003	-0.019	0.026
-4	-0.009	-0.024	0.007	-0.042	-0.134	0.051	-0.006	-0.023	0.011	0.000	-0.017	0.017
-3	-0.015	-0.025	-0.004	-0.078	-0.125	-0.030	-0.013	-0.021	-0.005	-0.001	-0.010	0.008
-2	-0.004	-0.011	0.002	-0.044	-0.083	-0.005	0.004	-0.005	0.014	0.000	-0.010	0.011
0	0.004	-0.001	0.010	0.030	-0.008	0.067	-0.008	-0.023	0.007	0.004	-0.001	0.009
1	0.009	0.001	0.018	0.060	0.012	0.108	-0.005	-0.021	0.012	-0.005	-0.014	0.004
2	0.019	0.001	0.037	0.100	0.016	0.183	-0.002	-0.022	0.019	-0.006	-0.020	0.009
3	0.026	0.001	0.050	0.156	0.045	0.266	-0.003	-0.036	0.030	-0.003	-0.016	0.010
4	0.027	0.000	0.053	0.143	0.011	0.275	-0.005	-0.038	0.027	-0.009	-0.027	0.010
5	0.037	0.008	0.065	0.209	0.087	0.331	-0.008	-0.043	0.026	-0.005	-0.025	0.014
6	0.049	0.018	0.080	0.349	0.136	0.561	-0.010	-0.054	0.035	-0.008	-0.033	0.017
7	0.056	0.024	0.088	0.301	0.124	0.478	-0.017	-0.061	0.027	-0.014	-0.036	0.008
8	0.041	-0.022	0.105	0.290	-0.072	0.651	-0.022	-0.084	0.041	-0.012	-0.031	0.008
9	0.068	0.003	0.133	0.377	0.000	0.754	-0.013	-0.078	0.051	-0.025	-0.063	0.013
10	0.085	0.034	0.135	0.487	0.191	0.783	-0.026	-0.101	0.050	0.019	-0.023	0.062
11	0.060	-0.045	0.166	0.233	-0.134	0.600	-0.079	-0.134	-0.025	0.020	-0.020	0.059
12	0.055	-0.050	0.161	0.206	-0.163	0.574	-0.108	-0.180	-0.036	0.009	-0.015	0.033
13	0.052	-0.048	0.153	0.188	-0.169	0.544	-0.090	-0.160	-0.019	0.019	-0.007	0.045
14	0.054	-0.048	0.156	0.188	-0.152	0.527	-0.082	-0.145	-0.019	0.013	-0.033	0.058
15	0.041	-0.051	0.133	0.116	-0.206	0.438	-0.110	-0.185	-0.036	0.008	-0.034	0.049
16	0.010	-0.093	0.113	0.006	-0.389	0.401	-0.129	-0.211	-0.047	0.010	-0.035	0.055
17	0.005	-0.099	0.109	-0.023	-0.394	0.348	-0.123	-0.209	-0.038	0.015	-0.033	0.063
18	-0.054	-0.077	-0.031	-0.230	-0.377	-0.084	-0.147	-0.171	-0.123	0.019	-0.023	0.062

Appendix Table 10: Robustness of Estimates using Callaway and Sant'Anna (2021) Estimator

	Excluding AZ	Excluding WA	Excluding WA, AZ
	Effect of PA Repeal	**Effect of PA Repeal**	**Effect of PA Repeal**
Likelihood of Buprenorphine	**0.03*** **	**0.03*** **	**0.03*** **
Number of Buprenorphine Claims	**0.19*** **	**0.18*** **	**0.19*** **
Number of Hospitalizations	**-0.02*** **	**-0.02*** **	**-0.02*** **
Number of ED Visits	-0.007	-0.006	-0.009
Fixed Effects			
State	Yes	Yes	Yes
Time (Quarter)	Yes	Yes	Yes
Observations	5,415,638	5,245,472	4,534,255

***p<0.1; **p<0.05; ***p<0.01**

Appendix Table 11: Robustness of Estimates excluding states with Data Quality Issues in TAF using Callaway and Sant'Anna (2021) Estimator

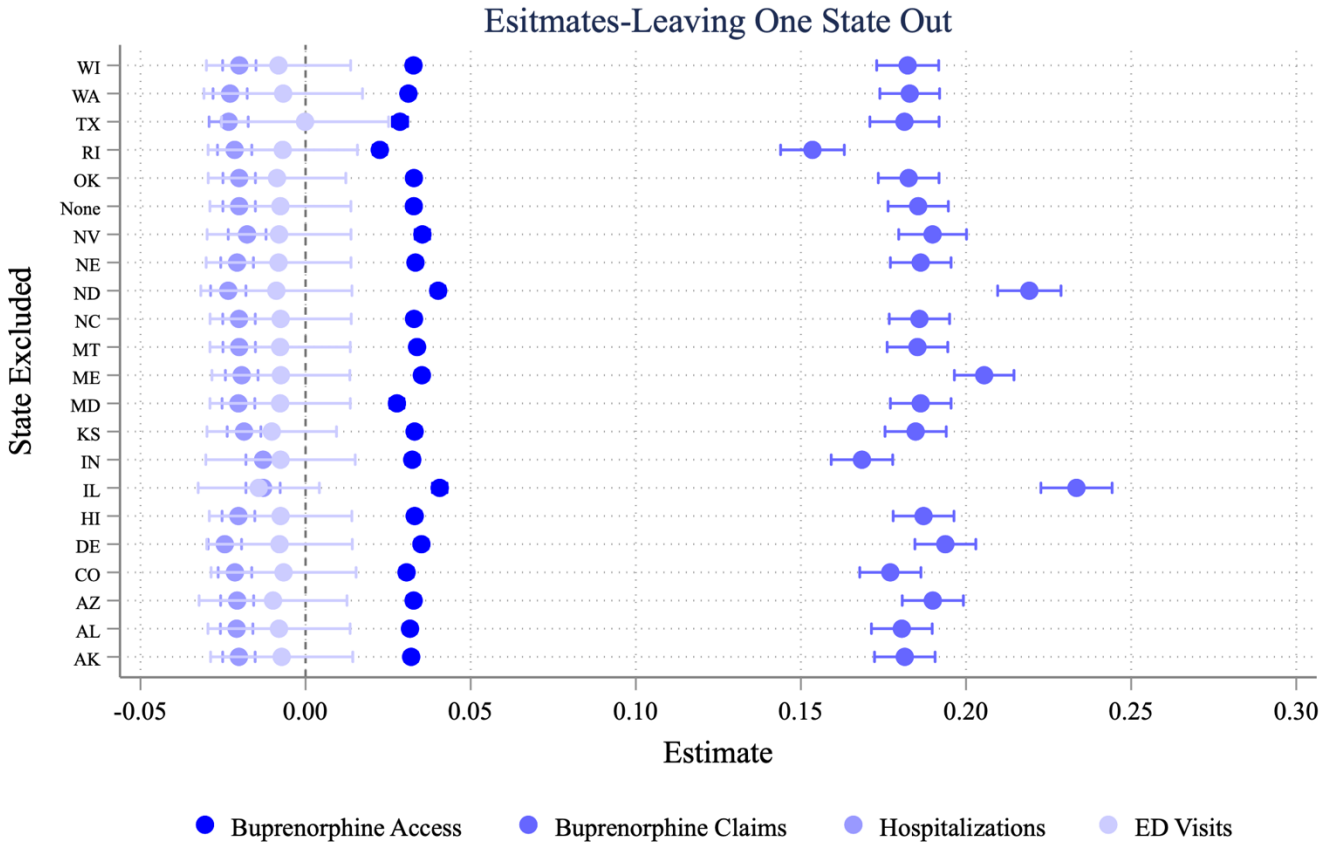
	Excluding AL	Excluding NC	Excluding ME	Excluding RI
	Effect of Policy Change	Effect of Policy Change	Effect of Policy Change	Effect of Policy Change
Likelihood of Buprenorphine	**0.03*** **	**0.04*** **	**0.04*** **	**0.02*** **
Number of Buprenorphine Claims	**0.19** **	**0.22** **	**0.21*** **	**0.15*** **
Number of Hospitalizations	**-0.02*** **	**-0.02*** **	**-0.02*** **	**-0.02*** **
Number of ED Visits	-0.008	-0.008	-0.007	-0.007
Fixed Effects				
State	Yes	Yes	Yes	Yes
Time (Quarter)	Yes	Yes	Yes	Yes
Observations	5,822,669	5,482,838	5,787,610	5,814,312

***p<0.1; **p<0.05; ***p<0.01**

Appendix Table 12: Robustness of Estimates to Leaving One State Out using Callaway and Sant'Anna (2021) Estimator

State Excl.	Likelihood of Receiving Buprenorphine			No. Of Buprenorphine Claims			No. of Hospitalizations			No. of ED Visits		
	Coef.	95% CI		Coef.	95% CI		Coef.	95% CI		Coef.	95% CI	
None	**0.033**	**0.031**	**0.035**	**0.186**	**0.176**	**0.195**	**-0.020**	**-0.025**	**-0.015**	**-0.008**	**-0.029**	**0.014**
AK	0.032	0.030	0.034	0.181	0.172	0.191	-0.020	-0.025	-0.015	-0.007	-0.029	0.014
AL	0.032	0.030	0.034	0.181	0.171	0.190	-0.021	-0.026	-0.016	-0.008	-0.030	0.013
AZ	0.033	0.031	0.035	0.190	0.181	0.199	-0.021	-0.026	-0.016	-0.010	-0.032	0.013
CO	0.031	0.029	0.033	0.177	0.168	0.186	-0.021	-0.026	-0.016	-0.007	-0.029	0.015
DE	0.035	0.033	0.037	0.194	0.185	0.203	-0.024	-0.030	-0.019	-0.008	-0.030	0.014
HI	0.033	0.031	0.035	0.187	0.178	0.196	-0.020	-0.025	-0.015	-0.008	-0.029	0.014
IL	0.041	0.038	0.043	0.233	0.223	0.244	-0.013	-0.018	-0.008	-0.014	-0.033	0.004
IN	0.032	0.030	0.034	0.168	0.159	0.178	-0.013	-0.018	-0.008	-0.008	-0.030	0.015
KS	0.033	0.031	0.035	0.185	0.175	0.194	-0.019	-0.024	-0.014	-0.010	-0.030	0.009
ME	0.035	0.033	0.037	0.205	0.196	0.215	-0.019	-0.024	-0.014	-0.007	-0.028	0.013
MD	0.028	0.026	0.030	0.186	0.177	0.195	-0.020	-0.025	-0.015	-0.008	-0.029	0.014
MT	0.034	0.032	0.036	0.185	0.176	0.194	-0.020	-0.025	-0.015	-0.008	-0.029	0.014
NC	0.033	0.031	0.035	0.186	0.177	0.195	-0.020	-0.025	-0.015	-0.008	-0.029	0.014
ND	0.040	0.038	0.042	0.219	0.210	0.229	-0.023	-0.029	-0.018	-0.009	-0.032	0.014
NE	0.033	0.031	0.035	0.186	0.177	0.195	-0.021	-0.026	-0.016	-0.008	-0.030	0.014
NV	0.035	0.033	0.038	0.190	0.180	0.200	-0.018	-0.023	-0.012	-0.008	-0.030	0.014
OK	0.033	0.031	0.035	0.183	0.173	0.192	-0.020	-0.025	-0.015	-0.009	-0.029	0.012
RI	0.022	0.020	0.025	0.153	0.144	0.163	-0.021	-0.027	-0.016	-0.007	-0.030	0.016
TX	0.029	0.026	0.031	0.181	0.171	0.192	-0.023	-0.029	-0.017	0.000	-0.025	0.025
WA	0.031	0.029	0.033	0.183	0.174	0.192	-0.023	-0.028	-0.018	-0.007	-0.031	0.017
WI	0.033	0.031	0.035	0.182	0.173	0.192	-0.020	-0.025	-0.015	-0.008	-0.030	0.014

Appendix Figure 1: Robustness of Estimates excluding states with Data Quality Issues in TAF using Callaway and Sant'Anna (2021) Estimator



Appendix Table 13: Robustness of Estimates to alternate Model Specifications

	Logistic Regression	Poisson Regression
	Effect of PA Repeal	**Effect of PA Repeal**
Likelihood of Buprenorphine	0.013	-
Number of Buprenorphine Claims	-	0.094
Number of Hospitalizations	-	-0.005
Number of ED Visits	-	-0.07
Fixed Effects		
State	Yes	Yes
Time (Quarter)	Yes	Yes
N	737,379	737,379
N-Y	6,281,448	6,281,448

Note: In the first column coefficients are marginal effects from a logistic regression model estimated controlling for age, sex insurance type, overdose deaths per 1000 and state and period fixed effects**.** In the second column coefficients are log incident rate ratios from a negative binomial regression model estimated controlling for age, sex insurance type, overdose deaths per 1000 and state and time period (quarter) fixed effects.