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BEYOND OVERDOSE PREVENTION: NALOXONE COPRESCRIPTION LAWS AND THE PATH TO TREATMENT

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ABSTRACT

Over one-third of U.S. states have enacted naloxone coprescription laws, requiring physicians to prescribe or offer naloxone alongside high-risk opioids. Using Medicare Part D data, the Treatment Episode Data Set, and a difference-in-differences design, we study how these laws affect naloxone access, broader prescription behavior, and treatment engagement. Coprescription laws increased naloxone prescribing by 275%, with effects twice as large in states requiring prescriptions as in those requiring only offers. We find no evidence of increased opioid or benzodiazepine prescribing, countering moral hazard concerns that broader naloxone access might encourage riskier opioid use. Instead, we document significant increases in prescribing of medications for opioid use disorder (MOUD) and gains in opioid use disorder treatment admissions, primarily in deliberate treatment-seeking settings. These results suggest that coprescription laws may have broader benefits beyond overdose prevention.

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1. **Introduction**

The opioid epidemic has claimed over 600,000 lives in the United States since 1999, with overdose deaths quadrupling from 8.2 per 100,000 population in 2002 to 32.6 in 2022 (Spencer et al., 2024). A cornerstone of the policy response has been expanding access to naloxone, an opioid antagonist that rapidly reverses the effects of an opioid overdose (Smart et al., 2021). Early naloxone access laws, widely adopted in the early 2010s (Davis & Carr, 2015), focused on removing legal barriers through immunity provisions for first responders (Smart et al., 2022), community organizations (Clark et al., 2014), and pharmacies distributing naloxone to laypeople (Smart et al., 2024; Xu & Mukherjee, 2021). Yet concerns persisted about whether these measures adequately reached individuals at the highest overdose risk—namely, those actively prescribed opioids.

Between 2017 and 2023, eighteen states shifted focus upstream to prescribers, adopting naloxone coprescription laws that mandate physicians prescribe or offer naloxone when prescribing high-dose opioids or opioids combined with benzodiazepines. Rather than targeting patients or distributors, these laws integrate overdose prevention directly into clinical encounters. Despite rapid adoption, rigorous causal evidence on their effectiveness and broader implications remains limited. Using Medicare Part D data and a staggered difference-in-differences design, we examine three central questions. First, do coprescription laws produce sustained effects on naloxone prescribing beyond the immediate post-implementation period, and do effects differ by statutory provisions? Second, do these laws encourage riskier opioid prescribing through moral hazard—the concern that naloxone availability may lead patients to accept greater opioid risk, resulting in increased or higher-dose opioid prescriptions—thereby undermining the very goal of overdose prevention (Cawley & Dragone, 2024; Doleac & Mukherjee, 2022)? Third, do mandated clinical conversations about overdose risk lower barriers to treatment engagement, leading to increased prescribing of medications for opioid use disorder (MOUD)?

We provide causal evidence on these questions by evaluating the impact of naloxone coprescription laws on prescription patterns across naloxone, opioids, and MOUD. Leveraging the CMS Medicare Part D Prescribers dataset from 2013 to 2023, we use a staggered difference-in-differences (DID) design with the Callaway and Sant'Anna (2021) doubly robust DID estimator to compare prescribing trends in states enacting coprescription laws before and after policy adoption to trends in states without such laws over the same period. To examine treatment

engagement, we complement this analysis with the Treatment Episode Data Set-Admissions (TEDS-A) from 2013 to 2023, using the same empirical framework.

Our results show that coprescription laws nearly quadrupled naloxone prescriptions, with effects roughly twice as large under prescribing mandates—which require coprescription—as under offer mandates, which require only discussion. Importantly, these laws have no meaningful impact on opioid or benzodiazepine prescribing, alleviating moral hazard concerns. Beyond naloxone access, we also document large positive spillovers into evidence-based treatment: MOUD prescriptions increase by 56% and opioid use disorder (OUD) treatment admissions increase by 297%, with effects concentrated in deliberate treatment-seeking settings such as outpatient and residential care rather than emergency interventions like detoxification. This pattern suggests that coprescription laws function not merely as overdose prevention tools but as treatment facilitation policies, with mandated clinical conversations about overdose risk creating touchpoints that raise awareness and motivate treatment-seeking.

This paper makes three key contributions to the literature on harm reduction policy. First, we provide causal evidence on how coprescription laws affect prescriber behavior and downstream treatment engagement. Prior research has focused primarily on naloxone dispensing using pharmacy data (Green et al., 2020; Sohn et al., 2019) or commercial insurance claims (Dong et al., 2024; Tormohlen et al., 2024), examining short-term effects in limited samples of early-adopting states without investigating spillovers into related drug prescribing or formal treatment. We extend this work using nationwide Medicare Part D data covering all eighteen states that adopted coprescription laws through 2023, with follow-up periods extending five years post-adoption, and complement this analysis with the Treatment Episode Data Set-Admissions (TEDS-A) to examine—for the first time—whether these laws affect MOUD prescribing and opioid use disorder treatment admissions.

Second, we contribute to ongoing debates about the unintended consequences of harm reduction policies. A prominent concern in the literature is that expanding naloxone access may reduce the perceived consequences of risky opioid use, encouraging patients to use opioids more recklessly—a moral hazard mechanism that could undermine overdose prevention goals (Doleac & Mukherjee, 2022). Evidence on whether these concerns apply to prescriber-focused mandates

specifically is lacking. Our findings speak directly to this debate, with implications for how policymakers design and evaluate harm reduction interventions.

Third, we demonstrate that policy design substantially affects effectiveness. By separately analyzing prescribing mandates and offer mandates, we contribute to broader evidence on the power of defaults in shaping clinical behavior—a dimension of coprescription law design that prior work has not systematically examined.

2. Background

2.1 Naloxone Coprescription Laws: Pharmacology, Risk Triggers, and Policy Design

Naloxone coprescription laws are state-level mandates requiring healthcare providers to prescribe or offer naloxone to patients at elevated risk of opioid overdose (Lieberman & Davis, 2024; Smart et al., 2021). Naloxone reverses opioid-induced respiratory depression by binding to opioid receptors in the brain and displacing opioids from these sites, restoring normal breathing within minutes and averting fatal overdose—making it a central tool in harm reduction (Dowell et al., 2016; Green et al., 2008). However, because naloxone has a short half-life relative to many opioids, multiple doses may be required in cases involving potent or long-acting opioid overdoses (Clark et al., 2014; Smart et al., 2021).

These mandates are generally triggered when patients are prescribed opioids over certain dosages,[1] and in most cases, when opioids are prescribed concurrently with benzodiazepines (benzos), sedatives that also depress the central nervous system. Taken together, the two can suppress breathing and sharply raise the risk of fatal overdose relative to opioids alone (Sun et al., 2017). Yet opioids and benzodiazepines are frequently coprescribed for patients with chronic pain and coexisting anxiety, muscle spasms, or insomnia, as well as in palliative care, postoperative pain management, and psychiatric treatment, as physicians work to address multiple symptoms at

[1] Provisions of coprescription laws vary by state. While many states require naloxone to be prescribed or offered when opioid prescriptions exceed a specific number of morphine milligram equivalents (MME) per day, this is not universally the case. For example, Florida mandates naloxone prescriptions when a Schedule II opioid is prescribed for pain related to a traumatic injury with an Injury Severity Score of nine or higher. In contrast, New Mexico requires naloxone to be prescribed when an opioid is prescribed for five days or more. Among states with MME-based requirements, the threshold ranges from 50 MME (as in Arkansas, Rhode Island, and South Carolina) to 120 MME (as in Virginia).

once. Despite this elevated risk, naloxone coprescribing to high-risk opioid recipients remained rare before these mandates (Jones et al., 2019).

By integrating naloxone access directly into the prescribing encounter, coprescription laws represent a targeted harm reduction strategy that does not restrict access to clinically necessary care (Dowell et al., 2016; Smart et al., 2021). These laws vary in their stringency: some states mandate that physicians actually prescribe naloxone alongside high-risk opioids, while others require only that physicians discuss or offer it. As we discuss below, this distinction in policy design has meaningful implications for effectiveness.

2.2 Variations in Mandate Design

The first naloxone coprescription law became effective in March 2017 when Virginia mandated prescribers to coprescribe naloxone for any patient with risk factors, including prior overdose, substance misuse, or opioid prescriptions exceeding 120 MME per day or concurrent benzodiazepine use. As of December 31, 2023, eighteen states, including Virginia, had such laws in effect.

While all these laws aim to expand naloxone access in the presence of opioid overdose risk factors, they differ in their requirements. Eleven states (Arizona, Arkansas, Florida, Indiana, New Jersey, New Mexico, New York, Rhode Island, Vermont, Virginia, Washington) mandate that providers prescribe naloxone when specified risk factors are present. Seven states (California, Colorado, Illinois, Kentucky, Ohio, South Carolina, Tennessee) require providers to offer or discuss naloxone but allow patient refusal. Prescribing mandates impose stricter legal obligations on providers and, as we show below, produce meaningfully larger effects on naloxone access than offer mandates.

Enforcement mechanisms and associated penalties for noncompliance with these laws also vary across states. Most states do not specify penalties, leaving these laws to function more as clinical guidelines than binding obligations. Four states have implemented explicit penalties, though enforcement is primarily left to the discretion of the licensing board (Lieberman & Davis, 2024). Despite this variation, early evidence from Virginia and Vermont suggests high physician compliance and significant increases in dispensed naloxone prescriptions following adoption (Sohn et al., 2019).

2.3 Theoretical Mechanisms and Empirical Evidence

Naloxone coprescription laws may affect physician prescribing through two distinct channels with opposing implications. The first is a *treatment facilitation* channel: by integrating overdose prevention into clinical encounters, these laws may lower barriers to treatment initiation and increase MOUD prescribing. The second is a *moral hazard* channel: by providing a safety net against overdose fatality, these laws may inadvertently reduce the perceived consequences of risky opioid use, encouraging more liberal opioid prescribing. Whether the net effect is beneficial depends on which channel dominates—an empirical question we address directly.

Treatment Facilitation. Although not immediately obvious, naloxone coprescription laws may significantly increase prescribing of medications for opioid use disorder (MOUD). The two MOUD we examine are buprenorphine and naltrexone (see Section 3.2 for our classification of methadone). Buprenorphine is a partial opioid agonist that binds to opioid receptors and partially activates them to reduce cravings and withdrawal symptoms. Naltrexone is an opioid antagonist that functions similarly to naloxone by blocking opioid receptors and is used for long-term relapse prevention rather than overdose reversal (Volkow et al., 2018).

Naloxone accessibility may facilitate the transition to MOUD because the risk of overdose rises after detoxification, when opioid tolerance has declined (Strang et al., 2003). Having naloxone available provides a safety net, encouraging safer detoxification and MOUD initiation. Additionally, coprescription laws increase healthcare interactions, giving providers more opportunities to discuss MOUD options and reframe opioid cessation as a manageable process (Dowell et al., 2016; Schnell & Currie, 2018; Wen et al., 2018). By reducing anxiety and reinforcing harm reduction, naloxone availability may encourage greater treatment engagement and improve opioid use disorder outcomes.

Moral Hazard. Despite the public health rationale for expanding naloxone access, critics have raised concerns about moral hazard or risk compensation: that reducing the perceived consequences of risky behavior might inadvertently encourage that behavior (Cawley & Dragone, 2024; Doleac & Mukherjee, 2022; Winograd et al., 2020). In the context of coprescription laws, moral hazard may operate through both patients and physicians. Patients with naloxone access may request stronger or higher-dose opioids, believing overdose reversal is available. Physicians, reassured that naloxone reduces overdose risk, may feel more comfortable prescribing higher

opioid doses or coprescribing opioids with benzodiazepines—either in response to patient demand or as an independent clinical judgment. While such prescribing decisions may address legitimate clinical needs, they could inadvertently increase aggregate overdose risk if the expanded safety net encourages prescribing patterns that would otherwise be avoided, as greater availability of prescription opioids has increased overdose deaths and diversion to nonmedical use (Alpert et al., 2022; Janssen & Zhang, 2023; Powell et al., 2020).

The empirical evidence on these behavioral responses remains mixed. Tse et al. (2022) conducted a systematic review of take-home naloxone programs and found no evidence that broader naloxone access increased opioid use or overdose rates. In contrast, Doleac and Mukherjee (2022) identified associations between take-home naloxone programs and higher opioid-related emergency room visits and crime rates, though they acknowledge difficulties in establishing causality.[2] Sohn et al. (2023) examined mortality effects of coprescription laws in ten states and found that the laws reduced prescription opioid overdose deaths by 16% but had no effect on illicit opioid overdose deaths, suggesting targeted benefits without broader behavioral spillovers. These conflicting findings underscore the importance of rigorous causal evidence on how coprescription laws affect not only naloxone access but also prescribing behavior and treatment engagement—questions we address directly in this study.

3. Data and Variables

We compile data from multiple sources spanning 2013-2023 to examine how naloxone coprescription laws affect physician prescribing behavior.

3.1 Naloxone Coprescription Laws

Our primary data source for legislation is the 2024 Fact Sheet – Naloxone Prescription Mandates released by the Network for Public Health Law, which provides detailed provisions, legal references, and effective dates for each state's coprescription laws (Lieberman & Davis, 2024). We validate this information by cross-referencing Green et al. (2020) and the RAND OPTIC-Vetted Policy Data Sets, supplemented by our original legal research. Details of the laws as of December 31, 2023, are presented in Table A1.

[2] Alexeev (2026) reassessed Doleac and Mukherjee's (2022) findings and found null effects on ER opioid admissions.

Figure 1 illustrates the temporal and geographic variation of law adoption. Panel A shows the adoption of naloxone coprescription laws by year and type. A sharp increase in adoptions occurred between 2017 and 2018, with early adopters primarily enacting "must prescribe" mandates (blue), a stronger regulatory stance on coprescription. The "must offer" mandates began expanding in 2018, and from 2021 onward, newly adopted laws are more likely to take this compromise approach, granting providers more discretion while still promoting naloxone accessibility. Panel B illustrates the geographic distribution of adopting states, which span all four Census regions.

3.2 Prescription Data

We draw our main outcome measures from the CMS Medicare Part D Prescribers by Geography and Drug database.[3] The dataset is structured at the state–year–drug level, where each observation represents a specific drug in a given state and year. For every covered Part D drug, the dataset reports the state identifier, drug brand and generic names,[4] the total number of dispensed prescriptions (total claims),[5] the number of standardized 30-day fills,[6] the number of unique prescribers, and the number of unique Medicare beneficiaries (total beneficiaries). To maintain consistency with existing literature, we normalize these prescription-related metrics by state annual Medicare enrollment (CMS, 2025a), so that all prescription outcomes are expressed per 100,000 Medicare beneficiaries (Smart et al., 2024; Sohn et al., 2019).[7] CMS suppresses Medicare Part D data when fewer than 11 beneficiaries receive a drug in a state-year. Although relatively rare, this suppression could meaningfully affect newly approved drugs such as Kloxxado and Zimhi. We show in Section 5.3 that our findings are robust to excluding these drugs.

[3] The CMS Medicare Part D Prescribers database is based on information gathered from CMS administrative claims data. It contains information on prescription drugs prescribed by individual physicians and other health care providers and paid for under the Medicare Part D Prescription Drug Program. For each drug, the dataset includes all prescriptions (both original prescriptions and refills) that were dispensed.

[4] CMS maps drugs to their brand and generic names based on their 10-digit National Drug Codes (NDCs; CMS, 2025b).

[5] In Medicare Part D data, the terms dispensed prescriptions and total claims both correspond to prescription drug event (PDE) records. Each PDE reflects a single adjudicated fill of a covered Part D drug at the pharmacy level. This measure differs from 30-day equivalent fills, where longer prescriptions are normalized to 30-day supply equivalents (CMS, 2025b).

[6] In Medicare Part D data, the standardized 30-day fill is derived from the number of days supplied on each Part D claim divided by 30. When the number of 30-day fills exceeds the number of claims, it indicates that some prescriptions cover more than a 30-day supply.

[7] Our results are consistent when normalized using the state total population to account for broader demographic shifts or when using the number of Medicare providers to account for supply-side dynamics.

We stratify the original dataset into three mutually exclusive subsamples of prescriptions, defined by generic drug names and verified against labeling information in Drugs@FDA (U.S. Food and Drug Administration [FDA], 2025): (1) naloxone, (2) opioids and benzodiazepines, and (3) MOUD (buprenorphine and naltrexone).[8] To identify opioids, we use the classification flag provided by CMS, excluding opioid-based MOUD from this category. Appendix Table B1 lists all brand-name drugs in each category.

One classification decision warrants clarification. Although methadone is a clinically important MOUD widely used in opioid treatment programs, it is not covered under Medicare Part D for OUD treatment—only methadone prescribed for pain is eligible under Part D formularies (Medicare.org, 2019). Any methadone appearing in our data therefore reflects pain management use rather than OUD treatment. We consequently classify methadone as an opioid rather than MOUD in our analyses, and would expect coprescription laws to influence methadone utilization similarly to other opioids prescribed for pain.

Medicare Part D data offer several advantages over the retail pharmacy data used in prior studies. First, Part D primarily covers older adults and individuals with disabilities—populations with higher chronic pain prevalence and opioid use, and therefore greater vulnerability to opioid-related harm (Powell et al., 2020). In 2022, 89.4% of U.S. adults aged 65 and older were enrolled in Medicare, with 80.4% having Part D coverage, providing near-universal representation of this high-risk group (CMS, 2025a). Moreover, nearly one in three adults in this age group reported experiencing chronic pain in the past three months—the highest prevalence across all age groups (Zelaya et al., 2020).[9] Second, Medicare Part D data include prescriber-linked information, allowing us to examine how naloxone coprescription mandates influence provider behavior—an essential mechanism determining naloxone access. Retail pharmacy data, by contrast, primarily reflect dispensing patterns and lack provider-level detail. Third, while both data sources may miss

[8] We classify MOUD according to FDA (2025) labeling. Sublingual, implant, and extended-release injectable buprenorphine products approved for opioid use disorder (e.g., Suboxone) are classified as MOUD, while formulations—Belbuca (buccal film), Butrans (transdermal patch), and Buprenex (injectable)—are approved only for pain management and are therefore classified as opioids. We also exclude three naltrexone-containing products—Contrave (bupropion/naltrexone), Embeda (morphine/naltrexone), and Relistor (methylnaltrexone)—as they are neither opioids nor approved MOUD.

[9] Zelaya et al. (2020) document that 30.8% of adults aged 65 and over reported experiencing chronic pain in the past three months—the highest among all age groups—compared to 25.8% for ages 45–64, 14.6% for ages 30–44, and 8.5% for ages 18–29.

cash-paid prescriptions or those covered by non-reporting insurers, Part D offers a more standardized view of prescription activity within a clearly defined population, reducing concerns about underreporting and improving the reliability of prescribing trend estimates.

Figure 2 shows naloxone prescription trends (claims per 100,000 beneficiaries) for treated and control states. Before 2017, both groups exhibit similar, relatively flat trends, supporting the parallel trends assumption required for difference-in-differences estimation. After 2017, naloxone prescriptions rise sharply in treated states relative to control states, providing strong visual evidence that coprescription laws increase naloxone prescribing. Control states also experience rising naloxone prescriptions, but at a slower rate, suggesting that other factors, such as broader naloxone access policies, CDC clinical guidelines, or increased provider awareness, may have contributed to rising prescriptions nationwide, though with less pronounced effects than coprescription mandates. Appendix Figure A1 further disaggregates prescribing trends by treatment cohort and shows sharp increases around each cohort's adoption year, suggesting that the divergence observed in Figure 2 is not driven solely by the earliest adopters.

3.3 Treatment Admissions Data

To examine treatment engagement effects, we supplement our prescription data with the Treatment Episode Data Set—Admissions (TEDS-A) for 2013–2023 (SAMHSA, 2026), a national administrative dataset that captures admissions to substance use treatment facilities receiving federal public funding and, in some states, additional facilities. We subset to admissions where the service type is opioid dependence or abuse and primary payer is Medicare, aggregate to admission counts at the state-year level, and normalize by state Medicare population (per 1,000 beneficiaries).

We also disaggregate admissions by four mutually exclusive service settings, each representing a distinct treatment pathway and ranging from most acute to least intensive. Detoxification provides acute, short-term medical care for managing intoxication and withdrawal symptoms, usually in response to an immediate medical crisis such as overdose or severe withdrawal. Rehabilitation/residential programs offer intensive 24-hour supervised treatment in a controlled setting, typically lasting 30–90 days. Intensive outpatient programs involve structured therapy sessions at least three times per week while patients continue living at home. Non-intensive outpatient programs are the least intensive option, requiring attendance fewer than three times weekly and sometimes as infrequently as biweekly one-hour sessions (McCarty et al., 2014). This

classification allows us to distinguish emergency medical responses from deliberate, patient-initiated treatment engagement—a distinction central to interpreting our results in Section 6.2.

3.4 Other State Variables

To account for state-level differences, we incorporate covariates from multiple sources. Relevant policy variables include pharmacy-based naloxone access laws (PA-NAL; Smart et al., 2024), prescription drug monitoring programs (PDMP; Alpert et al., 2024; Bao et al., 2018; Buchmueller & Carey, 2018), and Good Samaritan laws (GSL; Rees et al., 2019), all from the RAND-USC Schaeffer OPTIC Policy Database. We also control for state demographic characteristics that may influence prescribing patterns, including state age structure (population shares aged 20–34, 35–49, 50–64, and 65+), the share of the population that is White, and the share that is male, using data from the Surveillance, Epidemiology, and End Results Program (SEER, 2025). Finally, we control for state-level Part D characteristics: the share of the population enrolled in Part D and the weighted average monthly Part D premium, sourced from KFF Medicare Fact Sheets (KFF, 2025).

3.5 Summary Statistics

Table 1 presents summary statistics for our dependent variables by drug category: naloxone (columns 1–3), opioids and benzodiazepines (columns 4–6), and MOUD (columns 7–9). Within each category, we compare control states with treated states in both pre- and post-policy periods, with all outcomes expressed per 100,000 Medicare beneficiaries.

Treated states experienced substantial increases in naloxone prescribing following coprescription mandates. In the post-policy period, mean naloxone claims rose to 510.96 per 100,000 beneficiaries—more than sixfold relative to the pre-policy mean of 83.68—while 30-day fills increased similarly from 84.68 to 518.68. The number of unique prescribers grew from 16.62 to 111.83 and the number of beneficiaries receiving naloxone increased from 78.09 to 455.61, representing approximately a 5.8-fold increase. Control states never adopt a law, so Table 1 reports their means over the full sample period. However, control state prescribing—with mean claims of 204.43 and beneficiaries of 185.10—falls below the post-policy means of treated states, indicating that controls did not experience similar increases.

Opioids and benzodiazepines are prescribed far more frequently than naloxone in both periods, with average prescription measures broadly similar between control and treated states. Within

treated states, we observe a modest decline in opioid and benzodiazepine prescribing following mandate adoption, suggesting against concerns about increased prescribing due to greater naloxone availability. By contrast, MOUD prescriptions follow a pattern similar to naloxone: prescribing rose by 76% in treated states after coprescription mandates, from 364.02 to 639.53 claims per 100,000 beneficiaries.

Table 2 presents covariate means for control and treated states across the three drug categories, along with normalized mean differences to assess balance (Baker et al., 2025; Imbens & Rubin, 2015).[10] Overall, treatment and control states are well matched on demographic and Medicare-related characteristics.[11] For policy variables, treated states are somewhat more likely to have adopted Good Samaritan laws, while adoption of pharmacy-based naloxone access laws and prescription drug monitoring programs is widespread across both groups. In our preferred specification, we include these policy covariates to account for contemporaneous initiatives, but results remain robust when these controls are excluded.

4. Empirical Strategy

We implement a difference-in-differences strategy comparing changes in yearly outcomes between states that adopted naloxone coprescription mandates and states that did not.[12] We estimate the policy effect separately for each drug category (naloxone, opioids and benzodiazepines, and MOUD). Within each category, we examine four prescribing measures, yielding 12 treatment effect estimates (3 drug categories × 4 measures).

[10] Normalized mean differences are calculated as the difference in means between treated and control states, divided by the average standard deviation. Values close to zero indicate better balance, with absolute differences below 0.25 generally viewed as modest. Larger values may suggest potential imbalance; however, when the underlying standard deviation is very small, even trivial differences in means can generate relatively large normalized mean differences. Interpretation should therefore consider both the normalized mean differences and the raw mean values.

[11] In Table 2, some normalized mean differences—notably for male share and selected age groups—exceed the 0.25 threshold, but the corresponding raw means differ only slightly between treated and control states. For example, the male share differs by at most 0.26 percentage points, although the normalized difference reaches 0.35. This suggests that the apparent imbalance is mechanical rather than substantive, driven by small underlying standard deviations rather than meaningful differences between groups.

[12] Treated states include those mandating naloxone coprescription (AZ, AR, FL, IN, NJ, NM, NY, RI, VT, VA, WA) and those mandating that naloxone be offered (CA, CO, IL, KY, OH, SC, TN). In Section 6.1, we distinguish between these two types of mandates, and in Table A1 we provide details on the specific laws. Non-adopting states include AK, AL, CT, DC, DE, GA, HI, IA, ID, KS, LA, MA, MD, ME, MI, MN, MO, MS, MT, NC, ND, NE, NH, NV, OK, OR, PA, SD, TX, UT, WI, WV, and WY.

The staggered timing of policy adoption creates an identification challenge. Virginia and Vermont implemented mandates in 2017, followed by other states through 2023. If policy effects grow over time as physicians adjust behavior, a standard two-way fixed-effects estimator would produce biased estimates by using already-treated early adopters as controls for later-adopting states (Callaway & Sant'Anna, 2021; De Chaisemartin & d'Haultfoeuille, 2023; Goodman-Bacon, 2021; Roth et al., 2023; Sun & Abraham, 2021).

To accommodate the staggered adoption, we use Callaway and Sant'Anna's (2021) doubly robust estimator (CS-DID). This approach groups states by adoption year into cohorts and compares each cohort only to never-treated states, explicitly avoiding the use of already-treated states as controls. For each cohort g, we estimate the average treatment effect on the treated (ATT), which captures the change in drug prescribing from the year before adoption ($g-1$) to year $t \geq g$ in treated states, net of contemporaneous changes in never-treated states. The estimator further enhances robustness against model misspecification by combining outcome regression with propensity score weighting (Abadie, 2005; Heckman et al., 1997; Sant'Anna & Zhao, 2020). We cluster standard errors at the state level to account for intra-state correlation in prescribing patterns (Abadie et al., 2023; Rambachan & Roth, 2026).

After obtaining cohort-specific effects, we report two policy-relevant aggregated estimates. First, we report an overall ATT that combines all adoption cohorts and all post-adoption years, weighted by cohort-year size, providing a single summary of the policy's average impact. Second, we estimate event-study coefficients showing how effects evolve over time since adoption, offering insights into the persistence and long-run policy effectiveness.

Our identification strategy relies on three key assumptions. The primary assumption is conditional parallel trends: absent coprescription laws, prescribing outcomes in treated and never-treated states with similar observed characteristics would have evolved similarly. In our setting, adopting states may differ systematically from nonadopting states, for example in the severity of their overdose crises. Such differences do not by themselves violate conditional parallel trends, because the estimator compares changes relative to the year before adoption, which removes differences in levels. They threaten identification only if they are associated with divergent prescribing trends in the absence of the laws. We assess this possibility in two ways. In Section 5.2, we show that pre-treatment event-study estimates are statistically indistinguishable from zero.

In Section 5.3, we show that our estimates are robust to additionally conditioning on pre-policy overdose mortality.

Second, we assume no anticipation: states did not alter prescribing before the law took effect. This is plausible given that most laws were enacted and implemented within the same legislative year, leaving little time for anticipatory responses. Third, we assume limited spillovers across states. Two types of spillovers could threaten this assumption. First, patients in control states may cross state borders to see providers in treated states, inflating treated state prescribing and leading us to overestimate the law's effect. Second, providers in control states near the border may become aware of coprescription practices in neighboring treated states and voluntarily increase naloxone prescribing, raising control state prescribing and biasing our estimates toward zero. We address the first concern by excluding border county prescribers in Section 5.3 and finding similar results, suggesting that cross-border patient flows do not meaningfully drive our estimates. The second concern, if present, would cause us to underestimate the true effect of coprescription laws, making our findings conservative. We therefore view our estimates as a lower bound on the full policy impact.

5. Results

5.1 Main Results

Naloxone Prescribing. Table 3 presents the estimated impact of coprescription laws on naloxone prescribing. We find that the laws increased naloxone claims by 230.19 per 100,000 Medicare beneficiaries (column 1), a 275% increase relative to the pre-policy mean in treated states. The number of 30-day fills rose by a nearly identical amount. This close alignment between claims and fills indicates that most prescriptions result in a single fill, consistent with naloxone being prescribed on an as-needed basis rather than as a recurring medication.

The laws also substantially expanded the provider and patient base for naloxone. The number of prescribers increased by 64.13 per 100,000 beneficiaries (column 5), a 386% increase, while the number of beneficiaries receiving naloxone rose by 264%. The larger proportional increase in prescribers relative to beneficiaries aligns with expectations under a mandate-driven policy, where even reluctant prescribers may comply, but each contributes fewer prescriptions on average. These estimates remain remarkably stable when we exclude policy controls (even-numbered columns),

indicating that the observed effects are primarily driven by the coprescription mandate rather than contemporaneous policies.

Opioid and Benzodiazepine Prescribing. Table 4 examines whether coprescription laws affect opioid and benzodiazepine prescribing. In Panel A, we find no statistically or economically significant changes in prescribing for opioids and benzodiazepines combined.[13] To examine whether the null result holds for each drug class individually, we conduct subsample analyses for opioids alone (Panel B) and benzodiazepines alone (Panel C). The estimates show negligible changes in the number of claims, fills, and beneficiaries across both subsamples. The only marginally significant effect appears for the number of opioid prescribers (Panel B, column 3), but the magnitude is small.

We further examine whether coprescription laws, while not affecting total opioid prescribing, might shift the composition of prescribing between short-acting and long-acting formulations. This matters because the two formulations have distinct risk profiles and clinical uses. Long-acting opioids, such as extended-release formulations of oxycodone and morphine, are prescribed for chronic pain management and are subject to stricter regulatory oversight due to higher addiction and overdose risk (Ray et al., 2016). Short-acting opioids provide rapid pain relief for acute conditions and are more commonly prescribed but also frequently implicated in nonmedical opioid use (Cicero et al., 2017). CDC guidelines recommend coprescribing naloxone particularly for patients on long-term opioid therapy (Dowell et al., 2016), which typically involves long-acting formulations, but the broader prevalence of short-acting opioids and their role in the transition to opioid misuse make them equally policy-relevant. Table A6 presents effect estimates separately by opioid formulation. We find no statistically significant effects on prescribing of either formulation across claims, fills, and beneficiaries. The only marginally significant effect appears for the number of prescribers of long-acting opioids, but the magnitude is modest.

Taken together, these results provide a comprehensive and consistent picture: coprescription mandates do not meaningfully alter opioid prescribing patterns along any dimension—whether examining total prescribing, individual drug classes, or formulation type.

[13] In Appendix Table A2, we show that the findings for opioids and benzodiazepines and MOUD are also insensitive to the exclusion of policy controls, with results remaining stable across odd- and even-numbered columns.

MOUD Prescribing. Table 5 presents the effects of coprescription laws on MOUD prescribing. Panel A shows that coprescription laws increased MOUD claims and 30-day fills by 55–56%, and the number of beneficiaries receiving MOUD rose by 42%, though the increase in the number of MOUD prescribers (32%) is not statistically significant. Panels B and C decompose these effects by medication type. Panel B focuses on buprenorphine, the most widely used MOUD, and shows increases in claims and fills of 56–57% and a 44% rise in beneficiaries. Panel C examines naltrexone, which blocks opioid receptors and is used for long-term relapse prevention. We find modest positive effects across all outcomes, though none are statistically significant.

5.2 Effect Dynamics

In Figures 3–5**,** we examine how coprescription law effects evolve over time for naloxone, opioids and benzodiazepines, and MOUD. These event-study estimates also allow us to detect pre-existing trends and anticipatory responses.

Figure 3 shows the effects on naloxone prescribing. Pre-treatment coefficients are indistinguishable from zero across all four measures, supporting the parallel trends and no-anticipation assumptions. This pattern also provides reassurance against opioid-specific secular trends—such as growing provider awareness of addiction medicine or evolving CDC guidelines—that our placebo tests (Section 5.3) cannot directly rule out. If such trends were systematically correlated with law adoption timing, we would expect to see pre-treatment divergence between treated and control states; the flat pre-treatment coefficients suggest this is not the case. Effects turn positive and statistically significant immediately upon policy adoption (period 0) and persist at similar or growing magnitudes over time. This pattern indicates that coprescription laws produce immediate and sustained increases in naloxone access among Medicare beneficiaries.

Figure 4 examines opioid and benzodiazepine prescribing. Point estimates remain close to zero throughout, though we observe a slight uptick in the number of prescribers in later years. However, this effect remains statistically insignificant and economically small. These null dynamic effects reinforce our main finding that coprescription laws do not meaningfully alter opioid or benzodiazepine prescribing patterns.

Figure 5 presents MOUD prescribing dynamics. Pre-treatment coefficients again support parallel trends and no anticipatory responses. Following policy adoption, we observe gradual

increases in the number of claims, fills, and beneficiaries, with effects strengthening over time and becoming statistically significant by years 2–3. The number of prescribers shows little immediate change but trends upward in later periods, suggesting that physician adoption of MOUD prescribing may be a gradual process that takes time to materialize.

5.3 Robustness Checks

We conduct several robustness checks to assess the sensitivity of our main findings.

Falsification Tests. Although we find no increase in opioid or benzodiazepine prescribing, the positive effects on naloxone and MOUD could still reflect broad secular trends in healthcare delivery that happen to correlate with the timing of coprescription law adoption rather than the causal effect of the mandate itself. Two such trends are particularly relevant in our sample period. First, the COVID-19 pandemic dramatically accelerated telemedicine adoption, enabling patients to obtain prescriptions without in-person visits and potentially increasing prescription volumes broadly across many drug categories in states with greater telemedicine infrastructure—independently of any opioid-specific policy. Second, the provider workforce has shifted considerably over our sample period, with nurse practitioners and physician assistants accounting for a growing share of prescribers. These providers have increasingly broad prescribing authority and tend to be concentrated in states with more permissive scope-of-practice laws, which may also correlate with states more likely to adopt progressive opioid policies like coprescription mandates. If either trend were driving our results, we would expect to see increases across many drug categories, including those entirely unrelated to opioid policy.

To test for this, we examine two placebo outcomes prescribed through the same healthcare system by many of the same providers but with no connection to opioids or overdose prevention: antibiotics, prescribed for acute infections, and ACE inhibitors/ARBs, first-line treatments for chronic conditions such as hypertension and heart failure.[14] Table A3 shows no statistically or economically significant effects on either antibiotics or ACE inhibitors/ARBs across all four prescribing measures, providing reassurance that our findings are not driven by broader changes in prescribing unrelated to opioid treatment.

[14] A comprehensive list of brand names for both drug categories is provided in Table B2.

COVID-19 Pandemic. Beyond the general telemedicine trend addressed in the falsification tests, the COVID-19 pandemic may have confounded our estimates through a more direct channel: if pandemic severity correlates with policy adoption timing and differentially disrupts healthcare utilization across states, post-pandemic rebounds in outpatient care could inflate prescribing in treated states coincidentally rather than causally. To address this concern, we re-estimate effects controlling for each state's average weekly COVID-19 death rate (CDC, 2025) as a proxy for pandemic severity and the associated disruptions to in-person care, provider capacity, and prescription filling behavior. Column 2 of Table A4 shows that results remain consistent with baseline estimates across all drug categories, indicating that differential pandemic severity does not drive our findings.

Pre-Policy Overdose Mortality. Another potential threat to identification is that states with more severe overdose crises may have been more likely to adopt coprescription laws and may also have followed different prescribing trends in the absence of the laws. We re-estimate our baseline specifications adding each state's drug overdose mortality rate in 2016, the year preceding the first adoption of coprescription laws, to the conditioning covariates. Because this measure predates all adoptions, it is unaffected by the laws. We identify drug overdose deaths using data from CDC WONDER and ICD-10 underlying cause-of-death codes X40–X44, X60–X64, X85, and Y10–Y14. The results are similar to our baseline estimates across all drug categories (Table A4, column 3).

Cross-State Spillovers. As discussed in Section 4, spillovers could bias our estimates in either direction: patients crossing into treated states to see providers would inflate our estimates, while control state providers near the border voluntarily adopting coprescription practices would bias our estimates toward zero. To address the first concern, we leverage provider-level geographic data from CMS (2025c), which summarizes Medicare Part D claims by provider, drug, and year. We identify providers by county, exclude prescriptions written by providers in border counties, and aggregate this restricted sample to the state-drug-year level. Column 4 of Table A4 shows that this restriction yields similar results—naloxone claims increased by 176%, MOUD claims increased by 62%, and effects on opioids and benzodiazepines remain null—providing reassurance that our estimates are not contaminated by patients seeking care across state lines.

Composition of Medicare population. Medicare primarily covers adults aged 65 or older, but also provides coverage for younger individuals with long-term disabilities, end-stage renal disease, or amyotrophic lateral sclerosis, who accounted for approximately 12% of all Medicare beneficiaries in 2022 (KFF, 2023). These younger beneficiaries may have distinct opioid use and treatment profiles compared to older beneficiaries, and their geographic concentration could confound our estimates if it correlates with law adoption timing. To ensure that our findings are not primarily driven by this subpopulation, we re-estimate effects restricting to beneficiaries aged 65 and older. Column 5 of Table A4 confirms that effects remain consistent with baseline estimates.

Medicaid expansion. Medicaid expansion under the ACA could confound our estimates through two channels. First, expansion may influence drug prices and formulary access, potentially affecting prescription utilization among low-income Medicare beneficiaries (Ghosh et al., 2019; McInerney et al., 2017). Second, although the ACA expansion group excludes people eligible for Medicare, expansion may indirectly affect dually eligible individuals—those covered by both Medicare and Medicaid—who tend to have higher rates of chronic conditions and complex care needs, including opioid use disorder.[15] If expansion timing correlates with coprescription law adoption, it could bias our estimates. To account for this, we re-estimate all specifications controlling for whether states expanded Medicaid under the ACA. Column 6 of Table A4 shows results nearly identical to baseline, indicating that Medicaid expansion does not confound coprescription law effects.

Buprenorphine Prior Authorization Policies. Federal and state policies reducing prior authorization requirements for buprenorphine could confound our MOUD results if their timing correlates with coprescription law adoption. Specifically, CMS discouraged Medicare Part D plans from requiring prior authorization more than once yearly for buprenorphine in April 2018, and mandated complete removal of prior authorization requirements for buprenorphine in January 2023 (Keshwani et al., 2022; Treitler et al., 2023). While these national-level changes affect treated and control states equally and are therefore differenced out in our DID design, state-level variation in prior authorization policies could independently drive increases in buprenorphine prescribing (Andraka-Christou et al., 2023). To address this, we include a binary indicator for whether a state

[15] According to KFF (2024), in 2022, Medicaid provided supplemental coverage for approximately 11.6 million Medicare beneficiaries with limited incomes and assets, who accounted for 19% of all Medicare beneficiaries.

prohibited buprenorphine prior authorization.[16] Column 7 of Table A4 shows that results remain consistent across all drug categories, indicating that state-level prior authorization policies do not confound our MOUD estimates.

Alternative Control Group Definition. Our baseline specification defines the control group as the never-treated states. In column 8 of Table A4, we test an alternative definition that expands the control group to include both never-treated and not-yet-treated states. This approach leverages additional pre-policy variation, as states that have not yet adopted coprescription laws provide information on counterfactual trends prior to their implementation. The results remain virtually unchanged, indicating robustness to control group definition.

New Drug Approvals. New naloxone and MOUD formulations introduced in or after 2017 could mechanically inflate post-treatment means if drug approval coincides with policy implementation. To examine the impact of this indirect effect, we exclude any naloxone and MOUD products approved in or after the earliest law adoption year (2017). Specifically, for naloxone, we exclude Kloxxado (intranasal high-dose, 2021) and Zimhi (injectable, 2021). For MOUD, we exclude Sublocade (extended-release injectable buprenorphine, 2017). Table A5 shows that results remain consistent with the baseline, confirming our main results do not merely reflect pharmaceutical innovation.

6. Policy Design and Treatment Engagement

Our main results establish that coprescription laws substantially increase naloxone access and MOUD prescribing without altering opioid or benzodiazepine prescribing. These findings raise two deeper questions. First, does the specific design of coprescription mandates matter for effectiveness? Second, do these laws generate genuine treatment engagement beyond the observed increases in MOUD prescribing? We address each question in turn.

6.1 Prescribing Mandates versus Offer Mandates

As discussed in Section 2, coprescription laws vary in stringency. Some states require physicians to prescribe naloxone when prescribing opioids above specified thresholds (*prescribing* mandates),

[16] Data source for state MOUD prior authorization policies: RAND-USC Schaeffer Opioid Policy Tools and Information Center. 2023. OPTIC-Vetted Prior Authorization Restrictions on Medications for Opioid Use Disorder Policy Data. Obtained from https://www.rand.org/health-care/centers/optic/resources/datasets.html on March 25, 2025.

while others require physicians only to offer naloxone, leaving the decision to patients (*offer* mandates). This distinction has important behavioral and policy implications. Prescribing mandates create a strong default, where patients receive naloxone unless they actively decline. By contrast, offer mandates require patients' active acceptance, potentially reducing uptake if patients underestimate overdose risk, lack awareness of naloxone's protective role, or face stigma associated with accepting overdose-prevention medication. From a behavioral economics perspective, prescribing mandates leverage default effects and reduce transaction costs, while offer mandates impose a higher cognitive burden on patients at a moment when they may be poorly positioned to assess risk (Madrian & Shea, 2001).

To evaluate the effect heterogeneity by mandate types, we estimate effects separately for prescribing mandate states (Arizona, Arkansas, Florida, Indiana, New Jersey, New Mexico, New York, Rhode Island, Vermont, Virginia, Washington) and offer mandate states (California, Colorado, Illinois, Kentucky, Ohio, South Carolina, Tennessee), using never-treated states as controls for both subsamples. To ensure comparability across subsamples, we report percentage effects by normalizing level effects by pre-treatment means in treated states.

Figure 6 presents these subsample estimates for naloxone, opioids and benzodiazepines, and MOUD across all four prescribing measures.[17] Two patterns emerge. First, prescribing mandates produce substantially larger effects than offer mandates for both naloxone and MOUD. For naloxone, prescribing mandates increase prescribing by roughly twice the magnitude of offer mandates. For MOUD, only prescribing mandates generate statistically significant effects; offer mandate effects are smaller and statistically indistinguishable from zero. This asymmetry is consistent with the behavioral economics literature on defaults: when naloxone is prescribed automatically, patients need not overcome inertia, cognitive barriers, or stigma to obtain it. By contrast, offer mandates require patients to actively accept a medication that signals overdose risk—a decision that may be psychologically difficult in a clinical setting. Second, neither mandate type significantly affects opioid or benzodiazepine prescribing, reinforcing our main finding that coprescription laws expand naloxone availability without encouraging riskier prescribing regardless of mandate design.

[17] We present detailed estimates in Appendix Table A7.

6.2 Treatment Engagement Effects

Our findings that coprescription laws increase naloxone prescribing by 275% and MOUD prescribing by 56% raise a critical follow-up question: do the MOUD increases reflect genuine treatment engagement, or merely mechanical increases in prescribing that do not translate into patients actively entering treatment? While MOUD prescribing is a necessary condition for treatment, entry into structured treatment programs represents a crucial step in long-term recovery. The opioid crisis literature has increasingly emphasized the importance of connecting at-risk individuals to evidence-based treatment (Barrette et al., 2023; Volkow et al., 2014; Wakeman et al., 2020), yet pathways into treatment remain poorly understood.

Coprescription laws could plausibly increase treatment admissions through several behavioral mechanisms. First, the mandated coprescription encounter creates a clinical touchpoint in which physicians must discuss overdose risk, potentially prompting conversations about the severity of the patient's opioid use and available treatment options (Coffin et al., 2016). This represents a form of choice architecture, where the clinical environment is structured to facilitate treatment-seeking without mandating it. Second, coprescribing naloxone may function as a powerful signal of risk, increasing patients' awareness that their opioid use poses serious health threats and raising their readiness to seek help—consistent with the health belief model, which emphasizes perceived susceptibility and severity as key drivers of health behavior change (Rosenstock, 1974). Third, broader naloxone availability may increase the likelihood that overdose survivors are connected to treatment during moments of heightened receptivity following a near-fatal overdose event (Salisbury-Afshar et al., 2024). Finally, by preventing otherwise fatal overdoses, greater naloxone availability may increase the number of individuals who survive to subsequently seek treatment. This survival channel could increase treatment admissions even without changes in patients' treatment-seeking behavior or physicians' referral practices.

Using the TEDS-A data described in Section 3.3, we estimate effects on total OUD treatment admissions and disaggregate by the four service settings defined there. Table 6 presents the results. Coprescription laws increase total OUD treatment admissions by 0.62 per 1,000 Medicare beneficiaries, a 297% increase relative to the pre-treatment mean of 0.21.

Service-setting-specific results suggest that the increase is concentrated in settings reflecting deliberate treatment-seeking rather than acute medical crises. Non-intensive outpatient admissions

increase by 0.40 per 1,000 beneficiaries (227%) and rehabilitation/residential admissions increase by 0.07 per 1,000 (387%). Intensive outpatient admissions show a large percentage increase due to the very small baseline rate, but remain statistically insignificant. In contrast, detoxification admissions—typically driven by acute medical necessity such as emergency department visits following overdoses or severe withdrawal—show no significant change. The corresponding event-study estimates are in Figure A2.

Although we observe an increase in OUD treatment admissions, our data unfortunately do not allow us to distinguish among the underlying mechanisms. These may include increased physician referrals, changes in patients' treatment-seeking behavior, or improved overdose survival that expands subsequent opportunities to enter treatment. Distinguishing these mechanisms is an important direction for future research, as they have different implications for the design of harm reduction and addiction treatment policies. Nonetheless, our reduced-form evidence captures the overall effect of coprescription laws on treatment admissions, which points to potential policy benefits beyond expanding naloxone access.

7. Discussion and Conclusion

Naloxone coprescription laws represent a direct intervention requiring physicians to prescribe or offer naloxone when prescribing high-dose opioids. Using Medicare Part D data covering 2013–2023 and a staggered difference-in-differences design, we provide causal evidence on how these laws affect naloxone access, prescribing behavior, MOUD uptake, and treatment engagement. Our main findings are fourfold.

First, coprescription laws substantially and durably increase naloxone access. Naloxone claims rise by 275%, with effects persisting at least five years post-adoption and reflecting growth across all dimensions of access: prescription volumes, the number of prescribers (up 386%), and the number of beneficiaries receiving naloxone (up 264%). Our estimates are at the higher end of the 159-333% range documented for pharmacy-based access laws (Smart et al., 2024), reflecting how physician-targeted mandates systematically ensure naloxone reaches high-risk patients rather than relying on patient-initiated requests. Medicare's relatively low cost-sharing further amplifies

these effects, as uptake is constrained by prescriber behavior rather than price.[18] Our estimates are more modest than the nearly eightfold increase reported by Sohn et al. (2019) for Virginia and Vermont, likely because their study captured very low pre-policy baselines and short-term implementation surges. By examining all eighteen adopting states over a longer horizon, we capture more gradual but durable prescribing responses, providing a more comprehensive assessment of policy effectiveness.

Second, coprescription laws do not alter opioid or benzodiazepine prescribing patterns. We find null effects across claims, fills, prescribers, and beneficiaries for both drug categories, and these null results hold for both short-acting and long-acting opioid formulations. This finding directly addresses a central concern raised by critics of harm reduction policies: that expanding safety nets might encourage riskier behavior through moral hazard. We find no evidence of such incentives.

Third, coprescription laws generate substantial treatment engagement effects. MOUD prescriptions increase by 56%, and treatment admissions for opioid use disorder nearly quadruple, rising by 297% (0.62 per 1,000 beneficiaries). This effect is concentrated in settings reflecting deliberate treatment-seeking—non-intensive outpatient care and rehabilitation/residential programs—rather than acute crisis responses like detoxification. The absence of a significant increase in detoxification admissions is consistent with the interpretation that the observed rise in treatment utilization is not primarily driven by acute medical emergencies. Instead, the pattern supports a behavioral mechanism in which mandated clinical conversations about overdose risk raise awareness, signal severity, and motivate treatment entry (Grant & Smart, 2022; Salisbury-Afshar et al., 2024).

Fourth, policy design substantially affects effectiveness. Prescribing mandates—which create strong defaults by automatically providing naloxone—produce effects roughly twice as large as offer mandates, which require active patient acceptance. This asymmetry aligns with behavioral economics research demonstrating that defaults powerfully shape behavior (Johnson & Goldstein, 2003; Madrian & Shea, 2001) and suggests that the choice between prescribing and offer mandates

[18] As Peet et al. (2022) reported, in 2014 the average out-of-pocket cost per naloxone claim was $8.11 for Medicare beneficiaries, compared with $27.97 for the privately insured and $35.39 for the uninsured. Because costs are already minimal for Medicare patients, price does not represent a meaningful barrier to uptake.

has first-order implications for policy effectiveness. The stronger performance of prescribing mandates reflects reduced transaction costs, elimination of status quo bias, and removal of stigma-related barriers that might prevent patients from actively requesting overdose-prevention medication (O'Donoghue & Rabin, 1999).

Our findings contribute to several literatures. Within the harm reduction literature, we provide causal evidence that physician-targeted naloxone policies can achieve uptake rates comparable to or exceeding those of patient-initiated pharmacy access programs (Abouk et al., 2019; Peet et al., 2024; Smart et al., 2024), while simultaneously generating treatment engagement effects not previously documented. Within the MOUD access literature, we show that naloxone coprescription serves as an unexpected pathway to expanding MOUD prescribing, complementing direct interventions like prior authorization removal (Keshwani et al., 2022). More broadly, we contribute to the growing literature on behavioral interventions in healthcare by demonstrating that simple clinical touchpoints—when aligned with moments of heightened salience and patient receptivity—can produce sizable and durable effects on treatment engagement.

Several limitations should be noted. First, our prescription analysis focuses on Medicare Part D beneficiaries, primarily adults aged 65 and older and younger individuals with disabilities. While this population is relevant for evaluating policies targeting prescription opioid use, it is not representative of the broader population at risk of opioid overdose, especially individuals whose opioid use occurs primarily through illicit drug markets (Ruhm, 2019). Our findings may therefore not generalize to these populations. Second, because methadone administered for OUD treatment is not captured in Medicare Part D prescription data, our analysis does not cover this important treatment modality. Consequently, our MOUD estimates should not be interpreted as capturing changes in overall pharmacological treatment for OUD. Third, our state-year-drug level analysis cannot illuminate patient-level treatment adherence, prescriber decision-making processes, or individual treatment outcomes. Finally, claims data do not capture illicitly obtained opioids or naloxone, nor do they reflect non-prescription MOUD access (Chilcoat et al., 2019), potentially understating the laws' full impact on harm reduction and recovery. Future research should explore individual-level treatment engagement, long-term health outcomes, and potential interactions with other harm-reduction and addiction treatment policies.

The opioid crisis continues to claim over 80,000 lives annually in the United States, with synthetic opioids now driving the majority of overdose deaths. A longstanding tension in opioid policy has been the perceived tradeoff between harm reduction—keeping people alive—and treatment—helping them recover. Our findings suggest that naloxone coprescription laws ease this tension: they expand naloxone access without encouraging riskier prescribing, and in doing so create clinical touchpoints that catalyze MOUD uptake and treatment engagement. The finding that prescribing mandates outperform offer mandates further suggests that mandate design is not merely a technical detail but a first-order determinant of policy effectiveness.

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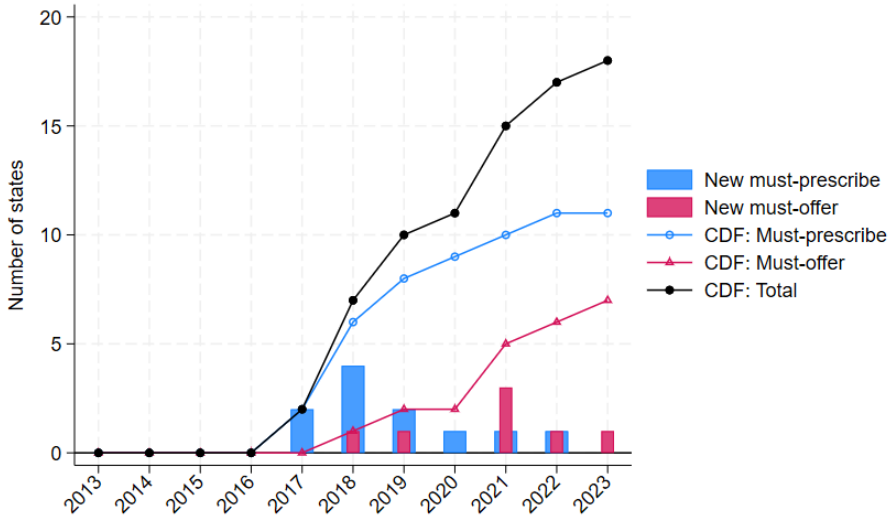
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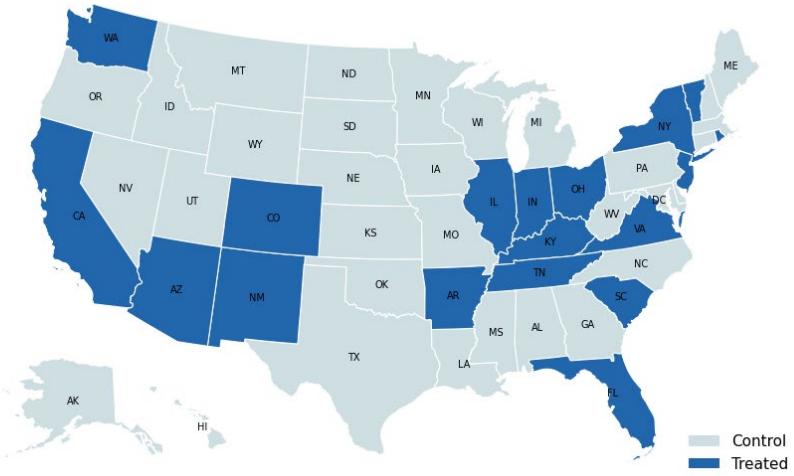
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Figures



(A) Number of states with each type of naloxone coprescription law



(B) State adoption as of December 31, 2023

Figure 1. Temporal and Geographic Distribution of Naloxone Coprescription Law Adoption

Notes: Panel (A) displays the cumulative and annual adoption of naloxone coprescription laws by state, distinguishing between prescribing mandates (blue), which require physicians to prescribe naloxone alongside high-risk opioids, and offer mandates (red), which require physicians to offer or discuss naloxone but allow patient refusal. Panel (B) presents the geographic distribution of adopting states as of December 31, 2023 (treated states: darker blue). Virginia's law, the first coprescription law, took effect in March 2017, followed by Vermont's later that year.

Data Sources: 2024 Fact Sheet – Naloxone Prescription Mandates, Network for Public Health Law (Lieberman & Davis, 2024), cross-referenced with Green et al. (2020), the RAND OPTIC-Vetted Policy Data Sets, and the authors' own legal search.

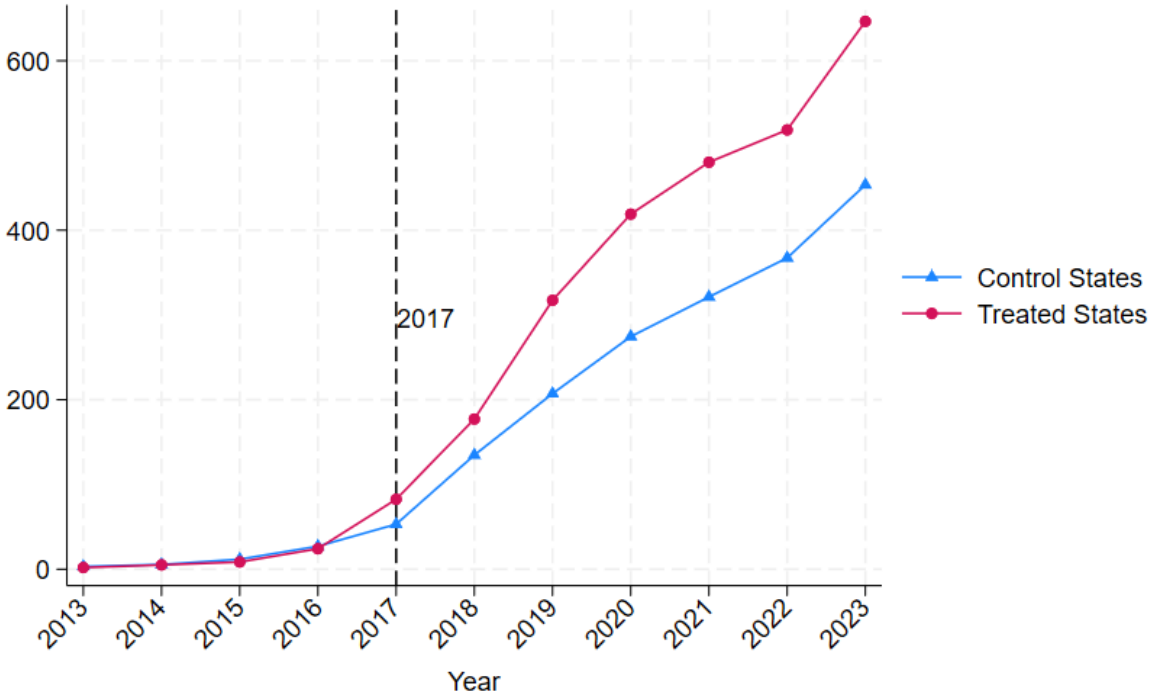


Figure 2. Naloxone Prescribing Trends in Treated and Control States, 2013-2023

Notes: This figure presents unadjusted trends in the average number of Part D naloxone claims per 100,000 Medicare beneficiaries for treated states (those that enacted any naloxone coprescription law) and control states (those that never enacted such laws). The dashed vertical line marks 2017, the year of first law adoption. Pre-treatment trends are broadly parallel across the two groups, consistent with the parallel trends assumption underlying our difference-in-differences design.

Data Sources: CMS Medicare Part D Prescribers – by Geography and Drug (2013-2023).

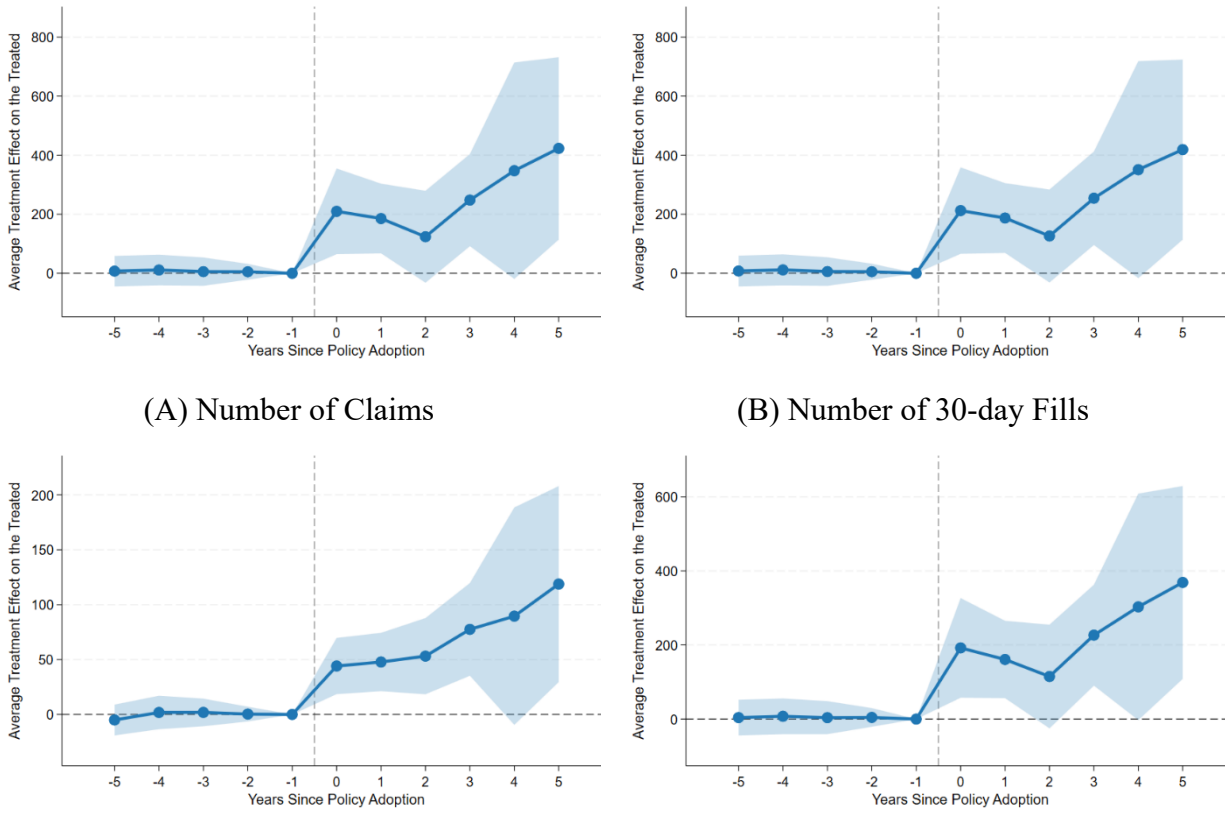
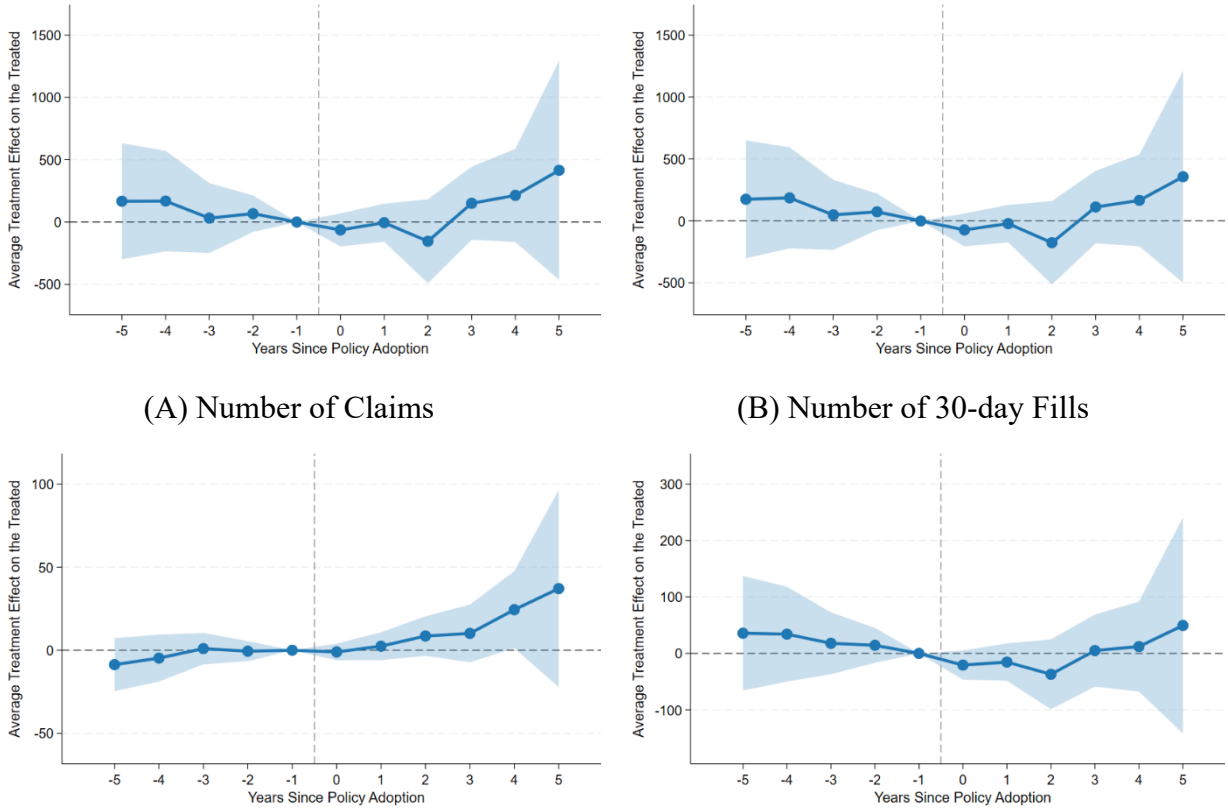


Figure 3. Effects of Coprescription Laws on Naloxone Prescribing: Event Study Estimates

Notes: Panels (A)–(D) present event-study estimates of the average treatment effect on the treated (ATT) with 95% confidence intervals, using the doubly robust CS-DID estimator. All outcomes are normalized by 100,000 Medicare beneficiaries of the corresponding state-year. Time is measured in years relative to coprescription law adoption, with year -1 serving as the base period. All models include the demographic, Medicare, and policy covariates described in Table 2.



(A) Number of Claims

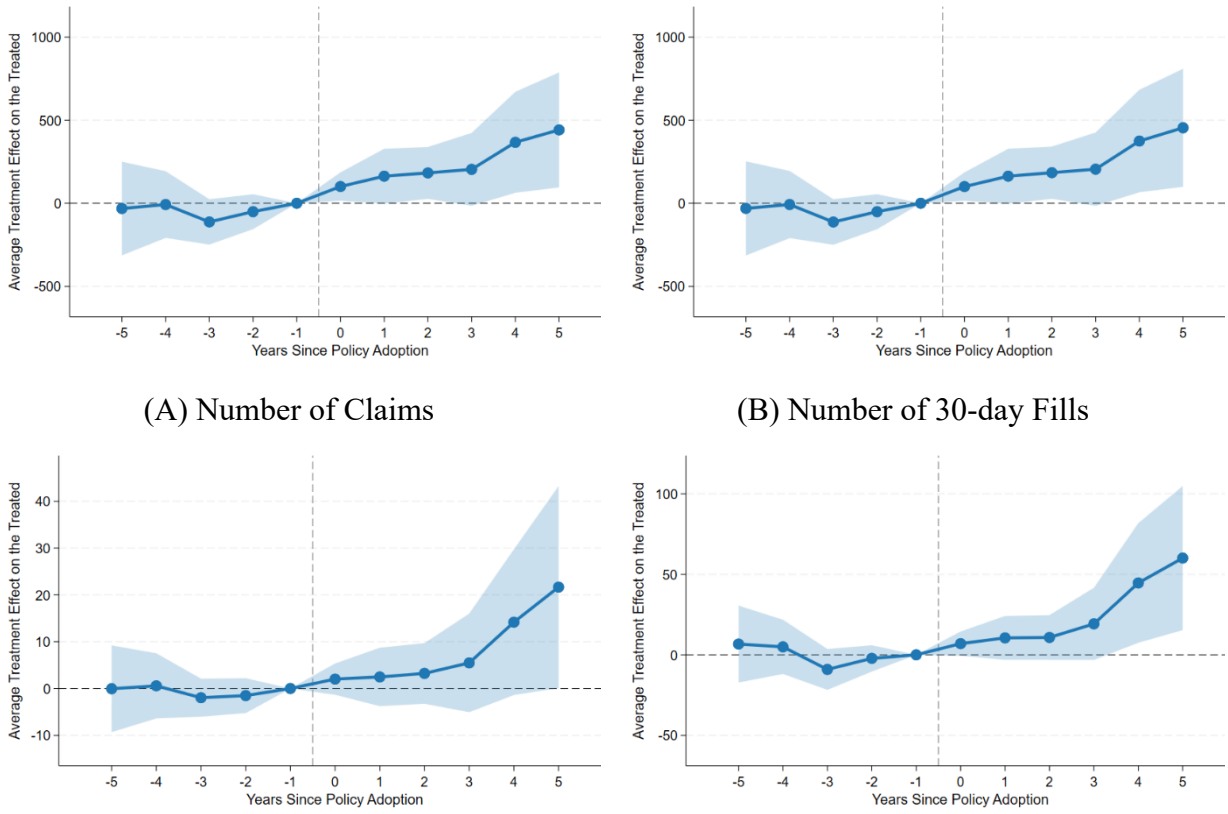
(B) Number of 30-day Fills

(C) Number of Prescribers

(D) Number of Beneficiaries

Figure 4. Effects of Coprescription Laws on Opioid and Benzodiazepine Prescribing: Event Study Estimates

Notes: Panels (A)–(D) present event-study estimates of the average treatment effect on the treated (ATT) with 95% confidence intervals, using the doubly robust CS-DID estimator. All outcomes are normalized by 100,000 Medicare beneficiaries of the corresponding state-year. Time is measured in years relative to coprescription law adoption, with year -1 serving as the base period. All models include the demographic, Medicare, and policy covariates described in Table 2.



(A) Number of Claims

(B) Number of 30-day Fills

(C) Number of Prescribers

(D) Number of Beneficiaries

Figure 5. Effects of Coprescription Laws on MOUD Prescribing: Event Study Estimates

Notes: Panels (A)–(D) present event-study estimates of the average treatment effect on the treated (ATT) with 95% confidence intervals, using the doubly robust CS-DID estimator. All outcomes are normalized by 100,000 Medicare beneficiaries of the corresponding state-year. Time is measured in years relative to coprescription law adoption, with year -1 serving as the base period. All models include the demographic, Medicare, and policy covariates described in Table 2.

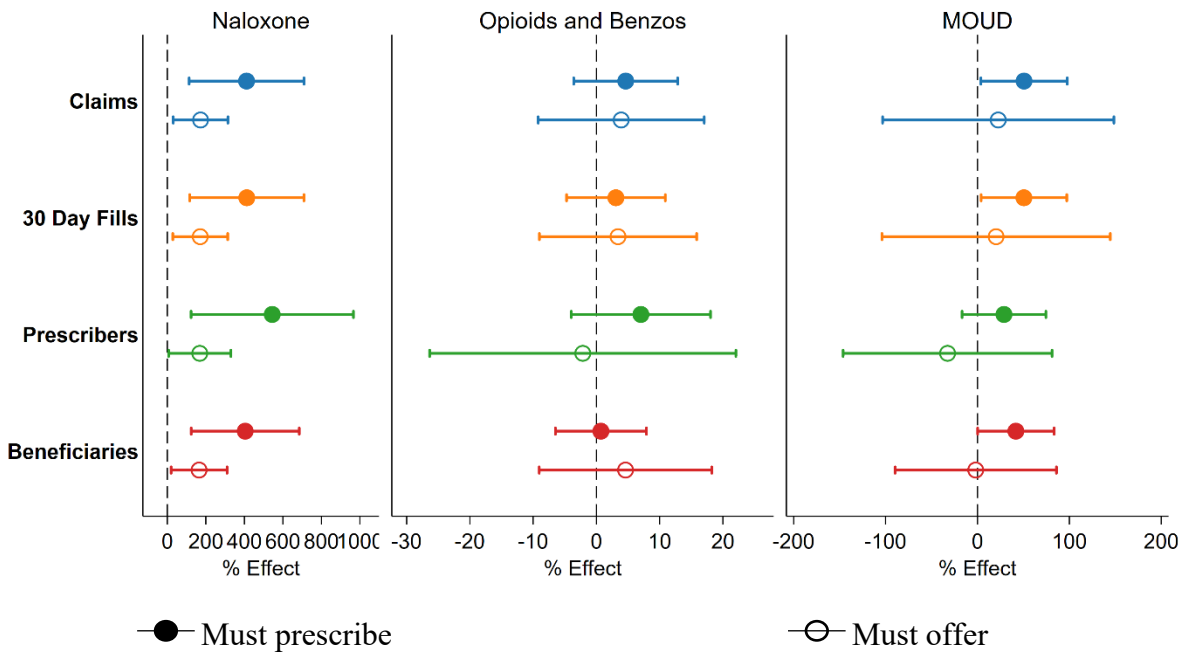


Figure 6. Coprescription Law Effects: Prescribing Mandates versus Offer Mandates

Notes: The three panels present percentage treatment effects (with 95% confidence intervals) for naloxone (left), opioids and benzodiazepines (middle), and MOUD (right), estimated separately for prescribing mandate states and offer mandate states using never-treated states as controls for both subsamples. Percentage effects are calculated as the ATT from the CS-DID estimator divided by the pre-treatment mean in treated states. For each outcome (claims, 30-day fills, prescribers, and beneficiaries per 100,000 Medicare beneficiaries), filled circles denote effects of laws that require physicians to prescribe naloxone ("must prescribe") and hollow circles denote effects of laws that require physicians to offer naloxone ("must offer"). See Table A7 for detailed estimation results.

Tables

Table 1. Summary Statistics (Dependent Variables)

	Naloxone			Opioids and Benzos			MOUD		
	Control	Treated Pre-Policy	Treated Post-Policy	Control	Treated Pre-Policy	Treated Post-Policy	Control	Treated Pre-Policy	Treated Post-Policy
	N=657	N=229	N=197	N=26,746	N=10,184	N=5,844	N=2,512	N=788	N=655
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Outcome per 100k Medicare Beneficiaries									
Number of Claims	204.43	83.68	510.96	2508.09	2403.93	2039.93	486.98	364.02	639.53
	(329.87)	(185.48)	(721.23)	(7779.54)	(8185.32)	(6016.75)	(930.14)	(786.34)	(1266.35)
Number of 30-day Fills	206.75	84.68	518.68	2623.63	2511.50	2118.72	504.13	371.57	661.72
	(333.55)	(188.12)	(729.99)	(8048.92)	(8438.20)	(6190.13)	(937.14)	(787.47)	(1270.25)
Number of Prescribers	54.24	16.62	111.83	125.86	108.11	100.27	28.50	18.30	35.14
	(90.12)	(30.87)	(136.85)	(264.50)	(239.18)	(212.71)	(34.28)	(19.65)	(39.06)
Number of Beneficiaries	185.10	78.09	455.61	615.65	578.47	497.49	68.49	46.18	83.98
	(291.31)	(172.78)	(636.10)	(1960.58)	(2004.35)	(1514.91)	(92.36)	(67.78)	(114.31)

Notes: This table presents summary statistics for dependent variables across three drug categories: naloxone (columns 1-3), opioids & benzodiazepines (columns 4-6), and MOUD (columns 7-9). For each category, means and standard deviations (in parentheses) are reported separately for never-treated states (Control), treated states before policy implementation (Treated Pre-Policy), and treated states after policy implementation (Treated Post-Policy). All outcomes are measured per 100,000 Medicare beneficiaries. Sample sizes reflect state-drug-year observations and are reported below each column header.

Table 2. Summary Statistics (Control Variables)

	Naloxone			Opioids and Benzos			MOUD		
	Control N=657	Treated N=426	Norm. Diff.	Control N=26,746	Treated N=16,028	Norm. Diff.	Control N=2,512	Treated N=1,443	Norm. Diff.
Covariates	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
State Demographics									
Share Aged 20-34	20.28	20.23	-0.03	20.38	20.26	-0.08	20.35	20.20	-0.09
	(1.87)	(0.96)		(1.96)	(0.96)		(2.04)	(0.95)	
Share Aged 35-49	18.81	19.01	0.25	18.77	19.04	0.34	18.76	18.97	0.26
	(0.84)	(0.74)		(0.86)	(0.74)		(0.87)	(0.77)	
Share Aged 50-64	19.31	19.57	0.18	19.52	19.69	0.12	19.40	19.67	0.18
	(1.78)	(0.96)		(1.74)	(0.99)		(1.81)	(1.05)	
Share Aged 65+	16.23	16.52	0.14	15.83	16.19	0.17	16.16	16.51	0.16
	(2.33)	(1.91)		(2.36)	(1.95)		(2.44)	(1.94)	
Share White	78.89	79.93	0.10	78.96	80.10	0.10	78.86	80.46	0.13
	(13.61)	(6.73)		(14.71)	(6.78)		(15.33)	(6.96)	
Share Male	49.60	49.45	-0.21	49.63	49.42	-0.29	49.69	49.43	-0.35
	(0.88)	(0.49)		(0.89)	(0.49)		(0.94)	(0.49)	
Medicare Characteristics									
Part D Enrollment Rate	13.36	13.71	0.15	12.81	13.31	0.21	13.10	13.68	0.24
	(2.58)	(2.00)		(2.70)	(2.05)		(2.76)	(2.05)	
Part D: Weighted Average	25.98	25.30	-0.22	25.79	25.23	-0.18	25.64	25.21	-0.13
Monthly Premium	(3.30)	(2.97)		(3.38)	(2.98)		(3.41)	(2.95)	
Other Policies									
Any PDMPs	0.99	1.00	0.14	0.98	1.00	0.22	0.99	1.00	0.13
	(0.10)	(0.00)		(0.13)	(0.00)		(0.11)	(0.00)	
Any PA-NALs	0.92	0.95	0.12	0.77	0.85	0.21	0.83	0.91	0.24
	(0.27)	(0.22)		(0.42)	(0.35)		(0.37)	(0.29)	
Any GSL	0.85	0.94	0.30	0.73	0.88	0.39	0.80	0.92	0.35
	(0.35)	(0.24)		(0.44)	(0.33)		(0.40)	(0.28)	

Notes: This table presents summary statistics for covariates across three drug categories: naloxone (columns 1-3), opioids & benzodiazepines (columns 4-6), and MOUD (columns 7-9). For each category, means and standard deviations (in parentheses) are reported separately for treated and never-treated (control) states. Normalized mean differences, calculated as the difference in means divided by the average standard deviation, measure covariate balance between the two groups following Baker et al. (2025). Values with absolute normalized mean differences below 0.25 are generally considered well-balanced; larger values should be interpreted alongside the raw means, as small underlying standard deviations can mechanically inflate normalized differences even when raw means are similar between groups.

Table 3. Effects of Naloxone Coprescription Laws on Naloxone Prescriptions

Outcome per 100k	Number of Claims		Number of 30-day Fills		Number of Prescribers		Number of Beneficiaries	
Medicare Beneficiaries	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ATT	230.19***	226.32***	232.24***	228.56***	64.13***	61.98***	206.15***	202.60***
	(67.38)	(58.32)	(67.81)	(58.63)	(17.59)	(14.75)	(59.20)	(51.41)
% Effect	275%	270%	274%	270%	386%	373%	264%	259%
N	1,083	1,083	1,083	1,083	1,083	1,083	1,083	1,083
Mean Y	83.68	83.68	84.68	84.68	16.62	16.62	78.09	78.09
Demographic and Medicare Controls	✓	✓	✓	✓	✓	✓	✓	✓
Policy Controls	✓		✓		✓		✓	

Notes: This table reports overall average treatment effects on the treated (ATT) from the doubly robust Callaway and Sant'Anna (2021) difference-in-differences estimator. Odd-numbered columns present our preferred specification including all demographic, Medicare, and policy controls (see Table 2 for details); even-numbered columns exclude policy controls as a sensitivity check. Standard errors clustered at the state level are reported in parentheses. "Mean Y" denotes the pre-treatment mean outcome among treated states. "% Effect" is calculated as (ATT / Mean Y) × 100. All outcomes are per 100,000 Medicare beneficiaries. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 4. Effects of Naloxone Coprescription Laws on Opioids and Benzodiazepines Prescriptions

Outcome per 100k Medicare Beneficiaries	N. Claims (1)	N. 30-Day Fills (2)	N. Prescribers (3)	N. Beneficiaries (4)
Panel A: Opioids and Benzodiazepines				
ATT	33.88	7.24	9.16	-9.09
	(121.94)	(120.26)	(6.78)	(24.29)
% Effect	1%	0%	8%	-2%
N	42,774	42,774	42,774	42,774
Mean Y	2403.93	2511.50	108.11	578.47
Covariates	✓	✓	✓	✓
	N. Claims (1)	N. 30-Day Fills (2)	N. Prescribers (3)	N. Beneficiaries (4)
Panel B: Opioids Alone				
ATT	89.04	86.35	13.19*	6.32
	(134.66)	(135.64)	(7.60)	(31.77)
% Effect	4%	4%	13%	1%
N	28,298	28,298	28,298	28,298
Mean Y	2243.92	2291.10	104.10	593.76
Covariates	✓	✓	✓	✓
	N. Claims (1)	N. 30-Day Fills (2)	N. Prescribers (3)	N. Beneficiaries (4)
Panel C: Benzodiazepines Alone				
ATT	-105.76	-179.35	0.50	-39.78
	(171.19)	(174.75)	(5.00)	(24.32)
% Effect	-4%	-6%	0%	-7%
N	14,163	14,163	14,163	14,163
Mean Y	2761.79	3016.86	118.14	544.62
Covariates	✓	✓	✓	✓

Notes: This table reports overall average treatment effects on the treated (ATT) from the doubly robust Callaway and Sant'Anna (2021) difference-in-differences estimator. Panel A examines opioids & benzodiazepines combined, Panel B examines opioids alone, and Panel C examines benzodiazepines alone. All models include demographic, Medicare, and policy controls (see Table 2 for details); results are robust to excluding policy controls, as shown in Table A2. Standard errors clustered at the state level are reported in parentheses. "Mean Y" denotes the pre-treatment mean outcome among treated states. "% Effect" is calculated as (ATT / Mean Y) × 100. All outcomes are per 100,000 Medicare beneficiaries. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 5. Effects of Naloxone Coprescription Laws on MOUD Prescriptions

Outcome per 100k Medicare Beneficiaries	N. Claims (1)	N. 30-Day Fills (2)	N. Prescribers (3)	N. Beneficiaries (4)
Panel A: All MOUD				
ATT	202.89**	205.24**	5.81	19.27**
	(88.92)	(89.74)	(4.28)	(9.21)
% Effect	56%	55%	32%	42%
N	3,955	3,955	3,955	3,955
Mean Y	364.02	371.57	18.30	46.18
Covariates	✓	✓	✓	✓
	N. Claims (1)	N. 30-Day Fills (2)	N. Prescribers (3)	N. Beneficiaries (4)
Panel B: Buprenorphine				
ATT	259.26**	260.42**	5.87	22.11**
	(108.24)	(108.17)	(3.82)	(9.65)
% Effect	57%	56%	37%	44%
N	2,880	2,880	2,880	2,880
Mean Y	458.87	460.92	15.73	50.70
Covariates	✓	✓	✓	✓
	N. Claims (1)	N. 30-Day Fills (2)	N. Prescribers (3)	N. Beneficiaries (4)
Panel C: Naltrexone				
ATT	38.27	44.96	5.52	10.97
	(44.80)	(52.40)	(6.55)	(9.98)
% Effect	29%	30%	23%	32%
N	1,067	1,067	1,067	1,067
Mean Y	129.79	150.63	24.34	34.70
Covariates	✓	✓	✓	✓

Notes: This table reports overall average treatment effects on the treated (ATT) from the doubly robust Callaway and Sant'Anna (2021) difference-in-differences estimator. Panel A presents results for all MOUD combined. Panels B and C decompose these effects by medication type, examining buprenorphine and naltrexone separately. All models include demographic, Medicare, and policy controls (see Table 2 for details). Standard errors clustered at the state level are reported in parentheses. "Mean Y" denotes the pre-treatment mean outcome among treated states. "% Effect" is calculated as (ATT / Mean Y) × 100. All outcomes are per 100,000 Medicare beneficiaries. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 6. Effects of Naloxone Coprescription Laws on Opioid Use Disorder Treatment Admissions

Admissions per 1k Medicare Beneficiaries	Total Admissions	Detoxification	Rehab/ Residential	Intensive Outpatient	Non-Intensive Outpatient
	(1)	(2)	(3)	(4)	(5)
ATT	0.62**	0.02	0.07**	0.17	0.40**
	(0.26)	(0.02)	(0.03)	(0.12)	(0.20)
% Effect	297%	69%	387%	1646%	227%
N	348	204	255	228	337
Mean Y	0.21	0.03	0.02	0.01	0.17
Covariates	✓	✓	✓	✓	✓

Notes: This table reports average treatment effects on the treated (ATT) from the doubly robust Callaway and Sant'Anna (2021) difference-in-differences estimator. The outcome is the number of opioid use disorder treatment admissions per 1,000 Medicare beneficiaries, aggregated to the state-year level and organized by service setting. Column (1) reports total admissions across all settings. Columns (2)–(5) report admissions by service setting: detoxification, rehabilitation/residential, intensive outpatient, and non-intensive outpatient, respectively. All models include demographic, Medicare, and policy controls (see Table 2 for details). Standard errors clustered at the state level are reported in parentheses. "Mean Y" denotes the pre-treatment mean outcome among treated states. "% Effect" is calculated as (ATT / Mean Y) × 100. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Data Sources: Treatment Episode Data Set—Admissions (TEDS-A) Concatenated, 2013–2023: Public Use File (SAMHSA, 2026)

Appendix A. Extra Figures and Tables

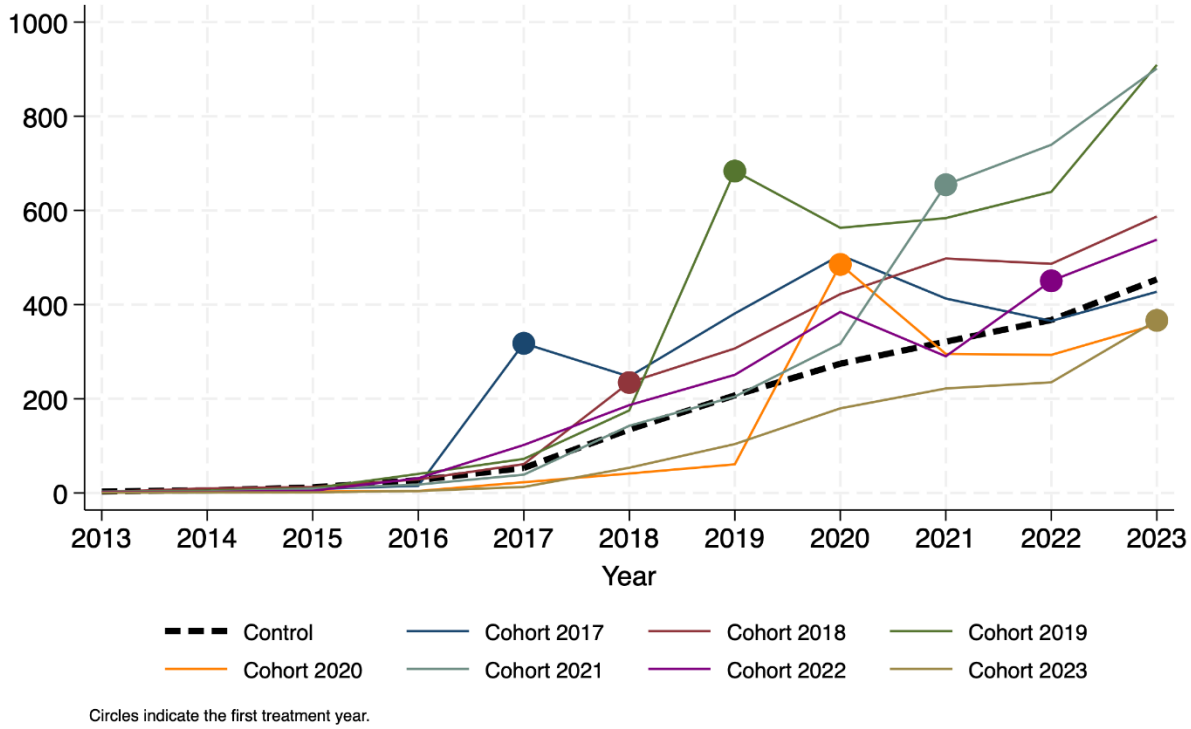
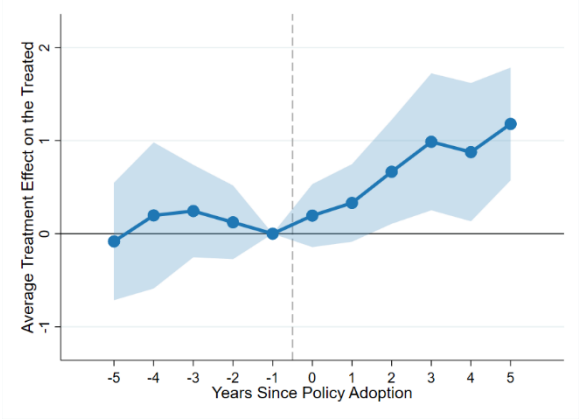


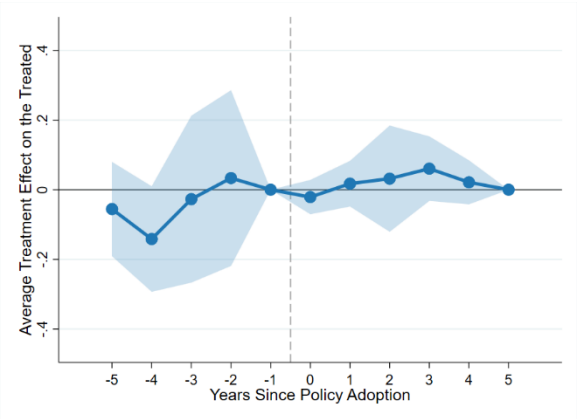
Figure A1. Naloxone Prescribing Trends in Treated and Control States (by Treatment Cohort)

Notes: This figure presents unadjusted trends in the average number of Part D naloxone claims per 100,000 Medicare beneficiaries for treatment cohorts (those that enacted naloxone coprescription law in year g) and control states (those that never enacted such laws). Circles mark each cohort's year of first law adoption.

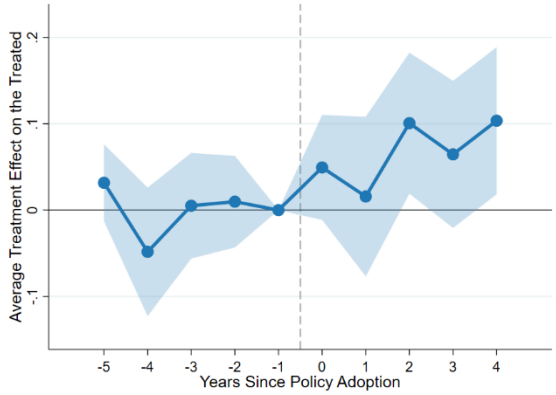
Data Sources: CMS Medicare Part D Prescribers – by Geography and Drug (2013-2023).



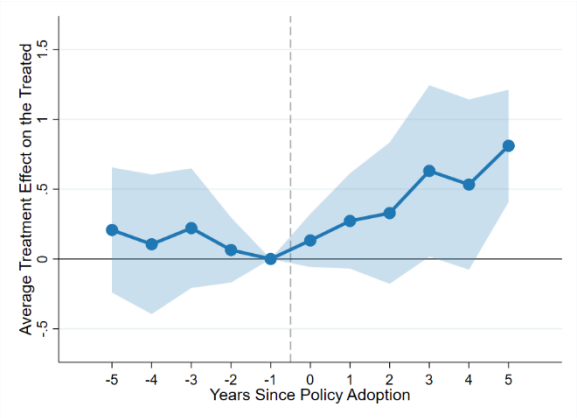
(A) Total Admissions



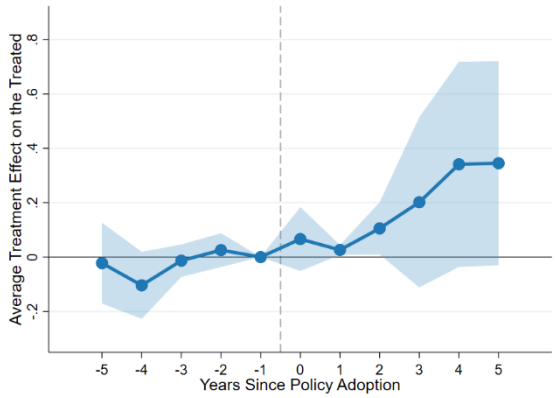
(B) Detoxification



(C) Rehab/Residential



(D) Intensive Outpatient



(E) Non-Intensive Outpatient

Figure A2. Effects of Coprescription Laws on OUD Treatment Admissions: Event Study Estimates

Notes: The figure presents event-study estimates for total OUD treatment admissions per 1,000 Medicare beneficiaries (Panel A) and for admissions disaggregated by service setting (Panels B–E). All models include the demographic, Medicare, and policy covariates described in Table 2.