Does Treating Addiction Rebuild Lives? Evidence from Buprenorphine

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Abstract

Does treating addiction improve economic circumstances as well as health? Using two decades of Veterans Health Administration records on over 140,000 patients with opioid use disorder and consumer credit data, we exploit staggered facility-level buprenorphine adoption and quasi-random emergency physician assignment. A five-percentage-point increase in the share of clinicians with buprenorphine prescribing experience doubles overdose survivors' long-term medication use and reduces two-year mortality by an estimated 3.7 percentage points. Instrumenting receipt yields a 135-point credit-score gain. At $300,000 per statistical life-year, estimated survival benefits alone exceed added medical costs within three years.

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1 Introduction

Does treating addiction rebuild lives? Medical innovation can generate substantial welfare gains (Murphy and Topel, 2006; Cutler and McClellan, 2001), but whether effective treatments also improve patients' economic circumstances is a separate question. Addiction presents a demanding case because illness and disadvantage are closely intertwined. First, medications shown to be effective in controlled trials may deliver smaller benefits in routine care, where adherence is imperfect and patients often have serious medical or psychiatric conditions that can exclude them from trials. Second, even when treatment succeeds clinically, the poverty, mental illness, and social isolation that accompany addiction may continue to hold down employment and financial stability. Whether medication improves economic circumstances despite these obstacles determines how far the benefits of expanding treatment extend beyond health and informs the role of complementary interventions addressing those disadvantages. These questions are especially pressing for opioid use disorder (OUD): more than 800,000 people died from opioid overdoses in the United States between 1999 and 2023 (Centers for Disease Control and Prevention, 2025).

We quantify the health and economic returns to expanding access to buprenorphine, a medication shown in clinical trials to improve treatment retention and reduce illicit opioid use under controlled conditions (Mattick et al., 2014). We study its delivery in routine care in the Veterans Health Administration, the largest integrated health system in the United States. The setting is well-suited to the question. Eligibility for care is stable and untied to employment, so patients can be followed with essentially no attrition. Because the Veterans Health Administration (VA) administers social programs alongside care, we observe health and economic life in the same administrative universe: every clinical encounter and prescription, complete mortality, enrollment in vocational and disability programs, and, for a subsample linked to consumer credit records, standing in the credit market. The data cover more than 140,000 patients with OUD over two decades.

We exploit two sources of quasi-experimental variation in access to buprenorphine treatment. The first is the staggered growth across facilities in the share of clinicians who prescribe buprenorphine, which until 2023 required a federal "X-waiver" that clinicians obtained and began using at very different times. We anchor this design on patients' first opioid overdose treated in a VA emergency department (ED), an objective marker of severe OUD and a moment at which treatment is often first offered. In a difference-in-differences design with

patient-level panel data, we compare each patient's change in outcomes with that of patients who overdosed at the same facility earlier or later in its prescribing expansion, and at other facilities at the same time, which identifies the policy effect of expanding treatment capacity. The second is a judge leniency design applied to physicians: within a facility and quarter, patients with OUD arriving at an ED are assigned essentially at random to physicians who differ widely and persistently in their propensity to initiate buprenorphine, and we use this propensity as an instrument for treatment receipt. The first design answers the policymaker's question, what expanding treatment capacity buys; the second answers the clinician's, what starting treatment does for the patient in front of them.

We find that expanding access to buprenorphine improves patients' economic circumstances as well as their health. The first step is treatment itself. We measure facility adoption as the caseload-weighted share of clinicians with prior buprenorphine prescribing experience. A five percentage point (pp) increase—roughly the gap between facilities at the 10th and 90th percentiles in 2018—raises patients' buprenorphine use by 10 pp in the month after an overdose. Averaged over the following two years, the increase in monthly use is 4.7 pp, roughly doubling the pre-overdose rate. Monthly engagement with substance use disorder treatment rises 26%.

The economic gains appear in both employment program participation and consumer credit records. The same expansion in facility prescribing raises enrollment in VA vocational rehabilitation and employment programs by 1.3 pp, a 65% increase from a 2% baseline. The judge leniency design extends the evidence to patients' financial lives. Among nearly 30,000 patients linked to consumer credit records, where the average score of 591 is subprime, assignment to an ED physician with a 5 pp higher propensity to initiate buprenorphine raises credit scores by about 5.5 points and increases the probability of crossing into "good" standing (a score above 670) by 4 pp from a 17% baseline. The improvement is concentrated in fewer delinquencies and collections, and patients also gain available credit. For scale, the 5.5 point reduced form effect is of the same order as the 2.4-point gain from a 7 pp increase in insurance coverage under the Massachusetts reform (Mazumder and Miller, 2016). Instrumenting buprenorphine receipt with physician propensity yields an estimated 135-point increase in credit scores (95% confidence interval roughly 18 to 253), a large effect for the high-risk patients physician assignment moves into treatment. Because veterans receive nearly free care, relief from medical bills is an unlikely channel; the gains point to a broader restoration of routine financial functioning.

These economic improvements accompany substantial estimated survival gains. The same five-point increase in clinician adoption reduces two-year mortality by an estimated 3.7 pp, a 23% reduction in a population where nearly one in six overdose survivors otherwise dies within two years. An imprecise cause-of-death decomposition suggests reductions in both drug-related and other deaths, with the latter accounting for most of the estimated decline, and other health measures move the same way: depression (PHQ-9) scores fall 11%, with no detectable increase in emergency visits or repeat overdoses. The physician-assignment design provides additional evidence: assignment to a higher-propensity physician within the same facility and quarter reduces two-year mortality by an estimated 0.6 pp from a 9% baseline and raises engagement with care. Instrumenting receipt with physician propensity yields a 13 pp reduction for patients whose treatment physician assignment shifts. That estimate applies to a high-risk group: these marginal patients would face two-year mortality of roughly 17% without buprenorphine. Both mortality estimates are imprecise, significant at the 10% level, but stable across specifications. The two designs rest on different assumptions, so their agreement does not depend on any single one.

Beyond these main results, we ask when the gains arise, for whom, and at what cost. Treatment uptake and engagement with care respond within a month of the overdose, while the survival and employment-program responses accumulate over the following two years. This sequence is consistent with treatment engagement preceding economic re-engagement. Across patients, subgroup estimates suggest the largest survival gains for those in poor physical health and the largest increases in engagement with care for those with serious mental illness, suggesting that medication is most life-saving where physical risk is highest and most valuable as a gateway to care where psychiatric illness is present. Buprenorphine use rises with little net displacement of methadone, the other main medication for OUD, so the expansion adds to medication treatment rather than reshuffling it. Finally, at a value of $300,000 per statistical life-year, our point estimates imply that survival benefits alone exceed the added medical spending over three years in the VA.

This paper contributes to the literature on the economic value of medical innovation. Foundational work quantifies the welfare gains from improvements in health and longevity (Murphy and Topel, 2006; Cutler and McClellan, 2001), while studies of specific technologies document benefits beyond clinical outcomes, including labor market effects of pain medication, antidepressant advertising, and radiotherapy (Garthwaite, 2012; Shapiro, 2022; Daysal et al., 2025). Research on technology diffusion links the adoption of effective treatments to improve-

ments in patient survival and healthcare productivity (Skinner and Staiger, 2015). Related work documents the financial costs of health shocks (Dobkin et al., 2018) and the protection provided by health insurance (Hu et al., 2018; Mazumder and Miller, 2016; Miller et al., 2021). We show that medical treatment can contribute to economic recovery from addiction: greater access to buprenorphine improves financial health as well as survival. We separately identify the effects of expanding prescribing capacity and of receiving buprenorphine, assessing both the returns to wider adoption of a medical innovation in routine care and the benefits to patients who receive the medication.

We also contribute to the literature on substance use disorder (SUD) treatment by identifying the effects of medication on economic recovery and survival.[1] Studies of treatment-facility openings and closings document effects on drug-related mortality, emergency visits, and crime (Swensen, 2015; Bullinger, Wang and Feder, 2022; Corredor-Waldron and Currie, 2022; Bondurant, Lindo and Swensen, 2018). The closest studies of expanded access to buprenorphine find that greater prescriber availability shifts patients toward medication-based treatment (Barrette, Dafny and Shen, 2023) and that expanded prescribing authority reduces population opioid mortality (Gui, 2025). We extend this evidence in two ways. On economic outcomes, we show that treatment improves patients' financial health and participation in employment programs.[2] On mortality, we identify the effect of receiving buprenorphine on patients' all-cause mortality, rather than the population effect of expanded access.[3] We exploit quasi-random assignment to emergency physicians within the same facility and quarter, accounting for shared changes in facility care that could confound estimates based on facility adoption.

The remainder of the paper proceeds as follows. Section 2 provides background on buprenorphine and its regulatory context. Section 3 describes the data, sample, and outcomes we use. Section 4 outlines our two complementary empirical strategies. Section 5 presents the main results and robustness checks. Section 6 examines dynamics, heterogeneity, and mechanisms; Section 7 turns to a cost-benefit calculation and aggregate counterfactuals.

[1]The broader economics literature on the opioid crisis (reviewed by Maclean et al., 2022) examines the epidemic's origins (e.g., Alpert et al., 2022; Schnell, 2026; Finkelstein, Gentzkow and Li, 2025; Eichmeyer and Zhang, 2022, 2023; Janssen and Zhang, 2023), evaluates supply-side interventions (e.g., Evans, Lieber and Power, 2019; Buchmueller and Carey, 2018; Soliman, 2025), and documents its economic and intergenerational costs (Laird and Nielsen, 2016; Currie, Jin and Schnell, 2019; Buckles, Evans and Lieber, 2023).

[2]Evidence on patients' economic outcomes includes education and employment following residential SUD treatment for adolescents (Kim, 2023) and employment following changes in waiting times for outpatient cannabis treatment (Williams, Bretteville-Jensen and Rose, 2026).

[3]Also related is work that examines the mortality effects of delays in mental health care among VA patients (Costantini, 2025).

Finally, Section 8 concludes.

2 Background: Buprenorphine

Buprenorphine is a semi-synthetic opioid first approved by the U.S. FDA in 2002 for the maintenance treatment of opioid use disorder. A single daily dose suppresses withdrawal and blocks the reinforcing effects of subsequent opioid use (Volkow and Blanco, 2020), and these properties place it at the center of the World Health Organization's and the Substance Abuse and Mental Health Services Administration's (SAMHSA) treatment guidelines (World Health Organization, 2009; Substance Abuse and Mental Health Services Administration, 2021). Unlike methadone, the other widely used medication for OUD, buprenorphine can be prescribed in office-based settings and dispensed in take-home supplies, sparing most patients the supervised dosing required at federally regulated opioid treatment programs and allowing treatment to scale far beyond specialized clinic capacity (Volkow and Blanco, 2020; Blanco and Volkow, 2019).[4] Yet most U.S. patients with OUD receive no treatment at all, and only about a third of those who do receive medication (Williams et al., 2018).

Prescribing and administering buprenorphine. For most of our study period, prescribing buprenorphine for OUD outside certified opioid treatment programs required a federal "X-waiver," which entailed eight hours of training for physicians (24 for nurse practitioners and physician assistants) and capped the number of patients a clinician could treat (SAMHSA, 2021). Adoption was slow and highly uneven: beyond the training cost, many clinicians were reluctant to treat addiction at all, citing stigma, lack of institutional support, and unfamiliarity with the medication (Blanco and Volkow, 2019). Within the VA, the share of clinicians who had begun prescribing buprenorphine rose at different times across facilities over two decades. This staggered, facility-level variation is the basis of our first research design.[5]

[4]Randomized trials establish buprenorphine's efficacy for treatment retention and reduced illicit opioid use, but evidence on mortality rests on few trials and very few deaths (Mattick et al., 2014). Observational studies associate buprenorphine with better health outcomes (e.g., Weiner et al., 2024; Larochelle et al., 2018). Evidence from other delivery settings links buprenorphine provided during incarceration to lower post-release recidivism (Evans, Wilson and Friedmann, 2022) and ambulance-based initiation after an overdose to greater short-term treatment engagement (Carroll et al., 2023).

[5]The training requirement was relaxed in April 2021 for eligible clinicians treating up to 30 patients, and the waiver was eliminated in January 2023; studies of both changes find little effect on buprenorphine treatment (Christine et al., 2024; Chua et al., 2024). These findings are not at odds with our study: we capture the effect of clinicians' *actual adoption* of prescribing, whereas removing a regulatory barrier may leave other obstacles to adoption in place.

Alongside the waiver regime, federal regulation provided a narrower pathway for unwaivered clinicians, known as the "3-day rule": any DEA-registered practitioner could *administer* (but not prescribe) one day's dose of buprenorphine to a patient in acute withdrawal, repeated for up to 72 hours, to stabilize the patient while arranging referral to formal treatment (U.S. Drug Enforcement Administration, 2020). The rule matters most in emergency departments, where patients in withdrawal frequently present. Whether a given ED physician uses this authority, however, is largely a matter of individual practice style: initiation rates vary widely and persistently across physicians working in the same department.

These two rules generate the variation behind our two empirical strategies. In the facility adoption difference-in-differences design, the treatment variable is the share of a facility's non-ED clinicians who have prescribed or administered buprenorphine, the clinicians able to provide sustained outpatient treatment; we exclude ED providers, whose 3-day-rule authority requires no waiver and does not reflect longer term prescribing capacity. The ED physician propensity design exploits persistent differences across ED physicians in their tendency to initiate buprenorphine, which the emergency exception makes possible: because any ED physician can administer it to a patient in withdrawal without a waiver, initiation is a matter of individual practice style, and the effectively random assignment of unplanned ED arrivals to physicians on shift turns that variation into quasi-experimental variation in receipt of the medication at the emergency visit.

3 Data

We analyze data from 2003 through 2025 from the Veterans Health Administration (VA), the largest integrated healthcare system in the United States, which provides care to more than 9 million eligible veterans through 170 medical centers and over 1,300 outpatient facilities.[6] Because eligibility is stable and untied to employment, we observe complete (within-VA) clinical and program histories, with virtually no insurance-related attrition, among the more than 140,000 veterans with OUD we study.

Our data cover every healthcare encounter delivered within the VA system (outpatient visits, inpatient admissions, and pharmacy dispensing) with structured diagnosis and procedure codes, provider and facility identifiers, and time-stamped clinical events. We additionally

[6]Eligibility includes prior active-duty service, discharge under conditions other than dishonorable, and either income-based qualification or a service-connected disability.

observe rich clinical metrics that are rarely available in claims data, such as structured mental health assessments (e.g., PHQ-9 depression scores). Beyond medical care, the data link to VA-administered social programs, including vocational rehabilitation, employment assistance, and disability compensation, allowing us to track economic and psychosocial outcomes within the same administrative universe.

To capture care veterans receive outside the VA system, we also link to Medicare Parts A, B, and D claims (2014–2019), Medicaid claims (2014–2015), and VA-reimbursed community-care records. The latter includes all emergency services delivered at non-VA facilities, which is particularly important for opioid-related health crises. Finally, we observe complete mortality data from the CDC National Death Index Plus, including date and cause of death, ensuring that long-run health outcomes are measured without loss to follow-up.

To measure patients' financial health, we additionally link veterans to consumer credit records from TransUnion, one of the three major U.S. credit bureaus. The linkage is built from a random sample of just over 300,000 veterans with an emergency department visit between 2014 and 2018, which we provided to TransUnion for matching based on veterans' social security numbers; approximately 93% of these veterans have at least one credit report record. For each such veteran, we observe a quarterly panel of credit report information for the two years before and the two years after the ED visit, including individual-level data on credit scores and on the balances, credit limits, delinquencies, and third-party collections reported across open accounts. These records provide a market-based, objectively reported summary of financial stability that complements the VA's administrative measures of economic engagement.

3.1 Sample

In our primary empirical research design, which exploits facility and time variation in provider buprenorphine adoption rates, we include all VA patients who experience a VA ED visit for an opioid overdose in the years 2004–2022. We restrict the sample to the patient's first qualifying ED opioid overdose event. The rationale for selecting overdose events is twofold. First, overdoses serve as a highly objective indicator of severe opioid use disorder, increasing the likelihood these individuals are clinically appropriate candidates for initiating buprenorphine treatment. Second, by relying on overdose rather than a previous diagnosis of OUD, we avoid endogenous selection biases related to variation across facilities and time in OUD diagnosis practices. One might worry that facility-time variation in buprenorphine adoption could itself

influence which patients present with an overdose; we find no evidence of such compositional selection (Section 5.3).

Our second strategy uses all VA patients with an ED visit between 2004 and 2022 and an OUD diagnosis in the prior year, again keeping each patient's first qualifying visit. Conditioning on a prior diagnosis rather than an overdose is possible here because the design compares patients assigned to different physicians within a facility and quarter, so facility-level variation in diagnosis practice cannot confound it, and it yields a sample nearly twenty times larger, which the IV analysis needs.

Our analysis of financial outcomes applies this second strategy to the TransUnion-linked subsample: veterans with an ED visit between 2014 and 2018, again keeping each patient's first qualifying visit and requiring a prior-year OUD diagnosis. Among the 82,889 patients meeting these criteria, 35.7% have an observed credit score and enter the financial outcomes sample.[7]

In all samples, we drop terminal cancer and end-of-life geriatric patients, as well as patients who die within seven days of the ED encounter. Our resulting samples consist of 7,517 patients for the facility buprenorphine adoption strategy, 142,880 patients for the ED provider instrumental variable strategy, and 29,567 patients for the analysis of financial outcomes. We report summary statistics for each sample in Table 1. They show that patients in the financial outcomes sample are broadly similar to the full ED-propensity sample on observable demographics and comorbidity.

3.2 Buprenorphine Treatment

We measure whether a patient is receiving buprenorphine for OUD via both prescription and administration of buprenorphine.[8] We construct monthly indicators for whether the patient fills any buprenorphine prescription or is administered buprenorphine for OUD. For our provider propensity design, we also construct a three-day (72 hour) indicator for buprenorphine initiation (via administration or prescription) from the time of the ED visit.[9]

3.3 Outcomes

We measure outcomes within two years, excluding the first seven days, following the index ED visit across three domains: medical utilization, health and mortality, and economic

[7] The facility adoption sample contains too few credit-linked overdose patients to support estimation.

[8] Specifically, we include sublingual and extended-release injection routes and exclude buccal and patch, which are typically used to treat pain and not OUD.

[9] Appendix B describes how we construct each physician's prescribing propensity from this indicator.

engagement.

Utilization Outcomes. We measure outpatient healthcare utilization across four settings: (i) overall outpatient care; (ii) substance use disorder treatment; (iii) mental health care; and (iv) physical health care. Each utilization outcome is coded as a monthly indicator equal to one if a patient has at least one relevant encounter during that month.

Health Outcomes. Our health outcomes include both acute events and intermediate or long-run measures of well-being. Acute events are captured by (i) ED visits and (ii) opioid overdose events, as measured by ED visits and hospital admissions with opioid overdose diagnosis codes. These outcomes are coded as monthly indicators. For a subset of patients, we also observe mental health through the Patient Health Questionnaire–9 (PHQ-9) depression score, a validated scale from 0 to 27, available from structured VA mental health assessments. This outcome is coded monthly as the average PHQ-9 score. Mortality is measured as the cumulative probability of death within 24 months of the index event. Together, these outcomes provide a direct assessment of patient morbidity, relapse, and survival, complementing the service use measures above.

Economic and Financial Outcomes. For economic outcomes, we observe two monthly indicators from VA administrative records: enrollment in VA vocational rehabilitation and employment programs (which provide paid job training, counseling, and placement support, and capture re-engagement with employment services) and the filing of a VA disability compensation claim, which can reflect worsening disability status or greater reliance on VA benefits. From the TransUnion records, we construct market-based measures of financial health. Our headline financial measure is the consumer credit score, analyzed both continuously and through indicators for exceeding thresholds of fair (580), good (670), very good (740), and excellent (800). The average patient in our credit sample is subprime, with a score of approximately 591. We further examine a set of supporting measures: the total borrowing limit across open accounts (excluding mortgage and home equity), the utilization rate on revolving accounts, total non-mortgage debt, and an eight-component standardized index of financial strain following Dobbie, Goldsmith-Pinkham and Yang (2017).

Outcome Alignment Across Empirical Research Designs. For the facility adoption design, which lends itself to event study analysis, we construct monthly measures of all non-mortality outcomes from 12 months before to 24 months after the index overdose event. To summarize treatment effects, we report a post-versus-pre difference-in-differences estimate averaged over

the post-treatment period, constructing the average only over those months in which the patient is alive. Similarly, for the ED physician buprenorphine prescribing propensity design, outcomes are constructed as the average of monthly outcomes over the 24 months following the index ED visit. Because the facility adoption design is not a balanced panel and is restricted to patient-months in which the patient is alive, we analogously average outcomes in the physician design only over months in which the patient remains alive. As a result, both the difference-in-differences and IV estimates can be interpreted as changes in average monthly outcomes over a 24-month post-ED period. Because treatment itself affects survival, conditioning on being alive could in principle alter the composition of the surviving sample; we assess the resulting bounds on the survival-conditioned outcomes in Appendix C.

4 Empirical Strategy

We use two complementary strategies. A dynamic difference-in-differences design exploits facility-level variation over time in physician adoption of buprenorphine prescribing, identifying the policy effect of expanding provider adoption. An instrumental-variable design exploits within-facility variation in ED physicians' propensity to initiate buprenorphine, identifying the causal effect of treatment receipt itself.

4.1 X-Waiver Adoption Across Facility-Time

We measure facility prescribing adoption from clinicians' treatment histories. For facility $s(i)$ of patient i and index quarter $t_0(i)$ (of first opioid overdose), $XWaiver_Frac_{s(i),t_0(i)}$ is the share of eligible non-ED clinicians who had prescribed or administered buprenorphine in any quarter before $t_0(i)$, weighted by the number of SUD patients each clinician sees in quarter $t_0(i)$.[10] The measure is therefore the share of a facility's SUD caseload seen by clinicians who have prescribed buprenorphine. We use this prescribing history as a proxy capturing both waiver status and prescribing initiation, since obtaining the waiver alone does not always immediately translate into prescribing (Huhn and Dunn, 2017). For brevity we refer to this prescribing-history measure as X-waiver adoption throughout, though it does not observe waiver status directly.

This classification uses prescribing histories through the quarter before the index overdose, so the patient's subsequent treatment does not enter it, and a clinician counts as an adopter

[10]Eligible clinicians treat at least 10 SUD patients in the facility-quarter. We exclude ED clinicians because emergency administration under the 3-day rule does not require an X-waiver.

after prescribing buprenorphine once, regardless of subsequent prescribing frequency. The measure thus captures prior prescribing experience among the clinicians serving the facility's SUD caseload.

Figure 1 plots our measure of provider adoption over time (the prescribing-history measure defined above) alongside the fraction of OUD patients receiving buprenorphine. The figure illustrates two points. First, there is a strong correlation between the fraction of prescribing providers and patient treatment rates, suggesting a first stage relationship. Second, while the aggregate trends in provider prescribing and patient uptake appear smoothly increasing over the 20-year period, this masks substantial heterogeneity across individual facilities. Figure A.1 demonstrates that facilities often experience discrete jumps in provider adoption, with considerable heterogeneity in both timing and intensity. This staggered pattern provides the discrete, differentially timed variation our design exploits.

With this measure in hand, we estimate the following event study specification:

$$Y_{it} = \alpha_i + \lambda_t + \sum_{k=-12,\ k\neq -1}^{24} \gamma_k \, D_{it}^k + \sum_{k=-12,\ k\neq -1}^{24} \beta_k \left(XWaiver_Frac_{s(i),t_0(i)} \times D_{it}^k \right) + \varepsilon_{it}, \quad (1)$$

where Y_{it} is outcome Y of patient i in calendar year-month t; α_i denotes patient fixed effects (which absorb facility) and λ_t denotes calendar-month fixed effects; event time is captured by the dummies D_{it}^k, which are relative to the patient's first opioid overdose ED event. We anchor the panel on the overdose event because it is an objective clinical "crisis moment" at which treatment for OUD is often first offered; aligning everyone on this event lets us follow comparable patients exactly when they become candidates for treatment. Event study coefficients β_k are the parameters of interest, which trace out the evolution of outcomes following an overdose, at facilities with higher versus lower buprenorphine prescribing.

We also estimate an aggregate difference-in-differences specification where we replace the event time dummies with a single $Post_{it}$ indicator variable. This allows us to report a simple aggregate estimate of the causal effect of buprenorphine adoption on outcomes in the two years following the ED visit, relative to the one year before.

Mortality requires a different specification. Because death is an absorbing state realized entirely in the post-period, a within-patient pre-post comparison is not possible. We therefore estimate the mortality effect cross-sectionally, regressing an indicator for death within two years of the index overdose on $XWaiver_Frac_{s(i),t_0(i)}$, with facility and overdose-quarter fixed effects. Because this specification carries no pre-trend test of its own, we subject it

to additional scrutiny, including calendar-time placebo tests around facility adoption, in Section 5.3.

Coefficients and standard errors are scaled by 0.05 (5 pp), which is roughly equivalent to the average increase in $XWaiver_Frac_{s(i),t_0(i)}$ between 2004 and 2022 (0.048), and the p90–p10 difference in the treatment variable across facilities in 2018 (0.050). This five-point change refers to the caseload-weighted share of clinicians with buprenorphine prescribing experience. It does not imply a five-point change in patients' medication use; the size of that response is estimated separately. To facilitate interpretation of estimates across the two strategies, we apply the same 5 pp scaling to the across-physician design. Heteroskedasticity-robust standard errors are clustered at the facility-(overdose) quarter level.

Identification Assumptions. Patient fixed effects absorb persistent differences in outcome levels across patients and facilities, and the difference-in-differences coefficients compare the change in outcomes around the overdose across patients exposed to different adoption levels, net of calendar-time and of the post-overdose trajectory common to all patients; the event study specification traces that comparison month by month. A causal interpretation requires parallel trends: absent differences in buprenorphine availability, these patients would have experienced similar changes in outcomes after the overdose. Two key threats could violate this assumption. First, adoption may be correlated with changes in patients' trajectories, in either direction: worsening patients may sort into facilities that are expanding prescribing, or clinicians may expand prescribing in response to deteriorating outcomes among the patients they already treat. Patient fixed effects absorb time-invariant differences across patients, but not time-varying selection of this kind. Second, buprenorphine adoption may coincide with other policy or facility-level changes, such as state opioid initiatives, Medicaid expansions, or shifts in local practice, that independently affect outcomes.

We probe these concerns in several ways, detailed in the robustness section. In brief: to address the possibility of patient composition or patient health changes driving the buprenorphine adoption rate among a facility's providers, we show in event study plots that outcomes are flat in the pre-period, and we regress patients' pre-overdose demographic and clinical covariates on facility adoption rates; both checks show no evidence of differential selection. Because these checks are aligned on patients' overdoses rather than on facilities' adoption dates, we also test, in specifications aligned on adoption dates, whether facility mortality trends differ before adoption, and find no evidence that they do (Section 5.3). To address contemporaneous state-level policy shocks, we further saturate our baseline

specification with state-by-calendar-quarter fixed effects, absorbing variation from state-level reforms that may coincide with adoption.

One could still worry that there are facility-level changes closely correlated in time with buprenorphine adoption changes (such that they would not show up in differential pre-trends) that independently influence outcomes, for example other changes to SUD treatment programs or shifts in facility resources. To convincingly rule this out, we would need to exploit variation in exposure to buprenorphine across patients that occurs *within* facility and time. This is what we do with our second empirical strategy, laid out in the next section, which leverages the quasi-random assignment of patients to ED physicians with different propensities to prescribe buprenorphine.

4.2 ED Provider Propensity Within Facility-Time

Under the 3-day rule described in Section 2, any ED clinician can administer buprenorphine to a patient requiring stabilization (e.g., in withdrawal) without holding an X-waiver. A first dose in the ED rarely ends at 72 hours: observational studies show that patients started on buprenorphine during an ED visit are far more likely to fill subsequent prescriptions and to remain in treatment, largely because ED clinicians schedule follow-up buprenorphine treatment with outpatient care (Jennings et al., 2021; Herring et al., 2024). Thus, the initial ED buprenorphine often sets patients on the path to sustained treatment with the drug, making clinician-level variation in this decision a probe of buprenorphine's downstream effects.

Motivated by the important role ED providers play in initiating medications for OUD (MOUD) treatment, and by evidence that clinicians vary in their willingness to do so (Lanham et al., 2022), we estimate each ED provider's propensity to administer or prescribe buprenorphine in emergency settings. We pair this with the quasi-random assignment of OUD patients to providers within EDs to implement a "judges' design," which identifies the causal effect of receiving buprenorphine while holding constant all facility-time-level factors. In contrast to our facility adoption design, which exploits across-facility-time variation in MOUD adoption, this within-facility design leverages variation in provider behavior, holding fixed facility-time factors. The design follows the judge leniency literature (Kling, 2006; Dobbie and Song, 2015) and its application to physician practice style (Eichmeyer and Zhang, 2022).

We detail the construction of patient i's ED provider j's buprenorphine prescribing propensity, labeled $\tilde{Z}_{ij}$, in Appendix B. Intuitively, $\tilde{Z}_{ij}$ is the leave-one-out average of

provider j's buprenorphine initiation across the *other* ED patients they treat (excluding patient i, to avoid mechanical endogeneity), residualized on time, facility, shift structure, and diagnosis, and shrunk toward the mean via empirical Bayes to limit the influence of providers observed in few encounters. This "residualized prescription rate" allows us to isolate variation in provider behavior that is plausibly exogenous to patient case mix. We then first estimate the reduced form impact of provider buprenorphine propensity on patient outcomes using Equation (2):

$$Y_i = \beta^{RF} \tilde{Z}_{ij} + \alpha_{st} + \lambda_{swd} + \gamma_{diagnosis} + \eta_i, \tag{2}$$

where α_{st} denotes facility-by-quarter fixed effects; λ_{swd} denotes fixed effects for facility-by-weekend-status-by-time-of-day[11]; and $\gamma_{diagnosis}$ captures 3-digit ICD-9 diagnosis fixed effects.

We scale β^{RF} by 0.05 (5 pp), approximately the 90th–10th percentile difference between ED providers in the same facility-year, matching the scaling of the facility adoption design.

Finally, we estimate the causal impact of receiving buprenorphine within 72 hours via two-stage least squares:

$$Y_i = \beta^{IV} Bup_i + \alpha_{st} + \lambda_{swd} + \gamma_{diagnosis} + \varepsilon_i, \tag{3}$$

$$Bup_i = \pi \tilde{Z}_{ij} + \alpha_{st} + \lambda_{swd} + \gamma_{diagnosis} + u_i, \tag{4}$$

where Bup_i is an indicator for whether patient i received buprenorphine within 72 hours of their ED visit, and where Equation (4) is the first-stage equation. In Equation (3), we instrument Bup_i with $\tilde{Z}_{ij}$.[12]

Identification Assumptions. The primary identification assumption is that, conditional on facility-by-quarter, facility-by-weekend-status-by-time-of-day, and broad diagnosis, patients are assigned to ED physicians as good as randomly. This conditional independence assumption enables a causal interpretation of the reduced form estimate in Equation (2) as the effect of being treated by a physician with a higher propensity to administer buprenorphine. It is plausible in the emergency setting because physician assignment follows rotational, shift-based staffing rather than case-based triage to particular physicians, and the acute presentation that brings a patient to the ED is unplanned, leaving little scope to sort patients to particular

[11] Time of day controls are meant to approximate ED physician shifts (7am–3pm, 3pm–11pm, 11pm–7am) and help capture variation in provider assignment.

[12] In both the reduced form and the IV, heteroskedasticity-robust standard errors are clustered at the facility-quarter level.

physicians on unobservables. Our balance checks find that high- and low-propensity physicians treat observably similar patients, on predicted treatment, predicted mortality, and a wide set of baseline covariates, in Section 5.3.

The IV interpretation additionally requires relevance, exclusion, and monotonicity. The strong first stage supports relevance, and the fact that it is monotonically increasing in prescribing propensity is consistent with individual-level monotonicity (see Section 5.2). Exclusion requires physician prescribing propensity to affect outcomes only through buprenorphine initiation and its subsequent consequences. This is plausible in emergency care, where physicians have no continuing relationship with the patient and few levers beyond the initial administration and referral to influence medium-term trajectories. We probe it by examining alternative care decisions and the sensitivity of the estimates to controlling for physician admission propensity (Section 5.3).

Financial outcomes. We apply the same design to our credit-based measures of financial health, using the reduced form and 2SLS specifications above. Because patients enter the credit sample only if they appear in the TransUnion data, a distinct concern is that selection into observation is itself related to the instrument. Reassuringly, the tests in Section 5.3 do not detect differential credit observation by physician propensity.

5 Results

We present the results in two steps. Section 5.1 traces the full set of patient outcomes (medication uptake, care engagement, survival, and economic re-engagement) through the facility adoption design, which estimates the policy effect of expanding prescribing capacity. Section 5.2 then turns to the physician propensity design, which serves two roles: because its variation lies within facility–time cells, it corroborates the adoption results through a different source of variation; and because it isolates quasi-random variation in individual treatment receipt, it delivers the effect of buprenorphine receipt for the patients whose treatment is shifted by physician assignment. Finally, its larger sample size allows us to investigate effects on financial outcomes for the credit-report-linked sub-sample.

5.1 Effects of Expanding Facility Buprenorphine Adoption

Patient-Level Buprenorphine Use. Facility prescribing adoption raises buprenorphine use among the patients who overdose there. Recall that the treatment variable, $XWaiver_Frac$,

is the caseload-weighted share of a facility's non-ED SUD clinicians who had prescribed or administered buprenorphine before the patient's index overdose quarter, and the outcome is an indicator for the patient's own buprenorphine use in each month. We report effects for a 5 pp increase in facility adoption, approximately the observed 10th–90th percentile gap across facilities in 2018. These five points refer to the caseload-weighted share of clinicians with prior prescribing experience, not the share of patients receiving medication. Figure 2 traces the resulting change in monthly buprenorphine use around the overdose. Use shows no differential trend in the twelve months before the overdose and rises by 10 pp in the month after it. The estimated increase averages 4.7 pp per month across the following two years, roughly doubling the pre-overdose mean (panel a of Table 2). This monthly average does not imply that treatment changes for only 4.7% of patients over the entire follow-up period. Different patients may receive medication in different months, and adoption may also extend treatment among existing users.

The effect on buprenorphine use is not mechanical, for three reasons. First, the treatment measure is built from clinicians' prescribing histories *before* the patient's index quarter, so the patient's own treatment after the overdose does not enter it. Second, a clinician counts as an adopter after prescribing buprenorphine at least once; they need not prescribe it routinely or offer it to overdose patients. Whether those patients receive treatment still depends on which clinicians they see, whether buprenorphine is offered, and whether they accept it. Third, patient fixed effects absorb any standing difference in use between high- and low-adoption facilities, so the coefficients capture the change in a patient's own use after the overdose, not the cross-sectional correlation between a facility's prescribing volume and its patients' use.

SUD Treatment Engagement. We then turn to our primary outcomes. Figure 3 plots the event studies for each primary outcome; Table 3 reports the aggregate estimates. We begin with engagement in substance use disorder treatment, the margin most directly tied to recovery from OUD: monthly SUD treatment engagement rises by 4.1 pp, a 26% increase over its 16% baseline, indicating that patients both enter and remain in treatment. This sustained engagement is the most plausible channel through which the downstream health and economic gains we document below are realized.

Mortality. Expanding access to buprenorphine can save lives. An expansion by 5 pp in facility-level prescribing adoption rates—comparable to the average rise in facility adoption over 2004–2022—reduces two-year mortality by an estimated 3.7 pp, a 23% reduction relative

to the baseline mortality risk of 16%. That baseline is high: nearly one in six patients who survive an opioid overdose dies within two years, reflecting both the severity of their disease and the elevated risk of a subsequent fatal overdose. Against this baseline, a 3.7 pp reduction, roughly four fewer deaths per hundred overdose patients, is substantial, though imprecisely estimated: it is significant at the 10% level, with a 95% confidence interval of [-8.0, 0.6] pp. The mortality estimate remains negative across specifications, although several robustness estimates are imprecise (Section 5.3); additionally, the physician-propensity design supplies a negative mortality estimate from a different source of variation (Section 5.2). Part of the decline plausibly reflects buprenorphine's pharmacology: by occupying opioid receptors and blunting the reinforcing effects of subsequent opioid use, sustained treatment directly lowers the risk of fatal overdose (Volkow and Blanco, 2020). As the cause of death decomposition below suggests, reductions in mortality from other causes appear to contribute as well.

The facility-adoption-based mortality effect estimate measures the survival benefit of expanding the share of providers who prescribe *any* buprenorphine by 5 pp—not the effect of treating 5 pp more patients with buprenorphine. Indeed, the five-point increase in clinician adoption raises buprenorphine use in the month after the overdose by a full 10 pp (and by an average of 4.7 pp per month over two years). The monthly average of 4.7 pp hence does not count the distinct patients whose treatment changes. That share is likely much larger, as patients may start at different times or remain in treatment longer. The physician design separately estimates the effect on patients of initiating buprenorphine (Section 5.2).

To put the magnitude in perspective, the flagship evaluations of U.S. health insurance expansions find proportional mortality reductions of 9.4% among likely-eligible low-income near-elderly adults (Miller, Johnson and Wherry, 2021) and about 3% among nonelderly adults under the 2006 Massachusetts reform (Sommers, Long and Baicker, 2014), with experimental evidence of effects of similar order among the previously uninsured (Goldin, Lurie and McCubbin, 2021). Our 23% reduction is larger in proportional terms and far larger in absolute terms, because baseline two-year mortality in our population, 16%, is many times that of even the low-income populations in those studies. The comparison is not like-for-like, since the interventions, populations, and follow-up periods differ; we offer it only to convey scale.

A second benchmark comes from the clinical literature itself. Because randomized trials

are too small to detect mortality effects,[13] the medical evidence on buprenorphine and survival rests on cohort comparisons of patients in versus out of treatment, which place the all-cause mortality hazard ratio at roughly 0.34–0.63 (Larochelle et al., 2018; Santo et al., 2021). These comparisons are correlational and, if anything, overstate protection: patients in treatment are selected for stability and engagement, and treatment spells end precisely when patients destabilize, so post-discontinuation deaths inflate the measured out-of-treatment risk. The correlational range should therefore be treated as an upper bound on the true protective effect. Relative to that upper bound, our estimate lies well inside that bound: a 3.7 pp reduction on a 16% baseline implies a hazard ratio of about 0.77, a smaller relative reduction than the selection-inflated clinical benchmark.[14]

Decomposing the survival gain by cause of death offers suggestive evidence on mechanisms (Table A.1). In the subsample with observed cause of death, two-year drug-related mortality falls by 1.0 pp on a 3% baseline and non-drug-related mortality by 1.9 pp on an 11% baseline; neither estimate is statistically significant on its own, and we treat them as indicative only. Taken at face value, the point estimates imply that buprenorphine access reduces mortality through both channels, but with different intensities: proportionally, drug-related deaths fall by about a third, roughly twice the proportional decline in non-drug-related deaths, consistent with buprenorphine's pharmacological blunting of fatal overdose risk. In levels, however, non-drug causes account for about two-thirds of the averted deaths, reflecting their dominant share of baseline mortality in this population. This pattern is plausible given the broad increases in care engagement documented in this section, and in line with evidence that patients with OUD face sharply elevated mortality from non-overdose causes as well (Larochelle et al., 2018).

The health gains extend beyond survival (Table A.2). The probability of any monthly outpatient visit rises by 2.2 pp, with mental and physical health visits up 2.3 and 2.7 pp, respectively: medication brings patients into broader medical care rather than crowding it out. This greater contact with the healthcare system does not reflect deteriorating health: there is no detectable increase in all-cause ED visits or repeat opioid overdoses, and where we observe symptoms directly, they improve: among patients with structured mental health

[13] The leading meta-analysis pools 36 correlational studies (≈750,000 patients) against just 15 trials (<4,000) (Santo et al., 2021).

[14] We use "hazard ratio" loosely. The value of 0.77 is the two-year risk ratio (treated relative to untreated cumulative mortality), whereas the cohort studies report hazard ratios. Under proportional hazards the two coincide for rare outcomes and remain close at the mortality rates here: a risk ratio of 0.77 corresponds to a hazard ratio of roughly 0.75. The distinction does not affect the comparison.

assessments, PHQ-9 depression scores decline by 1.3 points, an 11% reduction relative to the baseline of 11.7.

Economic Outcomes. The benefits extend beyond health into patients' economic lives. Enrollment in VA vocational rehabilitation and employment programs, which provide paid job training, counseling, and placement support, rises by 1.3 pp, a 65% increase over a low baseline enrollment rate of 2%. The increase foreshadows the broader economic gains, including the improvements in financial standing, that we document with the physician propensity design. As the dynamic analysis shows (Section 6.1), this economic response emerges only gradually, consistent with treatment engagement being a precondition for re-engagement with work.

5.2 Effects of Treatment Receipt and Financial Health

Our second design exploits variation orthogonal to that of the adoption design: within a given facility and quarter, which ED physician a patient happens to draw. It serves two purposes. Because its variation lies within facility–time cells, it corroborates the adoption results through a source of variation subject to different threats. As an instrumental variable design, it also delivers the effect of treatment receipt for compliers and, because the credit-linked sample overlaps only with this design, lets us trace effects into patients' financial lives.

First Stage. The first stage is strong. A 5 pp increase in an ED provider's buprenorphine prescribing propensity, roughly the 90th–10th percentile gap within a facility-year, raises the probability that the patient receives buprenorphine in the ED by 4.8 pp, an 87% jump over the 5.5% mean (panel b of Table 2), and this relationship increases monotonically across the entire distribution of physician propensity (panel a of Figure 4), consistent with the monotonicity assumption. The induced treatment persists, generating 1.4–2.2 pp higher buprenorphine use in each of the following 24 months (approximately 20% relative to the mean), suggesting that high-propensity ED providers both initiate treatment and refer patients to ongoing buprenorphine care. The Kleibergen–Paap F-statistic exceeds 125 for our preferred 3-day window, and comfortably exceeds 10 for each subsequent period within the 2-year window.

Mortality. Being assigned to a physician more inclined to start buprenorphine lowers a patient's chance of dying. Patients triaged to a physician with a 5 pp higher prescribing propensity are 0.6 pp less likely to die within two years, off a 9% baseline (SE 0.36 pp; Table 3). Instrumenting buprenorphine receipt with physician propensity scales this reduced form into the effect of treatment receipt for patients whose treatment is shifted by physician

assignment: initiating buprenorphine at or immediately after the ED visit reduces two-year mortality by an estimated 13.3 pp (SE 7.6 pp). Both estimates are significant at the 10% level. The receipt effect is larger than the facility-level effect, as expected, because the two differ in margin and population: the facility estimate averages the policy over all overdose patients, most of whose treatment does not change, whereas the receipt estimate applies only to patients induced onto medication.

The complier treatment effect is large, and its plausibility hinges on how sick compliers are: a 13 pp effect is arithmetically possible only if their untreated two-year mortality is at least that high, well above the 9% sample mean, and we verify that it is. Using Abadie (2003)'s κ-weighting to recover the complier population's potential outcome means, we estimate that compliers' counterfactual mortality absent buprenorphine is 17.2%, against 3.9% when treated; the 13.3 pp difference equals the IV estimate by construction and represents a 77% proportional reduction. A proportional reduction of this size is consistent with the instrument shifting into treatment the patients at relatively greater risk of dying from opioid use, for whom buprenorphine's protection is correspondingly large. Indeed, compliers are much more likely to have received buprenorphine previously (46% versus 13%) and to have an HIV/AIDS diagnosis (3.0% versus 1.6%; Table A.3), markers of established opioid use disorder and injection-related risk. They are also younger than the full sample (43 versus 51 years), which is another marker of higher risk: among hospitalized veterans with OUD, the odds of opioid-related death within a year fall steeply with age, from 2.4 times the reference (41–64) at ages 25–40 to 0.7 times at ages 65–80, the reverse of the pattern for all-cause mortality (Incze et al., 2025).

The untreated complier mortality closely tracks the 16% two-year baseline of the overdose population in our facility design, indicating that the marginal patients the instrument moves resemble, in underlying survival risk, the overdose patients the policy design concerns. Figure A.2 traces the treated and untreated complier mortality paths: they are indistinguishable for roughly the first six months and separate steadily thereafter, echoing the gradual mortality dynamics of the facility design and pointing to protection that accrues over sustained treatment rather than at the moment of initiation.

Estimates for SUD treatment engagement and vocational rehabilitation carry the same sign as the adoption results but are underpowered: the one-time initiation decision shifts long-run medication use far less than a sustained facility expansion, so slow-moving program outcomes are estimated with little power and on neither outcome do the two designs point

in opposite directions. Among the secondary outcomes (Table A.2), the physician design also indicates that initiating buprenorphine reduces the monthly probability of filing a VA disability compensation claim, by an estimated 1.7 pp from a 1.3% mean, consistent with improving health lowering reliance on benefits; the facility design's estimate for the same outcome is small, positive, and imprecise, so we regard the disability result as suggestive.

Financial Well-Being. The credit-bureau linkage lets us trace effects into patients' financial lives, and it is the one primary outcome we can examine only here. We build the result up in three steps.

First, the raw magnitude: a 5 pp increase in an ED physician's prescribing propensity raises a patient's average credit score over the following two years by about 5.5 points. In distributional terms this corresponds to roughly 0.067 standard deviations of the score distribution in our sample. The other available financial measures from the credit report move consistently: patients assigned to higher-propensity physicians show higher available borrowing limits and lower financial strain (Table 4). Within the strain index, the improvement is concentrated in reduced delinquencies and third-party collections, with little movement in repossessions, consistent with treatment restoring routine bill payment rather than resolving rare catastrophic events (Table A.4).[15] This effect is unlikely to be an artifact of survivorship. Because buprenorphine improves survival, the treated group's survivors include patients who would otherwise have died, and if their financial outcomes differ from other survivors', the comparison is biased, most plausibly toward zero, since the patients saved are the sickest. We address this formally with Lee (2009) bounds, which give the worst-case range of the effect under any such compositional difference. Because they require a binary treatment, we compare patients assigned to physicians above versus below the median prescribing propensity: assignment to an above-median physician raises credit scores by 3.57 points, and the Lee bounds are narrow, 3.05 to 4.21 points (Table A.5). These bounds show that survivor composition can move the estimate only slightly.

Second, the effect concentrates at the margin that gates access to mainstream credit: the same increase raises the probability that a patient crosses into "good" standing (a score of 670, the conventional threshold above which lenders extend credit on favorable terms) by 4 pp, a roughly 24% increase over the 17% baseline. That the effect is concentrated at this boundary indicates that the marginal patients are those poised to re-enter the formal credit

[15]The one component moving adversely is foreclosure, estimated on the small subsample of 863 patients with a mortgage.

market.

Third, instrumenting receipt with physician propensity yields an estimated 135-point increase in credit score for patients whose initiation is shifted by physician assignment. The point estimate is large and imprecise, and it is a local effect for marginal patients rather than an average effect of receiving buprenorphine. For scale, the reduced form effect of a 5 pp shift in physician propensity, 5.5 points, is of the same order as the 2.4-point gain from a 7 pp coverage increase under the Massachusetts reform (Mazumder and Miller, 2016). For a design benchmark, Dobbie, Goldsmith-Pinkham and Yang (2017) estimate with a similar judges design that Chapter 13 bankruptcy protection raises credit scores by about 15 points; our point estimate is far larger, though imprecise, and the studies differ in score measure, population, and treatment margin.

The mechanism behind the effects on credit scores is unlikely to operate through relief from medical bills: hospital admissions among insured nonelderly adults have been found to increase collections and reduce credit access, but move credit scores by only about 1.8 points after four years (Dobkin et al., 2018); similarly, randomized medical debt relief has limited effects on credit report outcomes (Kluender et al., 2025). Moreover, veterans in our sample all receive nearly-free medical care from the VA. That buprenorphine access nonetheless improves credit scores points to a broader restoration of financial functioning, such as steady work and regular bill payment, though our design does not isolate the channel.

5.3 Robustness

We now turn from the estimates to testing the assumptions behind them. The two designs rest on different identifying assumptions and face different threats; we summarize the checks here and report them in detail in Appendix C.

The facility adoption design identifies effects under parallel trends. Its two chief threats, contemporaneous facility-level shocks and changes in patient composition, have testable implications, and our tests show no sign of either. The estimates are stable to absorbing state-by-calendar-quarter policy variation (Column 2 of Table 5) and to replacing the continuous adoption measure with a binary adoption indicator (Column 4 of Table 5), and pre-overdose patient characteristics are unrelated to facility adoption, providing no evidence of differential selection on these observed characteristics (Figure A.3). Note that such level differences alone would not violate identification with patient fixed effects; the relevant concern is differential outcome trajectories correlated with adoption, for which the flat pre-trends provide an

additional check. Because the design applies a continuous, secularly rising treatment in a two-way fixed-effects event study, a setting in which heterogeneous effects can distort standard estimates (de Chaisemartin and D'Haultfœuille, 2020), we re-estimate the headline mortality effect with heterogeneity-robust estimators on data collapsed to the facility-by-quarter level. They yield negative point estimates that do not reject the baseline, but are imprecise (Table A.6).

Mortality warrants particular scrutiny, since as an absorbing state it is estimated cross-sectionally and, because patients must by construction survive to the index overdose, no specification in patient event time can test mortality pre-trends. The heterogeneity-robust estimators, however, operate in calendar time around facility adoption, where pre-adoption cohort mortality is defined; their pre-adoption placebo estimates show no differential trend (Table A.6). Across robustness specifications, the mortality estimate is consistently negative but imprecise (Table 5 and Table A.6), reflecting reduced identifying variation in the smaller 7,517 patient sample; the physician propensity design provides additional evidence of a mortality reduction.

Because non-mortality outcomes are measured over months in which patients are alive, estimated treatment effects could reflect changes in survivor composition. We assess this with Lee (2009) bounds for binary versions of each design, facility-level adoption versus not and above- versus below-median physician prescribing propensity (Table A.5). The bounds are narrow and remain positive for buprenorphine uptake and SUD engagement in both designs and for the credit score (which is only estimated in the physician design); only the low-baseline vocational outcome in the facility design yields a bound that includes zero.

The physician propensity design rests on quasi-random assignment of patients to ED physicians and on the exclusion restriction that a physician's prescribing propensity affects outcomes only through buprenorphine receipt. Assignment appears quasi-random: high- and low-propensity physicians treat observably similar patients (Figure 4), and, in the credit-linked subsample, neither the presence nor the timing of a credit record is related to physician propensity (Table A.7). The exclusion restriction is more demanding, since high-propensity physicians might differ on other margins of care. The instrument does not predict referrals, psychiatric consultations, methadone, or opioid analgesic prescribing; it does predict naloxone prescribing, which is routinely co-prescribed with buprenorphine and small relative to the other margins, and inpatient admission (Table C.1). To check whether admission is driving the results, we construct and control for each physician's admission propensity, which leaves

all four primary outcome effects essentially unchanged (Table A.8).

6 Dynamics, Heterogeneity, and Mechanisms

The baseline estimates indicate that greater buprenorphine capacity improves survival, mental health, and economic re-engagement. To understand when these gains unfold, for whom they arise, and how, we next examine the timing of responses, heterogeneity, and possible substitution from other medication.

6.1 Dynamics

The timing of the effects bears on two questions. The first is adherence: whether a response triggered at the moment of crisis persists once the crisis passes. The second is mechanism: if the downstream gains run through sustained engagement with treatment, the treatment response should come first and the survival and employment-program responses should build over time.

These questions call for different summaries. For treatment use and engagement with care, the response is largest at the start, and the monthly estimates show how much of it persists (Figure 3, Table 2). For effects on survival and employment-program participation, which rise gradually, the question is how quickly the response approaches its two-year level. We therefore express the latter two effects relative to their month-24 estimates in Figure 5.

The dynamics show an immediate and sustained treatment engagement response followed by longer-term survival and employment-program gains. A 5 pp increase in facility-level adoption raises buprenorphine use by 10 pp in the month after the overdose, with SUD treatment engagement rising alongside it. The medication-use response diminishes thereafter: the estimated increase averages 5.6 pp in months 1–6, 3.1 pp in months 7–12, 2 pp in months 13–18, and 1.6 pp in months 19–24 (Table 2). A sizable response thus remains in the final six months: the point estimate retains approximately 16% of the initial increase, although it is imprecise. This decline need not reflect waning engagement alone. Because facility-level adoption is measured at the index quarter, exposure differences narrow over the window as lower-adoption facilities catch up, and the composition of surviving patients shifts.[16]

The survival and employment-program responses develop more gradually than the initial increase in treatment use. By month 6, the estimated reduction in cumulative mortality

[16]Monthly non-mortality outcomes are measured among surviving patients. Appendix C reports bounds assessing sensitivity to changes in survivor composition.

is approximately 43% of its month-24 value, while the increase in monthly vocational rehabilitation participation is approximately 70% of its month-24 value (Figure 5).

Two conclusions follow. First, the response to the crisis moment is not transient. Medication use and engagement with care remain elevated two years after the overdose, though at a fraction of their initial level. Thus, the adherence concern raised in the introduction is a matter of degree: engagement attenuates but does not disappear. Second, the sequence is consistent with treatment engagement facilitating economic re-engagement, with treatment responding first and survival and employment-program gains following over the subsequent two years.

The timing also matters for valuation. The costs of expanded adoption arrive mostly in the first year while the survival benefits build over time, which is why the cost-benefit calculation in Section 7.1 turns positive only in the third year.

6.2 Heterogeneity

Medications for OUD may not translate into broad and sustained life improvements for everyone. Even if adherence is achieved, health gains may not automatically spill over into economic or psychosocial domains, especially for patients burdened by severe co-morbidities or serious mental illness. To probe these possibilities, we examine whether the effects of buprenorphine access differ systematically across patient subgroups defined by their prior health status and treatment history.

We consider four natural margins of heterogeneity, each intended to capture a distinct dimension of baseline patient risk. First, we split patients by whether their number of prior Elixhauser comorbidities is above or below the median, using this as a summary measure of their overall physical health. Second, we separate patients by the presence or absence of a serious mental illness diagnosis (such as schizophrenia, bipolar disorder, or major depressive disorder) in the prior year, to test whether psychiatric conditions influence patients' ability to benefit. Third, we distinguish between patients who did or did not receive buprenorphine treatment in the year prior to their overdose, using this as a proxy for OUD severity and prior engagement with treatment. Fourth, we split patients by whether they are of working age. For each subgroup, we re-estimate our facility-level adoption design, and present the results in Figure 6.

For medically sick patients, the return to medication appears to be survival rather than economic re-engagement. Patients with above-median Elixhauser scores have larger estimated

survival gains from buprenorphine access but smaller estimated increases in vocational rehabilitation participation than healthier patients, though the difference in mortality effects is imprecisely estimated (interaction p-value 0.11). The smaller economic response is likely a matter of age: patients with more comorbidities are also older, and for many of them returning to work is no longer realistic, which is what the age split below shows.

For patients with serious mental illness, medication appears to work mainly as a gateway to care. Their estimated increases in engagement with medical care are the largest of any group: outpatient visits and both mental and physical health encounters rise more sharply once buprenorphine is available. Their estimated survival improvement is smaller than for patients without serious mental illness, plausibly because a larger share of their baseline mortality risk runs through causes, such as suicide and chronic disease, that medication for OUD addresses only indirectly.

Prior buprenorphine users, patients who had already initiated treatment at some point before their overdose, respond strongly across every margin. They show higher gains in outpatient engagement, more pronounced survival benefits, and a larger boost in vocational rehabilitation enrollment, despite a smaller increase in buprenorphine uptake itself (since many were already on the medication). Because prior users are, by construction, patients already engaged with treatment, this pattern admits two readings: larger treatment effects for patients already connected to care, or favorable selection into prior use; we do not distinguish them here. This group's uptake response is smallest while its outcome responses are largest, suggesting a large effect per unit of induced treatment.

Finally, the age split sharpens the interpretation of the economic effects and helps disentangle the comorbidity results above, since worse physical health and older age are strongly correlated in our sample. The vocational rehabilitation response is concentrated largely among patients of prime working age (we split at 55, the upper bound of the standard 25–54 prime working age definition), at 1.8 pp versus 0.7 pp for patients 55 and older (panel (d) of Figure 6), though the difference between age groups is not statistically distinguishable. The pattern is consistent with enrollment in employment programs reflecting genuine movement toward work among those for whom work is a feasible margin, rather than an artifact of program reclassification. Survival gains, by contrast, are larger among older patients, indicating that the life-saving and economic margins of buprenorphine operate on partially distinct subpopulations.

These findings speak to the motivating question of whether and for whom medical

innovation translates into real-world improvements. Taken together, the heterogeneity results show that buprenorphine's benefits are not evenly distributed. They appear most life-saving for those in poor physical health, largest for care engagement among those with serious mental illness, most broad-based for those who had prior exposure to treatment, and, on the economic margin, operative chiefly among those of working age.

6.3 Substitution from Methadone

Buprenorphine and methadone are the two most widely used evidence-based, first-line treatments for opioid use disorder. It is therefore natural to ask whether expanding access to one therapy simply displaces patients from the other; answering this question clarifies whether the benefits we estimate reflect a reallocation of existing patients across medications or extensive margin responses of engaging new patients in MOUD treatment. To do so, we re-estimate our baseline facility adoption empirical design for the outcome of a patient receiving methadone treatment in a given month.

The estimated effect on methadone use is 0.6 pp and statistically indistinguishable from zero (Table A.9). The 95% confidence interval excludes a net decline larger than approximately 0.4 pp, less than a tenth of the 4.7 pp increase in buprenorphine use. Thus, substitution from methadone can account for at most a small share of the gain in buprenorphine treatment. Similarly, assignment to an ED physician with a higher buprenorphine prescribing propensity has no detectable effect on emergency methadone treatment or referral to an opioid treatment program at the index visit (Table C.1). The improvements we document therefore come largely from drawing new patients onto evidence-based medication rather than reshuffling existing patients across therapies.

7 Policy Implications

7.1 Cost-Benefit Analysis

We conduct a cost-benefit calculation that compares the value of the estimated survival gains with the medical and medication costs of expanding buprenorphine prescribing. We consider the same five-point increase in the caseload-weighted share of clinicians with prior buprenorphine prescribing experience, approximately the gap between facilities at the 10th and 90th percentiles in 2018. We measure the gains in life-years, the extra time that patients live within each period, and put a dollar value on each life-year. We use $300,000 per

statistical life-year as our central benchmark, informed by the life-year valuations in Murphy and Topel (2006).[17] This asks whether survival alone can justify the extra spending, before placing any value on improvements in patients' wellbeing or economic circumstances. All amounts are averages over all overdose patients in the sample. They add up costs or benefits over the first one, two, or three years after the overdose.

For a 5 pp increase in facility prescribing adoption, the estimated additional cost over three years is $8,087 per overdose patient: $7,572 in medical spending and $515 in buprenorphine. The estimated survival gain is 0.0347 life-years per patient, worth approximately $10,400 at the $300,000 valuation. The resulting net benefit is approximately $2,300 per patient.[18] The balance is less favorable over shorter horizons: most added costs arise in the first year, while survival benefits accumulate over time, leaving net benefits negative after one and two years. Table 6 reports the results for each time horizon.

The conclusion depends on the value assigned to an additional year of life. Over three years, benefits and costs break even at approximately $233,000 per life-year. At $200,000, costs exceed benefits by $1,148 per patient; at $400,000, benefits exceed costs by $5,792. Discounting future costs and benefits at 3% annually reduces the net benefit at the $300,000 valuation to approximately $1,900.

For perspective, our estimated cost of $233,000 per life-year gained is similar in magnitude to the approximately $220,000 in direct budgetary costs per life-year gained from Medicaid expansions estimated by Wyse and Meyer (2025), expressed in 2024 dollars.[19] Their calculation includes projected gains over patients' remaining lifetimes, while ours counts only life-years gained within three years of the overdose. The comparison therefore provides a benchmark for scale rather than a ranking of the interventions.

The positive balance is subject to uncertainty in the estimated survival gains and spending; we do not report a confidence interval for net benefits. For simplicity and clarity, the calculation also omits some costs and benefits. Costs exclude the expense of increasing adoption itself, such as clinician training. At the central valuation, our point estimates leave approximately $2,300 per overdose patient to cover such implementation costs within the three-year calculation. Benefits exclude survival gains beyond the three-year horizon.

[17] Murphy and Topel (2006) use a baseline value of $373,000 for a life-year for a 50-year-old man, expressed in 2004 dollars. Our $300,000 benchmark is expressed in 2024 dollars, and we examine sensitivity to lower and higher valuations below.

[18] Appendix D describes the construction of these figures.

[19] Their reported estimate is $179,000 in 2019 dollars; we convert it to 2024 dollars using the annual-average CPI-U.

Improvements in depression and participation in vocational rehabilitation and employment programs also remain unvalued.

7.2 Aggregate Counterfactuals

We use the above mentioned cost and benefit estimates to illustrate what wider buprenorphine adoption could mean for patients who have overdosed.[20] We first consider one future year of overdose patients in the VA, taking a cohort of 500 patients.[21] If the estimated per-patient effects and costs apply to this cohort, a 5 pp increase in facility prescribing adoption would imply approximately 19 fewer deaths over the following two years and $1.2 million in net benefits over three years, valuing each additional life-year at $300,000 (Table D.1).

We also ask what the same expansion could have achieved for the overdose patients treated over our study period. Across the 7,517 patients in our facility-adoption sample, a 5 pp increase in prescribing adoption implies approximately 278 fewer deaths within two years and 261 life-years gained over three years. At $300,000 per life-year, the three-year survival benefits amount to $78.2 million, compared with $60.8 million in added medical and buprenorphine costs.

These calculations illustrate the stakes of wider adoption: under our central assumptions, bringing an existing medication to more patients could yield substantial survival gains whose value exceeds the additional medical spending.

8 Conclusion

Medical treatment can contribute to economic recovery from addiction. Our evidence from the Veterans Health Administration shows that expanding access to buprenorphine produces benefits in routine care. A 5 pp increase in prescribing adoption raises treatment uptake and engagement with care and reduces two-year mortality by an estimated 23% in a population where one in six patients otherwise dies. The benefits extend beyond health: participation in employment programs rises, and the physician-assignment design provides evidence of improved financial health. These findings show that the disadvantages accompanying addiction need not prevent economic gains from treatment. At a value of $300,000 per statistical life-year, our point estimates imply that survival benefits alone exceed the added medical spending over three years.

[20] All calculations use the facility-adoption-based causal effect point estimates and inherit the uncertainty in the estimated survival gains and spending effects.

[21] During 2020–2024, approximately 490 VA patients per year presented to an ED with an opioid overdose.

The benefits develop over time. Treatment use and engagement with care rise promptly, while gains in survival and employment-program participation emerge more gradually. This pattern is consistent with treatment engagement supporting economic recovery. Whether complementary social and employment supports can strengthen that recovery remains an important question. Our findings also highlight the role of adoption in realizing the value of medical innovation. Studies of relaxed training requirements and X-waiver elimination find limited growth in the number of patients receiving treatment (Christine et al., 2024; Chua et al., 2024); our estimates show the potential gains when expanded prescribing capacity brings patients into care. Policies that help clinicians incorporate buprenorphine into routine practice, through training, institutional support, and reimbursement, may therefore be important complements to regulatory reform.

Several caveats apply. The estimates come from an older, predominantly male veteran population treated in an integrated health system; our economic measures include credit records and program participation, but not observed employment or earnings; and the physician-design estimates apply to patients whose treatment physician assignment shifts, who are at higher risk than the average patient with OUD. The estimates also capture only effects on the patient. Treatment may generate additional benefits for spouses and intergenerational impacts through children and other family members. Finally, the size of the estimated gains makes clinician adoption itself an important object of study: identifying which barriers matter most, and which policies actually increase prescribing, is the next step in converting clinical efficacy into recovery from one of the deadliest public health crises in U.S. history.

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Tables

Table 1: Summary Statistics

	Sample: X-Waiver (1)	ED Propensity (2)	Credit Scores (3)
Female	0.11	0.08	0.08
Race: Black	0.22	0.23	0.25
Race: Other	0.03	0.02	0.03
Race: White	0.69	0.70	0.69
Ethnicity: Hispanic	0.06	0.06	0.06
Age	53.88	50.90	52.00
Age: 18-34	0.17	0.17	0.18
Age: 35-49	0.17	0.23	0.18
Age: 50-64	0.39	0.45	0.46
Age: 65+	0.26	0.15	0.18
Number of Prior Year Elixhauser Comorbidities	3.37	2.83	3.15
Number of Patients	7,517	142,880	29,567

Notes: This table presents summary statistics for the three analysis samples. Column (1) includes patients' first qualifying opioid-overdose visits to VA emergency departments in 2004–2022. Column (2) includes first qualifying VA ED visits in 2004–2022 among patients with a prior-year OUD diagnosis. Column (3) restricts the latter sample to visits in 2014–2018 with linked TransUnion credit scores. Entries are means, except the final row, which reports patient counts. Age is measured at the index visit; Elixhauser comorbidities are measured over the preceding year.

Table 2: Effects of Buprenorphine Access on Patient Buprenorphine Use

	Dependent variable: Buprenorphine Treatment Receipt					
			Event Month:			
	3 Day	2-Year	1-6	7-12	13-18	19-24
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Effect of Facility Buprenorphine Adoption Rate						
Scaled by $\times 0.05$						
X-Waiver Adoption	—	0.047***	0.056***	0.031***	0.020**	0.016
		(0.008)	(0.008)	(0.009)	(0.010)	(0.011)
Mean Dep. Var.	—	0.050	0.050	0.050	0.050	0.050
Observations	—	252,838	252,838	252,838	252,838	252,838
Panel B: Effect of ED Physician Propensity						
Scaled by $\times 0.05$						
ED Physician Propensity	0.048***	0.016***	0.022***	0.014***	0.014***	0.015***
	(0.004)	(0.003)	(0.004)	(0.004)	(0.004)	(0.004)
Mean Dep. Var.	0.055	0.082	0.092	0.081	0.078	0.076
Observations	142,880	142,880	142,880	142,880	142,880	142,880

Notes: This table reports the effects of expanded buprenorphine access on treatment initiation and subsequent use. Panel A uses difference-in-differences regressions interacting facility adoption with a post-overdose indicator, as well as patient and calendar-month fixed effects and a post-overdose dummy (Section 4.1). Panel B regresses buprenorphine receipt on the assigned physician's prescribing propensity, with facility-by-quarter, facility-by-weekend-status-by-time-of-day, and three-digit ICD-9 diagnosis fixed effects (Section 4.2). Samples are defined in Table 1; observations are patient-months in Panel A and patients in Panel B. Estimates correspond to 5 pp increases in adoption or propensity. Column (1) is receipt within 72 hours, the physician design's IV first stage ($F = 125.3$; credit-linked sample: coefficient 0.040, SE 0.008, $F = 23.4$; Kleibergen–Paap statistics). Column (2) reports the pooled two-year estimate; columns (3)–(6) report six-month estimates, averaging event study coefficients in Panel A and monthly receipt in Panel B. Monthly receipt is measured while patients are alive. Panel A means refer to month -1; Panel B means refer to each column's dependent variable (its mean buprenorphine use in the month prior to the ED visit is 0.072). Standard errors, clustered at the facility-by-index-visit-quarter level, are in parentheses. $^{*}p < 0.10$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table 3: Effects of Buprenorphine Access on Primary Outcomes

	Health		Economic	
	Mortality, 2Y	SUD Treatment	Vocational Rehab. & Employment	Credit Score
	(1)	(2)	(3)	(4)
Panel A: Effect of Facility Buprenorphine Adoption Rate				
Reduced form, scaled by $\times 0.05$				
X-Waiver Adoption	$-0.037^{*\dagger}$	0.041^{***}	0.013^{***}	—
	(0.022)	(0.008)	(0.004)	
Mean Dep. Var.	0.16	0.16	0.02	—
Observations	7,517	252,838	252,838	—
Panel B: Effect of ED Physician Propensity				
Reduced form, scaled by $\times 0.05$				
ED Physician Propensity	-0.0063^{*}	0.0028	0.0002	5.47^{**}
	(0.0036)	(0.0036)	(0.0014)	(2.25)
IV effect of ED buprenorphine initiation				
Buprenorphine	-0.133^{*}	0.059	0.005	135.43^{**}
	(0.076)	(0.075)	(0.030)	(59.95)
Mean Dep. Var.	0.09	0.20	0.04	590.75
Observations	142,880	142,880	142,880	29,567

Notes: This table reports estimated effects of buprenorphine access and receipt on health and economic outcomes. Panel A estimates effects of expanded facility prescribing capacity (Section 4.1). Non-mortality regressions interact adoption with the post-overdose indicator and include patient and calendar-month fixed effects and a post-overdose dummy. †Mortality instead uses a patient-level regression of two-year death on adoption, with facility and overdose-quarter fixed effects. Panel B reports reduced form estimates from regressions on the assigned ED physician's prescribing propensity and IV estimates instrumenting receipt within 72 hours with that propensity; both include facility-by-quarter, facility-by-weekend-status-by-time-of-day, and three-digit ICD-9 diagnosis fixed effects (Section 4.2). Facility and reduced form estimates are scaled to 5 pp increases; IV receipt effects are unscaled. Samples are defined in Table 1; the designs concern different populations and treatment margins. Non-mortality outcomes summarize two-year follow-up while patients are alive, using monthly clinical/program records and quarterly credit records. Outcomes exclude the first seven days. Panel A means refer to month -1, except mortality, which is the two-year death rate; Panel B means are dependent-variable means. Observations are patient-months for Panel A non-mortality outcomes and patients otherwise. Standard errors, clustered at the facility-by-index-visit-quarter level, are in parentheses. $^{*}p < 0.10$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table 4: Physician Propensity Design: Impacts on Credit Report Outcomes

	Credit Score Thresholds				Other Financial Outcomes			
	Fair	Good	Very Good	Excellent	Borrowing Limit	Utilization Rate, %	Financial Strain	Total Debt
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Reduced form, scaled by $\times 0.05$								
ED Physician Propensity	0.018	0.040***	0.005	0.005	2,698***	0.822	−0.044**	405.6
	(0.013)	(0.010)	(0.006)	(0.004)	(1,023)	(1.672)	(0.020)	(702.1)
Mean Dep. Var.	0.437	0.166	0.075	0.024	23631	51.60	-0.03	18459
Observations	29,567	29,567	29,567	29,567	19,363	14,291	29,567	14,920

Notes: This table reports reduced-form estimates for patients' financial outcomes. Each outcome is regressed on the assigned physician's prescribing propensity, with facility-by-quarter, facility-by-weekend-status-by-time-of-day, and three-digit ICD-9 diagnosis fixed effects (Section 4.2). The sample comprises patients with first ED visits in 2014–2018 who have a prior OUD diagnosis and linked credit records. Estimates correspond to a 5 pp increase in propensity, approximately the within-facility-year 90th–10th percentile gap. Columns (1)–(4) use credit-score thresholds of 580, 670, 740, and 800. For each threshold, the outcome indicates whether the patient's average credit score over the two-year follow-up exceeds that threshold. Columns (5)–(8) report borrowing limits excluding mortgages and home equity, revolving-credit utilization, financial strain, and non-mortgage debt. Limits and debt are in dollars; utilization is in percent. The strain index averages eight components standardized within calendar year across all ED patients, following Dobbie, Goldsmith-Pinkham and Yang (2017); higher values indicate greater strain. Outcomes cover two years after the visit; sample sizes vary with availability. Standard errors, clustered at the facility-by-ED-visit-quarter level, are in parentheses. $^{*}p < 0.10$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table 5: Facility-Level Buprenorphine Adoption Design: Robustness and Alternate Specifications

	Baseline Spec. ×0.05 (1)	State-by -Quarter ×0.05 (2)	Baseline Spec. ×0.034 (3)	Binary Adoption (4)	Facility Time Trend ×0.05 (5)	With Controls ×0.05 (6)
Outcome:						
Buprenorphine	0.047***	0.050***	0.032***	0.028***	0.055***	-
	(0.008)	(0.008)	(0.005)	(0.007)	(0.008)	
SUD Treatment	0.041***	0.045***	0.028***	0.026***	0.051***	-
	(0.008)	(0.008)	(0.006)	(0.008)	(0.009)	
Mortality, 2Y	−0.037*	−0.035	−0.025*	−0.022	−0.020	−0.032
	(0.022)	(0.039)	(0.015)	(0.017)	(0.030)	(0.022)
Voc. Rehab. Emp.	0.013***	0.010***	0.009***	0.008***	0.007*	-
	(0.004)	(0.004)	(0.002)	(0.003)	(0.004)	

Notes: This table examines the sensitivity of the facility-adoption estimates to alternative specifications (Appendix C.1). Column (1) repeats the baseline regressions in Section 4.1. Column (2) introduces state-by-calendar-quarter fixed effects. For ease of interpretation, Column (3) rescales the baseline to a 3.4 pp increase in adoption, equal to the average jump in $XWaiver_Frac$ using the binary adoption definition applied in column (4). Column (4) uses a binary adoption indicator, defined in Appendix C.1. Column (5) adds facility-specific linear trends; column (6) adds baseline covariates to the mortality regression. Columns (1), (2), (5), and (6) are scaled to a 5 pp increase; column (4) is an unscaled binary contrast. Standard errors, clustered at the facility-by-overdose-quarter level, are in parentheses. $^{*}p < 0.10$; $^{**}p < 0.05$; $^{***}p < 0.01$.

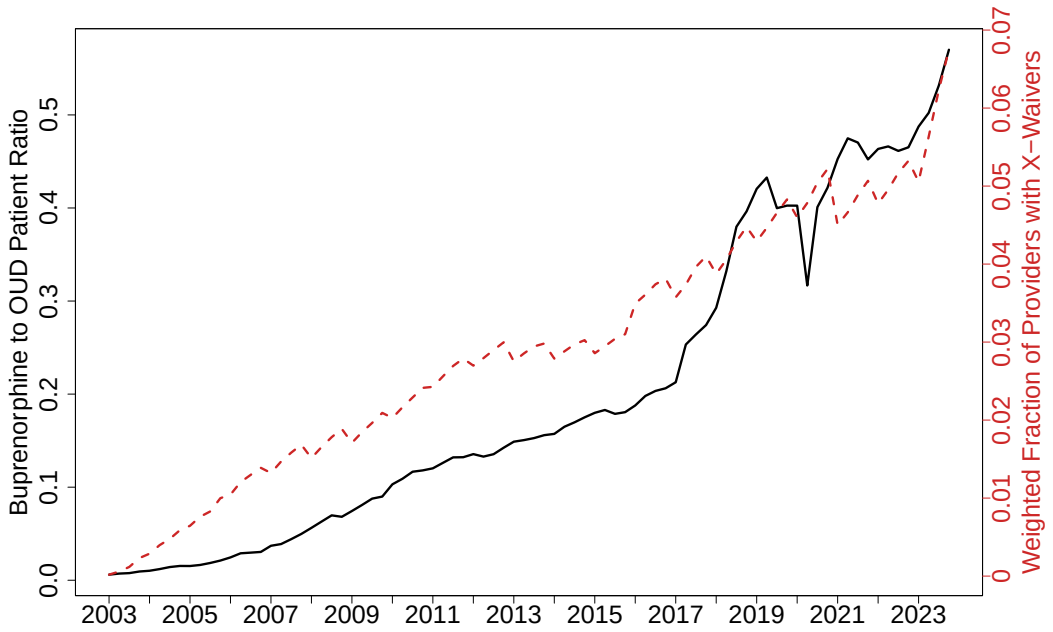
Table 6: Cost-Benefit Analysis

		Cumulative Time Horizon:		
		1 year	2 years	3 years
		(1)	(2)	(3)
Costs:				
Medical Spending		7,752	8,513	7,572
Buprenorphine		306	440	515
Total		8,058	8,953	8,087
Benefits:				
Life-Years Saved		0.0112	0.0192	0.0347
Net Benefit:	VSLY: 200K	−5,824	−5,110	−1,148
	VSLY: 300K	−4,706	−3,189	2,322
	VSLY: 400K	−3,589	−1,267	5,792

Notes: This table compares the added costs of a 5 pp increase in facility prescribing adoption with the value of the estimated survival gains (Section 7.1). Amounts are cumulative averages per overdose patient over one, two, and three years, whether or not the patient's treatment changes. Costs include VA medical spending excluding buprenorphine, which is valued separately; spending after death is zero. Survival gains are life-years gained within each horizon, calculated from the estimated mortality path (Appendix D). Net benefit equals life-years times the value of a statistical life-year (VSLY), minus added costs. Estimates are undiscounted 2024 dollars, calculated from unrounded values. Lower cumulative added medical spending at year three reflects an estimated spending reduction during that year. Costs of inducing adoption and benefits beyond survival within each horizon are excluded.

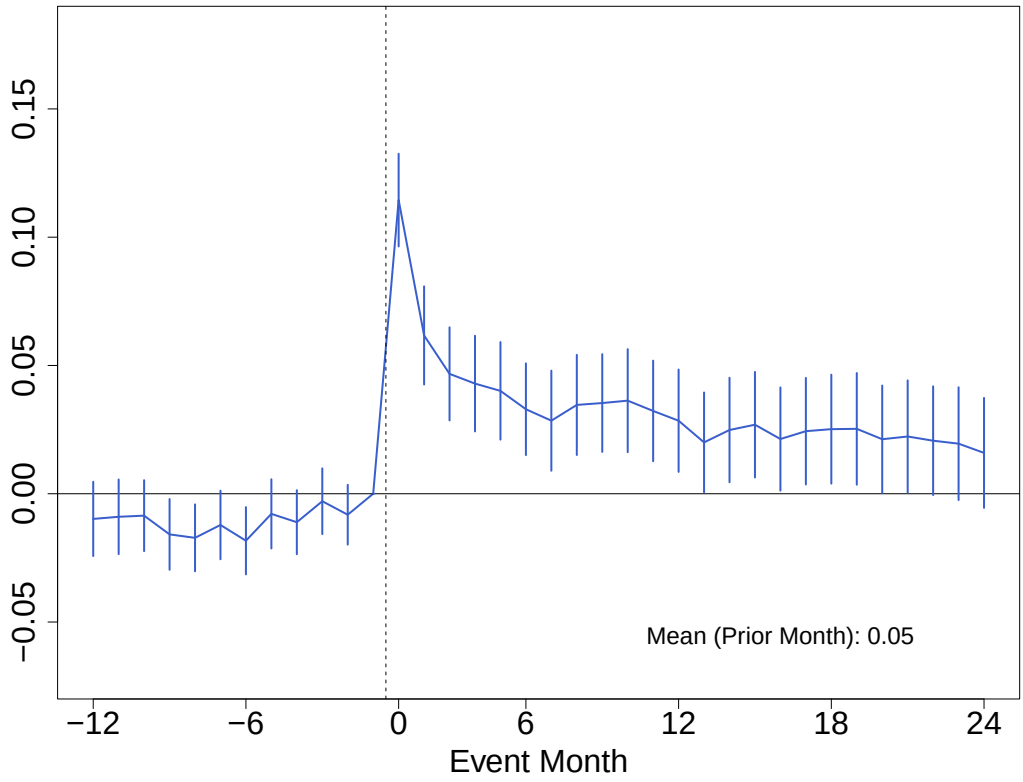
Figures

Figure 1: Patient Buprenorphine Use and Facility-Level Prescribing Adoption in the VA



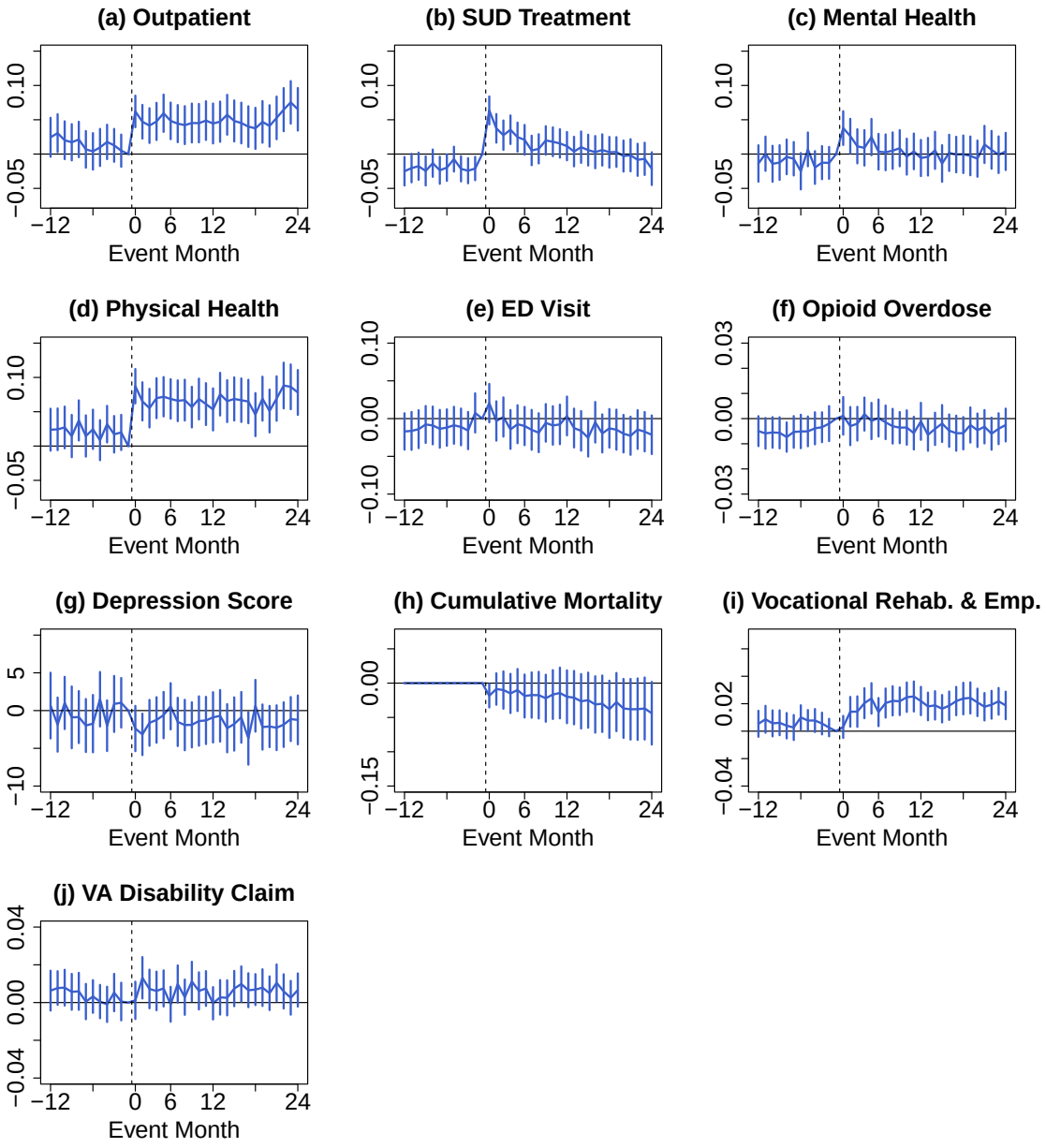
Notes: This figure shows quarterly buprenorphine treatment and prescribing adoption in the VA, 2003–2023. The solid black line (left axis) is the number of patients receiving buprenorphine at least once in the quarter divided by the number with a prior-year OUD diagnosis. The dashed red line (right axis) is the patient-volume-weighted share of eligible non-ED SUD clinicians with prior buprenorphine prescribing or administration (Section 4.1). Adoption is measured from prescribing histories.

Figure 2: Buprenorphine Treatment



Notes: This figure plots estimated effects of expanded facility prescribing capacity on patients' monthly buprenorphine use around an opioid overdose. Regressions interact adoption with event-time indicators and include patient, calendar-month, and event-time fixed effects (Section 4.1, Equation (1)). The sample comprises 7,517 patients with a first qualifying opioid-overdose ED visit in 2004–2022. Estimates correspond to a 5 pp increase in adoption, approximately the across-facility 90th–10th percentile gap in 2018. Month 0 is the overdose month; month −1 is the reference period. The pre-overdose monthly mean is displayed. Intervals are 95% confidence intervals, with standard errors clustered at the facility-by-overdose-quarter level.

Figure 3: Facility-Level Buprenorphine Adoption Design: Event Study Plots

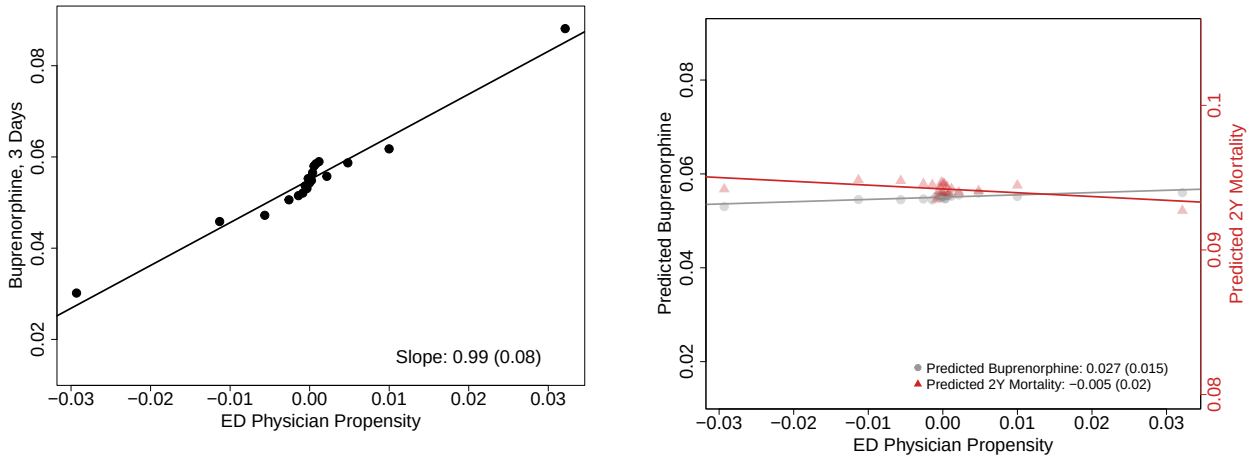


Notes: This figure shows the timing of estimated health and economic responses to expanded facility prescribing capacity. Based on monthly patient-level outcomes, regressions interact adoption with event-time indicators and include patient, calendar-month, and event-time fixed effects (Section 4.1, Equation (1)); observations are restricted to months when patients are alive. Estimates correspond to a 5 pp increase in adoption. Month 0 is the index overdose month; month −1 is the reference period. The mortality panel reports effects on cumulative death by each horizon, estimated with facility and overdose-quarter fixed effects. Intervals are 95% confidence intervals, with standard errors clustered at the facility-by-overdose-quarter level.

Figure 4: Physician Propensity Design: First Stage and Balance

(a) First Stage: Buprenorphine

(b) Balance: Predicted Buprenorphine and Mortality

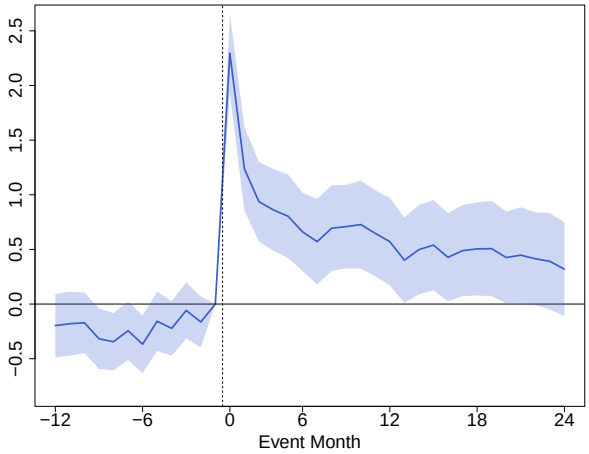


Notes: This figure presents the first stage and a covariate-balance check for the physician-propensity design (Section 4.2). Panel (a) plots buprenorphine receipt within 72 hours against the assigned physician's prescribing propensity. Panel (b) plots predicted receipt and predicted two-year mortality against the same propensity; predictions come from OLS regressions on the baseline characteristics in Figure C.1. Plotted relationships adjust for facility-by-quarter, facility-by-weekend-status-by-time-of-day, and three-digit ICD-9 diagnosis fixed effects. Fitted slopes and standard errors clustered at the facility-by-ED-visit-quarter level are displayed.

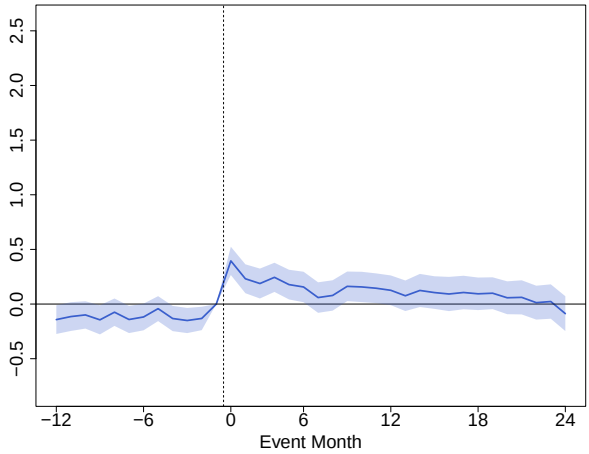
Figure 5: Dynamics: Timing of Health and Economic Gains

Top Panel: Fraction of Baseline

(a) Buprenorphine

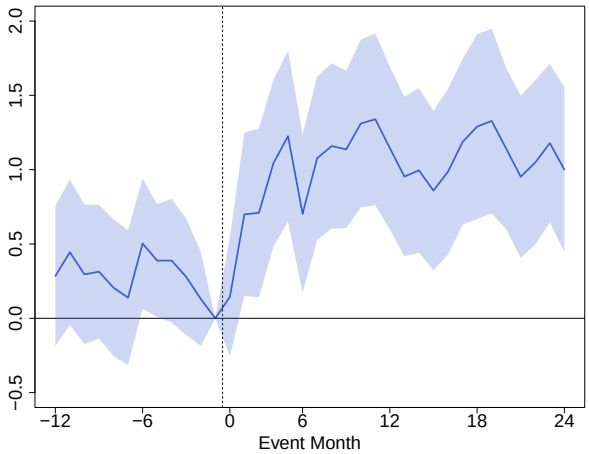


(b) SUD Treatment

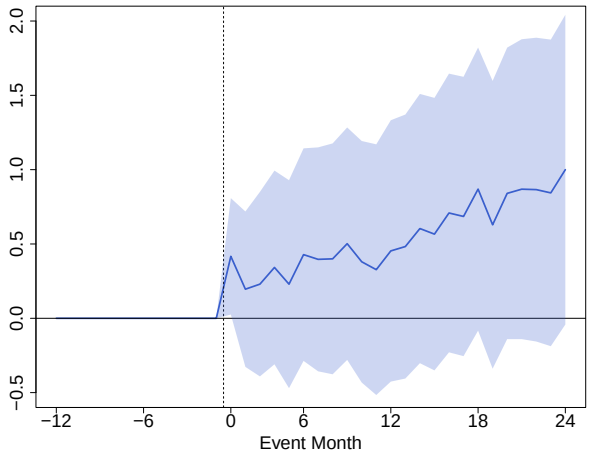


Bottom Panel: Percent of Month 24 Effect

(c) Vocational Rehab. & Employment

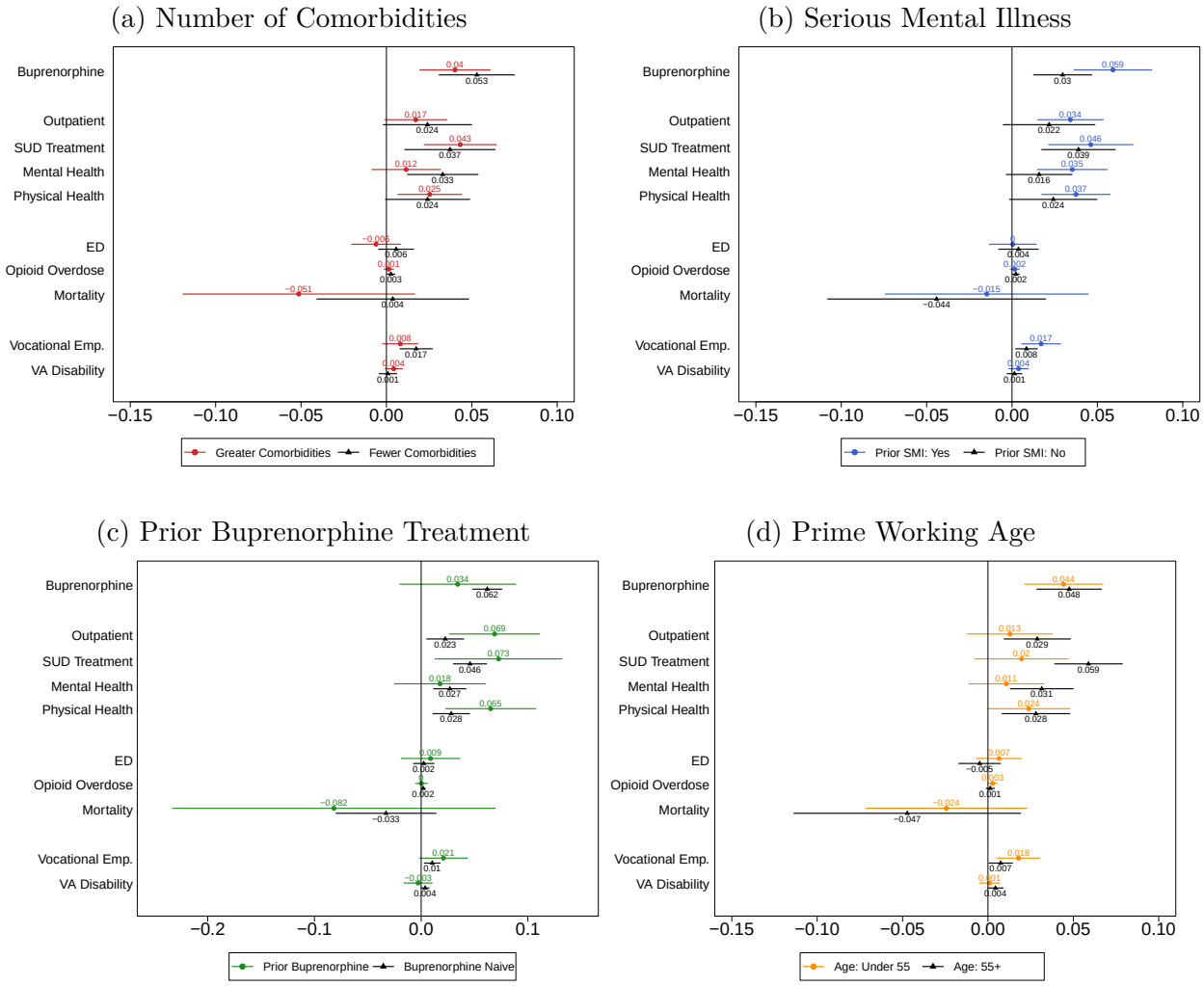


(d) Mortality



Notes: This figure compares the timing of treatment, survival, and employment-program responses to a 5 pp increase in facility prescribing adoption (Section 6.1). Monthly treatment and participation estimates interact adoption with event-time indicators, controlling for patient, calendar-month, and event-time fixed effects (Section 4.1). The top panels divide the buprenorphine-use and SUD-treatment effects by their pre-overdose means. The bottom panels express each horizon's effect relative to its month-24 value. Vocational rehabilitation measures monthly participation, not cumulative program entry. Mortality measures the probability of death by each horizon and is cumulative. Intervals are 95% confidence intervals, with standard errors for the underlying estimates clustered at the facility-by-overdose-quarter level.

Figure 6: Heterogeneity



Notes: This figure compares estimated effects of expanded facility prescribing capacity across patient subgroups (Section 6.2). Panels split patients by (a) Elixhauser comorbidity count, using a median cutoff of three; (b) prior-year serious mental illness; (c) prior-year buprenorphine treatment; and (d) age below 55 versus 55 or older. Each subgroup is estimated separately using the specifications in Section 4.1: mortality regressions include facility and overdose-quarter fixed effects; non-mortality regressions interact adoption with the post-overdose indicator and include patient and calendar-month fixed effects. Estimates correspond to a 5 pp increase in adoption. PHQ-9 depression score outcomes are very noisy, skew the figures, and are thus not plotted. The estimates on average depression score are −0.18 (0.12) and −0.30 (0.16) for greater and fewer comorbidities, −0.26 (0.10) and 0.53 (0.27) for those with and without prior SMI, and −0.07 (0.20) and −0.27 (0.11) for those with and without prior buprenorphine treatment, and −0.25 (0.13) and −0.11 (0.16) for under and over age 55, respectively.

Appendix

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A Appendix Tables and Figures

A.1 Appendix Tables

Table A.1: Cause of Death Analysis

	Dependent variable:	
	Drug-Related	Non-Drug-Related
	(1)	(2)
Reduced form, scaled by $\times 0.05$		
X-Waiver Adoption	−0.010	−0.019
	(0.011)	(0.021)
Mean Dep. Var.	0.030	0.112
Observations	6,745	6,745

Notes: This table reports estimated effects of expanded facility prescribing capacity on two-year mortality by cause. Each mortality indicator is regressed on adoption, with facility and overdose-quarter fixed effects (Section 4.1); estimates correspond to a 5 pp increase. Drug-related deaths use National Death Index Plus ICD-10 underlying cause-of-death codes X40–X44, X60–X64, X85, Y10–Y14 and/or T40.x record-axis multiple cause-of-death codes. Column (2) covers deaths not classified as drug-related. Index overdoses are restricted to 2004–2020 to allow two-year follow-up before cause-of-death coverage ends in 2022. Standard errors, clustered at the facility-by-overdose-quarter level, are in parentheses. $^{*}p < 0.10$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table A.2: Secondary Outcomes

	Dependent variable:						
	Outpat Visit	Mental Health	Physical Health	ED Visit	Opioid Overdose	Depression Score	Disability Claim
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Effect of Facility Buprenorphine Adoption Rate							
Reduced form, scaled by $\times 0.05$							
X-Waiver Adoption	0.022^{***}	0.023^{***}	0.027^{***}	0.0002	0.002	-1.315^{*}	0.003
	(0.008)	(0.007)	(0.008)	(0.005)	(0.001)	(0.691)	(0.002)
Mean Dep. Var.	0.76	0.31	0.7	0.26	0.01	11.72	0.02
Observations	252,838	252,838	252,838	252,838	252,838	5,155	182,850
Panel B: Effect of ED Physician Propensity							
Reduced form, scaled by $\times 0.05$							
ED Physician Propensity	0.0062	0.0112^{***}	0.0068^{*}	-0.0027	0.0007	0.1087	-0.0007^{*}
	(0.0038)	(0.0039)	(0.0037)	(0.0018)	(0.0004)	(0.1698)	(0.0004)
IV effect of ED buprenorphine initiation							
Buprenorphine	0.131	0.234^{***}	0.142^{*}	-0.056	0.014	1.751	-0.017^{*}
	(0.080)	(0.085)	(0.079)	(0.038)	(0.009)	(2.747)	(0.009)
Mean Dep. Var.	0.684	0.32	0.567	0.113	0.002	11.102	0.013
Observations	142,880	142,880	142,880	142,880	142,880	31,449	121,185

Notes: This table reports estimated effects on additional healthcare use, repeat overdoses, depression, and disability claims. Panel A estimates effects of expanded prescribing capacity by interacting facility adoption with a post-overdose indicator, controlling for patient and calendar-month fixed effects and a common post-overdose effect (Section 4.1). Panel B reports reduced form regressions on physician prescribing propensity and IV regressions instrumenting receipt within 72 hours with that propensity; both include facility-by-quarter, facility-by-weekend-status-by-time-of-day, and three-digit ICD-9 diagnosis fixed effects (Section 4.2). Facility and reduced form estimates are scaled to 5 pp increases; IV receipt effects are unscaled. Outcomes are monthly indicators except depression, measured by PHQ-9 scores (0–27) when recorded. Follow-up covers two years while patients are alive, excluding the first seven days. Disability claims use index visits in 2005–2019. Panel A means refer to month -1; Panel B means refer to the follow-up outcome. Observations are patient-months in Panel A and patients in Panel B. Standard errors, clustered at the facility-by-index-visit-quarter level, are in parentheses. $^{*}p < 0.10$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table A.3: Complier Characterization: Baseline Characteristics

	Compliers (1)	Full Sample (2)
Age	43.4	50.9
Female	0.07	0.08
Race: White	0.78	0.70
Race: Black	0.13	0.23
Ethnicity: Hispanic	0.03	0.06
Prior Buprenorphine	0.46	0.13
Prior Year Mental Health Cost ($)	238.8	204.1
Number of Elixhauser Comorbidities	1.78	2.83
Serious Adverse Event	0.002	0.006
Suicide Attempt	0.001	0.001
HIV/AIDS	0.030	0.016
Psychoses	0.031	0.108
Chronic Liver Disease	0.063	0.153
Chronic Pulmonary Disease	0.082	0.184

Notes: This table compares baseline characteristics of the IV-weighted marginal patients (column (1)) with the full physician-propensity sample (column (2)). Marginal-patient means use Abadie (2003)'s κ-weighting to characterize patients whose receipt is shifted by physician assignment (Appendix C.2). With a continuous instrument, these are weighted marginal-patient means, not means for a single discrete complier group. Age is measured at the index ED visit; clinical conditions, treatment histories, and healthcare costs are measured over the preceding year. Serious adverse events include accidents, overdoses, and suicide attempts.

Table A.4: Physician Propensity Design: Reduced Form Impacts on Components of Financial Strain

	Dependent variable:							
	Delinquency	Charge -Off	Collection	Bankruptcy	Foreclosure	Civil Judgments	Liens	Repossession
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Reduced form, scaled by $\times 0.05$								
ED Physician Propensity	-0.051^{*}	-0.009	-0.047^{*}	0.009	0.019^{***}	0.015	-0.049	-0.118
	(0.028)	(0.035)	(0.028)	(0.056)	(0.005)	(0.129)	(0.055)	(0.091)
Mean Dep. Var.	-0.004	0.08	0.026	-0.128	-0.027	-0.029	-0.029	0.124
Observations	25,371	16,004	29,567	8,293	863	2,053	8,162	3,693

Notes: This table reports reduced-form estimates for the eight components of the financial-strain index. Each component is regressed on the assigned physician's prescribing propensity, with facility-by-quarter, facility-by-weekend-status-by-time-of-day, and three-digit ICD-9 diagnosis fixed effects (Section 4.2). Components are standardized within calendar year across all ED patients and averaged over two years after the visit; higher values indicate greater strain. The sample comprises qualifying visits in 2014–2018 with linked credit records; sample sizes vary with component availability. Estimates correspond to a 5 pp increase in propensity. The aggregate index appears in Table 4. Standard errors, clustered at the facility-by-ED-visit-quarter level, are in parentheses. $^{*}p < 0.10$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table A.5: Lee Bounds: Accounting for Differential Survival

	Buprenorphine	SUD Treatment	Vocational Rehab. & Employment	Credit Score
	(1)	(2)	(3)	(4)
Panel A: Effect of Facility Buprenorphine Adoption Rate				
Binary X-Waiver Adoption	0.028***	0.026***	0.008***	-
	(0.007)	(0.008)	(0.003)	
	[0.022, 0.029]	[0.020, 0.027]	[−0.002, 0.009]	
Panel B: Effect of ED Physician Propensity				
Above Median Physician Propensity	0.0047***	0.0039**	0.0003	3.57*
	(0.0014)	(0.0017)	(0.0007)	(1.88)
	[0.0047, 0.0051]	[0.0038, 0.0042]	[0.0003, 0.0006]	[3.05,4.21]

Notes: This table reports reduced form estimates and Lee bounds allowing for differential survival (Lee, 2009). Panel A uses binary facility adoption interacted with post-overdose indicators, with patient and calendar-month fixed effects (Section 4.1). Panel B regresses outcomes on assignment to an above-median-propensity physician, with facility-by-quarter, facility-by-weekend-status-by-time-of-day, and three-digit ICD-9 diagnosis fixed effects (Section 4.2). These are unscaled binary contrasts. Under monotone selection into survival, trimming the excess survivor share yields bounds for patients who would survive under either assignment (Appendix C.1). Brackets contain lower and upper bounds, not confidence intervals. Bounds are reported for reduced forms only. Standard errors, clustered at the facility-by-index-visit-quarter level, are in parentheses. $^{*}p < 0.10$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table A.6: Heterogeneity-Robust Estimates of the Two-Year Mortality Effect

	TWFE	Binary Cohorts	Continuous DiD (dCDH)	
	(Baseline)	(BJS)	First Post Quarter	Dynamic Horizons 1–4
	(1)	(2)	(3)	(4)
Mortality, 2Y	−0.037*	−0.015	−0.196	−0.016
	(0.022)	(0.014)	(0.332)	(0.269)
Joint Placebo Test (p-value)	—	0.924	0.572	0.572
Facility-Quarter Cells	—	3,805	3,805	3,805

Notes: This table re-estimates the two-year mortality effect of facility buprenorphine adoption using heterogeneity-robust difference-in-differences estimators. Column 1 reproduces the baseline two-way fixed-effects estimate (scaled by ×0.05) from Table 3. Columns 2 to 4 collapse the patient-level data to the facility-by-calendar-quarter level, with the outcome defined as the share of a facility-quarter's overdose patients who die within two years. Column 2 applies the Borusyak, Jaravel and Spiess (2024) estimator using binary adoption cohorts (defined in Appendix C.1), weighted by the number of overdose patients and reports the average over eight post-adoption quarters. Columns 3 and 4 report heterogeneity-robust continuous-treatment difference-in-differences estimates following de Chaisemartin and D'Haultfœuille (2026). The estimator identifies the first quarter of treatment (waiver share) change; to reduce sensitivity to small quarter-to-quarter fluctuations, the facility waiver share is grouped into 2 pp intervals to define facility-specific treatment spells; the treatment assigned to each spell is the mean raw waiver share within that spell. Column 3 reports the normalized first-horizon effect, while column 4 reports the average total cumulative effect across horizons 1–4, allowing current and lagged treatment changes to affect outcomes; the contributing treatment histories span 2.51 quarters on average. Both estimates are weighted by the number of overdose patients and scaled to a five-percentage-point increase in the waiver share. Joint placebo tests are estimated on the four pre-adoption/jump quarters. *p<0.1; **p<0.05; ***p<0.01.

Table A.7: Selection into Credit Score Observation

Estimates are scaled by $\times 0.05$

	Dependent variable:	
	Any Credit Score	Quarter of First Observation
	(1)	(2)
Provider Propensity	−0.004	−0.083
	(0.008)	(0.050)
Mean Dep. Var.	0.357	0.842
Observations	82,889	29,567

Notes: This table investigates selection observation of credit score outcomes. Column 1 checks whether having any credit score in the two years after the first ED visit between 2014 and 2018 is correlated with provider propensity to emergency administer buprenorphine, $\tilde{Z}_{ij}$. Column 2 checks whether the timing (quarter of the first credit score observation relative to first ED visit) is correlated with provider propensity, conditional on having any credit score observation in the first year. Coefficients and standard errors are scaled by 0.05–approximately the 90th–10th percentile difference between ED physicians in the same facility-year. Heteroskedasticity-robust standard errors are clustered at the facility-by-(overdose) quarter level.

Table A.8: Primary Outcome Effects Controlling for Physician Admission Propensity

	Mortality, 2Y	SUD Treatment	Vocational Rehab. & Employment	Credit Score
	(1)	(2)	(3)	(4)
Panel A: Reduced form (*scaled by* $\times 0.05$)				
Baseline (no admission control)	-0.0063^{*}	0.0028	0.0002	5.47^{**}
	(0.0036)	(0.0036)	(0.0014)	(2.25)
Controlling for admission propensity	-0.0064^{*}	0.0028	0.0003	5.45^{**}
	(0.0036)	(0.0036)	(0.0014)	(2.25)
Panel B: IV effect of ED buprenorphine initiation				
Baseline (no admission control)	-0.133^{*}	0.059	0.005	135.43^{**}
	(0.076)	(0.075)	(0.030)	(59.95)
Controlling for admission propensity	-0.134^{*}	0.060	0.006	135.14^{**}
	(0.076)	(0.075)	(0.030)	(60.08)
Mean Dep. Var.	0.09	0.20	0.04	590.75
Observations	142,880	142,880	142,880	29,567

Notes: This table reports the reduced form effect of buprenorphine prescribing propensity (Panel A, scaled by $\times 0.05$) and the instrumental-variable effect of buprenorphine receipt within 72 hours (Panel B) on the four primary outcomes, with and without a control for the treating physician's admission propensity. Admission propensity is constructed identically to the buprenorphine prescribing propensity instrument (Appendix B): a leave-one-out, empirical-Bayes-shrunk average of the physician's residualized inpatient-admission rate, residualized on the same fixed effects (facility-by-quarter, facility-by-weekend-by-time-of-day, and 3-digit ICD-9 diagnosis). In the instrumental variable specifications, buprenorphine receipt is instrumented by prescribing propensity while admission propensity enters as an exogenous control. With the exception of mortality, outcomes are monthly averages over the 24 months following the index ED visit, conditional on survival. Credit score is observed only for the TransUnion-linked subsample (N = 29,567). Heteroskedasticity-robust standard errors, clustered at the facility-by-quarter level, in parentheses. *p<0.1; **p<0.05; ***p<0.01.

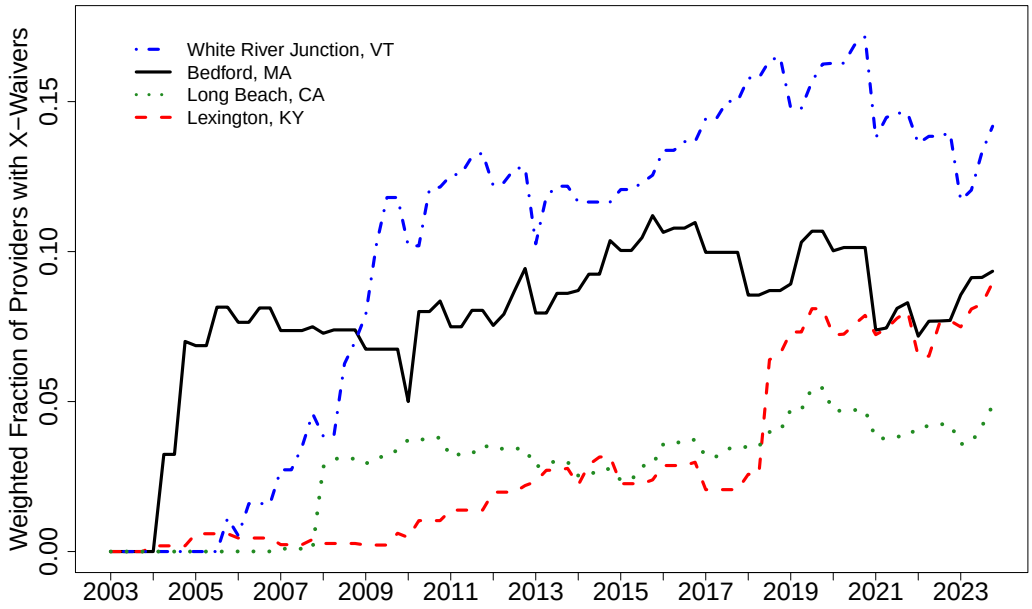
Table A.9: Buprenorphine Adoption Design: Methadone Treatment

	Dependent variable: Methadone Treatment				
		Event Month:			
	2-Year DiD	1-6	7-12	13-18	19-24
	(1)	(2)	(3)	(4)	(5)
Reduced form, scaled by $\times 0.05$					
X-Waiver Adoption	0.006	0.010*	−0.001	−0.006	−0.009
	(0.005)	(0.005)	(0.006)	(0.007)	(0.007)
Mean Dep. Var. (Prior Month)	0.050	0.050	0.050	0.050	0.050
Observations (Patient-Month)	252,838	252,838	252,838	252,838	252,838

Notes: This table presents the impact of $XWaiver_Frac$ on methadone treatment in opioid treatment programs. This table is displayed in the same format as Table 2. Column 1 presents the average treatment effect over 24 months (aggregate version of Equation (1) where event time indicators are replaced with a single post overdose indicator). Columns 2 to 5 present the average event study coefficients over six month horizons. For instance, column 2 presents the average of event study coefficients in post-period months 1 to 6 and its associated standard error. An observation is a patient-month. Coefficients and standard errors are scaled by 0.05, approximately the average increase in the treatment variable (the fraction of providers with an X-waiver) between 2004 and 2022, and also the 90th–10th percentile difference in the treatment variable across facilities in 2018. Heteroskedasticity-robust standard errors are clustered at the facility-by-(overdose) quarter level. *p<0.1; **p<0.05; ***p<0.01.

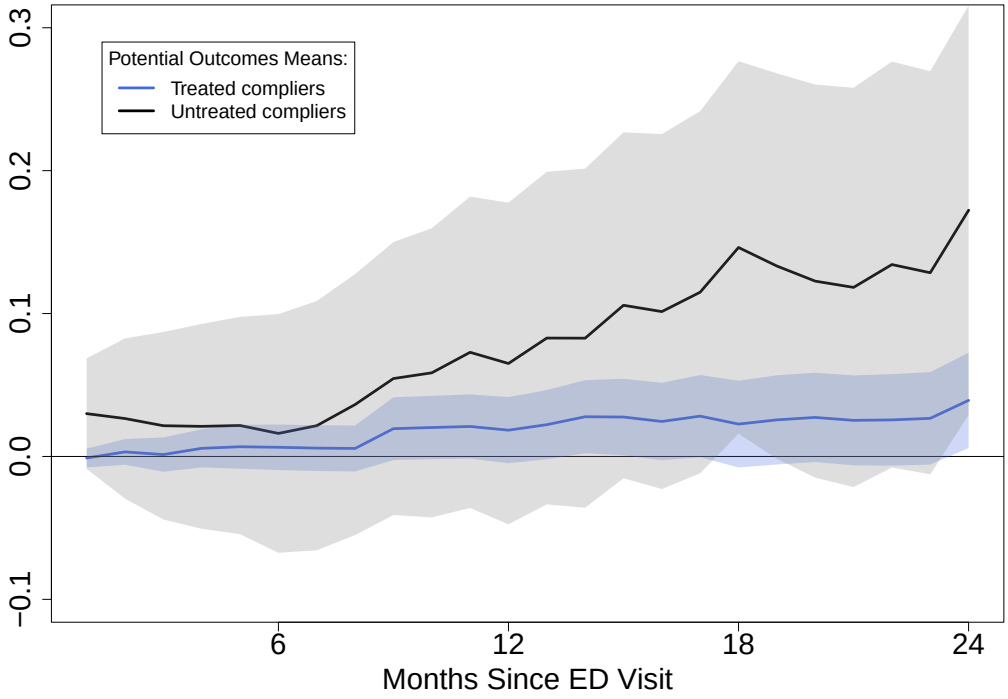
A.2 Appendix Figures

Figure A.1: X-Waiver Fraction, Selected VA Facilities



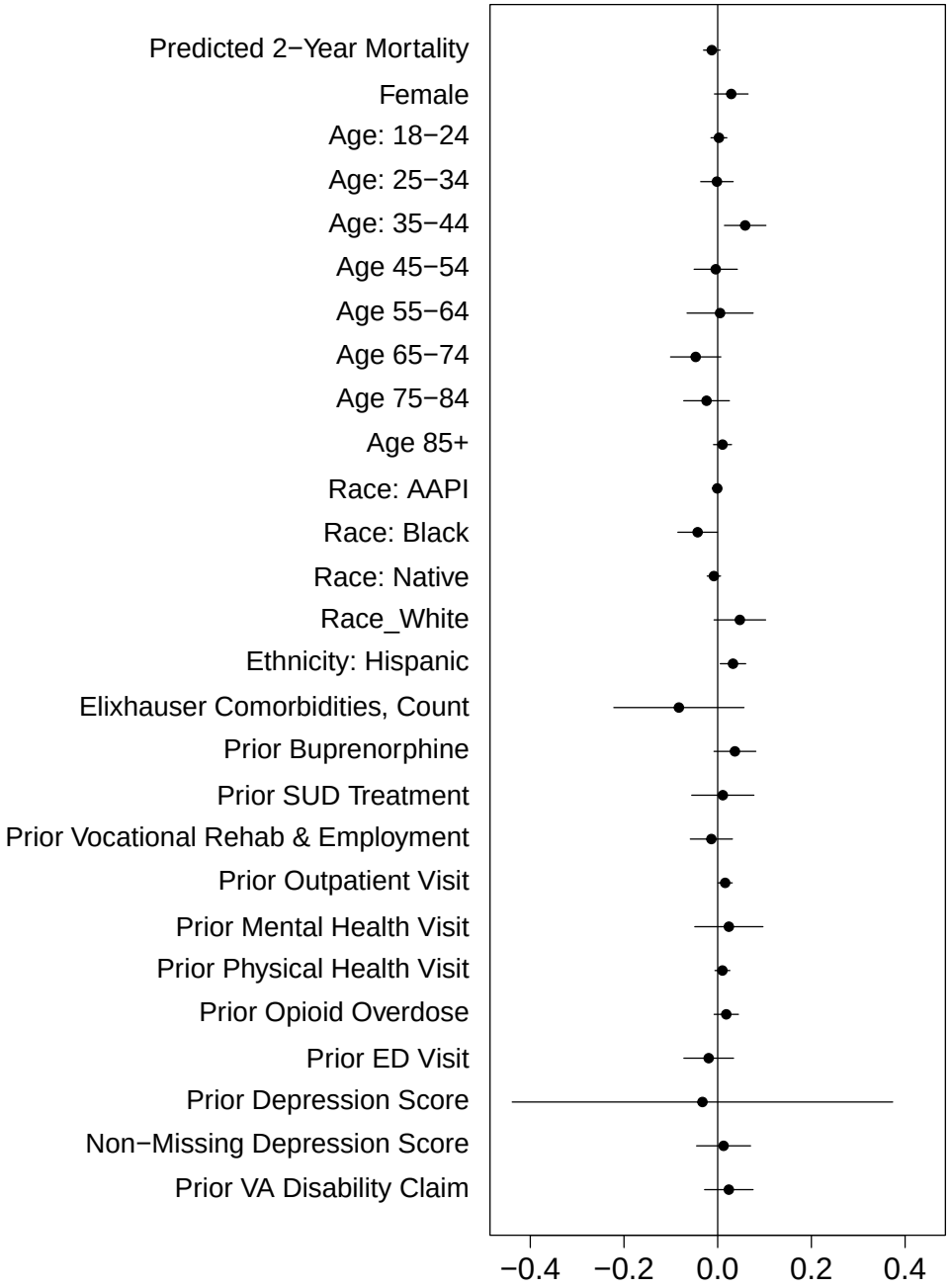
Notes: This figure plots the prescribing-adoption measure, $XWaiver_Frac$, for selected VA facilities. The measure is the patient-volume-weighted share of eligible non-ED SUD clinicians with prior buprenorphine prescribing or administration. The aggregate series appears in Figure 1.

Figure A.2: Complier Potential Outcome Mortality Paths



Notes: This figure plots the cumulative mortality compliers would experience if treated with buprenorphine (blue) versus untreated (black), by month since the index ED visit. Both series are recovered using Abadie (2003)'s κ-weighting applied to monthly cumulative mortality, following the same specification as the two-year potential-outcome means reported in the text. Because the instrument is continuous, these are IV-weighted marginal-patient means rather than literal binary complier means; shaded regions are 95% confidence intervals. Heteroskedasticity-robust standard errors are clustered at the facility-by-quarter level.

Figure A.3: Balance: Are Patient Characteristics Associated with Buprenorphine Adoption?



Notes: This figure tests whether patient characteristics among first opioid overdoses in EDs are associated with facility adoption rate $XWaiver_Frac$. Coefficients are scaled by $\times 0.05$ (a 5 pp increase in facility X-waiver adoption). Specifically, we regress each outcome Y_i on the treatment variable $XWaiver_Frac_{s(i),t_0(i)}$, controlling for facility fixed effects $\alpha_{s(i)}$ and overdose quarter fixed effects $\lambda_{t_0(i)}$. All non-binary outcomes are standardized.

B Measuring ED Provider Buprenorphine Prescribing Propensity

To construct provider propensities, we use a sample of all 1,650,803 ED visits between 2003 and 2024 by 188,556 unique patients with a prior year OUD diagnosis. This approach is motivated by the fact that a substantial share of ED visits by patients with diagnosed OUD are related to opioid use complications, such as overdose, withdrawal, or comorbid conditions frequently caused or exacerbated by opioid use, making buprenorphine initiation clinically relevant in many of these encounters.

We estimate the following regression to isolate residual variation in buprenorphine initiation within the facility, time, and diagnosis cells in which assignment is quasi-random:

$$Bup_i = \alpha_{st} + \lambda_{swd} + \gamma_{diagnosis} + \epsilon_i, \tag{5}$$

where Bup_i is an indicator for whether patient i received buprenorphine within 72 hours of their ED visit; α_{st} denotes facility-by-quarter fixed effects; λ_{swd} denotes fixed effects for facility-by-weekend-status-by-time-of-day[22]; and $\gamma_{diagnosis}$ captures 3-digit ICD-9 diagnosis fixed effects.

Using the residuals from Equation (5), we construct each patient i's provider j's emergency buprenorphine propensity, Z_{ij}, as a leave-one-out jackknife average of the residualized buprenorphine receipt in that year-quarter:

$$Z_{ij} = \frac{1}{N_{-i,j}} \sum_{i' \in \mathbb{J} \setminus i} \hat{\epsilon}_{i'}, \tag{6}$$

where $\hat{\epsilon}_{i'}$ is the residual from Equation (5) for encounter i', $\mathbb{J}$ is the set of all ED encounters with OUD patients treated by provider j in each quarter, and $N_{-i,j}$ is the number of such encounters excluding patient i. Finally, we shrink each provider's mean residual towards that facility-quarter average using an empirical Bayes adjustment. The resulting instrument, $\tilde{Z}_{ij}$, is a precision-weighted propensity that remains high for frequently observed providers, but is partly pooled towards the facility-quarter mean for low-volume providers.

[22] Time of day controls are meant to approximate ED physician shifts (7am–3pm, 3pm–11pm, 11pm–7am) and help capture variation in provider assignment.

C Robustness and Identification

This appendix details the robustness and identification analyses summarized in Section 5.3: Appendix C.1 treats the facility adoption design and Appendix C.2 the physician-propensity design. All estimates are scaled by $\times 0.05$ unless noted, matching the main-text convention.

C.1 Facility Adoption Design

Contemporaneous policy shocks. The chief threat to parallel trends is that facilities expand buprenorphine prescribing alongside other changes that independently affect outcomes. To absorb time-varying policies operating at the state level, such as state opioid initiatives, Medicaid expansions, and prescription drug monitoring reforms, we replace calendar-quarter fixed effects with state-by-calendar-quarter fixed effects. Column 2 of Table 5 shows that this barely moves the estimates on buprenorphine uptake, SUD treatment, or vocational rehabilitation; the mortality estimate is likewise stable at -0.035, though its standard error widens and it is no longer significant at conventional levels. State-by-quarter fixed effects cannot absorb VA-internal programs that vary at the *facility* level (for example, local naloxone distribution or academic detailing efforts); the within-facility-and-quarter physician-propensity design is our primary defense against such facility-level confounders.

Functional form of the treatment. Our baseline enters adoption as a continuous share, scaled so that reported coefficients correspond to a 5 pp increase. To check that the results are not an artifact of this linearity, we re-estimate using a binary adoption indicator that switches on in the first quarter in which $XWaiver_{Frac}$ exceeds 0.05 and remains above that level for four consecutive quarters; this is the definition used wherever adoption is binarized below. Column 4 of Table 5 reports estimates statistically indistinguishable from baseline across outcomes. Column 3 rescales the continuous specification by 0.034, the average within-facility jump in adoption around the binary adoption date, and yields the same pattern, indicating that the continuous and discrete representations of adoption tell the same story.

Covariate balance. Because we anchor the sample on the first overdose, a residual concern is that facility adoption itself shifts *which* patients present with an overdose. We test this in two ways. First, mirroring the balance test of the physician-propensity design, we construct predicted two-year mortality from the full set of 26 pre-overdose demographic and clinical covariates and regress it on the facility adoption rate, with facility and overdose-quarter fixed effects (the cross-sectional specification used for mortality): adoption is unrelated to patients' predetermined mortality risk (coefficient -0.007, SE 0.008, compared to the estimated mortality effect of -0.037). Second, while a few covariates are individually significant in covariate-by-covariate balance regressions (Figure A.3), the mortality estimate is essentially unchanged when all 26 are included directly as controls (-0.032 vs. -0.037 at baseline), so the modest imbalances are immaterial to the estimate.

Differential survival and the flow outcomes. Because our non-mortality outcomes are measured only over months in which a patient is alive, and treatment itself improves survival, the composition of the surviving sample differs by treatment: the treated group retains marginal survivors who would otherwise have died. If these marginal survivors have systematically different outcomes, the survival-conditioned estimates are biased. We bound this bias using Lee (2009). Intuitively, the treated group contains an "excess" share of survivors equal to the treatment effect on survival; trimming that share from the top and

bottom of the treated survivors' outcome distribution yields worst-case bounds on the effect for the population of patients who would survive regardless of treatment. Implementation requires a binary treatment, so we use binary X-waiver adoption for the facility design and above-median physician propensity for the ED design; where trimming reaches a mass point, as it does for the binary outcomes, we use fractional weights at that point rather than dropping observations, and we apply the event month survival differential as the trim share for the corresponding event month. We report bounds for the reduced forms only: the instrumental variable estimate is a ratio of reduced form to first stage, and bounds on the two components do not compose into sharp bounds on the ratio. Table A.5 shows that the bounds are narrow and remain positive for buprenorphine uptake and SUD-treatment engagement in both designs and for the credit score in the physician design, where the above- versus below-median comparison yields bounds of [3.05, 4.21] around a point estimate of 3.57; survivor composition can therefore move these estimates only slightly. The one exception is vocational rehabilitation: with a mean of only 2.5%, the worst-case trim mechanically reassigns a large share of the rare positive outcomes, and the facility adoption design's lower bound crosses zero.

Heterogeneity-robust estimation. The baseline is a two-way fixed-effects event study with a continuous, secularly rising treatment, a setting in which comparisons between earlier- and later-treated units can bias standard estimates when treatment effects are heterogeneous (de Chaisemartin and D'Haultfœuille, 2020; Callaway, Goodman-Bacon and Sant'Anna, forthcoming). Because the heterogeneity-robust estimators operate at the level at which treatment varies, we collapse the patient-level data to the facility-by-calendar-quarter cell, defining each cell's outcome as the share of its overdose patients who die within two years and its treatment as the facility's adoption rate. Two-year mortality is well suited to this aggregation: as an absorbing state measured once per patient, it collapses to a single cell-level mean, free of the survivor-conditioning and balance considerations that complicate the monthly flow outcomes. We estimate two versions. The first applies the Borusyak, Jaravel and Spiess (2024) imputation estimator to binary adoption cohorts, defined as a facility's first quarter with $XWaiver_{Frac}$ above 0.05 sustained for four consecutive quarters, weighting cells by the number of overdose patients. The second applies the de Chaisemartin and D'Haultfœuille (2026) (dCDH) continuous-treatment estimator to the adoption share itself, grouping the share into 2 pp intervals to define treatment spells. Because cells are unbalanced in calendar time (small facilities lack qualifying overdoses in some quarters), the dCDH implementation, which accommodates such gaps, is the natural fit for the continuous measure. Neither estimator detects a differential pre-adoption trend (joint placebo p-values of 0.92 and 0.57). The point estimates are -0.015 (SE 0.014) per 5 pp for the binary-cohort estimator and -0.016 (SE 0.269) for the cumulative dCDH effect over horizons 1-4 (Table A.6). Both are negative and neither rejects the baseline estimate of -0.037, but the binary-cohort estimate is less than half the baseline magnitude and the continuous estimator is too imprecise to be informative, reflecting the loss of within-cell variation once the data are collapsed to 3,805 facility-quarter cells. We therefore read Table A.6 as providing no evidence that forbidden comparisons drive the result, since both heterogeneity-robust point estimates remain negative; neither estimate is precise enough to exclude zero, and the continuous estimator cannot exclude effects of either sign.

C.2 Physician Propensity Design

Quasi-random assignment. The design requires that, within facility-by-quarter, facility-by-weekend-by-shift, and broad diagnosis, patients are assigned to ED physicians as good as randomly. Figure 4 shows that physicians with high and low prescribing propensity treat patients with nearly identical predicted buprenorphine receipt and predicted mortality, and Figure C.1 shows balance on the full set of baseline covariates used to form those predictions, with a joint test failing to reject ($F = 0.8$). Institutionally, ED physician assignment follows rotational and shift-based protocols rather than case-based triage to particular physicians, making within-shift steering of withdrawal cases toward high-propensity physicians implausible, though balance on observed characteristics cannot rule out sorting on unobserved ones.

Selection into the credit sample. Because financial outcomes are observed only for patients who appear in the TransUnion data, a concern is that selection into the credit sample is itself related to the instrument. Table A.7 shows that neither the probability of having any credit record in the two years after the ED visit nor the timing of a record's first appearance is related to physician prescribing propensity, providing no evidence of differential selection on these measured margins.

Exclusion: alternative physician behaviors. The exclusion restriction requires that a physician's buprenorphine propensity affect outcomes only through buprenorphine receipt, and would fail if high-propensity physicians systematically differ on other margins of addiction-engaged care. We test this by regressing a set of alternative physician actions at the index visit on the instrument, using the reduced form specification (Table C.1). The instrument does not predict SUD referrals made without buprenorphine initiation (-0.001), psychiatric consultation (-0.001), emergency methadone or OTP referral (0.001), or opioid-analgesic prescribing (0.003). Naloxone co-prescribing rises with buprenorphine propensity (0.001, significant), but this is expected (naloxone is routinely co-prescribed with buprenorphine) and small relative to other margins.

Exclusion: hospital admission. The one behavior that covaries appreciably with the instrument is acute inpatient admission (0.027; Table C.1), which could in principle affect mortality independently of buprenorphine. Two features suggest it is not the operative channel. First, these admissions are overwhelmingly for conditions unrelated to substance use, with pulmonary, cardiac, and general surgical diagnoses predominating, rather than the withdrawal management or overdose observation admissions through which buprenorphine-oriented care would plausibly operate, and the correlation persists even among patients who do not receive buprenorphine. Second, and most directly, we construct each physician's admission propensity exactly as we construct their buprenorphine propensity, a leave-one-out average of the physician's residualized admission rate shrunk via empirical Bayes (Appendix B), and include it as an exogenous control in the reduced form and instrumental-variable specifications for all four primary outcomes. Table A.8 shows that the estimates change little: the mortality reduced form moves from -0.0063 to -0.0064 and the credit-score IV estimate from 135.4 to 135.1. This stability indicates that physicians' admission propensity is not the channel driving the effects.

Complier characterization. Table A.3 reports Abadie (2003)'s κ-weighted complier means of baseline characteristics alongside full-sample means. Section 5.2 discusses the

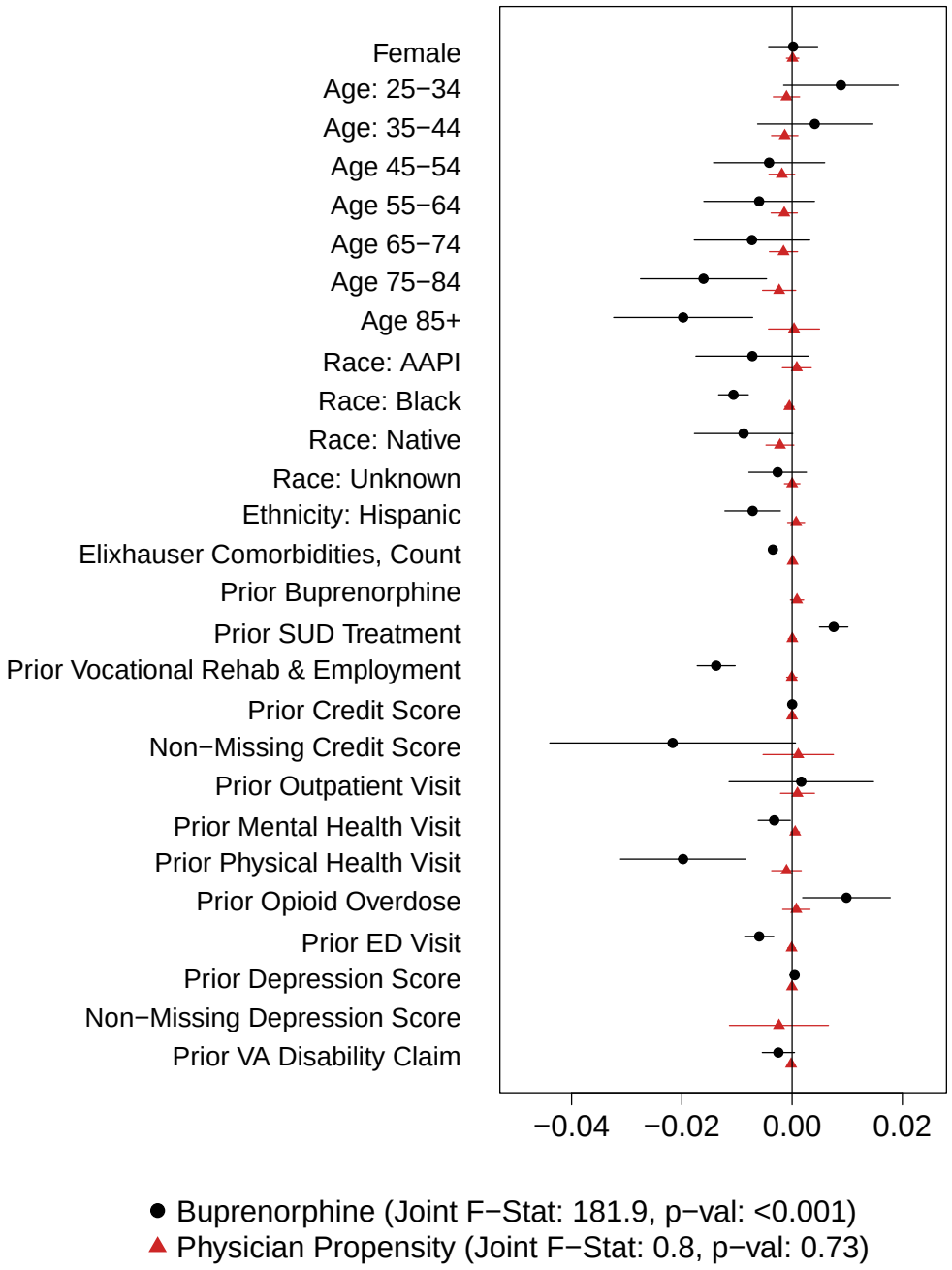
complier mortality means. Compliers are younger (43 vs. 51 years) and less comorbid (1.8 vs. 2.8 Elixhauser conditions), with lower recorded psychosis, liver disease, and chronic pulmonary disease, yet carry far higher counterfactual mortality. Their risk is specific to opioid use disorder rather than to general medical frailty: they are much more likely to have received buprenorphine previously (46% vs. 13%), a marker of established chronic OUD, and more likely to be HIV-positive (3.0% vs. 1.6%), reflecting injection-related risk. This profile bears on the exclusion restriction: an "engaged-physician" interpretation in which the instrument proxies for physicians who hospitalize frailer patients would predict compliers who are more comorbid, the opposite of what we find. Standard comorbidity indices thus appear to understate overdose-specific mortality risk in this population.

Table C.1: Exclusion Restriction: Alternative Physician Actions at the Index ED Visit

	SUD Referral	Psych Consult	Methadone/ OTP	Opioid Analgesic	Naloxone	Inpat Admission
	(1)	(2)	(3)	(4)	(5)	(6)
Reduced form, scaled by $\times 0.05$						
ED Physician Propensity	−0.001	−0.001	0.001	0.003	0.001**	0.027***
	(0.003)	(0.002)	(0.003)	(0.003)	(0.001)	(0.004)
Mean Dep. Var.	0.068	0.024	0.033	0.074	0.001	0.100
Observations	142,880	142,880	142,880	142,880	142,880	142,880

Notes: This table reports reduced form regressions of alternative physician actions at the index ED visit on the buprenorphine prescribing-propensity instrument $\tilde{Z}_{ij}$, using the same specification as Equation (2) (facility-by-quarter, facility-by-weekend-by-time-of-day, and 3-digit ICD-9 diagnosis fixed effects). Column 1 is an indicator for a SUD referral via follow-up appointment made without buprenorphine initiation; column 2, a psychiatric consultation at the ED visit; column 3, an emergency methadone or opioid treatment program referral; column 4, an opioid analgesic prescription; column 5, naloxone prescribing or dispensing; column 6, an acute inpatient admission. Estimates are scaled by $\times 0.05$. Heteroskedasticity-robust standard errors, clustered at the facility-by-quarter level, in parentheses. *p<0.1; **p<0.05; ***p<0.01.

Figure C.1: Physician Propensity Design: Covariate Balance



Notes: This figure displays the estimated coefficients and 95% confidence interval from two regressions. The black circles are coefficients from a regression of our treatment variable (buprenorphine within 72 hours) on a set of covariates. The red triangles are coefficients from a regression of our instrumental variable (ED physician propensity to administer buprenorphine within 72 hours) on the same covariates. Heteroskedasticity-robust standard errors are clustered at the facility-quarter level. The F statistic corresponding to a joint test on all covariates is displayed.

D Cost-Benefit Analysis

This appendix explains how we measure the costs and benefits in Table 6. Both refer to a 5 pp increase in facility prescribing adoption, and both are amounts per overdose patient. We add them up over three horizons: the first year, the first two years and the first three years after the overdose. All figures use point estimates, and all dollars are real 2024 dollars. The entries in the table are undiscounted. The discounted figure in Section 7.1 uses a rate of 3% a year and counts each year's added costs and life-years as arriving at the end of that year.

Costs are medical spending and buprenorphine. For medical spending, we add up each patient's total inpatient and outpatient medical costs over each horizon. We take these costs from the VA's average cost data, which assign VA spending to individual encounters based on their characteristics, such as length of stay, diagnosis-related group, intensive care days and procedure codes (Wagner, Chen and Barnett, 2003; Phibbs et al., 2003). These costs include staff and overhead. For buprenorphine, we add up all buprenorphine dispensed over each horizon and price it at the VA's contract cost for its formulation and dose, taken from the Federal Supply Schedule. The added costs in Table 6 are the estimated changes in these totals for a 5 pp increase in facility prescribing adoption.

Benefits are the life-years saved within each horizon. For this calculation, we extend the event study in Equation (1) to 36 months after the overdose, with cumulative mortality as the outcome, and add up the area between the estimated mortality curves to get the life-years saved over each horizon. The life-year calculation uses the estimated mortality path over the full three-year horizon rather than the headline estimate of mortality at two years. We do not count survival gains beyond the reported horizon. We use $300,000 per statistical life-year as our central benchmark and $200,000 and $400,000 for sensitivity analysis; Section 7.1 discusses these choices. Table D.1 scales the costs and benefits per patient to two VA populations.

Table D.1: Aggregate Policy Counterfactuals: A 5 pp Rise in Facility Prescribing Adoption

	In-Sample (2004–2022) (1)	Future VA Cohort (one annual cohort) (2)
Patients	7,517	∼500
Two-Year Deaths Averted	278	∼19
Three-Year Life-Years Saved	261	∼17
Additional Medical and Buprenorphine Cost ($M)	60.8	∼4.0
Survival Benefit, VSLY 300K ($M)	78.2	∼5.2
Net Benefit, VSLY 300K ($M)	17.5	∼1.2
Net Benefit, VSLY 200K ($M)	−8.6	∼−0.6
Net Benefit, VSLY 400K ($M)	43.5	∼2.9

Notes: This table applies the effects per patient of a 5 pp increase in facility prescribing adoption (Table 6) to two VA populations. Column 1 covers the 7,517 patients in our sample for the facility adoption design. Column 2 covers a future annual cohort of 500 patients; from 2020 to 2024, about 490 VA patients a year on average came to a VA ED with an opioid overdose, according to the authors' tabulation of VA records. Deaths averted are deaths from any cause within two years of the index overdose and use the 3.7 pp estimate. Life-years, costs and benefits use the horizon of three years in Table 6. The life-years come from the event study, a different estimate from the one behind deaths averted. Costs are additional medical spending plus buprenorphine. VSLY is the value of a statistical life-year. Both columns hold the effects and costs per patient fixed and use point estimates; entries are computed from unrounded life-years. Column 2 assumes that the estimates apply to future VA patients. All dollars are real 2024 dollars. The deaths-averted figures refer to two-year outcomes per cohort, not deaths averted within one calendar year.