The Effects of Naloxone Access Laws on Opioid Abuse, Mortality, and Crime*

Jennifer L. Doleac Anita Mukherjee

September 17, 2021

The U.S. is experiencing an epidemic of opioid abuse. In response, states have implemented a variety of policies including increased access to naloxone, a drug that can save lives when administered during an overdose. There is a concern that widespread naloxone access may unintentionally lead to increased or riskier opioid use by reducing the risk of death from overdose, however. In this paper, we use the staggered timing of state-level naloxone access laws as a natural experiment to measure the effects of broadening access to this lifesaving drug. We find that broadened access led to more opioid-related emergency room visits and more opioid-related theft, with no net measurable reduction in opioid-related mortality. We conclude that naloxone has a clear and important role in harm-reduction, yet its ability to combat the opioid epidemic's death toll may be limited without complementary efforts.

JEL Codes: I18, K42, D81

*We thank Stephen Billings, David Bradford, Kitt Carpenter, Phillip Cook, David Eil, Jason Fletcher, Benjamin Hansen, Paul Heaton, Jason Hockenberry, Peter Hull, Keith Humphreys, Priscillia Hunt, Mark Kleiman, Jens Ludwig, Ellen Meara, Jonathan Meer, John Mullahy, Murat Mungan, Derek Neal, Rosalie Pacula, Nicholas Papageorge, Harold Pollack, Christopher Ruhm, Daniel Sacks, Joan T. Schmit, Kosali Simon, Sebastian Tello-Trillo, Glen Waddell, Abigail Wozniak; seminar participants at Indiana University-Bloomington, the University of Virginia, West Point, the University of Wisconsin-Madison, and UC-Irvine; and conference participants at the 2017 IRP Summer Research Workshop, the 2017 Western Economic Association annual meeting, the 2017 NBER Summer Institute Crime Working Group, the 2017 Transatlantic Workshop on the Economics of Crime, the 2017 APPAM fall research conference, the 2017 Southern Economic Association annual meeting, the 2018 Risk Theory Society Annual Seminar, the 2018 Economics of Risky Behavior Conference, and the 2018 Western Economic Association annual meeting for helpful comments and conversations. Junhao Liu, Kelsey Pukelis, and Katharine Sadowski provided excellent research assistance.
Doleac: Texas A&M University, jdoleac@tamu.edu; Mukherjee: Wisconsin School of Business, University of Wisconsin-Madison, anita.mukherjee@wisc.edu.

1 Introduction

The United States is grappling with an epidemic of opioid abuse and overdoses: in 2019, nearly 50,000 people died due to an opioid overdose and now constitutes over two-thirds of all drug overdose deaths (Centers for Disease Control and Prevention, 2021). Policymakers have struggled to reduce the lethal effects of this class of drugs. Many have turned to naloxone. Naloxone is a drug that can reverse an opioid overdose if administered quickly; it therefore has the potential to reduce this epidemic's death toll. Every U.S. state has passed a law that facilitates widespread distribution and use of naloxone. Prominent health officials have even called for naloxone in every medicine cabinet (Shesgreen, 2016; Scutti and Jimison, 2018). But while the drug is clearly effective at preventing opioid overdose deaths, it can also introduce unintended consequences.

Unintended consequences could emerge if opioid use increases in response to the reduced risk of death from overdose. Some who are addicted may become criminally active to fund their continued or increased drug use. Additionally, some opioid abusers are saved by naloxone (as intended) and may continue their previous drug use and criminal behavior. This intended effect of naloxone will mechanically increase observed rates of both behaviors. Furthermore, expanding naloxone access might not in fact reduce mortality. Though the risk of death per opioid use falls, an increase in the number or potency of uses means the expected effect on mortality is ambiguous.

We use the gradual adoption of state-level naloxone access laws as a natural experiment to measure the effects of broadened access, and find that it increased opioid abuse and crime, with no net reduction in mortality. We focus our analysis on cities, since we expect naloxone access laws to have a bigger effect there. We expect a larger effect in urban areas because of the greater density of potential bystanders who could administer the drug, more efficient distribution by community groups, and shorter 911 response times.[1]

[1] We will also show results for rural areas and for all areas combined. Results for all areas combined are similar to those for urban areas.

We estimate the effects of naloxone using a panel fixed effects model; this model controls for pre-existing differences and trends across jurisdictions, so that we do not confuse those differences with changes caused by expanding access to naloxone. We also control for a variety of other opioid-related policies, as well as the number of police officers per capita as a proxy for local law enforcement resources.

Local data on actual naloxone distribution are unavailable, so we use data on Google searches as a proxy for local awareness of and interest in naloxone (a relevant first stage outcome). We find that naloxone access laws increased internet searches for "naloxone" by 7%. We then consider a variety of outcome measures, and find consistent evidence that broadening naloxone access may have increased opioid abuse, at least among some groups. After naloxone access laws take effect, Google searches for "drug rehab" (a proxy for interest in drug treatment[2]) fell by 1.4%, arrests for possession and sales of opioids increased by 17% and 27%, respectively, opioid-related visits to the emergency room (ER) increased by 15%, and opioid-related theft increased by 30% (though the effect on overall theft rates is much smaller).

We note that interpreted alone, the increase in opioid-related ER visits could reflect the effectiveness of naloxone — following access to the drug, individuals may exhibit a higher propensity to seek care following an overdose. If this were the case, we would expect a reduction in opioid-related mortality, which we do not find (at least on average); our confidence intervals on the mortality result are large, however, and cannot rule out substantial reductions or increases in opioid-related mortality in response to naloxone access laws.[3] We also highlight that the inability to detect small but statistically significant mortality effects for policy interventions is not surprising given challenges in statistical power; for example, Black et al. (2021) shows that state-level Medicaid expansions offer only enough variation to detect mortality reductions of at least 2% in a standard difference-in-differences framework.

[2]We will show that this measure is highly correlated with actual drug treatment admissions.

[3]As expected, effects in rural areas were typically statistically insignificant. We also explore regional differences in the appendix.

A variety of robustness checks support our main results. We find no evidence that pre-existing trends are driving these effects. Most importantly, we show that pre-existing trends in fentanyl use do not explain our findings, though (as discussed in Section 6.5) the underlying trends in fentanyl supply may interact with the policy changes we study to exacerbate their effects. Our estimates are robust to controlling for an array of other state policies aimed at reducing opioid abuse and mortality. We consider impacts on broader categories of theft and mortality and find no evidence that our results are due to a simple improvement in recording when opioids were involved in the event. Finally, our results are robust to using different definitions of "urban," dropping individual states one-by-one, and using alternate dates for naloxone access laws (in the few cases where there was ambiguity about when access was broadly expanded).

The paper proceeds as follows: Section 2 provides a review of the related literature, Section 3 discusses relevant background information about naloxone access laws and the effects of other opioid-related policies, Section 4 describes the data we will use to study the effects of naloxone access laws on behavior, Section 5 details our empirical strategy, Section 6 presents our results, Section 7 presents our robustness checks, and Section 8 concludes.

2 Related Literature

This study is related to several academic literatures in economics. The backbone of the moral hazard model we explore in this paper is from Peltzman (1975), who argued that the benefits from innovations in driving safety such as seatbelts would be muted at least somewhat due to compensatory behavior due to riskier driving. Cohen and Einav (2003) found that the moral hazard from seatbelts that Peltzman hypothesized is small relative to the safety-improving effect of seatbelts. But Cohen and Dehejia (2004) find that automobile insurance, which also incentivizes riskier driving through moral hazard, causes a large increase in traffic fatalities. In another public health context, Lakdawalla, Sood and Goldman (2006) consider the moral hazard effects of HIV treatment breakthroughs on risky sexual behavior. They find that treating HIV-positive individuals more than doubles their number of sexual partners and contributed to a large increase in HIV incidence during the same period. Related work by Chan, Hamilton and Papageorge (2015) provides a dynamic model of this behavioral response to the availability of life-saving HIV treatment. They show that both HIV-negative and HIV-positive men increase their risky sexual behavior when the cost of contracting HIV falls.

Recent work by Packham (2020) is also related to the context we study here. That study considers the effects of opening syringe exchange programs (SEPs) on local HIV prevalence and opioid use. It finds that, though SEPs have their intended effect of reducing HIV, they increase opioid abuse as measured by ER visits due to overdose (by 18%), drug-related arrests (by 16%), and opioid-related mortality (by 13-15%). SEPs also increase local rates of theft by 24%. Packham argues that this is evidence of moral hazard: by reducing the risk associated with drug use, SEPs are linked with increased opioid use. Like the current paper, Packham (2020) finds that access to substance abuse treatment appears to explain heterogeneity in the effects of SEPs across areas. These results provide an important, complementary example of the tradeoffs involved in harm reduction efforts.

It may seem surprising that drug users respond to incentives in a sophisticated way. One may think that drug users are poor decision-makers or that addiction makes rational choices impossible. Addiction can cloud judgement and makes policy in this area difficult, but there is substantial evidence that drug users respond to incentives. A theoretical literature hypothesizes that consumption of addictive substances such as drugs and alcohol is rational and sensitive to prices (Becker and Murphy 1988; Grossman and Chaloupka 1998), and a large body of empirical evidence documents such a causal relationship. For example, increasing taxes on alcohol reduces alcohol consumption (Cook and Durrance, 2013).

Alcohol abuse also responds favorably to increasing the likelihood of punishment, as seen in evaluations of the 24/7 Sobriety program (Kilmer et al., 2013). Hansen, Miller and Weber (2017) show that marijuana consumption is price inelastic in the short run, but quickly becomes price elastic, with consumers reducing their consumption in the face of higher marijuana taxes. And finally, Moore and Schnepel (2021) show that a massive reduction in the heroin supply in Australia resulted in a long-term reduction in heroin consumption among those using heroin at the time, due to a spike in the price of the drug. If drug use is sensitive to prices, then it is reasonable to hypothesize that it is also sensitive to non-monetary costs such as the risk of death.

There is an overlapping literature on the effects of drug and alcohol consumption on crime. We are interested in the effects on crime in part because the welfare implications of drug and alcohol abuse themselves are unclear: some argue that people can do whatever they want to their own bodies, no matter how harmful. Externalities in the form of crime are more clearly negative and so could justify government intervention. For instance, there is substantial evidence that policies that increase alcohol consumption also increase violent crime (Cook and Durrance 2013; Anderson, Crost and Rees 2017). Substance abuse may affect crime (1) by leading users to steal or engage in illegal behavior to generate income to purchase drugs, (2) through a direct physiological effect that makes users more aggressive, or (3) by creating an illicit market where violence is required to defend turf, enforce contracts, and so on (MacCoun, Kilmer and Reuter, 2003). We are interested in whether naloxone access laws increase crime rates through their effect on opioid abuse. Because violent behavior is not typically associated with opioid use or opioid dealing (Quinones, 2015), we expect the main effect of these laws to be on theft.

Our paper is one of a handful examining naloxone's causal effect on opioid abuse.[4] Rees et al. (2019) use annual, state-level CDC mortality data from 1999 through 2014 and find that naloxone access laws substantially reduce deaths; our results with additional data show that the true effect may be noisy and difficult to estimate. Deiana and Giua (2021) also use aggregated mortality data through 2014, and do not find statistically significant effects of naloxone access on mortality or several broad crime categories. Using richer, higher-frequency data on both mortality and crime, as well as an additional year of data, we provide more precise mea-

[4] Another related paper considers the association between naloxone laws and opioid-related mortality but it is unclear from the analysis whether the correlations found represent causal effects (McClellan et al., 2018).

sures of these national effects. Most recently, Abouk, Pacula and Powell (2019) differentiate between types of naloxone laws and find that granting pharmacists authority to prescribe or dispense naloxone appears to reduce opioid overdose mortality; Erfanian, Grossman and Collins (2019), however, uses a spatial difference-in-differences framework and finds that certain naloxone access laws increase opioid overdose mortality by 11 per 100,000 population. We contribute evidence on when naloxone is available by third party prescription or without any specific prescription, as with state-wide standing orders.

3 Background

Opioid addiction now claims nearly 136 lives each day (Centers for Disease Control and Prevention, 2021). Individuals are prescribed these drugs to treat pain, but many patients develop addictions that lead them to illegal use of prescription opioids and cheaper substitutes such as heroin. (In addition, many people begin abusing prescription opioids and heroin without a prescription, particularly now that these drugs are more easily accessible; Quinones, 2015.) Such drug abuse can have fatal consequences, and policymakers across the country are searching for policies that can reduce the death toll.

Naloxone is an opioid antagonist that can effectively reverse overdose symptoms when administered properly, typically via injection or nasal spray. Public health officials have pushed to broaden access to naloxone, so that the drug is available and nearby whenever needed. Since addiction symptoms are often hidden, this effort has reached far beyond standard target populations of known drug-abusers. For example, Baltimore's former health commissioner, Dr. Leana Wen, widely advocated for naloxone to "be part of everyone's medicine cabinet" (Shesgreen, 2016); and, as U.S. surgeon general, Dr. Jerome Adams issued an advisory encouraging such action (Scutti and Jimison, 2018).

To broaden access to and use of naloxone, states began addressing these concerns by implementing policies that made it easier for residents to obtain the drug. The level of naloxone access varies by state, with the most generous laws including a "standing order" allowing any resident to obtain the drug at local pharmacies with no justification; generally, standing orders do not even include an age requirement beyond a provision for pharmacist discretion on this factor. Slightly less broad than standing order laws are third party prescription laws, which allow residents to buy naloxone with the "reasonable intention" of administering the drug to someone else. Because naloxone remains a prescription drug as categorized by the U.S. Food and Drug Administration, standing orders and third party prescriptions are enabled by the physician-general of a state writing a prescription for all residents.

Other laws regulating naloxone access can cover: prescriber or dispenser immunity (civil, criminal, disciplinary), layperson administration immunity (civil, criminal), layperson distribution or possession (including without a prescription), and whether prescriptions are allowed by pharmacists. In practice, we could not find any evidence that the lack of provider or layperson immunity was a significant barrier to naloxone administration. Since standing orders and third party prescriptions appear to be the laws most relevant to broadening naloxone access, we focus our analysis on the effects of these laws.

By mid-2017, all states had implemented third party prescriptions or standing orders, which represent significantly broadened naloxone access. We use the staggered effective dates of such laws as a natural experiment. However, since states typically passed multiple naloxone-access laws as a package or in close succession, we will be unable to separate the effects of individual law types (e.g., requirements to receive training from a pharmacist prior to obtaining naloxone is a relatively minor law that almost always accompanies a broader law enabling standing orders or third party prescriptions). Readers should interpret our estimates as measuring the impact of naloxone-access laws as a package, though we expect third party prescription and standing-order laws to be driving any effects.

There is widespread anecdotal evidence that these laws led to meaningful increases in naloxone access—particularly in the form of distribution by community organizations and by enabling police officers to carry naloxone. However, state and local data on actual naloxone distribution during this period are typically unavailable.[5] Two exceptions provide numerical

[5]ARCOS (Automation of Reports and Consolidated Orders System) data are commonly used to study drug distribution, but focus on controlled substances, so naloxone is most frequently reported only when

evidence that these laws resulted in an increase in naloxone distribution. The first is North Carolina, which broadened naloxone access in April 2013. Over the three years afterward, the state's Harm Reduction Coalition distributed naloxone kits to over 27,000 high-risk individuals (Reed, 2016). Another example is Maryland, which broadened access to naloxone in October 2015. As shown in Figure 1, the number of naloxone kits distributed by the Health Commissioner's Staying Alive program in Baltimore increased from a steady average of 508 kits per year between 2010 and 2014, to 2,515 kits in 2015 and 9,339 kits in 2016 (Baltimore City Health Department, 2018). These numbers imply that distribution jumped 1,731% after the law went into effect, from 42 kits per month to 769 kits per month.[6]

During this time period, states implemented a variety of other policies aimed at reducing opioid abuse and opioid-related deaths, and a rapidly-growing literature estimates those policies' effects. Meara et al. (2016) constructed a database of such policies, most of which were aimed at changing opioid prescription behavior. That database includes policies that limit doctor-shopping and regulate pain clinics, but does not include naloxone access laws. The authors measure the policies' impacts on opioid abuse for an at-risk population, finding no association between opioid abuse and specific policies or the number of policies enacted.

Other papers focus specifically on the effects of Prescription Drug Monitoring Programs (PDMPs), which track patients' opioid prescriptions and provide that information to physicians. Buchmueller and Carey (2018) find that PDMPs reduce measures of opioid misuse in Medicare Part D. Kilby (2015) finds that PDMPs reduce the distribution of opioids as well as overdose deaths. However, she notes that this reduction in mortality comes at the cost of reducing legitimate pain management. Back-of-the-envelope estimates suggest that the welfare gains from this policy are roughly equivalent to the welfare losses. In related work, Schnell (2017) finds that physicians consider the secondary market for opioids and

it is found in conjunction with another drug. Pharmaceutical distribution data from the IMS Health (now IQVIA) are targeted to industry purchasers, and do not include information on ground-level distribution by harm reduction campaigns that are likely most important in this context.

[6] Since similar data are not available across the country, we will use Google trends data on internet searches for "naloxone" as a proxy for interest in and awareness of the drug, and show that naloxone access laws resulted in a significant increase in such searches across the country.

alter their prescribing behavior in response: prescriptions would have been 13% higher in 2014 if a secondary market did not exist. This reduction in opioid prescriptions (some to patients in legitimate pain), in addition to the reallocation of prescription opioids in the secondary market, results in a net social cost of $15 billion per year due to health losses.

Two studies find that a change in the formulation of the prescription opioid OxyContin, to make it tamper-resistant and thus harder to abuse, did not reduce opioid-related deaths. Instead, this change led users to switch to heroin (Alpert, Powell and Pacula, 2018; Evans, Lieber and Power, 2017). Similarly, Mallatt (2017) finds that PDMPs increase heroin crime (a proxy for heroin abuse) in the places with the highest rates of oxycodone abuse before the policy change. These findings highlight the importance of considering the behavioral consequences of policies in this area, and the difficulty of reducing opioid abuse.

4 Data

We hand-collected information on the timing of naloxone access laws in each state. That information was cross-checked to the extent possible with previous research on the topic (e.g., Davis and Carr, 2015). Our main treatment variable, "naloxone law," is coded as whether a state has broadened access to naloxone through either third party prescriptions or standing orders. Figure 2 shows how the number of states with naloxone access laws evolved over time, and Figure 3 shows maps of the states with broad naloxone access laws in each year. As these figures show, naloxone access laws were adopted by a geographically and politically diverse set of states. All states eventually pass such laws, though our data only go through the end of 2015. Table 1 lists the precise dates we use in our analysis.[7]

To measure the impacts of those laws on opioid abuse, mortality, and crime, we use a variety of datasets. Ideal outcome measures would perfectly reveal risky consumption of opioids and opioid-related mortality and criminal behavior. Unfortunately, actual behavior is imperfectly observed. While each of the datasets we use is an imperfect proxy for

[7] In five states, the specific date of broadened naloxone access is somewhat ambiguous due to the passage of related legislation, and one could argue that we should be using an earlier date than we do in our analysis. We will show that using these alternate dates does not affect our results.

our outcomes of interest, in combination they paint a compelling picture of opioid-related behaviors.

Data on actual awareness and distribution of naloxone are unavailable, so to approximate a first stage we use Google Trends data on internet searches for "naloxone" over time. These data are available at the national, state, and metropolitan-area levels. "Search interest" for a specified term is quantified on a 0 to 100 scale that is normalized to the region and time period, with 100 representing peak popularity for that search term, relative to all other searches in that region during that period. The site groups related search terms into "topics"—for instance, the "naloxone (drug)" topic includes searches for naloxone, Narcan, and some other highly-similar terms (such as common misspellings). We verified that this grouping was nearly identical to an aggregation of search terms that we independently created and focus our analysis on data for the "naloxone (drug)" topic search. We use monthly data for 2010-2015 at the metropolitan area level. Scores therefore measure changes in search intensity within a metropolitan area between 2010 and 2015.

Similarly, data on interest in drug treatment is also unavailable, so we again use Google Trends data as a proxy. We use data on searches related to the "drug rehabilitation" topic (which includes searches for "drug treatment"), to measure interest in treatment for addiction. Table B.1 shows that this search index is highly correlated with actual admissions to drug treatment programs for opioid addiction, as recorded in the Treatment Episode Data Set (TEDS): A one-unit increase in the search index is associated with 306 additional opioid-related treatment admissions, a 3.5% increase ($p < 0.05$). One problem with using TEDS data directly is that the data are only available at the state-year level; Google Trends data provide a more local and higher-frequency measure of drug treatment interest that coincides with the broadened naloxone laws.[8] As before, we use monthly data for 2010-2015 at the metropolitan area level. Scores measure changes in search intensity within a metropolitan

[8]Another problem with using TEDS data is that drug treatment facilities often operate at capacity; if there is no room for new patients, then interest in treatment may not result in admission for treatment. Google Trends data provide an unconstrained measure of interest.

area between 2010 and 2015.

To consider effects on opioid-related criminal behavior (including supply of and demand for illegal opioids), we use data from the National Incident-Based Reporting System (NIBRS) from 2010 through 2015. NIBRS is an incident-level dataset that collects information on reported crimes from local, state, and federal law enforcement agencies. The NIBRS dataset includes rich incident-level information on reported offenses and arrests. Important to our study, drug or narcotic offenses included specific codes for a variety of opioids and other substances involved with the crime. One drawback of NIBRS is that not all jurisdictions[9] participate. We create a balanced panel of jurisdictions that report offenses in all months of 2010–2015.[10] During that time period, 2,831 jurisdictions in 33 states submitted information to NIBRS, representing roughly 24% of the country's population. In our analysis, we aggregate incidents to the jurisdiction-month level.

Each incident may record up to three offense types, and we code an incident as including a particular type of crime if that crime was any of the three recorded offenses. For drug or narcotic violations, the NIBRS data also include information on up to three different drug types involved with the offense. We categorize opioid-related crimes as those involving heroin, morphine, opium, and other narcotics (which include synthetic opioids such as prescription pills and—of particular interest—fentanyl).

We use these data to construct the following outcome variables: possession of opioids[11] (a proxy for quantity demanded), selling of opioids[12] (a proxy for quantity supplied), all opioid-related offenses (that is, any offense that included an opioid-related violation), opioid-related theft, and all theft.[13] For offenses such as theft (and other serious crimes), the variable

[9]In NIBRS, a jurisdiction is defined as a reporting law enforcement agency. Most jurisdictions are city or town police departments, but some are state police, college campus police, public transit police, and similar.

[10]Data from earlier years are available, but because fewer jurisdictions report before 2010 we lose a substantial number of jurisdictions when creating a balanced panel. We therefore focus on years 2010 and later.

[11]This category includes the following official codes: Buying/receiving, possession/concealing, and using/consuming.

[12]This category includes the following official codes: Distributing/selling, and transporting/transmitting/importing.

[13]Theft includes pocket-picking, purse-snatching, shoplifting, theft from a building, theft from a coin-

measures reported crime. For offenses such as possession of or selling opioids, the variable is more accurately interpreted as measuring arrests. All variables are converted into rates per 1,000,000 local residents.

We are interested in theft as an outcome because opioid abusers may steal in order to fund their addictions. (Violence is not generally an expected outcome of opioid abuse.) While the detection and reporting of opioids involved in other crimes (such as theft) are surely imperfect, the presence of that drug indicator is a clear sign that opioids were involved in some way: for instance, the offender was in possession of illegal opioids at the time of arrest, or was stealing prescription pills. Looking at all theft in addition to opioid-related theft allows us to test for the overall impact on public safety, but all theft is a function of many factors and the share of theft that is in some way the result of opioid abuse is likely small; for these reasons, it may be difficult to precisely measure effects of naloxone law changes on this broader category.

To measure abuse and overdose involving opioids, we use data on opioid-related ER visits from the Healthcare Cost and Utilization Project (HCUP) for years 2006-2015. We acknowledge that the probability of visiting the ER is a compound probability, i.e., the probability of overdose multiplied by the probability of ER visit conditional on overdose. As such, an increase in ER visits could indicate an uptick in only the latter probability, which would be a sign of naloxone access effectiveness—visiting the ER is strongly recommended upon receiving the opioid overdose antidote. If an increase in ER visits is accompanied by a detectable reduction in mortality, we could conclude that the source of increase is coming from the probability of ER visit conditional on overdose. If we do not observe reductions in mortality, however, increased ER visits are consistent with at least some rise in the probability of overdose from increased or riskier opioid use.

The emergency room data provide a quarterly measure of the number of ER visits by reason for the visit, by state and by metropolitan-area-type within the state.[14] (Since we

operated machine or device, theft from a motor vehicle, and all other larceny.

[14]In contrast to the other datasets, we don't have county or city identifiers in the HCUP data.

only have quarterly instead of monthly data, we use a slightly longer time period to improve statistical power.) Opioid-related visits are those coded as relating to "opioid-related disorders", and "poisoning by, adverse effect of, and underdosing of" opium, heroin, other opioids, methadone, other synthetic narcotics, unspecified narcotics, or other narcotics. This gives us a measure of how often local residents sought medical attention due to opioid abuse. As mentioned, if naloxone access leads to more overdoses—because users expect that naloxone will save their lives—then we would expect the number of ER admissions to increase, even if mortality falls or stays the same. This proxy for opioid abuse may be biased downwards if individuals administer naloxone and (against medical advice) don't subsequently seek medical attention for the person who had overdosed. There is some evidence that this happens: a survey of naloxone training participants in Baltimore found that fewer would call 911 for help after naloxone training (Mueller et al., 2015). On the other hand, it could be biased upwards if more bystanders call 911 for help knowing that naloxone is available—in this case, we might expect to see an increase in ER visits for the same number of overdoses. However, if this is driving the effect then we would expect a corresponding decrease in mortality. (We will therefore interpret an increase in ER visits *without* a corresponding decrease in mortality as evidence of a true increase in overdoses, though we acknowledge that making conclusions is difficult if estimates are noisy.)

Finally, we use restricted-access mortality data for 2010-2015 from the Centers for Disease Control and Prevention (CDC) to measure deaths due to opioid overdose. We identify opioid-related deaths as those that include the following ICD-10-CM Diagnosis Codes: T40.0 (opium), T40.1 (heroin), T40.2 (other opioids), T40.3 (methadone), T40.4 (other synthetic narcotics), and T40.6 (other/unspecified narcotics). Deaths due to "other synthetic narcotics" are our measure of fentanyl-related deaths. In a robustness check, we also use data on deaths due to an unspecified drug. These data are available at the county-month level, and we convert them into rates so that they represent deaths per 100,000 local residents.

Throughout our analyses, we focus on urban areas, since that is where we expect broadening naloxone access to have the greatest impact. We define urban areas as those having populations greater than or equal to 40,000. In the NIBRS data, there are 410 jurisdictions across 31 states with populations greater than or equal to 40,000, and they represent approximately 14% of the U.S. population. (The largest cities tend not to report to NIBRS, so we interpret the NIBRS analysis as representing the experience of small- and medium-sized cities—like Cleveland and Salt Lake City—but perhaps not the experience of major cities like Chicago, Los Angeles, and New York City.) In the CDC data, we include all counties with at least one jurisdiction of at least 40,000 residents, and in the HCUP analysis we focus on ER admissions in metropolitan areas.[15] We will show that our results are not sensitive to this definition of "urban", and will also show results for rural areas as well as for all jurisdictions combined.

We use the database from Meara et al. (2016) to control for the implementation of other state policies that could affect opioid use. That database goes through 2012; we extend it through 2015. These policies include: Good Samaritan laws, PDMPs, doctor-shopping restrictions, pain-clinic regulations, physician examination requirements, pharmacy verification requirements, patient identification requirements, and requirements related to tamper-resistant prescription forms. While that study found that none of these policies had meaningful impacts on their targeted population (alone or in combination), they may have effects more broadly. To ensure that we are isolating the effects of naloxone access laws, and not picking up effects of other policies that might have been enacted around the same time, we control for this set of policies in all of our analyses.

In our preferred specification, we also control for the log of police officers per capita as a proxy for local investment in law enforcement and other crime-control policies. These data are from the FBI's Law Enforcement Officers Killed and Assaulted (LEOKA) database. They are available at the jurisdiction-year level. Note that because we do not have city or

[15]HCUP data aggregates data by type of urban area: large central metropolitan, large fringe metropolitan, medium metropolitan, small metropolitan, rural. Our definition of "metropolitan" combines all categories except the last one.

county identifiers in the HCUP data, we are not able to control for police per capita in those analyses.

Finally, we consider whether our effects vary with the availability of local drug treatment. Following Bondurant, Lindo and Swensen (2018), we use the number of drug treatment facilities per 100,000 residents as a proxy for the likelihood that treatment is available to someone who needs it. (A treatment facility is defined as a single physical location. Obviously the patient capacity of these facilities would be an even better proxy for treatment availability, but to our knowledge such data are unavailable.) These annual, county-level data come from the County Business Patterns (CBP) dataset maintained by the Census Bureau. Swensen (2015) also uses these data to find that a 10% increase in treatment facilities reduces a county's drug-induced mortality rate by 2%.

Summary statistics are in Table 2. Columns 1 and 2 show means and standard deviations for relevant variables in all jurisdictions. Overall, there were 1,938 opioid-related ER visits per state and 0.7 opioid-related deaths per 100,000 population; there were also 47.7 opioid-related crimes per million population, 1.9 of which were opioid-related theft. Columns 3 and 4 show 2010 baseline means for states that adopted naloxone access laws relatively early (before the median month), while Columns 5 and 6 show baseline measures for late-adopting states (those implementing naloxone access laws after the median month). Early- and late-adopters look different on some measures (particularly ER visits), but quite similar on others (most notably, opioid-related mortality). We will control for jurisdiction fixed effects and state-specific trends in our outcome measures to account for these pre-existing differences across states.

5 Empirical strategy

To estimate the effect of naloxone access on behavior, we exploit variation in the timing of state laws that broaden naloxone access. We use the effective dates of naloxone access policies as exogenous shocks to the risk of death from opioid use, in a difference-in-differences (DD) framework. States vary considerably in the timing of law passage, as shown in Figure 3.

We categorize each state as having expanded naloxone access if a naloxone law is passed at any date within the month, and for all months afterward.

The DD framework relies on the assumption that places that have not (yet) expanded access to naloxone are informative counterfactuals for places that have expanded access. The identifying assumption is that, absent the policies, and conditional on a broad set of control variables, our outcome measures of interest would have evolved similarly in treatment and control jurisdictions. (This is commonly referred to as the parallel trends assumption.) An ideal experiment would randomly assign some places to have broad access to naloxone and others not. Expansion of naloxone access is not random, and may be a response to increasing mortality from opioid use. It might also be correlated with other local efforts to address the opioid epidemic.

Given these concerns, we pay close attention to the parallel trends assumption. We control for a variety of factors and examine pre-existing trends to ensure as best we can that changes in the outcomes studied are attributable to the causal effects of broadening naloxone access, rather than to other differences between places that broaden access to this drug. In particular, we will control for other laws that states adopted that might affect opioid use and abuse.

The DD regression specification for crime rates is as follows (we use analogous specifications for other outcomes):

$$\begin{aligned} CrimeRate_{jt} &= \beta\ NaloxoneLaw_{jt} + \alpha_j + m_t + S_j \times t \\ &+ Police_{jt} + OtherLaws_{jt} + \epsilon_{jt}, \end{aligned} \tag{1}$$

where j denotes the jurisdiction (i.e., city, county, or state) and t denotes the month-year (or quarter-year) of observation. The treatment variable, $NaloxoneLaw$, is a dummy variable that equals one if the state has a naloxone access law as of time t. The term α_j is a fixed effect for each jurisdiction (accounting for average differences across places), and m_t is a month-of-sample (or quarter-of-sample) fixed effect (controlling flexibly for national trends in opioid abuse). The $S_j \times t$ terms are state-specific linear time trends that absorb pre-existing state trends in the outcome measure. (Our results are robust to including jurisdiction-specific linear time trends instead.) $Police_{jt}$ is the log of police officers per capita in the jurisdiction, and it varies over time; we include this as a proxy for law enforcement policies and public safety investments that might independently affect opioid abuse and crime rates. $OtherLaws_{jt}$ is a time-varying vector of other state-specific laws that the literature has identified as relevant to opioid use and abuse.[16] The term ϵ is an error term that is clustered at the state level for estimation. All estimates that use rates as the dependent variable are population-weighted.

Our identifying assumption is that we are controlling for all relevant trends and policies that are correlated with the timing of naloxone access laws. We will show pre-trends in coefficient plots for our outcome measures, as visual evidence that our controls are adequately absorbing pre-existing variation. We will also show how our estimates are affected as we layer in our various controls: to the extent that estimates stabilize and are unaffected by additional variables, that should reduce concerns about omitted variable bias.

Note that our treatment variable, the implementation of naloxone access laws, represents an *intent to treat*. The actual treatment of interest is lowering the risk of death associated with a particular opioid dose. The amount that this risk falls will depend on a variety of factors that affect naloxone availability, including naloxone access laws as well as naloxone's price and the number of doses distributed for free by community groups and public health organizations. In an ideal scenario, naloxone access laws lead immediately to everyone having easy access to naloxone when they need it. To the extent that this does not happen—i.e., that the intent to treat does not indicate actual treatment—our estimates will be biased toward zero.

[16] This list of laws is taken from Meara et al. (2016); as described above, we use their database of policy timing and extend it through 2015.

6 Results

6.1 Salience of naloxone and interest in drug treatment

We first consider the salience of naloxone access laws: is there evidence that the laws affected residents' knowledge about naloxone and interest in obtaining it? (Without data on actual naloxone distribution or purchases, this is as close as we can get to a first stage.) To address this, we use Google Trends data from 2010 through 2015, quantifying online searches for "naloxone" and related queries.[17]

Results are show in Figure 4, and in column 1 of Table 3. The figure is a coefficient plot, showing estimates in each quarter before and after the law change. The coefficient for the quarter just before the law change is indexed to 0. The left-most coefficient shows the estimate for 5+ quarters before the law change. The right-most coefficient shows the estimate for 3+ quarters after the law change. Pre-law effects on "naloxone" searches are flat and near-zero, indicating that our control variables sufficiently absorb pre-period trends. At the date of the law's implementation, the coefficient increases; it remains above the earlier coefficients in quarters 1 through 3+. On average, the coefficients after the law change are higher than the coefficients before the law change. The regression results tell a similar story: naloxone access laws cause the local intensity of Google searches for "naloxone" to increase by 7.2% ($p < 0.05$). This indicates that the laws had a meaningful impact on residents' knowledge of and interest in naloxone.

Next we consider whether naloxone access laws affected interest in drug treatment or rehabilitation programs. If moral hazard is operating in this context, we would expect that reducing the risks associated with using opioids would reduce opioid users' interest in getting treatment. We again use Google Trends data as an indicator of local residents' interest. The effect of naloxone access laws on searches for "drug rehab" (and related queries) is shown in column 2 of Table 3. We find that the intensity of searches for "drug rehab" falls by 1.4%

[17] Google aggregates a number of related search terms into the "naloxone (drug)" category. We use this aggregation as our outcome measure, as described above.

($p < 0.10$). This effect is small and marginally significant, so provides suggestive evidence that naloxone access reduces local interest in treatment for opioid addiction.

6.2 Effects on opioid-related arrests

Here, we show the results of naloxone access on arrests related to selling or possessing opioids. We consider possession as a proxy for the quantity of illegal opioid demanded, acknowledging that it is an equilibrium outcome. Column 3 of Table 3 shows an increase in the arrest rate for possession of opioid drugs after naloxone access laws go into effect: the monthly arrest rate increases by 4.0 per million residents (17% of the baseline, $p < 0.05$). Column 4 of Table 3 shows the effect of naloxone access laws on arrests for the illegal sale of opioids. We consider this an indicator of quantity supplied, which should move with quantity demanded. Indeed, we find that monthly arrests for the sale of opioids increase by 1.9 per million residents each month (27%, $p < 0.01$) after naloxone access laws are implemented. Given increases in both quantity demanded and quantity supplied, it appears that naloxone access laws increased the level of activity in the illegal opioid market; this suggests an increase in consumption of illegal opioids. (At the very least, more people are being arrested for their use and sale of opioids, which is costly to them and to society.)

We also examine effects by opioid type, particularly fentanyl which was increasing in use over this time period. Columns 5 and 6 of Table 3 consider effects on arrests that involve "other opioids" (a category likely dominated by fentanyl). Distinguishing between heroin and fentanyl is difficult at the time of arrest (drugs would need to be sent to a lab for testing), so we expect these data to be noisy and interpret the results as suggestive. We find that 64% of the increase in arrests for opioid possession involves fentanyl, and that this increase represents a 21% increase in fentanyl possession over its baseline ($p < 0.05$). About 41% of the increase in arrests for selling opioids comes from selling fentanyl, representing a 29% increase in fentanyl sales over the baseline (not statistically significant). These estimates are about the same as for all opioids.

Not all opioid abuse will show up in arrest data. In addition, we might worry that the implementation of naloxone access laws makes opioid abuse more salient to police and that this in turn makes police more likely to record opioid possession in their reports. We expect the bias to go in the opposite direction—Good Samaritan laws and naloxone access laws typically reduced or eliminated criminal liability for drug offenses when someone is overdosing, and there was a general trend toward treating opioid addiction as a health problem instead of a criminal offense during this period. For example, Good Samaritan laws provide "immunity from arrest, charge, or prosecution for drug possession or paraphernalia when individuals who are experiencing or witnessing an overdose summon emergency services."[18] Yet, we cannot rule out the possibility that reporting of opioid involvement increased. This is a shortcoming of using crime data in this context, and is one reason we use a variety of data sources to investigate the impacts of these laws.

6.3 Effects on ER visits and mortality

We use HCUP data to consider the effect of naloxone access laws on opioid-related ER visits. These results are shown in the bottom-left of Figure 5 and in column 1 of Table 4. The figure shows that pre-law effects are flat and near-zero; after the law change, the effects increase. Consistent with this visual evidence of a change, our regression results show that broadening naloxone access led to more opioid-related ER visits: naloxone access laws increased the quarterly number of visits by 266 (15%, $p < 0.05$). This effect is large, and likely some of the increase comes from a greater propensity to seek care in the ER following expanded naloxone access.

The next question is the effect of naloxone on opioid-related mortality, shown in the top-left of Figure 5 and Column 2 of Table 4. On average across all urban areas, we find that these laws have no significant impact on the opioid-related death rate. This is also true for deaths attributable to fentanyl, as shown in the top-right of Figure 5 and Column 3 of Table 4. These results are compelling, as they suggest that naloxone may require complementary services to reduce deaths from opioid overdose, the ultimate goal of policy.

[18] https://www.countyhealthrankings.org/take-action-to-improve-health/what-works-for-health/strategies/good-samaritan-drug-overdose-laws

6.4 Effects on opioid-related crime

Naloxone access saves, or at least extends, the lives of many existing opioid abusers and may increase the number of new opioid abusers. Both effects could increase criminal activity, particularly theft committed to fund an addiction. Table 5 considers the effect of naloxone access on crime rates. Columns 1 and 2 show that broadening naloxone access increases all opioid-related crime by 6.0 per million (15%, $p < 0.05$), and opioid-related theft by 0.4 per million (30%, $p < 0.10$). The bottom-right of Figure 5 shows a coefficient plot for the effect on opioid-related theft; while the pre-period trend is flat, there is a clear increase after naloxone access laws went into effect.

These opioid-related crimes are offenses where we know for sure that opioids were related in some way (for example, the offender may have had illegal opioids on them at the time of the offense, or was stealing opioids), but the policy-relevant question is whether the total amount of crime increases. Column 3 of Table 5 shows the effect of naloxone access laws on all theft: the coefficient is imprecisely estimated, but positive and larger than the effect on opioid-related theft alone. The magnitude of the coefficient suggests that 4.8 (0.3%, not significant) more thefts per million residents were reported each month after naloxone laws are passed. This effect is larger than the impact on opioid-related thefts alone, but suggest that the social costs of naloxone laws in terms of additional property crime are small.[19]

6.5 Increasing availability of fentanyl

Throughout this period, the supply of fentanyl—a potent synthetic opioid—was increasing across the country. It's likely that this contributed to rising opioid-related mortality

[19] We also investigated the interaction of naloxone laws with drug treatment availability (as measured by county counts of drug treatment facilities per 100,000 residents) and Medicaid expansion. Table B.4 shows effects for opioid- and fentanyl-related mortality, by quartile of treatment availability (Q1 is low, Q4 is high). Since we do not have county identifiers in the HCUP data, we could not conduct this analysis for ER visits. The only statistically significant result is in column (5), indicating that areas with low access to drug treatment experience more fentanyl deaths. Given the lack of statistical significance on any other outcome to help map out this relationship, however, we cannot be sure whether naloxone access interacts with drug treatment availability. Table B.5 shows the effects of broadening naloxone access separately for states that did and did not expand Medicaid eligibility by 2015. The only statistically significant coefficient is on the positive effect of naloxone on opioid-related ER visits in states that did not expand Medicaid (column (1)) – we refrain from interpretation here given the wide confidence intervals on the mortality outcomes.

rates, and it is crucial that our estimates not confound the effects of naloxone access laws with independent changes in fentanyl supply. We believe we have successfully isolated the effect of naloxone access laws on mortality. Recall that Figure 5 shows coefficient plots for fentanyl-related mortality. If our estimates were driven by pre-existing trends in fentanyl availability, we would see an increase in fentanyl-related deaths before the naloxone access laws go into effect. Instead, we see flat pre-trends before the laws are implemented; this gives us confidence that we are isolating the causal effect of those laws.

That said, naloxone access laws may be interacting with the increasing availability of fentanyl in a way that has particularly deadly consequences. Many overdoses occur when users don't realize their heroin is spiked with fentanyl. Since a single dose of naloxone usually won't be enough to save someone overdosing on fentanyl[20], those overdoses could become deaths, even when naloxone is available. We might expect that a rational drug user would perfectly compensate for the reduction in risk in a way that leads to no net change in expected mortality. In practice, such perfect compensation is extremely difficult and requires a level of information that individual users are unlikely to have (including the likelihood that their heroin is contaminated, and the likelihood that naloxone will save them). While we cannot test this hypothesis directly, we consider variation in the local availability of fentanyl to be another potential source of variation in the effects of naloxone access laws.

7 Robustness checks and additional analyses

7.1 Alternative controls for pre-treatment trends

Our main estimation strategy includes state-specific linear time trends. A potential concern is that these time trends may dilute our findings if the treatment effect increases over time, leading to an estimation of null effects for various outcomes (Meer and West 2016; Borusyak and Jaravel 2017). To alleviate this concern, we estimate a model in which the state-specific linear time trends are estimated only based on the pre-treatment period

[20] Medical professionals recommend bringing someone to the emergency room after they've been revived using naloxone, so that additional doses of naloxone can be administered if needed.

(Goodman-Bacon 2021), i.e., before the state passed a naloxone law. Table B.6 shows the estimates from this alternative approach. Estimated effects on mortality are slightly more positive but still statistically insignificant.

7.2 Differences by urban and rural classification

We focus our main analysis on urban areas, as that is where the majority of opioid-related deaths occur and is where we expect naloxone availability and distribution to have the biggest impact. We check whether naloxone saves lives in rural areas in Table B.7. Panel A shows results across rural areas, and Panel B shows results for the entire U.S. The coefficients related to the mortality outcomes are not statistically significant in either sample. We also show how our mortality and theft results change by varying the population cutoff for "urban" in Table B.8. Recall that the definition we use in our main analyses is a city population of at least 40,000. The estimated effects of naloxone access on mortality are near zero and statistically insignificant at all population cutoffs from 10,000 through 55,000. For opioid-related theft, the coefficients are actually a bit smaller and less statistically significant at higher populations, though they are qualitatively similar across the table.

7.3 Checking for a change in recording of opioid involvement

Deaths due to opioid abuse have often been labeled as due to an "unspecified" drug (Ruhm 2017; Ruhm 2018). To consider whether our mortality results could be driven by improved labeling of opioid involvement in CDC data, we test the effect of naloxone access laws on a broader category of mortality: deaths due to opioids or an unspecified drug. The top panel of Table B.9 shows the results. The coefficient remains non-zero and statistically-insignificant, suggesting that our mortality results are not driven by a change in how opioid-related deaths are recorded. We observe the same pattern for the result on all theft, a broader category of the opioid-related thefts in the main analysis.

7.4 Sensitivity of estimates to additional controls

Since the adoption of naloxone access laws is not random, we control for a variety of factors that might be correlated with the adoption of these laws and could independently affect our outcomes of interest. In Tables B.10 through B.18 we show how each of our estimates change as we layer additional controls. We find a stable pattern, limiting the scope for omitted variable bias in our analysis. For example, in Table B.10, adding month-of-sample fixed effects has a large impact on the coefficients (which is not surprising), but the subsequent changes are smaller. Adding state-specific linear trends, which account for pre-existing trends in opioid abuse, reduces the coefficient slightly. After that, controlling for police per capita (our proxy for law enforcement investment), Good Samaritan laws, and an array of opioid laws had essentially no effect on the estimate – the estimate in Column 3 is nearly identical to that in Column 8. This stable pattern is similar for the other outcomes.

7.5 Timing of laws

We have coded naloxone access laws based on whether they substantially broadened access to naloxone (in particular, allowing third party prescriptions or standing orders throughout the state). In some states, earlier pilot programs at the county level or related legislation could be reasonably interpreted as substantially broadening naloxone access. In such cases, where reasonable people could disagree about what the correct date is, we code alternate dates, and check the robustness of our results to using those alternate dates instead. These are detailed in Table B.19; the estimates are extremely similar to those discussed above.

There may also be a concern that states which expanded naloxone access are not comparable to states that did not, at least during the study period. To address this possibility, we re-estimated our model on all main outcomes using the sample of 40 states that expanded naloxone access prior to 2016. Table B.2 presents the results – we find that all the point estimates are similar to those in our main analysis, though we lose some statistical significance due to the reduced sample. We also note that much of the variation in naloxone access laws occurs in 2014 and 2015. To address a concern of whether policies in these years confound the effect of naloxone access in our estimation, we examine heterogeneity in naloxone laws by year of passage (19 states in 2014 or before versus 21 states in 2015). We plot the main outcomes in Figure B.1, and present the comprehensive set of results in Table B.3. We find that the effect of naloxone laws is similar across the two groups for all outcomes that are statistically significant in the main analysis.

7.6 Comparison to previous literature

A closely related paper is Rees et al. (2019), which finds that naloxone laws reduce opioid-related mortality. In an effort to reconcile this finding with the present analysis that fails to find statistically significant effects, we estimate that paper's specification on our sample. We are able to replicate all aspects of that paper except one: Rees et al. (2019) are able to use data going back to 1999, while our dataset begins in 2010. It turns out that this difference is responsible for that paper finding a negative and statistically significant effect of naloxone laws on opioid-related mortality.

We attempt to replicate the estimate in Rees et al. (2019) in Table B.20 despite not being able to extend the data back to 1999.[21] In column 8, our specification is identical to Rees et al. (2019) with the exception of going back to 1999; yet, our estimate is *positive* and marginally significant ($p < 0.10$). For this reason, we suspect (but cannot confirm directly) that the difference in estimates comes from the longer pre-period used in that paper. While that paper's longer pre-period is advantageous in some ways, it may also have generated a misleading linear time trend in opioid-related deaths when the epidemic shifted towards heroin (versus prescription opioid) deaths in the 2010 to 2013 period as noted in Maclean et al. (2020). For example, Rees et al. (2019) may have overestimated the time trend in opioid-related deaths in the 2010 to 2015 period because of fitting a line to this sudden spike in deaths, leading them to uncover negative effects of naloxone access on this outcome.

[21] Column 1 shows that paper's estimate of -0.188 (p-value < 0.10), and column 2 shows our estimate of 0.006 (p-value > 0.10). Columns 3 through 8 then layer in the Rees et al. (2019) specification to our analysis to demonstrate how the coefficient changes as we modify the specification choice. For example, column 2 aggregates to the state level, column 3 to the year level, column 5 converts the dependent variable to log mortality rate, column 6 drops the 2015 data not used in that paper, column 7 adds other controls such as beer taxes, and column 8 matches the dates of naloxone law passage.

While we cannot be sure about the better specification, this exercise demonstrates an effort to reconcile the estimate with an important prior paper to help readers distinguish these analyses.

8 Discussion

Policymakers have multiple levers available to fight opioid addiction, and broadening naloxone access aims to directly address the most dire risk of opioid abuse: death. Naloxone can save lives and provide a second chance for addicted individuals to seek treatment. There is a concern, however, that widespread access to the safety net drug can unintentionally increase riskier opioid use and its related problems. This paper shows that, as implemented through 2015, naloxone access laws—a proxy for expanding access to naloxone—did not measurably reduce opioid-related mortality. Our analysis is limited by the lack of data on actual naloxone distribution, so our estimates represent ITT effects. If naloxone access laws did not broaden naloxone access as much as proponents had hoped, then our estimates will be biased toward zero. We acknowledge that to fully estimate the empirical implications of behavioral response to naloxone access, we would need to observe the change in mortality from naloxone access with and without such response. Our results remain suggestive as such an analysis is not possible.

We emphasize strongly that our findings do not imply that we should limit naloxone availability to individuals suffering from opioid addiction, or those who are at risk of overdose; naloxone has been available through prescription for a long time to these groups, and serves an important role in reducing the risk of opioid overdose deaths. Instead, we should try to find ways to mitigate possible unintended consequences.

References

Abouk, Rahi, Rosalie Liccardo Pacula, and David Powell. 2019. "Association Between State Laws Facilitating Pharmacy Distribution of Naloxone and Risk of Fatal Overdose." JAMA Internal Medicine.

Alpert, Abby, David Powell, and Rosalie Liccardo Pacula. 2018. "Supply-side drug policy in the presence of substitutes: Evidence from the introduction of abuse-deterrent opioids." American Economic Journal: Economic Policy, 10(4): 1–35.

Anderson, D. Mark, Benjamin Crost, and Daniel I. Rees. 2017. "Wet laws, drinking establishments and violent crime." The Economic Journal, 128(611): 1333–1366.

Baltimore City Health Department. 2018. "Substance use and misuse." Available at https://health.baltimorecity.gov/programs/substance-abuse.

Becker, Gary S., and Kevin M. Murphy. 1988. "A theory of Rational Addiction." Journal of Political Economy, 96(4): 675–700.

Black, Bernard, Alex Hollingsworth, Leticia Nunes, and Kosali Simon. 2021. "Simulated Power Analyses for Observational Studies: An Application to the Affordable Care Act Medicaid Expansion." NBER Working Paper No. 25568.

Bondurant, Samuel R., Jason M. Lindo, and Isaac D. Swensen. 2018. "Substance abuse treatment centers and local crime." Journal of Urban Economics, 104: 124–133.

Borusyak, Kirill, and Xavier Jaravel. 2017. "Revisiting event study designs." Working paper.

Buchmueller, Thomas C., and Colleen Carey. 2018. "The effect of Prescription Drug Monitoring Programs on opioid utilization in Medicare." American Economic Journal: Economic Policy, 10(1): 77–112.

Centers for Disease Control and Prevention. 2021. "Drug overdose death data." Available at: https://www.cdc.gov/drugoverdose/data/statedeaths.html.

Chan, Tat Y., Barton H. Hamilton, and Nicholas W. Papageorge. 2015. "Health, risky behaviour and the value of medical innovation for infectious disease." Review of Economic Studies, 83(4): 1465–1510.

Ciccarone, Daniel. 2009. "Heroin in brown, black and white: Structural factors and medical consequences in the US heroin market." International Journal of Drug Policy, 20(3): 277–282.

Cohen, Alma, and Liran Einav. 2003. "The effects of mandatory seat belt laws on driving behavior and traffic fatalities." Review of Economics and Statistics, 85(4): 828–843.

Cohen, Alma, and Rajeev Dehejia. 2004. "The effect of automobile insurance and accident liability laws on traffic fatalities." Journal of Law and Economics, 47(2).

Cook, Philip J., and Christine P. Durrance. 2013. "The virtuous tax: lifesaving and crime-prevention effects of the 1991 federal alcohol-tax increase." Journal of Health Economics, 32(1): 261–267.

Davis, Corey S., and Derek Carr. 2015. "Legal changes to increase access to naloxone for opioid overdose reversal in the United States." Drug and Alcohol Dependence, 157: 112–120.

Deiana, Claudio, and Ludovica Giua. 2021. "The Intended and Unintended Effects of Opioid Policies on Prescription Opioids and Crime." The BE Journal of Economic Analysis & Policy, 21(2): 751–792.

Erfanian, Elham, Daniel Grossman, and Alan R. Collins. 2019. "The impact of naloxone access laws on opioid overdose deaths in the US." Review of Regional Studies, 49(1): 45–72.

Evans, William N., Ethan Lieber, and Patrick Power. 2017. "How the reformulation of OxyContin ignited the heroin epidemic." Review of Economics and Statistics, accepted.

Goodman-Bacon, Andrew. 2021. "Difference-in-differences with variation in treatment timing." Journal of Econometrics.

Grossman, Michael, and Frank J. Chaloupka. 1998. "The demand for cocaine by young adults: a rational addiction approach." Journal of Health Economics, 17(4): 427–474.

Hansen, Benjamin, Keaton Miller, and Caroline Weber. 2017. "The taxation of recreational marijuana: Evidence from Washington state." National Bureau of Economic Research Working Paper No. 23632.

Kilby, Angela. 2015. "Opioids for the masses: welfare tradeoffs in the regulation of narcotic pain medications." Working Paper, available at http://economics.mit.edu/files/11150.

Kilmer, Beau, Nancy Nicosia, Paul Heaton, and Greg Midgette. 2013. "Efficacy of frequent monitoring with swift, certain, and modest sanctions for violations: Insights from South Dakota's 24/7 Sobriety Project." American Journal of Public Health, 103(1): e37–43.

Lakdawalla, Darius, Neeraj Sood, and Dana Goldman. 2006. "HIV breakthroughs and risky sexual behavior." Quarterly Journal of Economics, 121(3): 1063–1102.

Lewis, Nicole, Emma Ockerman, Joel Achenbach, and Wesley Lowery. 2017. "Fentanyl linked to thousands of urban overdose deaths." Washington Post. Available at https://www.washingtonpost.com/graphics/2017/national/fentanyl-overdoses/.

MacCoun, Robert, Beau Kilmer, and Peter Reuter. 2003. "Research on drugs-crime linkages: The next generation." NIJ special report.

Maclean, Catherine, Justine Mallatt, Christopher J. Ruhm, and Kosali I. Simon. 2020. "Review of economic studies on the opioid crisis." NBER Working Paper No. 28067.

Mallatt, Justine. 2017. "The effect of Prescription Drug Monitoring Programs on opioid prescriptions and heroin crime rates." Working paper, available at https://sites.google.com/site/justinemallatt/research/jobmarketpaper.

McClellan, Chandler, Barrot H. Lambdin, Mir M. Ali, Ryan Mutter, Corey S. Davis, Eliza Wheeler, Michael Pemberton, and Alex H. Kral. 2018. "Opioid-overdose laws association with opioid use and overdose mortality." Addictive Behaviors, 86: 90–95.

Meara, Ellen, Jill R. Horowitz, Wilson Powell, Lynn McClelland, Weiping Zhou, A. James O'Malley, and Nancy E. Morden. 2016. "State legal restrictions and prescription-opioid use among disabled adults." New England Journal of Medicine, 375(1): 44–53.

Meer, Jonathan, and Jeremy West. 2016. "Effects of the minimum wage on employment dynamics." Journal of Human Resources, 51(2): 500–522.

Moore, Tim, and Kevin Schnepel. 2021. "Opioid use, health and crime: Insights from a rapid reduction in heroin supply." NBER Working Paper No. 28848.

Mueller, Shane R., Alexander Y. Walley, Susan L. Calcaterra, Jason M. Glanz, and Ingrid A. Binswanger. 2015. “A review of opioid overdose prevention and naloxone prescribing: Implications for translating community programming into clinical practice.” Substance Abuse, 36(2): 240–253.

Packham, Analisa. 2020. “Are syringe exchange programs helpful or harmful? New evidence in the wake of the opioid epidemic.” NBER Working Paper No. 26111.

Peltzman, Sam. 1975. “The effects of automobile safety regulation.” Journal of Political Economy, 83(4): 677–725.

Quinones, Sam. 2015. Dreamland: The true tale of America’s opiate epidemic. Bloomsbury Press.

Reed, Patrick. 2016. “The Opioid Epidemic: What Works, and What is Still Needed?” Kentucky Education Television: Inside the Opioid Epidemic. Available at https://www.ket.org/opioids/the-opioid-epidemic-what-works-and-what-is-still-needed/.

Rees, Daniel I, Joseph J Sabia, Laura M Argys, Dhaval Dave, and Joshua Latshaw. 2019. “With a little help from my friends: The effects of good Samaritan and naloxone access laws on opioid-related deaths.” The Journal of Law and Economics, 62(1): 1–27.

Ruhm, Christopher J. 2017. “Geographic variation in opioid and heroin involved drug poisoning mortality rates.” American Journal of Preventive Medicine, 53(6): 745–753.

Ruhm, Christopher J. 2018. “Corrected US opioid-involved drug poisoning deaths and mortality rates, 1999–2015.” Addiction, 113(7): 1339–1344.

Schnell, Molly. 2017. “Physician behavior in the presence of a secondary market: The case of prescription opioids.” Working paper, available at https://scholar.princeton.edu/schnell/files/schnell_jmp.pdf.

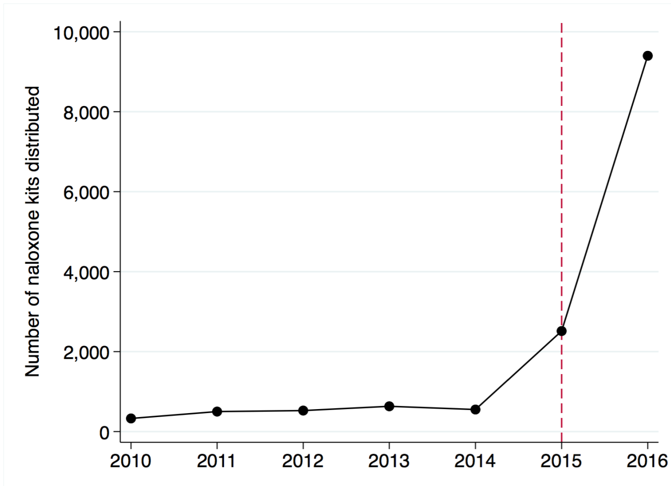
Scutti, Susan, and Robert Jimison. 2018. “Surgeon general urges more Americans to carry opioid antidote naloxone.” CNN. Available at https://www.cnn.com/2018/04/05/health/surgeon-general-naloxone/index.html.

Shesgreen, Deirdre. 2016. "Doctor wants overdose antidote in every medicine cabinet." USA Today. Available at https://www.usatoday.com/story/news/health/2016/03/04/doctor-wants-overdose-antidote-every-medicine-cabinet/81291850/.

Simon, Kosali, Aparna Soni, and John Cawley. 2017. "The impact of health insurance on preventive care and health behaviors: Evidence from the first two years of the ACA Medicaid expansions." Journal of Policy Analysis and Management, 36(2): 390–417.

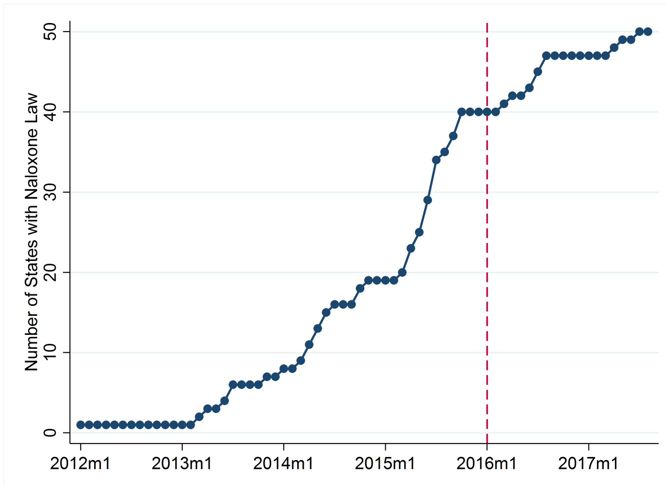
Swensen, Isaac D. 2015. "Substance-abuse treatment and mortality." Journal of Public Economics, 122: 13–30.

Figure 1: Distribution of naloxone kits in Baltimore



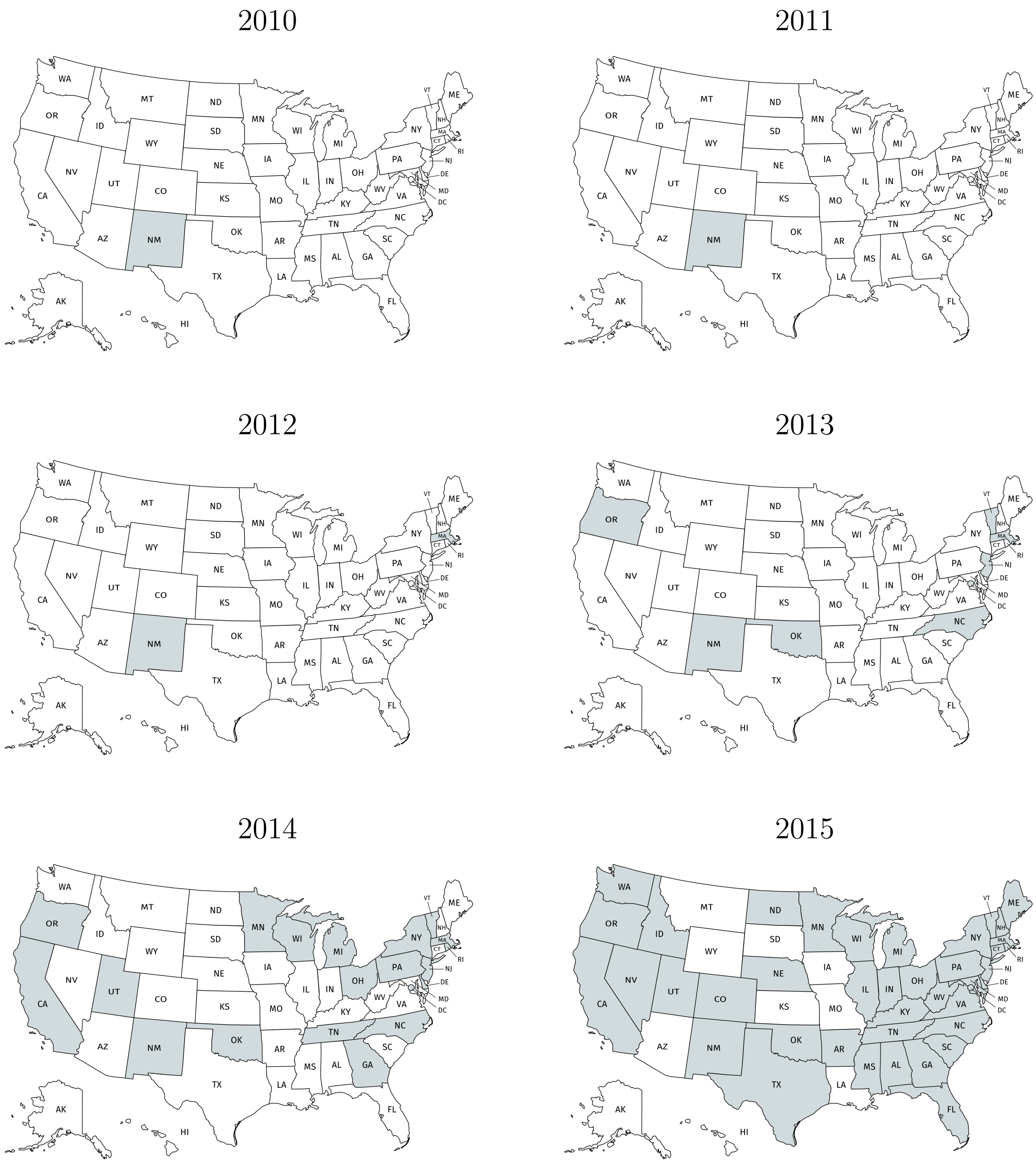
Notes: Figure shows the number of naloxone kits distributed in Baltimore before and after Maryland broadened naloxone access. Maryland's naloxone access law went into effect in October 2015.

Figure 2: Timeline of naloxone access laws



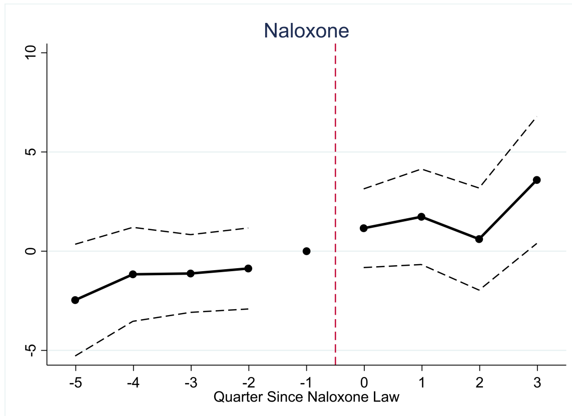
Notes: Figure shows the number of states with any broadened naloxone access law in each month-year between January 2012 and July 2017, by which point all states had such laws. The data include all 50 states. Categorization of state-by-state naloxone laws was done using hand-collected data. Our analyses use data through December 2015 (indicated by the vertical line).

Figure 3: States with naloxone access laws, by year



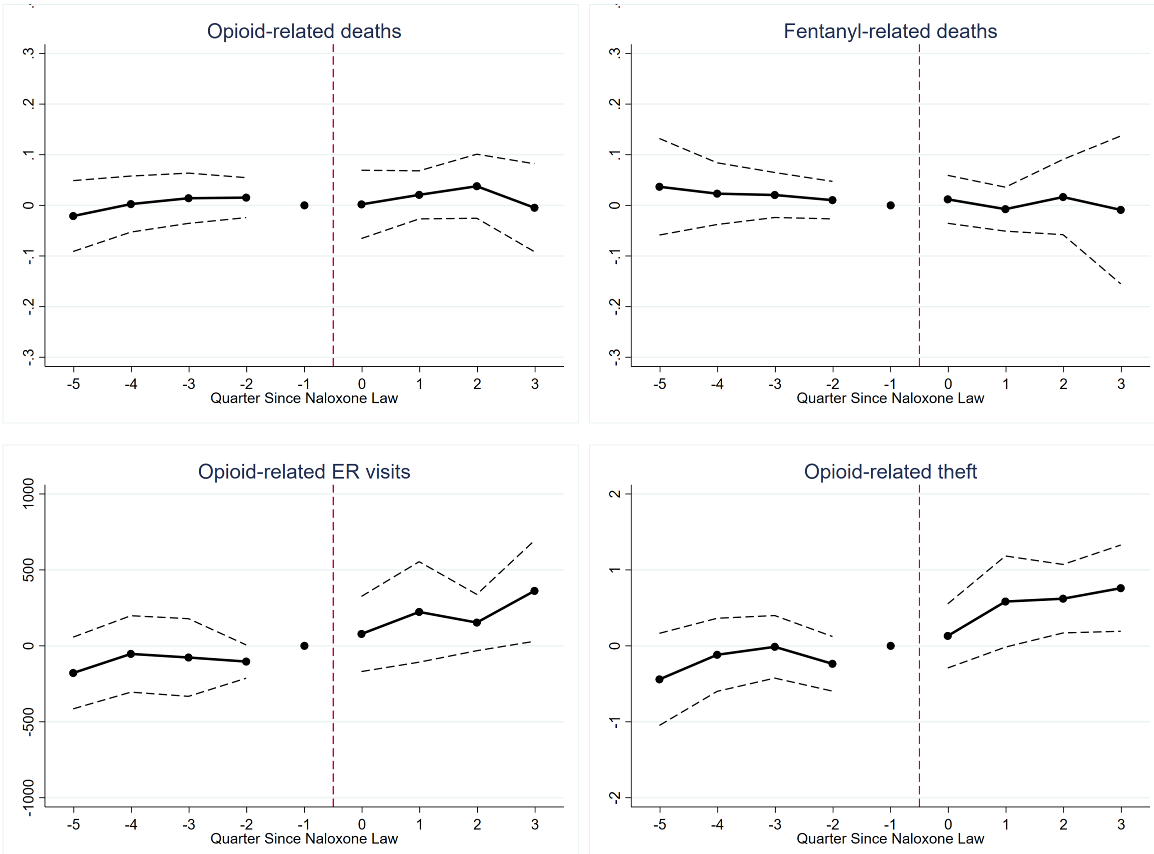
Notes: Figure shows the states with naloxone access laws by December 31 of each year. These states are shaded; New Mexico was the first state to broaden access and did so in 2001.

Figure 4: Effect of naloxone access laws on Google searches for “naloxone”



Notes: Figure shows coefficients of the impact of broadened naloxone access on Google searches for the “naloxone” topic. The specification follows equation (1), but includes dummies for the quarter from the passage of naloxone laws instead of a dummy for whether there is such a law. I.e., the covariate $NaloxoneLaw_{jt}$ becomes $\sum_{q=-4}^{q=3} NaloxoneLaw_{jq}$, where $q = 0$ for the month of and two months following the effective date of a broadened naloxone law. The first category $q = -4$ represents four or more quarters prior to broadened naloxone access, and the final category $q = 3$ equals 1 for the third quarter and all subsequent quarters. The reference category is the period $q = -1$, which is the quarter prior to naloxone access. The vertical bands represent 95% confidence intervals. Data source: Google trends. Date range: 2010-2015.

Figure 5: Effect of naloxone access laws on outcomes



Notes: See notes for Figure 4. Data source: CDC (for mortality), HCUP (for ER admissions), and NIBRS (for arrests and crime). Date range: 2010-2015 (CDC and NIBRS), 2006-2015 (HCUP).

Table 1: Timing of naloxone laws by state

State (1)	Date (2)	State (3)	Date (4)
AL	Jun 2015	NV	Oct 2015
AR	Jul 2015	NH	Jun 2015
CA	Jan 2014	NJ	Jul 2013
CO	Apr 2015	NM	Apr 2001
CT	Jun 2015	NY	Jun 2014
DE	Jun 2014	NC	Apr 2013
DC	Mar 2013	ND	Aug 2015
FL	Jun 2015	OH	Mar 2014
GA	Apr 2014	OK	Nov 2013
ID	Jul 2015	OR	Jun 2013
IL	Sep 2015	PA	Nov 2014
IN	Apr 2015	RI	Oct 2014
KY	Mar 2015	SC	Jun 2015
LA	Jun 2016	TN	Jul 2014
ME	Oct 2015	TX	Sep 2015
MD	Oct 2015	UT	May 2014
MA	Aug 2012	VT	Jul 2013
MI	Oct 2014	VA	Apr 2015
MN	May 2014	WA	Jul 2015
MS	Jul 2015	WV	May 2015
NE	May 2015	WI	Apr 2014

Notes: Table shows the month-year of broadened naloxone access via third party prescription or standing order by state. Nine states broadened naloxone access in January 2016 or afterwards: AK, IA, SD, HI, AZ, MO, KS, MT, and WY. (Source: Hand-collected information.)

Table 2: Summary statistics

	All years		Baseline rates (2010)			
	All jurisdictions		Early Adopters		Late Adopters	
	Mean	SD	Mean	SD	Mean	SD
	(1)	(2)	(3)	(4)	(5)	(6)
Google Trends search intensity						
"Naloxone"	27.98	25.19	26.75	31.91	23.33	33.39
"Drug rehab"	50.25	19.95	57.10	23.09	53.61	25.88
N (City-months)	21,528		2,172		1,416	
Opioid-related ER visits						
	1938	2360	2420	2149	898.1	739.5
N (State-quarters)	1,108		64		52	
Mortality rates						
Opioid-related deaths	0.716	0.693	0.595	0.600	0.613	0.560
Fentanyl-related deaths	0.119	0.291	0.083	0.205	0.073	0.157
N (County-months)	55,512		6,576		2,676	
Crime rates						
Possession of opioids	29.50	39.96	24.20	35.74	19.78	22.48
Selling opioids	8.255	17.93	7.645	17.57	3.262	7.963
All opioid-related crime	47.72	58.04	40.95	52.54	30.45	29.39
Heroin	27.62	46.74	18.66	34.66	8.331	12.54
Other Narcotics	18.93	29.72	21.40	34.05	21.10	27.47
Opioid-involved theft	1.862	5.183	1.322	4.213	1.618	4.181
Theft (all)	1727	961.0	1766	980.2	2194	904.6
Marijuana-related crime	229.2	193.0	234.8	202.6	283.6	147.8
N (Jurisdiction-months)	29,952		4,200		792	

Notes: Google Trends data are a normalized index from 0 to 100; observations are at the metro area-month level. Opioid-related ER visits are counts from HCUP and recorded at the metro area-quarter level. Mortality is from restricted-use CDC data, recorded at the county-month level. Mortality rates are per 100,000 residents. Crime data is from NIBRS and is aggregated to the jurisdiction-month level. Arrest and crime rates are per million residents. Sample includes urban areas during years 2010-2015 (2006-2015 for HCUP data). "Early adopters" are states that adopted naloxone access laws before the median adoption month; "late adopters" are the states that adopt later.

Table 3: Effect of naloxone laws on Google searches and opioid-related arrests

	Google trends		Arrests			
	"Naloxone" searches	"Drug rehab" searches	Possession of opioids	Selling opioids	Possession of fentanyl	Selling fentanyl
	(1)	(2)	(3)	(4)	(5)	(6)
Naloxone Law	1.847**	-0.799*	4.030**	1.917***	2.578**	0.780
	(0.809)	(0.450)	(0.675)	(0.214)	(1.155)	(0.479)
Observations	20,232	21,528	29,808	29,808	29,808	29,808
2010 baseline	25.49	55.72	23.52	6.972	12.28	2.729

Notes: * $p < .10$, ** $p < .05$, *** $p < .01$. Standard errors are clustered by state and shown in parentheses. Sample includes metro areas (for Google trends data) and jurisdictions with populations $\geq$ 40,000 (for NIBRS data on arrests). Date range: 2010-2015. All regressions include: jurisdiction FEs, month of sample FEs, state-specific linear trends, police per capita, and the following laws/regulations: Good Samaritan laws, PDMP, Doctor Shopping, Pain Clinic regulations, Physician exams, Pharmacy verification, require ID, and tamper-resistant PF. Coefficients show the effect of expanding naloxone access on the index for the specified Google search term (columns 1 and 2), and arrests per million residents (columns 3-6). Coefficients in columns 1 to 4 are population-weighted as the dependent variable is a rate.

Table 4: Effect of naloxone laws on opioid-related ER visits and mortality

	Opioid-related ER visits	Opioid-related deaths	Fentanyl-related deaths
	(1)	(2)	(3)
Naloxone Law	265.9**	0.006	-0.003
	(121.6)	(0.027)	(0.030)
Observations	1,108	55,512	55,512
2010 baseline	1738	0.601	0.080

Notes: * $p < .10$, ** $p < .05$, *** $p < .01$. Standard errors are clustered by state and shown in parentheses. Sample includes metro areas (for HCUP data on ER admissions) and counties with at least one jurisdiction with population $\geq$ 40,000 (for CDC data on mortality). Date range: 2006-2015 for HCUP data and 2010-2015 for CDC data. All regressions include: jurisdiction FEs, month of sample FEs, state-specific linear trends, police per capita (except column 1), and the following laws/regulations: Good Samaritan laws, PDMP, Doctor Shopping, Pain Clinic regulations, Physician exams, Pharmacy verification, require ID, and tamper-resistant PF. Coefficients show the effect of expanding Naloxone access on ER visits (column 1), and deaths per 100,000 residents (columns 2 and 3, which are also population-weighted).

Table 5: Effect of naloxone laws on crime

	Opioid-related crime (1)	Opioid-related theft (2)	All theft (3)
Naloxone Law	6.053**	0.414*	4.810
	(2.213)	(0.214)	(12.843)
Observations	29,808	29,808	29,808
2010 baseline	39.34	1.367	1832

Notes: * $p < .10$, ** $p < .05$, *** $p < .01$. Standard errors are clustered by state and shown in parentheses. Data source: NIBRS. Sample includes jurisdictions with population $\geq$ 40,000. Date range: 2010-2015. All regressions include: jurisdiction FEs, month of sample FEs, state-specific linear trends, police per capita, and the following laws/regulations: Good Samaritan laws, PDMP, Doctor Shopping, Pain Clinic regulations, Physician exams, Pharmacy verification, require ID, and tamper-resistant PF. Coefficients are population-weighted and show the effect of expanding Naloxone access on reported crimes per million residents.

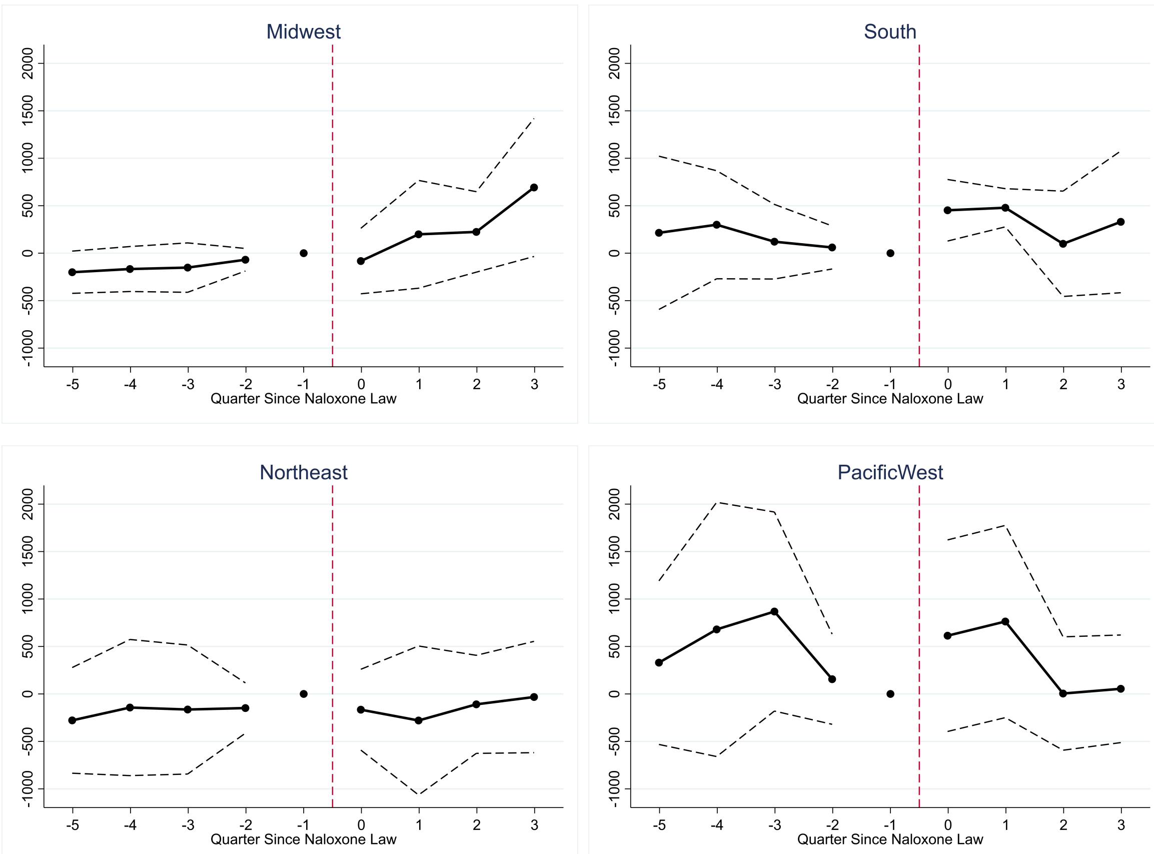
A Appendix—for online publication

A.1 Regional analysis

There are regional differences in the types of opioids available as well as in health care access; these likely produce regional differences in the effects of broad naloxone access. For instance, we expect naloxone access to have more beneficial effects in the West because of the greater prevalence of black tar heroin in that region (Quinones, 2015; Ciccarone, 2009). Black tar heroin (in contrast to powder heroin) does not mix easily with fentanyl, so it would be more difficult for users or dealers to increase the potency of opioid consumption in response to naloxone laws. This should increase the effectiveness of naloxone in individual cases. In addition, we would expect more beneficial effects in places where those who become addicted to opioids or are saved by naloxone can more easily access drug treatment. For instance, states in the Northeast and West tend to provide broader access to Medicaid, which covers drug treatment.

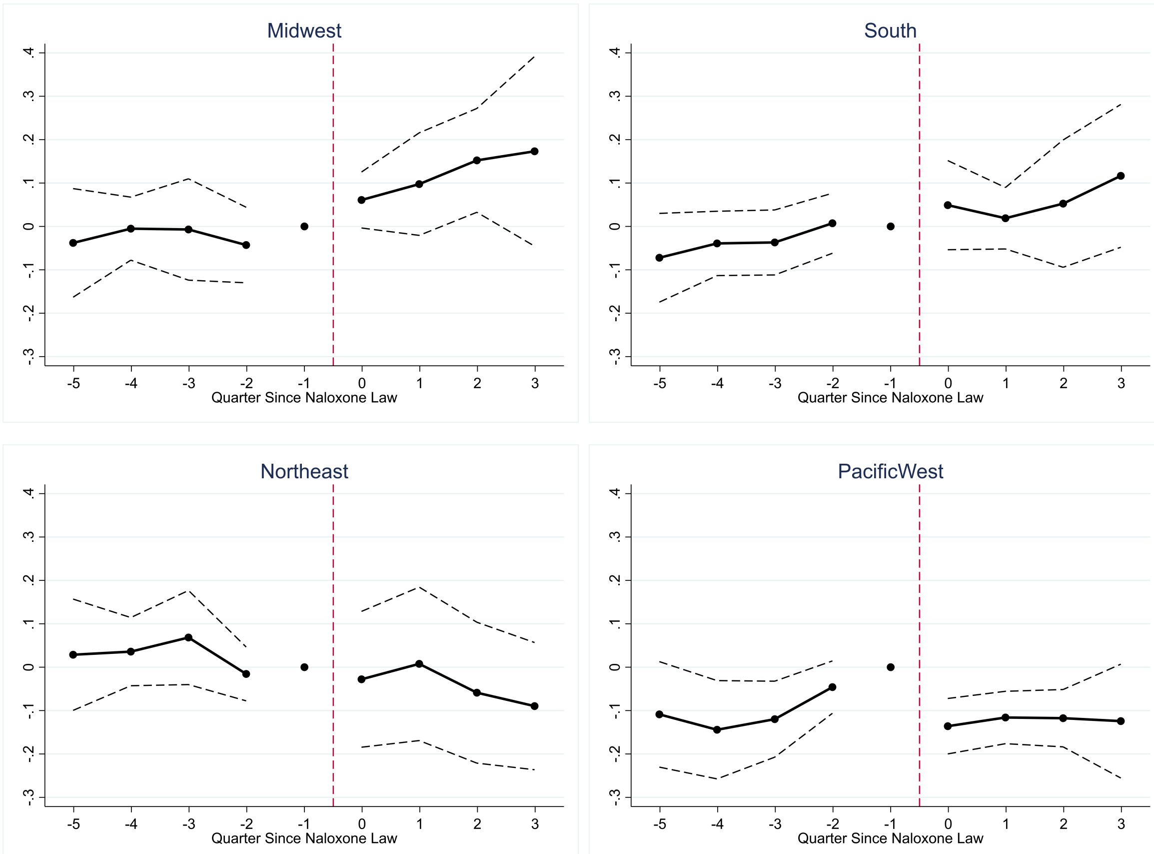
Figures A.1 through A.4 show effects on ER visits, mortality, and opioid-related theft separately by Census region, while Table A.1 presents all of our main results separately for each region. The most striking difference from the average effects discussed above is that—as expected—those averages masked substantial heterogeneity in mortality effects. In the Midwest, we find that broadening naloxone access increased opioid-related mortality by 14% ($p < 0.05$) and fentanyl-related mortality by 84% ($p < 0.10$). Pre-trends are flat, evidence that the parallel trends assumption holds and that laws are not passed in response to differential increases in opioid or fentanyl use. Effects on mortality are also positive in the South, but negative in the Northeast and West (all not significant, except that the negative effect on fentanyl-related mortality is statistically significant in the West). Since the opioid crisis has been most consequential in the Midwest and South, these results suggest that naloxone access may have exacerbated the crisis in the places that were hardest-hit (and perhaps where public health resources could not keep up). Our other outcome measures suggest increases in opioid abuse in the Midwest, South, and the Northeast. In the West, the directions of effects are more mixed, suggesting that the (insignificant) decrease in mortality is the primary finding for this region.

Figure A.1: Effect of naloxone access laws on opioid-related ER visits



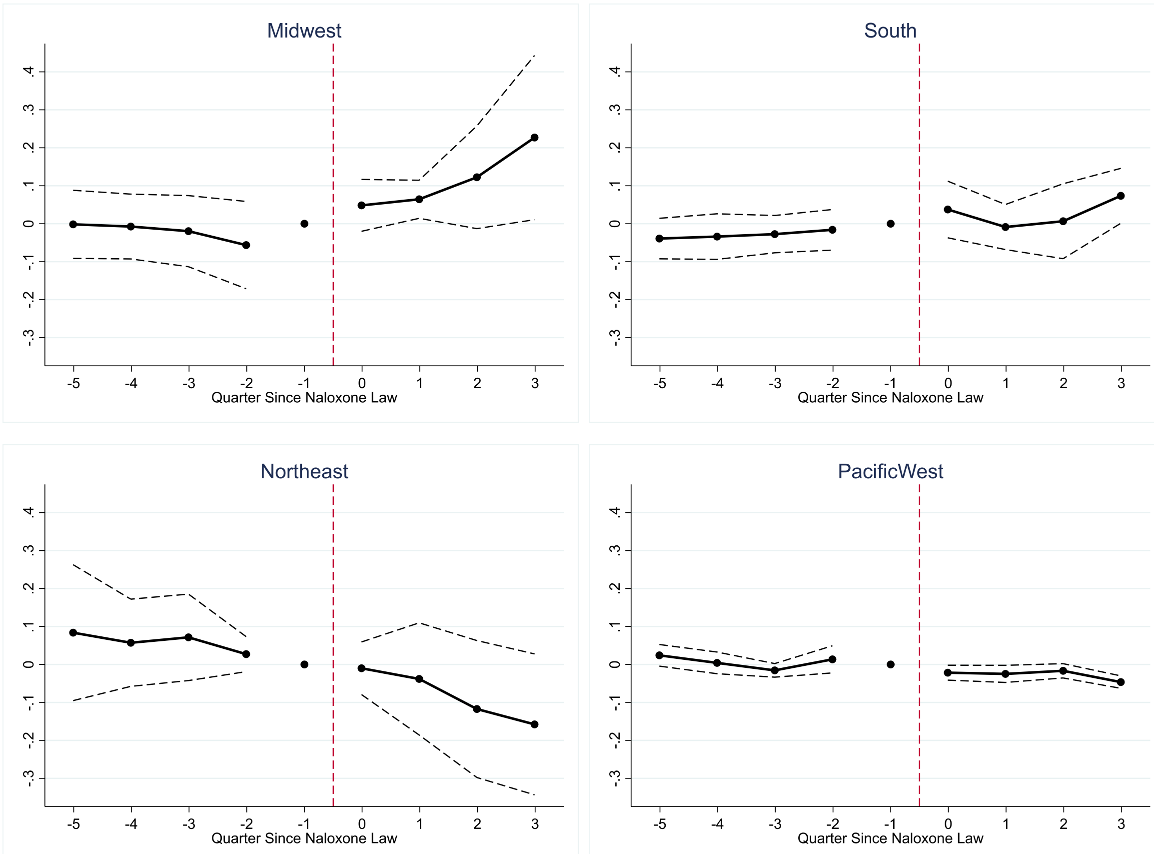
Notes: See notes for Figure 4. Data source: HCUP. Sample includes metro areas. Date range: 2006-2015.

Figure A.2: Effect of naloxone access laws on opioid-related mortality



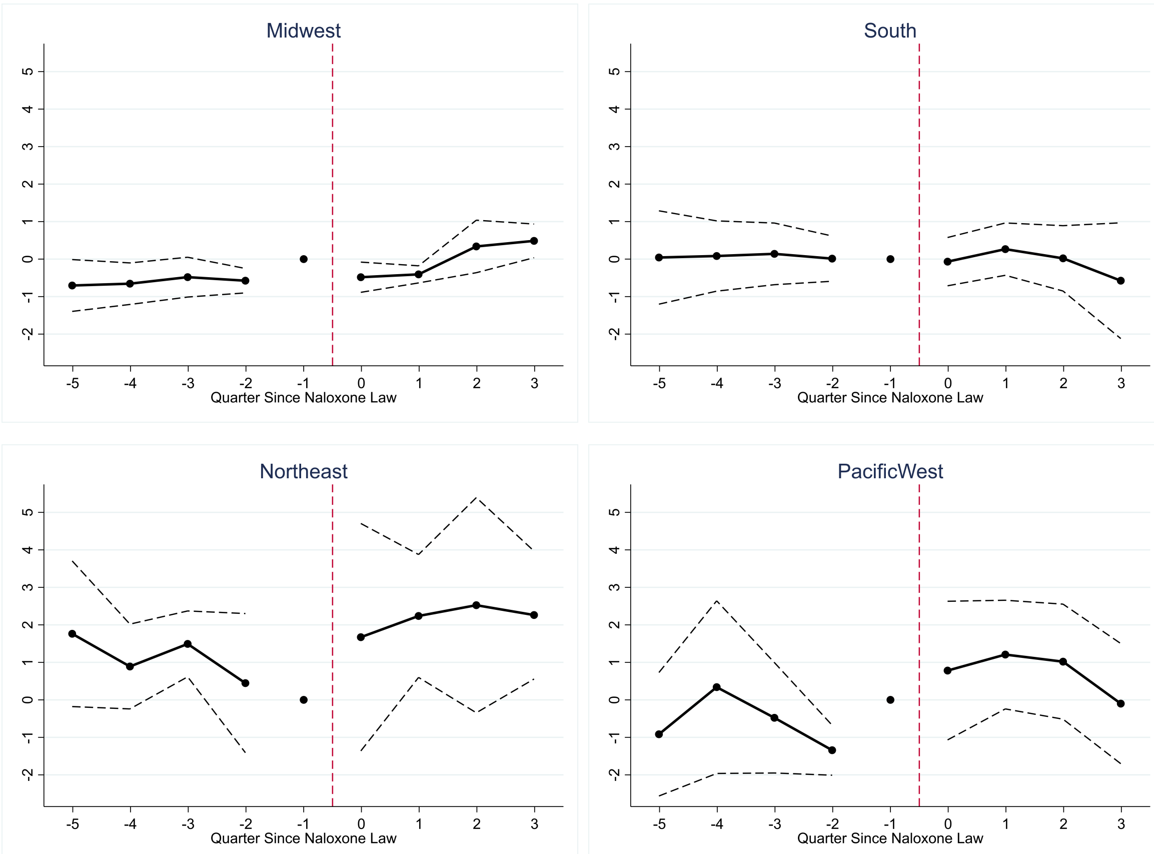
Notes: See notes for Figure 4. Data source: CDC. Sample includes counties that include at least one city with population $\geq$ 40,000. Date range: 2010-2015.

Figure A.3: Effect of naloxone access laws on fentanyl-related mortality



Notes: See notes for Figure 4. Data source: CDC. Sample includes counties that include at least one city with population $\geq$ 40,000. Date range: 2010-2015.

Figure A.4: Effect of naloxone access laws on opioid-related theft



Notes: See notes for Figure 4. Data source: NIBRS. Sample includes jurisdictions with population $\geq$ 40,000. Date range: 2010-2015.

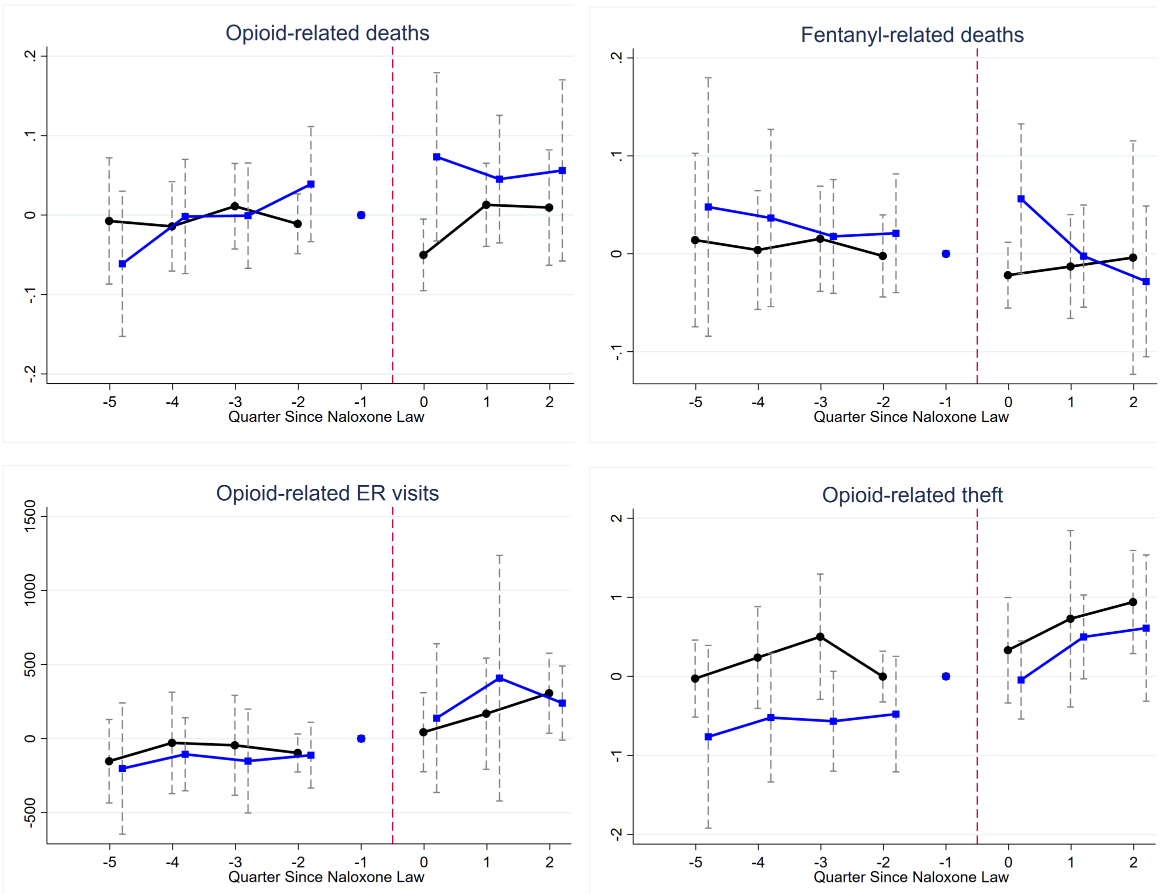
Table A.1: Effect of naloxone laws by region

	Possession of opioids (1)	Selling opioids (2)	Opioid-related ER visits (3)	Opioid-related deaths (4)	Fentanyl-related deaths (5)	Opioid-related crime (6)	Opioid-related theft (7)
Midwest							
Naloxone Law	4.925*	0.874**	293.9	0.094**	0.076*	5.481*	0.034
	(2.140)	(0.363)	(240.2)	(0.041)	(0.041)	(2.542)	(0.278)
Observations	9,432	9,432	404	12,240	12,240	9,432	9,432
2010 baseline	21.99	5.165	1223	0.664	0.090	34.98	0.955
South							
Naloxone Law	3.783	1.694*	309.1**	0.052	0.033	5.333	0.136
	(3.415)	(0.780)	(111.9)	(0.037)	(0.020)	(4.349)	(0.312)
Observations	11,520	11,520	260	25,488	25,488	11,520	11,520
2010 baseline	23.95	7.398	1636	0.589	0.086	40.32	1.327
Northeast							
Naloxone Law	6.408**	5.286*	-24.93	-0.047	-0.092	12.10**	0.860
	(1.803)	(2.073)	(142.5)	(0.064)	(0.081)	(3.146)	(0.619)
Observations	3,888	3,888	260	8,136	8,136	3,888	3,888
2010 baseline	31.72	14.78	2032	0.523	0.074	57.56	1.973
West							
Naloxone Law	-1.854	0.649	57.08	-0.059	-0.023***	-0.226	1.417***
	(3.130)	(1.252)	(41.82)	(0.040)	(0.006)	(2.589)	(0.363)
Observations	4,968	4,968	184	9,648	9,648	4,968	4,968
2010 baseline	20.40	4.568	2498	0.619	0.068	34.03	1.843

Notes: * $p < .10$, ** $p < .05$, *** $p < .01$. Standard errors are clustered by state and shown in parentheses. Sample includes jurisdictions with population $\geq$ 40,000 (for NIBRS data), counties with any such jurisdictions (for CDC data), and metro areas (for HCUP data). Date range: 2010-2015 for NIBRS and CDC data, 2006-2015 for HCUP data. All regressions include: jurisdiction FEs, month of year FEs, year FEs, state-specific linear trends, police per capita (except column 3), and the following laws/regulations: Good Samaritan laws, PDMP, Doctor Shopping, Pain Clinic regulations, Physician exams, Pharmacy verification, require ID, and tamper-resistant PF. Coefficients show the effect of expanding naloxone access on arrests per million residents (columns 1 and 2), number of ER visits (column 3), deaths per 100,000 residents (columns 4 and 5), and reported crimes per million residents (columns 6 and 7). All coefficients except ER visits are also population-weighted.

Additional Exhibits

Figure B.1: Effect of naloxone access laws on outcomes by year of passage



Notes: See notes for Figure 4. The black line (circle markers) indicates results for states that expanded naloxone in 2014 or before; the blue line (square markers) indicates results for states that expanded naloxone in 2015. Data source: CDC (for mortality), HCUP (for ER admissions), and NIBRS (for arrests and crime). Date range: 2010-2015 (CDC and NIBRS), 2006-2015 (HCUP).

Table B.1: Relationship between Google searches and drug treatment admissions

	Drug treatment admissions for opioid abuse (TEDS data)
Google searches for "drug rehab" topic	306.1**
	(115.9)
Observations	293
2010 baseline	8837.8

Notes: Standard errors in parentheses. * $p < .10$, ** $p < .05$, *** $p < .01$. Number of drug treatment admissions related to opioids are measured at the state-year level. Sample includes state-year observations from the Treatment Episode Data Set (TEDS), produced by the Substance Abuse and Mental Health Services Administration (SAMHSA). Date range: 2010-2015.

Table B.2: Effect of naloxone laws using only timing variation

	Possession of opioids (1)	Selling opioids (2)	Opioid-related ER visits (3)	Opioid-related deaths (4)	Fentanyl-related deaths (5)	Opioid-related crime (6)	Opioid-related theft (7)
Naloxone Law	3.835**	1.884**	161.471	-0.003	-0.013	6.069**	0.390
	(1.833)	(0.706)	(105.606)	(0.027)	(0.031)	(2.446)	(0.231)
Observations	27,216	27,216	616	49,752	49,752	27,216	27,216
2010 baseline	24.07	7.37	2,420	0.595	0.080	40.596	1.379

Notes: * $p < .10$, ** $p < .05$, *** $p < .01$. Standard errors are clustered by state and shown in parentheses. Data sources: NIBRS (monthly, 2010-2015), CDC (monthly, 2010-2015), and HCUP (quarterly, 2006-2015). Sample is restricted to the 40 states that expanded naloxone access by December 31, 2015. All regressions include: jurisdiction FEs, month of sample FEs, state-specific linear trends, police per capita (except column 3), the dates of Medicaid expansion (as in Simon, Soni and Cawley (2017)), and the following laws/regulations: Good Samaritan laws, PDMP, Doctor Shopping, Pain Clinic regulations, Physician exams, Pharmacy verification, require ID, and tamper-resistant PF. Coefficients show the effect of expanding naloxone access on arrests per million residents (columns 1 and 2), number of ER visits (column 3), deaths per 100,000 residents (columns 4 and 5), and reported crimes per million residents (columns 6 and 7). All coefficients except those in column (3) are also population-weighted.

Table B.3: Effect of naloxone laws by year of passage

	Possession of opioids (1)	Selling opioids (2)	Opioid-related ER visits (3)	Opioid-related deaths (4)	Fentanyl-related deaths (5)	Opioid-related crime (6)	Opioid-related theft (7)
Naloxone Law ($\leq$ 2014)	4.062*	2.562***	253.378**	-0.026	-0.007	6.392*	0.371
	(2.063)	(0.892)	(118.798)	(0.029)	(0.043)	(3.132)	(0.250)
Naloxone Law (2015)	3.986	1.046	318.566	0.063	0.004	5.596	0.472
	(2.552)	(0.867)	(377.075)	(0.045)	(0.038)	(3.341)	(0.306)
Observations	29,808	29,808	1,108	55,512	55,512	29,808	29,808
2010 baseline	23.52	6.972	1738	0.601	0.080	39.34	1.367

Notes: * $p < .10$, ** $p < .05$, *** $p < .01$. Standard errors are clustered by state and shown in parentheses. Data sources: NIBRS (monthly, 2010-2015), CDC (monthly, 2010-2015), and HCUP (quarterly, 2006-2015). Naloxone Law ($\leq$ 2014) indicates that the law was passed in 2014 or earlier; Naloxone Law ($\geq$ 2015) indicates that the law was passed in 2015 or later. All regressions include: jurisdiction FEs, month of sample FEs, state-specific linear trends, police per capita (except column 3), the dates of Medicaid expansion (as in Simon, Soni and Cawley (2017)), and the following laws/regulations: Good Samaritan laws, PDMP, Doctor Shopping, Pain Clinic regulations, Physician exams, Pharmacy verification, require ID, and tamper-resistant PF. Coefficients show the effect of expanding naloxone access on arrests per million residents (columns 1 and 2), number of ER visits (column 3), deaths per 100,000 residents (columns 4 and 5), and reported crimes per million residents (columns 6 and 7). All coefficients except those in column (3) are also population-weighted.

Table B.4: Effect of naloxone laws by availability of drug treatment

	Q1 (low) (1)	Q2 (2)	Q3 (3)	Q4 (high) (4)	Q1 (low) (5)	Q2 (6)	Q3 (7)	Q4 (high) (8)
	Opioid-related deaths				**Fentanyl-related deaths**			
Naloxone Law	0.032	0.032	0.016	-0.028	0.052*	0.012	0.010	-0.038
	(0.035)	(0.040)	(0.034)	(0.054)	(0.026)	(0.018)	(0.035)	(0.054)
Observations	13,896	13,896	13,896	13,824	13,896	13,896	13,896	13,824
2010 baseline	0.555	0.599	0.576	0.694	0.078	0.081	0.070	0.099

Notes: * $p < .10$, ** $p < .05$, *** $p < .01$. Standard errors are clustered by state and shown in parentheses. Sample includes counties with any cities with population $\geq$ 40,000. Date range: 2010-2015. All regressions include: jurisdiction FEs, month of sample FEs, state-specific linear trends, police per capita, and the following laws/regulations: Good Samaritan laws, PDMP, Doctor Shopping, Pain Clinic regulations, Physician exams, Pharmacy verification, require ID, and tamper-resistant PF. Coefficients are population-weighted and show the effect of expanding naloxone access on deaths per 100,000 residents.

Table B.5: Effect of naloxone laws by Medicaid expansion status

	Opioid-related ER Visits	Opioid-related deaths	Fentanyl-related deaths
	(1)	(2)	(3)
No Medicaid Expansion by 2015			
Naloxone Law	439.0**	0.042	0.018
	(201.0)	(0.040)	(0.023)
Observations	460	25,272	25,272
2010 baseline	1102	0.575	0.084
Medicaid Expansion by 2015			
Naloxone Law	47.48	-0.020	-0.025
	(133.4)	(0.033)	(0.047)
Observations	648	30,240	30,240
2010 baseline	2186	0.615	0.077

Notes: * $p < .10$, ** $p < .05$, *** $p < .01$. Standard errors are clustered by state and shown in parentheses. Sample includes counties containing jurisdictions with population $\geq$ 40,000 (for CDC data), and metro areas (for HCUP data). Date range: 2010-2015 for CDC data, 2006-2015 for HCUP data. All regressions include: jurisdiction FEs, month of sample FEs, state-specific linear trends, police per capita, and the following laws/regulations: Good Samaritan laws, PDMP, Doctor Shopping, Pain Clinic regulations, Physician exams, Pharmacy verification, require ID, and tamper-resistant PF. Coefficients on CDC data are population-weighted as the dependent variables are rates (number of deaths per 100,000 residents). Medicaid expansion dates are same as in Simon, Soni and Cawley (2017) and include: AZ, AR, CA, CO, CT, DE, HI, IL, IA, KY, MD, MA, MN, NV, NJ, NM, NY, ND, OH, OR, RI, VT, WA, WV, WI, MI, NH, PA, IN, and AK.

Table B.6: Mortality results with Goodman-Bacon correction for pre-treatment trends

	Main results		**Goodman-Bacon correction**	
	Opioid-related deaths	Fentanyl-related deaths	Opioid-related deaths	Fentanyl-related deaths
	(1)	(2)	(3)	(4)
Naloxone Law	0.006	-0.003	0.015	0.006
	(0.027)	(0.030)	(0.027)	(0.030)
Observations	55,512	55,512	55,512	55,512

Notes: * $p < .10$, ** $p < .05$, *** $p < .01$. Standard errors are clustered by state and shown in parentheses. Sample includes counties with any cities with population $\geq$ 40,000. Date range: 2010-2015. All regressions include: jurisdiction FEs, month of sample FEs, state-specific linear trends, police per capita, and the following laws/regulations: Good Samaritan laws, PDMP, Doctor Shopping, Pain Clinic regulations, Physician exams, Pharmacy verification, require ID, and tamper-resistant PF. Coefficients are population-weighted and show the effect of expanding naloxone access on deaths per 100,000 residents.

Table B.7: Effects by population cutoffs

	Possession of opioids (1)	Selling opioids (2)	Opioid-related ER visits (3)	Opioid-related deaths (4)	Fentanyl-related deaths (5)	Opioid-related crime (6)	Opioid-related theft (7)
Panel A: Rural areas							
Naloxone Law	1.862	-0.833	48.70	-0.017	0.000	1.298	0.310
	(2.174)	(1.403)	(43.02)	(0.037)	(0.015)	(3.634)	(0.237)
Observations	169,692	169,692	1,014	155,616	155,616	169,692	169,692
2010 baseline	29.78	11.21	304.2	0.578	0.102	50.40	2.473
Panel B: All areas							
Naloxone Law	3.132*	0.839	335.3**	0.001	-0.003	3.987	0.402**
	(1.671)	(0.699)	(132.6)	(0.025)	(0.025)	(2.384)	(0.193)
Observations	199,500	199,500	1,108	211,128	211,128	199,500	199,500
2010 baseline	26.07	8.697	2063	0.596	0.084	43.84	1.817
Observations	29,808	29,808	1,108	55,512	55,512	29,808	29,808
2010 baseline	23.52	6.972	1738	0.601	0.080	39.34	1.367

Notes: * $p < .10$, ** $p < .05$, *** $p < .01$. Standard errors are clustered by state and shown in parentheses. Sample includes jurisdictions with population $< 40{,}000$ (for NIBRS data), counties without any urban jurisdictions (for CDC data), and rural areas (for HCUP data). Date range: 2010-2015 for NIBRS and CDC data, 2006-2015 for HCUP data. All regressions include: jurisdiction FEs, month of sample FEs, state-specific linear trends, police per capita (except column 3), and the following laws/regulations: Good Samaritan laws, PDMP, Doctor Shopping, Pain Clinic regulations, Physician exams, Pharmacy verification, require ID, and tamper-resistant PF. Coefficients show the effect of expanding naloxone access on arrests per million residents (columns 1 and 2), number of ER visits (column 3), deaths per 100,000 residents (columns 4 and 5), and reported crimes per million residents (columns 6 and 7). All coefficients except ER visits are also population-weighted.

Table B.8: Impact of naloxone laws with different population cutoffs for “urban”

	Minimum population for jurisdictions included in the sample									
	10,000	15,000	20,000	25,000	30,000	35,000	40,000	45,000	50,000	55,000
Opioid-related mortality										
Naloxone Law	0.000	-0.000	0.000	0.000	0.002	0.006	0.006	0.007	0.007	0.009
	(0.026)	(0.026)	(0.026)	(0.026)	(0.026)	(0.026)	(0.027)	(0.026)	(0.027)	(0.027)
Observations	152,568	121,896	100,800	83,880	69,984	60,984	55,512	49,536	45,144	41,256
2010 Baseline	0.605	0.605	0.603	0.601	0.603	0.599	0.601	0.598	0.601	0.603
Opioid-related theft										
Naloxone Law	0.471**	0.488**	0.500**	0.482**	0.492**	0.484**	0.414*	0.347*	0.380*	0.383*
	(0.195)	(0.192)	(0.213)	(0.221)	(0.225)	(0.234)	(0.214)	(0.202)	(0.204)	(0.217)
Observations	108,912	83,028	64,644	52,368	41,292	34,416	29,808	25,560	22,536	20,232
2010 Baseline	1.631	1.546	1.530	1.488	1.436	1.389	1.367	1.356	1.355	1.339

Notes: * $p < .10$, ** $p < .05$, *** $p < .01$. Standard errors are clustered by state and shown in parentheses. Sample includes jurisdictions with population greater than the reported cutoffs (for NIBRS data on opioid-related theft) and counties with any such jurisdictions (for CDC data on mortality). Date range: 2010-2015. All regressions include: jurisdiction FEs, month of sample FEs, state-specific linear trends, police per capita, and the following laws/regulations: Good Samaritan laws, PDMP, Doctor Shopping, Pain Clinic regulations, Physician exams, Pharmacy verification, require ID, and tamper-resistant PF. Coefficients are population-weighted and show the effect of expanding naloxone access on deaths per 100,000 residents (panel 1), and reported crimes per million residents (panel 2).

Table B.9: Effect of naloxone laws on broader categories of deaths and crime

Deaths due to opioids or unspecified-drug poisoning	
Naloxone Law	0.003
	(0.029)
Observations	55,512
2010 baseline	0.942
All theft	
Naloxone Law	4.810
	(12.84)
Observations	29,808
2010 baseline	1832

Notes: * $p < .10$, ** $p < .05$, *** $p < .01$. Standard errors are clustered by state and shown in parentheses. Sample includes jurisdictions with population $\geq 40{,}000$ (for NIBRS data on arrests and crime), counties with any such jurisdictions (for CDC data on mortality). Date range: 2010-2015 for NIBRS and CDC data. All regressions include: jurisdiction FEs, month of sample FEs, state-specific linear trends, police per capita, and the following laws/regulations: Good Samaritan laws, PDMP, Doctor Shopping, Pain Clinic regulations, Physician exams, Pharmacy verification, require ID, and tamper-resistant PF. Coefficients are population-weighted and show the effect of expanding naloxone access on deaths per 100,000 residents (panel 1), and reported crimes per million residents (panel 2).

Table B.10: Effect of naloxone laws on Google searches for "Naloxone"

	Google trends: "Naloxone" searches (metro areas)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Naloxone Law	13.942***	3.903***	1.921**	1.937**	1.910**	1.877**	1.831**	1.847**
	(1.321)	(1.211)	(0.814)	(0.807)	(0.817)	(0.808)	(0.813)	(0.809)
Observations	20,232	20,232	20,232	20,232	20,232	20,232	20,232	20,232
2010 baseline	25.49	25.49	25.49	25.49	25.49	25.49	25.49	25.49
Adjusted R^2	0.047	0.084	0.106	0.106	0.106	0.106	0.106	0.106
Controls:								
Jurisdiction FE	X	X	X	X	X	X	X	X
Month of sample FE		X	X	X	X	X	X	X
State-specific linear trends			X	X	X	X	X	X
Police per capita				X	X	X	X	X
Good Samaritan Laws					X	X	X	X
PDMP, Doctor Shopping, Pain Clinic regs						X	X	X
Physician exam, Pharm verification, Require ID							X	X
Tamper Resistant PF								X

Notes: * $p < .10$, ** $p < .05$, *** $p < .01$. Standard errors are clustered by state and shown in parentheses. Estimates indicate naloxone access laws' impact on search intensities, indexed on a 0-100 scale. Observations are at the metro area-month level. Data source: Google Trends. Sample includes metro areas. Date range: 2010-2015.

Table B.11: Effect of naloxone laws on Google searches for “Drug rehab”

	Google trends: “Drug rehab” searches (metro areas)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Naloxone Law	-2.093***	0.266	-0.725	-0.695	-0.773*	-0.744*	-0.744*	-0.799*
	(0.473)	(0.646)	(0.435)	(0.448)	(0.457)	(0.440)	(0.441)	(0.450)
Observations	21,528	21,528	21,528	21,528	21,528	21,528	21,528	21,528
2010 baseline	55.72	55.72	55.72	55.72	55.72	55.72	55.72	55.72
Adjusted R^2	0.002	0.159	0.168	0.168	0.168	0.168	0.168	0.169
Controls:								
Jurisdiction FE	X	X	X	X	X	X	X	X
Month of sample FE		X	X	X	X	X	X	X
State-specific linear trends			X	X	X	X	X	X
Police per capita				X	X	X	X	X
Good Samaritan Laws					X	X	X	X
PDMP, Doctor Shopping, Pain Clinic regs						X	X	X
Physician exam, Pharm verification, Require ID							X	X
Tamper Resistant PF								X

Notes: * $p < .10$, ** $p < .05$, *** $p < .01$. Standard errors are clustered by state and shown in parentheses. Estimates indicate naloxone access laws’ impact on search intensities, indexed on a 0-100 scale. Observations are at the metro area-month level. Data source: Google Trends. Sample includes metro areas. Date range: 2010-2015.

Table B.12: Effect of naloxone laws on arrests for possession of opioids

	Possession of opioids (arrests)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Naloxone Law	3.766	6.570**	3.113*	2.963*	3.795**	4.211**	4.148**	4.030**
	(3.449)	(2.829)	(1.663)	(1.659)	(1.795)	(1.733)	(1.759)	(1.673)
Observations	29,808	29,808	29,808	29,808	29,808	29,808	29,808	29,808
2010 baseline	23.52	23.52	23.52	23.52	23.52	23.52	23.52	23.52
Adjusted R^2	0.003	0.046	0.100	0.101	0.102	0.103	0.103	0.103
Controls:								
Jurisdiction FE	X	X	X	X	X	X	X	X
Month of sample FE		X	X	X	X	X	X	X
State-specific linear trends			X	X	X	X	X	X
Police per capita				X	X	X	X	X
Good Samaritan Laws					X	X	X	X
PDMP, Doctor Shopping, Pain Clinic regs						X	X	X
Physician exam, Pharm verification, Require ID							X	X
Tamper Resistant PF								X

Notes: * $p < .10$, ** $p < .05$, *** $p < .01$. Standard errors are clustered by state and shown in parentheses. Observations are at the jurisdiction-month level. Data source: NIBRS. Sample includes jurisdictions with population $\geq$ 40,000. Date range: 2010-2015. Coefficients are population-weighted and show the effect of expanding naloxone access on arrests per million residents.

Table B.13: Effect of naloxone laws on arrests for selling opioids

	Selling opioids (arrests)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Naloxone Law	0.873	1.206*	1.651**	1.509**	1.911**	1.933***	1.919***	1.917***
	(0.613)	(0.638)	(0.668)	(0.631)	(0.702)	(0.687)	(0.688)	(0.675)
Observations	29,808	29,808	29,808	29,808	29,808	29,808	29,808	29,808
2010 baseline	6.972	6.972	6.972	6.972	6.972	6.972	6.972	6.972
Adjusted R^2	0.000	0.007	0.017	0.020	0.020	0.021	0.021	0.021
Controls:								
Jurisdiction FE	X	X	X	X	X	X	X	X
Month of sample FE		X	X	X	X	X	X	X
State-specific linear trends			X	X	X	X	X	X
Police per capita				X	X	X	X	X
Good Samaritan Laws					X	X	X	X
PDMP, Doctor Shopping, Pain Clinic regs						X	X	X
Physician exam, Pharm verification, Require ID							X	X
Tamper Resistant PF								X

Notes: * $p < .10$, ** $p < .05$, *** $p < .01$. Standard errors are clustered by state and shown in parentheses. Observations are at the jurisdiction-month level. Data source: NIBRS. Sample includes jurisdictions with population $\geq$ 40,000. Date range: 2010-2015. Coefficients are population-weighted and show the effect of expanding naloxone access on arrests per million residents.

Table B.14: Effect of naloxone laws on opioid-related ER visits

	Opioid-related ER visits						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Naloxone Law	1928***	1136**	236.8**	256.2*	244.2*	265.7**	265.9**
	(484.4)	(430.3)	(98.50)	(129.8)	(125.4)	(122.2)	(121.6)
Observations	1,108	1,108	1,108	1,108	1,108	1,108	1,108
2010 baseline	2063	2063	2063	2063	2063	2063	2063
Adjusted R^2	0.235	0.437	0.928	0.928	0.928	0.929	0.929
Controls:							
Jurisdiction FE	X	X	X	X	X	X	X
Month of sample FE		X	X	X	X	X	X
State-specific linear trends			X	X	X	X	X
Good Samaritan Laws				X	X	X	X
PDMP, Doctor Shopping, Pain Clinic regs					X	X	X
Physician exam, Pharm verification, Require ID						X	X
Tamper Resistant PF							X

Notes: * $p < .10$, ** $p < .05$, *** $p < .01$. Standard errors are clustered by state and shown in parentheses. Estimates indicate naloxone access laws' impact on the number of opioid-related ER visits. Observations are at the metro area-quarter level. Data source: NIBRS. Sample includes metropolitan areas. Date range: 2006-2015.

Table B.15: Effect of naloxone laws on opioid-related mortality

	Mortality due to any opioid overdose							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Naloxone Law	0.232***	0.058	0.013	0.014	0.009	0.006	0.005	0.006
	(0.068)	(0.063)	(0.025)	(0.025)	(0.027)	(0.027)	(0.027)	(0.027)
Observations	55,512	55,512	55,512	55,512	55,512	55,512	55,512	55,512
2010 baseline	0.601	0.601	0.601	0.601	0.601	0.601	0.601	0.601
Adjusted R^2	0.026	0.042	0.095	0.095	0.095	0.095	0.095	0.095
Controls:								
Jurisdiction FE	X	X	X	X	X	X	X	X
Month of sample FE		X	X	X	X	X	X	X
State-specific linear trends			X	X	X	X	X	X
Police per capita				X	X	X	X	X
Good Samaritan Laws					X	X	X	X
PDMP, Doctor Shopping, Pain Clinic regs						X	X	X
Physician exam, Pharm verification, Require ID							X	X
Tamper Resistant PF								X

Notes: * $p < .10$, ** $p < .05$, *** $p < .01$. Standard errors are clustered by state and shown in parentheses. Observations are at the county-month level. Data source: CDC. Sample includes counties that include at least one city with population $\geq$ 40,000. Date range: 2010-2015. Coefficients are population-weighted and show the effect of expanding naloxone access on deaths per 100,000 residents.

Table B.16: Effect of naloxone laws on fentanyl-related deaths

	Mortality due to synthetic opioid overdose (fentanyl)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Naloxone Law	0.156***	0.034	-0.001	-0.001	-0.002	-0.005	-0.005	-0.003
	(0.050)	(0.040)	(0.033)	(0.033)	(0.032)	(0.032)	(0.032)	(0.030)
Observations	55,512	55,512	55,512	55,512	55,512	55,512	55,512	55,512
2010 baseline	0.080	0.080	0.080	0.080	0.080	0.080	0.080	0.080
Adjusted R^2	0.047	0.075	0.166	0.166	0.166	0.168	0.168	0.169
Controls:								
Jurisdiction FE	X	X	X	X	X	X	X	X
Month of sample FE		X	X	X	X	X	X	X
State-specific linear trends			X	X	X	X	X	X
Police per capita				X	X	X	X	X
Good Samaritan Laws					X	X	X	X
PDMP, Doctor Shopping, Pain Clinic regs						X	X	X
Physician exam, Pharm verification, Require ID							X	X
Tamper Resistant PF								X

Notes: * $p < .10$, ** $p < .05$, *** $p < .01$. Standard errors are clustered by state and shown in parentheses. Observations are at the county-month level. Data source: CDC. Sample includes counties that include at least one city with population $\geq$ 40,000. Date range: 2010-2015. Coefficients are population-weighted and show the effect of expanding naloxone access on deaths per 100,000 residents.

Table B.17: Effect of naloxone laws on opioid-related crime

	All opioid-related crime							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Naloxone Law	3.463	8.808**	4.964**	4.581*	5.742**	6.312**	6.230**	6.053**
	(4.627)	(3.740)	(2.379)	(2.313)	(2.467)	(2.293)	(2.337)	(2.213)
Observations	29,808	29,808	29,808	29,808	29,808	29,808	29,808	29,808
2010 baseline	39.34	39.34	39.34	39.34	39.34	39.34	39.34	39.34
Adjusted R^2	0.001	0.052	0.099	0.102	0.102	0.104	0.104	0.104
Controls:								
Jurisdiction FE	X	X	X	X	X	X	X	X
Month of sample FE		X	X	X	X	X	X	X
State-specific linear trends			X	X	X	X	X	X
Police per capita				X	X	X	X	X
Good Samaritan Laws					X	X	X	X
PDMP, Doctor Shopping, Pain Clinic regs						X	X	X
Physician exam, Pharm verification, Require ID							X	X
Tamper Resistant PF								X

Notes: * $p < .10$, ** $p < .05$, *** $p < .01$. Standard errors are clustered by state and shown in parentheses. Observations are at the jurisdiction-month level. Data source: NIBRS. Sample includes jurisdictions with population $\geq$ 40,000. Date range: 2010-2015. Coefficients are population-weighted and show the effect of expanding naloxone access on reported crimes per million residents.

Table B.18: Effect of naloxone laws on opioid-related theft

	Opioid-related theft							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Naloxone Law	0.340	0.609*	0.423*	0.419*	0.428*	0.445*	0.434*	0.414*
	(0.341)	(0.331)	(0.224)	(0.224)	(0.222)	(0.224)	(0.224)	(0.214)
Observations	29,808	29,808	29,808	29,808	29,808	29,808	29,808	29,808
2010 baseline	1.367	1.367	1.367	1.367	1.367	1.367	1.367	1.367
Adjusted R^2	0.001	0.008	0.022	0.022	0.022	0.023	0.023	0.023
Controls:								
Jurisdiction FE	X	X	X	X	X	X	X	X
Month of sample FE		X	X	X	X	X	X	X
State-specific linear trends			X	X	X	X	X	X
Police per capita				X	X	X	X	X
Good Samaritan Laws					X	X	X	X
PDMP, Doctor Shopping, Pain Clinic regs						X	X	X
Physician exam, Pharm verification, Require ID							X	X
Tamper Resistant PF								X

Notes: * $p < .10$, ** $p < .05$, *** $p < .01$. Standard errors are clustered by state and shown in parentheses. Observations are at the jurisdiction-month level. Data source: NIBRS. Sample includes jurisdictions with population $\geq$ 40,000. Date range: 2010-2015. Coefficients are population-weighted and show the effect of expanding naloxone access on reported crimes per million residents.

Table B.19: Effect of naloxone laws with robustness to law timing

	Possession of opioids (1)	Selling opioids (2)	Opioid-related ER visits (3)	Opioid-related deaths (4)	Fentanyl-related deaths (5)	Opioid-related crime (6)	Opioid-related theft (7)
Entire U.S.							
Naloxone Law	4.030**	1.917***	262.3**	0.017	0.007	6.053**	0.414*
	(1.673)	(0.675)	(109.2)	(0.025)	(0.027)	(2.213)	(0.214)

Notes: * $p < .10$, ** $p < .05$, *** $p < .01$. Standard errors are clustered by state and shown in parentheses. Data sources: NIBRS (monthly, 2010-2015), CDC (monthly, 2010-2015), and HCUP (quarterly, 2006-2015). In this table, we examine different dates for five states: 5/2013 for CO, 10/2012 for CT; 8/2015 for LA; and 4/2014 for ME because there were some third-party prescriptions allowed as of these dates. We also test 6/2010 for WA because a Good Samaritan Law at that time made naloxone available to individuals at risk of overdose. All regressions include: jurisdiction FEs, month of sample FEs, state-specific linear trends, police per capita (except column 3), and the following laws/regulations: Good Samaritan laws, PDMP, Doctor Shopping, Pain Clinic regulations, Physician exams, Pharmacy verification, require ID, and tamper-resistant PF. Coefficients show the effect of expanding naloxone access on arrests per million residents (columns 1 and 2), number of ER visits (column 3), deaths per 100,000 residents (columns 4 and 5), and reported crimes per million residents (columns 6 and 7). All coefficients except those in column (3) are also population-weighted.

Table B.20: Robustness to specification used in Rees et al. (2019)

	Mortality due to any opioid overdose							
	Rees et al. estimate (1)	Our estimate (2)	Aggregate to state level (3)	+ Aggregate to year level (4)	+ Use ln(Rate) as outcome (5)	+ Drop 2015 (6)	+ Add controls (7)	+ Match dates (8)
Naloxone Law	-0.188*	0.006	0.054	0.502	0.041	0.036	0.052	0.093*
	(0.098)	(0.027)	(0.059)	(0.525)	(0.049)	(0.067)	(0.060)	(0.048)
Observations	816	55,512	3,600	300	300	250	250	250
2010 baseline	–	0.601	0.596	7.151	1.894	1.894	1.894	1.894

Notes: * $p < .10$, ** $p < .05$, *** $p < .01$. Standard errors are clustered by state and shown in parentheses. Data source for columns (2) through (8): CDC (2010-2015). Column (1) presents the main estimate shown in column (3) of Table 4 in Rees et al. (2019), which uses state-year observations from 1999-2014. Column (2) presents our estimate from column (2) of Table 4. The remaining columns present cumulative changes to our data and specification, to match those in Rees et al. (2019) as closely as possible. Column (3) aggregates our data (for all jurisdictions) to the state level. Column (4) further aggregates our data to the year level, using the method in Rees et al. (2019) by which states are considered as having broadened naloxone access if they did so at any point during the year. Column (5) converts the dependent variable to log rates. Column (6) drop observations from 2015, as Rees et al. (2019) does not use this data, and column (7) adds the other control variables used in Rees et al. (2019). Finally, column (8) uses the dates of naloxone access used in Rees et al. (2019), which vary slightly due to that paper's focus on naloxone access in any form (not broadened access). This different definition results in different years of naloxone access for the following states: CT (2003), CA (2008), IL (2010), WA (2010), RI (2012), CO (2013), KY (2013), VA (2013), MD (2013), and ME (2014). Coefficient in column (1) is weighted by state population as reported in Rees et al. (2019); coefficients in columns (2) through (8) are weighted by 2010 state population.