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

Jennifer L. Doleac *Texas A&M University*
Anita Mukherjee *University of Wisconsin–Madison*

Abstract

The United States is experiencing an epidemic of opioid abuse. In response, states have implemented 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.

1. Introduction

The United States is grappling with an epidemic of opioid abuse and overdoses: in 2019, nearly 50,000 people died of opioid overdoses, and such cases now constitute over two-thirds of all drug overdose deaths (CDC 2021). Policy makers

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have struggled to reduce the lethal effects of this class of drugs. Many have turned to naloxone. Naloxone can reverse an opioid overdose if administered quickly; it therefore has the potential to reduce this epidemic's death toll. Every US 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 an overdose. Some who are addicted may become criminally active to fund their continued or increased drug use. In addition, 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 likely 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 decreases, 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 because of the greater density of potential bystanders who could administer the drug, more efficient distribution by community groups, and shorter emergency (911) response times.[1]

We estimate the effects of naloxone using a panel fixed-effects model; this model controls for preexisting 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 other opioid-related policies and the number of police officers per capita as a proxy for local law enforcement resources.

Local data on 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 percent. 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 took effect, Google searches for "drug rehab" (a proxy for interest in drug treatment)[2] fell by 1.4 percent; arrests for possession and sales of opioids increased by 17 percent and 27 percent, respectively; opioid-related visits to the emergency room (ER) increased by 15 percent; and opioid-related theft increased by 30 percent (though the effect on overall theft rates is much smaller).

We note that, interpreted alone, the increase in opioid-related ER visits could

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

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

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) show that state-level Medicaid expansions offer only enough variation to detect mortality reductions of at least 2 percent in a standard difference-in-differences (DD) framework.

A variety of robustness checks support our main results. We find no evidence that preexisting trends are driving these effects. Most important, preexisting trends in fentanyl use do not explain our findings, although (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," excluding individual states one by one, and using alternate dates for naloxone access laws (in the few cases in which there is 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, 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 argues that the benefits from innovations in driving safety such as seat belts would be muted at least somewhat because of compensatory behavior from riskier driving. Cohen and Einav (2003) find that the moral hazard from seat belts that Peltzman hypothesizes is small relative to the safety-improving effect of seat belts. But Cohen and Dehejia (2004) find that automobile insurance, which also incentivizes riskier driving through moral hazard, results in 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 in-

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

dividuals more than doubles their number of sexual partners and contributes to a large increase in HIV incidence during the same period. Chan, Hamilton, and Papageorge (2016) provide 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 decreases.

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 overdoses (by 18 percent), drug-related arrests (by 16 percent), and opioid-related mortality (by 13–15 percent). The SEPs also increase local rates of theft by 24 percent. 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 trade-offs 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 judgment 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 and a spike in the drug's price resulted in a long-term reduction in consumption among those using heroin at the time. If drug use is sensitive to prices, then it is reasonable to hypothesize that it is also sensitive to nonmonetary 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 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 2018). Substance abuse may affect crime by leading users to steal or engage in illegal behavior to generate income to purchase drugs, through a direct physiological effect that makes users more aggressive, or 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 Centers for Disease Control and Prevention (CDC) mortality data for 1999–2014 and find that naloxone access laws substantially reduce deaths; our results using 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 mortality and crime, as well as an additional year of data, we provide more precise measures of these national effects. Most recently, Abouk, Pacula, and Powell (2019) differentiate between types of naloxone laws and find that granting pharmacists the authority to prescribe or dispense naloxone appears to reduce opioid overdose mortality; Erfanian, Collins, and Grossman (2019), however, use a spatial DD framework and find that certain naloxone access laws increase opioid overdose mortality by 11 per 100,000 people. We contribute evidence on when naloxone is available by third-party prescription or without a prescription, as with statewide standing orders.

3. Background

Opioid addiction now claims nearly 136 lives each day (CDC 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 the drugs are more easily accessible [Quinones 2015].) Such drug abuse can have fatal consequences, and policy makers 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 symptoms of addiction 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

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

Wen, widely advocated for naloxone to "be part of everyone's medicine cabinet" (Shesgreen 2016), and, US 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 make 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 include an age requirement beyond a provision for pharmacists' discretion on this factor. Slightly less broad than standing-order laws are third-party prescription laws, which allow purchase of naloxone by a resident "who is not at risk of overdose for use on someone else" (Substance Abuse and Mental Health Services Administration 2018). Because naloxone remains a prescription drug as categorized by the US Food and Drug Administration, standing orders and third-party prescriptions are enabled by the surgeon general of a state writing a prescription for all residents.

Other laws regulating naloxone access can cover prescribers' or dispensers' immunity (civil, criminal, and disciplinary), administering laypersons' immunity (civil and criminal), laypersons' distribution or possession (including without a prescription), and whether prescriptions by pharmacists are allowed. In practice, we could not find any evidence that the lack of immunity for providers or laypeople 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 those 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 are unable to separate the effects of individual law types (for example, the requirement to receive training from a pharmacist prior to obtaining naloxone is a relatively narrow 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 the laws led to meaningful increases in naloxone access—particularly by enabling community organizations to distribute and police officers to carry naloxone. However, state and local data on naloxone distribution during this period are typically unavailable.[5] Two exceptions provide numerical evidence that the laws resulted in an increase in naloxone distribution. The first is North Carolina, which broadened naloxone

[5] Automation of Reports and Consolidated Orders System data are commonly used to study drug distribution but focus on controlled substances, and so naloxone is most frequently reported only when it is found in conjunction with another drug. Pharmaceutical distribution data from 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.

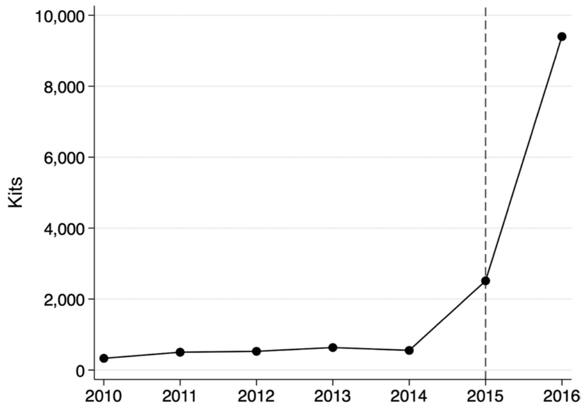

Figure 1. Distribution of naloxone kits in Baltimore

access in April 2013. Between August 2013 and August 2016, the state's Harm Reduction Coalition distributed naloxone kits to over 37,700 individuals (County Health Rankings and Roadmaps 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 9,399 kits in 2016.[6] These numbers imply that distribution jumped 1,764 percent after the law went into effect, from 42 kits per month to 783 kits per month.[7]

During this time period, states implemented other policies aimed at reducing opioid abuse and opioid-related deaths, and a rapidly growing literature estimates the policies' effects. For Meara et al. (2016), the authors constructed a database of such policies, most of which were aimed at changing opioid prescription behavior. The 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 particular policies or the number of policies enacted.

Other papers focus on the effects of prescription drug monitoring programs (PDMPs), which track patients' opioid prescriptions and provide the 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 and overdose deaths but notes that the reduction in mortality comes at the cost of reducing pain management. Back-of-the-envelope estimates suggest that the welfare gains from the programs are roughly equiv-

[6] See Baltimore City Health Department, Substance Use Disorder (https://health.baltimorecity.gov/programs/substance-abuse).

[7] Since similar data are not available across the country, we 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.

alent to the welfare losses. In related work, Schnell (2017) finds that physicians consider the secondary market for opioids and alter their prescribing behavior in response: the number of prescriptions would have been 13 percent higher in 2014 if a secondary market did not exist. This reduction in opioid prescriptions (some to patients in 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, the change led users to switch to heroin (Alpert, Powell, and Pacula 2018; Evans, Lieber, and Power 2019). Similarly, Mallatt (2017) finds that PDMPs increase heroin-related crimes (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 about the timing of naloxone access laws in each state. That information was cross-checked to the extent possible with previous research on the topic (for example, Davis and Carr 2015). Our main treatment variable Naloxone Law is coded according to whether a state has broadened access to naloxone through either third-party prescriptions or standing orders. Figure 2 shows month-year data to indicate how the number of states with naloxone access laws evolved over time (the vertical line indicates the end of our data), and Figure 3 shows states with broad naloxone access laws by year (New Mexico was the first in 2001). As Figures 2 and 3 show, naloxone access laws were adopted by a geographically and politically diverse set of states. All states eventually passed such laws, though our data go through only the end of 2015. Table 1 lists the dates we use in our analysis.[8]

To measure the impacts of those laws on opioid abuse, mortality, and crime, we use a variety of data sets. Ideal outcome measures would perfectly reveal risky consumption of opioids and opioid-related mortality and criminal behavior. Unfortunately, behavior is imperfectly observed. While each data set we use is an imperfect proxy for our outcomes of interest, in combination they paint a compelling picture of opioid-related behaviors.

Data on 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 in a specified term is quantified on a 0–100 scale 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.

[8] In five states, the specific date of broadened naloxone access is somewhat ambiguous because of the passage of related legislation, and one could argue that we should use an earlier date in our analysis. Using these alternate dates does not affect our results.

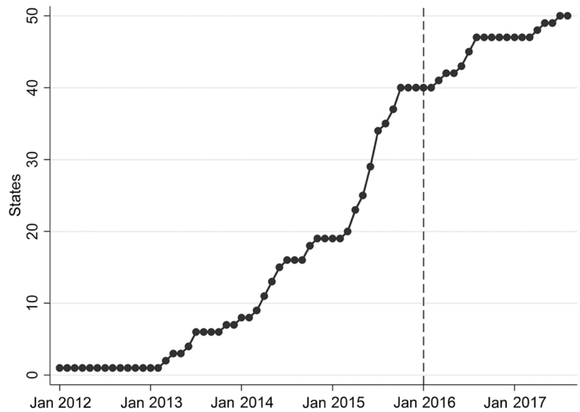

Figure 2. Timeline of naloxone access laws

Google Trends data are grouped by related search terms into topics—for instance, the topic "naloxone (drug)" includes searches for naloxone, Narcan, and some other highly similar terms (such as common misspellings). We verified that this grouping is nearly identical to an aggregation of search terms that we independently created and focus our analysis on data for searches of the topic "naloxone (drug)." We use monthly data for 2010–15 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 are also unavailable, so we again use Google Trends data as a proxy. We use data on searches related to the topic "drug rehabilitation" (which includes searches for "drug treatment") to measure interest in treatment for addiction. Table OB1 in the Online Appendix shows that this search index is highly correlated with admissions to drug treatment programs for opioid addiction, as recorded in the Treatment Episode Data Set (TEDS): a 1-unit increase in the search index is associated with 306 additional opioid-related treatment admissions, a 3.5 percent increase ($p < .05$). One problem with using TEDS data directly is that the data are available at only the state-year level; Google Trends data provide a more local and higher-frequency measure of interest in drug treatment that coincides with the broadened naloxone laws.[9] As before, we use monthly data for 2010–15 at the metropolitan-area level. Scores measure changes in search intensity in a metropolitan area between 2010 and 2015.

To consider effects on opioid-related criminal behavior (including the supply of and demand for illegal opioids), we use data from the National Incident-Based Reporting System (NIBRS) for 2010–15. This incident-level data set collects in-

[9] Another problem with using Treatment Episode Data Set 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.

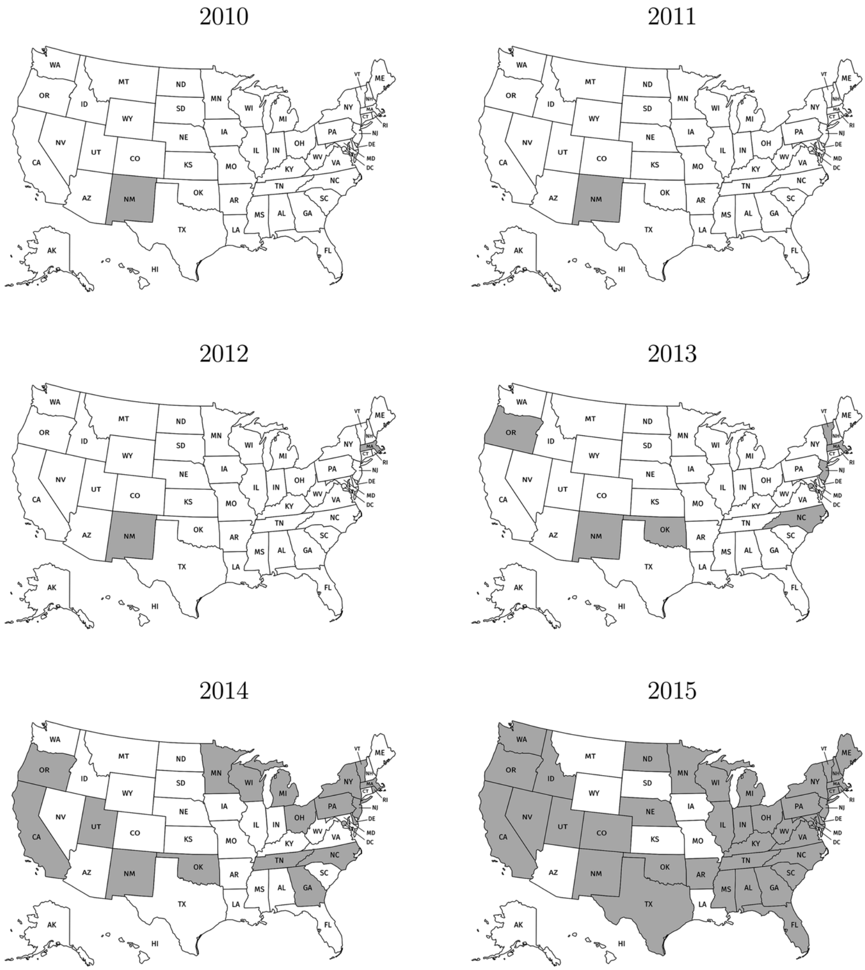

Figure 3. States with naloxone access laws

formation about reported crimes from local, state, and federal law enforcement agencies. The NIBRS data set includes rich incident-level information about reported offenses and arrests. Important to our study, drug and narcotic offenses include codes for a variety of opioids and other substances. One drawback of NIBRS is that not all jurisdictions[10] participate. We create a balanced panel of jurisdictions that report offenses in all months in 2010–15.[11] During that time

[10] In the National Incident-Based Reporting System, 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 agencies.

[11] 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 2010 and later.

Table 1
Timing of Naloxone Laws by State

State	Date	State	Date
Alabama	June 2015	Nevada	October 2015
Arkansas	July 2015	New Hampshire	June 2015
California	January 2014	New Jersey	July 2013
Colorado	April 2015	New Mexico	April 2001
Connecticut	June 2015	New York	June 2014
Delaware	June 2014	North Carolina	April 2013
District of Columbia	March 2013	North Dakota	August 2015
Florida	June 2015	Ohio	March 2014
Georgia	April 2014	Oklahoma	November 2013
Idaho	July 2015	Oregon	June 2013
Illinois	September 2015	Pennsylvania	November 2014
Indiana	April 2015	Rhode Island	October 2014
Kentucky	March 2015	South Carolina	June 2015
Louisiana	June 2016	Tennessee	July 2014
Maine	October 2015	Texas	September 2015
Maryland	October 2015	Utah	May 2014
Massachusetts	August 2012	Vermont	July 2013
Michigan	October 2014	Virginia	April 2015
Minnesota	May 2014	Washington	July 2015
Mississippi	July 2015	West Virginia	May 2015
Nebraska	May 2015	Wisconsin	April 2014

Note. Dates are hand collected for laws broadening naloxone access via third-party prescription or standing order by state. Alaska, Arizona, Hawaii, Iowa, Kansas, Missouri, Montana, South Dakota, and Wyoming broadened naloxone access in January 2016 or afterward.

period, 2,831 jurisdictions in 33 states submitted information to NIBRS, which represents roughly 24 percent 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 and narcotic violations, the NIBRS data also include information about up to three 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[12] (a proxy for quantity demanded), selling of opioids[13] (a proxy for quantity supplied), all opioid-related offenses (that is, any offense that includes an opioid-related violation), opioid-related theft, and all theft.[14] For offenses such

[12] This category includes the following official codes: buying or receiving, possession or concealing, and using or consuming.

[13] This category includes the following official codes: distributing or selling and transporting, transmitting, or importing.

[14] Theft includes pocket picking, purse snatching, shoplifting, theft from a building, theft from a coin-operated machine or device, theft from a motor vehicle, and all other larceny.

as theft (and other serious crimes), the variable 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 million local residents.

We are interested in theft as an outcome because opioid abusers may steal 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 the effects of changes in naloxone law on this broader category.

To measure abuse and overdoses involving opioids, we use data on opioid-related ER visits from the Healthcare Cost and Utilization Project (HCUP) for 2006–15. We acknowledge that visiting the ER has a compound probability, that is, the probability of overdose multiplied by the probability of an ER visit conditional on overdose. As a result, an increase in ER visits could indicate an uptick in only the latter probability, which would be a sign of the effectiveness of naloxone access—visiting the ER is strongly recommended on 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 an 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 ER data provide a quarterly measure of the number of ER visits by reason for the visit by state and by metropolitan-area type in the state.[15] (Since we 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.[16] This gives us a measure of how often local residents sought medical attention related to opioid abuse. As mentioned, if naloxone access leads to more overdoses—because users expect that it 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 downward if individuals administer naloxone and (against medical advice) do not subsequently seek medical attention for the person who overdosed. There is

[15] In contrast to the other data sets, we do not have county or city identifiers in the Healthcare Cost and Utilization Project (HCUP) data.

[16] Agency for Healthcare Research and Quality, Healthcare Cost and Utilization Project User Support, Opioid-Related Hospital Use (https://www.hcup-us.ahrq.gov/faststats/OpioidUseServlet).

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 upward 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 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 2010–15 from the CDC to measure deaths from 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 and 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 of 40,000 or more. In the NIBRS data, there are 410 jurisdictions across 31 states with populations of 40,000 or more, and they represent approximately 14 percent of the US 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.[17] We show that our results are not sensitive to this definition of "urban" and show results for rural areas and 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 includes data through 2012; we extend them 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 Meara et al. (2016) find 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 ac-

[17] Data in HCUP are aggregated by type of urban area: large central metropolitan, large fringe metropolitan, medium metropolitan, small metropolitan, and rural. Our definition of "metropolitan" combines all categories except the last one.

cess 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 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 database and are available at the jurisdiction-year level. Note that because we do not have city or county identifiers in the HCUP data, we are not able to control for police per capita in those analyses.

Finally, we consider whether the 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 are from the County Business Patterns data set maintained by the Census Bureau. Swensen (2015) also uses these data to find that a 10 percent increase in treatment facilities reduces a county's drug-induced mortality rate by 2 percent.

Summary statistics are in Table 2. Overall, there were 1,938 opioid-related ER visits per state and .7 opioid-related deaths per 100,000 residents; there were also 47.7 opioid-related crimes per million residents, 1.9 of which were opioid-related thefts. Table 2 also shows 2010 baseline means for states that adopted naloxone access laws relatively early (before the median month) and 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 control for jurisdiction fixed effects and state-specific trends in our outcome measures to account for these preexisting 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 DD framework. States vary considerably in the timing of the laws' passage, as shown in Figure 3. We categorize each state as having expanded naloxone access if a naloxone law is passed at any date in 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

Table 2
Summary Statistics, 2006–15

	All Jurisdictions and Years			Early Adopters in 2010			Late Adopters in 2010		
	N	Mean	SD	*N*	Mean	SD	*N*	Mean	SD
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
Metropolitan-area–months	21,528			2,172			1,416		
Opioid-related emergency room visits		1,938	2,360		2,420	2,149		898.1	739.5
State-quarters	1,108			64			52		
Mortality rates:									
Opioid related		.716	.693		.595	.600		.613	.560
Fentanyl related		.119	.291		.083	.205		.073	.157
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)		1,727	961.0		1766	980.2		2,194	904.6
Marijuana-related crime		229.2	193.0		234.8	202.6		283.6	147.8
Jurisdiction-months	29,952			4,200			792		

Note. Google Trends data are a normalized index from 0 to 100; observations are at the metropolitan-area-month level. Opioid-related emergency room visits are counts from the Healthcare Cost and Utilization Project recorded at the state-quarter level for metropolitan areas. Mortality rates (per 100,000 residents) are from restricted-use Centers for Disease Control and Prevention data. Crime rates (per million residents) are from the National Incident-Based Reporting System. Early adopters adopted naloxone access laws before the median adoption month; late adopters adopted laws after the median adoption month.

randomly assign some places to have broad access to naloxone and others not to have it. 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 preexisting 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 the drug. In particular, we 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):

$$\begin{aligned}\text{Crime Rate}_{jt} = \beta\text{Naloxone Law}_{jt} + \alpha_j + m_t + S_j \times t \\ + \text{Police}_{jt} + \text{Other Laws}_{jt} + \varepsilon_{jt},\end{aligned} \tag{1}$$

where j denotes the jurisdiction (that is, police jurisdiction, metropolitan area, county, or state) and t denotes the month-year (or quarter-year) of observation. The treatment variable, Naloxone Law, 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 preexisting state trends in the outcome measure. (Our results are robust to including jurisdiction-specific linear time trends instead.) The variable 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. The variable Other Laws_{jt} is a time-varying vector of other state-specific laws that the literature has identified as relevant to opioid use and abuse.[18] The error term ε 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. Pretreatment trends in coefficient plots for our outcome measures are visual evidence that our controls adequately absorb preexisting variation. We also show how the estimates are affected as we layer in 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, Naloxone Law, 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 fac-

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

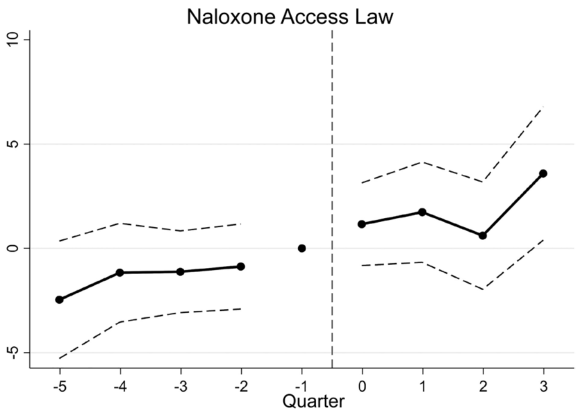

Figure 4. Effect of naloxone access laws on Google searches for "naloxone," 2010–15

tors that affect the availability of naloxone, including naloxone access laws, 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—that is, that the intent to treat does not indicate actual treatment—our estimates will be biased toward 0.

6. Results

6.1. Salience of Laws 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 naloxone distribution or purchases, this is as close as we can get to a first stage.) To address this, we use Google Trends data for 2010–15, quantifying online searches for "naloxone" and related queries.[19]

The results are shown in Figure 4 and Table 3. In Figure 4, the specification follows equation (1) but includes dummies for the quarter from the passage of a naloxone law instead of a dummy for whether there is such a law; that is, the covariate Naloxone Law_{jt} becomes $\sum_{q=-4}^{q=3} \text{Naloxone Law}_{jq}$, where $q = 0$ for the month of and 2 months following the effective date of a broadened naloxone law. The final category, $q = 3$, equals one for the third quarter and all subsequent quarters. The reference category is the period $q = -1$, which is the quarter prior to passage of the access law. The dotted curves indicate 95 percent confidence intervals. Pre-law effects on searches for "naloxone" are flat and near 0, which indicates that our control variables sufficiently absorb pretreatment-period trends. At the date of the law's implementation, the coefficient increases, and it remains above the ear-

[19] Google aggregates related search terms into the topic "naloxone (drug)." We use this aggregation as our outcome measure, as described above.

Table 3

Effect of Naloxone Laws on Google Searches and Opioid-Related Arrests, 2010–15

	Google Search		Arrests			
	Naloxone	Drug Rehab	Possession of Opioids	Selling Opioids	Possession of Fentanyl	Selling Fentanyl
Naloxone Law	1.847*	−.799+	4.030*	1.917**	2.578*	.780
	(.809)	(.450)	(.675)	(.214)	(1.155)	(.479)
N	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

Note. Standard errors, in parentheses, are clustered by state. The sample includes metropolitan areas for Google Trends data and jurisdictions with 40,000 or more residents for National Incident-Based Reporting System data on arrests. All regressions include jurisdiction and month-of-sample fixed effects, state-specific linear trends, police per capita, and the following laws or regulations: Good Samaritan laws, prescription drug monitoring programs, doctor shopping, pain-clinic regulations, physician exams, pharmacy verification, identification requirements, and tamper-resistant prescription forms. Coefficients in the first four columns are population weighted, as the dependent variable is a rate.

+ $p < .10$.
* $p < .05$.
** $p < .01$.

lier coefficients. On average, the coefficients after implementation are higher than the coefficients before implementation. The regression results tell a similar story: naloxone access laws cause the local intensity of Google searches for "naloxone" to increase by 7.2 percent ($p < .05$). 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 Table 3. The intensity of searches falls by 1.4 percent ($p < .10$). This effect is small and marginally significant, so it provides suggestive evidence that naloxone access reduces local interest in treatment for opioid addiction.

6.2. Effects on Opioid-Related Arrests

For the effects 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. 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 percent of the baseline, $p < .05$). Table 3 also 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, monthly arrests for the sale of opioids increase by 1.9 per million residents each month

(27 percent of the baseline, $p < .01$) 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. Table 3 considers 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 percent of the increase in arrests for opioid possession involves fentanyl and that this increase represents a 21 percent increase in fentanyl possession over its baseline ($p < .05$). About 41 percent of the increase in arrests for selling opioids comes from selling fentanyl, which represents a 29 percent increase in fentanyl sales over the baseline (not statistically significant). These estimates are about the same as for all opioids.

Not all opioid abuse shows 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 reduce or eliminate 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."[20] 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 Emergency Room 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 Figure 5*C* and Table 4. Figure 5*C* shows that pretreatment effects are flat and near 0; after the change in law, 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 percent,

[20] County Health Rankings and Roadmaps, Good Samaritan Drug Overdose Laws (https://www.countyhealthrankings.org/take-action-to-improve-health/what-works-for-health/strategies/good-samaritan-drug-overdose-laws).

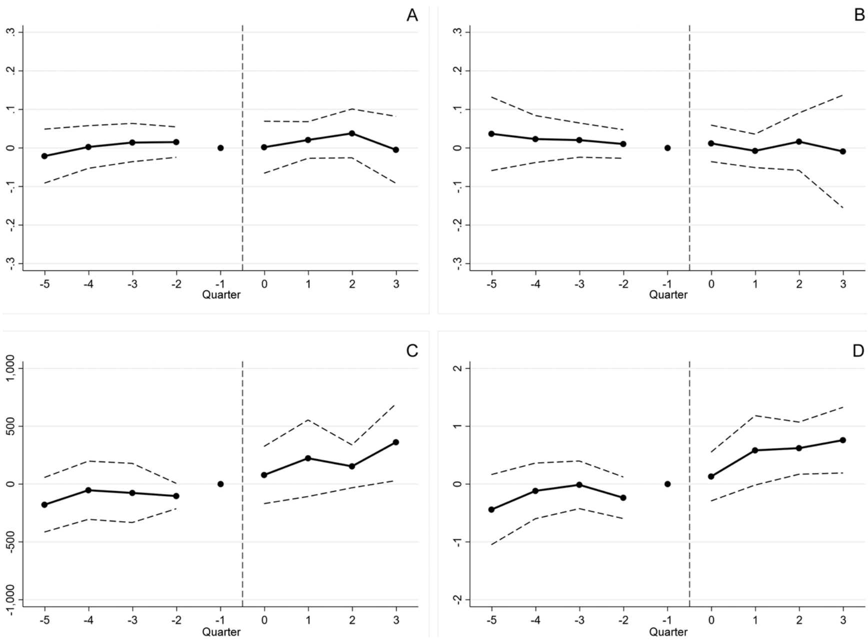

Figure 5. Effects of naloxone access laws on outcomes, 2006–15. *A*, Opioid-related deaths; *B*, fentanyl-related deaths; *C*, opioid-related emergency room visits; *D*, opioid-related theft.

$p < .05$). 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 Figure 5*A* and Table 4. On average across all urban areas, these laws have no significant impact on the opioid-related death rate. This is also true for deaths attributable to fentanyl, as shown Figure 5*B* and 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.

6.4. *Effects on Opioid-Related Crime*

Naloxone access saves, or at least extends, the lives of many 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. Broadening naloxone access increases all opioid-related crime by 6.0 per million (15 percent, $p < .05$), and opioid-related theft by .4 per million (30 percent, $p < .10$). Figure 5*D* shows a coefficient plot for the effect on opioid-related theft; while the pretreatment-period trend is flat, there is a clear increase after naloxone access laws went into effect.

These opioid-related crimes are offenses for which we know for sure that opi-

Table 4

Effect of Naloxone Laws on Opioid-Related Emergency Room Visits and Mortality, 2006–15

	Opioid-Related Emergency Room Visits	Opioid-Related Deaths	Fentanyl-Related Deaths
Naloxone Law	265.9+	.006	−.003
	(121.6)	(.027)	(.030)
N	1,108	55,512	55,512
2010 baseline	1,738	.601	.080

Note. Standard errors, in parentheses, are clustered by state. The sample includes metropolitan areas (for Healthcare Cost and Utilization Project data on emergency room admissions) and counties with at least one jurisdiction with 40,000 or more residents (for Centers for Disease Control and Prevention data on mortality). All regressions include jurisdiction and month-of-sample fixed effects, state-specific linear trends, police per capita (for deaths), and the following laws or regulations: Good Samaritan laws, prescription drug monitoring programs, doctor shopping, pain-clinic regulations, physician exams, pharmacy verification, identification requirements, and tamper-resistant prescription forms. Coefficients for deaths are population weighted.

$^{+}p < .10$.

oids were related in some way (for example, the offender may have had illegal opioids at the time of the offense or was stealing opioids), but the policy-relevant question is whether the total amount of crime increases. 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 (.3 percent, not significant) more thefts per million residents were reported each month after naloxone laws were passed. This effect is larger than the impact on opioid-related thefts alone but suggests that the social costs of naloxone laws in terms of additional property crime are small.[21]

[21] 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 OB4 in the Online Appendix shows effects for opioid- and fentanyl-related mortality by quartile of treatment availability. Since we do not have county identifiers in the HCUP data, we could not conduct this analysis for emergency room (ER) visits. The only statistically significant result is in column 5, which indicates 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 OB5 shows the effects of broadening naloxone access 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—we refrain from interpretation here given the wide confidence intervals on the mortality outcomes.

Table 5
Effect of Naloxone Laws on Crime, 2010–15

	Opioid-Related Crime	Opioid-Related Theft	All Theft
Naloxone Law	6.053*	.414+	4.810
	(2.213)	(.214)	(12.843)
2010 baseline	39.34	1.367	1,832

Note. Standard errors, in parentheses, are clustered by state. The sample includes jurisdictions with 40,000 or more residents. All regressions include jurisdiction and month-of-sample fixed effects, state-specific linear trends, police per capita, and the following laws or regulations: Good Samaritan laws, prescription drug monitoring programs, doctor shopping, pain-clinic regulations, physician exams, pharmacy verification, identification requirements, and tamper-resistant prescription forms. Coefficients are population weighted. $N = 29{,}808$.
$^{+}\ p < .10$.
$^{*}\ p < .05$.

6.5. Increasing Availability of Fentanyl

Throughout this period, the supply of fentanyl—a potent synthetic opioid—was increasing across the country. It is likely that this contributed to rising opioid-related mortality rates, and it is crucial that our estimates not confound the effects of naloxone access laws with independent changes in fentanyl supply. We believe that we successfully isolate 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 preexisting trends in fentanyl availability, there would be an increase in fentanyl-related deaths before the naloxone access laws go into effect. Instead, there are flat pretreatment trends; 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 do not realize their heroin is spiked with fentanyl. Since a single dose of naloxone usually will not be enough to save someone overdosing on fentanyl,[22] 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.

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

7. Robustness Checks and Additional Analyses

7.1. Alternative Controls for Pretreatment 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, which would lead to an estimation of null effects for various outcomes (Meer and West 2016; Borusyak and Jaravel 2018). To alleviate this concern, we estimate a model in which the state-specific linear time trends are estimated only on the basis of the pretreatment period (Goodman-Bacon 2021), that is, before the state passed a naloxone law. Table OB6 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 where we expect naloxone availability and distribution to have the biggest impact. We check whether naloxone saves lives in rural areas in Table OB7. The coefficients related to the mortality outcomes are not statistically significant in either rural areas or the full United States. Table OB8 shows how our mortality and theft results change by varying the population cutoff for urban areas. Recall that the definition in our main analyses is a city of at least 40,000 residents. The estimated effects of naloxone access on mortality are near 0 and statistically insignificant at all population cutoffs from 10,000 through 55,000. For opioid-related theft, the coefficients are a bit smaller and less statistically significant at higher populations, though they are qualitatively similar.

7.3. Checking for a Change in Recording of Opioid Involvement

Deaths from opioid abuse have often been labeled as being due to an unspecified drug (Ruhm 2017, 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. Table OB9 shows the results. The coefficient remains nonzero and statistically insignificant, which suggests that our mortality results are not driven by a change in how opioid-related deaths are recorded. The same pattern occurs 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 the laws and could independently affect our outcomes of interest. In Tables OB10–OB18 our estimates change as we layer additional controls. There is a stable pattern, which limits the scope for omitted-variable bias in our analysis. For example, in Table OB10, adding month-of-sample fixed effects has a large impact on the coefficient (which is not surprising), but the subsequent changes are smaller. Adding state-specific linear trends, which account for preexisting trends in opioid abuse, reduces the coefficient slightly. After that, controlling for police per capita (our proxy for investment in law enforcement), Good Samaritan laws, and an array of opioid laws has essentially no effect on the estimate—the estimate in column 3 is nearly identical to that in column 8. The results are similarly stable for the other outcomes.

7.5. Timing of Laws

We code naloxone access laws on the basis of 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 dates instead. The result are detailed in Table OB19; the estimates are extremely similar to those discussed above.

There may also be a concern that states that expanded naloxone access are not comparable to states that did not, at least during the period of the study. To address this possibility, we reestimated our model on all main outcomes using the sample of 40 states that expanded naloxone access prior to 2016. Table OB2 presents the results: all the point estimates are similar to those in our main analysis, though we lose some statistical significance because of the reduced sample. Because 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 OB1 and present the comprehensive set of results in Table OB3. 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, which 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) use data going back to 1999, while our data set 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 OB20 despite not being able to extend the data back to 1999.[23] In column 8, our specification is identical to the one in Rees et al. (2019) with the exception using data beginning in 1999, yet our estimate is positive and marginally significant ($p < .10$). For this reason, we suspect (but cannot confirm directly) that the difference in estimates comes from the longer pretreatment period used in Rees et al. (2019). While that paper's longer pretreatment period is advantageous in some ways, it may also have generated a misleading linear time trend in opioid-related deaths when the epidemic shifted toward heroin (versus prescription opioid) deaths in 2010–13, as noted in Maclean et al. (2020). For example, Rees et al. (2019) may overestimate the time trend in opioid-related deaths in 2010–15 because of fitting a line to this sudden spike in deaths, which leads them to uncover negative effects of naloxone access on this outcome. 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

Policy makers 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 naloxone distribution, so our estimates represent intent-to-treat effects. If naloxone access laws did not broaden naloxone access as much as proponents had hoped, then our estimates will be biased toward 0. We acknowledge that to fully estimate the empirical implications of the behavioral response to naloxone access, we would need to observe the change in mortality from naloxone access with and without such a 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.

[23] Table OB20 shows that paper's estimate of $-.188$ ($p < .10$) and our estimate of .006 ($p > .10$). Subsequent columns layer in the Rees et al. (2019) specification to our analysis to demonstrate how the coefficient changes as we aggregate to the state level, aggregate to the year level, convert the dependent variable to log mortality rate, drop the 2015 data not used in Rees et al., add other controls such as beer taxes, and match the dates of naloxone law passage.

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