The Moral Hazard of Lifesaving Innovations: Naloxone Access, Opioid Abuse, and Crime*

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The United States is experiencing an epidemic of opioid abuse. In response, many states have increased access to Naloxone, a drug that can save lives when administered during an overdose. However, Naloxone access may unintentionally increase opioid abuse through two channels: (1) saving the lives of active drug users, who survive to continue abusing opioids, and (2) reducing the risk of death per use, thereby making riskier opioid use more appealing. By increasing the number of opioid abusers who need to fund their drug purchases, Naloxone access laws may also increase theft. We exploit the staggered timing of Naloxone access laws to estimate the total effects of these laws. We find that broadening Naloxone access led to more opioid-related emergency room visits and more opioid-related theft, with no reduction in opioid-related mortality. These effects are driven by urban areas and vary by region. We find the most detrimental effects in the Midwest, including a 14% increase in opioid-related mortality in that region. We also find suggestive evidence that broadening Naloxone access increased the use of fentanyl, a particularly potent opioid. While Naloxone has great potential as a harm-reduction strategy, our analysis is consistent with the hypothesis that broadening access to Naloxone encourages riskier behaviors with respect to opioid abuse.

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1 Introduction

The United States is grappling with an epidemic of opioid abuse and overdoses: in 2016, over 42,000 people died due to an opioid overdose, a number that has increased steadily over the past decade and now constitutes two-thirds of all drug overdose deaths (Centers for Disease Control and Prevention, 2018). 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. One prominent public health official has even called for Naloxone in every medicine cabinet (Shesgreen, 2016). But reducing the risk associated with abusing opioids might have the unintended consequence of increasing opioid abuse. Increased abuse could lead to higher crime rates, even higher death rates from overdose.

We expect these unintended consequences to occur through two channels: (1) opioid abusers are saved by Naloxone and continue their previous drug use and criminal behavior, and (2) the reduced risk of death makes opioid abuse more appealing, leading some to begin using opioids—or to use greater quantities than they did before—when they have Naloxone as a safety net. Some of those abusers may become criminally active to fund their increased drug use.

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.

Media reports offer anecdotal evidence of these effects. Stories about Naloxone parties—where attendees use heroin and prescription painkillers knowing that someone nearby has Naloxone in case they overdose—have worried legislators.[1] News reports also highlight cases

[1] "With Narcan [the brand name of Naloxone], 'kids are having opioid parties with no fear of overdose,' Sen. Lisa Boscola, D-Northampton, said Tuesday at a public hearing in the Allegheny County Courthouse conducted by a House-Senate task force exploring solutions to opioid abuse. ... 'I can tell you, drug dealers are throwing Narcan parties,' said Rep. Daniel McNeill, D-Lehigh County" (Siegelbaum, 2016).

where police find Naloxone alongside opioids when they search a home or car, and quote first-responders who are frustrated that the same individuals are saved again and again by Naloxone without getting treatment.[2]

Our analysis of panel data from across the United States confirms these anecdotal reports. We use the gradual adoption of state-level Naloxone access laws as a natural experiment to measure the effects of broadening Naloxone access, and find that the moral hazard generated by Naloxone is indeed a problem—resulting in increased opioid abuse and crime, and 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.

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.

We first confirm that the laws had an impact on residents' awareness of and interest in Naloxone: using data on Google searches, 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 increased opioid abuse. After Naloxone access laws take effect, Google searches for "drug rehab" (a proxy for interest in drug treatment) fell by 1.4%, arrests for possession and sales of opioids increased by 17% and 27%, respectively, opioid-related visits to the emergency room increased by 15%, and opioid-related theft increased by 30%. Meanwhile, expanding access to Naloxone had no

[2] "'We've Narcan'd the same guy 20 times,' Dayton police Major Brian Johns said. 'There has to be some sort of mechanism or place for people like that. If you're not going to get help, we're going to require you to get some sort of treatment going. Because that is a waste of police resources'" (Gokavi, 2017). See also Stoffers (2015) and Russell and Anderson (2016) .

effect on opioid-related mortality, on average.[3]

The average effect on mortality masks substantial regional differences, which might be expected given the geographic variation in opioid-related deaths documented in Case and Deaton (2015). In Midwestern states, Naloxone access led to a 14% increase in opioid-related mortality, and an 84% increase in fentanyl-related mortality. It appears that Naloxone access exacerbated the opioid-mortality crisis in this area. Effects in other regions were statistically insignificant but non-zero: mortality increased in the South but fell in the West and Northeast.[4]

Differences in access to drug treatment may explain this heterogeneity in policy effects: we find that places with fewer drug treatment facilities per capita experienced bigger increases in mortality when they broadened Naloxone access. (Alternatively, easier access to treatment is associated with more beneficial policy effects.) This is consistent with the hypothesis that treatment availability helps mitigate the detrimental effects of opioid abuse, and provides an opportunity for those whose lives are saved by Naloxone to learn how to manage their addiction.

A variety of robustness checks support our main results. We find no evidence that pre-existing trends are driving these effects, and our estimates are robust to controlling for an array of other state policies aimed at reducing opioid abuse and mortality. "Placebo" tests on outcomes that should not be directly affected by Naloxone access—deaths due to suicide, heart disease, and motor vehicle accidents—provide additional evidence that our effects are not driven by other trends or policy changes (in particular, those related to economic despair, broad health trends, or risky behaviors). 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," controlling for more flexible time trends, and dropping

[3] As expected, effects in rural areas were typically statistically insignificant.

[4] As we'll show in Section 5.4.7, mortality also significantly increases in the Northeast when we exclude New York State.

individual states one-by-one.

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 a context closer to our own, 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.

The mechanical effect of saving lives on the pool of opioid abusers is closer to the mechanism explored in Donohue and Levitt (2001). That paper found that legalizing abortion reduced crime. The intuition is that the children who would have been born into unstable environments or who would have been cared for less were less likely to be born after abortion became an option; since such individuals are at higher risk of criminal activity, this reduced crime approximately twenty years later. Our paper considers the inverse of this mechanism: does saving the lives of criminally-active opioid users increase crime rates? Consistent with this story, we find that Naloxone access laws increase opioid-related crime and opioid-related theft specifically. (However, we will not be able to separately identify this mechanical effect from the effect of moral hazard; our estimates will represent the combined effect of both channels.)

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 surely clouds judgement and makes policy in this area difficult, but there is substantial evidence that even drug users respond to incentives. A large body of empirical evidence documents that the consumption of addictive substances is sensitive to prices. 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 (2017) 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. These findings suggest that, at least on the margin, drug abuse may be 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. Policies that increase alcohol consumption also increase violent crime (Cook and Durrance 2013; Anderson, Crost and Rees 2017). Substance abuse may also 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.

The only other study of Naloxone's impact on opioid-related mortality is Rees et al. (2017), written contemporaneously. That paper uses annual, state-level CDC mortality data from 1999 through 2014 to measure the effects of Naloxone access laws and Good Samaritan laws on opioid-related mortality. They find that Naloxone access laws substantially reduce deaths – very different from our finding of no effect overall and an increase in mortality in the Midwest. We believe that by using monthly city- or county-level data instead of annual state-level data, and by controlling for a larger suite of opioid-related legislation, we more precisely measure the effects of these laws on mortality. Our inclusion of 2015 data allows us to examine effects in more jurisdictions, since the vast majority of states passed Naloxone access laws in 2014 or later.

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

2 Background

Opioid addiction now claims nearly 115 lives each day. 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 tar-

get populations of known drug-abusers. For example, Baltimore's health commissioner, Dr. Leana Wen, has widely advocated for Naloxone to "be part of everyone's medicine cabinet" (Shesgreen, 2016).

Until very recently, Naloxone required a doctor's prescription to obtain, and many worried about civil or criminal liability that might come from prescribing the drug to someone at risk of overdose, or administering it to someone who appeared to be overdosing (Network for Public Health Law, 2017). 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. 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 "third party" entities (such as pharmacists). By mid-2017, all states had implemented some sort of Naloxone-access law. Since states typically passed these laws as a package or in close succession, we will be unable to separate their effects. We therefore focus our attention on the date at which *any* Naloxone access law became effective (we define this more precisely in the next section).

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 physi-

cians. 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 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 recent papers 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, 2017 and 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 finding highlights the importance of considering the behavioral consequences of policies in this area, and the difficulty of reducing opioid abuse.

3 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 in at least one of three ways: (1) provided legal immunity to prescribers of Naloxone, (2) provided legal immunity to laypersons who administer Naloxone, or (3) allowed third-person prescription of Naloxone, including standing orders (allowing anyone to walk into a pharmacy and purchase Naloxone without a prescription from a doctor). Figure 1 shows how the number of states with Naloxone access laws evolved over time, and Figure 2 shows maps of the states with 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.

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

We use Google Trends data on internet searches as proxies for local interest in Naloxone and drug treatment 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 do the same for a "drug rehabilitation" topic search, to measure interest in treatment for addiction. 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.

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 because the data collection program is still being rolled out, not all jurisdictions[5] participate. We create a balanced panel of jurisdictions that report offenses in all months of 2010–2015.[6] 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 would 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[7] (a proxy for quantity demanded), selling of opioids[8] (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.[9] For offenses such as theft (and other serious crimes), the variable measures reported crime. For offenses such as possession of or selling opioids, the variable measures 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

[5]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.

[6]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.

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

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

[9]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.

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 emergency room (ER) visits from the Healthcare Cost and Utilization Project (HCUP) for years 2006-2015. These 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.[10] (Since we 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. 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 (but in this case we

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

would expect a corresponding decrease in mortality).

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.[11] 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, prescription-drug monitoring programs (PDMPs), doctor-shopping restrictions, pain-clinic regulations, physician examina-

[11]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.

tion requirements, pharmacy verification requirements, patient identification requirements, and requirements related to tamper-resistant prescription forms. While that study's analysis suggests 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 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 (2016), 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.

Summary statistics are in Table 1. Columns 1 and 2 show means and standard deviations for relevant variables in all jurisdictions. Overall, there were 1,938 opioid-related ER visits 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.

4 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 2. 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 specifica-

tions 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. $Police_{jt}$ 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.[12] The term ϵ is an error term that is clustered at the state level for estimation. All estimates 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 graphs of 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

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

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.

5 Results

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.[13]

Results are show in Figure 3, and in column 1 of Table 2. Conditional on our control variables, the pre-trend in "Naloxone" searches is essentially flat, but jumps upward at the date of the law's implementation and then trends upward. 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 2. We find that the intensity of searches for "drug rehab" falls by 1.4% ($p < 0.10$). This effect is small but marginally significant, and if Google searches are correlated

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

with actual behavior it is consistent with the hypothesis that Naloxone access reduces opioid abusers' interest in treatment for their addiction.

Column 3 of Table 2 sheds light on whether opioid abusers' behavior is indeed changing. In particular, we test whether Naloxone access laws affect arrest rates for possession of opioids. We consider this a proxy for quantity of illegal opioids demanded. Consistent with the moral hazard story, we find 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 2 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 increases 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, and 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.)

Broadening availability of Naloxone may have encouraged the distribution of fentanyl – a more potent opioid that achieves "higher highs" but at greater risk to the user. (Fentanyl is often mixed into heroin; the more fentanyl is mixed in, the stronger the drug, but the less effective Naloxone will be in stopping an overdose.) Indeed, the abuse of fentanyl increased tremendously during this period (Lewis et al., 2017). Columns 5 and 6 of Table 2 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, so do not provide evidence that Naloxone access is having a disproportionate impact on fentanyl distribution – but again, these recorded drug types may not be accurate. Since fentanyl-laced heroin may be mistaken for heroin at the the time of arrest, these estimates probably represent a lower bound on the true effect.

Not all opioid abuse will show up in arrest data.[14] To further investigate changes in opioid abuse, and corroborate the findings above, we use HCUP data to consider the effect of Naloxone access laws on opioid-related ER visits. These results are shown in Figure 4 and in column 1 of Table 3. We find that broadening Naloxone access led to more opioid-related ER visits: Naloxone access laws increased the quarterly number of visits by 266 per 100,000 residents (15%, $p < 0.05$). This effect is large and consistent with the hypothesis that Naloxone access increases the abuse of opioid drugs.

Naloxone access reduces the risk of death for each use of a given quantity of opioids, but it also appears to increase the number of uses (and/or the potency of each use) – consistent with the idea that moral hazard leads users to "seek higher highs" that increase their risk of an overdose. This leads to more ER visits, but many of those lives will be saved. What is the net impact on mortality?

Figure 5 and Column 2 of Table 3 show the effect of Naloxone access laws on all opioid-related mortality as recorded in CDC data. On average across all urban areas, we find that these laws have no significant impact on the opioid-related death rate. Thus, while the risk per use has gone down due to Naloxone access, the number of uses increases enough that we find no net effect on opioid-related mortality.

[14] 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 – but 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.

We again consider the possibility that the "safety net" of Naloxone may have led users and sellers to trade in more potent forms of opioids – in particular, fentanyl. Figure 6 appears to show an increase in fentanyl-related deaths that is not explained by pre-trends in mortality. However, Column 3 of Table 3 shows no effect on fentanyl-related mortality, at least in the aggregate. (We will consider regional differences in mortality effects below.)

Naloxone access saves, or at least extends, the lives of many existing opioid abusers and increases the number of new opioid abusers. Both effects could increase criminal activity, particularly theft committed to fund an addiction. Table 4 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%, $p < 0.10$). These opioid-related crimes are ones 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 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 any social costs of Naloxone laws – in terms of additional property crime – are small.

5.1 Differences by region

There have been regional differences in opioid abuse and there may therefore be regional differences in how behavior changes in response to Naloxone access. Figures 8 and 9 show mortality trends by Census region, while Table 5 presents all of our main results separately for each region.

The most striking difference from the average effects discussed above is that 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% ($p < 0.10$). 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 proxies for opioid abuse also show large increases in the Midwest and the South. Effects on crime and arrests are positive in the Northeast, though opioid-related ER visits appear to fall in that region. In the West, the directions of effects are more mixed, suggesting the the (insignificant) decrease in mortality is the primary story there.[15]

5.2 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. This is partly because cities have more funding to purchase and supply Naloxone, and partly because the concentration of bystanders and shorter 911 response times should increase the likelihood than someone will administer Naloxone in the case of an overdose. However, opioid abuse is also an important problem in rural areas and Naloxone distribution has occurred there as well. If Naloxone saves lives in rural areas, that could counterbalance the increases in mortality that we see in urban locations.

To check this, we consider effects in rural areas, shown in Table 6. The first panel shows results across the entire U.S. and the remaining panels show results by region. Estimates are generally statistically insignificant, though the coefficients on the mortality estimates are negative outside of the Midwest. Table 7 shows effects of Naloxone laws across all areas (that

[15] One possible explanation for the decline in mortality in the West is that black tar heroin is more common there than elsewhere in the United States (Quinones, 2015). 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. Unfortunately, we are unaware of data on black tar heroin distribution that would allow us to test this hypothesis.

is, combining urban and rural areas into a single sample). Since estimates are population-weighted, and most people live in cities, the results are very similar to the main results above for urban areas. Overall, we find that Naloxone access increases opioid abuse and has no net effect on mortality rates.

Finally, not all cities are the same in terms of their public health infrastructure and resources. Indeed, we find that the effects of Naloxone vary within our urban sample.

The first panel of Table 8 shows effects of Naloxone access laws in the largest cities: the top 25 by population for the mortality measures and large central metropolitan areas for the ER visit measure. All of the coefficients on our proxies for opioid abuse are negative, though statistically insignificant. This suggests that Naloxone access may be having beneficial effects in the very largest cities.[16]

The second panel of Table 8 considers the remaining cities in our urban sample: those that are not in the "largest cities" category. In these small to large cities, we find that Naloxone access laws increase the number of opioid-related ER visits by 23% ($p < 0.05$), opioid-related mortality by 5.5% (not significant), and fentanyl-related mortality by 42% ($p < 0.05$). Again, the average effect across urban areas masks important heterogeneity, this time by city size.

5.3 Differences by availability of drug treatment

One possible explanation for these differences by region and city size is the availability of drug treatment for those who seek rehabilitation. We use county-level data from the Census on the number of drug treatment facilities per 100,000 residents, to explore whether effects of Naloxone access laws vary with this measure. It is, of course, not random that some places have more drug treatment facilities than others: this could be a proxy for public health infrastructure and investment more broadly (including distribution of free or cheap Naloxone). But it is, most directly, a measure of the likelihood that there is capacity to treat someone who is struggling with addiction. To the extent that the intention of broadening

[16]NIBRS contains very few large cities and so we do not report crime outcomes here.

Naloxone access is to give addicts a chance to get help, the availability of treatment seems like an important factor that could explain this policy's widely-varying effects. In addition, access to treatment could provide help to new addicts who increased their opioid use in response to broad Naloxone access, thus mitigating the worst effects of opioid abuse.

Table 9 shows effects for our mortality and crime outcomes, by quartile of treatment availability (Q1 is low, Q4 is high).[17] It appears that Naloxone access increases opioid-related mortality in places with limited treatment and decreases it in places with more treatment. We do not have enough statistical power to be sure that these effects are statistically different from one another, but this pattern is consistent with the hypothesis that broadening Naloxone access has less detrimental effects in places with more resources available to help those suffering from addiction.

Patterns in opioid-related crime and arrests are less clear. We tend to see larger increases in opioid-related crime and arrests for possession in places with more drug treatment facilities. This seems consistent with the hypothesis that saving more addicts' lives increases the stock of drug users and the pool of people who need to fund their addictions, but it is suggestive evidence at best.

5.4 Robustness checks

5.4.1 Placebo test

Our goal is to isolate the effect of Naloxone laws on opioid abuse. Because these laws are not implemented at random, we might worry that they are correlated with other trends or policy changes that could explain our results. We conduct three placebo tests to rule out alternative explanations for our mortality findings (and, by extension, our findings that support an increase in opioid abuse). The results are in Table A.1.

The first panel considers the effect of Naloxone access on deaths due to suicide. This outcome should not be affected by access to Naloxone but would be affected by a general

[17] Since we do not have county identifiers in the HCUP data, we could not conduct this analysis for ER visits.

increase in economic despair, which Case and Deaton (2017) hypothesize is a driver of the opioid epidemic (and might have driven policy-makers to expand Naloxone access). We see no effect of Naloxone laws on suicide rates.

The second panel considers the effect of Naloxone access on deaths due to heart disease, which again should not be affected by Naloxone but would be affected by a general decline in health. We find no effect of Naloxone access on death rates due to heart disease.

The third panel considers the effect of Naloxone access on deaths due to motor vehicle accidents. This outcome should not be affected by Naloxone but would be affected by a general increase in risky behavior, which may be a driver of opioid abuse. We find no effect of Naloxone access on death rates due to motor vehicle accidents.

Overall, these placebo tests support the main findings presented above. It appears that our empirical strategy is successfully isolating the effect of broadening Naloxone access from other trends that might drive opioid abuse.

5.4.2 Checking for a change in recording of opioid involvement

It is possible that Naloxone access laws increased the likelihood that opioids were correctly recorded as being involved in an event, rather than the likelihood that they were involved in the first place. While we cannot directly test the accuracy of recording, we can measure effects on broader outcome categories to see if the overall effect is similar to our estimates for opioid-related outcomes. Looking at these broader categories adds substantial noise to the data, so our estimates will be less precise. But the magnitudes of the coefficients should still be informative.

Deaths due to opioid abuse have often been labeled as due 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 A.2 shows the results for the full country and by Census region. While no

longer significant, the coefficients are very similar to our main results. This suggests that our mortality results are not being driven by a change in how opioid-related deaths are being recorded.

As described above, Column 3 of Table 4 shows the effect of Naloxone access laws on all theft. The coefficient is 4.8, which is twelve times larger than the 0.4 estimate found for opioid-related theft. This non-zero result is consistent with the claim that the increase in opioid-related theft is not simply due to better labeling of other thefts as opioid-related. The second panel in Table A.2 shows this effect for the entire U.S., along with effects by region. The effect is largest (and marginally significant) in the Midwest, where our results indicate the largest increases in opioid abuse. It suggests that Naloxone access increased total theft by 2.6% ($p < 0.10$) in the Midwest.

5.4.3 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. This concern about omitted variables is impossible to test directly. However, in Tables A.3 through A.11 we show how each of our estimates change as we layer in additional controls. Where the estimates stabilize, not changing substantially as new controls are added, we can be more confident that adding more controls would not have a meaningful impact on our findings.

This is what we find. For instance, in Table A.3, adding month-of-sample fixed effects has a large impact on the coefficients (which is not surprising), but from then on the 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 laws aimed at reducing opioid prescriptions and abuse, had essentially no effect on the estimate. The estimate in Column 3 is nearly identical to that in Column 8. This pattern is similar

for the other outcomes.

5.4.4 Types of opioids involved in crime

We expect most of the effect of Naloxone access laws to be on abuse of heroin, prescription pills, and fentanyl. Table A.12 shows the effects on opioid-related crime separately by opioid type. About 40% of the increase comes from heroin-related crime, and the other 60% comes from crime related to "other narcotics", the category that includes both prescription pills and fentanyl.

5.4.5 Varying the population cutoff for "urban"

Table A.13 shows how our opioid-related mortality and theft results change with different definitions of "urban". 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.

5.4.6 More flexible state-specific trends

While the flat pre-trends in our graphs suggest that our main specification is adequately soaking up pre-existing variation in our outcome measures, one might be worried that the state-specific linear trends are too restrictive. For this reason, we implement our analyses with state-specific cubic trends; these results are in Table A.14. This specification strains our statistical power but the results are qualitatively similar.

5.4.7 Dropping one state at a time

Tables A.15 through A.18 show how the estimates change as we drop one state at a time from the analysis, region-by-region.

Of particular interest are the effects on opioid-related mortality, measured in deaths per 100,000 residents. The estimated effects in the Midwest range from 6.1 (not significant, when dropping Michigan) to 12.7 ($p < 0.05$, when dropping Ohio). Estimates in the South range from 1.6 (not significant, when dropping Florida) to 8.7 ($p < 0.05$, when dropping North Carolina). Estimates in the Northeast range from -8.6 (not significant, when dropping New Jersey) to 14.6 ($p < 0.05$, when dropping New York). Estimates in the West range from -15.5 ($p < 0.05$, when dropping California) to -3.5 (not significant, when dropping Arizona).

The sensitivity of the results in the Northeast to the inclusion of New York is particularly striking. In that region overall, we see a decline in opioid-related mortality when Naloxone access is expanded. But when New York is excluded, we see a larger increase in opioid-related mortality in the remaining Northeastern states than we do in the Midwest (14.6 vs. 9.4 deaths per 100,000 residents).

The estimate ranges for the other outcomes contain fewer surprises.

Effects on "Naloxone" Google searches range from: 0.670 (not significant, when dropping Michigan) to 3.184 ($p < 0.10$, when dropping Ohio) in the Midwest; 0.548 (not significant, when dropping Louisiana) to 2.299 ($p < 0.10$, when dropping Georgia) in the South; 2.283 (not significant, when dropping New Hampshire) to 8.103 ($p < 0.05$, when dropping New York) in the Northeast; and 0.876 (not significant, when dropping California) to 3.253 ($p < 0.10$, when dropping Idaho) in the West.

Effects on opioid-related ER visits range from: 24.59 (not significant, when dropping Ohio) to 488.5 ($p < 0.05$, when dropping Nebraska) in the Midwest; 148.1 (not significant, when dropping North Carolina) to 310.6 ($p < 0.05$, when dropping Tennessee) in the South; -154.8 (not significant, when dropping Massachusetts) to 54.73 (not significant, when dropping New Jersey) in the Northeast; and -35.86 (not significant, when dropping Utah) to 137.1 (not significant, when dropping California) in the West.

Effects on opioid-related theft range from: -0.185 (not significant, when dropping Iowa) to 0.323 ($p < 0.05$, when dropping Ohio) in the Midwest; -0.213 (not significant, when dropping

Virginia) to 0.457 ($p < 0.05$, when dropping Texas) in the South; 0.509 (not significant, when dropping New Hampshire) to 3.256 (not significant, when dropping Connecticut) in the Northeast; and 1.13 ($p < 0.05$, when dropping Oregon) to 1.557 ($p < 0.01$, when dropping Idaho) in the West.

6 Discussion

Policymakers have multiple levers available to fight opioid addiction, and broadening Naloxone access aims to directly address the most dire risk of opioid overdose: death. Naloxone can save lives and provide a second chance for addicted individuals to seek treatment, but access to this lifesaving drug may unintentionally increase opioid abuse by providing a safety net that encourages riskier use. This paper shows that expanding Naloxone access increases opioid abuse and opioid-related crime, and does not reduce opioid-related mortality. In fact, in some areas, particularly the Midwest, expanding Naloxone access has increased opioid-related mortality. Opioid-related mortality also appears to have increased in the South and most of the Northeast as a result of expanding Naloxone access.

Our findings do not necessarily imply that we should stop making Naloxone available to individuals suffering from opioid addiction, or those who are at risk of overdose. They do imply that the public health community should acknowledge and prepare for the behavioral effects we find here. Our results show that broad Naloxone access may be limited in its ability to reduce the epidemic's death toll because not only does it not address the root causes of addiction, but it may exacerbate them. Looking forward, our results suggest that Naloxone's effects may depend on the availability of local drug treatment: when treatment is available to people who need help overcoming their addiction, broad Naloxone access results in more beneficial effects. Increasing access to drug treatment, then, might be a necessary complement to Naloxone access in curbing the opioid overdose epidemic.

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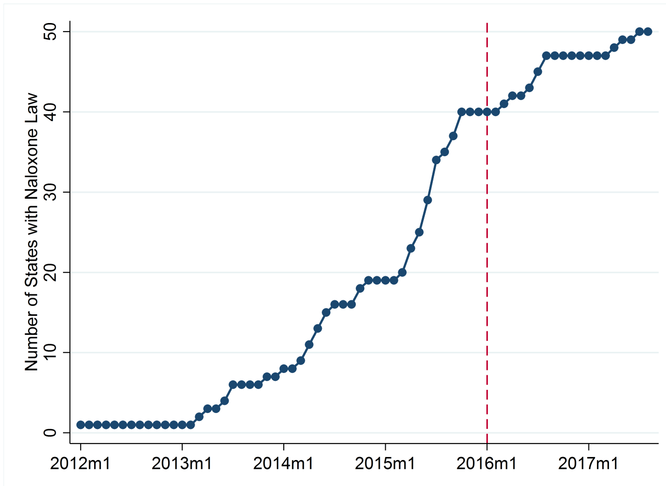
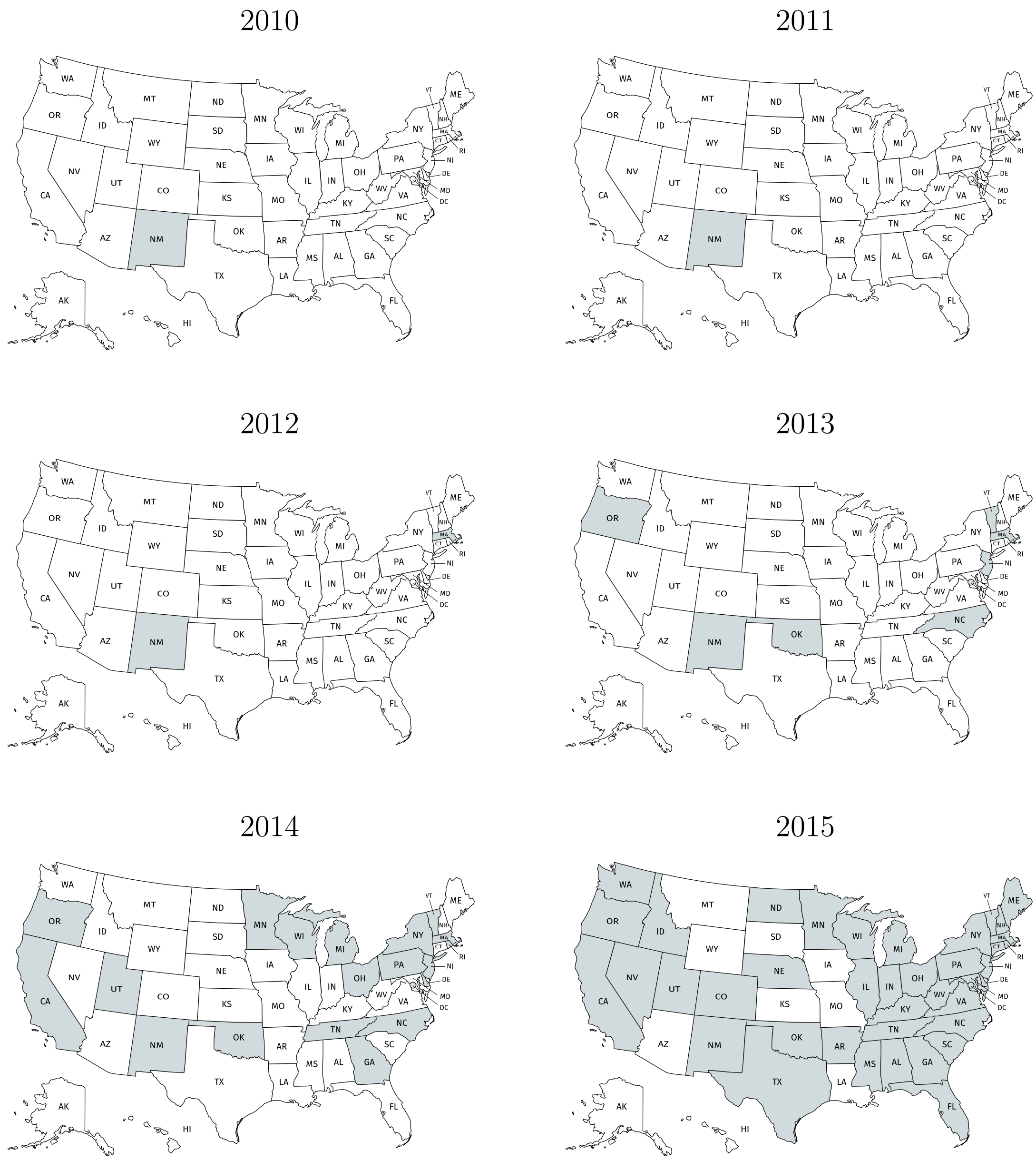
Figure 1: Timeline of Naloxone access laws



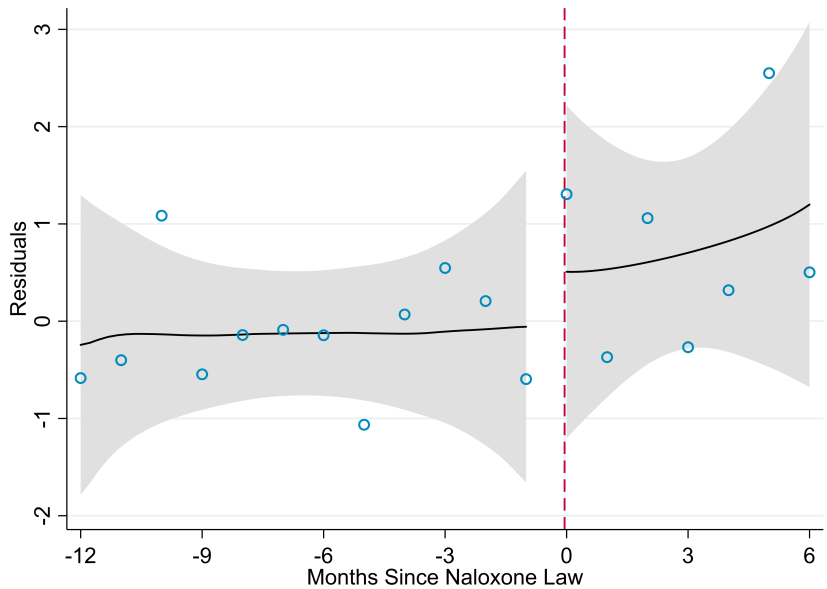
Notes: Figure shows the number of states with any broadened Naloxone access law in each month-year between January 2012 and July 2017, by which point all states had such laws. The data include all 50 states. Categorization of state-by-state Naloxone laws was done using hand-collected data. Our analyses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

Figure 2: States with Naloxone access laws, by year



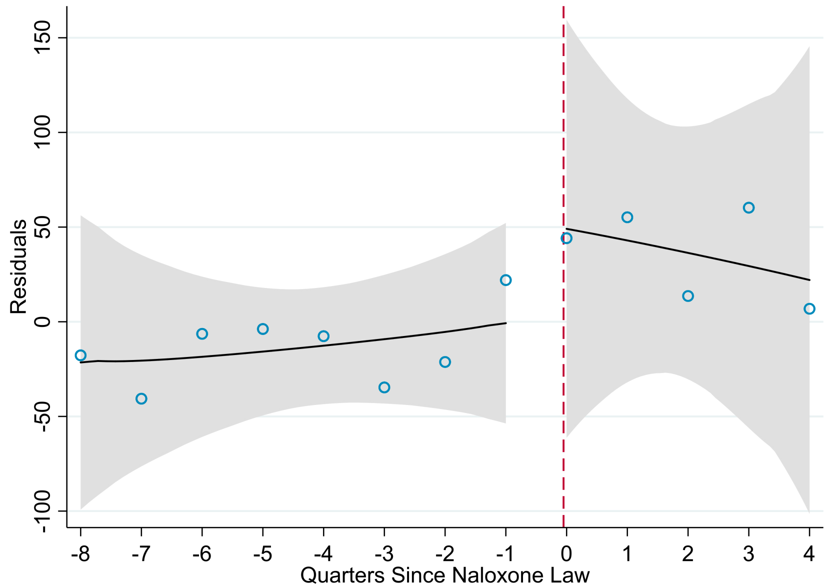
Notes: Figure shows the states with Naloxone access laws by December 31 of each year. These states are shaded; New Mexico was the first state to broaden access and did so in 2001.

Figure 3: Effect of Naloxone access laws on Google searches for "Naloxone"



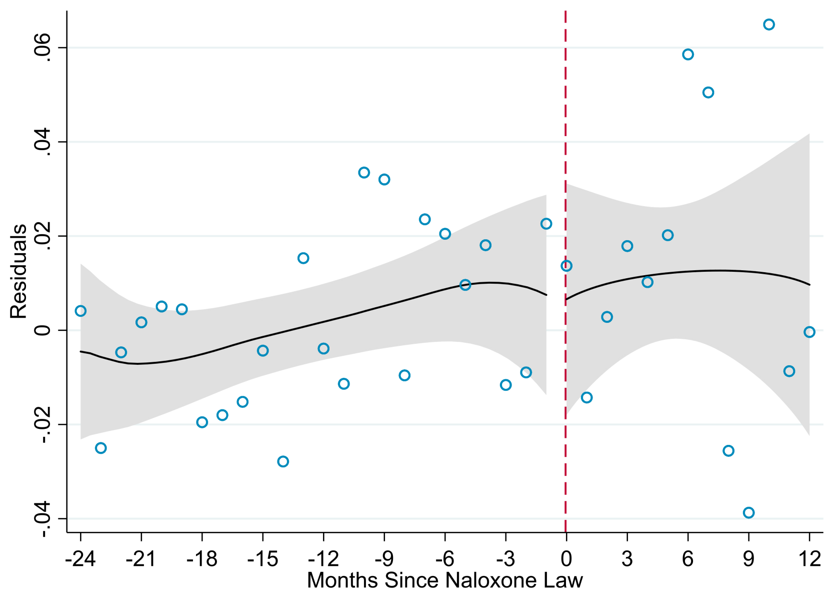
Notes: Figure shows residuals from a regression of Google searches related to "Naloxone". The regression uses 2010-2015 monthly data for all metro areas across the country. The regression includes all controls in column (1) of Table 2 except for "Naloxone Law".

Figure 4: Effect of Naloxone access laws on opioid-related ER admissions



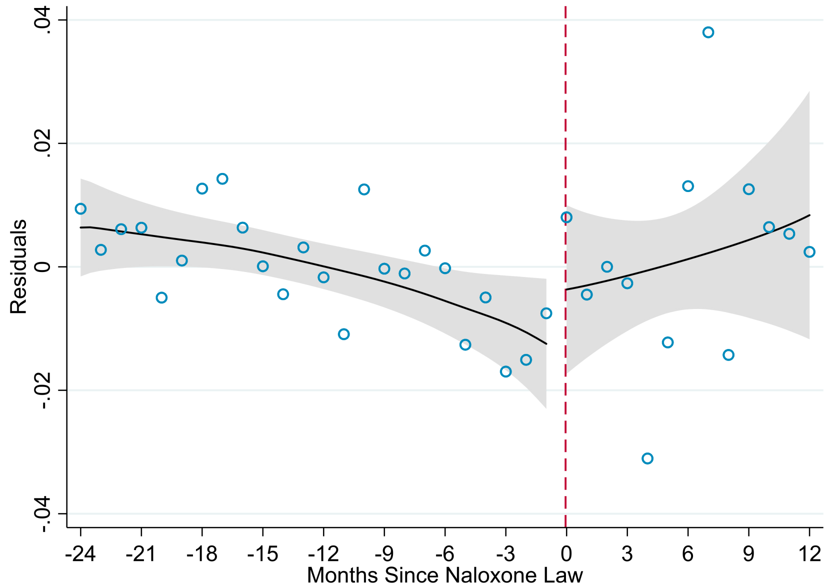
Notes: Figure shows residuals from a regression of opioid-related ER admissions in metro areas. The regression uses 2006-2015 quarterly data for all states that report this information to the Health Care Utilization Project (HCUP) and includes all controls reported in column (1) of Table 3 except for "Naloxone Law".

Figure 5: Effect of Naloxone access laws on opioid-related mortality



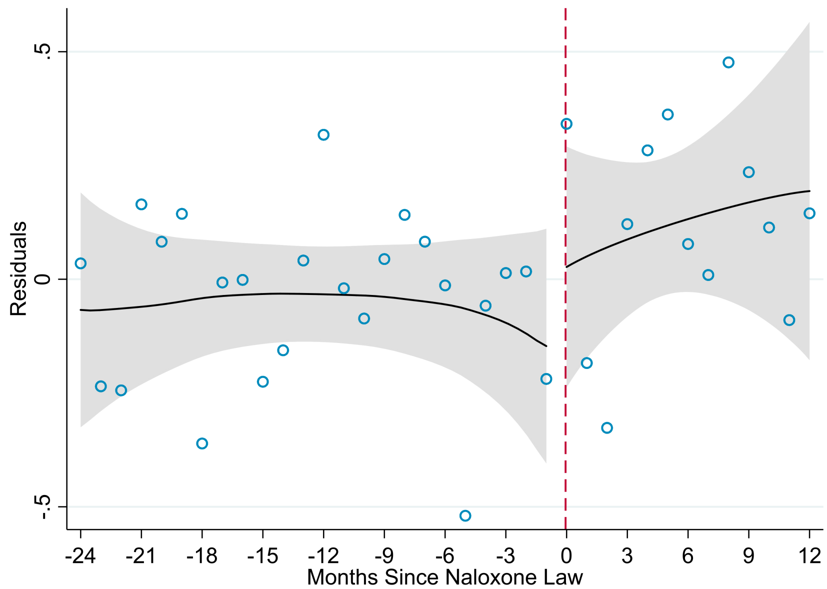
Notes: Figure shows residuals from a regression of the death rate from opioids (all categories). The regression uses 2010-2015 monthly CDC data from urban areas, and includes all controls reported in column (2) of Table 3 except for "Naloxone Law".

Figure 6: Effect of Naloxone access laws on fentanyl-related mortality



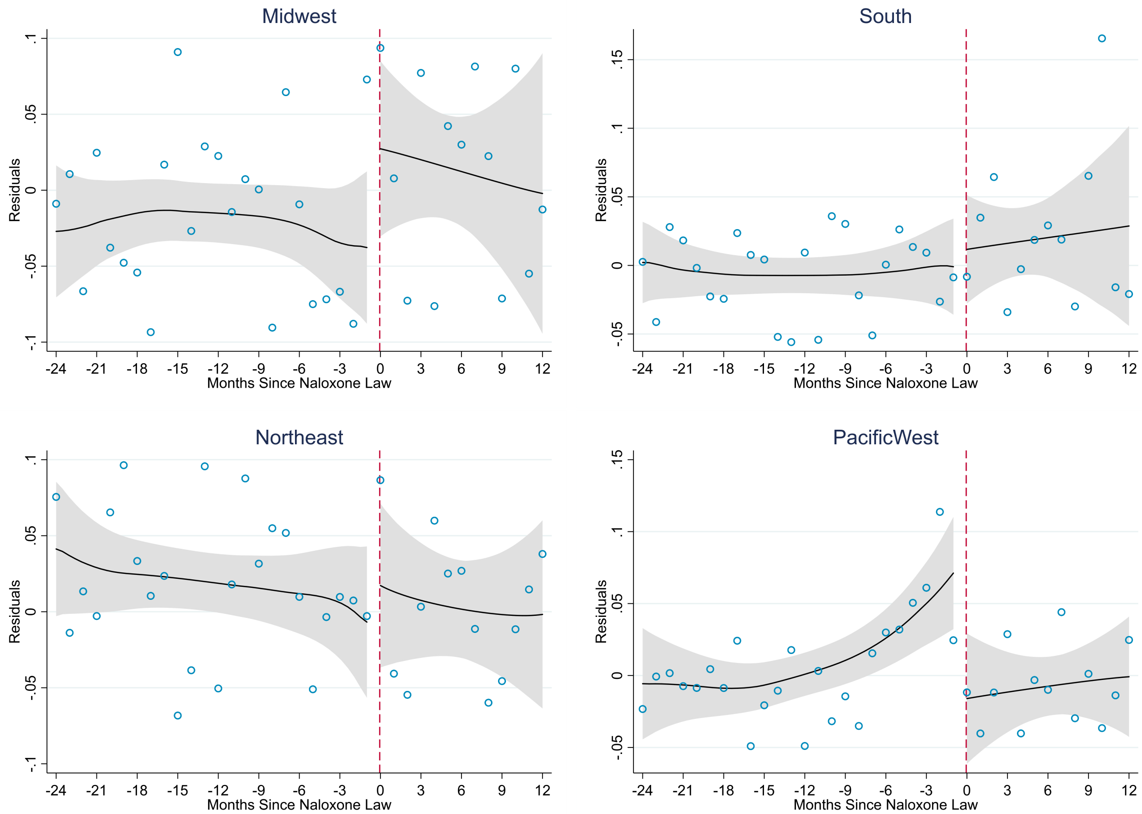
Notes: Figure shows residuals from a regression of the death rate from synthetic opioids, the category that contains fentanyl. The regression uses 2010-2015 monthly CDC data from urban areas, and includes all controls reported in column (3) of Table 3 except for "Naloxone Law".

Figure 7: Effect of Naloxone access laws on opioid-related theft



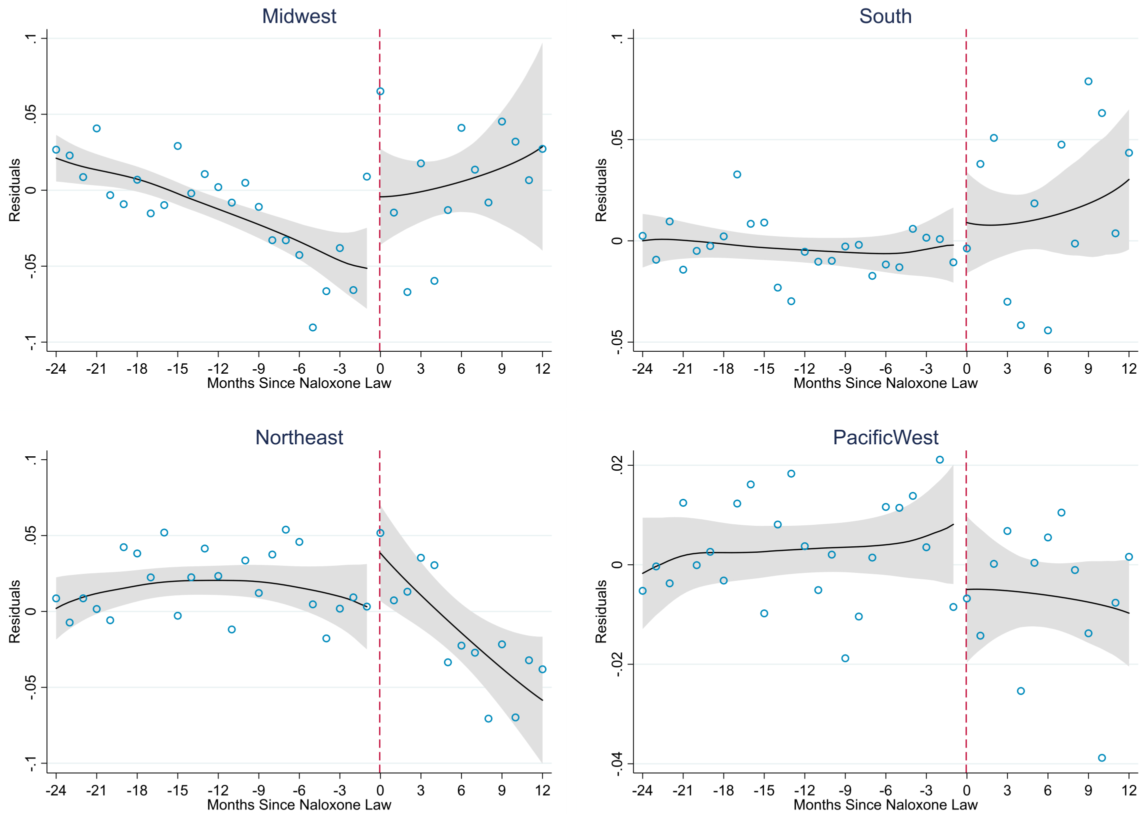
Notes: Figure shows residuals from a regression on opioid-related theft. The regression uses 2010-2015 monthly NIBRS data from urban areas, and includes all controls reported in column (2) of Table 4 except for "Naloxone Law".

Figure 8: Effect of Naloxone access laws on opioid-related mortality, by region



Notes: Figure shows residuals from a regression on opioid-related mortality, by Census region. The regression uses 2010-2015 monthly CDC data from urban areas, and includes all controls reported in Table 5 except for "Naloxone Law".

Figure 9: Effect of Naloxone access laws on fentanyl-related mortality, by region



Notes: Figure shows residuals from a regression on fentanyl-related mortality, by Census region. The regression uses 2010-2015 monthly CDC data from urban areas, and includes all controls reported in Table 5 except for "Naloxone Law".

Table 1: 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 from HCUP and recorded at the metro area-quarter level. ER visit rates are per 100,000 residents. 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 2: 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 are population-weighted and 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).

Table 3: 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 are population-weighted and show the effect of expanding Naloxone access on ER visits per 100,000 residents (column 1), and deaths per 100,000 residents (columns 2 and 3).

Table 4: 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.

Table 5: 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 are population-weighted and show the effect of expanding Naloxone access on arrests per million residents (columns 1 and 2), ER visits per 100,000 residents (column 3), deaths per 100,000 residents (columns 4 and 5), and reported crimes per million residents (columns 6 and 7).

Table 6: Effect of Naloxone laws in rural areas

	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	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
Midwest							
Naloxone Law	2.112*	0.972	107.0	0.027	0.006	3.940*	-0.034
	(0.971)	(1.157)	(65.23)	(0.040)	(0.029)	(1.880)	(0.250)
Observations	55,320	55,320	399	56,448	56,448	55,320	55,320
2010 baseline	17.93	6.736	282.5	0.439	0.098	30.13	1.402
South							
Naloxone Law	-2.379	-4.676**	-57.53*	-0.031	-0.007	-6.821*	0.092
	(2.082)	(1.459)	(25.85)	(0.061)	(0.020)	(3.481)	(0.587)
Observations	65,316	65,316	260	72,288	72,288	65,316	65,316
2010 baseline	43.54	20.25	460.7	0.736	0.128	77.31	3.684
Northeast							
Naloxone Law	2.631	-0.909	-6.738	-0.098	-0.108	4.749	0.699**
	(2.211)	(0.686)	(44.53)	(0.143)	(0.112)	(3.052)	(0.217)
Observations	29,724	29,724	171	7,080	7,080	29,724	29,724
2010 baseline	31.34	6.531	232.5	0.424	0.064	48.66	2.310
West							
Naloxone Law	6.121	2.352*	140.05*	-0.046	-0.017	8.945*	0.818
	(3.153)	(1.051)	(61.22)	(0.108)	(0.039)	(4.480)	(0.495)
Observations	19,332	19,332	184	19,800	19,800	19,332	19,332
2010 baseline	17.80	4.121	200.0	0.732	0.102	27.18	2.087

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 are population-weighted and show the effect of expanding Naloxone access on arrests per million residents (columns 1 and 2), ER visits per 100,000 residents (column 3), deaths per 100,000 residents (columns 4 and 5), and reported crimes per million residents (columns 6 and 7).

Table 7: Effect of Naloxone laws in all areas (no population cutoff)

	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	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
Midwest							
Naloxone Law	3.555**	1.094	374.6	0.074**	0.064	4.615*	0.020
	(1.346)	(0.679)	(312.1)	(0.033)	(0.038)	(2.052)	(0.048)
Observations	64,752	64,752	404	68,688	68,688	64,752	64,752
2010 baseline	20.25	5.836	1521	0.601	0.092	32.91	1.146
South							
Naloxone Law	1.020	-1.331*	231.1**	0.037	0.022	-0.355	0.067
	(3.330)	(0.712)	(93.12)	(0.039)	(0.020)	(4.451)	(0.470)
Observations	76,836	76,836	260	97,776	97,776	76,836	76,836
2010 baseline	31.11	12.09	2118	0.621	0.095	53.84	2.188
Northeast							
Naloxone Law	3.995*	1.835*	68.44	-0.055	-0.093	7.722**	0.766***
	(1.565)	(0.889)	(146.8)	(0.066)	(0.075)	(2.827)	(0.156)
Observations	33,612	33,612	260	15,216	15,216	33,612	33,612
2010 baseline	31.49	9.957	2284	0.498	0.071	52.36	2.170
West							
Naloxone Law	1.167	1.210*	334.1***	-0.060	-0.024***	2.972	1.256***
	(2.552)	(0.582)	(64.30)	(0.039)	(0.003)	(2.572)	(0.226)
Observations	24,300	24,300	184	29,448	29,448	24,300	24,300
2010 baseline	19.62	4.434	2763	0.628	0.071	31.98	1.916

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). 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 are population-weighted and show the effect of expanding Naloxone access on arrests per million residents (columns 1 and 2), ER visits per 100,000 residents (column 3), deaths per 100,000 residents (columns 4 and 5), and reported crimes per million residents (columns 6 and 7).

Table 8: Effect of Naloxone laws on opioid-related ER visits and mortality, by city size

	Opioid-related ER visits (1)	Opioid-related deaths (2)	Fentanyl-related deaths (3)
Largest cities only			
Naloxone Law	-11.22	-0.042	-0.088
	(61.80)	(0.053)	(0.071)
Observations	760	5,616	5,616
2010 baseline	910.1	0.527	0.059
Dropping largest cities			
Naloxone Law	244.9**	0.035	0.037**
	(105.6)	(0.025)	(0.018)
Observations	1,108	49,896	49,896
2010 baseline	1078	0.631	0.089

Notes: * $p < .10$, ** $p < .05$, *** $p < .01$. Standard errors are clustered by state and shown in parentheses. This table shows the results of breaking the sample from Table 3 into two subsamples. "Largest cities" sample: counties containing the largest 25 cities in the U.S. by population (for CDC data, 2010-2015), and large, central metropolitan areas (for HCUP data, 2006-2015). "Dropping largest cities" sample: the remaining urban counties (for the CDC data, 2010-2015) and metro areas (for HCUP data, 2006-2015). 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 are population-weighted and show the effect of expanding Naloxone access on ER visits per 100,000 residents (column 1), and deaths per 100,000 residents (columns 2 and 3).

Table 9: 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
	Opioid-related crime				**Opioid-related theft**			
Naloxone Law	4.766**	3.505	4.838	12.008*	-0.457	-0.068	0.620**	1.084**
	(1.580)	(2.144)	(2.895)	(5.699)	(0.591)	(0.319)	(0.253)	(0.441)
Observations	3,096	10,728	11,520	4,464	3,096	10,728	11,520	4,464
2010 baseline	24.94	35.23	43.82	48.93	1.113	1.086	1.652	1.430
	Possession of opioids				**Selling opioids**			
Naloxone Law	2.562	1.710	3.390	10.464**	-0.372	2.072**	1.629*	0.284
	(1.517)	(1.501)	(2.209)	(4.203)	(0.929)	(0.826)	(0.827)	(2.328)
Observations	3,096	10,728	11,520	4,464	3,096	10,728	11,520	4,464
2010 baseline	14.77	22.61	25.95	25.30	4.433	5.339	7.627	12.12

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 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), reported crime per million residents (panel 2), and arrests per million residents (panel 3).

A Appendix

Table A.1: Placebo test: Effect of Naloxone laws on outcomes that should not be affected

	Entire U.S.	Midwest	South	Northeast	West
	(1)	(2)	(3)	(4)	(5)
Deaths due to suicide					
Naloxone Law	-0.004	-0.022	-0.006	-0.023	0.005
	(0.013)	(0.033)	(0.019)	(0.025)	(0.027)
Observations	55,512	12,240	25,488	8,136	9,648
2010 baseline	1.001	0.977	1.026	0.813	1.102
Deaths due to heart disease					
Naloxone Law	-0.031	0.106	-0.082	0.041	0.095
	(0.164)	(0.206)	(0.147)	(0.385)	(0.115)
Observations	55,512	12,240	25,488	8,136	9,648
2010 baseline	27.97	27.66	26.63	34.69	25.81
Deaths due to motor vehicle accidents					
Naloxone Law	0.001	-0.061	0.003	-0.024	0.016
	(0.016)	(0.041)	(0.027)	(0.019)	(0.019)
Observations	55,512	12,240	25,488	8,136	9,648
2010 baseline	0.837	0.747	1.039	0.675	0.732

Notes: * $p < .10$, ** $p < .05$, *** $p < .01$. Standard errors are clustered by state and shown in parentheses. Data source: CDC. Sample includes counties with at least one jurisdiction with population $\geq$ 40,000. Date range: 2010-2015. Regression includes: 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 A.2: Effect of Naloxone laws on broader categories of deaths and crime

	Entire U.S. (1)	Midwest (2)	South (3)	Northeast (4)	West (5)
Deaths due to opioids or unspecified-drug poisoning					
Naloxone Law	0.003	0.076	0.041	-0.035	-0.021
	(0.029)	(0.049)	(0.048)	(0.057)	(0.037)
Observations	55,512	12,240	25,488	8,136	9,648
2010 baseline	0.942	1.014	0.933	0.812	0.984
All theft					
Naloxone Law	4.810	48.04*	-7.045	-10.81	56.74
	(12.84)	(25.81)	(16.39)	(22.25)	(42.45)
Observations	29,808	9,432	11,520	3,888	4,968
2010 baseline	1832	1831	1888	1469	1907

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 A.3: 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
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 A.4: 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
Controls:								
Jurisdiction FE	X	X	X	X	X	X	X	X
Month FE, Year 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 A.5: 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
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 A.6: 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
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 A.7: 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
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 opioid-related ER visits. ER visit rates are per 100,000 residents. Observations are at the metro area-quarter level. Data source: NIBRS. Sample includes metropolitan areas. Date range: 2006-2015.

Table A.8: 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
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 A.9: 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
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 A.10: Effect of Naloxone laws on opioid-related reported crime

	All opioid-related crime (reported)							
	(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
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 A.11: 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
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 A.12: Impact of Naloxone laws on opioid-related crimes

	All (1)	Heroin (2)	Other Narcotics (inc. Fentanyl) (3)	Morphine (4)	Opium (5)
Naloxone Law (NL)	6.053**	2.603***	3.795*	0.188*	-0.121
	(2.213)	(0.900)	(2.133)	(0.098)	(0.091)
Observations	29,808	29,808	29,808	29,808	29,808
2010 baseline	39.34	17.08	21.35	1.035	0.757

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.

Table A.13: 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 A.14: Effect of Naloxone laws, controlling for states-specific cubic trends

	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	2.787**	1.433**	64.76	-0.000	0.008	4.572**	0.035
	(1.311)	(0.554)	(106.04)	(0.021)	(0.018)	(2.046)	(0.185)
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 $\geq$ 40,000 (for NIBRS data on arrests and crime), counties with any such jurisdictions (for CDC data on mortality), and metro areas (for HCUP data on ER visits). 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 cubic 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 arrests per million residents (columns 1 and 2), ER visits per 100,000 residents (column 3), deaths per 100,000 residents (columns 4 and 5), and reported crimes per million residents (columns 6 and 7).

Table A.15: Effect of Naloxone laws in the Midwest, dropping one state at a time

	Obs.	"Naloxone" searches	Obs.	Opioid-related ER visits	Obs.	Opioid-related mortality	Obs.	Opioid-related theft
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Drop IA	5,184	1.233	364	363.3*	11,592	0.081*	8,568	-0.185
		(1.885)		(195.2)		(0.041)		(0.230)
Drop IL	4,896	1.846	376	456.7*	11,016	0.089	9,360	0.046
		(2.120)		(247.6)		(0.059)		(0.304)
Drop IN	4,968	2.940	368	373.1	10,584	0.118***		
		(1.845)		(216.3)		(0.032)		
Drop KS	5,328	1.797	364	358.4	11,664	0.085*	8,928	-0.096
		(1.956)		(200.1)		(0.041)		(0.288)
Drop MI	5,040	0.670			10,152	0.061	5,040	0.262
		(1.616)				(0.035)		(0.303)
Drop MN	5,256	2.196	364	428.2*	11,376	0.109**		
		(2.065)		(221.4)		(0.045)		
Drop MO	5,112	0.897	364	401.6*	11,448	0.087*	9,288	0.034
		(1.841)		(203.3)		(0.041)		(0.316)
Drop ND	5,472	1.923	384	391.1*	11,952	0.095**	9,144	0.026
		(1.861)		(211.9)		(0.041)		(0.303)
Drop NE	5,184	2.080	364	488.5**	11,952	0.096**	9,432	0.034
		(1.972)		(200.7)		(0.042)		(0.278)
Drop OH	4,752	3.184*	364	24.59	9,720	0.127**	7,056	0.323
		(1.734)		(78.71)		(0.056)		(0.418)
Drop SD	5,472	1.804	364	359.5*	12,096	0.097**	9,288	0.067
		(1.908)		(188.9)		(0.041)		(0.286)
Drop WI	5,112	1.894	364	413.5*	11,088	0.108**	8,784	0.078
		(1.959)		(209.3)		(0.048)		(0.329)

Notes: * $p < .10$, ** $p < .05$, *** $p < .01$. Standard errors are clustered by state and shown in parentheses. Data sources: Google trends, HCUP, CDC, NIBRS. Sample: urban areas. Each coefficient is from a separate regression. All regressions include: jurisdiction FEs, month of sample FEs, state-specific linear trends, and the following laws/regulations: Good Samaritan laws, PDMP, Doctor Shopping, Pain Clinic regulations, Physician exams, Pharmacy verification, require ID, and tamper-resistant PF. All regressions except for those on opioid-related ER visits also include police per capita. Coefficients are population-weighted and show the effect of expanding Naloxone access on Google searches for "Naloxone" (column 2), ER visits per 100,000 residents (column 4), deaths per 100,000 residents (column 6), and reported crimes per million residents (column 8).

Table A.16: Effect of Naloxone laws in the South, dropping one state at a time

	Obs.	"Naloxone" searches	Obs.	Opioid-related ER visits	Obs.	Opioid-related mortality	Obs.	Opioid-related theft
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Drop AL	8,280	1.596			24,192	0.059		
		(1.212)				(0.037)		
Drop AR	8,208	1.481	252	226.7**	24,840	0.055	10,584	0.127
		(1.277)		(86.77)		(0.037)		(0.336)
Drop DE	8,568	1.515			25,272	0.052	11,232	0.097
		(1.238)				(0.038)		(0.335)
Drop FL	7,992	0.974	220	233.1*	22,536	0.016		
		(1.296)		(110.8)		(0.031)		
Drop GA	7,992	2.299*	224	211.6**	23,040	0.034		
		(1.093)		(72.32)		(0.039)		
Drop KY	7,992	1.129	228	182.3*	24,552	0.054	10,728	0.125
		(1.131)		(85.92)		(0.037)		(0.323)
Drop LA	8,208	0.548			24,048	0.039	11,160	0.125
		(1.042)				(0.033)		(0.317)
Drop MS	8,280	1.417			24,768	0.051		
		(1.182)				(0.038)		
Drop MD	8,496	1.598	224	237.1*	24,264	0.040		
		(1.204)		(96.98)		(0.036)		
Drop NC	8,136	1.247	228	148.1	22,104	0.087**		
		(1.600)		(100.4)		(0.039)		
Drop OK	8,280	1.302			24,984	0.048		
		(1.283)				(0.037)		
Drop SC	8,208	1.487	224	181.5*	23,760	0.059	9,504	0.174
		(1.222)		(77.10)		(0.037)		(0.360)
Drop TN	8,280	1.723	220	310.6**	24,192	0.061	9,432	0.013
		(1.257)		(94.68)		(0.042)		(0.344)
Drop TX	7,416	1.437			22,176	0.049	9,936	0.457**
		(1.327)				(0.043)		(0.183)
Drop VA	8,136	1.545			22,752	0.059	8,712	-0.213
		(1.209)				(0.037)		(0.246)
Drop WV	8,208	1.479			24,840	0.052	10,872	0.142
		(1.242)				(0.039)		(0.311)

Notes: * $p < .10$, ** $p < .05$, *** $p < .01$. Standard errors are clustered by state and shown in parentheses. Data sources: Google trends, HCUP, CDC, NIBRS. Sample: urban areas. Each coefficient is from a separate regression. All regressions include: jurisdiction FEs, month of sample FEs, state-specific linear trends, and the following laws/regulations: Good Samaritan laws, PDMP, Doctor Shopping, Pain Clinic regulations, Physician exams, Pharmacy verification, require ID, and tamper-resistant PF. All regressions except for those on opioid-related ER visits also include police per capita. Coefficients are population-weighted and show the effect of expanding Naloxone access on Google searches for "Naloxone" (column 2), ER visits per 100,000 residents (column 4), deaths per 100,000 residents (column 6), and reported crimes per million residents (column 8).

Table A.17: Effect of Naloxone laws in the Northeast, dropping one state at a time

	Obs.	"Naloxone" searches	Obs.	Opioid-related ER visits	Obs.	Opioid-related mortality	Obs.	Opioid-related theft
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Drop CT	2,160	3.595	224	-33.29	7,776	-0.085	2,952	3.256
		(1.910)		(119.2)		(0.046)		(1.539)
Drop MA	2,016	4.970*	224	-154.8	7,416	-0.007	1,656	1.803
		(2.068)		(176.4)		(0.072)		(1.168)
Drop ME	2,088	2.728	224	-50.19	7,848	-0.043	3,816	0.873
		(1.848)		(126.8)		(0.064)		(0.644)
Drop NH	2,016	2.283			7,992	-0.031	3,672	0.509
		(1.774)				(0.062)		(0.383)
Drop NJ	2,088	3.804	220	54.73	6,912	-0.086		
		(2.089)		(88.73)		(0.063)		
Drop NY	1,512	8.103**	224	-5.156	5,256	0.146**		
		(3.209)		(244.0)		(0.045)		
Drop PA	1,584	3.644			5,832	-0.058		
		(2.014)				(0.059)		
Drop RI			224	-48.95	7,992	-0.062	3,456	0.520
				(124.4)		(0.063)		(0.485)
Drop VT	2,160	3.001	220	-132.3	8,064	-0.047		
		(1.979)		(159.6)		(0.065)		

Notes: * $p < .10$, ** $p < .05$, *** $p < .01$. Standard errors are clustered by state and shown in parentheses. Data sources: Google trends, HCUP, CDC, NIBRS. Sample: urban areas. Each coefficient is from a separate regression. All regressions include: jurisdiction FEs, month of sample FEs, state-specific linear trends, and the following laws/regulations: Good Samaritan laws, PDMP, Doctor Shopping, Pain Clinic regulations, Physician exams, Pharmacy verification, require ID, and tamper-resistant PF. All regressions except for those on opioid-related ER visits also include police per capita. Coefficients are population-weighted and show the effect of expanding Naloxone access on Google searches for "Naloxone" (column 2), ER visits per 100,000 residents (column 4), deaths per 100,000 residents (column 6), and reported crimes per million residents (column 8).

Table A.18: Effect of Naloxone laws in the West, dropping one state at a time

	Obs.	"Naloxone" searches	Obs.	Opioid-related ER visits	Obs.	Opioid-related mortality	Obs.	Opioid-related theft
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Drop AK	3,600	1.694			9,576	-0.059		
		(1.854)				(0.040)		
Drop AZ	3,456	2.149	144	-26.32	8,928	-0.035	4,824	1.525**
		(2.010)		(88.27)		(0.035)		(0.384)
Drop CA	2,664	0.876	144	137.1	6,552	-0.157**		
		(2.246)		(73.65)		(0.056)		
Drop CO	3,456	2.611			8,784	-0.039	3,312	1.525*
		(2.006)				(0.040)		(0.613)
Drop HI	3,600	2.339	144	34.71	9,360	-0.063		
		(1.868)		(62.14)		(0.040)		
Drop ID	3,456	3.253*			9,216	-0.059	4,104	1.557***
		(1.548)				(0.040)		(0.298)
Drop MT	3,384	1.113	176	5.449	9,288	-0.064	4,536	1.374**
		(1.495)		(64.95)		(0.039)		(0.420)
Drop NM	3,456	1.865			9,072	-0.069		
		(1.947)				(0.040)		
Drop NV	3,456	1.701	164	-4.296	9,216	-0.073*		
		(1.961)		(54.80)		(0.039)		
Drop OR	3,240	2.209			8,784	-0.043	4,536	1.130**
		(2.170)				(0.042)		(0.432)
Drop UT			148	-35.86	9,216	-0.053	3,888	1.171*
				(36.18)		(0.039)		(0.534)
Drop WA	3,384	1.118			8,280	-0.070	4,608	1.352***
		(1.845)				(0.044)		(0.333)
Drop WY	3,240	2.335			9,504	-0.060		
		(2.097)				(0.040)		

Notes: * $p < .10$, ** $p < .05$, *** $p < .01$. Standard errors are clustered by state and shown in parentheses. Data sources: Google trends, HCUP, CDC, NIBRS. Sample: urban areas. Each coefficient is from a separate regression. All regressions include: jurisdiction FEs, month of sample FEs, state-specific linear trends, and the following laws/regulations: Good Samaritan laws, PDMP, Doctor Shopping, Pain Clinic regulations, Physician exams, Pharmacy verification, require ID, and tamper-resistant PF. All regressions except for those on opioid-related ER visits also include police per capita. Coefficients are population-weighted and show the effect of expanding Naloxone access on Google searches for "Naloxone" (column 2), ER visits per 100,000 residents (column 4), deaths per 100,000 residents (column 6), and reported crimes per million residents (column 8).