

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

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The U.S. is experiencing an epidemic of opioid abuse. In response, states have implemented a variety of policies including increased access to naloxone, a drug that can save lives when administered during an overdose. However, widespread naloxone access may lead to increased opioid abuse by reducing the risk of death per use, thereby enabling continued or riskier opioid use. By increasing the demand for opioids, naloxone access may also impact crime, especially theft. In this paper, we use the staggered timing of state-level naloxone access laws as a natural experiment to measure the effects of broadening access to this lifesaving drug. We find that broadened access led to more opioid-related emergency room visits and more opioid-related theft, with no reduction in opioid-related mortality. These effects are driven by urban areas and vary with local access to substance abuse treatment. We find the most detrimental effects in the Midwest, including a 14% increase in opioid-related mortality in that region. While naloxone has great potential as a harm-reduction strategy, our analysis supports the concern that it 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, a sentiment recently echoed by the U.S. Surgeon General ([Shesgreen, 2016](#); [Scutti and Jimison, 2018](#)). 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. First, the reduced risk of death makes opioid abuse more appealing, leading some to increase their opioid use—or use more potent forms of the drug—when they have naloxone as a safety net. Some of those abusers may become criminally active to fund their increased drug use. Second, some opioid abusers are saved by naloxone and may continue their previous drug use and criminal behavior (this intended effect of naloxone will mechanically increase rates of both behaviors).

Furthermore, expanding naloxone access might not in fact reduce mortality. Though the risk of death per opioid use falls, an increase in the number or potency of uses means the expected effect on mortality is ambiguous.

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, though it is unclear if or how often

such parties actually occur.<sup>1</sup> News reports also highlight cases 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.<sup>2</sup>

Our analysis of panel data from across the U.S. shows that there are indeed reasons to be concerned about unintended consequences from broadened naloxone access. We use the gradual adoption of state-level naloxone access laws as a natural experiment to measure the effects of broadened access, and find that the moral hazard generated by naloxone is a meaningful 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.<sup>3</sup>

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

Local data on actual naloxone distribution are unavailable, so we use data on Google searches as a proxy for local awareness of and interest in naloxone (a relevant first stage outcome). We find that naloxone access laws increased internet searches for “naloxone” by

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<sup>1</sup>Examples of concerned legislators: “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](#)).

<sup>2</sup>“‘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 \(2016\)](#).

<sup>3</sup>We will also show results for rural areas and for all areas combined. Results for all areas combined are similar to those for urban areas.

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<sup>4</sup>) 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% (though the effect on overall theft rates is much smaller). Meanwhile, expanding access to naloxone had no effect on opioid-related mortality, on average.<sup>5</sup>

There are at least two reasons to expect that this average effect masks regional variation in the effects of broad naloxone access. The first is geographic variation in the types of opioids available. Ciccarone (2009) and Quinones (2015) discuss the prevalence of black tar heroin in the western U.S., in contrast to the powder heroin that has traditionally been more prevalent in the eastern U.S. Powder heroin is more easily mixed with—or contaminated by—highly-potent opioids such as fentanyl, which make a standard dose of naloxone less effective at reversing an overdose. If users become less careful about the source or composition of their powder heroin when naloxone is available, mortality rates could climb. Given this, we would expect that broadening naloxone access would have more beneficial effects in the West. The second reason to expect differential policy effects by region is geographic variation in access to health care such as drug treatment (Commonwealth Fund, 2018); this could contribute to differences in outcomes for those who suffer from opioid addiction. In the best case, naloxone gives someone who overdoses a second chance to seek treatment for their addiction. If treatment is unavailable or unaffordable, the benefits of broadening naloxone access should be lower.

Consistent with these hypothesized mechanisms, we find substantial regional differences in the effect of broadening naloxone access. 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

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<sup>4</sup>We will show that this measure is highly correlated with actual drug treatment admissions.

<sup>5</sup>As expected, effects in rural areas were typically statistically insignificant.

in other regions were statistically insignificant but non-zero: mortality increased in the South but fell in the West, where black tar heroin is more prevalent.<sup>6</sup>

Knowing about the moral hazard consequences of broad naloxone access is helpful because it gives us a chance to find ways to mitigate those effects. Since jurisdictions have direct control over the local availability of drug treatment, we investigate the possibility that this policy lever could be a helpful complement to broad naloxone access. We show that naloxone access laws increased mortality more in places with fewer drug treatment facilities per capita, or more limited eligibility for Medicaid (which covers substance abuse treatment). In other words, 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. Most importantly, we show that pre-existing trends in fentanyl use do not explain our findings, though (as discussed in Section 5.3) the underlying trends in fentanyl supply may interact with the policy changes we study to exacerbate their effects. Our estimates are robust to controlling for an array of other state policies aimed at reducing opioid abuse and mortality, including Medicaid expansions. “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, dropping individual

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<sup>6</sup>We also find a statistically insignificant decrease in mortality in the Northeast, but this effect is driven entirely by New York. As we’ll show in Section 5.5.9, mortality significantly *increases* in the Northeast when we exclude New York state.

states one-by-one, and using alternate dates for naloxone access laws (in the few cases where there was ambiguity about when access was broadly expanded).

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

New work by Packham (2019) is most directly related to the context we study here. That study considers the effects of opening syringe exchange programs (SEPs) on local HIV prevalence and opioid use. It finds that, though SEPs have their intended effect of reducing HIV, they increase opioid abuse as measured by ER visits due to overdose (by 18%), drug-related arrests (by 16%), and opioid-related mortality (by 13-15%). SEPs also increase local rates of theft by 24%. Packham argues that this is evidence of moral hazard: by reducing the risk associated with drug use, SEPs incentivize increased opioid use. Like the current paper, Packham (2019) finds that access to substance abuse treatment appears to explain heterogeneity in the effects of SEPs across counties and regions. These results provide an important, complementary example of the tradeoffs involved in harm reduction efforts.

It may seem surprising that drug users respond to incentives in a sophisticated way. One may think that drug users are poor decision-makers or that addiction makes rational choices impossible. Addiction surely clouds judgement and makes policy in this area difficult, but there is substantial evidence that drug users respond to incentives. A theoretical literature hypothesizes that consumption of addictive substances such as drugs and alcohol is rational and sensitive to prices (Becker and Murphy 1988; Grossman and Chaloupka 1998), and a large body of empirical evidence documents such a causal relationship. For example, increasing taxes on alcohol reduces alcohol consumption (Cook and Durrance, 2013). Alcohol abuse also responds favorably to increasing the likelihood of punishment, as seen in evaluations of the 24/7 Sobriety program (Kilmer et al., 2013). Hansen, Miller and Weber (2017) show that marijuana consumption is price inelastic in the short run, but quickly becomes price elastic, with consumers reducing their consumption in the face of higher marijuana taxes. And finally, Moore and Schnepel (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. If drug use is sensitive to prices, then it is reasonable to hypothesize that it is also sensitive to non-monetary costs such as the risk of death.

There is an overlapping literature on the effects of drug and alcohol consumption on crime. We are interested in the effects on crime in part because the welfare implications of drug and alcohol abuse themselves are unclear: some argue that people can do whatever they want to their own bodies, no matter how harmful. Externalities in the form of crime are more clearly negative and so could justify government intervention. For instance, there is substantial evidence that policies that increase alcohol consumption also increase violent crime (Cook and Durrance 2013; Anderson, Crost and Rees 2017). Substance abuse may affect crime (1) by leading users to steal or engage in illegal behavior to generate income to purchase drugs, (2) through a direct physiological effect that makes users more aggressive, or (3) by creating an illicit market where violence is required to defend turf, enforce contracts, and

so on (MacCoun, Kilmer and Reuter, 2003). We are interested in whether naloxone access laws increase crime rates through their effect on opioid abuse. Because violent behavior is not typically associated with opioid use or opioid dealing (Quinones, 2015), we expect the main effect of these laws to be on theft.

There are only two other studies of naloxone’s causal effect on opioid abuse, both written contemporaneously.<sup>7</sup> Rees et al. (2017) use annual, state-level CDC mortality data from 1999 through 2014 to measure the effects of naloxone access 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 show that our results are robust to the specification used in Rees et al. (2017); in fact, using their specification with data through 2014 would have led us to conclude that broadening naloxone access increased opioid-related mortality at the national level (see Section 5.5.11 for a more detailed discussion). Deiana and Giua (2018) also use aggregated mortality data through 2014, and do not find statistically significant effects of naloxone access on mortality or several broad crime categories. Using richer, higher-frequency data on both mortality and crime, as well as an additional year of data, we provide more precise measures of these national effects. We also investigate whether those national averages mask heterogeneity by region, availability of drug treatment, and other factors that are relevant to understanding the effects of naloxone access on opioid abuse.

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.

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<sup>7</sup>Another paper considers the association between naloxone laws and opioid-related mortality but it is unclear from the analysis whether the correlations found represent causal effects (McClellan et al., 2018).

## 2 Background

Opioid addiction now claims nearly 115 lives each day ([Centers for Disease Control and Prevention, 2018](#)). Individuals are prescribed these drugs to treat pain, but many patients develop addictions that lead them to illegal use of prescription opioids and cheaper substitutes such as heroin. (In addition, many people begin abusing prescription opioids and heroin without a prescription, particularly now that these drugs are more easily accessible; [Quinones, 2015](#).) Such drug abuse can have fatal consequences, and policymakers across the country are searching for policies that can reduce the death toll.

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

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; generally, standing orders do not even include an age requirement beyond a provision for pharmacist discretion on this factor. Only slightly less broad than standing order laws are third party prescription laws, which allow residents to buy naloxone with the "reasonable intention" of administering the drug to someone else (by contrast, naloxone purchases under standing

orders require no justification).<sup>8</sup>

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

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

There is widespread anecdotal evidence that these laws led to meaningful increases in naloxone access – particularly in the form of distribution by community organizations and by enabling police officers to carry naloxone. However, state and local data on actual naloxone distribution during this period are typically unavailable.<sup>9</sup> Two exceptions provide numerical evidence that these laws resulted in an increase in naloxone distribution. The first is North Carolina, which broadened naloxone access in April 2013. Over the three years afterward,

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<sup>8</sup>Because naloxone remains a prescription drug as categorized by the U.S. Food and Drug Administration, standing orders and third-party prescriptions are enabled by the physician-general of a state writing a prescription for all residents.

<sup>9</sup>ARCOS (Automation of Reports and Consolidated Orders System) data are commonly used to study drug distribution, but focus on controlled substances, so naloxone is most frequently reported only when it is found in conjunction with another drug. Pharmaceutical distribution data from the IMS Health (now IQVIA) are targeted to industry purchasers, and do not include information on ground-level distribution by harm reduction campaigns that are likely most important in this context.

the state's Harm Reduction Coalition distributed naloxone kits to over 27,000 high-risk individuals (Reed, 2016). Another example is Maryland, which broadened access to naloxone in October 2015. As shown in Figure 1, the number of naloxone kits distributed by the Health Commissioner's Staying Alive program in Baltimore increased from a steady average of 508 kits per year between 2010 and 2014, to 2,515 kits in 2015 and 9,339 kits in 2016 (Baltimore City Health Department, 2018). These numbers imply that distribution jumped 1,731% after the law went into effect, from 42 kits per month to 769 kits per month.<sup>10</sup>

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

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

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<sup>10</sup>Since similar data are not available across the country, we will use Google trends data on internet searches for "naloxone" as a proxy for interest in and awareness of the drug, and show that naloxone access laws resulted in a significant increase in such searches across the country.

secondary market, results in a net social cost of \$15 billion per year due to health losses.

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

### 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 through either third party prescriptions or standing orders. Figure 2 shows how the number of states with naloxone access laws evolved over time, and Figure 3 shows maps of the states with broad naloxone access laws in each year. As these figures show, naloxone access laws were adopted by a geographically and politically diverse set of states. All states eventually pass such laws, though our data only go through the end of 2015. Table 1 lists the precise dates we use in our analysis.<sup>11</sup>

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.

Data on actual awareness and distribution of naloxone are unavailable, so to approximate

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<sup>11</sup>In five states, the specific date of broadened naloxone access is somewhat ambiguous due to the passage of related legislation, and one could argue that we should be using an earlier date than we do in our analysis. We will show that using these alternate dates does not affect our results.

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

Similarly, data on interest in drug treatment is also unavailable, so we again use Google Trends data as a proxy. We use data on searches related to the “drug rehabilitation” topic (which includes searches for “drug treatment”), to measure interest in treatment for addiction. Table A.1 shows that this search index is highly correlated with actual admissions to drug treatment programs for opioid addiction, as recorded in the Treatment Episode Data Set (TEDS): A one-unit increase in the search index is associated with 306 additional opioid-related treatment admissions, a 3.5% increase ( $p < 0.05$ ). One problem with using TEDS data directly is that the data are only available at the state-year level; Google Trends data provide a more local and higher-frequency measure of drug treatment interest that coincides with the broadened naloxone laws.<sup>12</sup> As before, we use monthly data for 2010-2015 at the metropolitan area level. Scores 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)

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<sup>12</sup>Another problem with using TEDS data is that drug treatment facilities often operate at capacity; if there is no room for new patients, then interest in treatment may not result in admission for treatment. Google Trends data provide an unconstrained measure of interest.

from 2010 through 2015. NIBRS is an incident-level dataset that collects information on reported crimes from local, state, and federal law enforcement agencies. The NIBRS dataset includes rich incident-level information on reported offenses and arrests. Important to our study, drug or narcotic offenses included specific codes for a variety of opioids and other substances involved with the crime. One drawback of NIBRS is that not all jurisdictions<sup>13</sup> participate. We create a balanced panel of jurisdictions that report offenses in all months of 2010–2015.<sup>14</sup> During that time period, 2,831 jurisdictions in 33 states submitted information to NIBRS, representing roughly 24% of the country’s population. In our analysis, we aggregate incidents to the jurisdiction-month level.

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

We use these data to construct the following outcome variables: possession of opioids<sup>15</sup> (a proxy for quantity demanded), selling of opioids<sup>16</sup> (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.<sup>17</sup> 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 is more accurately interpreted as measuring arrests. All variables are converted into rates per

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<sup>13</sup>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.

<sup>14</sup>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.

<sup>15</sup>This category includes the following official codes: Buying/receiving, possession/concealing, and using/consuming.

<sup>16</sup>This category includes the following official codes: Distributing/selling, and transporting/transmitting/importing.

<sup>17</sup>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.

1,000,000 local residents.

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

To measure abuse and overdose involving opioids, we use data on opioid-related emergency room (ER) visits from the Healthcare Cost and Utilization Project (HCUP) for years 2006-2015. Without reductions in mortality, increased emergency room visits are indicative of moral hazard because they would stem from increased or riskier opioid use.

The emergency room data provide a quarterly measure of the number of ER visits by reason for the visit, by state and by metropolitan-area-type within the state.<sup>18</sup> (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

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<sup>18</sup>In contrast to the other datasets, we don’t have county or city identifiers in the HCUP data.

for the person who had overdosed. There is some evidence that this happens: a survey of naloxone training participants in Baltimore found that fewer would call 911 for help after naloxone training (Mueller et al., 2015). On the other hand, it could be biased upwards if more bystanders call 911 for help knowing that naloxone is available – in this case, we might expect to see an increase in ER visits for the same number of overdoses. However, if this is driving the effect then we would expect a corresponding decrease in mortality.

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.<sup>19</sup> 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

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<sup>19</sup>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.

jurisdictions combined.

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

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

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

Summary statistics are in Table 2. Columns 1 and 2 show means and standard deviations

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

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

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

Given these concerns, we pay close attention to the parallel trends assumption. We

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

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

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

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  $\alpha_j$  is a fixed effect for each jurisdiction (accounting for average differences across places), and  $m_t$  is a month-of-sample (or quarter-of-sample) fixed effect (controlling flexibly for national trends in opioid abuse). The  $S_j \times t$  terms are state-specific linear time trends that absorb pre-existing state trends in the outcome measure. (Our results are robust to including jurisdiction-specific linear time trends instead.)  $Police_{jt}$  is the log of police officers per capita in the jurisdiction, and it varies over time; we include this as a proxy for law enforcement policies and public safety investments that might independently affect opioid abuse and crime rates.  $OtherLaws_{jt}$  is a time-varying vector of other state-specific laws that the literature has identified as relevant to opioid use and abuse.<sup>20</sup> The term  $\epsilon$  is an error term that is clustered at the state level for estimation. All estimates that use rates as the dependent variable are population-weighted.

Our identifying assumption is that we are controlling for all relevant trends and policies

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<sup>20</sup>This list of laws is taken from Meara et al. (2016); as described above, we use their database of policy timing and extend it through 2015.

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

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

## 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.<sup>21</sup>

Results are show in Figure 4, and in column 1 of Table 3. The figure is a coefficient plot, showing estimates in each quarter before and after the law change. The coefficient for the quarter just before the law change is indexed to 0. The left-most coefficient shows the estimate for 5+ quarters before the law change. The right-most coefficient shows the estimate for 3+ quarters after the law change. Pre-law effects on “naloxone” searches are flat

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<sup>21</sup>Google aggregates a number of related search terms into the “naloxone (drug)” category. We use this aggregation as our outcome measure, as described above.

and near-zero, indicating that our control variables sufficiently absorb pre-period trends. At the date of the law’s implementation, the coefficient increases; it remains above the earlier coefficients in quarters 1 through 3+. On average, the coefficients after the law change are higher than the coefficients before the law change. The regression results tell a similar story: naloxone access laws cause the local intensity of Google searches for “naloxone” to increase by 7.2% ( $p < 0.05$ ). This indicates that the laws had a meaningful impact on residents’ knowledge of and interest in naloxone.

Next we consider whether naloxone access laws affected interest in drug treatment or rehabilitation programs. If moral hazard is operating in this context, we would expect that reducing the risks associated with using opioids would reduce opioid users’ interest in getting treatment. We again use Google Trends data as an indicator of local residents’ interest. The effect of naloxone access laws on searches for “drug rehab” (and related queries) is shown in column 2 of Table 3. We find that the intensity of searches for “drug rehab” falls by 1.4% ( $p < 0.10$ ). This effect is small and marginally significant, so provides suggestive evidence that naloxone access reduces local interest in treatment for opioid addiction.

Column 3 of Table 3 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 3 shows the effect of naloxone access laws on arrests for the illegal sale of opioids. We consider this an indicator of quantity supplied, which should move with quantity demanded. Indeed, we find that monthly arrests for the sale of opioids increase by 1.9 per million residents each month (27%,  $p < 0.01$ ) after naloxone access laws are implemented. Given increases in both quantity demanded and quantity supplied, it appears that naloxone access laws increased the level of activity in the illegal opioid market; this

suggests an increase in consumption of illegal opioids. (At the very least, more people are being arrested for their use and sale of opioids, which is costly to them and to society.)

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.) Naloxone availability may lead some users to actively seek out fentanyl. It may lead others to simply be less careful about the source and content of their heroin. Being less careful when naloxone provides a safety net can result in increased consumption and sale of fentanyl – if only by accident. Indeed, the abuse of fentanyl increased tremendously during this period ([Lewis et al., 2017](#)).

As a preliminary test for these effects, Columns 5 and 6 of Table 3 consider effects on arrests that involve “other opioids” (a category likely dominated by fentanyl). Distinguishing between heroin and fentanyl is difficult at the time of arrest (drugs would need to be sent to a lab for testing), so we expect these data to be noisy and interpret the results as suggestive. We find that 64% of the increase in arrests for opioid possession involves fentanyl, and that this increase represents a 21% increase in fentanyl possession over its baseline ( $p < 0.05$ ). About 41% of the increase in arrests for selling opioids comes from selling fentanyl, representing a 29% increase in fentanyl sales over the baseline (not statistically significant). These estimates are about the same as for all opioids, so do not provide evidence that naloxone access is having a disproportionate impact on fentanyl distribution – but again, these recorded drug types in the crime data may not be accurate. Since fentanyl-laced heroin may be mistaken for heroin at 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.<sup>22</sup> To further investigate changes in

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<sup>22</sup>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

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 the bottom-left of Figure 5 and in column 1 of Table 4. The figure shows that pre-law effects are flat and near-zero; after the law change, the effects increase. Consistent with this visual evidence of a change, our regression results show that broadening naloxone access led to more opioid-related ER visits: naloxone access laws increased the quarterly number of visits by 266 (15%,  $p < 0.05$ ). This effect is large and 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?

The top-left of Figure 5 and Column 2 of Table 4 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.

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. The top-right of Figure 5 and Column 3 of Table 4 show no effect of broadening naloxone access 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 may increase the number of new opioid abusers. Both effects could increase criminal activity, particularly theft committed to fund an addiction. Table 5 considers the effect of naloxone 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.

access on crime rates. Columns 1 and 2 show that broadening naloxone access increases all opioid-related crime by 6.0 per million (15%,  $p < 0.05$ ), and opioid-related theft by 0.4 per million (30%,  $p < 0.10$ ). The bottom-right of Figure 5 shows a coefficient plot for the effect on opioid-related theft; while the pre-period trend is flat, there is a clear increase after naloxone access laws went into effect.

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

## 5.1 Differences by region

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

Figures 6 through 9 show effects on ER visits, mortality, and opioid-related theft sepa-

rately by Census region, while Table 6 presents all of our main results separately for each region.

The most striking difference from the average effects discussed above is that – as expected – those averages masked substantial heterogeneity in mortality effects. In the Midwest, we find that broadening naloxone access increased opioid-related mortality by 14% ( $p < 0.05$ ) and fentanyl-related mortality by 84% ( $p < 0.10$ ). Pre-trends are flat, evidence that the parallel trends assumption holds and that laws are not passed in response to differential increases in opioid or fentanyl use. Effects on mortality are also positive in the South, but negative in the Northeast and West (all not significant, except that the negative effect on fentanyl-related mortality is statistically significant in the West). Since the opioid crisis has been most consequential in the Midwest and South, these results suggest that naloxone access may have exacerbated the crisis in the places that were hardest-hit (and perhaps where public health resources could not keep up).

Our other outcome measures suggest increases in opioid abuse in the Midwest, South, and the Northeast. In the West, the directions of effects are more mixed, suggesting the the (insignificant) decrease in mortality is the primary story there.

## 5.2 Differences by availability of drug treatment

As noted above, one possible explanation for these regional differences in the effects of naloxone access is regional variation in the availability of drug treatment. If this is the case then it suggests a way to mitigate the negative consequences of broadening naloxone access.

We investigate this possibility in two ways. First, 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

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 7 shows effects for opioid- and fentanyl-related mortality, by quartile of treatment availability (Q1 is low, Q4 is high).<sup>23</sup> The coefficients suggest 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.

Next, we consider whether the effects of naloxone access laws vary with states' Medicaid eligibility rules. Recent state-level Medicaid expansions have increased access to substance abuse treatment for a broader set of low-income adults. As shown in [Wen, Hockenberry and Cumming \(2017\)](#) and [Vogler \(2017\)](#), this has led to lower crime rates in those states. We will show in Section 5.5.2 that controlling for these expansions does not affect our main results. But perhaps the effect of naloxone availability varies with access to Medicaid, as it did with the number of drug treatment facilities. Table 8 shows the effects of broadening naloxone access separately for states that do and do not expand Medicaid eligibility by 2015. As before, places with easier access to drug treatment (which Medicaid expansion proxies for) see declines in mortality when naloxone access expands. In places where it is more difficult to access drug treatment (stricter Medicaid eligibility), broadening naloxone access leads to increases in opioid-related and fentanyl-related mortality, as well as a statistically-significant increase in opioid-related ER visits. Again, these coefficients are imprecisely estimated, but we interpret them as suggestive evidence that broadening access to drug treatment can help mitigate the negative consequences of broadening naloxone access.

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<sup>23</sup>Since we do not have county identifiers in the HCUP data, we could not conduct this analysis for ER visits.

### 5.3 Increasing availability of fentanyl

Throughout this period, the supply of fentanyl – a potent synthetic opioid – was increasing across the country. It’s likely that this contributed to rising opioid-related mortality rates, and it is crucial that our estimates not confound the effects of naloxone access laws with independent changes in fentanyl supply. We believe we have successfully isolated the effect of naloxone access laws on mortality. Recall that Figure 8 shows coefficient plots for fentanyl-related mortality. If our estimates were driven by pre-existing trends in fentanyl availability, we would see an increase in fentanyl-related deaths before the naloxone access laws go into effect. Instead, we see flat pre-trends, followed by an increase in fentanyl-related mortality once the naloxone access laws are implemented. This combination of no trend before the effective date of these laws, followed by a trend after the naloxone access laws, gives us confidence that we are isolating the causal effect of the laws themselves.

That said, naloxone access laws may be interacting with the increasing availability of fentanyl in a way that has particularly deadly consequences. Some opioid users may actively seek out fentanyl for a higher high, and naloxone may increase such behavior. However, many overdoses occur when users don’t realize their heroin is spiked with fentanyl. This means that to be safe, users should be extremely careful about the source of their heroin, making sure to only buy from someone they trust. If naloxone makes people a little bit less careful – because they have a safety net in the unlikely case that their heroin is contaminated – then this could lead to more overdoses. Since a single dose of naloxone usually won’t be enough to save someone overdosing on fentanyl<sup>24</sup>, those overdoses could become deaths, even when naloxone is available.

We might expect that a rational drug user would perfectly compensate for the reduction in risk by being less careful about the source of their drugs in a way that leads to no net change in expected mortality. In practice, of course, such perfect compensation is extremely difficult

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<sup>24</sup>Medical professionals recommend bringing someone to the emergency room after they’ve been revived using naloxone, so that additional doses of naloxone can be administered if needed.

and requires a level of information that individual users are unlikely to have (including the likelihood that their heroin is contaminated, and the likelihood that naloxone will save them). It is very easy to imagine users making mistakes in this recalibration of optimal risk-taking, and such mistakes may be driving the rise in fentanyl-related deaths due to the expansion of naloxone access.

While our data don't allow us to distinguish between these types of risky behaviors – users actively seeking fentanyl vs. users being less careful in a context where heroin might be spiked with fentanyl – the precise mechanism matters for determining policy responses. If the rise in mortality is due to carelessness rather than active fentanyl-seeking, then distributing kits that allow users to test their heroin for fentanyl may help mitigate this unintended consequence of broadening naloxone access.

## 5.4 Robustness checks

## 5.5 Alternative controls for pre-treatment trends

Our main estimation strategy includes state-specific linear time trends (and we show robustness to the inclusion of linear, quadratic, and cubic state-specific time trends in Table A.19.) A potential concern is that these time trends may dilute our findings if the treatment effect increases over time, leading to an estimation of null effects for various outcomes (Meer and West 2016; Borusyak and Jaravel 2017). To alleviate this concern, we estimate a model in which the state-specific linear time trends are estimated only based on the pre-treatment period (Goodman-Bacon 2018, Packham 2019), i.e., before the state passed a naloxone law. We partial out pre-treatment trends after de-meaning all the dependent and independent variables in the estimation model in Equation 1.<sup>25</sup> Table A.2 shows the estimates from this alternative approach, and the results corroborate our main findings in Table 6. In most cases, coefficients are larger in magnitude and more statistically significant.

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<sup>25</sup>For states that do not experience a change in naloxone laws over the time period studied, we only de-mean the variables.

### 5.5.1 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 A.3. 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 A.4 shows effects of naloxone laws across all areas (that is, combining urban and rural areas into a single sample). Since 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.

### 5.5.2 Medicaid expansion

As discussed above, several states expanded eligibility for Medicaid during this period, to include low-income childless adults. These expansions might have independently affected opioid use – either by increasing access to low-cost prescription opioids, or by increasing access to health care and substance abuse treatment. The timing of these expansions typically did not coincide with the expansion of naloxone access, so should not be driving our effects, but we can add the dates of Medicaid expansions as a control to confirm this. We use the dates of Medicaid expansion used in Simon, Soni and Cawley (2017), and present the results in Table A.5.

The results are extremely similar to those discussed above. Of particular note, the effect

of naloxone access on mortality in the Midwest is slightly larger and more precisely estimated than before. The negative effect of naloxone access on mortality in the West is also larger and more precisely estimated.

### 5.5.3 Placebo tests

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 we have not controlled for. 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).<sup>26</sup> The results are in Table A.6, which shows effects in the full country as well as by Census region.

The first panel considers the effect of naloxone access on deaths due to suicide. This outcome should not be affected by access to naloxone – even if the suicide involves opioids (because those who want to kill themselves will not care whether a life-saving drug is available). However the suicide rate would be affected by a general 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 or improvement in health (as might be expected if broader trends in health care policy are confounding our estimates). 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

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<sup>26</sup>Unfortunately, we cannot conduct placebo tests on the emergency room visits because we could only obtain that data for opioid-related visits.

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.

#### 5.5.4 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 to an “unspecified” drug (Ruhm 2017; Ruhm 2018). To consider whether our mortality results could be driven by improved labeling of opioid involvement in CDC data, we test the effect of naloxone access laws on a broader category of mortality: deaths due to opioids or an unspecified drug. The top panel of Table A.7 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 5 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.7 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.

### 5.5.5 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.8 through A.16 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 stable pattern is what we find. For instance, in Table A.8, 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.

### 5.5.6 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.17 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.

### 5.5.7 Varying the population cutoff for “urban”

Table A.18 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.

### 5.5.8 More flexible state-specific trends

While the flat pre-trends in our coefficient plots 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.19. This specification strains our statistical power but the results are qualitatively similar.

### 5.5.9 Dropping one state at a time

Tables A.20 through A.23 show how the estimates change as we drop one state at a time from the analysis, region-by-region. The goal of this exercise is to determine the sensitivity of results to the composition of the sample. If coefficients change substantially when one state is dropped, that tells us about heterogeneity in effects within the region, and which states (if any) are driving the average effects reported above.

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.

### 5.5.10 Timing of laws

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

The alternate dates are: May 2013 (instead of April 2015) for Colorado, October 2012 (instead of June 2015) for Connecticut, August 2015 (instead of June 2016, after the end of our data) for Louisiana, April 2014 (instead of October 2015) for Maine, and June 2010 (instead of July 2015) for Washington. In the first four cases, some third-party prescriptions were allowed as of these dates. In the case of Washington, an earlier Good Samaritan Law made naloxone available to individuals at risk of overdose.

Table A.24 presents the results. The estimates are extremely similar to those discussed above, with one exception: there is no longer a statistically-significant decline in fentanyl-related mortality in the West.

### 5.5.11 Using the specification from Rees, et al.

Rees et al. (2017) also examine the effect of naloxone laws on opioid-related mortality, but find a reduction in mortality. This is very different from our result. Table A.25 attempts to determine which differences in our specifications might be leading to these different results. Column 1 shows the Rees, et al., estimate and column 2 shows our estimate. Columns 3 through 8 then layer in changes to show how it would affect our estimate to use their specification choices. By the final column, we are unable to match their result; in fact, the estimate is positive and marginally significant ( $p < 0.10$ ). Our estimate is therefore robust to their specification choices.

The one difference between our analyses that we cannot check is the effect of using

a longer pre-period. Rees, et al. use data back through 1999, while our data begin in 2010. Given that all but one of the naloxone laws of interest were implemented in 2012 or later, we view this shorter pre-period as sufficient to estimate causal effects of relevant law changes.<sup>27</sup> Furthermore, recent changes in the opioid epidemic mean that mortality trends from before 2010 may be uninformative or even misleading when trying to learn about the current context. Forcing longer pre-period trends through 1999 could result in incorrect conclusions about the effect of policy changes during the present decade.

### 5.5.12 Differential effects by baseline mortality rate

Areas with high levels of pre-period opioid mortality may have been affected differently by naloxone access laws than areas with lower levels of pre-period opioid mortality. With more lives that can be saved in the short term, we might expect to see bigger benefits in places with high baseline mortality. On the other hand, high baseline levels of opioid abuse might make populations more responsive to the safety net that naloxone provides, increasing risk-taking by the large stock of existing users. Given these possibilities, we consider differential effects of naloxone access laws across areas with different baseline mortality rates.

This analysis is also useful for considering differential effects for early versus late adopters of naloxone access laws. As we showed in Table 2, states that broadened naloxone access early had higher baseline levels of opioid abuse based on some measures. Geographic fixed effects should account for such baseline differences, and the flat pre-trends in our coefficient plots showed that our specification accounts for any differential pre-period trends. However, checking for differential effects by baseline mortality rates is one more way to confirm that our estimates are not confounded by different growth patterns across places that might be at different stages of a natural epidemic process.

Table A.26 shows the impact of naloxone laws by quartile of baseline (year 2010) opioid-

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<sup>27</sup>New Mexico broadened naloxone access in 2001, and so is likely unrepresentative of the more recent movement to broaden naloxone access. By focusing on the recent opioid epidemic, our analysis excludes that law change.

related mortality rates. Columns 1 through 4 of Table A.26 show the impacts of naloxone access laws on all opioid-related deaths, by quartile of baseline opioid mortality, and columns 5 through 8 repeat this analysis for the subset of fentanyl-related deaths. Statistical power is limited, but we see no clear pattern in the coefficients. On average, broadening naloxone access increased opioid-related mortality in places with both high and low baseline mortality rates. These estimates do not support the hypothesis that places with high baseline mortality are affected differently than places with low baseline mortality.

## 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 abuse: death. Naloxone can save lives and provide a second chance for addicted individuals to seek treatment. There is a concern, however, that widespread access to the safety net drug can unintentionally increase riskier opioid use and its related problems. This paper shows that, as implemented through 2015, naloxone access laws—a proxy for expanding access to naloxone—increased opioid abuse and opioid-related crime, and did not reduce opioid-related mortality. In fact, in some areas, particularly the Midwest, naloxone access laws increased opioid-related mortality. Opioid-related mortality also appears to have increased in the South and most of the Northeast as a result of these laws.

Our analysis is limited by the lack of data on actual naloxone distribution, so our estimates represent ITT effects. If naloxone access laws did not broaden naloxone access as much as proponents had hoped, then our estimates should be biased toward zero. In other words, we would expect even larger increases in opioid abuse in contexts where naloxone is easily available to everyone.

Our findings do not imply that we should limit naloxone availability to individuals suffering from opioid addiction, or those who are at risk of overdose; naloxone has been available through prescription for a long time to these groups, and serves an important role in reducing the risk of opioid overdose deaths. The results do imply that the public health community

should prepare for the behavioral effects documented in this paper. In particular, the findings in this analysis are consistent with a broad economics literature on moral hazard, as well as recent work on unintended effects of syringe exchange programs as documented in Packham (2019). Overall, the results support the hypothesis that making opioid use less risky increases opioid abuse.

Looking forward, this analysis suggests 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. Similarly, experimenting with complementary interventions such as fentanyl-testing strips could provide new insights into how to mitigate the unintended consequences of broad naloxone access. Future research in this area would be valuable.

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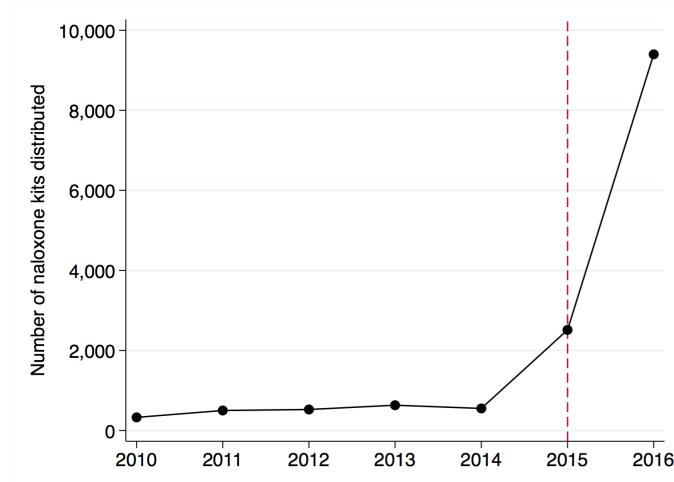
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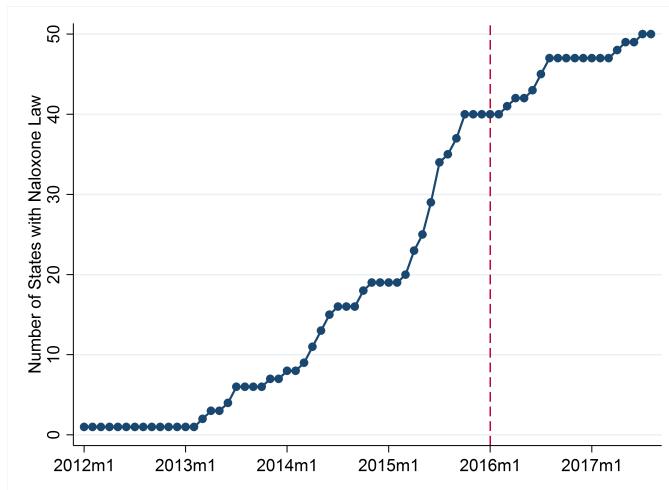
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Figure 1: Distribution of naloxone kits in Baltimore



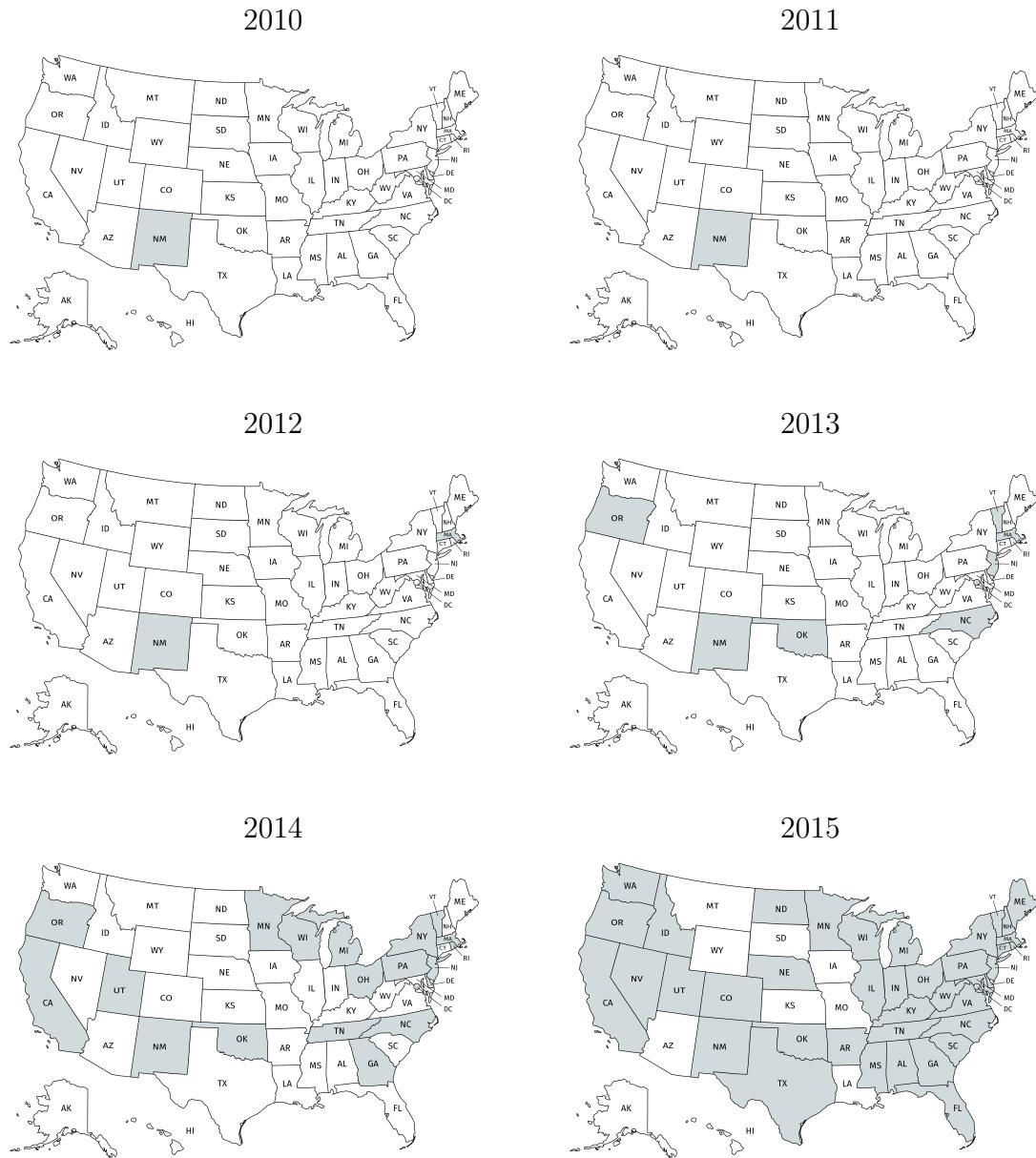
*Notes:* Figure shows the number of naloxone kits distributed in Baltimore before and after Maryland broadened naloxone access. Maryland's naloxone access law went into effect in October 2015.

Figure 2: Timeline of naloxone access laws



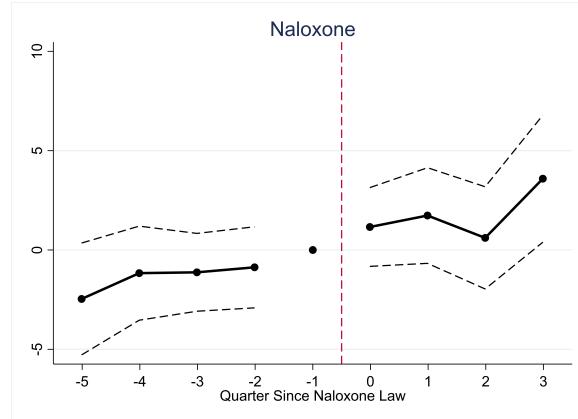
*Notes:* Figure shows the number of states with any broadened naloxone access law in each month-year between January 2012 and July 2017, by which point all states had such laws. The data include all 50 states. Categorization of state-by-state naloxone laws was done using hand-collected data. Our analyses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

Figure 3: States with naloxone access laws, by year



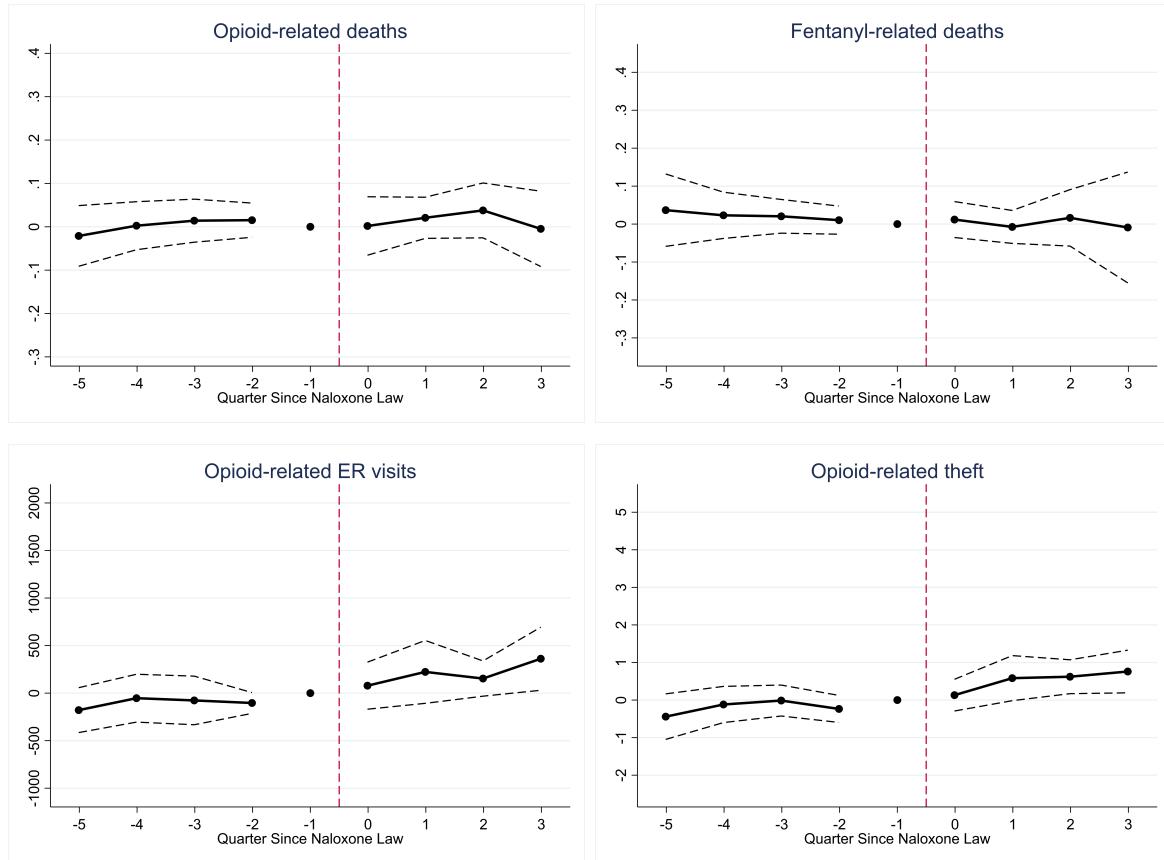
*Notes:* Figure shows the states with naloxone access laws by December 31 of each year. These states are shaded; New Mexico was the first state to broaden access and did so in 2001.

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



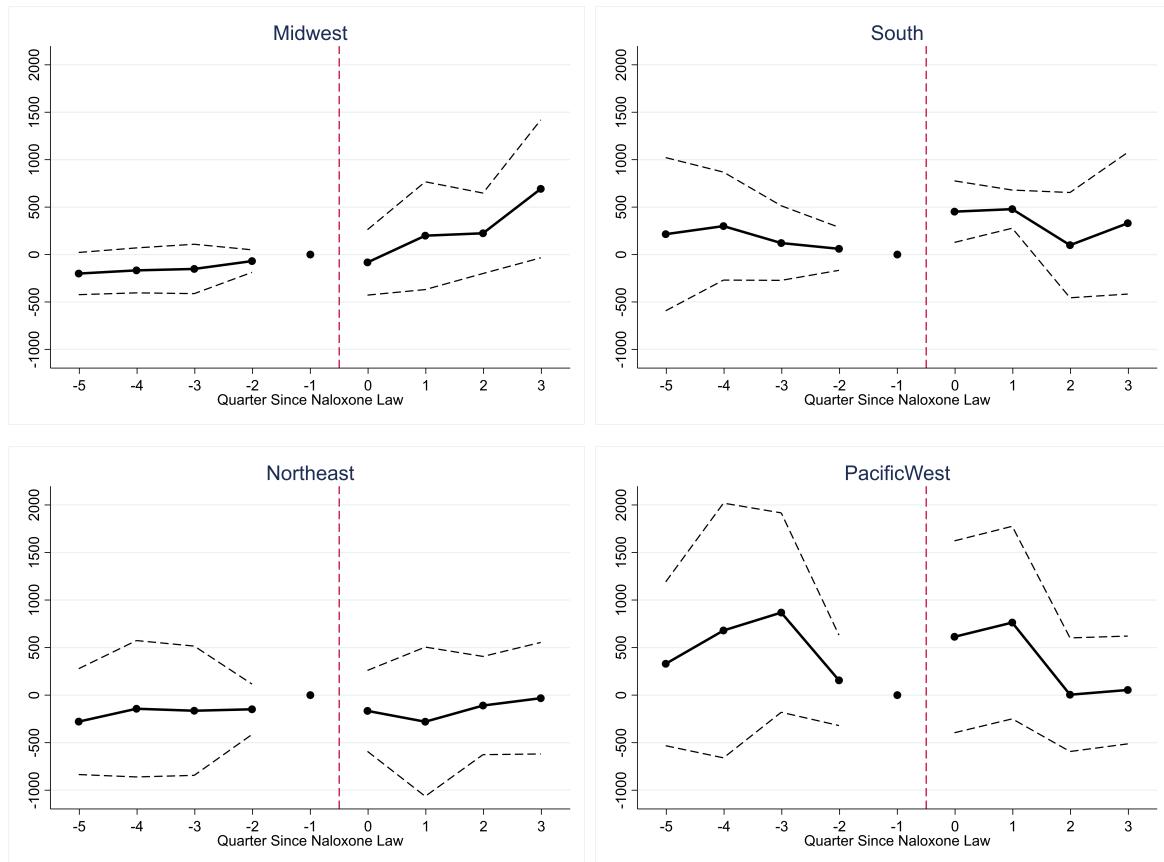
*Notes:* Figure shows coefficients of the impact of broadened naloxone access on Google searches for the “naloxone” topic. The specification follows equation (1), but includes dummies for the quarter from the passage of naloxone laws instead of a dummy for whether there is such a law. I.e., the covariate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

Figure 5: Effect of naloxone access laws on outcomes



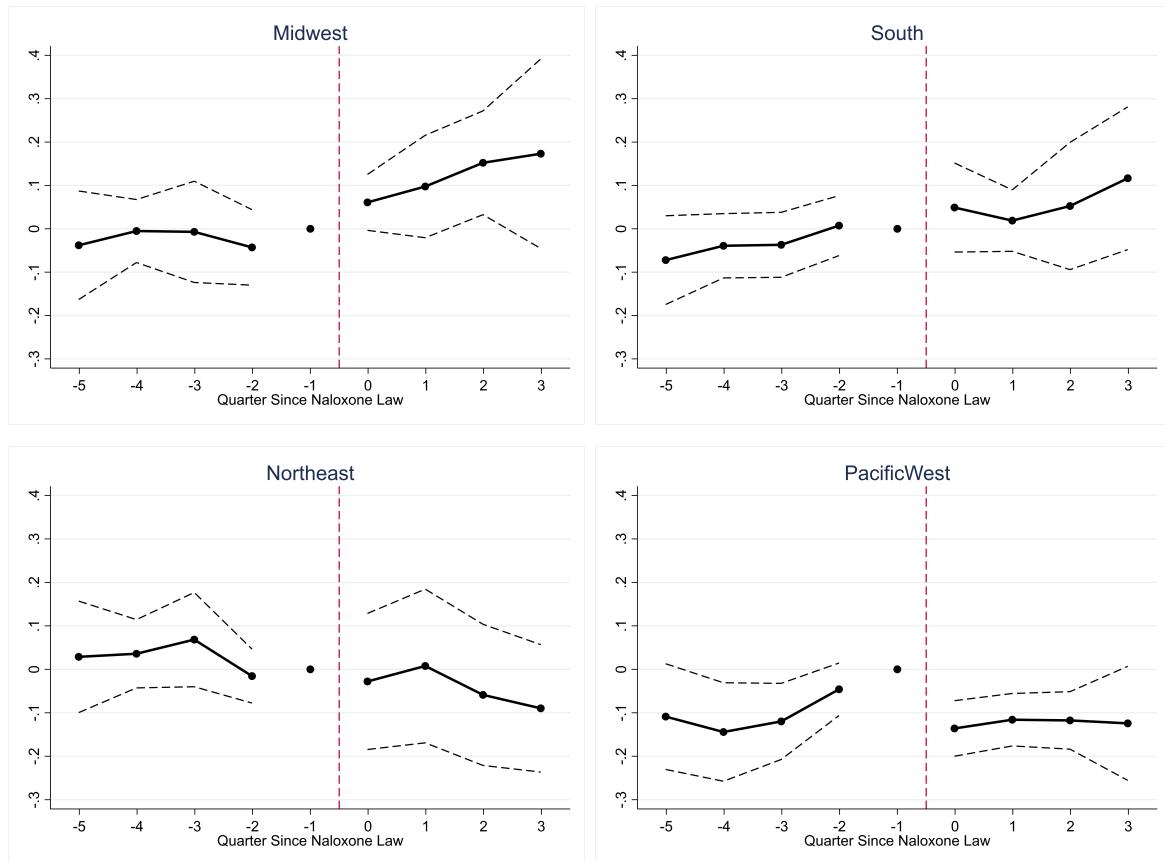
*Notes:* See notes for Figure 4. Data source: CDC (for mortality), HCUP (for ER admissions), and NIBRS (for arrests and crime). Date range: 2010-2015 (CDC and NIBRS), 2006-2015 (HCUP).

Figure 6: Effect of naloxone access laws on opioid-related ER visits



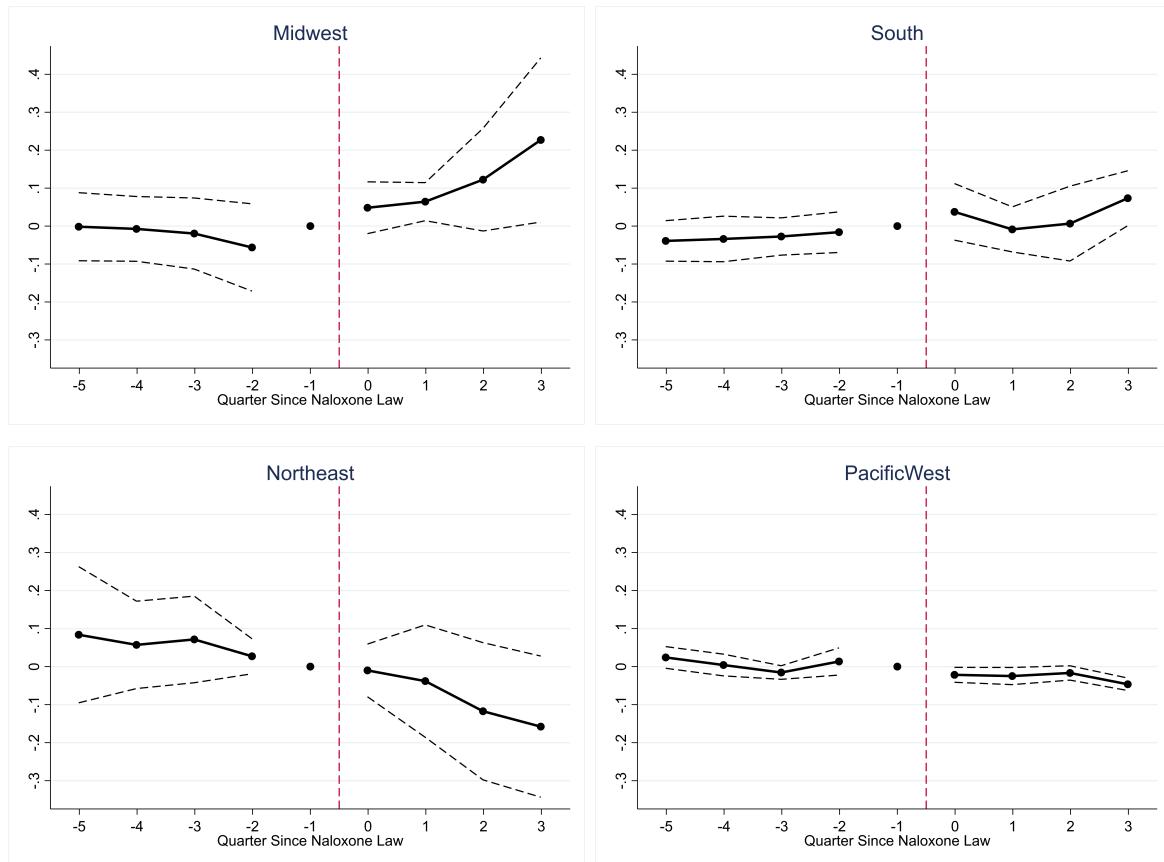
*Notes:* See notes for Figure 4. Data source: HCUP. Sample includes metro areas. Date range: 2006-2015.

Figure 7: Effect of naloxone access laws on opioid-related mortality



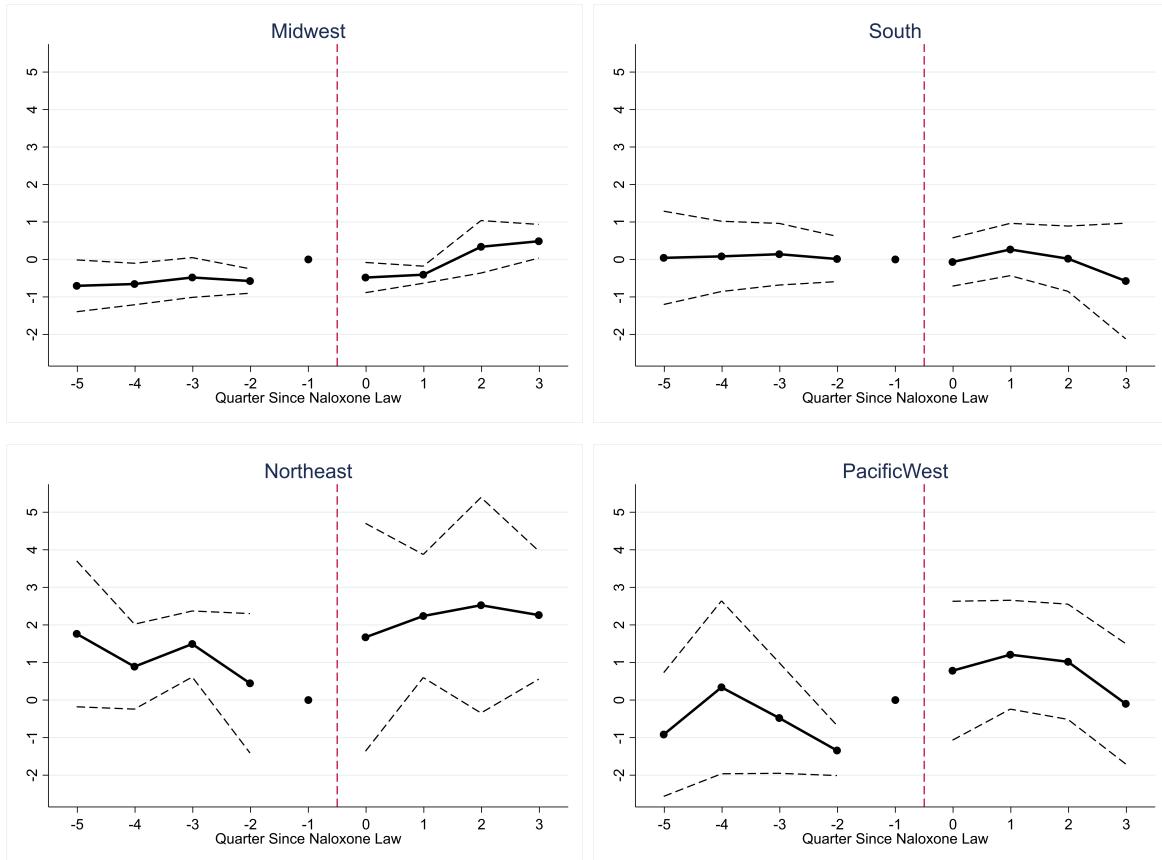
*Notes:* See notes for Figure 4. Data source: CDC. Sample includes counties that include at least one city with population  $\geq 40,000$ . Date range: 2010-2015.

Figure 8: Effect of naloxone access laws on fentanyl-related mortality



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

Figure 9: Effect of naloxone access laws on opioid-related theft



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

Table 1: Timing of naloxone laws by state

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

*Notes:* Table shows the month-year of broadened naloxone access via third party prescription or standing order by state. Nine states broadened naloxone access in January 2016 or afterwards: AK, IA, SD, HI, AZ, MO, KS, MT, and WY. (Source: Hand-collected information.) \*In a robustness check shown in Table A.24, we examine different dates for some states: 5/2013 for CO, 10/2012 for CT; 8/2015 for LA; and 4/2014 for ME because there were some third-party prescriptions allowed as of these dates. We also test 6/2010 for WA because a Good Samaritan Law at that time made naloxone available to individuals at risk of overdose.

Table 2: Summary statistics

	All years		Baseline rates (2010)			
	All jurisdictions		Early Adopters		Late Adopters	
	Mean (1)	SD (2)	Mean (3)	SD (4)	Mean (5)	SD (6)
<b>Google Trends search intensity</b>						
“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	
<b>Opioid-related ER visits</b>						
	1938	2360	2420	2149	898.1	739.5
N (State-quarters)	1,108		64		52	
<b>Mortality rates</b>						
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	
<b>Crime rates</b>						
Possession of opioids	29.50	39.96	24.20	35.74	19.78	22.48
Selling opioids	8.255	17.93	7.645	17.57	3.262	7.963
All opioid-related crime	47.72	58.04	40.95	52.54	30.45	29.39
Heroin	27.62	46.74	18.66	34.66	8.331	12.54
Other Narcotics	18.93	29.72	21.40	34.05	21.10	27.47
Opioid-involved theft	1.862	5.183	1.322	4.213	1.618	4.181
Theft (all)	1727	961.0	1766	980.2	2194	904.6
Marijuana-related crime	229.2	193.0	234.8	202.6	283.6	147.8
N (Jurisdiction-months)	29,952		4,200		792	

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

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

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

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

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

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

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

Table 5: Effect of naloxone laws on crime

	Opioid-related crime (1)	Opioid-related theft (2)	All theft (3)
Naloxone Law	6.053** (2.213)	0.414* (0.214)	4.810 (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 6: 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)
<b>Midwest</b>							
Naloxone Law	4.925* (2.140)	0.874** (0.363)	293.9 (240.2)	0.094** (0.041)	0.076* (0.041)	5.481* (2.542)	0.034 (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
<b>South</b>							
Naloxone Law	3.783 (3.415)	1.694* (0.780)	309.1** (111.9)	0.052 (0.037)	0.033 (0.020)	5.333 (4.349)	0.136 (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
<b>Northeast</b>							
Naloxone Law	6.408** (1.803)	5.286* (2.073)	-24.93 (142.5)	-0.047 (0.064)	-0.092 (0.081)	12.10** (3.146)	0.860 (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
<b>West</b>							
Naloxone Law	-1.854 (3.130)	0.649 (1.252)	57.08 (41.82)	-0.059 (0.040)	-0.023*** (0.006)	-0.226 (2.589)	1.417*** (0.363)
Observations	4,968	4,968	184	9,648	9,648	4,968	4,968
2010 baseline	20.40	4.568	2498	0.619	0.068	34.03	1.843

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

Table 7: 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)
<b>Opioid-related deaths</b>				<b>Fentanyl-related deaths</b>				
Naloxone Law	0.032 (0.035)	0.032 (0.040)	0.016 (0.034)	-0.028 (0.054)	0.052* (0.026)	0.012 (0.018)	0.010 (0.035)	-0.038 (0.054)
Observations	13,896	13,896	13,896	13,824	13,896	13,896	13,896	13,824
2010 baseline	0.555	0.599	0.576	0.694	0.078	0.081	0.070	0.099

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

Table 8: Effect of naloxone laws by Medicaid expansion status

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

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

## A Appendix – for online publication

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

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

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

Table A.2: Mortality results with Goodman-Bacon correction for pre-treatment trends

	<b>Main results</b>		<b>Goodman-Bacon correction</b>	
	Opioid-related deaths (1)	Fentanyl-related deaths (2)	Opioid-related deaths (3)	Fentanyl-related deaths (4)
<b>Entire U.S.</b>				
Naloxone Law	0.006 (0.027)	-0.003 (0.030)	0.015 (0.027)	0.006 (0.030)
Observations	55,512	55,512	55,512	55,512
<b>Midwest</b>				
Naloxone Law	0.094** (0.041)	0.076* (0.041)	0.114** (0.040)	0.085* (0.047)
Observations	12,240	12,240	12,240	12,240
<b>South</b>				
Naloxone Law	0.052 (0.037)	0.033 (0.020)	0.058* (0.032)	0.034* (0.018)
Observations	25,488	25,488	25,488	25,488
<b>Northeast</b>				
Naloxone Law	-0.047 (0.064)	-0.092 (0.081)	-0.075 (0.049)	-0.086 (0.078)
Observations	8,136	8,136	8,136	8,136
<b>West</b>				
Naloxone Law	-0.059 (0.040)	-0.023*** (0.006)	-0.039 (0.040)	-0.015* (0.007)
Observations	9,648	9,648	9,648	9,648

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

Table A.3: 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)
<b>Entire U.S.</b>							
Naloxone Law	1.862 (2.174)	-0.833 (1.403)	48.70 (43.02)	-0.017 (0.037)	0.000 (0.015)	1.298 (3.634)	0.310 (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
<b>Midwest</b>							
Naloxone Law	2.112* (0.971)	0.972 (1.157)	107.0 (65.23)	0.027 (0.040)	0.006 (0.029)	3.940* (1.880)	-0.034 (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
<b>South</b>							
Naloxone Law	-2.379 (2.082)	-4.676** (1.459)	-57.53* (25.85)	-0.031 (0.061)	-0.007 (0.020)	-6.821* (3.481)	0.092 (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
<b>Northeast</b>							
Naloxone Law	2.631 (2.211)	-0.909 (0.686)	-6.738 (44.53)	-0.098 (0.143)	-0.108 (0.112)	4.749 (3.052)	0.699** (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
<b>West</b>							
Naloxone Law	6.121 (3.153)	2.352* (1.051)	140.05* (61.22)	-0.046 (0.108)	-0.017 (0.039)	8.945* (4.480)	0.818 (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 show the effect of expanding naloxone access on arrests per million residents (columns 1 and 2), number of ER visits (column 3), deaths per 100,000 residents (columns 4 and 5), and reported crimes per million residents (columns 6 and 7). All coefficients except ER visits are also population-weighted.

Table A.4: 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)
<b>Entire U.S.</b>							
Naloxone Law	3.132* (1.671)	0.839 (0.699)	335.3** (132.6)	0.001 (0.025)	-0.003 (0.025)	3.987 (2.384)	0.402** (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
<b>Midwest</b>							
Naloxone Law	3.555** (1.346)	1.094 (0.679)	374.6 (312.1)	0.074** (0.033)	0.064 (0.038)	4.615* (2.052)	0.020 (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
<b>South</b>							
Naloxone Law	1.020 (3.330)	-1.331* (0.712)	231.1** (93.12)	0.037 (0.039)	0.022 (0.020)	-0.355 (4.451)	0.067 (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
<b>Northeast</b>							
Naloxone Law	3.995* (1.565)	1.835* (0.889)	68.44 (146.8)	-0.055 (0.066)	-0.093 (0.075)	7.722** (2.827)	0.766*** (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
<b>West</b>							
Naloxone Law	1.167 (2.552)	1.210* (0.582)	334.1*** (64.30)	-0.060 (0.039)	-0.024*** (0.003)	2.972 (2.572)	1.256*** (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 show the effect of expanding naloxone access on arrests per million residents (columns 1 and 2), number of ER visits (column 3), deaths per 100,000 residents (columns 4 and 5), and reported crimes per million residents (columns 6 and 7). All coefficients except those in column (3) are also population-weighted.

Table A.5: Effect of naloxone laws controlling for Medicaid expansion

	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)
<b>All regions</b>							
Naloxone Law	3.924** (1.697)	1.951*** (0.669)	252.7* (103.0)	-0.004 (0.035)	0.053 (0.036)	6.006** (2.221)	0.436** (0.205)
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
<b>Midwest</b>							
Naloxone Law	3.876* (1.845)	0.750* (0.388)	180.7 (212.1)	0.123*** (0.037)	0.101*** (0.025)	4.079 (2.286)	-0.036 (0.048)
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
<b>South</b>							
Naloxone Law	3.734 (3.344)	1.796** (0.775)	333.7** (103.0)	0.051 (0.037)	0.035 (0.022)	5.444 (4.472)	0.146 (0.309)
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
<b>Northeast</b>							
Naloxone Law	3.995* (1.565)	1.835* (0.889)	-69.4 (145.3)	-0.052 (0.064)	-0.097 (0.082)	12.090** (3.243)	0.859 (0.618)
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
<b>West</b>							
Naloxone Law	1.167 (2.736)	1.210* (0.582)	59.1 (69.9)	-0.068* (0.036)	-0.024*** (0.005)	-0.113 (2.572)	1.430*** (0.313)
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. 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), the dates of Medicaid expansion (as in Simon, Soni and Cawley (2017)), and the following laws/regulations: Good Samaritan laws, PDMP, Doctor Shopping, Pain Clinic regulations, Physician exams, Pharmacy verification, require ID, and tamper-resistant PF. Coefficients show the effect of expanding naloxone access on arrests per million residents (columns 1 and 2), number of ER visits (column 3), deaths per 100,000 residents (columns 4 and 5), and reported crimes per million residents (columns 6 and 7). All coefficients except those in column (3) are also population-weighted.

Table A.6: Placebo test: Effect of naloxone laws on outcomes that should not be affected

	Entire U.S. (1)	Midwest (2)	South (3)	Northeast (4)	West (5)
<b>Deaths due to suicide</b>					
Naloxone Law	-0.004 (0.013)	-0.022 (0.033)	-0.006 (0.019)	-0.023 (0.025)	0.005 (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
<b>Deaths due to heart disease</b>					
Naloxone Law	-0.031 (0.164)	0.106 (0.206)	-0.082 (0.147)	0.041 (0.385)	0.095 (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
<b>Deaths due to motor vehicle accidents</b>					
Naloxone Law	0.001 (0.016)	-0.061 (0.041)	0.003 (0.027)	-0.024 (0.019)	0.016 (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.7: Effect of naloxone laws on broader categories of deaths and crime

	Entire U.S. (1)	Midwest (2)	South (3)	Northeast (4)	West (5)
<b>Deaths due to opioids or unspecified-drug poisoning</b>					
Naloxone Law	0.003 (0.029)	0.076 (0.049)	0.041 (0.048)	-0.035 (0.057)	-0.021 (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
<b>All theft</b>					
Naloxone Law	4.810 (12.84)	48.04* (25.81)	-7.045 (16.39)	-10.81 (22.25)	56.74 (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.8: 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*** (1.321)	3.903*** (1.211)	1.921** (0.814)	1.937** (0.807)	1.910** (0.817)	1.877** (0.808)	1.831** (0.813)	1.847** (0.809)
Observations	20,232	20,232	20,232	20,232	20,232	20,232	20,232	20,232
2010 baseline	25.49	25.49	25.49	25.49	25.49	25.49	25.49	25.49
Adjusted <i>R</i> <sup>2</sup>	0.047	0.084	0.106	0.106	0.106	0.106	0.106	0.106
<b>Controls:</b>								
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.9: 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*** (0.473)	0.266 (0.646)	-0.725 (0.435)	-0.695 (0.448)	-0.773* (0.457)	-0.744* (0.440)	-0.744* (0.441)	-0.799* (0.450)
Observations	21,528	21,528	21,528	21,528	21,528	21,528	21,528	21,528
2010 baseline	55.72	55.72	55.72	55.72	55.72	55.72	55.72	55.72
Adjusted <i>R</i> <sup>2</sup>	0.002	0.159	0.168	0.168	0.168	0.168	0.168	0.169
<b>Controls:</b>								
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.10: 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 (3.449)	6.570** (2.829)	3.113* (1.663)	2.963* (1.659)	3.795** (1.795)	4.211** (1.733)	4.148** (1.759)	4.030** (1.673)
Observations	29,808	29,808	29,808	29,808	29,808	29,808	29,808	29,808
2010 baseline	23.52	23.52	23.52	23.52	23.52	23.52	23.52	23.52
Adjusted <i>R</i> <sup>2</sup>	0.003	0.046	0.100	0.101	0.102	0.103	0.103	0.103
<b>Controls:</b>								
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.11: Effect of naloxone laws on arrests for selling opioids

	Selling opioids (arrests)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Naloxone Law	0.873 (0.613)	1.206* (0.638)	1.651** (0.668)	1.509** (0.631)	1.911** (0.702)	1.933*** (0.687)	1.919*** (0.688)	1.917*** (0.675)
Observations	29,808	29,808	29,808	29,808	29,808	29,808	29,808	29,808
2010 baseline	6.972	6.972	6.972	6.972	6.972	6.972	6.972	6.972
Adjusted $R^2$	0.000	0.007	0.017	0.020	0.020	0.021	0.021	0.021
<b>Controls:</b>								
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.12: Effect of naloxone laws on opioid-related ER visits

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

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

Table A.13: 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*** (0.068)	0.058 (0.063)	0.013 (0.025)	0.014 (0.025)	0.009 (0.027)	0.006 (0.027)	0.005 (0.027)	0.006 (0.027)
Observations	55,512	55,512	55,512	55,512	55,512	55,512	55,512	55,512
2010 baseline	0.601	0.601	0.601	0.601	0.601	0.601	0.601	0.601
Adjusted <i>R</i> <sup>2</sup>	0.026	0.042	0.095	0.095	0.095	0.095	0.095	0.095
<b>Controls:</b>								
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.14: 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*** (0.050)	0.034 (0.040)	-0.001 (0.033)	-0.001 (0.033)	-0.002 (0.032)	-0.005 (0.032)	-0.005 (0.032)	-0.003 (0.030)
Observations	55,512	55,512	55,512	55,512	55,512	55,512	55,512	55,512
2010 baseline	0.080	0.080	0.080	0.080	0.080	0.080	0.080	0.080
Adjusted <i>R</i> <sup>2</sup>	0.047	0.075	0.166	0.166	0.166	0.168	0.168	0.169
<b>Controls:</b>								
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.15: Effect of naloxone laws on opioid-related crime

	All opioid-related crime							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Naloxone Law	3.463 (4.627)	8.808** (3.740)	4.964** (2.379)	4.581* (2.313)	5.742** (2.467)	6.312** (2.293)	6.230** (2.337)	6.053** (2.213)
Observations	29,808	29,808	29,808	29,808	29,808	29,808	29,808	29,808
2010 baseline	39.34	39.34	39.34	39.34	39.34	39.34	39.34	39.34
Adjusted <i>R</i> <sup>2</sup>	0.001	0.052	0.099	0.102	0.102	0.104	0.104	0.104
<b>Controls:</b>								
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.16: Effect of naloxone laws on opioid-related theft

	Opioid-related theft							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Naloxone Law	0.340 (0.341)	0.609* (0.331)	0.423* (0.224)	0.419* (0.224)	0.428* (0.222)	0.445* (0.224)	0.434* (0.224)	0.414* (0.214)
Observations	29,808	29,808	29,808	29,808	29,808	29,808	29,808	29,808
2010 baseline	1.367	1.367	1.367	1.367	1.367	1.367	1.367	1.367
Adjusted <i>R</i> <sup>2</sup>	0.001	0.008	0.022	0.022	0.022	0.023	0.023	0.023
<b>Controls:</b>								
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.17: Impact of naloxone laws on opioid-related crimes

	All	Heroin	Other Narcotics (inc. Fentanyl)	Morphine	Opium
	(1)	(2)	(3)	(4)	(5)
Naloxone Law	6.053** (2.213)	2.603*** (0.900)	3.795* (2.133)	0.188* (0.098)	-0.121 (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.18: 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
<b>Opioid-related mortality</b>										
Naloxone Law	0.000 (0.026)	-0.000 (0.026)	0.000 (0.026)	0.000 (0.026)	0.002 (0.026)	0.006 (0.026)	0.006 (0.027)	0.007 (0.026)	0.007 (0.027)	0.009 (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
<b>Opioid-related theft</b>										
Naloxone Law	0.471** (0.195)	0.488** (0.192)	0.500** (0.213)	0.482** (0.221)	0.492** (0.225)	0.484** (0.234)	0.414* (0.214)	0.347* (0.202)	0.380* (0.204)	0.383* (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.19: Effect of naloxone laws, controlling for state-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** (1.311)	1.433** (0.554)	64.76 (106.04)	-0.000 (0.021)	0.008 (0.018)	4.572** (2.046)	0.035 (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 show the effect of expanding naloxone access on arrests per million residents (columns 1 and 2), number of ER visits (column 3), deaths per 100,000 residents (columns 4 and 5), and reported crimes per million residents (columns 6 and 7). All coefficients except those in column (3) are also population-weighted.

Table A.20: Effect of naloxone laws in the Midwest, dropping one state at a time

	Obs. (1)	“Naloxone” searches (2)	Obs. (3)	Opioid-related ER visits (4)	Obs. (5)	Opioid-related mortality (6)	Obs. (7)	Opioid-related theft (8)
Drop IA	5,184	1.233 (1.885)	364	363.3* (195.2)	11,592	0.081* (0.041)	8,568	-0.185 (0.230)
Drop IL	4,896	1.846 (2.120)	376	456.7* (247.6)	11,016	0.089 (0.059)	9,360	0.046 (0.304)
Drop IN	4,968	2.940 (1.845)	368	373.1 (216.3)	10,584	0.118*** (0.032)		
Drop KS	5,328	1.797 (1.956)	364	358.4 (200.1)	11,664	0.085* (0.041)	8,928	-0.096 (0.288)
Drop MI	5,040	0.670 (1.616)			10,152	0.061 (0.035)	5,040	0.262 (0.303)
Drop MN	5,256	2.196 (2.065)	364	428.2* (221.4)	11,376	0.109** (0.045)		
Drop MO	5,112	0.897 (1.841)	364	401.6* (203.3)	11,448	0.087* (0.041)	9,288	0.034 (0.316)
Drop ND	5,472	1.923 (1.861)	384	391.1* (211.9)	11,952	0.095** (0.041)	9,144	0.026 (0.303)
Drop NE	5,184	2.080 (1.972)	364	488.5** (200.7)	11,952	0.096** (0.042)	9,432	0.034 (0.278)
Drop OH	4,752	3.184* (1.734)	364	24.59 (78.71)	9,720	0.127** (0.056)	7,056	0.323 (0.418)
Drop SD	5,472	1.804 (1.908)	364	359.5* (188.9)	12,096	0.097** (0.041)	9,288	0.067 (0.286)
Drop WI	5,112	1.894 (1.959)	364	413.5* (209.3)	11,088	0.108** (0.048)	8,784	0.078 (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), number of ER visits (column 4), deaths per 100,000 residents (column 6), and reported crimes per million residents (column 8). Coefficients 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). All coefficients except those in column (4) are also population-weighted.

Table A.21: Effect of naloxone laws in the South, dropping one state at a time

	Obs. (1)	“Naloxone” searches (2)	Obs. (3)	Opioid-related ER visits (4)	Obs. (5)	Opioid-related mortality (6)	Obs. (7)	Opioid-related theft (8)
Drop AL	8,280	1.596 (1.212)			24,192	0.059 (0.037)		
Drop AR	8,208	1.481 (1.277)	252	226.7** (86.77)	24,840	0.055 (0.037)	10,584	0.127 (0.336)
Drop DE	8,568	1.515 (1.238)			25,272	0.052 (0.038)	11,232	0.097 (0.335)
Drop FL	7,992	0.974 (1.296)	220	233.1* (110.8)	22,536	0.016 (0.031)		
Drop GA	7,992	2.299* (1.093)	224	211.6** (72.32)	23,040	0.034 (0.039)		
Drop KY	7,992	1.129 (1.131)	228	182.3* (85.92)	24,552	0.054 (0.037)	10,728	0.125 (0.323)
Drop LA	8,208	0.548 (1.042)			24,048	0.039 (0.033)	11,160	0.125 (0.317)
Drop MS	8,280	1.417 (1.182)			24,768	0.051 (0.038)		
Drop MD	8,496	1.598 (1.204)	224	237.1* (96.98)	24,264	0.040 (0.036)		
Drop NC	8,136	1.247 (1.600)	228	148.1 (100.4)	22,104	0.087** (0.039)		
Drop OK	8,280	1.302 (1.283)			24,984	0.048 (0.037)		
Drop SC	8,208	1.487 (1.222)	224	181.5* (77.10)	23,760	0.059 (0.037)	9,504	0.174 (0.360)
Drop TN	8,280	1.723 (1.257)	220	310.6** (94.68)	24,192	0.061 (0.042)	9,432	0.013 (0.344)
Drop TX	7,416	1.437 (1.327)			22,176	0.049 (0.043)	9,936	0.457** (0.183)
Drop VA	8,136	1.545 (1.209)			22,752	0.059 (0.037)	8,712	-0.213 (0.246)
Drop WV	8,208	1.479 (1.242)			24,840	0.052 (0.039)	10,872	0.142 (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 show the effect of expanding naloxone access on Google searches for “naloxone” (column 2), number of ER visits (column 4), deaths per 100,000 residents (column 6), and reported crimes per million residents (column 8). All coefficients except those in column (4) are also population-weighted.

Table A.22: 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 (1.910)	224	-33.29 (119.2)	7,776	-0.085 (0.046)	2,952	3.256 (1.539)
Drop MA	2,016	4.970* (2.068)	224	-154.8 (176.4)	7,416	-0.007 (0.072)	1,656	1.803 (1.168)
Drop ME	2,088	2.728 (1.848)	224	-50.19 (126.8)	7,848	-0.043 (0.064)	3,816	0.873 (0.644)
Drop NH	2,016	2.283 (1.774)			7,992	-0.031 (0.062)	3,672	0.509 (0.383)
Drop NJ	2,088	3.804 (2.089)	220	54.73 (88.73)	6,912	-0.086 (0.063)		
Drop NY	1,512	8.103** (3.209)	224	-5.156 (244.0)	5,256	0.146** (0.045)		
Drop PA	1,584	3.644 (2.014)			5,832	-0.058 (0.059)		
Drop RI			224	-48.95 (124.4)	7,992	-0.062 (0.063)	3,456	0.520 (0.485)
Drop VT	2,160	3.001 (1.979)	220	-132.3 (159.6)	8,064	-0.047 (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 show the effect of expanding naloxone access on Google searches for "naloxone" (column 2), number of ER visits (column 4), deaths per 100,000 residents (column 6), and reported crimes per million residents (column 8). All coefficients except those in column (4) are also population-weighted.

Table A.23: Effect of naloxone laws in the West, dropping one state at a time

	Obs. (1)	“Naloxone” searches (2)	Obs. (3)	Opioid-related ER visits (4)	Obs. (5)	Opioid-related mortality (6)	Obs. (7)	Opioid-related theft (8)
Drop AK	3,600	1.694 (1.854)			9,576	-0.059 (0.040)		
Drop AZ	3,456	2.149 (2.010)	144	-26.32 (88.27)	8,928	-0.035 (0.035)	4,824	1.525** (0.384)
Drop CA	2,664	0.876 (2.246)	144	137.1 (73.65)	6,552	-0.157** (0.056)		
Drop CO	3,456	2.611 (2.006)			8,784	-0.039 (0.040)	3,312	1.525* (0.613)
Drop HI	3,600	2.339 (1.868)	144	34.71 (62.14)	9,360	-0.063 (0.040)		
Drop ID	3,456	3.253* (1.548)			9,216	-0.059 (0.040)	4,104	1.557*** (0.298)
Drop MT	3,384	1.113 (1.495)	176	5.449 (64.95)	9,288	-0.064 (0.039)	4,536	1.374** (0.420)
Drop NM	3,456	1.865 (1.947)			9,072	-0.069 (0.040)		
Drop NV	3,456	1.701 (1.961)	164	-4.296 (54.80)	9,216	-0.073* (0.039)		
Drop OR	3,240	2.209 (2.170)			8,784	-0.043 (0.042)	4,536	1.130** (0.432)
Drop UT			148	-35.86 (36.18)	9,216	-0.053 (0.039)	3,888	1.171* (0.534)
Drop WA	3,384	1.118 (1.845)			8,280	-0.070 (0.044)	4,608	1.352*** (0.333)
Drop WY	3,240	2.335 (2.097)			9,504	-0.060 (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 show the effect of expanding naloxone access on Google searches for “naloxone” (column 2), number of ER visits (column 4), deaths per 100,000 residents (column 6), and reported crimes per million residents (column 8). All coefficients except those in column (4) are also population-weighted.

Table A.24: Effect of naloxone laws with robustness to law timing

	Possession of opioids (1)	Selling opioids (2)	Opioid-related ER visits (3)	Opioid-related deaths (4)	Fentanyl-related deaths (5)	Opioid-related crime (6)	Opioid-related theft (7)
<b>All regions</b>							
Naloxone Law	4.030** (1.673)	1.917*** (0.675)	262.3** (109.2)	0.017 (0.025)	0.007 (0.027)	6.053** (2.213)	0.414* (0.214)
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
<b>Midwest</b>							
Naloxone Law	4.925* (2.140)	0.874** (0.363)	293.9 (240.2)	0.094** (0.041)	0.076* (0.041)	5.481* (2.542)	0.034 (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
<b>South</b>							
Naloxone Law	3.783 (3.415)	1.694* (0.780)	309.1** (111.9)	0.042 (0.035)	0.030 (0.020)	5.333 (4.349)	0.136 (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
<b>Northeast</b>							
Naloxone Law	6.408** (1.803)	5.286* (2.073)	-13.0 (139.4)	-0.043 (0.061)	-0.089 (0.071)	12.098** (3.146)	0.860 (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
<b>West</b>							
Naloxone Law	-1.854 (3.130)	0.649 (1.252)	57.1 (41.8)	-0.047 (0.031)	-0.004 (0.009)	-0.226 (2.589)	1.417*** (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. Data sources: NIBRS (monthly, 2010-2015), CDC (monthly, 2010-2015), and HCUP (quarterly, 2006-2015). In this table, we examine different dates for five states: 5/2013 for CO, 10/2012 for CT; 8/2015 for LA; and 4/2014 for ME because there were some third-party prescriptions allowed as of these dates. We also test 6/2010 for WA because a Good Samaritan Law at that time made naloxone available to individuals at risk of overdose. All regressions include: jurisdiction FEs, month of sample FEs, state-specific linear trends, police per capita (except column 3), and the following laws/regulations: Good Samaritan laws, PDMP, Doctor Shopping, Pain Clinic regulations, Physician exams, Pharmacy verification, require ID, and tamper-resistant PF. Coefficients show the effect of expanding naloxone access on arrests per million residents (columns 1 and 2), number of ER visits (column 3), deaths per 100,000 residents (columns 4 and 5), and reported crimes per million residents (columns 6 and 7). All coefficients except those in column (3) are also population-weighted.

Table A.25: Robustness to specification used in Rees et al. (2017)

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

*Notes:* \*  $p < .10$ , \*\*  $p < .05$ , \*\*\*  $p < .01$ . Standard errors are clustered by state and shown in parentheses. Data source for columns (2) through (8): CDC (2010-2015). Column (1) presents the main estimate shown in column (3) of Table 4 in [Rees et al. \(2017\)](#), which uses state-year observations from 1999-2014. Column (2) presents our estimate from column (2) of Table 4. The remaining columns present cumulative changes to our data and specification, to match those in [Rees et al. \(2017\)](#) as closely as possible. Column (3) aggregates our data (for all jurisdictions) to the state level. Column (4) further aggregates our data to the year level, using the method in [Rees et al. \(2017\)](#) by which states are considered as having broadened naloxone access if they did so at any point during the year. Column (5) converts the dependent variable to log rates. Column (6) drop observations from 2015, as [Rees et al. \(2017\)](#) does not use this data, and column (7) adds the other control variables used in [Rees et al. \(2017\)](#). Finally, column (8) uses the dates of naloxone access used in [Rees et al. \(2017\)](#), which vary slightly due to that paper's focus on naloxone access in any form (not broadened access). This different definition results in different years of naloxone access for the following states: CT (2003), CA (2008), IL (2010), WA (2010), RI (2012), CO (2013), KY (2013), VA (2013), MD (2013), and ME (2014). Coefficient in column (1) is weighted by state population as reported in [Rees et al. \(2017\)](#); coefficients in columns (2) through (8) are weighted by 2010 state population.

Table A.26: Effect of naloxone laws by baseline opioid mortality

	Q1 (low) (1)	Q2 (2)	Q3 (3)	Q4 (high) (4)	Q1 (low) (5)	Q2 (6)	Q3 (7)	Q4 (high) (8)
<b>Opioid-related deaths</b>				<b>Fentanyl-related deaths</b>				
Naloxone Law	0.022 (0.032)	-0.013 (0.021)	-0.001 (0.041)	0.013 (0.046)	0.012 (0.017)	-0.002 (0.023)	-0.028 (0.056)	0.014 (0.034)
Observations	13,896	13,896	13,896	13,824	13,896	13,896	13,896	13,824
2010 baseline	0.190	0.420	0.658	1.178	0.022	0.066	0.085	0.153

Notes: \*  $p < .10$ , \*\*  $p < .05$ , \*\*\*  $p < .01$ . Standard errors are clustered by state and shown in parentheses. Sample includes counties with any cities with population  $\geq 40,000$ . Date range: 2010-2015. Quartiles are based on 2010 opioid-related deaths for all columns. 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.