Opioid Prescribing Limits and Opioid-Related Mortality

Mallory Avery (Monash University)*

This version: April 4, 2024

Abstract

This paper examines the consequences of prescribing limits on opioid overdose mortality in the United States. Against the backdrop of a national health crisis driven by opioid overuse and addiction, this study employs an event study framework to assess the efficacy of legislation restricting opioid prescriptions as well as a theoretical framework to link this paper's findings with prior work to better understand the pathway from prescription to addiction and overdose. Analyzing temporal and spatial variations in policy implementation, the findings indicate significant reductions in fatal heroin overdoses by 37% and fatal semi-synthetic opioid overdoses by 16% within a year following the enactment of prescribing limits. No systematic changes are observed in deaths from synthetic opioids like Fentanyl. The findings suggest that the quantity of, rather than exposure to any, prescribed opioids drives addiction from legal prescribing practices.

Key Words: opioids; addiction; overdose; mortality

JEL codes: I12, I14, I18

* Avery, Monash University, Wellington Road, Clayton VIC 3800, Australia. Email: mallory.avery@monash.edu
Thanks to David Bradford, Neeraja Gupta, Kelly Hyde, Marissa Lepper, and Andreas Leibbrandt as well as participants at the NARSC, SEA, and ASHEcon annual conferences. All errors are my own.

1. Introduction

Opioid overuse and addiction have become a national health crisis in the United States, with almost 40,000 opioid-involved deaths in 2015 (Ruhm, 2018) and almost 50,000 in 2017 (Scholl et al., 2019). Beyond these deaths, opioid misuse has been connected with many societal ills, such as increased crime (Dave et al., 2021; Maclean et al., 2022), increased suicide rates (Borgschulte et al., 2018), decreased labor force participation and increased unemployment (Aliprantis et al., 2019; Harris et al., 2019; Greenwood et al., 2022b), increased foster care admissions (Gihleb et al., 2019), increased intimate partner violence (Dave et al., 2023), and even threats to democracy (Arteaga and Barone, 2023). Unlike other misused substances, such as cocaine, many cases of opioid abuse can be traced back to a doctor's prescription. While some of these prescriptions were from "pill mills" (Kaestner and Ziedan, 2019; Soliman, 2022), many came from fully well-intentioned (over-)prescribing for legitimate acute pain management (Ancona et al., 2015; Barnett et al., 2017; Meisel et al. 2019; Eichmeyer and Zhang, 2021).

This paper studies the impact of legislation that restricts the quantity of opioids that can be prescribed at one time: prescribing limits. These policies limit opioid prescriptions by primary care providers and emergency room physicians to a certain number of days, typically 7, and extensions can only happen via a new prescription by the physician. Unlike other legislation, like Prescription Drug Monitoring Programs (PDMPs) and Pill Mill bans, which aim to prevent those with Substance Use Disorder (SUD) from accessing opioids, prescribing limits seek to prevent addiction from occurring in the first place by better controlling legal and medically necessary exposure to opioids. I study the impact of this legislation on opioid overdose deaths, both overall and from three major categories of opioids: semi-synthetic opioids (e.g. oxycodone and hydrocodone), heroin, and synthetic narcotics (e.g. fentanyl).

I provide a brief conceptual framework to understand the potential implications of prescribing limits for opioid overdose deaths. One may think opioid overdose deaths must indubitably decrease following such legislation. However, there are two primary reasons to question its efficacy. First, these prescribing limits have been found to be ineffective at reducing the total amount of opioids prescribed; instead, there are more opioid prescriptions written to more people, for shorter durations (e.g. Sacks et al., 2021). Depending on the relative importance of exposure vs. supply, such prescribing limits may actually lead to increases in SUD and overdose

mortality. The second concern has to do with diversion: when fewer prescription opioids are available, people may be diverted to illegal and less safe opioids like heroin. This pattern, found strongly with the reformulation of Oxycotin (Alpert et al., 2018; Evans et al., 2019; Mallatt, 2019), could also occur here. The theoretical framework provided in this paper uses findings from the literature to understand how different elements of the prescription-to-addiction pipeline may be affected by prescribing limits, indicating different implications for overdose from different opioid categories. Combined with the empirical findings, we can then identify which elements are particularly important and which are less so, providing further evidence for how to address the ongoing opioid crisis.

This paper uses an event study framework to exploit the roll-out of prescribing limit legislation across time and space to identify the effect of prescribing limits on opioid overdose mortality. This event study framework is important for two reasons: first, it allows for dynamic effects, which the theoretical framework suggests will be important for overdoses from certain opioid classes, particularly heroin. Secondly, the use of the event study framework addresses concerns about potential bias in two-way fixed effects models (Goodman-Bacon, 2021). Exogeneity in timing comes from random variation in the legislative process across states. While I find that states that select into this policy are systematically different from states that do not, primarily in having higher death rates from synthetic narcotics like Fentanyl, I find no systematic differences in pre-legislation mortality across legislation timing for those states that ever select into the policy, indicating that the timing of treatment is essentially random within the subset of states that pass such legislation. Thus, the primary sample is the sample of states that ever pass prescribing limit laws.

I find that implementing prescribing limits decreases heroin overdoses by 37% and semisynthetic opioid overdoses by 16% within a year. There are no systematic changes in deaths from synthetic opioids such as Fentanyl following prescribing limits. These results are robust to various specifications and to controlling for other related legislation changing at the time. Finally, I consider heterogeneity by race, in line with the finding that the opioid crisis integral for the anomaly of the increasing mortality rate for mid-life non-Hispanic whites (Case and Deaton, 2015; Kochanek et al., 2016; Ruhm, 2019). However, I find that these laws are equally efficacious at decreasing mortality for both whites and non-whites.

These findings indicate that SUD is driven not by exposure to any amount of prescription opioids, but by the quantity of opioids one is prescribed at a given time. Furthermore, there is no evidence of diversion in this sample, as would be indicated by an increase in heroin and possibly synthetic narcotic deaths in the immediate aftermath of the legislation. These findings further emphasize the importance of controlling prescribing patterns of opioids, while reducing concerns that any exposure to these useful, though potentially dangerous, drugs will necessarily result in addiction. These findings also further emphasize the role of prescribers, including well-intentioned prescribers, in the opioid crisis.

Prescribing limits emerge in this work as a positive way of decreasing opioid overdose deaths, suggesting that they should continue to be used as a legislative treatment of the opioid crisis. However, it is important to recognize that these laws were not instituted in a vacuum: other interventions that had already taken place created the environment in which prescribing limits were able to be successful. PDMPs, already adopted throughout most of the US by the time prescribing limits were being put into place, prevent people from easily circumventing prescribing limits by doctor shopping, and the Oxycontin reformulation potentially changed the balance between exposure and supply when it comes to causing addiction. Thus, prescribing limits without the above interventions may have led to very different results.

This paper continues as follows: Section 2 describes the policy and related literature, and outlines the theoretical framework; Section 3 describes my data and empirical strategy; Section 4 presents the results, both for all opioid overdose deaths and split out by opioid category; Section 5 combines the supply and demand results to evaluate market-level outcomes; Section 6 discusses robustness to other legislation and heterogeneity by race; and Section 7 concludes.

2. Prescribing Limits: Policy, Literature, and Conceptual Framework

2.1 Description of Prescribing Limit Legislation

Prescribing limit legislation limits the quantity, typically measured in days, of opioids that can be prescribed at a single time in acute settings. This limit is typically set to 7 or fewer days, with variation by state and by whether the individual is a minor or an adult. Prescribing limits arose from the growing concern about how opioid prescribing patterns may have been generating the opioid addiction epidemic. Research has found that prescribing for acute care was significantly in excess of what was actually required, leading most patients to use only a portion of their

prescriptions (Pino and Covington, 2019), often keeping the remaining pills rather than throwing them away. Mundkur et al. (2019) found that, based on 2014 data, 7 days-worth of pills was adequate for most patients in primary or emergency care settings in which the target was reducing acute pain. Not only does excess prescribing lead to more opioids in the environment that could be accidentally or intentionally consumed, given, or sold to others; it also increases the probability that someone will become a long-term opioid user (Barnett et al., 2017).

As a result of this evidence that prescribers were responsible, in part, for the growth of opioid mis-use and dependence, in 2016 the CDC established guidelines suggesting that prescribing for acute pain be limited to the lowest-effective dose and shortest duration, with a general recommendation of a 3-day limit and a hard limit suggested at 7 days (Dowell et al. 2016).[1] States began establishing legislation that limited prescribing amounts to 7 or fewer days for certain prescriber types, such as primary care doctors and emergency room physicians (Davis et al. 2019). As of 2019, 24 states had prescribing limits of 7 days or fewer, with 12 of those becoming active during 2016 and 2017.[2] These laws vary in terms of the duration and amount limits, the specific types of opioids covered, whether the law applies differently to minors and adults, whether they apply only to an initial prescription, and whether there are exemptions made for surgical pain and based on professional judgement (see Davis et al. (2019) for a summary for all states with legislation passed in 2017 and earlier). Violating these laws typically puts a physician in the position of being investigated by the state's medical board, with punishments ranging from sanctions and fines to revocation of their medical license (LARA, 2019).

2.2 Literature on the Impact of Prescribing Limits

Evaluations of prescribing limits have, until now, entirely focused on the impact of these limits on prescribing practices. Most of these papers have been limited in geographic or

[1] These guidelines were more generally focused on prescribing for chronic pain, due to the risks associated with long-term opioid use. The role of acute pain prescribing limits in this recommendation was to prevent patients' long-term opioid use and dependence. The 3- and 7-day limits on opioid prescribing for acute pain were some of the only strict guidelines set in terms of dosage amounts or durations, which otherwise focused on how doctors evaluate opioid use costs and benefits in a chronic setting. Thus, while the guidelines were specifically about chronic pain, the prescribing limits regarding acute pain ended up receiving most of the attention, resulting in state legislation.

[2] As Davis et al. (2019) points out, some states as early as 1989 had laws limiting prescribing of opioids in acute settings to 30 days. Given how early and loose these limits are compared to the rest of the sample, I will not be including them in the analysis and treating them as un-treated states.

practitioner scope and have found mixed outcomes (Sun et al., 2017; Lowenstein et al., 2019; Kim et al., 2023; Allen et al., 2024). However, the two comprehensive papers in this area, Cramer et al. (2021) studying Medicare beneficiaries and Sacks et al. (2021) studying opioid naïve patients, find decreases in the quantity of opioids prescribed per prescription, primarily in the number of days for which the prescription is written: a decrease of 1.75 days in the former and 0.477 days in the latter. Sacks et al. (2021) also finds a decrease of prescriptions of more than 7 days by 21%, in line with the policy requirement of the modal law.

Despite these clear indications that the policy is effective and is being followed, at least some of the time, there is evidence that prescribing limits further change prescriber behavior. Sacks et al. (2021), in their sample of opioid naïve patients, also find that prescribing limit legislation leads to a 0.12 percentage point increase in the rate of prescribing for prescriptions lasting less than 7 days, leading to an 8.7% increase in the chance that an opioid naïve patient is exposed to opioids. The authors argue that their findings are consistent with opioid prescriptions seeming safer to prescribers, so long as they are within that 7-day limit, rather than other potential explanations.

2.3 Conceptual Framework

In this section, I outline the ways in which changing initial prescribing lengths may impact overdose mortality from a variety of types of opioids, in the face of the current research on the impact of prescribing limits on prescribing practices. This contributes to a literature that has sought to model drug addiction both individually and societally (e.g. Becker and Murphy, 1988; Strulik, 2021; Greenwood et al., 2022a). While this paper does not propose a unique theoretical framework, it instead uses predictions based on prior empirical literature and the responses of different opioid classes to the prescribing limits to better understand various pathways of addiction specific to the prescribing process, specifically triggering addiction through prescribed opioids and moderating the supply of opioids on the market. A summary of the predicted directions of effect through these various channels can be found in Table 1.

2.3.1 Triggering Addiction through Prescribing

Opioid addiction has been strongly associated with legitimate prescribing practices both anecdotally and in the literature (Eichmeyer and Zhang, 2022 & 2023). However, the findings of Sacks et al. (2021) indicate that multiple dimensions of prescribing may be important. Specifically, they find that while prescribing limits decrease the quantity of opioids prescribed at any given

time, they also increase exposure to opioids among opioid naïve patients. Thus, the impact of opioid prescribing limits on mortality through the pathway of triggering addiction will depend on the balance of the effect of exposure vs. quantity: if exposure to any quantity of opioids is what triggers addiction, then prescribing limits will increase addiction and thus mortality from overdose; if instead quantity is the important factor, prescribing limits should decrease overdose mortality (Shah et al., 2017). Either dynamic should be present similarly for semi-synthetic opioids and for heroin, though any changes for heroin should be delayed, as those who become or are prevented from becoming addicted in the prescribing process will take time to filter through to using heroin.

Table 1: Changes in Mortality of Predicted Channels by Opioid Class

		Expected response to prescribing limits:		
		Semi-Synthetic	Heroin	Synthetic
If addiction is triggered by:	Exposure	Increase	Increase (Delayed)	Increase (Delayed)
	Quantity	Decrease	Decrease (Delayed)	Decrease (Delayed)
If mortality is exacerbated by:	Availability	Decrease	--	--
	Diversion	--	Increase	Increase

2.3.2 Availability of Opioids, Legal and Illegal

Opioid prescribing practices are related to both the availability of opioids for the intended patient, as well as for the black market (e.g. Soliman, 2022; Bradford and Lozano-Rojas, 2023). Providing a large quantity of opioids at a given time increases the risk of the intended patient or a family member, such as a child, accidentally overdosing, which would contribute to the mortality rate for semi-synthetic opioids. However, overprescribing opioids also can perpetuate addiction and mortality beyond the intended party, as most people with leftover opioids do not dispose of them and they instead may make their way into the hands of others, either through being sold, given away, or stolen (Han et al., 2017; Lipari and Hughes, 2017). Central to these pathways is the presence of excess pills, rather than just the total number of pills prescribed. Thus, while I cannot disentangle whether overdose from semi-synthetic opioids like hydrocodone or oxycodone resulted from prescribed or illegally obtained pills, the evidence similarly predicts that if availability is important for overdose mortality from this class of pills, prescribing limits should result in a decrease in semi-synthetic opioid overdose through limiting the excess opioids in the market.

Diversion to heroin is a major concern when discussing any opioid-restricting policies. The literature has found evidence of shifts to heroin in response to both PDMPs (Mallatt, 2019) and the Oxycontin reformulation (Alpert et al., 2018; Evans et al., 2019; Mallatt, 2019; McInerney, 2019; Powell et al., 2019; Graham et al., 2023), as well as from individual pill mill closures (Soliman, 2022). We should expect any diversion to happen fairly quickly, as people who are addicted become unable to access opioids through legal channels. However, there is strong reason to doubt that prescribing limits will have any impact on diversion to heroin, given the current policy environment. PDMPs, particularly mandatory PDMPs (mPDMPs), have been widely adopted and implemented by this point, already making it difficult for people to get prescription opioids for misuse.

2.3.3 What about Fentanyl?

Fentanyl, and other fully synthetic opioids, have rapidly become an important dimension of the opioid crisis since 2012 (Moore et al., 2023). While initially a class of prescribed opioids, they have largely been removed from outpatient prescribing given their high overdose rate – they are 50 times more potent than heroin (Miller, 2020; Moore et al., 2023). Thus, any synthetic opioids outside of hospitals in our sample period are coming through illegal channels. These opioids are easily manufactured by non-experts and are suspected to be imported from outside the US (Miller, 2020; Moore et al., 2023). While some people do intend to take Fentanyl for its potency (Mars et al., 2019), many people are exposed to it unintentionally (Unick and Ciccarone, 2017). Drug dealers cut Fentanyl into other drugs because it is much cheaper and much more addictive than other drugs (Miller, 2020). While this has primarily been studied in the case of adulterating other opioids, like oxycodone and hydrocodone pills (Mars et al., 2019), Fentanyl has also been found in non-opioids such as cocaine, cannabis, and methamphetamines (Nolan et al., 2019; Hopwood et al., 2020; LaForge et al., 2022; Bazazi et al., 2024). Fentanyl and other synthetic opioids are thus less predictable given the conceptual framework laid out in this section – anything that changes addiction to or overdose from other classes of opioids should matter for Fentanyl, both through the direct use of Fentanyl and the indirect consumption of Fentanyl through adulterated opioids; however, the fact that Fentanyl is also found surreptitiously in other classes of drugs otherwise unassociated with opioids and depends so much on external supply factors (Zoorob, 2019) suggests that Fentanyl may not respond to opioid prescribing limits like either semi-synthetic opioids or heroin.

3. Data and Methodology

3.1 Sources of Data

Data on prescribing limits was collected from the NCLS Injury Prevention Legislation Database, Opioid Abuse Prevention section. This online database collates state laws related to opioid abuse prevention and makes them searchable based on law type. For this paper, laws related to prescribing guidelines/limits were selected. Then, each law that either passed, failed, or was adjourned to that state's next congressional session were found. I went through the laws for each state and found states that passed laws with limits on how many days' worth of opioids could be prescribed. I then found the date the limits were to be enacted and verified those with external sources as much as possible. These were then checked against Davis et al. (2019) where applicable, though their review only reports laws passed through 2017 and includes longer (30 and 31 day) limits not included in this paper's analysis. They also include limits that are more restrictive in to which prescribers these limits apply – this paper only considers laws in which at least emergency room physicians are held to these restrictions. Appendix Table A.1 presents the limits used in this paper, including the date of enactment, whether it was a 7-, 5-, or 3-day limit, and whether there was an accompanying morphine milligram equivalents limit.

Data on mortality comes from the National Vital Statistics Individual Level Mortality Files from 2013-2018. From here I collected death counts from opioids in general (T40) as well as particular types of opioids: heroin (T40.1), semisynthetic opioids such as Oxycodone and Hydrocodone (T40.2), and synthetic opioids such as Fentanyl (T40.4) (Packham, 2019). I have this mortality at the state-by-month level from 2013-2018.

The American Community Survey, accessed through IPUMS, provides year-by-state data used for controls: the percent of the population living in a metropolitan area, the percent of the population categorized as young (18-40), middle age (41-65), and older (65+), the percent of the population that is white, the percent of the population with health insurance, the percent of the population with less than high school education, high school education, or more than high school education, the percent of the population unemployed and not in the labor force, the percent of the population with a disability, the percent of the population that is a veteran, and the percent of the population living below the poverty line. Year-by-state population data was downloaded from the United States Census Bureau page "Annual Estimates of the Resident Population for the United

States, Regions, States, and Puerto Rico: April 1, 2010 to July 1, 2018". This population data was used to create mortality rates from the counts data from the CDC.

3.2 Methodology

To study the impact of prescribing limits on opioid overdose mortality, I use an event study framework. The event study framework is superior to a two-way fixed effects (or staggered difference-in-difference) framework in this environment for two main reasons. The first is that the recently outlined concerns about bias when difference-in-differences with staggered treatment are obviated under an event study framework (Goodman-Bacon, 2021). The second reason is that the proposed effects of the policy are not necessarily immediate or uniform over time across the different classes of opioids. For example, if the policy results in a decrease in the number of new addictions, we would expect to see a delayed decrease in overdose deaths from heroin, rather than an immediate one. Furthermore, concerns such as diversion from legal to illegal opioids may be more pronounced immediately after the policy change and then trending towards zero with time.

The main sample of interest is the sample of all states that had laws on the books to implement prescribing limits as of 2019. Such laws could have been implemented before, during, or after 2019. 2019 is used as a cut-off because the COVID-19 pandemic changed legal priories, the legislative process, and the opioid environment in ways incompatible with the previous environment. As the mortality data runs through 2018, this methodology provides a sample of not-yet-but-would-be treated states that ensures a better comparison group for the event study framework. Furthermore, by only considering the states that ever did or would get a prescribing limit, we have a more similar comparison group than the entire sample of states – as Davis et al. (2019) points out, there is substantial geographic variation in the states that do vs. do not adopt prescribing limits. As demonstrated in Appendix Table A.2, while there are substantial differences in treated vs. never-treated states, particularly in opioid overdose mortality rates, these numbers are much more similar for the treated vs. not-yet-but-would-be treated states. Furthermore, one would want to ensure that the timing of the policy implementation is not predicted by or predictive of something in the dataset. Again, in Appendix Table A.2, we can see little differences between the states that implement prescribing limits in 2016, 2017, and 2018. Appendix Table A.3 also shows that the timing of prescribing limit implementation is not predicted by opioid overdose rates prior to 2016.

Specifically, the specification used here is:

$$y_{st} = \alpha + \theta_s * \gamma_y + \theta_s * \gamma_m + \sum_{k=-13}^{13} \tau_k treated_s 1(t - t* = k) + \epsilon_{st} \quad (1)$$

where y_{st} is the mortality rate in question for state s in time period t, where time period t is a continuous measure across months and years. $\theta_s * \gamma_y$ and $\theta_s * \gamma_m$ are state-by year and state-by-month-of-year fixed effects, respectively.[3] $1(t - t^* = k)$ are event year dummies, which are equal to 1 when the year of observation is -13 (or earlier), -12, … ,12, 13 (or later) months from the initial treatment month, and $treated_s$ is a binary variable equal to 1 if state s ever gets treated in 2016-2017. The coefficients of interest are $\tau_k - \tau_{-13}$ to τ_{-1} describe the evolution of the outcome variable in the months prior to treatment and provide an analysis of pre-trends; τ_1 through τ_{13} describe the difference in outcomes between treated and untreated states however many months after treatment.[4] All regressions are population weighted and only include states that do not have 0 deaths or omitted values for the entire time period, which runs from 2013-2018. In all regressions standard errors are clustered at the state level.

4. Results

4.1 Semisynthetic Opioids (e.g. Oxycodone, Hydrocodone)

In Table 1, I lay out the predicted effects of prescribing limits on semisynthetic opioid overdose deaths through three potential mechanisms: (1) if pure exposure to these opioids is important for triggering addiction, and thus increasing the chance of overdose, we should expect to see an increase in overdose deaths through prescribing limits, as the prescribing limits have been found to be associated with more prescriptions to opioid naïve patients; (2) if the intensity of exposure, rather than exposure itself, matters more for triggering addiction, we should instead

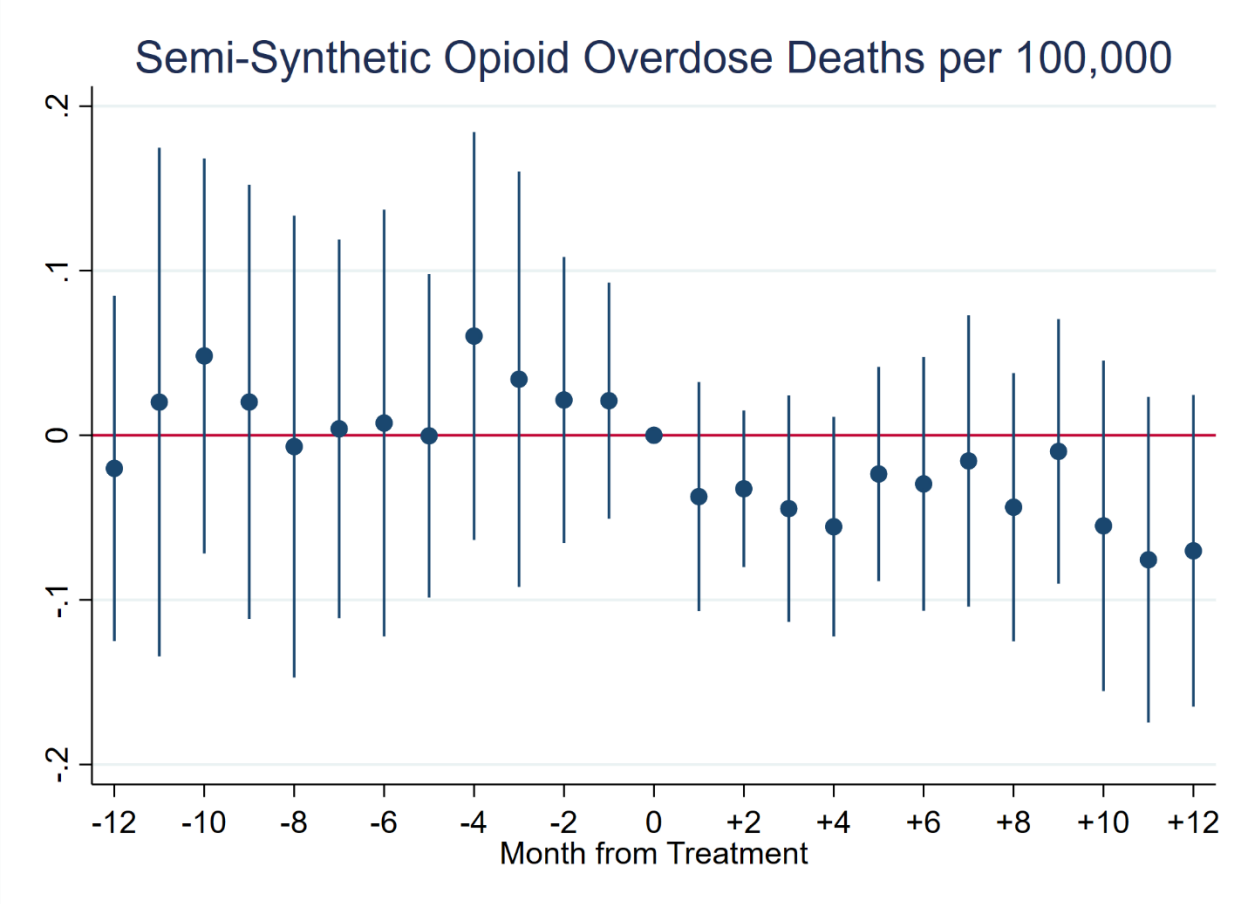
[3] By using state-by-year fixed effects, there is less concern for the effect of other policies related to Opioid use or abuse that occurred not within the year before or after the Opioid prescribing limit was implemented. Results with state specific linear time trends, rather than state-by-year fixed effects will be provided in the appendix. In regressions with state-specific linear time trends, the regression also includes the following state-by-year controls: the percent of the population living in a metropolitan area, the percent of the population categorized as young (18-40), middle age (41-65), and elderly (65+), the percent of the population that is white, the percent of the population with health insurance, the percent of the population with less than high school education, high school education, or more than high school education, the percent of the population unemployed and not in the labor force, the percent of the population with a disability, the percent of the population that is a veteran, and the percent of the population living below the poverty line.

[4] k=0 is omitted and other coefficients are interpreted relative to the month of treatment.

expect to see a decrease in semisynthetic opioid overdose deaths, as fewer people are prescribed large amounts of opioids; (3) if the availability of excess semisynthetic opioids is important for overdose risk, then we should expect to see a decrease in overdose deaths due to the smaller pool of excess opioids that can either be accidentally or intentionally misused.

Figure 1 presents the event study results from the specification in equation 1. There is a clear drop in the level of overdose deaths from semi-synthetic opioids starting immediately after prescribing limits are implemented that is sustained at a similar level throughout the study period. Also, we can see no evidence of pre-trends, indicating that prescribing limits are not merely coming into states where a drop in semisynthetic opioid overdose deaths is already occurring for some other reason.

Figure 1: Event Study Results: Semisynthetic Opioid Overdose Deaths



Notes: The figure represents results of Equation 1 for Semisynthetic opioid overdose deaths per 100,000 people. Bars present 95% confidence intervals. 0 presents the month that the prescribing limit took effect, negative values are months prior to the prescribing limits being implemented, and positive values are months post prescribing limit implementation.

Table 2: Regression Results, Overdose Deaths per 100,000

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Semi-Synthetic		Heroin		Synthetic		All	
13+ months prior	0.011	-0.054	0.018	-0.060	-0.0194	-0.313**	-0.063	-0.265*
	(0.048)	(0.042)	(0.078)	(0.067)	(0.210)	(0.132)	(0.224)	(0.150)
10-12 months prior	0.017	-0.040	0.011	-0.024	-0.135	-0.186	-0.018	0.134
	(0.053)	(0.032)	(0.066)	(0.050)	(0.217)	(0.122)	(0.242)	(0.137)
7-9 months prior	0.007	-0.046	-0.020	-0.056	-0.132	-0.167	-0.063	-0.161
	(0.055)	(0.038)	(0.072)	(0.054)	(0.238)	(0.137)	(0.265)	(0.159)
4-6 months prior	0.018	-0.007	0.005	-0.029	-0.013	-0.039	0.040	-0.021
	(0.046)	(0.041)	(0.051)	(0.043)	(0.238)	(0.132)	(0.195)	(0.153)
1-3 months prior	0.018	0.007	0.020	-0.000	0.009	-0.019	0.051	0.009
	(0.037)	(0.036)	(0.040)	(0.035)	(0.094)	(0.078)	(0.122)	(0.107)
1-3 months post	-0.050*	-0.053**	-0.069*	-0.065	-0.008	-0.017	-0.059	-0.067
	(0.026)	(0.024)	(0.036)	(0.035)	(0.059)	(0.064)	(0.066)	(0.076)
3-6 months post	-0.044	-0.056**	-0.070	-0.062	-0.019	-0.070	-0.074	-0.116
	(0.028)	(0.026)	(0.045)	(0.041)	(0.088)	(0.083)	(0.098)	(0.087)
7-9 months post	-0.075	-0.063**	-0.136***	-0.095***	-0.053	-0.107	-0.142	-0.178*
	(0.038)	(0.026)	(0.045)	(0.030)	(0.127)	(0.112)	(0.137)	(0.095)
10-12 months post	-0.075	-0.102***	-0.140**	-0.092*	-0.058	-0.080	-0.170	-0.178
	(0.044)	(0.036)	(0.057)	(0.049)	(0.182)	(0.135)	(0.197)	(0.137)
13+ months post	-0.085*	-0.079***	-0.187***	-0.142**	-0.064	-0.003	-0.202	-0.132
	(0.047)	(0.023)	(0.055)	(0.064)	(0.160)	(0.150)	(0.181)	(0.113)
State-By-Year FE	Y	N	Y	N	Y	N	Y	N
State Specific Linear Time Trends	N	Y	N	Y	N	Y	N	Y
N	1872	1872	1872	1872	1872	1872	1872	1872
R^2	0.858	0.825	0.902	0.848	0.947	0.905	0.923	0.884

Notes: Each column represents an individual OLS regression. The omitted variable is the month of implementation. Each regression includes all states that would ever be treated, including after our data is completed. Each regression only includes those states that have any month with non-zero values. Odd columns present the specification with state-by-year FE as outlined in Equation 1. Even columns present a specification with state-specific linear time trends with additional state-by-year level controls for the

percent of the population living in a metropolitan area, the percent of the population categorized as young (18-40), middle age (41-65), and elderly (65+), the percent of the population that is white, the percent of the population with health insurance, the percent of the population with less than high school education, high school education, or more than high school education, the percent of the population unemployed and not in the labor force, the percent of the population with a disability, the percent of the population that is a veteran, and the percent of the population living below the poverty line. Significance levels are *** p<0.01, ** p<0.05, * p<0.1.

Table 2 presents these results at a higher level of time-aggregation, which shrinks the variability in the outcome measure and thus the standard errors. In Column 1, we see that our primary specification with state-by-year fixed effects indicates a drop of 0.05-0.08 deaths per 100,000 in every time period after the implementation of the policy. A secondary specification, instead using state-specific linear time trends, presents very similar results. These decreases are equivalent to a decrease of 10-16% of the pre-2016 mean.

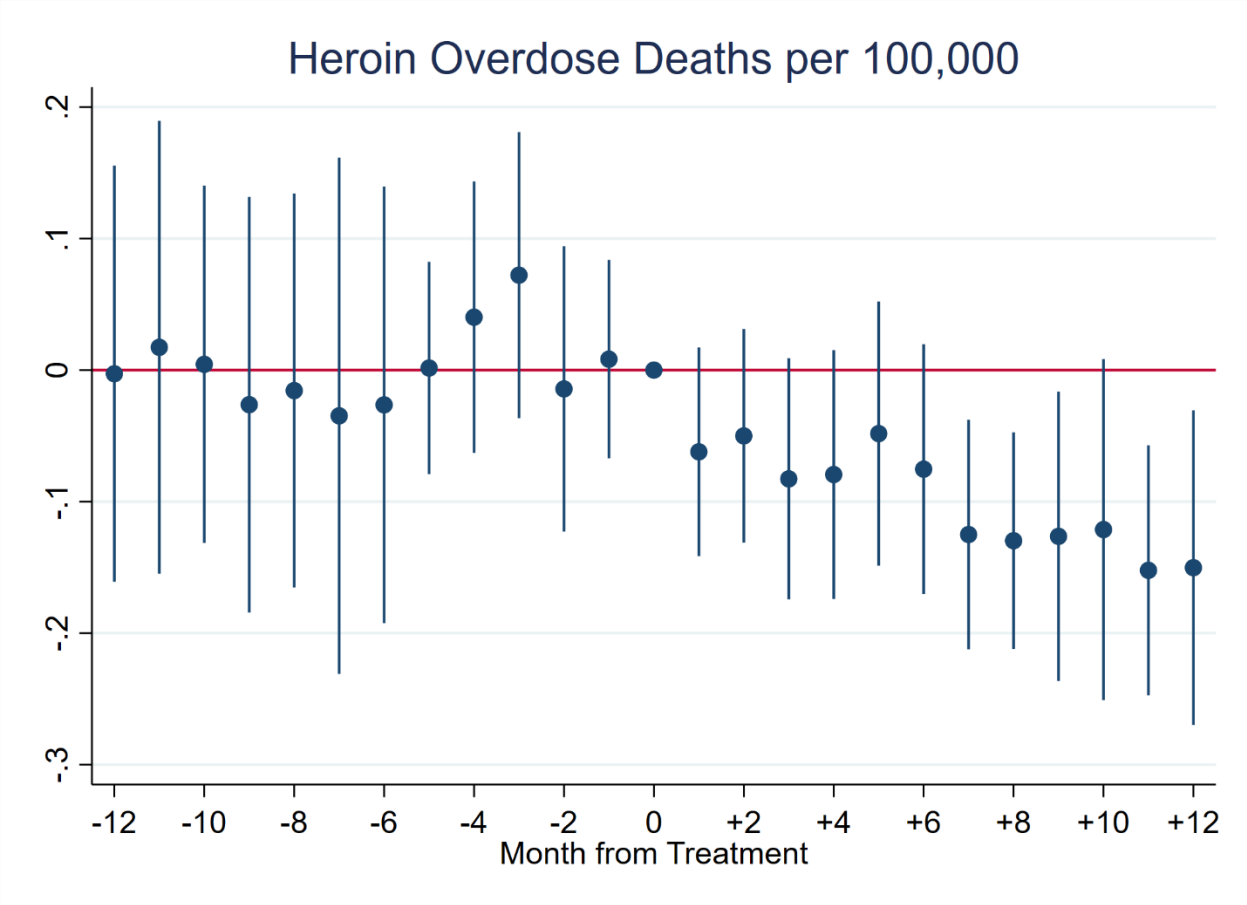
4.2 Heroin

The predictions set forth in Table 1 for heroin follow a similar pattern to that of the semi-synthetic opioids in that there are competing mechanisms that could drive outcomes in different directions. First, the patterns for those mechanisms causing addiction, specifically the pure exposure to and intensity of exposure to opioids, follow the expectations for semi-synthetic opioids, but delayed. The reason for the delay is that people who develop an addiction through legally prescribed opioids will first be using their legitimately prescribed opioids before turning to heroin. However, we would still expect to see that if pure exposure is most important, then prescribing limits should result in greater heroin overdose mortality, whereas if intensity of exposure is more important, we should see a decrease in deaths from heroin overdose. When considering heroin, however, we need to also consider diversion as a possible implication of opioid-access-restricting legislation. If prescribing limits lead to diversion, we should expect to see immediate and sustained increases in mortality from heroin overdose as people lose access to semisynthetic opioids either through prescription or through illicit markets.

Figure 2 presents the event study results for heroin, which are then reinforced by Columns 3 and 4 of Table 2. We see that heroin overdose deaths seem to first decrease by about 0.07 per 100,000 population for the first 6 months, though these effects are small and not stably significant at conventional levels. A much larger and statistically significant drop of 0.09-0.19 deaths follows

after 6 months and is consistent for the remainder of the study period. This decrease is equivalent to a 24-50% decrease in heroin overdose mortality per month after about a 6-month delay.

Figure 2: Event Study Results: Heroin Overdose Deaths



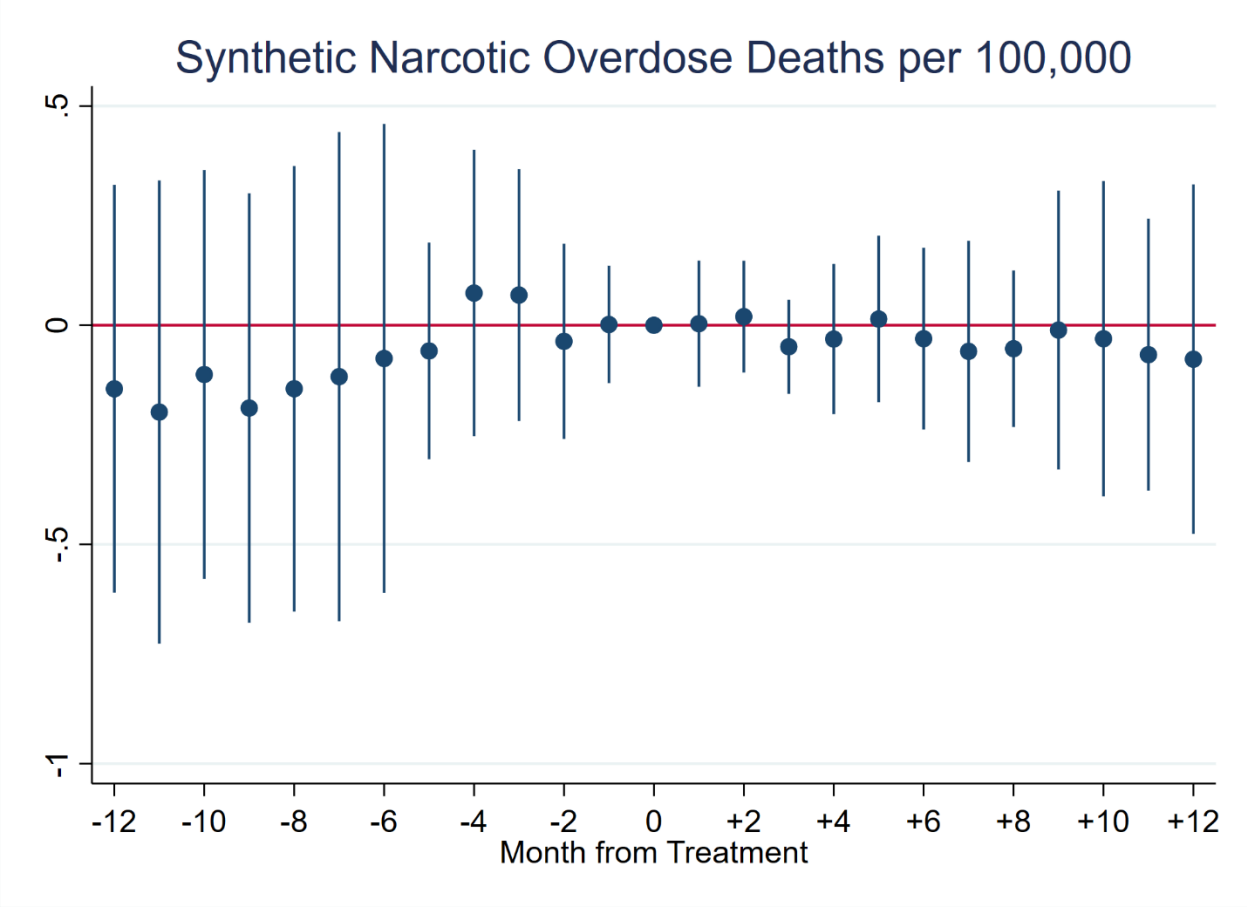
Notes: The figure represents results of Equation 1 for heroin overdose deaths per 100,000 people. Bars present 95% confidence intervals. 0 presents the month that the prescribing limit took effect, negative values are months prior to the prescribing limits being implemented, and positive values are months post prescribing limit implementation.

4.3 Synthetic Opioids

Turning to synthetic opioids, the predictions set forth in Table 1 follow those of heroin, with the caveat that we should only expect similar patterns if the driving forces behind synthetic opioid addiction and overdose are similar to those of heroin. However, in section 2.3.3, I outline that there is mounting evidence that while some demand-side factors may be similar between the two categories of opioids, synthetic opioids seem to be significantly affected by different supply-side factors that may result in very different behavior patterns.

Figure 3 and Columns 5-6 of Table 2 indicate that heroin and synthetic opioid overdose do not follow similar patterns in response to prescribing limits. Specifically, there seems to be no response to the prescribing limits in terms of synthetic overdose mortality, either immediately or delayed. This suggests that the supply-side factors discussed in the recent literature on Fentanyl and other synthetic opioids outweigh demand-side factors common to other illicit opioids like heroin.

Figure 3: Event Study Results: Synthetic Opioid Overdose Deaths



Notes: The figure represents results of Equation 1 for synthetic opioid overdose deaths per 100,000 people. Bars present 95% confidence intervals. 0 presents the month that the prescribing limit took effect, negative values are months prior to the prescribing limits being implemented, and positive values are months post prescribing limit implementation.

4.4 Section Discussion

In this section, we have seen three distinct patterns of response to prescribing limits, varying in predictable and suggestive ways based on the category of opioid being considered. First,

we can see that prescribing limits cause a decrease in mortality from both semisynthetic opioids and heroin, with the latter being delayed. This decrease indicates that the intensity of exposure to initial prescriptions of opioids is more important for triggering addiction and subsequent mortality than any exposure. This conclusion is in line with medical literature indicating that the quantity of opioids per prescription is highly correlated with continued long-term use (Barnett et al., 2017; Shah et al., 2017).

Second, the results indicate no sign of significant diversion to illicit opioid sources. It is important to clarify, however, that prescribing limits have been passed in an already crowded legislative space: PDMPs, pill mill bans, and the Oxycontin reformulation had already been put into practice in almost all of these states prior to the implementation of prescribing limits. Thus, it would be difficult, in this legislative environment, to have been able to continue an opioid addiction through prescription medications for an extended period, and much of the diversion of those addicted to prescription pills to heroin had already likely occurred before prescribing limits were ever enacted. Therefore, it seems likely that much more diversion to illicit, and more deadly, opioids would have happened if prescribing limits had been enacted without these other pieces of protective legislation already in place.

Synthetic narcotics do not respond at all to the passing of prescribing limits. As the channel from prescribing limits to synthetic narcotic use would be primarily through demand-side factors, (i.e. causing opioid addictions,) it thus stands to reason that supply-side factors are much more important when understanding synthetic narcotic addiction and overdose mortality.

Altogether, the results suggest that overall opioid overdose mortality should be reduced through the implementation of prescribing limits, though this measure will be watered down by the presence of unresponsive categories like synthetic narcotics. This reduction is exactly what we see. Appendix Figure A.1 and Columns 7-8 of Table 2 indicate a decrease in mortality from any opioid overdose after prescribing limits that reaches about 0.2 saved lives per 100,000 people, or 18%, per month, though this is measured imprecisely and does not reach conventional levels of significance. This emphasizes both that considering different classes of opioids by their access points is important in understanding the effects of opioid prescribing limits, and that a large portion of opioid use and overdose is coming through less movable categories, like synthetic narcotics.

5 Robustness and Heterogeneity

5.1 Robustness: Competing Policy Changes

Prescribing limits are just one of many policy tools that have been leveraged in the past decade to attempt to combat the opioid epidemic. In this section, we consider the two policies with the greatest temporal overlap with the implementation of prescribing limits in order to address whether the decreases in opioid overdose deaths we are attributing to prescribing limits may actually be arising from other policies: mPDMPs, which sought to prevent people from being prescribed or accessing excessive amounts of prescription opioids, and Good Samaritan Laws, which allow civilians to carry Naloxone in order to treat opioid overdose.[5] The timing of mPDMP laws come from Mallatt (2019) and Earlywine et al. (2020), while the timing of Naloxone/Good Samaritan Laws come from Doleac and Mukherjee (2019) and LAPPA (2020).

While these laws have similar mechanisms of action and overlapping implementation timing to that of prescribing limits, there are three primary reasons why we would not expect that the findings thus far are due to either or both laws. First, prescribing limits were not bundled with these other pieces of legislation but were rather a response to new CDC guidelines that were shared in 2016. In reading both the CDC guidelines and the legislation enacting prescribing limits in the various states, they are clearly specifically geared towards prescribing limits and do not also encompass either mPDMPs or Good Samaritan Laws. Second, the state-by-year fixed effects framework used in Table 2 should absorb any changes to state policy that happened outside the year in which prescribing limits were implemented. Third, there does not appear to be a strong correlation between the implementation of prescribing limits with the implementation of either mPDMPs (0.20, p=0.40) or Good Samaritan Laws (0.22, p=0.32).[6]

Appendix Table A.4 confirms that the findings have not been due to these other opioid-related laws. Controlling for the co-existence of mPDMPs or Good Samaritan Laws (even rows) results in very similar patterns in opioid overdose mortality from semi-synthetic opioids and heroin as is found without controlling for those additional pieces of legislation (odd rows).

5.2 Heterogeneity by Race

[5] Other laws of concern, such as non-mandatory PDMPs, Pill Mill laws, and changes to Medicaid have no overlap within a year of any treated state, and typically were implemented in most if not all states before any opioid prescribing limiters were implemented.

[6] However, there is some evidence that the implementation of mPDMPs and Good Samaritan Laws may be weakly correlated (0.29, p=0.10).

Previous work has found that the opioid epidemic is not equally distributed: non-Hispanic Whites seem to be disproportionately associated with opioid overdose mortality (Case and Deaton, 2015; Ruhm, 2019). Various explanations have been put forward for this phenomenon, ranging from economic explanations such as labor market stagnation (e.g., Pierce and Schott, 2020) to differential prescribing patterns for White and non-White patients (e.g., Anderson et al., 2009; Singhal et al., 2016). Given the higher impact of the opioid epidemic on White individuals, one may predict that opioid prescribing limits would have a differentially large impact on Whites over non-Whites, particularly if differences in prescribing patterns were driving these racial disparities.

Appendix Table A.5 shows that this is not the case: there are no significant differences in the change in opioid overdose mortality between White and non-White individuals after the implementation of prescribing limits. This suggests that more excessive prescribing among Whites than non-Whites is unlikely to explain the racial gap in the opioid epidemic, as both groups seem to benefit similarly from reducing that excessive prescribing.

6. Conclusion

The opioid epidemic has killed over 645,000 people in the United States since 1999 (CDC, 2023). In that time, many attempts were made to control the spread of this epidemic, with a focus on limiting access to opioids by those who are already misusing them (i.e. through mPDMPs and Pill Mill laws). However, these methods generate the problem of diversion – pushing those who are already addicted away from relatively safer prescription opioids to illegally sourced opioids like heroin. A decade of research has shown that while these policies can have some positive impacts, they often lead to unintended consequences as more people are pushed towards illicit opioids and criminal activity.

This paper considers instead a legislative attempt to prevent opioid addiction, rather than to restrict access to opioids for the already addicted. Prescribing limits are based on the premise that much of the opioid epidemic started in doctor's offices, with well-intentioned but excessive prescribing of opioids for acute or post-operative pain. Through the analysis of the outcome of prescribing limits on opioid overdose deaths, we can both study whether this policy tool is effective in reducing mortality and better understand the mechanisms driving opioid overdose mortality in the first place.

Prescribing limits are found to be effective at reducing opioid overdose mortality for both semi-synthetic opioids, such as oxycodone and hydrocodone, and heroin, indicating that this legislation has improved mortality outcomes in the opioid epidemic. These results are robust to different specifications and controlling for other interventions, and do not differ by race despite ample evidence that the opioid epidemic has disproportionately hit White Americans. There is no evidence that prescribing limits have any impact on deaths from synthetic opioids, such as Fentanyl; this is in line with previous research suggesting that synthetic opioid use and overdose are more related to supply-side factors (Zoorob, 2019). Thus, as the opioid epidemic shifts further towards synthetic opioids, prescribing limits would likely have less impact than they do in this sample.

Using the patterns of responses to the prescribing limits for different classes of opioids, one can start to investigate the underlying mechanism of addiction that stems from prescription opioid prescribing. Using the framework in Table 1, along with the findings of Sacks et al. (2021) which show that prescribing limits decrease the amount of opioids prescribed to a particular patient while increasing the number of patients to whom opioids are prescribed, we can argue that quantity of opioids prescribed, rather than exposure to any opioid prescription, drives opioid misuse and overdose. Furthermore, the delayed decrease in heroin overdose mortality indicates that these individuals who are prevented from becoming exposed to excessive amounts of opioids are then less likely to divert to heroin. The lack of evidence of diversion, which would have been seen through an initial increase in heroin overdose mortality after the prescribing limits were implemented, suggests that prior policies, such as mPDMPs and Pill Mill laws, have already pushed those who were sustaining addictions to opioids away from prescription sources.

These results suggest that opioid prescribing limits can prevent one death each from semi-synthetic opioids and heroin per 100,000 people every month. However, given the limited scope of this study, these estimates may understate the impact that prescribing limits have on the opioid epidemic: for example, Cutler and Donahoe (2024) find that the thick market externalities of opioid use indicate that changes in opioid use can have large spillovers that increase over time, which would likely not be captured during the short timespan covered by this paper; furthermore, this paper does not capture the benefits from decreased non-fatal opioid misuse, which has been associated with various societal ills such as increased crime and violence (Dave et al., 2021;

Maclean et al., 2022; Dave et al., 2023). As such, these estimates likely underestimate the societal benefits of preventing addiction through prescribing limits.

References

Aliprantis, Dionissi, Kyle Fee, and Mark E. Schweitzer. "Opioids and the Labor Market." (2019).

Allen, Lindsay D., Robin A. Pollini, Richard Vaglienti, and David Powell. "Opioid Prescribing Patterns After Imposition of Setting-Specific Limits on Prescription Duration." *JAMA Health Forum*, 5(1), e234731-e234731. (2024).

Alpert, Abby, David Powell, and Rosalie Liccardo Pacula. "Supply-side drug policy in the presence of substitutes: Evidence from the introduction of abuse-deterrent opioids." *American Economic Journal: Economic Policy* 10.4 (2018): 1-35.

Ancona, R., Butler, M., Beauchamp, G., Yamin, C., Winstanley, E., Hart, K., Ruffner, A., Ryan, S., Ryan, R., Lindsell, C. and Lyons, M., 2015. "ED Prescription Opioids are a Frequent Initial Exposure Preceding Addiction". *Academic Emergency Medicine* 68.2 (2015): 202-208.

Anderson, Karen O., Carmen R. Green, and Richard Payne. "Racial and ethnic disparities in pain: causes and consequences of unequal care." *The journal of pain* 10, no. 12 (2009): 1187-1204.

Arteaga, Carolina, and Victoria Barone. "Democracy and the opioid epidemic." (2023).

Barnett, Michael L., Andrew R. Olenski, and Anupam B. Jena. "Opioid-prescribing patterns of emergency physicians and risk of long-term use." *New England Journal of Medicine* 376.7 (2017): 663-673.

Bazazi, Alexander R., Patrick Low, Bryson O. Gomez, Hannah Snyder, Jeffrey K. Hom, Christine S. Soran, Barry Zevin, Michael Mason, Joseph Graterol, and Phillip O. Coffin. "Overdose from Unintentional Fentanyl Use when Intending to Use a Non-opioid Substance: An Analysis of Medically Attended Opioid Overdose Events." Journal of Urban Health (2024): 1-7.

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

Borgschulte, Mark, Adriana Corredor-Waldron, and Guillermo Marshall. "A path out: Prescription drug abuse, treatment, and suicide." *Journal of Economic Behavior & Organization* 149 (2018): 169-184.

Bradford, W. David, and Felipe Lozano-Rojas. *The Effect of a Large Prescription Opioid Diversion Event on Opioid Mortality in the US*. Available at SSRN (2023).

Case, Anne, and Angus Deaton. "Rising morbidity and mortality in midlife among white non-Hispanic Americans in the 21st century." *Proceedings of the National Academy of Sciences* 112.49 (2015): 15078-15083.

CDC. "Understanding the Opioid Overdose Epidemic". Centers for Disease Control and Prevention, National Center for Injury Prevention and Control. (2023). https://www.cdc.gov/opioids/basics/epidemic.html#:~:text=From%201999%2D2021%2C%20nearly%20645%2C000,outlined%20in%20three%20distinct%20waves.

Cramer, John D., Vidhya Gunaseelan, Hsou Mei Hu, Mark C. Bicket, Jennifer F. Waljee, and Michael J. Brenner. "Association of State Opioid Prescription Duration Limits With Changes in Opioid Prescribing for Medicare Beneficiaries." *JAMA Internal Medicine* (2021).

Cutler, David M., and J. Travis Donahoe. *Thick Market Externalities and the Persistence of the Opioid Epidemic.* No. w32055. National Bureau of Economic Research, 2024.

Dave, Dhaval M., Bilge Erten, Pinar Keskin, and Shuo Zhang. *From Addiction to Aggression: The Spillover Effects of Opioid Policies on Intimate Partner Violence*. No. w31609. National Bureau of Economic Research, 2023.

Dave, Dhaval, Monica Deza, and Brady Horn. "Prescription drug monitoring programs, opioid abuse, and crime." *Southern Economic Journal* 87.3 (2021): 808-848.

Davis, Corey S., Amy Judd Lieberman, Hector Hernandez-Delgado, and Carli Suba. "Laws limiting the prescribing or dispensing of opioids for acute pain in the United States: A national systematic legal review." *Drug and alcohol dependence* 194 (2019): 166-172.

Doleac, Jennifer L., and Anita Mukherjee. "The moral hazard of lifesaving innovations: naloxone access, opioid abuse, and crime." *SSRN Working Paper* (2019).

Dowell, Deborah, Tamara M. Haegerich, and Roger Chou. "CDC guideline for prescribing opioids for chronic pain—United States, 2016." *Jama* 315.15 (2016): 1624-1645.

Earlywine, Joel J., Scott E. Hadland, and Julia Raifman. "State-level prescription drug monitoring program mandates and adolescent injection drug use in the United States, 1995–2017: A difference-in-differences analysis." *PLoS medicine* 17.9 (2020): e1003272.

Eichmeyer, Sarah, and Jonathan Zhang. "Pathways into Opioid Dependence: Evidence from Practice Variation in Emergency Departments." *American Economic Journal: Applied Economics* 14, no. 4 (2022): 271-300.

Eichmeyer, Sarah, and Jonathan Zhang. "Primary care providers' influence on opioid use and its adverse consequences." *Journal of Public Economics* 217 (2023): 104784.

Evans, William N., Ethan MJ Lieber, and Patrick Power. "How the reformulation of OxyContin ignited the heroin epidemic." *Review of Economics and Statistics* 101.1 (2019): 1-15.

Gihleb, Rania, Osea Giuntella, and Ning Zhang. "The effect of mandatory access prescription drug monitoring programs on foster care admissions." *Journal of Human Resources* (2019): 0918-9729R2.

Goodman-Bacon, Andrew. "Difference-in-differences with variation in treatment timing." *Journal of Econometrics* 225, no. 2 (2021): 254-277.

Graham, Francis, Sonja C. de New née Kassenboehmer, Suzanne Nielsen, and Dennis Petrie. "Revisiting the OxyContin reformulation: The role of licit substitutes." (2023).

Greenwood, Jeremy, Nezih Guner, and Karen A. Kopecky. *The Downward Spiral*. No. w29764. National Bureau of Economic Research, 2022.

Greenwood, Jeremy, Nezih Guner, and Karen Kopecky. *Substance Abuse during the Pandemic: Implications for Labor-Force Participation*. No. w29932. National Bureau of Economic Research, 2022.

Han, Beth, Wilson M. Compton, Carlos Blanco, Elizabeth Crane, Jinhee Lee, and Christopher M. Jones. "Prescription opioid use, misuse, and use disorders in US adults: 2015 National Survey on Drug Use and Health." *Annals of internal medicine* 167.5 (2017): 293-301.

Harris, Matthew C., Lawrence M. Kessler, Matthew N. Murray, and Beth Glenn. "Prescription Opioids and Labor Market Pains: The Effect of Schedule II Opioids on Labor Force Participation and Unemployment." *Journal of Human Resources* (2019): 1017-9093R2.

Hopwood, Taylor, Caitlin Dowd-Green, Melissa Mason, and Rosalyn W. Stewart. "Unintentional use of fentanyl attributed to surreptitious cannabis adulteration." *Journal of the American Pharmacists Association* 60, no. 6 (2020): e370-e374.

Kaestner, Robert, and Engy Ziedan. *Mortality and Socioeconomic Consequences of Prescription Opioids: Evidence from State Policies*. No. w26135. National Bureau of Economic Research, 2019.

Kim, Yun Ji, Ian Kim, Ido Badash, Jonathan West, and Kevin Hur. "Opioid-limiting legislation and prescribing habits of otolaryngologists among Medicare beneficiaries." *Laryngoscope Investigative Otolaryngology* (2023).

Kochanek, Kenneth D., Elizabeth Arias, Brigham A. Bastian, and National Center for Health Statistics. "The effect of changes in selected age-specific causes of death on non-Hispanic white life expectancy between 2000 and 2014." *Heart disease* 1 (2017): 0-581.

LaForge, Kate, Erin Stack, Sarah Shin, Justine Pope, Jessica E. Larsen, Gillian Leichtling, Judith M. Leahy et al. "Knowledge, attitudes, and behaviors related to the fentanyl-adulterated drug supply among people who use drugs in Oregon." *Journal of substance abuse treatment* 141 (2022): 108849.

LAPPA. (2020). *Naloxone access: Summary of state laws*. Legislative Analysis and Public Policy Association. Retrieved December 17, 2021, from http://legislativeanalysis.org/wp-content/uploads/2021/03/Naloxone-Access-Summary-of-State-Laws-Final.pdf

LARA. *Michigan Opioid Laws: Frequently Asked Questions, 2019*. Ann Arbor: Department of Licensing and Regulatory Affairs and the Michigan Department of Health and Human Services (2019).

Lipari, Rachel N., and Arthur Hughes. "How people obtain the prescription pain relievers they misuse." *The CBHSQ report*. Substance Abuse and Mental Health Services Administration (US) (2017).

Lowenstein, Margaret, Erik Hossain, Wei Yang, David Grande, Jeanmarie Perrone, Mark D. Neuman, Michael Ashburn, and M. Kit Delgado. "Impact of a State Opioid Prescribing Limit and Electronic Medical Record Alert on Opioid Prescriptions: a Difference-in-Differences Analysis." *Journal of general internal medicine* (2019): 1-10.

Maclean, Johanna Catherine, Justine Mallatt, Christopher J. Ruhm, and Kosali Simon. "The opioid crisis, health, healthcare, and crime: A review of quasi-experimental economic studies." *The ANNALS of the American Academy of Political and Social Science* 703, no. 1 (2022): 15-49.

Mallatt, Justine. "Unintended Consequences of Prescription Monitoring: Policy-Induced Substitution to Illicit Drugs." *Available at SSRN 3418615* (2019).

Mars, Sarah G., Daniel Rosenblum, and Daniel Ciccarone. "Illicit fentanyls in the opioid street market: desired or imposed?." *Addiction* 114, no. 5 (2019): 774-780.

McInerney, Mark. "The Introduction of Abuse-Deterrent Opioids and Rates of Viral Infection." (2019).

Meisel, Zachary F., Nicoleta Lupulescu-Mann, Christina J. Charlesworth, Hyunjee Kim, and Benjamin C. Sun. "Conversion to Persistent or High-Risk Opioid Use After a New Prescription From the Emergency Department: Evidence From Washington Medicaid Beneficiaries." *Annals of Emergency Medicine* (2019).

Miller, Jacob N. "The war on drugs 2.0: Darknet fentanyl's rise and the effects of regulatory and law enforcement action." *Contemporary Economic Policy* 38, no. 2 (2020): 246-257.

Moore, Timothy J., William W. Olney, and Benjamin Hansen. *Importing the Opioid Crisis? International Trade and Fentanyl Overdoses.* No. w31885. National Bureau of Economic Research, 2023.

Mundkur, Mallika L., Jessica M. Franklin, Younathan Abdia, Krista F. Huybrechts, Elisabetta Patorno, Joshua J. Gagne, Tamra E. Meyer, Judy Staffa, and Brian T. Bateman. "Days' Supply of Initial Opioid Analgesic Prescriptions and Additional Fills for Acute Pain Conditions Treated in the Primary Care Setting—United States, 2014." *Morbidity and Mortality Weekly Report* 68.6 (2019): 140.

Nolan, Michelle L., Sindhu Shamasunder, Cody Colon-Berezin, Hillary V. Kunins, and Denise Paone. "Increased presence of fentanyl in cocaine-involved fatal overdoses: implications for prevention." *Journal of Urban Health* 96 (2019): 49-54.

Pierce, Justin R., and Peter K. Schott. "Trade liberalization and mortality: evidence from US counties." *American Economic Review: Insights* 2, no. 1 (2020): 47-63.

Pino, Carlos A., and Melissa Covington. "Prescription of opioids for acute pain in opioid naïve patients." *UpToDate.com* (2019).

Powell, David, Abby Alpert, and Rosalie L. Pacula. "A Transitioning Epidemic: How The Opioid Crisis Is Driving The Rise In Hepatitis C." *Health Affairs* 38.2 (2019): 287-294.

Ruhm, Christopher J. "Corrected US opioid-involved drug poisoning deaths and mortality rates, 1999–2015." *Addiction* 113, no. 7 (2018): 1339-1344.

Ruhm, Christopher J. "Drivers of the fatal drug epidemic." *Journal of health economics* 64 (2019): 25-42.

Sacks, Daniel W., Alex Hollingsworth, Thuy Nguyen, and Kosali Simon. "Can policy affect initiation of addictive substance use? Evidence from opioid prescribing." *Journal of Health Economics* 76 (2021): 102397.

Scholl, Lawrence. "Drug and opioid-involved overdose deaths—United States, 2013–2017." *MMWR. Morbidity and mortality weekly report* 67 (2019).

Shah, Anuj, Corey J. Hayes, and Bradley C. Martin. "Characteristics of initial prescription episodes and likelihood of long-term opioid use—United States, 2006–2015." *MMWR. Morbidity and mortality weekly report* 66.10 (2017): 265.

Singhal, Astha, Yu-Yu Tien, and Renee Y. Hsia. "Racial-ethnic disparities in opioid prescriptions at emergency department visits for conditions commonly associated with prescription drug abuse." *PloS one* 11, no. 8 (2016): e0159224.

Soliman, Adam. *Disrupting Drug Markets: The Effects of Crackdowns on Rogue Opioid Suppliers*. Available at SSRN (2022).

Strulik, Holger. 2021. “From pain patient to junkie: An economic theory of painkiller consumption and its impact on wellbeing and longevity.” *Journal of Health Economics*, 76 (March) (102432).

Sun, Benjamin C., Nicoleta Lupulescu-Mann, Christina J. Charlesworth, Hyunjee Kim, Daniel M. Hartung, Richard A. Deyo, and K. John McConnell. "Impact of hospital “best practice” mandates on prescription opioid dispensing after an emergency department visit." *Academic emergency medicine* 24, no. 8 (2017): 905-913.

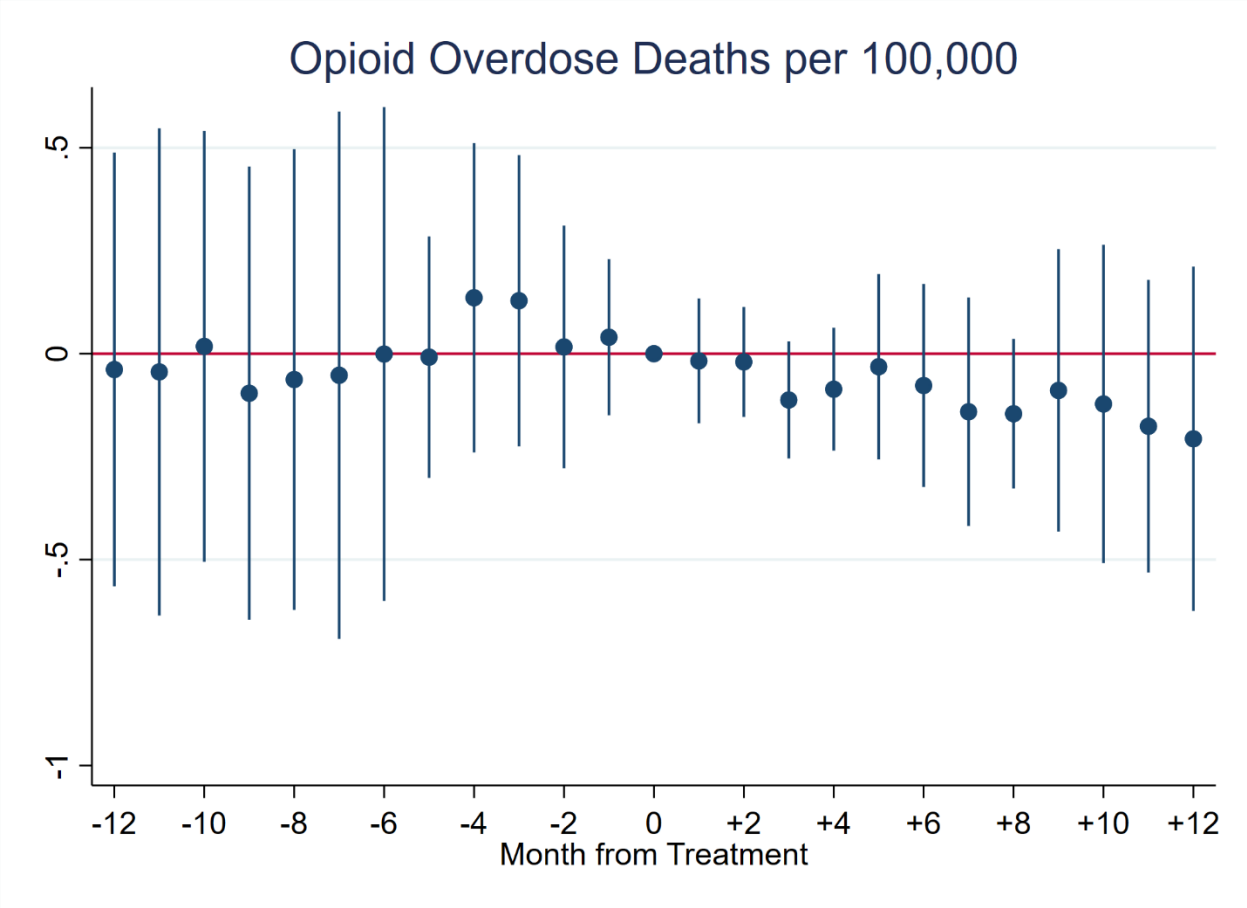
Unick, George Jay, and Daniel Ciccarone. "US regional and demographic differences in prescription opioid and heroin-related overdose hospitalizations." *International Journal of Drug Policy* 46 (2017): 112-119.

Zoorob, Michael. "Fentanyl shock: the changing geography of overdose in the United States." *International Journal of Drug Policy* 70 (2019): 40-46.

FOR ONLINE PUBLICATION

Appendix A. Supplemental Figures and Tables

Figure A.1: Event Study Results: All Opioid Overdose Deaths



Notes: The figure represents results of Equation 1 for all opioid overdose deaths per 100,000 people. Bars present 95% confidence intervals. 0 presents the month that the prescribing limit took effect, negative values are months prior to the prescribing limits being implemented, and positive values are months post prescribing limit implementation.

Table A.1: Experiment 1 Summary Statistics

State	Year Enacted	Month Enacted	Day Enacted	Limit Length	Daily MME limit
Alaska	2019	7	1	7	No
Arizona	2018	12	31	5	No
Colorado	2021	9	1	7	No
Connecticut	2018	7	1	7	No
Delaware	2017	4	1	7	No
Florida	2018	7	1	7	No
Hawaii	2017	7	1	7	No
Indiana	2017	7	1	7	No
Kentucky	2017	6	29	3	No
Louisiana	2017	7	10	7	No
Maine	2016	7	29	7	100 MME
Massachusetts	2016	3	14	7	No
Missouri	2018	7	6	7	No
Nebraska	2018	4	4	7	No
Nevada	2017	7	16	14	90 MME
New Hampshire	2017	1	1	7	No
New Jersey	2017	3	1	5	No
New York	2016	7	22	7	No
North Carolina	2018	1	1	5	No
Ohio	2017	8	31	7	30 MME
Oklahoma	2018	11	1	7	No
Pennsylvania	2017	1	1	7	No
South Carolina	2018	6	4	7	No
Tennessee	2018	7	1	3	No
Utah	2017	5	9	7	No
Virginia	2017	3	15	7	No

Notes: This table reports the dates of legislation enactment for prescribing limits by state as collected by the author from NCLS Injury Prevention Legislation Database, Opioid Abuse Prevention section and checked against Davis et al. (2019) where applicable.

Table A.2: Regressions of Completed Applicant Characteristics by Gender and Treatment

		Whole Sample		Ever-Treated Sample		Treated before 2019 Sample					
	Treated	Un-Treated	Difference	Un-Treated	Difference	Treated 2016	Treated 2017	Difference	Treated 2018	Diff from 2016	Diff from 2017
Number	24	27		2		3	12		9		
Death Rates/100,000 pop											
All Opioids	1.12	0.75	-0.37***	0.84	-0.28	1.24	1.08	0.17	1.13	0.11	-0.06
	(0.11)	(0.08)	(0.13)	(0.09)	(0.40)	(0.19)	(0.14)	(0.31)	(0.24)	(0.44)	(0.26)
Semi-synthetic Opioids	0.49	0.34	-0.16**	0.45	-0.04	0.40	0.44	-0.04	0.59	-0.19	-0.15
	(0.07)	(0.03)	(0.07)	(0.07)	(0.24)	(0.11)	(0.08)	(0.16)	(0.15)	(0.27)	(0.16)
Heroin	0.38	0.26	-0.12*	0.33	-0.05	0.43	0.40	0.03	0.33	0.10	0.06
	(0.05)	(0.04)	(0.06)	(0.06)	(0.17)	(0.11)	(0.06)	(0.14)	(0.10)	(0.19)	(0.11)
Synthetic Opioids	0.39	0.20	-0.20**	0.13	-0.27	0.63	0.38	0.25	0.35	0.28	0.03
	(0.07)	(0.04)	(0.07)	(0.02)	(0.23)	(0.19)	(0.10)	(0.23)	(0.09)	(0.19)	(0.14)
Control Variables: Percent											
Metropolitan	0.69	0.55	-0.14*	0.51	-0.17	0.75	0.72	0.03	0.62	0.13	0.10
	(0.04)	(0.06)	(0.07)	(0.31)	(0.17)	(0.15)	(0.06)	(0.13)	(0.07)	(0.15)	(0.09)
Young	0.27	0.28	0.01	0.30	0.03**	0.27	0.27	0.00	0.26	0.01	0.01
	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.00)	(0.01)	(0.00)	(0.01)	(0.01)
Middle Age	0.35	0.34	-0.01	0.33	-0.02	0.36	0.35	0.01	0.35	0.01	0.00
	(0.00)	(0.00)	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)	(0.02)	(0.00)	(0.01)	(0.01)
White	0.79	0.79	0.00	0.69	-0.09	0.82	0.76	0.06	0.81	0.01	-0.05
	(0.03)	(0.03)	(0.04)	(0.17)	(0.11)	(0.07)	(0.05)	(0.11)	(0.03)	(0.06)	(0.07)
With Health Insurance	0.91	0.90	-0.01	0.86	-0.05**	0.94	0.92	0.02	0.90	0.04*	0.02
	(0.01)	(0.01)	(0.01)	(0.06)	(0.02)	(0.02)	(0.01)	(0.02)	(0.01)	(0.02)	(0.01)
Less than HS	0.28	0.29	0.01	0.31	0.03	0.26	0.28	-0.03	0.29	-0.03*	0.00
	(0.01)	(0.01)	(0.01)	(0.04)	(0.02)	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)
HS Diploma	0.50	0.49	-0.00	0.47	-0.03	0.48	0.49	-0.02	0.51	-0.03	-0.01
	(0.01)	(0.01)	(0.01)	(0.03)	(0.03)	(0.04)	(0.01)	(0.03)	(0.01)	(0.03)	(0.01)
Unemployed	0.03	0.03	0.00	0.04	0.01**	0.03	0.03	0.00	0.03	0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Not In Laborforce	0.33	0.32	-0.01	0.27	-0.06**	0.33	0.32	0.01	0.34	-0.02	-0.02
	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)	(0.01)	(0.02)	(0.01)	(0.03)	(0.01)
Any Disability	0.16	0.15	-0.01	0.13	-0.03	0.15	0.15	0.00	0.17	-0.02	-0.02*
	(0.01)	(0.00)	(0.01)	(0.00)	(0.02)	(0.02)	(0.01)	(0.02)	(0.01)	(0.02)	(0.01)
Veteran	0.07	0.07	0.00	0.08	0.00	0.07	0.07	-0.01	0.08	-0.01	0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.01)	(0.01)	(0.00)	(0.01)	(0.00)	(0.01)	(0.01)

Below Poverty Line	0.17	0.17	0.01	0.17	-0.00	0.17	0.16	0.01	0.18	-0.01	-0.03*
	(0.01)	(0.01)	(0.01)	(0.03)	(0.02)	(0.01)	(0.01)	(0.02)	(0.01)	(0.02)	(0.01)

Notes: T-tests of equivalence of variable across different samples. Mortality data are from National Vital Statistics Individual Level Mortality Files from 2013-2018 and control variables are from the American Community Survey, accessed through IPUMS. Significance levels are *** p<0.01, ** p<0.05, * p<0.1.

Table A.3 Determinants of Rollout of Laws

Deaths from:	Treatment Month
Semisynthetic Opioids	7.66 (5.33)
Heroin	-5.49 (7.67)
Synthetic Narcotics	-7.29 (5.57)
All Opioids	-0.20 (3.36)

Notes: Each cell represents the outcome of an individual regression, regressing the death rate from the particular cause of death on the month of treatment. Significance levels are *** p<0.01, ** p<0.05, * p<0.1.

Table A.4: Regression Results Controlling for Other Legislation, Overdose Deaths per 100,000

	(1)	(2)	(3)	(4)	(5)	(6)
	Semi-Synthetic		Heroin		All Opioids	
13+ months prior	-0.053	-0.060	-0.061	-0.059	-0.265*	-0.273*
	(0.044)	(0.043)	(0.067)	(0.068)	(0.152)	(0.153)
10-12 months prior	-0.040	-0.042	-0.026	-0.028	-0.136	-0.139
	(0.033)	(0.032)	(0.049)	(0.049)	(0.138)	(0.138)
7-9 months prior	-0.045	-0.048	-0.058	-0.058	-0.163	-0.166
	(0.040)	(0.040)	(0.054)	(0.053)	(0.161)	(0.163)
4-6 months prior	-0.007	-0.008	-0.029	-0.030	-0.022	-0.024
	(0.041)	(0.041)	(0.043)	(0.043)	(0.154)	(0.155)

1-3 months prior	0.007	0.005	-0.000	0.000	0.010	0.007
	(0.036)	(0.036)	(0.035)	(0.035)	(0.107)	(0.109)
1-3 months post	-0.053**	-0.050*	-0.064*	-0.067*	-0.066	-0.063
	(0.024)	(0.027)	(0.035)	(0.033)	(0.076)	(0.076)
3-6 months post	-0.056**	-0.053*	-0.060	-0.063	-0.113	-0.110
	(0.026)	(0.028)	(0.041)	(0.040)	(0.086)	(0.090)
7-9 months post	-0.063**	-0.064**	-0.094***	-0.087***	-0.175*	-0.176*
	(0.026)	(0.027)	(0.030)	(0.030)	(0.094)	(0.095)
10-12 months post	-0.103**	-0.104***	-0.090*	-0.080	-0.175	-0.176
	(0.037)	(0.036)	(0.050)	(0.052)	(0.136)	(0.138)
13+ months post	-0.080***	-0.083***	-0.137**	-0.120*	-0.122	-0.123
	(0.024)	(0.026)	(0.064)	(0.070)	(0.109)	(0.113)
mPDMP		-0.032		0.052		-0.028
		(0.020)		(0.050)		(0.063)
Good Samaritan Law		0.009		0.017		0.012
		(0.015)		(0.022)		(0.042)
N	1728	1728	1728	1728	1728	1728
R^2	0.832	0.833	0.852	0.853	0.886	0.886

Notes: Each column represents an individual OLS regression. The omitted variable is the month of implementation. Each regression includes all states that would ever be treated, including after our data is completed. Each regression only includes those states that have any month with non-zero values. Each regression uses state-specific linear time trends with additional state-by-year level controls for the percent of the population living in a metropolitan area, the percent of the population categorized as young (18-40), middle age (41-65), and elderly (65+), the percent of the population that is white, the percent of the population with health insurance, the percent of the population with less than high school education, high school education, or more than high school education, the percent of the population unemployed and not in the labor force, the percent of the population with a disability, the percent of the population that is a veteran, and the percent of the population living below the poverty line.. Odd columns present the specification without additional controls. Even columns present specifications with additional indicators for that state in that month having a mandatory prescription drug monitory program (mPDMP) or having a Good Samaritan Law. Significance levels are *** p<0.01, ** p<0.05, * p<0.1.

Table A.5: Regression Results by Race, Overdose Deaths per 100,000

	(1)	(2)	(3)	(4)	(5)	(6)
	Semi-Synthetic		Heroin		All Opioids	
	White	Non-White	White	Non-White	White	Non-White
13+ months prior	-0.051	-0.093	-0.047	-0.100	-0.272	-0.293*
	(0.051)	(0.067)	(0.072)	(0.062)	(0.163)	(0.163)
10-12 months prior	-0.035	-0.073	-0.018	-0.027	-0.137	-0.127
	(0.036)	(0.063)	(0.050)	(0.061)	(0.145)	(0.153)
7-9 months prior	-0.052	-0.037	-0.053	-0.070	-0.174	-0.153
	(0.046)	(0.061)	(0.054)	(0.070)	(0.169)	(0.156)
4-6 months prior	-0.004	-0.019	-0.016	-0.065	-0.010	-0.100
	(0.047)	(0.060)	(0.043)	(0.063)	(0.163)	(0.141)
1-3 months prior	0.021	-0.052	0.017	-0.042	0.036	-0.095
	(0.046)	(0.061)	(0.041)	(0.054)	(0.116)	(0.164)
1-3 months post	-0.052	-0.078	-0.070*	-0.033	-0.076	-0.042
	(0.033)	(0.050)	(0.039)	(0.053)	(0.087)	(0.125)
3-6 months post	-0.056	-0.065	-0.049	-0.090*	-0.113	-0.113
	(0.036)	(0.051)	(0.046)	(0.045)	(0.101)	(0.099)
7-9 months post	-0.067*	-0.060	-0.096***	-0.082	-0.177*	-0.188
	(0.034)	(0.043)	(0.030)	(0.057)	(0.103)	(0.140)
10-12 months post	-0.106**	-0.083	-0.081	-0.080	-0.174	-0.123
	(0.047)	(0.053)	(0.058)	(0.070)	(0.151)	(0.160)
13+ months post	-0.092**	-0.028	-0.146*	-0.085	-0.149	0.030
	(0.035)	(0.045)	(0.076)	(0.051)	(0.131)	(0.112)
N	1728	1728	1728	1728	1728	1728
R^2	0.797	0.482	0.839	0.685	0.867	0.792

Notes: Each column represents an individual OLS regression. The omitted variable is the month of implementation. Each regression includes all states that would ever be treated, including after our data is completed. Each regression only includes those states that have any month with non-zero values. Each regression uses state-specific linear time trends with additional state-by-year level controls for the percent of the population living in a metropolitan area, the percent of the population categorized as young (18-40), middle age (41-65), and elderly (65+), the percent of the population that is white, the percent of the population with health insurance, the percent of the population with less than high school education, high school education, or more than high school education, the percent of the population unemployed and not in

the labor force, the percent of the population with a disability, the percent of the population that is a veteran, and the percent of the population living below the poverty line. Odd columns present results for only White mortality. Even columns present results for only non-White mortality. Significance levels are *** $p<0.01$, ** $p<0.05$, * $p<0.1$.