Understanding the opioid epidemic: How pain management policy contributed to opioid mortality

By

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A thesis submitted in satisfaction of the Honors program in pursuit of the

Bachelor of Arts

For the

Department of Economics

In the

COLLEGE OF LETTERS AND SCIENCE

At the

UNIVERSITY OF CALIFORNIA, BERKELEY

Faculty Advisor

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Spring 2019

Abstract

I analyze how opioid supply contributed to the opioid epidemic by examining the relationship between state pain management policy and opioid mortality from 1990 to 2017. I use a difference in difference model to assess if there is a statistical difference in opioid mortality between states that implemented pain management policies and states that did not. I combine medical spillover effects with a policy indicator and find that state policy has a decreasing relationship with mortality. This indicates that while pain policy may have contributed to initial abuse, it is no longer a relevant factor. I show that state medical board policies and legislation introduced before 2000 play important roles in opioid abuse. My research suggests that future health policy should be centered on reframing prescribing laws and creating interventions that encourage safe use of opioids.

Introduction

Opioid addiction has run rampant across the United States. The epidemic has a death toll of 750,000 and counting and is responsible for over 115 deaths each day (Weiss et al. 2017; CDC, 2018). Opioid abuse single handedly contributes to the declining life expectancy of middle class Americans and ranks above car accidents for the leading cause of preventable death (Case and Deaton, 2017; National Council of Safety, 2017). This crisis is a national tragedy and alarmingly, stems from addiction to opioids that are legally prescribed to treat chronic pain.

There is much we do not know about the epidemic. While supply-side factors such as pharmaceutical presence, medical spillover, and pain policy have been proposed as determinants of opioid abuse, there is little causal evidence that explains why opioid addiction proliferated (Powell et al. 2015). Moreover, there is no explanation for the significant geographic variation in opioid abuse across states. The motivation behind this paper is to pinpoint casual factors in opioid abuse that can help health policy makers and state medical boards target solutions to curb addiction.

In this thesis, I hypothesize that states with earlier and more extreme pain management laws are disproportionately affected by the epidemic and its disastrous consequences. State pain management policies in the late 1990s and early 2000s encouraged extensive opioid use as a method to treat chronic pain. Therefore, pain legislation could be the source of excess opioid supply that encouraged more severe addiction. While changes in pain management policy are widely cited as a potential cause of the epidemic, there is no current empirical analysis that tests this relationship.

I build off previous research by controlling for the effects of medical spillover and labor market conditions. Powell et al. (2015) shows that prescription spillover from Medicare eligible populations is a large source of excess opioid supply. Krueger (2015), Hollingsworth et al. (2017), and Currie et al. (2018), find that unemployment and poverty may play important roles in determining opioid demand.

I use annual state level opioid mortality from 1990 to 2017 from the Center for Disease Control and Prevention. I take advantage of policy data from the Pain Policy Studies Group at the University of Wisconsin and the Federation of State Medical Boards. This data records the year and type of pain management policy that states introduced.

I use a difference in difference model to examine dissimilarities in opioid death rates between states that implemented pain management policies and states that did not. I repeat my analysis for multiple year ranges between 1990 and 2017 and compare policies introduced before and after 2000. Throughout my analysis, I control for state and year fixed effects, medical spillover, labor market variables, and population size.

I find that states that implemented pain policies have statistically higher death rates from states that did not. However, this difference decreases over time and is not present in my overall sample. As the crisis advanced, exogenous variables such as community interventions, prescription drug monitoring programs (PMDPs), and physician awareness could have had stronger effects on death rates, eradicating the impact of state policy. States that implemented policies before 2000 have 41% higher death rates than states that implemented pain policies after 2000. State medical board policies have a particularly strong effect on opioid mortality in comparison to state recommendations and intractable pain statutes. Overall, state medical board policy and legislation implemented before 2000 had the strongest impacts on opioid mortality.

The rest of my paper is outlined as follows: Section 1 contains background information on the history of the opioid epidemic and existing literature. Section 2 describes my data, and section 3 provides my methodology and specifications. Section 4 presents descriptive statistics for my variables. Section 5 details the regression results, limitations, and a discussion of my findings. Section 6 concludes my analysis and presents potential policy implications.

1. Background Information

The opioid epidemic is just one aspect of deteriorating health measures within the United States. In a 2015 groundbreaking paper, Case and Deaton found that overall American life expectancy is decreasing. However, upon further investigation, they concluded that life expectancy is decreasing for white, non-Hispanic, middle income Americans and increasing for Americans with a college degree (Case and Deaton, 2015). Coined 'deaths of despair', these discrepancies were explained as cumulative disadvantages over the lifetime of lower class Americans (Case and Deaton, 2017). The results from these papers describe how opioid abuse is detrimental to public health. The epidemic is a source of massive social and economic disruption and places a several hundred billion dollar burden on public sector and healthcare organizations (Florence et al. 2016).

Incidences of opioid abuse began to rise in the mid 1990s and opioid related deaths have more than tripled since (Dave et al. 2018). The epidemic peaked in 2012 with a national average of 81.2 opioid prescriptions for every 100 persons (CDC, 2018). In

2018, there were 58.7 prescriptions for every 100 persons, still a 200% increase from 1999 levels (US Census Bureau, 2018). While, in recent years, the number of prescriptions per 100 individuals has decreased nationwide, many states such as West Virginia, Ohio, and Pennsylvania still witness climbing death rates.

Prior to 1990, medical practice norms and threat of legal action prohibited doctors from writing excess prescriptions. However, these cultural norms shifted due to the emergence of opioid-based pain relievers such as OxyCotin, and public concern that chronic pain was undertreated (Dave et al. 2018; Manchikanti, 2012). In response, medical boards and state legislatures dramatically liberalized prescription drug laws. These policies either protected physicians from facing legal action if their patients became addicted to drugs they prescribed or implied that undertreating chronic pain was a criminal offense (Hoffmann and Tarzian, 2007).

This medical movement declared that pain was equivalent to the ‘5th vital sign’ and should be taken as seriously as objective health measures such as blood pressure (Hoffmann and Tarzian, 2007). Simultaneously, opioid supply increased as pharmaceutical companies developed a record number of opioid-based drugs and oversaw aggressive marketing campaigns (Doctor and Menchine, 2017). Moreover, medical research during the late 1990s and early 2000s urged physicians to practice aggressive pain management and discouraged the idea that opioids have addictive qualities (Hoffmann and Tarzian, 2007).

Opioid availability continued to grow as Medicare Part D expanded prescription drug access to Medicare beneficiaries in 2006 (Chen et al. 2011). Powell et al (2015), demonstrates that states with larger 65+ populations have higher amounts of opioid abuse for both Medicare eligible and non-Medicare eligible populations. According to their publication, on average a 10% increase in prescription opioid access will increase opioid mortality by 7.4% (Powell et al. 2015). They propose that 73% of opioid deaths are attributable to prescription drug spillover effects.

Similarly, place-specific factors and migration patterns are correlated with changes in prescription drug rates. Individuals that migrate to areas with higher rates of opioid prescriptions are 6% more likely to abuse opioids, signifying that supply side constraints are important in initiating opioid misuse (Gentzhow et al, 2018).

There is significant debate as to how demand-side factors such as economic status and individual health characteristics affect opioid rates. Gentzhow et al. (2018) identifies mental health, work-related injury, and financial trouble as potential person-specific determinates that impact opioid use. There is some evidence that labor market conditions affected the epidemic. For example, Hollingsworth et al. (2017) finds that higher unemployment rates increase opioid mortality by 3.6% and opioid overdose by 7%. Deteriorating mental health could be the link between opioid overdose and macroeconomic fluctuations due to declines in mental health during periods of economic instability (Hollingsworth et al, 2017; Krueger, 2017).

Krueger (2017) shows that labor force participation is lower in counties with more opioid abuse. Contrastingly, Currie et al. (2018) uses employment-to-population ratios to examine the relationship between employment status and opioid use, finding little evidence that employment influences prescription rates. Moreover, there is building evidence that a majority of opioid users are in the labor force. This finding suggests that economic conditions are not a causal factor for opioid use (Currie et al. 2018). Overall, there is mixed research regarding how demand-side characteristics impacted the crisis.

Prescription Drug Monitoring Programs, or PDMPs, are a policy tool that states have implemented to prevent opioid misuse. PDMPs are prescription-tracking systems that attempt to reduce inappropriate prescribing and 'doctor shopping' by monitoring physician and patient data. Currently, there is limited evidence that PDMPs prevent overprescribing, as studies show that they influence only 9.5% of prescribing behavior (Griggs et all, 2015). While PDMPs could be a useful policy tool, it is unlikely that these programs initially impacted opioid abuse because this response was formulated post epidemic and prior to 2012, PDMPs did not mandate reporting (Dave et al, 2018; Horwitz et al, 2018).

Overall, pain management policy and Medicare spillover are widely cited as potential explanations for the epidemic. However, Powell et al. (2015) is the only study that uses empirical analysis to show how medical spillover increases opioid mortality. It is not known how state policy impacted opioid abuse. Therefore, I will attempt to determine a relationship between supply side determinants and opioid abuse by focusing on how state policies, in combination with Medicare spillover, affects opioid death rates.

2. Data

I construct prescription opioid death rates to analyze how state policies impacted the epidemic. Due to data limitations, opioid prescriptions were only accessible for the year 2015. Thus, I use opioid death rates from 1990 to 2017 as a measure of opioid abuse.

I obtained opioid mortality and general prescription drug mortality by state from the CDC. This dataset includes population counts, raw death counts, and mortality rates measured in deaths per 100,000 individuals. I acquired this data from the CDC Wonder database, a query system that compiles all publically available CDC health data including opioid mortality from 1990 to 2017. Death rates from 1990 to 1998 were extracted from Compressed Mortality files that code death rates by the International Classification of Diseases (ICD)-9. Deaths attributable to legal opioid use are categorized as E 850.1-E 850.2. Death rates from 1999 to 2017 were extracted using ICD-10 codes from Multiple Cause of Death files also found in the Wonder database. ICD-10 classifies opioid mortality as drug poisoning deaths coded by X40-X44, X60-X64, X85, Y10-Y14, and Y35.2, where the underlying cause of opioid related death is T40.2-T40.4.

The ICD system, a classification tool upheld by the World Health Organization (WHO), was upgraded from ICD-9 to ICD-10 in 1999, creating a difference in diagnostic codes from 1990-1998 and 1999-2017. Due to the change in classification, there is a possibility that ICD-9 opioid deaths are underestimated. To control for this discontinuity, I construct a measure of prescription drug deaths from 1990 to 2017. ICD -9 codes general prescription drug deaths under E850-E853, E854.0, E854.3, E854.8, E855.0, E855.1, E855.3- E855.6, E855.8-E857, E858.0-E858.6, E950.0- E950.3, and E980.0 - E980.3. ICD-10 codes prescription drug poisoning as X40-44, X60-64, X85, and Y10-14 with underlying cause of death as T 36-39, T40.2-T40.4, T41-T43.5, T43.8-T43.9, and T44-T50.8.

Figure (1) investigates the relationship between ICD-9 and ICD-10 by comparing mean opioid mortality to mean all prescription drug mortality by year and analyzing the discontinuities for both rates. This figure shows that all prescription drug deaths and opioid deaths are similarly affected by the ICD discontinuity, which is shown as a jump in death rates from 1998 to 1999. This jump alludes to the possibility that drug deaths prior to 1999 are underestimated. This is an unfortunate limitation of my dataset. However, since all states are equally impacted by the change in classification codes, this discontinuity is constant across states. Therefore, the change in ICD coding will not impact my regression results when I control for time fixed effects and population size.

Due to issues of confidentiality, CDC data is ‘suppressed’ if the count of deaths in a given state is less than 10 (0-9). To account for these unknowns, I assign the median (4.5) to all suppressed values. The true distribution of suppressed values is not known. While this is a limitation of my analysis, it is the best approximation available. Suppressed values appear in the early 1990s but are not present as time goes on.

I obtain data on pain management legislation from the Pain Policy Studies Group (PPSG) at the University of Wisconsin and the Federation of State Medical Boards. PPSG details all known pain management policy by state and year. To ensure accuracy, I merged this dataset with statute and legislation records from the Nexis-Uni law database[1]. My policy variable includes the year that each state passed legislation encouraging physicians to prescribe opioids as treatment for chronic pain. When the opioid epidemic came to public attention, many states eliminated their pain management policies to discourage opioid abuse. My data records these changes in legislation as well.

Table (1) summarizes the types of pain policy and the count of states that implemented each policy. Policy A describes ‘Intractable Pain Statutes’, or laws that prohibit physicians from facing disciplinary action when treating chronic pain and are

[1] I accessed the pain legislation data from PPSG. However, for unknown reasons this data is no longer available on the Internet. I compare PPSG pain policy data with policy data from the Federation of State Medical Boards and the Nexis- Uni law database to avoid any potential bias and to ensure that all legislation was accurately recorded.

known to be the type of pain policy that allows for the most opioid abuse (Hoffmann and Tarzian, 2007; Doctor and Menchine, 2017). Policy B refers to state medical board policies, which hint that undertreating chronic pain is a serious crime and medical best practice includes aggressively treating pain with substance II drugs like opioids. Policy C encompasses medical board recommendations that encourage physicians to 'take advantage' of opioid based drugs but differ in that they do not place legal restrictions on physicians.

I use this data to create several policy dummies that signify the year pain management legislation was first enacted. The first variable is a general policy dummy that turns on in the year a state initially enacted pain policy and turns off if the policy was later eliminated. The second set codes for the policy groups described in table (1) where Policy A allows for the most opioid abuse and Policy C allows for the least.

As additional controls in my analysis, I use the unemployment rate from the Bureau of Labor Statistics and the poverty rate from US Census data for each state and year from 1990 to 2017. These controls will isolate the effect that policy has on opioid mortality because they may have contributed to opioid use and could be endogenous to state policy.

To determine if opioid deaths are an appropriate proxy for opioid prescriptions, I use 2015 county level opioid prescriptions measured in the morphine milligram equivalents (MME) from Alan B. Krueger's 2017 paper "Where have all the workers gone?" Krueger obtained this data from the American Time Use Survey (ATUS-WB) and the CDC. While data limitations prevent the use of prescription data for 1990 to 2017, I can compare 2015 prescription rates to opioid death rates to evaluate if mortality serves as a valid approximation for opioid abuse.

Figure (2) uses this data to graph the relationship between opioid prescriptions and opioid mortality, weighted by state. The fitted line shows that there is a linear relationship between prescriptions and mortality. However, the fitted line is flatter than the 45 degree fit line, indicating that there are higher prescription rates than deaths. Therefore, my data can only make conclusions regarding the effect that policy has on opioid mortality, as extrapolating my findings to general opioid abuse will underestimate the effect on opioid prescriptions.

3. Methods

I test the hypothesis that states with pain management legislation have higher rates of opioid mortality. To justify that opioid death rates are an acceptable alternative to opioid prescriptions, I use data on opioid prescriptions and opioid death rates for 2015. I run the following cross sectional regression, where opioid death rates are regressed on prescription rates in state s and year t, controlling for population size, unemployment, and poverty.

(1) $Opioid\ Death\ Rate_{st} = \alpha + \beta(2015\ \ OpioidPrescription\ Rate_{S}) + \theta X'_{st} + \varepsilon_{st}$

Using this specification, I can determine if opioid prescription rates in 2015 are correlated with mortality. If the coefficient in my regression is positive and significant, it indicates that death rates track prescriptions.

To evaluate my main hypothesis, I create a difference in difference model regressing a state policy dummy on opioid death rates controlling for medical spillover, state and year fixed effects, and several labor market factors.

(2) $Y_{st} = \alpha + \beta T_{st} + \delta W_{st} + \theta X'_{st} + \varphi_s + \gamma_t + \varepsilon_{st}$

In this baseline specification, Y_{st} represents opioid death rates in state s and year t. S equals all 50 states and the District of Columbia and t represents the years 1990 to 2017. T equals 1 if a pain policy is in place in state s and year t and 0 otherwise. W signifies the proportion of the population that is white and over the age of 65. X denotes other controls such as poverty, population, and the unemployment rate. φ_s and γ_t equal state and year fixed effects respectively. I estimate this model with and without state and year fixed effects, population weights, and controls.

I use white 65+ populations as a control for medical spillover. This builds off the Powell et al. (2015) finding that populations with greater shares of elderly people have higher opioid abuse rates from excess pharmaceutical supply. I restrict the share of elderly to whites throughout my analysis because whites have disproportionately higher enrollment rates in Medicare Part D and are a more accurate approximation for Medicare participation (Hansen and Netherland, 2016; Chen et al. 2011). I use the log of state population to control for differences in magnitude that affect medical access such as increased number of doctors, pill clinics, and pharmacies. Similarly, poverty can influence access to opioids through Medicaid prescription cards. Unemployment could increase demand for opioids through depressed economic conditions that persuade people to abuse drugs (Krueger, 2017; Currie et al. 2018).

I use state fixed effects to control for time-invariant differences between states that influence opioid abuse such as cultural attitudes regarding prescription drugs and other socioeconomic differences. I apply year fixed effects to control for variation in opioid death rates over time that is common across states and is not attributable to state policy or other controls. I weight by state population to evaluate how state policy impacts opioid abuse for the average person. Specifications that do not include population weights measure the effect of policy for the average state.

In some specifications, as shown in equation (3), I add a state specific linear time trend to capture any time related specification error in my regressions. I control for heteroskedasticity by using robust standard errors. Additionally, I cluster my standard errors by state to control for correlation in state death rates over time. Incorporating state clusters, a time trend, and robust standard errors will legitimize my model and ensure robustness.

(3) $Y_{st} = \alpha + \beta T_{st} + \delta W_{st} + (\varphi_s * year) + \theta X'_{st} + \varphi_s + \gamma_t + \varepsilon_{st}$

I replicate specification (2) while excluding the District of Columbia and restricting the number of years in my sample. It is possible that, over time the epidemic gained public attention and factors other than state policy influenced opioid supply. I then change Y_{st} to all prescription drug mortality to use as a comparison point.

In equation (4), I examine the effect that different policy types have on opioid deaths. In this specification, the explanatory variables are indicators that represent the policy types described in table (1).

(4) $Y_{st} = \alpha + \beta_1 Policy\ A_{st} + \beta_2 Policy\ B_{st} + \ \beta_3 Policy\ C_{st} + \delta W_{st} + \theta X'_{st} + \varphi_s + \gamma_t + \varepsilon_{st}$

In equation (5), I add an interaction variable to estimate the differential effect between policies that were implemented before and after the year 2000. These regressions will indicate whether there is a statistically significant difference between policies that were introduced in earlier and later years. I repeat specification (5) for all policies, Policy A, Policy B, and Policy C.

(5) $Y_{st} = \alpha + \beta_1(Policy)_{st} + \beta_2(Policy * Before\ 2000)_{st} + \delta W_{st} + \theta X'_{st} + \varphi_s + \gamma_t + \varepsilon_{st}$

4. Descriptive Statistics

In table (2), I include a statistical summary of my variables weighted by population. The mean opioid mortality rate is 8.86 deaths per 100,000 and the mean all prescription drug rate is 9.12 deaths per 100,000. Each death rate has large a standard deviation, indicating that there is considerable state and year variation. This variation is desirable, as it will indicate if there is a correlation between higher death rates and earlier pain policies.

Table (3) details summary statistics for individual states and specifies which year states first implemented pain management policies. North Dakota has the lowest average opioid death rate of 3.06 deaths per 100,000 individuals while West Virginia has the highest average death rate of 16.29 deaths per 100,000. The first opioid related pain management policy was California's Intractable Pain Statute in 1990. 25 states had implemented policies by 1999, with 8 of the 25 implemented in 1997. The last two states to implement pain legislation were Idaho in 2013 and Indiana in 2014.

Figure (3) graphs opioid mortality by state from 1990 to 2017 and mean opioid mortality by year. This figure graphically shows substantial state variation in opioid death rates between states. There is a small jump in deaths from 1998 to 1999, indicating where the ICD system changed. As discussed in the data section and figure (1), this jump is constant across states.

Some states have more mortality variation than others. Figure (4) depicts death rates by year from 1990 to 2017 for the District of Columbia, which has the most variation in opioid deaths. This variation is important to keep in mind going forward in my analysis. Since the District of Columbia is not under the same jurisdiction as the 50 states, it may be beneficial to exclude D.C. from my analysis as discussed in the methods section.

Figure (5) shows the number of states with pain management policies in place by year, accounting for states that later retracted legislation. 48 of 50 states implemented some type of pain management policy that encouraged opioid use between the years of 1990 and 2017. Illinois and Montana are the exception. 12 states amended their legislation in attempt to control opioid abuse. As of 2017, 36 states have statutes that either encourages the use substance II drugs to treat chronic pain or protect physicians from disciplinary action when prescribing medication. This is not to say that state medical boards have not implemented other programs in attempt to curb opioid abuse, rather that these pain policies have not been formally removed from legislation.

Figure (6) compares the mean death rates for states that implemented pain policies before and after 2000. This figure is useful because it shows significant differences between states that implemented pain policy in the 1990s and the 2000s. In my analysis I will examine the differential effect of adopting policy before or after 2000. This graph may be an indication that states that implemented pain policy earlier are significantly different from those that enacted policies in later years.

Figure (7) shows how prescription opioid abuse varied by state over time. These figures detail the average number of prescriptions per 100 persons by state for the years 2006, 2009, 2012, and 2015. These heat maps indicate that prescriptions peaked in 2012. In many cases, states had over 100 prescriptions per 100 people. It appears that southern and western regions were particularly hit by high prescription rates.

5.1 Results and Discussion

Table (4) shows the results from equation (1), depicting a positive, statistically significant relationship between opioid prescriptions and opioid mortality. A 1% increase in prescription opiates will result in an average increase of 0.10297 deaths per 100,000 individuals. Thus, it is likely that my data is an appropriate measure of opioid abuse.

Table (5) depicts variations of equation (2) where opioid mortality is regressed on the presence of pain policy including all years, states, and the District of Columbia. All regressions use robust standard errors to control for heteroskedasticity. Columns (1)-(7) replicate equation (2) with and without controls, state and year fixed effects, and state population weights. In column (1), without controls, I learn that policy could have a statistically significant relationship with opioid mortality. In column (2), the approximation for Medicare beneficiaries, or the percent of the population that is over 65 and white, is statistically significant and decreases the policy coefficient. Column (3) adds the poverty rate and unemployment rate as controls, indicating that while poverty may play a role in opioid mortality, the unemployment rate may not. This is consistent with the results of Currie et al (2018).

Column (4) shows that without population weights, there is no statistically significant difference between states. This finding could shed light onto the fact that the 'average person' may be more likely to abuse opioids if their state allows for more liberal prescribing. This is evidence for the strong role of supply side determinants and matches the conclusions of Powell et al. (2015) and Gentzhow et al. (2017). In column (5), without state and year fixed effects, omitting fixed effects decreases the R^2 by 0.6, showing that state and year fixed effects play a large role in explaining the variation within my model.

When including controls, fixed effects, and population weights, columns (6) and (7) produce a policy coefficient of 0.9671. Column (6) shows that, on average, there is a 10.92% difference in opioid mortality between states that have pain policies and states that do not. Column (7) builds on this finding by including clustered standard errors by state to control for correlation in state death rates over time. Clustering standard errors reduces all statistical significance. This indicates that there is no difference in opioid mortality between states that have pain policies and states that do not, perhaps due to persistent trends within states as discussed in equation (3).

Table (6) investigates the effect of adding a state linear time trend and clustering standard errors by state. These regressions suggest that there is not a significant relationship between policy and opioid deaths. Columns (3) and (4) show that adding a state linear time trend decreases the policy coefficient from 0.9671 to 0.5662. However, this result is not statistically significant. Similarly, the R^2 only increases by 0.003, meaning that a state time trend does not help explain the variation within my model.

For the rest of my analysis, I will use specification (7) in table (5) to dissect the relationship between pain policy and opioid mortality. This specification includes the poverty rate, unemployment rate, and log of the population, as well as state and year fixed effects, state population weights, and both robust and clustered standard errors. The R^2 in this specification demonstrates that my model can explain 85.79% of the variation in death rates. Therefore, I believe that regression (7) is the most robust and accurate measure of how policy impacts opioid mortality.

Table (7) presents variations of equation (2) by restricting the years in my sample and dropping the District of Columbia. As shown in column (1), while excluding the District of Columbia increases the policy coefficient to 0.9869, this result is not statistically significant. Columns (2) – (6) suggest that pain management policies become insignificant over time. On average from 1990 to 2011, there is a 16.06% difference in opioid deaths between states that have pain management policies and states that do not. This finding is significant at the 95% confidence level. In column (3), from 1990 to 2012, this difference decreases to 15.5% and then to 11.3% from 1990 to 2015. These regressions indicate that while pain policy does impact opioid mortality, this effect weakens over time.

I can test the validity of this finding by comparing table (7) to table (8) where the outcome variable is general prescription drug mortality. Similar to opioid mortality, in column (1), my overall sample indicates that policy does not impact prescription drug deaths. However, columns (2)- (4) show that policy has a decreasing effect on mortality. Unlike opioid mortality, these results are significant at the 90% confidence level. This proves that the effect on all prescription drug deaths is weaker than that of opioid deaths and is evidence that state policies have an isolated effect on opioid mortality.

Overall, these findings suggest that while state pain policy did impact the opioid epidemic, it had a decreasing effect on opioid mortality over time. With context, these results could mean that external intervention became more relevant as the opioid abuse received national attention. It is possible that when the epidemic began, supply side factors, like state policies, had larger affects on death rates. As the epidemic intensified, exogenous variables like public health initiatives, addiction clinics, and community movements intervened, decreasing the impact of state policy.

Table (9) presents an analysis of the different types of pain management policies as discussed in equation (4). Similar to earlier results, from 1990 to 2017 states with pain management policies do not have statistically different death rates from states without pain management policies. When examining the impact of policy over time, it appears that Policy B significantly impacted opioid death rates. On average, until 2013 states that implemented Policy B had 18.41% higher opioid death rates than states that did not implement policy. On average, until 2014 states that implemented Policy B had a 17.16% difference in opioid death rates than states without pain legislation.

It is likely that Policy B is solely responsible for the relationship that policy has on opioid mortality. This could be because state medical boards have jurisdiction over physician licensing and are the cause of any disciplinary action that physicians would face for overprescribing opioids. This is evidence that opioid oversupply is a main driver for opioid abuse. States that implemented intractable pain statutes or state recommendations do not have statistically significant differences in death rates from states without policy. These differences are mirrored in Panel B, where the outcome variable is general prescription drug mortality.

I preform an F test to see if policies A, B, and C are jointly statistically different. I use the null hypothesis that coefficients for intractable pain statutes, state medical board policies, and state recommendations are equal to 0. When analyzing pain policies from 1990 to 2017, I get an F statistic of 0.53 and cannot reject the null that Policy A, B, and C have statistically different death rates at the 95% confidence level. For the years 1990 to 2012, I calculate an F-statistic of 2.34, which is significant at the 90% confident level. Thus, there is limited evidence that different pain management policies result in statistically different death rates. This finding is due to the fact that my coefficients have large standard error and over time Policy B impacted opioid mortality while Policies A and C did not.

Table (10) presents equation (5) by comparing states that implemented policies before 2000 to states that implemented policy after 2000. States with policies before 2000 tend to have statistically higher opioid death rates. Column (1) shows that, on average, states that implemented pain policy before 2000 have 41% higher opioid mortality rates than states that implemented policies after 2000. For Policy A, shown in column (2), on average states that implemented pain policy before 2000 are not statistically different from states that implemented policy after 2000. Column (3) shows that on average states that implemented Policy B before 2000 have a 44% difference in opioid mortality then states that implemented policy B after 2000. For Policy C, on average, there is a 67% difference in opioid mortality between states that proposed state recommendations before 2000 and after 2000. Column (4) is especially intriguing because it indicates that there is a negative relationship between states that implemented Policy C after 2000 and opioid mortality, suggesting that weaker, later legislation could have prevented opioid abuse.

Any policy, Policy B, and Policy C have statistically significant F statistics. For these groups, I reject that null hypothesis that the difference between implementing these policies before and after 2000 is 0. These results suggest that timing played a strategic role in determining the severity of state opioid mortality and are further evidence that greater opioid supply induced higher rates of opioid abuse.

5. 2 Limitations

There are several data limitations to my analysis. Due to CDC suppression constraints, opioid mortality by race, education, and gender were not available. This would be useful in determining which groups are circumstantially affected by the epidemic. These mortality rates could help determine if there is any spillover between socioeconomic or racial groups. Currie et al (2018) showed that opioids might help women remain in the workforce. Access to opioid mortality stratified by gender would be helpful to see if state policy circumstantially affected women more then men. Other studies suggest that opioid abuse is a greater problem in men. This data could help determine if men are more susceptible to abuse given excess opioid supply.

Since I examine opioid mortality rather than prescription rates, it is possible that my results undermine the full effect that policy had on opioid abuse. If more data regarding opioid prescriptions becomes available in the future, it would be ideal to repeat my analysis using opioid prescriptions as the outcome variable.

Another plausible error in my analysis is that policy is endogenous to pharmaceutical presence, which is the true explanation for opioid deaths. Pharmaceutical presence could take form in many ways, from lobbying states to pass certain policies, to advertising the benefits of opioid prescriptions to physicians (Satel, 2017). However, pharmaceutical presence is only an issue if it varies across states. If, when companies develop new drugs, they are made equally available to physicians, then pharmaceutical presence would be constant and accounted for by controlling for fixed effects. Similarly, if pharmaceutical presence depends on the number of hospitals, medical centers, or doctors in a given state, then this effect will be included when controlling for state population. If neither of these cases is true and pharmaceutical presence varies across states for other reasons, then my variable is endogenous and my results are biased. Further research looking into pharmaceutical presence combined with state policy and Medicare spillover would be helpful.

6. Conclusion

The opioid epidemic caught America by surprise, creating more devastation than was ever thought imaginable. To fix this crisis, we must identify the sources of opioid addiction and misuse. In this paper, I sought to dissect the relationship between opioid supply and opioid abuse through pain management legislation enacted by states. To do this, I examined the effect that pain management policy had on opioid mortality.

I found that pain policy decreasingly effects opioid mortality and that these policies were a likely causal channel for the initial part of the crisis. I show that state medical boards and early legislation play important roles in opioid abuse, meaning that opioid oversupply is a likely determinant for high rates of opioid addiction. For the last several years, pain policy did not have a strong role in opioid abuse. I find that as the epidemic progressed, external intervention became more important than pain legislation in determining state level abuse.

I build on previous literature from Powell et al (2015) by controlling for Medicare spillover effects in my analysis. This control isolates the effect that pain policy has on opioid mortality. Additionally, I build off studies that suggest supply side constraints are the main channel for opioid abuse (Gentzhow et al. 2018; Hollingsworth et al. 2017). Since previous research focuses on abuse after 2000, I construct an opioid mortality measure from 1990 to 2017. This aspect of my analysis was crucial because states that implemented policy before 2000 have 41% higher death rates than states that implemented policy after 2000.

As we go forward we must deal with the consequences of widespread opioid abuse. My research suggests that directing medical policy to reconsider prescription drug availability and prescribing culture will be imperative to prevent future epidemics. While I suggest that public mobilization and community interventions could play a role in influencing current abuse rates, there should be more research into which exogenous factors caused pain policy to become insignificant over time. While we cannot erase the tragedy of the opioid epidemic, going forward citizens and policy makers alike can be vigilant about examining policy externalities. It is important to remember that the battle is not over. Death rates climb despite national attempts to curb abuse. However, in 2019 national opioid prescriptions are at a 4-year low, hopefully signifying a light at the end of the tunnel.

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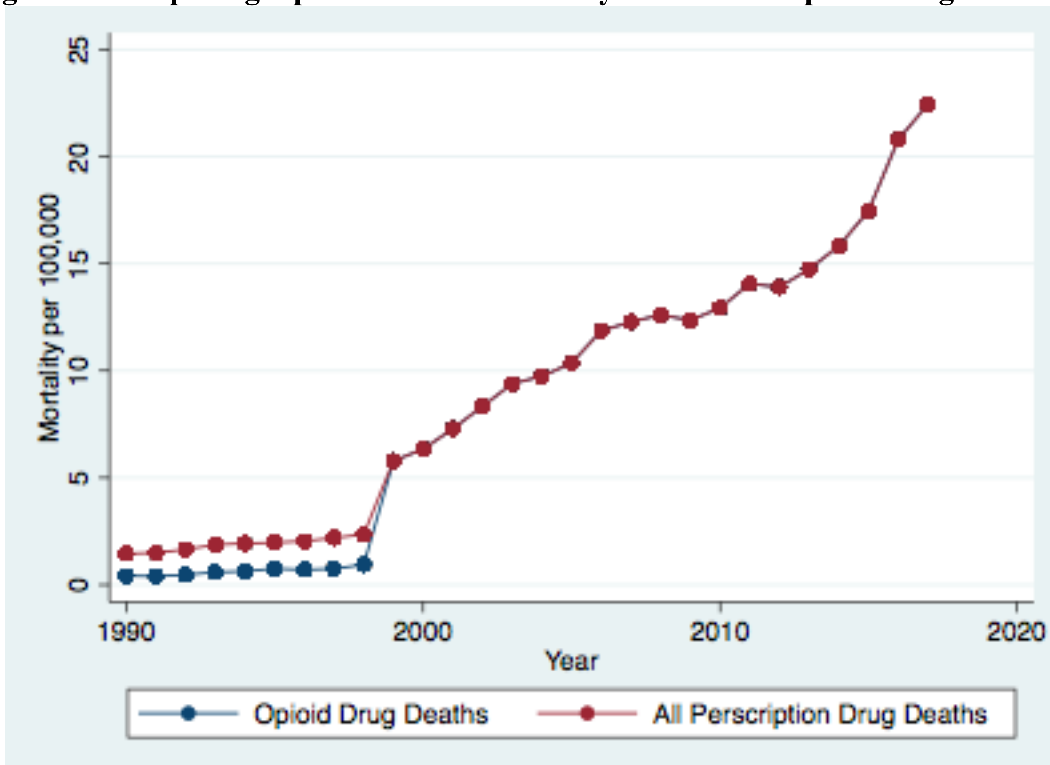
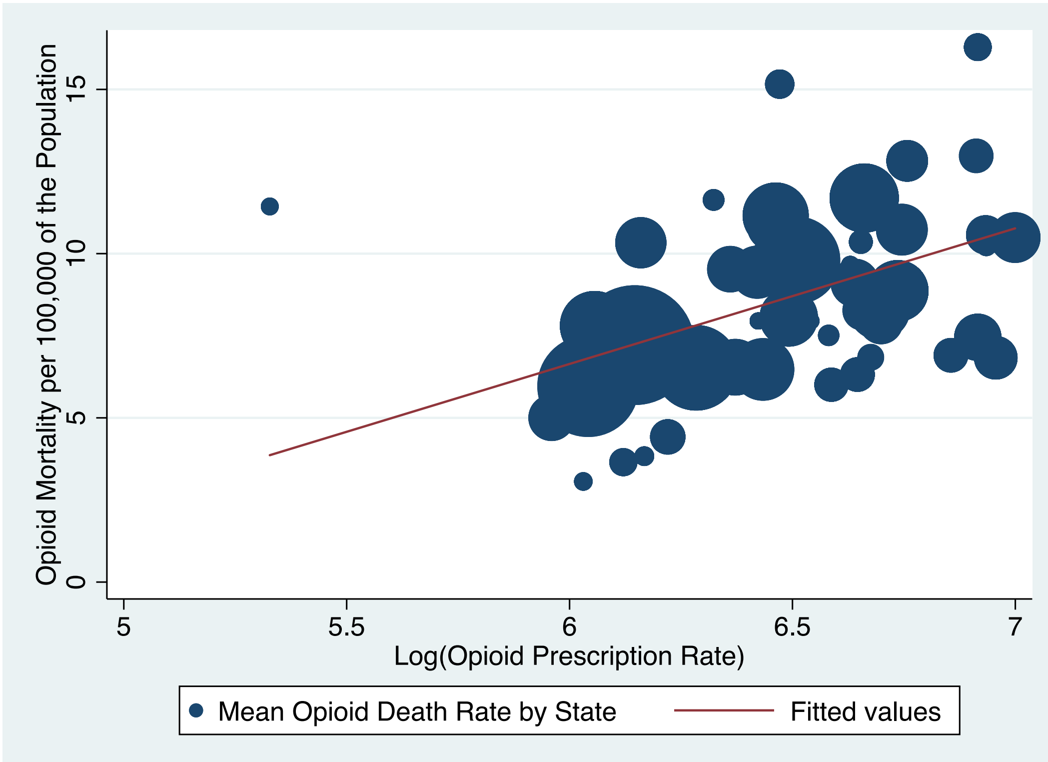
Figures

Figure 1. Comparing Opioid Related Mortality to All Prescription Drug Mortality



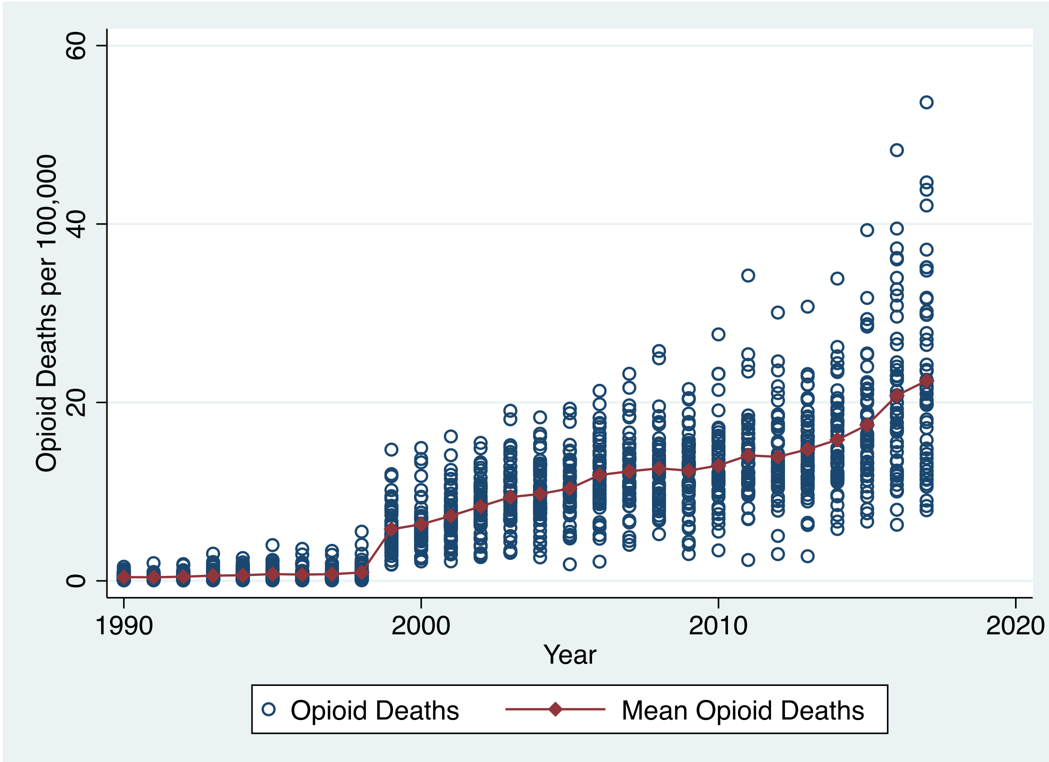
Notes: The blue line depicts mean opioid mortality from 1990 to 2017 measured by deaths per 100,000 individuals. The red line indicates mean all prescription drug mortality from 1990 to 2017 measured by deaths per 100,000 individuals. This figure depicts how opioid deaths make up almost all prescription drug deaths after the ICD upgrade in 1999. This is most likely because the ICD system changed to incorporate coding for more types of prescription opioid drugs like Oxycotin and Oxycodone (CDC, 2018). According to my data, opioids account for 96% of all prescription drug related deaths from 1990 to 2017.

Figure 2. Comparing 2015 Prescriptions and Mean Opioid Deaths, Weighted by State Population



Notes: Both the scatter plot and the fitted line is weighted by state population.

Figure 3. Mean Opioid Mortality by State for All States and the District of Columbia

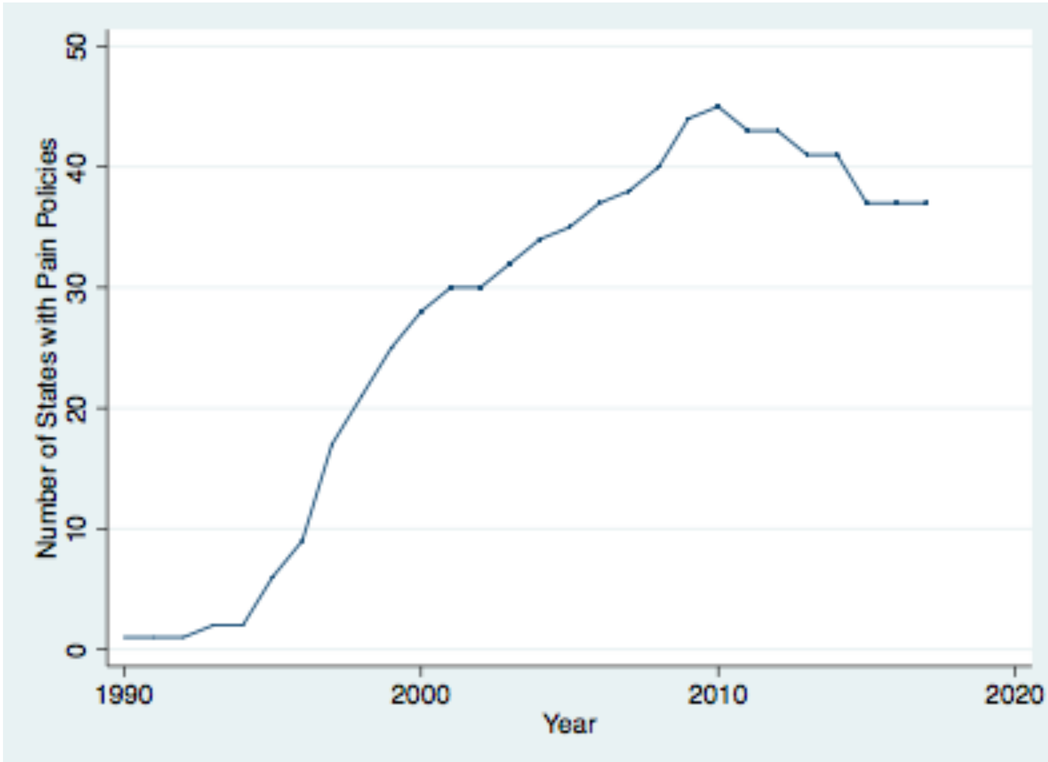


Notes: The blue open circles represent individual death rates for each state and the District of Columbia. The red filled circles show the mean opioid death rate by year.

Figure 4. Opioid Mortality for the District of Columbia Over Time

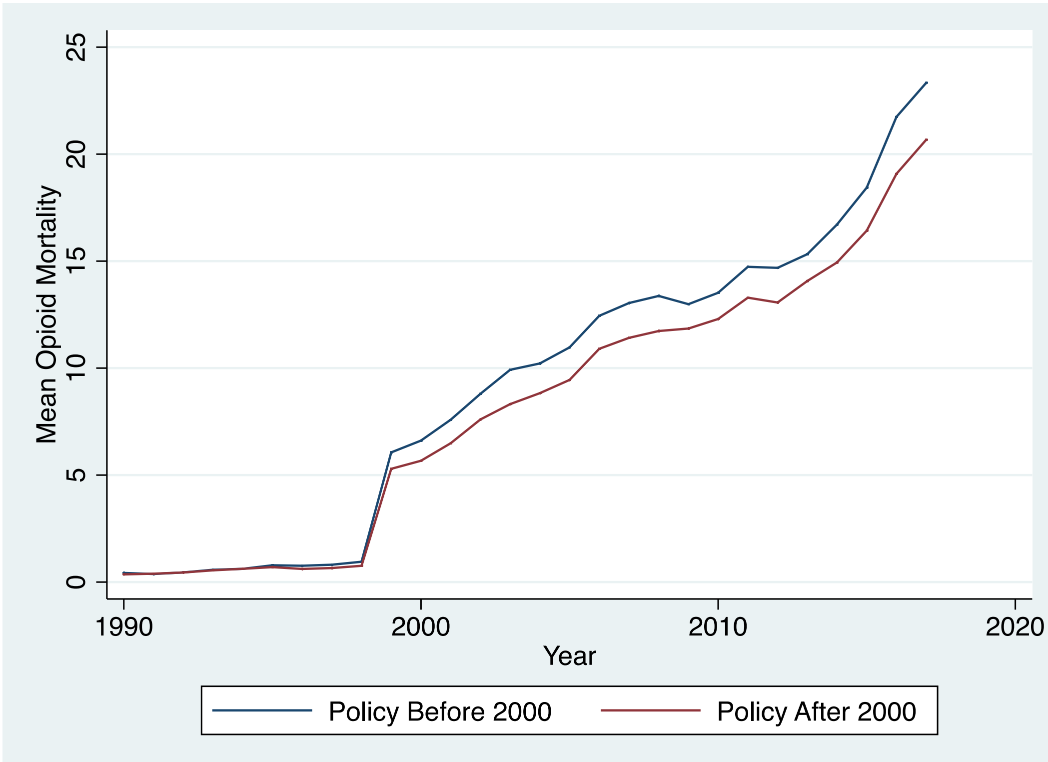
Note: This graph represents opioid mortality by year for the District of Columbia. There is significant variation in these mortality rates.

Figure 5. Number of States with Pain Management Policies by Year



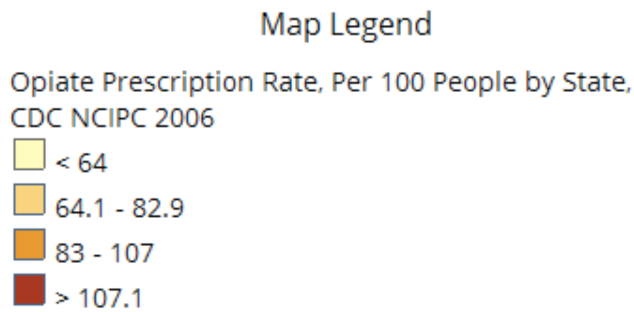
Notes: This figure represents the number of states with pain management policies from 1990 to 2017. The number of states with policies peaks and then decreases because some states eliminated legislation after the extent of the opioid crisis became apparent.

Figure 6. Comparing Opioid Deaths for States that Implemented Policies Before and After 2000

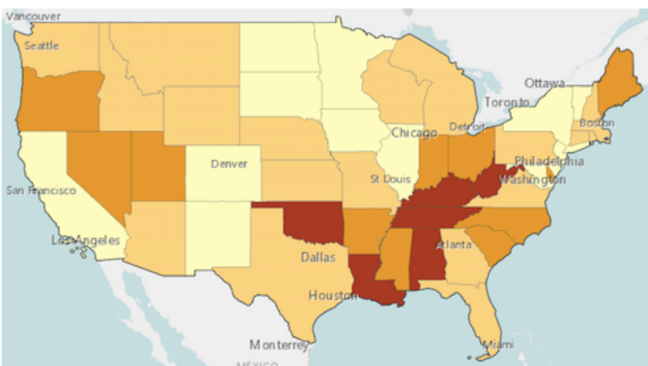


Note: The blue line represents mean opioid mortality by year for states that implemented pain management legislation before 2000. The red line represents mean opioid mortality by year for states that implemented pain management legislation after 2000. Opioid mortality is measured in deaths per 100,000 of the population.

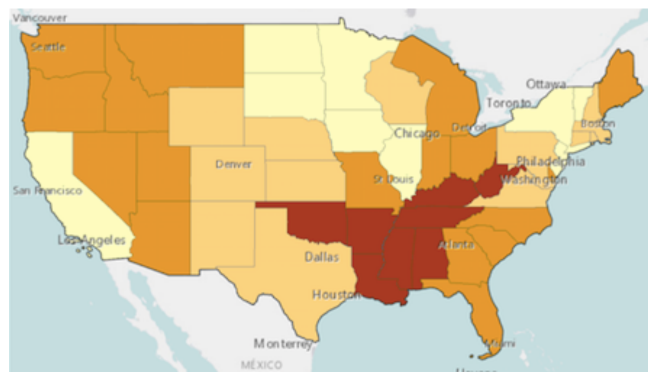
Figure 7. Opioid Prescriptions per 100 People, by State



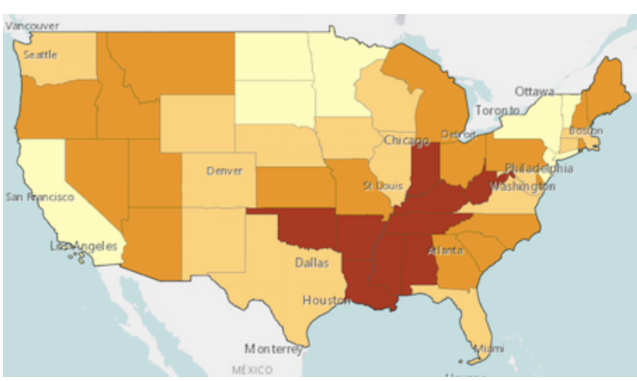
2006 Prescription Rate per 100 People



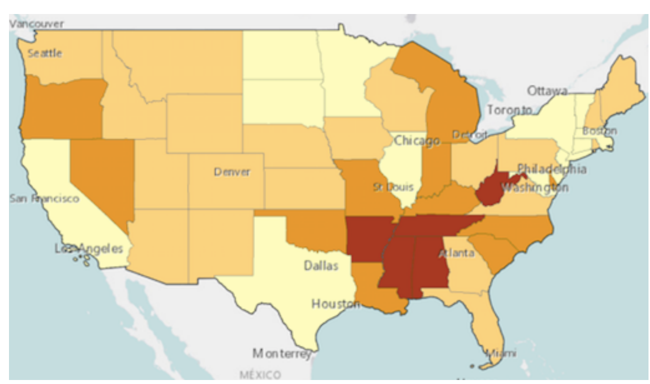
2009 Prescription Rate per 100 People



2012 Prescription Rate per 100 People



2015 Prescription Rate per 100 People



Source: CDC NCIPC 2006, CARES Engagement Network

Tables

Table 1. Types of Pain Management Policies

Policy Type	Description	Number of States Implemented
Policy A: Intractable Pain Statutes	Legislation that protects physicians from facing legal action if their patients become addicted to drugs that they prescribe	14
Policy B: State Medical Board Policy	State medical board implemented policy encouraging physicians to treat pain aggressively and acknowledging potential pain management benefits of opioid drugs, discourages physicians from preoccupation over legal action	24
Policy C: State Recommendation	State medical board recommendation that physicians treat pain more aggressively, no formal legislation	10
No Policy	No statutes, codes, or bills regarding pain management were passed	2
Amended Pain Management Policy	State amended law to prevent opioid abuse	12

Source: Pain Policy Studies Group at University of Wisconsin; Federation of State Medical Boards

Table 2. Opioid Death Rate Descriptive Statistics

Variable	N	Mean	Std. Dev.	Min	Max
Opioid Death Rate	1,428	8.855	7.2589	0.047	53.639
Prescription Drug Death Rate	1,428	9.1257	6.878	0.363	53.639
Poverty Rate	1,428	13.4	3.1	4.5	26.4
Unemployment Rate	1,428	6.0	1.9	2.3	13.7

Notes: Death rates are per 100,000 of the population; all statistics are weighted by state population
Source: CDC 2019a and CDC 2019b, Bureau of Labor Statistics, US Census

Table 3. Policy Year and Opioid Death Rates per 100,000 by State:

State	Year Pain Policy was Introduced	Overall Death Rate	White Death Rate
California	1990	7.215	6.668
Alaska	1993	9.654	7.409
Missouri	1995	9.105	8.273
North Dakota	1995	3.064	2.626
Oregon	1995	8.272	8.450
Wisconsin	1995	7.453	6.771
Maryland	1996	10.829	8.117
Nevada	1996	12.977	12.605
North Carolina	1996	8.313	7.578
Arkansas	1997	6.905	6.632
Colorado	1997	9.527	9.398
Louisiana	1997	9.357	8.006
Minnesota	1997	5.003	4.506
Mississippi	1997	6.319	5.789
New Jersey	1997	8.057	6.921
Ohio	1997	11.160	10.146
Rhode Island	1997	11.635	11.689
Kansas	1998	6.010	5.715
Oklahoma	1998	10.572	9.370
Pennsylvania	1998	11.693	10.579
West Virginia	1998	16.287	16.199
Florida	1999	9.798	9.482
Massachusetts	1999	10.333	10.770
Nebraska	1999	3.652	3.551
New Mexico	1999	15.161	14.616
Alabama	2000	6.831	6.322
Arizona	2000	10.732	10.191
New Hampshire	2000	10.357	10.659
Kentucky	2001	12.825	12.656
Tennessee	2001	10.490	9.799
Michigan	2003	8.866	7.648
Texas	2003	5.966	5.621
South Dakota	2004	3.834	3.140
Virginia	2004	6.543	5.741
Connecticut	2005	9.594	9.033
Hawaii	2006	6.735	3.968
New York	2007	6.525	5.539
Georgia	2008	6.475	5.668
Iowa	2008	4.421	4.459
South Carolina	2009	7.914	7.152
Utah	2009	11.475	11.933
Vermont	2009	7.951	7.956
Wyoming	2009	7.951	7.600
Maine	2010	9.089	9.089
Delaware	2012	10.238	9.207
Washington	2012	9.435	8.918
Idaho	2013	6.844	6.887
Indiana	2014	8.616	8.172
Illinois	None	7.806	6.159
Montana	None	7.512	6.911

Source: Pain Policy Studies Group; Federation of State Medical Boards; CDC 2019a, CDC 2019b

Table 4: Comparing 2015 Prescription Opioid Rates to Opioid Mortality

	(1)
	Opioid Mortality
2015 Opioid Prescriptions	10.297***
	(2.377)
Constant	-49.944***
	(15.306)
Obs.	51
R-squared	0.277

Standard errors are in parenthesis, Robust standard errors
Weighted by state population
*** p<0.01, ** p<0.05, * p<0.1

Table 5: The Effect of Policy on Opioid Deaths with and without Clustering by State

Outcome: Opioid Mortality	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Policy	1.0429**	0.9350**	0.9755**	0.1752	5.639***	0.967***	0.9671
	(0.4130)	(0.4009)	(0.3843)	(0.2833)	(0.4966)	(0.3634)	(0.8528)
Percent 65+, White		0.950***	0.804***	0.909***	0.911***	0.7493**	0.7493
		(0.3084)	(0.2966)	(0.2538)	(0.1328)	(0.2935)	(0.8787)
Poverty Rate			0.339***	0.1398**	0.0244	0.330***	0.330***
			(0.0767)	(0.0685)	(0.0670)	(0.0690)	(0.1159)
Unemployment Rate			0.1813	-0.0185	0.322***	0.2225*	0.2225
			(0.1338)	(0.1175)	(0.1188)	(0.1317)	(0.2028)
Log (Population)				-4.1586*	-0.0717	-13.32***	-13.3185*
				(2.1371)	(0.2710)	(2.6212)	(7.1587)
Constant	0.2981	-10.35***	-14.34***	50.4849	-5.9084	197.4***	197.410*
	(0.4315)	(3.4741)	(3.6684)	(31.3697)	(4.7707)	(40.5203)	(110.226)
Obs.	1428	1428	1428	1428	1428	1428	1428
R-squared	0.8419	0.8443	0.8492	0.8384	0.2412	0.8579	0.8579
Year Fixed Effects	Yes	Yes	Yes	Yes	No	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes	No	Yes	Yes
Weighted by Population	Yes	Yes	Yes	No	Yes	Yes	Yes
Clustered Standard Errors	No	No	No	No	No	No	Yes

Standard errors are in parenthesis, All regressions use robust standard errors
*** p<0.01, ** p<0.05, * p<0.1

Table 6: The Effect of Policy on Opioid Deaths including State Linear Time Trends and Clustered Standard Errors

Outcome: Opioid Mortality	(1)	(2)	(3)	(4)
Policy	0.9671***	0.9671	0.5662	0.5662
	(0.3634)	(0.8528)	(0.3825)	(0.8662)
Percent 65+, White	0.7493**	0.7493	0.7444**	0.7444
	(0.2935)	(0.8787)	(0.3033)	(0.8913)
Poverty Rate	0.3300***	0.330***	0.351***	0.352***
	(0.0690)	(0.1159)	(0.0738)	(0.1260)
Unemployment Rate	0.2225*	0.2225	0.2441**	0.2441
	(0.1317)	(0.2028)	(0.1209)	(0.2124)
Log (Population)	-13.3185***	-13.3185*	-13.4714***	-13.4714*
	(2.6212)	(7.1587)	(2.7211)	(7.3788)
Constant	197.4102***	197.4102*	199.5201***	199.5201*
	(40.5203)	(110.2257)	(41.8444)	(113.4038)
Obs.	1428	1428	1428	1428
R-squared	0.8579	0.8579	0.8608	0.8608
State Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
State Linear Time Trend	No	No	Yes	Yes
Clustered Standard Errors	No	Yes	No	Yes

Standard errors are in parenthesis, Using robust standard errors, clustered standard errors, population weights, state and year fixed effects
*** p<0.01, ** p<0.05, * p<0.1

Table 7: The Effect of Policy on Opioid Mortality, with Regression Restrictions

Outcome: Opioid Mortality	(1) Excluding DC	(2) Years until 2011	(3) Years until 2012	(4) Years until 2013	(5) Years until 2014	(6) Years until 2015
Policy	0.9869	1.0371**	1.0513**	1.0713**	1.0437*	0.9983*
	(0.8450)	(0.5163)	(0.5220)	(0.5174)	(0.5347)	(0.5792)
Percent 65+, White	0.7714	0.5074	0.5094	0.5420	0.6505	0.7414
	(0.8848)	(0.9411)	(0.8812)	(0.8278)	(0.8039)	(0.7997)
Constant	197.4763*	-71.0298	-45.5041	-13.0605	24.1175	70.1876
	(110.627)	(87.6779)	(88.2694)	(88.4925)	(88.9316)	(92.3354)
Obs.	1400	1122	1173	1224	1275	1326
R-squared	0.8592	0.9020	0.9009	0.9008	0.8989	0.8927

Standard errors are in parenthesis, using robust standard errors, clustered standard errors, population weights, and state and year fixed effects

*** p<0.01, ** p<0.05, * p<0.1

Table 8: The Effect of Policy on All Prescription Drug Mortality, with Year Restrictions

Outcome: Prescription Drug Mortality	(1) All Years	(2) Years until 2012	(3) Years until 2013	(4) Years until 2014
Policy	0.9521	1.0290*	1.0517*	1.0259*
	(0.8704)	(0.5497)	(0.5410)	(0.5556)
Percent 65+, White	0.8219	0.6123	0.6382	0.7412
	(0.8846)	(0.9150)	(0.8563)	(0.8257)
Constant	215.9632**	-23.6515	8.1274	44.5479
	(105.933)	(81.3098)	(81.8420)	(82.7454)
Obs.	1428	1173	1224	1275
R-squared	0.8481	0.8903	0.8906	0.8892

Standard errors are in parenthesis, Using robust standard errors, clustered standard errors, population weights, and state and year fixed effects

*** p<0.01, ** p<0.05, * p<0.1

Table 9: The Effect of Different Policy Types on Opioid Mortality, with Year Restrictions

Panel A:

Outcome: Opioid Mortality	(1) 1990-2017	(2) Years until 2012	(3) Years until 2013	(4) Years until 2014
Policy A: Intractable Pain Statutes	1.511	0.452	0.533	0.6555
	(1.339)	(0.859)	(0.856)	(0.8848)
Policy B: State Medical Board Policy	0.679	1.661**	1.673**	1.521**
	(0.935)	(0.662)	(0.669)	(0.703)
Policy C: State Recommendation	0.128	0.033	-0.058	-0.048
	(1.641)	(1.434)	(1.354)	(1.731)
No Policy: Constant	202.157*	-6.828	-33.354	-7.347
	(117.970)	(9.240)	(86.174)	(88.852)
Obs.	1428	1173	1224	1275
R-squared	0.859	0.901	0.903	0.9004

Panel B:

Outcome: All Prescription Drug Mortality	(1) 1990-2017	(2) Years until 2012	(3) Years until 2013	(4) Years until 2014
Policy A: Intractable Pain Statutes	1.548	.4588	0.571	0.6946
	(1.3389)	(0. 854)	(0.848)	(0.878)
Policy B: State Medical Board Policy	0.654	1.666**	1.529**	1.478**
	(0.928)	(0.674)	(0.687)	(0.714)
Policy C: State Recommendation	0.003	-0.255	-0.195	-0.184
	(1.519)	(1.197)	(1.212)	(1.236)
No Policy: Constant	220.980	-45.448	-12.218	27.748
	(114.389)	(78.674)	(80.438	(83.636)
Obs.	1428	1173	1224	1275
R-squared	0.8492	0.8935	0.8932	0.8991

Standard errors are in parenthesis, Using robust standard errors, clustered standard errors, population weights, and state and year fixed effects
*** p<0.01, ** p<0.05, * p<0.1

Table 10: Comparing Opioid Mortality for Policies Introduced Before and After 2000

Outcome: Opioid Drug Mortality	(1) Any Policy	(2) **Policy A:** Intractable Pain Statutes	(3) **Policy B:** State Medical Board Policy	(4) **Policy C:** State Recommendation
Policy Variable	-0.9531	-2.0084	0.0333	-3.0735***
	(0.6062)	(1.3154)	(1.0498)	(0.7052)
Policy*Before 2000	3.3438***	2.6929*	3.5519***	5.4974***
	(0.9321)	(1.5124)	(1.3079)	(1.8151)
Constant	179.5376**	168.4193	199.6382**	206.0787*
	(89.3886)	(108.7923)	(90.1445)	105.4485
Obs.	1425	1425	1425	1425
R-squared	0.8662	0.8569	0.8633	0.8583
F Statistic	6.59	1.67	5.38	12.06
P Value	0.0031	0.1988	0.0077	0.0001

Standard errors are in parenthesis, using robust standard errors, clustered standard errors, population weights, and state and year fixed effects

*** p<0.01, ** p<0.05, * p<0.1