Estimating the Impact of Opioid Control Policies

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EXECUTIVE SUMMARY

The opioid crisis in the US is a term used to describe the exponential increase in opioid consumption and opioid-related deaths in the early to late 2000s. In response to these changes, several states implemented policies in an attempt to control opioid intake and mortality in their states, such as, educating the public, implementing prescription drug monitoring programs, and enforcing policies to regulate pain clinics to name a few. This report's objective is to gain a better understanding of the effectiveness of opioid control policy changes on opioid consumption and opioid-related mortality, specifically in Texas, Florida, and Washington (i.e., the treatment states). These three states were chosen since each of them had high opioid-consumption but also implemented a succession of policy changes over the span of a few years (2007, 2010, and 2012 respectively), all in response to the opioid crisis. It was hypothesized that the opioid-control policies had a causal effect on the reduction of opioid consumption and opioid-related mortality rates in the treatment states.

To test our hypothesis about the effectiveness of opioid control policies, we conducted two types of analyses: pre-post and difference-in-difference. The pre-post analysis gave us a straightforward view of how opioid consumption and related deaths changed before and after policy implementation in each state. The difference-in-difference (DiD) analysis, on the other hand, offered a more detailed perspective by comparing states where an intervention occurred, which we will refer to as treatment states, with states that had similar opioid consumption and mortality trends to our treatment states but did not undergo the same intervention, which we will refer to as control states. DiD allows for more nuanced analysis of the cause-effect relationship between our interventions and outcomes, by reducing the possibility of making conclusions without considering potential external factors influencing consumption and mortality in our treatment states that were not related to the intervention. Data was cleaned and refined to ensure a meaningful and manageable scope. To address missing mortality data and streamline the analysis, we focused our scope on counties with populations above a certain threshold for each treatment state and its associated control states. This criterion aids in managing missing data and upholds a consistent scope, particularly in mortality, where smaller counties may have incomplete records due to privacy concerns.

After data cleaning, refining and scope setting, the pre-post analysis and DiD analysis were conducted, and the results presented mixed results state-to-state. For Florida, there was a clear positive effect of policy implementation. That is, the opioid consumption per capita and opioid-related deaths significantly reduced after the policy was implemented in 2010. The DiD analysis also showed that this downward trend after policy implementation was unique to Florida in comparison to the control states chosen (Kentucky, West Virginia, Tennessee, Nevada, Oregon). These trends were not as clear for Texas and Washington. For Texas, the opioid consumption data began in 2006, thus not allowing for a fuller analysis of the change in opioid consumption trends before and after policy implementation. However, from the available data, it was found that while there was a noticeable upward trend in opioid consumption before policy implementation, this stabilized in the months following post policy implementation. For mortality, there was a much clearer result in Texas, where it was found that the policy changed the trends of opioid-related deaths in the population from going upwards to downwards. Again, this was in comparison to control states

(Missouri, Minnesota, Arkansas), where the control states continued to show an upward trend after the policy implementation year 2007. In Washington, the trends in opioid consumption and opioid-related deaths which initially surged pre-policy, showed signs of stabilization after policy implementation. Furthermore, its control states (Ohio, Michigan, Maine, and Hawaii) showed a more favorable trajectory for consumption post-2012 while Washington exhibited a notably improved mortality trend after policy implementation. These changes in the steep rise of consumption and mortality suggest potential effectiveness of policy implementation in Washington as well but are still open to interpretation and further evaluation for a more concrete conclusion.

In conclusion, this study offers a comprehensive evaluation of opioid-control policies in Texas, Florida, and Washington, shedding light on the varied impact of interventions on opioid consumption and mortality. Strengths of the study lie in its rigorous methodologies, combining pre-post and DiD analyses for a nuanced understanding. Florida demonstrated a notable positive effect, showcasing a significant reduction in opioid consumption and related deaths post-policy implementation. Texas displayed a stabilized trend in opioid consumption, with a marked positive impact on mortality rates. Washington, however, exhibited a nuanced response, with a plateau in both consumption and mortality trend. The study's robust analysis underscores the need for tailored policies and continued examination of evolving public health challenges. Limitations include the potential influence of external factors, such as changing drug landscapes, warranting cautious interpretation. Future research should delve into nuanced regional factors influencing policy outcomes and explore additional indicators for a comprehensive assessment. In addition, investigating broader time frames for pre- and post-policy may be needed to accurately understand the policy’s effect, as seen when discussing opioid consumption for Texas and mortality death rates in Washington. Despite these limitations, this study contributes essential insights to the ongoing discourse on optimizing opioid-control strategies for public health benefits.

The Opioid Crisis

Opioid-related deaths were recorded to be as high as approximately 80,000 in 2022, which increased from approximately 21,000 in 2010 (CDC, 2022). It is estimated that around half of these deaths are due to prescription opioids (Soelberg et al., 2017). This has been suggested to be attributed to three main factors: physicians' need to relieve patients' pain, perceived under-treatment of pain for many years before the opioid crisis, and lastly (arguably most importantly), the aggressive marketing of opioids for non-cancer pain (Weiner et al., 2017).

In response to these exponential increases in opioid-related deaths, many states changed their policies to limit opioid abuse and/or increase awareness about the opioid crisis. For example, in a study conducted in 2015, it was found that of the 50 states and the District of Columbia, all reported initiatives to educate the public, prescribers, families etc., 29 states increased their funding for medication-assisted opioid addiction rehabilitation, 26 established guidelines for "safe" prescription, 23 put forth requirements for prescriber use of prescription monitoring, and 14 enforced policies to regulate pain clinics (Wickramatilake et al., 2017).

Policy Implementation

Many states have implemented policies in attempts to control opioid prescription/distribution and in turn, consumption, and mortality. The most notable of these policy implementations were in the states we have chosen as our treatment states: Florida, Texas, and Washington. Following is a brief outline of the history of these policy implementations:

Florida: From about 2007 to 2010, Florida was becoming infamous with respect to their opioid prescription quantities. To mitigate the consequences of this over prescription, many policies were implemented in succession from 2010 to 2012 in Florida, ranging from policy to ensure pain clinics were being registered (Operation Pill Nation, 2010) to prescription drug monitoring programs (PDMP) implemented in 2012.

Texas: Controls were established in 2007 to have more checks before prescribing opioids, mandating consent from patients, and maintaining detailed medical records in addition to reviewing the state's PDMP data.

Washington: From 2012, Washington State implemented new rules for opioid prescription in pain treatment, including annual reviews for stable patients on low doses, mandatory consultations for higher doses, and strict documentation and monitoring for physicians prescribing over a certain limit.

Research Design Motivation

These current analyses were designed to help answer these two questions:

1. What is the effect of opioid drug prescription regulations on the volume of opioids prescribed in Florida, Washington, and Texas?
2. What is the effect of opioid drug prescription regulations on drug overdose deaths in Florida, Washington, and Texas?

By answering these two questions, this paper hopes to assess the causal effects of opioid drug prescription regulations/policies in Florida, Washington, and Texas.

This study hypothesizes that the opioid-control policies will lead to a reduction in opioid consumption and opioid-related mortality rates in the treatment states. As a result, the model should reveal a distinct downward trend in both variables over time, with a noticeable inflection point coinciding with the implementation of the policies.

Data

In both these analyses, three datasets were used to help answer the research design:

1. Opioid Shipments Data: Gathered from Washington Post, it details the drug transactions of pharmaceutical companies/suppliers to pharmacies between the years 2006 to 2019 inclusive. These drug transactions were reported to the Drug Enforcement Administration and lists information on the pharmaceutical companies/suppliers, pharmacies, opioid drug type, opioid drug quantity, and transaction date of sales.
2. Vital Statistics Mortality Data: Gathered from the U.S. Vital Statistics Record, the dataset provides a summary of mortality records for drug and non-drug related causes in every county of the United States from 2003 to 2015. It details the number of deaths in each county for each year and the cause of death.
3. U.S. Census Data: Gathered from the U.S. Census, it presents population estimates for all US counties from 2002 – 2018 alongside their respective FIPS codes.

All three datasets were merged into two final datasets such that for every county in each state, it lists the number of opioid shipments, weight of the shipment, the number of drug overdose deaths, and the year and month, respectively.

Dealing with the Data

Setting a Scope: Population Threshold

Setting a population threshold limits population information and estimates to counties with populations larger than a given threshold. Thresholds of 40,000 for Florida and Texas, and 45,000 for Washington (and their respective control states) were determined based on extensive exploratory analyses and visualizations (see Appendix A). This was done to maintain data integrity and limit the need for imputation. The intent in implementing these thresholds is to ensure the reliability and consistency of the remaining data. By focusing on counties with populations above these thresholds, we create a more homogeneous dataset, enhancing the reliability of our conclusions. This approach allows us to balance the potential loss of data with the quality and consistency of information retained. Despite thresholding the data, the sample size remaining post-thresholding is sufficient to draw generalizable conclusions, enabling robust and reliable analyses.

Refining Precision in Mortality Data

The mortality dataset was filtered to only include drug-related deaths (i.e., excluding alcohol-related and non-alcohol or drug related deaths), resulting in the remaining data including all drug-related deaths. However, it is possible that drugs other than opioids were the cause of death for a proportion of this population, thus, skewing or influencing our models and inferences. According to the CDC, around 75.4% of all drug-related deaths are opioid-related (CDC, 2023). This raised the question of whether reducing the mortality figures to 75% of their actual value would be appropriate. However, doing so might raise concerns about discounting valid data from states with varying proportions of opioid-related deaths (especially, in the current project where the treatment states may have higher than 75% rates of opioid-related deaths). Therefore, while no further refinement of mortality data was performed, readers are advised to exercise caution in interpreting the data.

Missing Values in Mortality

The challenge of missing values in our study primarily stemmed from the CDC's data censoring policy, which excludes counties with fewer than 10 deaths per year for anonymity reasons. While this policy is reasonable for privacy, it posed a challenge as it led to the exclusion of many small or rural counties from our analyses. To address this, we implemented a threshold to define the scope of our study, excluding counties that fell outside the predetermined population range.

However, it is noteworthy that some counties with larger populations also exhibited sporadic missing mortality data without discernible patterns. This inconsistency could be attributed to various factors, including inconsistencies in data collection, underreporting, and infrastructural limitations that might affect the accuracy of opioid-related death reports. In response to this challenge, we calculated the death rate for each county using available data (total deaths divided by total population per year). For counties with missing mortality rates for certain years, we employed imputation, replacing them with the average death rates of their respective counties for the remaining years to ensure their inclusion in the analysis. One drawback to this is that we assume average death rate adequately represents the missing values, overlooking potential differences within the county over time. To counter this, counties passing the population threshold but lacking any mortality data for the entire range of years were removed to uphold data integrity. Imputing at a state level was deemed inappropriate due to the considerable heterogeneity among counties within a state, a topic further explored in Appendix A.

Missing Values in Opioid Shipments

The study faced challenges in not being able to see the average yearly opioid shipments per capita for Texas, as the data before the state's policy implementation was sparse, where there was no data available before 2006. The monthly average opioid shipments per capita were calculated instead of the yearly average opioid shipments per capita. This analysis thus focuses on the trend of average opioid shipments per capita 12 months before the policy implementation and 12 months after the policy implementation.

Span of Years Used

For ease of interpretation, the span of years used for each state was based on the year of implementation, where equal number of years on either side of the year of implementation were used based on availability of data. For example, the mortality dataset had data for the years 2003-2015 and the year of policy implementation for Florida was 2010. Therefore, the years used for this regression were 2005-2009 for pre-policy implementation and 2010 to 2015 for post-policy implementation.

Pre-Post Analysis

To assess the impact of policy implementation on opioid consumption and related mortality in our treatment states, we conducted a pre-post analysis for each state, examining opioid consumption per capita and opioid-related deaths. Mortality rates were measured as the number of deaths caused by opioids per 100,000 people providing a scale roughly equivalent to the seating capacity of the Los Angeles Memorial Coliseum within the entire treatment state. For all the descriptive statistics tables, figures were rounded to two decimal places to avoid false precision pretenses.

Florida

Table 1 provides an exploratory analysis of the means of consumption and opioid-related mortality before and after policy implementation in 2010 in Florida. The trend lines of these metrics pre- and post-policy implementation are further visualized in Figure 1.

Table 1. Florida opioid trends: Changes in mean opioid consumption and opioid-related deaths pre- and post-policy implementation in 2010.

Florida	Pre-2010 Mean	Post-2010 Mean
Consumption (MME per cap)	483.00	617.42
Mortality (deaths per 100,000)	10.29	10.56

Figure 1. Pre-post 2010 policy implementation analysis for Florida.

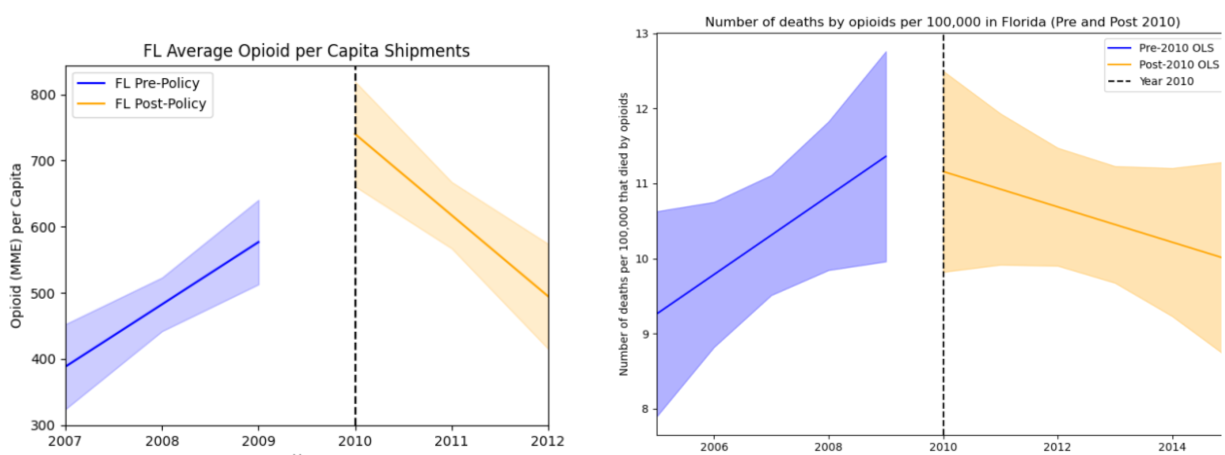

Figure 1 (left) shows the trend of the average annual opioid (Morphine milligram equivalent) shipments per capita for the state of Florida before and after their policy implementation. Figure 1 (right) depicts the decline in opioid-related deaths following the policy change. The y-axis represents opioid related mortality as the number of deaths per 100,000 people in Florida.

Interpretation

Table 1 provides seemingly counter-intuitive results regarding opioid consumption and mortality post-policy implementation in Florida. However, Figure 1 reveals a nuanced and more comprehensive understanding of the results: while descriptive statistics show an apparent increase, the actual impact of the policy shows a clear decreasing trend. The overall effect of policy implementation aligns with expectations, notably affecting both opioid consumption and mortality rates. Before their policy implementation in 2010, the trend of the average annual opioid shipments per capita was positive. This trend shows that every year the average number of shipments increases to around 95 per year. Conversely, after the policy was implemented, the trend becomes negative decreasing around 125 per year. These differences in trends support the hypothesis that the policy was effective in reducing the number of opioids shipments. Similarly, policy implementation shows the same switch in opioid-related mortality trends. The number of deaths per 100,000 people in Florida was increasing till 2010, and the trend showed a decline after 2010's policy implementation, highlighting the policy's effectiveness.

Texas

Summary statistics of the mean consumption and opioid-related deaths are presented in Table 2, and further analyses of the actual change in trends pre- and post-policy implementation are presented in Figure 2.

Table 2. Texas opioid trends: Changes in mean opioid consumption and opioid-related deaths pre- and post-policy implementation in 2007.

Texas	Pre-2007 Mean	Post-2007 Mean
Consumption (MME per cap)	13.08	14.86
Mortality (deaths per. 100,000)	9.28	9.95

Figure 2. Pre-post 2007 policy implementation analysis for Texas.

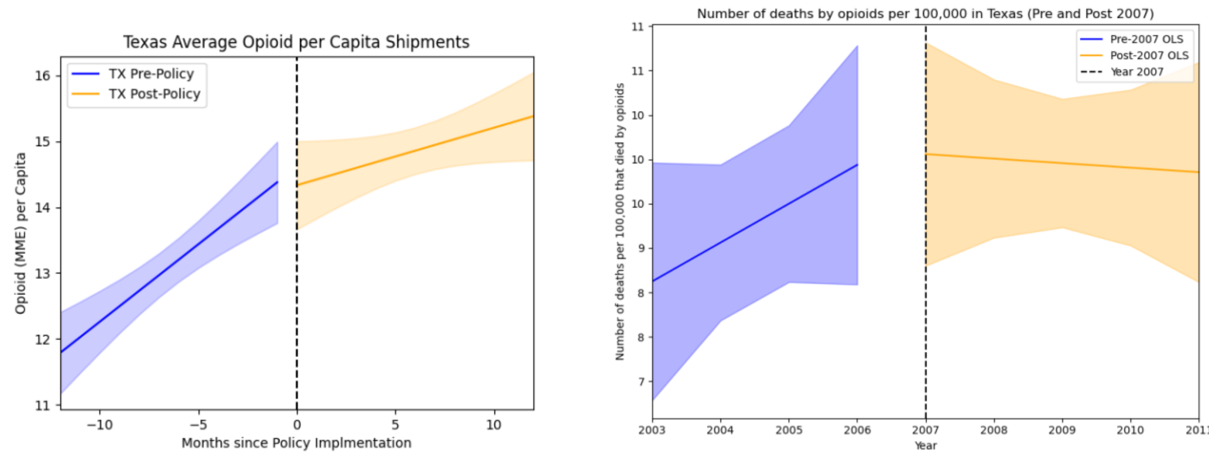

Figure 2 (left) shows the trend of the average monthly opioid (Morphine milligram equivalent) shipments per capita for the state of Texas before and after their policy implementation (scaled to number of months pre- and post-policy implementation in 2007 in Texas, i.e., up to a year before and after 2007). Figure 2 (right) shows the change in trends pre- and post-policy implementation in Texas, scaled to years (as annual data was available for mortality but not consumption). The y-axis represents opioid related mortality as the number of deaths per 100,000 people in Texas.

Interpretation

As with Florida, the summary statistics for Texas are somewhat misleading as well because they suggest an increase in opioid consumption and opioid related mortality after policy implementation. Figure 2 shows us that while these statistics reflect the average numeric value of mortality counts and opioid consumption pre and post intervention, they don't reflect the change in trends accurately. Before the policy implementation in 2007, the trend of the average monthly opioid shipments per capita was positive. This trend indicates an average monthly increase of approximately 1 shipment before the policy implementation. After the policy implementation, the trend continues but at a slower pace where the average monthly increase is approximately .5 shipments. Although there are slight differences in the pace of the trend, it partially supports the hypothesis that the policy was effective in the sense that it did help decrease the growth of the number of shipments. However, a contrasting perspective emerges when examining the number of deaths per 100,000 over years. There is an obvious decline in deaths, suggesting that the policy implementation might have had an impact. Though this decline is not as rapid/steep as seen in Florida, there is still a clear decrease in opioid-related mortality after policy implementation in Texas. This discrepancy could be attributed to the time frame considered, urging a need for further investigation before confirming or disputing our hypothesis.

Washington

The same pre-post analyses were conducted for Washington, with Table 3 presenting the mean consumption and mortality before and after policy implementation in 2012 and Figure 3 presenting the actual trends of pre- and post-policy opioid consumption and opioid-related deaths in Washington.

Table 3. Washington opioid trends: Changes in mean opioid consumption and opioid-related deaths pre- and post-policy implementation in 2012.

Washington	Pre-2012 Mean	Post-2012 Mean
Consumption (MME per cap)	361.92	387.17
Mortality (deaths per 100,000)	11.30	11.14

Figure 3. Pre-post 2012 policy implementation analysis for Washington.

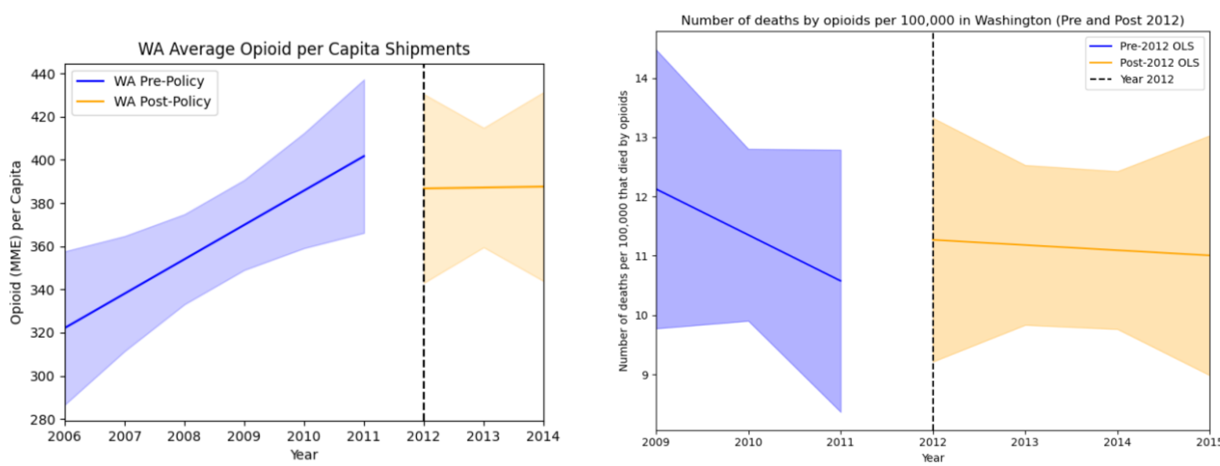

Figure 3 (left) shows the trend of the average annual opioid (Morphine milligram equivalent) shipments per capita for the state of Washington before and after their policy implementation. Figure 3 (right) shows the change in mortality trends pre- and post-policy implementation in Washington.

<u>Interpretation</u>

For Washington, the summary statistics (Table 3) present results that align with our intuition of pre-post analyses. However, Figure 3 puts forth more to unpack. Before the policy implementation in 2012, the trend of the average annual opioid shipments per capita was positive. This trend shows that every year the average number of shipments increases by around 20 per year. After the policy implementation, the trend flatlines, suggesting the policy had only a moderate effect. The post-policy trend neither confirms nor denies the hypothesis that the policy was effective in reducing the number of opioid shipments. However, we see a contrasting perspective emerge when examining deaths. From 2009 to 2011, the number of deaths per

100,000 decreases although the average annual opioid shipments per capita continues to rise. This downward trend in deaths persists even after the policy implementation, contrasting with the flattened trend in shipments. Since these results were counterintuitive, further analyses were done to understand these results for mortality (see Appendix B). It was found that these results can be attributed to the limited period selected for pre-post analyses for Washington. When the pre- period is extended to the beginning of the mortality dataset (2003), we can see a clearer increase pre-policy and decrease in mortality post-policy implementation (Figure 4).

Figure 4. Pre-post 2012 policy implementation analysis for opioid-related mortality in Washington, with the pre-policy time period extended to 2003.

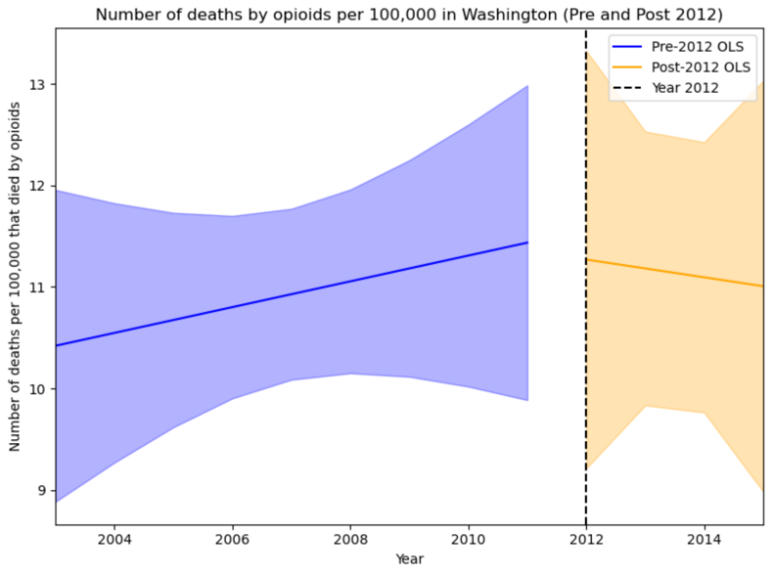

Figure 4. shows a redone pre-post 2012 policy implementation analysis for opioid-related mortality in Washington the pre-policy period extended to be 2003 to 2011 (compared to 2009 to 2011 in Figure 3). The y-axis represents opioid related mortality as the number of deaths per 100,000 people in Washington.

Difference-in-Difference Analysis

Difference-in-difference (DiD) analysis is employed to enhance the robustness of our evaluation by comparing the changes in outcomes between two groups over time—one group exposed to the intervention (treatment states) and another not exposed (control states). The choice of DiD over a simple pre-post analysis is motivated by several considerations. In a straightforward pre-post analysis, it can be challenging to attribute changes solely to the intervention, as various external factors may concurrently influence the outcomes. For instance, economic fluctuations, national health trends, or changes in healthcare infrastructure could impact opioid consumption and mortality rates, making it difficult to isolate the intervention's effects. DiD, by incorporating control states with similar pre-intervention trends, helps control for these external influences. This is crucial because changes observed in treatment states might be due to broader contextual factors rather than the implemented policies.

Examples of factors that could confound a simple pre-post analysis include the evolving landscape of opioid abuse, socioeconomic shifts, or changes in healthcare access. Suppose, for instance, that during the same period as policy implementation, there is a national campaign against opioid misuse affecting all states. A pre-post analysis might mistakenly attribute changes in opioid consumption and deaths to the specific policies when they are part of a broader nationwide trend. By using control states with similar pre-intervention trends, DiD accounts for such confounding factors. It helps create a counterfactual scenario by estimating what would have happened to the treatment states in the absence of the intervention, based on the behavior of the control states. This comparison allows us to attribute changes more confidently in outcomes to the implemented policies rather than external influences.

However, it is crucial to acknowledge that no statistical analysis is foolproof, and certain confounding factors could still influence DiD results. Factors such as unobserved policy differences between treatment and control states, changes in population characteristics, or regional disparities in opioid-related issues may introduce complexities. Nevertheless, DiD remains a valuable method for mitigating the impact of potential confounders and providing a more nuanced understanding of policy effectiveness.

Choosing the Control States

Selecting suitable control states is a crucial step in ensuring the robustness of our causal inference when evaluating the effects of opioid-related policies. Control states serve as reference points for comparison, allowing for a more accurate assessment of the intervention's impact. Our selection process involves a thorough analysis of trends both before and after the intervention, ensuring that chosen states exhibit patterns similar to those undergoing treatment, encompassing both opioid consumption and drug-related deaths. By identifying control states with trends closely resembling the treatment states pre-intervention, our approach captures states that we hypothesize have experienced trends post-intervention similar to what the treatment state would have had if no intervention occurred, covering both opioid consumption and mortality rates.

Taking Florida, which implemented policies in 2010, as an example, we scrutinized pre-2010 trends in opioid consumption and drug-related deaths per capita for each state. Control states were selected based on their demonstrated similarity in trends, considering our hypothesis about post-intervention dynamics for both opioid shipments and mortality. For example, a trend analysis identified Ohio, Michigan, Maine, and Hawaii as control states for Washington (other control states are specified in the following subsections).

The emphasis was on choosing control states that shared similar dynamics in both opioid trends and drug-related mortality, reflecting our hypothesis about the post-intervention trends the treatment state would have had if no intervention occurred. This meticulous approach ensures that our control states not only mirror the treatment states in pre-intervention trends but also align with our hypothesis about their patterns post-intervention.

Florida

An initial surface-level DiD analysis for Florida is presented in Table 4, which extended Table 1 by presenting pre-post consumption and mortality means for Florida and its control states (Kentucky, West Virginia, Tennessee, Nevada, and Oregon). The DiD trends are visualized in Figure 5.

Table 4. Mean opioid consumption and opioid-related mortality rates pre- and post-policy implementation in Florida and control states.

Metric	Florida/Controls	Pre-Policy	Post-Policy
Consumption (MME per cap)	Mean Opioid Per Capita for Treatment Group	483.00	617.42
	Mean Opioid Per Capita for Control Group	483.64	633.55
Mortality (deaths per 100,000)	Mean Death Rate for Treatment Group	10.29	10.56
	Mean Death Rate for Control Group	43.23	54.02

Figure 5. DiD analysis for opioid consumption and mortality rate in Florida against Control States.

Control states: Kentucky, West Virginia, Tennesse, Nevada, Oregon.

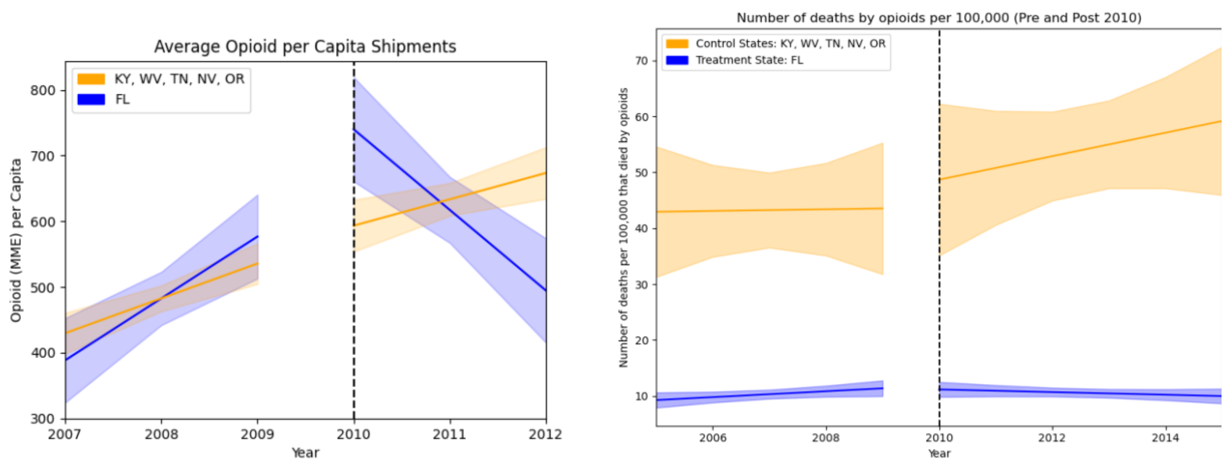

Figure 5 (left) shows the before and after policy implementation trend of the average annual opioid (Morphine milligram equivalent) shipments per capita for the state of Florida (blue) against Florida's control states (Kentucky, West Virginia, Nevada, Tennessee, and Oregon; depicted with the orange trend lines). Figure 5 (right) shows the before and after policy implementation trends of the number of deaths due to opioids per 100,000 people in the state of Florida (blue) against the same controls (orange).

Interpretation

As with the pre-post analyses, Table 4 provides counterintuitive results as it shows an increase in mean opioid consumption as well as opioid-related deaths in Florida and its control states pre- and post-policy implementation in 2010. However, we can see that this increase in means seems to be much larger in control states compared to our treatment state, suggesting a positive impact of policy implementation, which is more concretely shown in Figure 5. Comparing both trends, the control states opioid shipments continue to increase after Florida's policy implementation whereas Florida's opioid shipments decrease significantly. This difference in trend also supports the hypothesis that Florida's policy was effective in reducing the number of opioid shipments to Florida. The same results were found for mortality, however, just on a different scale. Florida's opioid-related deaths (per 100,000 individuals in the state) due to opioids was low compared to control states to begin with, and this saw a downwards trend after policy implementation. On the other hand, control states' number of deaths per 100,000 individuals continued to increase even after the year of policy implementation. Thus, when compared to states that did not implement the policy, these findings support the hypothesis that the decrease in opioid consumption and opioid-related deaths could confidently be attributed to policy implementation in 2010 in Florida.

Texas

Table 5 provides the initial summary statistics of the DiD analysis conducted for Texas, which show similar patterns as those observed for Florida (Table 4). However, a more comprehensive analysis of DiD for Texas can be observed in Figure 6, which provides the DiD trend lines for opioid consumption and mortality pre- and post-policy implementation in 2007 in Texas versus its control states (Missouri, Minnesota, Arkansas). It is important to note the DiD analysis for opioid consumption for Texas was done on a scale of months rather than years, as sufficient data was not available for pre-policy years (no data available before 2006).

Table 5. Mean opioid consumption and opioid-related mortality rates pre- and post-policy implementation in Texas and control states.

Metric	Texas/Controls	Pre-Policy	Post-Policy
Consumption (MME per cap per month)	Mean Opioid Per Capita for Treatment Group	13.08	14.86
	Mean Opioid Per Capita for Control Group	16.79	19.28
Mortality (deaths per 100,000)	Mean Death Rate for Treatment Group	9.28	9.95
	Mean Death Rate for Control Group	37.24	44.69

Figure 6. DiD analysis for opioid consumption and mortality rate in Texas against Control States.

Control States: Missouri, Minnesota, Arkansas.

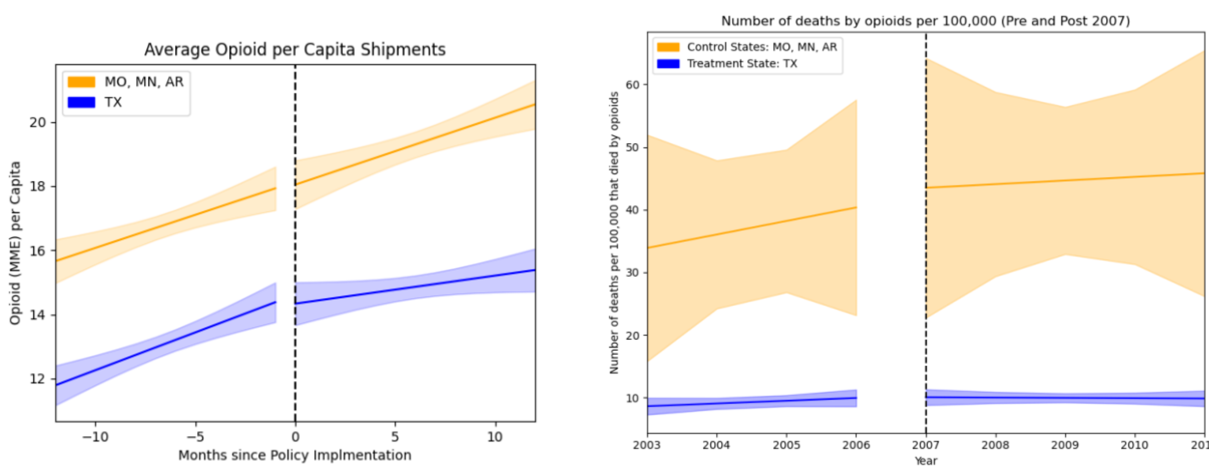

Figure 6 (left) shows the before and after policy implementation trend of the average monthly opioid (Morphine milligram equivalent) shipments per capita for the state of Texas against Texas' control states (Missouri, Minnesota, and Arkansas). Figure 6 (right) presents the number of deaths due to opioids per 100,000 individuals in Texas, pre- and post-policy implementation in the year 2007. This is scaled up to years in comparison to the opioid consumption plot. Texas' trend line (blue) is compared against its combined controls' trend line (orange).

Interpretation

Again, the mean consumption and mortality numbers for Texas alone seem misleading, however, when compared to its treatment states, it is observed that the increase in means in Texas is noticeably smaller. These results are better elaborated upon in the DiD visual analysis (Figure 6). Comparing both trends, the control states and Texas' monthly shipments continue to increase after Texas's policy implementation. While it could be argued that the policy did not entirely achieve its desired outcomes due to the noticeable upward trend post-implementation, there are a few factors which must be considered. First, this increase post-intervention is much steeper in control states compared to Texas, presenting the possibility of a 'slowly but surely' effect of the policy in Texas. We can also discern that even though there is an upwards trend for opioid consumption in Texas post-intervention, the trend is not as fast (or steep) as it was pre-intervention, indicating a decline in the growth rate of opioid consumption post-intervention. Furthermore, as mentioned in the pre-post analysis interpretation, the results for Texas are only up to a year before and after policy implementation which significantly narrows the scope for interpreting the effectiveness of the policy. It is possible that if more data were available both before and after policy implementation, we could have seen clearer results regarding the effectiveness of implementing the policy in Texas in 2007 compared to the control states (which would have shown a continued upwards trend according to our hypothesis). For mortality, again, the results are slightly more difficult to visually interpret because of the difference in the scale of number of deaths per 100,000 in Texas versus its control states. However, overall, the analysis indicates the anticipated impact of the policy on mortality rates. Following its implementation in 2007, opioid-related deaths per 100,000 significantly decreased, contrasting with the upward trajectory of deaths per 100,000 which persists in the control states, emphasizing the effectiveness of policy implementation in Texas.

Washington

The same process of DiD analysis was carried out for Washington, with the summary statistic results presented in Table 6 and the visual analysis of DiD trends represented in Figure 7. Based on analyzing similar trends in opioid consumption and mortality, the control states chosen for Washington were Ohio, Michigan, Maine, and Hawaii.

Table 6. Mean opioid consumption and opioid-related mortality rates pre- and post-policy implementation in Washington and control states.

Metric	Washington/Controls	Pre-Treatment	Post-Treatment
Consumption (MME per capita)	Mean Opioid Per Capita for Treatment Group	361.99	377.65
	Mean Opioid Per Capita for Control Group	337.99	379.65
Mortality (deaths per 100,000)	Mean Death Rate for Treatment Group	11.30	11.14
	Mean Death Rate for Control Group	24.36	31.59

Figure 7. DiD analysis for opioid consumption and mortality rate in Washington against Control States.

Control States: Ohio, Michigan, Maine, Hawaii

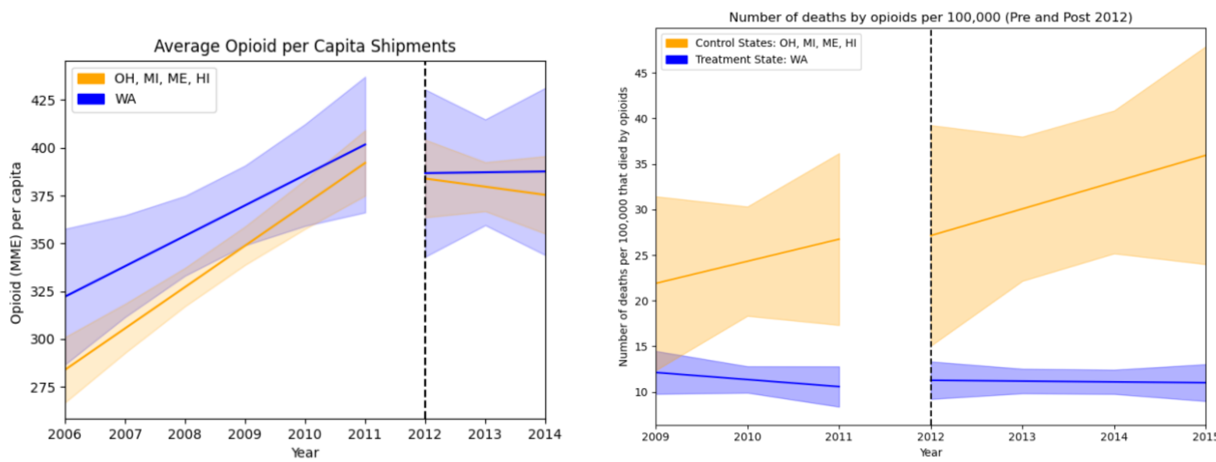

Figure 7 (left) shows the before and after policy implementation trend of the average annual opioid (Morphine milligram equivalent) shipments per capita for the state of Washington against Washington's control states (Ohio, Michigan, Maine, and Hawaii). Figure 7 (right) presents the number of deaths due to opioids per 100,000 individuals in Washington, pre- and post-policy implementation in 2012, where Washington's trend lines (blue) are compared against its combined controls' trend lines (orange).

Figure 8. DiD analysis for mortality in Washington against control states with a larger time frame pre-policy implementation (2003 to 2011).

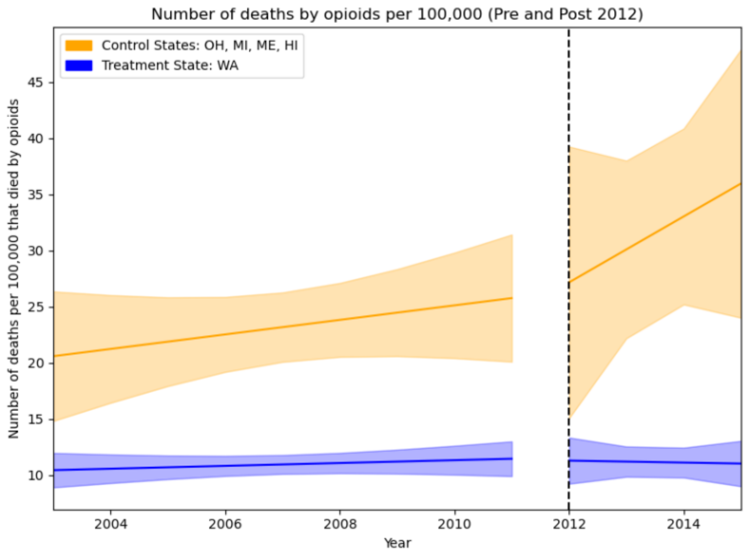

Figure 8 presents the number of deaths due to opioids per 100,000 individuals in Washington, pre- and post-policy implementation in 2012 (with a larger pre-policy span of 2003 to 2011), where Washington's trend lines (blue) are compared against its combined controls' trend lines (orange).

Interpretation

The DiD analysis for Washington also presents expected results. Beginning with the summary statistics presented in Table 6, it is seen that the mean opioid consumption rates for Washington are quite comparable to its control states both before and after policy implementation. When the trends are compared in Figure 7, the control states opioid shipments decrease after Washington's policy implementation whereas Washington's opioid shipments flatlines. This difference in trend brings the effectiveness of policy implementation in Washington to question. However, it is important to note that the opioid consumption slope increases at a much steeper rate pre-policy implementation in Washington, introducing the possibility of the effectiveness of policy change which is not as noticeable due to the limitation of available data post-policy implementation. Nevertheless, this does not adequately address the decrease in opioid consumption in control states post-2012, bringing us back to the argument that even DiD is susceptible to confounding factors which may be reflected in the current results. For example, it is possible that one or more of the control states implemented rigorous opioid-control policies, there were demographic changes, or other population/regional disparities which could result in this downward trend observed in control states.

Contrarily, these means show the expected results for mortality, where it decreases in Washington, while there is a significant increase in control states. However, the trends in deaths per 100,00 due to opioids were a bit difficult to interpret. The trend line for Washington seems to go downwards before the policy implementation in 2012 and then stabilizes after policy implementation, rather than going further down. Again, this could be attributed to the limited period used for the analysis (see Appendix B). Figure 8 provides a clearer understanding of the DiD analysis, which clearly shows the expected result of policy implementation on opioid-related mortality in Washington. While there are clear upward trends in opioid-related mortality pre-policy implementation in both Washington and its control states, these trends have a stark divergence post-policy implementation with Washington’s mortality showing a decreasing trend while the control states opioid-related mortality is still on the rise, emphasizing the effectiveness of policy implementation on opioid-related deaths in Washington.

CONCLUSION

The opioid crisis gripping the United States demands a comprehensive understanding and effective policy responses to alleviate its devastating toll. Our study, centered on Texas, Florida, and Washington, engaged in a nuanced exploration of the impact of opioid-control policies on consumption and mortality. Grounded in the escalating human cost of opioid-related deaths, which surged from approximately 21,000 in 2010 to a staggering 80,000 in 2022, our research sought to bridge the gap between academic inquiry and tangible public health outcomes.

Policymakers in the treatment states responded to the crisis with a range of interventions, reflecting the multifaceted nature of the problem. Florida, known for excessive opioid prescriptions, implemented measures such as registering pain clinics and launching prescription drug monitoring programs in 2010. Texas established controls in 2007, emphasizing patient consent and meticulous record-keeping. Washington, in 2012, enforced rules in pain treatment, signaling a comprehensive approach to opioid control.

The outcomes of our analyses revealed divergent responses across states. Florida showed a clear positive impact post-policy implementation, witnessing reductions in opioid consumption and related deaths. Texas displayed stabilization in opioid consumption trends and a noticeable shift in mortality rates post-intervention. However, Washington's experience was nuanced, with an unexpected plateau in opioid consumption and mortality. It was concluded that analyses for Texas and Washington could benefit from a longer span of years pre- and post-intervention to gain a clearer understanding of policy effectiveness.

While our study provides valuable insights, it also underscores the complexity of the opioid crisis and the need for tailored, context-specific policy approaches. The use of a population threshold, while ensuring statistical reliability, accentuates the imperative for nuanced strategies in smaller communities. Future research should delve into these intricacies to refine policy recommendations and address regional variations in policy effectiveness. Our findings highlight the imperative for responsive and adaptable policies. Acknowledging the diverse responses observed across states, a one-size-fits-all approach may fall short. As the opioid crisis evolves, our study serves as a call to action for ongoing exploration, adaptation, and refinement of opioid-control strategies to maximize public health benefits.

The unexpected plateau in consumption and mortality rates post-policy implementation in Washington prompts reflection on the dynamic nature of the opioid crisis. It underscores the need for a holistic understanding of the intricate relationship between policy interventions and public health outcomes, acknowledging the potential for unintended consequences.

Managing missing values in mortality data introduces potential biases. The decision to use a population threshold for the data, while consistent with established methodologies, limits generalizability to smaller communities. Variability in opioid shipments data availability, notably in Texas, complicates the establishment of comprehensive trends, requiring cautious interpretation. Relying on a pre-post analysis oversimplifies the multifaceted interplay of policies, societal responses, and external factors influencing opioid dynamics, emphasizing the need for nuanced, context-aware interpretations.

In closing, our study contributes not only to academic discourse but, more importantly, to the ongoing efforts of policymakers, public health officials, and communities grappling with the far-reaching impact of the opioid crisis. Through a human-centered lens, we aspire to guide evidence-based policymaking that fosters healthier, more resilient futures for all. The complexities unveiled in our exploration urge continuous vigilance and responsiveness in the face of a crisis that demands multifaceted and evolving solutions.

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Appendices

Appendix A: Population Threshold and Imputation

Thresholding is a method used to filter data based on a specified criterion, to refine datasets and improve the quality of subsequent analyses and ensure that the considered datapoints all fall within a consistent scope. In the context of this study, a population threshold was applied to limit the dataset to counties with populations above a certain level. The rationale behind this approach is to address concerns related to missing data and ensure that the selected counties contribute meaningfully to the overall analysis.

To determine a population threshold, some exploratory analyses were conducted to understand the extent to which cutting down this dataset would impact further analyses. The number of counties with all null mortality data was calculated for each population threshold (with increments of 5000). That is, the analysis began with no threshold, thus showing the total number of counties with all null mortality values in the dataset for a given state. Then, a threshold of 5000 was set, thus, only counties with populations higher than 5000 were included, and the total number of counties with all null values were revealed again. This iteration continued till no more missing values were present in the data.

For each state, a point of stabilization was determined from initial visual interpretation. After this, further analyses were done to ensure that approximately 90% of counties will *all* null values were excluded at the set threshold, the total information loss was not higher than 40%, and that threshold was around 50th quantile for each state. This was not the case for Texas; however, this is further justified in its subsection.

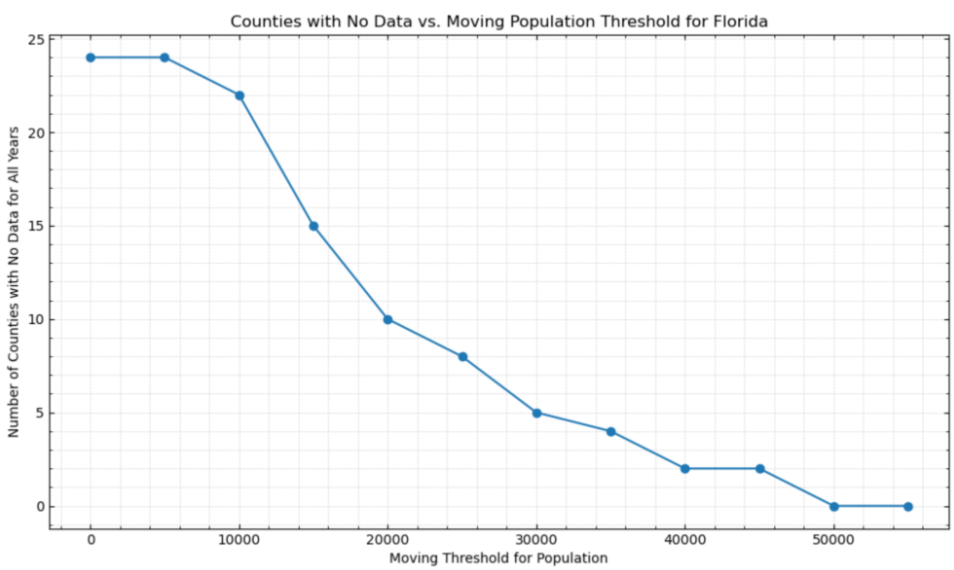
The philosophy behind this thresholding lies in balancing the need for a manageable dataset with the requirement for a sufficiently representative sample. By excluding counties with lower populations, where data might be sparser due to privacy concerns or other factors, the analysis focuses on areas where more comprehensive and reliable information is available. A lower number of counties post-threshold filtering suggests a more focused analysis but raises the question of whether the remaining data is sufficient to draw meaningful conclusions about the impact of opioid-related policies. The hypothesis is that the refined dataset, though smaller, still contains enough representative information to provide insights into the studied phenomenon. However, this hypothesis should be further validated through rigorous analysis and interpretation of the remaining data. The same thresholds selected for each state were then used as a threshold for their respective control states to maintain consistency across each difference-in-difference analysis.

Finally, after the thresholding was applied, all remaining counties with *all null* mortality values across our entire range of years were removed. This was to maintain data integrity, as imputing at a state level was deemed inappropriate due to the severe heterogeneity among counties within each state. For example, in Texas, the least populous county in 2022 was Loving County with a population of 51 people, while the most populous county was Harris County with a population approaching 5 million as per the Texas Demographics report in 2022. Followingly, for those counties that remained, some had mortality values for the entire range of years needed for each state, while some had values missing for only certain years. Afterwards, we calculated the death *rates* for each county from their death counts and populations for years where our death count data was not missing. Finally, imputation was used to complete the dataset, filling in the observations for the missing years for each county with the average of the available death rates for that county only.

Florida

As seen in Figure A1, the number of counties with all missing values for mortality stabilizes at approximately 40,000. That is, counties with populations over 40,000 are more likely to have incomplete or complete data for mortality for all years we are focusing on (2005-2015). The initial dataset included 67 counties for Florida. After applying the population threshold, the dataset was refined to 44 counties. Furthermore, applying this threshold reduced the number of counties with all null values from 24 to 3. Total information lost due to this 40,000 threshold was limited to 27.13% and the missingness percentage went from 37.47% to 14.69%. The remaining counties with all null values were removed, as we wanted to avoid imputing values to these counties without any basis of comparison, thus, mitigating the possibility of overfitting with these new fake values. Of the remaining counties, there were 14 counties with missing data (80 missing out of 764 total observations). Imputation using the mean death rate of each county based on the number of deaths divided by the population was used to impute values.

Figure A1. Population threshold vs. The number of counties with no deaths reported for the entire range of years examined in Florida

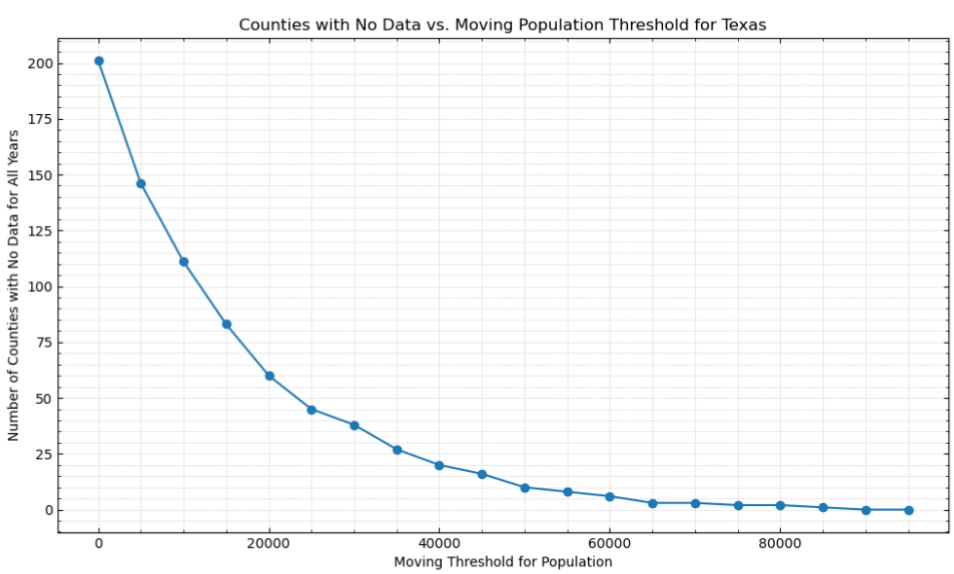


Texas

Similar thresholding procedures were applied to the dataset for Texas. For Texas, with an initial dataset of 254 counties, the population threshold led to a refined dataset of 75 counties. Figure A2 shows the decrease in the number of counties with all missing data with an increase in the threshold, stabilizing at approximately 40,000. This resulted in the total proportion of missingness going from 83.38% to 48.54%, with a total information loss of 67.89%. Though this rate of information loss seems extremely high, this was intentionally done to maintain a representative population and enhance data quality while maintaining a sufficient sample size (*n* = 829). By focusing on larger counties, the refined dataset could capture regions with substantial populations, enhancing the statistical power and generalizability of analyses. Furthermore, the thresholding substantially reduced the missingness, significantly improving the quality of completeness of data. Therefore, although this resulted in information loss, it prioritized data accuracy and reliability by excluding counties with sparse and potentially less reliable data, and furthermore, reducing the potential to add fake data with excessive imputation.

After this threshold was set, there were still 22 counties with no mortality data available for all the years in question (2003-2011) which were dropped. Furthermore, there were 35 counties with missingness for only some years (260 missing out of 829 total observations). Death rates were imputed for these counties. These adjustments through threshold filtering aim to strike a balance between dataset manageability and maintaining a representative sample for robust analyses.

Figure A2. Population threshold vs. The number of counties with no deaths reported for the entire range of years examined in Texas



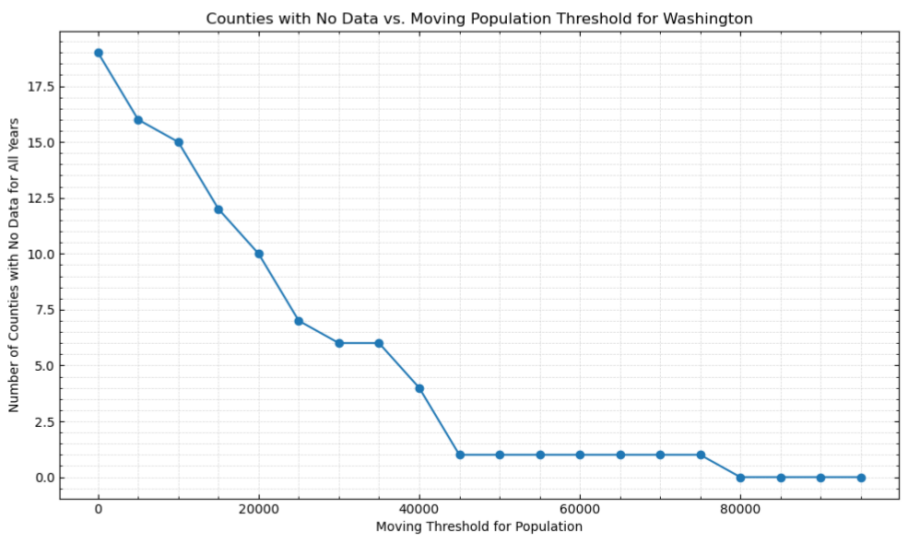
Thresholding is a crucial step in refining the dataset for meaningful analyses, particularly in the context of a state as vast and diverse as Texas. The state's substantial number of counties, 254 in total, includes a wide range of population sizes. By implementing a population threshold, we address the practical challenge of comparing counties with vastly different scales of population. The necessity of thresholding extends beyond merely controlling for missing values. It also acknowledges the impracticality of directly comparing counties with very small populations to those with significant demographic sizes. For instance, the dataset includes both sparsely populated counties where individual deaths might be easily identifiable and densely populated counties like Harris County, with a population approaching 5 million.

Thresholding helps ensure that the selected counties for analysis are not only representative but also comparable in scale. This approach allows for a more meaningful examination of the impact of opioid-related policies, considering the heterogeneity in county sizes across the state of Texas.

Washington

Finally, the same steps were carried out for Washington, where the initial dataset included 39 counties, and after applying the population threshold, it was refined to 21 counties. Here, the threshold was set to 45,000 as this is the point where the stabilization of counties with all missing values occurred (Figure 3A). This resulted in the total proportion of missingness going from 52.64% to 22.90%, with a total information loss limited to 39.01%. After applying this threshold, only one county had all null mortality observations, and this was dropped. From the remaining counties, there were 10 counties with some missing mortality information (64 missing out of 340 total observations). Mean death rates were then imputed for these counties to complete the dataset.

Figure A3. Population threshold vs. The number of counties with no deaths reported for the entire range of years examined in Washington



Appendix B: Washington Mortality Trends

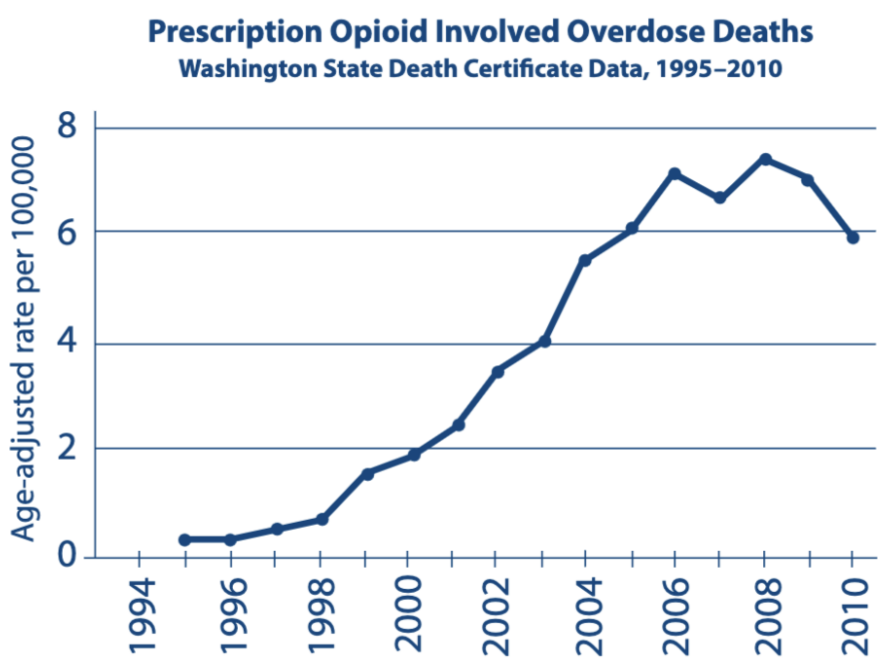
After examining Figure 3, we opted to do further investigation after comparing the other pre-post analyses. Unlike the other two treatment states pre-policy, we can see that although the yearly average opioid per capita is increasing, the number of deaths per 100,000 people drop during our initial time frame from 2009-2012. This trend appears counterintuitive, especially considering that the analyses of the other treatment states demonstrated similar increasing patterns for both opioid shipments per capita and the number of deaths per 100,000 people.

A plausible explanation for this anomaly could be the influence of additional policies we may have overlooked. For instance, Washington implemented a 'naloxone-related Samaritan law' in 2010, granting legal immunity to opioid users during overdoses or to bystanders assisting them in seeking medical attention (Franklin et al., 2015). Despite limited funding, this law could contribute to the observed downward trend in deaths and needs consideration in our analysis.

Furthermore, it was noted by Franklin et al., although they could not attribute what specific intervention caused this, the prescribed opioid death rates did decline by 27% from 2008 to 2012. Figure 3 (right) does support this statement as we do see this downward trend from 2009 to 2011.

After examining the article, we concluded that our time-frame choice for our initial analysis may be confounding our visualizations. Looking at the Injury and Violence Prevention Guide published by Washington State, we confirm that the age-adjusted death rate per 100,000 does start declining from 2008 to 2012 for prescription opioids.

Figure B1. Age-adjusted death rate per 100,000 over a time period of years 1995-2010 (Washington State Department of Health)



To account for this, we felt it was important to extend the timeframe to 2003 such that we look at the years from 2003 to 2011 before the policy implementation this study analyzes, and that extended range is depicted in figure 4 and figure 8. This extension is essential to provide a more comprehensive perspective on the trend in deaths per 100,000, allowing us to capture the impact of opioids over a longer timeframe and paint a more detailed picture of their effects.