The Effect of Medical Cannabis Legalization on the Medicaid Opioid Prescription Rate

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Abstract

The opioid epidemic has become one of the most pressing public health issues over the past two decades as both prescription and overdose rates have risen. One proposal to curb the prescription rate of opioids is the use of medical cannabis as a substitute painkiller. This paper examines the effect of medical cannabis on the prescription rate of opioids among Medicaid participants. Using three variations on a difference-in-differences approach, I find that medical cannabis implementation produces a reduction in the Medicaid opioid prescription rate of around 10%, which first appears about a year post-implementation and remains steady in subsequent years. These results suggest that medical cannabis legalization could potentially shrink the market for prescription opioids and help to curb the opioid crisis.
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I. Introduction

A. A Brief History of Opioids in the United States

While opioid painkillers have been used in medicine for centuries, they were used sparingly until the production of synthetic drugs with a perceived low risk of addiction. In 1995, Purdue Pharma first released OxyContin, marking the beginning of a rapid increase in opioid prescription rates that has had widespread public health effects. Since 2000, the national opioid prescription rate has more than doubled, despite a drop-off in the past five to six years (Cleveland Fed, 2018). The effects of this increase are largely considered to have come in three waves: in the early to mid-2000s, high prescription rates caused a mild increase in overdose rates, accompanied by increasing rates of addiction; following the introduction, in the early 2010s, of restrictions on prescription opioids intended to curb potential addiction, high prices drove users addicted to prescription drugs to cheaper, more dangerous street alternatives; the late 2010s have seen the rise of fentanyl, a very cheap but extremely dangerous street opioid (poison.org, accessed 2019). In short, the rise of intensive opioid prescribing caused high rates of addiction, which has led to high overdose rates among users of both prescription and street opioids (Cleveland Fed, 2018). This three wave phenomenon can be seen in Figures 1 and 2, showing the early rise in prescription opioid overdoses.
followed by a rise in illicit opioid overdoses that was simultaneous with decreases in prescription rates and overdoses.

Today, drug overdose rates are 137% higher than in 2000, driven by a 200% increase in opioid overdose rates, and overdose is now the leading cause of death for Americans under the age of fifty (Meldrum, 2016). In addition, evidence has shown that the most important determinant of opioid overdose rates is a region’s opioid prescribing rate, implying that reducing prescribing rates may have significant public health effects (Boston Fed, 2019). Thus, the body of evidence suggests that a balance must be struck; while reducing the prescription rate may reduce overdose rates, simple supply-side restrictions may have additional negative public health effects as addicts turn to street drugs instead. Therefore, affecting prescription rates by reducing demand may be one of the most effective methods to ameliorate the opioid crisis.

B. Thesis Summary

One proposed demand-side solution to the problem of high opioid prescription rates is the legalization and use of medical cannabis. Given the purported painkilling properties of marijuana, some have suggested that cannabis could function in the market as an effective substitute for opioids (Hopkins Bloomberg, 2018). However, actual painkilling ability of cannabis aside, it is unknown whether legalization does in
fact induce this substitution effect, or how any effect legal medical cannabis may have persists or fades over time once legalization has occurred.

This analysis seeks to quantify the effects of medical cannabis legalization on opioid prescriptions among Medicaid recipients. I believe that the reduced risk of addiction and less severe side effects would draw these individuals away from opioids if given the opportunity, despite the relatively high cost of medical cannabis. If this phenomenon were to occur, one would see a reduction in the opioid prescription rate after cannabis implementation. I will also explore the time paths of these effects, in an attempt to determine if any substitution effect changes in magnitude over time, or if it first occurs in a period other than the first period after implementation.

This study uses data from the Center for Medicaid and Medicare Services’ State Drug Utilization dataset for the years from 2011 to 2016. Additional data are drawn from the Center for Disease Control and the Marijuana Policy Project as primary data. This analysis uses three different regressions: a simple difference-in-differences, a parametric difference-in-differences, and an event study analysis.

The results of these regression analyses suggest that a substitution effect may take place following medical cannabis implementation. In short, legalization induces roughly a 10% reduction in the Medicaid opioid prescription rate that occurs around a year after cannabis implementation. This effect stays steady at this level in the
subsequent years after it first appears, although it may fluctuate slightly. These results imply that medical cannabis legalization may have a lasting negative effect on the opioid prescription rate.

Section II discusses the previous literature related to the painkilling qualities of cannabis and the potential association between medical cannabis use and reduced opioid use. This is followed by an examination of the data sources in Section III and an explanation of the regression methodology in Section IV. Regression results are presented in Section V. Section VI is a discussion of the results and their implications, in addition to a discussion of this thesis’s limitations and a few suggestions for future research. The paper then concludes with a summary of the results and related policy implications in Section VII.

II. Literature Review

A. Cannabis as a Painkiller

One of the factors that could significantly influence the substitution effect between cannabis and opioids is the efficacy of cannabis as a pain reliever. In short, the more effective cannabis is at relieving pain, the better a substitute it can be for opioids, and the larger the effect may be. While the body of literature is relatively small, cannabis has so far been shown to provide relief for chronic and severe pain, the types of pain for which opioids are usually prescribed (Hill, HMS, 2016). In addition, some
studies have found a biological interaction between cannabis and opioids that increases the painkilling potency of opioids, thereby requiring a smaller opioid dose to receive the same level of pain relief (Abrams et al., 2011). Therefore, it may be reasonable to hypothesize that medical cannabis could serve both as an outright substitute and as a product that can substitute for a portion of a dose of opioids, and thus could affect the prescription rate through both channels.

B. The Substitution Effect

While it may be the case that cannabis serves as an effective painkiller, that might not translate into any real-life substitution between opioids and medical marijuana. Again, although the body of research is relatively small given the short time since legalization of cannabis in most states, there have been a number of studies that have examined the relationship between medical marijuana and opioids.

Lucas and Walsh (2017) surveyed medical cannabis users on a variety of topics, including the patients’ medical reasons for using cannabis, and any drugs they may be substituting away from into medical marijuana. First, they found that 53% of respondents were using medical cannabis for pain relief, with 36% of people using it for chronic pain specifically. Second, they found that 63% of respondents used medical cannabis as a substitute for prescription medication, and that 32% of respondents were substituting for opioids in particular. Additionally, the two most frequently reported
reasons for such substitution were that cannabis had fewer adverse side effects and that cannabis was safer than prescription medication. While this study does not empirically show any substitution effect, it lends credence to the notion of substitution, as many medical cannabis patients do in fact use marijuana as an opioid substitute.

Boehnke et al. (2016) surveyed medical cannabis patients with chronic pain on their patterns of opioid use. Of those surveyed, 45% reported improved quality of life and a reduction in adverse side effects since beginning use of medical cannabis. Furthermore, the patients surveyed reported a mean reduction in opioid use of 64% from a baseline level of use before beginning treatment on cannabis. Similar to Lucas and Walsh (2017), this study does not empirically show any substitution effect in the market for pain relief as a whole, but does provide evidence of a behavioral reason for substitution to take place, as patients use fewer opioids for pain relief and report higher quality of life once on cannabis.

Bradford and Bradford (2016) examined the number of prescriptions filled on Medicare Part D for a variety of conditions purportedly treated by medical cannabis. They used a simple difference-in-differences regression framework, looking at the number of prescriptions written, for certain classes of drugs at the physician-year level, clustering standard errors at the physician level. While they found reductions in the number of prescriptions written for a multitude of drugs, they found that the largest
change occurred with pain medication. According to this study, physicians prescribed
over 1,800 fewer daily doses of pain medication (on Medicare Part D) per year following
legalization of medical cannabis. While this study may not directly show any
substitution effect with opioids in particular, it does show that such a substitution effect
may take place for pain medications in general, which hints at the efficacy, both real
and perceived, of cannabis both as a painkiller and as a substitute. In addition, given
that only around 20% of medical cannabis users are over the age of fifty, the measured
substitution effect for all pain medications as elucidated in this study may actually be
understated, given that the demand for medical cannabis comes mostly from people
younger than those on Medicare. Therefore, this study serves as solid evidence
supporting the hypothesis that legalization would reduce the opioid prescription rate.

McMichael et al. (2019) examined the effect of medical and recreational cannabis
legalization on the nationwide opioid prescription rate over a period from 2011 to 2017.
They used an ordinary least squares regression similar to a difference-in-differences,
with year fixed effects and provider fixed effects, estimating the morphine milligram
equivalents (MMEs) prescribed by a provider annually. Using a dataset of 1.3 billion
individual prescriptions aggregated at the provider level, they found a 6.1% reduction
in MMEs due to medical cannabis legalization, and a 6.9% reduction due to recreational
cannabis legalization. This reduction represents an annual difference of about half a
kilogram of morphine per provider. In addition, medical cannabis legalization reduced the total days supply of opioids prescribed per provider by 7.7%, while recreational cannabis legalization reduced that rate by 5.5%. In short, this paper, while it is concerned with the national opioid prescription rate and not the Medicaid rate, and while it does not examine how the substitution effect may change over time, serves as empirical evidence of a mean effect post-legalization.

Wen and Hockenberry (2018) studied the effect of both medical and recreational legalization on the Medicaid prescription rate, similar to the method undertaken in this thesis. They used State Drug Utilization Data from the Centers for Medicare and Medicaid Services from the years 2011 to 2016, aggregated at the state-quarter level. Methodologically, they used a simple difference-in-difference model with state and year effects to estimate the prescription rate per 1,000 Medicaid enrollees. They found a 5.88% reduction in the prescription rate with medical cannabis legalization, and an additional 6.38% reduction in that rate with adult-use recreational cannabis legalization. These changes represent a difference of about 40 opioid prescriptions per 1,000 enrollees annually, from a pre-legalization mean of about 670 prescriptions per 1,000 enrollees. In addition, adult-use legalization lowered Medicaid spending on opioids by 9.78%, representing an $1815 change per 1,000 enrollees. Although Wen and Hockenberry’s
study does not examine the nature of the effect over time, it does show the exact mean substitution effect I hope to find, giving a basis for results I will attempt to replicate.

Given this body of literature, there are a few goals I hope to accomplish with this thesis. First, as previously stated, I hope to replicate the results of Wen and Hockenberry (2018) by producing an estimated effect reasonably close to their paper’s result, as my simple difference-in-differences regression specification is only slightly different. Second, I hope to expand on this body of literature by examining how the substitution effect, if it does occur, evolves over time, and whether there exists a sort of “post-legalization equilibrium” at a lower prescription rate than before legalization. Lastly, I hope to examine more closely the timing of any substitution effect, specifically whether there is any lag in the effect post-legalization to a period some time after medical cannabis has been implemented rather than in the same period as implementation. In short, I hope to confirm the results found in other work and to gain a more nuanced picture of those results.

III. Data

A. Opioid Prescription Data

Prescription data were acquired through the National Bureau of Economic Research, as originally obtained and cleaned from the State Drug Utilization Data from the Center for Medicare and Medicaid Services. These data represent all outpatient drug
prescriptions covered by Medicaid fee-for-service and managed care, in quarterly totals for each drug by state. I then kept only the opioid prescriptions using the CDC’s list of opioid National Drug Codes (NDCs), and aggregated different opioids into a total for prescriptions in a given state and quarter. These totals were simple counting totals, not MME totals or a similar dosage equivalent. I used the aggregate data values from 2011 to 2016 in the regression analyses. The reason I used only this time period is twofold: first, Medicaid data collection improved considerably in 2011 following the passing of the Affordable Care Act, and, second, this time period is close to that used by Wen and Hockenberry (2018), allowing me to better replicate their results. Lastly, I employed a 99% winsorization to deal with extreme outliers that are likely due to poor data reporting in a state in a specific quarter.

B. Other Data

I used dates provided by the Marijuana Policy Project as my medical cannabis legalization times. Importantly, I use the dates on which medical cannabis was first available to patients, not when the law was first passed, to eliminate the delay that would occur in a substitution effect if the timing of the law were treated as the determining factor. Thus, in states in which legalization has occurred, a policy indicator is equal to 1 in the quarters after cannabis implementation, and is equal to 0 in prior quarters. In states without any legalization, this indicator is always equal to 0. In
addition, I also include a variable to represent the number of quarters either until or since implementation. The quarters and years for implementation for all states that have legalized as of the end of 2018 can be seen in Table 1.

I also include Medicaid enrollment data for the period from 2011 to 2016, in order to produce values for the number of quarterly prescriptions per 1,000 Medicaid enrollees in a particular state. These data were acquired through the Center for Medicare and Medicaid Services, and include enrollment numbers for each state as of the beginning of the third quarter of a particular year. Therefore, enrollment remains constant for the last two quarters of one year into the first two quarters of the next year, and then changes, which presents some minor limitations that will be discussed later.

Lastly, I include three other covariates that may have an effect on the opioid prescription rate. First, I include an indicator for whether or not a state has adopted Medicaid expansion, based on dates provided by the Kaiser Family Foundation. While the effect expansion has on pure enrollment numbers would not have any effect on the prescription rate per 1,000 enrollees, the change in demographics of Medicaid enrollees might cause some change in the effect of cannabis legalization on opioid prescriptions. Second, I include a yearly measure of the number of primary care physicians in a state per 100,000 residents, acquired from the Association of American Medical Colleges’s State Physician Workforce Book. This gives a relative measure of healthcare access, as
more access to physicians may increase the prescription rate. Lastly, I include a state’s poverty rate in a given year, as given by the U.S. Census Bureau’s Current Population Surveys. The poverty rate has been found to affect the Medicaid opioid prescription rate (Wen and Hockenberry, 2018), so I am including it in my analysis as well. An example of what a few data points in their “final state”, with all necessary controls included, look like, can be seen in Table 2.

IV. Methods

To estimate the effect of medical cannabis legalization on the Medicaid opioid prescription rate, I use a set of three regression specifications based on a difference-in-differences (DD) framework. After accounting for differences in prescription rates between states and across years, this framework compares states that have experienced medical cannabis implementation (a treatment group) against a counterfactual of states that have not experienced implementation (a control group). The assumption that allows for this DD specification to function properly is the “parallel trends” assumption; namely, that prescription rates in legalization and non-legalization states moved parallel to one another pre-implementation. Wen and Hockenberry (2018) gives evidence that the parallel trends assumption does hold, thereby suggesting that DD is a
valid specification to be using. In addition, I hope to confirm the existence of parallel
trends.

My most basic regression specification is a simple difference-in-differences, thus
modeling an aggregate post-treatment shift in the Medicaid opioid prescription rate in
states having undergone medical cannabis implementation:

(1) \[ p_{st} = \delta_s + \tau_t + 1(t > t_s^*)\beta_1 + \epsilon_{st} \]

Here, \( p_{st} \) represents the number of prescriptions filled per 1,000 Medicaid
enrollees in state \( s \) in year \( t \). \( \delta_s \) and \( \tau_t \) represent state and year fixed effects, respectively,
to control for differences between states and across years. \( t_s^* \) represents the quarter in
which state \( s \) implemented medical cannabis; therefore, \( 1(t > t_s^*) \) is equal to 1 for any
legalization state that is post-implementation, and is equal to 0 for legalization states
pre-implementation and for non-legalization states.

With any underlying differences across states and years controlled for, any
estimated change in prescription rate induced by the triggering of this dummy can be
attributed to implementation. However, as there is no distinguishing factor to denote
how far out from implementation a certain data point is in this regression, this
estimation only represents the average change in the prescription rate of any particular
state in the treatment group post-implementation. Thus, the coefficient estimate \( \beta_1 \)
represents the mean post-treatment change in the opioid prescription rate in states
having implemented medical cannabis. In addition, I cluster standard errors at the state level both for this specification and both other specifications.

Medical cannabis implementation may not affect the opioid prescription rate immediately, nor would it necessarily reach its maximum effect until a bit of time has passed since implementation. Additionally, states with medical cannabis may begin to drift away from states without medical cannabis even before implementation, which has implications related to the randomness of legalization/implementation. Therefore, I add two trend variables to (1) to capture variations over time, producing a parametric difference-in-differences:

\[
 p_{st} = \delta_s + \tau_t + 1(t > t_{s}^{*})\beta_1 + (t - t_{s}^{*})\beta_2 + 1(t > t_{s}^{*})(t - t_{s}^{*})\beta_3 + \varepsilon_{st}
\]

The variable \(1(t > t_{s}^{*})(t - t_{s}^{*})\) increases by a value of 1 for each period post-implementation; any change in prescription rates induced by this increase can be interpreted as due to the magnitude of a substitution effect increasing over time. Thus, \(\beta_3\) represents the estimated change in prescription rates induced by an additional period of time passing post-implementation. \(\beta_2\) represents a test of the randomness of legalization; this measures changes in state \(s\) prior to \(t_{s}^{*}\) in comparison to states without any medical cannabis implementation. Thus, \(\beta_2 =/= 0\) would suggest that treatment group states diverge from control group states before the date of implementation. This
then implies that, if $\beta_2 \neq 0$, medical cannabis legalization is not a random event, as it is anticipated through changes in the medicaid opioid prescription rate.

Lastly, I wish to look at how the substitution effect may change over time on a more granular level. Thus, I use an event study analysis:

\[ p_{st} = \delta_s + \tau_t + \sum 1(t = t_s^* + r)\beta_r + \varepsilon_{st} \]

In this specification, $1(t = t_s^* + r)$ triggers when a data point is in a period $r$ quarters before after implementation. Therefore, any changes in the opioid prescription rate due to changes in these dummies can be interpreted as the difference in prescription rate from the period in question to the baseline period. Thus, when $r > 0$, $\beta_r$ represents the effect of legalization on the prescription rate $r$ quarters after implementation. These values are compared to $r = 0$, the period of implementation, which is excluded from the regression. In addition, $r$ ranges from a minimum of -10 and a maximum of 60, thus treating all data points more than 2.5 years before or more than 15 years after implementation as if they were 2.5 years before or 15 years after implementation, respectively. Using this specification allows for a more nuanced picture of how the effect evolves over time, by giving estimates of the effect in every quarter after legalization.

These three regressions are intended to examine the effect of medical cannabis legalization on the opioid prescription rate with varying degrees of precision and
accuracy. While the simple DD regression gives the most precise estimate of the mean
effect of legalization, it cannot give any information regarding how such an effect
changes over time. The event study analysis, on the other hand, gives the most detailed
picture of how a substitution effect may change as time passes, but is less precise in its
estimate of the effect’s magnitude. The parametric DD is in between the two, as it is
more precise than the event study, but also gives some information about time-related
changes in the substitution effect. In short, these specifications are all extensions of a
difference-in-differences framework, designed to give different levels of detail into how
any substitution effect may evolve over time.

V. Results

A. Simple Difference-in-Differences

First, let us consider the most basic measure of the substitution effect, with no
time variation. In Table 3, we can see that the coefficient on the dummy for
implementation, or $\beta_1$, is significantly different from 0 at the 10% level, with a p-value of
.051. This estimated coefficient value represents the average quarterly change in opioid
prescriptions per 1,000 Medicaid enrollees in legalization states post-implementation.
This coefficient is estimated to be -15.69 (95% CI: [-31.48, 0.096]) per quarter,
representing a negative change of 62.25 prescriptions per 1,000 enrollees per year, and
representing a 9.75% reduction in the prescription rate.
While these regression results do contain a significant estimate of the change in prescription rate, they do not, as previously stated, give any indication as to how that change may vary over time.

B. Parametric Difference-in-Differences

Let us next examine the results for the parametric DiD, which does give a picture of how the substitution effect may evolve over time. First, however, let us consider the estimates of both $\beta_2$ and $\beta_3$, representing the falsification test and the per-quarter change in the substitution effect post-implementation, respectively. As can be seen in Table 4, neither of these estimated results are significantly different from 0. As this relates to $\beta_2$, this shows that the timing of medical cannabis implementation is effectively random, thus eliminating any bias in the estimation of the other coefficients that may be due to non-random event timing. In the case of $\beta_3$, this coefficient estimate implies post-implementation variation relative to the average treatment effect is not smooth or is non-existent; the estimate of $\beta_3$ suggests that there is a “jump” effect at some point post-implementation, but that it is does not increase in magnitude over time. Lastly, let us look the estimate for $\beta_1$, or the mean post-treatment change. In this case, the coefficient estimate is significantly different from 0 at the 10% level, with a p-value of .093. The post-implementation change is predicted at -13.60 prescriptions per 1,000 enrollees, per
quarter (95% CI: [-29.58, 2.376]), or 54.4 prescriptions a year, an 8.5% reduction in prescription rate.

These results serve two functions: first, the estimate of $\beta_2$ suggests that medical cannabis implementation is random in relation to the opioid prescription rate, and, second, the estimates of $\beta_1$ and $\beta_3$ imply the existence of a “jump” effect post-treatment, but that such an effect does not increase smoothly over time in the years after legalization. However, this specification cannot give detailed results as to the variation of any effect over time, especially in relation to the specific timing of any “jump” effect.

C. Event Study Analysis

Let us now examine the results of the event study, which gives the most nuanced view of how the substitution effect changes over time. In this case, each $\beta_r$ represents the change in prescription rate in that period in comparison with the prescription rate in the baseline period. Note that, in this case, the baseline period is $\beta_0$, or the period of medical cannabis implementation. When looking at these results, we get a more clear picture of how the substitution effect evolves over time. Although not every estimate of $\beta_r$ is significantly different from 0, the results nevertheless give us a good sense of the trend of the effect. As can be seen in the full regression output (found in the Appendix), all periods of $\beta_r$ before implementation are either significantly positive or insignificantly positive, suggesting that any effects come after treatment, and are thus related to
medical cannabis implementation, and that parallel trends before implementation can be reasonably assumed to hold. In addition, we see that the first few periods (about 3 to 7) do not show any significant decrease in prescription rate. This suggests that the substitution effect is delayed after implementation by somewhere from half a year to a year and a half. After this lag, the substitution effect increases dramatically in absolute value and does not change in magnitude, instead remaining at roughly the same level in each period. While there is some variation, and not every coefficient estimate is significant, the general trend seems to show that the effect grows to a reduction of around 40 prescriptions per 1,000 enrollees per quarter about two and a half years after implementation, and then remains around that level. This represents an estimated substitution effect over twice as large as the one estimated in the other two regressions, but given the wide confidence interval of the estimates of $\beta_r$ and the implied lag in the effect, this does not seem out of place. Given the fact that there are multiple periods post-implementation with no estimated effect, and the “jump” effects are mean effects, they would underestimate in comparison to the event study estimations post-lag; furthermore, these “jump” estimates are well within the confidence intervals of the event study estimates. This overall trend can be seen in Figure 3, which maps the estimated change in prescription rate in each period and the associated confidence intervals.
These results, while not always significant due to the large number of variables used and the small number of states providing observations when the number of quarters post approaches 60, give a detailed examination of the substitution effect’s evolution. They imply that there is a lag between implementation and substitution, and that the effect quickly reaches its maximum post-lag and then levels off soon afterwards.

VI. Discussion

A. Discussion of Results and Implications

The results from the three regression specifications support my hypothesis that medical cannabis legalization and implementation reduced the Medicaid opioid prescription rate in the period from 2011 to 2016, although in a slightly different manner than I predicted. While my general hypothesis of the existence of a substitution effect was proven to be correct, I predicted that each additional period post-treatment would cause this effect to continuously increase, and that does not seem to be the case. Rather, the effect seems to be lagged, occurring about a year after medical cannabis implementation, and stays fairly steady afterwards, with additional periods of time not having much of an effect. That all being said, the fact remains that medical marijuana does seem to serve as a substitute for opioids, and that this substitution occurs even
among Medicaid recipients, who generally have less access to medical cannabis than the general population due to its high cost.

Given these results, this thesis has some interesting implications for policy concerning medical cannabis. As it is the case that the opioid prescription rate is very closely linked with the rate of opioid overdoses, this observed substitution effect suggests that cannabis legalization may have significant effects on the opioid overdose rate via reducing the number of prescriptions (Boston Fed, 2019). Furthermore, past cases have shown that supply-side restrictions on opioid prescriptions can actually have negative effects on overdose rates and public health; as medical cannabis substitution is a demand-side effect, it can reduce prescriptions in a similar manner without risking the negative externalities caused by supply-side policy. All these facts, when taken together, seem to suggest that medical cannabis legalization could be one of the most effective ways to curb the opioid crisis, particularly in comparison with supply-based policy.

In addition, the results of this analysis have implications when it comes to the procedure for implementing medical cannabis. The existence of the “lag period” in between implementation and the substitution effect may imply that the process of a portion of the market switching from opioids to cannabis is rather slow, because of high costs of entry into the medical cannabis market or a lot of red tape imposed on
suppliers, prescribers, or patients. Therefore, ease of access to medical cannabis may have a significant effect on both the timing and magnitude of any substitution effect.

The cheaper medical cannabis is, and the easier it is to get on a medical cannabis program, the less time it may take for the substitution effect to appear, and the stronger it may be. Hence, policy that makes medical cannabis a more readily available resource may induce a more immediate and dramatic change in the opioid prescription rate, which may increase the public health benefits gained from legalization. In short, this thesis suggests that policy that improves access to medical cannabis may have a significant positive effect on public health via its effects on opioid prescriptions and, consequently, opioid overdoses.

B. Limitations

One limitation of this study is its inability to entirely replicate the results given by Wen and Hockenberry (2018). As I did not have access to much of the data included as additional controls in Wen and Hockenberry, my regression specifications were, inevitably, slightly different from theirs. Therefore, I was unable to fully match their methodology and thus truly replicate their results. While my results are in line with theirs, and are valid in their own right, it would have been interesting and valuable to fully recreate the methodology used by Wen and Hockenberry as a proof of concept.
Another limitation of this study is that it cannot make any distinction between changes on the extensive margin (i.e., the number of individuals with any opioid prescription) and the changes on the intensive margin (i.e., the number of prescriptions to those already been prescribed opioids). As the data are in quarterly aggregate form, there is no way to determine whether the observed substitution effect comes from fewer people getting any form of opioid or if it comes from people already on opioids using smaller amounts. If the necessary data were available, I would have been able to get a much more refined picture of the substitution effect.

A third limitation of this study is that it only measures numbers of prescriptions, rather than the strength of those opioids. Given the data available, I was unable to include the necessary morphine milligram equivalent (MME) values for all the opioids included in the dataset, and thus could not measure the total strength of opioids prescribed in a particular quarter. Therefore, I used overall prescription totals rather than MME totals as the dependent variable in my regressions.

A final limitation is that this study, as it is observational, cannot definitively determine causality between medical cannabis legalization and opioid prescribing. Mainly, it is not possible to decisively claim that the timing of legalization is random, or that it is randomly assigned to states. Although there is evidence that suggests that event timing is random, it is in no way conclusive, and thus making any conclusive
statements regarding randomness is misplaced. Due to this, definitive causality can not be established.

C. Suggestions for Future Work

This study and its limitations lead to numerous avenues of future research, to further investigate the nuances of any substitution effect. In addition, considering the fact that the body of literature pertaining to this topic is relatively small, it would be valuable to simply replicate these results as further evidence towards the potential existence of an effect.

As mentioned in the limitations section, this study is unable to determine if any changes to prescription rate come at the extensive or intensive margin. Furthermore, the body of literature gives evidence for changes on both the extensive and the intensive margins; surveys have shown that medical cannabis users both use cannabis as a complete replacement for and a supplement to opioid painkillers. A study that had individualized data could separate these effects and determine which marginal changes have the greatest effect on the overall prescription rate.

Another interesting topic on which future research could focus is how certain types of medical cannabis access affect the prescription rate, as opposed to simply looking at all forms of cannabis access at once. For example, certain states have medical cannabis dispensaries, while others have a caretaker system with smaller-scale
distributors. In addition, every state has different conditions and qualifications to be eligible for medical marijuana use. By looking at how the different forms of legalization affect the opioid prescription rate separately from each other, future research could give more nuanced policy recommendations to maximize the reduction in prescription rate.

McMichael et al. (2019) and Wen and Hockenberry (2018) argue that recreational cannabis legalization results in an additional decline in the opioid prescription rate. But each of these papers uses a simple differences-in-differences methodology, which my event study has shown to be inappropriate. Thus, it is possible that the inclusion of an indicator for the recreational legalization event simply enables these authors to approximate the pattern evident in Figure 3 and does not provide an estimate of the impact of recreational cannabis legalization. Future work could extend the event study approach to allow for two events, medical cannabis legalization and legalization of recreational use. That would provide a more convincing estimate of the impact on the prescription rate of the legalization of recreational use.

Finally, a future study could examine the overall net benefit of medical cannabis from a public health perspective. Similar to the work done in Bradford and Bradford (2016), future research could try and estimate the monetary gain derived from reductions in prescription rates due to legalization. In addition, future research could estimate the net gain derived from the effects of legalization on other factors, such as the
opioid overdose rate, to get a sense of public health benefits outside of changing
prescription rates. This sort of work could attach numerical values to the potential
public health effects of cannabis legalization, and could thus serve as a more concrete
body of evidence.

VII. Conclusion

Using data on the timing of medical cannabis legalization and the Medicaid
opioid prescription rate, I have used variants of a differences-in-differences model to
estimate both the mean change in the prescription rate post-implementation of medical
marijuana and the pattern of change in the post-implementation period. I estimated
both the overall mean change post-implementation and the additional change due to
every additional period post-implementation. I found that an overall post-treatment
change does exist, but that it does not increase for each period post-implementation.
Furthermore, I found that the change that does exist lags behind medical cannabis
implementation. More specifically, I found that medical cannabis legalization is
associated with around a 10% reduction in the Medicaid opioid prescription rate, and
that this change comes about a year after cannabis implementation, and stays steady at
that level in the following years. These results suggest that medical cannabis
legalization may serve as a demand-side response to the opioid crisis, and could have
positive public health effects in that regard. Thus, cannabis could be effective as part of a multi-faceted approach to reducing the harm of the opioid crisis and improving national drug-related health.
References


Appendix

A. Tables and Figures

Figure 1: Opioid Prescription Rate and Death Trends

Figure 2: Overdose Deaths by Type of Opioid
Figure 3: Event Study Analysis Results

Event Study Analysis

- 95% CI Lower Bound
- Coefficient
- 95% CI Upper Bound

Change in Opioid Prescription Rate

Quarters From Implementation
<table>
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<th>State</th>
<th>Year Of Implementation</th>
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<tbody>
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<td>1999</td>
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<tr>
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</tr>
<tr>
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<tr>
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<tr>
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<tr>
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<tr>
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<td>Florida</td>
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<tr>
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<tr>
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### Table 2: Example Data

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<th>Implementation</th>
<th>Time From Implementation</th>
<th>Expansion</th>
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### Table 3: Simple Difference-in-Differences Results

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<th>Standard Error</th>
<th>P-Value</th>
<th>Confidence Interval</th>
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<tbody>
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<td>Implementation $1(t &gt; t_s^*)$</td>
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<td>7.861</td>
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Table 4: Parametric Difference-in-Difference Results

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<td>Implementation $1(t &gt; t_s^*)$</td>
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<td>7.955</td>
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<td>Trend $t - t_s^*$</td>
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<td>Post-Treatment Trend $1(t &gt; t_s^<em>)(t - t_s^</em>)$</td>
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### Regression Outputs

**Table 5: Simple Difference-in-Differences Regression Output**

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N: 1224  
ar2: 0.794

P-values in parentheses  
+ p<0.10, * p<0.05, ** p<0.01, *** p<0.001
Table 6: Parametric Difference-in-Differences Regression Output

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p-values in parentheses
+ p<0.10, * p<0.05, ** p<0.01, *** p<0.001
Table 7: Event Study Analysis Regression Output

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<th>(0.024)</th>
<th>_Itme_fir_63</th>
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<td>_Itme_fir_49</td>
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p-values in parentheses: + p<0.10, * p<0.05, ** p<0.01, *** p<0.001