Recreational Marijuana's Effect on Crime Using Difference-in-Differences with Multiple Time Periods

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Abstract

Over the last decade, marijuana has been legalized for recreational use in a number of states in the United States, a policy change which has important socioeconomic effects. One such effect is that on crime; as such, I ask "What is the effect of recreational marijuana legalization (RML) on crime?" I employ the brand-new Difference-in-Differences with Multiple Time Periods methodology introduced by Callaway and Sant'Anna (2021), which allows me to examine effects on property and violent crime trends during the time period 2008-2020 across the entire U.S. in the same study. There are a number of ways to aggregate my results, and using a simple aggregation, I find that property crime trends are positively affected, leading to an average increase of about 145.08 property crimes per 100,000 people per year in states with RML (which is 5.5% of the average property crime rate over this period). Other aggregations show an increase in property crime from RML as well, though the effect on violent crime trends is not conclusive.

1. Introduction

Though marijuana was officially banned in the United States with the passage of the Controlled Substances Act of 1970, some U.S. states have legalized sale and possession of the drug, primarily throughout the past decade. The pioneering states legalized marijuana for medical use starting in the mid-1990s, and recreational sale and use of THC was first legalized by Colorado and Washington in 2012. Further legalization remains controversial due to the numerous socioeconomic questions brought about by the policy. One of the key areas of interest with regards to wider availability of marijuana is the effect on crime. Opponents of legalization argue that crime will increase due to the drug's impairment of decision-making processes, and because marijuana could act as a gateway drug, leading users to partake in other illegal, more dangerous substances. Supporters claim that legalizing marijuana would free up law enforcement to focus on other types of crime and argue that illicit markets for the drug would be reduced due to easier, safer access (McGinty et al. 2017). There remains no clear consensus as to the truth of any of these claims, but the fact remains that marijuana's effect on crime is an integral question for the entire debate around legalization. Therefore, I ask this major question: What is the effect of recreational marijuana legalization (RML) on crime?

Using the Federal Bureau of Investigation's Uniform Crime Reporting Program (UCR), I use yearly violent and property crime data from all 50 U.S. states and the District of Columbia and analyze how crime trends differed for the entire United States with regards to RML. I am choosing to examine recreational legalization as opposed to medical with the hope that a larger effect will be observed; medical legalization, while surely having a nonzero effect on the states in which it is implemented, is still restrictive enough that access to marijuana does not increase to the same degree that it does under recreational legalization.

Given that there are some states which have legalized and some that have not, one might seek to answer this question by using a difference-in-differences methodology, which calculates a treatment effect by comparing a treated group to an untreated group (those who have legalized versus those who have not, respectively). However, for real-world empirical analysis such as this one, diff-in-diff fails because it only uses two time periods: before treatment and after. In the case of non-uniform policy adoption at the discretion of individual states, there needs to be distinction between time periods of various policy adoption points. The most common methodology for this kind of question in the last couple decades has been two-way fixed effects. However, this methodology is also flawed; de Chaisemartin and D'Haultfœuille found that two-way fixed effects methodology estimates values for average treatment effects which are negatively weighted, leading to bias of results (2020). As such, I implement a modified version of the difference-in-differences methodology introduced in "Difference-in-Differences with Multiple Time Periods" by Callaway and Sant'Anna (2021). This new econometric technology provides mechanisms to account for the staggered adoption of marijuana legalization. It also provides robust tools through which to engage the parallel trends assumption (the idea that treated and untreated subjects would have behaved the same if neither was treated at all); I am able to estimate treated states' crime statistics as if they had never been treated, then compare those estimations to the actual states' crime numbers. Using this cutting-edge methodology, I calculate the difference-in-differences in violent and property crime trends between the states with RML and those without; in other words, my results provide an average treatment effect for the RML-treated states.

The broadest contribution I make to economics literature comes from the fact that I apply Callaway and Sant'Anna's brand-new difference-in-differences with multiple time periods methodology, which allows my estimations to provide a treatment effect of RML regardless of heterogeneity across states. Though there exists a large body of research investigating the effects of marijuana legalization on crime, none have used the cutting-edge econometric technology found in Callaway and Sant'Anna (2021). One study which is very similar to my own is Dragone et al. (2019). We both use a version of difference-in-differences methodology and data from the FBI's UCR; using my methodology, though, I am able to examine the US as a whole instead of just two states, and I have six more years of data to work with in my analysis.

2. Related Literature

Preceding this paper investigating the intersection of marijuana law and crime is a gradually broadening body of research investigating the sociological effects of marijuana use and legislation. There is a subset of that which focuses primarily on crime, especially from the last decade. There is still no clear consensus on the effects that marijuana has on crime, and often any findings about a change in crime are isolated to specific crimes or types of crime, and different papers estimating the same trends conflict with each other. Marijuana-crime research has coalesced around three distinct areas as well: the effects of recreational marijuana, medical marijuana, and marijuana dispensaries.

Since recreational marijuana legalization (RML) has only been around in the US since 2013 when Colorado and Washington state's policies went into effect, the main work done to estimate its effects has focused on the earliest adopters in comparison with small control groups, and most of it is fairly recent (Lu et al., 2019; Wu et al., 2021; Maier et al., 2017). Lu et al. (2019) examined crime in Colorado and Washington compared to a group of control states with no marijuana laws over a slightly longer period (until 2016), and their results showed no changes in crime at all. Wu et al. (2021), however, looked at Oregon against states with no marijuana laws, and found a significant increase in all crime rates. Maier et al. (2017) compared states that changed their marijuana laws between 2010 and 2014 against those that did not change using the FBI's UCR, checking for crime rates and arrests for drug abuse violations. In examining changes of all kinds, including decriminalization of marijuana, MML, and RML, Maier et al. (2017) saw no statistical difference in crime due to marijuana laws. All of these papers took their crime data from the FBI's UCR as I do, and my research contributes to this literature by looking at the entire country and finding estimates that may be more statistically powerful than some of the conflicting results.

Research into medical marijuana legalization (MML) in the US has been around for longer than that of RML due to earlier adoption of MML in a variety of states, so the data has had more time to mature and longer-term trends can be estimated (Morris et al., 2014; Shepard and Blackley, 2016; Gavrilova et al., 2017). Morris et al. (2014) investigated crime trends using the UCR for states with MML over the span of 1990-2006 and found a potential decrease in homicide and assault rates, whereas Shepard and Blackley (2016) found a decrease in violent crime in Western states. Gavrilova et al. (2017) looked at crime nearest the US-Mexico border as it relates to MML and found that crime reduction was strongest in counties that are closer to the border, and they found that MMLs in inland states have a spillover effect at reducing crime in border states; the crimes that were most reduced were drug trafficking crimes, which is in line with arguments that illicit markets for marijuana will be reduced by legalization and easier public access to the drug. I am studying RML, but my work will add to this literature by doing what MML papers did in including as many states as possible up to the present date.

There is also research which has analyzed the local-level effects of marijuana outlets and dispensaries and how their presence affects crime (Contreras, 2017; Freisthler et al., 2017; Brinkman and Mok-Lamme, 2019). These studies aim to estimate the difference in crime from MML or RML on a neighborhood or county level as directly influenced by geographic location of legal marijuana vendors. Contreras (2017) estimated a change in block-level crime in Los Angeles, California for medical marijuana dispensaries, finding that crime rates on the block where a dispensary is located (and in the surrounding area) increase, specifically for violent crime; Contreras asserts that dispensaries are vulnerable to crime and that they disrupt the ecological continuity of a block. Likewise, Freisthler et al. (2017) looked at Denver, Colorado's dispensaries as the state transitioned from MML to RML and found that while crime was not affected in the block where dispensaries were established, there was an increase in crime in spatially adjacent areas for violent and property crime, as well as for marijuana-specific crime. Brinkman and Mok-Lamme (2019) also studied the impact of dispensaries in Denver under RML, and they estimated that for every additional dispensary in a neighborhood, there were 17 fewer crimes committed there. Their conclusion did not show a geospatial spillover into adjacent areas, though, and may be explained by increased security or police presence in the immediate vicinity of the marijuana outlets. Overall, the mechanisms in this subcategory of research depend on the legalization of marijuana in some form, so they represent a more granular look at the marijuana-crime relationship – my analysis captures the aggregate trends that then influence these neighborhood-level numbers.

The most closely related paper is Dragone et al. (2019). That study compared crime in RML-adopted Washington state to Oregon before it adopted RML in 2014 (and to pre-RML Washington) and found a reduction in crime, specifically reductions in rape and all property crime. Both of our research uses difference-in-differences and the UCR, and both analyses are concerned with RML. However, I am able to access and utilize more data than their paper, and I am not limited by my methodology to only examine two states – I can examine the entire U.S. thanks to Callaway and Sant'Anna (2021). This should allow me to make a more statistically powerful conclusion while avoiding statistical bias of other multi-group methodologies, such as two-way fixed effects.

3. Data

My completed data is a panel dataset, observing crime at the state-year level. Since the first recreational legalizations happened in 2012, my dataset ranges from 2008 to 2020 in order to provide lead-in time before the first treatments and establish a baseline crime trend for every state before any RML was initiated. I do not go back further because the parallel trends assumption is more likely to hold over shorter periods of time, increasing the accuracy of my estimations. Specifically, each state-year combination includes two crime statistics: violent crime rate per 100,000 people and property crime rate per 100,000 people. These two categories contain a variety of crimes, and I wanted to examine if RML would affect each differently. I also created two variables for each state-year which take into account a state's legalization status (dummy variable coded "1" for RML treated and "0" for not treated) and what year the state legalized marijuana. The latter signifies that every state that legalized in the same year is assigned to the same treatment group as shown in Table 1, and that information was employed during the calculation phase of my work as required by the modified difference-in-differences methodology. Fifteen states adopted RML between 2012 and 2020. There are states that have adopted RML since 2020, though they were not counted as treated due to crime data limitations.

My dataset includes entries for all 50 states and Washington D.C. It does not include any U.S. territories even though some have legalized marijuana, because they have different cultural and socioeconomic factors that may violate the parallel trends assumption. Each of these states' data is augmented with the poverty rate for that state-year entry. Poverty is a variable that has been identified as a positive covariate for violent and property crime throughout criminological literature (Ellis et al. 2009), which I can use in my methodology to allow for heterogeneity across states. I obtained my poverty rates from the U.S. Census Bureau's "Historical Poverty Tables: People and Families – 1959 to 2020 (Table 21)".

Table 1: States with RML by Year Adopted	
Year	**List of States**
2012	Colorado, Washington
2014	Alaska, D.C., Oregon
2016	California, Maine, Massachusetts, Nevada
2018	Michigan, Vermont
2019	Illinois
2020	Arizona, Montana, New Jersey

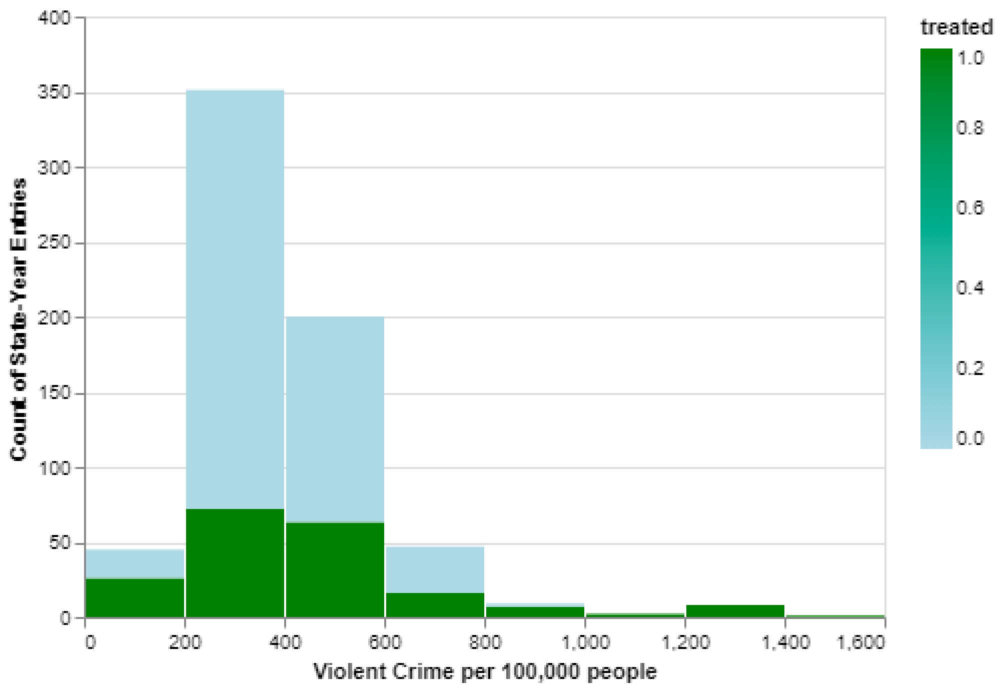
Over the years sampled by my data, property crime has generally decreased, and violent crime has seen very little change. Figure 1 illustrates this trend, as shown by the color shift down in property crime that is not mirrored for violent crime. As one might expect, Figure 1 also shows a positive relationship between property crime and violent crime, though not a perfect one.

Each point represents a single state-year data entry, colored by the year.

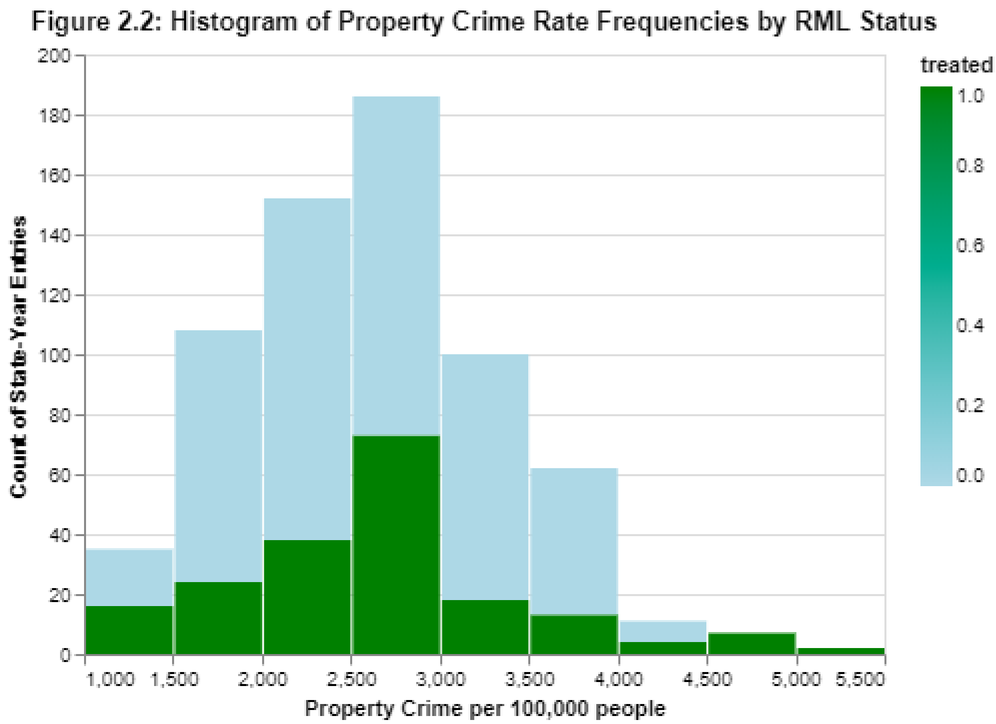
One can also see the relative magnitude of property and violent crime rates in Figure 1: property crime rates range in the low thousands, whereas violent crime rates tend to remain within 1,000 crimes per 100,000 people per year.

Figures 2.1 and 2.2 are histograms displaying how common certain crime rates are for violent and property crime respectively.

Figure 2.1: Histogram of Violent Crime Rate Frequencies by RML Status



Average Violent Crime rate: 386.37
States with RML adoption between 2012 and 2020 are highlighted in green.



Average Property Crime rate: 2614.48
States with RML adoption between 2012 and 2020 are highlighted in green.

The average violent crime rate between 2008 and 2020 for all 50 states and Washington D.C. was 386.37, and for RML states it was 432.96. This difference is statistically significant in a linear regression at $\alpha < 0.001$, indicating that states with RML tend to have higher rates of violent crime by 66 crimes per 100,000 people per year. However, since all RML states are grouped together in this linear case, claiming causality would be a mistake. The average property crime rate was 2614.48, and for RML states it was 2628.75. No statistically significant difference was found between those groups for property crime rates using a linear regression.

4. Methodology

In this investigation, I employ the Difference-in-Differences with Multiple Time Periods methodology first described in Callaway and Sant'Anna's 2021 paper of the same name. Generally, in order to ascertain the effect that legalizing marijuana has on crime, I estimate an average treatment effect on the treated (*ATT*), or the mean change in crime rates experienced by RML states as a result of that policy change. The *ATT* for a population is shown by equation (1),

$$ATT(g,t) = E[Y_{i,t}(g) - Y_{i,t}(0)|G_g = 1] \tag{1}$$

where *g* is a group component signifying the year the treatment went into effect, and *t* is any year after a given *g* (such that *t* > *g* always). *ATT* as represented here is the average effect on crime rates (*Y*) at time *t* for states that did legalize in treatment year *g* (G_g = 1 ensures this will be true).

Equation (2) is one of the ways identified in the methodology's source paper that can estimate $\widehat{ATT}$, known as the outcome-regression approach (hence the *OR* subscript) (Callaway & Sant'Anna 2021).

$$\hat{ATT}_{OR}(g,t) = \frac{1}{n}\sum_{i=1}^{n}\left[\frac{G_{i,g}}{\frac{1}{n}\sum G_{g,j}}(Y_{t,i} - Y_{g-1,i} - \hat{m}_{g,t,i}(X_i))\right] \tag{2}$$

The summation notation and fraction at the beginning of the specification are used to estimate the average of the change in trends of every states' crime for a given *g* and *t*. Now, the equation asks for the difference in crime trends in year *t* and crime trends a year before treatment year *g* for group *g*. The methodology creates this estimation by essentially comparing states with RML (treated) and states without (untreated), which is viable due to the parallel trends assumption. This estimation equation also subtracts $\widehat{m}$, a term which functions as a parallel trends control by taking a vector of covariates X_i and regressing on crime as specified by

$$\hat{m}_{g,t,i}(X_i) = E[Y_{t,i} - Y_{g-1,i}|X_i, C_i = 1] \approx \beta_0 + \beta_1 X_{1i} + \ldots + \beta_k X_{ki} \tag{3}$$

where $\widehat{m}$ is the difference in crime with respect to the X_i variables. In my case, X_i represents the poverty rate in any state-year. In other words, $\widehat{m}$ estimates a counterfactual change in crime in the absence of any treatment. Since each state is subject to differing cultural, social, and economic pressures, the parallel trends assumption at the center of difference-in-differences methodology can be violated. Implementing $\widehat{m}$ allows for the parallel trends assumption to be violated as long as the variation in crime is due to these X_i covariates. As such, X_i variables should be the best-correlated factors to crime trends (which poverty level seems to be) (Ellis et al. 2009) to make any causal effect from marijuana legalization stand apart from other pre-existing trends. I calculate average treatment effects with and without X_i to understand the effect of controlling for poverty.

I end up with a different value for $\widehat{ATT}$ for each possible *g* and *t* combination possible in the data, resulting in a large matrix of calculated $\widehat{ATT}$s. After this, all $\widehat{ATT}$ values are aggregated using the specifications from Callaway and Sant'Anna 2021, which will result in several perspectives on RML's effect on U.S. states. There are four key aggregation types as described in Callaway and Sant'Anna (2020): the 'simple' aggregation, which produces a single weighted average of all group-time average treatment effects with weights proportional to group size; 'dynamic' aggregation, which produces average effects across different lengths of treatment exposure for all groups, illustrating how the effects of treatment fluctuate the longer that treatment is in place; 'group' aggregation, which finds average treatment effects for each group *g*, potentially highlighting differences between groups; and 'calendar' aggregation, which calculates effects for each time *t* after the initial exposure, demonstrating potential differences between different times. Each of the aggregations estimates a different object, and each lends a different angle through which to examine RML's effect on crime.

The largest source of bias in this methodology comes from the potential violation of the conditional parallel trends assumption. If the X_i variable I selected, poverty, does not accurately capture variation in crime trends across states and time, then the estimations done with the above specifications are surely biased – the $\widehat{ATT}$ estimation can't claim to predict what will happen to crime after a marijuana legalization across the US if every state is subject to factors that differentiate them from their peers. Otherwise, staggered-adoption diff-in-diff is one of the least biased ways to calculate a treatment effect for a treatment with multiple adoption times.

5. **Results**

Tables 2.1 and 2.2 show every individual $\widehat{ATT}$ value (and standard error) for violent and property crime respectively and for both versions of the estimation (without controlling for poverty, then with that control). Though $\widehat{ATT}$ values were calculated for every (g, t) combination including for years t which were before treatment, only (g, t) pairs where $t \geq g$ are considered relevant for this analysis. There are a few of these values which are statistically significant, meaning that for certain (g, t) pairs, the crime rate did change with regard to RML. Besides the significance found in the early years for the 2012 cohort with respect to property crime (property crime was positively affected by RML), there is little visible consistency in magnitude or direction of the individual $\widehat{ATT}$ values across groups or times. Aggregation will provide more insight than can be gained from these 'raw' $\widehat{ATT}$ statistics.

Table 2.1: $\widehat{ATT}$ values for Violent Crime			
Group *g*	**Time *t***	**$\widehat{ATT}$ without controls**	**$\widehat{ATT}$ using controls for poverty**
2012	2012	-3.18 (5.27)	-4.91 (4.51)
	2013	-4.22 (5.29)	-6.00 (4.40)
	2014	0.31 (5.79)	0.24 (6.16)
	2015	-11.10 (8.75)	-8.59 (8.16)
	2016	-1.21 (12.63)	4.00 (11.13)
	2017	12.40 (22.92)	18.35 (20.49)
	2018	44.52 (29.28)	52.86 (26.50)
	2019	31.21 (27.73)	39.74 (24.98)
	2020	22.82 (47.58)	35.27 (42.13)
2014	2014	-8.11 (20.51)	-9.94 (21.55)
	2015	19.05 (30.71)	16.58 (37.07)
	2016	9.91 (65.33)	4.58 (76.35)
	2017	-35.88 (139.99)	-41.40 (154.44)
	2018	-11.53 (151.58)	-19.61 (169.02)
	2019	-2.15 (130.60)	-10.16 (148.37)
	2020	-49.79 (140.72)	-60.52 (162.19)
2016	2016	-15.99 (7.77)	-15.08 (7.67)
	2017	-50.72 (34.78)	-49.80 (35.89)
	2018	-48.73 (35.01)	-46.64 (36.35)
	2019	-64.81 (50.36)	-62.90 (50.59)
	2020	-103.48 (57.60)	-100.37 (57.87)

2018	2018	18.36* (5.24)	24.57* (6.41)
	2019	23.66 (23.03)	30.55 (12.86)
	2020	3.62 (12.85)	13.98 (10.70)
2019	2019	5.05 (2.78)	5.59 (2.90)
	2020	-7.19 (7.25)	-3.00 (6.53)
2020	2020	3.26 (19.53)	8.66 (16.12)

Note: * = 95% simultaneous confidence band does not cover 0
Terms are rounded to two decimal places
Standard errors listed in parentheses

Table 2.2: $\widehat{ATT}$ values for Property Crime

Group g	**Time t**	**$\widehat{ATT}$ without controls**	**$\widehat{ATT}$ using controls for poverty**
2012	2012	146.72* (20.73)	138.07* (16.65)
	2013	266.66* (40.78)	261.68* (38.83)
	2014	334.02* (80.25)	329.32* (76.06)
	2015	369.68* (75.12)	368.28* (75.25)
	2016	469.55* (103.31)	475.22* (108.46)
	2017	367.78 (198.35)	376.14 (183.95)
	2018	438.98 (282.62)	435.01 (210.40)
	2019	363.68 (335.86)	357.08 (206.88)
	2020	628.31 (405.59)	583.77 (277.72)
2014	2014	196.57 (154.62)	193.75 (160.14)
	2015	102.76 (48.45)	100.35 (53.66)
	2016	332.58 (192.58)	324.67 (220.25)
	2017	322.20 (279.86)	305.63 (376.49)

2014	2018	440.69 (240.49)	438.95 (263.43)
	2019	342.45 (147.16)	339.39 (153.35)
	2020	-49.17 (238.23)	-7.83 (178.73)
2016	2016	-84.11* (26.89)	-81.51* (26.08)
	2017	-62.36 (60.70)	-58.40 (57.79)
	2018	-46.20 (64.28)	-46.74 (61.70)
	2019	-44.71 (71.53)	-46.02 (70.61)
	2020	-97.93 (64.74)	-114.75 (35.24)
2018	2018	10.25 (22.73)	-6.66 (32.80)
	2019	125.50 (66.61)	96.11 (54.77)
	2020	54.78 (74.70)	-22.71 (52.94)
2019	2019	10.39 (17.93)	14.27 (23.20)
	2020	-134.02* (35.02)	-165.61* (38.65)
2020	2020	-14.88 (43.21)	-56.94 (53.60)

Note: * = 95% simultaneous confidence band does not cover 0
Terms are rounded to two decimal places
Standard errors listed in parentheses

Using simple aggregation, I find that the property crime rate increases by 145.08 crimes per year per 100,000 people with regard to RML when controlling for poverty rate; this result is statistically significant at $\alpha = 0.10$. There may also be policy relevance for this result: this point estimate is 5.5% of the average property crime rate over this period. Violent crime rate falls by 14.71 crimes, a result which is not statistically significant (though that estimate makes up about 4% of the average violent crime rate).

Table 3: $\widehat{ATT}$ Values from Simple Aggregation		
	$\widehat{ATT}$ without controls	**$\widehat{ATT}$ using controls for poverty**
Violent Crime	-15.55 (27.78)	-14.71 (31.27)
Property Crime	152.95* (83.54)	145.08* (86.6)

Note: * denotes significance at $\alpha = 0.10$

When using dynamic aggregation, I calculate an $\widehat{ATT}$ value for every possible treatment timing in the data, resulting in a range from one to eight years. For violent crime, the effect of RML is estimated to be negative or close to zero for the first six years before turning positive in years seven and eight. None of those estimates are statistically significant. For property crime, every point estimate is relatively large and positive; after five years of being treated, property crime rate increases by a very statistically significant margin of 354.09 crimes. Based on Figure 3.1, the increase in property crime appears to continue to grow larger with each year after treatment is implemented, though the variance of each of the estimates also progressively increases. Only the 2012 cohort experiences seven and eight years of treatment, so diminishing amounts of data as cohorts become more recent likely explains the largest reason for the growing variances in this aggregation. Interestingly, event timings of two and seven lose a level of statistical significance when controlling for poverty, suggesting that poverty may have factored into property crime trends over this time period (see Table 4).

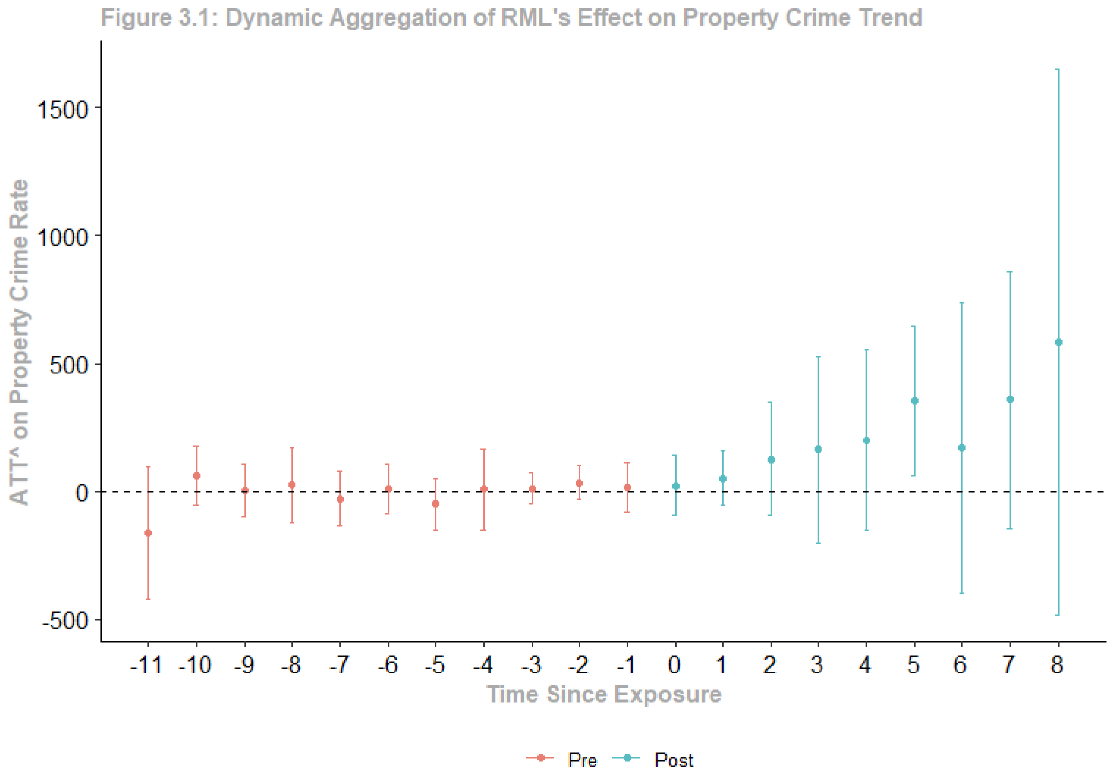


Figure 3.1: Dynamic Aggregation of RML's Effect on Property Crime Trend
ATT^ on Property Crime Rate
1500
1000
500
0
-500
-11 -10 -9 -8 -7 -6 -5 -4 -3 -2 -1 0 1 2 3 4 5 6 7 8
Time Since Exposure
Pre
Post

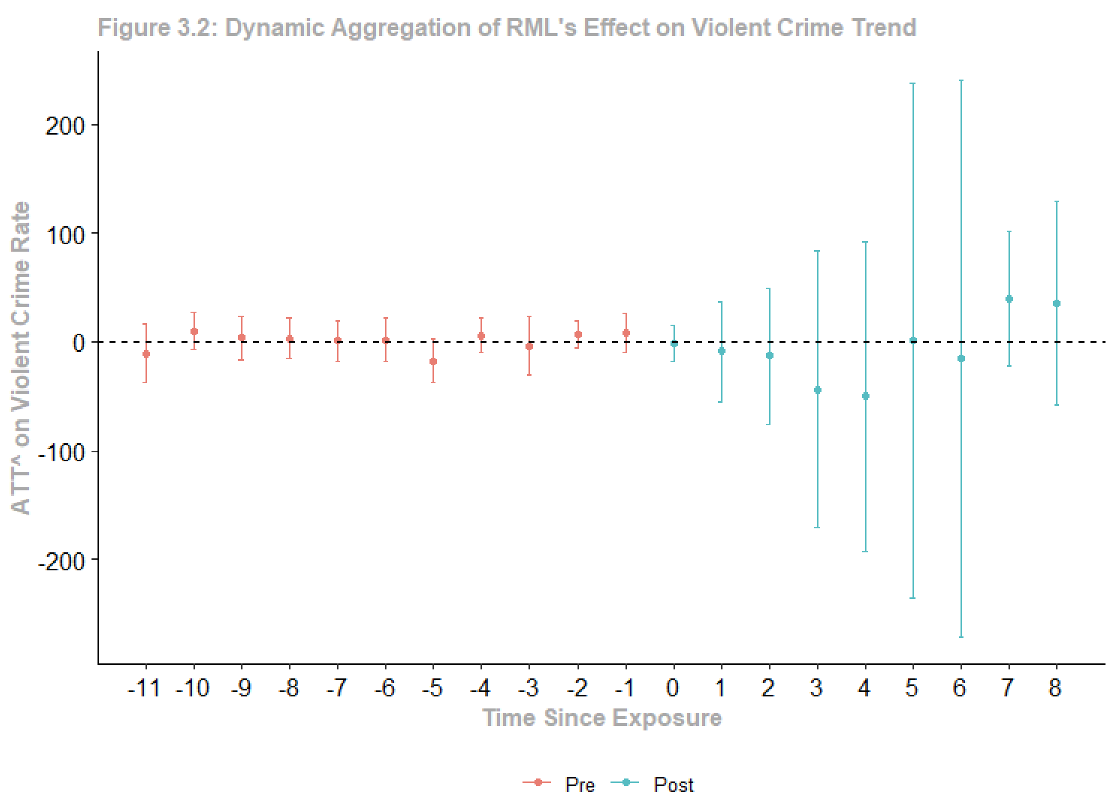


Figure 3.2: Dynamic Aggregation of RML's Effect on Violent Crime Trend
ATT^ on Violent Crime Rate
200
100
0
-100
-200
-11 -10 -9 -8 -7 -6 -5 -4 -3 -2 -1 0 1 2 3 4 5 6 7 8
Time Since Exposure
Pre
Post

Table 4: $\widehat{ATT}$ Values from Dynamic Aggregation				
	Violent Crime		**Property Crime**	
Event Time	$\widehat{ATT}$ without controls	$\widehat{ATT}$ using controls for poverty	$\widehat{ATT}$ without controls	$\widehat{ATT}$ using controls for poverty
1	-9.50 (18.42)	-8.61 (18.67)	59.09 (47.58)	51.45 (43.08)
2	-14.30 (22.46)	-13.13 (25.44)	144.59* (83.74)	127.30 (87.91)
3	-43.23 (46.63)	-43.66 (51.34)	169.68 (139.44)	163.27 (146.59)
4	-50.10 (55.37)	-50.26 (57.52)	207.72 (135.17)	200.92 (141.70)
5	3.67 (75.95)	1.25 (95.43)	352.58*** (104.55)	354.09*** (118.46)
6	-12.07 (96.29)	-15.17 (103.08)	146.09 (242.59)	169.30 (228.08)
7	31.21 (26.15)	39.74 (24.86)	363.68** (183.58)	357.08* (202.11)
8	22.82 (46.13)	35.27 (37.65)	628.31 (412.50)	583.77 (428.82)

Note: * = $p < 0.10$; ** = $p < 0.05$; *** = $p < 0.01$
Terms are rounded down to two decimal places

Group aggregation gives an $\widehat{ATT}$ value for each of the six groups, such that differences between groups can be identified more easily. Results are displayed in Table 5. Cohorts 2012 and 2014 saw large increases in property crime rate of 369.4 and 242.13 respectively (both are statistically significant at $\alpha = 0.10$), but no consistent pattern for the remaining groups. Interestingly, the 2019 cohort (which only consists of Illinois) saw a statistically significant decrease in property crime rate, though it was not a very large change at 75.67 property crimes. For violent crime, groups were again inconsistent, showing only minor changes due to RML. The 2018 cohort experienced a significant increase of 23.03 to violent crime rate. This aggregation may suffer in power due to low amounts of data for each cohort, with the most reliable estimates coming from the 2012 and 2014 cohorts. The inconsistencies found throughout estimates for the other cohorts could be due to small pools of data or to a lagging effect inherent in RML.

Table 5: $\widehat{ATT}$ Values from Group Aggregation				
	Violent Crime		**Property Crime**	
Group g	$\widehat{ATT}$ without controls	$\widehat{ATT}$ using controls for poverty	$\widehat{ATT}$ without controls	$\widehat{ATT}$ using controls for poverty
2012	10.17 (14.98)	14.55 (13.81)	376.15*** (134.26)	369.40** (174.62)
2014	-11.21 (51.38)	-17.12 (105.56)	241.15* (140.98)	242.13* (133.80)
2016	-56.74 (37.59)	-54.96 (36.91)	-67.06 (47.07)	-69.48 (51.25)
2018	15.21*** (4.69)	23.03*** (8.37)	63.51 (43.28)	22.25 (30.07)
2019	-1.07 (4.49)	1.30 (4.33)	-61.81*** (22.98)	-75.67*** (24.44)
2020	3.26 (18.14)	8.66 (15.02)	-14.88 (42.29)	-56.94 (52.04)

Note: * = p < 0.10; ** = p < 0.05; *** = p < 0.01
Terms are rounded down to two decimal places

Finally, the calendar aggregation estimates the effect of RML on crime within each year since the first treatment. Results for both types of crime are fairly consistent (Table 6): property crime rate is positively affected in every single year, and most of those effects are large in magnitude; violent crime rate is either not affected or is affected negatively by a small amount. Every year from 2012 to 2016 saw a statistically significant increase in property crime, the largest of which was in 2014 (an increase of 247.98 crimes). These changes in trends would have been primarily driven by the 2012 and 2014 RML cohorts. There were no statistically significant changes to violent crime trends using the calendar aggregation.

Table 6: $\widehat{ATT}$ Values from Calendar Aggregation				
	Violent Crime		**Property Crime**	
Post-treatment time *t*	$\widehat{ATT}$ without controls	$\widehat{ATT}$ using controls for poverty	$\widehat{ATT}$ without controls	$\widehat{ATT}$ using controls for poverty
2012	-3.18 (5.04)	-4.91 (4.71)	146.72*** (20.55)	138.07*** (17.78)
2013	-4.22 (5.20)	-6.00 (4.27)	266.66*** (37.57)	261.68*** (38.05)
2014	-4.74 (13.38)	-5.51 (14.52)	251.55** (103.89)	247.98** (99.18)
2015	6.99 (22.67)	6.51 (23.85)	209.53*** (77.97)	207.52** (80.58)
2016	-4.07 (22.27)	-4.29 (21.69)	177.82* (104.82)	177.60* (104.95)
2017	-31.74 (49.57)	-31.85 (51.07)	161.41 (139.51)	159.51 (147.95)
2018	-9.43 (44.88)	-8.23 (52.51)	185.06 (114.23)	180.60 (114.64)
2019	-12.57 (36.80)	-11.32 (39.99)	153.10* (91.10)	146.23* (87.46)
2020	-33.85 (32.68)	-30.77 (36.54)	43.22 (116.37)	20.21 (108.82)

Note: * = $p < 0.10$; ** = $p < 0.05$; *** = $p < 0.01$
Terms are rounded down to two decimal places

6. **Discussion/Conclusions**

After examining each of the four aggregation paradigms, the general effects of RML on property and violent crime trends are straightforward: property crime is positively affected, leading to changes in trends by at least single digit percentage points, and violent crime is either negatively affected or does not change. Additionally, the largest effects on property crime are experienced primarily by the earliest adopters, which may be indicative of either heterogeneous time factors that disproportionately affect 2012 and 2014 cohort states or a lagging effect that

RML has on property crime, which would currently be unknowable due to the yet brief period since RML was introduced in U.S. states.

The general conclusion made here exists despite the fact that property crime has steadily decreased over this time period – it is possible that states who adopted RML would have even greater reductions in property crime in the period 2008-2020 if they had not adopted RML. Conversely, counter to some research indicating an increase in violent crime borne from RML, the lack of a consistent positive effect on violent crime trends serves as an argument in favor of RML; for example, the contention that marijuana will serve as a gateway drug is not supported by the lack of movement in the violent crime trend. As such, the policy implications of these complementary findings are ambiguous and could be used in favor or against RML policy depending on policymakers' and constituents' risk preferences.

The largest limitation in this study is the lack of data available for later RML cohorts. Though there are convincing conclusions to be drawn from the earlier cohorts, the estimates for cohorts 2018, 2019, and 2020 likely have not had RML long enough for this methodology to see the true effects. Statistically significant results from these later cohorts are interesting and indicate the value of further study, but those measured effects may end up being transient as more data is produced over time.

This study is also a general examination of crime which narrows only as close as RML's average state-level effect on crime trends, and acquiring a full picture of marijuana's effect on crime should also include a discussion of crime spillover effects. My study does not account for the potential of crime to move across state borders in reaction to RML adoption, most importantly in the case of drug trafficking. One might expect the legalization of marijuana in a state like Colorado to stimulate the illicit market for drugs in a neighboring state like Kansas, and that could in turn stoke other kinds of crime. The structure of this current study does not account for that possibility.

Additionally, the core assumption of conditional parallel trends which is essential for all forms of difference-in-differences methodology could still be violated by heterogeneity between states that I was not able to account for. My decision to use poverty as my key control covariate had the possibility to introduce confounding bias into my results if poverty does not account for a large amount of variability between crime trends of states. Criminological research is still considering which variables are the strongest predictors of crime, and while poverty seems to be a decent covariate of crime, there could be another that I was not able to identify which would have fit the data better and which would have made my analysis more robust than it currently is. It is also true that specific crimes are more strongly affected by various specific factors, and my analysis necessitated choosing a very small number of covariate controls to cover all of violent crime and property crime; as such, future research using this methodology may consider employing different covariates for different categories of crime in order to best keep the parallel trends assumption intact.

Though more research certainly needs to be done after more time has passed, and states who legalized in 2021 and 2022 need to be included, the consistent indications of a positive effect of RML on property crime rate are striking. There is always the possibility of those trends to be transient as well, and heterogeneity between states and over time may shift these effects as more research is completed. The effect on violent crime trends in aggregate remains unclear. While this finding is important evidence in the overall discussion of marijuana legalization, health, education, and cultural outcomes, as well as effects to the economy must also be considered by policymakers for this and all drug-related policy decisions.

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