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Recreational Cannabis Legalization and Homelessness in the U.S.: A Quasi-Experimental National Policy Analysis

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RECREATIONAL CANNABIS LEGALIZATION AND HOMELESSNESS IN THE U.S.: A QUASI-EXPERIMENTAL NATIONAL POLICY ANALYSIS

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ABSTRACT

In analyzing rising homelessness across the country, a comparison of state policies uncovered a trend: many states which were early adopters of adult-use recreational cannabis law also exhibited a high incidence of homelessness. As legalizing cannabis undoubtedly affects the number of substance users who are imprisoned, such changes to drug enforcement policy may also be impacting homeless populations. Now, there is substantial research on the relationship between incarceration and homelessness, and on co-occurring mental health and substance use problems known to be prevalent among these populations. Despite such similarities, and the impacts of recreational cannabis legalization on jail populations, there is scant empirical research on its potential impact on homelessness. To test for such an aggregate effect, recreational cannabis legalization (RCL) is coded as a treatment variable for 44 states, and data is collected from 307 Continuum-of-Care (CoC) programs providing services to those experiencing homelessness. Because states receive RCL treatment (i.e., legalizing adult-use cannabis) at different times, the effect is compared across groups who were treated in the same year using an advanced causal method: difference-in-differences with multiple time periods (DID-MTP). For the time range 2007 to 2020, the differences between the average pre- and post-treatment effects on treated versus untreated groups are compared. In testing, the DID-MTP model estimates with statistical significance that treated CoCs in year 2016 have higher rates of homelessness than CoCs which never received the treatment, while other early-adopting CoCs exhibit similar trends that are not significant. Further, there is statistical support that length of exposure to treatment increases the estimated effects. Within the context of changing cannabis law, and the interrelated socioeconomic, political, and cultural factors, an analysis of these findings will follow.

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CHAPTER ONE

INTRODUCTION

Since 2015, at least ten cities or municipal regions in California, Oregon, and Washington have declared states of emergency due to homelessness, a designation that is usually reserved for natural disasters (Flaccus 2017). While the U.S. saw a 10.3% decline in homelessness since 2007, the homeless populations in California, Massachusetts and D.C. increased by 16.2%, 18.8%, and 19.9%, respectively (Henry et al. 2021). As all of these regions legalized recreational cannabis between 2012 and 2016 (NCSL 2021), and experienced spikes in homelessness thereafter, the potential relationship between cannabis policy and homelessness merits further research.

Indeed, substance use, together with mental illness, are major risk factors for homelessness (HUD 2017; Tsai et al. 2017). Many policy makers do fear that recreational cannabis legalization (RCL) leads to increased drug-related activity, which may in turn contribute to homelessness. While most states exhibited no clear associations in this regard, California, Oregon, and Massachusetts did exhibit relative increases in cocaine use after RCL adoption (Dills et al. 2021). It has also been shown that admissions to drug rehabilitation centers spiked after RCL adoption in D.C., Nevada, Oregon, and Washington (Zyonarev et al. 2019).

Public health impacts notwithstanding, other research highlights the potential impact of RCL on crime. For example, cannabis dispensaries are said to attract a “certain criminal element” especially among homeless youth and adults (Hughes et al. 2020). This potential effect of RCL on crime extends from the individual to the enterprise; states with favorable cannabis cultivation laws may provide legal cover for black market operations. In California, for example, the black market for cannabis outsells the legal market by a factor of three (Sabet 2021). This sentiment is echoed for states like Alaska and Colorado with isolated rural terrain, prime for unlicensed cannabis grows, which is appealing to drug cartels looking to set up trafficking operations (Sabet 2018). Despite these potential increases in drug access and use, no known studies have examined regional cannabis policy's effect on aggregate homelessness, a long-standing and increasingly pressing social problem. In the midst of strong momentum for cannabis legalization, and unsettled debates over its impacts, evaluating any unintended effects of cannabis legalization on homelessness is critical for informing future policy reforms.

The goal of this research is to estimate: 1) whether RCL adoption increases rates of homelessness, and 2) how the effect of RCL on homelessness varies by length of exposure to the policy. This study aims to answer these research questions by using unique longitudinal data from 2007 to 2020 which is aggregated from multiple government agencies and non-profit organizations. Homelessness data is derived from homeless counts conducted annually by Continuum of Care (CoC) programs, which provide services and shelter on behalf of the Department of Housing and Urban Development (HUD).

The analytic sample includes fourteen years of data on homelessness, and various other attributes, for 307 CoC programs in 44 states. Such longitudinal data allows for the use of quasi-experimental methods, such as the difference-in-differences (DID) model, to estimate for the causal effect of RCL on homelessness. Because state RCL adoption begins at different times, this project employs the most advanced causal inference method. The chosen DID approach uses staggered treatment adoption to estimate both the overall treatment effect and the treatment's effect over time (Callaway et al. 2021). Meanwhile, to strengthen the causal validity of this analysis, the model will control for a list of attributes that could potentially confound the relationship between RCL and homelessness. For states considering RCL adoption, the findings can serve as a guide for more strategic policy on mitigating homelessness.

CHAPTER TWO

BACKGROUND

As the War on Drugs has affected views around cannabis, related laws and public perceptions have been in flux for much of the last century. To dissuade cannabis use, its effects have historically been characterized using the 'criminality theory' and the 'gateway drug theory'. The 'criminality theory' suggested that cannabis was linked to "addiction, insanity, and crime", later favoring the 'gateway drug theory', which purported that cannabis consumption leads to more addictive substances like cocaine and heroin (Patton 2020). Such narratives supplanted many efforts to decriminalize cannabis in the 2000s, and over seven million people were arrested for cannabis offenses between 2001 and 2010 (ACLU 2020). As a result, decriminalization of cannabis soon became a prominent thought, along with the broader ideals of harm reduction and compassionate use.

As the first state actor of the compassionate use movement, California legalized medical cannabis in 1996 with fifteen more states following suit by 2010 (NCSL 2021). Just as the Obama Administration declared an end to the War on Drugs in 2009, governments reoriented policies towards treatment for substance use in lieu of incarceration (Ahrens 2020). Sentiment around RCL then began to shift; in 2012, Colorado and Washington were the first to enact cannabis for adult recreational use (NCSL 2021).

In states with RCL adoption such as California, Oregon, and Washington, total combined unsheltered homelessness increased by 18% from 2015 to 2017 (Flaccus 2017). Similarly, Colorado adopted RCL in 2012 and saw a 13% increase of homelessness between 2015 and 2016

(Sabet 2018). Although there is still no direct causal evidence that RCL increases homelessness, research does suggest it is a contributing factor. As further evidence, the success of cannabis industries may attract housing developers, causing a rise in rent prices, housing insecurity, and therefore homelessness. Indeed, Denver saw an increase of 17% in housing prices between 2010 and 2014, and homeless rights groups claim it resulted in more homelessness compared to the previous year (Milkman 2021). Still yet, despite sweeping cannabis policy reform, the potential effects of cannabis law on rates of homelessness remain largely unstudied today.

Meanwhile, such increases in localized homelessness required actions by state and local government, and they also underscored the need for a more coordinated federal response. As such, the Homeless Emergency Assistance and Rapid Transition to Housing Act of 2009 (HEARTH Act) consolidated three homeless assistance programs into a single program now known as the CoC Program. Its primary objective is to provide community-wide planning services for individuals experiencing homelessness. It is further tasked with improving data collection, and regional CoCs must plan and conduct point-in-time (PIT) counts of sheltered and unsheltered homeless persons in January of each year (Dunton et al. 2014).

While these overall counts will be the basis of this study, additional data is aggregated on homeless subpopulations such as the chronically homeless, families, youth, veterans, individuals, and a host of other demographics (HUD 2012). From 2020 PIT reports, for example, the highest rates of homelessness for individuals in major CoCs were overwhelmingly found in Washington, California, and Oregon (Henry et al. 2021). That is, these early adopters of RCL account for 80% of non-rural CoCs with the highest rates of homeless individuals.

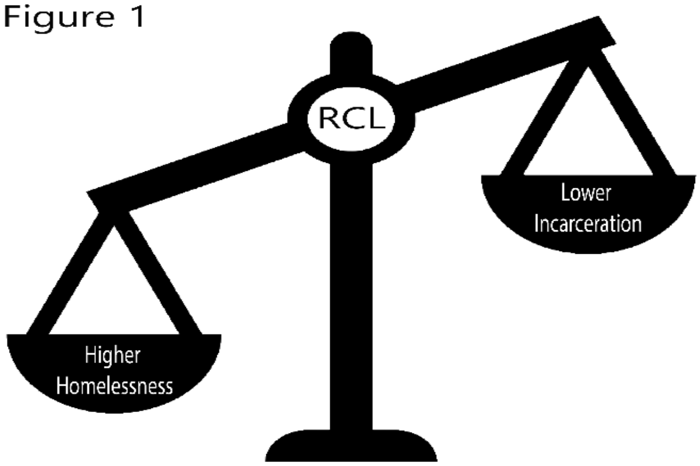
Research Hypotheses

Regarding the first research question, this research hypothesizes that *regions adopting RCL will exhibit high rates of homelessness when compared to regions without (H1)*. The mechanisms supporting this hypothesis are two-fold.

First, as the bidirectional relationship between incarcerated and homeless populations is well-documented (Cusack et al. 2017; National Health Care 2013; Schneider 2018; Tsai et al. 2013), the effects of RCL on homelessness may be attributed to population 'rebalancing effects' (see Figure 1 on the next page). Given a significant overlap among homeless and drug-offending risk populations, reductions in drug-related arrests related to RCL may increase the proportion of these risk populations in the community, increasing the prevalence of homelessness. For instance, over two thirds of youth and young adults experiencing homelessness reported lifetime alcohol and/or marijuana use (Tyler et al. 2019).

There is also evidence that RCL has reduced the rate of incarceration, especially for drug offenses, in part by affecting correctional methods. Some of these effects include sentence pardons, jail diversion programs, and cannabis records-clearing laws (Crusto 2020; Hartman 2020). As further evidence of RCL's effect on drug-related arrests, Oregon is the first state to have decriminalized minor possession of heroin and cocaine, a "measure funded in large part by its legal cannabis industry" (Oregon State Legislature 2020). In a national example, there have been 15% fewer cannabis arrests from 2010 to 2018 when comparing to the prior decade (ACLU 2020). In a local comparison between 2010 and 2020, California exhibited a decline in marijuana felonies and misdemeanors of 93.8% and 82.1% respectively (California DOJ 2015; 2020). Much of the reduction occurred in the year immediately following California's adoption of RCL; from 2016 to 2017, the state exhibited a decline in marijuana felonies and misdemeanors of 73% and 32% respectively (California DOJ 2017). Meanwhile, after California adopted RCL (i.e., from 2016 to 2020), non-cannabis drug misdemeanor arrests declined by 9.4% (California DOJ 2015; 2020).

Figure 1



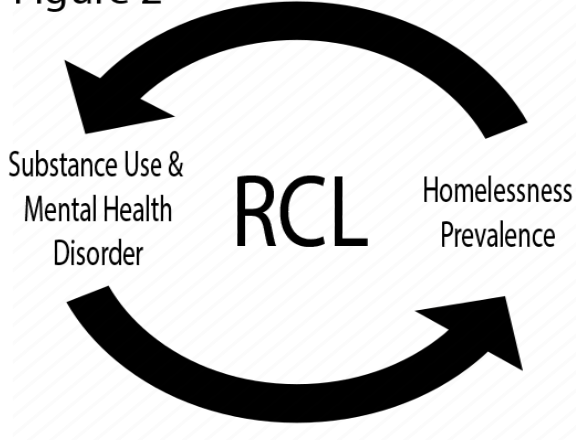
Through a second mechanism, RCL adoption may also increase the prevalence of homelessness by perpetuating mental health issues related to cannabis use (see Figure 2 on the next page). Considering the resilience of black-market suppliers in states adopting RCL (Bodwitch et al. 2019; Sabet 2018), increased access to drugs may exacerbate co-occurring mental health and substance use disorders (SUD), as well as expand the risk population by introducing new customers to the market. As perceived risk of imprisonment declines, RCL adoption may also increase demand for drugs, resulting in more overt (i.e., street-level) consumption behaviors, and directly impact homeless substance use. Importantly, since jails have historically served as drug treatment and detoxification centers, any reduction in related drug arrests could allow substance users to continue daily use patterns unmitigated.

Indeed, research on previously homeless inmates found them to be 10-22% as prone to psychosis and substance use than those not previously homeless, with 65% of inmates experiencing co-occurring SUD and mental illness (HCH 2013). Of the 18% of adults with mental illness in 2015 and 2016, 24% had also used cannabis within the prior year (Satre et al.

2018). Further, making up more than 12% of the national homeless population in 2020 (Henry et al. 2021), it is noteworthy that homeless youth and veterans are commonly exposed to trauma-related events. Research also suggests that homeless youth with PTSD have a "propensity to self-medicate" (Tabar et al. 2020), and other individuals with PTSD are likely to use cannabis "to alleviate symptoms and associated distress" (Boden et al. 2018). In combination with untreated mental health problems, such patterns of self-medication are predicted to increase rates of homelessness through rising migration into CoCs with RCL adoption.

In one example, most states saw homeless veteran populations drop an average of seventeen percent while Colorado was one of eight states with increasing veteran homelessness post-adoption (Mitchell 2016); related research suggests that homeless veterans migrated to Colorado for PTSD medication (McGettigan 2018). Given all the aforementioned population attributes, sizable subpopulations of the homeless may be affected by RCL adoption.

Figure 2



Regarding the second research question, this study hypothesizes that *regions adopting RCL will experience a cumulative increase on rates of homelessness over time (H2)*. That is, the longer a region has adopted RCL, the greater the positive association between RCL and homelessness rates will be. While it is plausible that this effect will taper in the long-run, fourteen years of longitudinal data does not constitute a long-run timeframe in this study's context. In the coming decade, as the cycles of homelessness, incarceration, and addiction are broken, the effects of historically punitive drug laws may also begin to wane. Setting the stage for future research, RCL adoption, along with changing treatment programs, may then be expected to cause a reduced rate of homelessness.

CHAPTER THREE

METHODS

Data Aggregation

Data for this study are aggregated from multiple sources (see Appendix A: Table 2.1). All data, except for housing inventory counts and homelessness data, is first collected at the county level. Rates of homelessness and housing inventory counts are collected at the CoC level. To merge these data, all independent variables and covariates are then aggregated at the HUD CoC level. There are CoCs that are Statewide, and there are CoCs for a Balance of State (BoS). Both statewide and BoS CoCs are removed from the sample as they do not include county-level data; this therefore excludes more rural regions of the country with extremely low per capita homelessness. The states excluded on this criterion are Delaware, Maine, Montana, North Dakota, Rhode Island, and Wyoming.

Given that CoCs contain one or more counties, all CoCs are matched to a list of counties so that datasets can be merged. To accomplish this, a dictionary of CoCs and corresponding counties is created and then used to merge data sets with Python 3.8. The common, unique IDs on which the datasets are merged are FIPS state county codes and years. Note that the FIPs state county codes, county names, and area names come from HUD Fair Market Rent (FMR) rent data. The following paragraphs detail how the dictionary key is manually constructed.

In cases where CoCs are regional, there exists no corresponding county. As such, these CoCs are deleted from the dictionary key. In cases where the CoC has no corresponding FIPS county, data is also truncated. Note that some CoCs correspond to multiple counties within HUD FMR areas and/or Metropolitan Statistical Areas (MSAs). Where there are FIPS counties that are duplicated, or matched to more than one CoC, the FIPS county is deleted from the CoC in which there are multiple counties, or deleted from the CoC whose name does not explicitly contain the name of the county, unless that would allow for a major CoC to be truncated from the data set. In such a case, the largest of the two CoCs is kept, deleting the smaller CoC.

There are also cases where the CoC is named after the town name (e.g., MA and VA), as opposed to the county, city, or metro area. In such cases, the HUD FIPS town names are used to keep the closest town-to-CoC matches, deleting the duplicate FIPS county from the larger territory. This allows for each county to appear only once in the sample, preventing double-counting, while including as many CoCs in the analytic sample as possible. Since variable data is aggregated for each CoC's list of counties, this method should provide the best CoC to FIPS county matches possible.

The final analytic sample includes 307 unique CoC facilities from 44 states and will be tested for the period 2007 to 2020. Among the 44 states, 13 adopted RCL during this time frame. Of these states, the sample includes 1,550 observations which have undergone treatment and 2,704 which have never been treated (see Appendix A: Table 2.3).

Measurements

All variables in the model are aggregated at the CoC level and operationalization is detailed in Appendix A: Table 2.1. The dependent variable, which is the *overall* rate of homelessness, is aggregated by HUD using the sum of a few homeless subpopulations. The HUD subpopulations include sheltered homelessness and unsheltered homelessness (i.e., homelessness in places not meant for human habitation). Sheltered homelessness is comprised of homeless counts for transitional shelters (TS), emergency shelters (ES), and safe havens (SH; introduced in 2010 for most regions). The following formulas depict how these populations are constructed by HUD:

$$Sheltered\ Homelessless = TH + SH + ES$$

$$Overall\ Homelessness = Sheltered + Unsheltered$$

Overall homelessness is recoded to a rate variable per 10,000 residents based on the summed population data for all county populations in a corresponding CoC region (U.S. Census 2019). Missing data is handled here by deleting CoCs if there are more than three years of missing data for overall homelessness. This would occur in situations where CoCs were either merged or later created during the time period of 2007 to 2020. As a result of Covid-19, it should be noted that over half of CoCs had limited data in 2021, and HUD provides no estimates for overall homelessness for year 2021. As such, this year is excluded from the study.

RCL as the treatment, or independent variable, is coded as a categorical variable indicating the year that each state legalized recreational cannabis use for adults ages 21 or over (i.e., year treatment started). This variable is used to define the treatment groups and the comparison group. Today, 18 states have passed RCL either by ballot initiative or state legislature (see Appendix A: Table 2.2). However, there are lag times as to the legally effective date for the new laws. For example, the date at which cannabis possession is no longer criminal is typically earlier than the date at which retail sales are taxed and fully regulated.

For the purposes of this study, the year used for RCL adoption is the year in which the ballot initiative or state legislature was passed. Here, it is noteworthy that most states decriminalized what is commonly referred to as 'minor possession' prior to RCL, and the rebalancing effect (introduced in Chapter Two) may have begun earlier than the date of RCL adoption. Given this rebalancing effect between incarceration and homelessness, the date at which retail sales are finally taxed and regulated is deemed less relevant than the effective change towards legal cannabis possession.

In this way, RCL adoption represents a legal and cultural change in attitude away from cannabis prohibition, and therefore, is an adequate predictor towards more open consumption patterns (even in the absence of retail dispensaries). That is, RCL adoption indicates *full prohibition* of policing against cannabis minor possession, and related arrests are expected to decline at or around the same time. Further, while retail dispensaries and regulatory frameworks do change the operating environment for both legal *and* illegal markets, the primary mechanisms supporting this thesis remain RCL adoption's effects on incarceration, co-occurring substance use patterns, and finally its impact on rates of homelessness.

To establish comparability between the treatment and comparison groups, a set of six control variables is included in the analyses and are depicted in Appendix A: Table 2.1. Population control variables are recoded to per 10,000 residents based on the summed population data for all county populations in a corresponding CoC region (U.S. Census 2019). All non-population control variables for the counties in each CoC are aggregated by mean, median, or sum. Finally, all control variables are recoded as time-invariant using its respective statistical type; for example, rates will be time-invariant means and median rent will be a time-invariant median. Missing data for control variables are imputed using the same time-invariant value from its respective CoC group.

With respect to Mechanism 1, it is first important that the model account for the rebalancing effect between incarceration and homeless populations. As such, data on jail populations is collected from the Vera Institute of Justice (Vera). Jail population data is chosen over prison data for two primary reasons: 1) RCL is dependent on state law and many prisons house individuals not from their home state, and 2) low-level drug offenders are most commonly sentenced to jail over prison.

As urbanization has resulted in more densely populated city centers, the model will also control for the total county populations aggregated at the CoC level. As some U.S. Census population data was not available for all years, missing total county populations are imputed using a mean. Next, to account for the general effects of area socioeconomic conditions, the model will control for the percentage of the area's population which is at or below the poverty line, as defined and aggregated by the U.S. Census Bureau (U.S. Census 2019). Additionally, given the clear relationship between homelessness and housing insecurity, along with rising housing costs over the last decade, the model will control for median rent. As access to transitional shelters is also important to exiting homelessness, and shelter bed availability may serve as a proxy for investments in social services more generally, the model will also control for Housing Inventory Counts (HICs).

Finally, HUD homeless counts are conducted in January, which is one of the coldest months of the year. As HUD policy suggests, only those who cannot find or access shelter would endure the frigid cold (Dunton et al. 2014), and the aim of the policy is to get a truer count of year-round *unsheltered* homelessness. Since regional variations in weather could significantly impact model estimations, the average low temperature in January for all CoC regions is collected from the National Oceanic and Atmospheric Association (NOAA).

Analytic Strategy

As defined in Appendix A: Table 2.3, the year in which the state first adopted RCL will be considered the year in which all associated groups become treated. The following is an explanation of the causal method chosen to test for a treatment effect. As a widely practiced causal inference method for panel data, the classical Difference-in-Differences (DID) model estimates the effect of a treatment on an outcome by comparing the pre- and post-treatment changes in the outcome between the treatment group and the control group. The causal effect, often represented by the *average effect of treatment on the treated* (ATT), is estimated as:

$$ATT = E\left[Y_{post} - Y_{pre} \middle| D = 1\right] - E\left[Y_{post} - Y_{pre} \middle| D = 0\right]$$

, where D= represents a binary treatment status with D=1 for the treated group and D=0 for the comparison group, Y_{post} and Y_{pre} are the potential outcomes post- and pre-treatment.

However, a major limitation for this conventional DID model is its ignorance of the treatment timing: it assumes that all units in the treated group received the treatment at the same time. In the present project, the policy of RCL (i.e., the treatment) was adopted at different times by different states (e.g., California in 2016, Washington in 2012, etc.), which created an insurmountable challenge for the standard DID method. To address this issue, this project proposes to use the most cutting-edge method, the DID Model with Multiple Time Periods (DID-MTP) (Callaway & Sant'Anna, 2021), to estimate the group-specific average treatment effects for different groups of geographic units where RCL was adopted at different times. The DID-MTP will also allow us to estimate how the treatment effects of RCL evolve over time for different treatment groups.

The DID-MTP provides a generalized framework for estimating the Group-Time Average Treatment Effects for a specific treatment group defined by the timing of treatment, which is defined as:

$$ATT(g,t) \ = E[Y_t(g) - Y_t(0)|\ G = g]$$

, where G defines the group membership based on the timing of treatment g (i.e., year of RCL adoption) , $Y_t(g)$ is the potential outcome for the treatment group g at time t, and $Y_t(0)$ is the potential outcome for the comparison group at time t. The comparison group could be set as either the "never-treated" units, or "not-yet-treated" units (which includes both the never-treated units and some later-treated but not-yet-treated at the time of comparison). To test the robustness of estimation, this project experiments with both approaches in constructing the comparison group. Finally, to establish the comparability of the treatment group and the comparison group, the analyses adjusted for covariates mentioned in Chapter Three.

To test the second hypothesis, the DID-MTP model is used to aggregate the Group-Time Average Treatment Effects estimated above by the length of exposure to the treatment (i.e., RCL), which will allow us to see how the treatment effect vary over time. The aggregated treatment effect is represented as:

$$\theta(e) = \sum_{g \in G} 1\{g + e \leq T\} P(G = g|G + e \leq T)\, ATT(g, g + e)$$

, where e denotes the time elapsed since RCL was adopted, $G = g$ denotes the year of RCL adoption, T represents the maximum observed time period. Analyses will be performed in the *R* environment using the DID package (Callaway & Sant'Anna, 2020).

CHAPTER FOUR

RESULTS

As shown in Table 1.1 below, aggregate mean homelessness for untreated samples is 22.44 individuals per 10,000 residents. Comparing to CoCs in treated states, aggregate mean homelessness is 17.8% higher, with a statistically significant true mean difference ($P = 0.007$).

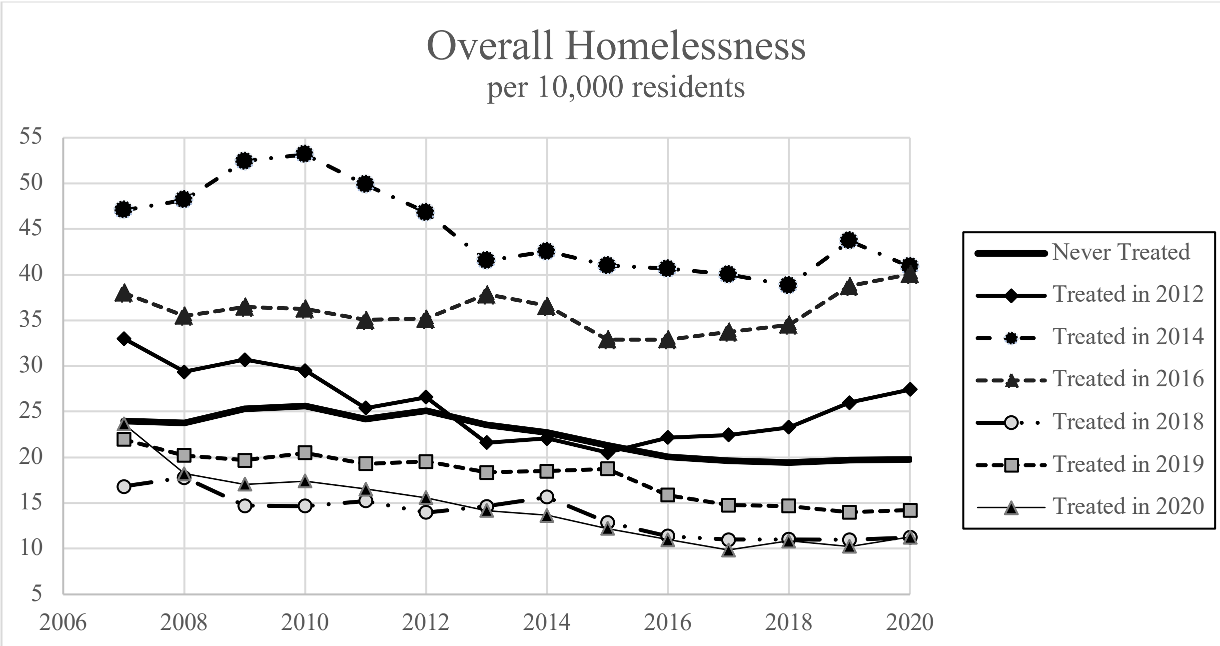
Table 1.1: Descriptive Statistics

Variable	**Treated**	**Untreated**	**P-value**[1]
Overall Homeless	26.4494	22.438508	0.00756
Climate	26.9261	27.1587	0.501
Total County Populations	702,855.5	351,608.9	0.000
Poverty	12.9942	13.8072	0.000
Median Rent	1330.0501	1096.9677	0.000
Shelter Beds	14.4119	13.2029	0.000
Jail Population	19.6318	27.0269	0.000

[1] P-values are based on t-tests.

While many regional homeless populations grew overall, rates of homelessness for treated groups were relatively stable or even declining (see Figure 1.1 below). Given the variant geographic panel data, state-level policy for RCL is tested as a predictor on rates of homelessness. The estimated effects employ the DID-MTP model with all six control variables in the table above.

Figure 1.1: Overall Homelessness by Treatment Group



In analyzing the output of the DID-MTP package, it is important to mention that Groups 2012 and 2014 are flagged as small groups; the model cannot estimate the groups' effects with statistical significance. By contrast, CoCs in Group 2016 account for 42% of the observations within the treated groups (see Appendix A: Table 2.3), and it is the only group consistently demonstrating significant effects of RCL on rates of homelessness (with or without confounders). Further, estimated group-time treatment effects are similar in magnitude, direction, and statistical significance regardless of whether the never-treated group or the not-yet-treated group was used as the comparison group.

As such, the following findings come from a DID-MTP model using never-treated as the comparison group. The model is then used to test multiple time periods: 2007 to 2018, 2009 to 2018, and 2007 to 2020. The only period for which the model estimates significant effects is the longest time frame. Thus, the 2007 to 2020 model appears most robust as it adequately reflects delays inherent in policy changes, and incidentally, it also includes regional peaks in homelessness occurring in the late 2010s.

With respect to H1, which suggests that RCL adoption will have a positive effect on rates of homelessness, the model estimates with statistical significance that this holds true for Group 2016 in years 2019 and 2020 (see Table 1.2 on the following page). In other words, CoCs which adopted RCL in the year of 2016 have seen significantly higher levels of homelessness in 2019 and 2020 compared to CoCs that have never adopted RCL. Such a pattern suggests that, for CoCs adopting RCL in 2016, there is a multi-year time lag for the impact of RCL on homelessness to manifest. Per Appendix A: Table 2.3, Group 2016 includes CoCs in California, Massachusetts, and Nevada.

Table 1.2: Group-Time Average Treatment Effects

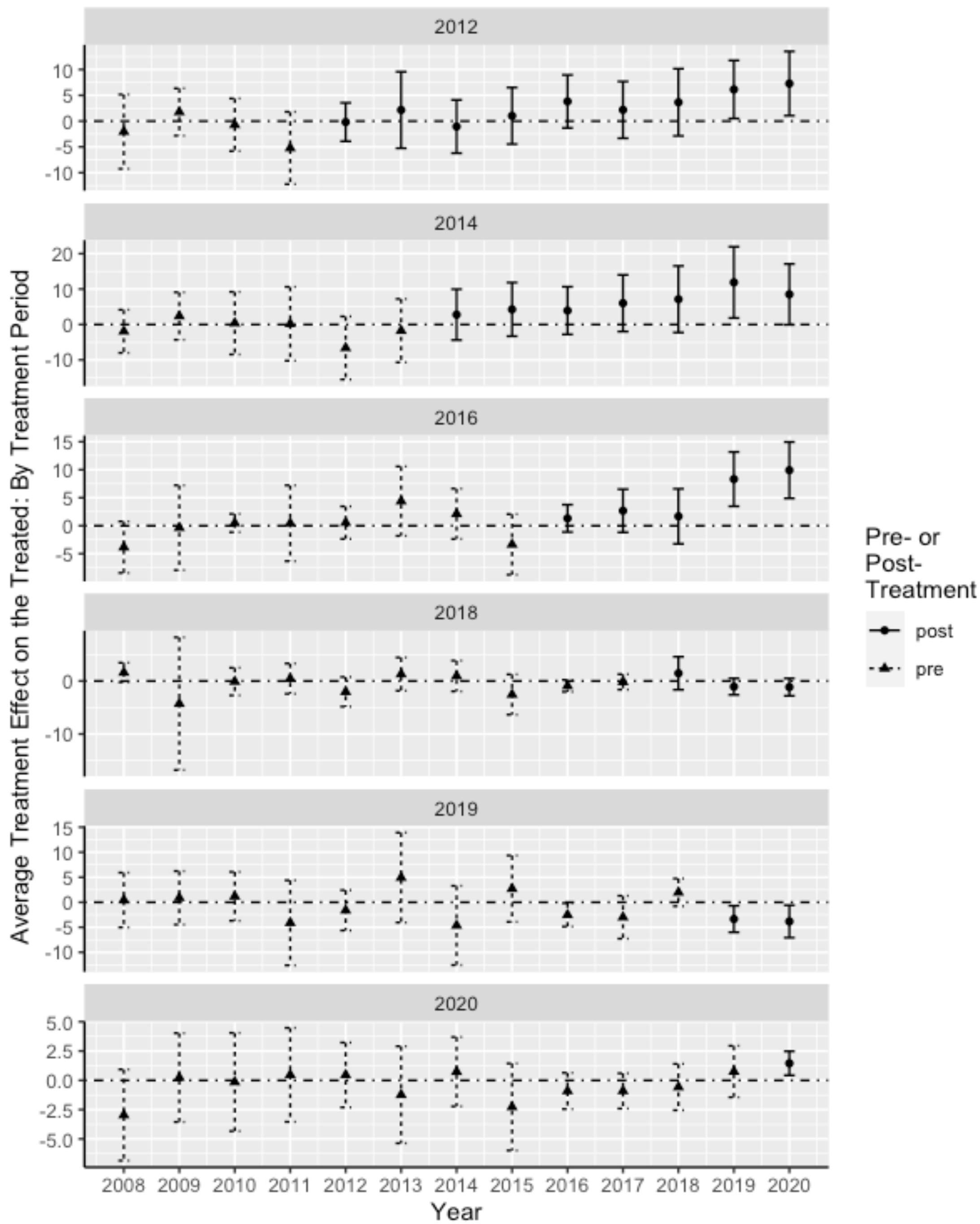
Group (defined by year treated)	Time	ATT	Standard Error	95% Confidence Band	
2012	2012	-0.1829	1.804	-5.3557	4.9898
	2013	2.1573	3.5968	-8.156	12.4705
	2014	-1.0529	2.6852	-8.7523	6.6465
	2015	1.0191	2.6246	-6.5066	8.5448
	2016	3.8151	2.5917	-3.6161	11.2463
	2017	2.1805	2.5834	-5.227	9.5879
	2018	3.6471	3.1791	-5.4684	12.7627
	2019	6.1348	2.7747	-1.8212	14.0907
	2020	7.2799	2.9648	-1.2212	15.781
2014	2014	2.7712	3.8001	-8.1252	13.6675
	2015	4.2571	4.1126	-7.5352	16.0495
	2016	3.9238	3.6082	-6.4223	14.2699
	2017	6.0064	4.1312	-5.8394	17.8521
	2018	7.1267	4.8463	-6.7693	21.0228
	2019	11.8861	5.0851	-2.6946	26.4668
	2020	8.5087	4.6067	-4.7002	21.7177
2016	2016	1.305	1.3733	-2.6329	5.2428
	2017	2.6649	2.0428	-3.1924	8.5223
	2018	1.6693	2.7958	-6.3472	9.6858
	2019	8.3122	2.25	1.8606	14.7638 *
	2020	9.909	2.3911	3.0529	16.7650 *
2018	2018	1.4608	1.6146	-3.1689	6.0905
	2019	-1.0658	0.8138	-3.3992	1.2675
	2020	-1.1532	0.9257	-3.8076	1.5011
2019	2019	-3.3484	1.381	-7.3081	0.6113
	2020	-3.8287	1.6378	-8.5247	0.8674
2020	2020	1.4517	0.5037	0.0074	2.8960 *

*Confidence band does not cover 0

From the above table, the model estimates with 95% confidence that RCL adoption will have an average effect of 8.31 more homeless per 10,000 residents in 2019 for CoCs in California, Massachusetts, and Nevada than in never-treated groups (falling in the range CI(1.8606,14.7638)); this estimated effect increases in year 2020 with an average of 9.91 more homeless per 10,000 residents (falling in the range CI(3.0529,16.7650)). Group 2020 (i.e., CoCs in Arizona and New Jersey) exhibited a significant but smaller positive instantaneous effect: RCL adoption is associated with 1.45 more homeless per 10,000 residents than in never-treated groups in the same year that the policy was adopted.

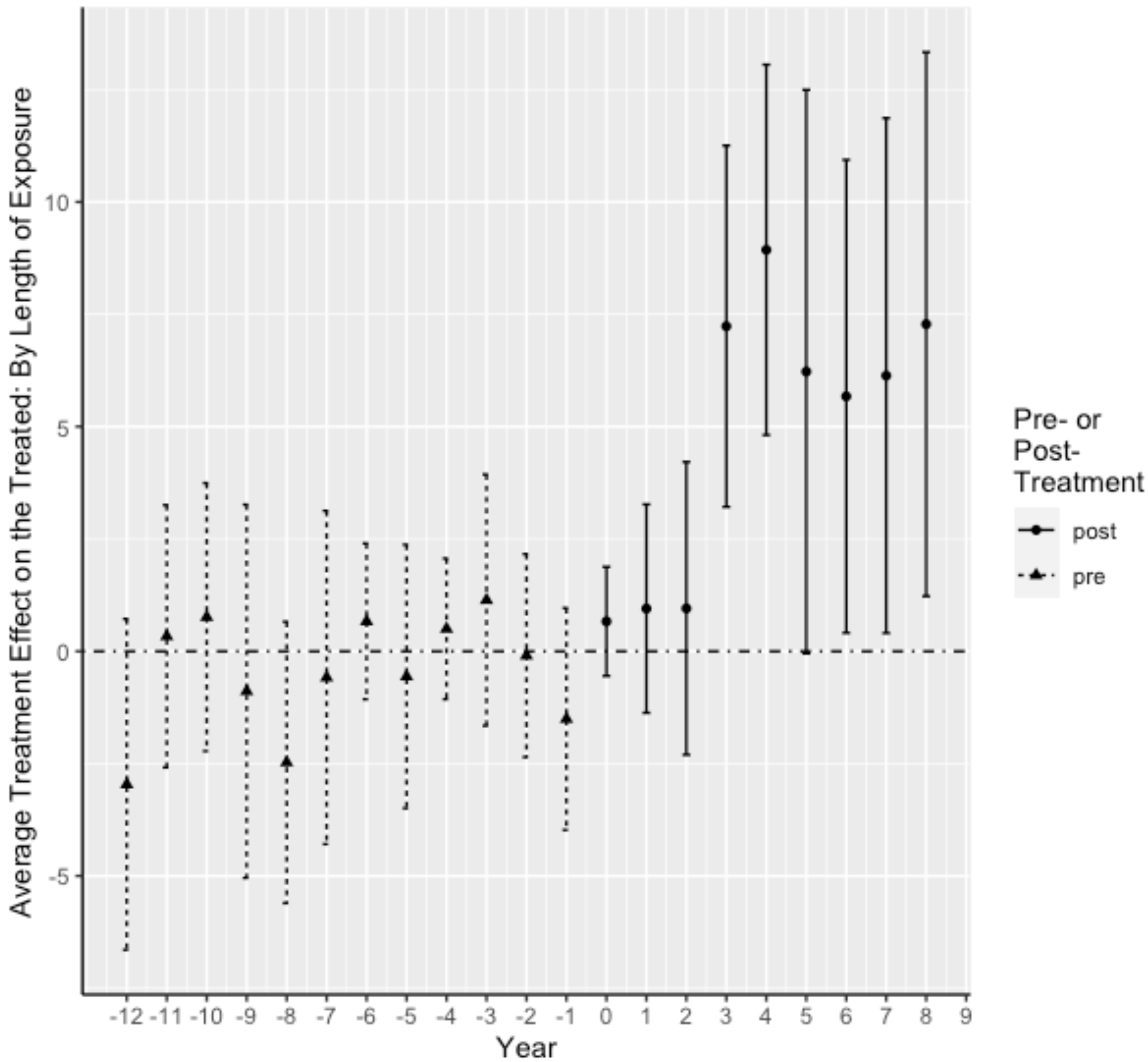
These positive relationships between RCL and homelessness for CoCs adopting RCL in 2016 and 2020 are consistent with trends observed for CoCs adopting the policy in 2012 and 2014 (see Figure 1.2 below), for which the effects are nevertheless milder and not significant. A closer examination of the sample distribution (Appendix A: Table 2.3) reveals that the sample sizes for Groups 2012 and 2014 are small and therefore have lower statistical power in the model.

Figure 1.2: Average Treatment Effect on the Treated: By Treatment Period



Further, model estimates provide partial support to H2, suggesting that the association between RCL adoption and rates of homelessness becomes stronger over time. From Figure 1.3 below, homelessness rates were not significantly different between the treated groups and the never treated group in the first three years after RCL adoption.

Figure 1.3: Average Treatment Effect on the Treated: By Length of Exposure



However, RCL adoption was associated with significantly higher levels of homelessness by the fourth and fifth year into RCL. In the fourth year after RCL, for example, the treated CoCs had on average 7.23 more homeless per 10,000 residents compared to never-treated CoCs (see Table 1.3 below). In the fifth year, the effect size increases to 8.93. However, while the relative trend continues, the long-term effect is not statistically significant in later years.

Table 1.3: Average Treatment Effect by Length of Exposure

Years After Treatment	Estimate	Standard Error	95% Confidence Band	
1	0.6644	0.6197	-1.0481	2.377
2	0.9493	1.1838	-2.322	4.2207
3	0.9539	1.6625	-3.6406	5.5483
4	7.2332	2.0514	1.5641	12.9024 *
5	8.9337	2.1029	3.1222	14.7451 *
6	6.2245	3.1995	-2.6174	15.0664
7	5.6728	2.6858	-1.7495	13.0952
8	6.1348	2.9235	-1.9445	14.214
9	7.2799	3.0888	-1.256	15.8157

*Confidence band does not cover 0

The significant relationship of RCL on homelessness among CoCs in California, Massachusetts, and Nevada holds when the model does not control for any confounder, and this group of states remains the only group exhibiting statistically significant effects here. When introducing all controls, the magnitude of the estimated effect increases. When controlling only for jail populations, the effect for Group 2016 holds but with relatively lower magnitude. Given the above, the most robust model includes all six controls to estimate for the difference-in-differences, with Group 2016 exhibiting statistically significant, positive effects of RCL on rates of homelessness that *increase* over the years 2019 and 2020.

CHAPTER FIVE

DISCUSSION AND CONCLUSION

To further evaluate the analytic sample, it is noteworthy that overall homelessness is an aggregated total on the rate of homelessness without distinction on age, gender, race, or ethnicity. Given that drug enforcement law, both past and present, disproportionately affects people of color, future studies may address this lack of heterogeneity by comparing the effect of RCL on specific homeless subpopulations.

Since this is a quantitative national policy study, some additional limitations include variations in regional policies that are more qualitative; for example, anti-homeless policing practices, a rise in sanctioned homeless encampments, changes in attitudes towards homelessness, and varying right-to-shelter laws. Especially common in cannabis reform cultures are attitudes towards medicinal use, which speak to compassionate mental and physical health doctrines, and these ideologies may similarly be applied to homeless management practices. Increases in cannabis consumption may also foster more peacekeeping and compassion among residential populations that *nurture rather than neglect* the homeless. Future studies could then consider the qualitative relationship between RCL adoption and cultural attitudes towards homelessness, also addressing local homeless policies that are not specific to drug enforcement law, all within the broader context of a changing criminal justice reform landscape.

Regional policy differences notwithstanding, population migration patterns common among the homeless are also an important consideration. Since the homeless are inherently transient and have a tendency towards substance use, migration into RCL adopting states may occur over a multi-year period. Such migration could cause a delayed but increasing effect of RCL on homelessness that is most pronounced in the early adopting states. The effects of this migration are likely compounded by changes in weather. That is, CoCs in states with favorable year-round weather may entice seasonal homeless migration that is not depicted in the January HUD homeless counts. Additional considerations on study limitations are related to group sampling.

Due to delayed implementation times for RCL policies, not all CoCs in each treatment group will have fully repealed cannabis prohibition at the same time, which does introduce the possibility of in-group disparity. For example, the lion's share of the estimated effect for CoCs in Group 2016 would come from California (not Massachusetts or Nevada). Importantly, it is also possible that RCL adoption is a *lagging* indicator for changes in homelessness with respect to incarceration levels as described in Mechanism 1. In this case, cannabis decriminalization would be a *leading* indicator for such effects on rates of homelessness.

Other delays in RCL's effects on homelessness may be attributed to rising user risk behaviors, which may manifest into other substance use problems. In this light, since perceived risk of drug arrest is reduced by RCL, competition for buyers across legal and illegal drug markets may ensue. Even as licensed cannabis dispensaries become ubiquitous, the question of their impact on alternative black-market activity remains; actors may be forced to increase distribution and sale of other more addictive drugs. As this leads to an increased risk of exposure for substance users, it is critical to improve accessibility to community and street-level treatment programs (especially *outside* of the criminal justice system).

In a convergence of supply-side and demand-side market pressures, changes in legal and illegal markets can also create opportunities for cartel footholds. Such favorable conditions are advanced largely by rising user demand, levels of taxation, retail regulatory mechanisms, and variations in cannabis cultivation licensing laws (both in medically and recreationally legal states). For example, as the first state to legalize medical cannabis in 1996, California had highly favorable cannabis cultivation laws for two decades prior to adoption of RCL (i.e., longer than any other state). The significant effects estimated by the DID-MTP model for CoCs in Group 2016 may partially be attributed to this early adoption of medical cannabis legalization.

With fewer regulatory checks, and increased profit opportunities for growers, cartels and other organized crime rings could more easily establish their operations *prior to* RCL adoption. By extension, CoCs in Groups 2012 and 2014, which exhibited similar but insignificant trendlines compared to CoCs in Group 2016, are in close proximity to California. In this context, CoCs in Groups 2012 and 2014 may have experienced some spillover effects from California's cultivation laws, causing increased rates of homelessness.

Whether by population rebalancing effects, rising drug consumption and crime, or changes in cultural attitudes, the results of the model strongly suggest that RCL adoption may cause an increase in aggregate rates of homelessness. Importantly, this effect is most distinct to CoCs in states adopting RCL in the early to mid-2010s, where CoCs later adopting RCL exhibited more mixed results. As early adopting states are pushing the boundaries of policy, it logically follows that CoCs in these states will bear the greatest initial burden from such changes. Similarly, as later-adopting states can learn from early regulatory missteps, the effects of RCL adoption on homelessness may be expected to decline over time. Still, as more states adopt RCL, prior regional effects of shifting supply and demand markets can redistribute geographically, and the estimated effects of RCL on homelessness may become more muted.

For this reason, a lack of continued RCL adoption could allow the observed effects on homelessness to continue, whereas an increase in RCL adoption may disperse homeless populations such that mitigation strategies become more effective. Still, only using more longitudinal data can one observe if the trends will continue. In analyzing trends from a longer-term study, policymakers could cross-examine RCL implementations of adopting states. Future efforts to mitigate risk of rising homelessness would then include an evaluation of community drug treatment programs as well as varying RCL implementations. Finally, states adopting RCL should design its regulatory, tax, and cannabis licensing infrastructures to stimulate legal markets while also stifling black markets.

APPENDICES

Appendix A: Variable Sourcing, Coding, and Treatment Groups

Table 2.1: Variables

	Variable	Unit of Measure	Source	Notes on Operationalization
Dependent Variable	Overall Homeless Counts	Per 10,000 residents	Department of Housing and Urban Development (HUD)	Overall homelessness comes from the PIT counts and includes all sheltered and unsheltered homelessness.[1]
Independent Variable	Recreational Cannabis Legalization (RCL); Year of State Adoption	For treated samples → year of treatment Otherwise → 0	National Conference of State Legislatures (NCSL); Marijuana Policy Project (MPP)	A sample is considered treated for CoCs when its state has passed adult-use cannabis law (either by ballot initiative or state legislature). This variable is coded as the first year in which RCL is adopted to create treatment groups. See Tables 1.2 and 1.3 below for more information on RCL adoption and treatment groups.
Control Variables[2]	Poverty	Percentage of total county population	U.S. Census Bureau	This is an aggregated percentage of all county residents associated with a CoC region at/below the poverty line.
	Total County Population	Aggregate Numerical Count	U.S. Census Bureau	This is an aggregated total population count of all counties associated with a CoC program.
	Jail Population Size	Per 10,000 residents	Vera Institute of Justice	This includes the aggregated jail populations for all counties associated with a CoC program.[3]
	Weather: A Measure of Colder Climates	Fahrenheit	National Oceanic and Atmosphere Association (NOAA)	This is the aggregated average low temperature in January for all counties associated with a CoC program.
	Median Rent	U.S. Dollar ($)	HUD Fair Market Rent (FMR)	Using 50th percentile rent, this is a composite aggregation for all studio and 1-4-bedroom homes in counties associated with a CoC program.
	Housing Inventory Counts (HIC)	Per 10,000 residents	HUD Housing Management Information Systems (HMIS)	This includes the total number of year-round shelter beds in transitional housing, emergency shelters, safe havens, and rapid rehousing programs.

[1]HUD excludes overall homeless counts for 2021 as over half of CoCs had limited data
[2]All control variables are time-invariant averages of mean or median
[3]Jail population data is published through 2018

Table 2.2: Recreational Cannabis Legalization – State Policy Enactment[1]

	Adult-Use Legalized	By Ballot Initiative	By State Legislature	Sales are Taxed/Regulated[2]
Alaska	Nov-14	Measure 2	-----------	Yes
Arizona	Nov-20	Proposition 207	-----------	Yes
California	Nov-16	Proposition 64	-----------	Yes
Colorado	Nov-12	Amendment 64	-----------	Yes
D.C.	Nov-14	Initiative 71	-----------	No
Illinois	Jun-19	-----------	House Bill 1438	Yes
Massachusetts	Nov-16	Question 4	-----------	Yes
Michigan	Nov-18	Proposal 1	-----------	Yes
Nevada	Nov-16	Question 2	-----------	Yes
New Jersey	Nov-20	Public Question 1	-----------	Yes
Oregon	Nov-14	Measure 91	-----------	Yes
Vermont	Oct-20	-----------	S. 54	Yes
Washington	Nov-12	I-502	-----------	Yes

[1]Sourced from the Marijuana Policy Project: https://www.mpp.org/states/
[2]Indicates if the state legally taxed and regulated sales prior to the end of 2020 (i.e., the end of the study period)

Table 2.3: Treatment Groups

These groups signify the year in which the state first adopted RCL	**States in Group**	**Number of Observations**
Group 2012	CO, WA	98
Group 2014	AK, D.C., OR	96
Group 2016	CA, MA, NV	651
Group 2018	MI, VT	243
Group 2019	IL	210
Group 2020	AZ, NJ	252
Group 0 (i.e., never treated)	AL, CT, FL, GA, HI, IA, ID, IN, KS, KY, LA, MD, MN, MO, MS, NC, NE, NH, NM, NY, OH, OK, PA, SC, TN, TX, UT, VA, WI, WV	2,704

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