Colorado Amendment 64: Examining The Spillover Effect Of Recreational Marijuana Retail on Labor Productivity

Jaime Li Wu*

Advisor: Enrique López-Bazo◆

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Abstract:

The spillover effect of the initiation of recreational marijuana retail activity on labor productivity remains to be an important topic despite the increasing number of studies that show the positive and negative effects of such type of policy shifts. In the case of the Colorado Amendment 64, studies have found multiple results for the several implications of this policy shift on crime, tax revenue benefits, safety, mortality, and drug dependence exacerbation. However, not until recently, the effect of policy shifts of recreational marijuana sale status on labor productivity has been empirically measured. Common narrative has associated the policy changes around marijuana to potential negative impact on labor productivity, both at the extensive and intensive margin. Moreover, current literature on the topic of recreational marijuana policy changes lacks evidence on the potential spatial dynamics existing between areas that might play a major role in inter-regional economic growth. Spatial spillovers represent a functioning and dynamic mechanism through which areas next to each other can be affected by the policy changes their neighbors undertake. In this study, I apply Spatial Difference-in-Difference to estimate the spillover effect of recreational marijuana retail activity on labor productivity. The results suggest that the recreational marijuana retail activity has an average indirect impact of approximately $1,676 USD, indicating that spillover effects are far from negligible and neglecting them lead to undermined results of the real economic impact of the recreational marijuana retail activity.

Keywords: Marijuana, spatial spillover, Difference-In-Difference, productivity.

JEL Codes: C4, C5, I1, K2, R1, O2, O4

*University of Barcelona School of Economics. Email: jliwulix47@alumnes.ub.edu

◆Department of Econometrics, Statistics and Applied Economics and Regional Quantitative Analysis Group (AQR)

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1. Introduction

Liberalization and legalization of cannabis (commonly referred as marijuana), for medical and recreational use, have generated profound and extensive discussions in the political and legislative sphere in several countries for many years, leading to policies and programs aiming to establish guidelines and regulatory framework for its use. For instance, the 2020 elections in the U.S., understandably focused on the Presidential election, also included ballots measures for other decisions to be taken at the state level, and marijuana reform was one of them for many states. During the 2020 elections, voters in states such as Arizona, Florida, New Jersey, New Mexico and New York had to decide whether to legalize recreational marijuana use. Moreover, at the beginning of 2020, 11 U.S. states had fully legalized marijuana (CNN, 2020), and as reported in the Politico newspaper, by the end of 2020, up to 40 states would have some form of marijuana legalization (Zhang, 2020). In the Netherlands, marijuana use is illegal, but decriminalized for personal use, resulting in popular use and without significant repercussions for individuals and hence tolerated, additionally it is widely available in coffee shops, with its commercialization being relatively widespread. Another example of marijuana legalization is Uruguay. In 2013, Uruguay became the first country to widely legalize marijuana across its entire territory, establishing regulations on cannabis use and cultivation, controlled dispensaries regimes, and institutional regulatory framework for its production, distribution and commercialization.

In November 6, 2012, the people of Colorado, U.S. voted to pass Colorado Amendment 64, a ballot measure with the objective to amend the Constitution of Colorado to outline a statewide drug policy for cannabis (Gessler, 2012), becoming the first state in the U.S. to legalize recreational marijuana use. This resulted into a law (Article 18, section 16 of the state constitution), which addresses personal use and regulation of marijuana for adults of 21 and over, commercial cultivation, manufacture, and sale; in general, the law intends to regulate marijuana in a similar manner to alcohol, particularly for recreational use (The Economist, 2012). In January 1, 2014, commercial sale of marijuana to the general public began at establishments licensed by the regulatory framework of the redacted law. Based on the information available in the Colorado Municipal League (2018), as of April 2017, approximately 176 out of 272 municipalities in Colorado prohibit retail sale of marijuana within their boundaries. Under this amendment, the consumption rules of marijuana across the state of Colorado are similar to that of alcohol, meaning that within the boundaries of the state, it can be consumed inside residences, for adults 21 and over, and consuming it in public spaces result in legal penalties. Similarly, driving under the influence of marijuana is penalized with equivalent offenses prescribed to driving under the influence of alcohol.

For years now, the arguments for supporting or opposing some form of legalization of marijuana have influenced the decisions of policymakers, paving the road to diverse approaches of legalization for its use or to simply ban it. For instance, a common concern is whether worker's productivity would be negatively affected by the increased access to marijuana. The legalization of recreational marijuana and its commercialization has an important implication in regards to potential unexpected outcomes on the productivity of workers who decide to purchase and consume it, as it is associated with short and long-run cognitive impairment and psychoses in adulthood (Hall, 2014). In addition, advocates highlight the potential health benefits of marijuana use for individuals with chronic pain and other conditions acting as pain soothing substance that might contribute to individuals' overall productivity (Nicholas & Maclean, 2019). It important to note that recreational marijuana legalization facilitates its consumption, similarly to changes in drinking age or tobacco purchasing, hence a change in consumption of marijuana is the underlying implicit mechanism through which labor productivity of a county potentially changes. Therefore, with the policy change favoring commercialization of recreational marijuana addressed by the Colorado Amendment 64, I am motivated to investigate the overall immediate effect of the recreational marijuana retail activity on labor productivity and additionally address potential shortcomings in the assumptions made in regards to the empirical strategy applied for identification of causal effect.

I will be focusing on the immediate effect of the recreational marijuana retail activity on labor productivity at the county-level in Colorado. This as opposed to state-level, for multiple reasons: (i) previous studies already show that labor productivity decreases in states which legalized recreational marijuana and allowed retail commercial activity, (ii) policies at state-level are not necessarily implemented across the entire state's territory, allowing local county governments to allow or ban retail sale and retail activity of marijuana, (iii) spillover effects at the county-level can be analyzed as they are more likely to take place due to proximity in distances among counties as opposed to states, where the spillover effect among states might only take place in the bordering areas on states' boundaries since people can simply go to the nearest neighboring town allowing marijuana sale next to their state border. Therefore, this study aims to evaluate the potential spillover effect of the recreational marijuana retail on labor productivity, meaning that in addition to comparing the impact of recreational marijuana retail between counties that implemented and counties that remained banning it, it also evaluates the indirect effect that counties that started allowing recreational marijuana sale have on the labor productivity of counties that after 2014 remained banning the commercial sale of recreational marijuana. Consequently, it is crucial to emphasize that under the Colorado Amendment 64, local governments (counties and towns) choose whether sales are allowed or prohibited within their boundaries. In order to capture spatial spillovers, this paper applies a Spatial Difference-in-Difference (Diff-in-Diff) approach. Delgado & Florax (2015) and Kolak & Anselin (2019) provide the theoretical framework of this approach as well as develop empirical studies applying this method on policy evaluation, suggesting that it consistently estimates spillover effects of policy changes. The results obtained indicate (1) positive average direct effect of recreational marijuana retail activity on labor productivity in counties that implemented it in their territory, and (2) average indirect effect of approximately $1,676 USD from counties that allowed recreational marijuana retail activity on labor productivity of neighboring counties defined as sharing a physical border or based on different neighboring distances. The results suggest that spillover effects are far from negligible and neglecting them lead to undermined results of the real economic impact of the recreational marijuana retail activity. The paper elaborates on the causal mechanism at work and the potential shortcomings and limitations in regards to the treatment definition as well as the unconfoundedness assumption evoked in the traditional Diff-in-Diff setup.

The remaining of the paper contains the following structure: related literature, empirical strategy, data and variables, main results, and finishes with the concluding remarks.

2. Related Literature

2.1. Evidence on The Effect of Marijuana Policy Changes

The debate about the pros and cons of marijuana legalization and changes of marijuana policies is an ongoing affair among policymakers, politicians, and government officials. Some argue that the pros outweigh the cons and therefore marijuana legalization is the optimal strategy to regulate the substance (and to some extent limit the informal market of marijuana sale) and at the same time to benefit from taxing it, similar to alcohol and cigarettes, and hence, invest this tax revenue on public goods. Those against it, argue that marijuana legalization will exacerbate the problems associated with marijuana use and its effect on health and socioeconomic conditions of individuals and communities. There exists vast literature dedicated to analyze the effect of changes of marijuana policy on several health, social, and economic outcomes. Thus, taking into consideration these effects is crucial in the process of policy making and substance regulation, since empirical evidence allows local governance to justify their policy decisions on marijuana laws.

For instance, a study conducted by Stohr et al. (2020) analyzes the effect of marijuana legalization on crime rates for Washington state by estimating a series of multi-group interrupted time-series models comparing Washington to a set of 21 "control" states (those without any laws permitting legal access to marijuana) from 1999 to 2016 on monthly violent and property crimes. Their results did not reveal any broad findings suggesting that legalization increased or decreased serious crime rates in Washington compared to the control states. Moreover, they also estimate the effect of marijuana legalization on crime rate at the county-level in Washington to explore heterogeneous effects of marijuana legalization, finding considerable county-to-county level variation in violent crime trends, nevertheless; overall violent crime rates remained stable for most counties, regardless of their rules on cannabis sales. Hence, the authors suggest there is no evidence that crime rate in counties that, banned sales, temporarily banned sales, or temporarily allowed sales differ systematically from counties that allow recreational sales. Moreover, Dragone et al. (2018) exploit the staggered legalization of recreational marijuana enacted by the adjacent states of Washington (end of 2012) and Oregon (end of 2014). They combine county-level Diff-in-Diff and spatial regression discontinuity designs. Their results suggest that the policy caused a significant reduction in rapes and property crimes on the Washington side of the border in 2013-2014 relative to the Oregon side and relative to the pre-legalization years 2010-2012. The legalization also increased consumption of marijuana and reduced consumption of other drugs and both ordinary and binge alcohol.

Another important topic in regards to the legalization of marijuana is the relationship between marijuana use and mortality, and particularly opioid overdose mortality. Alcocer (2020) applies synthetic control method and state-level data to estimate the effect of marijuana legalization in Colorado on opioid death rate, finding that the estimated negative 5% drop in overdose death rates was deemed insignificant on conducting a placebo in-space analysis, meaning there is not enough evidence to prove that opening recreational marijuana dispensaries as a result of recreational marijuana legislation was instrumental in reducing Colorado's ongoing opioid crisis depicted through opioid overdose deaths. However, the author states that owing to the lack of additional post-treatment data and captured lagged effects, it is too soon to dismiss this policy as inadequate in combating the opioid epidemic.

Two other studies conducted around the same time show opposite results suggesting that recreational marijuana legalization and opening of dispensaries are associated with reduced opioid related death rates. Chan et al. (2020) use Diff-in-Diff approach to estimate the effect of medical marijuana laws (MMLs) and recreational marijuana laws (RMLs) on fatalities from opioid overdoses across 29 U.S. states including the district of Columbia, and find that marijuana access induces sharp reductions in opioid mortality rates. Their research corroborates prior findings on MMLs and offers the first causal estimates of RMLs impacts on opioid mortality to date, the latter of which is particularly important given that RMLs are far more expansive in scope and reach than MMLs. In their preferred econometric specification, they estimate that RMLs reduce annual opioid mortality in the range of 20%–35%, with particularly pronounced effects for synthetic opioids. The authors demonstrate how RMLs impacts vary among demographic groups, shedding light on the distributional consequences of these laws. Hsu & Kovacs (2020) find similar results by using panel regression methods with information for 812 counties in the U.S. in the 23 states that allowed legal forms of cannabis dispensaries to operate by the end of 2017. They find that an increase from one to two storefront dispensaries in a county is associated with an estimated 17% reduction in all opioid related mortality rates.

The economic implications of marijuana legalization have also been a controversial topic among policy makers, particularly as regards its impact on the labor market, considering the association of marijuana use with short- and long-run cognitive impairment and psychoses in adulthood (Hall, 2014). Previous studies have explored the effect of MMLs and marijuana liberalization on labor market outcomes in the context of the United States. For instance, Sabia & Nguyen (2016) conducted a study to examine the impact of MMLs on the labor market by using repeated cross-sections of the U.S. Current Population Survey from January 1990 to December 2014. Overall, they find no evidence that MMLs are associated with statistically significant or economically important changes in employment, hours worked, or wages among working-age individuals across much of the age distribution. They find some evidence that MMLs are negatively related with hourly earnings of young adult males, particularly those ages 20 to 39, a population whose marijuana use has been on the rise in response to MMLs. They found a negative effect in the form of a 2 to 3 percent reduction in hourly earnings for young males between the ages of 20-29, and for those ages 30-39 a statistically not significant 1.3 percent decline in wages. The authors find little evidence for women and older males of adverse labor market effects of MMLs and conclude that the health effects of MMLs may adversely affect labor market productivity of young males.

Albino (2017) takes a similar approach to that of Sabia & Nguyen (2016) but at the aggregate level, using a panel of the 50 U.S. states between 2000–2014. The study examines the effects of marijuana policy changes on labor productivity, both overall and in selected industries, and uses Diff-in-Diff approach to exploit the variation in timing of policy changes between states as they shift marijuana policy. The results of this study suggest that there is a statistically significant decrease in labor productivity of about 1.3% in the year following marijuana policy shifts, pointing to a lagged effect of marijuana policy shifts, but the effect is not consistent across sectors, with some showing concentrated impacts and others showing none.

A similar study by Nicholas & Maclean (2019) emphasizes that the labor supply of older adults is at risk due to poor health, but that many of their symptoms could be alleviated by medical marijuana. Given the lack of evidence, the authors seek to determine how older adults respond to MMLs. To do so, they quantify the effects of state MMLs on the health and labor supply of adults age 51 and older, concentrating their attention on the 55 percent with one or more medical conditions presenting symptoms that could respond to medical marijuana. They use longitudinal data from the Health and Retirement Study to estimate event study and Diff-in-Diff regressions models. The authors report three principal findings regarding the impact of active state MMLs; first, they lead to lower pain and better self-assessed health among older adults; second, they increase older adult labor supply, with effects concentrated on the intensive margin; and, third, their effects are most evident among older adults with a health condition that would qualify for legal medical marijuana use under current state laws. Similarly, Abouk et al. (2021) find evidence that marijuana use, but not misuse, increases after RML adoption, which is in line with additional medical use among older adults. They also show that prescription fills for medications used to treat chronic pain decrease post-RML. They also observe that reduction in welfare compensation benefits (WC) is not due to a concurrent decrease in labor supply mechanically reducing WC participation or due to industry composition shifts which lead to a higher share of the workforce in safer industries. Instead, they observe an increase in labor supply due to RML adoption, which is further in line with RMLs improving work capacity among older adults. They observe complementary evidence that RMLs reduce (non-fatal) workplace injury rates and self-reported work-limiting disability propensities. The results by Abouk et al. (2021) suggest that RMLs reduce work limitations related to chronic health conditions.

Most of the current evidence on the effect of the use of marijuana are analyzed under the context of medical marijuana and very little has been analyzed under the context of recreational marijuana. With countries, states, and counties adopting more flexible recreational marijuana policies on its availability and commercialization within their territory, it is crucial to examine and further provide evidence on the impact of changes of recreational marijuana retail activity on labor productivity in those areas that decide to allow its retail activity but also on the neighboring areas that due to proximity might be affected by the policy changes. The aim of this study is to contribute to the spatial econometrics and policy evaluation literature by providing evidence from the Colorado legislative change on recreational marijuana.

2.2. Policy Changes Creates Spatial Spillover Effects

Marijuana legalization in the U.S. remains to be introduced at the state-level, while marijuana use is still illegal under federal law. Policy makers and the public alike are concerned about the spillover effects of marijuana legalization from one state to another, particularly as regards recreational marijuana. In this case, distance proximity is a key factor to consider when analyzing spillover effects. Studies have used spatial econometric techniques and models in order to capture these spillovers effects in the context of marijuana policy change; nevertheless, these studies are scarce and very recent. For example, Wu et al. (2020), adopt a spatial Diff-in-Diff approach to estimate the spillover effects of recreational marijuana legalization (RML) on crime by examining county-level data in neighboring states before and after Washing and Colorado legalized marijuana. Their results provide some evidence of a spillover crime reduction effect of RML, reflected falls in rates of property crime, larceny, and simple assault in the Colorado region that includes six neighboring states. Their results also suggest that the effects of RML on crime in neighboring states vary according to on crime type and state. Likewise, Hao & Cowan (2020) examine the spillover effects of RML in Colorado and Washington on marijuana possession in neighboring states using county-level data. They find that RML causes a marked increase in marijuana possession arrests in border counties of neighboring states relative to non-border counties in these states. RML has no impact on juvenile marijuana possession arrests, being concentrated fully on adults. They find mixed results regarding the source of these changes. Drawing on self-reported marijuana use data, they show that RML is accompanied by an increase in marijuana use in neighboring states relative to non-neighboring states.

From a methodological point of view, a seminal study by Vega & Elhorst (2015) provides a comprehensive overview of the strengths and weaknesses of different spatial econometric model specifications[1] for estimating spillover effects. They advocate taking the SLX spatial model as a point of departure when a well-founded theory indicating which model is most appropriate is lacking. The following section describes the methodology underpinning the SLX model and its formulation. The SLX spatial model is the preferred model for the empirical analysis of the spillover effect of recreational marijuana retail activity for multiple reasons that will be described in the next section. Vega & Elhorst (2015) argue that, in contrast to other spatial econometric models, the SLX model permits the spatial weights matrix W to be parameterized and standard econometric techniques to be applied to test for endogenous explanatory variables. To illustrate the pitfalls of the standard spatial econometrics approach and to demonstrate the benefits of their proposed SLX alternative approach for empirical analysis, the authors estimate the cigarette demand model first proposed by the Baltagi & Li (2004). By using a panel of 46 states from 1963−1992, Vega & Elhorst (2015) show that applying the SLX model, a parameterized inverse distance matrix, and price treatment (i.e. change in price of cigarette) in the state as endogenous, their results indicate a significant price spillover effect of 0.192, indicating that if cigarette prices rise by 1 percent, cigarette demand in neighboring states increases by 0.192 percent.

Kolak & Anselin (2019) also analyze spatial spillovers using spatial econometric models. The authors replicated and extend a standard policy study of measuring drinking age policy effects on motor vehicle related mortality and overall mortality. They find that not

[1] The first seminal book and related papers (Anselin, 1988; Anselin et al., 1996) demonstrate the methodology and the formulation of these spatial econometric model specifications, which eventually were further analyzed and empirically proven to provide consistent estimation of spatial spillover (i.e. indirect) effects.

[2] Angrist et al. (1996) and Delgado & Florax (2015) refer to SUTVA as the stable unit treatment value assumption, which implies that potential outcomes for unit *i* are unrelated to the treatment status of other units.

only was the effect of the policy change a highly spatial phenomenon, but also its distribution reflected multiple spatial processes. The authors claim that "spatial effects can serve as a proxy for many underlying processes in complex problems common to urban and regional policy" (p. 18). They argue that by specifying these correctly and after having diagnosed the observed spatial pattern, the SUTVA[2] of no intraunit interaction may be relaxed. Moreover, they highlight that a wide range exploratory spatial data analysis techniques can be implemented to define the spatial processes and thus avoid misspecification of the types of spatial effects.

Despite the evidence pointing to the effects of marijuana policy changes on crime and mortality, and, particularly, on labor productivity at both the aggregate level across states (Albino, 2017) and at the individual level (Sabia & Nguyen, 2016; Nicholas & Maclean, 2019), surprisingly little attention has been paid to the specific context of the spillover effects of changes in marijuana policy on the labor productivity of neighboring areas that have remained resistant to similar changes in policy within their boundaries. Therefore, by focusing on the specific case of Colorado amendment 64, and by applying the empirical approach used by Vega & Elhorst (2015), Kolak & Anselin (2019), and the methodological description of the implementation of spatial Diff-in-Diff estimation by Delgado & Florax (2015), Gibbons & Overman (2012), and LeSage & Pace (2009), this paper aims to contribute to the analysis of Colorado Amendment 64 by providing empirical evidence of spillover effects of the recreational marijuana policy change, particularly, the retail activity of recreational marijuana that began in January 2014. This study seeks to derive the empirical model and results from three particular factors, namely (1) the proximity between the counties in Colorado, (2) the variation in marijuana retail status across the Colorado counties, and (3) the available data on labor market outcomes. These factors facilitate the estimation of the indirect (i.e. spillover) causal effect of counties with recreational marijuana retail sales on the labor productivity of counties banning recreational marijuana retail.

[2] Angrist et al. (1996) and Delgado & Florax (2015) refer to SUTVA as the stable unit treatment value assumption, which implies that potential outcomes for unit *i* are unrelated to the treatment status of other units.

3. Empirical Strategy

3.1. Spatial Difference-in-Difference Framework

The Diff-in-Diff model for estimating causal effect is a standard tool for program evaluation (e.g., Ashenfelter, 1978; Ashenfelter & Card, 1985). It consists in the estimation of treatment effect corresponding to the difference between two potential outcomes, with potential outcomes being a function of treatment status (Rubin, 1974). The fundamental and common issue addressed by empirical studies using Diff-in-Diff approach is that units are never observed in both treated and untreated states (Holland, 1986). Hence, identification requires comparison between treated and untreated (control) units. Given a set of time-varying covariates, X_{it}, the standard and generalized Diff-in-Diff equation is:

$$y_{it} = \alpha_0 + \alpha_1 X_{it} + \alpha_2 D_{it} + \alpha_3 T_{it} + \alpha_4 D_{it} \times T_{it} + \varepsilon_{it} \quad (1)$$

Where observations $i = (1,2\ldots n)$ are observed at least two time periods $T \in (0,1)$, and are grouped via $\mathrm{D} \in (0,1)$ such that $D_{it} = 1$ indicates treatment. ε_{it} is the mean-zero error term that is uncorrelated with D_{it} and T_{it}. The identifying assumptions require (1) correct linear specification, (2) homogenous treatment effect, and (3) the parallel-trends assumption denoting that in the absence of treatment both treated and untreated units evolve along the same temporal path. Moreover, strong or weak ignorability (unconfoundedness) is assumed, implying that treatment assignment is independent of the outcome, y_{it}, conditional on X_{it}.

Another key assumption for identification is SUTVA (see footnote 2). Violation of the SUTVA invalidates identification of causal effect in the standard Diff-in-Diff setup due to omission of relevant variable (i.e. variable capturing indirect effect) and therefore adjustments are needed in order to generate consistent and unbiased estimates (Delgado & Florax, 2015). In other words, the standard Diff-in-Diff estimation only consider one's own treatment status but when the SUTVA[3] is relaxed then the treatment status of other units must be taken into account as well. In this study, the model proposed is explicitly interested

[3] For more detailed explanation on the implications of the SUTVA, and the particularity of spatial correlation see (Delgado & Florax, 2015). They emphasize that it is important to note that the model proposed in their paper is explicitly interested in spatial correlation caused by dependence in potential outcomes, as opposed to spatial correlation stemming from spatially correlated treatments or any other sort of spatial sorting process. Spatial interaction in treatment responses requires a plausible causal mechanism to be at work; existing spatial patterns due to spatial sorting need to be excluded as a source of spatially correlated treatment responses.

in spatial correlation caused by dependence in potential outcomes as opposed to spatial correlation stemming from spatially correlated treatments or any other sort of spatial sorting process, meaning that the study analyzes the effect of the treatment on the outcome of treated units but also the effect of the treatment the treated units have on the outcome of a untreated unit that is close in distance. In principle, a specification that explicitly includes a parameter capturing the indirect (i.e. spillover) effect of neighbors' treatment status on potential outcome allows for consistent estimation of the average treatment effect (ATE) conditional on the three identifying assumptions and ignorability. Delgado & Florax (2015) suggest a spatially structured Diff-in-Diff specification (SDID). The specification without loss of generality and with the spatial interaction in the response is given by:

$$y = \alpha_0 + \alpha_1 X + \alpha_2 D + \alpha_3 T + \alpha_4 D \times T + \alpha_5 WD \times T + \varepsilon \qquad (2)$$

In which $\alpha_5 = \rho\alpha_4$, where ρ is a spatial autoregressive parameter, D represents random or spatially correlated treatments, and W is a block diagonal $(N \circ T) \times (N \circ T)$ spatial weight matrix[4]. In contrast to equation 1, variables and the error term denote now the corresponding vectors with observations for every unit in each time period. In this setting, α_4 corresponds to the average direct treatment effect (ADTE), while $\alpha_5 WD$ corresponds to the average indirect treatment effect (AITE). The necessary control group consists of units that are neither treated directly nor indirectly (Delgado & Florax, 2015).

Gibbons & Overman (2012) and Vega & Elhorst (2015) explicitly label equation 2 as SLX spatial model containing one or more spatially lagged exogenous variables, and suggest it as part of the spatial econometric models 'family' (See Figure A1). Both of these papers even argue that this model must be considered as point of departure as opposed to any other spatial econometric model (i.e. Spatial Autoregressive Model, Spatial Autocorrelation Model, Spatial Error Model, Spatial Durbin Model, Spatial Durbin Error Model). The vast literature in spatial econometric models has applied the above specification without explicitly labeling it as SLX model (e.g., Holtz-Eakin & Schwartz, 1995; Dalenberg et al., 1998; Fischer et al., 2009; De Lima & Barbosa, 2018; Triaca et al., 2020; Qiu & Tong, 2021). The SLX model is

[4] See (Anselin, 1988), (LeSage & Pace, 2009) and (Gibbons & Overman, 2012) for more detailed information on the definition of a spatial autoregressive parameter and the construction of spatial weights.

also referred as spatially lagged treatment variable model in the context of policy evaluation (Kolak & Anselin, 2019).

This paper opts for using the SLX spatial model and its application in the form of SDID estimator as causal identification strategy in contrast to, for instance, Regression Discontinuity Design (RDD) or Instrumental Variable (IV) approach for two important reasons: (1) the spatial weight matrix, W, can be parameterized as opposed to other spatial econometric models; and hence, account for spatial spillovers at different neighboring distances, and (2) that instrumental variables (IV) approach, developed outside of the spatial econometrics literature, can be used to analyze whether the $D \times T$ variable and their spatially lagged values, $WD \times T$, are endogenous (Vega & Elhorst, 2015). In this analysis, the IV approach on the treatment is not applied given the plausibility of the ignorability and unconfoundedness assumption of the treatment explained in the following subsection. However, it is important to note that in the case in which the treatment is known to be endogenous due to reverse causality or unobservable variables the SLX model is a flexible and advantageous empirical model for identification of causal effect.

Other studies have implemented SDID (Heckert & Mennis, 2012; Wu et al., 2020; Hao & Cowan, 2020) by comparing treated and control groups under the assumption that neighboring units surrounding a particular unit adopting a treatment are considered the treated group while neighboring units surrounding a particular unit that never adopted a treatment are considered the control group. While this SDID technique accounts for spatial dynamics and allows comparing the potential outcomes between treated and control groups, this technique relies on the assumption that the treated unit has a direct and unidirectional effect on neighboring units and these neighboring units are untreated themselves during the entire time period of analysis. In the case of this study, this assumption does not hold because treated and untreated units are both surrounded by other treated and untreated units during the time period under analysis. Therefore, the proposed SDID model by Delgado and Florax (2015) and applied by Diao et al. (2017); De Lima & Barbosa (2019); Qiu & Tong (2021); Triaca et al. (2020) is a more flexible and feasible model to estimate the indirect causal effect of the treatment when treated and untreated units are surrounded by other treated and untreated units. As mentioned before, a shortcoming to be aware of is that if spatial sorting or spatial correlation originating from spatially correlated treatments are present, then the SDID estimation generates biased and inconsistent results because the

SDID, despite accounting for the spatial interaction, fails to recognize the spatial sorting. In other words, the spatial sorting is picked up as a treatment effect, which it is not.

Hence, for the empirical analysis of the spillover effect of recreational marijuana retail activity on labor productivity, the SLX spatial model is the point of departure, allowing the recreational marijuana retail activity status to be considered the treatment in a Diff-in-Diff setting. By parameterizing the spatial weight[5], W, further robustness checks can be performed; and hence, neighboring units are not only defined on the basis of binary contiguity (i.e. sharing a physical border), but also on the distance between units which do not necessarily share a physical border. The distance between units decays meaning that 'all units are related to all units, but closer ones more so'; therefore, this will allow setting different neighboring distances and observe the heterogeneous effects of the direct and indirect average treatment effect based on distance as well as physical border sharing. The spatial weight matrix based on distance is commonly computed using the Euclidian distance[6] between the centroids of the units.

The following subsections elaborates on the causal mechanism at work of recreational marijuana retail activity on labor productivity under the context of Colorado Amendment 64, and Section 4 describes the variables and the reason behind using them for estimating average direct and indirect treatment effect. Moreover, considering the Diff-in-Diff setup and the spatially structured Diff-in-Diff design, additional counterfactual exercises as robustness checks corresponding to the parallel-trend assumption are performed to test the validity of the main results.

3.2. Direct Effects of Colorado Amendment 64

To consider the average effect of recreational marijuana retail activity status on labor productivity, the analysis starts with a two-way fixed effects model that includes county fixed effect and year fixed effect, which allows capturing the difference between the labor

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5 Vega & Elhorst (2015) provide a detailed explanation on the advantages of the SLX spatial model in regards to parameterization of the spatial weight. They argue that although interaction between units can be independent from geographical proximity (see Neumayer & Plumper, 2013 for examples), for those cases in which researchers can place more weight on closer observations, they demonstrate the flexibility that occurs if researchers take one step forward by using a simple parametric approach applied to the elements of an inverse distance matrix: $w_{ij} = \frac{1}{d_{ij}^{\gamma}}$, where d_{ij} denotes the distance between observations i and j, and γ is the distance decay parameter to be estimated.

6 See Drukker et al. (2013) for the description and the elaboration of an inverse distance spatial weight matrix and the use of the Euclidian distance between units.

productivity before and after adopting recreational marijuana retail activity in treated counties. This empirical strategy, widely used across all areas of empirical economics and policy evaluation, has been recently applied to estimate the economic impact of marijuana and substances policy changes on a regional scale (e.g., Sabia & Nguyen, 2016; Albino, 2017; Nicholas & Maclean, 2019). The area units are counties in Colorado. The treated counties are those allowing recreational marijuana retail activity in year *t*, while the untreated (control) counties are those that never adopted to allow recreational marijuana retail activity within their boundaries. The first year of intervention is 2014; meaning that for the years 2011, 2012 and 2013, all the counties are untreated. The total number of counties is 64. The period of time under analysis is 2011-2017. Some counties adopted recreational marijuana retail activity in 2014 while others in the following years. Table A2 shows a table of the counties that implemented recreational marijuana retail activity by entry year respectively. The benchmark specification is given by the following equation:

$$y_{it} = \alpha + \beta Sale_{it} + \gamma X_{it} + \delta C_i + \sigma Yr_t + \varepsilon_{it} \qquad (3)$$

Where y_{it} is the outcome of interest – average income per capita – in county *i* in year *t*, $Sale_{it}$ is the dummy treatment variable that takes the value of 1 if county *i* allows recreational marijuana retail activity in year *t*, and 0 otherwise. Hence, for county *i* if in 2014 recreational marijuana retail was allowed then $Sale_{it}$ = 1 for *t* = 2014, 2015, 2016 and 2017. X_{it} is a vector of controls that includes total population, average age of total population, labor force participation rate, poverty level, unemployment rate, and high school graduation rate. C_i is the county fixed effect, and Yr_t is the year fixed effect. ε_{it} represents the residual errors, clustered at the county level in all estimations to allow for an arbitrary covariance structure within counties over time.

As widely known for fixed effects models, the county level fixed effect included in the model parametrically allows to control for county time-invariant unobservable characteristics for each county, such as counties' fixed geographical aspects. The year fixed effect included parametrically allows controlling for yearly differences in outcome level common to all counties, such as economic shocks that can affect outcome levels. X_{it} refers to the set of covariates controlling for time-varying characteristics that might be correlated with the outcome. The parameter of interest is given by β, which represents the causal effect of recreational marijuana retail activity on average income per capita. In principle, it calculates the difference between the average of the outcome of interest after the treatment and the average of the outcome of interest before the treatment for treated and untreated counties.

As previously mentioned in regards to the Diff-in-Diff approach, it is not possible to directly test if trajectories differ substantially between treated and control counties since the treated group cannot be observed in the absence of treatment (Angrist & Pischke, 2008). Hence, the robustness of the estimates of the specification in equation 3 is further tested to the existence of dynamic changes that might coincide with the occurrence of the policy intervention. Therefore, equation 3 is modeled with additional pseudo-exposure treatment dummy variables, $Sale_{i2012}$ and $Sale_{i2013}$, indicating adoption of the treatment before the actual implementation of recreational marijuana retail activity in 2014 for the treated counties. That is, the counties treated throughout the 2014−2017 time period are assigned treatment equals 1 for the year 2012 and 2013. This placebo tests allows checking for potential pre-existing trends in the outcome of the treatment effect; capturing any pre-trend differences between treated and controls counties (De Lima & Barbosa, 2019; Triaca et al., 2020). This way, checking whether causes happen before consequences by allowing the model in equation 3 to have heterogeneous anticipatory effects (leads), denoted by $Sale_{i2012}$ and $Sale_{i2013}$. Hence the estimation is as follow:

$$y_{it} = \alpha + \zeta Sale_{i2012} + \varphi Sale_{i2013} + \beta Sale_{it} + \gamma X_{it} + \delta C_i + \sigma Yr_t + \varepsilon_{it} \qquad (4)$$

If the model estimated in equation 3 incorrectly attributes pre-existing trends in the outcome to our treatment effect, then the coefficient of the dummies indicating years before the initiation of recreational marijuana retail activity in 2014 (ζ and φ) should be significant in equation 4, rendering the parallel-trend assumption invalid.

As it is commonly addressed in the policy evaluation and empirical economics literature, to interpret the β parameter as causal and unidirectional the analysis must rely on the (ignorability) assumption that treatment assignment is independent of the outcome, y_{it}. This assumption is however, strong for several empirical applications. In the context of Colorado Amendment 64, there were several legally binding conditions and propositions that were set up after voters chose to pass the amendment. Based on the information in regards to Colorado Amendment 64 available in the Colorado General Assembly Legislative Council Staff, the amendment required the state of Colorado to establish a regulatory structure for the retail marijuana industry and also required the state legislature to enact an excise tax on retail marijuana with the first $40 million per year dedicated to fund public school construction (Colorado General Assembly, Legislative Council Staff). Moreover, there are additional 17.9% taxes on retail marijuana sale, with the revenues from these taxes assigned to the Marijuana Tax Cash Fund, the General Fund and the State Public School Fund[7]. The revenue in the Marijuana Tax Cash Fund is required to be spent the year after it is collected and used for health care, health education, substance abuse prevention and treatment programs and law enforcement. It might be argued that counties decide to adopt recreational marijuana retail activity in their territory if they foresee economic benefits from the tax revenues of sales; in other words, the contemporaneous economic wellbeing of the county might cause legislators in the county to adopt the policy. While this might be plausible, in this case it is doubtfully to hold for the time period under analysis. As mentioned before, the tax revenues from sales are dedicated to specific programs primarily in education and healthcare, and not for instance, to economic aid stimulus packages that might have an immediate effect on average income per capita, hence legislators might decide to adopt the policy on the basis of improvement to health and education rendering the assumption that average income per capita affects the treatment decision invalid. Furthermore education and healthcare investments driven by the tax revenues might take several years to have an impact on the economy, education attainment, poverty or even employment rate of a county. Therefore the ignorability assumption can be assumed to hold conditionally on the covariates.

The covariates added to the specifications estimated allow increasing the precision of the Diff-in-Diff estimations. It is important to note that the included covariates could be themselves affected by the policy, and hence adding these controls would generate a bias in the estimate of the Diff-in-Diff. However, in this study it is plausible to assume that the policy has no effect on the covariates during the time period of analysis given that the values of the covariates do not vary significantly and, in the case that the policy has an effect on the

[7] The Colorado General Assembly website, in the legislative council staff section dedicated to marijuana taxes provide further information on the tax structure and tax distribution of the regulatory framework of recreational marijuana retail. The Colorado Department of Education (https://www.cde.state.co.us/communications/2019marijuanarevenue) provides detailed information on the use of the recreational marijuana sale taxes on school projects.

covariates it would take several years (Aka & Dumont, 2008; Ogundari & Awokuse, 2018; Ahsan & Haque, 2017).

3.3. Indirect Effects of Colorado Amendment 64

In order to consider the potential spillovers effects of recreational marijuana retail activity on labor productivity, an important assumption for the validity of the Diff-in-Diff estimator requires being somewhat relaxed, that is SUTVA. As mentioned in subsection 3.1, failing to account for potential spillover effects of recreational marijuana retail activity on labor productivity of neighboring counties renders the standard Diff-in-Diff to be biased due to omission of potential relevant variable. Spillover effects across counties in Colorado can be expected due to the proximity among them and the free mobility of individuals across county borders. Moreover, as discussed in section 2, there is strong evidence in favor of the existence of spatial relation between counties that allowed recreational marijuana retail activity and those did not allow it, particularly in the context of crime, marijuana possession, and inter-state diversion (Brinkman & Mol-Lamme, 2019; Wu et al., 2020; Hao & Cowan, 2020; Hansen et al., 2017).

Following the SDID proposed by Delgado & Florax (2015), this strategy allows to explicitly consider the local spatial dependence of the treatment variable, so that the outcome of county *i* depends not only on its own treatment but also on the treatment status of close neighbors. The specific SDID considered in this study is given by the following equation:

$$y_{it} = \alpha + \beta Sale_{it} + \mu \sum_{j=1}^{n} w_{ij}\, Sale_{it} + \gamma X_{it} + \delta C_i + \sigma Yr_t + \varepsilon_{it} \quad (5)$$

Equation 5 maintains all the other variables as described for equation 3 but includes a spatial lag of the treatment dummy $w_{ij}Sale_{it}$, where the term w_{ij} is the *i-j* element of the spatial weight matrix required for the SDID, capturing the neighborly relationship between county *i* and county *j* (remembering that $j \neq i$). The coefficient β measures the average direct treatment effect (ADTE) of the county's own treatment status, and the coefficient μ measures the average indirect treatment effect (AITE) of neighboring treated counties, or in other words, the spillover effect of recreational marijuana retail activity. Remember, the control group consists of counties that are neither directly nor indirectly treated, as mentioned before.

Taking into consideration the advantage of the SLX spatial model on the parameterization of W, this study uses two types of spatial matrices: (1) a binary contiguity matrix in which w_{ij} = 1 if *i* borders *j* and 0 otherwise, and (2) an inverse-distance matrix defined in miles radius for neighbors in which w_{ij} = 1 if *j* is within the radius defined in miles and 0 otherwise.

4. Data and Variables

The data required to investigate the direct and indirect effects of recreational marijuana retail activity on labor productivity of the Colorado counties consists of a balanced panel for the years between 2011 and 2017 for the 64 counties in Colorado. This time interval is chosen particularly due to potential pre-existing anticipatory cultivation and possession of marijuana, which passed to the Colorado constitution in December 2012. Hence, if underground selling of marijuana for recreational purposes increased before 2014 then by comparing pre and post-treatment in 2014 generates the impact of the recreational marijuana retail activity in regulated dispensaries that started to operate in 2014. This is tested and performed as robustness check for the main results obtained. Moreover, the period 2015-2017 is chosen as the post-treatment to observe the evolution over time of the outcome of interest recognizing that there were no major policy changes around the retail activity of recreational marijuana during these three years.

Dependent Variable. The dependent outcome variable Y_{it} is the average income per capita at the county-level for the year 2011-2017. This data is available in the Census Bureau For Midyear Population of the U.S. Bureau of Economic Analysis. Previous studies, such as that of Wu et al. (2020) have used this data showing that the information available is appropriate, for instance, in the analysis of spillover effects of recreational marijuana retail activity on crime. Moreover, this information is open source and widely used for similar studies analyzing policy changes across states and counties in the U.S. Average income per capita might arguably not be a good proxy for labor productivity; nevertheless, many studies use it as proxy due to data limitation and the theoretical uncertainty behind the most appropriate measurement of labor productivity. Recent studies such as that of Albino (2017) use average income per capita as indicator of labor productivity, with an important and strict assumption that income is the true marginal product of labor. That is, average income per capita reflects the real productive capacity of individuals. Van Ours (2007); Sabia & Nguyen (2016); and Nicholas & Maclean (2019) use wages as proxy for labor productivity, which in the general aggregated-level analysis in macroeconomics framework is interpreted as average income per capita. Average income per capita is transformed logarithmically in order to have a potentially skewed variable into a more normally distributed variable. Hence, the coefficients associated to the treatment and covariates in the estimations in the following section represent the relative changes caused by the treatment and covariates on average income per capita as opposed to absolute change.

Treatment Definition. An important detail when estimating the effect of recreational marijuana retail activity is related to the opening of recreational marijuana stores that obtained a license in a particular year and were able to operate while those who obtained it but may not have local (i.e. county) approval by the authorities. For this study, I rely on the Annual Recreational Marijuana Tax Report of the Colorado Department of Revenue, which is available from the first year recreational marijuana sales were allowed ─ year 2014 ─, and up to 2020. This annual report contains information on the counties that reported tax collection from recreational marijuana sale in a given year. Counties that reported tax collection from recreational marijuana sales are considered as the treated counties whereas counties that did not report tax collection are considered as the untreated counties. It is important to emphasize that some counties started to allow recreational marijuana sale after 2014. Based on the annual reports, counties that implemented recreational marijuana retail in 2014 remained allowing it all the way up to 2017, while other counties allowed it after 2014, namely either in 2015 or 2016. There exists vibrant recent literature on the implementation of treatment at multiple time periods, where treatment for different units takes place in different time periods. This literature is quite recent (Callaway & Sant'Anna, 2020). This study takes a simplified version since counties that implemented recreational marijuana retail in 2014 or later remained allowing it up to 2017. The results generated in this study intend to demonstrate the spillover effect of the recreational marijuana retail activity induced by counties that reported sales in the treatment year and post-treatment years. An important caveat of this analysis is the definition of the treatment effect. In this study the treatment effect is assumed to be homogenous. That is, the treatment effect of a treated county is the same for all the years under analysis and that all the treatment effect for all the treated

Table 1: Descriptive Statistics

Panel A:

Descriptive Statistics: Pre-intervention period

Variable	Treated Counties Before 2014 Obs.	Mean	Std. Dev.	Untreated Counties Before 2014 Obs.	Mean	Std. Dev.	Mean Difference
Income	105	42711.84	14111.63	87	38974.06	10228.98	3737.78
Total Population	105	128972.67	203178.26	87	23570.65	54387.05	105402.02
Avg. age of total population	105	42.09	5.79	87	41.76	5.52	0.33
Labor force participation rate	105	77.83	5.07	87	75.91	8.78	1.92
Poverty level	105	14.50	5.28	87	16.23	8.34	-1.73
Unemployment rate	105	8.33	2.06	87	7.26	2.42	1.07
High School graduation rate	105	0.78	.156	84	0.83	.098	-0.05

Panel B:

Descriptive Statistics: Post-intervention period

Variable	Treated Counties After 2014 Obs.	Mean	Std. Dev.	Untreated Counties After 2014 Obs.	Mean	Std. Dev.	Mean Difference
Income	140	50696.57	18712.83	115	41720.52	10405.94	8975.85
Total Population	140	136419.81	215823.45	115	24696.51	59360.39	111723.30
Avg. age of total population	140	41.72	5.49	115	42.28	5.78	-0.56
Labor force participation rate	140	78.24	5.30	115	76.93	9.66	1.31
Poverty level	140	13.09	5.59	115	15.62	8.34	-2.53
Unemployment rate	140	3.87	1.52	115	3.65	1.71	0.22
High School graduation rate	140	0.81	0.131	111	0.85	0.076	-0.04

counties does not vary. This is partly plausible given that recreational marijuana sale is generally accessible to all individuals above the age of 21 once a county decides to implement it. Moreover, individuals above the age of 21 constitute the core working group contributing to the aggregated average income per capita of a county.

Control Variables. The set of control variables were selected based on the empirical literature of policy impact on regional growth (Kolak & Anselin, 2015; Sabia & Nguyen, 2016; Albino, 2017; Wu et al., 2020). Table A3 shows the definition of all the variables used in this study. The data and information for the included control variables X_{it} were extracted from different sources. The total population, average age of total population, labor force participation were obtained from the State Demography Office Data of the Colorado Department of Local Affairs State, which is available for several years from the 1990s and also contains forecasted information for the incoming years. The high school graduation rate was extracted from The Annie E. Casey Foundation Kids Count Data Center; the poverty level control was obtained from the U.S. Census Bureau's Small Area Income and Poverty Estimates; and the unemployment rate from the Local Area Unemployment Statistics of the Bureau of Labor Statistics at the U.S Department of Labor. The availability of open source data for the control variables is somewhat reliable given the consistency and frequency it is reported.

Panel A of Table 1 shows the descriptive statistics to all variables used in the analysis and the differences for all the variables between treated and untreated counties before the implementation of recreational marijuana retail activity in 2014. Table A4 shows the correlation matrix of the control variables. The information obtained from the descriptive statistics in Table 1 Panel A indicates that the counties that adopted the implementation of recreational marijuana retail (i.e. treated) are in average richer, more populated, and slightly older, with higher unemployment rate, with larger labor force participation, lower poverty level, and slightly lower high school graduation rate.

Panel B of Table 1 shows the descriptive statistics of the same variables and the differences between treated and untreated counties but for after the implementation of recreational marijuana retail activity in 2014 to 2017. The mean difference in average income per capita between treated and untreated counties after the treatment in 2014 and the following years compared to the mean difference for the years before the intervention increased by almost $5,238.07 USD, showing a significant increase in absolute change of average income per capita suggesting a potential effect of the policy on contributing to regional average income per capita growth favoring in larger proportion to the treated counties. Poverty level decreased and unemployment rate decreased while high school graduation rate increased. These can be the result of potential positive effect of regional wealth growth that has taken place at a larger scale in those counties adopting recreational marijuana retail activity compared to those who did not.

Figure A5 shows maps of the average income per capita of the Colorado counties from year 2011 to 2017. During this time interval, a potential spatial positive correlation can be observed. Counties with high average income per capita (dark blue counties in Appendix A5) tend to be surrounded by other high average income per capita counties. While this observation is purely descriptive, it suggests the potential existence of spatial dynamics that contribute to localized regional income per capita growth. Similarly, when observing both, Figure A5 and Figure A6, which is the map for the recreational marijuana retail status for the year 2014, 2015, and 2016, certain degree of correlation can be seen between the average income per capita and recreational marijuana retail status of counties. Counties with high average income per capita that adopted the retail status in 2014 are surrounded by new adopters in the following years; new adopters that also experienced an increase in average income per capita in the years they implemented it. In general, counties experiencing high average income per capita compared to other counties under the time period of analysis implemented recreational marijuana retail activity, and those who did but after 2014 experienced an increase of average income per capita, as Panel A suggests.

5. Results

In this section I present the direct and indirect effects of recreational marijuana retail activity on average income per capita. This section also shows the estimations of the spillover (i.e. indirect) effects using binary contiguity and distance-based spatial weight matrices and the robustness check corresponding to the validity of the parallel-trend assumption underlying the Diff-in-Diff estimator.

5.1 Direct Effects of the Initiation of Recreational Marijuana Retail in 2014

Table 2 presents the estimation of the causal effect of recreational marijuana retail activity on average income per capita of the treated counties. As stated in the empirical strategy section, the benchmark specification for estimating average effect is described by equation 3. Column (1) refers to the model that only includes time and county fixed effects and the treatment variable. Column (2) refers to the benchmark model that is similar to column (1) but additionally includes the set of covariates. According to column (1) the recreational marijuana retail activity led to an average increase in average income per capita of those counties that implemented it; this increase is statistically significant and positive in

Table 2: Effect of recreational marijuana retail activity on income per capita: benchmark specification.

Dependent Variable: Logarithm of Income Per Capita

Variables	(1)	(2)
Sale	0.0713***	0.0442***
	(0.0194)	(0.0138)
Logarithm of total population		0.371**
		(0.168)
Average age of total population		-0.000452
		(0.000606)
Labor force participation rate		-0.000420
		(0.00265)
Poverty level		-0.00817***
		(0.00288)
Unemployment rate		-0.0172**
		(0.00789)
High school graduation rate		-0.0404
		(0.0648)
Time fixed effects	✓	✓
County fixed effects	✓	✓
Observations	448	448
R-squared	0.606	0.642
Number of Counties	64	64

Robust standard errors in parentheses clustered at the county level
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

magnitude given the estimated coefficient. This result aligns to the results of similar studies suggesting a potential positive effect of recreational marijuana use on labor productivity found in the very recent literature by Abouk et al. (2021). The results remain quantitatively the same for the benchmark specification in column (2) that additionally includes the control variables, however, the coefficient decreases. Overall, the average treatment effect of recreational marijuana retail activity on average income per capita is in average of 4.42% for

Table 3: Robustness check: Placebo treatment.

Dependent Variable: Logarithm of Income Per Capita

Variables	(1)	(2)
Sale	0.0667***	0.0417***
	(0.0185)	(0.0138)
Logarithm of total population		0.370**
		(0.170)
Average age of total population		-0.000486
		(0.000605)
Labor force participation rate		-0.000435
		(0.00266)
Poverty level		-0.00808***
		(0.00288)
Unemployment rate		-0.0169**
		(0.00796)
High school graduation rate		-0.0396
		(0.0654)
Sale 2012	-0.0208*	-0.0136
	(0.0105)	(0.0113)
Sale 2013	0.00176	0.00226
	(0.0144)	(0.0149)
Time fixed effects	✓	✓
County fixed effects	✓	✓
Observations	448	448
R-squared	0.607	0.643
Number of counties	64	64

Robust standard errors in parentheses clustered at the county level
*** p<0.01, ** p<0.05, * p<0.1

the treated counties compared untreated counties for every year they allowed it. Note that all that the specifications in column (1) and (2) in Table 2 are estimated with standard errors clustered at the county-level.

In order to test the parallel-trend assumption an alternative specification including time pseudo-exposure placebo treatment as robustness check is performed, as mentioned in section 3. In Table 3, placebo treatment estimation is conducted in order to test the robustness of the results obtained in Table 2 column (1) and (2). Dummies as pseudo-exposure variable for the year 2012 and 2013 are added to reflect as if the treatment had taken place in these two periods for the treated counties. Column (1) does not include the covariates whereas column (2) does. The pseudo-exposure dummy variables for 2012 and 2013 are statistically not significant. This indicates that in 2012 and 2013 the average income per capita trend of the counties that implemented recreational marijuana retail in 2014 and following years (treated counties) was not different from the average income per capita trend of the control counties, suggesting the validity of the parallel-trend assumption. The results obtained in Table 3 corroborate the results in column (1) and column (2) of Table 2 in that the recreational marijuana retail activity has positive impact on average income per capita for the treated counties.

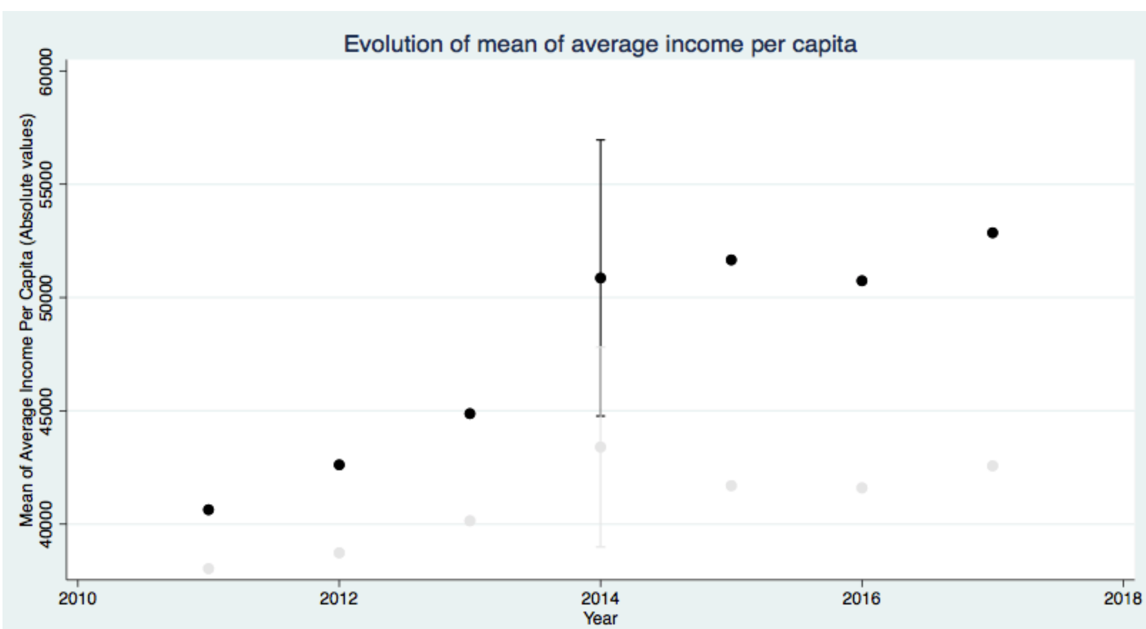

Figure 1: Evolution of the mean of average income per capita. *Source*: Own elaboration with information from Census Bureau For Midyear Population of the U.S. Bureau of Economic Analysis.

Furthermore, Figure 1 shows the graph of the evolution of the mean average income per capita for the control (light gray) and treated (black) groups between the years 2011 and 2017. Each dot represents the mean of each group, control and treated, for the corresponding year. As it can be seen, before the treatment in 2014, both groups were evolving similarly along the temporal path, with an increasing mean for both groups. In 2014, the first year recreational marijuana retail was allowed, the mean difference between treated and control counties substantially increased indicating a possible effect of the treatment. For the treated group, the mean average income per capita steadily increases after the year 2014 but in small percentages with respect to the previous year; in the other hand, for the control group, the mean decreases after the significant effect in 2014 evolving similarly to pre-treatment levels. However, for the post-treatment period, the mean average income per capita for both groups significantly increased compared to the pre-treatment years, and the gap in the mean differences between the two groups remains similarly to that of the year 2014. Thus, the overall mean difference between treated and control groups remains despite both groups experienced an increase in mean of average income per capita indicating potential regional economic growth.

The graph in Figure 1 corroborates with the results from Table 2 and 3, suggesting that the parallel-trend assumption might hold showing that in the absence of treatment the treated group would evolve along the same temporal as the control group. This is arguably reasonable given the results of Table 2 and 3 in which control variables allow to account for differences in observable characteristics and the county and year fixed effects allowing accounting for the differences in unobservable characteristics. Hence, the potential economic recovery after the Great Recession of 2008 in the case of truly generating an increase in the average income per capita of counties, it is plausible to believe that policies aimed at fast economic recovery from the Great Recession were mostly at the state level, in this manner, all counties were equally benefitting from this recovery programs, potentially captured by the fixed effects. In addition, as mentioned in the introduction, counties are generally more similar in their characteristics, as opposed to states that might significantly differ from one another due to geographical characteristics as well as governances. This similarity among counties allows estimating causal treatment effect of the recreational marijuana retail using treated and control groups composed of comparable counties that are similar in observable and unobservable characteristics. Therefore, after controlling for these observable and unobservable characteristics, the treatment remains significant indicating the positive effect of recreational marijuana on average income per capita of treated counties.

5.2 Spatial Spillovers of the Initiation of Recreational Marijuana Retail in 2014

The results obtained for the average treatment effect reveal that recreational marijuana retail activity caused immediate economic impact on the counties that adopted it within their borders. However, the existence of spatial effects to neighboring areas needs to be considered given the proximity and ease of travelling across counties' border. A particular county may be directly affected by the policy (when the policy change occurs within its own boundary) or may be affected indirectly (when the policy change occurs in the vicinity of the county). Additionally, as mentioned in section 3, the SUTVA assumption is unlikely to hold in studies focused on the impact of substance-use policy changes in a regional perspective. Therefore, the estimated average treatment effect in the previous subsection would be biased. Following the methodology proposed by Delgado and Florax (2015), Table 4 shows the estimated results from Equation 5.

Initially, column (1) of Table 4 shows a spatial specification similar to those estimated in subsection 5.1 but with the geographical interaction given by a binary contiguity matrix and column (2) shows the same specification but including the spatial lags for poverty level, unemployment rate and population given that these covariates were statistically significant in the specifications estimated in the previous section and might show spatial effects similar to the treatment variable. For instance, poverty level and unemployment rate in a county might affect the average income per capita in neighboring counties through individuals moving across borders for work and in this manner contributing to the neighboring counties' economic growth. Columns (3) to (8) show specifications that employ different inverse-distance spatial weight matrices. As mentioned in section 3, these specifications are widely used in the spatial econometric literature and are known as SLX model. In addition to reporting the estimated coefficients, Table 4 also reports the corresponding spillover effects or average indirect treatment effects (AITE), which indicates the magnitude of the indirect effect a treated county has on the average income per capita of a neighboring county.

As it can be observed, the results related to the direct impact remain unchanged when compared to the benchmark specification (see Table 2). However, the set of evidence in Table 4 reveals that spatial spillovers are positive and statistically significant, showing that there are also counties indirectly affected by the recreational marijuana retail activity of their

Table 4: The indirect impact of recreational marijuana retail activity on average income per capita: Spatial Diff-in-Diff specifications.

Dependent Variable: Logarithm of Average Income Per capita

Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Sale	0.0394***	0.0405***	0.0379***	0.0350***	0.0396***	0.0373***	0.0444***	0.0452***
	(0.0106)	(0.0101)	(0.0104)	(0.0101)	(0.0106)	(0.0102)	(0.0108)	(0.0104)
W*Sale	0.1421***	0.0962***	0.219***	0.164***	0.154***	0.111***	0.0997***	0.0633***
	(0.0193)	(0.0215)	(0.0273)	(0.0340)	(0.0212)	(0.0251)	(0.0179)	(0.0200)
Log of total population	0.0865	-0.0853	-0.0653	-0.196	0.0299	-0.0932	0.0978	-0.0113
	(0.129)	(0.134)	(0.133)	(0.141)	(0.132)	(0.140)	(0.136)	(0.139)
Avg. age of population	-0.000388	-0.000327	-0.000425	-0.000386	-0.000411	-0.000356	-0.000515	-0.000407
	(0.000488)	(0.000466)	(0.000482)	(0.000465)	(0.000489)	(0.000470)	(0.000501)	(0.000481)
Labor force participation rate	0.00131	0.00184	0.000738	0.000970	0.00107	0.00236	-0.000881	-0.000566
	(0.00189)	(0.00183)	(0.00186)	(0.00182)	(0.00189)	(0.00184)	(0.00193)	(0.00186)
Poverty Level	-0.00738***	-0.00759***	-0.00648***	-0.00700***	-0.00660***	-0.00708***	-0.00663***	-0.00742***
	(0.00246)	(0.00239)	(0.00244)	(0.00238)	(0.00247)	(0.00242)	(0.00254)	(0.00247)
Unemployment rate	-0.0124***	-0.00205	-0.0124***	-0.00178	-0.0145***	-0.00348	-0.0162***	-0.00571
	(0.00378)	(0.00396)	(0.00372)	(0.00408)	(0.00374)	(0.00406)	(0.00383)	(0.00407)
High school graduation rate	-0.0201	0.00331	-0.00767	0.0256	-0.0186	0.0142	-0.0236	0.00617
	(0.0384)	(0.0368)	(0.0381)	(0.0373)	(0.0385)	(0.0374)	(0.0395)	(0.0382)
W*Log of total population		0.677***		0.602*		0.633**		0.352*
		(0.236)		(0.328)		(0.255)		(0.191)
W*Poverty level		0.00394		0.00270		0.00723		0.00225
		(0.00551)		(0.00860)		(0.00591)		(0.00434)
W*Unemployment rate		-0.0363***		-0.0450***		-0.0360***		-0.0321***
		(0.00609)		(0.00845)		(0.00667)		(0.00567)
Time and county fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
Weight matrix	Binary	Binary	100 Miles	100 Miles	150 Miles	150 Miles	200 Miles	200 Miles
Observations	448	448	448	448	448	448	448	448
Pseudo R-squared	0.256	0.099	0.128	0.099	0.408	0.079	0.199	0.075
Log likelihood	576.72	595.82	581.24	595.88	576.13	592.36	566.44	583.11
Number of counties	64	64	64	64	64	64	64	64

Standard errors in parentheses clustered at the county level
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

neighbors. The evidence of positive spillovers is robust to any kind of spatial weight matrix, including those that impose a limited number of geographic interaction (for instance, binary matrix) and those that impose a large number of interactions (100, 150 and 200 miles radius). Despite the robustness of the general result, it is noted that the magnitudes of spillovers vary considerably between each different spatial matrix. The evidence in Table 4 shows that the neighbors of the treated counties were positively impacted for the years the treated counties allowed recreational marijuana retail activity. Overall, the coefficient of the direct and indirect treatment effect are positive and statistically significant. As the results show, as the neighboring distance increases the indirect treatment effect decreases as is suggested by the decrease of the coefficient corresponding to *W*Sale*. In the other hand, the direct treatment effect remains quantitatively around the same value, and as the neighboring distances increase the direct effect slightly increases, suggesting the potential benefit for counties of adopting the policy within their own boundaries.

As discussed earlier, such positive spillovers can be spatially transmitted by a set of channels not mutually exclusive such as inter-regional diversion of recreational marijuana across counties and statewide decriminalization of marijuana. The proximity and the ease of crossing counties' borders allows individuals to move back and forth in order to purchase recreational marijuana, specially those individual who live in counties where dispensaries are not allowed to operate. Moreover, the decriminalization of marijuana possession in small quantities that passed before Colorado Amendment 64 is an important factor contributing to the consumption of the substance that might positively impact individuals who benefit from the associated stimulating effects of these types of non-synthetic drugs. It is important to note that this type of substance-related policies that generate positive growth spillovers have the opposite direction of spatial spillovers generated by other type of substances such as alcohol and synthetic drugs. Therefore, substances and the health implications that they have on individuals drives the direction of spillover that is expected to generate; however, they can act in similar ways but generating opposite results.

Indirect impacts have a higher magnitude compared to direct impacts, indicating the economic relevance of spatial spillovers. Compared to a county whose neighbors prohibited recreational marijuana retail, a county whose neighbors allowed recreational marijuana retail experienced a larger increase in average income per capita over time. Considering the average income per capita of counties that are at the immediate boundary of directly treated counties and the parameter obtained with the spatial specification in column (3) including the neighbors within 100 miles radius, a back-of-the-envelope calculation shows that the recreational marijuana retail activity of a treated county has an average indirect impact of approximately $1,676 USD. Thus, the evidence showed in this section indicates that spillover effects are far from negligible and neglecting them lead to undermined results of the real economic impact of the recreational marijuana retail activity. Furthermore, substance-regulating policies should take into account the potential indirect effects of substance use.

6. Concluding remarks

Substance dependency and the health consequences of substance abuse have generated large controversy and debate spanning many arguments in favor and in opposition to policies that intend to relax the consumption of these substances. Prior research has connected the increased use of marijuana to poorer labor market outcomes for individuals in terms of wages and employment (Van Ours, 2007; Sabia & Nguyen, 2016; Albino, 2017; Nicholas & Maclean, 2019), implying a potential causal path from a liberalized legal status of marijuana to broad negative results in the economy. This study contributes to literature by analyzing the Colorado Amendment 64 passed in 2012 that allowed later in 2014 the implementation of recreational marijuana retail activity. This analysis explicitly estimates both direct and spillover effects from neighboring areas. The results obtained indicate that the recreational marijuana retail activity caused positive and significant direct and indirect effects on average income per capita induced by the counties that implemented it within their boundaries between 2014 and 2017. In this manner, generating results that are opposite to some of previous studies on the effect of marijuana legalization mentioned in section 2.

The estimated direct and indirect impact of the policy provides evidence of the potential benefit of liberalization of recreational marijuana retail by increasing its accessibility. This study shows the extent to which such policies affect geographically linked areas. In particular, there is evidence that the average income per capita gain in neighboring areas is equivalent to approximately between $479 to $1676 USD depending on the neighboring distance under consideration.

However, a number of issues may by taken into account for future research and further improvement. The results are limited to the data available. For instance, the use of individual-level data might generate different results, perhaps indicating to a negative or nonexistent impact of the treatment. Moreover, when information at the individual-level is available in regards to race, age, occupation, residence address and gender, then the heterogeneity of the treatment effect can be examined by categorizing individuals by subgroups and this way obtaining a better understanding of the treatment effect on labor productivity. Furthermore, the limited and incomplete open data for other indicators of labor productivity represents a limitation for assessing the treatment effect. For instance, number of hours worked, number of people with part/full time jobs, or hourly wage information would allow estimating the heterogeneous treatment effect based on measurements that are commonly used a indicators of labor productivity.

Another issue is that of potential sources of variation in the treatment and the existence of self-selection into treatment by counties, having the choice of adopting recreational marijuana retail or not. This is in relation to the endogeneity of the treatment. As discussed earlier, this is linked to the unconfoundedness assumption; in this analysis the violation of the unconfoundedness assumption is plausible but with certain caveats in the causal mechanisms at work. Therefore, the possibility of applying an IV-DID technique as discussed earlier represents an opportunity to further analyze the direct and indirect effect of these types of substance-related policies that in nature remain to be controversial due to the ambiguity surrounding the health consequences of substances.

Moreover, additional covariates such as county-level elections and electoral cycles could be added in order to control for potential economic shocks due to politicians' political campaigns that might have an effect on average income per capita and this way detect if the treatment effect estimated might be capturing the economic boost of political campaigns. Due to data availability these covariates are not added and can be the object of future research; however, county-level (municipal) elections are held every two years with minor changes in the political structure of local governments, hence it is expected political indicators to have little effect on average income per capita in a every two year cycle.

Additionally, with more open source data then IV approach can be applied instrumenting the treatment variable with instruments such as county health and education expenditure, increase in the number of schools, increase in medical facilities, general citizens' sentiment towards marijuana, and other instruments indicating the potential determinants of the decision of counties to adopt or not recreational marijuana retail. Even so with data available for the proxies of these instrumental variables, the challenge of framing the exclusion restriction underlying the IV approach represents an interesting and more detailed future research. The application of IV in a Diff-in-Diff setup is a recent area of research and there is little empirical application of this technique (Hansen et al., 2017). The application of IV in a Diff-in-Diff is beyond the scope of this paper but is suggested as future research and improvement of the results generated in this analysis. In this study, violation of the uncofoundedness assumption is present to some extent; nevertheless, the bias in the estimate of the treatment effect in this analysis could be relatively small due to the reasons explained in section 3.

It is relevant to mention that the effect of the estimated spatial spillover effects of the treatment might be capturing the more dynamic interaction of specific spatial clusters of counties (i.e. urban vs. rural counties). Therefore, it is important to highlight that the estimated effect should be interpreted as localized potential spillover effect of the treatment. However, if this was the case, it is an important sign of the relevance of the policy for regional growth. Although not applied in this study, and potentially appropriate for further research, the Geographically Weighted Regression technique (Brunsdon et al. 2002; LeSage, 2004; Wheeler & Páez, 2010) is an alternative method to estimate the coefficient of interest for every county, in this manner, being able to identify the spatial clusters of counties and analyze whether the estimated treatment effect might be capturing dynamic interactions favoring either urban or more rural counties.

While some studies have suggested that commercialization of marijuana have had a crime-reducing effect on neighboring states next to Colorado, others have suggested no effect on motor vehicle crash-mortality and no effect on increased marijuana possession at the country-level in the U.S. The results obtained in this study contributes to the vast literature analyzing the effect of marijuana policy changes and adds providing evidence that commercialization of recreational marijuana can have positive effects, particularly, in labor productivity at the county-level. Moreover, the positive spatial spillovers of counties that adopted the policy on the labor productivity of non-adopters and other neighboring adopters act in a similar way to the analysis finding crime-reducing effects, showing the existence of spatial relation across areas. Therefore, policies aiming at improving labor productivity and in a more general sense; work productivity, might require to be framed in assessing workers' propensity to consume along with updated health evidence supporting potential benefits of marijuana use across different subgroups of individuals. Nevertheless, due to the complexity of marijuana as a stimulating or impairing substance itself and the legal framework of the substance as to being a drug or a low-risk substance, many other factors play a role in the policy-making process for its use and commercialization.

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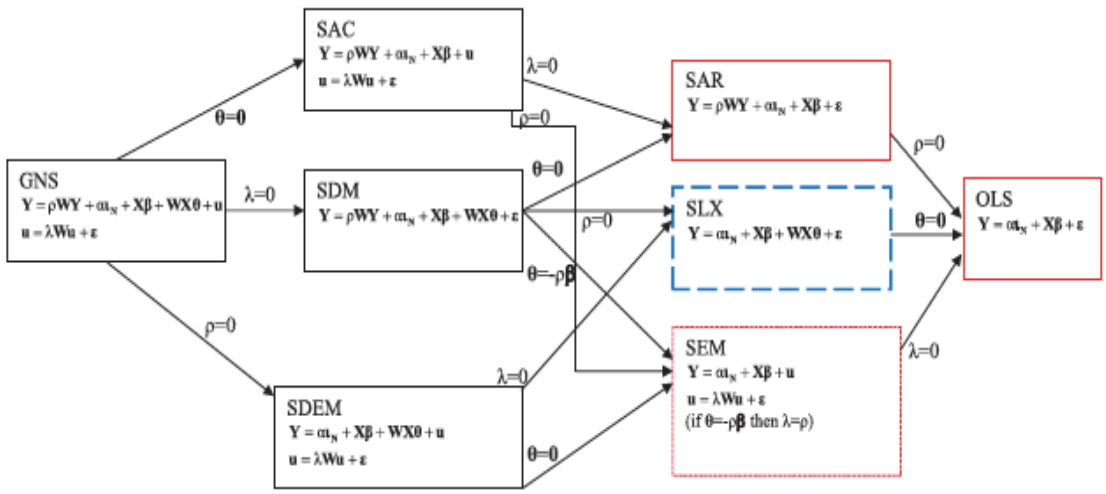
Appendix

Figure A1: Comparison of different spatial econometric model specifications. *Source*: (Vega & Elhorst, 2015).



Note: GNS = general nesting spatial model, SAC = spatial autoregressive combined model, SDM = spatial Durbin model, SDEM = spatial Durbin error model, SAR = spatial autoregressive model, SLX = spatial lag of **X** model, SEM = spatial error model, OLS = ordinary least squares model.

Table A2: List of counties that implemented recreational marijuana retail activity by entry year.

	Year	
2014	2015	2016
Adams	Arapahoe	Conejos
Clear Creek	Archuleta	Huerfano
Denver	Boulder	
Garfield	Chaffee	
Gilpin	Costilla	
Gunnison	Eagle	
Jefferson	El Paso	
Lake	La Plata	
Ouray	Larimer	
Park	Las Animas	
Pueblo	Mesa	
Routt	Montezuma	
San Miguel	Morgan	
Summit	Pitkin	
Weld	Saguache	
	San Juan	
	Sedgwick	

Source: own elaboration with information from the Colorado Department of Revenue.

Table A3. Definition of variables

Average income per capita: Per capita personal income aggregated at the county level for every county for each year between 2011 and 2017, measured in U.S. Dollars.

Total population: Total number of people in each county for every year between 2011 and 2017.

Average age of total population: The average age of the total population in each county for every year between 2011 and 2017 measured in years, and every decimal is equivalent to 24 days.

Labor force participation rate: Percent of total labor force actively working for every county for each year between 2011 and 2017. It excludes those actively looking for a job.

Poverty level: Estimated percent of people of all ages in poverty. Based on the SAIPE database definition: Poverty status is defined by family; either everyone in the family is in poverty or no one in the family is in poverty. The characteristics of the family used to determine the poverty thresholds are: number of people, number of related children under 18, and whether or not the primary householder is over age 65. Family income is then compared to the poverty threshold; if that family's income is below that threshold, the family is in poverty.

Unemployment rate: The unemployed percent of the civilian labor force [i.e., 100 times (unemployed/civilian labor force)].

High school graduation rate: The high school graduation rate for each county for every year between 2011 and 2017 for the total number of students that are expected to graduate in a given year. In the new rate, a student is assigned a graduating class that does not change. These early and late graduates are reflected in 3-year, 5-year, 6-year, and 7-year graduation rates based on their assigned anticipated year of graduation. A major change involves a shift to a four-year "on-time" graduation rate. The shift to the new fourth-year "on –time" graduation rate is being made in order to comply with The No Child Left Behind Act of 2001. Under this act, the state of Colorado must move to an accountability system that measures and reports the "on-time" graduation rate. The formula and methodology is based on the National Governors Association (NGA) "Graduation Counts Compact."

Table A4. Correlation matrix

	Income per capita	Population	Avg. age of total population	Participation rate	Poverty level	Unemployment rate	High school graduation rate
Income	1.0000						
Population	0.2789	1.0000					
Avg. age of total population	-0.0507	-0.0645	1.0000				
Participation rate	-0.0682	-0.2967	-0.0399	1.0000			
Poverty level	-0.2184	-0.2539	-0.0106	-0.0791	1.0000		
Unemployment rate	-0.2408	-0.0150	0.0918	-0.0646	-0.0418	1.0000	
High School graduation rate	-0.0231	-0.0182	-0.1011	-0.0158	0.0528	-0.1137	1.0000

Source: own elaboration with main data set with information gathered from different sources described in section 4.

Figure A5: Maps of the average income per capita for the counties in Colorado from 2011-2017.

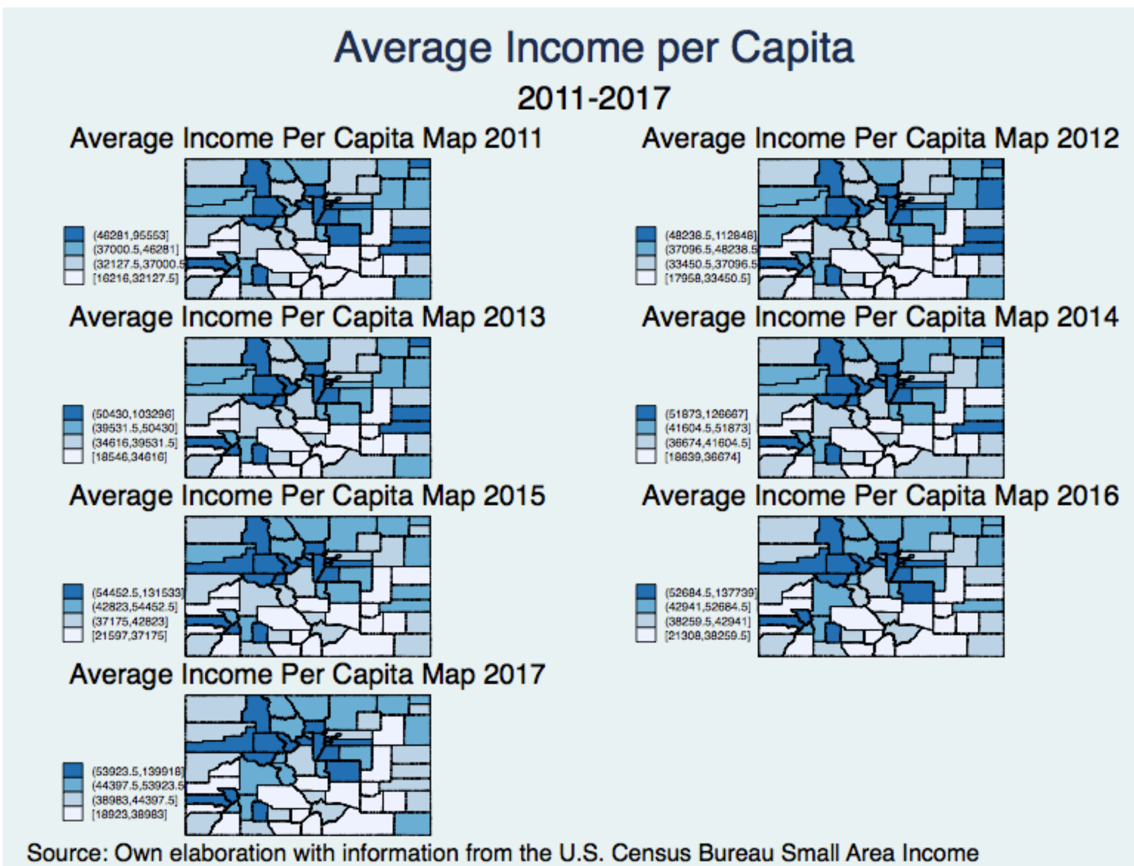
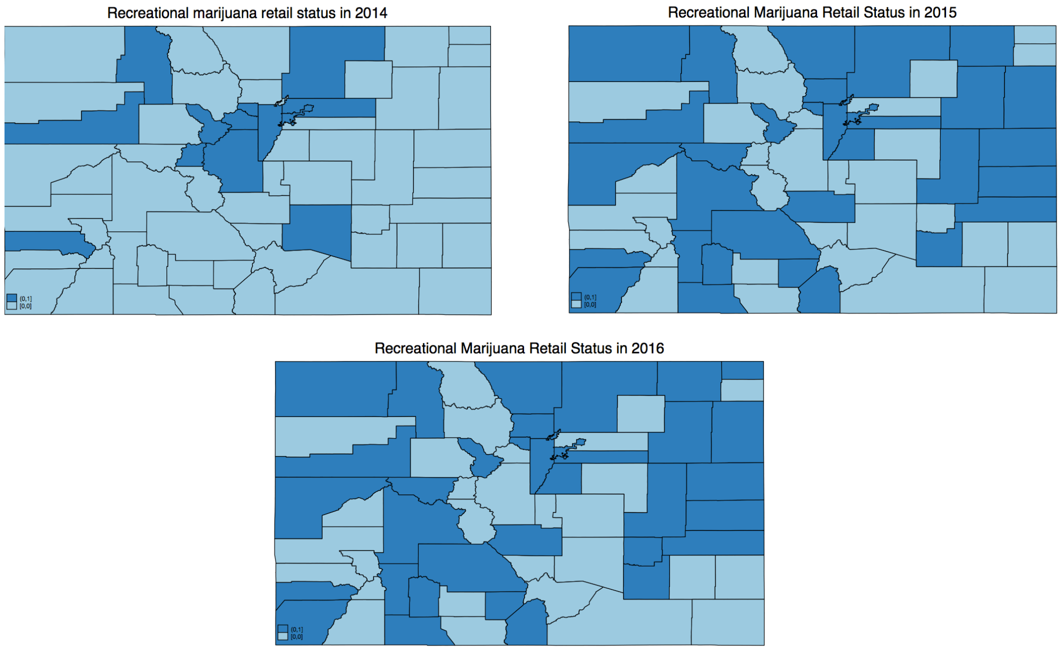


Figure A6: Recreational marijuana retail status in 2014, 2015, and 2016.



Source: Own elaboration based on information from the Colorado Department of Revenue Annual Report 2014.