Summer 2019

Three Essays in the Economics of Crime

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by

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Submitted in Partial Fulfillment of the Requirements

for the Degree of Doctor of Philosophy in

Economics

Darla Moore School of Business

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2019

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Abstract

I examine the effects of three different policies on crime related outcomes. First, I consider whether access to mental health care effects crime rates. In particular, I consider whether the effect on arrest rates of increasing access to mental health care for those that already have private insurance by exploiting the state and time variation in the adoption of mental health parity mandates. I find no evidence to suggest that even the most far-reaching of these mandates are effective at curbing crime. I also follow up on prior studies, applying this approach to determine the effect on suicide rates. I similarly find no change in suicide rates as a result of expansions in mental health care access.

Second, I examine whether same-sex marriage legalization announcements impact the occurrence of LGBT hate-crimes. I exploit variation in the timing of same-sex marriage legalization announcements across states, using a difference-in-differences design. I find that a same-sex marriage legalization announcement leads to a reduction in the LGBT hate-crime rate of 0.111 per 100,000 people from a base of 0.3. This result is mostly driven by reductions in violent hate-crimes. There is also evidence of a reduction in property hate-crimes. Additional analyses indicate that the effect is stronger in counties with a large share of likely perpetrators. The results show suggestive evidence that same-sex marriage bans have the opposite effect on the LGBT hate-crime rate. The results demonstrate that salient LGBT-specific policy announcements are effective at reducing hate-crimes based on sexual orientation.

Third, I study the impact that recreational marijuana legalization has on airline travel. Using origin to destination flight data and marijuana legalization and availabil-

ity dates, I find no evidence of an increase in airline travel as a result of marijuana legalization. The null results are robust to difference-in-differences models and synthetic control models. My initial estimates may be attenuated by business travelers, drivers, or enforcement of marijuana prohibition. I control for these circumstances and still find no effect.

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Chapter 1

Does Access To Mental Health Care Deter Crime?[1]

1.1 Introduction

Mentally ill individuals commit crimes at a higher rate than the population as a whole (Swanson 1990; Van Dorn, Volavka, and Johnson 2012). In particular, both the severely mentally ill (SMI) and those with minor mental health problems are more likely to commit sexual offenses and assaults compared to the general population (Silver, Felson, and Vaneseltine 2008). Additionally, SMI individuals are more likely to commit property crime than the general population. Accordingly, the number of those at risk to commit crimes due to mental illness is high, meaning there are potentially large stakes to finding a solution to this problem. Approximately one in five adults in the United States are burdened by mental health problems every year, according to the National Institute of Mental Health 2017. About one in four of those (or about ten million people) have mental illnesses that are considered to be serious enough to impair regular tasks. Of those, a quarter are diagnosed with schizophrenia; two-thirds are diagnosed with bipolar disorder. Mental health coverage has significant gaps, especially for youth, those living away from urban centers, and for middle-income families (Cohen and Hesselbart 1993). Eighty percent of youth that need mental health services don't get care (Kataoka, Zhang, and Wells 2002). If treatment of these individuals is effective in preventing criminal behavior, then

[1] Robert Pettis. To be submitted to *Law and Economics*.

increasing access to this treatment may be an effective mechanism to reduce crime.

In this paper, I ask whether increased mental health coverage deters crime. To answer this, I exploit the variation in legislation intended to increase access to mental health coverage across states and over time through a difference-in-differences (DiD) model. To this end, I utilize mental health parity mandates. These mandates require a level of parity between physical and mental health coverage.

I find little evidence to support the notion that increasing access of mental health care through parity legislation is an effective deterrent on crime.

These results further contribute to the growing literature of mental health and its association with crime. Edwards 2014a concludes that increasing the minimum stay required for involuntary commitment to mental health facilities can reduce crime. Edwards 2015 wrote that if voluntarily committed patients are allowed to refuse medication, this may increase the number of voluntarily committed, but optional consumption of medication (and the associated side effects) could reduce treatment and therefore increase violence. Yoon 2007 concluded that decreases in the number of publicly funded available psychiatric hospital beds (a measure of the supply of mental health facilities) leads to a large increase in both violent crime and property crime. Additionally, there were results suggesting that these decreases in available beds could increase the probability of jail detention. Edwards 2014b studies the effect of duty-to-warn laws, which describe the inherent responsibility of a health care professional to inform third parties if they have reason to believe that the individual poses a threat to themselves or others, on crime. He notes that if patients are aware of this law (which could breach confidentiality), they may opt to not get treatment, thus increasing the number of untreated mentally ill individuals. Edwards concludes that the laws were associated with an increase in homicides. Access to substance abuse centers has also shown to be important as Bondurant, Lindo, and Swensen 2018 find evidence through the closure and opening of substance abuse facilities that both violent and financial crimes were reduced as a result of the presence of these facilities.

A common theme in these papers is that crime may be reduced as a result of mental health treatment. Because of this, access to treatment could be an important factor in determining if those with mental illness engage in criminal activity. In the last 25 years, states have passed legislation aimed at increasing individuals' mental heathcare coverage. An integral assumption for this study is that the channel through which this legislation affects crime is through increased access to mental health services. It is therefore important that the mandates have legislative bite. I will follow Dave and Mukerjee 2011 who study whether Mental Health Parity Laws were successful in increasing access to mental health services. They determine that mandates reduce the probability of being uninsured by a net 2.4 percentage points, but only when the mandated mental health coverage is comprehensive. I therefore treat only broad levels of parity as a potential vehicle to increase mental health care access. The results in Dave and Mukerjee 2011 are backed by Harris, Carpenter, and Bao 2006 which also used a DiD approach, where the authors stratify adults according to their most stressful month of the year. They conclude that for those with lower and middle distress levels, parity laws increased the probability of accessing health care by 1.2 and 1.8 percent respectively. No effect was found for those in the upper distress group.

The most similar study is Klick and Markowitz 2006. They use a DiD approach seeking to determine if the increased access to mental health services granted by parity laws would be successful in reducing suicides, which are more prevalent for the mentally ill than the general population. As their paper is more reserved in regard to an assertion about the effectiveness of access gains to treatment due to the legislation[2] and its results showed no effect on suicide rates as a result of parity laws, in contrast to the crime outcomes listed above. I support this conclusion by extending my analysis

[2]This may be a result of examining the effect of parity laws as a whole without controlling for the strength of those laws.

to suicide rates and find that even the strongest parity laws have no effect on suicides.

This paper adds value to the above literature in that it exploits differences in legally mandated levels of mental health care parity to examine a direct link between access to voluntary or preventative mental health service and overall crime. This is important because while there is evidence that mandatory access to mental health services can reduce crime and that mental health parity laws can increase this access, Klick and Markowitz 2006 showed that this legislation was ineffective in reducing suicide, a prominent, and in cases related to depression and anxiety, treatable consequence of mental illness. This result is surprising on the surface. Suicide is a problem that is greatly more prevalent among the mentally ill. However, Bertolote 2004 concluded that it is difficult to set up standardized plans for suicide prevention, because what works in some instances may not work in others. This may explain the lack of evidence that the legislation was effective at reducing suicides. Though the outcomes are different (crime/violence against others versus violence against themselves), there is need for clarity on this issue. This study attempts to establish if there is a direct link between increasing voluntary passive mental healthcare access, as opposed to being involuntarily committed, can curb crime or suicide.

The remainder of the paper will be organized as follows: In Section 1.2, I describe mental health parity legislation in detail. In Section 1.3, I introduce the data. In Section 1.4, I detail the empirical framework. In Section 1.5, I analyze the results and I test the assumptions of the model. I also provide further explorations, including how states that already had limited legislation in place may have affected the results, as well as the effect of these mandates on suicides. I conclude in Section 1.6.

1.2 Background

Mental Health Parity Mandates are laws that require private insurance to treat mental health in parallel to how they treat physical ailments. This is applied in varying degrees over time and is particularly effective where there are obvious parallels between mental illness and physical wellness, such as a given copay on each type of routine checkup being mandated to be similar. The classification on mandate strength is based on the following: Broad mandates include all or nearly all mental illnesses in their mandates, including substance abuse. Limited mandates have some sort of weakness that prevents broad application, doesn't cover substance abuse, or limits the application in some other way. Low/No Mandates indicate that either a state has no mental health parity legislation at all or the legislation in place does not effectively mandate parity for mental health for insured citizens. My definition is very similar to that in Dave and Mukerjee 2011, with minor exceptions. Figure 1.1 counts the number of states that have implemented laws of different strengths over time. Notice that there were no broad mandates prior to 1994. Eleven states reached this level and once increasing the strength of the mandates, no state decreased the strength afterward. As Dave and Mukerjee 2011 conclude that only broad mandates were successful in increasing access to mental health care, I consider an area treated only under broad mandates. A potential drawback in using mental health parity legislation is that it targets those that already have private insurance. Those that already have insurance may be relatively less likely to commit crimes, which could make it less likely to observe an effect statistically different from zero.

1.3 Data

I use monthly data at the county level from 1990 to 2007. I categorize Mental health mandates by reviewing state laws using the National Council of State Legislatures 2018.

Additionally, the categorizations are guided by the previous use of these data by Dave and Mukerjee 2011. Over time, 11 states gain broad parity mandate characterization.[3] Of these, six of them already had some form of limited mandates.[4]

Arrests data come from the Uniform Crime Report(UCR), published by the Federal Bureau of Investigation 2018. The UCR reports using a hierarchical system. This means that if multiple crimes are committed at the same time, only the most severe crime is reported. As a result, the most serious crimes are likely to be the most accurately reported. I include arrests for both violent crime, such as rape and murder, and property crime, such as burglary and vandalism. Figure 1.2 graphs the arrests by year over treatment through broad mental health parity mandates. Treated (ever-treated) states and non-treated (never-treated) states have similar arrest rates to start, but the treated states eventually

I use several standard controls for crime analyses: as a proxy for burdens placed upon minorities through discrimination, I use percent black. Total and black population counts were attained from the U.S. Bureau of the Census 2019 for 1990, 2000, and 2010. I geometrically interpolated for intercensal years, and then calculated the estimate of the percentage of the population that are black. Additionally, I use unemployment rate, and employment (count) which come from the Bureau of Labor Statistics 2019.

Table 1.1 presents the summary statistics stratified by treatment status. An observation is considered treated if it ever passes a mental health parity mandate before 2007. Treated states have slightly more arrests per 100,000, have 0.79% percentage points less black population, have 0.08% percentage points smaller unemployment rate, and have over 17,000 more employed workers than treated states.

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[3] These states are: Arkansas, Connecticut, Indiana, Maine, Maryland, Minnesota, Oregon, Rhode Island, Vermont, Virginia, West Virgina. My categorization differs from Dave and Mukerjee 2011 in that I do not consider North Carolina to be treated due to weaknesses in its legislation.

[4] These states are: Arkansas, Indiana, Maine, Minnesota, Oregon, West Virginia.

1.4 Empirical Methodology

Dave and Mukerjee 2011 find that broad parity mandates was effective at increasing access to mental health care. If this access translates into effective treatment, it should reduce deviant behavior. To investigate, I estimate the effect of broad mandates on crime. In particular, I estimate:

$$y_{cst} = \psi_t + \chi_c + \boldsymbol{\beta_1} \mathbb{1}(Treated_{cst} = 1 \times Post_{cst} = 1) + \boldsymbol{\beta_4} X_{cst} + \epsilon_{cst} \qquad (1.1)$$

where, y_{cst} is the number of arrests for violent and property crimes, $\mathbb{1}(Treated_{st} = 1 \times Post_{st} = 1)$ indicates if county c in state s is treated in time t. The parameters ψ_t and χ_c represent month-year and county fixed effects, respectively; and X_{st} represents a set of controls that vary across counties and time. These include percentage black, unemployment rate, and employment. Standard errors are clustered at the state level. This procedure assumes that trends in crime-rates in both treated and non-treated counties would be the same, absent treatment.

1.5 Results

1.5.1 Difference-in-Differences Results

Table 3.5 presents the results from estimating the baseline model with an expanding set of controls. Starting with no controls, the following are added in this order across columns: unemployment rate, employment (count), and percent black. Once unemployment rate is controlled for, results vary little and are consistently not statistically significant. Coefficients range from -4.442 to -4.981, compared to the average total arrests rate of 117.52, a decrease of 3.8%.[5]

[5]Additionally, I stratified the regressions by population, but did not find any statistically significant outcomes. Tables 1.4 and 1.5 present the results of these regressions, while Tables 1.7 and 1.6 present the differential effects of changing from a more limited mandate to a broad one, as before, on the stratified regressions.

The effects may be muted by those already having insurance being less likely to commit crime. Additionally, in Table 1.3, I decompose the DiD into a series of 2×2 models a simple estimate of the effect of parity mandates on crime in the manner of Goodman-Bacon 2018, and find that the effect of treatment on treated states when compared to states that are not treated, produce an estimate of a reduction 5.1 crimes per 100,000. This effect is dampened because 7.7% of the DiD estimate that comes from timing has the opposite sign. Most of this is driven by the comparison between states that are treated relatively early and are compared against states that are treated later, prior to treatment. Without the biased-timing terms, the result is a decrease of 5.1 crimes, a decrease of 4.3%. Figure 1.4 graphs the 2x2 estimates against their weights. Note that a large portion of the estimation is coming from only a few estimates. These are terms where treatment occurred in the middle of the panel and/or have more variation.

1.5.2 Event Study

In order to support parallel trends assumption required for using the DiD technique and to test whether or not the effect of the legislation on crime changes relative to time to and after its implementation, I perform an event study in the manner of Jacobson, LaLonde, and Sullivan 1993 and Kline 2011. To reduce noise in this analysis, periods have been grouped into twelve month (one year) bins. Specifically, I estimate the following model:

$$y_{cst} = \psi_t + \chi_c + \sum_{i} \delta_t D_{cst} + \boldsymbol{\beta} X_{cst} + \epsilon_{cst} \tag{1.2}$$

where D_{cst} is a vector of dummies that equal one if and only if parity levels are changing to broad mandates in county c and state s exactly t years away. This replaces the indicator, $\mathbb{1}(Treated_{cst} = 1 \times Post_{cst} = 1)$, and instead allows the effect of broad mandates to vary over time. I omit the dummy for the year prior to the event (that is, $t = -1$) making all coefficients relative to that year.

The results of the event study are displayed in Figure 1.3. Years relative to the implementation of broad mandates are on the x-axis and the effect of the mandate in that year is on the y-axis. Shaded areas represent 90% confidence intervals while 95% confidence intervals are denoted by dotted lines. To test that the parallel trends assumption holds in the pre-treatment period, I should not observe coefficients before the event being significantly different than zero. A test of the joint significance of all δ_t where $t < 0$ produced a p-value of 0.1950, meaning that one cannot reject the null hypothesis of common trends at normal levels of statistical significance. Additionally, there is not an individually significant estimate at any period post-treatment.

1.5.3 Differential Effects

These results may have been weakened because some of these states already had some form of mental health parity. By including the differential effect of a state changing to a broad mandate from a limited one, Table 1.8 shows the results of including the differential effect of a state changing from a limited mandate to a broad one. The coefficient itself is not statistically significant at 7.16 (a positive sign), the overall effect of broad mandates in states that changed from limited to broad parity levels is not significant at -2.54. When controlling for the prior mental health parity level, the effect of mental health parity laws going from low/no mandates to broad mandates on crime to be -9.7, which is significant at the 10% level.

1.5.4 Suicides

A ubiquitous violent behavior induced by mental illness is suicide. In Figure 1.6, I graph the rank in terms of number of deaths associated with suicide by age group. There are three age groups where suicide is the number two killer. Even being aged 65 or older, which is omitted because it is not a top ten killer, is considered a risk factor for suicide. It simply doesn't show up because elderly individuals have many illness related causes of death. If increased access to mental health care through parity laws was not effective at reducing crime, perhaps it still affects suicide rates. Recall that Klick and Markowitz 2006 did not find evidence that suicides decreased as a result of mental health parity laws. As I did when modeling arrests, I consider a county treated only if it is within a state that has passed a broad parity mandate. Suicide data were gathered from the Center for Disease Control and Prevention 2019. They include any death that occurred with known intention to self-harm. This includes all methods that induced the suicide. Figure 1.7 shows the rate over time.

I estimate the models in Equations 1.1 and 1.2 with suicide as the dependent variable. I report the results of the DiD and Event Study designs in Table 1.9 and Figure 1.8, respectively. The coefficient of interest reported is 0.53, which is both practically and statistically insignificant. The event study reports small statistical, but little practical, significance prior to treatment and no significance post treatment.[6]

1.6 Conclusion

In this paper, I study whether access to mental health care affects crime. In particular, I focus on those that already have private insurance and how their plans would increase their access to mental health care as a result of broad parity mandates. I find no evidence that broad mandates are effective at reducing crime. Further, I extended the analysis to include suicides. I found no evidence of a decrease in suicides as a result of broad mental health parity legislation. This supports the work of Klick and Markowitz 2006 which studies the effect of all mental health parity laws on suicide and found no effect.

These results may be explained by those with private insurance, the target of parity legislation, not being the primary perpetrators of crimes. This is additionally compounded with the difficulty in predicting future violence in SMI individuals and the

[6]With a p-value of 0.0294, the hypothesis that there are no pre-trends is rejected.

ineffectiveness of standardized suicide prevention plans. Accordingly, future research should focus on how access to mental health care for more likely perpetrators could change their behavior.

Table 1.1: Summary Statistics

	Full		Control Counties		Treated Counties		Difference
	mean	sd	mean	sd	mean	sd	p
Total Arrest Rate	87.12	67.02	81.22	62.68	88.66	68.02	(0.00)
black_pct	7.39	12.58	7.55	12.57	7.35	12.58	(0.00)
unemp_rt	5.78	2.97	5.84	2.97	5.77	2.97	(0.00)
empl	50482.88	150405.81	37475.99	74942.14	53869.30	164298.25	(0.00)
total_pop	105558.41	318714.50	75518.64	144374.02	113379.43	349718.10	(0.00)
Observations	462984		95640		367344		462984

For summary statistics, data is at the county level.

Table 1.2: The Effect of Broad Mental Health Parity Mandate on Crime

	(1) Total Arrest Rate	(2) Total Arrest Rate	(3) Total Arrest Rate	(4) Total Arrest Rate
Broad Parity Mandate	-4.442	-4.608	-4.981	-4.970
	(4.503)	(4.575)	(4.551)	(4.442)
Unemployment		X	X	X
Employment			X	X
Percent Black				X
R-Squared	0.035	0.035	0.036	0.036
Observations	462,984	462,984	462,984	462,984

Standard errors in parentheses

Standard errors are robust and clustered at the state level.

OLS estimates.

* p<0.10, ** p<0.05, *** p<0.01

Table 1.3: Decomposition of Difference in Differences Estimates by Treatment Timing

DD Comparison	Weight	Avg DD Estimate
Earlier T vs. Later C	0.046	7.106
Later T vs. Earlier C	0.031	0.078
T vs. Never treated	0.923	-5.103
T vs. Already treated	0.000	-1.609
T = Treatment; C = Comparison		

This represents the decomposition of a simple OLS DiD.

Table 1.4: The Effect of Broad Mental Health Parity Mandate on Crime, County Populations >= 10,000

	(1)	(2)	(3)	(4)
	Total Arrest Rate	Total Arrest Rate	Total Arrest Rate	Total Arrest Rate
Broad Parity Mandate	-4.374	-4.513	-4.984	-5.013
	(4.770)	(4.845)	(4.823)	(4.758)
Unemployment		X	X	X
Employment			X	X
Percent Black				X
R-Squared	0.041	0.041	0.042	0.043
Observations	391,980	391,980	391,980	391,980

Standard errors in parentheses

Standard errors are robust and clustered at the state level.

OLS estimates.

* p<0.10, ** p<0.05, *** p<0.01

Table 1.5: The Effect of Broad Mental Health Parity Mandate on Crime, County Populations >= 50,000

	(1) Total Arrest Rate	(2) Total Arrest Rate	(3) Total Arrest Rate	(4) Total Arrest Rate
Broad Parity Mandate	-0.058	-0.071	-0.653	-1.001
	(6.110)	(6.145)	(6.072)	(6.034)
Unemployment		X	X	X
Employment			X	X
Percent Black				X
R-Squared	0.067	0.067	0.070	0.070
Observations	162,216	162,216	162,216	162,216

Standard errors in parentheses

Standard errors are robust and clustered at the state level.

OLS estimates.

* $p<0.10$, ** $p<0.05$, *** $p<0.01$

Table 1.6: Differential Effect of a State Changing From Limited Parity to Broad Parity

	(1) Total Arrest Rate
Broad Parity Mandate	-8.962
	(5.511)
Broad Mandate=1×Limited to Broad Mandate Change=1	6.017
	(6.411)
Effect in States that Changed From Limited to Broad Mandates	-2.945
P-Value	0.585
R-Squared	0.043
Observations	391,980

Standard errors in parentheses

Standard errors are robust and clustered at the state level.

OLS estimates.

* $p<0.10$, ** $p<0.05$, *** $p<0.01$

Table 1.7: Differential Effect of a State Changing From Limited Parity to Broad Parity

	(1) Total Arrest Rate
Broad Parity Mandate	-6.785
	(4.915)
Broad Mandate=1×Limited to Broad Mandate Change=1	9.785
	(8.932)
Effect in States that Changed From Limited to Broad Mandates	3.000
P-Value	0.721
R-Squared	0.071
Observations	162,216

Standard errors in parentheses

Standard errors are robust and clustered at the state level.

OLS estimates.

* p<0.10, ** p<0.05, *** p<0.01

Table 1.8: Differential Effect of a State Changing From Limited Parity to Broad Parity

	(1) Total Arrest Rate
Broad Parity Mandate	-9.699*
	(5.091)
Broad Mandate=1×Limited to Broad Mandate Change=1	7.164
	(5.873)
Effect in States that Changed From Limited to Broad Mandates	-2.535
P-Value	0.601
R-Squared	0.036
Observations	462,984

Standard errors in parentheses

Standard errors are robust and clustered at the state level.

OLS estimates.

* p<0.10, ** p<0.05, *** p<0.01

Table 1.9: The Effect of Broad Mental Health Parity Mandate on Crime

	(1) Suicide
Broad Parity Mandate	0.529
	(1.438)
R-Squared	0.083
Observations	7,375

Standard errors in parentheses

Standard errors are robust and clustered at the state level.

OLS estimates.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

FIGURES

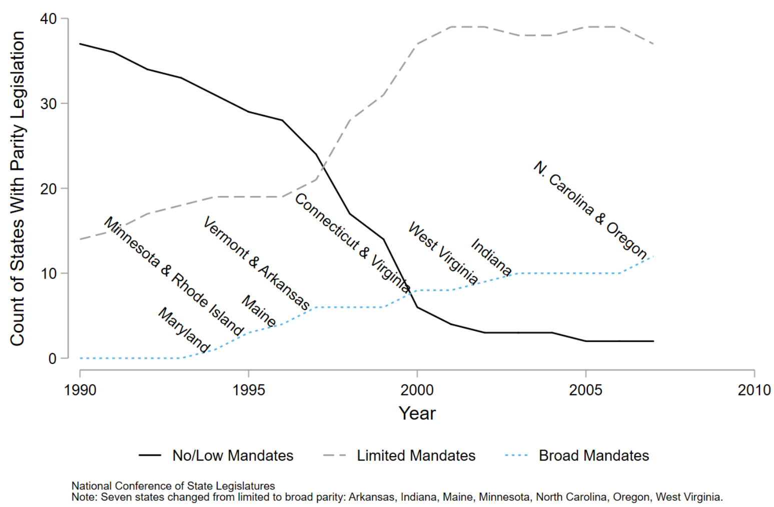

Figure 1.1: Mental Health Parity Legislation Over Time

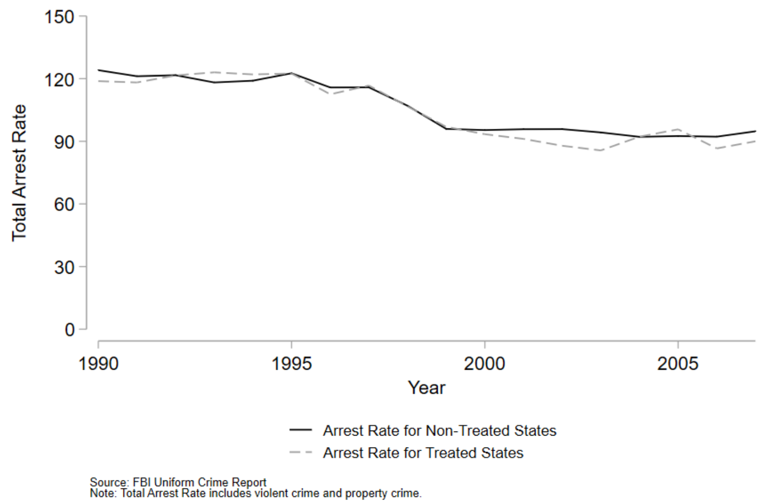

Figure 1.2: Arrests by Year

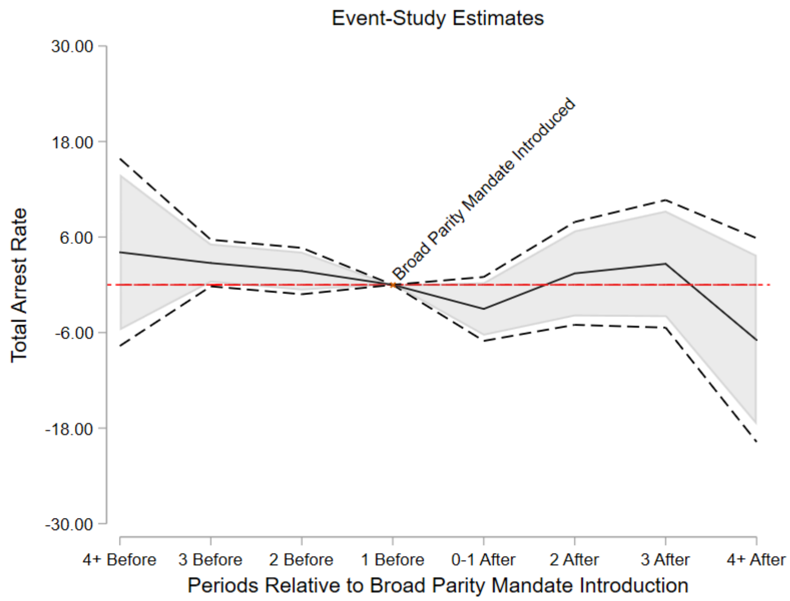

Figure 1.3: Event Study Estimates of the Effect of Mental Health Parity Laws on Crime

Note: This figure depicts the effect of broad mental health parity on overall crime rate when the effect is allowed to vary by time. The periods are grouped into one year bins relative to treatment. The year prior to treatment is omitted, thus the other periods are relative to that year. The solid line reports the estimate of the effect of being treated in that time relative to treatment. The gray highlighted area represents the 90% confidence interval for the estimation and the dotted lines represent the 95% confidence interval. A test of the joint significance of all δ_t where $t < 0$ produced a p-value of 0.1950.

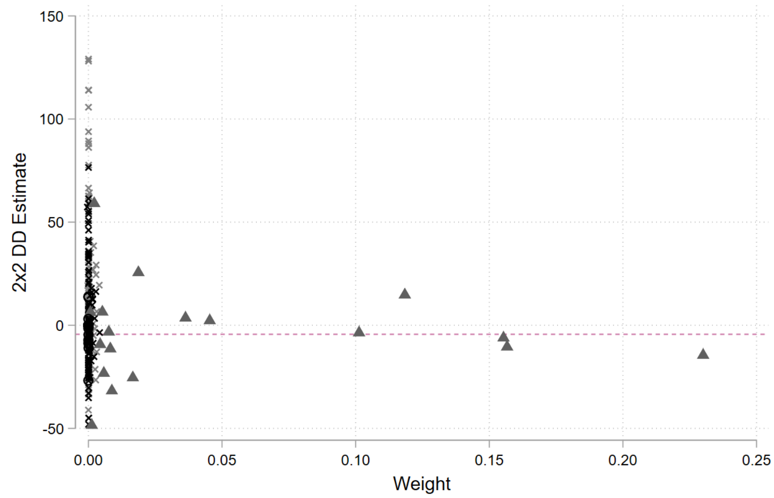

Figure 1.4: 2X2 Decomposition

Note: This figure plots the results from the decomposition of the DiD model, as in Goodman-Bacon(2018). Open circles represent 2x2 terms for when a timing group is compared to pre-1990 treated groups. These do not exist in practice for this study, and the weight for such terms equals zero. Closed triangles represent treated vs. non-treated states. The x's represent the timing terms: a light x is early treated vs later treated, prior to treatment. A dark x is later treated vs. earlier treated, post treatment. Weighting these averages against their weights (the x-axis) yields a DiD result of -4.38.

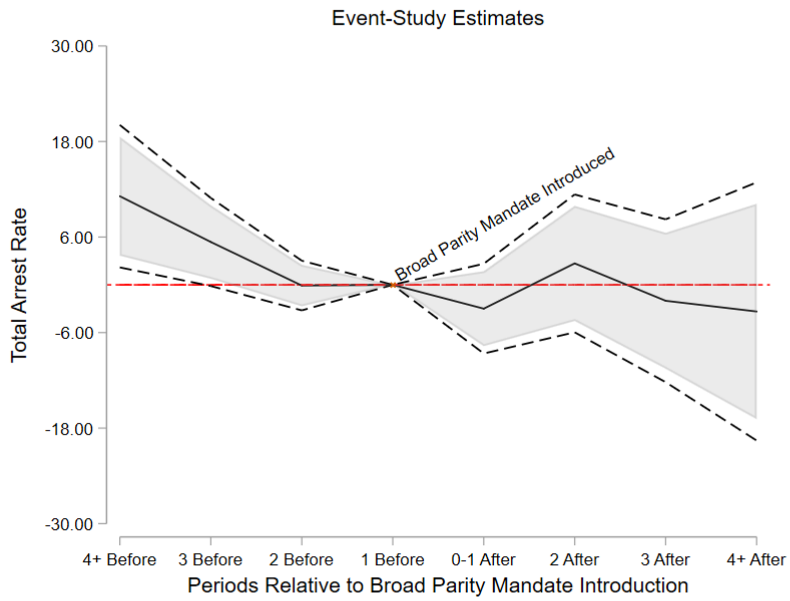

Figure 1.5: Event Study Estimates of the Effect of Mental Health Parity Laws on Arrest Rates - States That Changed From Low/No Parity Mandates to Broad

Note: This figure depicts the effect of broad mental health parity on suicide rate when the effect is allowed to vary by time for states that changed from low/no parity mandates to broad mandates. The periods are grouped into one year bins relative to treatment. The year prior to treatment is omitted, thus the other periods are relative to that year. The solid line reports the estimate of the effect of being treated in that time relative to treatment. The gray highlighted area represents the 90% confidence interval for the estimation and the dotted lines represent the 95% confidence interval. With a p-value of 0.014, the hypothesis that there are no pre-trends is rejected.

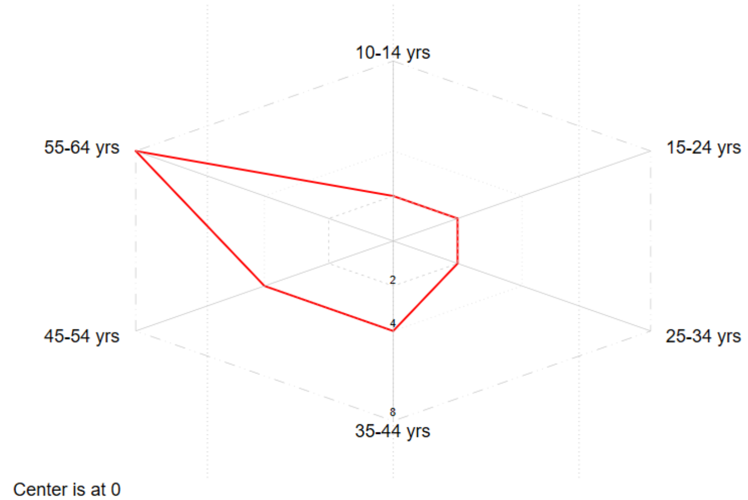

Figure 1.6: Suicide Ranking by Age Group

Note: This figure illustrates the leading cause of death rank for suicide by age groups, thus, the smaller the number, the higher the ranking. Omitted age groups did not include suicide among their top ten causes of death.

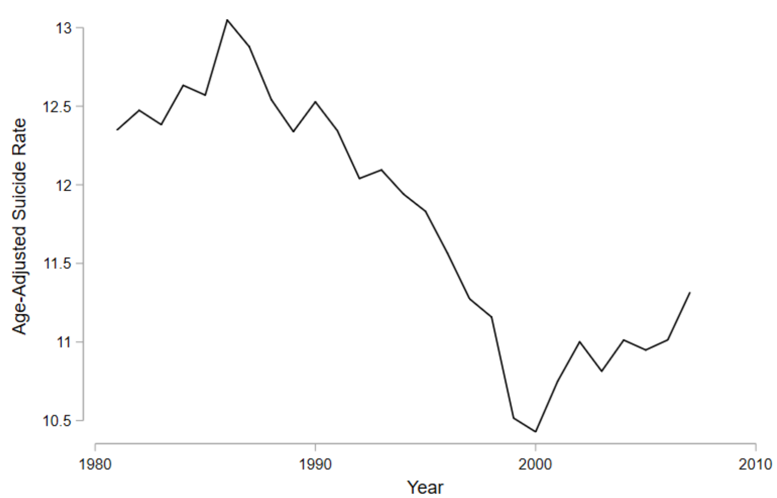

Figure 1.7: Suicide Rate Over Time

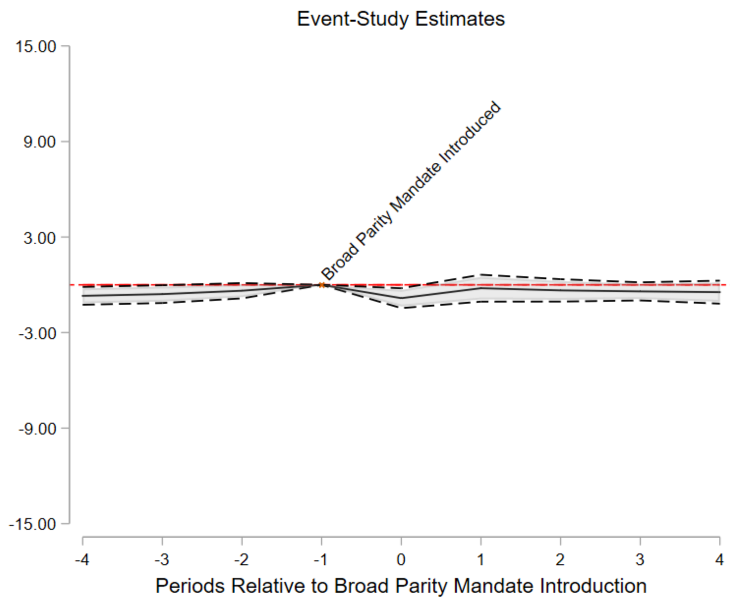

Figure 1.8: Event Study Estimates of the Effect of Mental Health Parity Laws on Suicide Rate

Note: This figure depicts the effect of broad mental health parity on suicide rate when the effect is allowed to vary by time. The periods are grouped into one year bins relative to treatment. The year prior to treatment is omitted, thus the other periods are relative to that year. The solid line reports the estimate of the effect of being treated in that time relative to treatment. The gray highlighted area represents the 90% confidence interval for the estimation and the dotted lines represent the 95% confidence interval. With a p-value of 0.0294, the hypothesis that there are no pre-trends is rejected.

Chapter 2

Pride and Prejudice: Same-Sex Marriage Legalization Announcements and LGBT Hate-Crimes [1]

2.1 Introduction

On the night of October 6, 1998, Matthew Shepard, a 21-year-old gay man, was beaten, tortured, tied to a fence and left for dead by two men he had met at a bar. Six days later, Mr. Shepard died from his injuries. His murder brought national attention to hate-motivated acts against lesbian, gay, bisexual, and transgender (LGBT) people. Despite progress on civil rights for LGBT people since Mr. Shepard's death, the level of LGBT hate-crimes has remained steady over time, according to annual FBI reports (Figure 2.1). The LGBT community comprises 4.5% of the overall population; however, they are the target of 17% of all hate-crimes.[2] In fact, LGBT people are more likely to be the target of a hate-crime than any other individual minority group (The New York Times 2016). Such incidents can reinforce a culture of homophobia, resulting in society incurring an economic cost (Badgett 2014; Badgett, Park, and Flores 2018).

In response to public concerns surrounding LGBT hate-crimes, the federal government and several states have added LGBT people as a protected group under existing

[1] Robert Pettis with Zehra Valencia and Breyon Williams. *Submitted to Journal of Law and Economics*, 3/26/19.

[2] Gallup Daily tracking survey and the Gallup-Sharecare Well-Being Index survey, 2017.

hate-crime laws, although such laws have not proven to be effective in reducing LGBT hate-crimes.[3] Given continued concerns surrounding LGBT hate-crimes and calls to consider prevention strategies aside from hate-crime laws (Meyer 2014), we ask whether same-sex marriage legalization announcements reduce the LGBT hate-crime rate. To the best of our knowledge, this study is the first to provide credible estimates of the casual effects of same-sex marriage laws on hate-crimes based on sexual orientation.

We consider same-sex marriage laws because existing research suggests same-sex marriage legalization leads to greater tolerance of LGBT people.[4] Further, the issue of same-sex marriage is one of the most salient social issues in recent U.S. politics. If such a notable issue has little impact on the LGBT hate-crimes, it is unlikely that other LGBT-specific policies impact such crimes (Flores and Barclay 2016). We consider announcements of same-sex marriage legalization because of the clear relationship between the timing of high-exposure events relating to same-sex marriage and information seeking on the issue, suggesting that the level of attentiveness is greater around announcement dates than enactment dates (Flores and Barclay 2016). Considering these links, it is plausible that greater tolerance of sexual minorities is manifested by reductions in the LGBT hate-crimes following a same-sex marriage legalization announcement.

Studying same-sex marriage legalization is timely, because these policy changes can serve as a model for the countries where same-sex unions are not legally recognized. For instance, even in Europe, which is the most hospitable location for LGBT individuals (Mccarthy 2015), there are countries that still forbid same-sex marriages (Gillet 2018).[5]

To estimate the effect of a same-sex marriage legalization announcement on the LGBT hate-crime rate, we exploit variation in the timing of same-sex marriage

[3]Franklin 2002; Meyer 2014; Spade 2015; Valcore and Dodge 2016; CNN 2018.

[4]Takács and Szalma 2011; Hooghe and Meeusen 2013; Kreitzer, Hamilton, and Tolbert 2014; Flores and Barclay 2016; Takács, Szalma, and Bartus 2016; Kenny and Patel 2017; Aksoy et al. 2018.

[5]Poland, Bulgaria, Latvia, Lithuania.

legalization announcements across U.S. states, using a difference-in-differences design and quarterly data from U.S. counties between 2000 and 2015. Our analysis indicates that same-sex marriage legalization announcements have a substantial effect on hate-crimes motivated by sexual orientation. We find that a legalization announcement leads to a reduction in the LGBT hate-crime rate of 0.111 per 100,000 people. Interpreting the estimated marginal effect in percent changes, same-sex marriage legalization announcements leads to a 30 percent reduction from the base LGBT hate-crime rate, which averaged 0.37 per 100,000 people. This result is largely driven by reductions in violent LGBT hate-crimes, although there is also evidence of a reduction in LGBT property hate-crimes. Together, our findings contribute to the existing literature on the impact of public policies on hate-crimes based on sexual orientation, demonstrating that salient LGBT-specific public policy announcements are, by themselves, effective at benefiting the LGBT community.[6]

We next examine whether observed reductions in the LGBT hate-crime rate are most pronounced in counties with high shares of likely perpetrators of LGBT hate-crimes. We argue that following a same-sex marriage legalization announcement, the effect is stronger in counties with a large share of likely perpetrators. Since we are unable to identify differences across counties in actual shares of perpetrators, we examine the mechanism using likely perpetrators. Perpetrators of LGBT hate-crimes tend to be young white males and ideologically conservative franklin2000antigay,herek2002victim. We find that highly conservative counties with a large share of young white males see bigger reductions in the LGBT hate-crime rate following a same-sex marriage legalization announcement. The result provides suggestive evidence supporting our claim that same-sex marriage legalization announcements impact the behavior of perpetrators of LGBT hate-crimes.

[6]Flores and Barclay 2016, Takács, Szalma, and Bartus 2016, Kreitzer, Hamilton, and Tolbert 2014, Aksoy et al. 2018, Hooghe and Meeusen 2013.

To support our claim that the observed reductions in LGBT hate-crimes are a result of the same-sex marriage legalization announcements rather than unrelated factor(s), we conduct the following analyses: First, we show that the main results hold against several robustness checks that are meant to alleviate concerns of non-randomness in the assignment of treatment. Second, event-study estimates provide evidence that the main results are not driven by differences in trends between the treated and non-treated counties in the pre-treatment period, and that the effect of the legalization announcements on LGBT hate-crimes are long lasting. Third, we show that effects from same-sex marriage legalization announcements are unique to the LGBT hate-crimes: same-sex marriage legalization announcements do not impact other types of crimes. Fourth, we show that same-sex marriage bans had the opposite effect on LGBT hate-crimes; a result we would expect if, indeed, our proposed causal mechanism is true. Our findings suggest the importance of legislation that prevents stigmatizing LGBT people, as any such legal discrimination can cultivate increased violence against them.

The remainder of the paper is organized as follows: Section 2 provides background of same-sex marriage laws and attitudes. Section 3 discusses the empirical strategy and our data. Section 4 reviews the main results along with robustness checks, the event-study estimates of the effect of a same-sex marriage legalization announcement on the LGBT hate-crime rate, and falsification tests. Section 5 provides further examinations: analyses of the locations where likely perpetrators reside and the impact of same-sex marriage bans on LGBT hate-crimes. Section 6 concludes with a summary of our findings.

2.2 Background

2.2.1 Same-Sex Marriage in the United States

Americans' attitudes toward homosexuality have become increasingly liberal since 1990 (Loftus 2001). According to General Social Survey data, in 1990, 73 percent of Americans believed that sexual relations between two adults of the same-sex is "always wrong". By 2012, the percentage expressing this belief dropped to 43.4. Similarly, in 1988, 2.6 percent of Americans "strongly agree" that homosexuals should have the right to marry; this percentage increased to 24.9 percent in 2012. Despite these trends, The Defense of Marriage Act (DOMA), which defined marriage for federal purposes as the union of one man and one woman, passed in May of 1996 and allowed states to refuse to recognize same-sex marriages granted under the laws of other states (Cahill and Cahill 2004). Many states had legislation banning same-sex marriages even before the federal law. Maryland became the first state to pass a statute banning marriage between same-sex couples in 1973. From 1998 to 2008, ballots in 30 states had initiatives to ban same-sex marriage (McVeigh and Maria-Elena 2009).

More recently, state and federal appellate courts have repealed state bans on same-sex marriage. On June 26, 2013, in *United States v. Windsor*, the Supreme Court struck down a major portion of DOMA, ruling that the U.S. federal interpretation of "marriage" and "spouse" to apply only to opposite-sex unions was unconstitutional and that married same-sex couples are entitled to federal benefits. On June 26, 2015, with *Obergefell v. Hodges*, the Supreme Court of the United States ruled that same-sex couples have the right to marry in the United States. Before the Supreme Court decisions, states set different paths towards marriage equality. Some states voluntarily implemented these laws by legislation or voter initiatives while others were forced to repeal bans on same-sex marriage by state and federal courts. In 2004, Massachusetts became the first state to legally recognize same-sex marriage. Prior to *Obergefell v.*

Hodges, 34 states and the District of Columbia had legalized same-sex marriage (see Figure 2.2 and Table A.1).

2.2.2 Attitudes and Hate-Crimes Based on Sexual Orientation

Hate-crimes are the criminal acts that are perpetrated against an individual because of his or her perceived membership in or connection with a particular group (Herek 1989; Craig and Waldo 1996). They are especially serious because the motive behind the crime is to terrorize a group of people (Herek 1989). Therefore, the incidence of hate-crimes are understood as a serious social problem (Herek 1989; Jenness and Broad 1997).

LGBT individuals, in particular have been targets of violence and discrimination in society. Approximately 50 percent of LGBT adults in the U.S. experience bias-motivated aggression at some point (Herek 2009). There were 7,121 hate-crime victims in 2015 and 17.7 percent were targeted because of a sexual orientation bias (Federal Bureau of Investigation 2018). Sexual prejudice, or negative attitudes toward homosexual behavior or LGBT individuals (Herek 2000; McDevitt, Levin, and Bennett 2002) is believed to be a major determinant of antigay violence (Parrott 2008; Rayburn and Davison 2002).

2.2.3 Pro-Equality Laws and Attitudes

The aim of legal regulation is to change behaviors. Legal regulation can achieve its goals directly, by building fear of punishment, or indirectly, by changing attitudes about certain behaviors. Changing attitudes can be particularly effective, especially if moral principles have been influenced (Bilz and Nadler 2014). If this is the case, we can expect same-sex legislation to impact attitudes about LGBT individuals which can impact crime against them.

Researchers and policy makers have long been interested in the effects of pro-equality policies and programs on societal attitudes. Flores and Barclay 2016 discuss four models of attitude change with policy development: backlash, legitimacy, polarization, and consensus. According to the backlash model, disapproval against LGBT people would increase after same-sex marriage legalization. Since attitudes can affect hate-crime rates, the overall hate-crime rate against LGBT individuals should increase following same-sex marriage legalization. For instance, a related paper finds that hate-crime laws that include sexual orientation reduce hate-crime incidences in the U.S (Levy and Levy 2017). A legitimacy model predicts that legal rulings on same-sex marriage may increase acceptance and approval of LGBT people. Based on this model, the hate-crime rate against LGBT individuals would decrease after same-sex marriage legalization. In a polarization model, same-sex marriage policies should effect different groups of people in different ways: people who approve of LGBT individuals should increase their approval after a same-sex marriage legalization, and those who are opposed to LGBT individuals would intensify their opposition. If this hypothesis holds, we would expect, after a same-sex marriage legalization law, the perpetrators to commit even more hate-crimes. Finally, according to a consensus model, attitudes form policy but that policy has no impacts on attitudes. Under this model, we would not expect any impact of same-sex marriages on hate-crime rates.

Literature on how same-sex relationship policies may be associated with public opinion and attitudes is limited. Takács, Szalma, and Bartus 2016 examine same-sex adoption policies in European countries using the 2008-2010 European Values Survey (EVS). They show that the introduction of legal rights for same-sex adoption contributes to increasing levels of acceptance towards homosexual couples' adoption rights. Kreitzer, Hamilton, and Tolbert 2014 use individual-level panel data conducted before and after Iowa's state Supreme Court legalized same-sex marriage. They show that the support for same-sex marriage increased after legalization, especially from democrats, non-religious, non-evangelical, educated, and younger individuals.

Flores and Barclay 2016 use individual-level panel data from the American National Election Study for 2012 - 2013. They show, in line with the consensus model, most of the people surveyed did not change their positions on the question of whether they favor legal marriage rights for same-sex couples, favor civil unions, or do not think there should be any legal recognition. However, in line with the legitimacy model, they show people in states that introduced same-sex marriage experienced the highest reduction in anti-gay attitudes. Hooghe and Meeusen 2013 and Aksoy et al. 2018 use European Social Surveys for period 2002-2010 and 2002-2016, respectively. They both analyze the effect of same-sex marriage recognition policies on attitudes toward sexual minorities. They find that in countries with relationship recognition policies for same-sex couples, there is an increase in the the share of citizens who agree that "gay men and lesbians should be free to live their own life as they wish". In addition, Aksoy et al. 2018 show that men, older individuals, less educated individuals, individuals who have a partner, individuals who live in rural areas, and more religious individuals report significantly more negative attitudes toward sexual minorities than others. Additionally, they show that legal same-sex relationship recognition policies were associated with statistically significant improvements in attitudes toward sexual minorities especially for men, partnered individuals and individuals who live in the rural areas. State and Wernerfelt 2017 use data from Facebook profiles over the course of 2014. They show that same-sex legalization laws are associated with LGBT-support measures such as changing one's sexual orientation to gay or lesbian, or 'liking' a page for LGBT rights organizations.

Our paper is in line with the legitimacy model and contributes to the literature in several aspects. First, our study is the first study to examine the credible casual relationship between same-sex marriage laws and hate-crimes that are based on sexual orientation. Second, while prior studies mostly provide evidence on the effect of the pro-equality laws on societal approval of LGBT individuals, we show that the actions of the perpetrators are changing. Therefore, we are able to show how an important outcome, hate-crime based on sexual orientation, can be impacted by same-sex marriage legalization announcements.

2.3 Data and Empirical Strategy

2.3.1 Data

We use a variety of data sources that predate the 2015 Supreme Court ruling legalizing same-sex marriage nationwide. LGBT hate-crimes are identified from incident-level data from the Federal Bureau of Investigation's (FBI) Uniform Crime Reporting (UCR) Data Series for 2000-2015.[7] The Hate-Crime Statistics Act of 1990 mandates the Attorney General to collect data annually. The FBI defines a hate-crime as a crime that manifests evidence of prejudice based on disability, ethnicity, race, religion, and sexual orientation. Local enforcement agencies voluntarily report hate-crime incidents to the FBI UCR Program.[8] The FBI UCR Program provides guidelines to local enforcement agencies regarding the identification and reporting of crimes motivated by bias.

Bias motivation is used to indicate whether or not an offense was motivated by the offenders' bias and, if so, what type of bias. We define an LGBT hate-crime as a crime with bias motivated by sexual orientation against gay (male), lesbian (female), bisexual, or transgender individuals. The nature of each incident, e.g., assault, murder,

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[7]UCR Data Series are used in former studies (Kaushal, Kaestner, and Reimers 2007, Ryan and Leeson 2011, Mulholland 2013, Anderson, Crost, and Rees 2018).

[8]According to Ryan and Leeson 2011, more than 80 percent of the U.S. population is covered by hate-crime reporting. However, we can not rule out the fact that FBI hate-crime reports depend on the cooperation of local law enforcement agencies. Boyd, Berk, and Hamner 1996 show that understandings or definitions of hate-crimes may vary across divisions. Another issue with the UCR data is under-reporting (Masucci and Langton 2017, Ruback, Gladfelter, and Lantz 2018). Therefore, our results should be interpreted with caution since we can not eliminate the possible biases that might occur due to these data issues. Table A.7 provides summary statistics for reporter and never-reporter counties between 2000 and 2015. During the study period, the number of counties that have ever reported is 1,845, whereas the number of counties that have never reported is 2,793. The characteristics of both groups are mostly similar.

destruction of property, etc., is reported in the data and is used to classify an LGBT hate-crime as either a violent crime or a property crime. We characterize a violent LGBT hate-crime as an LGBT hate-crime in which a perpetrator uses or threatens to use force upon a victim. An LGBT hate-crime against property is an LGBT hate-crime in which property was either damaged or stolen.[9] We aggregate the hate-crime incidents to the county/quarter-year level.

We gather the county-level estimates of total population and the percentages Black, Hispanic, male, young adults (ages 15-34), middle-aged adults (ages 35-54), older adults (ages 55-64), and senior adults (65 and up) from the U.S. Census, 2000-2015. We also obtain annual county-level estimates for the rate of urbanization from the U.S. Census, 2000-2015. We find estimates of county-level share of the population that are frequent religious service attendees using Gallup's annual Economy and Personal Finance Survey, which has collected data about American's religious service attendance since 2003. Respondents are classified as frequent religious service attendees if they attend religious services at least every week. We draw from county-level estimates on educational attainment from the U.S. Census. Shares of the population in the educational attainment groups are based on the total population aged 25 or higher. To measure the ideology of a state's citizens, we use the revised citizen ideology measure originally reported in Berry et al. 1998. Also, to measure the ideology of a state's political leaders, we use the updated government ideology measure from Berry et al. 2010. For these ideology measures, larger values reflect a more liberal ideology. We obtain county level popular vote share won by Democratic presidential candidate in the last presidential election from Harvard Dataverse U.S. Presidential General County Election Results (Leip 2016). We get county-level annual data on other types of crimes from the FBI UCR Data Series for years 2000-2015. For county-level annual

[9] The violent crimes in the UCR Program compose of murders and non-negligent manslaughters, rapes, robberies, and aggravated assaults; whereas property crimes include the offenses of burglary, larceny-theft, motor vehicle theft, and arson (Federal Bureau of Investigation 2018).

unemployment rates, we gather data from the Bureau of Labor Statistics for years 2000-2015. We get county-level annual poverty estimates and average household incomes from the U.S. Census for years 2000-2015. Lastly, we control for whether or not there are other policy changes for LGBT individuals' rights in the states:[10] policies on hate-crimes based on sexual orientation,[11] employment discrimination protections for sexual identity,[12] and civil union rights.[13]

We use the U.S. Census data to estimate the percentage of same-sex households in the following way. First, we use the Census 2000 U.S. 5-Percent Public Use Microdata Sample to estimate the percentage of households that are same-sex at the Public Use Microdata Area (PUMA) level. Similar to Antecol, Jong, and Steinberger 2008, we classify a household as same-sex if the head of household is in an unmarried partnership with a person of the same-sex.[14] Second, we collect PUMA-level total household counts from Census 2000 data and, together with the percentage of households that are same-sex, we estimate the number of same-sex households at the PUMA level. Lastly, we match the counties and PUMAs to derive county-level estimates of the share of households that are same-sex as of 2000.

We consider a county "treated" if the county was ever exposed to a same-sex marriage legalization announcement during that quarter of that year and the following quarters. Summary statistics describing the average characteristics of the full sample,

[10]We gather data for other policy changes for LGBT rights from The U.S. Deparment of Justice 2019 and Movement Advancement Project 2014.

[11]Currently, 45 states have a hate-crime statute and 30 of them include sexual orientation (Valcore 2018).

[12]Many states outlaw bias in hiring, promotion, job assignment, termination, and compensation, as well as harassment on the basis of one's sexual orientation. Some states broaden those protections to cover sexual identity (Tilcsik 2011).

[13]A civil union is a legally recognized relationship between two people similar to marriage, but don't provide federal protections, benefits, or responsibilities to couples. Vermont created the first civil union law in 2000 (Goodnough 2010).

[14]Same-sex marriages were not legal in 2000.

ever treated counties before and after they get the treatment, and control counties are shown in Table 2.1. We have 21,795 observations in the sample. The sample includes 1,845 U.S. counties in total, with 1,492 ever treated counties, and 353 control counties. LGBT hate-crime rates are slightly higher across quarter-year observations in control counties than in treated counties, on average (Figure 2.3). These differences in LGBT hate-crime levels are possibly due to the treated counties being in more urbanized areas and having larger populations. Also, the control counties have a higher poverty rate, on average. Regarding other characteristics, treated counties in the pre-treatment period and the control counties are largely similar.

2.3.2 Empirical Model

We exploit the variation across states in the timing of same-sex marriage legalization announcements to estimate the impact of such announcements on LGBT hate-crimes. We argue that the announcement of a same-sex marriage law change can, in and of itself, impact the behavior of perpetrators of LGBT hate-crimes as news coverage and discussions surrounding LGBT persons and the legalization of same-sex marriage would likely be at their peak around this time. In this way, announcement dates are more applicable than enactment dates. Counties situated in states where there were multiple same-sex marriage legalization announcements are excluded from the sample because the post-treatment period is more clearly defined.[15] The following difference-in-differences model is estimated:

$$Y_{scqt} = \alpha + \beta_1 \mathbb{1}(Treated_c \times Post_{cqt}) + \boldsymbol{\beta_2 X_{ct}} + \boldsymbol{\beta_3 X_{st}} + \boldsymbol{\delta_c} + \boldsymbol{\gamma_{qt}} + \epsilon_{scqt}, \quad (1)$$

where the dependent variable, Y_{scqt}, is $\dfrac{LGBT\ hate\text{-}\ crime\ incidents_{scqt}}{(Population_{scqt}/100,000)}$, which is the LGBT hate-crime rate in county c in state s during quarter q and year t. In the presence of county fixed effects, $\boldsymbol{\delta_c}$, the effect of having ever been exposed to a same-sex

[15] Alabama, California, Kansas, Maine, Maryland, New Jersey, and Washington.

marriage legalization announcement is absorbed. $\mathbb{1}(Treated_c \times Post_{cqt})$ is our variable of interest and is equal to 1 if (i) the county is ever exposed to a same-sex marriage legalization announcement and (ii) the time period the county is observed in is on or after that announcement and 0 otherwise. $\boldsymbol{X_{ct}}$ is a vector of time-varying, county-level controls. This vector includes (1) demographic controls: shares of the population that are Black, Hispanic, male, young adults, middle-aged adults, older adults, senior adults, frequent religious service attendees, high school graduates without a college degree, and college graduates, and the urbanization rate, (2) socio-political controls: the popular vote share won by the Democratic presidential candidate in the last presidential election, and (3) economic controls: the unemployment rate, the share of the population that is in poverty, and median household income. $\boldsymbol{X_{st}}$ is a vector of time-varying, state-level controls. This vector includes the citizen and government ideology measures and controls for other LGBT-specific state laws: LGBT hate-crime laws, non-discrimination work-place laws protecting LGBT workers, and civil union laws. $\boldsymbol{\gamma_{qt}}$ is a vector of quarter-year fixed effects. The county and quarter-year fixed effects account for time-invariant county heterogeneity and national trends in crimes over time, respectively. We cluster standard errors at the state level (Bertrand, Duflo, and Mullainathan 2004). An underlying assumption of the analysis is that any unobservable differences between treated and control counties are not predictive of different trends in LGBT hate-crimes independent of treatment.

2.4 Main Results

2.4.1 Main Results

Table 2 shows the effect of a same-sex marriage legalization announcement on the likelihood of a LGBT hate crime occurring. Model (1) shows the effect without controls. Based on the results from model (1), the likelihood of a LGBT hate crime occurring is reduced by 5.7 percentage points following a same-sex marriage legalization announcement. To consider the possibility that confounding variables might be biasing the estimated treatment effect, we control for a comprehensive set of county- and state-level characteristics in models (2) through (5). The estimated treatment effect does not vary greatly across specifications, which is suggestive evidence supporting the underlying assumption of the analysis. Given model (5) includes a comprehensive set of controls, it is our preferred model. Based on the results from model (5), the likelihood of a LGBT hate crime occurring is reduced by 5.2 percentage points following a same-sex marriage legalization announcement.

Given we argue that announcement dates are more salient than enactment dates, we examine whether or not a similar effect on LGBT hate crimes is observed if we, instead, exploit variation in the timing of same-sex marriage law enactments across states. Table 3 shows the results from that analysis using the preferred model. Based on the results of Table 3, the estimated treatment effect is negative although less precisely estimated relative to if announcement dates are exploited. This result provides evidence supporting our assertion that announcement dates are more salient than enactment dates. In Table A1, we consider several alternative models: models (1) and (2) estimate the treatment effect from a logistic and probit regression, respectively, models (3) and (4) estimate the treatment effect from a poisson and negative binomial count model, respectively, with the number of LGBT hate crimes as the outcome variable, and models (5) through (7) estimate the treatment effect on the LGBT hate crime incidence, the likelihood of a violent LGBT hate crime occurring, and the likelihood of a property LGBT hate crime occurring, respectively. Together, the results from Table A1 show that the occurrence of LGBT hate crimes is reduced following a same-sex marriage legalization announcement.

2.4.2 Robustness Checks

Table A2 provides results from several robustness checks. Model (1) shows that the main results are robust to restricting the treated counties to ones where exposure to a same-sex marriage legalization announcement occurred via court-order. Models (2) and (3) show that the main results are mostly robust to comparing treatment groups where all counties are urban and rural, respectively. Models (4) and (5) show that the main results are robust to comparing treatment groups where all counties have other pro-LGBT laws and no other pro-LGBT laws, respectively. Model (6) shows that the main results are robust to including counties with multiple same-sex marriage legalization announcements, where the most recent announcement date is examined. Lastly, model (7) shows that the main results are robust to restricting the treated counties to ones where enactment occurred prior to the 2015 Supreme Court ruling.

2.5 Further Examination

2.5.1 Probing the Mechanism: Might the Effect Be Driven by Changes in the Behavior of Likely Perpetrators of LGBT Hate Crimes?

The main results demonstrate that the likelihood of a LGBT hate crime occurring is reduced following a same-sex marriage legalization announcement. We argue that following a same-sex marriage legalization announcement, perpetrators of LGBT hate crimes respond by reducing the frequency in which they commit such crimes. If this is true, we would expect that a same-sex marriage legalization announcement would be most impactful in counties with high shares of perpetrators. If the estimated treatment effect shares no connection with perpetrators of LGBT hate crimes, we would not expect to observe differences in the treatment effect between counties with different shares of perpetrators. Table 4 provides the results from our probe of this potential mechanism.

Since we do not have data on actual perpetrators, we examine the mechanism using likely perpetrators. Likely perpetrators, we argue, tend to be young males and ideologically conservative. The share of likely perpetrators should serve as a good proxy for the share of perpetrators in a county. Further, taking into account that areas with high shares of LGBT hate crime perpetrators might also have low shares of LGBT persons, we control for the county-level share of households that are same-sex. Model (1) estimates the differential effect of a same-sex marriage legalization announcement on the likelihood of a LGBT hate crime occurring for counties with a high share of young males. The estimated coefficient on the × *% Young Males* variable (-0.062) is the differential effect and tests whether or not the effect of a same-sex marriage legalization announcement is statistically different for counties with high shares of young males. The coefficient is negative and statistically different from zero. This result shows that counties high shares of young males are most impacted by the same-sex marriage legalization announcement. Further, the linear combination of the coefficients on the *Post Pro-Same-Sex-Marriage Law Announcement* and × *% Young Males* variables (-0.112) is the effect of a same-sex marriage legalization announcement on the likelihood of a LGBT hate crime occurring in counties with high shares of young males.

Model (2) tests whether or not the estimated treatment effect is statistically different for counties situated in states with high ideological conservative measures. Although the differential effect is less precisely estimated, the coefficient on the differential effect is negative. Also, the overall effect for these counties (-0.078) is larger than the effect on all other counties (-0.060). Lastly, model (3) controls for the percentage of young males and the ideological conservative measure. Results are similar. Overall, the results from Table 4 demonstrate that the estimated treatment effect on LGBT hate crimes, identified from exploiting the variation in the timing of same-sex marriage legalization announcements, is largest in counties with high shares of perpetrators.

2.5.2 Supportive Evidence: The Impact of Same-Sex Marriage Bans on LGBT Hate Crime Occurrences

Our findings show that a same-sex marriage legalization announcement leads to a reduction in the likelihood of a LGBT hate crime occurring. If providing rights to LGBT individuals would improve behavior, it is plausible to expect that restricting rights would have the opposite effect. For instance in Russia, hate crimes against LGBT people have doubled in the next five years after a law banning "gay propaganda" which designed to stop gay pride marches and to detain gay rights activists (Litvinova 2017).

Prior to *Obergefell v. Hodges* (2015), many states enacted amendments to their state constitutions which prevented the recognition of same-sex marriages. Before 2000, most of them had legislation banning same-sex marriages . After 2000, some states began passing state constitutional amendments banning same-sex marriages as a response to court rulings deeming legislative bans as unconstitutional[16]. Table A.1 gives information on the ban dates. Since our findings provide evidence that LGBT hate crimes declined following to same-sex marriage announcements, we would expect an increase in LGBT hate crimes after same-sex marriage bans. We would anticipate even more impact from constitutional bans in areas where there were no prior legislative bans.

To explore these possibilities, we exploit the variation in the timing of bans on same-sex marriage across states. We estimate equation (1) where $Treated_c$ is a binary variable equal to 1 if a county is ever exposed to same-sex marriage ban, and 0 otherwise. Our analyses include all counties in the sample. Table 2.9 presents the

[16]Colorado, Indiana, Nebraska, Oregon and Wisconsin are the states that did not have ban prior to 2000, but has ban afterward.

preferred models which show the effect of a same-sex marriage ban on the likelihood of an LGBT hate crime occurring. Model (1) shows that the likelihood of an LGBT hate crime occurring is increased by 1.1 percentage points, on average, following a ban on same-sex marriage. Even if the effect is not statistically precise, it provides supportive evidence that restricting LGBT rights has the reverse effect on LGBT hate crimes. In model (2), we estimate the differential effect of a ban on same-sex marriage on the likelihood of an LGBT hate crime occurring. We do this for the counties of states that did not have bans before 2000, but do have bans afterward. The estimated coefficient on the *States Never Banned Before 2000* variable (0.095) is the differential effect and tests whether or not the effect of a same-sex marriage ban is statistically different for counties of states that have only ban after 2000. The coefficient is positive and statistically different from zero. Since areas with bans prior to 2000 are only reinforcing existing laws, counties without prior bans are most impacted. Model (3) and (4) are the results for LGBT violent and property hate crime occurring, respectively. The results show that the effect is mainly driven from the change in violent crimes. The coefficient for the differential effect for violent crime (0.085) is positive and statistically different from zero. This means that, following a same-sex marriage ban, the violent crimes increase in the counties without prior ban. For property crimes however, the coefficient is 0.004 which is close to zero and is not significant. This indicates that the same-sex marriage bans have no effect on property crimes.

2.5.3 Falsification Tests

If the main results are because of some spurious relationship, we might expect to see significant reductions in other crimes. Table 5 shows the results from several falsification tests. Models (1) through (4) estimate the effect of a same-sex marriage legalization announcement on the likelihood of any race-based hate crime occurring, the likelihood of any religious-based hate crime occurring, the likelihood of any non-LGBT hate crime occurring, and the total crime incidence, respectively. The results of Table 5 demonstrate that these crimes are not impacted following a same-sex marriage legalization announcement.

2.6 Conclusion

This paper examines the impact of same-sex marriage legalization announcements on LGBT hate crimes. We exploit the variation across states in the timing of same-sex marriage legalization announcements to estimate the impact of such announcements on LGBT hate crimes. We find that following a same-sex marriage legalization announcement, the likelihood of a LGBT hate crime occurring is reduced by 5.2 percentage points. This result holds against several robustness checks. Further, we argue that reductions in LGBT hate crime occurrences following a same-sex marriage legalization announcement is due to perpetrators of LGBT hate crimes reducing the frequency in which they commit such crimes. Results from our probe of this potential mechanism provides evidence that the behavior of perpetrators is impacted. Together, results demonstrate that pro-LGBT laws can help reduce hate crimes against LGBT persons.

Tables

Table 2.1: Summary Statistics

	Full		Treated Counties Pre-Treatment		Treated Counties Post-Treatment		Control Counties	
	Mean	Standard Deviation	Mean	Standard Deviation	Mean	Standard Deviation	Mean	Standard Deviation
Outcomes								
LGBT Hate-Crime per 100,000	0.37	1.45	0.37	1.42	0.33	1.05	0.40	1.73
Violent LGBT Hate-Crime per 100,000	0.25	1.14	0.25	1.14	0.23	0.86	0.26	1.30
Property LGBT Hate-Crime per 100,000	0.12	0.90	0.12	0.87	0.10	0.63	0.14	1.13
Any LGBT Hate-Crime	0.30	0.46	0.30	0.46	0.36	0.48	0.27	0.44
Any Violent LGBT Hate-Crime	0.23	0.42	0.22	0.42	0.29	0.45	0.20	0.40
Any Property LGBT Hate-Crime	0.11	0.32	0.11	0.31	0.14	0.35	0.10	0.30
County-Level Demographic Controls								
% of Households Same-Sex	0.01	0.02	0.01	0.02	0.02	0.03	0.01	0.02
Total Population	310.804	464.349	312.175	487.047	407.686	489.725	241.867	303.271
% Black	0.10	0.12	0.10	0.12	0.09	0.10	0.12	0.13
% Hispanic	0.08	0.10	0.09	0.11	0.10	0.10	0.03	0.02
% Male	0.49	0.01	0.49	0.01	0.49	0.01	0.49	0.01
% Young Adults (ages 15-34)	0.27	0.05	0.28	0.05	0.27	0.05	0.27	0.05
% Middle-Aged Adults (ages 35-54)	0.28	0.03	0.28	0.03	0.27	0.03	0.28	0.02
% Older Adults (ages 55-64)	0.11	0.02	0.11	0.02	0.13	0.02	0.11	0.02
% Senior Adults (ages 65 and up)	0.14	0.04	0.13	0.04	0.15	0.03	0.14	0.03
Urbanization Rate	0.54	0.40	0.54	0.40	0.63	0.38	0.50	0.41
% Frequent Religious Service Attendees	0.49	0.19	0.49	0.20	0.38	0.14	0.55	0.17
% HS Diploma, No Bachelors	0.60	0.08	0.60	0.08	0.58	0.09	0.62	0.07
% Bachelors or More	0.25	0.10	0.25	0.10	0.31	0.10	0.22	0.09
State- & County-Level Socio-Political Controls								
% Democratic Presidential Vote	0.46	0.12	0.46	0.12	0.53	0.14	0.43	0.11
Citizen Conservative State Measure	50.49	13.43	50.32	13.02	58.88	18.19	45.84	8.13
Government Conservative State Measure	44.52	15.11	44.43	14.73	52.22	18.51	39.99	12.12
County-Level Economic Controls								
Unemployment Rate	6.19	2.54	6.14	2.66	6.09	1.68	6.51	2.46
% in Poverty	13.54	5.17	13.35	5.18	13.40	4.92	14.48	5.19
Median Household Income	$47,355	$12,401	$46,596	$12,001	$57,224	$13,650	$44,374	$10,107
# of Counties	1,845		1,453		641		353	
# of Observations	21,795		15,955		2,296		3,544	

Table 2.2: The Effect of a Same-Sex Marriage Legalization Announcement on the LGBT Hate-Crime Rate

	(1)	(2)	(3)	(4)	(5)
After Legalization Announcement	-0.070*	-0.103**	-0.107**	-0.113***	-0.111***
	(0.036)	(0.039)	(0.039)	(0.042)	(0.039)
County-Level Demographic Controls	No	Yes	Yes	Yes	Yes
State- & County-Level Socio-Political Controls	No	No	Yes	Yes	Yes
County-Level Economic Controls	No	No	No	Yes	Yes
Other LGBT Policy Controls	No	No	No	No	Yes
R-Squared	0.004	0.006	0.006	0.006	0.007
Observations	21,795	17,527	17,527	17,522	17,522

Standard errors in parentheses.

Standard errors are robust and clustered at the state level.

OLS estimates.

* p<0.10, ** p<0.05, *** p<0.01

Note: Regressions include quarter and county fixed effects.

Table 2.3: The Effect of a Same-Sex Marriage Legalization Announcement on the LGBT Hate-Crime Rate by Crime Type

	(1) Violent LGBT Hate-Crime Rate	(2) LGBT Property Hate-Crime Rate
After Legalization Announcement	-0.072**	-0.039*
	(0.034)	(0.023)
R-Squared	0.008	0.004
Observations	17,522	17,522

Standard errors in parentheses.

Standard errors are robust and clustered at the state level.

OLS estimates.

* p<0.10, ** p<0.05, *** p<0.01

Note: Regressions include quarter and county fixed effects. Regressions also include county-level demographic controls, state- and county-level socio-political controls, county-level economic controls, and other LGBT policy controls.

Table 2.4: The Effect of a Same-Sex Marriage Legalization Announcement on the Likelihood of an LGBT Hate-Crime Occurring

	(1)	(2)	(3)	(4)	(5)
After Legalization Announcement	-0.060**	-0.057***	-0.058***	-0.057***	-0.056***
	(0.022)	(0.020)	(0.020)	(0.020)	(0.019)
County-Level Demographic Controls	No	Yes	Yes	Yes	Yes
State- & County-Level Socio-Political Controls	No	No	Yes	Yes	Yes
County-Level Economic Controls	No	No	No	Yes	Yes
Other LGBT Policy Controls	No	No	No	No	Yes
R-Squared	0.006	0.008	0.008	0.009	0.009
Observations	21,795	17,527	17,527	17,522	17,522

Standard errors in parentheses.
Standard errors are robust and clustered at the state level.
OLS estimates.
* $p<0.10$, ** $p<0.05$, *** $p<0.01$

Note: Regressions include quarter and county fixed effects. Regressions also include county-level demographic controls, state- and county-level socio-political controls, county-level economic controls, and other LGBT policy controls.

Table 2.5: The Effect of a Same-Sex Marriage Legalization Announcement on the Likelihood of an LGBT Hate-Crime Occurring by Crime Type

	(1) Any Violent LGBT Hate-Crime	(2) Any LGBT Property Hate-Crime
After Legalization Announcement	-0.053**	-0.020
	(0.020)	(0.021)
R-Squared	0.008	0.006
Observations	17,522	17,522

Standard errors in parentheses.
Standard errors are robust and clustered at the state level.
OLS estimates.
* $p<0.10$, ** $p<0.05$, *** $p<0.01$

Note: Regressions include quarter and county fixed effects. Regressions also include county-level demographic controls, state- and county-level socio-political controls, county-level economic controls, and other LGBT policy controls.

Table 2.6: Robustness Checks

	(1) Court-Order Only	(2) Urban-Only	(3) Non-Urban	(4) No Other Pro-LGBT Laws	(5) With Other Pro-LGBT Laws	(6) No County Restrictions
After Legalization Announcement	-0.144***	-0.054**	-0.329	-0.165***	-0.106**	-0.122***
	(0.047)	(0.023)	(0.219)	(0.056)	(0.045)	(0.038)
R-Squared	0.009	0.008	0.032	0.007	0.015	0.009
Observations	13,525	13,352	4,170	8,580	8,942	13,692

Standard errors in parentheses.

Standard errors are robust and clustered at the state level.

OLS estimates.

* p<0.10, ** p<0.05, *** p<0.01

Note: Regressions include quarter and county fixed effects. Regressions also include county-level demographic controls, state- and county-level socio-political controls, county-level economic controls, and other LGBT policy controls.

Table 2.7: Falsification Tests: The Effect of a Same-Sex Marriage Legalization Announcement on Other Types of Crimes

	(1) Race-Based Hate-Crime Rate	(2) Religious-Based Hate-Crime Rate	(3) Other Hate-Crime Rate	(4) Total Crime Rate
After Legalization Announcement	0.039	-0.043	0.007	-78.605
	(0.073)	(0.046)	(0.098)	(130.133)
R-Squared	0.012	0.005	0.012	0.038
Observations	17,522	17,522	17,522	17,473

Standard errors in parentheses.

Standard errors are robust and clustered at the state level.

OLS estimates.

* p<0.10, ** p<0.05, *** p<0.01

Note: Regressions include quarter and county fixed effects, county-level demographic controls, state- and county-level socio-political controls, county-level economic controls, and other LGBT policy controls.

Table 2.8: The Effect of Same-Sex Marriage Legalization on the LGBT Hate-Crime Rate In Areas With Likely Perpetrators: A Heterogeneity Test.

	(1) LGBT Hate-Crime Rate	(2) Violent LGBT Hate-Crime Rate	(3) LGBT Property Hate-Crime Rate
After Legalization Announcement	-0.138***	-0.100**	-0.038
	(0.048)	(0.039)	(0.026)
× County Has a Large % of Young White Males & a High Citizen Conservative Measure	-0.076	-0.061	-0.015
	(0.046)	(0.037)	(0.033)
× County Has a Large % of Same-Sex Households	0.092**	0.093***	-0.001
	(0.034)	(0.033)	(0.023)
Effect for Counties with a Large % of Young White Males & a High Citizen Conservative Measure	-0.214**	-0.161**	-0.053
P-Value	0.001	0.008	0.251
R-Squared	0.007	0.008	0.004
Observations	17,473	17,473	17,473

Standard errors in parentheses.
Standard errors are robust and clustered at the state level.
OLS estimates.
* p<0.10, ** p<0.05, *** p<0.01

Note: Regressions include quarter and county fixed effects, county-level demographic controls, state- and county-level socio-political controls, county-level economic controls, and other LGBT policy controls.

Table 2.9: The Effect of a Same-Sex Marriage Ban on the LGBT Hate-Crime Rate

	(1)	(2)	(3)	(4)
	LGBT Hate-Crime Rate		Violent LGBT Hate-Crime Rate	LGBT Property Hate-Crime Rate
After Ban	0.014	-0.005	0.019	-0.024
	(0.048)	(0.050)	(0.029)	(0.033)
× States Never Banned Before 2000		0.134	0.086	0.049
		(0.120)	(0.125)	(0.048)
Effect for States w/ Ban Conditional on no Prior Ban		0.130	0.105	0.025
P-Value		0.243	0.412	0.425
R-Squared	0.007	0.007	0.008	0.004
Observations	17,527	17,522	17,522	17,522

Standard errors in parentheses.
Standard errors are robust and clustered at the state level.
OLS estimates.
* p<0.10, ** p<0.05, *** p<0.01

Note: Regressions include quarter and county fixed effects. Regressions also include county-level demographic controls, state- and county-level socio-political controls, county-level economic controls, and other LGBT policy controls.

Figures

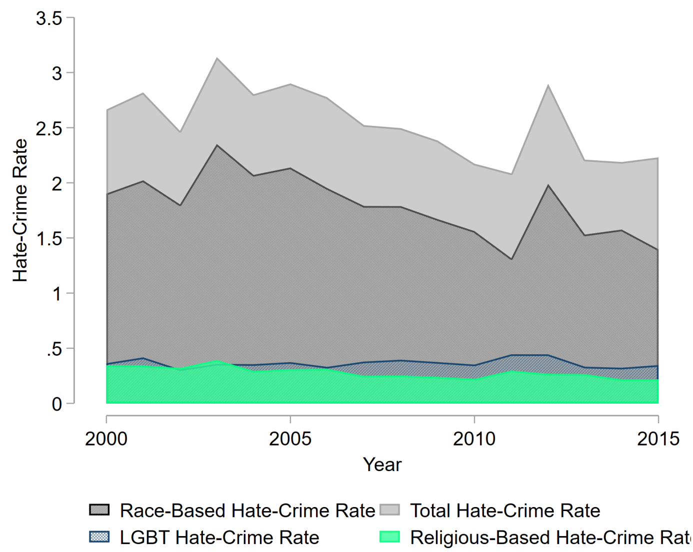


Figure 2.1: Hate-Crime Rates by Year, Type

Source: FBI's Uniform Crime Reporting (UCR) Data Series

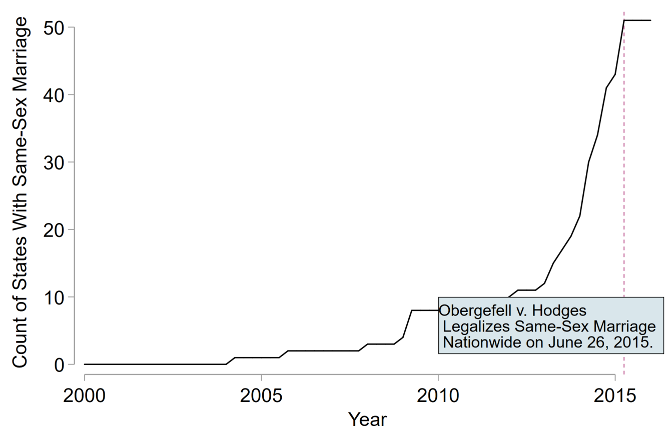


Figure 2.2: Number of States that Legalize Same-Sex Marriages by Time

Source: National Conference of State Legislatures, the Human Rights Campaign and various news sources

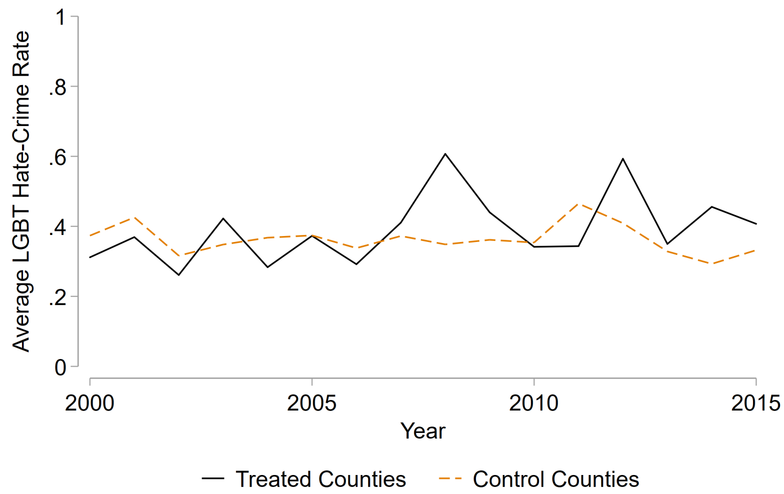


Figure 2.3: Average LGBT Hate-Crime Rate by Treatment Group and Year

Source: FBI's Uniform Crime Reporting (UCR) Data Series

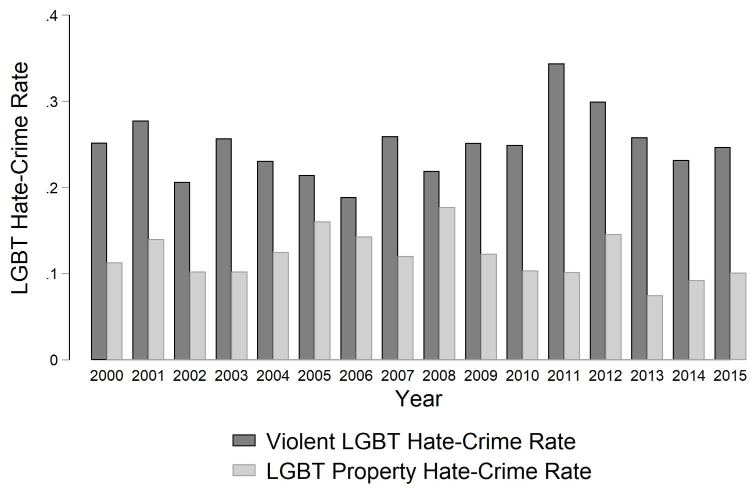
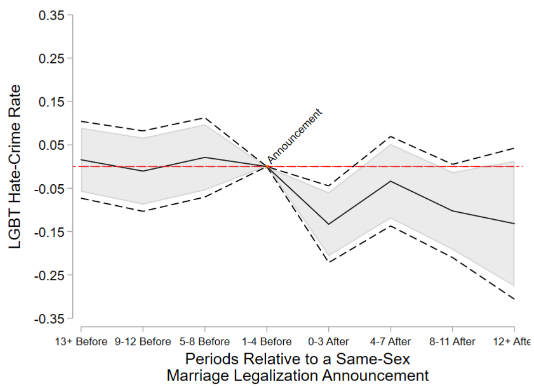
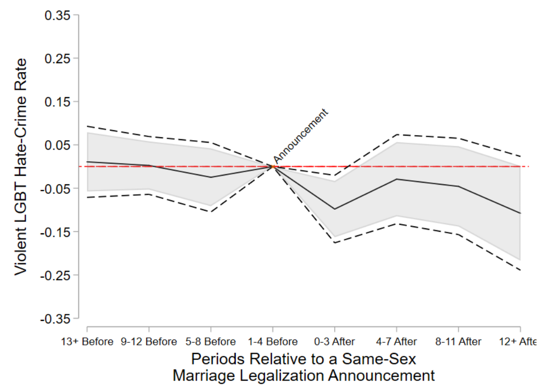


Figure 2.4: LGBT Hate-Crime per 100,000 People by Crime Type

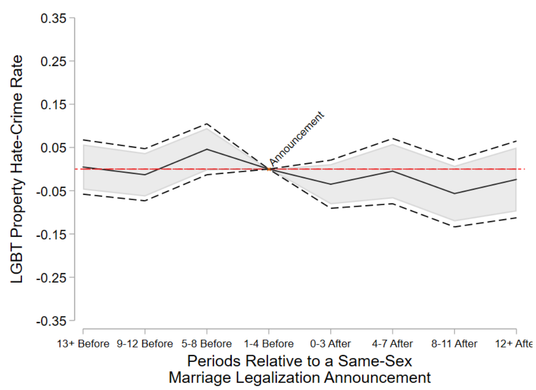
Source: FBI's Uniform Crime Reporting (UCR) Data Series



(a) Effect on the Total LGBT Hate-Crime Rate



(b) Effect on the Violent LGBT Hate-Crime Rate



(c) Effect on the LGBT Property Hate-Crime Rate

Figure 2.5: Event Study Estimates of the Effect of a Same-Sex Marriage Legalization Announcement on LGBT Hate-Crime Rate

Note: The figure graphically depicts the effect of same-sex marriage legalization announcements on LGBT hate-crime rates when the effect is allowed to vary by time. Each panel is estimated from a different regression. Panel (a) estimates the effect on total LGBT hate-crimes, panel (b) estimates the effect on violent LGBT hate-crimes, and panel (c) estimates the effect on LGBT property hate-crime. The periods are grouped into four quarter (1 year) bins relative to treatment. The year prior to treatment is omitted, thus the other periods are relative to that year. The solid line reports the estimate of the effect of being treated in that time relative to treatment. The gray highlighted area represents the 90% confidence interval for the estimation and the dotted lines represent the 95% confidence interval. Regressions include quarter and county fixed effects, county-level demographic controls, state and county level socio-political controls, county level economic controls, and other LGBT policy controls.

Chapter 3

Mile High: How Does Marijuana Legalization Affect Air Travel?[1]

3.1 Introduction

As of November 2018, recreational marijuana is legal in ten states and is available for purchase in six of those, becoming an important policy issue. The availability of recreational marijuana could increase demand for travel to legalized states. Colorado, the first state to legalize recreational marijuana, is also seeing record numbers of tourists (Wenzel 2018) and there is public interest on how much this can be attributed to marijuana. According to a Denver Post article Blevins 2016, two years after the state opened its first dispensaries, 23% of passengers stated that marijuana availability positively impacted their choice to vacation in Colorado.[2] Survey evidence like this has potential issues, such as incentives for those being interviewed to under report interest in marijuana. Still, it is clear that tourism numbers have increased. Since 2009, domestic visitors have increased by 41%, double the national growth rate of 20%. While these increase in tourism coincide with the legalization of recreational marijuana, it is unclear how much of its success in tourism can be attributed to marijuana legalization. After all, 2017 marked the eighth consecutive year of record setting growth in tourism in Colorado (Wenzel 2018). This trend began well before recreational marijuana

[1]Robert Pettis. To be submitted to *Economics of Transportation*.

[2]However,only 4% of tourists stated that they came in part for marijuana and actually visited a dispensary.

was legalized in Colorado. Understanding the effects of marijuana legalization on tourism is important. The effects on tourism affect public finance and regional economic development and are part of the cost-benefit analysis of any state considering legislation.

In this paper, I study the overall effect of the legalization of recreational marijuana on airline travel, making effort to distinguish tourism travel where possible. Specifically, I estimate changes in the number of passengers as a result of legalization. Additionally, unless supply is perfectly elastic, fares should increase as a result of a shift in demand for air travel, thus I consider the effect of legalization on airline fares. To do this, I use a difference-in-differences approach that exploits variation in the adoption of legal recreational marijuana across states and time.

Difference-in-Differences estimation shows no effect of marijuana legalization on the number of airline travelers. There is some evidence of a seasonal effect on the average price of a ticket. Using a synthetic control approach, I find evidence of a temporary drop in passenger traffic after legalization. Possible causes of the drop could be passengers choosing to vacation in other states due to a negative reaction to marijuana legalization or passengers delaying their trip until after dispensaries opened. After dispensaries open, this negative effect reverses.

This paper contributes to the growing literature on the effects of legal marijuana on consumer behavior, such as Cheng, Mayer, and Mayer 2018 who provide evidence that demand for housing has increased in Colorado as a result of legalization. Additionally, to my knowledge, there is little other literature that studies the effect of quickly situated amenities that vary by state on airline travel. This is more limited when studying recreational amenities, though there are some examples such as in Weiler and Seidl 2004 which studies the effect of national park designation on tourism. There have been studies that investigate travel for medical reasons, such as traveling to gain access to safe and legal abortion (Sethna and Doull 2012 and Sethna et al. 2013). This paper fills in the gap by examining the effect of recreational marijuana legalization on passenger air travel.

In the next section, I provide background on marijuana legalization in the United States. In Section II, I describe my data. Section III outlines my main empirical approach while Section IV describes the results. In Section and Section VI concludes.

3.2 Background on Marijuana Legalization

Marijuana's legal status in the United States is complicated. It is criminalized at the federal level, but at the state (or even municipal) level, the legal status of marijuana can vary. In particular, a state can legalize marijuana for all purposes including for recreation, marijuana could be decriminalized - prompting a fine but not jail time, it could be legalized for medical use, or they could have no legal marijuana. Even among states that legalize for medical use, the range of products legalized and for whom can vary. Depending on the state, marijuana could be available in edible or smokable forms (MMJ Recs 2016) and could be used recreationally. On the other hand, they could allow only a limited number of marijuana derivatives for medical use and could limit who can purchase to very specific illnesses. In the latter case, the legality of the psychoactive cannabinoid in marijuana, tetrahydrocannabinol (THC) also varies by state.

Municipalities within states that legalize recreational marijuana may ban the sale of marijuana. However, a consumer could travel to a nearby legalized jurisdiction for consumption. Thus, I focus on state level legal status as I consider travel to and from these states. Figure 3.1 shows the composition of legal marijuana as of the end of 2017. Note the close geographical proximity of many of the states that have legalized recreational marijuana at this time.

State-level recreational legalization began in late 2012 in Colorado where voters approved legalization at the ballot box.[3] As of November 2018, nine states have legalized marijuana and more votes are forthcoming. While later adopting states may be able to learn about the process of adopting legal marijuana from early adopters, the process appears to take a relatively similar time from legalization passing to availability of marijuana through dispensaries. This gap consists of the time required to set up appropriate agencies to oversee the production and sales of legal marijuana, to review and distribute applications for licenses to produce and sell marijuana, and for producers and sellers to legally generate enough product to begin sales. Figure 3.2 highlights this difference by graphing both the count of states that have legalized marijuana and the count of states that have legalized marijuana and have access through open dispensaries.

3.3 Data

To understand how marijuana legalization affects air travel, I draw from several data sources: The Origin and Destinations survey for air travel data, dates of marijuana legalization and date of availability of recreational marijuana. For synthetic control models, I also control for natural amenities such as highest mountain in a state and miles of coastline. In addition, some model specifications control for state-level economic conditions by using gross state product (GSP) by year, linearly interpolated for each quarter. In some specifications, I seasonally adjust the outcome variables such that the adjustment varies by route but not year, where convenient.[4] For example, in a synthetic control model, not all pre-treatment periods are included, as is typical, so it would be convenient to adjust for seasonality prior to model implementation.

[3] In fact, all such legalization was done through referendum within the scope of this study. Vermont became the first state to legalize recreational marijuana through the state's legislature in 2018.

[4] The process of seasonal adjustment is documented in Section B.1.

3.3.1 Air Travel

I use the Origin and Destination Survey from the Bureau of Transportation Statistics at the US Department of Transportation (US DOT 2005-2017). These data represent a 10% sample of all flights, from the first quarter of 1995 to the fourth quarter of 2017. For some analyses, I use only data from the first quarter of 2005 to the fourth quarter of 2017. This is because of availability of covariates and the fact that unobserved latent variables may have caused a long term shift in passenger traffic for treated states in 2004. Figure 3.3 provides an illustration of this change in 2004. I generate airport to airport origin/destination pairs, a route, and their associated passenger count and average fare. These data include information on each leg of a full journey, including hubs. I specify a route using actual origin to final destination pairs. Furthermore, I keep only itineraries that are round trips. There are approximately 3.7 million observations of such trips in these data. If the travel to and from the destination occur on opposite sides of a quarter changing date (such as Jan 1st), the flights are automatically recorded as one way trips each way, and thus will be dropped.[5] In other words, a round trip must be completed within a particular quarter in order to not be dropped. I also drop routes with few (under 10) recorded passengers per quarter. A further limitation is that I do not have information on when the ticket was purchased or if the passenger used a promotion to get a discount.

3.3.2 Recreational Marijuana Legalization

I collect dates for the passage of legislation or referenda on marijuana legalization, and the date on which the first legal sale of marijuana took place. This is important due to lag between the passage of a resolution and its implementation. The lag is,

[5]This means that I would not be able to record those traveling for New Years celebrations, a holiday known for substance use.

on average, almost 457 days (or 5 quarters). Figure 3.2 shows this discrepancy over time and Table 3.1 details when and where this occurs. While cannabis businesses may still be banned in some municipalities even in legalized states, I use state-level legalization, rather than municipal-level rules because visitors in such an area may travel to a nearby dispensaries even if marijuana is illegal in the municipality that contains the destination airport (Colorado Municipal League 2018, Misulonas 2017). For dispensary openings, I rely on articles indicating the opening of the first dispensary in a state.These data were collected through review of online news articles and, in some cases, interviews with dispensary staff. For date of referenda, I accessed the public record.

The states that have legalized (as of 12/31/2017) are shown in Figure 3.1. Note that California did not open their first legal recreational dispensary until 1/1/2018, and so will not be considered treated when considering the date of availability.The dataset does not contain data on air travel to Alaska, thus policy variation will come from west coast states. If air travel to these states change in terms of passenger count at different rates than the rest of the country, this could violate the parallel trends assumption that is necessary for validity of the difference-in-differences model.

3.3.3 Natural Amenities

Many of the states that have introduced legal marijuana have significant natural amenities. I use the static variables of tallest point in the state and miles of coastline. These do not vary over time and so would be differenced out of any difference-in-differences estimator; however, they are useful in constructing a synthetic control of treated states.

Summary statistics are reported in Tables 3.2 (using date of marijuana policy passage) and 3.3 (using date of marijuana availability).

3.4 Methods

3.4.1 Difference-in-Differences

To estimate the impact of recreational marijuana legalization on air travel, I employ a difference-in-differences (DD) model. Using indicators for the treatment status of the states on each end of a route (both origin and destination), I estimate the effect of marijuana legalization on round trip routes to and from treated states. The model is as follows:

$$\begin{aligned} Y_{r,p,t,q} = \alpha + \delta_p + \gamma_{t,q} + \psi_{r,p} + \kappa_{r,p,q} + \phi_1 Dest_{r,p,t,q} + \phi_2 Origin_{r,p,t,q} \\ + \phi_3 (Dest \times Origin)_{r,p,t,q} + \beta \mathbf{X}_{r,p,t,q} + \epsilon_{r,p,q,t}, \end{aligned} \tag{3.1}$$

where: indexes are route (r), city-pair(p), quarter-year(t), and quarter(q); $y_{r,p,t,q}$ is the outcome; δ_p, $\gamma_{t,q}$, $\psi_{r,p}$, $\kappa_{r,p,q}$ are city pair, time, route, and quarter varying route fixed effects; ϕ_1, ϕ_2, and ϕ_3 are coefficients on treatment status of dest, origin states. By differencing each city in a city-pair, thus comparing differences in the outcome variables between routes in the same city pair, the model becomes:

$$\begin{aligned} y_{1,p,t,q} - y_{2,p,t,q} = \tilde{\psi}_p + \tilde{\kappa}_{p,q} + \phi_1 (Dest_{1,p,t,q} - Dest_{2,p,t,q}) \\ + \beta (X_{1,p,t,q} - X_{2,p,t,q}) + (\epsilon_{1,p,q,t} - \epsilon_{2,p,q,t}), \end{aligned} \tag{3.2}$$

where $\tilde{\psi}_p$=$\psi_{1,p} - \psi_{2,p}$ and $\tilde{\kappa}_{p,q} = \kappa_{1,p,q} - \kappa_{2,p,q}$. Notice that, through differencing, ϕ_2 and ϕ_3 are no longer estimated. ϕ_2 is not estimated due to collinearity with ϕ_1. This is because, on a given city pair, the destination for a route is the origin for the other route in the city pair. Thus, $\phi_2 = -\phi_1$. ϕ_3 is omitted because, by definition, each city in a given city pair for $(Dest \times Origin)_{r,p,t,q}$ has the same treatment. Thus, all values for the differenced variable are zero. This model allows the coefficient of interest, ϕ_1, to be interpreted as the difference between routes from untreated to treated states and routes from treated to untreated states. If travel from on a route from a non-treated state to a treated state increases relative to that of a treated to non-treated route, ϕ_1 should be a positive. Outcomes I consider include the natural log of passengers and the natural log of the average market fare.[6] Specifications are separately run considering the date of legislation/initiative and the date a dispensary first opened (signifying the availability of legal marijuana). In some specifications, I allow the treatment effect to vary based on the quarter of travel.

3.4.2 Event Study

In order to test the parallel trends assumption for the DD model, as well as to see how the effect of treatment changes over time, I employ an event study similar to that of Jacobson, LaLonde, and Sullivan 1993 and Kline 2011. Specifically, I estimate the following:

$$\begin{aligned} y_{1,p,t,q} - y_{2,p,t,q} = \tilde{\psi}_p + \tilde{\kappa}_{p,q} + \sum_{K=-k}^{-2} \delta_k D_{k,p,t,q} + \sum_{K=0}^{k} \delta_k D_{k,p,t,q} \\ + \beta(X_{1,p,t,q} - X_{2,p,t,q}) + (\epsilon_{1,p,q,t} - \epsilon_{2,p,q,t}), \end{aligned} \tag{3.3}$$

where I include a vector of dummies, $D_{k,p,t,q}$, which are equal to $(Dest_{p,t,q} - Dest_{p,t,q})$ only when treatment is exactly k periods away in city-pair p in quarter of the year q. I omit the period prior to treatment such that results can be interpreted as being relative to the period prior to treatment. Coefficients and their 95% confidence intervals are reported graphically with the coefficient on the y-axis and time on the x-axis.

3.5 Results

3.5.1 Effects of Marijuana Legalization on Air Passenger Travel

Here, I report the difference-in-differences estimates of the effect of marijuana legalization on the number of passengers on routes from non-treated states to treated

[6] Given a change in demand for tickets, the behavior of the supplier in terms of price and supply of flights are important to investigate.

states relative to routes from treated states to non-treated states. First, in Table 3.4, I present the results of several specifications of the effect of treatment on the natural log of passengers, with the top panel using the date of treatment as the date of passage and the bottom panel the date of availability.

Column 1 uses only time fixed effects, Column 2 adds city pair fixed effects, Column 3 adds GSP, and Column 4 allows the effect to vary by quarter. Estimates for the effect range from a 1.3% to 1.8% increase in number of passengers in each case. I am unable to reject the hypothesis that there is no effect, however. The fourth quarter is least affected with a post-availability effect being zero to three digits.

Figure 3.4 presents the results from the event study model specified in Equation 3.3. The results show the pre-trends assumption is satisfied, providing validity to these models. Additionally, post-treatment trends are not statistically different from zero.

The estimated effects prior to treatment do not statistically differ from zero in any case, suggesting that the parallel trends assumption could be satisfied. Similarly, the estimated effects post treatment do not statistically differ from zero. Moreover, the estimates appear to be centered around zero.

3.5.2 Effects of Marijuana Legalization on Average Market Fare

In Table 3.5 I report the difference-in-differences estimates of the effect of marijuana legalization on fares for routes from non-treated states to treated states relative to routes from treated states to non-treated states. Column 1 uses only time fixed effects, Column 2 adds city pair fixed effects, Column 3 adds GSP, and Column 4 allows the effect to vary by quarter. While I cannot reject the hypothesis that there is no effect, the signs are mostly negative. The exception to the negative sign comes when I allow the effect to vary by quarter, specifically in the fourth quarter of the year. Note that this is the same period that the size of the effect on passengers is smallest. While

Colorado gets large numbers of tourism year-round, families tend to visit more in the summer. This demographic is less likely to participate in marijuana consumption. Other tourists, specifically those that may wish to travel for marijuana, would be relatively more likely to travel during winter. Therefore, a plausible explanation is that the airlines anticipated the increase in demand and adjusted the price while not increasing supply to adjust for seasonal capacity constraints.[7]

Figure 3.5 illustrates the coefficients from the event study model on average fares over time relative to legalization as in the prior analysis. There is no evidence of pre-trends using either the date of policy passage or the date of availability. Additionally, as with passengers, post-treatment effects are not statistically different from zero. In summary, there does not appear to be an effect different from zero of marijuana legalization on number of passengers or average fares from non-treated to treated states in excess of that from non-treated to treated states.

3.6 Synthetic Control

3.6.1 Model

The synthetic control method (SCM) developed in Abadie and Gardeazabal 2003, Abadie, Diamond, and Hainmueller 2010, and Abadie, Diamond, and Hainmueller 2015 creates a synthetic version of a treated state through the weighted average of control (donor) states. In addition to relaxing the assumptions of the difference-in-differences model, SCM allows for the weighting of states that provide for a more appropriate match than does the DD model. Additionally, if its assumptions are not violated, it allows for a visual estimation of the counterfactual. Observations that involve an

[7]There is currently a pilot shortage (Wall and Tangel 2018). As flight crews generally live on one end of a route, it would require some shifting of human resources that are in short supply. In at least some cases, this would seem to be the restraint, as in an interview with the a media spokesperson at the Denver Airport, a key airport in this study, I was told that overall there would not be a capacity problem if an airline decided to add more flights. More interviews with airports forthcoming.

origin or destination to or from treated states other than one treated unit of interest are dropped from the model. Outcome variables have been seasonally adjusted.

To estimate a causal effect through the synthetic control method, the first step is to estimate the vector of weights (w) to be given to each state to minimize the mean squared prediction error in the pre-treatment period:

$$\hat{\boldsymbol{w}}_1 = argmin_{\phi_1} \sum_{t=1}^{T_0} (Y_{1t} - \sum_{j=2}^{N} \phi_j^1 Y_{jt})^2 \tag{3.4}$$

$$\text{s.t. } \hat{w_j^1} \geq 0 \text{ for all } 2 \leq j \leq N \text{ and } \sum_{j=2}^{N} \hat{w_j^1} = 1,$$

where the superscript on the weight indexes the state on which state j (the subscript) is a synthetic control for, with the convention being that 1 indexes the treated state. T_0 is the period prior to treatment, from which I estimate the causal effect of legalization:

$$\hat{\alpha_{1t}} = Y_{1t} - \sum_{j=2}^{N} \hat{w_j^1} Y_{jt} \tag{3.5}$$

The parameter $\hat{\alpha_{it}}$ is the estimate of the effect of treatment in state i in quarter-year t. In periods $t < T_0$, the estimates can be used as a measure of pre-treatment fit, and during periods $t > T_0$, the estimates can be interpreted as causal effects.

To compare among similar states to those treated, I use a standard synthetic control using only one state, in this case Colorado.[8] All routes with an origin or destination in a state that becomes treated are removed from the donor pool. There are two main assumptions for the synthetic control method. First, the control state must remain within the convex hull of the donor pool states. Figure 3.6 graphs Colorado vs the min and max values of the outcome variable, seasonally adjusted passengers.[9] At least in terms of the outcome variable, it is possible that a weighted combination of donor states could approximate Colorado.

[8]Synthetic Controls for other treated states are forthcoming

[9]For an explanation of my method for seasonal adjustment, see Appendix B.1.

The second assumption is perfection of the synthetic control. In other words, the synthetic control produced should perfectly match the outcome for Colorado. While this assumption rarely actually holds in practice, an inspection of the pre-treatment match may provide some insight into the appropriateness of the synthetic control.

3.6.2 Results

Figure 3.7 graphs Colorado's seasonally adjusted passenger count against the synthetic Colorado in the first panel. The second panel plots the gap between Colorado and synthetic-Colorado. There is one period in particular where the model has large gaps starting around 2005. This suggests that the model is subject to transitory shocks. I investigate a possible source of this shock and whether it could be affecting the post-treatment outcome. Snowfall is very important to tourist travel to Colorado, due to its natural amenities: Colorado supports the largest ski industry in the United States Burakowski and Magnusson 2012. I use snowfall data National Weather Service 2018 to illustrate a three year moving average of the disparity between snowfall and its overall average in Figure 3.8. The 2005 gap between Colorado and synthetic Colorado corresponds to the lowest point in the three year moving average of snowfall discrepancy, which could explain the reduction in passengers. There is not a similar snowfall disparity post treatment. While this does not mean that Colorado could have been subject to some other type of shock during the post-treatment period, it does suggest that it is not related to snowfall.[10]

There are some possible explanations for the gap. Passengers, once made aware

[10] I also studied if wildfires could have deterred visitors, as this has been a problem in recent years in western states. Figure B.2 illustrates wildfires in Colorado over time. While there does appear to be a relatively moderate spike in wildfires during the post-passage dip, the acreage on fire decreases as the dip decreases, suggesting that it is not wildfires deterring visitors. There is otherwise no correlation with the treatment gap, despite there being years with much higher wildfire acreage. Additionally, the largest of these fires occurred hours away from the larger cities in Colorado (Gabbert 2018). The other, smaller fires around this time were in small towns and should not have affected tourism(Handy 2014 and Moylan 2015).

of the legal change, prior to availability, could adjust their behavior based on their preferences. A pro-marijuana traveler may wish to put off a vacation until legalization. An anti-marijuana traveler may have been discouraged from entering the state or conventions may have been canceled as a result of the legislation. As marijuana legalization becomes a new norm, over time the anti-marijuana traveler may return to traveling to treated states.

The gap between the curves, (α), is shown in Figure 3.9 with placebos generated by performing the synthetic control on non-treated states. Associated pseudo p-values (effect divided by its pre-treatment root mean squared predicted error (RMSPE) as in Galiani and Quistorff 2016) are reported in the second panel. This means that there is a statistically significant reduction in airline traffic to Colorado for the majority of the time post-passage and pre-availability. In particular, Colorado is more than 200,000 passengers less than its synthetic control in the first periods after treatment.[11] Once dispensaries open, the statistical significance goes away. In the survey mentioned earlier, Blevins 2016, there were some respondents that answered that the marijuana legalization negatively impacted their choice to fly to Denver (but they went anyway). Obviously, travelers who chose not to travel because of recently legalize recreational marijuana are not in the dataset. This period after policy passage and before availability could represent this as well as the passengers that want to travel to Colorado but want to wait for the availability of marijuana. This is suggestive of a change in passenger composition.

I also estimate a synthetic control model on fares. Figure 3.10 illustrates the average fares of going to Colorado from non-treated states over time. While Colorado does not have the min or max value of fares at any point, it does approach both. This introduces difficulty in finding a match as there would be few states, if any, that are

[11] Remember that these figures are a 10% random sample of airline traffic. I multiply my estimates by 10 to get a representative estimate of the population

some of the more expensive flights in the early 2000s and are some of the cheapest flights more recently. Indeed, Figure 3.11a (with gap plotted in Figure 3.11b) shows a sizable difference between the fares of traveling to Colorado and to the synthetic control be pre and post treatment. The poor pre-treatment fit appears to violate the perfection assumption of SCM, meaning the model is subject to transitory shocks. In Figure 3.12, I report the p-values for each period after treatment. Despite one of the larger post-treatment root mean squared errors, there is no statistical significance. This is, in part, due to the poor pre-treatment fit.

3.7 Alternative Models

In this section, I address two potential causes of the null results in Section 3.5: substitution to driving, and low arrest rates for marijuana use in untreated states. For the latter, potential users would have a low transaction cost of consuming marijuana in-state, making travel for consumption unnecessary. For brevity, I only report event study models in this section. Regression tables for these alternate models can be found in Appendix B

3.7.1 Travel Distance

First, I weight by the average number of miles flown from an origin to destination.[12] This is an effort to account for driving. Vacationers have a higher elasticity of demand than do business travelers. Additionally, long distance travelers are more likely vacation travelers, rather than business travelers. Therefore, flights with a shorter distance, more likely business passengers, will receive a lesser weight.

Figure 3.13 report results from the distance weighted event studies using date of policy passage and availability respectively. These results are similar to the baseline

[12]The average number of miles flown varies by quarter. This is due to varying intermediate legs of a given origin/destination pair.

model in shape and lack of significance both in the pre and post treatment periods. However, this model has noticeably smaller standard errors across the board, providing further evidence that there is no effect of marijuana legalization on passengers.

I repeat the analysis for fares in Figure 3.14. As before, there are no statistically significant effects, pre or post treatment. This provides further evidence that fares are also not affected by the availability of marijuana.

3.7.2 Arrest Rates

To capture the fact that states that do not have a high arrest rate may not be as incentivized to travel due to the low transaction cost of consuming at home, I weight each state by their 2012 (the year that legalization began) drug arrest rate. Figure 3.15 reports the event studies. As before, there are no significant values, pre or post treatment. Figure 3.15b, however, gives the best argument yet for an increase. The pre-trends are very close to zero and there is an immediate and constant increase after marijuana availability, though it is not statistically significant.

3.8 Conclusion

In this paper, I analyze the effects of marijuana legalization on airline passenger travel and fares. Using a difference-in-differences model, I find no effect of marijuana legalization on either air passenger travel or average fares. Through the synthetic control method, I also find no evidence of an increase. In fact, there is some evidence of a temporary decrease in passengers.

There are some limitations to my estimates. Synthetic Control estimates of the effect of recreational marijuana legalization on passengers is limited in that the point estimates are trending upward and may be statistically significant for post-2017 years. Synthetic Control estimates for fares are, in part, not statistically significant due to large transitory shocks in the pre-period which get the outcome for Colorado close to failing the convex hull assumption. A logical next step is to apply the Imperfect Synthetic Control Method, developed in Powell 2017, which instead requires that Colorado be given positive weight in the synthetic control for another state; however, this may prove difficult as well, as the fares for Colorado get close to both the minimum and maximum for all fares at various points. A further limitation is that my outcome is the difference between traffic going from a non-treated state to a treated state. The effect on passenger traffic is a total effect, not a direct effect, that is, marijuana legalization could improve GSP and thus indirectly increase passenger traffic in both directions. If the effect of marijuana significantly increases income in a treated state, treated state residents may be able to take more vacations out of state which will decrease the effect I see from the differences.

Despite the narrative that marijuana tourism is a force to be reckoned with, I find no evidence to support the claim by air travel. While governments and constituents hoping to swell their coffers may be able to do with a tax, the tax may be paid more by locals than previously thought. Additionally, as the marijuana legalization movement continues, there will be more options for marijuana tourism. If early adopters were not able to see an effect, it is less likely that later adopters would.

Table 3.1: List of States with Legal Recreational Marijuana

State	Date Passed	Date Available
Alaska	11/4/2014	10/29/2016
California	11/8/2016	1/1/2018
Colorado	11/6/2012	1/1/2014
Maine	11/8/2016	
Massachusetts	11/8/2016	
Michigan	11/6/2018	
Nevada	11/8/2016	7/1/2017
Oregon	11/4/2014	10/1/2015
Vermont	1/4/2018	
Washington	11/6/2012	7/8/2014

Table 3.2: Summary Statistics, Treatment Date is Date of Policy Passage

	Non-treated to Non-Treated	Non-Treated to Treated	Treated to Non-Treated	Treated to Treated
Passengers (Count)	204.66	261.61	261.58	654.99
	(813.1)	(1023.1)	(1022.8)	(1936.8)
Market Fare ($)	247.63	291.33	291.74	238.94
	(117.0)	(90.96)	(90.67)	(100.3)
GDP ($ Millions)	605,563.38	824,497.55	674,715.56	1,088,279.96
	(594062.7)	(974328.9)	(672710.2)	(1113599.2)
N	1,949,508	63,024	63,028	9,105

Table 3.3: Summary Statistics, Treatment Date is Date of Availability

	Non-treated to Non-Treated	Non-Treated to Treated	Treated to Non-Treated	Treated to Treated
Passengers (Count)	207.51	279.14	279.47	369.30
	(824.4)	(1105.9)	(1106.3)	(1303.3)
Market Fare ($)	248.90	290.11	290.68	230.44
	(116.4)	(94.17)	(94.23)	(93.44)
GDP ($ Millions)	618,335.60	348,414.55	788,851.32	358,607.68
	(616846.7)	(92373.5)	(813557.2)	(107154.7)
N	2,013,274	34,488	34,445	2,458

Table 3.4: Effect of Recreational Marijuana Legalization on ln(Passengers) Using Policy Passage Date as Treated Date

	(1)	(2)	(3)	(4)
Effect (Passage Date)	0.018	0.018	0.018	
	(0.047)	(0.047)	(0.049)	
Effect - Quarter 1				0.017
				(0.049)
Effect - Quarter 2				0.016
				(0.057)
Effect - Quarter 3				0.030
				(0.056)
Effect - Quarter 4				0.012
				(0.054)
R^2		0.000	0.000	0.000
Effect (Avail. Date)	0.013	0.013	0.013	
	(0.074)	(0.074)	(0.074)	
Effect - Quarter 1				0.002
				(0.073)
Effect - Quarter 2				0.017
				(0.090)
Effect - Quarter 3				0.035
				(0.079)
Effect - Quarter 4				-0.000
				(0.083)
Observations	747263	747263	747263	747263
R^2		0.000	0.000	0.000
GDP			X	X
City Pair FEs		X	X	X

Standard errors in parentheses

Standard Errors are clustered at the city-pair level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.5: Effect of Recreational Marijuana Legalization on ln(Fares) Using Policy Passage Date as Treated Date

	(1)	(2)	(3)	(4)
Effect (Passage Date)	-0.080	-0.047	-0.020	
	(0.072)	(0.074)	(0.075)	
Effect - Quarter 1				-0.036
				(0.145)
Effect - Quarter 2				-0.173
				(0.144)
Effect - Quarter 3				-0.124
				(0.148)
Effect - Quarter 4				0.170
				(0.103)
R^2		0.000	0.000	0.000
Effect (Avail. Date)	-0.063	-0.038	-0.069	
	(0.103)	(0.106)	(0.107)	
Effect - Quarter 1				-0.050
				(0.197)
Effect - Quarter 2				-0.101
				(0.209)
Effect - Quarter 3				-0.243
				(0.199)
Effect - Quarter 4				0.105
				(0.147)
Observations	747263	747263	747263	747263
R^2		0.000	0.000	0.000
GDP			X	X
City Pair FEs		X	X	X

Standard errors in parentheses

Standard Errors are clustered at the city-pair level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

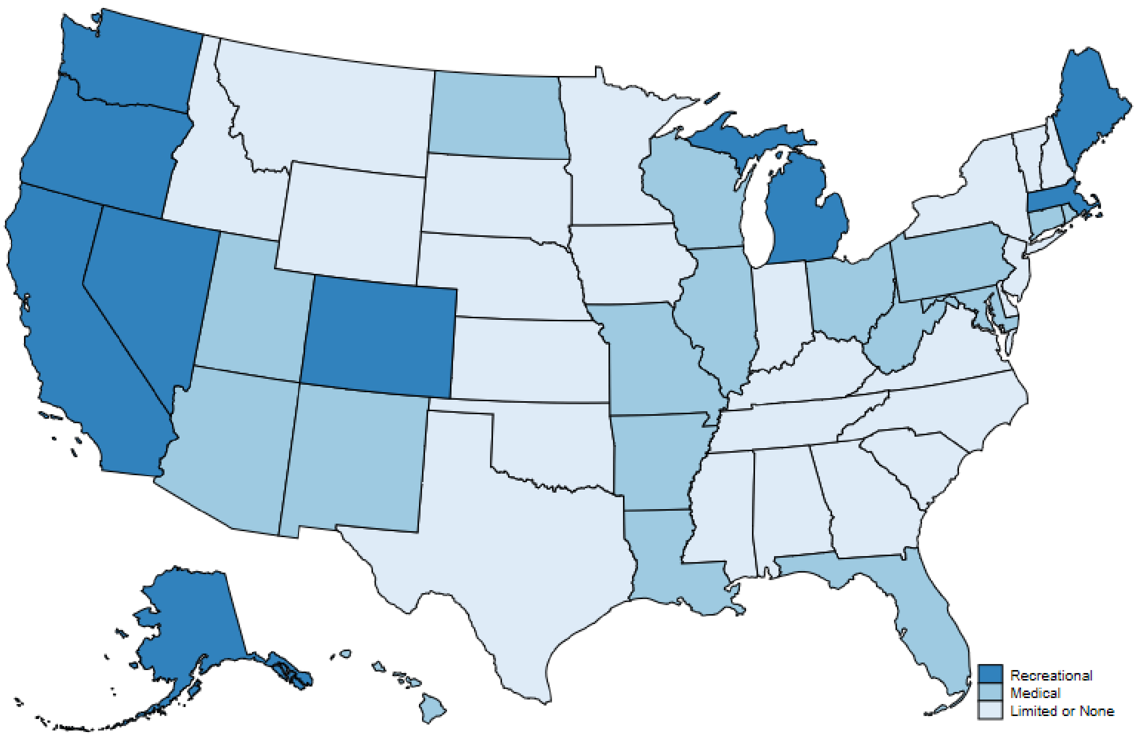


Figure 3.1: Legal Status of Marijuana Across States

Note: Figure B.1 lists the date of legalization for all states, as of 2018.

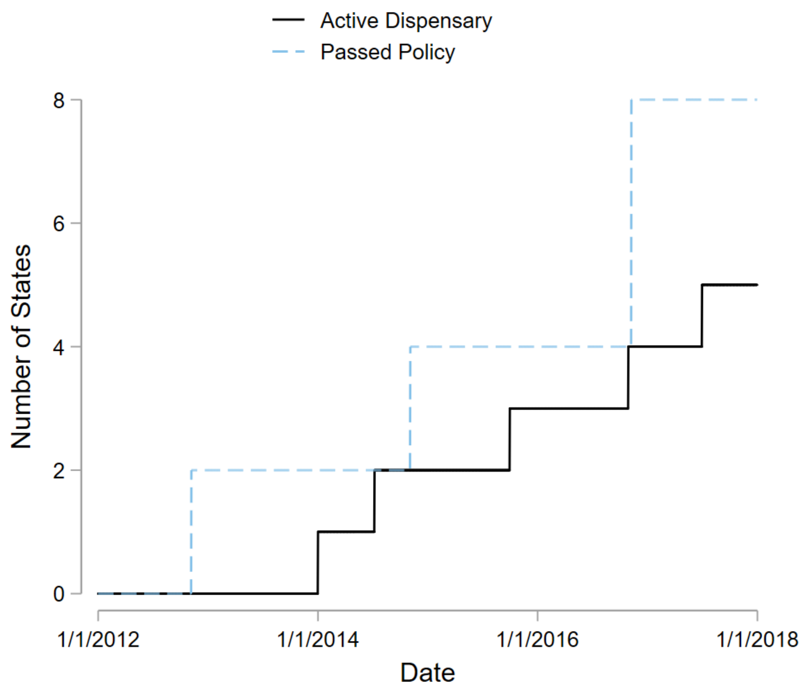


Figure 3.2: Legality of and Access to Recreational Marijuana Over Time

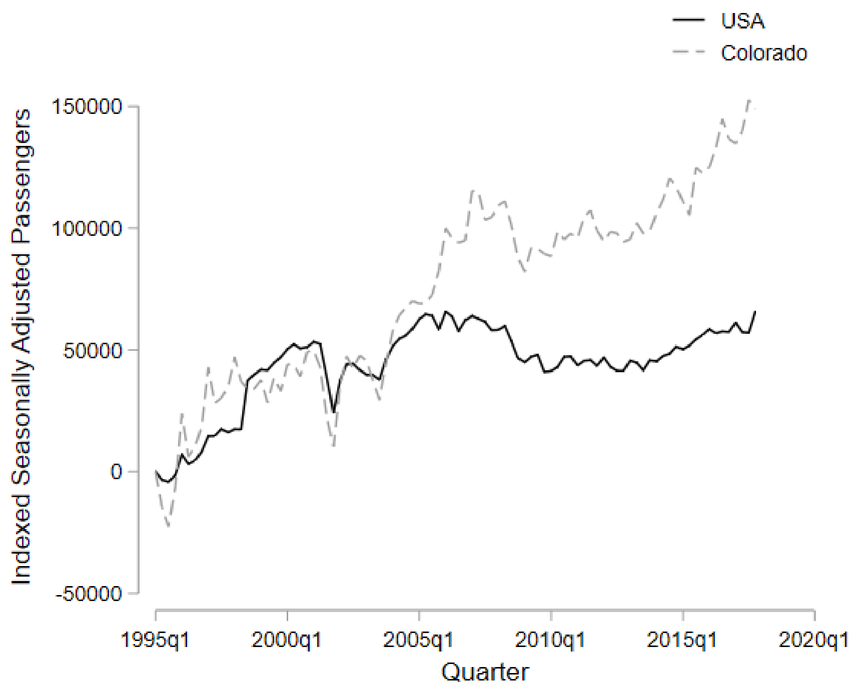
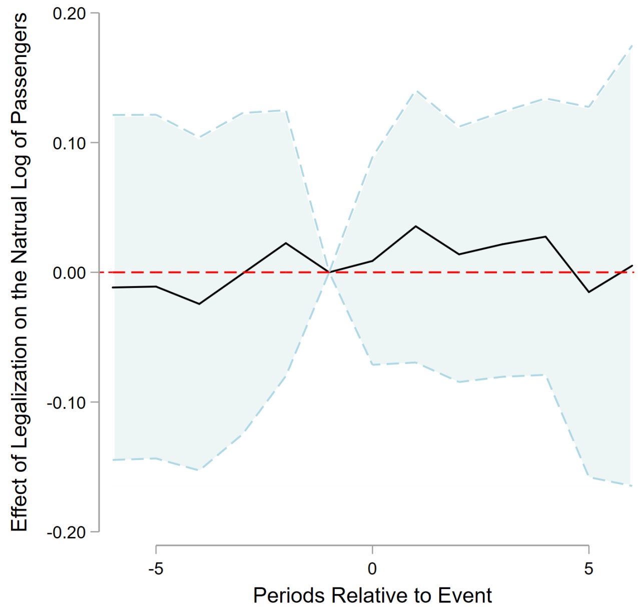
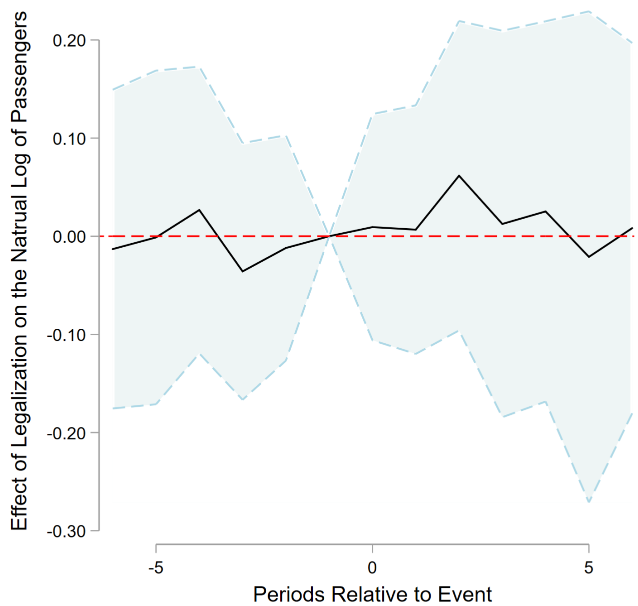


Figure 3.3: Comparison of Colorado and average of non-treated U.S. states, indexed such that each graph starts at zero.

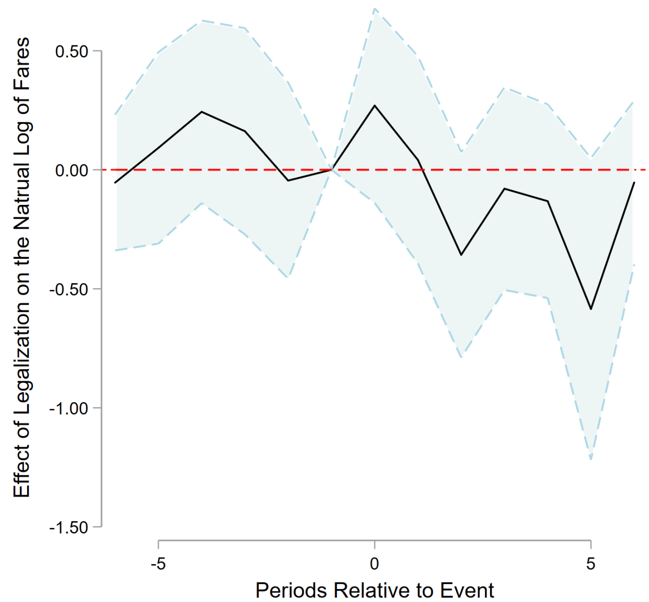


(a) Date of Policy Passage

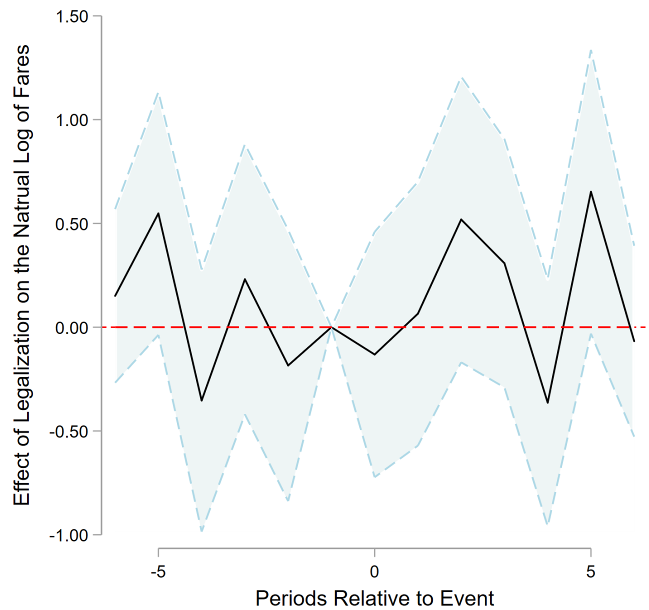


(b) Date of Availability

Figure 3.4: Effects of Recreational Marijuana Legalization on ln(Passengers)



(a) Uses Date of Policy Passage



(b) Uses Date of Availability

Figure 3.5: Effects of Recreational Marijuana Legalization on ln(Average Market Fare)

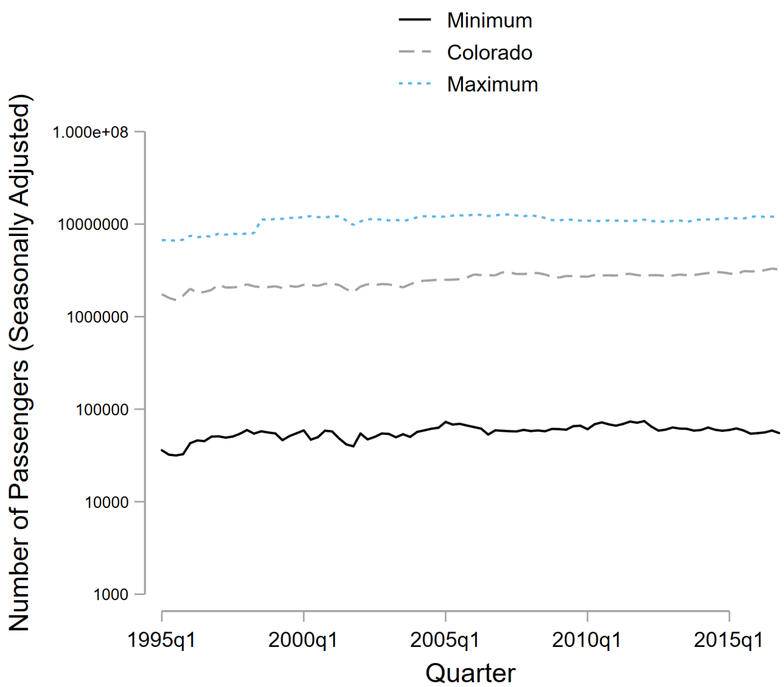
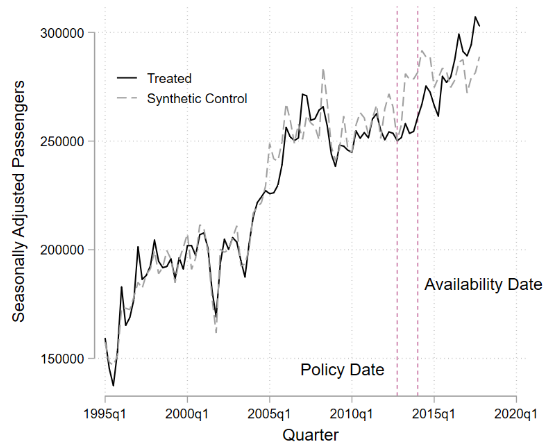
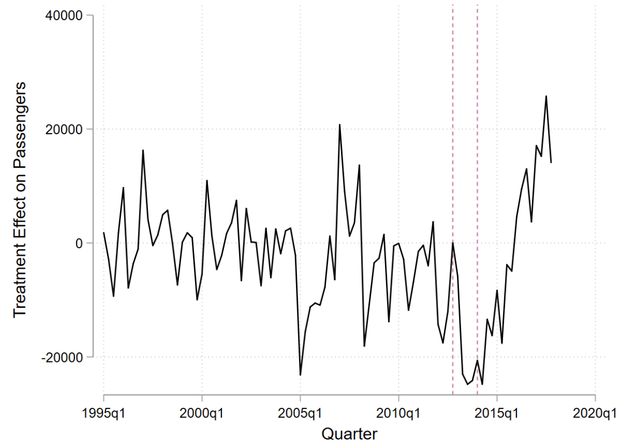


Figure 3.6: Seasonally adjusted passengers in Colorado as well as the max and min values for donor pool states

y-axis is on a logarithmic scale.



(a) Colorado vs Synthetic Colorado



(b) Colorado vs. Synthetic Colorado Gap

Figure 3.7: Effects of Recreational Marijuana Legalization on Passengers

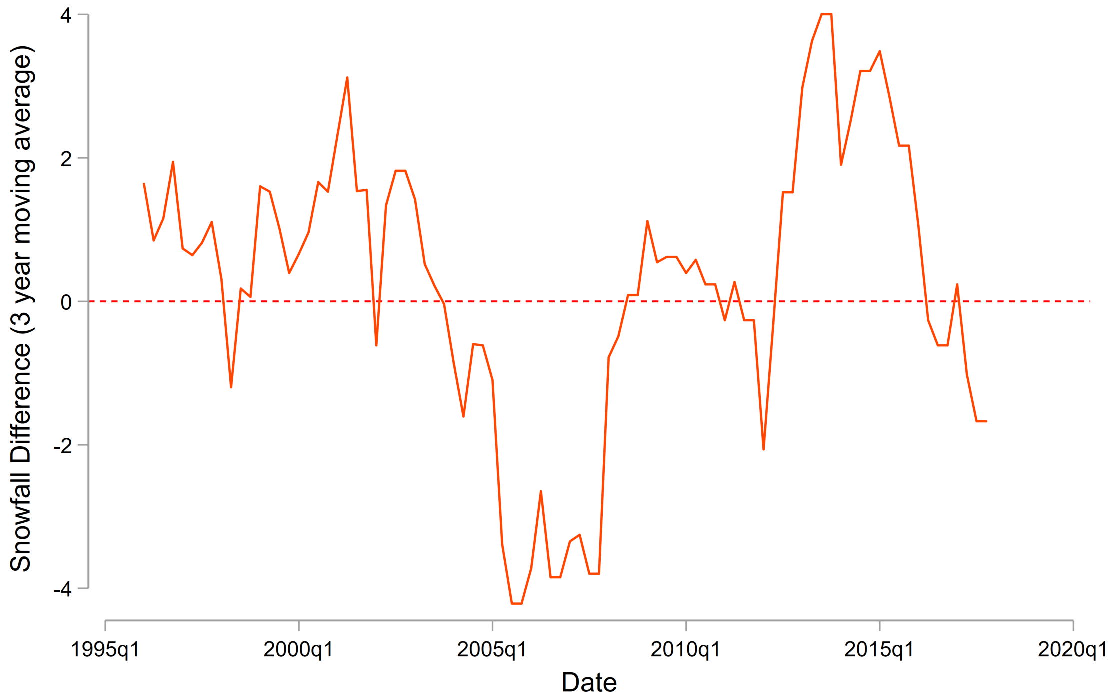
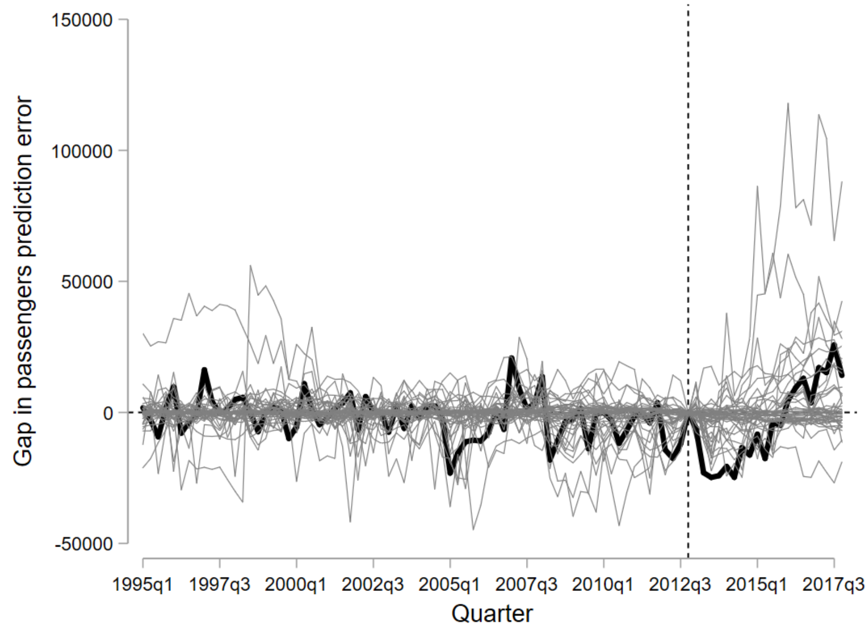
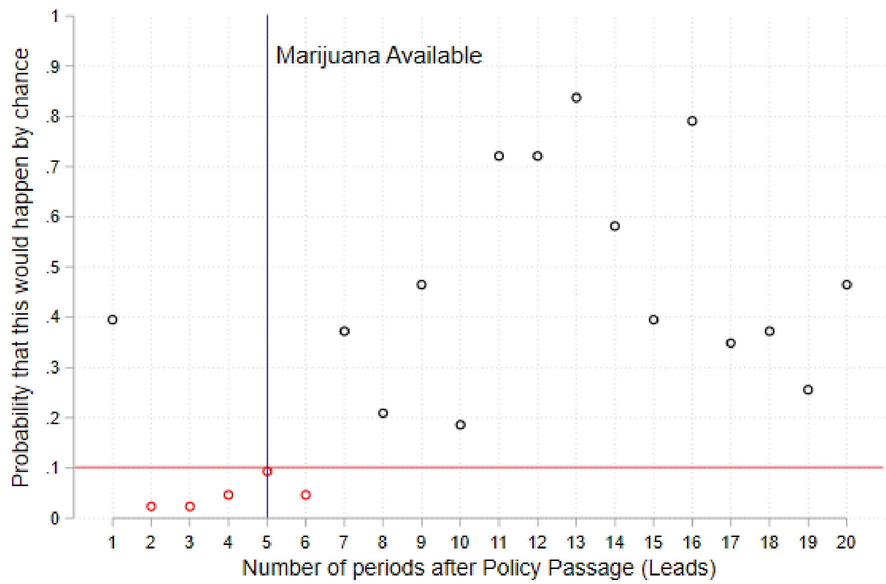


Figure 3.8: Three-Year Moving Average of Snowfall Disparity from Overall Mean



(a) Placebo Tests

This figure graphs the gap between actual and synthetic states. The dark black line represents Colorado. Grey lines represent the analysis run on all donor states. The dotted line represents treatment.



(b) Colorado vs. Synthetic Colorado pseudo p-values

Figure 3.9: Colorado vs. Synthetic Colorado pseudo p-values, Passengers

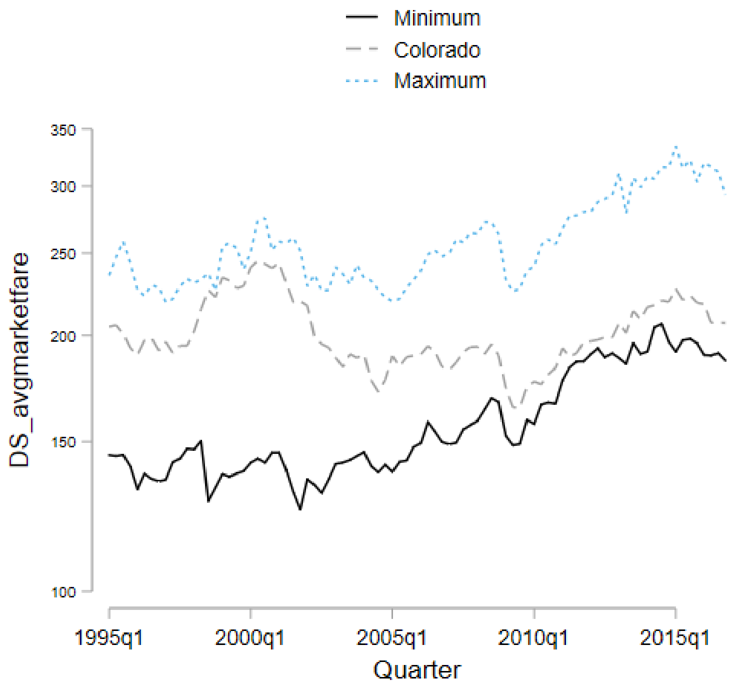
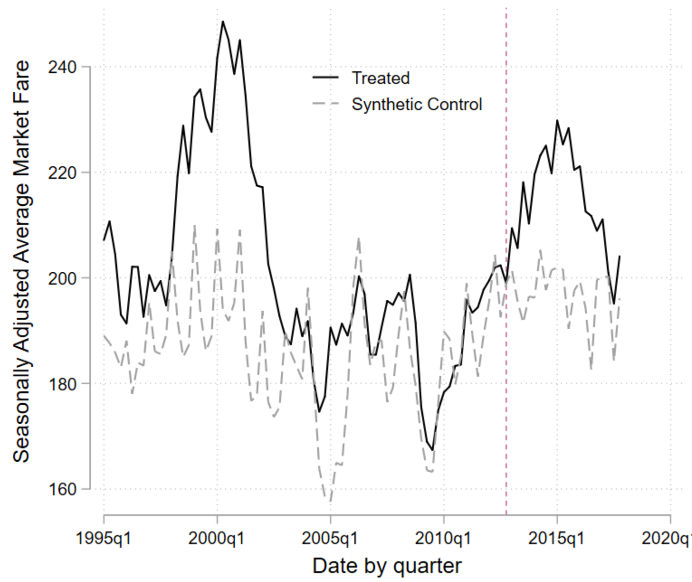
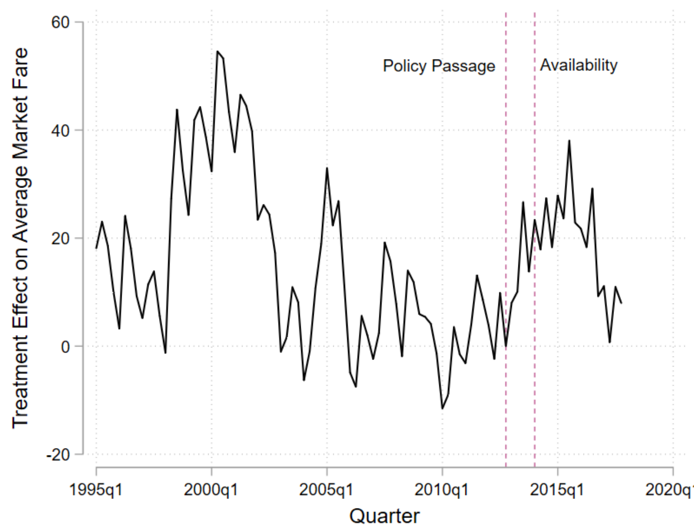


Figure 3.10: Seasonally adjusted fares in Colorado as well as the max and min values for donor pool states

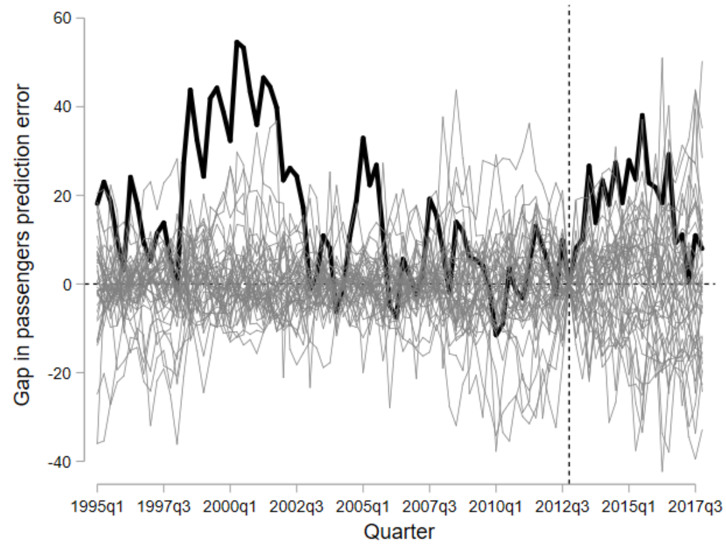


(a) Colorado vs Synthetic Colorado



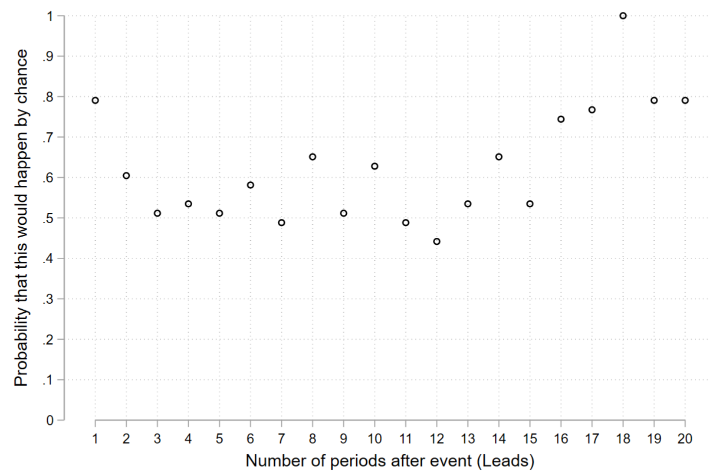
(b) Colorado vs. Synthetic Colorado Gap

Figure 3.11: Effects of Recreational Marijuana Legalization on Average Market Fare



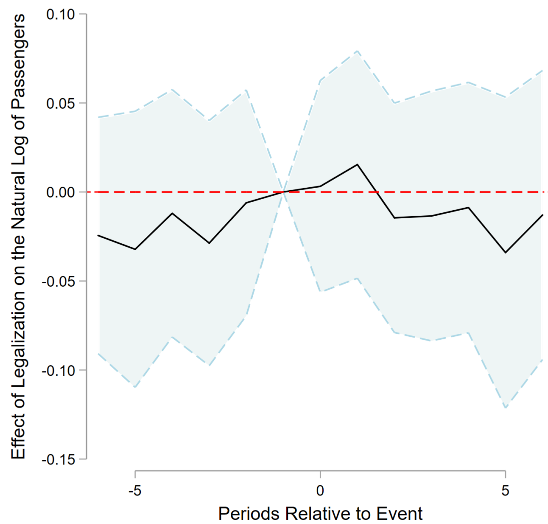
(a) Placebo Tests

Bold represents treated state.

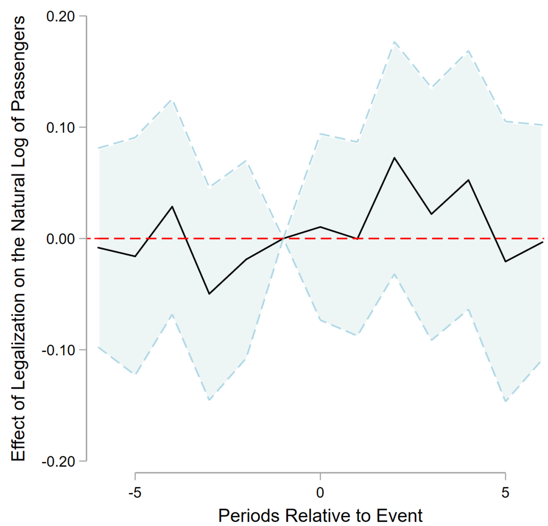


(b) Colorado vs. Synthetic Colorado pseudo p-values

Figure 3.12: Colorado vs. Synthetic Colorado pseudo p-values, Fares

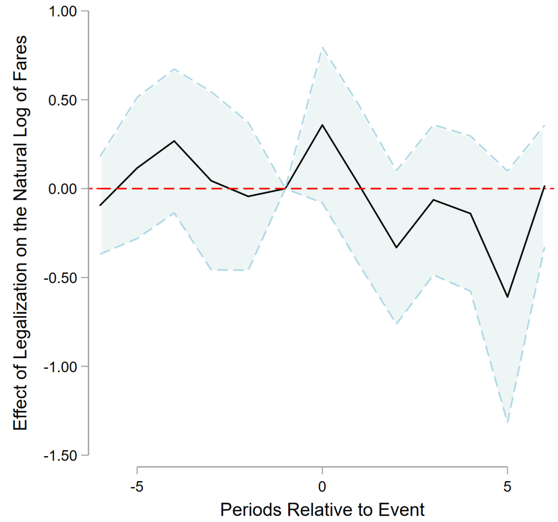


(a) Uses Date of Policy Passage

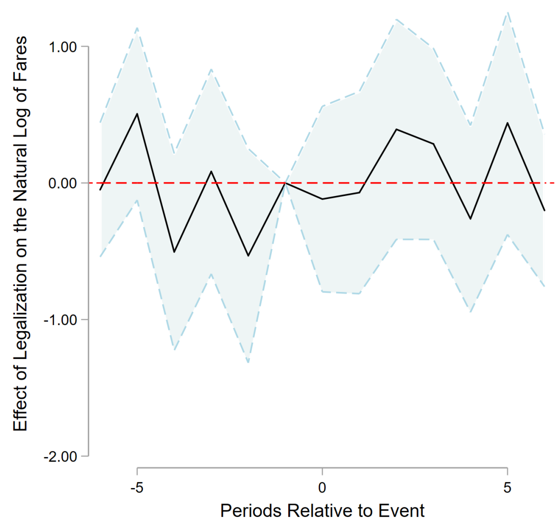


(b) Uses Date of Availability

Figure 3.13: Effect of Recreational Marijuana Legalization on Passengers, Weighted by Distance

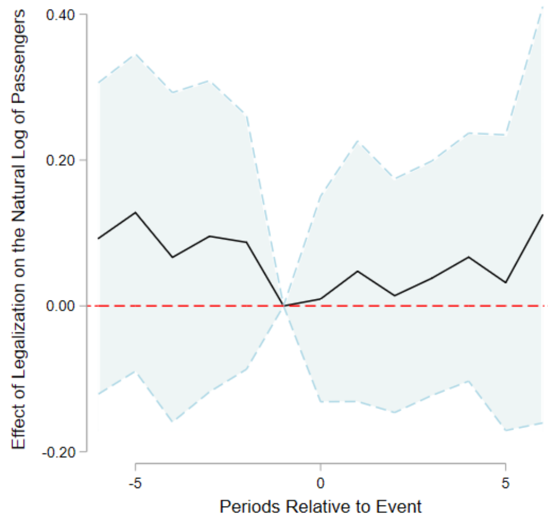


(a) Uses Date of Policy Passage

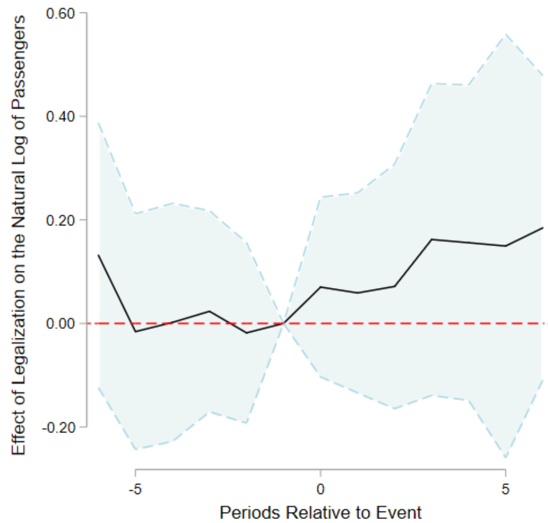


(b) Uses Date of Availability

Figure 3.14: Effect of Marijuana Legalization on Average Market Fare, Weighted by Distance



(a) Uses Date of Policy Passage



(b) Uses Date of Availability

Figure 3.15: Effect of Recreational Marijuana Legalization on Passengers, Weighted by Arrest Rate

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Appendix A

Same-Sex Marriage

Table A.1: Same-Sex Marriage Legalization and Ban Dates

State	Initial passage	Enactment	Implementation Type	Ban Date
Alabama	01/23/2015	02/09/2015	Court Order	06/06/2006
Alaska	10/12/2014	10/17/2014	Court Order	11/03/1998
Arizona	10/17/2014	10/17/2014	Court Order	11/04/2008
Arkansas	05/09/2014	06/26/2015	Court Order	11/02/2004
California	09/06/2005	06/26/2013	Court Order	11/04/2008
Colorado	07/09/2014	10/07/2014	Court Order	11/07/2006
Connecticut	10/10/2008	11/12/2008	Court Order	
Delaware	05/07/2013	07/01/2013	Legislation	
District of Columbia	12/15/2009	03/09/2010	Legislation	
Florida	08/21/2014	01/06/2015	Court Order	11/04/2008
Georgia	06/26/2015	06/26/2015	Court Order	11/02/2004
Hawaii	11/13/2013	12/02/2013	Legislation	09/10/1996
Idaho	05/13/2014	10/15/2014	Court Order	02/06/2006
Illinois	11/20/2013	06/01/2014	Legislation	06/01/1997
Indiana	06/25/2014	10/06/2014	Court Order	06/07/2001
Iowa	04/03/2009	04/27/2009	Court Order	
Kansas	11/04/2014	06/26/2015	Court Order	04/05/2005
Kentucky	02/12/2014	06/26/2015	Court Order	11/01/2004
Louisiana	06/26/2015	06/26/2015	Court Order	09/18/2004
Maine	05/06/2009	11/06/2012	Voter	
Maryland	02/23/2012	01/01/2013	Legislation	01/01/1973
Massachusetts	05/17/2004	05/17/2004	Court Order	
Michigan	03/21/2014	06/26/2015	Court Order	11/02/2004
Minnesota	05/14/2013	08/01/2013	Legislation	
Mississippi	06/26/2015	06/26/2015	Court Order	11/02/2004
Missouri	06/26/2015	06/26/2015	Court Order	08/03/2004
Montana	11/19/2014	11/19/2014	Court Order	11/01/2004
Nebraska	03/02/2015	06/26/2015	Court Order	11/02/2000
Nevada	10/07/2014	10/9/2014	Court Order	11/02/2002
New Hampshire	05/06/2009	01/01/2010	Legislation	
New Jersey	09/27/2013	10/21/2013	Court Order	
New Mexico	12/19/2013	12/19/2013	Court Order	
New York	06/24/2011	06/24/2011	Legislation	
North Carolina	07/28/2014	10/10/2014	Court Order	05/08/2012
North Dakota	06/26/2015	06/26/2015	Court Order	11/02/2004
Ohio	06/26/2015	06/26/2015	Court Order	11/02/2004
Oklahoma	01/14/2014	10/06/2014	Court Order	11/02/2004
Oregon	05/19/2014	05/19/2014	Court Order	11/02/2004
Pennsylvania	05/20/2014	05/20/2014	Court Order	
Rhode Island	05/02/2013	08/01/2013	Legislation	
South Carolina	07/28/2014	11/20/2014	Court Order	11/07/2006
South Dakota	01/12/2015	06/26/2015	Court Order	11/01/2006
Tennessee	06/26/2015	06/26/2015	Court Order	11/07/2006
Texas	02/26/2014	06/26/2015	Court Order	11/05/2005
Utah	12/20/2013	10/06/2014	Court Order	11/01/2004
Vermont	04/07/2009	09/01/2009	Legislation	
Virginia	02/13/2014	10/06/2014	Court Order	11/07/2006
Washington	02/08/2012	02/13/2012	Legislation	
West Virginia	07/28/2014	10/09/2014	Court Order	
Wisconsin	06/06/2014	10/06/2014	Court Order	11/07/2006
Wyoming	10/17/2014	10/21/2014	Court Order	05/30/2004

Table A.2: The Effect of a Same-Sex Marriage Legalization Announcement on the LGBT Hate-Crimes, Alternative Models

	(1)	(2)	(3)	(4)
	LGBT Hate-Crime Rate		Any LGBT Hate-Crime	
	Poisson Model	Negative Binomial Model	Logit Model	Probit Model
After Legalization Announcement	-0.286	-0.300***	-0.067***	-0.068***
	(0.000)	(0.099)	(0.022)	(0.022)
Observations	17,527	17,527	15,051	15,051

Standard errors in parentheses.

Standard errors are robust and clustered at the state level.

* p<0.10, ** p<0.05, *** p<0.01

Note: Regressions include quarter and county fixed effects. Regressions also include county-level demographic controls, state- and county-level socio-political controls, county-level economic controls, and other LGBT policy controls.

Table A.3: The Effect of a Same-Sex Marriage Legalization Announcement on the LGBT Hate-Crime Rate with State Trends

	(1) LGBT Hate-Crime Rate	(2) Violent LGBT Hate-Crime Rate	(3) LGBT Property Hate-Crime Rate
After Legalization Announcement	-0.074*	-0.046	-0.029
	(0.037)	(0.030)	(0.024)
County-Level Demographic Controls	Yes	Yes	Yes
State- & County-Level Socio-Political Controls	Yes	Yes	Yes
County Level Economic Controls	Yes	Yes	Yes
Other LGBT Policy Controls	Yes	Yes	Yes
R-Squared	0.010	0.012	0.006
Observations	17,522	17,522	17,522

Standard errors in parentheses.

Standard errors are robust and clustered at the state level.

OLS estimates.

* $p<0.10$, ** $p<0.05$, *** $p<0.01$

Note: Regressions include quarter and county fixed effects, state year trends, county-level demographic controls, state- and county-level socio-political controls, county-level economic controls, and other LGBT policy controls.

Table A.4: The Effect of a Same-Sex Marriage Legalization Enactment on the LGBT Hate-Crime Rate

	(1) LGBT Hate-Crime Rate	(2) Violent LGBT Hate-Crime Rate	(3) LGBT Property Hate-Crime Rate
After Legalization	-0.070	-0.040	-0.030
Announcement	(0.044)	(0.032)	(0.023)
R-Squared	0.007	0.008	0.004
Observations	17,522	17,522	17,522

Standard errors in parentheses.

Standard errors are robust and clustered at the state level.

OLS estimates.

* p<0.10, ** p<0.05, *** p<0.01

Note: Regressions include quarter and county fixed effects, county-level demographic controls, state- and county-level socio-political controls, county-level economic controls, and other LGBT policy controls.

Table A.5: The Effect of Same-Sex Marriage Legalization on the Likelihood of an LGBT Hate-Crime Occurring In Areas With Likely Perpetrators: A Heterogeneity Test.

	(1)	(2)	(3)
	Any LGBT Hate-Crime	Any Violent LGBT Hate-Crime	Any LGBT Property Hate-Crime
After Legalization Announcement	-0.066***	-0.064***	-0.015
	(0.020)	(0.020)	(0.022)
× County Has a Large % of Young White Males & a High Citizen Conservative Measure	-0.089**	-0.073*	-0.032
	(0.038)	(0.036)	(0.025)
× County Has a Large % of Same-Sex Households	0.046	0.046	-0.008
	(0.029)	(0.031)	(0.022)
Effect for Counties with a Large % of Young White Males & a High Citizen Conservative Measure	-0.155**	-0.137**	-0.047
P-Value	0.001	0.002	0.210
R-Squared	0.010	0.009	0.006
Observations	17,473	17,473	17,473

Standard errors in parentheses.
Standard errors are robust and clustered at the state level.
OLS estimates.
* p<0.10, ** p<0.05, *** p<0.01

Note: Regressions include quarter and county fixed effects, county-level demographic controls, state- and county-level socio-political controls, county-level crime & economic controls, and other LGBT policy controls.

Table A.6: The Effect of a Same-Sex Marriage Ban on the Likelihood of an LGBT Hate-Crime Occurring

	(1)	(2)	(3)	(4)
	Any LGBT Hate-Crime		Any Violent LGBT Hate-Crime	Any LGBT Property Hate-Crime
After Ban	0.013	-0.000	0.009	-0.006
	(0.022)	(0.021)	(0.019)	(0.011)
× States Never Banned Before 2000		0.085*	0.076	0.019
		(0.050)	(0.052)	(0.021)
Effect for States w/ Ban Conditional on no Prior Ban		0.084*	0.085*	0.013
P-Value		0.069	0.094	0.519
R-Squared	0.009	0.009	0.008	0.006
Observations	17,522	17,522	17,522	17,522

Standard errors in parentheses.

Standard errors are robust and clustered at the state level.

OLS estimates.

* p<0.10, ** p<0.05, *** p<0.01

Note: Regressions include quarter and county fixed effects. Regressions also include county-level demographic controls, state- and county-level socio-political controls, county-level economic controls, and other LGBT policy controls.

Table A.7: Summary Statistics for Reporter and Never-Reporter Counties

	Reporters		Never-Reporters	
	Mean	Standard Deviation	Mean	Standard Deviation
Outcomes				
LGBT Hate-Crime per 100,000	0.37	1.45	.	.
Violent LGBT Hate-Crime per 100,000	0.25	1.14	.	.
Property LGBT Hate-Crime per 100,000	0.12	0.90	.	.
Any LGBT Hate-Crime	0.30	0.46	.	.
Any Violent LGBT Hate-Crime	0.23	0.42	.	.
Any Property LGBT Hate-Crime	0.11	0.32	.	.
County-Level Demographic Controls				
% of Households Same-Sex	0.01	0.02	0.00	0.01
Total Population	310,804	464,349	48,493	116,044
% Black	0.10	0.12	0.09	0.15
% Hispanic	0.08	0.10	0.07	0.13
% Male	0.49	0.01	0.50	0.02
% Young Adults (ages 15-34)	0.27	0.05	0.25	0.04
% Middle-Aged Adults (ages 35-54)	0.28	0.03	0.28	0.03
% Older Adults (ages 55-64)	0.11	0.02	0.12	0.02
% Senior Adults (ages 65 and up)	0.14	0.04	0.16	0.04
Urbanization Rate	0.54	0.40	0.11	0.27
% Frequent Religious Service Attendees	0.49	0.19	0.51	0.20
% HS Diploma, No Bachelors	0.60	0.08	0.64	0.07
% Bachelors or More	0.25	0.10	0.17	0.07
State- & County-Level Socio-Political Controls				
% Democratic Presidential Vote	0.46	0.12	0.39	0.13
Citizen Conservative State Measure	50.49	13.43	45.79	11.61
Government Conservative State Measure	44.52	15.11	41.83	14.65
County-Level Economic Controls				
Unemployment Rate	6.19	2.54	6.41	2.73
% in Poverty	13.54	5.17	15.83	6.43
Median Household Income	$47,355	$12,401	$40,103	$10,394
# of Counties	1,845		2,793	
# of Observations	21,795		152,239	

Appendix B

Marijuana Legalization

Table B.1: Passage Dates For Various Types of Legalization of Marijuana (Pass Dates)

State	Recreational	Medical	Partial
Alabama			4/1/2014
Alaska	11/4/2014		
Arizona		11/2/2010	
Arkansas		11/8/2016	
California	11/8/2016		
Colorado	11/6/2012	11/7/2000	
Connecticut		5/5/2012	6/7/2011
Delaware			5/13/2011
Florida		11/8/2016	3/20/2014
Georgia		2/14/1980	4/16/2015
Hawaii		6/15/2000	
Illinois		8/2/2013	
Indiana			4/26/2017
Iowa			1/9/2015
Louisiana		6/30/2015	
Maine	11/8/2016		11/3/2009
Maryland		5/2/2013	
Massachusetts	11/8/2016	11/6/2012	
Michigan		11/4/2008	
Minnesota			5/29/2014
Mississippi			7/1/2014
Missouri			7/14/2014
Nevada	11/8/2016	11/7/2000	
New Hampshire			7/23/2013
New Jersey			1/18/2010
New Mexico		4/2/2007	
New York			7/4/2014
North Carolina			7/17/2015
North Dakota		11/8/2016	
Ohio		6/6/2016	
Oklahoma			4/30/2015
Oregon	11/4/2014	7/8/2013	11/3/1998
Pennsylvania		4/17/2016	
Rhode Island		6/16/2009	
South Carolina			6/2/2014
Tennessee			5/4/2015
Texas			6/1/2015
Utah			3/25/2014
Vermont	1/4/2018		5/19/2004
Virginia			4/16/2017
Washington	11/6/2012		
West Virginia		4/19/2017	
Wisconsin		4/17/2017	
Washington D.C.		12/14/2009	

I only include laws that were effective at expanding marijuana access. For example, Kentucky is not listed here because, although the state passed a law in 2014 legalizing CBD oil, the law did not include legalization to grow or sell marijuana, thus the law had no practical effect Cheves 2015 Partial is a catch all for limited legalization. For example, legalization of CBD oil that can only treat certain conditions would fall under this category. If there was a change to these laws that made them effective whereas before they had not been, the most recent date is reported.

Alternate Model Regression Tables

Here I report regression tables analogous to those presented as event studies in Section 3.7. Tables B.2 and B.3 report the results of applying this weighting to Equation 3.2 for passengers and fares, respectively. Despite putting emphasis on longer flights, I see no substantial change to prior results from the baseline model. Tables B.4 and B.5 repeat this process weighting by arrest rates. As before, there is no substantial change to the estimated effects.

Table B.2: Effect of Recreational Marijuana Legalization on ln(Passengers) Using Availability Date as Treated Date. Weighted by distance.

	(1)	(2)	(3)
Effect (Avail. Date)	0.010	0.007	
	(0.034)	(0.033)	
Effect - Quarter 1			0.011
			(0.037)
Effect - Quarter 2			-0.007
			(0.043)
Effect - Quarter 3			0.022
			(0.042)
Effect - Quarter 4			0.002
			(0.038)
R^2	0.000	0.000	0.000
Effect (Passage Date)	0.005	0.009	
	(0.022)	(0.023)	
Effect - Quarter 1			0.019
			(0.025)
Effect - Quarter 2			-0.002
			(0.028)
Effect - Quarter 3			0.013
			(0.031)
Effect - Quarter 4			0.006
			(0.026)
Observations	747263	747263	747263
R^2	0.000	0.000	0.000
GDP		X	X
City Pair FEs	X	X	X

Standard errors in parentheses

Standard Errors are clustered at the city-pair level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table B.3: Effect of Recreational Marijuana Legalization on ln(Fares) Using Policy Passage Date as Treated Date. distance.

	(1)	(2)	(3)
Effect (Passage Date)	0.009	0.038	
	(0.082)	(0.085)	
Effect - Quarter 1			-0.036
			(0.175)
Effect - Quarter 2			-0.104
			(0.163)
Effect - Quarter 3			0.011
			(0.161)
Effect - Quarter 4			0.204*
			(0.122)
R^2	0.000	0.000	0.000
Effect (Avail. Date)	0.040	0.022	
	(0.121)	(0.121)	
Effect - Quarter 1			-0.025
			(0.254)
Effect - Quarter 2			-0.023
			(0.250)
Effect - Quarter 3			-0.033
			(0.213)
Effect - Quarter 4			0.141
			(0.169)
Observations	747263	747263	747263
R^2	0.000	0.000	0.000
GDP		X	X
City Pair FEs	X	X	X

Standard errors in parentheses

Standard Errors are clustered at the city-pair level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table B.4: Effect of Recreational Marijuana Legalization on ln(Passengers) Using Policy Passage Date as Treated Date, Weighted by Arrest Rate.

	(1)	(2)	(3)
Effect (Passage Date)	-0.024	-0.016	
	(0.072)	(0.073)	
Effect - Quarter 1			-0.037
			(0.071)
Effect - Quarter 2			-0.017
			(0.083)
Effect - Quarter 3			0.019
			(0.082)
Effect - Quarter 4			-0.025
			(0.080)
R^2	0.000	0.000	0.000
Effect (Avail. Date)	0.039	0.024	
	(0.105)	(0.106)	
Effect - Quarter 1			-0.004
			(0.101)
Effect - Quarter 2			0.042
			(0.125)
Effect - Quarter 3			0.060
			(0.111)
Effect - Quarter 4			-0.000
			(0.118)
Observations	473404	473404	473404
R^2	0.000	0.000	0.000
GDP		X	X
City Pair FEs	X	X	X

Standard errors in parentheses

Standard Errors are clustered at the city-pair level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table B.5: Effect of Recreational Marijuana Legalization on ln(Fares) Using Policy Passage Date as Treated Date, Weighted by Arrest Rate.

	(1)	(2)	(3)
Effect (Passage Date)	-0.085	-0.064	
	(0.093)	(0.093)	
Effect - Quarter 1			-0.035
			(0.174)
Effect - Quarter 2			-0.195
			(0.184)
Effect - Quarter 3			-0.197
			(0.189)
Effect - Quarter 4			0.106
			(0.121)
Observations	473404	473404	473404
R^2	0.000	0.000	0.000
Effect (Avail. Date)	-0.020	-0.059	
	(0.127)	(0.127)	
Effect - Quarter 1			-0.010
			(0.214)
Effect - Quarter 2			-0.006
			(0.245)
Effect - Quarter 3			-0.283
			(0.245)
Effect - Quarter 4			0.065
			(0.172)
R^2	0.000	0.000	0.000
GDP		X	X
City Pair FEs	X	X	X

Standard errors in parentheses

Standard Errors are clustered at the city-pair level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Passenger counts and average fares are seasonally adjusted for the synthetic control model. The procedure I used is as follows:

$$\tilde{y}_{r,t,q} = \frac{(\frac{\sum_t y_{r,t,q}}{T})}{(\frac{\sum_q \sum_t y_{r,t,q}}{4T})} \tag{B.1}$$

This allows the seasonal adjustment to vary by route but not by year, thus increases in seasonal traffic would not be differenced out. The effect of this seasonal adjustment is seen in Figure B.1.

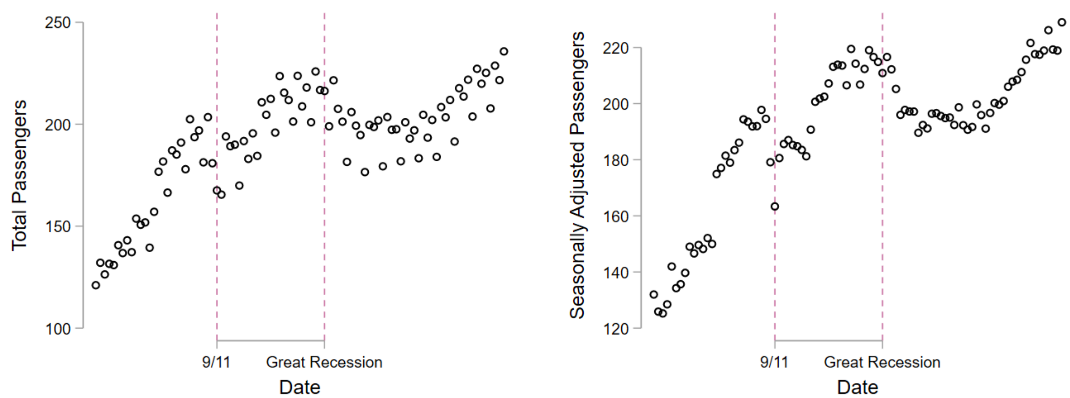

Figure B.1: Seasonal Adjustment of Total Passengers

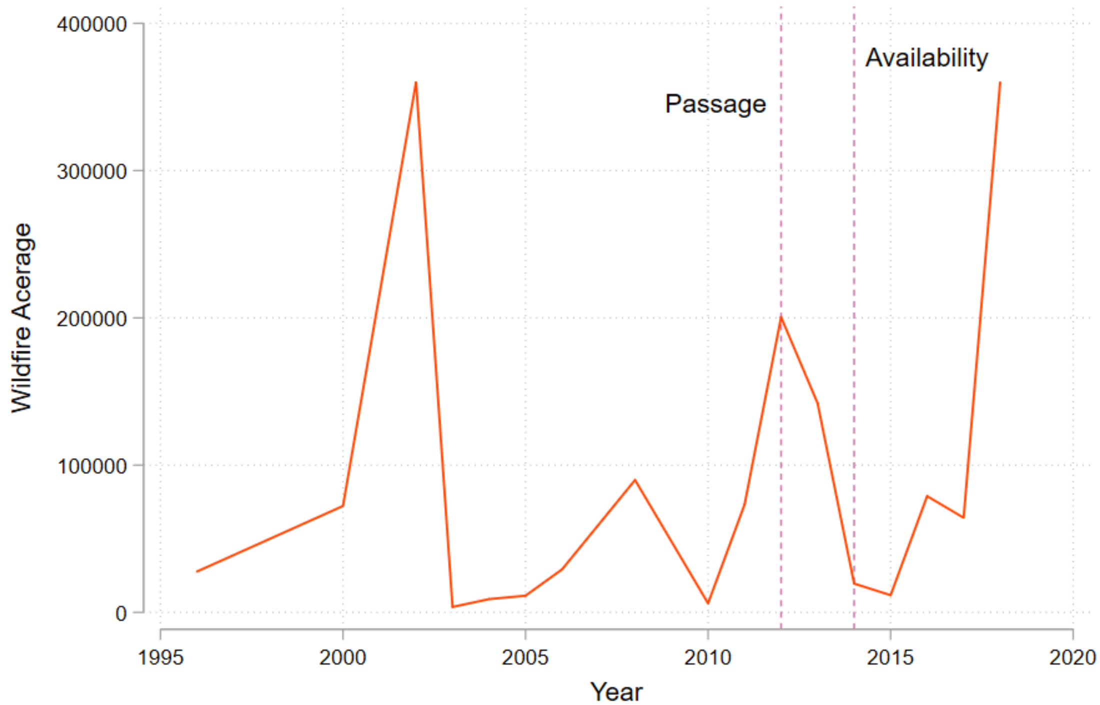

Figure B.2: Wildfires in Colorado