ESSAYS IN LABOR ECONOMICS

by

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A DISSERTATION

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DISSERTATION ABSTRACT

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This dissertation includes three essays in labor economics.

Youth Employment Opportunities and Crime: Criminal involvement has been shown to peak at a young age. While Becker's theory of the rational criminal is often referenced as a justification for increasing punishments and policing, his model also suggests that improving labor market options reduces criminality. For this reason, I estimate the impact of youth labor market opportunities on arrest rates. I instrument for shocks in local employment demand with national industry trends using a shift share approach. My estimates suggest that a 1 percent increase in labor market opportunities leads to a 1.08 percent decrease in arrests for 14-18-year-olds.

Information and the Persistence of the Gender Wage Gap; Early Evidence from California's Salary History Ban: Reductions in wage disparities across race and gender have stagnated in the recent decades. Recent popular focus on these inequalities has led to demands for policy interventions to reduce pay gaps. The most recent legislation intended to improve wage equality prohibits employers from asking about previously earned salaries. The intent of this legislation is to redress persistent pay inequalities. Salary history bans (SHBs) have been implemented in varying degrees (public and private) in multiple cities and states. I use a synthetic control approach to measure the impact of a statewide SHB in California. After the passing of a statewide SHB, statewide female-male earnings ratios increased from 0.77 (where they have been stagnant for the last 12 years) to 0.81. Moreover, I find these results are driven by an increase of the earnings ratio in male-dominated industries.

Marijuana Legalization and Violent Crime: Marijuana legalization has spread rapidly across the United States. Recently, after decades of decreases, violent crime rates have rebounded slightly in the United States. We test whether marijuana legalization has contributed to increased violence using a synthetic control design approach using the first two recreational marijuana adopters: Colorado and Washington. Using data from the Supplemental Homicide Reports and Uniform Crime and Reports, we largely find evidence that crime trends closely follow those predicted by synthetic control methods.

This dissertation includes previously unpublished co-authored material.

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CHAPTER I

YOUTH EMPLOYMENT OPPORTUNITIES AND CRIME

Introduction

The age profile of criminal activity peaks in late teenage years, then falls (Hirschi and Gottfredson, 1983; Steffensmeier and Ulmer, 2008). Decreasing juvenile criminal participation is therefore an important public policy objective. While criminal sentences are more severe for young adults than juveniles, criminal participation has been shown to drop only slightly across this age threshold due to harsher punishments (Lee and McCrary, 2005; McCrary and Lee, 2009). Juvenile incarceration has been shown to reduce the likelihood of high school completion and increase the likelihood of adult incarceration (Aizer and Doyle, 2015). This evidence suggests that the "stick" may not be the most effective tool for reducing crime, and may actually increase future crime. Becker's model suggests the "carrot" may also reduce incentives to engage in criminal behaviors by increasing the payoff to non-crime activities. This makes the steady decline in youth employment over the last few decades particularly concerning (Mixon Jr. and Stephenson, 2016). The employment-population ratio for 16-19-year-olds is at an all-time low and is expected to be even lower by 2024 (Morisi, 2017). Employment may be becoming more difficult for youth to attain (Goodman, 2008). According to Becker's model, Decreased opportunities in the labor market could increase the incentive to participate in the criminal market. This paper tests this potential mechanism for youth. Specifically, I look at the effects of changes in labor market opportunities on juvenile and young adult arrests.

Theoretically, whether youth employment results in more or less crime is unclear. First, I discuss the possible mechanisms through which employment could reduce crime, then I offer some ways in which employment could cause crime. Time spent working could simply incapacitate individuals from committing crime. Additionally, if youth are concerned about losing their job if caught committing crime, there might be a deterrence effect of employment on crime. On the other hand, employment could stimulate crime if the workplace represents the first time individuals come in contact with a cash register or if new employees learn illegal activities from existing employees or customers. The average juvenile does not have a job, which means working youth receive greater income than do their non-working peers. Additional income could be a catalyst for juvenile delinquency. Particularly, this income could be used to purchase alcohol, which has a well-established positive relationship with crime (Carpenter, 2005, 2007; Carpenter and Dobkin, 2015).

The paper proceeds as follows: Section 2.2 provides background on SHB policies. Section 3.3 describes the data. Section 2.4 presents my empirical methodology. Section 3.4 describes my results. Section 2.6 discusses and concludes.

Background

Since Becker's (1968) introduction of the theory of the rational criminal, economists have been testing the model's predictions empirically. The empirical literature that has developed can broadly be split into two categories, one of which tests the responsiveness of crime to changes in punishments or policing (the stick) (Levitt, 1995; Chiras and Crea, 2004; Evans and Owens, 2007; Corman and Mocan, 2005; Kessler and Levitt, 1999), and the other of which tests the responsiveness of crime to local labor market conditions (the carrot). Research analyzing the response of crime to labor market opportunities utilizes two distinct methods of measuring the opportunity cost of crime. One method looks at the responsiveness of crime to unemployment; the other looks at the responsiveness of crime to changes in wages. Wages are thought to be the legal opportunity cost of committing crime, while unemployment is a proxy for the opportunity cost of crime. Unemployment generates incentives to participate in criminal activity through the consumption smoothing motive. Additionally, being unemployed could trigger frustration and anger, which subsequently leads to violent behavior (Agnew, 1992).

In the empirical research examining the relationship between unemployment and crime, most studies find small positive effects for property crime and no effect for violent crime (Raphael and Rudolf, 2001; Fougere et al., 2009; Lin, 2008; Gronqvist, 2013). These empirical estimates are small and sensitive to the population and time period being studied, despite the clear theoretical predictions (Chalfin and McCrary, 2017).

In the body of research analyzing the effects of wages on crime, the effects are much larger and robust (Grogger, 1998; Doyle et al., 1999; Machin and Meghir, 2004). Within the wage literature, some studies consider only changes in the minimum wage and its effect on crime (Corman and Mocan, 2005; Hansen and Machin, 2002; Fernandez and Pepper, 2014). Most of these studies find a strong negative relationship between minimum wages and crime. Gould et al. (2002) span the two literatures by looking at wages and unemployment contemporaneously. They find higher wages and lower unemployment reduce property crime for male youths.

Employment measures like wages and unemployment are equilibrium observations, which means they occur at the intersection of labor supply and labor demand. Using these observed labor equilibria as an explanation for changes in crime confounds whether changes in crime that are attributed to changes in employment conditions are driven by shocks to labor supply or labor demand; a crucial question from a policy perspective. To disentangle the effect of shifts in demand, one can use use demand shifting events that affect only the demand side of the economy or an instrumental variable that is highly correlated with the employment measure but is only driven by shifts from the demand side of the economy. I create an instrument for shocks in labor demand, following the shift share method first used by Bartik (1991) and later by Katz and Murphy (1992); Blanchard et al. (1992). I construct estimated quarterly employment demand at the state level. I use predicted changes in employment demand to explain changes in crime. The panel analysis is similar to approaches used in research on employment conditions and crime. These studies often consider the level of unemployment, which is the number of people who are looking for a job but remain jobless. I exploit predicted changes in employment demand, which measures predicted changes in labor market opportunities, to isolate a causal impact on arrests.

Existing research considering youth employment opportunities and crime utilizes concentrated populations, for example Heller (2014) finds that teen employment does reduce crime in a randomized control trial among disadvantaged youth in Chicago. Gelber et al. (2014) find aligned results looking at summer employment lotteries in New York City. The job corps has also been show to be an effective way to decrease crime, though at a negative net benefit due to the cost of the program (Schochet et al., 2008). This paper analyzes systematic changes in employment opportunities for the entire U.S. population of employed youth over a 14-year time period.

Data

I use the Federal Bureau of Investigation's Uniform Criminal Reporting (UCR) monthly files, which report the number of men and women arrested by age, type of offense[1], and agency at the monthly level from 2000-2012[2]. Arrest data has both its benefits and drawbacks. Arrests may not be the best measure of criminal activity, because not all crime that occurs results in an arrest. Arrests are also highly dependent on the level of policing. However, Cook et al. (2014) suggest they provide a reasonably accurate measure of criminal activity. An advantage to using arrests instead of reports is that arrests, unlike reports, generate individual-specific information like sex, age, and race.

The UCR arrest files are voluntarily reported at the agency level. While these agencies voluntarily report crime data through the UCR program or directly to the FBI, between 88 and 96 percent of the U.S. population is covered by agencies that do report to the UCR (Maltz, 1999). Proper use of these data requires thorough cleaning. To ensure the arrest observations are as clean as possible, I plot agencies' shares of state arrests over time. This allows me to see how much each agency contributes to total state arrests for each time period. I drop agencies that have erratic reporting patterns, agencies that report only in month 12 of a year, and

[1] There are 29 offense categories and14 sub offense categories; this results in 43 offense classifications.

[2] These data are available for download from the NACJD.

agencies that drop out of voluntary reporting during the sample.[3] Arrest counts are aggregated to the quarterly level to match the employment data.

Quarterly Workforce Indicator (QWI) data are used for quarterly employment totals by state, industry[4], and age group. Stable counts of employment, which are measured as jobs that are held for the duration of the quarter, are my key employment variable. The QWI job counts are aggregated from employment data reported by firms to each state's Unemployment Insurance wage reporting system[5]. The Longitudinal Employer Household Dynamics (LEHD) program creates a longitudinal employment and earnings database with demographic characteristics by matching records from state unemployment insurance programs to Census Bureau data. These data are aggregated to the quarterly level to create the QWI.

Population data come from The National Cancer Institute's Surveillance, Epidemiology, and End Results Program (SEER). I obtain population estimates by state, age, sex, and year for the duration of my panel. The SEER data are a modification of the intercensal and vintage 2015 annual estimates produced by the US Census Bureau's Population Estimates Program. I aggregate these data to the

[3]I drop the following agencies from their respective states: Hoover and Mobile from Alabama; Arvada, Grand Junction and Greeley from Colorado; Boston from Massachusetts; Apple Valley, Eagan, Minneapolis, and St Paul from Minnesota; Nassau, and New York from New York; Columbus, Lima and Toledo from Ohio; and Seattle from Washington. Additionally, I drop Rhode Island before 2005, and Wisconsin before 2003. Washington DC and Illinois are dropped completely. These agencies are all dropped due to inconsistent reporting.

[4]The 20 industry categories include: (1) Agriculture, Forestry, Fishing and Hunting; (2) Mining, Quarrying, and Oil and Gas Extraction; (3) Utilities; (4) Construction; (5) Manufacturing; (6) Wholesale Trad; (7) Retail Trade; (8) Transportation and Warehousing; (9) Information; (10) Finance and Insurance; (11) Real Estate and Rental and Leasing; (12) Professional, Scientific, and Technical Services; (13) Management of Companies and Enterprises; (14) Administrative and Support and Waste Management and Remediation Services; (15) Educational Services; (16) Health Care and Social Assistance; (17) Arts, Entertainment, and Recreation; (18) Accommodation and Food Services; (19) Other Services (except Public Administration); (20) Public Administration.

[5]Consequently, these data do not include informal employment opportunities, which may be of importance for youth and young adults.

state level and group them by sex and age so they can be merged with the UCR and QWI data.

The number of sworn officers employed in each state-year is obtained from the UCR Law Enforcement Officer Killed in Action (LEOKA) files. Police employees are used as a proxy for the amount of policing in a particular state. My final sample consists of quarterly observations of arrests and employment for 46 states from 1998 to 2012 with two age-bins (14-18 and 19-21), and sex identifiers.

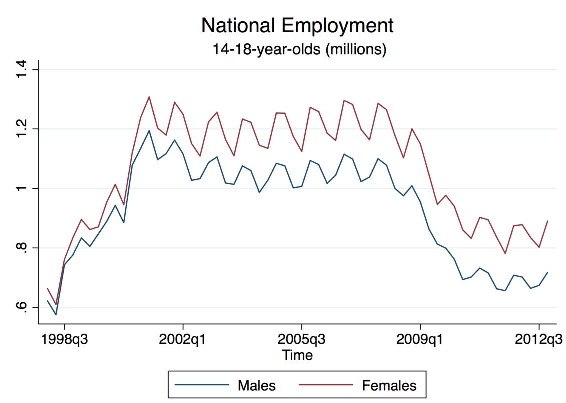
Figure 1 shows national employment levels and the corresponding growth rate for each age group and sex at the quarterly level for the duration of my sample. Figure 2 shows national arrest levels and the corresponding growth rate for each age group and sex at the quarterly level for the duration of my sample. Both arrests and employment are highly seasonal. Females are employed at slightly higher rates than males, but males dominate arrest counts for all age groups. Generally, employment growth is strong in the late '90s, flattens out from 2000 to late 2008, then decreases in 2008. These trends are not surprising as these data capture the transition from the dot-com boom to the great recession. Arrests follow a similar pattern, albeit with smaller magnitudes of growth and decline.

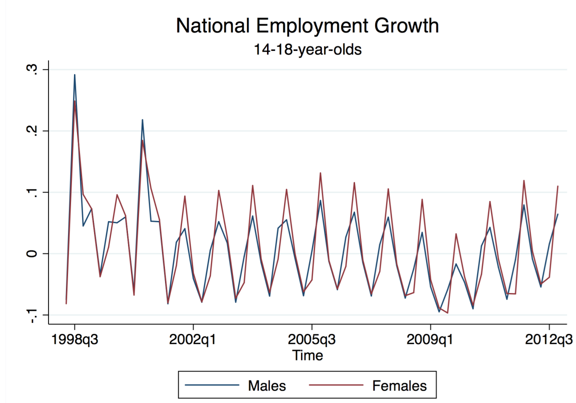
Methodology and Estimation

I create an estimate of predicted employment growth to determine how changes in the number of arrests can be explained by predicted changes in the employment level. Predicted employment growth is calculated by predicting the level of employment in the next time period, then calculating the growth rate from the actual level of employment in previous time periods. The next period's

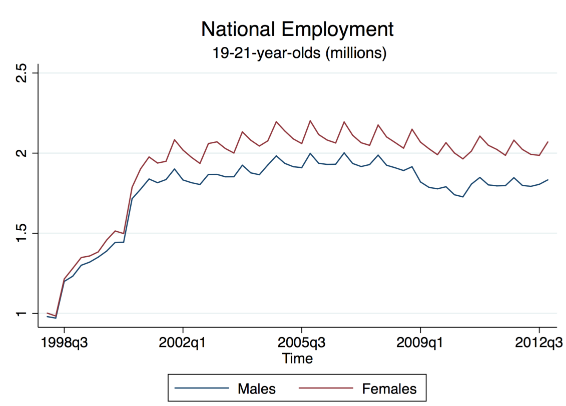
FIGURE 1.
Employment Trends

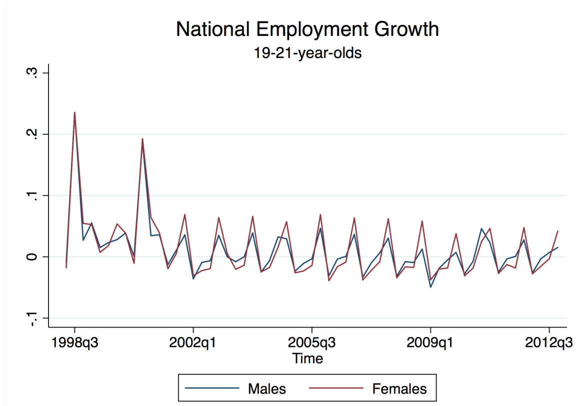
(a) (b)



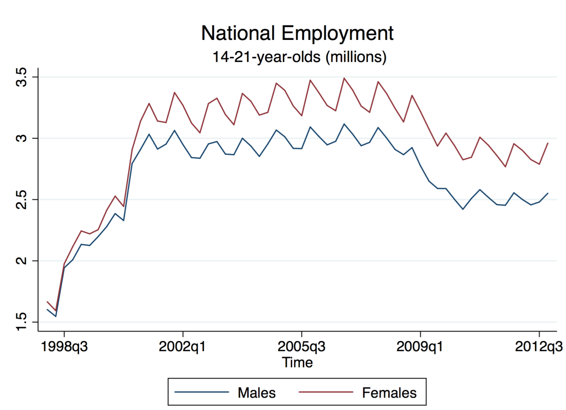


(c) (d)





(e) (f)



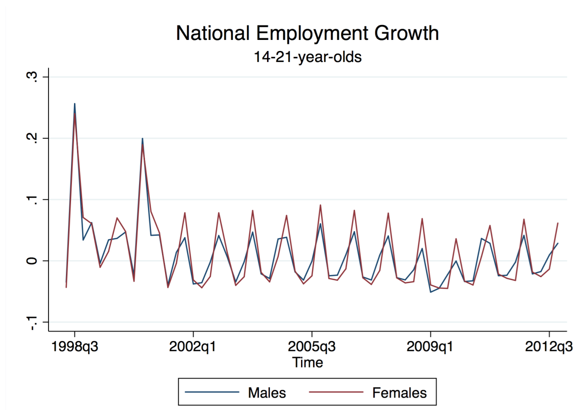
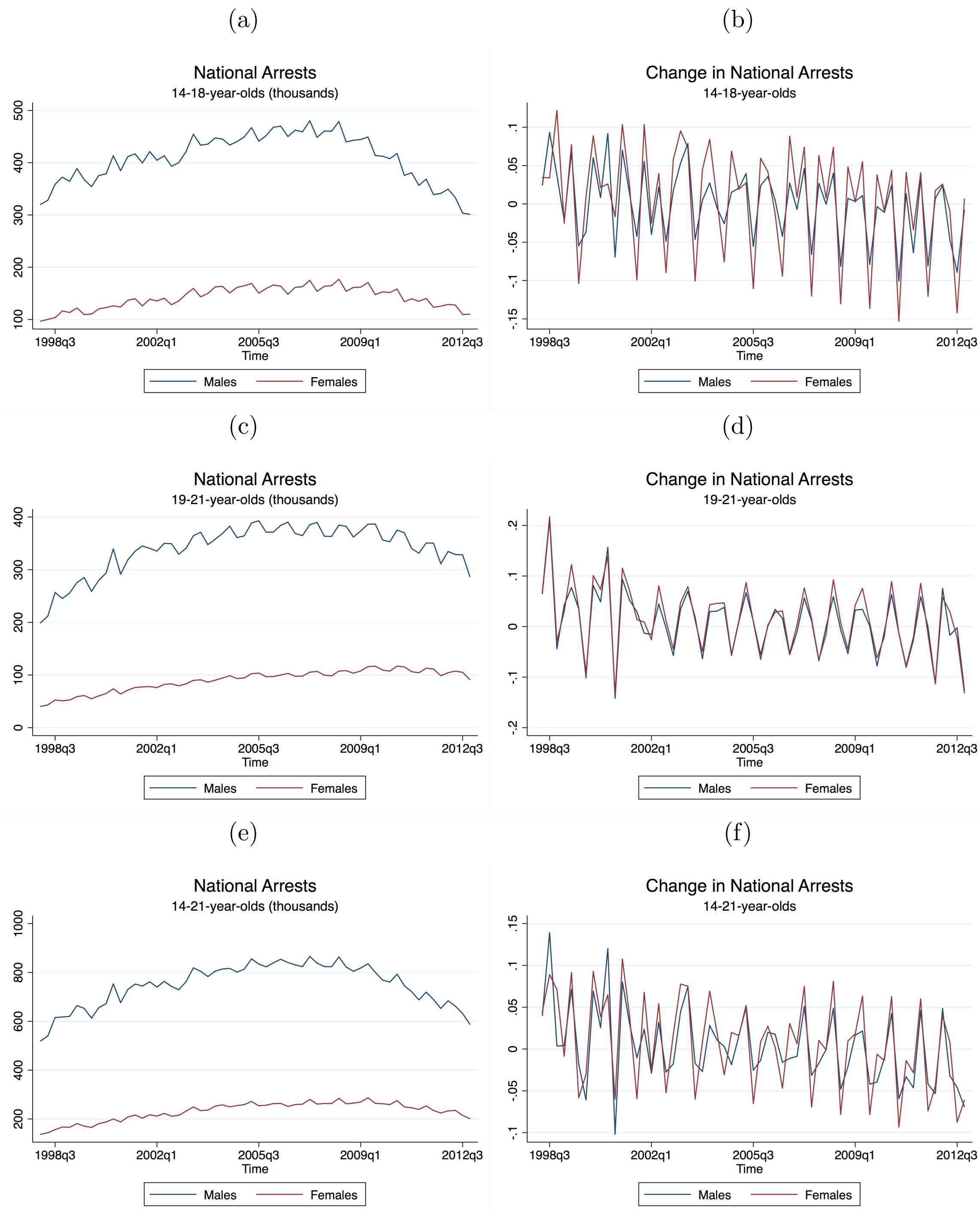


FIGURE 2.
Arrest Trends



employment for each state $\hat{L}_{st}$ is calculated as follows:

$$\hat{L}_{st} = \sum_i \left[\left(\frac{\text{US Emp in Ind } i \text{ at time } t}{\text{US Emp in Ind } i \text{ at time } t-1} \right) \times (\text{State } s \text{ Emp in Ind } i \text{ at } t-1) \right]$$

Where s indexes states and t indexes time by quarter. The predicted employment level for the next quarter relies on national industry-specific growth rates and state-industry composition. Predicted employment growth is then

$$\hat{g}_{st} = \frac{\hat{L}_{st} - L_{s,t-1}}{L_{s,t-1}},$$

which can be written as

$$\hat{g}_{st} = \frac{\sum_i \left[\overbrace{\left(\frac{L_{it} - L_{i,t-1}}{L_{i,t-1}} \right)}^{G_{it}} L_{si,t-1} \right]}{L_{s,t-1}} = \frac{\sum_i G_{it} L_{si,t-1}}{L_{s,t-1}}. \tag{1}$$

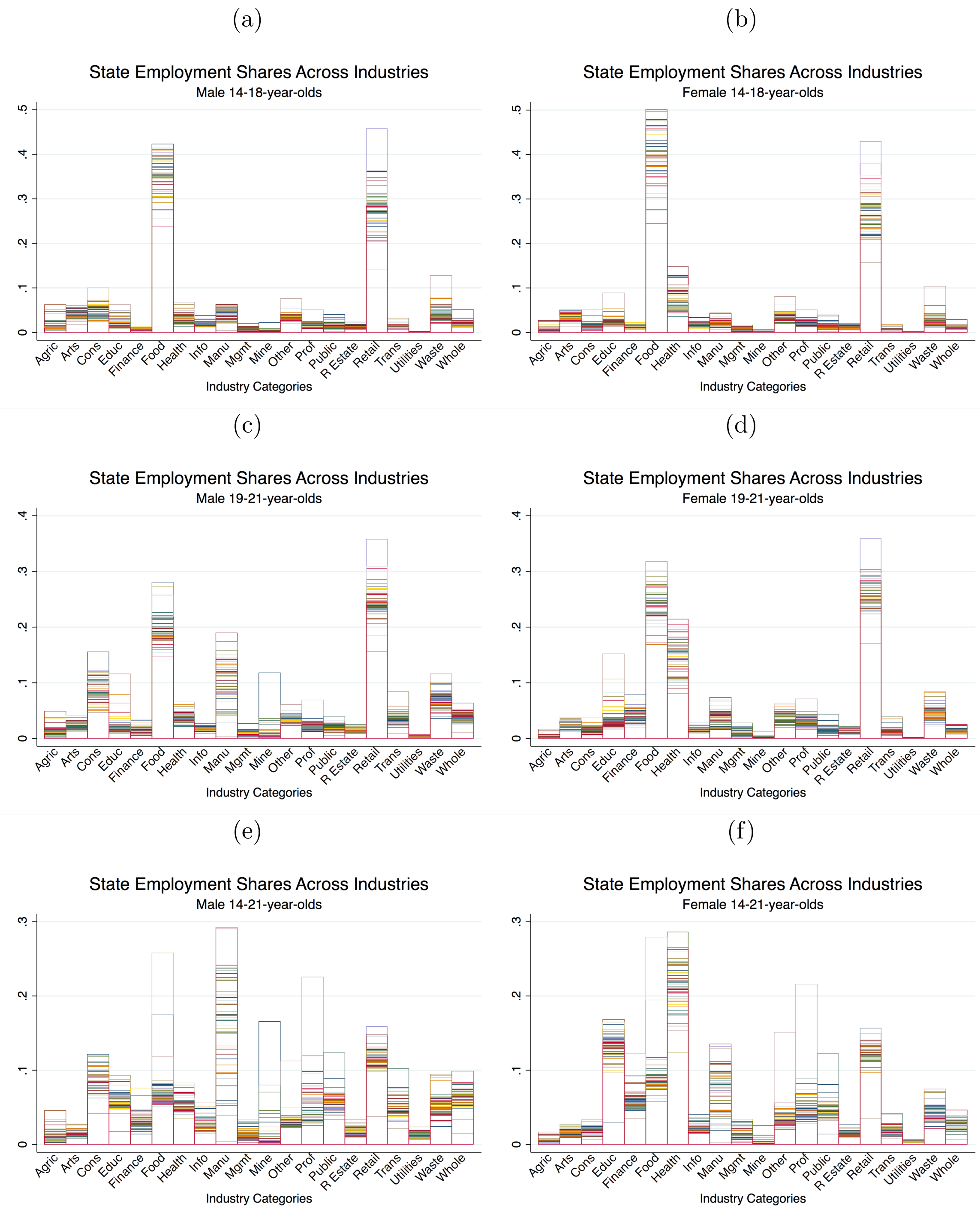
This allows us to see how the national industry-specific growth G_{it} interacts with the state-level industry composition to create predicted employment growth. Each state's specific industry sector is predicted to grow at the national rate. Blanchard et al. (1992) note that this predicted employment growth is a valid instrument if industry national growth rates are uncorrelated with state-level labor-supply shocks. This is true if there is no industry for which employment is concentrated in any state and there is sufficient variation in state-level industry composition. Figure 3 shows average industry shares for all 46 states for each age-sex group in my sample. Each bar represents a state's average industry composition over the 14-year period. Each column has 46 horizontal bars; each bar represents the share of total state employment in that particular industry. Each color represents a state; the shares across all industries for each state add to one. The retail sector and food-service sector dominate most states for youth and vary more than 20% in share of employment across states. Additionally, the maximum sector share is less than 50% for any particular state.

Figure 4 plots actual growth against instrumented growth for each state-quarter in the sample for each age-sex group, weighted by population. Actual growth plotted on the vertical axis is an equilibrium outcome, the change in employment due to changes in supply and demand. Estimated growth on the horizontal axis is growth due only to estimated changes on the demand side of the labor market. The slope coefficient from the regression of estimated growth on actual growth is reported near the bottom of each plot in Figure 4.

Generally, the clusters of large estimated growth in the right of the plots is made up of observations from 1998-2000, when both employment and arrests were increasing. Years before 2001 are excluded in Figure 5, and the slope coefficient from the regression of actual growth on estimated growth is around one for all age groups and sexes. From 2001 onward, estimated growth is an excellent predictor of actual growth. This is less true from 1998-2000, which may be due to the instrument's variance in high-growth periods. Figure 6 plots actual growth and estimated growth for every state over time side-by-side for each sex-age group. Figure 6 illustrates how each state's instrumented growth is similar to the national growth trend but different due to state-specific industry composition.

Table 1 reports regression results from estimated growth regressed on actual growth across various fixed-effect specifications. Columns 2-5 incrementally add time and location fixed effects. The exogeneity of the instrument requires industry national growth rates to be uncorrelated with state-level labor-supply shocks.

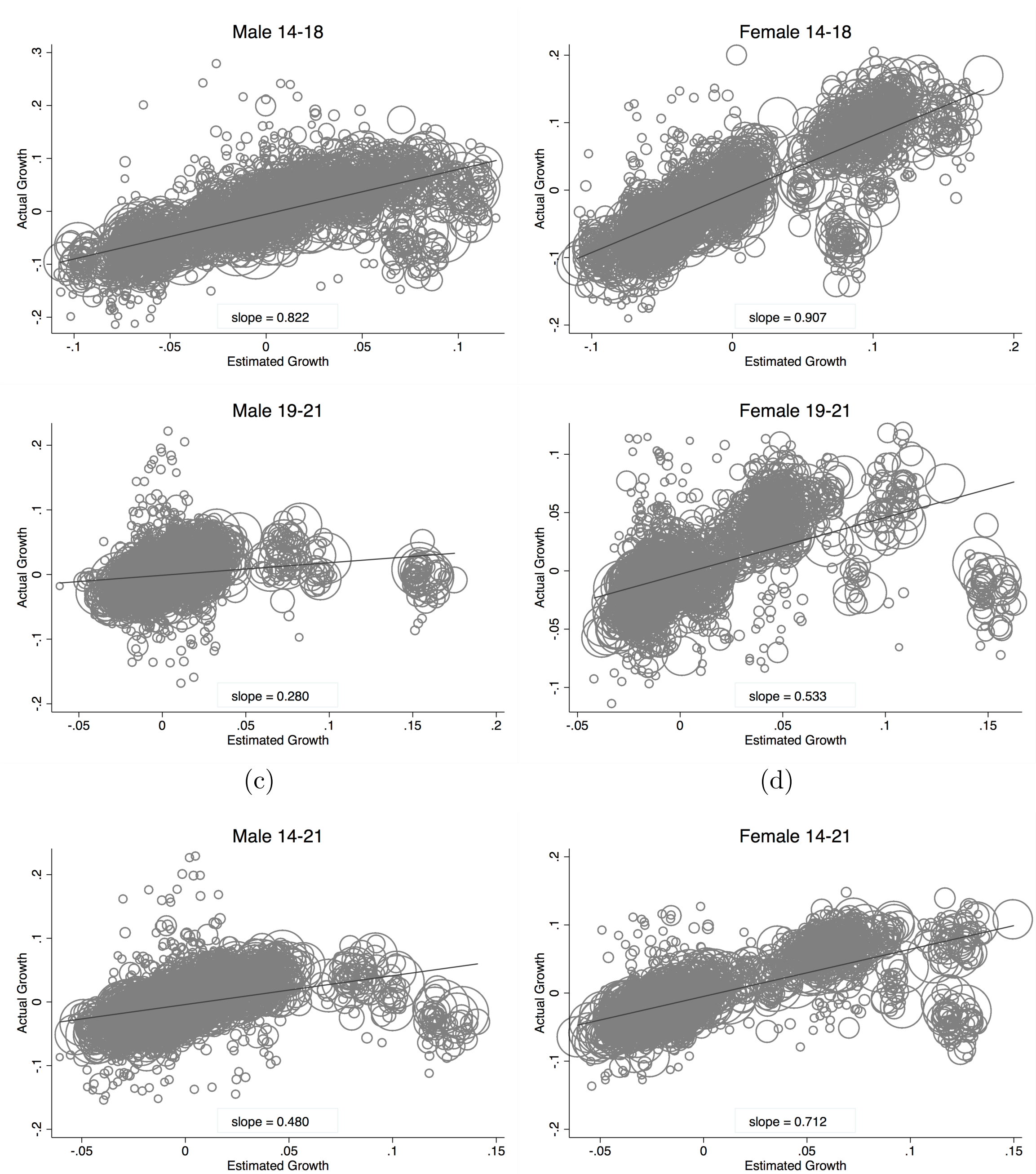
FIGURE 3.
Industry Composition by State



Notes: This figure illustrates the variation of industry composition across states. Each bar represents a state's industry-share of total employment. Each column has 46 bars, one representing each state in the sample.

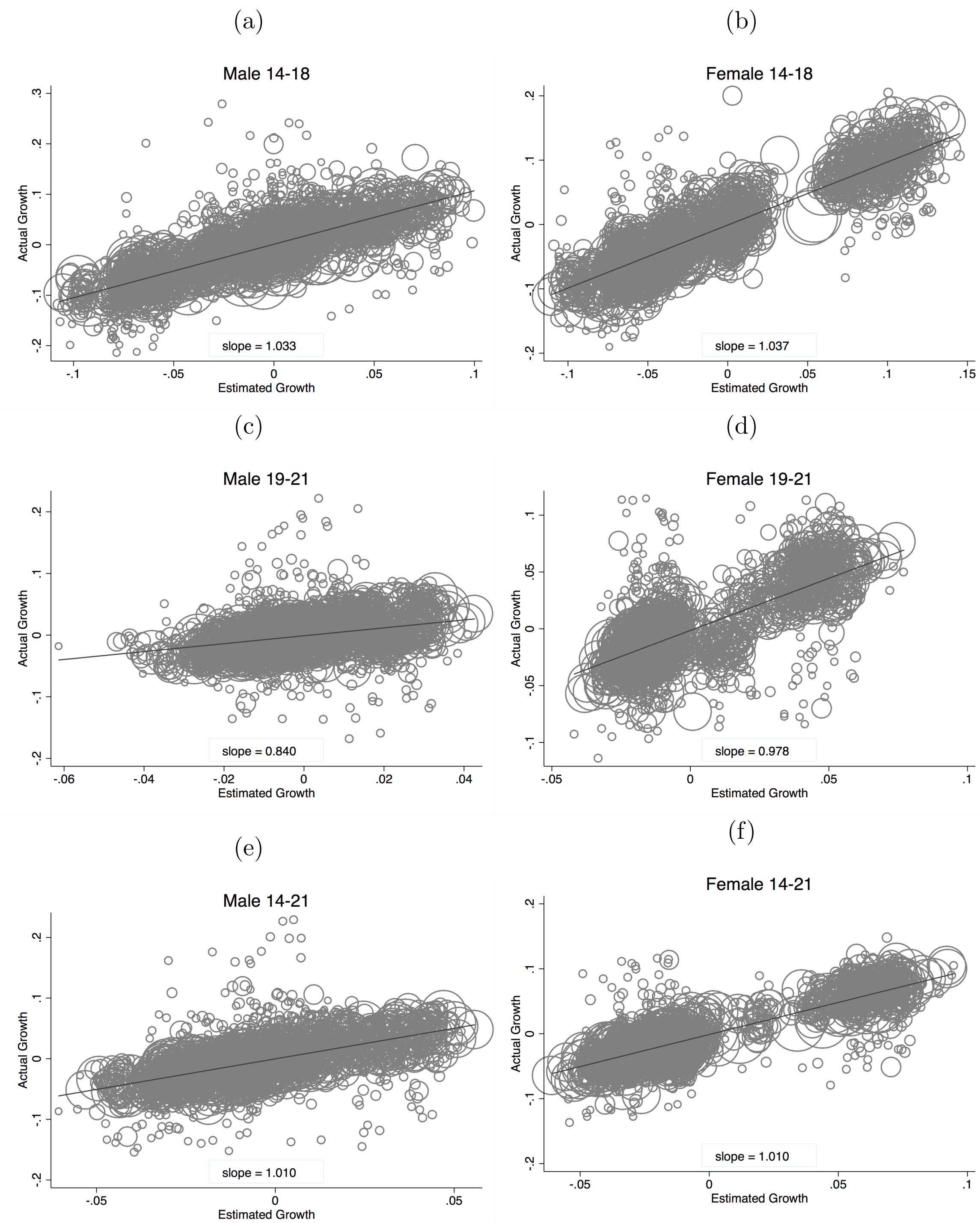
FIGURE 4.
Instrument Relevance

(a) (b)



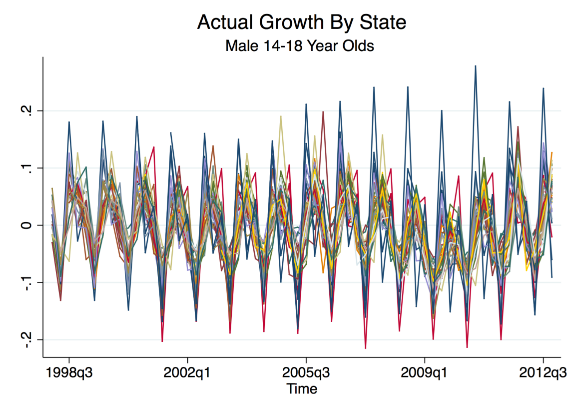
Notes: This figure plots estimated employment growth due to demand shocks against actual state employment growth from 1998-2012. Each observation is a state-quarter. Observations weighted by population.

FIGURE 5.
Instrument Relevance After Year 2000

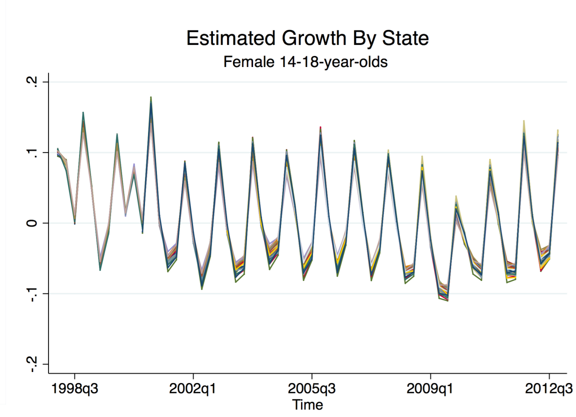


Notes: This figure plots estimated employment growth due to demand shocks against actual state employment growth from 2001-2012. Each observation is a state-quarter. Observations weighted by population.

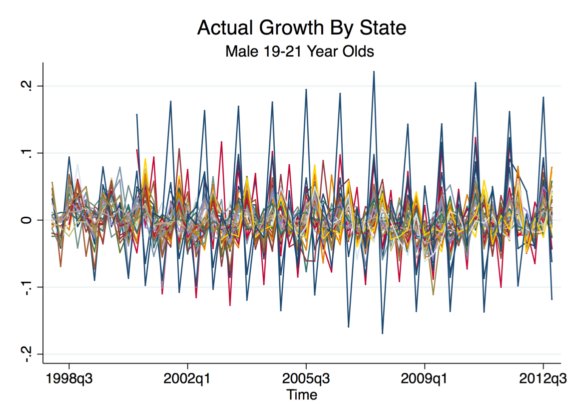
FIGURE 6.
Instrument Compared to Actual

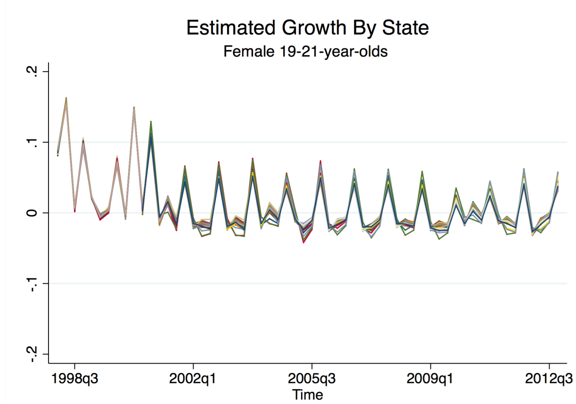


(a)

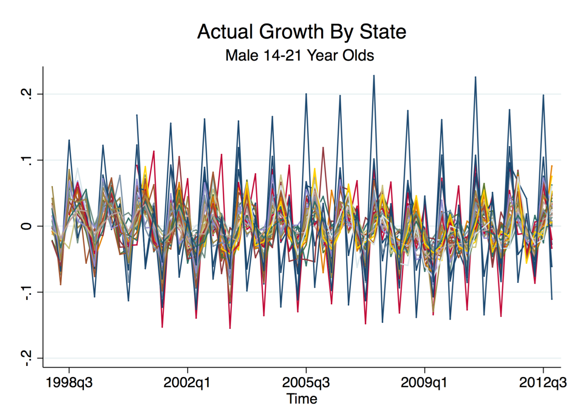


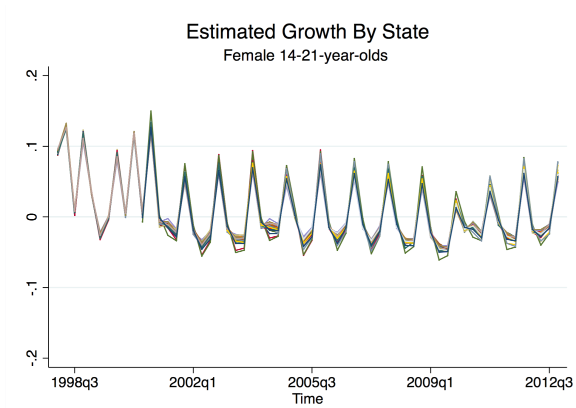
(b) (c)





(d) (e)





Notes: This figure plots actual employment growth on the right and estimated employment growth on the left. Estimated growth is driven by national industry specific growth but varies across state due state specific industry composition.

This may not be the case if a particular state is driving the national shock. To address this concern, I calculate the instrument for each state, while leaving out its own contribution to national employment growth. The formula to calculate the leave-own-out predicted employment growth $\hat{loog}_{st}$ is below in equation (2), where g_{sit} is state-specific industry employment growth. Column 6 reports the same specification as column 5 using the leave-own-out predicted employment growth specification for predicted employment growth.

$$\hat{loog}_{st} = \frac{\sum_i \left[G_{it} - g_{sit}\right] L_{si,t-1}}{L_{s,t-1}} \tag{2}$$

The construction of a valid instrument for predicting employment growth allows me to employ a simple empirical strategy. I use ordinary least squares linear regression to analyze the effect of the instrument directly on my dependent variable. I am estimating the reduced-form effect of estimated employment growth on arrests, instead of a typical two-stage instrumental variables approach.

My estimating equation is a linear regression with time and location fixed effects as follows:

$$\%\Delta arrests_{sayq} = \alpha + \beta \hat{g}_{sayq} + \delta Police_{sy} + \phi_y + \omega_q + \gamma_s + \epsilon_{sayq},$$

where $\%\Delta arrests$ is calculated as $\frac{Arrests_t - Arrests_{t-1}}{Arrests_{t-1}}$ for each state and age-sex group from quarter to quarter. Indices $sayq$ index state, age-sex[6], year, and quarter for each observation. Estimated growth $\hat{g}_{sayq}$ is constructed from employment data according to equation (1). $Police_{sy}$ is the count of payroll officers in a given

[6] Age-sex categories include male, female, and all sexes for ages 14-18, 19-21, and 14-21.

TABLE 1.
Actual Employment Growth Predicted by
Estimated Employment Growth

		(1)	(2)	(3)	(4)	(5)
14-18	Male	0.826***	0.346***	0.227***	0.353***	0.242***
		(0.015)	(0.033)	(0.032)	(0.036)	(0.035)
	Female	0.910***	0.427***	0.289***	0.438***	0.308***
		(0.011)	(0.034)	(0.033)	(0.037)	(0.036)
	All Sexes	0.875***	0.383***	0.261***	0.392***	0.278***
		(0.013)	(0.033)	(0.031)	(0.036)	(0.034)
19-21	Male	0.278***	0.137***	0.086***	0.142***	0.090***
		(0.016)	(0.023)	(0.021)	(0.026)	(0.024)
	Female	0.534***	0.122***	0.076***	0.130***	0.083***
		(0.013)	(0.02)	(0.019)	(0.022)	(0.021)
	All Sexes	0.403***	0.116***	0.073***	0.123***	0.078***
		(0.014)	(0.02)	(0.019)	(0.023)	(0.021)
14-21	Male	0.482***	0.152***	0.095***	0.158***	0.102***
		(0.017)	(0.025)	(0.024)	(0.028)	(0.026)
	Female	0.714***	0.147***	0.082***	0.157***	0.092***
		(0.012)	(0.024)	(0.022)	(0.026)	(0.025)
	All Sexes	0.612***	0.139***	0.082***	0.148***	0.091***
		(0.014)	(0.024)	(0.022)	(0.026)	(0.024)
State FE		n	y	y	y	y
Year FE		n	y	y	y	y
Quarter FE		n	y	y	y	y
State×year FE		n	n	n	y	y
Leave-out-own Growth		n	n	y	n	y

This table reports estimates for the percentage change in total arrests due to a one percent increase in youth employment opportunities.
Significance is indicated by ***p<0.01, **p<0.05, *p<0.1. Standard Errors are reported in Parenthesis.
Errors are clustered at the state level. Regressions are weighted by annual state population for a particular age-sex group.
Columns (3) and (5) calculate predicted growth using national growth that was calculated for each leaving out own state contribution to national growth.

state-year. Year fixed effects ϕ_y are included to capture broader economic trends that may be simultaneously affecting employment levels and arrests across all states. Seasonal variation is controlled for with quarter fixed effects ω_q. Systematic differences in states that are constant across time are controlled for with state fixed effects γ_s. Finally, ϵ_{sayq} is the error term.

Identifying variation comes from changes in arrest levels within a state, quarter, and year for a particular sex-age group. Estimated standard errors are clustered at the state level. Regressions are weighted by the age-sex population in a given state-year. The coefficient of interest is β, which is interpreted as the percentage change in arrests due a one percent increase in estimated employment growth. My identifying assumption is that the predicted measure of employment growth is conditionally uncorrelated with the unobservable component of change in arrests.

Results

Table 2 reports regression results across various specifications. Column 1 reports estimates for estimated growth regressed on change in arrests. Column 2 adds state, year, and quarter fixed effects. Column 4 adds state-by-year fixed effects. Columns 3 and 5 are similar specifications to 2 and 4, but are estimated using leave-own-out growth as in equation (2). Robustness across these columns rules out the concern that states are driving their own employment shocks via the national growth rate. Table 3 reports the same specification as column 2, including average arrest levels and average employment levels. These averages are combined with the elasticities to calculate the estimated effect size. Effect size is interpreted as the change in arrests caused by 100 new jobs for the age-sex group for a given state-quarter.

I find increased employment opportunities lead to decreased arrests. 14-18 year old males are most responsive to increases in job opportunities. Young adults

TABLE 2.
Effects of Employment Opportunities on Crime

Specification Analysis

$$\%\Delta arrests_{sayq} = \alpha + \beta \hat{g}_{sayq} + \delta Police_{sy} + \phi_y + \omega_q + \gamma_s + \epsilon_{sayq}$$

Age	Sex	(1)	(2)	(3)	(4)	(5)
14-18	Male	-0.348*	-1.056***	-0.992***	-1.063***	-0.980***
		(0.206)	(0.337)	(0.31)	(0.393)	(0.348)
	Female	0.323	-0.985***	-0.921**	-0.980**	-0.890**
		(0.202)	(0.382)	(0.358)	(0.439)	(0.389)
	All Sexes	-0.064	-1.081***	-1.005***	-1.085***	-0.988***
		(0.221)	(0.36)	(0.332)	(0.416)	(0.367)
19-21	Male	-0.724***	-0.296**	-0.286**	-0.312*	-0.282*
		(0.196)	(0.135)	(0.13)	(0.173)	(0.147)
	Female	-1.103***	-0.260	-0.238	-0.276	-0.233
		(0.176)	(0.175)	(0.168)	(0.217)	(0.192)
	All Sexes	-1.030***	-0.270*	-0.253*	-0.285	-0.249
		(0.2)	(0.149)	(0.145)	(0.189)	(0.164)
14-21	Male	-0.716***	-0.684***	-0.624***	-0.697**	-0.615**
		(0.245)	(0.232)	(0.214)	(0.282)	(0.242)
	Female	-0.187	-0.573**	-0.509*	-0.581*	-0.491*
		(0.219)	(0.288)	(0.27)	(0.343)	(0.298)
	All Sexes	-0.564**	-0.663***	-0.593**	-0.674**	-0.580**
		(0.242)	(0.255)	(0.237)	(0.306)	(0.265)
State FE		n	y	y	y	y
Year FE		n	y	y	y	y
Quarter FE		n	y	y	y	y
State×year FE		n	n	n	y	y
Leave-out-own Growth		n	n	y	n	y

This table reports estimates for the percentage change in total arrests due to a one percent increase in youth employment opportunities.
Significance is indicated by ***p<0.01, **p<0.05, *p<0.1. Standard Errors are reported in Parenthesis.
Errors are clustered at the state level. Regressions are weighted by annual state population for a particular age-sex group.
Columns (3) and (5) calculate predicted growth using national growth that was calculated for each leaving out own state contribution to national growth.

TABLE 3.
Effect Size of 100 New Employment Opportunities on Arrests

Age	Sex	β	R^2	$\overline{\text{Crime}}$	$\overline{\text{Emp}}$	Effect Size
14-18	Male	-1.055***	0.089	9924	22646	-46.4
		(0.336)				
	Female	-0.985***	0.163	3442	25857	-13.1
		(0.382)				
	All Sexes	-1.081***	0.107	13366	48503	-29.8
		(0.36)				
19-21	Male	-0.298**	0.201	8254	42817	-5.7
		(0.136)				
	Female	-0.261	0.192	2181	46755	-1.2
		(0.139)				
	All Sexes	-0.270*	0.204	10435	89572	-3.1
		(0.15)				
14-21	Male	-0.685***	0.106	18178	65463	-19
		(0.233)				
	Female	-0.574**	0.121	10435	72612	-4.4
		(0.289)				
	All Sexes	-0.664***	0.107	23801	138075	-11.4
		(0.256)				

This table reports estimates for the percentage change in total arrests due to a one percent increase in youth employment opportunities.
Significance is indicated by ***p<0.01, **p<0.05, *p<0.1. Standard Errors are reported in Parenthesis.
Errors are clustered at the state level. Regressions are weighted by annual state population for a particular age-sex group.
Effect size is the change in arrests due to 100 new job opportunities at the state-quarter level.

are much less responsive. For male youth the coefficient of -1.055 is interpreted as the percentage change in arrests at the state level for a given quarter due to a one percent increase in employment opportunities. This translates to 46 fewer youth arrests due to 100 new employment opportunities for 14-18 year old males in a particular state-quarter. Young adults see 0.298 percent fewer arrests due to a one percent increase in employment opportunities. Females are slightly less responsive, but have a much smaller effect size. This is consistent with the fact that females engage in much less criminal activity than males. These estimates are generated using arrest data, which is a lower bound estimate of criminal activity since all crime is not reported as an arrest. The reduction in criminal activity not resulting in arrest could be much larger.

These results are similar in direction but larger in magnitude than those of Heller and Gelber, who look at participation in different summer employment opportunities. Heller finds a 43-percent reduction in violent crime arrests per youth for disadvantaged youth who were randomly offered summer employment opportunities through Chicago public schools. Gelber finds participation in New York City's Summer Youth Employment Program reduced the probability of incarceration by 0.10 percentage points. Both of these studies examine only disadvantaged populations and summer employment. Of the disadvantaged youth, 96 percent are black in Heller's study, and 48 percent are black in Gelber's. Neither study uses a nationally representative sample.

I analyze the differential effect by race for juveniles in Table 4. I use arrest counts by race as the dependent variable across 4 categories: White, Black, Asian, and Native American. I report estimates only for males and females combined, since arrests by race are not recorded by sex. The elasticities vary only slightly across white and black. The difference in effect size, however, is quite large. The effect size of 5.4 fewer arrests for blacks due to 100 new employment opportunities at the state level is comparable to Heller's findings, which translate to 4 fewer arrests if 100 disadvantaged youth (96 percent of which are black) are given summer employment opportunities. While blacks are responding proportionally similarly to whites, whites make up a much larger fraction of the population; thus, including whites explains the difference in magnitudes seen between my results and those of previous studies. Asians and Native Americans have more than twice the response of whites and black, but due to their relatively low level of criminal activity, their effect sizes are small relative to other races. An important note is that I am allowing only the dependent variable to differ by race; I do not have employment data by race. These results, therefore, do not capture the fact that job opportunities are likely not equally distributed across races. In fact, my results are consistent with differential job opportunities across races. One possible explanation that whites see a larger reduction in crime due to predicted job opportunities is that whites are filling proportionally more of the potential job opportunities, which means that more whites are removed from the criminal labor market.

TABLE 4.
Juvenile Crime by Race

Race	β	$\overline{\text{Emp}}$	$\overline{\text{Crime}}$	Effect Size
White	-1.283***	45,627	6531	-19.2
	(0.404)			
Black	-0.926*	48,523	2852	-5.4
	(0.419)			
Asian	-2.142***	51,166	160	-0.7
	(0.419)			
Native American	-2.433*	48,503	131	-0.7
	(1.258)			
All	-1.081	48,503	13366	-29.8
	(0.36)			

This table reports estimates for the percentage change in arrests (by race) due to a one percent increase in youth employment opportunities.
Only arrests are categorized by race, employment opportunities is not.
Significance is indicated by ***p<0.01, **p<0.05, *p<0.1. Standard Errors are reported in Parenthesis.
Errors are clustered at the state level. Regressions are weighted by annual state population for a particular age-sex group.
Effect size is the change in arrests due to 100 new job opportunities at the state-quarter level.

To further investigate the mechanisms driving the results in Tables 2 and 3, I categorize arrests by offense type. Arrests are recorded in 29 offense groups and 14 subgroups, which makes 43 categories and subcategories. I group these categories into non-mutually exclusive groups by offense type in Table 5[7]. Group 1 is violent crimes; group 2 is financially motivated crimes; group 3 is mischief type crimes; group 4 is personal offenses; group 5 is drug related crimes; and group 6 is substance abuse crimes. The incentives to commit offenses in different groups vary substantially. Violent crimes are personal offenses often triggered by anger and other emotional responses. Financially motivated crimes are categorized as crime that could be an arguable substitute for income. Mischief crimes are crimes that youth "up to no good" may commit. These crimes seem to be driven by boredom, so the incapacitation of employment is expected to play a role in decreasing these crimes. Personal offenses are sex crimes or family crimes. Drug-related crimes are any arrests dealing with drug sale or drug possession. Finally, substance-abuse crimes are alcohol-related crimes or drug-possession crimes.

The regression results for each of these groups by age-sex group are presented in Table 6. I adjust significance levels for multiple hypothesis testing using the Benjamini-Hochberg step-up method (Benjamini and Hochberg, 1995). Table 6 identifies which grouping of crimes are driving the aggregate results in Tables 2 and 3. All types of crimes for all sex-age groups still seem to be decreasing in predicted employment opportunities. For male youth, all groups except drug-related crimes are estimated to decrease as employment opportunities are expected to increase. Female youth are much less responsive, as only three groups have coefficients

[7] The total number offenses listed in table 5 is less than 43 because some sub-categories are omitted to prevent double-counting. For instance, drug sale and drug possession aggregate to equal drug offenses.

TABLE 5.
Grouping of Offenses

Violent Crimes	Financially Motivated	Mischief	Personal Offenses
Murder	Larceny	Vandalism	Sex Offense
Manslaughter	Motor Theft	Arson	Family Offense
Rape	Forgery	Disorderly	
Aggravated Assault	Fraud	Vagrancy	**Drug-related**
Weapon	Embezzlement	Suspicion	
Other Assault	Stolen Property	Curfew	Drug
	Prostitution	Runaway	
	Gambling		**Substance Abuse**
	Robbery		
	Burglary		DUI
	Drug Sale		Liquor Laws
			Drunkenness
			Drug Possession

These groupings are not mutually exclusive. Drug includes several categories for sale and possession. Drug sale is included in financially motivated offenses while drug possession is included in substance abuse. Both categories are included in drug-related offenses.

significantly different from zero. The negative coefficient of financially motivated crimes is suggestive that they are inferior goods, decreasing as income increases.

Consistent with the aggregate regressions, young adults are less responsive than youth. For financially motivated crimes, young adult males are about a third as responsive as youth to predicted increases in employment opportunities. The result that youths respond more to predicted changes in employment opportunities for financially motived crimes is suggestive that financially motivated crimes are more of a substitute for youth than for young adults. Youth are constrained in the types of jobs they are eligible to work at due to many over-18 policies. This can been seen in Figure 6. Youths tend to be employed only in retail, and food. The relatively lower availability of employment opportunities could be an

TABLE 6.
Effects of Employment Opportunities on Crime by
Offense Group and Age

Group:	14-18 Year Olds			19-21 Year Olds		
	Male	Female	All Sexes	Male	Female	All Sexes
Violent Crimes	-0.693**	-0.659	-0.667*	-0.422*	-0.744	-0.494*
Financially Motivated	-1.257***	-0.595*	-1.073***	-0.405*	-0.271	-0.343*
Mischief	-1.241***	-1.404*	-1.304**	-0.466	-0.129	-0.494
Drug-related	-0.073	0.199	-0.150	-0.249	-1.499	-0.402
Personal Offense	-1.069**	-0.931	-1.172**	-0.258	-0.351	-0.374
Substance Abuse	-0.902***	-1.08**	-1.018***	-0.084	-0.343	-0.367
	14-21 Year Olds					
	Male	Female	All Sexes			
Violent Crimes	-0.662**	-0.825	-0.705**			
Financially Motivated	-0.847***	-0.341	-0.743***			
Mischief	-0.907**	-0.922	-0.903*			
Drug-related	-0.247	-0.496	-0.325			
Personal Offense	-0.702**	-0.691	-0.696*			
Substance Abuse	-0.408**	-0.418	-0.381*			

This table reports estimates for the percentage change in arrests (by grouping) due to a one-percent increase in youth employment opportunities.
Significance is indicated by ***p<0.01, **p<0.05, *p<0.1.
Errors are clustered at the state level. Regressions are weighted by annual state population for a particular age-sex group.
Significance levels are adjusted for multiple hypothesis testing using the Benjamini-Hochberg step-up method.

explanation for why youths seem to be substituting toward financially rewarding crimes. Another possible explanation for young adult arrests being less responsive to increases employment opportunities is that since the young adults are much less likely to be dependents and much more likely to be employed, the observed arrests are happening to employed individuals. Youth, on the other hand, are typically dependents and much less likely to be employed, so increases in employment opportunities have a larger incapacitating effect than for young adults. This is an intuitive finding if incapacitation is concave in employment. Since youth have a much lower level of employment than young adults, the marginal effect is much larger.

I split arrests by offense type to analyze which individual offenses respond to changes in employment opportunities. I adjust significance levels for multiple hypothesis testing using the Benjamini-Hochberg step-up method. Regressions by offense are included in Tables 7, 8, and 9. Table 7 is youth offenses, table 8 is young adult offenses, and Table 9 is both age groups. For youth, most offenses have a negative coefficient. This table illustrates which individual offenses are driving the aggregate results seen in Tables 2 and 3. For youth males, robbery, aggravated assault, burglary, larceny, motor theft, fraud, stolen property, vandalism, weapon, drug, drug possession, non-narcotic drug sale, liquor laws, and suspicion, are all decreasing as predicted employment opportunities increase. Young adult males see a reduction only in robbery arrests as employment opportunities increase. This result suggests that robbery is a substitute for income. For both age groups, robbery, burglary, and larceny, other assault, vandalism, weapon, drug sale, disorderly , and other offenses all decrease with employment opportunities. Many of these significant results are driven by the youth results. A few offenses, like disorderly conduct, are not significant for either age group but significant for the combined age group. Overall, income substitutes seem to be moving the most for young adult males, a result that is consistent with Gould et al. (2002) and Mocan and Rees (2005).

None of the offenses are changing at a rate significantly different from zero for females. This is likely due to the fact that females are committing significantly fewer crimes than males, so slicing the data by offense strips away any identifying power.

TABLE 7.
The Effect of Employment Opportunities on Arrests by Offense for 14-18 Year Olds

Offense	Male	Female	All Sexes
Murder	-2.508	-5.867	-2.249
Manslaughter	-1.127	5.071	0.257
Rape	-0.343	-4.281	-0.491
Robbery	-1.164**	1.046	-0.855*
Aggrivated Assault	-0.732**	-0.670	-0.696*
Burglary	-1.461***	-0.782	-1.424***
Larceny	-1.409***	-0.734	-1.156**
Motor Theft	-1.075**	-0.528	-1.022*
Other Assault	-0.593	-0.628	-0.604
Arson	-0.868	-4.305	-0.995
Forgery	-0.907	-0.461	-0.665
Fraud	-1.744**	0.208	-1.050*
Embezzlement	-1.021	-0.605	-0.763
Stolen Property	-1.830**	-2.439	-1.892**
Vandalism	-1.856***	-1.821	-1.813***
Weapon	-1.130**	-0.505	-1.131**
Prostitution	-1.470	-0.242	-1.581
Sex Offense	-0.364	-0.934	-0.235
Drug	-1.074**	-0.958	-1.173*
Drug Sale	-0.811	-0.662	-0.916
Drug Possession	-0.958**	-0.767	-1.081*
Opium Sale	-0.187	0.413	0.314
Marijuana Sale	-1.614	-0.752	-1.738
Synthetic Narcotic Sale	-0.887	-13.701	-6.134
Non Narcotic Sale	-0.230**	1.488	-0.370**
Opium Possession	-0.906	-1.610	-0.774
Marijuana Possession	-1.096	-1.495	-1.311
Synthetic Narcotic Possession	-2.402	-0.242	-2.347
Non-narcotic Possession	-0.801	0.572	-0.632
Gambling	-7.222	-1.357	-7.615
Family Offense	-0.607	2.548	4.009
DUI	-0.264	-0.096	-0.168
Liquor Laws	-0.445**	-0.928	-0.694*
Drunkenness	-1.914	-1.097	-2.953
Disorderly	-1.957**	-2.539	-2.217**
Vagrancy	-3.781	-6.550	-2.238
Other Offenses	-1.283	-1.711	-1.440
Suspicion	-12.001*	-5.072	-15.499*
Curfew	-2.212	-0.945	-1.876
Runaways	-0.794	-0.465	-0.647

This table reports estimates for the percentage change in arrests (by offense category) due to a one-percent increase in youth employment opportunities.
Significance is indicated by ***p<0.01, **p<0.05, *p<0.1.
Errors are clustered at the state level. Regressions are weighted by annual state population for a particular age-sex group.
Significance levels are adjusted for multiple hypothesis testing using the Benjamini-Hochberg step-up method.

TABLE 8.

The Effect of Employment Opportunities on Arrests by Offense for 19-21 Year Olds

Offense	Male	Female	All Sexes
Murder	-1.759	-6.855	-0.448
Manslaughter	-0.838	-1.969	-0.761
Rape	-0.203	-0.248	-0.354
Robbery	-1.321*	-1.721	-1.371
Aggrivated Assault	-0.584	-0.913	-0.693
Burglary	-0.254	-0.934	-0.305
Larceny	-0.36	-0.373	-0.422
Motor Theft	-0.62	0.808	-0.603
Other Assault	-0.391	-0.605	-0.456
Arson	1.108	-0.95	0.036
Forgery	-0.724	0.575	-0.277
Fraud	-0.371	-0.189	-0.257
Embezzlement	-0.419	-0.791	-0.325
Stolen Property	-0.725	1.201	-0.664
Vandalism	-0.59	-0.494	-0.59
Weapon	-0.659	-2.519	-0.675
Prostitution	-2.076	-1.363	-1.008
Sex Offense	0.281	-2.097	0.192
Drug	-0.258	-0.351	-0.195
Drug Sale	-0.505	0.166	-0.442
Drug Possession	-0.212	-0.278	-0.146
Opium Sale	-0.097	-0.902	-0.141
Marijuana Sale	-0.971	0.72	-0.783
Synthetic Narcotic Sale	-2.113	2.038	-1.55
Non-narcotoc Sale	-1.478	-0.802	-1.463
Opium Possession	-0.511	-0.32	-0.426
Marijuana Possession	-0.061	-0.135	-0.033
Synthic Narcotic Possession	-2.073	-2.099	-1.802
Non-narcotic Possession	-0.463	0.453	-0.373
Gambling	-5.618	1.575	-4.538
Family Offense	2.07	-0.2	-0.68
DUI	0.171	-0.034	0.223
Liquor Laws	-0.132	-0.54	-0.228
Drunkenness	-0.545	-0.917	-0.563
Disorderly	-0.837	0.213	-0.624
Vagrancy	-1.661	5.535	-0.401
Other Offenses	-0.481	-0.207	-0.383
Suspicion	0.769	-4.638	-1.641

This table reports estimates for the percentage change in arrests (by offense category) due to a one-percent increase in youth employment opportunities.
Significance is indicated by ***p<0.01, **p<0.05, *p<0.1.
Errors are clustered at the state level. Regressions are weighted by annual state population for a particular age-sex group.
Significance levels are adjusted for multiple hypothesis testing using the Benjamini-Hochberg step-up method.

TABLE 9.
The Effect of Employment Opportunities on Arrests by Offense for 14-21 Year Olds

Offense	Male	Female	All Sexes
Murder	-1.816	-4.585	-1.202
Manslaughter	3.317	-5.082	4.309
Rape	-0.406	-4.276	-0.51
Robbery	-1.407***	-0.43	-1.304***
Aggravated Assault	-0.53*	-0.814	-0.6*
Burglary	-0.857***	-1.101	-0.904***
Larceny	-0.94***	-0.388	-0.785**
Motor Theft	-0.553	0.052	-0.538
Other Assault	-0.654*	-0.779	-0.703
Arson	-0.517	-2.157	-0.651
Forgery	-0.577	0.236	-0.269
Fraud	-0.588	0.028	-0.319
Embezzlement	-0.364	-0.928	-0.569
Stolen Property	-1.18	-0.96	-1.184
Vandalism	-1.155**	-1.381	-1.129*
Weapon	-1.053***	-1.156	-1.106**
Prostitution	-3.154	-0.956	-1.098
Sex Offense	-0.371	-2.081	-0.378
Drug	-0.702	-0.686	-0.696
Drug Sale	-0.722*	-1.169	-0.697
Drug Poss	-0.527	-0.542	-0.559
Opium Sale	0.045	-0.331	0.267
Marijuana Sale	-1.577	0.007	-1.673
Synthetic Narc Sale	-2.244	-6.216	-2.513
Non-narcotic Sale	-0.82	-4.486	-1.092
Opium Possession	-0.713	-0.623	-0.633
Marijuana Possession	-0.529	-0.802	-0.634
Synthetic Narcotic Possession	-1.257	-2.7	-1.708
Non-narcotic Possession	-0.765	0.303	-0.64
Gambling	-6.007	1.468	-4.262
Family Offense	-0.709	2.606	0.603
DUI	0.048	0.038	0.146
Liquor Laws	-0.314	-0.445	-0.384
Drunkenness	0.105	0.134	0.862
Disorderly	-1.633*	-2.024	-1.785*
Vagrancy	-3.371	-0.543	-1.772
Other Offenses	-0.903*	-0.931	-0.904
Suspicion	-6.363	-3.296	-6.225

This table reports estimates for the percentage change in arrests (by offense category) due to a one-percent increase in youth employment opportunities.
Significance is indicated by ***p<0.01, **p<0.05, *p<0.1.
Errors are clustered at the state level. Regressions are weighted by annual state population for a particular age-sex group.
Significance levels are adjusted for multiple hypothesis testing using the Benjamini-Hochberg step-up method.

Conclusion

Effectively decreasing the incidence of juvenile crime is a central interest of public policy. While a large body of research shows that increasing the certainty of apprehension reduces crime, doing so comes at a cost. A rational model of crime posits alternative sources of income can also be an effective way to reduce crime. I test this relationship for youths by predicting employment growth and analyzing how youth arrests respond to predicted changes in employment levels.

This paper makes two major contributions to the economic literature on employment conditions and crime. The first contribution is that it provides external validity to the RCT findings of Heller (2014) and Gelber et al. (2014), which document the reduction of criminal activity due to the random assignment of youth employment opportunities in large cities. This paper looks at systematic predictions for job growth across 46 states, encompassing a much larger population over a much larger time period. The second is the distinction between labor market conditions, which are equilibrium outcomes, and labor market opportunities, which isolate predicted demand side shocks in the labor market.

To examine the effect of labor market opportunities on crime, I use a shift-share analysis to create predicted employment growth only due to demand side shocks. I then use changes in expected employment growth to explain changes in arrest rates. I find youth arrests decrease 1.08% due to a 1% increase in employment opportunities. This translates into 30 fewer youth arrests due to 100 new youth job opportunities in a given state for a given quarter. Arrests are a lower-bound estimate of criminal activity, so actual youth crime could be decreasing even more. For young adults, this response is considerably smaller. A 1% increase in employment opportunities leads to a 0.29% decrease in arrests, which translates to 6 fewer arrests for every 100 new job opportunities in a given state for a given quarter.

Violent, financially motivated, mischief, personal offenses, and substance-abuse-related arrests all decrease for male youths as employment opportunities increase. Young adult male arrests respond about a third as much as youth arrests for financially motivated crimes, which is an intuitive result if incapacitation is concave in employment. Since young adults are employed at much higher levels than youth, the marginal effect of increased employment is much larger for youth.

Slicing arrests by offense type, robbery, aggravated assault, burglary, larceny, motor theft, fraud, stolen property, vandalism, weapon, drug, drug possession, non-narcotic drug sale, liquor laws, and suspicion all decrease as a result of increased employment opportunities for youth males. A possible limitation of these findings is that they rely on voluntarily reported arrests. Only a fraction of criminal activity results in an arrest since many crimes go undetected or unreported, so these results likely understate the effect of employment opportunities on crime. Secondly, many youth employment opportunities, such as babysitting or yard work for a neighbor, will not be recorded in my data, since I see only employment for the duration of a quarter recorded by firms for unemployment insurance obligations. Nonetheless, these results are informative about youth and young adult responses to increased employment opportunities in the formal sector.

A natural extension of the work is to shift time periods for youths to capture summertime employment separately from school-year employment. Also, obtaining employment data by race would allow for an analysis of the opportunity of employment across races. Finally, in coming work, I plan to apply a similar shift-share analysis at the state level and use counties as the local geographies responding to changes statewide trends.

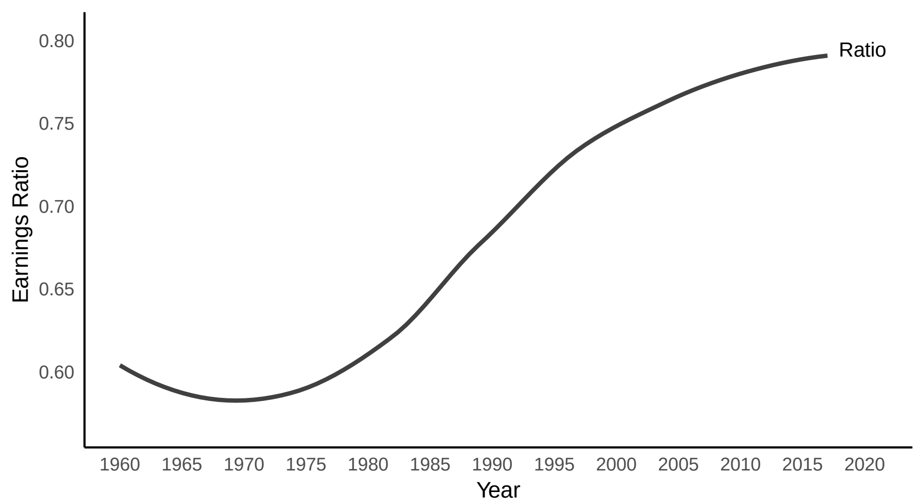
CHAPTER II

INFORMATION AND THE PERSISTENCE OF THE GENDER WAGE GAP; EARLY EVIDENCE FROM CALIFORNIA'S SALARY HISTORY BAN

Introduction

Wage inequality across genders improved substantially during the twentieth century but has since plateaued [1], despite the increase in female college enrollment (Goldin, 2014; Goldin et al., 2006). Figure 7 illustrates the female to male earnings ratio over the last 50 years.

FIGURE 7.
Female to Male Median Earnings Ratio of Full-Time Workers



The literature lacks consensus on the underlying drivers of the gender wage gap. O'Neill and Polachek (1993); Blau and Kahn (2006a); Mulligan and Rubinstein (2008) all present different possible explanations in the narrowing of the gender pay gap. Some explanations point to bargaining as a cause for the gender

[1] Evidence of the narrowing of the gender pay gap can be seen in Blau and Kahn (2013, 2017, 2000, 2006a,b).

pay gap, see Babcock et al. (2003). Others claim differences in competitiveness drives the gender pay gap, see Gneezy et al. (2003); Niederle and Vesterlund (2007). Alternatively, Manning and Saidi (2010) find little empirical evidence that competitiveness drives the gender pay gap. It is unclear what, if anything, federal, state, and municipal policy makers can do about the issue. A recent tool gaining popularity is the salary history ban (SHB).

This paper is the first to offer evidence on the causal impact of SHBs. Cities and states have recently been adopting variations of SHB laws, which prohibit employers from asking about applicants' previous compensation. These laws address wage discrimination in multiple ways with the intent to reduce salary disparity across genders. First, when current compensation is based on previous salary, past wage discrimination could be perpetuated. Second, women are more likely to work in female-dominated industries, which pay less than male dominated industries. The salary history question could be perpetuating the systemic undervaluation of women's work. SHB laws could eliminate path dependent compensation across multiple margins. Alternatively, SHB laws could have unintended consequences that cause employers to engage in more statistical discrimination. This has been seen before, notably due to the implementation of ban the box policies (Henry and Jacobs, 2007; Agan and Starr, 2016; Doleac and Hansen, 2016; Shoag and Veuger, 2016; Starr, 2014).

Specifically, this paper focuses on the SHB passed in California in January of 2018. The legislative commentary below demonstrates that pay equality was the motivation behind California's SHB.

> *"Gender wage discrimination is destructive not only for female workers but for our entire economy. Closing the wage gap starts with barring*

employers from asking questions about salary history so that previous salary discrimination is not perpetuated." - California legislature

In a competitive market, recent wages signal a worker's marginal productivity, which is of greatest interest to hiring firms (Kotlikoff and Gokhale, 1992; Altonji and Pierret, 2001; Oyer et al., 2011; Lange, 2007). Labor markets may not be perfectly competitive, but the assumption gives insight to why past wages matter to firms. A previous wage can serve as a signal about the value of previous work. And, firms do ask about prior earnings. Barach and Horton (2017) find over 80% of respondents to a nationally representative Google Survey were asked by their employer about past wages.

Some experimental work has been done on removing compensation history from an online contracting labor market (Barach and Horton, 2017). They find that banning wage history results in more call backs in general and more offers to workers with lower past average wages. Their field experiment takes place in a very unique online labor market. To my knowledge, no work has been done evaluating the implementation of salary ban laws in the United States.

This research contributes to a broader literature which examines how changes in employer screening affect labor outcomes for potential employees, specifically, in the context of SHBs. This question about employer screening methods has been addressed in the context of drug testing (Wozniak, 2015), credit screening (Bartik and Nelson, 2016), test-based worker screening (Autor and Scarborough, 2008), criminal history checks (Finlay, 2009; Bushway, 2004; Holzer et al., 2006, 2007; Stoll, 2009), and ban the box policies (Henry and Jacobs, 2007; Agan and Starr, 2016; Doleac and Hansen, 2016; Shoag and Veuger, 2016; Starr, 2014).

These papers suggest limiting information can often have unintended consequences, increasing gaps, or shifting them disproportionately to other marginalized groups.

I use a synthetic control approach to estimate the causal impact of SHB laws in the state of California on gender wage ratios and other labor outcomes of interest calculated from the Basic Monthly Current Population Survey. I find that California's state wide weekly earnings ratio, which has been stagnant around 0.77 for more than 12 years, increases from 0.77 to 0.81 after adoption of a statewide SHB. These results are driven by women earning more in male dominated industries.

The paper proceeds as follows: Section 2.2 provides background on SHB policies. Section 3.3 describes the data. Section 2.4 presents my empirical methodology. Section 3.4 describes my results. Section 2.6 discusses and concludes.

Background on SHBs

Salary history bans (SHBs) prohibit employers from inquiring about a candidates former or current compensation. Currently, SHBs have been adopted by a growing number of cities and states in varying degrees. Some affect the entire population, some only state employees, and some only city employees. The rapid uptake of SHBs suggest that many entities believe SHBs will improve gender pay inequalities. Table 10 summarizes different cities and states that have adopted SHB laws. Most of the SHBs only apply to a subset of the population. The states with SHBs that affect the entire population and that have been implemented long enough to exist in my data are Delaware and California. As of July 1st 2018, Massachusetts, and Vermont have also implemented SHBs affecting the entire population. [2]

TABLE 10.
Salary History Ban Laws by Date and Region

Adoption Date	Region	Population
12/4/16	NYC	City employees
1/9/17	New York	State employees
1/25/17	New Orleans	City employees
3/01/17	Pittsburgh	City employees
10/31/17	NYC	All
12/14/17	Delaware	All
1/1/18	California	All
4/10/18	Chicago	City employees
2/1/18	New Jersey	State employees
5/17/18	Louisville	City employees
7/1/18	Massachusetts	All
7/1/18	San Francisco *(strong)*	All
7/1/18	Vermont	All
7/26/18	Kansas City	City employees
1/1/19	Oregon	All
1/1/19	Hawaii	All

California's SHB became effective January 1 of 2018. Under California's SHB employers are prevented from seeking compensation history directly or through an agent. Like Delaware, applicants may volunteer, without prompting, their own salary history. Additionally, California restricts employers from basing salary solely on the grounds of prior salary. The SHB also requires employers to provide a salary range at the request of the applicant. After an offer has been extended, Californian employers may seek the applicant's compensation history.

[2] The treatment of these states is too recent to show up in the data. I plan to include them in future analysis.

States that have implemented SHBs affecting state employees only include New York and New Jersey. Cities with SHBs affecting city employees only include Pittsburgh, Chicago, and Louisville. New York City, which had adopted a SHB effective for city employees, recently extended their SHB to the entire population of New York City. Oregon and Hawaii will both adopt a SHB that affects the entire population in January of 2019.

Each SHB adopting entity clearly states that they have adopted the SHB to promote pay equality. The cities and states with a SHB law also tend to be more progressive on the pay equality front. With the recent uptake of SHBs, some states have implemented laws that prevent SHBs from being passed. These states include Michigan, Wisconsin, Iowa, North Carolina, and Tennessee. Philadelphia prevented the implementation of a SHB when a district judge found the SHB to be in violation of the First Amendment's free-speech clause. States preventing the adoption of SHBs have done so with employer compliance in mind. They argue that allowing employment law to change across regions is costly for small business owners.

Data

I use data from the Basic Monthly Current Population Survey (CPS). The CPS is a comprehensive survey containing monthly labor force statistics. Other potential useful data sources for employment measures include the American Community Survey, the Quarterly Workforce Indicators, and the Current Employment Statistics, but each of these alternative data sources have a delayed release schedule. The CPS is published roughly 10 days after each month's end.

This makes it particularly useful given that the rollout of SHBs is so recent [3]. It samples roughly 60,000 households each month using a rotating panel design and has a response rate averaging around 90 percent. I use the micro-level data, which has responses by all household members as reported by the call recipient; then I aggregate to the state level. My sample includes data from 2006 to the most recent month of the CPS available. I continually update my estimates with each data release.

The CPS is administer by the Census Bureau through personal and telephone interviews. Individuals must be 15 years of age or over and not in the Armed Forces. The person who responds to the phone call is the reference person. They answer questions about all persons in the household. In the case that the reference person is not knowledgeable, the Census Bureau attempts to contact those individuals in the household directly.

I create statewide average weekly earnings ratios of female to male earnings for each state. Additionally, I calculate earnings ratios by age, and by industry of employment. I also calculate employment probabilities by sex. I use each of these calculated values as a potential employment measure of interest.

Methodolgy

I use the synthetic control method introduced by Abadie et al. (2010). This method uses pretreatment data to create a counterfactual group similar in outcomes to entities experiencing a discrete change in policy. This method has been used to study many different policy changes including decriminalization of prostitution (Cunningham and Shah, 2017), highway police budget cuts (DeAngelo

[3] I obtained these data using the lowdown package for R. The data are available for download almost immediately after the end of a month.

and Hansen, 2014), economic liberalization (Billmeier and Nannicini, 2013), and increases in minimum wage (Jardim et al., 2017). I follow the work of Botosaru and Ferman (2019) and create synthetic control groups matching outcomes only for each treated entity.

Consider an outcome of interest Y_{it} that is measured over T years, where t indexes the time and the state is indexed by i if its treated and j if its not treated, among I treated states and J untreated states. The synthetic control approach aims to estimate the treatment effect, which is the difference between the treated state, and the unobserved counterfactual. The estimate for the unobserved counterfactual for state i in time period t is $\sum_j w_j Y_{jt}$, where w_j is the weight assigned to donor state j. The donor states chosen belong to the donor pool of potential control states. The chosen weights w_j^* minimize the distance between Y_{it} and $\sum_j w_j Y_{jt}$ for all pretreatment time periods. For treatment in period τ, the treatment effect α_i for state i in time period t is estimated as $\alpha_{it} = Y_{it} - \sum_j w_j^* Y_{jt}$ for $t \in [\tau, T]$. For each treated state, I create a synthetic control using lagged values of the dependent variable from 2006 to 2018.

To conduct hypothesis tests, I run a set of placebo tests following the method suggested by Abadie et al. (2010). I apply the same synthetic control method with the donor state removed, and the treated states added to the donor pool to create $Synth_{jt}$ for each donor state j and time period t. I compare the pre-treatment and post-treatment mean squared prediction error (MSPE) for each state. I calculate the MSPE ratio as follows:

$$MSPE\ ratio_j = \frac{\sum\limits_{t=\tau}^{T} (Y_{jt} - Synth_{jt})^2}{\sum\limits_{t=1}^{\tau-1} (Y_{jt} - Synth_{jt})^2}.$$

The MSPE measures a relative goodness of fit of the synthetic outcome generated for each state. It provides a metric of pre-treatment fit relative to post treatment fit for each state. A high MSPE ratio can be interpreted as poor post-treatment fit relative to pre-treatment fit. The ranking of the treated states relative to the placebo states provides a permutation based p-value.

I include a relatively long pretreatment window from 2006 to 2017. This allows me to match on pretreatment outcomes only. Botosaru and Ferman (2019) show that matching on covariates is not necessary if the match is made on a long set of pretreatment outcomes. I define treatment in California as the adoption of their state-wide SHB on January 1 of 2018. New York and Delaware are excluded from the potential donor pool as they each adopt a SHB at the end of 2017. The donor pool consists of 47 possible states and Washington D.C.

The synthetic control approach creates an estimate of the counterfactual for California. Absent treatment, the synthetic California should match actual California reasonably well. I test the ability of the synthetic control approach to forecast the earnings ratio in California prior to treatment. I do this by progressively rolling back a placebo treatment, matching on fewer and fewer years. Within this exercise, I examine how well the synthetic control approach does at predicting the earnings ratio within the pretreatment time period. The cross validation exercise shows that the synthetic control approach succeeds in forecasting one step ahead except for in 2018, the year actual treatment begins. This can bee seen in Figure 8. As treatment rolls back in time, the synthetic California matches the actual California in both levels and trends. This cross validation exercise also shows that the synthetic control is a reasonable counterfactual. Figure 8 shows the distribution of the MSPE ratios for the placebo states and for California. As treatment is rolled back in time, California's MSPE ratio goes from being an outlier to well within the mean of the distribution. This figure also illustrates the sensitivity of MSPE ratios. As there are fewer and fewer pretreatment years, the post treatment MSPE can get very large driving up the MSPE ratio.

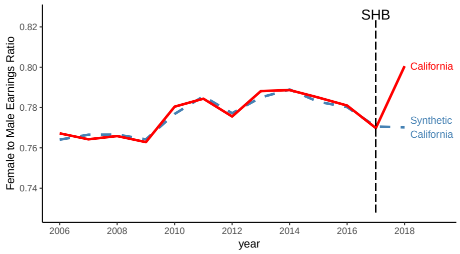
The composition of the synthetic control can be seen in Figure 9. The time-series of the female to male earnings ratio before California's SHB is best reproduced by a combination of 29% Nevada, 24% Arizona, 16% D.C., 12% North Carolina, 6% Hawaii, 4% Florida, and, 1% Oregon. All other donor states are assigned a weight of zero.

The weights chosen are consistent across the placebo treatments in the cross validation exercise. Table 11 shows the composition of weights as the placebo treatment is rolled back in time. Notably, the composition of the synthetic California is stable across fewer and fewer pre treatment years. The weights chosen are consistent for each of the cross validation years and they consistently predict the actual California earnings ratio.

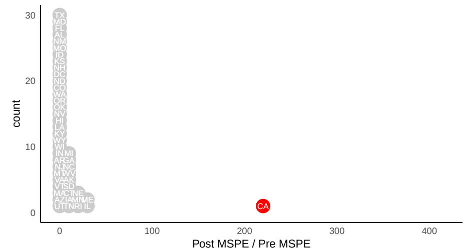
Using the detailed industry codes, I classify each industry in the CPS as male or female dominated. I classify male dominated industries as industries with more than 50 percent male workers and classify industries with more than 50% female workers to be female dominated. I use the industry gender compositions reported by the Equal Employment Opportunity Commission in Cartwright et al. (2011) to classify each industry as male or female dominated. Female dominated industries are in the service producing domain and male dominated industries tend to be in the goods producing domain.

FIGURE 8.
California Cross Validation

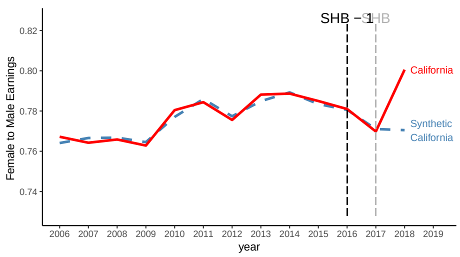
(a) Treatment in 2018



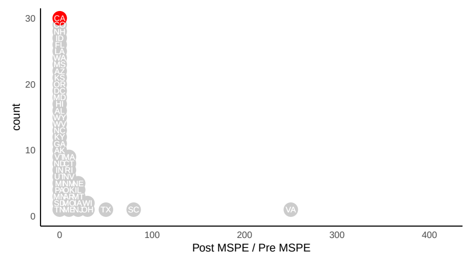
(b) MSPE Ratio Distribution for Treatment in 2018



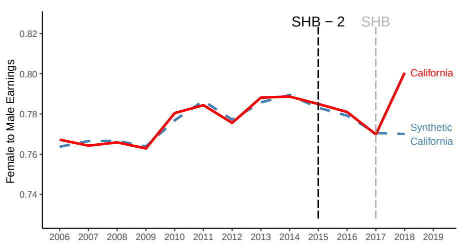
(c) Treatment in 2017



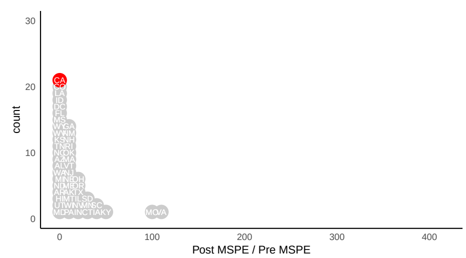
(d) MSPE Ratio Distribution for Treatment in 2018



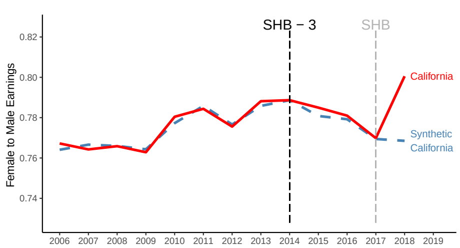
(e) Treatment in 2016



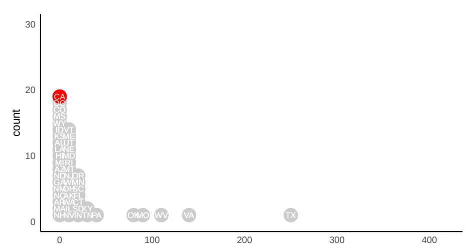
(f) MSPE Ratio Distribution for Treatment in 2016



(g) Treatment in 2015



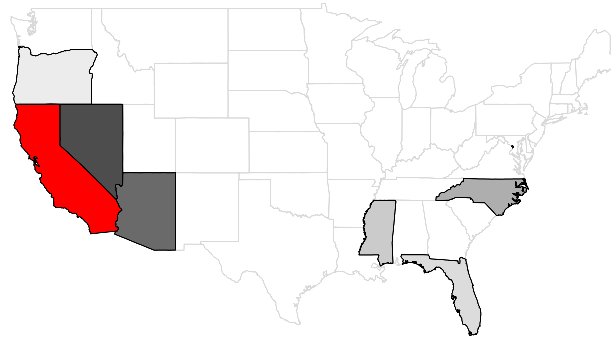
(h) MSPE Ratio Distribution for Treatment in 2015



Notes: This figure illustrates the ability the synthetic control approach to forecast out of sample. For each subfigure, matching only occurs to the left of the treatment line.

FIGURE 9.
Composition of Synthetic California

Donor State	Weight
Nevada	29%
Arizona	24%
District of Columbia	16%
North Carolina	12%
Mississippi	7%
Hawaii	6%
Florida	4%
Oregon	1%



Notes: This figure shows the composition of donor states used to make a synthetic California. States are shaded in proportion to their weighted contribution towards synthetic California.

Results

The cross validation exercise from the previous section showed that the synthetic California generated by the synthetic control approach does well at predicting out of sample. I first consider the effect of the SHB on state-wide earnings ratios. I then investigate weekly earnings, hours worked, and hourly wages by sex. Then I turn to industry classifications reported in the CPS and analyze the effect of SHB within predominately male and predominantly female industries, as well as goods vs service industries. I split my sample by age, and investigate the impact of the SHB among older and younger populations. Finally, I investigate the impact of SHBs on the probability of employment for both males and females at the state level and within industries.

Female to Male State-Wide Earnings Ratio

Figure 10a illustrates the female to male average weekly earnings ratio in California and its synthetic counterpart from 2006 to 2018. Over the 11 year window from 2006 to 2017 California's synthetic counterpart closely matches, both

TABLE 11.
Cross Validation Weights

	Weight by Placebo Treatment Year							
Donor State	2018	2017	2016	2015	2014	2013	2012	2011
Alabama	-	-	-	-	-	-	-	-
Alaska	-	-	-	-	-	-	-	-
Arizona	24%	23%	23%	26%	28%	26%	25%	28%
Arkansas	-	-	-	-	-	-	-	-
Colorado	-	-	-	-	-	-	-	-
Connecticut	-	-	-	-	-	-	-	-
District of Columbia	16%	16%	16%	15%	17%	16%	16%	14%
Florida	4%	5%	4%	-	-	10%	12%	-
Georgia	-	-	-	-	-	-	-	-
Hawaii	6%	6%	6%	6%	1%	1%	2%	25%
Idaho	-	-	-	-	-	-	-	-
Illinois	-	-	-	-	-	-	-	-
Indiana	-	-	-	-	-	-	-	-
Iowa	-	-	-	-	-	-	-	-
Kansas	-	-	-	-	-	-	-	-
Kentucky	-	-	-	-	-	-	-	-
Louisiana	-	-	-	-	-	-	-	-
Maine	-	-	-	-	-	-	-	-
Maryland	-	-	-	-	-	-	-	-
Massachusetts	-	-	-	-	-	-	-	-
Michigan	-	-	-	-	-	-	-	-
Minnesota	-	-	-	-	-	-	-	-
Mississippi	7%	6%	7%	7%	9%	-	-	-
Missouri	-	-	-	-	-	-	-	-
Montana	-	-	-	-	-	-	-	-
Nebraska	-	-	-	-	-	-	-	-
Nevada	29%	28%	30%	32%	32%	18%	18%	-
New Hampshire	-	-	-	-	-	-	-	-
New Jersey	-	-	-	-	-	-	-	-
New Mexico	-	-	-	-	-	-	-	-
North Carolina	12%	13%	13%	12%	12%	22%	23%	24%
North Dakota	-	-	-	-	-	-	-	-
Ohio	-	-	-	-	-	-	-	-
Oklahoma	-	-	-	-	-	-	-	-
Oregon	1%	1%	-	-	-	1%	-	7%
Pennsylvania	-	-	-	-	-	-	-	-
Rhode Island	-	-	-	-	-	-	-	-
South Carolina	-	-	-	-	-	-	-	-
South Dakota	-	-	-	-	-	-	-	-
Tennessee	-	-	-	-	-	-	-	-
Texas	-	-	-	-	-	-	-	-
Utah	-	-	-	-	-	-	-	-
Vermont	-	-	1%	1%	-	-	-	-
Virginia	-	-	-	-	-	-	-	-
Washington	-	-	-	-	-	-	-	-
West Virginia	-	-	-	-	-	-	-	-
Wisconsin	-	-	-	-	-	-	-	-
Wyoming	-	-	-	-	-	-	-	-

Table notes here

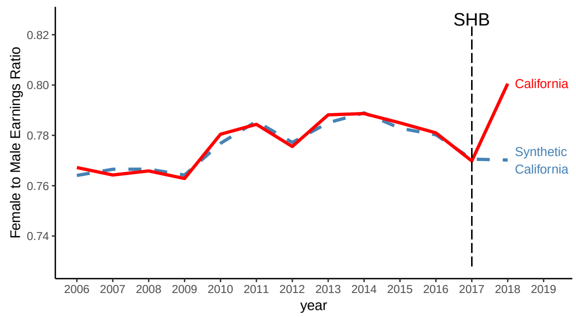
in trends and levels, the observed female to male earnings ratio. In the window after California adopts the state-wide SHB, its average female to male earnings ratio diverges from from 0.77 to 0.82. Not only does the California earnings ratio diverge from its synthetic counterpart, it also diverges from the level it has been close too for the past 11 years. Figure 10b visually illustrates the statistical precision of my synthetic control estimate. The red line represents the difference between California and its synthetic counterpart. The red line hovering around zero before the SHB illustrates that synthetic California is a close match for actual California. After the SHB, California's earnings ratio diverges from its synthetic counterpart. Only a few of the placebo states deviate close to as much as California post SHB. In Figure 10c I calculate the pre MSPE to post MSPE ratio for each placebo state and California. Notably, California has the highest MSPE ratio, over 3 times higher than the next highest MSPE ratio. There is probability 1/48=0.0208 that we would observe a MSPE ratio as large as California's if we randomly assigned a SHB to a state in the data[4]. Table 12 reports the point estimates and the permutation based p-values.

My synthetic control estimates suggest that California adopting a salary history ban increased average weekly female earnings relative to average weekly male earnings. I estimate the change in the earnings ratio from its synthetic counterpart to be .0239 which is a 10.4% decrease in the earnings gap. This finding suggests that the earnings ratio improved as a result of the SHB. I next explore potential mechanisms through which SHBs have caused the change in the earnings ratio.

[4]The p-value of 0.0208 is the lowest possible p-value given the number of states in my sample.

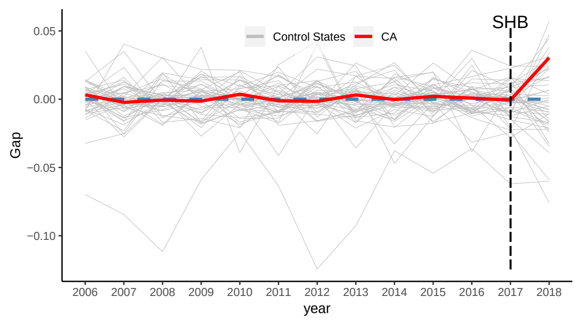
FIGURE 10.
California Female to Male Earnings Ratio

(a) Synthetic Control



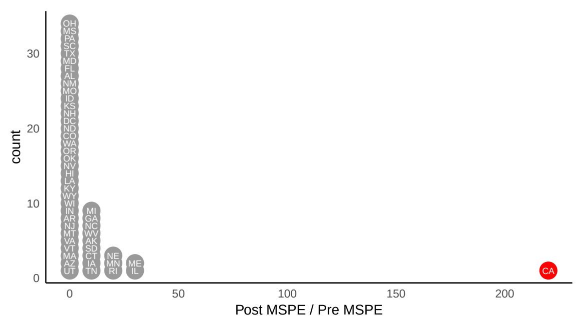
Notes: This figure shows the annual time series of California's earning (red) and the time series of the synthetic California (blue)

(b) Actual California - Synthetic California vs. Placebo States



Notes: This figure shows the difference between the synthetic control and the actual earnings ratio for each placebo state.

(c) MSPE Ratio Distribution



Notes: This figure shows the distribution of MSPE ratios for each of the control states and California. The ratio compares pre treatment versus post treatment fit for each state.

Weekly Earnings, Hours Worked, and Wages by Sex

The earnings ratio could change if there is a disproportional change in the level of either male or female earnings. Changes in earnings could be a result

TABLE 12.
Change in Average Weekly Female to Male Earnings Ratio

	State-Wide			
SHB	0.0303**			
P-Value	[0.020]			
	Younger Than 35	Older Than 35		
SHB	0.0115	0.0256**		
P-Value	[0.694]	[0.020]		
	Female Dominated Industries	Male Dominated Industries	Service Proving Industries	Good Producing Industries
SHB	0.0084	0.0419	0.0280**	0.0539**
P-Value	[0.429]	[0.102]	[0.020]	[0.020]

Table notes here

of changes in wages or changes in hours worked. For these reasons, I investigate average weekly earnings, hours worked, and hourly wages by gender.

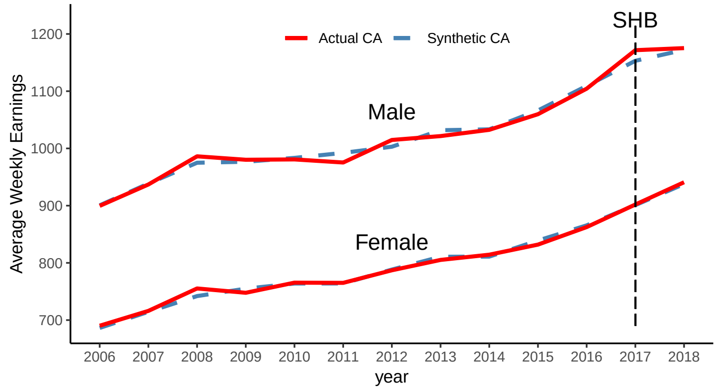
Figure 11a plots average weekly earnings data for California and its synthetic counterpart by sex. For both males and females, the trends and levels of the synthetic control group closely follow California's for the years prior to the SHB. After the SHB the male earnings slightly decrease and female earnings increase relative to each of their synthetic counterparts. The point estimates corresponding with Figure 11a are reported in Table 13 with permutation based p-values in brackets. While the increase in female earnings and decrease in male earnings are not significantly different from zero, we know they jointly significant. Figures 11b and 11c illustrate the precision of the synthetic control estimates. The solid red depicts California's deviation from its synthetic counterpart for each sex. The red line hovers around zero before the SHB, which illustrates that synthetic California is a close match for actual California for each sex. The red line lying well within the deviations observed in the post period for the placebo states illustrates that these slight deviations are likely due to noise.

TABLE 13.
Change in Average Weekly Earnings and Hours Worked

	Weekly Earnings		Weekly Hours Worked	
State-Wide				
	Female	Male	Female	Male
SHB	3.1296	3.1627	0.3279**	-0.299
P-Value	[0.857]	[0.837]	[0.041]	[0.388]
Male Dominated industries				
	Female	Male	Female	Male
SHB	54.2317	26.2456	0.3314*	0.2654
P-Value	[0.122]	[0.122]	[0.082]	[0.653]
Female Dominated Industries				
	Female	Male	Female	Male
SHB	-7.9003	-24.4772	0.2556	-0.8066
P-Value	[0.429]	[0.327]	[0.143]	[0.102]
Younger Than 35				
	Female	Male	Female	Male
SHB	25.9226	14.0185	0.6374**	-1.2215**
P-Value	[0.224]	[0.551]	[0.041]	[0.020]
Older Than 35				
	Female	Male	Female	Male
SHB	-5.6522	-7.811	0.1354	-0.5514
P-Value	[0.714]	[0.714]	[0.224]	[0.286]

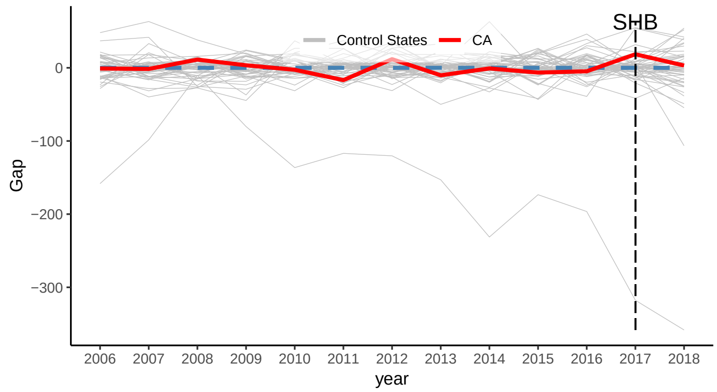
FIGURE 11.
Average Weekly Earnings by Gender

(a) Average Weekly Earnings by Gender



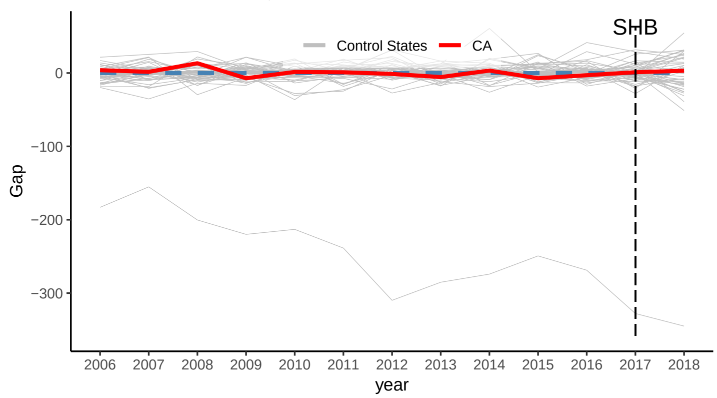
(b) Male Earnings

Actual California - Synthetic California vs Placebo States



(c) Female Earnings

Actual California - Synthetic California vs Placebo States

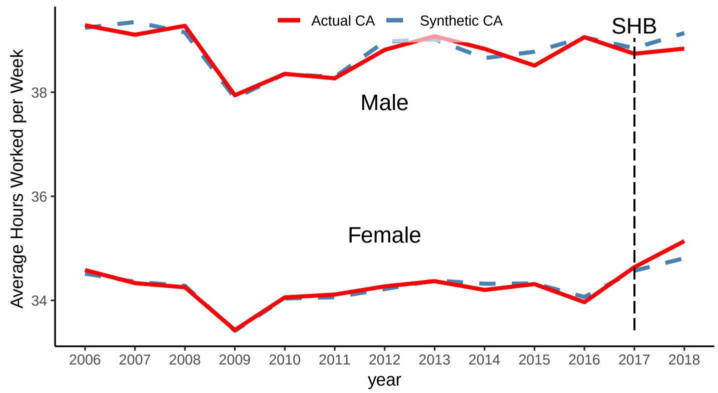


The analogous analysis for average weekly hours worked by sex is shown in Figure 12a. For both females and males, the synthetic control matches the levels and trends of average weekly hours worked. After the SHB, average weekly hours worked slightly decrease for males and slightly increase for females relative to their synthetic counterpart. The point estimates can be found in Table 13 with permutation based p-values in brackets. Females worked .37 hours more than their synthetic counterpart, and males worked .69 hours less than their synthetic counterpart. While the change in hours worked for males is larger than females, it is not statistically different from zero and is likely due to noise. Figures 12b and 12c illustrate the statistical precision of these point estimates. They show deviations from average weekly hours worked in California and its synthetic counterpart relative to the placebo states for each sex. For females, the synthetic counterpart matches actual hours worked very well. The tight match pre SHB and deviation post SHB results in a small p-value.

Figure 13a shows average weekly hourly wages for California and its synthetic counterpart by sex. The synthetic control group does a good job of matching in levels and trends for actual California's hourly wage for both sexes. After the SHB, average weekly hourly wage slightly decreases for males and slightly increases for females relative to their synthetic counterparts. Table 14 reports the point estimates and permutation based p-values in brackets. Figures 13b and 13c illustrate the statistical precision of these point estimates. Similar to the figures for earnings and wages, the solid red line represents the difference from California and its synthetic counterpart. For males, the red line hovers around zero both before and after the SHB. For females, the red line increases after the SHB, but the

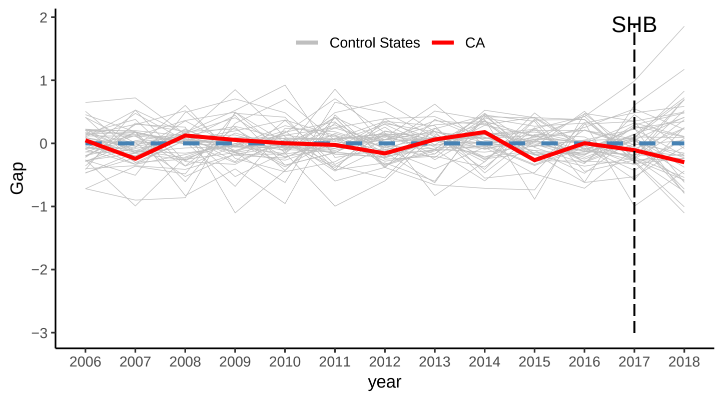
FIGURE 12.
Average Weekly Hours Worked by Gender

(a) Average Weekly Hours Worked by Gender



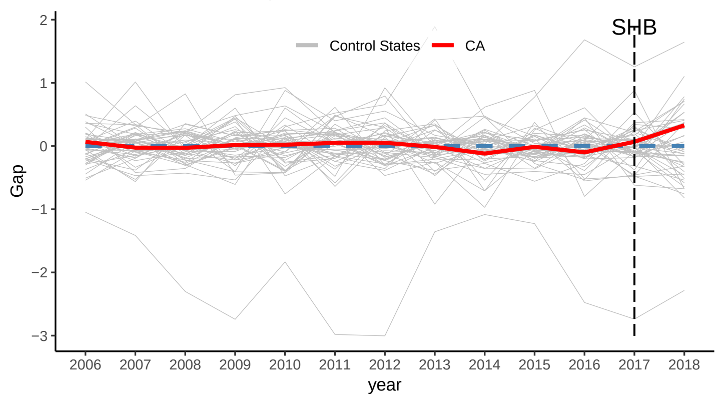
(b) Male Hours:

Actual - Synthetic California vs Placebo States



(c) Female Hours:

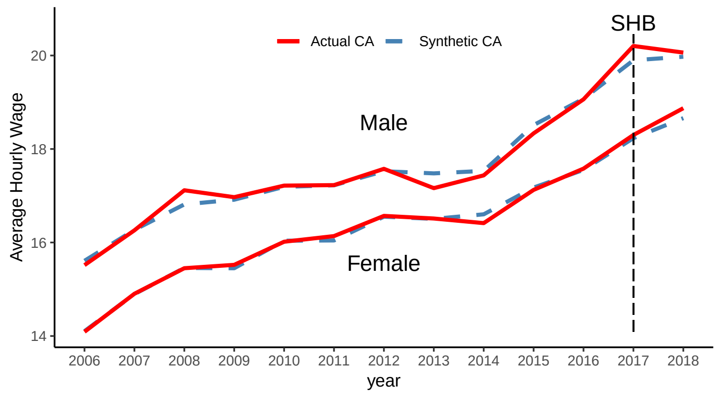
Actual California - Synthetic California vs Placebo States



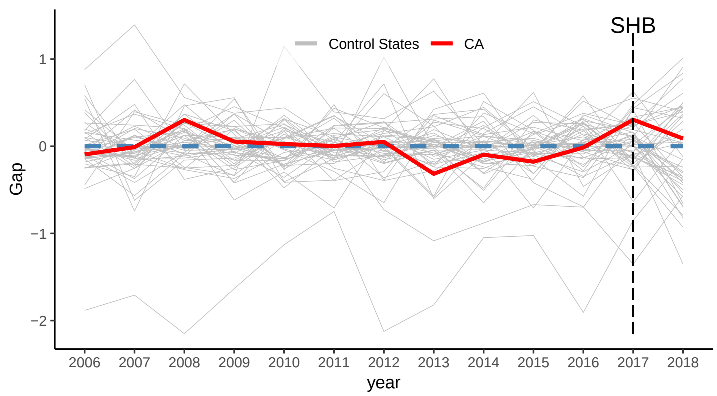
deviation is well within the deviations observed in the post period for the placebo states.

FIGURE 13.
Average Weekly Hourly Wage by Gender

(a) Average Weekly Hourly Wage by Gender



(b) Male Hourly Wage:
Actual California - Synthetic California vs Placebo States



(c) Female Hourly Wage:
Actual California - Synthetic California vs Placebo States

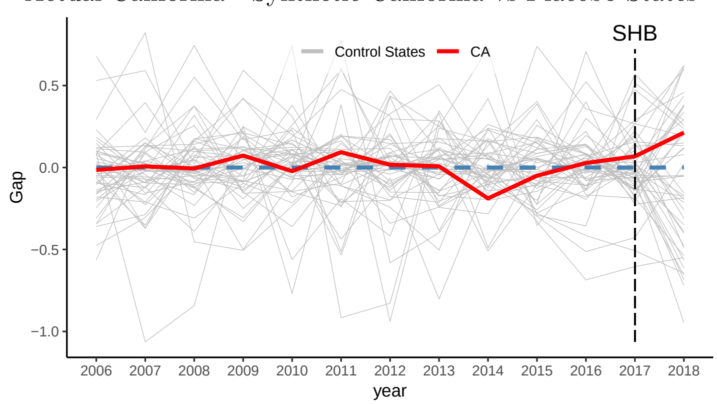


TABLE 14.
Change in Hourly Wage by Sex and Industry

	Female	Male
State-Wide		
	Female	Male
SHB	0.213	0.0889
P-Value	[0.224]	[0.857]
Male Dominated industries		
	Female	Male
SHB	0.9587**	0.3321
P-Value	[0.020]	[0.388]
Female Dominated Industries		
	Female	Male
SHB	0.0314	-0.5352
P-Value	[0.918]	[0.367]
Younger Than 35		
	Female	Male
SHB	0.4215	-0.1834
P-Value	[0.204]	[0.327]
Older Than 35		
	Female	Male
SHB	-0.0399	-0.3345
P-Value	[0.939]	[0.286]

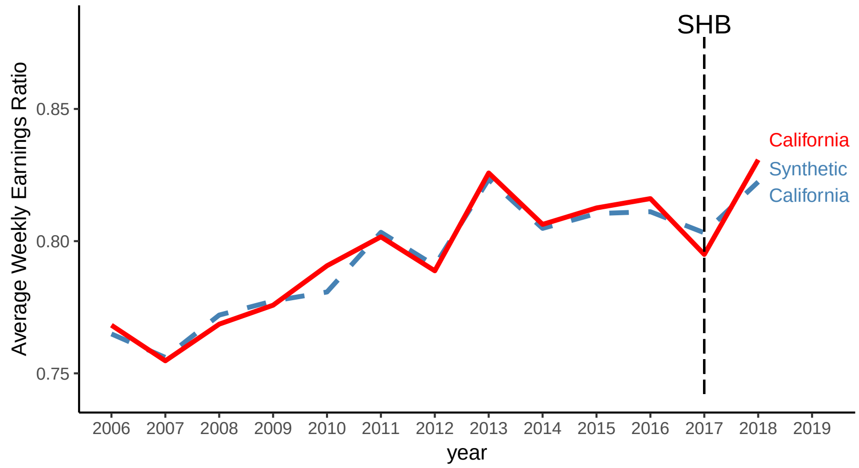
Industry of Employment

The above results are state-wide averages, however, it is possible that the effects of the SHB are not uniform across the state. For this reason, I investigate differential effects within male dominated and female dominated industries. As noted, I define an industry as female dominated if it has a composition of more than 50 percent females within that industry. Male dominated industries are defined analogously. I repeat the earnings ratio analysis within male and female dominated industries. Similar to above, I then investigate earnings, hours, and, wages within male and female dominated industries.

Figure 14 illustrates the California female to male average weekly earnings ratio and their synthetic counterpart for each industry. The synthetic California earnings ratio matches actual California in both levels and trends before the SHB in both male and female dominated industries. The match is slightly better for female dominated industries. After the SHB, the actual ratio does not deviate from the synthetic ratio for female dominated industries. In male dominated industries, however, the actual California earnings ratio increases relative to its synthetic counterpart. Table 12 reports the point estimates and the permutation based p-values. Figure 15 illustrates the statistical precision on these estimates by plotting the difference in California and each placebo state relative to its synthetic counterpart. Within female dominated industries, the difference between the actual California earnings ratio and its synthetic counterpart is close to zero both before and after the SHB. For male dominated industries, the difference between the California earnings ratio and its synthetic counterpart hovers around zero before the SHB. After the SHB, the ratio deviates from its synthetic counterpart by a large amount relative to the placebo state deviations. This results in a relatively small p-value for the changes in the earnings ratio within male dominated industries. Within female dominated industries the difference between the California earnings ratio and its synthetic counterpart hovers around zero before and after the SHB.

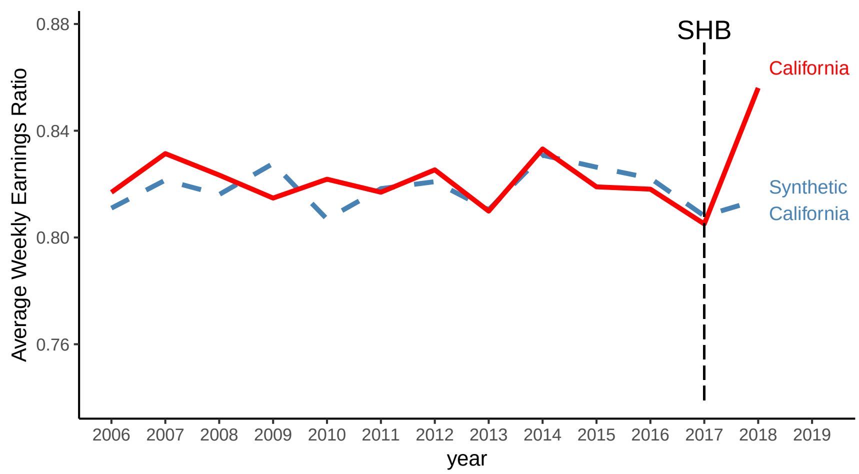
FIGURE 14.
Average Weekly Earnings Ratio by Industry Type

(a) Female Dominated Industries



This figure shows the average weekly earnings ratio in industries that are more than 50% female.

(b) Male Dominated Industries

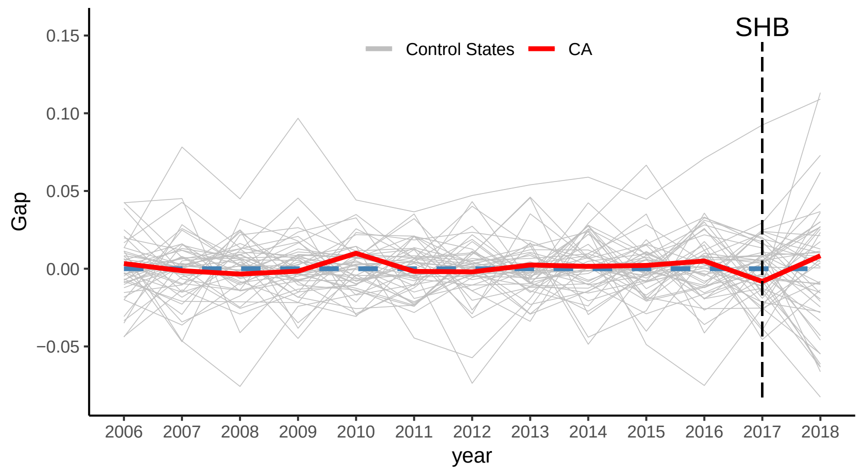


This figure shows the average weekly earnings ratio in industries that are more than 50% male.

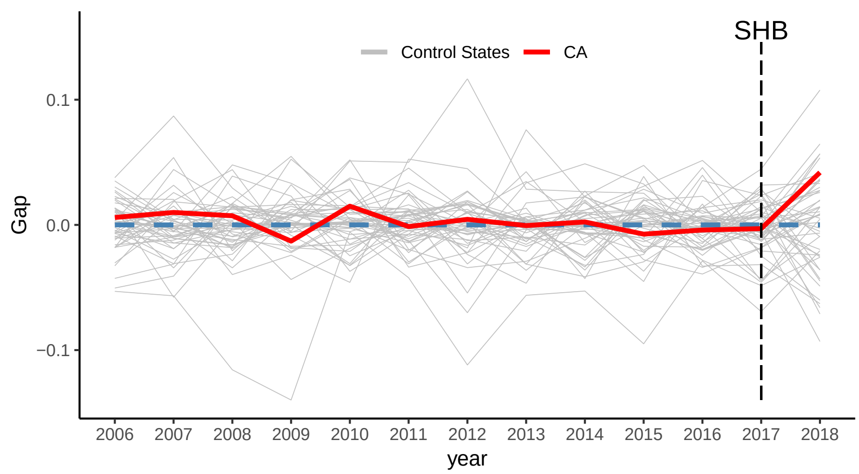
These results suggest the change in the state-wide earnings ratio is composed of larger changes in male dominated industries and relatively smaller changes within female dominated industries. I estimate the increase in the female to male earnings ratio to be .0579, which is a 32% decrease in the earnings gap (within

FIGURE 15.
Actual California - Synthetic California vs Placebo States by Industry Type

(a) Female Dominated Industries



(b) Male Dominated Industries



male dominated industries). Next I turn to mechanisms of these findings with in male and female dominated industries.

Weekly Earnings, Hours Worked, and, Wages by Sex and Industry

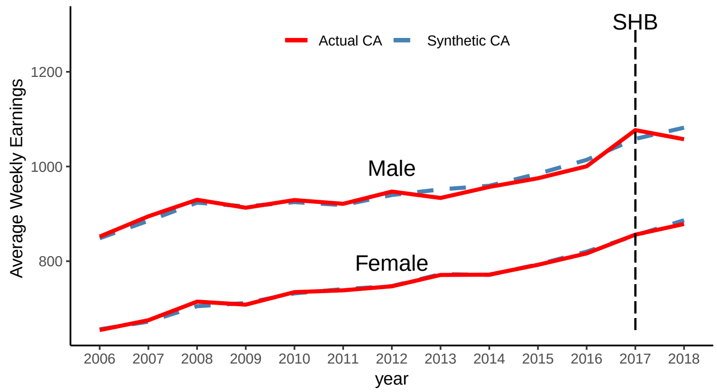
The earnings ratio within industries could change if there is a disproportional change in the level of either male or female earnings within industries. Changes in earnings could be a results of changes in wages or changes in hours worked. For these reasons, I investigate average weekly earnings, hours worked, and hourly wages by gender within male and female dominated industries.

Figure 16a illustrates male and female average weekly earnings in female dominated industries compared to their synthetic counterpart. For both sexes, the synthetic California matches actual California earnings in both levels and trends before the SHB. After the SHB, female earnings in female dominated industries continue to match their synthetic counterpart. Male earnings in female dominated industries deviate slightly below their synthetic counterpart. Table 13 provides the point estimates and the permutation based p-values in brackets. The p-values for both male and female earnings within female dominated industries indicate that I cannot reject the null hypothesis that the SHB caused no changes. Figures 16b and 16c illustrate the gap between California average weekly earnings and its synthetic counterpart vs the placebo states. For both females and males, post SHB deviations lie well within deviations of the placebo states.

Figure 17a illustrates male and female average weekly earnings in male dominated industries compared to their synthetic counterpart. Again, for both sexes, the synthetic California matches actual California earnings in both levels and trends before the SHB. After the SHB, male earnings in male dominated industries continue to match their synthetic counterpart. Female earnings in male dominated industries increase relative to their synthetic counterpart. Figure 17b and 17c illustrate the gap between California weekly earnings within male dominated industries and their synthetic counterpart. Both the female and male earnings gap hover around zero before the SHB, indicating that the synthetic control is a good match. After the SHB, the female earnings' deviation from their synthetic

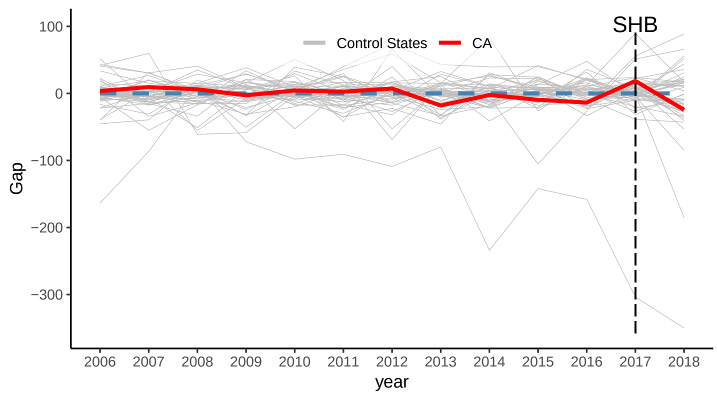
FIGURE 16.
Average Weekly Earnings by Gender Within Female Dominated Industries

(a) Average Weekly Earnings by Gender



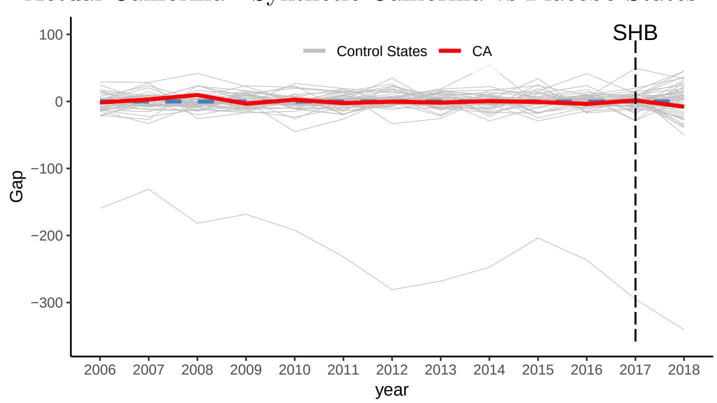
(b) Male Earnings

Actual California - Synthetic California vs Placebo States



(c) Female Earnings

Actual California - Synthetic California vs Placebo States



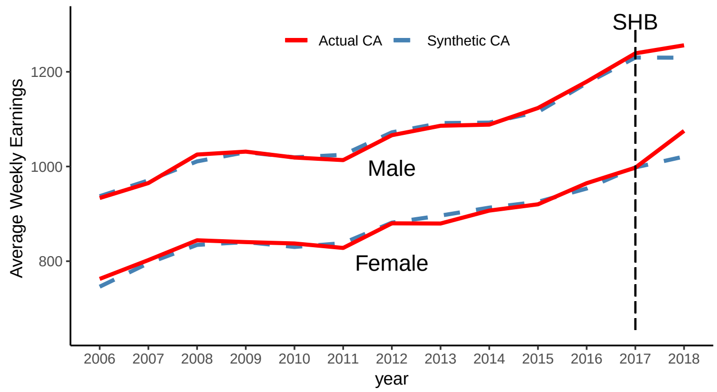
counterpart is among the highest of the placebo states. The point estimates and the permutation based p-values in brackets can be seen in Table 13.

Figure 18a illustrates male and female average weekly hours worked in female dominated industries compared to their synthetic counterpart. For both sexes, the synthetic California matches actual California hours worked in both levels and trends before the SHB. After the SHB, female hours worked in female dominated industries increase 1 hour per week relative to their synthetic counterpart. Male hours worked in female dominated industries continue to match their synthetic counterpart. Table 13 provides the point estimates and the permutation based p-values in brackets. The p-value for male hours worked within female dominated industries indicate that I cannot reject the null hypothesis that the SHB caused no changes. Figures 18b and 18c illustrate the gap between California average weekly earnings and its synthetic counterpart vs the placebo states. For females, the post SHB deviation is among the largest of deviations for the placebo states.

Figure 19a illustrates male and female average hours worked in male dominated industries compared to their synthetic counterpart. For both sexes, the synthetic California matches actual California worked in both levels and trends before the SHB. After the SHB, female hours worked in male dominated industries increase relative to their synthetic counterpart. Male weekly hours worked also slightly increase in male dominated industries post SHB. Table 13 provides the point estimates and the permutation based p-values in brackets. Figures 19b and 19c illustrate the gap between actual California's average weekly hours worked and its synthetic counterpart vs the placebo states. For females, the post SHB deviation is among the largest deviations of the placebo states. This results in a relatively

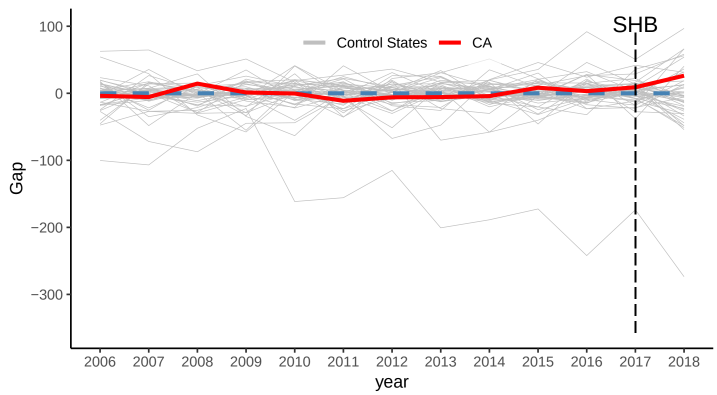
FIGURE 17.
Average Weekly Earnings by Gender Within Male Dominated Industries

(a) Average Weekly Earnings by Gender



(b) Male Earnings

Actual California - Synthetic California vs Placebo States



(c) Female Earnings

Actual California - Synthetic California vs Placebo States

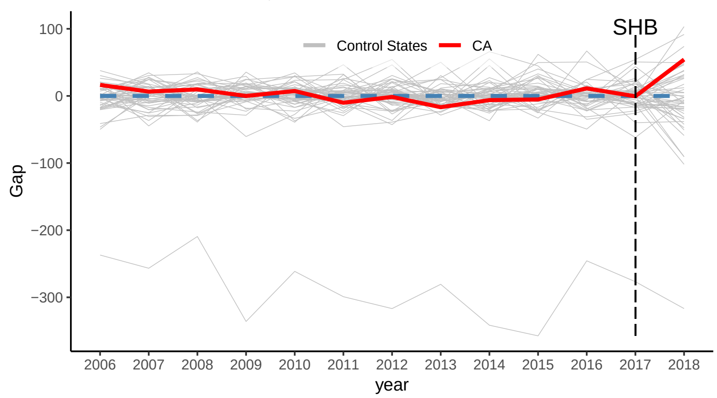
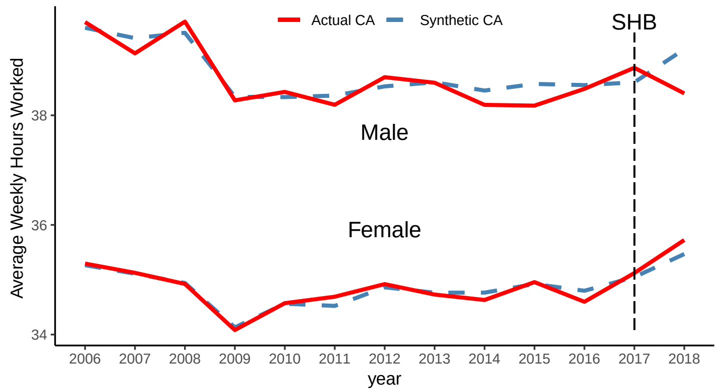


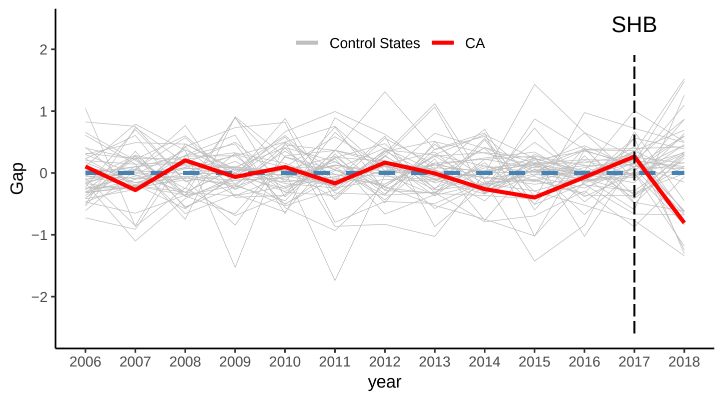
FIGURE 18.
Average Weekly Hours Worked by Gender Within Female Dominated Industries

(a) Average Weekly Hours Worked by Gender



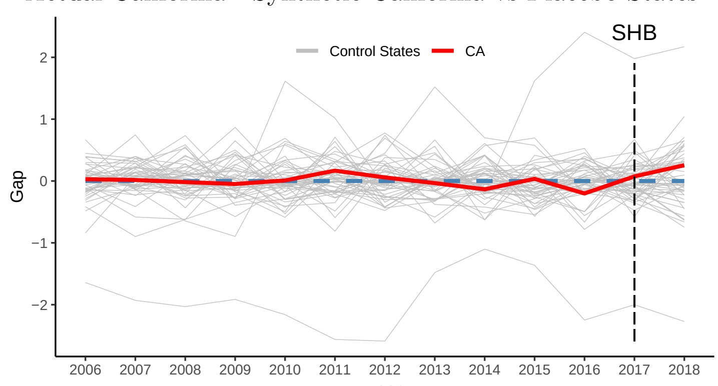
(b) Male Hours Worked

Actual California - Synthetic California vs Placebo States



(c) Female Hours Worked

Actual California - Synthetic California vs Placebo States



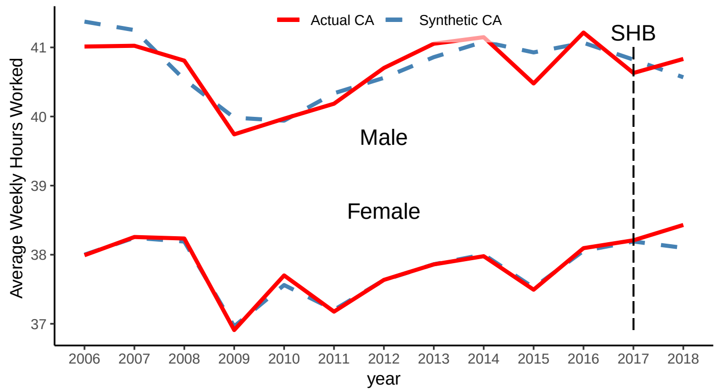
small p-value. For males, the p-value suggests the point estimate is not statistically distinguishable from zero.

Figure 20a illustrates male and female average hourly wage in female dominated industries compared to their synthetic counterpart. For both sexes, the synthetic California matches the overall trend of actual California but with less variation. After the SHB, female hourly wages in female dominated industries slightly increase relative to their synthetic counterpart. On the other hand, male hourly wages in male dominated industries slightly decrease relative to their synthetic counterpart. Table 14 provides the point estimates and the permutation based p-values in brackets. Figures 20b and 20c illustrate the gap between actual California's average hourly wage and its synthetic counterpart vs the placebo states. The poor match pre SHB for both males and females makes their deviation post SHB indistinguishable from zero.

Figure 21a illustrate male and female average hourly wages in male dominated industries compared to their synthetic counterpart. For both sexes, the synthetic California matches actual California in both levels and trends before the SHB. After the SHB, female hourly wage in male dominated industries increase relative to its synthetic counterpart. Male hourly wage in male dominated industries decrease slightly relative to its synthetic counterpart. Table 14 provides the point estimates and the permutation based p-values in brackets. Figures 21b and 21c illustrate the gap between actual California's average weekly earnings and its synthetic counterpart vs the placebo states. For females, The deviation post SHB is among the largest of the placebo states. This combined with a relatively good pretreatment fit results in a small p-value. The male deviation post SHB is well

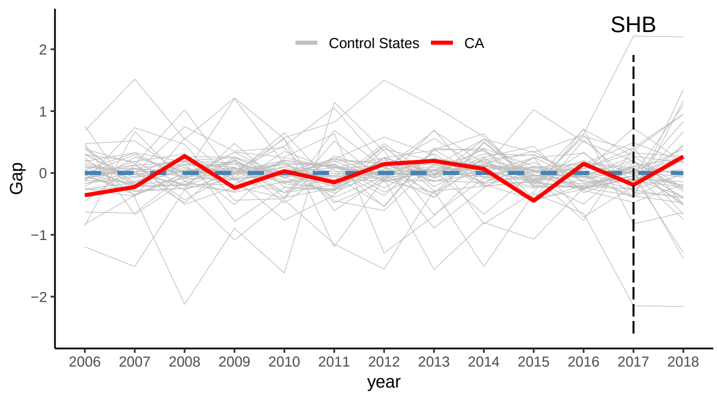
FIGURE 19.
Average Weekly Hours Worked by Gender Within Male Dominated Industries

(a) Average Weekly Hours Worked by Gender



(b) Male Hours Worked

Actual California - Synthetic California vs Placebo States



(c) Female Hours Worked

Actual California - Synthetic California vs Placebo States

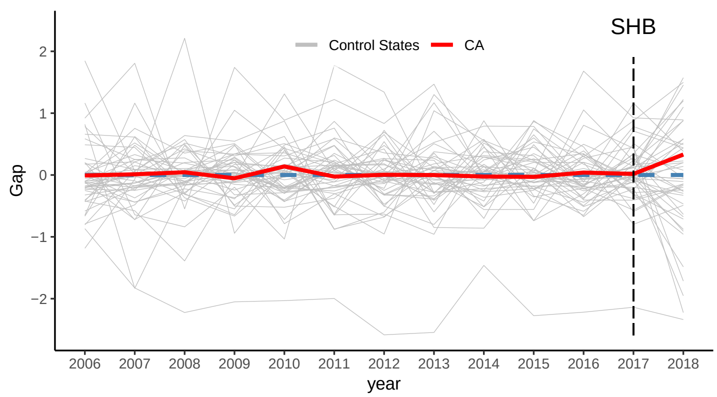
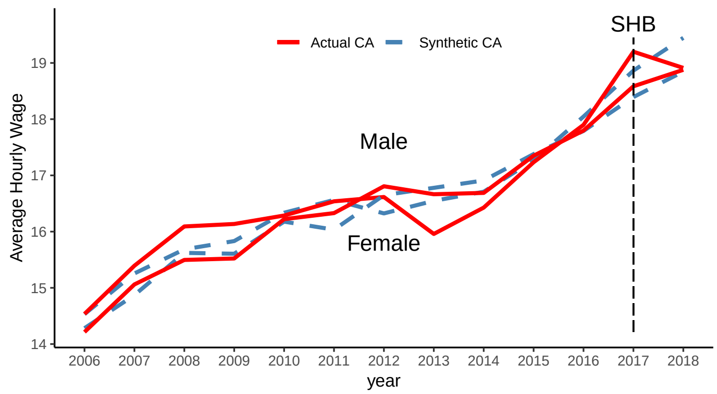


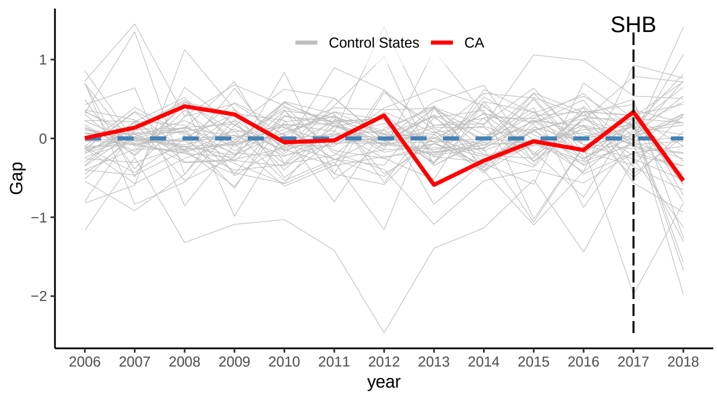
FIGURE 20.
Average Hourly Wage by Gender Within Female
Dominated Industries

(a) Average Hourly Wage by Gender



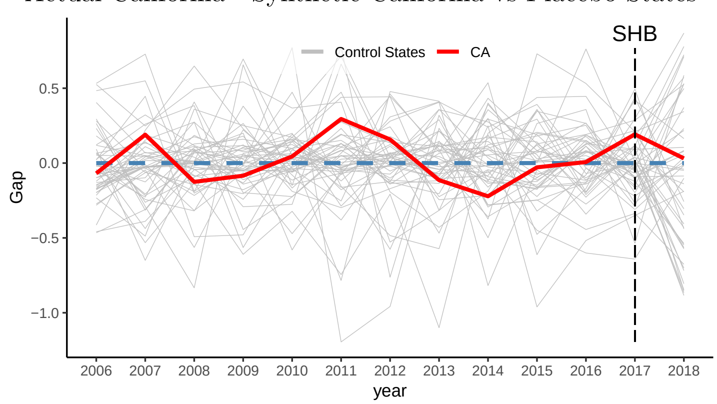
(b) Male Hourly Wage

Actual California - Synthetic California vs Placebo States



(c) Female Hourly Wage

Actual California - Synthetic California vs Placebo States



within the deviations of the placebo states and thus is not distinguishable from zero.

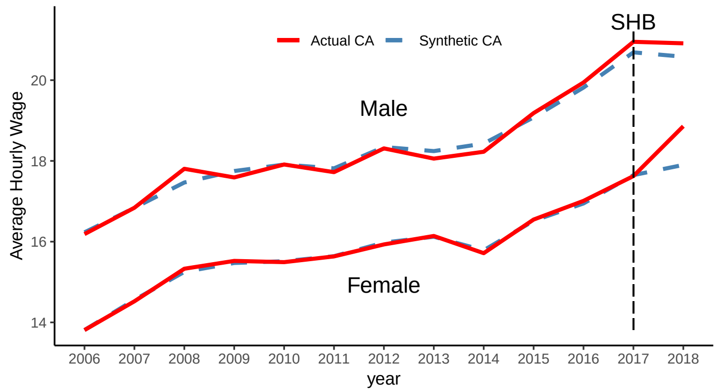
The above analysis informs which industries are contributing to the statewide increase in the earnings ratio. Within female dominated industries, the improvement in the earnings ratio is smaller than at the state level. This result is driven by a smaller decrease for females than males in the level of weekly earnings. Wages slightly increase for females and decrease for males. This combined with a larger decrease in hours worked by males than by females is consistent with the changes in weekly earnings by sex in female dominated industries referenced above. Within male dominated industries, the increase in the earnings ratio is much larger than the state level. This is driven by a larger increase in weekly earnings for females than for males. The changes in weekly earnings are driven by the joint effect of increased wages and hours for females and an increase in hours that off-set a slight decrease in wages for males.

By NAICS industries

The North American Industry Classification System (NAICS) is a slightly more common way to define industry splits. For this reason I repeat the analysis above for goods producing and service providing industries classified according to the (NAICS) codes included in the CPS. Goods producing industries are a subset of male dominated industries, while service producing industries include both female and male dominated industries. Figure 22 illustrates the earnings ratio and its synthetic counterpart for goods producing and service providing industries. In the goods producing industries, the synthetic California matches the actual California earnings in levels and trends almost perfectly. The service providing industries

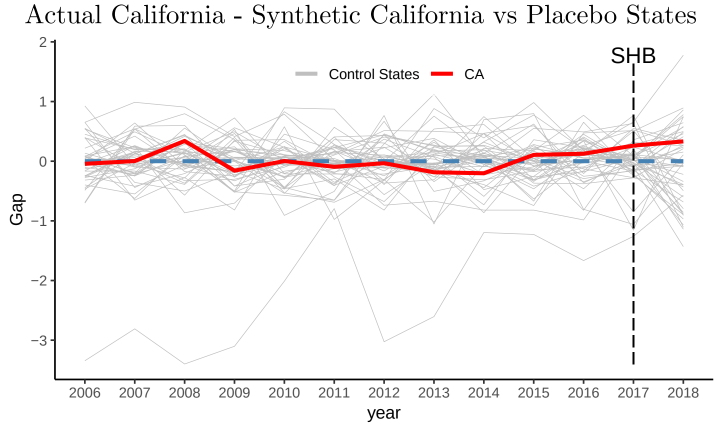
FIGURE 21.
Average Hourly Wage by Gender Within Male
Dominated Industries

(a) Average Hourly Wage by Gender



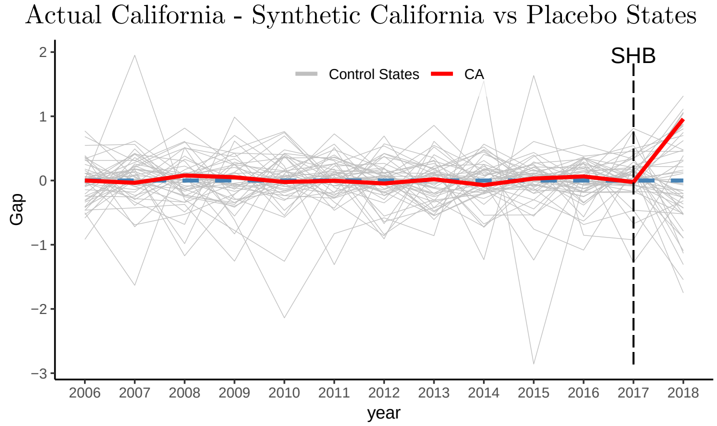
(b) Male Hourly Wage

Actual California - Synthetic California vs Placebo States



(c) Female Hourly Wage

Actual California - Synthetic California vs Placebo States



also provide a decent, but not as precise match in levels and trends of the female to male earnings ratio. The point estimates and permutation based p-values are reported in Table 12. Figure 23 illustrates the precision of these estimates. The almost exact match pre SHB and slight deviation post SHB for goods producing industries result in a large MSPE ratio and relatively small p-value. The service providing industry has a slightly noisier match. It has a considerable deviation relative to its synthetic counterpart amongst the placebo states which results in a small p-value.

The effect of the SHB becomes increasingly larger as populations decrease in size. The earnings ratio increases at the state level after the SHB. Within male dominated industries, a subset of the statewide population, the earnings ratio increases by a larger amount. Within goods producing industries, a subset of male dominated industries, the increase in the earnings ratio is larger yet. These results suggest the effects of the SHB are not uniform across subsets of the statewide population. Rather, they are largest amongst goods producing industries, all of which are composed by 50 % or more male workers.

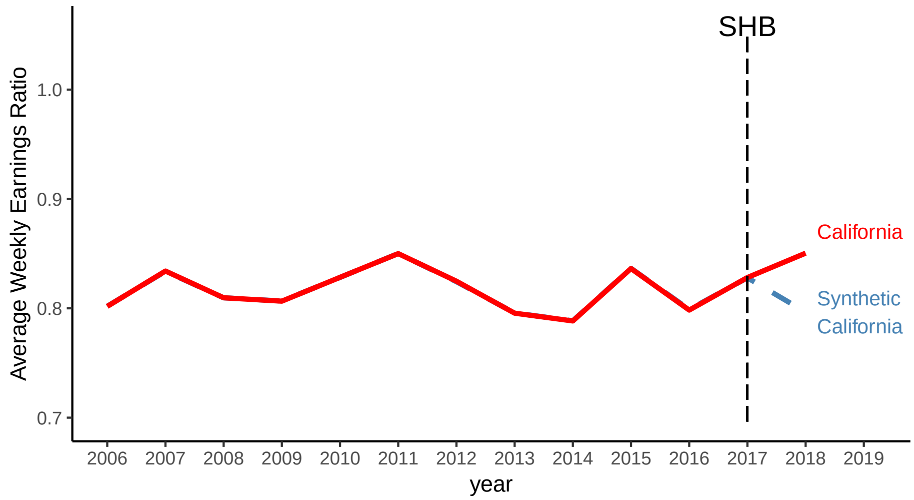
By Age

One argument for SHBs is their potential to eliminate path dependence. The length of compensation history will vary by an individual's time spent in the labor force. For this reason, I investigate the effect of the SHB on different age groups. I split the population at age 35.

Figure 24 plots the average weekly earnings ratio by age. For individuals younger than 35, the synthetic California matches the actual California earnings ratio in levels, but not trends. The variation in the data causes the synthetic

FIGURE 22.
Average Weekly Earnings Ratio by Industry Type

(a) Goods Producing Industries



Notes: This figure shows the average weekly earnings ratio in industries that are classified by NAICS as goods producing.

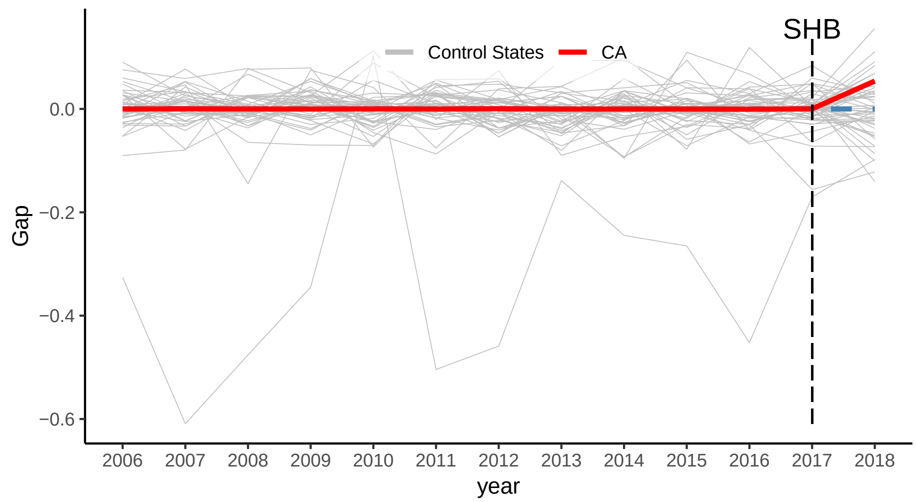
(b) Service Providing Industries

Notes: This figure shows the average weekly earnings ratio in industries that are classified by NAICS as service providing.

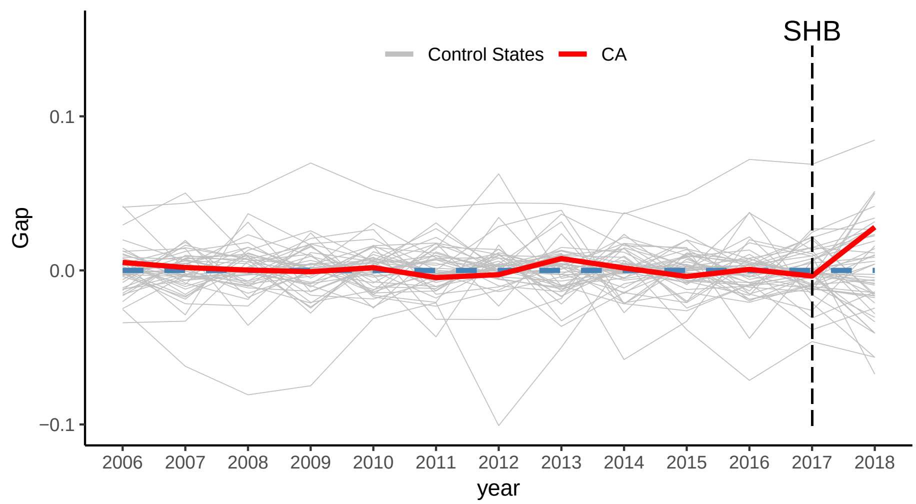
control approach to construct a poor match. After the SHB, the actual earnings ratio increases by more than its synthetic counterpart. The permutation based p-values suggest that this observed deviation is due to statistical noise, and there is little evidence supporting an actual causal deviation.

FIGURE 23.
Actual California - Synthetic California vs Placebo States by Industry Type

(a) Goods Producing Industries



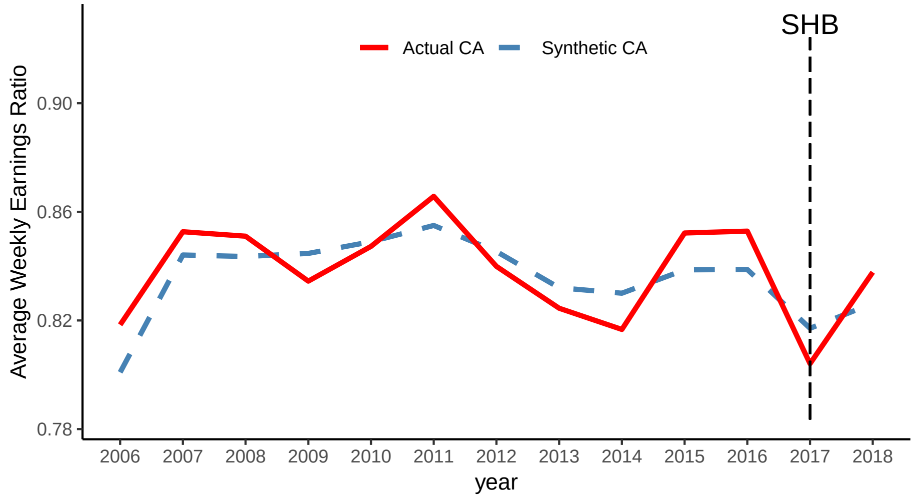
(b) Service Providing Industries



For individuals older than 35, the synthetic group closely mirrors actual California in both trends and levels from 2006-2017. After treatment, the actual California earnings ratio increases relative to its synthetic counterpart. The point estimates and permutation based p-values are included in Table 12. Figure 25 illustrates the precision of the estimates. For individuals older than 35 the

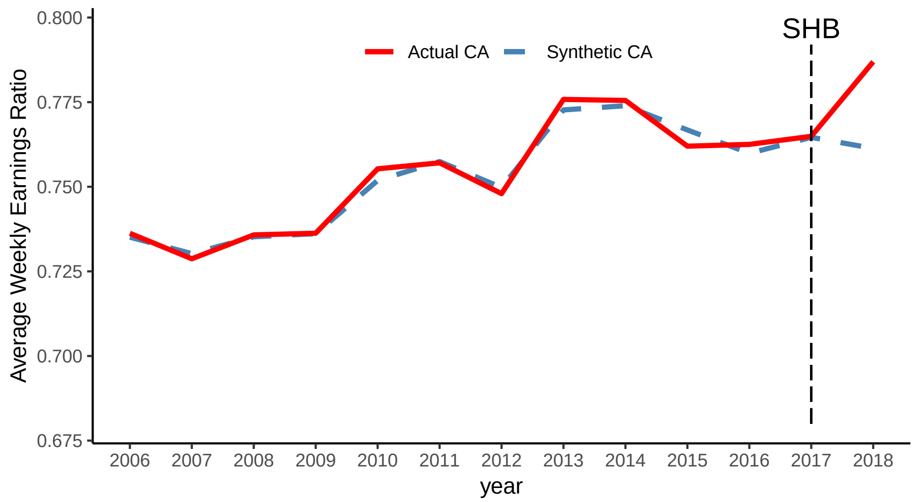
FIGURE 24.
Average Weekly Earnings Ratio by Age

(a) Younger Than 35



Notes: This figure shows the average weekly earnings ratio in California among individuals younger than 35 relative to its synthetic counterpart

(b) Older Than 35

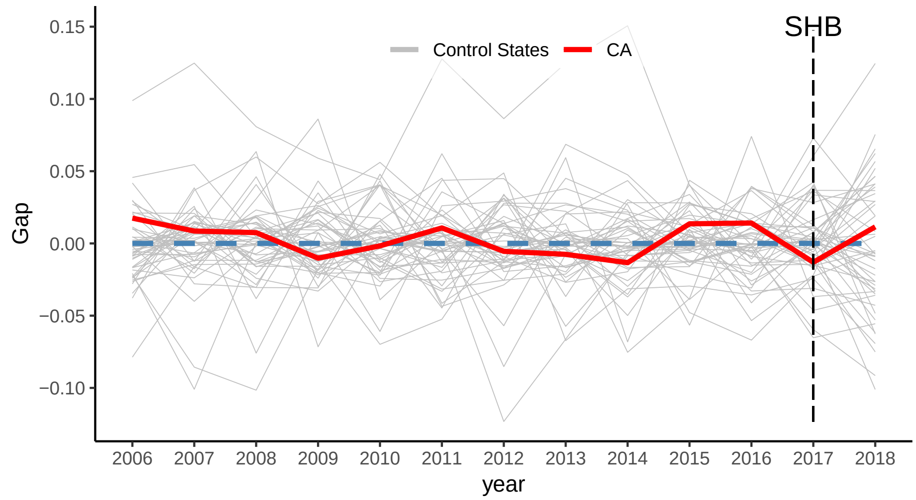


Notes: This figure shows the average weekly earnings ratio in California among individuals older than 35 relative to its synthetic counterpart.

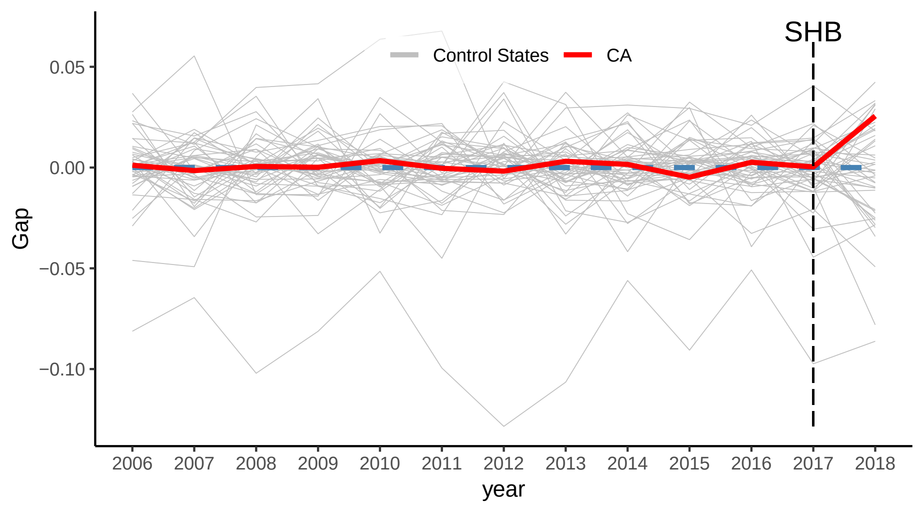
California earnings ratio is matched well by its synthetic counterpart prior to the SHB. After the SHB the deviation is among the largest of the placebo states, resulting in a large MSPE ratio and small p-value.

FIGURE 25.
Actual California - Synthetic California vs Placebo States by Age

(a) Younger Than 35.



(b) Older Than 35



Weekly Earnings, Hours Worked, and Wages by Sex and Age

The earnings ratio within industries could change if there is a disproportional change in the level of either male or female earnings within age groups. Changes in earnings could be a result of changes in wages or changes in hours worked. For these reasons, I investigate average weekly earnings, hours worked, and hourly wages by gender within old and young age groups.

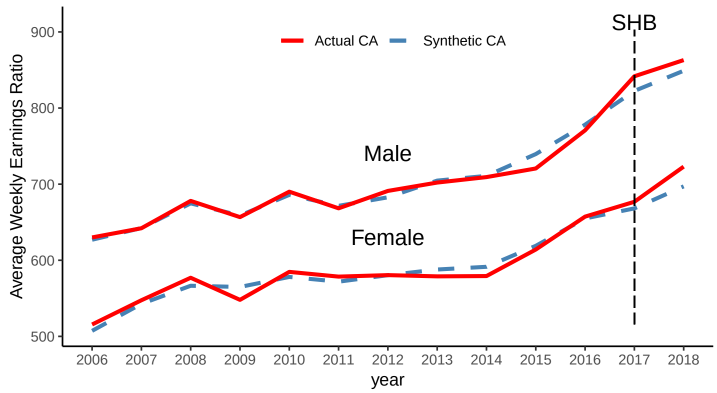
Figure 26a illustrates male and female average weekly earnings for individuals younger than 35 compared to their synthetic counterpart. For both sexes, the synthetic California matches actual California earnings in both levels and trends before the SHB. After the SHB, female earnings in female dominated industries continue to match their synthetic counterpart. Male earnings in female dominated industries deviate slightly below their synthetic counterpart. Table 13 provides the point estimates and the permutation based p-values in brackets. The p-values for both male and female earnings within female dominated industries indicate that the point estimates are not statistically different from zero. Figures 26b and 26c illustrate the gap between actual California average weekly earnings and their synthetic counterpart vs the placebo states. For both females and males, post SHB deviations lie well within deviations of the placebo states.

Figure 27a illustrates male and female average weekly earnings for individuals older than 35 compared to their synthetic counterpart. Again, for both sexes, the synthetic California matches actual California earnings in both levels and trends before the SHB. After the SHB, male earnings among individuals older than 35 deviate slightly below their synthetic counterpart while female earnings increase slightly relative to their synthetic counterpart. Table 13 reports the point estimates and permutation based p-values in brackets. Figures 27b and 27c illustrate the gap between California weekly earnings among individuals above 35 and their synthetic counterpart vs the placebo states.

Figure 28a illustrates male and female average weekly hours worked among individuals below age 35 compared to their synthetic counterpart. For both sexes,

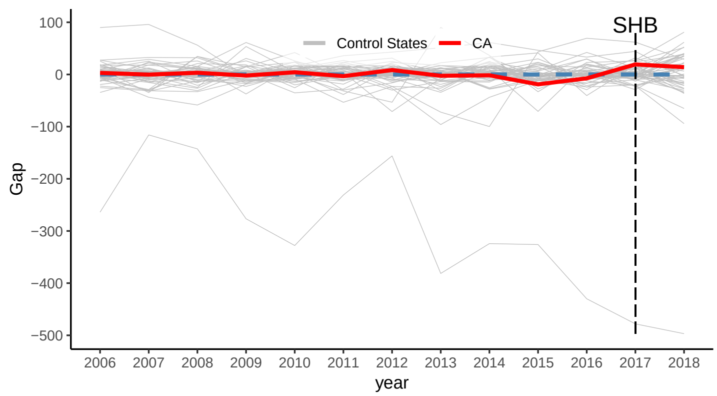
FIGURE 26.
Average Weekly Earnings by Gender Among Individuals Below Age 35

(a) Average Weekly Earnings by Gender



(b) Male Weekly Earnings

Actual California - Synthetic California vs Placebo States



(c) Female Weekly Earnings

Actual California - Synthetic California vs Placebo States

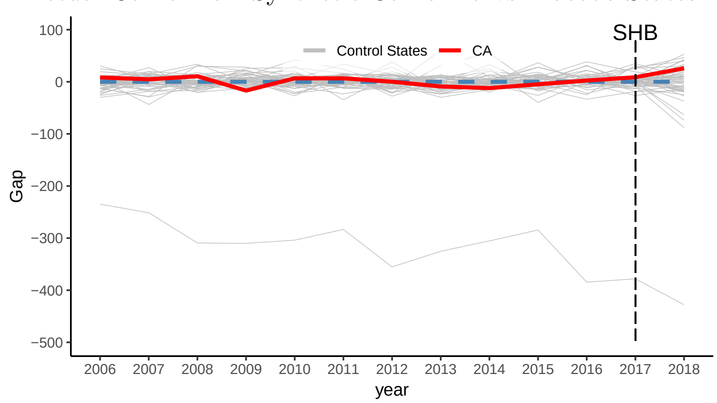
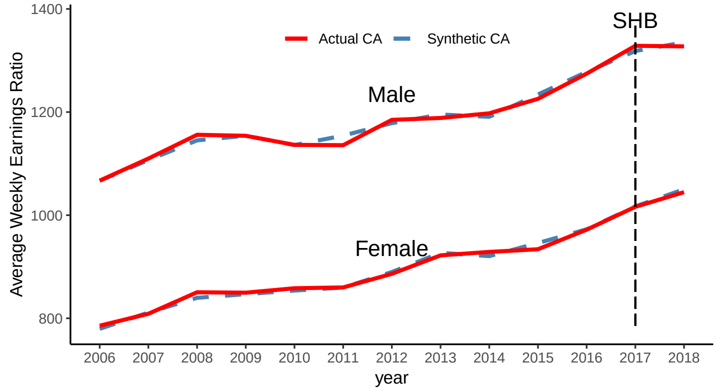


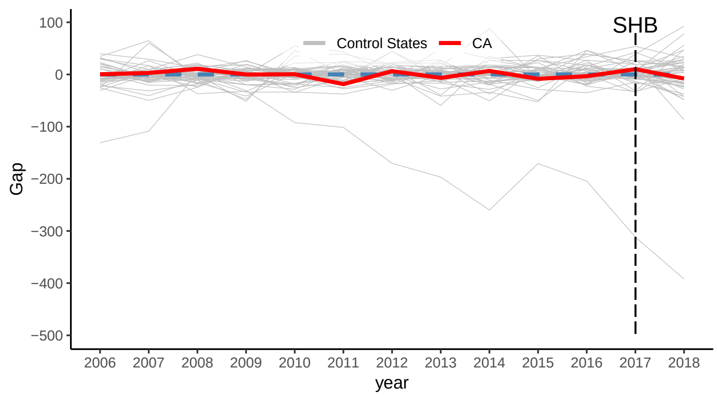
FIGURE 27.
Average Weekly Earnings by Gender Among Individuals Above Age 35

(a) Average Weekly Earnings by Gender



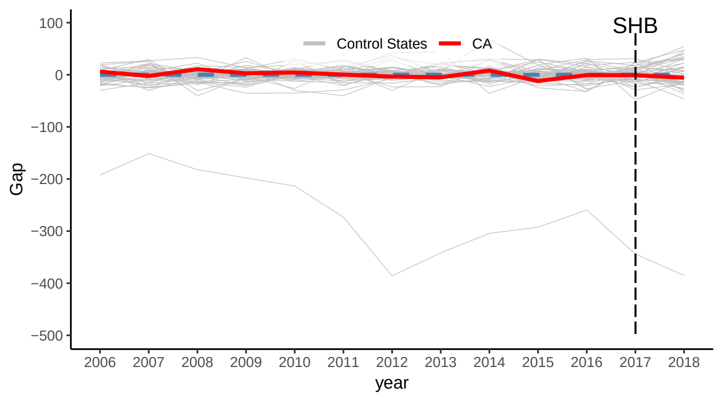
(b) Male Weekly Earnings

Actual California - Synthetic California vs Placebo States



(c) Female Weekly Earnings

Actual California - Synthetic California vs Placebo States



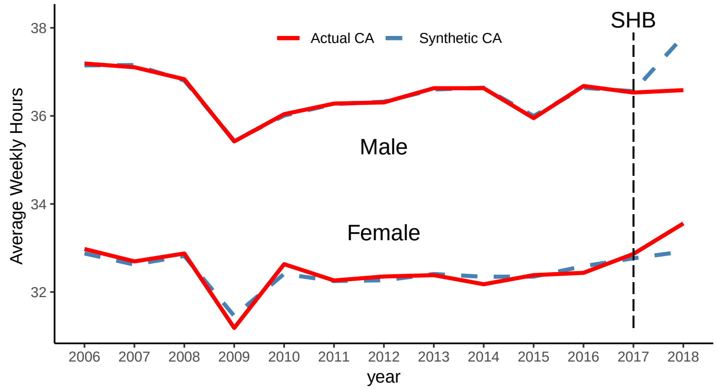
the synthetic California matches actual California hours worked in both levels and trends before the SHB. After the SHB, female hours worked continue to match their synthetic counterpart. Male hours worked continue on the same trajectory, but the synthetic counterpart increases after the SHB. Table 13 provides the point estimates and the permutation based p-values in brackets. Figures 28b and 28c illustrate statistical precision of the point estimates. They show the the gap between California average weekly hours worked and its synthetic counterpart vs the placebo states.

Figure 29a illustrates male and female average weekly hours worked among individuals above age 35 compared to their synthetic counterpart. For both sexes, the synthetic California matches actual California hours worked in both levels and trends before the SHB. After the SHB, female hours worked increase slightly relative to their synthetic counterpart. Male hours decrease slightly relative to their synthetic counterpart after the SHB. Table 13 provides the point estimates and the permutation based p-values in brackets. Figures 29b and 29c illustrate statistical precision of the point estimates. They show the gap between actual California average weekly hours worked and its synthetic counterpart vs the placebo states. Noticeably, for both males and females, the deviation from the synthetic counterpart is well within the deviations among placebo states.

Figure 30a illustrates male and female average hourly wage among individuals below age 35 compared to their synthetic counterpart. For both sexes, the synthetic California matches actual California average hourly wages in both levels and trends before the SHB. After the SHB, female average hourly wage among individuals below age 35 increases relative to their synthetic counterpart. Male average hourly wage among individuals below age 35 decreases slightly relative to their synthetic

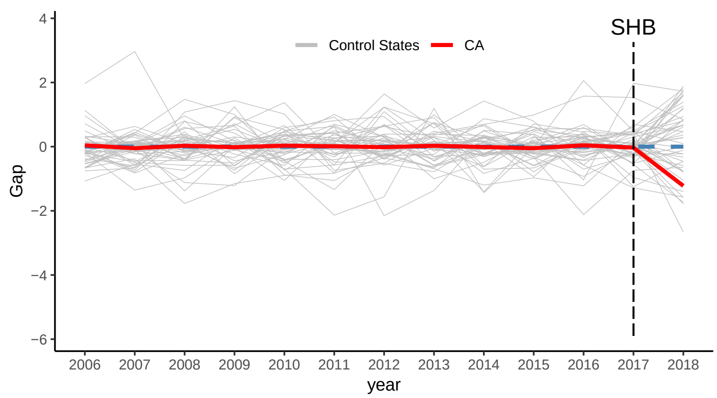
FIGURE 28.
Average Weekly Hours Worked by Gender Among Individuals Below 35

(a) Average Weekly Hours Worked by Gender



(b) Male Hours Worked

Actual California - Synthetic California vs Placebo States



(c) Female Hours Worked

Actual California - Synthetic California vs Placebo States

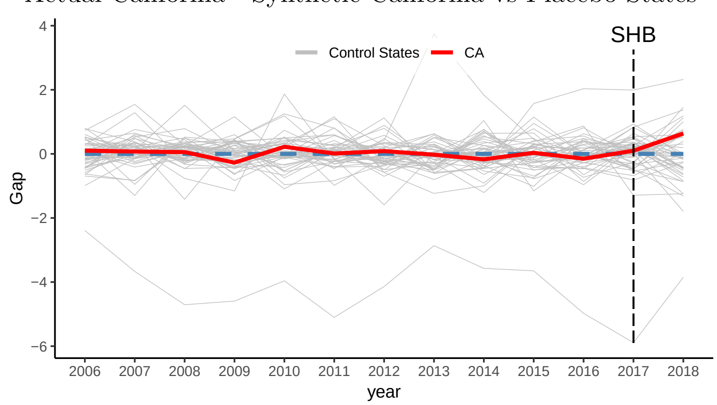
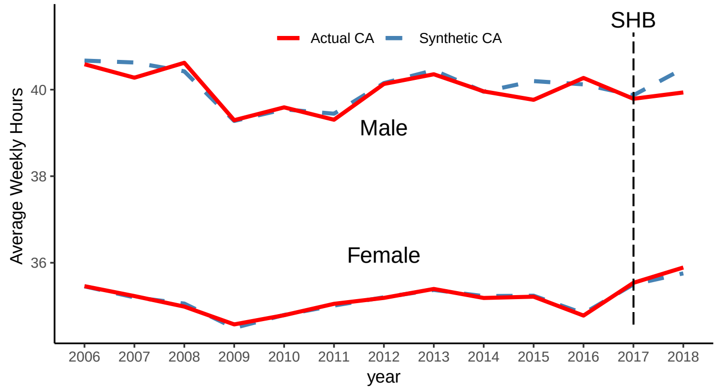


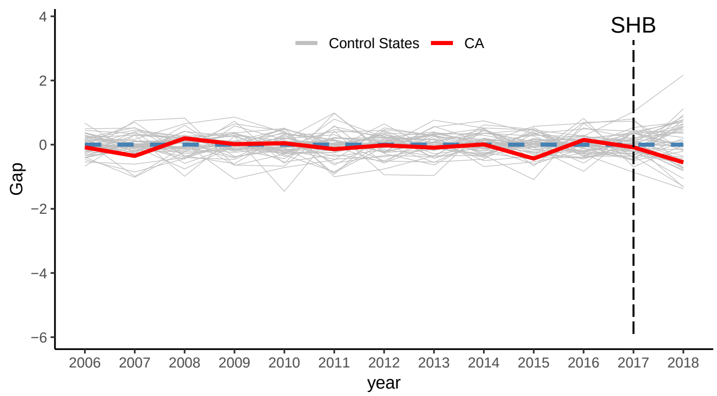
FIGURE 29.
Average Weekly Hours Worked by Gender Among Individuals Above 35

(a) Average Weekly Hours Worked by Gender



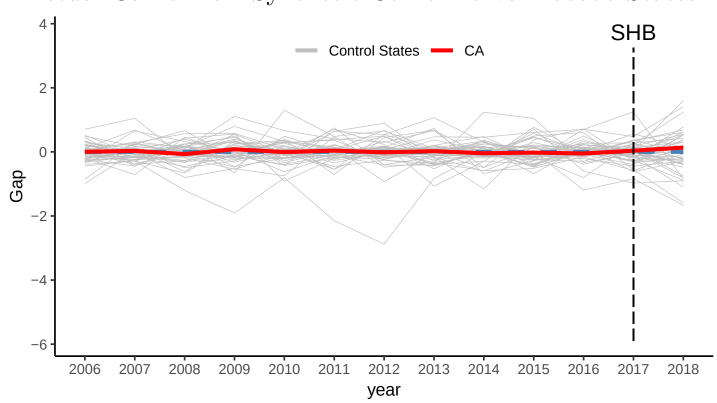
(b) Male Hours Worked

Actual California - Synthetic California vs Placebo States



(c) Female Hours Worked

Actual California - Synthetic California vs Placebo States



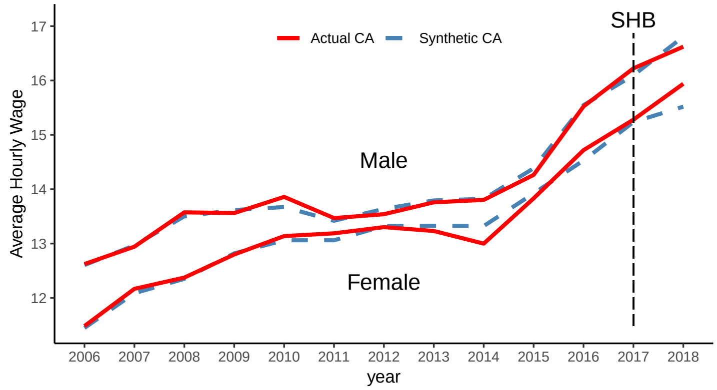
counterpart. Table 14 provides the point estimates and the permutation based p-values in brackets. Figures 30b and 30c illustrate the the statistical precision of these point estimates. They show the gap between actual California average hourly wages and their synthetic counterpart vs the placebo states. Deviations from their synthetic counterpart for both sexes post SHB are well within the deviations of the placebo states and thus is not distinguishable from zero.

Figure 31a illustrates male and female average hourly wage among individuals above age 35 compared to their synthetic counterpart. For both sexes, the synthetic California matches actual California average hourly wage in both levels and trends before the SHB. After the SHB, female average hourly wage among individuals above age 35 increases relative to their synthetic counterpart. Male average hourly wage among individuals above age 35 decreases relative to their synthetic counterpart. Table 14 provides the point estimates and the permutation based p-values in brackets. Figures 30b and 30c illustrate the the statistical precision of these point estimates. They show the gap between California average hourly wages and their synthetic counterpart vs the placebo states. Deviations from their synthetic counterpart for both sexes post SHB are well within the deviations of the placebo states and thus are not distinguishable from zero.

The above analysis suggests the increase in the earnings ratio among individuals above age 35 is driven by the joint increase in female earnings and decrease in male earnings. The increase in female earnings is likely a result of females increasing average hours worked per week.

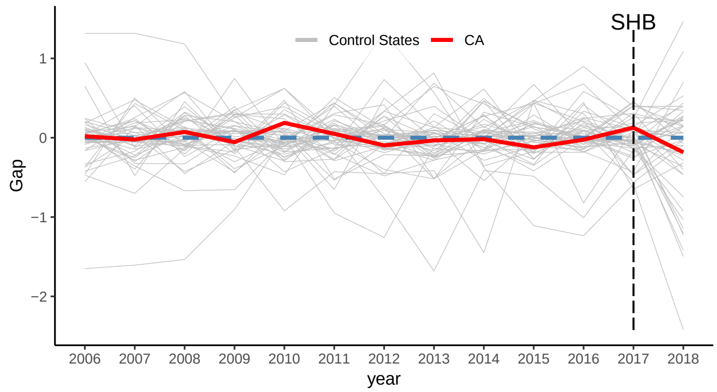
FIGURE 30.
Average Hourly Wage by Gender Among Individuals Below 35

(a) Average Hourly Wage by Gender



(b) Male Hourly Wage

Actual California - Synthetic California vs Placebo States



(c) Female Hourly Wage

Actual California - Synthetic California vs Placebo States

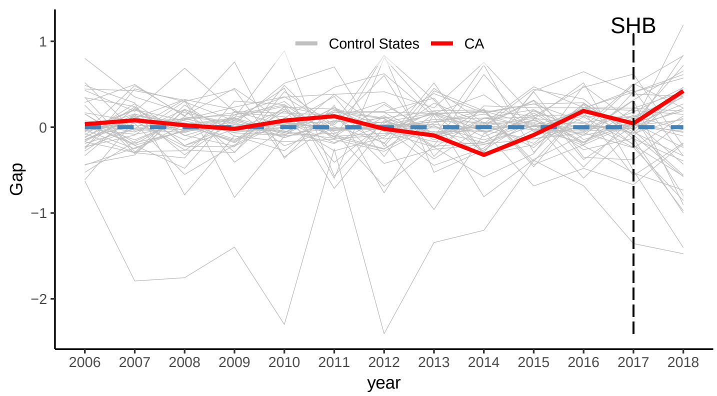
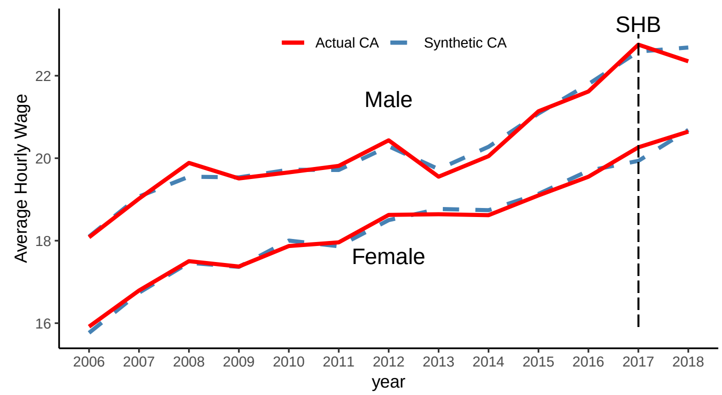


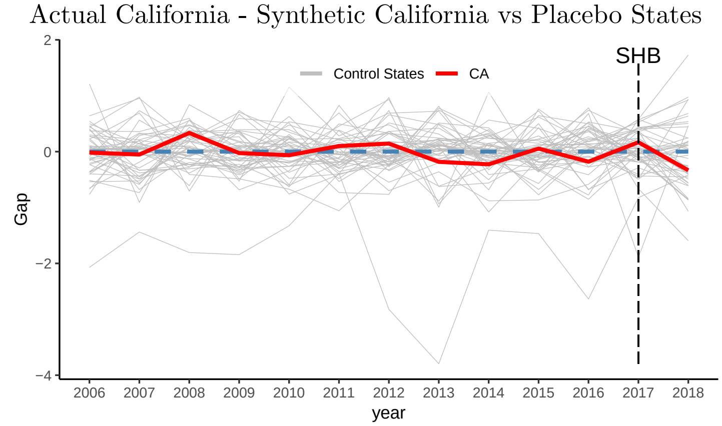
FIGURE 31.
Average Hourly Wage by Gender Among Individuals Above Age 35

(a) Average Hourly Wage by Gender



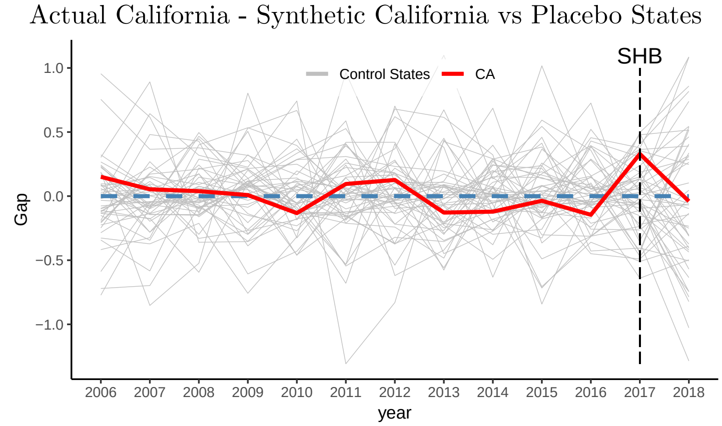
(b) Male Hourly Wage

Actual California - Synthetic California vs Placebo States



(c) Female Hourly Wage

Actual California - Synthetic California vs Placebo States



Employment Probabilities

The above effects in earnings and wages could be driven by systematic entrance to or exit from the labor market as a result of the SHB. With this in mind, I consider the effect of SHB policies on the probability that individuals are employed. I calculate employment probabilities

Figure 32 plots the probability of employment in California against its synthetic counterpart for both females and males. The synthetic control approach matches actual California employment probabilities in both levels and trends with almost no deviation. Female employment probability does not deviate from its synthetic counterpart after the SHB. Male employment probability increases slightly relative to its synthetic counterpart after the SHB. Neither of these deviations are statistically different from zero. Table 15 contains the point estimates and permutation based p-values in brackets. Figure 33 illustrates the statistical precision of the point estimates reported in Table 15. Both male and female employment probabilities fit their synthetic counterpart reasonably well before and after the SHB. I also calculate the change in employment probability within male and female dominated industries. Within both of these industries, the change in employment probability after the SHB is small and statistically indistinguishable from zero.

The implications of the employment probability findings are two fold. The SHB does not appear to be causing systematic entrance to or exit from the California labor market for either males or females; more specifically, within the labor markets of male dominated and female dominated industries, there does not appear to be systematic entrance or exit of either males or females. The observed

TABLE 15.
Change in Employment Probability by Sex and Industry

	Level Data		Demeaned Data	
State-Wide				
	Female	Male	Female	Male
SHB	0.0069	0.0004	-0.0462	0.0004
P-Value	[0.163]	[0.816]	[0.571]	[1.000]
Male Dominated industries				
	Female	Male	Female	Male
SHB	-0.0046	0.0086**	0.0023	0.0088**
P-Value	[0.102]	[0.041]	[0.918]	[0.020]
Female Dominated Industries				
	Female	Male	Female	Male
SHB	0.0048	-0.0076	-0.0527	-0.0253
P-Value	[0.796]	[0.245]	[0.653]	[0.510]

FIGURE 32.
Employment Probabilities by Sex

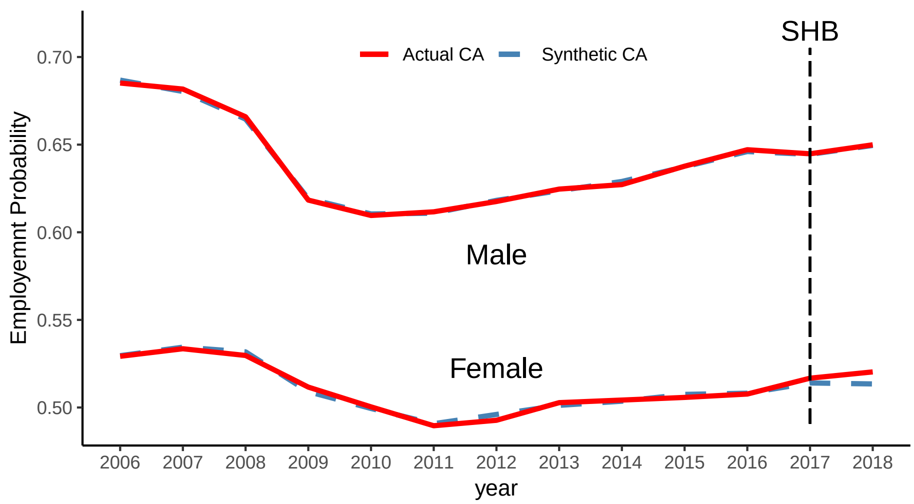
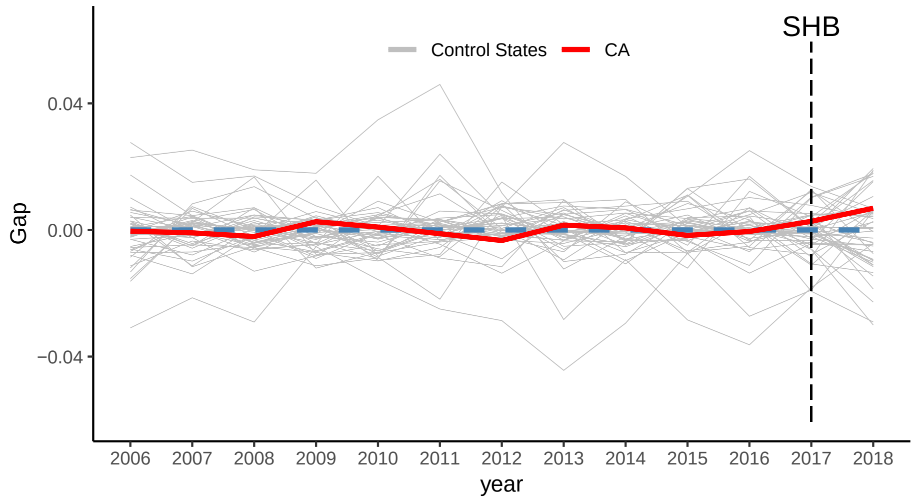
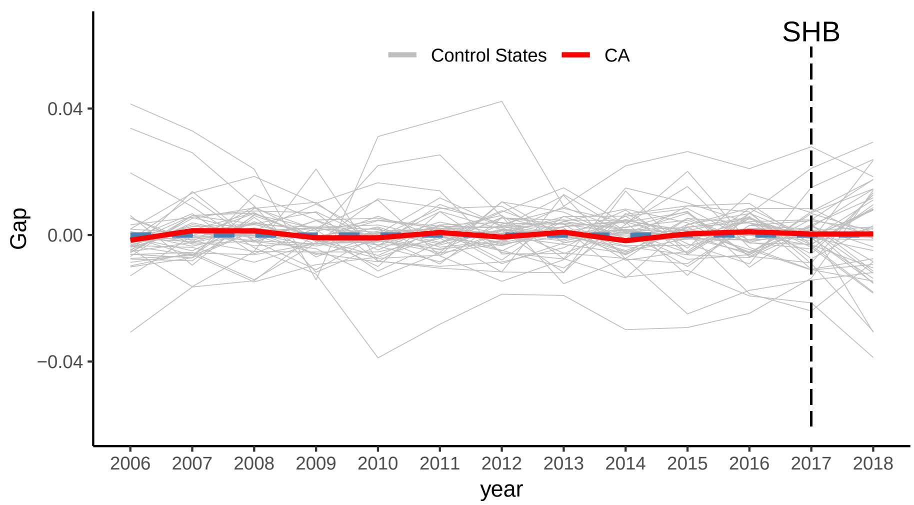


FIGURE 33.
Actual California - Synthetic California vs Placebo States by Sex

(a) Female Employment Probability



(b) Male Employment Probability



change in female to male earnings ratio after implementation of the SHB is likely driven by the SHB's impact on earnings, hours worked, and wages of individuals participating in the labor market before the SHB. Turnover within the labor market is likely driving the results observed at the state level.

If a subset of the population is driving results via turnover, the results must be larger for that subset of the population. According to the North America Mercer Turnover Survey, US companies had an average 22 percent turnover rate. With a turnover rate of 22 percent the change in the earnings ratio must be about 5 times as large as the state wide estimates among those who are turning over. I am unable to obtain a reliable measure of turnover from the CPS. The Quarterly Workforce Indicators (QWI) are quarterly administrative data which include variables such as total separation, total new hires, earnings of new hires by race age and sex. These data are published with a lag of multiple quarters. The most recent publication of the QWI suggests that statewide new hire rate for 2017 quarter 4 was roughly 10 percent. As QWI data are published, my results will be updated.

Robustness

I explore the sensitivity of my results to changes in the model specification. I replicate Table 12 using multiple model specifications.

In my baseline specification, I use levels of the data reported in the CPS. Synthetic control results can be sensitive to how the data are treated. For this reason I replicate Table 12 using demeaned data. I demean the data for each state by subtracting the pretreatment mean from the entire time series. The synthetic control approach chooses donor states by matching on pretreatment levels and trends. Demeaning the data allows the synthetic control approach to choose donors by matching on variation only. Column 2 of Table 16 reports my results using demeaned data. I include my baseline results in Column 1 of Table 16, previously reported in Table 12, for ease of comparison. The magnitude and statistical precision moving from column one to column two remain relatively stable. One exception is within good producing industries, where the demeaned estimates are less than half the size of the level estimates. This is likely due to synthetic California having a donor which matches levels well, but not trends. The result is a synthetic California with a higher predicted earnings ratio than in Figure 22.

Another possible way to scale the data is to divide the whole time series by the pretreatment mean for each state. Column 3 of Table 16 reports estimates produced using scaled data. The scaled data produces estimates similar in magnitude and statistical significance to Columns 1 and 2.

California is one of a few treated states that exist in the data. New York and Delaware also adopted SHBs around a similar time. The synthetic control approach is not able to create a synthetic counterpart that matches the actual data reasonably well for either of these states. I offer an alternative where I pool data across the three states and treat them as one state. I pool observations from California, Delaware, and New York using the micro level data. I replicate the analysis in Table 12 using pooled data from all three states. The sampling frequency of each state is population adjusted; the pooled data are roughly 60% California, 30% New York, and, 10% Delaware. The pooled estimates are reported in Column 4 of Table 16. The magnitude and statistical precision of the pooled point estimates are consistent with the baseline specification.

As a final test of model sensitivity, I replicate Table 16 in Table 17 using the replication weights provided by the CPS. The replication weights can be used for creating a representative sample. Across the two tables the point estimates are consistent in magnitude and statistical precision.

TABLE 16.
Robustness

	(1) Baseline	(2) Demeaned Data	(3) Scaled Data	(4) Pooled Data
State-Wide				
SHB	0.0303**	0.0232**	0.0301**	0.0218**
P-Value	[0.020]	[0.020]	[0.020]	[0.020]
Younger Than 35				
SHB	0.0115	-0.0109	-0.0140	0.0138
P-Value	[0.694]	[0.367]	[0.327]	[0.449]
Older Than 35				
SHB	0.0256**	0.0129**	0.0170**	0.021**
P-Value	[0.020]	[0.020]	[0.020]	[0.041]
Female Dominated Industries				
SHB	0.0084	0.0078	0.0102	0.0084
P-Value	[0.429]	[0.245]	[0.224]	[0.490]
Male Dominated Industries				
SHB	0.0419	0.0283	0.0329*	0.042
P-Value	[0.102]	[0.102]	[0.061]	[0.102]
Goods Producing Industries				
SHB	0.0539**	0.0288**	0.0298**	0.0103
P-Value	[0.020]	[0.020]	[0.020]	[0.245]
Service Providing Industries				
SHB	0.0280**	0.0211**	0.0215**	0.018**
P-Value	[0.020]	[0.041]	[0.041]	[0.020]

Table notes:
Column (1) reports my baseline estimates
Column (2) reports estimates using data that has been demeaned by its pretreatment mean for each state
Column (3) reports estimates using data that has been scaled by its pretreatment mean for each state
Column (4) reports estimates using pooled data from California, New York, and, Delaware.

TABLE 17.
Robustness Using CPS Replicate Weights

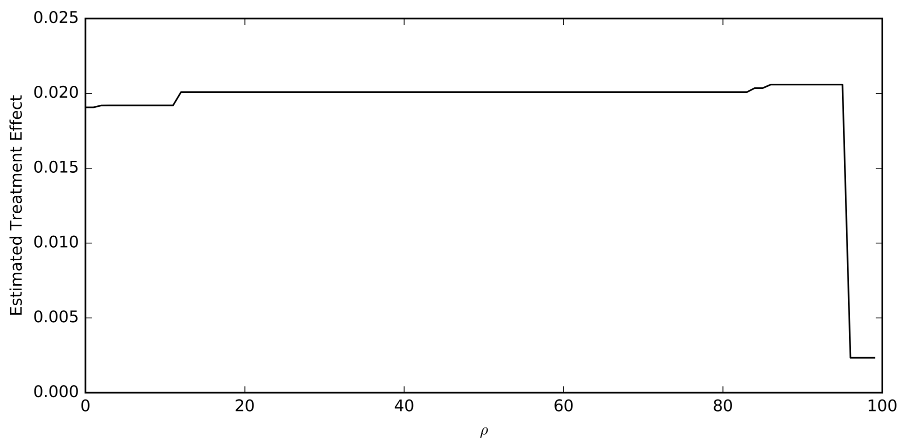
	(1) Baseline	(2) Demeaned Data	(3) Scaled Data	(4) Pooled Data
State-Wide				
SHB	0.0218*	0.0169**	0.0228**	0.0136
P-Value	[0.061]	[0.020]	[0.020]	[0.102]
Younger Than 35				
SHB	-0.0039	-0.0195	-0.0270	0.014
P-Value	[0.898]	[0.265]	[0.184]	[0.388]
Older Than 35				
SHB	0.0223**	0.0160**	0.0209**	0.0209**
P-Value	[0.020]	[0.020]	[0.020]	[0.041]
Female Dominated Industries				
SHB	0.0030	0.0050	0.0066	-0.0003
P-Value	[0.837]	[0.571]	[0.531]	[1.000]
Male Dominated Industries				
SHB	0.0370	0.0238	0.0274	0.0426*
P-Value	[0.122]	[0.102]	[0.102]	[0.082]
Goods Producing Industries				
SHB	0.0757**	0.0457**	0.0342**	0.0391**
P-Value	[0.020]	[0.020]	[0.020]	[0.020]
Service Providing Industries				
SHB	0.0220**	0.0205**	0.0269**	0.0126*
P-Value	[0.020]	[0.041]	[0.041]	[0.082]

Table notes:
Column (2) reports my baseline estimates
Column (2) reports estimates using data that has been demeaned by its pretreatment mean for each state
Column (3) reports estimates using data that has been scaled by its pretreatment mean for each state
Column (4) reports estimates using pooled data from California, New York, and, Delaware.

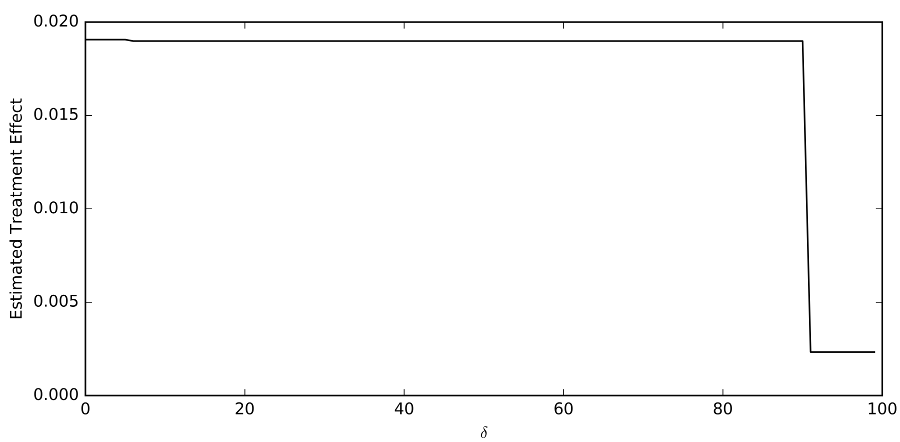
It's possible that one of the donors or combination of donors to synthetic California are driving the results I observe. Minard and Waddell (2018) provide a sensitivity analysis of point estimates to the dispersion of the donor states selected. Figure 34 illustrates that the point estimates are relatively stable for a wide range of dispersion requirements for the potential donor states.

FIGURE 34.
Sensitivity Analysis

(a) Dispersion Paremeter ρ



(b) Dispersion Paremeter δ



Conculsion

Salary history bans (SHBs) are being implemented in cities and states as a popular policy to help close the gender wage gap. The intent of these policies is to remove path dependence in compensation. Removing information from the hiring process has been shown to unintentionally incentivize statistical discrimination in other settings. For example, ban the box policies have been shown to increase statistical discrimination by employers (Henry and Jacobs, 2007; Agan and Starr, 2016; Doleac and Hansen, 2016; Shoag and Veuger, 2016; Starr, 2014).

In this paper, I provide the first evidence of the causal impact of statewide salary history bans. I find that implementation of a SHB increases female earnings relative to male earnings. I estimate SHBs cause the state level earnings ratio to increases by .0298, a 10% decrease in the gender earnings gap. The effect of the SHB is particularly robust across multiple model specifications, including pooling three treated states into one state. Based on the trend that existed in the earnings ratio before the SHB, a 0.0298 increase would have taken 5 or more years.

These results are driven by females earning more relative to males within male dominated industries. The earnings ratio increases by .0579 in industries with more than 50% males, a 30% decrease in the gender earnings gap within male dominated industries. Within male dominated industries, systemic gender discrimination is more likely to be present. SHBs create one more safeguard against pay discrimination, and thus have the largest impact where the most pay discrimination is taking place.

I find no evidence that males or females are systematically entering or exiting the labor market as a result of the SHB. The observed increase in the earnings ratio is likely driven by changes in earnings among individuals who participated in the labor market before implementation of the SHB.

Given the recent implementation of SHBs, this paper is limited to identifying the immediate impacts of SHBs. This paper also only examines one potential margin (female to male earnings) for pay disparity. Eliminating path dependent compensation could redress pay disparities across other margins as well. Future research will shed light on the long run impact of SHBs, as well as the impact of SHBs on other populations that have historically experienced disparities in pay. SHB policies were designed with the goal of reducing gender-based pay disparities. This research on the early effects of California's SHB shows that this policy has the intended result of reducing pay inequities experienced by female employees. The immediate effects of the SHB do not appear to cause an increase in unintended statistical discrimination toward the population for which the policy was designed to help, as in similar labor policies such as ban the box. The effects of California's SHB on the female to male earnings ratio suggests that SHBs may be an effective policy for reducing the gender pay gap.

CHAPTER III

MARIJUANA LEGALIZATION AND VIOLENT CRIME

This chapter is co-authored with Benjamin Hansen. He developed the initial question, while I preformed the analysis and programming. Ben wrote up the introduction, background, and conclusion. I wrote up the data, methods, and results sections.

Introduction

Marijuana remains a polarized issue in the United States. While a super majority of the population now supports legalization, much of the population remains opposed to its legalization. Chief among the concerns are the relationship between marijuana an crime. Will marijuana legalization encourage harder drug use? Will marijuana access increase violence or reduce it?

There are many reasons that this question remains a largely unanswered. Empirically, while marijuana has been legally available for 5 years in Colorado and Washington, we only recently have observed enough post period data to potentially have power to detect increases in crime. From a theoretical perspective, marijuana legalization has ambiguous effects. Many clinical studies on marijuana highlights that marijuana users are more likely to develop psychosis (Murray et al., 2016). These effects could be exacerbated in recreational marijuana that has increased THC potency and lower levels cannabinoils. However, many of these studies feature small samples, questionable control groups, and self-selection into marijuana consumption. Furthermore, marijuana consumption is often paired with alcohol consumption, but often not accounted for in observational studies of psychosis and marijuana. Two recent large scale studies failed find evidence linking marijuana with psychosis. However, recently Berenson (2019) cited prior evidence on the correlation between marijuana and psychosis and suggested recreational marijuana legalization was a significant contributing cause to the uptick homicides observed in the United States recently.

Even if marijuana does not directly affect violence at all, it could reduce violence or increase it depending on marijuana's substituability vs. complementary with alcohol Pacula and Kilmer (2003); Crost and Guerrero (2012); Mark Anderson et al. (2013); Wen et al. (2015). Previous research on alcohol suggests assaults and other impulsive crimes increase notably with legal access to alcohol and the increase in consumption, especially binge drinking Carpenter and Dobkin (2015); Carpenter et al. (2016); Hansen and Waddell (2018). Likewise, it could depend on where individuals get high vs. where they consume alcohol. Consumption in public spaces could result in more conflict, or perhaps more calls to the police. Given we have unclear pharmacological evidence on marijuana's direct effect on violent behavior, and alcohol and marijuana's cross-price elasticity remains unsettled, the net effect of marijuana legalization on violent behavior remains an empirical question.

Prior research has focused on variation in medical marijuana laws, recreational marijuana laws, and unanticipated city wide dispensary closures. Chu and Townsend (2019) investigate medical marijuana laws with a difference-in-difference approach, finding little evidence they shift assaults or homicides. Gavrilova et al. (2017) investigate the border counties of states that legal medical marijuana, and find evidence that violent crime decrease. They suggest this could be due to medical marijuana displacing violent drug trafficking organizations.[1] Chang and Jacobson (2017) medical marijuana dispensary closures in Los Angeles lead to moderate increases in localized property crime, likely due to fewer eyes on the street. Dragone et al. (2019) investigate recreational marijuana legalization in Washington and Oregon using a border county approach. They find evidence rapes fell in Washington in the period 2012-2014 following the legalization of marijuana possessions.

We test marijuana effects on violence using variation in early adopters. This includes the following states: Colorado (2014), Washington (2014), Oregon (2015), Alaska (2015), and Nevada (2017). We focus on two approaches. In this paper, we focus murders and homicides, given those are crimes where reporting concerns are minimized. First, we estimated a difference in difference approach using the early adopters as treated states. Following that approach, we estimate a synthetic control design approach of Abadie et al. (2010) for Colorado and Washington, given they are the earliest adopters with the most years of follow up data. We focus on the period of recreational legal access, as this is the time in which retailers sold marijuana to anyone over the age of 21. We also explicitly avoid border county comparisons for identification as prior research has suggested considerable cross border shopping occurred in neighboring regions when recreational stores opened to the pubic (Hao and Cowan, 2017; Hansen et al., 2017).

The remainder of the paper proceeds as follows. In section 3.2, we discuss the background of marijuana legalization. In Section 3.3 we review our data and

[1] It could also be that the increased pressure from legal competition increases violence south of the border. Lindo and Padilla-Romo (2018) find evidence the increased competition among drug trafficking organizations lead to more violence in Mexico.

methods. In section 3.4, we discuss our results and investigate their robustness. In Section 3.5, we review the policy implications and conclude.

Background

States had long varied in how they regulated marijuana in the United States. This changed in 1937 with the Marijuana Tax act which effectively made marijuana illegal at the federal level. This was re-enforced in 1970 with the Controlled Substances Act. This law create the common scheduling of drugs in 4 categories which remains today. Schedule I drugs are those with no known benefit and a high potential for abuse, while schedule IV drugs are those with minimal risks and known medical benefits. Marijuana remains scheduled as a class I today, along with heroin, meth, and MDMA.

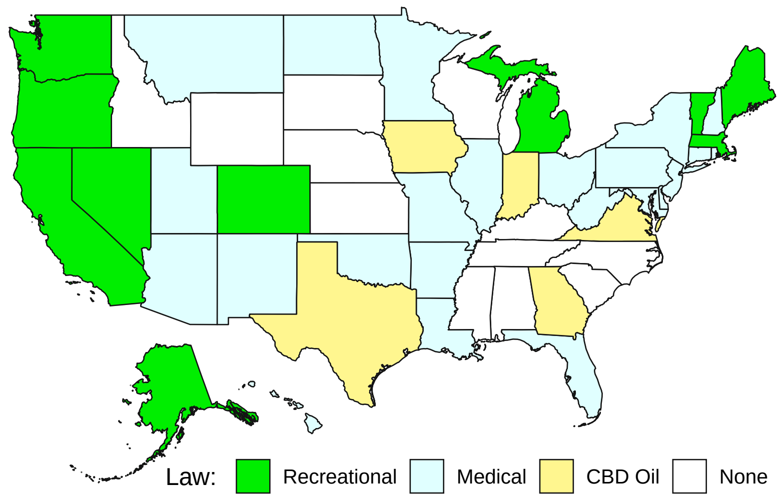
State individually began passing their own legislation, often first motivated by state wide ballot initiatives in the 1990s. California was first with the medical marijuana. Washington legalized medical marijuana soon thereafter in 1998, while Colorado did so in 2000. The number of states with medical marijuana has continued to increase over time.

In November 2012, Washington and Colorado became the first states to legalize marijuana for recreational use for all adults over 21. Legal sales began on January 1, 2014 in Colorado, and July 1, 2014 in Washington. Since those two first states legalized, Alaska (2014), Oregon (2014), Nevada(2016), California (2016), Vermont, and Maine and Massachusetts (2018). The states law passage and legal sale dates are provided in Figure 35.

Colorado, Washington, Oregon, Alaska, and Nevada all have legalized recreational marijuana laws prior to 2017, the end of our data. Because Oregon,

FIGURE 35.
Marijuana Laws by State

(a) Legalization Map



(b) Time-line of Legalization

State	Rec Law	Sales Start
Colorado	2012	Jan 1, 2014
Washington	2012	Jul 8, 2014
Oregon	2014	Oct 1, 2014
Alaska	2014	Oct 29, 2014
Washington DC	2014	No sales
California	2016	Jan 1, 2018
Main	2016	Est. 2020
Nevada	2016	Jul 1,2017
Vermont	2016	Est. 2021
Massachusetts	2016	Nov 20,2018

Alaska and Nevada have only a few years of post-legalization data, we focus on Colorado, and Washington, both of which started recreational marijuana sales in 2014. Figure 35 illustrates the medical marijuana, CBD oil, and recreational marijuana laws in the USA currently.

Data and Methods

To study the impacts of Marijuana legalization on homicide, we utilize data from the Federal Bureau of Investigations Uniform Criminal Reporting (UCR) program. We use data from the offenses known yearly files, and from the supplementary homicide report (SHR) files. We obtain these data from 2001 to 2017. The offenses known yearly files provide us with counts of reported murders at the state level. Every cleared murder and non-negligent manslaughter is recorded at the agency-month level, then aggregated up to the state-year level. Population data is then used to create the murder rate per 100,000. Population data come from The National Cancer Institutes Surveillance, Epidemiology, and End Results Program (SEER) [2]

We also utilize data from the supplementary homicide reports. These data provide detailed information on criminal homicides reported to the police. From these data, we are able to construct multiple subsets of homicide: homicides involving a gun, homicides not involving a gun, homicides where the offender knows the victim, homicides where the offender does not know the victim, homicides related to drugs, and homicides related to gang use.

Figures 36a, and 36b plot the murder rate per 100,000 over the 16 year sample period for both Colorado and Washington against all other states in the data. Figures 37a, and 37b subtract the pre-treatment mean from each of the treated states and the average of the non treated states. Figures 37a, and 37b illustrate that prior to legalization, neither Colorado or Washington move closely with the average of the rest of the United States. Parallel trends, needed for

[2] The SEER data are a modification of the intercensal and vintage 2017 annual estimates produced by the US Census Bureau's Population Estimates Program. These data are aggregated to provide yearly population estimates at the state level.

identification in a differences in differences model, do not hold. Figures 38,39, and 40 illustrate each treated state compared to the average of the rest of the US demeaned for homicides as reported in the SHR, gun related homicides, and gang related homicides.

FIGURE 36.
Reported Murders per 100,000 by State

(a) Colorado (b) Washington

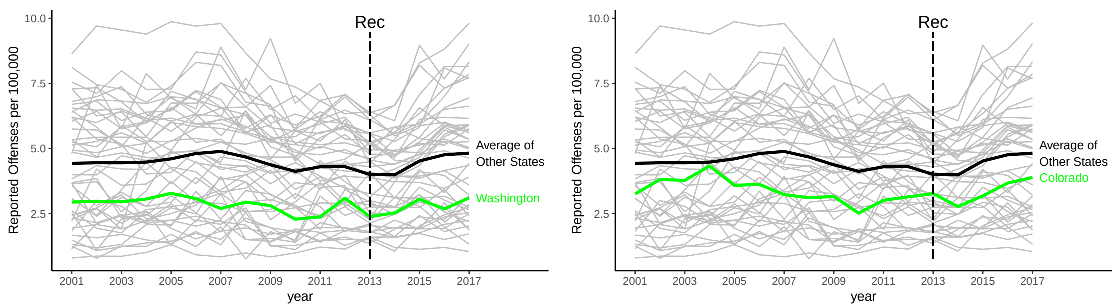
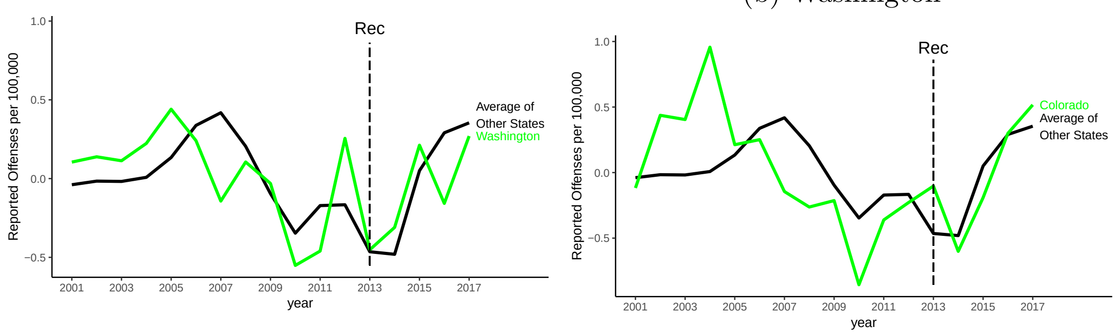


FIGURE 37.
Reported Murders per 100,000 by State, Demeaned

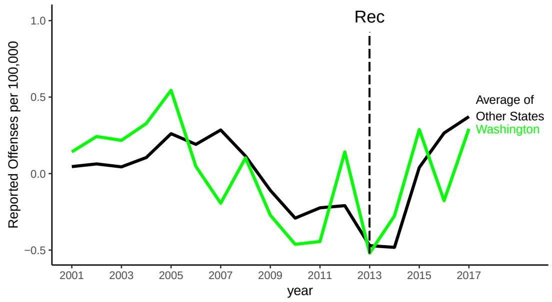
(a) Colorado (b) Washington



With this concern in mind, we employ a synthetic control approach following Abadie et al. (2010). This approach uses a data-driven process to make the assumption of parallel trends more believable. This method has been used to study many different policy changes including economics liberalization (Billmeier and Nannicini, 2013), highway police budget cuts (DeAngelo and Hansen, 2014),

FIGURE 38.
Reported Homicides per 100,000 by State, Demeaned

(a) Colorado



(b) Washington

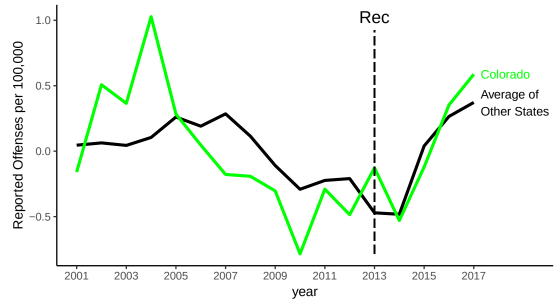
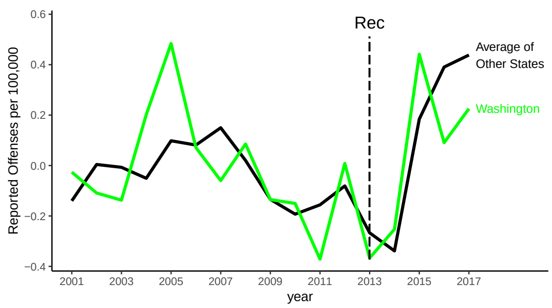


FIGURE 39.
Reported Gun Homicides per 100,000 by State, Demeaned

(a) Colorado



(b) Washington

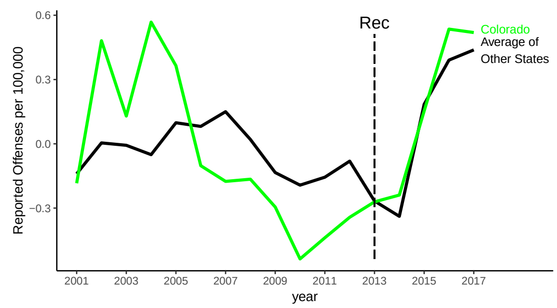
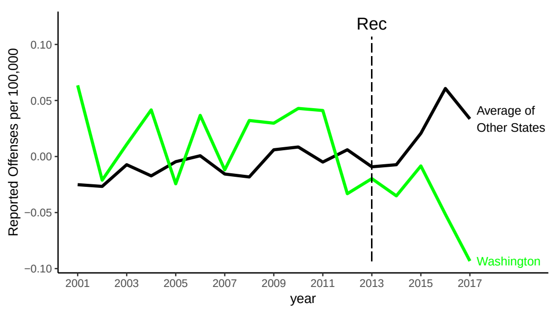
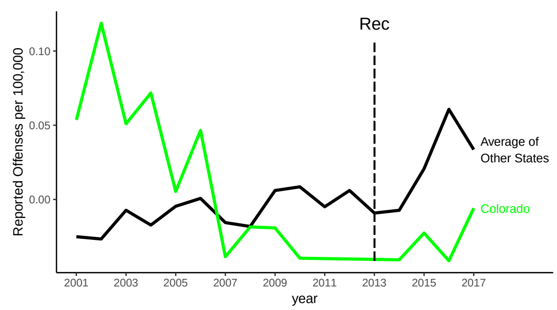


FIGURE 40.
Reported Gang Homicides per 100,000 by State, Demeaned

(a) Colorado



(b) Washington



decriminalization of prostitution (Cunningham and Shah, 2017), and increases in minimum wage (Jardim et al., 2017).

The synthetic control approach aims to construct a counter-factual such that parallel trends absent treatment is a believable assumption. Consider a outcome of interest Y_{it} where i represents a state and t represents a year. For a treated state i, the synthetic control approach estimates the treatment effect as $Y_{it} - \sum_{i \neq j}^{S} w_j Y_{jt}$ where w_j is the weight assigned to unit j. All non treated states are considered when choosing the weights w_j. Weights are usually chosen to minimize the distance between the treated unit and the synthetic control units for a set of variables chosen by the researcher. Ferman et al. (2017) show that estimates can be quite sensitive to the selection of variables chosen by the researcher. For this reason, we follow the work of Botosaru and Ferman (2019) and match on pre-treatment outcomes only. Our main specifications use data that has been demeaned by pre-treatment mean to decrease bias as suggested by Ferman and Pinto (2016).

To conduct hypothesis testing, we follow the placebo based approach suggested by Abadie et al. (2010). Using the same synthetic control approach, we estimate a synthetic control for each non treated state. We then compare the ratio of the mean square prediction error(MSPE), where MSPE is calculated using the difference between the actual outcome and the synthetic counterpart, before and after treatment ($\frac{PostMSPE}{PreMSPE}$) for each non treated state. The MSPE ratio provides a metric of post treatment fit relative to pre-treatment fit. A states ranking among the distribution of MSPE ratios provides and empirical p-value as a permutation based test.

Results

Murder as Reported in the UCR Offenses Known Files

Figure 41a illustrates the murder rate per 100,000 for Colorado and its synthetic counterpart. The actual murder rate per 100,000 in Colorado is represented by the green solid line and the synthetic counterpart is represented by the grey dashed line. Figure 41b illustrates the composition of states chosen to create synthetic Colorado. States are shaded by their relative contribution. Prior to treatment, synthetic Colorado matches the direction of deviations from pre-treatment mean but fails to match the magnitude of the deviations. Colorado is much higher than its synthetic counterpart in 2004 and much lower in 2010. After legalization, synthetic Colorado continues along a similar trajectory while actual Colorado increases relative to synthetic. Figure 41c illustrates that Colorado's deviation from its synthetic counterpart is relatively small compared to the deviations of each non treated state from their own synthetic counterpart. Figure 41d illustrates the distribution of pre MSPE to post MSPE ratio for each state. Notably, many of the placebo states fall to the right of Colorado, meaning many other states have larger post-treatment fit to pre-treatment fit ratios. The point estimate and permutation based p-value corresponding with Figure 41 are in Table 18. Figures 41e, and 41f illustrate synthetic Colorado when moving treatment back one and two years respectively. This exercise allows us to see how well synthetic Colorado predicts actual Colorado absent treatment. Notably, with a shorter window of data, the synthetic control approach does not predict the slight increase in the murder rate in 2013.

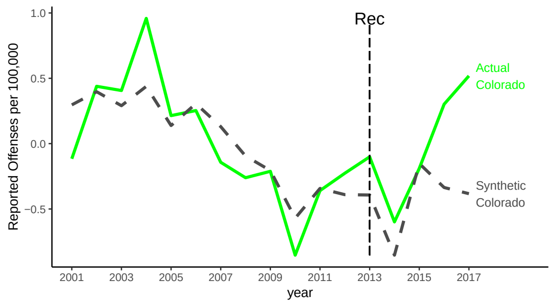
TABLE 18.
Syntheic Control Results for Marijuana Legalization

	Colorado			Washington		
	(Demeaned)	(Level)	(Rate)	(Demeaned)	(Level)	(Rate)
UCR Murder						
Rec	0.2543	0.0342	3.3712	-0.0037	-0.0817	2.8356
P-Value	[0.596]	[0.638]		[0.404]	[0.553]	
Homicide						
Rec	0.2609	0.2537	3.3004	0.1068	-0.1813	2.7317
P-Value	[0.773]	[0.841]		[0.341]	[0.591]	
Drug Related Homicide						
Rec	0.0711	-0.0814	0.1792	-0.0199	-0.0253	0.2782
P-Value	[0.409]	[0.477]		[0.250]	[0.114]	
Gang Related Homicide						
Rec	-0.0213	-0.0213	0.0594	-0.0573	-0.0113	0.1202
P-Value	[0.955]	[0.932]		[0.341]	[0.841]	
Gun Related Homicide						
Rec	0.4372	0.2861	1.924	0.1053	-0.0649	1.5299
P-Value	[0.250]	[0.273]		[0.159]	[0.568]	
Non-Gun Related Homicide						
Rec	-0.209	-0.1092	1.3764	0.0815	0.134	1.2018
P-Value	[0.841]	[0.932]		[0.886]	[0.568]	
Known Offender Homicide						
Rec	0.417	0.2883	1.831	0.0564	-0.0288	1.7231
P-Value	[0.682]	[0.864]		[0.455]	[0.295]	
Unknown Offender Homicide						
Rec	-0.2121	-0.306	1.4693	-0.1323	-0.1915	1.0086
P-Value	[0.841]	[0.659]		[0.136]	[0.205]	

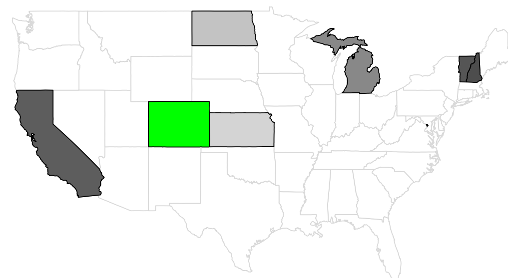
Permutation based p-vales are included in brackets. Demeaned data has been demeaned by the pre-treatment mean for each state.

FIGURE 41.
Colorado Synthetic Control for Murder (UCR) Reported, Demeaned

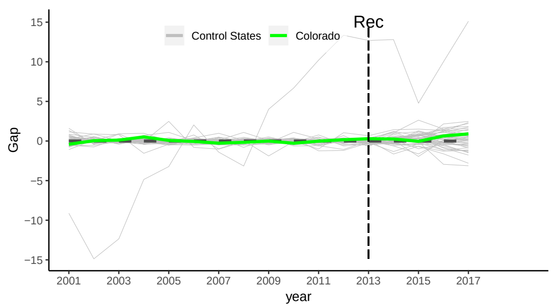
(a) Synthetic versus Actual



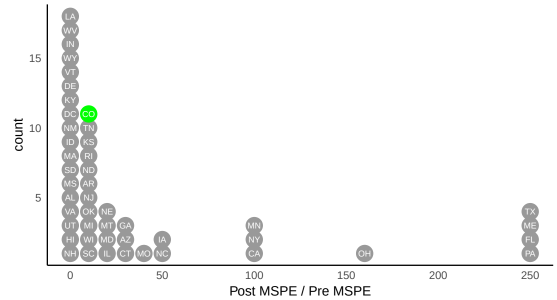
(b) Contributors to Synthetic Control



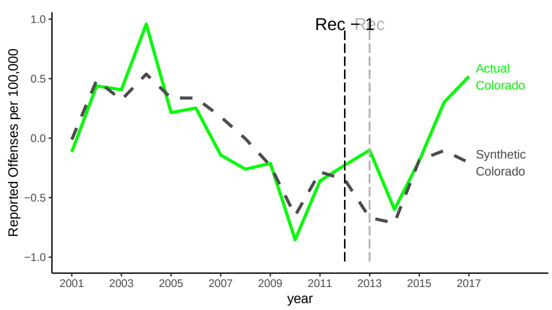
(c) Placebo Synthetic Controls



(d) MSPE Ratio Distribution



(e) Cross Validation



(f) Cross Validation

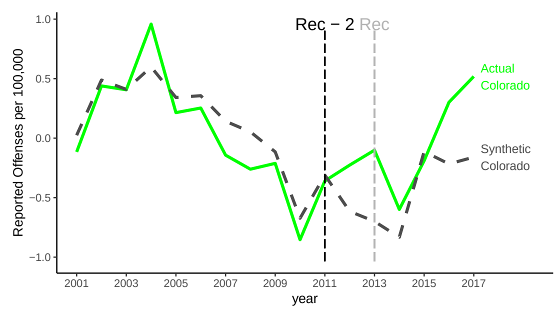


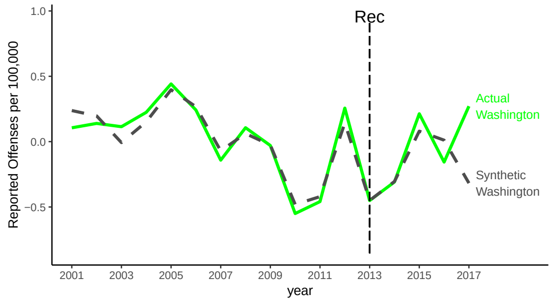
Figure 42a illustrates the murder rate per 100,000 for Washington and its synthetic counterpart. The actual murder rate per 100,000 in Washington is represented by the green solid line and the synthetic counterpart is represented by the grey dashed line. Figure 42b illustrates the composition of states chosen to create synthetic Washington by shading states by their relative contribution to synthetic Washington. Prior to treatment, synthetic Washington matches both the direction and magnitude of deviations from pre-treatment mean of actual Washington. After legalization, synthetic Washington continues along a similar trajectory to actual Washington for 3 years. In the fourth year after treatment, synthetic Washington deviates below its pre-treatment mean while synthetic Washington deviates above its pre-treatment mean. Figure 42c illustrates that Washington's deviation from its synthetic counterpart is relatively small compared to the deviations of each non treated state from their own synthetic counterpart. Figure 42d illustrates the distribution of pre MSPE to post MSPE ratio for each state illustrated in 42c. Notably, many of the placebo states fall to the right of Washington, meaning many other states have larger post-treatment fit to pre-treatment fit ratios. The point estimate and permutation based p-value corresponding with Figure 42 are in Table 18. Figures 42e, and 42f illustrate synthetic Washington when moving treatment back one and two years respectively. This exercise allows us to see how well synthetic Washington predicts actual Washington absent treatment. Notably, with a shorter window of data, the synthetic control continues to match actual Washington quite well when using one less year of pre-treatment data. When treatment is rolled back two years, the synthetic control method fails to predict the spike in the actual Washington murder rate in 2012.

Supplementary Homicide Report (SHR) Files

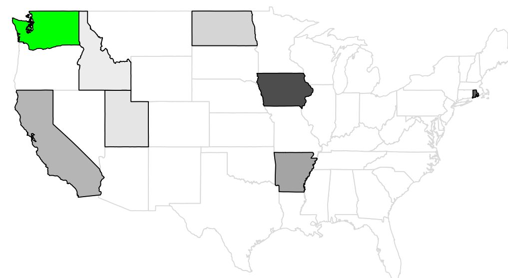
The UCR offenses known files provide only one count of total murders per state for a given year. The supplementary homicide report (SHR) files provide rich detail for every homicide in a state-year. Within the SHR, we are able to explore the sensitivity of our results to different definitions of murder. To avoid bias in

FIGURE 42.
Washington Synthetic Control for Murder (UCR) Reported, Demeaned

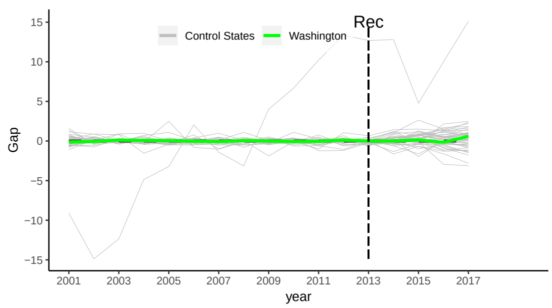
(a) Synthetic versus Actual



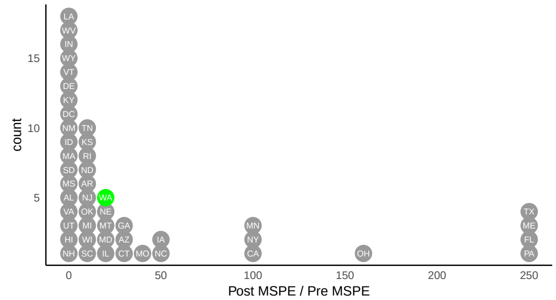
(b) Contributors to Synthetic Control



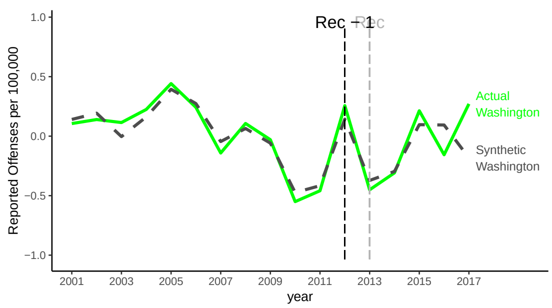
(c) Placebo Synthetic Controls



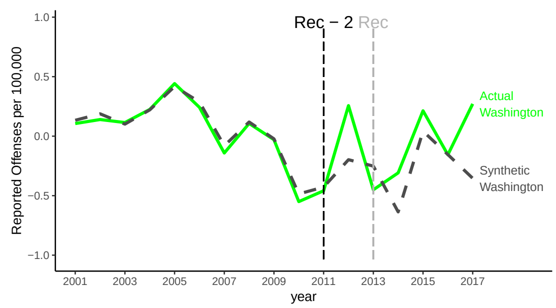
(d) MSPE Ratio Distribution



(e) Cross Validation



(f) Cross Validation



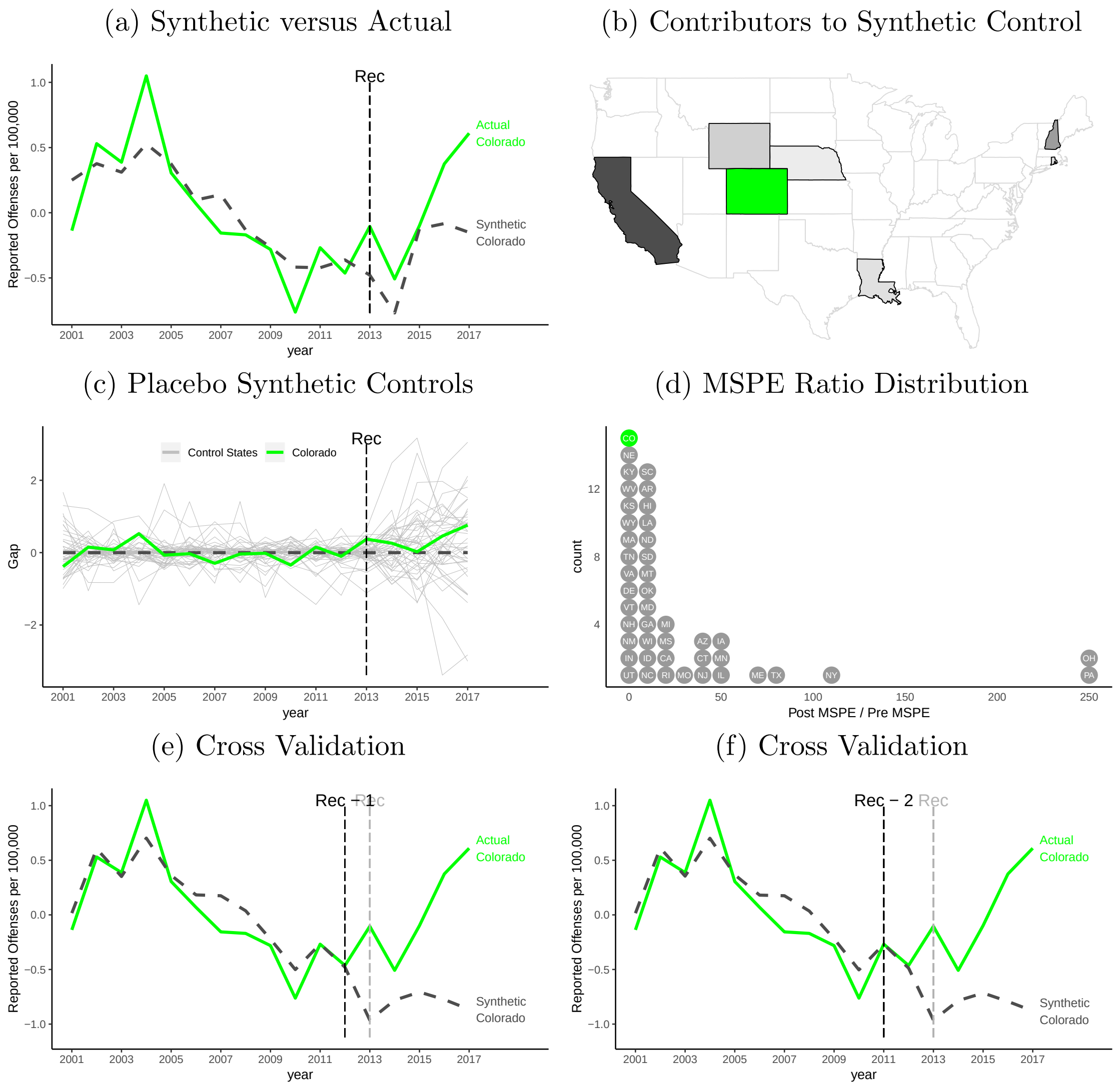
our estimates, we strip out counts of homicide that were a result of mass shooting or any incident with 4 or more victims. We consider multiple subsets of total homicides: homicides involving a gun, homicides not involving a gun, homicides where the offender knows the victim, homicides where the offender does not know the victim, homicides related to drugs, and homicides related to gang use.

Total Homicide as Reported in the SHR

Figure 43a illustrates the homicide rate per 100,000 for Colorado and its synthetic counterpart. The actual homicide rate per 100,000 in Colorado is represented by the green solid line and the synthetic Colorado is represented by the grey dashed line. Figure 43b illustrates the composition of states chosen to create synthetic Colorado by shading states by their relative contribution. Prior to treatment, synthetic Colorado matches the direction of deviations from pre-treatment mean but fails to match the magnitude of the deviations. Colorado is much higher than its synthetic counterpart in 2004 and much lower in 2010. After legalization, synthetic Colorado continues along a similar trajectory while actual Colorado increases relative to synthetic. Figure 43c illustrates that Colorado's deviation from its synthetic counterpart is relatively small compared to the deviations of each non treated state from their own synthetic counterpart. Figure 43d illustrates the distribution of pre MSPE to post MSPE ratio for each state. Notably, many of the placebo states fall to the right of Colorado, meaning many other states have larger post-treatment fit to pre-treatment fit ratios. The point estimate and permutation based p-value corresponding with Figure 43 are in Table 18. Figures 43e, and 43f illustrate synthetic Colorado when moving treatment back one and two years respectively. This exercise allows us to see how well synthetic Colorado predicts actual Colorado absent treatment. Notably, with a shorter window of data, the synthetic control approach does not predict the slight increase in the homicide rate in 2013.

Figure 44a illustrates the homicide rate per 100,000 for Washington and its synthetic counterpart. The actual homicide rate per 100,000 in Washington is represented by the green solid line and the synthetic Washington is represented

FIGURE 43.
Colorado Synthetic Control for Homicide Reported, Demeaned

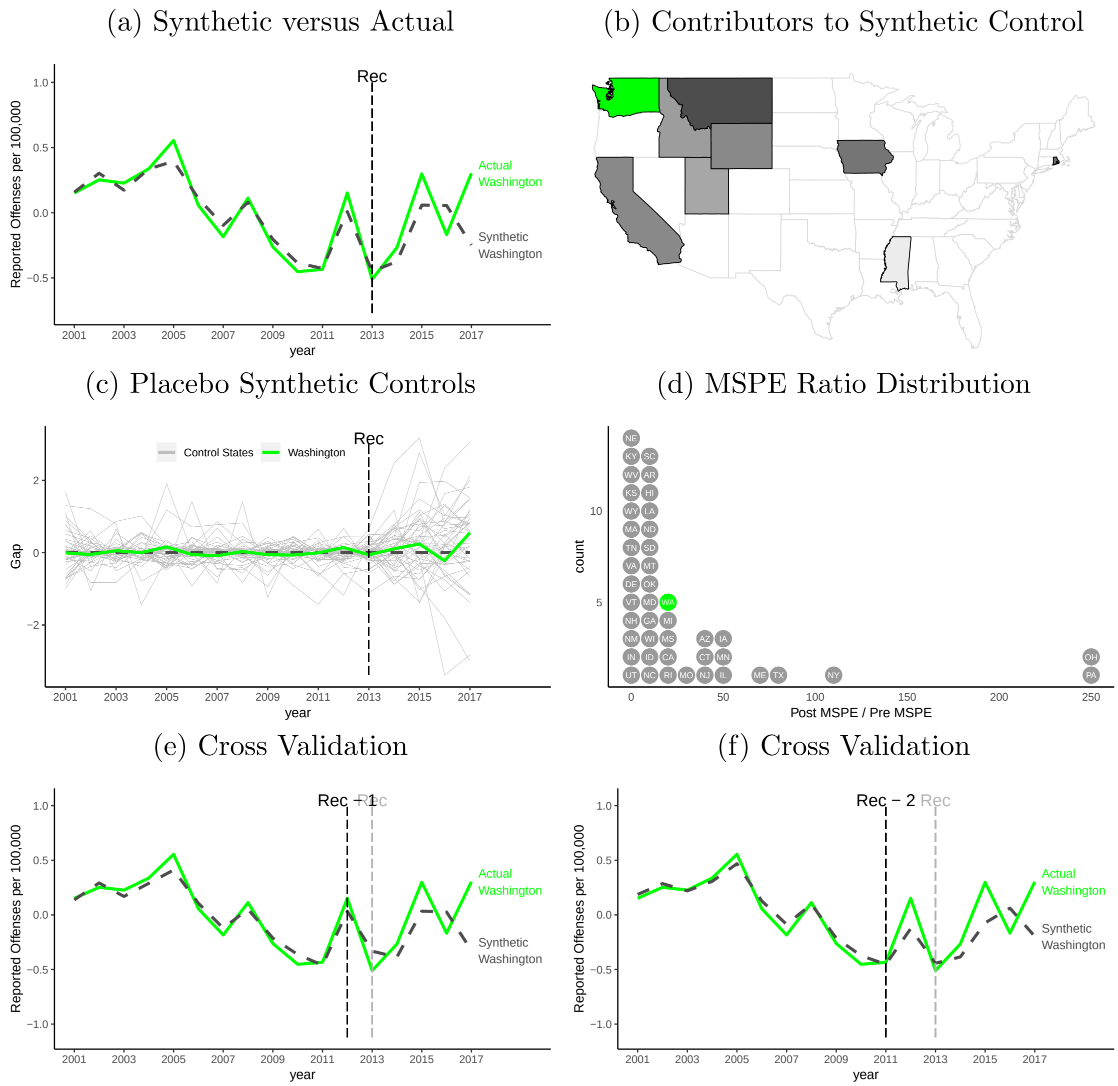


by the grey dashed line. Figure 44b illustrates the composition of states chosen to create synthetic Washington by shading states by their relative contribution to synthetic Washington. Prior to treatment, synthetic Washington matches both the direction and magnitude of deviations from pre-treatment mean. After legalization, synthetic Washington deviates from actual Washington. Figure 44c illustrates that Washington's deviation from its synthetic counterpart is relatively small compared to the deviations of each non treated state from their own synthetic counterpart. Figure 44d illustrates the distribution of pre MSPE to post MSPE for each state. Notably, many of the placebo states fall to the right of Washington, meaning Washington has one of the smallest post-treatment fit to pre-treatment fit ratios. The point estimate and permutation based p-value corresponding with Figure 44 are in Table 18. Figures 44e, and 44f illustrate synthetic Washington when moving treatment back one and two years respectively. This exercise allows us to see how well synthetic Washington predicts actual Washington absent treatment. Notably, with a shorter window of data, the synthetic control continues to match actual Washington quite well when using one and two years less pre-treatment data. The synthetic control approach does an arguably good job at predicting deviations from pre-treatment mean of the actual homicide rate for Washington for 2012 and 2013. After 2013, synthetic Washington begins to deviate from actual Washington.

Total Drug Related Homicide Reported in the SHR

Figure 45a illustrates the drug related homicide rate per 100,000 for Colorado and its synthetic counterpart. The actual drug related homicide rate per 100,000 in Colorado is represented by the green solid line and the synthetic Colorado is represented by the grey dashed line. Figure 45b illustrates the composition of states chosen to create synthetic Colorado by shading states by their relative contribution. Prior to treatment, synthetic Colorado matches the direction and magnitude of deviations from pre-treatment. After legalization, synthetic Colorado continues along a similar trajectory while actual Colorado increases relative to synthetic. Figure 46c illustrates that Washington's deviation from its synthetic counterpart is relatively small compared to the deviations of each non treated

FIGURE 44.
Washington Synthetic Control for Homicide Reported, Demeaned



(a) Synthetic versus Actual (b) Contributors to Synthetic Control (c) Placebo Synthetic Controls (d) MSPE Ratio Distribution (e) Cross Validation (f) Cross Validation

state from their own synthetic counterpart. Figure 45d illustrates the distribution of pre MSPE to post MSPE ratio for each state. Notably, many of the placebo states fall to the right of Colorado, meaning many other states have larger post-treatment fit to pre-treatment fit ratios. The point estimate and permutation based p-value corresponding with Figure 45 are reported in Table 18. Figures 45e, and 45f illustrate synthetic Colorado when moving treatment back one and two years respectively. This exercise allows us to see how well synthetic Colorado predicts actual Colorado absent treatment. Notably, with a shorter window of data, the synthetic control approach creates a similar counter-factual as with the full window of pre-treatment data.

FIGURE 45.
Colorado Synthetic Control for Drug Related Homicide Reported, Demeaned

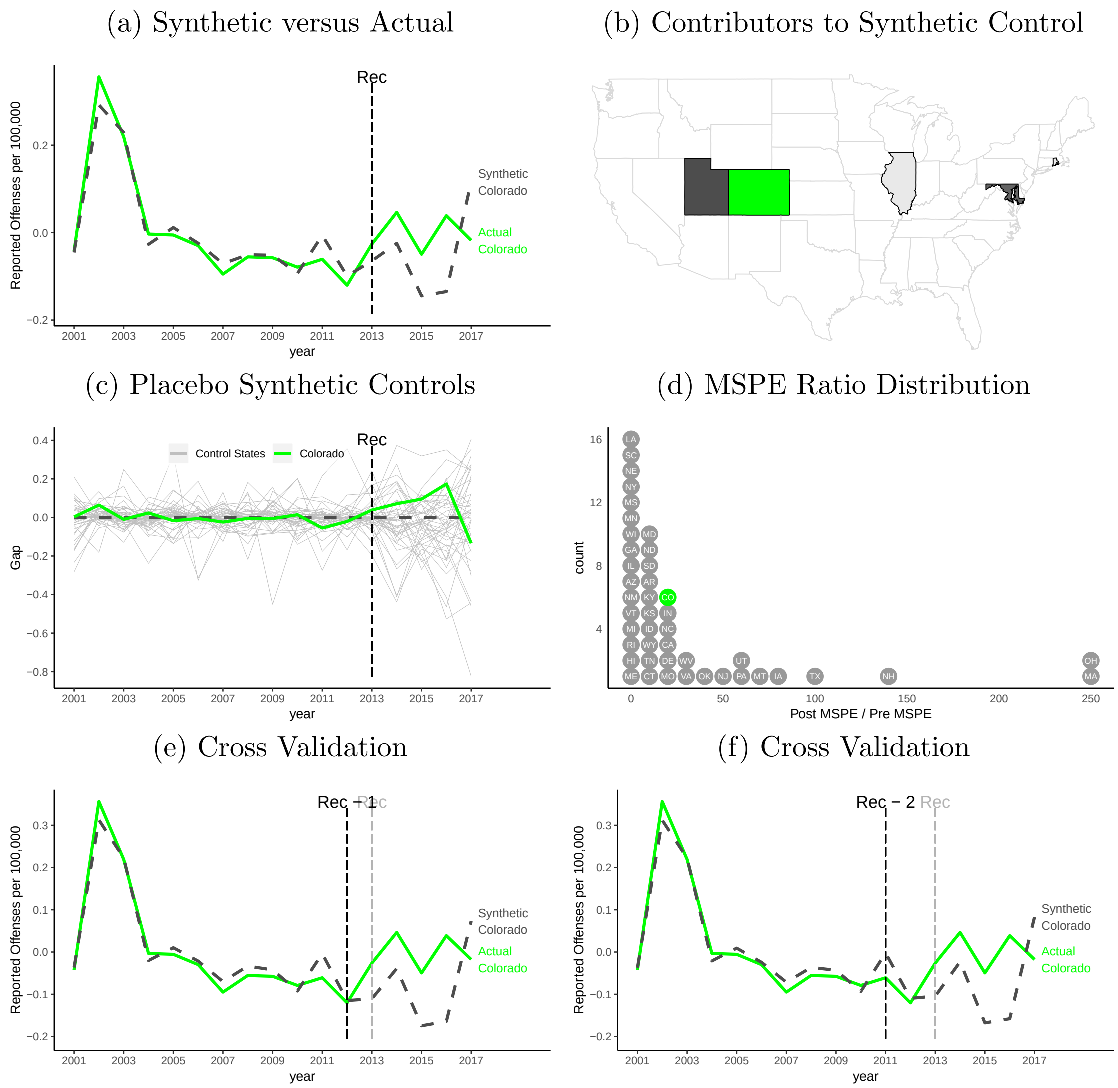
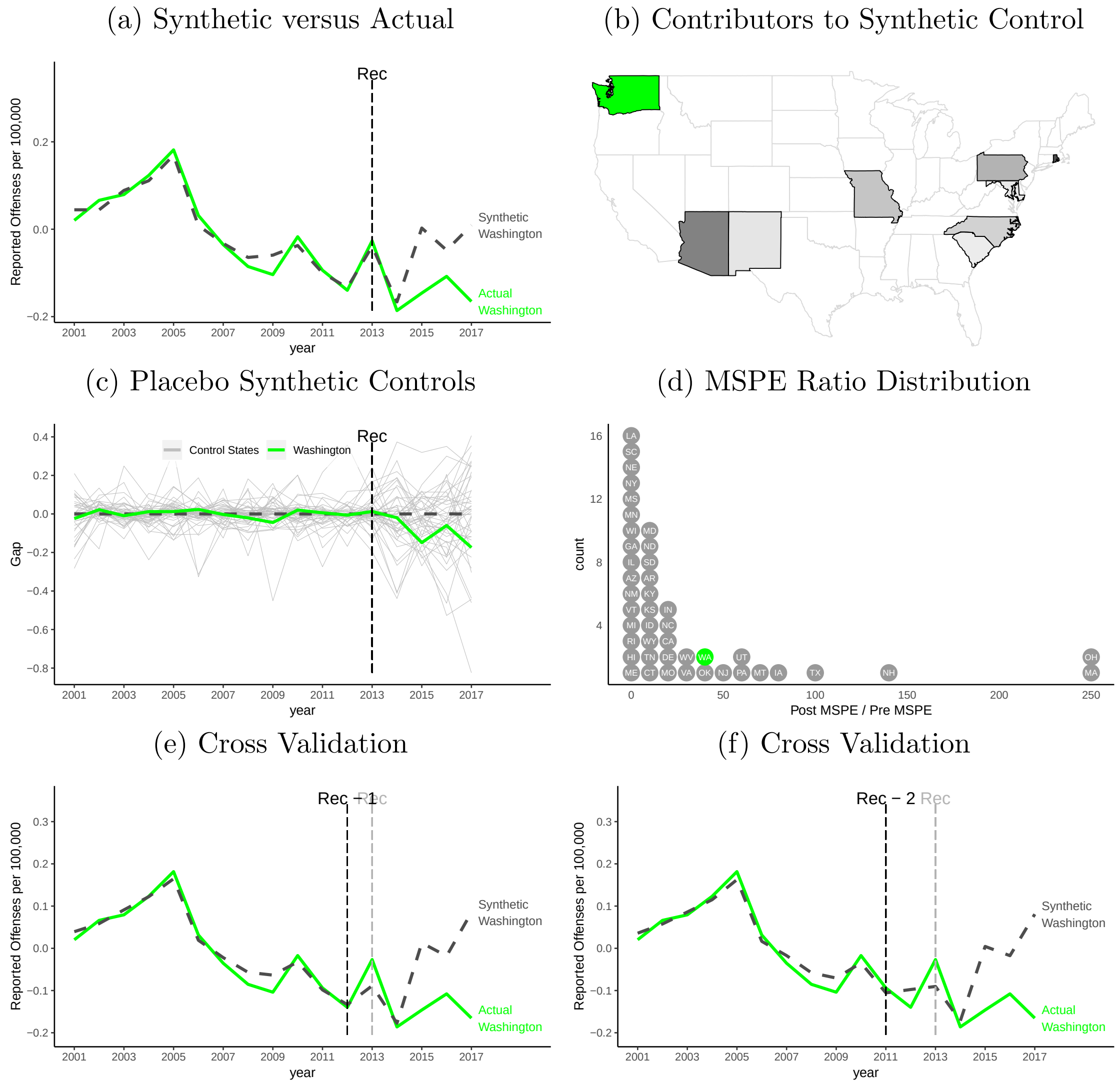


Figure 46a illustrates the drug related homicide rate per 100,000 for Washington and its synthetic counterpart. The actual drug related homicide rate per 100,000 in Washington is represented by the green solid line and the synthetic Washington is represented by the grey dashed line. Figure 46b illustrates the composition of states chosen to create synthetic Washington by shading states by their relative contribution to synthetic Washington. Prior to treatment, synthetic Washington matches both the direction and magnitude of deviations from pre-treatment mean. After legalization, synthetic Washington increases relative to actual Washington. Figure 46c illustrates that Washington's deviation from its synthetic counterpart is relatively small compared to the deviations from their synthetic counterpart of each non treated state. Figure 46d illustrates the distribution of pre MSPE to post MSPE for each state. Notably, many of the placebo states fall to the right of Washington, meaning Washington has one of the smallest post-treatment fit to pre-treatment fit ratios. The point estimate and permutation based p-value corresponding with Figure 46 are reported in Table 18. Figures 46e, and 46f illustrate synthetic Washington when moving treatment back one and two years respectively. This exercise allows us to see how well synthetic Washington predicts actual Washington absent treatment. Notably, with a shorter window of data, the synthetic control continues to match actual Washington up to 2014 quite well when using one and two years less pre-treatment data. After 2014, we see a similar increase in the synthetic control that is seen when using the full treatment window.

Total Gang Related Homicide Reported in the SHR

Figure 47a illustrates the gang related homicide rate per 100,000 for Colorado and its synthetic counterpart. The actual gang related homicide rate per 100,000 in Colorado is represented by the green solid line and the synthetic Colorado is

FIGURE 46.
Washington Synthetic Control for Drug Related Homicide Reported, Demeaned



(a) Synthetic versus Actual (b) Contributors to Synthetic Control

(c) Placebo Synthetic Controls (d) MSPE Ratio Distribution

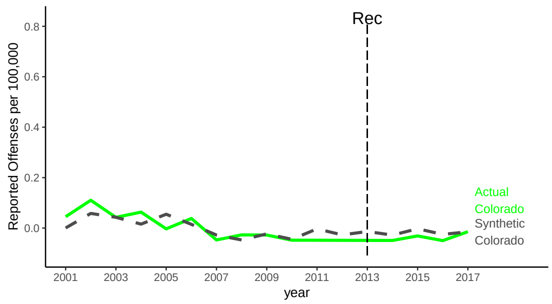
(e) Cross Validation (f) Cross Validation

represented by the grey dashed line. Figure 47b illustrates the composition of states chosen to create synthetic Colorado by shading states by their relative contribution. Prior to treatment, synthetic Colorado matches the direction and magnitude of deviations from pre-treatment mean. After legalization, synthetic Colorado continues to match the deviations of actual Colorado. Figure 47c illustrates that Colorado's deviation from its synthetic counterpart is relatively small compared to the deviations of each non treated state from their own synthetic counterpart. Figure 47d illustrates the distribution of pre MSPE to post MSPE ratio for each state. Notably, many of the placebo states fall to the right of Colorado, meaning many other states have larger post-treatment fit to pre-treatment fit ratios. The point estimate and permutation based p-value corresponding with Figure 47 are reported in Table 18. Figures 47e, and 47f illustrate synthetic Colorado when moving treatment back one and two years respectively. This exercise allows us to see how well synthetic Colorado predicts actual Colorado absent treatment. Notably, with a shorter window of data, the synthetic control approach creates a synthetic Colorado with an in crease in gang related homicides in 2016.

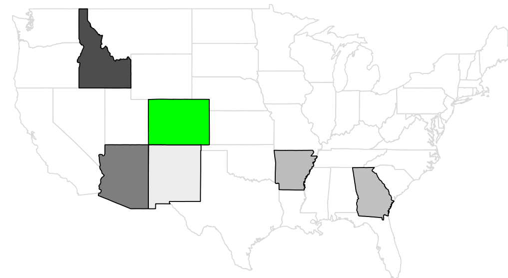
Figure 48a illustrates the gang related homicide rate per 100,000 for Washington and its synthetic counterpart. The actual gang related homicide rate per 100,000 in Washington is represented by the green solid line and the synthetic Washington is represented by the grey dashed line. Figure 48b illustrates the composition of states chosen to create synthetic Washington by shading states by their relative contribution to synthetic Washington. Prior to treatment, synthetic Washington matches both the direction and magnitude of deviations from pre-treatment mean. After legalization, synthetic Washington increases relative to actual Washington. Figure 48c illustrates that Washington's deviation from its synthetic counterpart is relatively large compared to the deviations from their synthetic counterpart of each non treated state. Figure 48d illustrates the distribution of pre MSPE to post MSPE for each state. Notably, many of the placebo states fall to the left of Washington, meaning Washington has one of the largest post-treatment fit to pre-treatment fit ratios. The point estimate and permutation based p-value corresponding with Figure 48 are reported in Table 18.

FIGURE 47.
Colorado Synthetic Control for Gang Related Homicide Reported, Demeaned

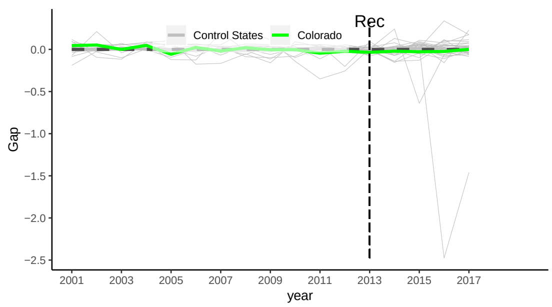
(a) Synthetic versus Actual



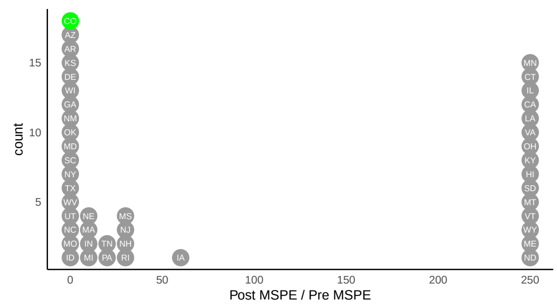
(b) Contributors to Synthetic Control



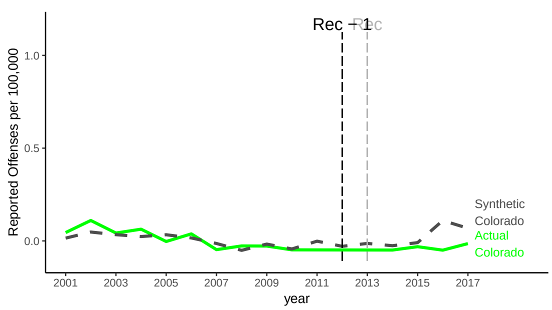
(c) Placebo Synthetic Controls



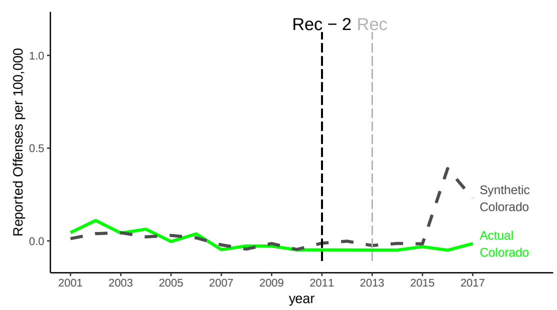
(d) MSPE Ratio Distribution



(e) Cross Validation



(f) Cross Validation



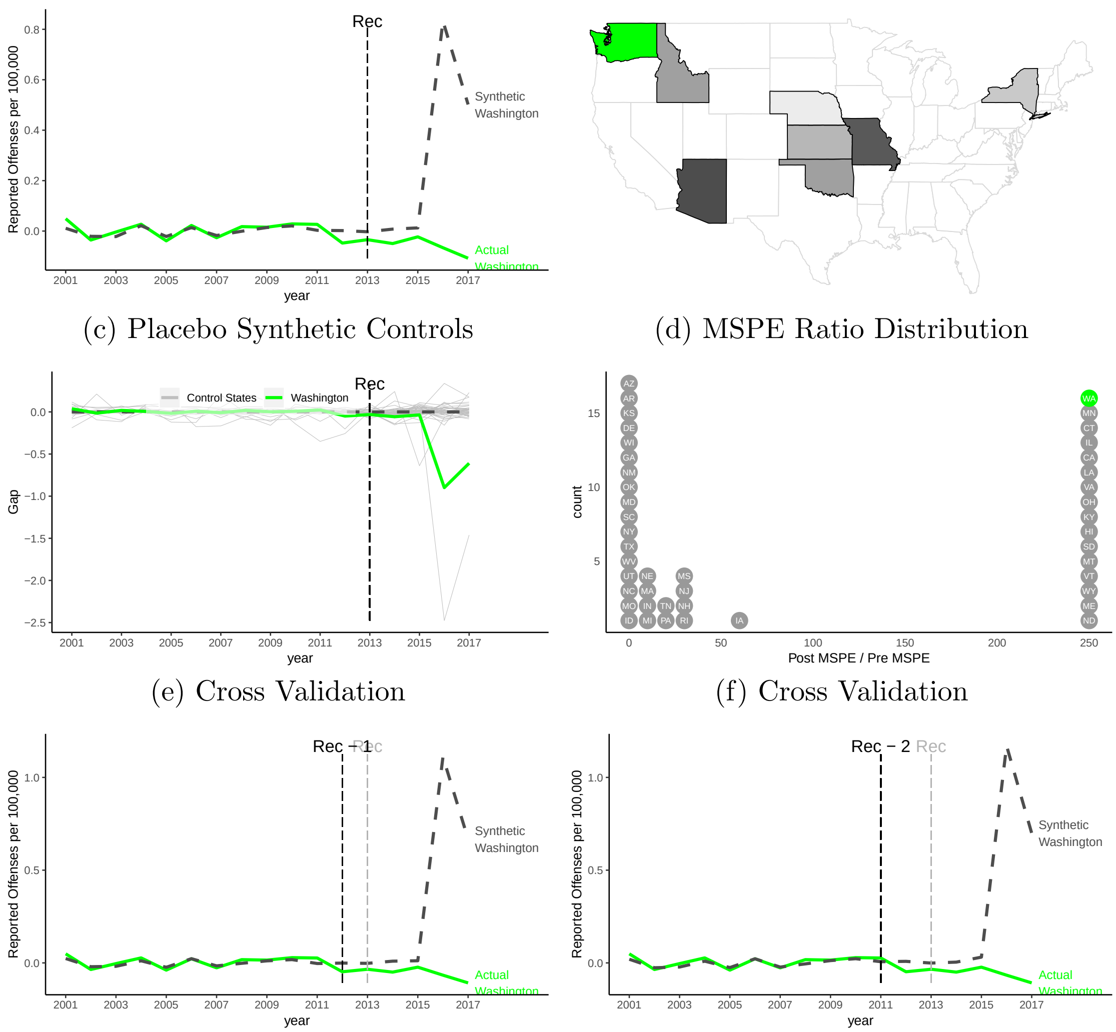
Figures 48e, and 48f illustrate synthetic Washington when moving treatment back one and two years respectively. This exercise allows us to see how well synthetic Washington predicts actual Washington absent treatment. Notably, with a shorter window of data, the synthetic control continues to match actual Washington up to 2015. After 2015, we see a similar increase in the synthetic control that is seen when using the full treatment window.

FIGURE 48.
Washington Synthetic Control for Gang Related Homicide Reported, Demeaned

(a) Synthetic versus Actual (b) Contributors to Synthetic Control

(c) Placebo Synthetic Controls (d) MSPE Ratio Distribution

(e) Cross Validation (f) Cross Validation

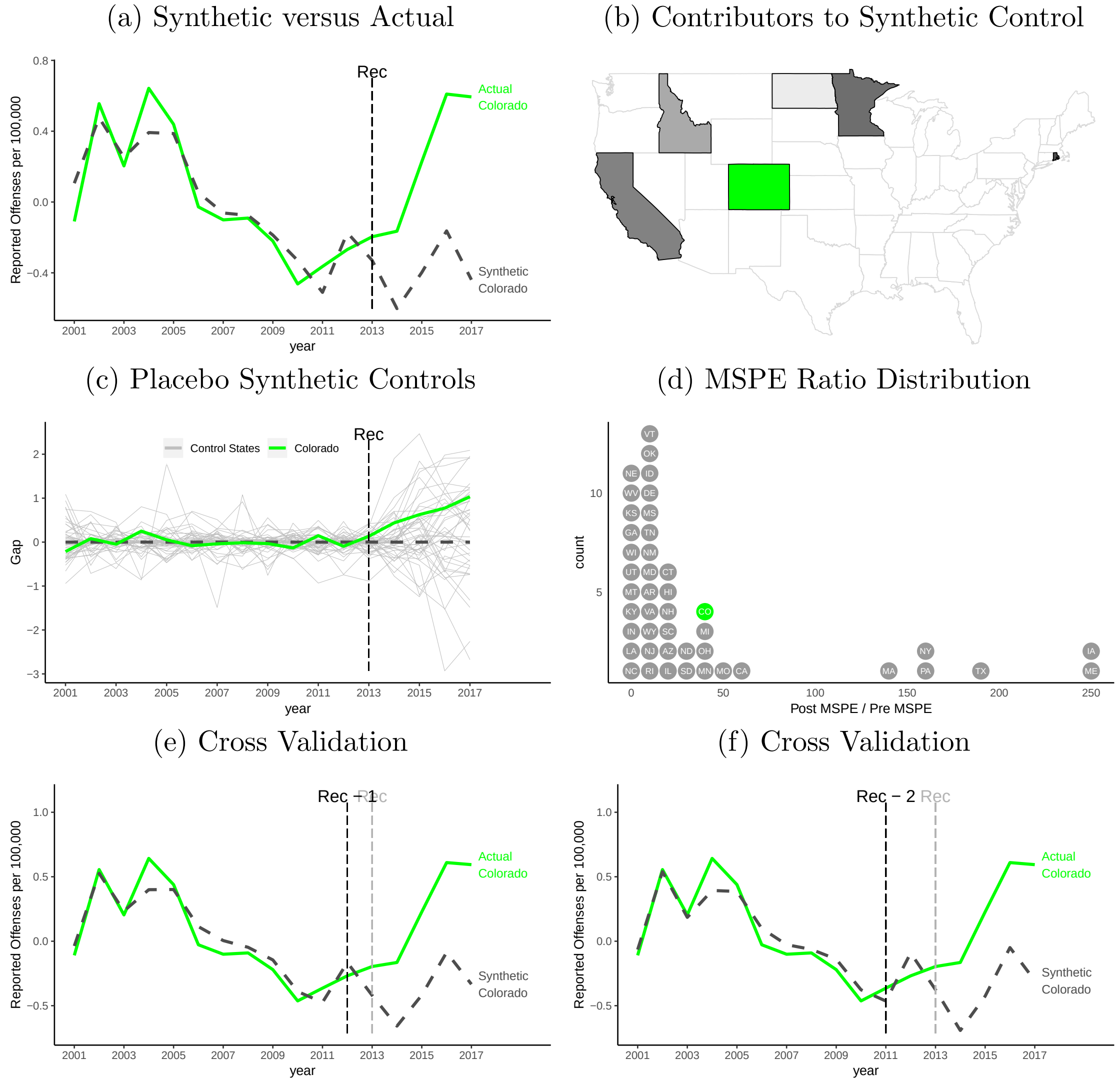


Total Gun Related Homicide Reported in the SHR

Figure 49a illustrates the gun related homicide rate per 100,000 for Colorado and its synthetic counterpart. The actual gun related homicide rate per 100,000 in Colorado is represented by the green solid line and the synthetic Colorado is represented by the grey dashed line. Figure 49b illustrates the composition of states chosen to create synthetic Colorado by shading states by their relative contribution. Prior to treatment, synthetic Colorado matches the direction and magnitude of deviations from pre-treatment mean. After legalization, actual Colorado increases relative to synthetic Colorado. Figure 49c illustrates that Colorado's deviation from its synthetic counterpart is relatively small compared to the deviations of each non treated state from their own synthetic counterpart. Figure 49d illustrates the distribution of pre MSPE to post MSPE ratio for each state. Notably, many of the placebo states fall to the right of Colorado, meaning many other states have larger post-treatment fit to pre-treatment fit ratios. The point estimate and permutation based p-value corresponding with Figure 49 are reported in Table 18. Figures 49e, and 49f illustrate synthetic Colorado when moving treatment back one and two years respectively. This exercise allows us to see how well synthetic Colorado predicts actual Colorado absent treatment. Notably, with a shorter window of data, the synthetic control approach creates a similar synthetic Colorado as with the full pre-treatment window of data.

Figure 50a illustrates the gun related homicide rate per 100,000 for Washington and its synthetic counterpart. The actual gun related homicide rate per 100,000 in Washington is represented by the green solid line and the synthetic Washington is represented by the grey dashed line. Figure 50b illustrates the composition of states chosen to create synthetic Washington by shading states by their relative contribution to synthetic Washington. Prior to treatment, synthetic Washington matches both the direction and magnitude of deviations from pre-treatment mean. After legalization, synthetic Washington tracks actual Washington until 2015. In 2015 actual Washington levels off and synthetic Washington continues to increase. Figure 50c illustrates that Washington's deviation from

FIGURE 49.
Colorado Synthetic Control for Gun Homicide Reported, Demeaned



(a) Synthetic versus Actual

(b) Contributors to Synthetic Control

(c) Placebo Synthetic Controls

(d) MSPE Ratio Distribution

(e) Cross Validation

(f) Cross Validation

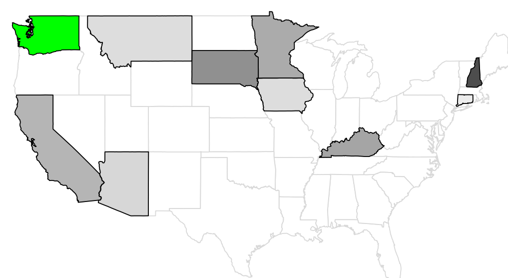
its synthetic counterpart is relatively small compared to the deviations from their synthetic counterpart of each non treated state. Figure 50d illustrates the distribution of pre MSPE to post MSPE for each state. Notably, many of the placebo states fall to the right of Washington, meaning Washington has one of the smallest post-treatment fit to pre-treatment fit ratios. The point estimate and permutation based p-value corresponding with figure 50 are reported in Table 18.

Figures 50e, and 50f illustrate synthetic Washington when moving treatment back one and two years respectively. This exercise allows us to see how well synthetic Washington predicts actual Washington absent treatment. Notably, with a shorter window of data, the synthetic control continues to match actual Washington up to 2015. After 2015, we see a similar increase in the synthetic control that is seen when using the full pre-treatment window of data.

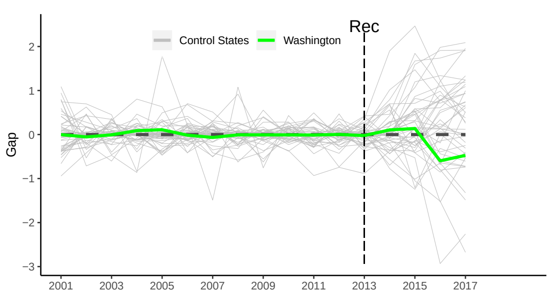
FIGURE 50.
Washington Synthetic control for Gun Homicide Reported, Demeaned

(a) Synthetic versus Actual

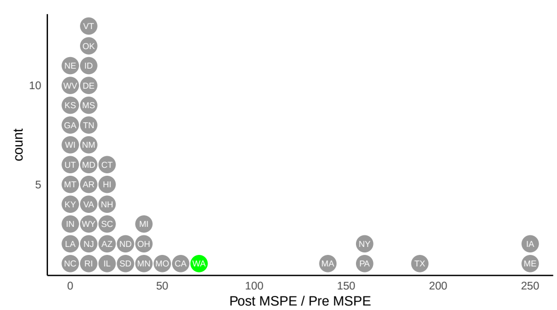
(b) Contributors to Synthetic Control



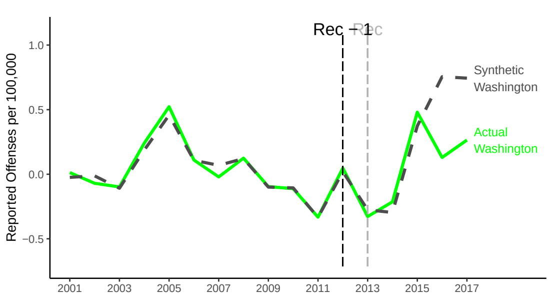
(c) Placebo Synthetic Controls



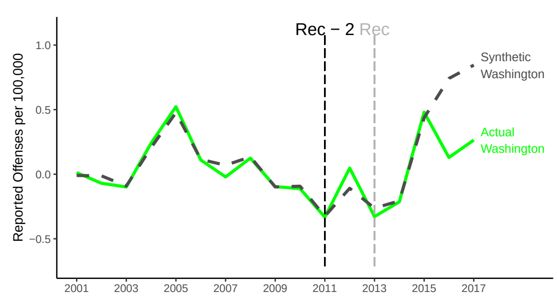
(d) MSPE Ratio Distribution



(e) Cross Validation



(f) Cross Validation



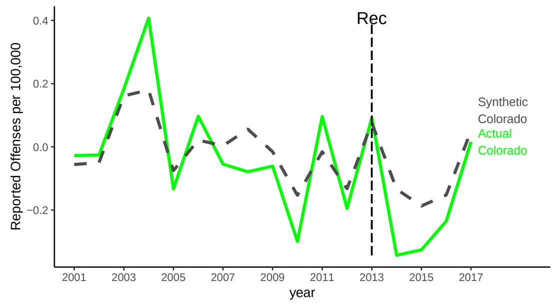
Total Non-gun Related Homicide Reported in the SHR

Figure 51a illustrates the non-gun related homicide rate per 100,000 for Colorado and its synthetic counterpart. The actual non-gun related homicide rate per 100,000 in Colorado is represented by the green solid line and the synthetic Colorado is represented by the grey dashed line. Figure 51b illustrates the composition of states chosen to create synthetic Colorado by shading states by their relative contribution. Prior to treatment, synthetic Colorado matches the direction of deviations from pre-treatment mean as actual Colorado but fails to match the magnitude of deviations from pre-treatment mean. After legalization, synthetic Colorado continues on a similar trend and actual Colorado drops off relative to synthetic. Figure 51c illustrates that Colorado's deviation from its synthetic counterpart is relatively small compared to the deviations of each non treated state from their own synthetic counterpart. Figure 51d illustrates the distribution of pre MSPE to post MSPE ratio for each state. Notably, many of the placebo states fall to the right of Colorado, meaning many other states have larger post-treatment fit to pre-treatment fit ratios. The point estimate and permutation based p-value corresponding with Figure 51 are reported in Table 18. Figures 51e, and 51f illustrate synthetic Colorado when moving treatment back one and two years respectively. This exercise allows us to see how well synthetic Colorado predicts actual Colorado absent treatment.

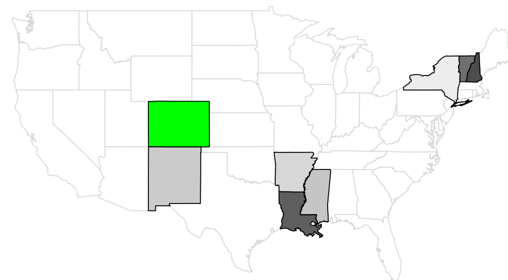
Figure 52a illustrates the non-gun related homicide rate per 100,000 for Washington and its synthetic counterpart. The actual non-gun related homicide rate per 100,000 in Washington is represented by the green solid line and the synthetic Washington is represented by the grey dashed line. Figure 52b illustrates the composition of states chosen to create synthetic Washington by shading

FIGURE 51.
Colorado Synthetic Control for Non-gun Homicide Reported,
Demeaned

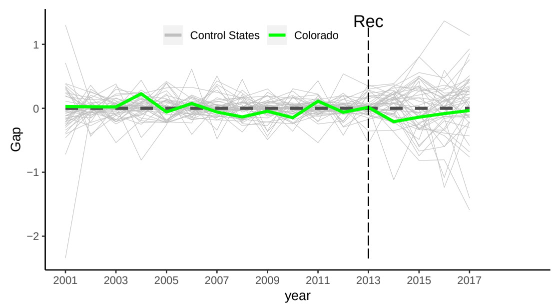
(a) Synthetic versus Actual



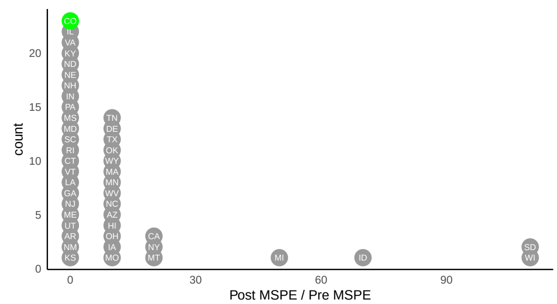
(b) Contributors to Synthetic Control



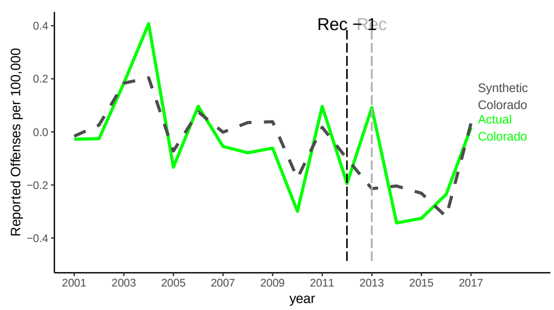
(c) Placebo Synthetic Controls



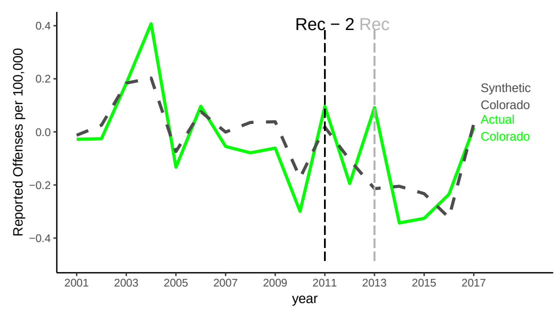
(d) MSPE Ratio Distribution



(e) Cross Validation



(f) Cross Validation



states by their relative contribution to synthetic Washington. Prior to treatment, synthetic Washington matches direction of deviations from pre-treatment mean but fails to match the magnitude of deviations. After legalization, synthetic Washington moves in the same direction as actual Washington but again, with smaller magnitude. Figure 52c illustrates that Washington's deviation from its synthetic counterpart is relatively small compared to the deviations from their synthetic counterpart of each non treated state. Figure 52d illustrates the distribution of pre MSPE to post MSPE for each state. Notably, many of the placebo states fall to the right of Washington, meaning Washington has one of the smallest post-treatment fit to pre-treatment fit ratios. The point estimate and permutation based p-value corresponding with figure 52 are reported in Table 18. Figures 52e, and 52f illustrate synthetic Washington when moving treatment back one and two years respectively. This exercise allows us to see how well synthetic Washington predicts actual Washington absent treatment.

Total Offender Known to Victim Homicide Reported in the SHR

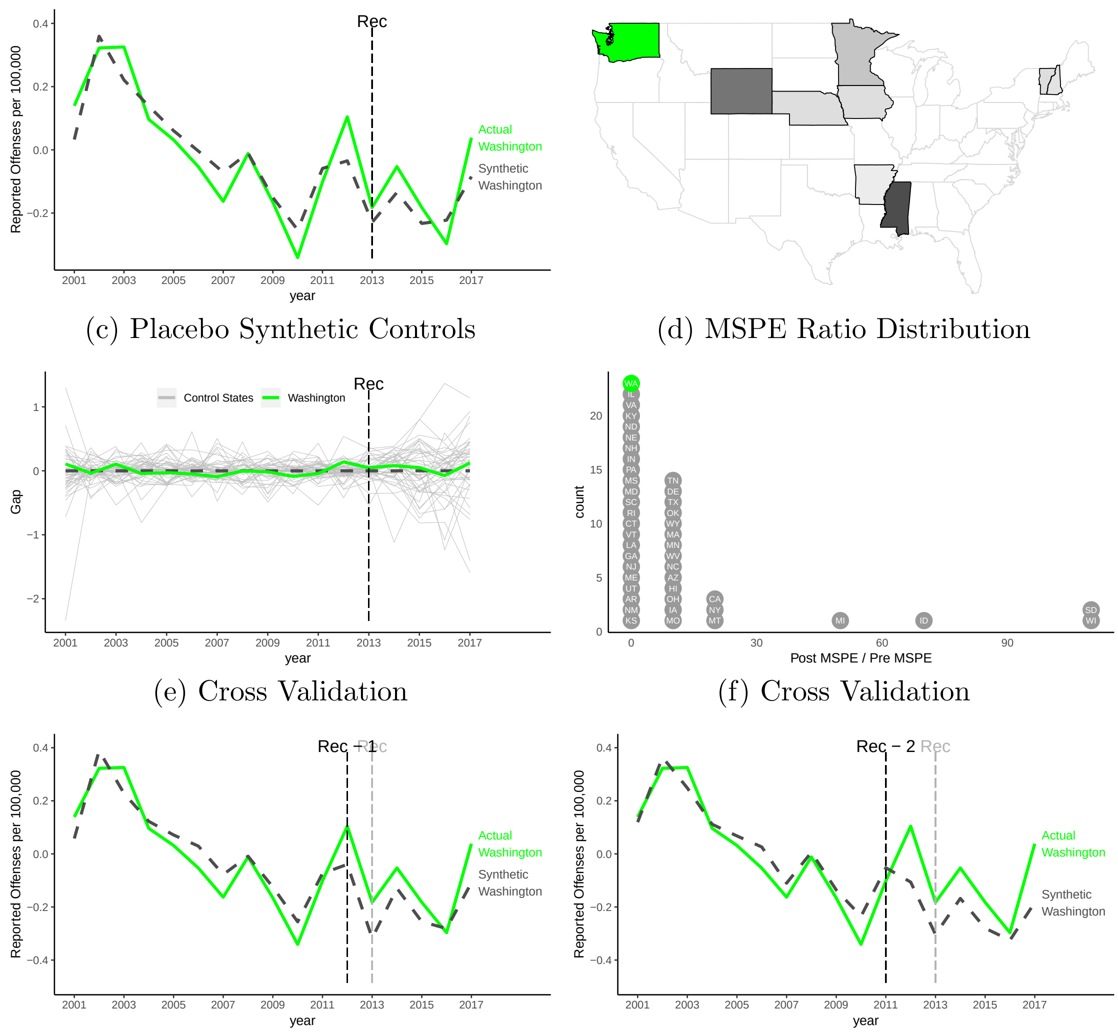
Figure 53a illustrates the offender known to victim homicide rate per 100,000 for Colorado and its synthetic counterpart. The actual offender known to victim homicide rate per 100,000 in Colorado is represented by the green solid line and the synthetic Colorado is represented by the grey dashed line. Figure 53b illustrates the composition of states chosen to create synthetic Colorado by shading states by their relative contribution. Prior to treatment, synthetic Colorado matches the general direction of deviations from pre-treatment mean as actual Colorado but fails to match the magnitude of deviations from pre-treatment mean. After legalization, synthetic Colorado continues on a similar trend and actual Colorado increases relative to synthetic. Figure 53c illustrates that Colorado's deviation from its synthetic counterpart is relatively small compared to the deviations of each non treated state from their own synthetic counterpart. Figure 53d illustrates the distribution of pre MSPE to post MSPE ratio for each state. Notably, many of the placebo states fall to the right of Colorado, meaning many other states have larger post-treatment fit to pre-treatment fit ratios. The point estimate and permutation

FIGURE 52.
Washington Synthetic Control for Non-gun Homicide Reported, Demeaned

(a) Synthetic versus Actual (b) Contributors to Synthetic Control

(c) Placebo Synthetic Controls (d) MSPE Ratio Distribution

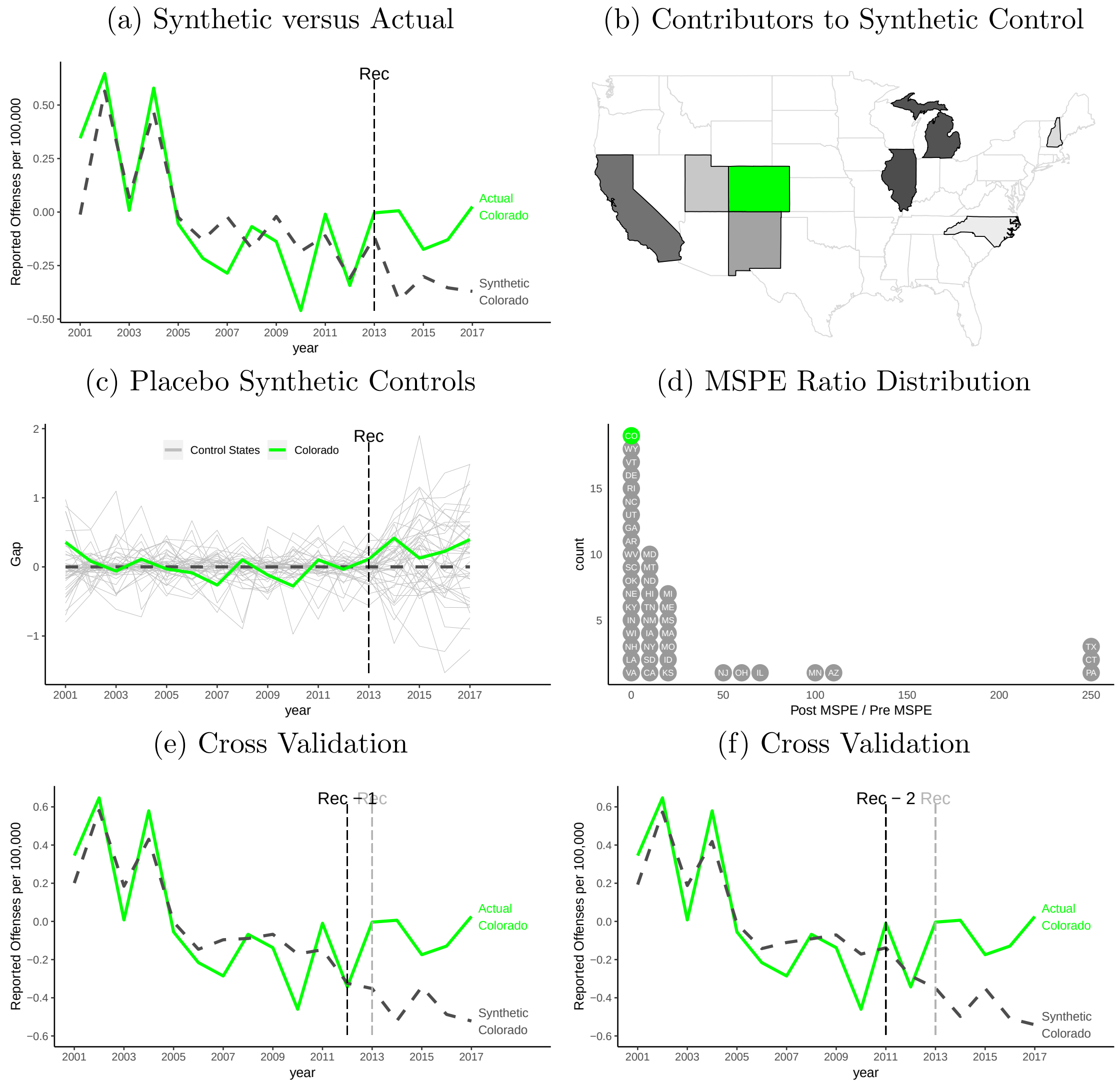
(e) Cross Validation (f) Cross Validation



based p-value corresponding with Figure 53 are reported in Table 18. Figures 53e, and 53f illustrate synthetic Colorado when moving treatment back one and two years respectively. This exercise allows us to see how well synthetic Colorado predicts actual Colorado absent treatment.

Figure 54a illustrates the offender known to victim homicide rate per 100,000 for Washington and its synthetic counterpart. The actual offender

FIGURE 53.
Colorado Synthetic Control for Homicide with
Known Offender Reported, Demeaned



known to victim homicide rate per 100,000 in Washington is represented by the green solid line and the synthetic Washington is represented by the grey dashed line. Figure 54b illustrates the composition of states chosen to create synthetic Washington by shading states by their relative contribution to synthetic Washington. Prior to treatment, synthetic Washington matches direction of deviations from pre-treatment mean but fails to match the magnitude of deviations in 2012. After legalization, synthetic Washington moves in the same direction as actual Washington for two years, then actual Washington decreases while synthetic increases. Figure 54c illustrates that Washington's deviation from its synthetic counterpart is relatively small compared to the deviations from their synthetic counterpart of each non treated state. Figure 54d illustrates the distribution of pre MSPE to post MSPE for each state. Notably, many of the placebo states fall to the right of Washington, meaning Washington has one of the smallest post-treatment fit to pre-treatment fit ratios. The point estimate and permutation based p-value corresponding with figure 54 are reported in Table 18. Figures 54e, and 54f illustrate synthetic Washington when moving treatment back one and two years respectively. This exercise allows us to see how well synthetic Washington predicts actual Washington absent treatment.

Total Offender Unknown to Victim Homicide Reported in the SHR

Figure 55a illustrates the offender unknown to victim homicide rate per 100,000 for Colorado and its synthetic counterpart. The actual offender unknown to victim homicide rate per 100,000 in Colorado is represented by the green solid line and the synthetic Colorado is represented by the grey dashed line. Figure 55b illustrates the composition of states chosen to create synthetic Colorado by shading states by their relative contribution. Prior to treatment, synthetic Colorado matches the general direction and magnitude of deviations from pre-treatment mean as actual Colorado. After legalization, synthetic Colorado increases but actual Colorado increases by a larger amount. Figure 55c illustrates that Colorado's deviation from its synthetic counterpart is relatively small compared to the deviations of each non treated state from their own synthetic counterpart.

FIGURE 54.
Washington Synthetic Control for Homicide with
Known Offender Reported, Demeaned

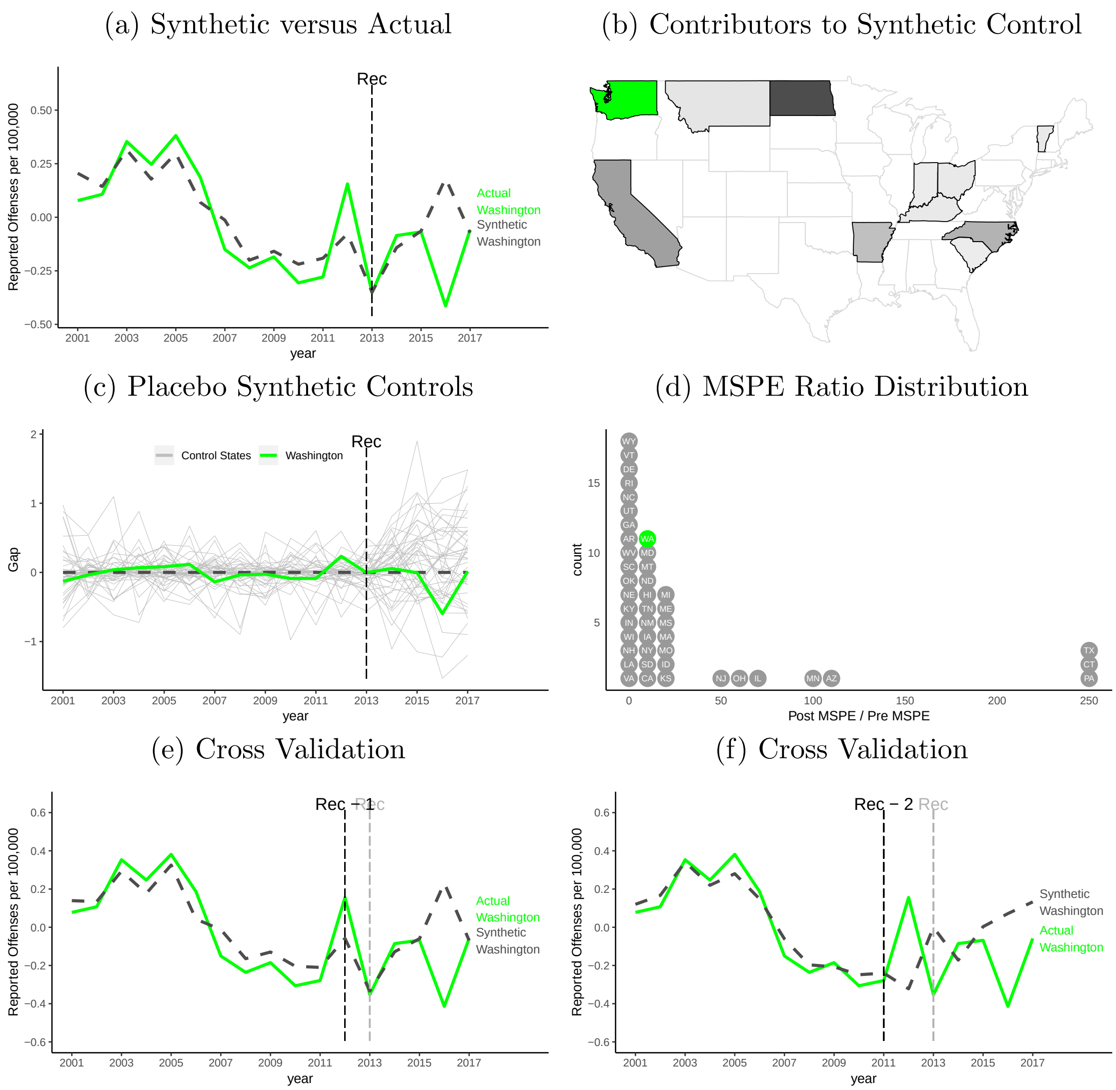
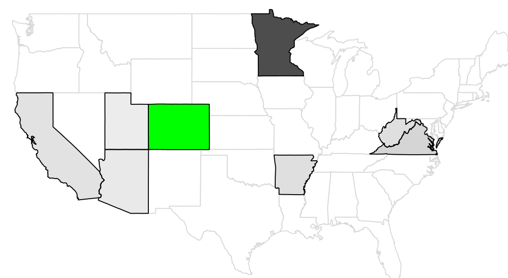


Figure 55d illustrates the distribution of pre MSPE to post MSPE ratio for each state. Notably, many of the placebo states fall to the right of Colorado, meaning many other states have larger post-treatment fit to pre-treatment fit ratios. The point estimate and permutation based p-value corresponding with Figure 55 are reported in Table 18. Figures 55e, and 55f illustrate synthetic Colorado when moving treatment back one and two years respectively. This exercise allows us to see how well synthetic Colorado predicts actual Colorado absent treatment. When the pre-treatment window is two years smaller, synthetic Colorado matches actual Colorado in deviations from mean up to 2017.

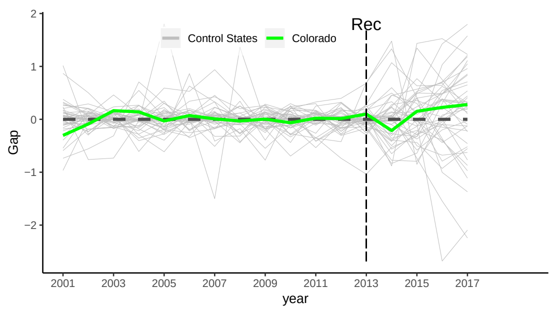
FIGURE 55.
Colorado Synthetic Control for Homicide with
Unknown Offender Reported, Demeaned

(a) Synthetic versus Actual

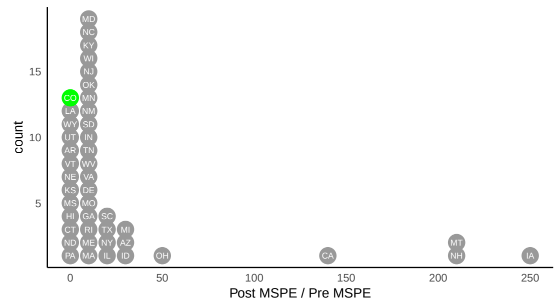
(b) Contributors to Synthetic Control



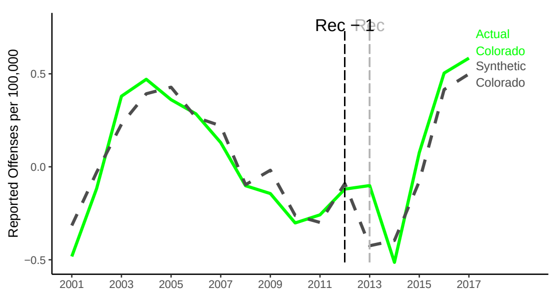
(c) Placebo Synthetic Controls



(d) MSPE Ratio Distribution



(e) Cross Validation



(f) Cross Validation

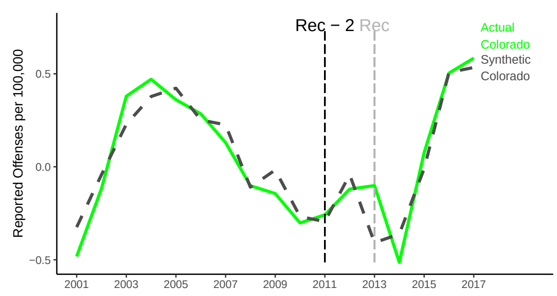


Figure 56a illustrates the offender unknown to victim homicide rate per 100,000 for Washington and its synthetic counterpart. The actual offender unknown to victim homicide rate per 100,000 in Washington is represented by the green solid line and the synthetic Washington is represented by the grey dashed line. Figure 56b illustrates the composition of states chosen to create synthetic Washington by shading states by their relative contribution to synthetic Washington. Prior to treatment, synthetic Washington matches direction and magnitude of deviations from pre-treatment mean of actual Washington. After legalization, synthetic Washington moves in the same direction as actual Washington for two years, then actual Washington increases while synthetic Washington levels off. Figure 56c illustrates that Washington's deviation from its synthetic counterpart is relatively small compared to the deviations from their synthetic counterpart of each non treated state. Figure 56d illustrates the distribution of pre MSPE to post MSPE for each state. Notably, many of the placebo states fall to the right of Washington, meaning Washington has the fifth largest post-treatment fit to pre-treatment fit ratios. The point estimate and permutation based p-value corresponding with figure 56 are reported in Table 18. Figures 56e, and 56f illustrate synthetic Washington when moving treatment back one and two years respectively. This exercise allows us to see how well synthetic Washington predicts actual Washington absent treatment.

Differences in Differences

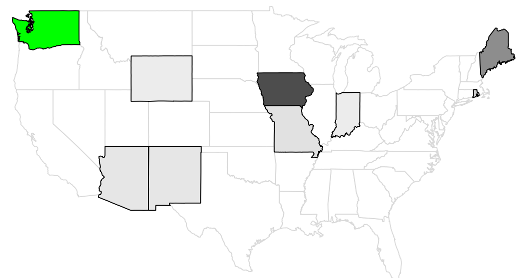
In addition to synthetic control, we analyze the effects of marijuana legalization using a differences in differences model. We estimates the following differences in in differences model.

$$\text{Rate}_{it} = D_{it} + \gamma_t + \delta_i + \epsilon_{it}$$

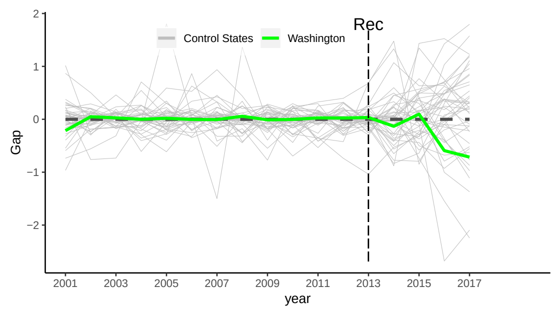
FIGURE 56.
Washington Synthetic Control for Homicide with
Unknown Offender Reported, Demeaned

(a) Synthetic versus Actual

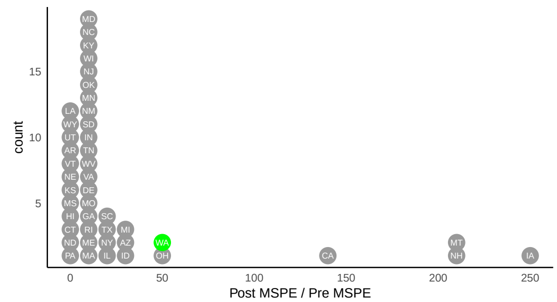
(b) Contributors to Synthetic Control



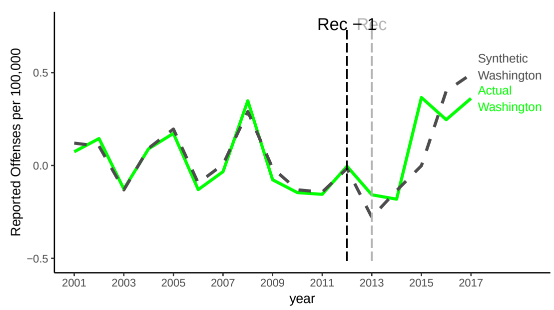
(c) Placebo Synthetic Controls



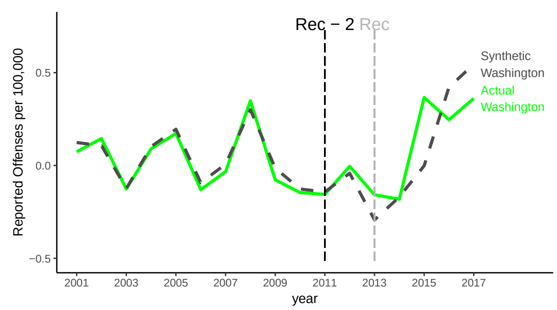
(d) MSPE Ratio Distribution



(e) Cross Validation



(f) Cross Validation



Rate_{it} is the homicide rate per 100,000 people in a given state. D_{it} is a binary treatment indicator turning on after a state legalizes marijuana. γ_t and δ_i are year and state fixed effects respectively, and ϵ_{it} in an error term. Treated states include Washington-2014, Colorado-2014, Oregon-2015, Alaska-2015, and Nevada 2017. Table 19 includes the point estimates for our 8 different definitions of homicide with the standard errors in parenthesis below each point estimate. Standard errors are clustered at the state level. Column 2 of Table 19 includes estimates using the log of the homicide rate as the dependent variable ln(Rate).

Across all definitions of homicide in column one of Table 19, we fail to reject the null hypothesis that our point estimates are significantly different from zero. In column two we find evidence that gang related and non-gun related homicides slightly decrease after marijuana legalization. For all other definitions of homicide, we fail to reject the null hypothesis that our point estimates are significantly different from zero.

Most states did not start legal marijuana sales January 1 of their legalizing year. For this reason we include a specification that allows for partial treatment. Legalization and sales start dates can be seen in Figure 35. Table 20 reports the point estimates where we allow for partial treatment. We specify partial treatment as follows: Washington-0.5-2014, Colorado-1-2014, Oregon-0.25-2015, Alaska-0.25-2015, and Nevada-0.5-2017. The point estimates estimated when using a differences in differences model allowing for partial treatment are consistent with estimates from a differences in differences model with binary treatment. With the exception of gang related homicides, we fail to reject the null hypothesis that our point estimates are significantly different from zero. For gang related homicides, we find evidence that there may be a slight decrease in homicide rates in marijuana legalizing states.

Conclusions and Policy Implications

Marijuana policy continues to evolve quickly in the United States and internationally where legal sales began nationwide in Canada in 2018. While states have seen some benefits of legalization through increased tax revenue,

TABLE 19.
Differences in Differences Results for Marijuana Legalization

	Legalizing States		
	(Level)	(Logged)	(Rate)
UCR Murder			
Rec	0.475	0.001	4.1787
	(0.423)	(0.007)	
Homicide			
Rec	0.340	-0.002	4.0729
	(0.372)	(0.007)	
Drug Related Homicide			
Rec	-0.086	0.011	0.341
	(0.071)	(0.012)	
Gang Related Homicide			
Rec	-0.057	-0.012***	0.0924
	(0.037)	(0.004)	
Gun Related Homicide			
Rec	0.329	0.003	2.3622
	(0.359)	(0.013)	
Non-Gun Related Homicide			
Rec	0.011	-0.010**	1.7107
	(0.073)	(0.005)	
Known Offender Homicide			
Rec	0.130	-0.004	2.3614
	(0.238)	(0.006)	
Unknown Offender Homicide			
Rec	0.210	0.007	1.7115
	(0.150)	(0.013)	

Differences in differences regressions include the homicide rate as the the dependent variable, a treatment indicator turning on after a state legalizes, state fixed effects, and year fixed effects. Treated states include Washington-2014, Colorado-2014, Oregon-2015, Alaska-2015, Nevada-2107. Standard errors are clustered at the state level

TABLE 20.
Differences in Differences Results for Marijuana Legalization
Partial Treatment

	Legalizing States		
	(Level)	(Logged)	(Rate)
UCR Murder			
Rec	0.338	-0.000	4.1787
	(0.407)	(0.007)	
Homicide			
Rec	0.286	-0.003	4.0729
	(0.365)	(0.006)	
Drug Related Homicide			
Rec	-0.002	0.013	0.341
	(0.072)	(0.013)	
Gang Related Homicide			
Rec	-0.066*	-0.014***	0.0924
	(0.039)	(0.003)	
Gun Related Homicide			
Rec	0.167	-0.002	2.3622
	(0.266)	(0.010)	
Non-Gun Related Homicide			
Rec	0.119	-0.002	1.7107
	(0.138)	(0.004)	
Known Offender Homicide			
Rec	0.123	-0.004	2.3614
	(0.273)	(0.007)	
Unknown Offender Homicide			
Rec	0.164	0.003	1.7115
	(0.123)	(0.009)	

Differences in differences regressions include the homicide rate as the the dependent variable, a treatment indicator turning on after a state legalizes, state fixed effects, and year fixed effects. Treated states include Washington(0.5)-2014, Colorado-2014, Oregon(0.25)-2015, Alaska(0.25)-2015, Nevada(0.5)-2017. Standard errors are clustered at the state level the consequences of legalization remain unknown. Optimal taxes would serve to potentially offset externalities. If legalization increases homicides, then the external costs might exceed current revenues given the high social costs associated with deaths, particularly those of young individuals.

Following marijuana's legalization, homicides have increased in Colorado and Washington. What would have happened in the counterfactual is the most important question as many other factors including policing (Evans and Owens, 2007; Chalfin and McCrary, 2017) punishment , and illegal drug markets Evans et al. (2018) could have influenced violence as well. To this end, we investigate marijuana's legalization and its potential role in the recent nationwide increase in homicides. We use both difference-in-differences and synthetic control methods. We fail to reject the null that homicide rates did not shift in Colorado and Washington in ways that would not be predicted by counterfactual trends.

In our analyses, we investigate both murders (based on the UCR), homicides (based on the SHR), and a wide variety of sub categories of homicides. While on average many of our estimates are close to zero, there are some instances where our point estimates would suggest treatment could have lead to a 10-20 percent increase in homicides. However, even these cases, we fail to reject the null based on randomization inference. This suggests homicides have shifted by a large amount in many parts of the country. This is partly a result of a broader national trend (as homicide rates have increased by roughly 20 percent since 2013), and because some as the categories of homicides are split up, they naturally become more rare.

So what has caused in the increased homicides nationally? The question remains open. Prior research suggests many plausible channels. It could be a result of the improving economy Ruhm (2000), increases in gun ownership (Depetris-

Chauvin, 2015; Levine and McKnight, 2017), emerging illegal drug markets (Grogger and Willis, 2000; Fryer Jr et al., 2013; Evans et al., 2018), or a "Ferguson Effect" (Gross and Mann, 2017; Pyrooz et al., 2016). Each of these potential channels merits attention in future research.

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