Testing Public Policy at the Frontier: The Effect of the $15 Minimum on Public Safety in Seattle*

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Abstract

Research Summary: In 2017, Seattle, Washington, became the first city in the United States to increase its minimum wage to $15 per hour, more than double the federal minimum wage. Not only was a $15 minimum wage unprecedented, but the increase was also extremely rapid, with the minimum wage rising by nearly 60 percent in just two years. Using a synthetic differences-in-differences estimator, we consider the impact of Seattle's landmark minimum wage legislation on public safety. While there is speculative evidence for an increase in commercial burglaries, we find little evidence that Seattle experienced a change in its aggregate rate of violent or property offending relative to other U.S. cities. To better understand the mechanisms underlying our findings, we investigate the impacts of the local wage law on employment and earnings for Seattle's low-skilled labor market. We detect no meaningful adverse effects on the employment rates of low-wage workers.

Policy Implications: Our results suggest that Seattle increased its minimum wage without compromising public safety. Seattle's experience shows that increasing wages can be a tool for reducing the opportunity cost of crime without reducing employment levels. To the extent that other cities enact higher minimum wages to a level that generates unemployment among low-skilled workers, public safety changes could be considerably different.

Keywords: minimum wage, crime, unemployment, synthetic control method

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1 Introduction

Reflecting the historically popular idea that crime is the result of material deprivation, research on the relationship between labor market conditions and crime is among the oldest and most enduring topics in social science scholarship on public safety (Wright, 1893; Macilwee, 2011; Emsley, 2013). Criminologists have long theorized that a lack of acceptable employment opportunities or a properly remunerated job may lead some individuals to turn to crime, as unemployment may lead to psychological strain (Agnew, 1992; Agnew et al., 1996), changes in daily routines (Cohen and Felson, 1979; Cook, 2010; Andresen, 2012), or a weakening of social ties (Aaltonen et al., 2013; Sampson and Laub, 1990). In economics, the neoclassical economic model of crime draws on a simple expected utility model introduced in a seminal contribution by Becker (1968). This model envisions crime as a gamble undertaken by a rational individual and, according to this framework, the aggregate supply of offenses will depend on social investments in police and prisons as well as on labor-market opportunities that increase the relative cost of time spent in illegal activities (Chalfin and McCrary, 2017).[1] The recognition that market wages are an input into the crime production function is likewise a prominent feature of the large literature on rational choice theory in criminology which posits that offending is often motivated by low wages and harsh conditions in legal work (Fagan and Freeman, 1999; Loughran et al., 2016; Nguyen and Loughran, 2017).[2]

Due to the centrality of the relationship between labor market conditions and crime in both criminological and economic theory, there is now a large and prominent empirical literature that studies the effects of macroeconomic conditions and the vibrancy of low wage labor markets on public safety. At the macro-level, this research considers the impact of fluctuations in the business cycle (Cantor and Land, 1985; Cook and Zarkin, 1985; Raphael and Winter-Ebmer, 2001; Lin, 2008; Cook, 2010; Bushway et al., 2012) and changes in the wage structure of law wage labor markets (Grogger, 1998; Gould et al., 2002; Machin and Meghir, 2004; Schnepel, 2018).[3] At the individual level, the literature considers whether individuals increase their participation in crime in response to job loss (Thornberry and Christenson, 1984; Farrington et al., 1986; Witte and Tauchen, 1993; Crutchfield and Pitchford, 1997; Aaltonen et al., 2013).

[1] This conceptualization of the market wage as a determining factor in a potential offender's utility function has given rise to important follow-up work which seeks to fine-tune the model of Becker (1968) including particularly prominent contributions by Ehrlich (1973) who introduces the concept of "demand" for crime, Polinsky and Shavell (1984) who explores the dimensions of using fines versus imprisonment and research by Nagin and Pogarsky (2001) and McCrary (2010) who extend the neoclassical model to incorporate time preferences among offenders. Beginning in the late 1970s, there have been many attempts to develop an empirical test of Becker's theory. Early and especially prominent work includes that of Witte (1980), Myers (1983) and Cornwell and Trumbull (1994).

[2] For reviews of the literature on work and crime see Mustard (2010), Chalfin and Raphael (2011) and Draca and Machin (2015). An older but excellent reference can be found in Fagan and Freeman (1999).

[3] There is also a literature that considers whether there is a link between wage *inequality* and crime. This research reaches mixed conclusions (Fowles and Merva, 1996; Kelly, 2000; Wu and Wu, 2012).

While findings are mixed (Chiricos, 1987), the majority of the available literature — especially that which is newer and more methodologically rigorous — is consistent with the theory and reports a causal link between property offending and unemployment (Raphael and Winter-Ebmer, 2001; Machin and Meghir, 2004; Yang, 2017) as well as the generosity of wages in low-skilled industries (Grogger, 1998; Gould et al., 2002; Machin and Meghir, 2004; Schnepel, 2018). On the other hand, there is little evidence that violent crimes are sensitive to macroeconomic conditions (Cook, 2010).

Given that there is strong theoretical and empirical support for the idea that wages are an important input into an offender's decision function, in recent years, scholars have focused their attention on the effects of minimum wage laws which legally mandate the lowest wage that an employer can pay an employee for an hour of work. While rational choice theory predicts that, other things equal, an increase in wages will increase the opportunity cost of crime thus leading to diminished incentives to offend, economic theory suggests the possibility that minimum wage laws may not leave other things equal. On the one hand, a higher minimum wage increases the opportunity cost of crime among those who are employed yielding a prediction that is consistent with theory. On the other hand, minimum wage legislation may lead employers to substitute capital for labor thus yielding greater unemployment among workers for whom the minimum wage is binding (Neumark and Wascher, 1992, 2006). As such, the effects of minimum wage legislation on offending are theoretically ambiguous. While there continues to be ongoing debate about the empirical effects of minimum wages on employment rates for low-skilled workers (Neumark et al., 2014; Reich et al., 2017; Jardim et al., 2018b), the majority of the literature finds that, at least in the short-run, employment is less sensitive to increases in the minimum wage than has been supposed (Card, 1992; Card and Krueger, 1995; Dube et al., 2010a).

Consistent with the economics literature which finds limited evidence of a large employment-minimum wage elasticity, the majority of the research on public safety finds that when areas — typically states in the United States or counties in the United Kingdom — become subject to minimum wage ordinances, crime either does not change (Fone et al., 2019) or, in many cases, declines (Hansen and Machin, 2001, 2002; Fernandez et al., 2014; Agan and Makowsky, 2018). However, the findings are not uniform. Indeed research by Beauchamp and Chan (2014) finds that increases in the minimum wage increase both unemployment and offending, an effect which may be especially prominent for younger workers whose employment may be more sensitive to the effects of minimum wage legislation (Fone et al., 2019).[4]

Prior to the last decade, state and, to a lesser extent, federal laws have been the standard means by

[4]There is likewise, evidence that the relationship between minimum wages and crime may be more complex, with scholarship by Braun (2019) reporting evidence of a "U-shaped" response function of crime to the minimum wage.

which minimum wage laws have impacted low-skilled labor markets. However, in the last decade, cities have found themselves at the forefront of efforts to supplement the income of low-wage workers through the passage of minimum wage laws or "living" wage laws for employees who work within the boundary of their jurisdiction. Local ordinances are especially relevant not only due to their widespread and growing use across the United States but also because they have set the pathway to substantial minimum wage increases in a short period, sometimes leading to instantaneous increases in the minimum wage of more than 30 percent.

In May 2014, Seattle, Washington, became the first city in the United States to pass minimum wage legislation fixing the minimum wage as high as $15 per hour, a level that is more than double the current federal minimum wage of $7.25 and which led to a cumulative increase in the city's minimum wage of nearly 60 percent in less than three years. Such an increase in the minimum wage is unprecedented in the United States and, as such, Seattle's minimum wage law has become the subject of intense academic scrutiny. Interestingly, the research that has studied the employment impacts of Seattle's minimum wage law has reached opposing conclusions with Jardim et al. (2018b) and Jardim et al. (2018a) finding that hours worked and net earnings among low-wage workers fell in the aftermath of the minimum wage law and research by Reich et al. (2017) and Allegretto et al. (2018) finding a rise in earnings without discernible effects on employment.

While other research (Fernandez et al., 2014; Fone et al., 2019) studies the "national effect" of minimum or living wage laws using state- or city-level panel data, in this paper we consider the effect of public policy at the frontier, focusing on the case of Seattle.[5] We do so for two reasons. First, Seattle's unprecedented minimum wage law increased the city's minimum wage by 60 percent and may therefore generate effects that are substantially different than the effect of a more typical increase in a jurisdiction's minimum wage.[6] Second, in the aftermath of Seattle's decision to push the policy frontier, a number of other jurisdictions have planned to follow suit. As of the writing of this paper, California, Illinois, and Massachusetts are all set to raise their minimum wages to $15 per hour by January 1, 2025.[7] Among cities, Minneapolis, Los Angeles and Washington, D.C. have each passed legislation which will raise the minimum wage to $15 in the near future and the New York State legislature approved a $15 minimum wage for New York City. As such, the question of how high minimum wages can be pushed without compromising public safety is

[5] We note that case studies have, for a long time, been central to the study of policy evaluation — see e.g., Card (1990) and Card (1992).

[6] Recognition that treatment effects may be substantially nonlinear has been of considerable importance to a number of literatures in criminology including the study of peer effects (Zimmerman and Messner, 2011; Rees and Zimmerman, 2016) and neighborhood effects on crime (Hannon and Knapp, 2003; Hipp and Yates, 2011) .

[7] See https://www.dir.ca.gov/dlse/faq_minimumwage.htm.

particularly timely.

In order to identify the public safety impact of Seattle's minimum wage legislation, we use a differences-in-differences strategy, comparing crime trends in Seattle to other U.S. cities which did not experience a minimum wage increase. To select a credible counterfactual condition for Seattle, we turn to the method of synthetic controls for case studies pioneered by Abadie et al. (2010) and which has since been extended in Doudchenko and Imbens (2016), Hahn and Shi (2017) and Chernozhukov et al. (2017) among others. In recognition of the central identifying assumption of differences-in-differences estimation — that of parallel trends — the methodology selects a weighted average of potential comparison units, in this case, other U.S. cities that did not experience a minimum wage increase, to closely approximate pre-intervention crime trends in Seattle. The methodology likewise offers a powerful placebo test which allows for statistical inferences to be made even though there is only a single treated unit.

Using the method of synthetic controls, we are able to identify a weighted average of U.S. cities which follow a nearly identical crime trajectory to Seattle prior to the enactment of its 2015 minimum wage law. For violent crimes, there is little evidence that Seattle's crime trends diverge from its weighted comparison group after 2015. With respect to property crimes, the evidence is a little more nuanced. While there is evidence that Seattle's burglary rate may have risen by between 15 and 30 percent during the study period, we do not detect evidence of an aggregate change in the rate of property offending. In examining arrest rates among a number of key demographic groups — including young men — we likewise find little evidence that offending increased among any particular subgroup within the population. Overall, the evidence suggests that despite the massive increase in Seattle's minimum wage that occurred after 2015, public safety has remained mostly unchanged. These results are robust to a variety of sensitivity checks including removing cities in Washington's border states from the donor pool, limiting the donor pool to cities with a similar industrial and demographic composition to Seattle, limiting the donor pool to cities which, like Seattle, were experiencing secular growth in wages during the study period, conditioning on a city's exposure to the opioid epidemic and to using a number of new and cutting edge analytic approaches to identify a synthetic comparison group for Seattle.

The lack of a detectable impact on aggregate crime rates may be explained, at least in part, by the limited evidence that Seattle's minimum wage law created large distortions in its low wage labor market. In particular, consistent with research by Reich et al. (2017), using data from the U.S Census Bureau's American Community Survey we find little evidence of employment impacts among low-skilled workers. As this research reports the results of a case study in a single city, there are natural limits with respect to our ability to generalize the findings to other contexts. In order to further assess the external validity of

our principal finding, we perform an auxiliary analysis of four other U.S. cities — Chicago, San Francisco, San Jose, and Sunnyvale — which, like Seattle, have increased their minimum wages substantially, albeit to a lesser degree. While this analysis cannot rule out that minimum wage increases have had a modest impact on some types of property crimes, the evidence, on the whole, suggests that these laws have not had an important impact on public safety.

The remainder of the article is organized as follows. Section 2 provides a brief history of minimum wage legislation in the United States leading up to Seattle's historic passage of a $15 minimum wage law in 2015. Sections 3 and 4 describe the data sources and empirical strategy, respectively. Section 5 presents results and Section 6 concludes.

2 Institutional Background

2.1 U.S. Minimum Wage Legislation

Minimum wages emerged more than fifty years after the end of the Industrial Revolution, principally as a means of addressing the problem of sweatshop labor in densely-packed urban areas. Supported by progressive reformers and early trade unionists, the first minimum wage law appears to have been passed in Belgium in 1886 (Rubinstein, 1936) though some sources point to legislation passed in New Zealand in 1894 (Douglas, 1919). In the United States, Massachusetts became the first state to pass a minimum wage statute in 1912 (Thies, 1990).[8] Within a decade, fifteen more states had passed a minimum wage law though these laws, in practice, did not withstand litigation initiated by employers in the U.S. federal courts. In 1933, during the height of the Great Depression, the United States Congress passed the National Industrial Recovery Act which, among other things, included the first national minimum wage law in the United States.[9] While the U.S. Supreme Court declared the law to be unconstitutional in its 1935 decision in *Schechter Poultry Corp. v. United States*, in 1937, Court reversed course, ruling in favor of the constitutionality of minimum wage legislation in the state of Washington.[10] In 1938, the Congress passed the Fair Labor Standards Act which re-instituted the federal minimum wage (setting a wage floor of 25 cents per hour) and which applied to employees working for firms that were engaged in interstate commerce (Grossman, 1978).[11] During the 1960s, the legislation was expanded to include a larger number

[8] The law was passed in the aftermath of the Lawrence textile strike.

[9] In practice, minimum wages were set on a regional basis.

[10] See *West Coast Hotel Co. v. Parrish.*

[11] Early research in economics notes that, within a few years, the minimum wage had been effectively "repealed" — that is, become non-binding — due to inflation (Stigler, 1946). However, the law also provided for "time-and-a-half" overtime pay based on a 40-hour work week and curtailed the use of child labor in certain industries.

of workers.

While the federal minimum wage has, since 1938, been fixed by the U.S. Congress, in practice, states have been the most active driver of minimum wage legislation, as many states have elected to set a minimum wage that is higher than the federal wage floor. As of January 2018, 29 states had passed minimum wage legislation fixing a minimum wage that is higher than the current federal standard of $7.25 — among states, California, Massachusetts, and Washington have the highest state minimum wages at $13 per hour.[12] Another fifteen states have minimum wages for which the federal law binds. Only six states — Alabama, Louisiana, Mississippi, New Hampshire, South Carolina and Tennessee — have never passed a minimum wage law.

In the last decade, there has been a seismic shift in the policy environment surrounding the minimum wage as cities and counties, rather than states, have taken center stage in pushing the policy frontier. As recently as 2003 only two cities in the United States — San Francisco and Santa Fe, NM — had passed a local minimum wage ordinance, with the remainder of local jurisdictions adhering to the applicable state law. By May 2020, 48 cities and 6 counties in the United States have passed their own minimum wage legislation, fixing a minimum wage that is higher than that which is set by state legislators.[13] Thirty-six of the 48 jurisdictions are in California and, within California, the San Francisco Bay Area is particularly well-represented. While jurisdictions with a local minimum wage ordinance include a number of wealthy smaller cities like Berkeley, Fremont, Malibu and Palo Alto, minimum wage laws have also been passed by a number of large jurisdictions including Los Angeles County, CA and Cook County, IL as well as Los Angeles, Chicago, San Diego and San Jose, four of the ten largest cities in the United States.[14] These newly-passed minimum wage laws compliment a patchwork of more than 120 cities and counties have have passed "living wage laws," which fix the minimum wages of municipal employees and government contractors.[15] In order to provide a sense for the enormity of the shift in the policy regime, in **Figure 1**, we plot the absolute minimum wage (Panel A) and the percentage change in the minimum wage (Panel B) among cities which experienced a change in the minimum wage since 2004. The data are plotted separately for changes that are due to federal, state and local legislation. While changes in state law are the primary shifter of minimum wage legislation prior to 2010, in the last decade local legislation has become the dominant approach. To date, Seattle stands alone in having implemented a $15 minimum wage law that

[12] See https://www.dol.gov/agencies/whd/minimum-wage/state.

[13] See: http://laborcenter.berkeley.edu/minimum-wage-living-wage-resources/inventory-of-us-city-and-county-minimum-wage-ordinances/.

[14] New York City is also subject to minimum wage legislation though its $15 minimum wage was set by legislation at the state level.

[15] See: https://www.nelp.org/wp-content/uploads/2015/03/LocalLivingWageOrdinancesandCoverage.pdf. In 1994 Baltimore, Maryland was the first city in the United States to pass such a living wage ordinance.

is broadly applicable to nearly the entirety of the city's workforce. However, a number of additional cities have passed minimum wage laws which will become active within the next few years.

2.2 Minimum Wage Legislation in Seattle

In May 2014, the City of Seattle approved legislation that increased the city's hourly minimum wage from \$9.47 to \$11 and created a pathway to a \$15 minimum wage by January 2017. The political origin of the legislation lies in the fulfillment of a campaign promise by Seattle Mayor Ed Murray who publicly endorsed a \$15 minimum wage during his election campaign in 2013 and was buoyed substantially by *Fight for \$15*, a political movement which started in the fast food industry and which advocates for a \$15 minimum wage nationally (Hannah, 2016). Shortly after his election, Mayor Murray formed the city's Income Inequality Advisory Committee (IIAC) which included twenty-four representatives from Seattle's labor, small and large business, non-profits, chambers of commerce and philanthropy communities. The IIAC was responsible for fashioning the details of the law.

Seattle's minimum wage ordinance phased in gradually and was tailored to have differential effects on small businesses and large employers who employ at least 500 employees. Employers paying medical benefits and employers with employees who earn a substantial share of their income from tips were allowed to pay a minimum wage between \$0.50 and \$3 lower than their counterparts. Likewise, recognizing the importance of non-wage compensation, during the first five years after the law's passage, the higher minimum wage could have been achieved by a combination of medical contributions, tips, and salaries.[16]

In April 2015, the minimum wage increased from \$9.47 to \$11 per hour, a 15.8 percent increase. Seven months later, in January 2016, the minimum wage was increased further to \$13, an 18.2 percent increase relative to April 2015 and a 37 percent increase relative to the minimum wage which existed prior to the law's passage. By January 2017, large employers paying no medical benefits had to pay a minimum wage of \$15, a 58 percent increase relative to baseline. While other cities have passed similar minimum wage legislation during this time period (see Figure 1, Panel B), Seattle's local ordinance generated a cumulative minimum wage increase in the city;s minimum wage of nearly 60 percent in less than 21 months, an unprecedented rate of change which, in turn, led to an unprecedented minimum wage level.

Several additional features of the policy environment are worth noting. First, there was a 10-month period between the law's legislative approval and the first minimum wage increase. Consequently, employers and workers had some flexibility to adapt or relocate before the implementation went into effect. Second, upon endorsing the \$15 minimum wage, Mayor Murray also declared he would continue to raise the

[16] For a summary of the local ordinance, see http://murray.seattle.gov/minimumwage/.

minimum wage further provided he had the support of the city council. As such, employers were making decisions under some uncertainty about the future policy landscape. Third, Seattle, which has a population of approximately 750,000, comprises a relatively small share of the 2.2 million people who live in King County, Washington or the 5.9 million people who live in the Seattle–Tacoma–Bellevue metropolitan statistical area. To put this in perspective, while Seattle's population comprises 34 percent of its county population and 13 percent of its metro population, Chicago, another city which has passed prominent minimum wage legislation comprises 52 percent of its county population and 29 percent of the population of its metro area. As such, employers may have had greater opportunity to relocate business operations in response to the legislation (Jardim et al., 2018b). Finally, it is worth pointing out that Seattle's booming tech industry and economy could mean that the impact of the minimum wage may be different than it would be in other cities. While we perform a battery of robustness checks to ensure the internal validity of our differences-in-differences estimates, questions will naturally remain about the applicability of our findings to cities which have crime or labor markets which are substantively different from Seattle.

The novelty of Seattle's local minimum wage ordinance has attracted considerable attention from labor economists who have sought to study its impact on the health and vitality of Seattle's labor market. That literature now includes at least four papers by two sets of authors and finds differential effects. Jardim et al. (2018b) used confidential quarterly payroll records for workers covered by unemployment insurance in the state of Washington and a differences-in-differences strategy (employing interactive fixed effects and the method of synthetic controls for case studies). The authors found that Seattle's first minimum wage increase (which raised the city's minimum wage to $11) increased the hourly wages of low-wage workers by 1.7 percent but reduced hours worked by 2-3 percent. For the city's second minimum wage increase (which raised the city's minimum wage to $13), the authors found a 3 percent increase in hourly wages at the cost of a 6-7 percent decline in hours worked.[17]

This research has been criticized by Zipperer and Schmitt (2017) for excluding roughly 40 percent of the workforce from the analysis and for failing to account for Seattle's booming labor market which has roughly coincided with the timing of the legislation.[18] Other research has replicated the study using city-level aggregate data, finding positive impacts on earnings, while no adverse employment effects in the food-service industry, a sector traditionally used in the minimum wage literature (Reich et al., 2017;

[17]Seattle's minimum wage legislation may have had heterogeneous effects on the labor market based on working experience, according to Jardim et al. (2018a). The authors find that less-experienced workers offset the gain in hourly wages by scaling back the hours worked, leading to small, statistically insignificant net impacts on earnings. In contrast, more experienced workers had positive, significant effects on earnings. This latter group was able to offset working hours reductions by finding work outside the city of Seattle.

[18]An updated version of Jardim et al. (2018b) has addressed some of these concerns.

Allegretto et al., 2018). While explicitly adjudicating this debate is beyond the scope of our paper, we note in Section 5.4.2, that we detect little evidence of aggregate changes in Seattle's low-skilled labor market.

3 Data

3.1 Sources of Data

This research relies on two primary data sources. Crimes reported to the police were retrieved from the Federal Bureau of Investigation's Uniform Crime Reports (UCR), the standard source of data on crimes at the agency level that is employed in aggregate-level crime research.[19] Data were obtained from the specific compilation made available on ICPSR by Kaplan (2019a,b) for UCR crimes Return A and the Supplement to Return A. The Return A file provides the Part 1 offenses (murder, murder, rape, robbery, aggravated assault, burglary, theft, and motor vehicle theft).[20] The Supplement to Return A breakdowns the types of burglaries (i.e., residential and nonresidential).

The second primary data source used in this research was sociodemographic and employment data, retrieved from the American Community Survey. This survey is conducted annually by the U.S. Census Bureau.[21] The sociodemographic variables include the percentage of the black, white, and Hispanic population, and the following age groups 0-14, 15-24, 25-39, 40-54, and over 55 years old. We also included the schooling attainment (less than high school, high school, some college, and college education), and the poverty level. The employment-related variables include the unemployment rate and employment to population ratio for different groups based on age (16-24, 25-44, 45-64 years old), race (white and nonwhite), gender (male and female), schooling level (less than high school, high school, some college, and a college degree), and earnings.

3.2 Analytic Dataset

To construct an analytic dataset, we focus on the period since 2010, which approximately marks the end of the "Great Recession" in the United States. To ensure that our donor pool is comprised of cities that are

[19] Since 1934, the UCR has, either directly or through a designated state reporting agency, collected monthly data on index crimes reported to local law enforcement agencies. The index crimes collected consistently since 1960 are: murder (criminal homicide), forcible rape, robbery, aggravated assault (hereafter "assault"), burglary, larceny and motor vehicle theft. The UCR employs an algorithm known as the "hierarchy rule" to determine how crimes involving multiple criminal acts are counted. In order to avoid double counting, the UCR classifies a given criminal transaction according to the most serious statutory violation that is involved. For example, a murder-robbery is classified as a murder.

[20] Rape was analyzed but not presented in the analysis as the UCR changed its definition in 2013, affecting the pre-treatment period. See https://ucr.fbi.gov/recent-program-updates/new-rape-definition-frequently-asked-questions

[21] Data collected from https://data.census.gov/cedsci/

sufficiently large so as to be comparable to Seattle, we focus on the sample of cities which, in 2010, had at least 50,000 inhabitants. We included cities that reported crimes every month between 2010 and 2017.[22] We exclude from the donor pool cities subject to a minimum wage local ordinance or state law during the 2010-2017 study period. To identify these cities, we used the Inventory of U.S. City and County Minimum Wage Ordinances maintained by the University of California, Berkeley Labor Center, the Federal Reserve Bank of St. Louis Economic Research Data, and the Minimum Wage Tracker developed by the Economic Policy Institute to identify cities and states that approved minimum wage legislation.[23]

We collapse the crime data to the city-by-semester (six month period) level and the employment data to the city-by-year level. The final sample for the Part I offenses includes information from 118 cities in 18 U.S. states, in addition to Seattle.[24] The Supplement to Return A file (i.e., sub-types of crime such as residential burglary), includes 104 out the 118 cities used in the Part 1 offenses. The employment-related data file contains 78 out of the 118 cities. The data sources lead to different sample sizes because some cities do not report the Supplement A information, and the American Community Survey does not provide employment-related data for all cities. We exclude any city with missing data on at least one year from the dataset.

Finally, we perform a supplemental analysis of spatial and temporal variation of crime using microdata from the Seattle Police Department. These data include, for each crime known to law enforcement, the date and time when the offense occurred and its approximate spatial coordinates. We also use data on active business licenses which we obtained from Seattle's Department of Finance and Administrative Services. The data includes the North American Industry Classification System code and the zip code of each active license. We retrieved both datasets from Seattle's Open Data portal.[25]

Table 1 presents descriptive statistics for Seattle and the available donor pool of cities which we use to construct a comparison group. For the 2010-2017 study period, Seattle had higher property crime rates than the average city in the donor pool. For violent crimes, Seattle lies close to the central tendency of the donor pool though its murder rate is notably lower. Overall, Seattle's aggregate crime rate per six-month period is approximately 3,000 per 100,000 inhabitants; consistent with national data, 10 percent of its crime are violent and the remainder are property crimes.

[22] We conducted a manual inspection of each city, excluding those that reported zero crimes on any month, and were a missing observation.

[23] See http://laborcenter.berkeley.edu/minimum-wage-living-wage-resources/inventory-of-us-city-and-county-minimum-wage-ordinances/, https://fred.stlouisfed.org/release/tables?rid=387&eid=243906, and https://www.epi.org/minimum-wage-tracker/#/min_wage/

[24] The donor cities come from the states of Georgia, Iowa, Idaho, Indiana, Kansas, Kentucky, Louisiana, North Carolina, North Dakota, New Hampshire, Oklahoma, Pennsylvania, South Carolina, Tennessee, Texas, Utah, Virginia, and Wisconsin.

[25] https://data.seattle.gov/

Concerning the demographic characteristics of the city's inhabitants, Seattle is roughly 70 percent white, closely approximating the donor pool. However, it has a larger Asian population and a smaller black and Hispanic population than donor pool cities. Seattle's population is also slightly older than that of the donor pool — 30 percent of its population consists of adults between the ages of 25 and 39 which is considerably higher than in other U.S. cities. With respect to socioeconomic indicators, Seattle is considerably more educated than other U.S. cities — 65 percent of Seattle residents have a college degree compared to 37 percent among cities in the donor pool. Likewise, just 14 percent of Seattle residents live in poverty compared to 19 percent among the donor pool. These socioeconomic differences have implications for the structure of Seattle's labor market. In particular, during the pre-intervention period, Seattle's unemployment rate was two percentage points lower than among its donor pool. Perhaps most notable is the share of Seattle residents earning below $30,000 (43 percent) prior to the minimum wage legislation. This is considerably lower than the comparable share (59 percent) among the donor cities.

While Seattle differs in some important ways from the cities which we ultimately use to construct a comparison group, we note that differences-in-differences estimation does not require that treated and comparison units have similar baseline characteristics; only their *trends* must be similar (Imbens and Wooldridge, 2009; Goodman-Bacon, 2018), a feature which we enforce by re-weighting the donor pool cities using synthetic controls estimator. Nevertheless, in Section 5.3 we explore the robustness of our findings to the construction of several alternative comparison groups — including comparison groups that are specifically matched using pre-intervention covariates — and find that our results do not differ substantively across all such comparisons.

4 Empirical Strategy

The primary purpose of this research is to estimate the effect of Seattle's minimum wage law on public safety. While this is a straightforward goal, there are two broad challenges to uncovering the law's causal effect. The first concern relates to identification. In particular, two conditions are needed to generate a credible causal estimand. First, we need to identify a credible counterfactual for Seattle to plausibly identify the change in crime that Seattle would have experienced in the absence of its minimum wage law. Here, our mandate is to identify a comparison region that experiences observably comparable trends, a problem which we address through the use of synthetic control methods. Second, we need to rule out the possibility that there may be other shocks that affect crime and which coincided with the passage of Seattle's minimum wage law. This issue can be addressed only through a more in-depth investigation of

the policy setting, an issue which we address in Section 5.3. These challenges are not unique to case studies and arise in any policy research setting in which the intervention of interest is not randomly assigned. A second challenge that is unique to case studies concerns statistical inference. In particular, while many policy settings can make use of many different geographic units that are treated multiple times, in our setting, we have only a single treated unit, which raises considerable challenges for statistical inference (Abadie et al., 2010).

The dominant — and indeed the sole — approach to estimating the effect of minimum wages in the literature is difference-in-differences, a strategy which compares the change in an outcome (i.e., crime) before and after the roll-out of an intervention (i.e., a minimum wage law) among treated versus untreated units (Card and Krueger, 1994; Dube et al., 2010a; Fernandez et al., 2014; Agan and Makowsky, 2018; Fone et al., 2019). In our context, the main challenge to generating a valid differences-in-differences estimate is to select an appropriate comparison group for Seattle. Further complicating this challenge is that there are a nearly limitless number of ways in which a comparison group for a single city might be selected. Prior research has constructed comparison groups using adjacent administrative units (Card and Krueger, 1994; Dube et al., 2010b), heuristic approaches in which a comparison group is selected based on similar pre-intervention covariates (Card, 1990) or using either an unweighted or population-weighted average of all other areas in a particular jurisdiction — for example, all other cities of a given size in the United States (Fernandez et al., 2014). However, each of these strategies has substantial limitations and raises questions about the quality of the selected counterfactual condition. First, there is no guarantee that comparison groups chosen this way will experience the same crime trends as a treated city and, critically, there is no off-the-shelf mechanism to re-weight observations in the event that parallel trends are observed not to hold. Second, using adjacent administrative units is not suitable for our study as the state of Washington had several minimum wage increases during our period of study (2010 to 2017), which affected cities adjacent to Seattle.[26] Finally, the breadth of researcher discretion to choose an ad hoc comparison group may contribute to researchers' ability to p-hack and thus may be one of the potential drivers of the "replication crisis" which has generated concern across the social and behavioral sciences (Benjamin et al., 2018). As such, there is a considerable utility in automating away researcher discretion whenever possible (Chalfin et al., 2019b).

In order to select a comparison group for Seattle in a transparent, principled and critically, an automated way, we appeal to the method of synthetic controls for case studies, a form of counterfactual

[26]Specifically, between 2010 and 2016, the state minimum wage increased by 10 percent due to yearly cost-of-living adjustments ($8.55 to $9.47). In November 2016, the government approved a statewide minimum wage increase of $11 effective in January 2017.

estimation developed by Abadie et al. in a 2010 article published in the *Journal of the American Statistical Association* which has since been extended in Doudchenko and Imbens (2016), Hahn and Shi (2017) and Chernozhukov et al. (2017) among others. The method, which uses a data-driven algorithm to identify a synthetic comparison group from among a pool of potential comparison units, represents an advance in the estimation of treatment effects for discrete aggregate-level policy interventions.[27] In the context of a city-level intervention in the United States, the methodology works by assigning an analytic weight to each U.S. city that has not implemented a given policy (e.g., a minimum wage law), where the weights are computed such that the difference in a given pre-intervention outcome (e.g., crime) between a treated city (e.g., Seattle) and its pool of potential comparison cities is minimized. In this way, the methodology generates a comparison group which follows parallel trends prior to the intervention. The estimator therefore has the virtue of considerably narrowing the scope for estimates to be contaminated by the effects of mean reversion. **Appendix B** describes the estimator in greater detail.

Synthetic controls represent an advance on designs that select comparison units based on arbitrary or ad hoc criteria and standard differences-in-differences approaches which implicitly use an unweighted or population-weighted average of the remainder of the United States as a comparison group. By using a data-driven method to generate an appropriate control group, the estimated treatment effect is robust to a common misspecification problem. Moreover, the method offers a series of placebo tests that allow for formal inferences to be generated and which help to ensure that the resulting estimate is not the result of an intervention whose timing is insufficiently random. The method has become ubiquitous in recent years and, in crime research, has been used to estimate treatment effects of political corruption (Grier and Maynard, 2016), police management (Kapustin et al.), immigration enforcement (Chalfin and Deza, 2020), drug cartel crackdowns (Calderon et al., 2015), gun control laws (Donohue et al., 2017; Williams Jr, 2017) and place-based crime policies (Saunders et al., 2015; Robbins et al., 2017). Synthetic controls is likewise the mainstay analytic approach among the recent literature in economics that has sought to understand the effect of Seattle's minimum wage law on its low-skilled labor market (Reich et al., 2017; Allegretto et al., 2018; Jardim et al., 2018b), thus making it a particularly convenient choice to study public safety in the same context.

In addition to lagged crime, we match on the pre-intervention average of the following covariates: unemployment rate, percentage of the black, white, and Hispanic population, the proportion of the population between 0-14, 15-24, 25-39, 40-54, and over 55 years old, and based on their schooling attainment (less than

[27] **?** apply the methodology to estimate the effect of the passage of Proposition 99, a California ballot proposition designed to reduce consumption of tobacco. An older reference can be found in Abadie and Gardeazabal (2003) who study the effects of terrorism on economic development in Spain's Basque Country.

high school, high school, some college, and college education), as well as the poverty level. We estimated the pre-treatment root mean square error (RMSE) and mean prediction error (MPE) between Seattle and its synthetic control to measure the model's fit. To allow more flexibility in the model, we estimate the synthetic control method separately by crime type so that the weights are allowed to differ across outcomes. Finally, instead of the traditional standard errors reported in regression analysis, the synthetic control method relies on a placebo test to provide a sampling distribution for the estimated treatment effect. The placebo test works by applying the synthetic control method to each city in Seattle's donor pool and observing the relative position of Seattle's estimate in the distribution of placebo estimates. The implied one-tailed p-value is equal to the rank of the treatment effect divided by the number of donors.

5 Results

5.1 First Stage Results

We begin our discussion of the results by verifying that Seattle's minimum wage law had the effect of increasing earnings for individuals at the bottom of the city's wage distribution. In **Figure 2**, we plot the share of Seattle's population whose annual earnings fall within a given $5,000 interval, limiting our analysis to individuals whose earnings are below $55,000. The figure presents two wage distributions — one for the 2010-2014 pre-legislation period and the other for the 2015-2017 post-legislation period. Consistent with the stated goals of the legislation, after 2015, the wage distribution shifted upwards with the greatest shift occurring in the lowest income brackets. Given that the legislation raised the minimum wage to $15 per hour, which, over 2000 annual work-hours, projects to a $30,000 yearly salary, we would expect to see little evidence for a shift in the share of the working population earning more than $40,000. Consistent with this prediction, there is little evidence for a shift in the share of individuals earning incomes in this range. Overall, the data suggest that the minimum wage legislation was successful in raising wages among the targeted population but not for individuals whose earnings are at least one third higher than the minimum wage.

5.2 Main Results

Next, we consider the effect of Seattle's local minimum wage ordinance on UCR Part I offenses which include murder, robbery, aggravated assault (all of which are classified as violent crimes) and burglary, larceny and motor vehicle theft (each of which is classified as a property crime). We further break out the burglary category into residential and non-residential. In our main specifications, the dependent variable

is the natural logarithm of the crime rate per 100,000 inhabitants. To preserve statistical power to the greatest extent possible, we aggregate the crime rates to six-month bins though; for completeness, we also present estimates using quarterly data as a robustness check.[28,29]

Seattle's minimum wage ordinance took effect in the second quarter of 2015, the period in which Seattle's minimum wage increased by 16 percent from $9.47 to $11 per hour. The city's minimum wage subsequently increased to $13 per hour in the first quarter of 2016 and to $15 per hour in the first quarter of 2017. We begin our investigation of the legislation's effect on public safety in **Figure 3** which plots our synthetic control estimates for each crime type, along with the associated placebo test proposed by (Abadie et al., 2010). Each panel corresponds to a different crime type and, in each sub-figure, the vertical lines drawn at 2015S1, 2016S1 and 2017S1 correspond with the minimum wage schedule in the legislation. Collectively the figures suggest that we achieved a close approximation to Seattle's pre-intervention crime trends by re-weighting units in the donor pool.[30]

Panel A presents results for aggregate property crimes. The figure on the left-hand side shows that Seattle and its synthetic control region followed similar crime trends before the first semester of 2015 when the minimum wage legislation took effect — in both cases, property crime was relatively flat during the 2010-2013 period, then it had a rapid increment between 2013S1 and 2014S2. After 2015, Seattle and its synthetic control group experienced a decline in property crimes to roughly what the rate had been in 2013. However, the crime decline in Seattle after 2015 may have been slightly less steep than in comparison cities. On the right-hand side of Panel A, we plot the estimated treatment effect for Seattle (in black) and the corresponding estimate (in gray) for each donor city that did not experience a change in its minimum wage. The small relative increase in property crimes in Seattle lies in the middle of the placebo distribution, indicating that the point estimate is consistent with sampling variability.

Next, we turn our attention to specific types of property crimes. With respect to burglary (Panel B), while Seattle and its synthetic control group follow similar trends before 2015, this type of crime increased by approximately 20 percent relative to Seattle's synthetic control group in the post-intervention period. The size of this impact is fairly unusual in the distribution of placebo effects and suggests that burglaries may have risen as a result of Seattle's minimum wage legislation. In Panels C and D, we further explore this result by disaggregating burglaries into those occurring on residential properties (Panel C) and those

[28] For homicide, the dependent variable occasionally includes counts of zero. For these observations, we transform the data using the inverse hyperbolic sine function, which approximates to $\log(2y_i)$ (Burbidge et al., 1988).

[29] We also re-run our primary models using the count of crimes in levels. These results are presented in Table 4.

[30] In order to provide a more precise sense for the quality of the match, below each sub-figure we list the mean prediction error (MPE) and root mean square prediction error (RMSPE), computed for the pre-intervention period. In each case, the both MPE and RMSPE are comparatively low.

occurring on non-residential properties (Panel D). The increase in burglary appears to be almost entirely driven by non-residential burglaries, which increased dramatically in Seattle relative to its synthetic control group. In Panels E and F, we study the two remaining Part I property offenses: theft and motor vehicle theft. We see little evidence of a significant change after 2015 on both crimes. Finally, panels G, H, I, and J study violent crimes. To the extent that violence tends to be primarily motivated by impulsivity and low-self control rather than financial incentives (Gottfredson and Hirschi, 1990; Piquero et al., 2005; Chalfin et al., 2019a), violent crimes are generally thought to be less sensitive to market wages and employment conditions (Raphael and Winter-Ebmer, 2001; Gould et al., 2002). Our results are consistent with this hypothesis as there is little sense that either aggregate violent crimes or the constituent crimes of murder, robbery, or aggravated assault changed as a function of Seattle's minimum wage law.

Table 2 presents numerical synthetic control estimates along with the associated p-values in tabular form. In the table, the first column presents the mean difference between Seattle and its synthetic control group for the pre-2015 period. Most of these differences are very close to zero reassuringly indicating that Seattle and its re-weighted donor pool experienced parallel crime trends before 2015. Next, for each post-intervention period (2015S1, 2016S1 and 2017S1), we provide the difference in the crime rate between Seattle and its synthetic counterpart and the implied one-tailed p-value from the placebo test proposed by (Abadie et al., 2010). These p-values accord with Seattle's relative position in each of the placebo plots in Figure 3. In the final column, we aggregate across the entire post-intervention period, during which the minimum rose by 58 percent from \$9.50 per hour to \$15 per hour. Formally, the differences-in-differences estimate for a given post-intervention period can be obtained by subtracting column (1) from each of the subsequent columns. Consistent with the graphical presentation, there is evidence that, after 2015, Seattle experienced a relative increase in burglaries ranging between 21 and 33 percent, an effect which is driven by an especially large increase in commercial burglaries. For all other crime types, the estimates are small and are often inconsistently signed. For example, upon the passage of Seattle's minimum wage law in 2015S1, we observe a 2.5 percent decrease in property crimes followed by a 6.5 percent increase and a 3.5 increase relative to baseline in the following two years. As such, except for a rise in commercial burglaries, there is little evidence of a marked change in public safety following the passage of Seattle's minimum wage law.

5.3 Robustness

In the previous section, we presented evidence that violent crimes and most types of property offending did not change after the passage of Seattle's minimum wage law. At the same time, we noted that there is evidence of a large increase in the number of burglaries, especially non-residential burglaries in Seattle

after 2015. In this section, we assess the sensitivity of the results to choices made during the research process. First, recognizing that Seattle is, in some ways, unusual among U.S. cities, we re-estimate our synthetic control models using a wide array of alternative control groups in which the donor pool is limited to cities which were broadly similar to Seattle at baseline. While similarity in baseline characteristics is not required for differences-in-differences to return an unbiased estimate of the treatment effect, to the extent that counterfactual crime trends during the post-2015 period may be correlated with a city's pre-intervention characteristics, these analyses provide additional assurance that our estimates are not an artifact of unmeasured shocks which are temporally correlated with the minimum wage law. We also test for whether our estimates are sensitive to the functional form of our synthetic control models. Accordingly, we re-estimate models using several newer and more advanced synthetic controls estimators. In all cases, the results closely mirror those reported in Table 2.

Table 3 presents a summary of key robustness checks in tabular form. In column (1), we replicate our primary estimates that are reported in column (5) of Table 2. In column (2), we remove cities in Oregon and Idaho — Washington's border states — from the donor pool as a broad test for robustness to spatial spillovers. Next, we address the concern that Seattle differs in several important dimensions from its synthetic control group, even after weighting on prior crime trends. In **Appendix Figure A.1** we present covariate differences between Seattle and its synthetic comparison group, which remain after the synthetic control weights are applied. While Seattle and its synthetic comparison group are similar in most respects, several exceptions are notable. First, Seattle is wealthier and better educated than other U.S. cities. Second, Seattle has a considerably smaller black population than the cities which comprise synthetic Seattle.

In column (3), we re-construct the synthetic control estimates limiting the donor pool to cities which, like Seattle, are above the median with respect to the share of residents earning more than $30K before 2015. Similarly, in column (4), we limit the donor pool to cities above the median of the average growth rate in the share of workers earning more than $55,000 between 2010 and 2014. In column (5), we limit the donor pool to cities that, like Seattle, are above the median concerning the share of residents with a college degree. In column (6), we limit the donor pool to cities which, like Seattle, have a black population below the median proportion in the donor pool.

In column (7), we consider the potential impacts of the opioid epidemic which largely coincides with the period after the passage of Seattle's minimum wage law and which has brought about a surge in drug-related death rates during this period .[31] To the extent that the opioid epidemic is affecting crime, either by

[31] See https://www.cdc.gov/drugoverdose/epidemic/index.html

increasing the number of motivated offenders or placing an additional burden on law enforcement, it could be possible that changes in the crime rates are a consequence of the opioid crisis rather than the minimum wage ordinance (Doleac and Mukherjee, 2019). To address this concern, we modify our donor pool by selecting cities with over 50,000 inhabitants and which, like Seattle, are located in the 100 counties that in our dataset had the highest opioid death rates in 2010.[32] We choose 2010, the year when drug-related deaths had an inflection point and started to increases substantially.

Given that while Seattle's local ordinance was approved in June 2014, the first minimum wage took effect in April 2015, we assess the presence of anticipatory impacts on the crime outcomes by specifying the treatment period as 2014S1 rather than 2015S1. This analysis is reported in column (8). Referring to Table 3, across all of the specifications, the evidence is consistent with our baseline estimates reported.

We further explore the robustness of our findings by focusing on the functional form of the synthetic control estimator and the measurement of our dependent variable. These estimates are presented in **Table 4**. Our baseline findings which use the original synthetic controls estimator proposed by Abadie et al. (2010) are presented in column (1). In column (2), we re-estimate the model conditioning only on the pre-intervention values of our dependent variable, excluding covariates. In column (3), we use quarterly data instead of semesterly data. In column (4) we re-estimate the model using levels of the crime rate rather than logs. Finally, referring to advances in the synthetic controls methodology proposed by Doudchenko and Imbens (2016), Hahn and Shi (2017), Chernozhukov et al. (2017), and Ben-Michael et al. (2018), we re-estimate our synthetic control models changing the way in which the weights are estimated. These extensions relax the non-negative weights restriction and add an outcome model (i.e., ridge regression) to the objective function to reduce the bias due to an imbalance in pre-treatment outcomes. In columns (5), (6), and (7), we use ridge regression, elastic net regression, and the method of matrix completion as the outcome models. As is apparent from the table, estimates are not sensitive to our decision to condition on covariates or our choice of functional form.

Finally, we subject our estimates to several additional robustness checks designed to address the possibility that Seattle may have been changing in other ways after 2015, which may have had an impact on public safety. First, recognizing that crime tends to fall when cities invest greater resources in police departments (Marvell and Moody, 1996; Chalfin and McCrary, 2018; Mello, 2019; Weisburst, 2019) we investigate the possibility that the police workforce may have changed in Seattle after 2015. Using data from the FBI's LEOKA program, we plot the number of sworn officers per capita for the 2010-2017 period

[32] Choosing the top opioid-related deadly cities rather than counties would be ideal, but the Center for Disease Control and Prevention does not release information at the city level, only at the county level. See https://wonder.cdc.gov/mcd.html

in **Appendix Figure A.2**. As is evident from the figure, there is no evidence for a change in the number of police officers during the study period.

Next, while we have identified a synthetic control region for Seattle, which closely matches Seattle's crime trends prior to 2015, the minimum wage legislation occurred during a period of broad economic growth in Seattle. One source of concern that is particularly noteworthy is that the share of high-earning workers in Seattle — those earning more than $55,000 per year — had been increasing for several years, including after 2015 (see **Appendix Figure A.3**). This secular increase is the result of broader changes in Seattle's economy and, in particular, its technology sector. Naturally, we might be concerned that the effects of broad-based wage growth may have an independent effect on crime, potentially masking the impact of the minimum wage legislation.

While it is not formally possible to un-bundle the effects of Seattle's minimum wage law from secular economic growth, to investigate further investigate the possibility of confounding, we identify ten cities which, like Seattle, experienced a large increase in the proportion of workers earning more than $55,000 during the treatment period. We focus on ten cities rather than a larger number of cities as there are relatively few cities which experience a sizeable secular increase in high-wage workers during this period. We then compare the evolution of the aggregate property crime rate and the burglary rate in Seattle against this smaller subset of cities with the same secular growth in high-wage industries. This analysis is presented in **Figure 4**. We focus on burglaries, the crime for which we observe some evidence of a treatment effect. Referring to Panel C, we see evidence that while burglaries were falling in these ten cities during the study period, Seattle did not experience the same decline in burglaries that we observe in other cities that also experienced growth in the share of high earning individuals. This result is especially true for non-residential burglaries, which increased considerably in Seattle while remaining flat in the comparison cities. While the comparison group is small and does not match Seattle perfectly to its pre-intervention trends, the evidence suggests that Seattle is unique, even among cities experiencing secular wage growth, in its increase in non-residential burglaries. Likewise, there is little evidence that Seattle experienced differential property crime trends compared to other high-wage growth cities that did not pass minimum wage legislation.

5.4 Extensions

Next, we present five further analyses which provide a more detailed understanding of the public safety impacts of Seattle's minimum wage law. First, we consider whether Seattle's minimum wage law impacted offending among specific demographic subgroups within the population. Second, we consider whether the

results we observe can be explained by the effect of Seattle's minimum wage law on rates of employment and unemployment among low-skilled workers. Third, we consider whether there were any regional dynamics in the State of Washington at the time of Seattle's minimum wage law. Fourth, leveraging detailed microdata on crimes known to law enforcement in Seattle, we investigate whether Seattle's minimum wage law led to a temporal or spatial re-allocation of criminal offenses within the city. Finally, we compare our estimate of the effect on crime in Seattle to other cities that, like Seattle, also rapidly raised their minimum wage over a short time window. While none of these cities raised their minimum wage as quickly or to as high a level as Seattle, these analyses provide additional support for our case study's external validity.

5.4.1 Heterogeneity

With the exception of commercial burglaries, we find little evidence that Seattle's crime rate has changed in the aftermath of its landmark 2015 minimum wage law. However, as has been pointed out by (Fone et al., 2019) among others, the effects of the minimum wage may be heterogeneous across demographic subgroups. To test for heterogeneity of treatment effects, we use arrest data from the FBI's UCR program to estimate the impact of the minimum wage local ordinance on arrests among six demographic subgroups defined by the interaction between age (above and below the age of 25), race (white or non-white) and gender (male or female). For each subgroup, estimates are generated by comparing the evolution of arrests in Seattle against Seattle's synthetic comparison group. Estimates, along with an associated p-value, are reported in **Table 5**. While some observers have worried that young men are most likely to experience unemployment as a result of increases in the minimum wage (Neumark and Wascher, 2000), we observe little evidence that this subgroup — or any other subgroup — experienced a differential increase in its arrest rate. There are no clear patterns in the data that would indicate that any specific subgroup of Seattle's population changed its involvement in crime after 2015.

5.4.2 Labor Market Impacts

In Section 5.1, we reported evidence that Seattle's minimum wage law had the intended effect of increasing wages among the city's low wage workers. In this section, we further explore how the law may have impacted the city's low-skilled labor market. In particular, drawing on the economic theory, we note that the effect of minimum wage legislation on public safety depends critically on whether it creates unemployment. That is, to the extent that the law has raised wages without generating a corresponding decline in employment, we would have little reason to expect that the legislation would compromise public safety. On the other hand, to the extent that the law raises wages at the cost of lower levels of employment (Jardim et al.,

2018b), crime might rise to the extent that the public safety effects of the employment shock outweigh the benefits of an increased market wage.

We begin, in **Table 6**, by estimating the effect of Seattle's minimum wage law on unemployment rates by educational attainment, gender, and race.[33] In Panel A, we consider unemployment among individuals with a high school degree or less as well as individuals with some college credits but no college degree or less. For both groups, we see little evidence of a change in unemployment rates. In Panel B, we focus on younger workers, aged 16-24, whose employment is thought to be most sensitive to increases in the minimum wage Neumark et al. (2014). Here again, there is no consistent evidence for an increase in unemployment among these groups.

Of course, unemployment is not the only margin through which the minimum wage could affect low-skilled labor markets. In order to test whether Seattle's minimum wage law affected employment, in **Table 7** we consider the impact of the law on the share of the population that is employed. We begin, in Panel A, with an analysis of Seattle workers with lower education levels. There is little evidence that these workers are less likely to be employed after 2015. Next, in Panel B, we classify young age workers by race and gender. Here, again there is little evidence of lower employment as a result of the law. If anything, there is evidence that employment rates are increasing among young white male workers. Finally, in Panel C, we consider effects by industry. The minimum wage literature often focuses on the restaurant sector as it demands intensively minimum wage workers (Dube et al., 2010b). In the extant literature, an increase of job openings in the construction and manufacturing industries translate into lower recidivism among criminal offenders (Schnepel, 2018). Consequently, we conducted the synthetic control estimates over four industries: accommodations and food services, construction, manufacturing, and retail trade. We show the financial services industry for comparison purposes. The results show no significant changes in the employment to population ratio on accommodation and food services, construction, finance and insurance, and manufacturing. For retail trade, there is, if anything, evidence of sector-wide growth.[34]

In summary, we see very little evidence that any particular low wage labor market experienced adverse employment effects as a result of the minimum wage. If anything, we found that white young adults (16-24 years old), retail trade workers, and low-wage earners are better off after the minimum wage increase in Seattle. These results are consistent with our finding that crime rates did not rise after the introduction

[33] Due to data limitations, we conducted the synthetic control method at the annual rather than the semesterly level. The Current Population Survey is a monthly survey that does not provide information at the city level. The Bureau of Labor Statistics' Local Area Unemployment Statistics provides city-monthly unemployment rates but does not break down on sub-population groups. Therefore, our primary data source was the American Community Survey.

[34] **Appendix Figures A.4** and **A.5** show the graphical representation of Tables 6 and 7. All synthetic unemployment and share of workers' outcomes matched Seattle's pre-treatment period.

of Seattle's minimum wage legislation.

5.4.3 Regional crime patterns

Next, we assessed whether there were any changes in the regional crime patterns at the same time that Seattle's minimum wage ordinance went into effect. Any crime effects in Seattle could potentially have public safety implications for the broader metropolitan area. While the theory predicts that minimum wage laws might bring higher crime to the extent that they create excess unemployment, the policy change could also reduce economic activity in Seattle relative to its neighboring areas. For example, Jardim et al. (2018b) found that experienced workers offset their reduction in hours by finding jobs outside Seattle after the minimum wage ordinance was implemented. To the extent that these patterns are changing worker's commuting time and lifestyles, routine activity theory (Cohen and Felson, 1979) suggests that there could be an increase in crime in outlying areas. In order to test for these effects, we assess whether there were any changes in criminal offenses after 2015 in other parts of King County, Washington, in Seattle's metropolitan statistical area as well as in other outlying areas in the state of Washington.[35] **Figure A.6** presents a schematic of this analysis. In the figure, the black region represents Seattle. The gray section denotes other areas in the state that have a local, county, or state police force; the white areas represent agencies excluded from the analysis due to incomplete reporting to the UCR-data. Despite excluding such agencies, the most populous regions were included in the analysis.

Our analysis of potential changes in regional crime patterns is presented in **Table 8**. We focus on three regions: King County, the Seattle metropolitan area, and Washington State; in each case, we exclude the city of Seattle and limit the post-treatment period to 2016 as, in 2017, there was a statewide minimum wage increase which affected the entirety of Washington. Columns (1) and (2) focus on the remainder of King County. Here, we compare crime in King County, WA exclusive of Seattle to a synthetic control group that is re-weighted to match its crime trends in the pre-2015 period. In column (1), we report the average pre-treatment difference in the log crime rate between King County and its synthetic counterpart. In column (2), we report the estimated change in crime in other areas of King County after 2015 and an associated p-value. The same analysis is applied to the remainder of the Seattle metropolitan area (columns 3 and 4) and the rest of Washington state (columns 5 and 6). Overall, there is little systematic evidence of changes in crime rates in adjacent areas. However, two results are worth noting. First, while it may seem that property and theft crimes increased in Seattle's metropolitan area, once subtracting the pre-treatment mean to the synthetic control estimates, the effects are marginal. Second, concerning

[35] Counties in the Seattle metro area include King County, Snohomish County, and Pierce County.

non-residential burglary, the crime for which we found substantial impacts in Seattle, we observe evidence of an increased crime to outlying areas within King County and possibly to other regions in Washington State. This result could be explained by the fact that offenders sometimes operate across jurisdictional boundaries (Leipnik et al., 2013) and are more likely to commit crimes in the immediate surrounding areas (Hipp and Williams, 2020). However, another explanation is that the non-residential burglary result is an artifact of a regional increase in offending rather than to Seattle's minimum wage law.

5.4.4 Spatial and Temporal Re-Allocation of Crimes

Next, using public microdata from the Seattle police department, we test for whether there has been either a spatial or temporal reallocation of crimes within Settle's borders. **Appendix Figure A.7** presents a descriptive analysis of the temporal allocation of property crimes in Seattle. Each graph presents the share of crimes which transpired during daylight hours. Panel A plots trends in the share of crimes which transpired during daytime hours; Panel B plots trends in the share of crimes which took place during the weekend. Referring to Panel A, there is, on the whole, little evidence for a major reallocation of crimes from daytime to nighttime hours after 2015.

In the remainder of this section, we focus on non-residential burglary, the crime for which we observe aggregate impacts. Referring to Panel A, in 2015, there is a modest increase in the share of burglaries which transpired during daytime hours. However, by 2016, there is a return to trend. There are no appreciable changes in the share of non-residential burglaries committed on weekends (Panel B). In **Appendix Figure A.8** we consider whether non-residential burglaries shifted appreciably to certain communities after 2015. The figure plots the number of crimes by police district in 2017 against the number of crimes by police district in 2010. The correlation across space over time is greater than 0.9 indicating that neighborhoods that reported a high number of non-residential burglaries in 2010 are still reporting high crime levels in 2017. There is little evidence of spatial re-allocation of burglaries as a result of the city's minimum wage law.

Finally, as offenders perpetrating commercial burglaries target establishments rather than people, we assess if changes in the growth of commercial businesses in Seattle could explain the increase in commercial burglaries. **Figure 5**, Panel A, shows that there was an increase in business offices and retail trade establishments in 2015. However, when we measure the number of non-residential burglaries per 10,000 businesses as opposed to per 100,000 residents, the trends during the study period are extraordinarily similar (Panel B). We conclude that the increase in commercial burglaries is unlikely to be an artifact of an increase in the number of businesses during the study period.

5.4.5 Evidence from Other Cities

One common and, of course, valid concern regarding the use of case studies is that the results may not generalize to other jurisdictions. To address this concern, we extend our analysis to four additional cities which, like Seattle, passed sweeping minimum wage legislation during the past decade: Chicago, IL and San Francisco, Sunnyvale, and San Jose in California.[36] While none of these cities implemented a $15 minimum wage, they approved local ordinances that phased-in cumulative minimum wage increases ranging from 26 to 62 percent in less than three years.[37] Excepting Sunnyvale, all other are large cities, and, like Seattle, they are hubs of high-tech or finance industries.

Table 9 shows the effects of the minimum wage on index crimes. For conciseness, the table presents the cumulative effects due to each city's first minimum wage increase until 2017S2 rather than broken down by each minimum wage increase.[38] In section 5.2, we found that burglary rates — and, in particular, non-residential burglary rates — increased in Seattle after the minimum wage went into effect. San Jose, San Francisco, Chicago, and Sunnyvale each experienced an increase in burglaries after passing a minimum wage ordinance. However, only in Sunnyvale, there is a statistically significant change ($p < 0.1$). Nevertheless, as in Seattle, there is evidence for a substantial increase in non-residential burglaries in San Francisco and Sunnyvale; San Jose also experienced a spike in non-residential burglaries, but it is slightly above conventional levels of significance (p-value $= 0.11$).

With respect to overall crime levels, there is little consistent evidence for an impact of large increases in local minimum wage laws on either violent or property offending. While there was an increase in property crimes in San Francisco and an increase in aggregate violent crimes in Chicago, there is no consistent evidence across cities.[39] Our reading of the evidence is that the large and discrete changes in the minimum wage which have recently been implemented in a number of U.S. cities have not led to detectable impacts on public safety.

[36] While Oakland, CA, and Washington DC also experienced large minimum wage increases, we were unable to generate a reasonable synthetic match for Oakland and Washington DC does not disaggregate among the different types of burglary. The four cities included in the analysis have over 100,000 inhabitants and had the most substantial minimum wage increases between 2010 and 2017.

[37] Chicago, IL, went from $8.25 (July-2015) to a $11.0 (July-2017) minimum wage. San Francisco, CA, increased it from $11.05 (May-2015) to $14 (July-2017). Washington, DC, moved from a $8.25 (July-2014) to a $12.5 (July-2017) minimum wage. Sunnyvale, CA, moved from $8.0 in June-2014 to $13.0 in January-2017. San Jose, CA, went from $8.0 in February-2013 to $10.0 in March-2013.

[38] We are not able to disambiguate between residential and non-residential burglaries in Chicago as it did not report this information to the FBI. Sunnyvale implemented a local ordinance in January-2015, but in July-2014, its minimum wage increased 12.5 percent (one dollar) due to a state-wide law. We used 2014S2 rather than 2015S1 as the treatment period.

[39] In Chicago, the timing of its minimum wage law, coincides with the shooting of Laquan McDonald. Hence it is difficult to disambiguate between changes in the labor market and broader shocks to policing and public safety.

6 Conclusion

A large and prominent empirical literature has studied the effect of macroeconomic conditions on crime. Recent, methodologically rigorous research has found a causal link between unemployment rates and property offenses (Raphael and Winter-Ebmer, 2001; Machin and Meghir, 2004; Yang, 2017) but reports little evidence for a causal relationship between unemployment and violent offending (Cook, 2010). Previous literature focusing on minimum or "living wage" laws has found, if anything, that such laws lead to improvements in public safety (Hansen and Machin, 2001, 2002; Fernandez et al., 2014; Agan and Makowsky, 2018). However, none of the previous studies have evaluated the effect of a $15 minimum wage or a discrete change in a jurisdiction's minimum wage of nearly 60 percent over a two-year period.

In 2015, Seattle, Washington dramatically shifted the policy frontier with respect to local regulation of low wage labor markets, becoming the first city in the United States to increase its minimum wage to $15. Over a two-year period, the city's minimum wage increased by nearly 60 percent to more than double the federal minimum wage. While labor economists have reached divergent conclusions about the impact of Seattle's ordinance on its labor market (Jardim et al., 2018b,a; Allegretto et al., 2018; Reich et al., 2017), to the best of our knowledge, there has not been an assessment of the impact of this landmark legislation on public safety.

In this paper, we consider the effect of public policy at the frontier, focusing on the case of Seattle due to two main reasons. First, Seattle's unprecedented minimum wage law may generate different effects than the small increases in the minimum wage which are typical of minimum wage laws passed in most other jurisdictions. Second, in the aftermath of Seattle's decision to push the policy frontier, a number of other cities are soon planning to follow suit. As such, the question of how high minimum wages can increase without jeopardizing public safety is particularly timely.

Using a synthetic differences-in-differences strategy, we compare crime trends in Seattle to other U.S. cities that did not experience a minimum wage increase during the last decade. There is little evidence that Seattle's violent crime trends diverge from its weighted comparison group after the local ordinance. While there is evidence that Seattle's burglary rate may have increased by between 15 and 30 percent, an effect driven by an especially substantial increase in commercial burglaries, we do not detect an aggregate change in property crimes. These results are robust to a variety of robustness checks including removing cities in Washington's border states from the donor pool, limiting the donor pool to jurisdictions with a similar industrial and demographic composition to Seattle, restricting the donor pool to cities that also experienced a growth in high paying jobs and conditioning on Seattle's exposure to the opioid epidemic.

Results are also robust to a comparison of Seattle to other cities which experienced broad-based wage growth during the prior decade.

This research suggests that Seattle was able to increase its minimum wage considerably without compromising public safety, at least in the short-run. The increase in the minimum wage appears to have had little impact on arrest rates among any major demographic including young men, who drive an outsize share of offending. Critically though, this result may hinge on the finding that Seattle's minimum wage legislation did not end up reducing employment for low-skilled workers. While this result is consistent with much of the empirical literature in this area that finds little evidence that minimum wages reduce employment (Dube et al., 2010a), to the extent that other cities follow Seattle's lead but increase their minimum wage to a level that generates unemployment among low-skilled workers, public safety changes could be markedly different. A second point worth noting concerns the inevitable impacts that the COVID-19 pandemic are likely to have on regional economic dynamics. While a $15 minimum wage increase appears to have had little disruption to Seattle's labor market during a period of broad economic growth, it is possible that the city's minimum wage law may become a structural barrier to employment during the coming months. This may, in turn, have implications for public safety.

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Table 1: Descriptive statistics

	Seattle		Donor pool	
	Mean	Std. Dev	Mean	Std. Dev
Crime rate per 100K	3,035.42	235.62	2,179.92	794.38
Violent	295.89	18.68	248.00	151.00
Murder	1.67	0.77	3.15	3.74
Robbery	117.55	8.55	74.75	60.12
Assault	166.79	13.70	149.84	97.85
Property	2,739.53	227.66	1,931.91	684.17
Burglary	542.85	32.33	424.97	224.73
Residential	340.19	29.89	321.02	177.80
Nonresidential	202.67	20.37	95.33	47.96
Theft	1,878.97	182.98	1,373.92	453.77
Motor vehicle theft	317.71	58.50	133.02	100.87
% white	0.70	0.004	0.69	0.16
% black	0.07	0.004	0.18	0.16
% hispanic	0.06	0.005	0.20	0.19
% age 0-14	0.13	0.004	0.20	0.04
% age 15-24	0.13	0.005	0.17	0.06
% age 25-39	0.30	0.01	0.22	0.03
% age 40-54	0.20	0.01	0.19	0.03
% age 55+	0.23	0.004	0.22	0.04
% less than high school	0.07	0.01	0.14	0.07
% high school	0.11	0.01	0.26	0.06
% some college	0.17	0.01	0.23	0.04
% college+	0.65	0.02	0.37	0.11
% in poverty status	0.14	0.01	0.19	0.07
Population (thousands)	636.69	24.82	227.45	307.20
Unemployment rate (%)	0.07	0.02	0.09	0.03
White, 16-24 years old	0.11	0.02	0.14	0.06
Nonwhite 16-24 years old	0.18	0.05	0.23	0.10
High school or less	0.11	0.04	0.11	0.05
Some college or less	0.09	0.03	0.09	0.04
Share of workers (%)	0.75	0.02	0.67	0.06
Earning less than $30K	0.43	0.02	0.59	0.07
Earning more than $30K	0.57	0.02	0.41	0.07
Accommodation and food services	0.05	0.01	0.05	0.01
Construction	0.02	0.002	0.03	0.01

Notes: Unweighted mean and standard deviation from 2010S1 to 2014S2 (pre-treatment period). Crime variables come from city-semesterly data, and sociodemographic, and employment-related outcomes come from city-yearly data. The subtypes of burglary (residential and nonresidential) do not add to the total amount of burglary in the donor pool as they come from a smaller sample size (see text for explanation).

Table 2: Synthetic control estimates of increasing the minimum wage on semesterly crime rates

	Pretreatment	2015S1	2016S1	2017S1	Overall
	(1)	(2)	(3)	(4)	(5)
Property	-0.003	-0.025	0.066	0.033	0.024
		[0.377]	[0.263]	[0.421]	[0.385]
Burglary	0.008	0.226*	0.217	0.336	0.260*
		[0.087]	[0.114]	[0.105]	[0.052]
Residential	-0.006	0.035	-0.026	0.117	0.041
		[0.470]	[0.480]	[0.294]	[0.411]
Nonresidential	0.042	0.449**	0.558*	1.050***	0.686***
		[0.049]	[0.058]	[0.009]	[0.000]
Theft	0.003	-0.023	0.087	0.093	0.052
		[0.473]	[0.184]	[0.307]	[0.289]
Motor vehicle theft	0.018	-0.211	-0.069	-0.149	-0.143
		[0.184]	[0.377]	[0.192]	[0.228]
Violent	0.010	0.044	-0.016	0.002	0.010
		[0.473]	[0.500]	[0.543]	[0.500]
Murder	0.005	-0.180	-0.489	-0.299	-0.323
		[0.333]	[0.157]	[0.228]	[0.149]
Robbery	0.060	-0.021	0.002	0.034	0.005
		[0.438]	[0.491]	[0.456]	[0.508]
Assault	0.005	0.097	-0.082	-0.003	0.003
		[0.359]	[0.263]	[0.412]	[0.570]
Minimum wage ($)	9.5	11	13	15	
Minimum wage increase (%)		15.8	36.8	57.9	

Notes: The synthetic control method includes covariates, as explained in the text. Column (1) is the mean difference between Seattle and its synthetic control before the minimum wage increase (2015S1). Column (2) is the mean difference after the first but before the second minimum wage increase (between 2015S1 and 2015S2). Column (3) is the mean difference between 2016S1 and 2016S2, while Column (4) is the mean difference between 2017S1 and 2017S2. Column (5) is the mean difference between 2015S1 and 2017S2, which is all the post-treatment period. Implied p-values are in brackets. The p-value was estimated as the rank of the treatment effect divided by the number of donors. The dependent variable is the log of the crime rate, hence the coefficients are interpreted as semi-elasticities (i.e., the percentage change in crime rates due to introducing the local wage ordinance). Murder rates use the inverse hyperbolic sine transformation as it has zero city-semesterly crime rates. $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

Table 3: Robustness checks: effects of increasing the minimum wage on semesterly crime rates

	Baseline	Borders	30K	55K	College	Black	Opioid	Approval
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Property	0.024	0.024	0.080	0.090	0.056	0.012	0.037	0.156*
	[0.385]	[0.380]	[0.216]	[0.166]	[0.300]	[0.366]	[0.341]	[0.078]
Burglary	0.260*	0.260*	0.199	0.236	0.177	0.177	0.206	0.284**
	[0.052]	[0.053]	[0.166]	[0.116]	[0.166]	[0.100]	[0.170]	[0.043]
Residential	0.041	0.041	0.058	0.083	-0.030	0.061	0.010	0.147
	[0.411]	[0.405]	[0.358]	[0.226]	[0.452]	[0.358]	[0.500]	[0.215]
Nonresidential	0.686***	0.686***	0.593**	0.725***	0.265*	0.261*	0.534***	0.509**
	[0.000]	[0.000]	[0.018]	[0.000]	[0.075]	[0.094]	[0.000]	[0.019]
Theft	0.052	0.052	0.090	0.086	0.048	0.033	0.043	0.127
	[0.289]	[0.283]	[0.116]	[0.183]	[0.316]	[0.316]	[0.317]	[0.131]
Motor vehicle theft	-0.143	-0.143	0.008	0.012	-0.328	-0.209	-0.304	0.163
	[0.228]	[0.212]	[0.550]	[0.633]	[0.100]	[0.183]	[0.195]	[0.377]
Violent	0.010	0.027	0.062	-0.050	0.087	0.066	-0.080	0.000
	[0.500]	[0.460]	[0.316]	[0.350]	[0.316]	[0.300]	[0.292]	[0.412]
Murder	-0.323	-0.323	-0.011	-0.135	-0.265	-0.262	-0.358	-0.253
	[0.149]	[0.141]	[0.483]	[0.333]	[0.266]	[0.233]	[0.170]	[0.184]
Robbery	0.005	0.000	0.064	-0.025	0.103	0.031	-0.082	-0.008
	[0.508]	[0.477]	[0.300]	[0.516]	[0.300]	[0.433]	[0.390]	[0.447]
Assault	0.003	0.020	0.146	-0.082	0.116	0.088	-0.076	0.159
	[0.570]	[0.539]	[0.283]	[0.366]	[0.400]	[0.283]	[0.365]	[0.271]

Notes: Each column shows the mean difference during the post-treatment period (2015S1 to 2017S2) under alternative difference-in-difference synthetic control estimates. Column (1) shows our preferred estimates (same results as in Table 2, Column (5)). Column (2) excludes cities in border states to the State of Washington. Column (3) only includes cities above the median of the percentage of the population earning more than $30,000 in the donor pool. Column (4) uses donors above the median of the average annual percentage change in the share of workers earning more than $55,000 between 2010 and 2014. Column (5) includes only cities above the median of the percentage of the population with a college degree in the donor pool. Column (6) only contains cities below the median of the percentage of people who self-identify as black in the donor pool. Column (7) only includes cities in counties with an opioid death rate above 7 per 100,000 persons between 2010 and 2014, reducing the donor sample to 40 cities. Column (8) uses the time of the approval of the local ordinance as the treament period (2014S1 rather than 2015S1). Implied p-values are in brackets. The implied p-value was estimated as the rank of the treatment effect divided by the number of donors. The dependent variable is the log of the crime rate, hence the coefficients are interpreted as semi-elasticities (i.e., the percentage change in crime rates due to introducing the local wage ordinance). Murder rates use the inverse hyperbolic sine transformation as it has zero city-semesterly crime rates. *p<0.1; **p<0.05; ***p<0.01.

Table 4: Alternative specifications: effects of increasing the minimum wage on semesterly crime rates

	Baseline	No covariates	Quarterly	Levels	SynthRidge	SynthEN	SynthMC
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Property	0.024	-0.026	0.042	0.026	0.059	0.039	0.039
	[0.385]	[0.485]	[0.333]	[0.307]	[0.280]	[0.342]	[0.342]
Burglary	0.260*	0.260*	0.228*	0.228**	0.337***	0.142	0.142
	[0.052]	[0.086]	[0.096]	[0.035]	[0.008]	[0.166]	[0.166]
Residential	0.041	0.047	0.062	0.074	0.153	0.022	0.022
	[0.411]	[0.446]	[0.340]	[0.225]	[0.176]	[0.392]	[0.392]
Nonresidential	0.686***	0.670***	0.607***	0.608***	0.569***	0.499***	0.499***
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Theft	0.052	-0.068	0.066	0.035	0.109	0.045	0.045
	[0.289]	[0.391]	[0.254]	[0.245]	[0.122]	[0.324]	[0.324]
Motor vehicle theft	-0.143	-0.125	-0.149	-0.184**	-0.133	-0.118	-0.118
	[0.228]	[0.282]	[0.175]	[0.017]	[0.201]	[0.192]	[0.192]
Violent	0.010	-0.018	-0.071	-0.003	-0.048	-0.048	-0.048
	[0.500]	[0.391]	[0.368]	[0.473]	[0.394]	[0.377]	[0.377]
Murder	-0.323	-0.319	-0.216	-0.431	-0.411	-0.300	-0.300
	[0.149]	[0.159]	[0.210]	[0.271]	[0.105]	[0.184]	[0.184]
Robbery	0.005	0.032	0.003	-0.001	0.007	-0.068	-0.067
	[0.508]	[0.485]	[0.447]	[0.535]	[0.508]	[0.298]	[0.307]
Assault	0.003	0.051	0.104	-0.073	0.011	-0.039	-0.039
	[0.570]	[0.434]	[0.315]	[0.263]	[0.438]	[0.385]	[0.385]

Notes: Each column shows the mean difference during the post-treatment period (2015S1 to 2017S2) under alternative difference-in-difference synthetic control estimates. Column (1) shows our preferred estimates (same results as in Table 2, Column (5)). Column (2) uses only lagged values of the dependent variable, excluding covariates, increasing the sample size of the donor pool. Column (3) uses quarterly rather than semesterly crime rates, being 2015Q2 the treatment period. Column (4) estimates the dependent variable in levels, rather than in logarithms; the results are divided over the pre-treatment mean to compare them with the baseline estimates. Column (5), (6), and (7) use the augmented synthetic control with ridge regression, elasticnet regression, matrix completion, respectively. Implied p-values are in brackets. The implied p-value was estimated as the rank of the treatment effect divided by the number of donors. The dependent variable is the log of the crime rate, hence the coefficients are interpreted as semi-elasticities (i.e., the percentage change in crime rates due to introducing the local wage ordinance). Murder rates use the inverse hyperbolic sine transformation as it has zero city-semesterly crime rates. $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$.

Table 5: Effects of increasing the minimum wage on semesterly arrest rates

	<25 years	≥25 years	Female	Male	White	Nonwhite
	(1)	(2)	(3)	(4)	(5)	(6)
Property	0.099	-0.113	-0.194	0.147	0.189	0.079
	[0.247]	[0.474]	[0.288]	[0.175]	[0.175]	[0.319]
Burglary	0.160	0.514**	0.474	0.483*	0.541**	0.364
	[0.350]	[0.041]	[0.103]	[0.082]	[0.030]	[0.144]
Theft	0.074	-0.161	-0.137	0.027	-0.097	-0.073
	[0.340]	[0.371]	[0.329]	[0.309]	[0.463]	[0.494]
Motor vehicle theft	0.167	0.025	-0.223	-0.031	0.134	0.036
	[0.412]	[0.536]	[0.463]	[0.505]	[0.402]	[0.474]
Violent	-0.118	0.059	-0.138	0.021	0.133	0.021
	[0.278]	[0.402]	[0.432]	[0.474]	[0.340]	[0.381]
Murder	-0.192	-0.069	-0.072	0.056	-0.099	0.083
	[0.347]	[0.416]	[0.422]	[0.525]	[0.333]	[0.443]
Robbery	-0.071	0.136	-0.027	0.074	0.091	0.018
	[0.463]	[0.288]	[0.536]	[0.329]	[0.329]	[0.371]
Assault	-0.162	-0.001	-0.074	-0.146	0.204	-0.076
	[0.268]	[0.505]	[0.536]	[0.226]	[0.288]	[0.402]

Notes: Each column shows the mean difference during the post-treatment period (2015S1 to 2017S2) for different population groups. The pre-treatment period goes from 2012S1 to 2014S2, as Seattle did not report complete arrest data before 2012. Columns (1) and (2) use arrest rates on the population under and over 25 years old. Columns (3) and (4) estimate arrest rates for the female and male population. Columns (5) and (6) use arrest rates for white and nonwhite individuals. Implied p-values are in brackets. The implied p-value was estimated as the rank of the treatment effect divided by the number of donors. The dependent variable is the log of the crime rate, hence the coefficients are interpreted as semi-elasticities (i.e., the percentage change in crime rates due to introducing the local wage ordinance). Murder rates use the inverse hyperbolic sine transformation as it has zero city-semesterly crime rates. *p<0.1; **p<0.05; ***p<0.01.

Table 6: Synthetic control estimates of increasing the minimum wage on yearly unemployment rates

	Pretreatment (1)	2015 (2)	2016 (3)	2017 (4)	Overall (5)
Panel A: educational attainment					
High school or less	0.000	-0.008	0.019	0.005	0.005
		[0.391]	[0.256]	[0.459]	[0.337]
Some college or less	0.000	0.004	0.024	-0.004	0.008
		[0.337]	[0.108]	[0.445]	[0.283]
Panel B: 16-24 years old					
White	0.000	-0.026	-0.039	-0.037	-0.034
		[0.405]	[0.202]	[0.310]	[0.202]
White male	0.000	0.014	-0.055	-0.084	-0.041
		[0.297]	[0.216]	[0.135]	[0.229]
Nonwhite	0.000	0.046	0.009	-0.022	0.011
		[0.202]	[0.405]	[0.310]	[0.405]
Nonwhite male	0.000	0.054	0.087	-0.060	0.027
		[0.216]	[0.189]	[0.297]	[0.391]

Notes: The synthetic control method includes covariates, as explained in the text. Column (1) is the mean difference between Seattle and its synthetic control before the minimum wage increase (2010 to 2015). Columns (2), (3), and (4) show the mean difference between Seattle and its synthetic control in 2015, 2016, and 2017, respectively. Column (5) is the mean difference during all the post-treatment period (2015 to 2017). Implied p-values are in brackets. The implied p-value was estimated as the rank of the treatment effect divided by the number of donors. The dependent variable is the unemployment rate in levels (a value of 0.1 means ten percent), so that the coefficients are interpreted as the marginal change in the unemployment rate. $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$.

Table 7: Synthetic control estimates of increasing the minimum wage on yearly share of workers

	Pretreatment (1)	2015 (2)	2016 (3)	2017 (4)	Overall (5)
Panel A: schooling attainment					
High school or less	0.000	-0.060*	0.025	0.022	-0.004
		[0.094]	[0.202]	[0.243]	[0.445]
Some college or less	-0.001	-0.005	0.008	0.017	0.007
		[0.500]	[0.405]	[0.229]	[0.310]
Panel B: age-race					
White 16-24 years old	0.001	0.069*	0.022	0.034	0.041
		[0.081]	[0.405]	[0.189]	[0.162]
White male 16-24	0.000	0.112**	0.100*	0.050	0.088*
		[0.040]	[0.094]	[0.283]	[0.094]
Nonwhite 16-24 years old	0.000	-0.013	-0.036	0.008	-0.013
		[0.432]	[0.540]	[0.351]	[0.594]
Nonwhite male 16-24	0.001	-0.079	-0.053	0.009	-0.041
		[0.270]	[0.378]	[0.418]	[0.391]
Panel C: industry					
Accommodation and food services	0.000	0.008	0.004	0.000	0.004
		[0.135]	[0.270]	[0.405]	[0.189]
Construction	0.000	0.000	-0.001	0.003	0.000
		[0.405]	[0.540]	[0.297]	[0.364]
Finance and insurance	0.000	0.003	0.003	0.000	0.002
		[0.310]	[0.216]	[0.378]	[0.297]
Manufacturing	0.000	-0.004	0.000	0.000	-0.001
		[0.337]	[0.500]	[0.459]	[0.364]
Retail trade	0.000	0.006	0.013	0.021**	0.013**
		[0.162]	[0.148]	[0.013]	[0.027]

Notes: The synthetic control method includes covariates, as explained in the text. Column (1) is the mean difference between Seattle and its synthetic control before the minimum wage increase (2010 to 2015). Columns (2), (3), and (4) are the mean difference between Seattle and its synthetic control in 2015, 2016, and 2017, respectively. Column (5) is the mean difference during all the post-treatment period (2015 to 2017). Implied p-values are in brackets. The implied p-value was estimated as the rank of the treatment effect divided by the number of donors. The dependent variable is the share of workers (a value of 0.1 means ten percent) so that the coefficients are interpreted as the marginal change in the share of workers. *p<0.1; **p<0.05; ***p<0.01.

Table 8: Synthetic control estimates on regional semesterly crime rates

	King County		Metro Area		Washington State	
	(1)	(2)	(3)	(4)	(5)	(6)
Property	-0.007	0.051	0.076	0.114*	-0.015	0.020
		[0.282]		[0.094]		[0.384]
Burglary	0.000	-0.001	0.001	0.001	0.005	-0.005
		[0.418]		[0.581]		[0.376]
Residential	-0.009	0.030	0.000	-0.010	0.005	0.013
		[0.407]		[0.485]		[0.485]
Nonresidential	-0.018	0.375*	0.000	0.177	0.020	0.201
		[0.087]		[0.300]		[0.262]
Theft	0.043	0.096	0.159	0.199**	0.015	0.029
		[0.119]		[0.017]		[0.333]
Motor vehicle theft	0.000	0.071	0.000	0.084	0.009	0.069
		[0.452]		[0.435]		[0.461]
Violent	0.000	-0.008	-0.009	-0.008	0.013	0.000
		[0.444]		[0.444]		[0.478]
Murder	0.009	0.010	0.003	-0.115	0.001	-0.085
		[0.529]		[0.341]		[0.358]
Robbery	0.009	-0.025	0.018	-0.049	-0.010	-0.121
		[0.435]		[0.393]		[0.222]
Assault	-0.004	-0.210	0.015	0.072	0.020	0.002
		[0.128]		[0.384]		[0.564]

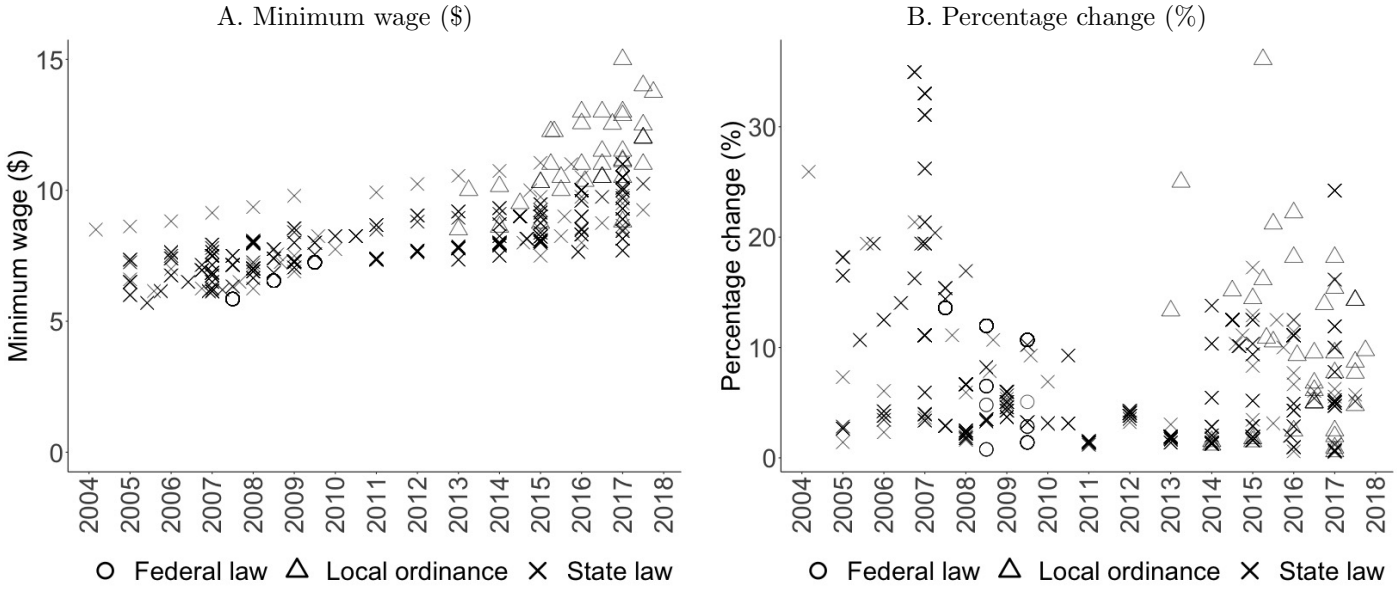
Notes: Each column shows the mean difference during the pre-treatment (2010S1 to 2014S2) and post-treatment period (2015S1 to 2016S2) for different administrative units. The post-treatment is limited to 2016S2 as in 2017 a state-wide minimum wage increase went into effect. Columns (1) and (2) show the pre-treatment mean and post-treatment estimates for King County, respectively. Columns (3) and (4) present the pre-treatment mean and post-treatment estimates for the Metropolitan Area (King, Snohomish, and Pierce counties). Columns (5) and (6) show Washington state-wide pre-treatment mean and post-treatment estimates, respectively. Implied p-values are in brackets. The implied p-value was estimated as the rank of the treatment effect divided by the number of donors. The dependent variable is the log of the crime rate, hence the coefficients are interpreted as semi-elasticities (i.e., the percentage change in crime rates due to introducing the local wage ordinance). Murder rates use the inverse hyperbolic sine transformation as it has zero city-semesterly crime rates. *p<0.1; **p<0.05; ***p<0.01.

Table 9: Synthetic control estimates of increasing the minimum wage on semesterly crime rates in selected U.S. cities

	Seattle		San Jose		San Francisco		Chicago		Sunnyvale	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Property	-0.003	0.024	-0.007	-0.018	-0.005	0.235**	0.001	0.090	-0.041	0.118
		[0.385]		[0.473]		[0.017]		[0.210]		[0.136]
Burglary	0.008	0.260*	0.005	0.139	0.005	0.137	-0.006	0.052	-0.011	0.235*
		[0.052]		[0.236]		[0.219]		[0.394]		[0.094]
Residential	-0.006	0.041	-0.005	-0.057	-0.006	0.080			-0.021	0.157
		[0.411]		[0.450]		[0.284]				[0.223]
Nonresidential	0.042	0.686***	0.004	0.291	0.002	0.437**			-0.005	0.471**
		[0.000]		[0.117]		[0.039]				[0.038]
Theft	0.003	0.052	-0.007	-0.022	0.002	0.313***	0.000	0.117	-0.080	0.113*
		[0.289]		[0.429]		[0.008]		[0.175]		[0.094]
Motor vehicle theft	0.018	-0.143	0.024	0.197	0.010	0.076	0.008	0.127	0.000	-0.146
		[0.228]		[0.307]		[0.482]		[0.377]		[0.222]
Violent	0.010	0.010	0.000	-0.024	0.000	0.065	0.007	0.269**	-0.025	-0.064
		[0.500]		[0.385]		[0.377]		[0.035]		[0.290]
Murder	0.005	-0.323	-0.001	-0.150	0.004	-0.304	0.007	0.201	-0.023	-0.446
		[0.149]		[0.350]		[0.157]		[0.298]		[0.111]
Robbery	0.060	0.005	-0.019	0.002	0.008	0.016	0.010	0.231	0.000	-0.009
		[0.508]		[0.429]		[0.473]		[0.140]		[0.470]
Assault	0.005	0.003	0.015	0.005	0.000	-0.050	0.034	0.278	-0.083	0.129
		[0.570]		[0.561]		[0.315]		[0.114]		[0.273]
Min. wage ($)	9.5	15	8	10	11.05	14	8.25	11	8	13
Min. wage increase (%)		57.9		25		26.7		33.3		62.5

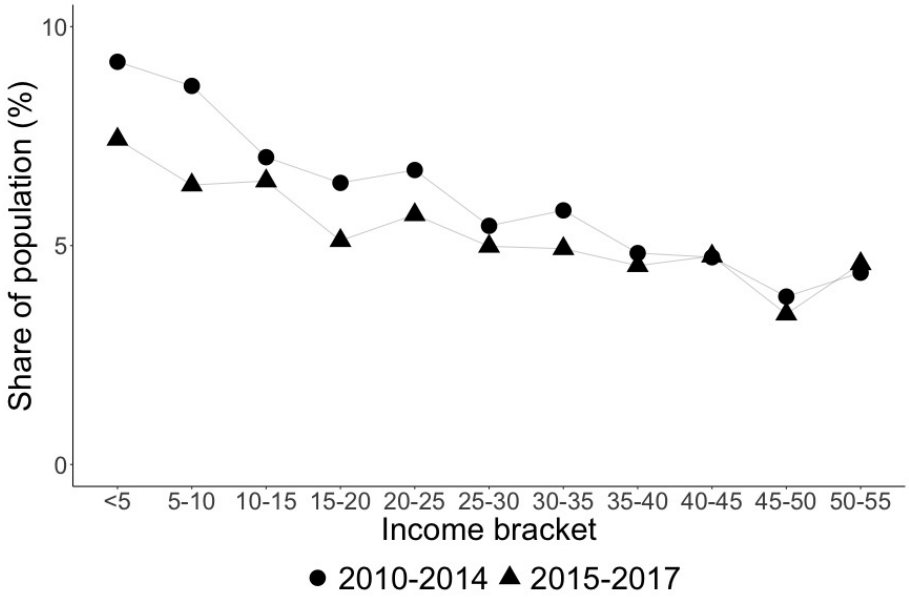
Notes: The synthetic control method includes covariates, as explained in the text. Columns (1) and (2) show the pre-treatment mean and post-treatment estimates for Seattle, WA (same results as in Table 2, Column (5)). Columns (3), (5), (7), and (9) show the pre-treatment mean for each city, that is, the difference between the city and its synthetic control before the first minimum wage increase (2015S1 for Seattle, WA, and San Francisco, CA, 2015S2 for Chicago, IL, 2014S2 for Sunnyvale, CA, and 2013S1 for San Jose, CA). Columns (4), (6), (8), and (10) present the post-treatment estimates, that is, the mean difference between the city and its synthetic control after the first minimum wage increase until 2017S2. Seattle, WA, had a cumulative minimum wage increase of 57.9 percent (changed from $9.5 in March-2014 to $15 in January-2017). San Francisco, CA, had a cumulative minimum wage of 26.7 percent (moved from $11.05 in April-2015 to $14 in July-2017). Chicago, IL had an increase of 33.3 percent (went from $8.25 in June-2015 to $11.0 in July-2017). Sunnyvale, CA, had an increase of 62.5 percent (moved from $8.0 in June-2014 to $13.0 in January-2017). San Jose, CA, had an increment of 25.0 percent (went from $8.0 in February-2013 to $10.0 in March-2013). Implied p-values are in brackets. The implied p-value was estimated as the rank of the treatment effect divided by the number of donors. The dependent variable is the log of the crime rate, hence the coefficients are interpreted as semi-elasticities (i.e., the percentage change in crime rates due to introducing the local wage ordinance). Murder rates use the inverse hyperbolic sine transformation as it has zero city-semesterly crime rates. *p<0.1; **p<0.05; ***p<0.01.

Figure 1: Minimum wage in U.S. cities by enforcing minimum wage legislation, 2004-2017



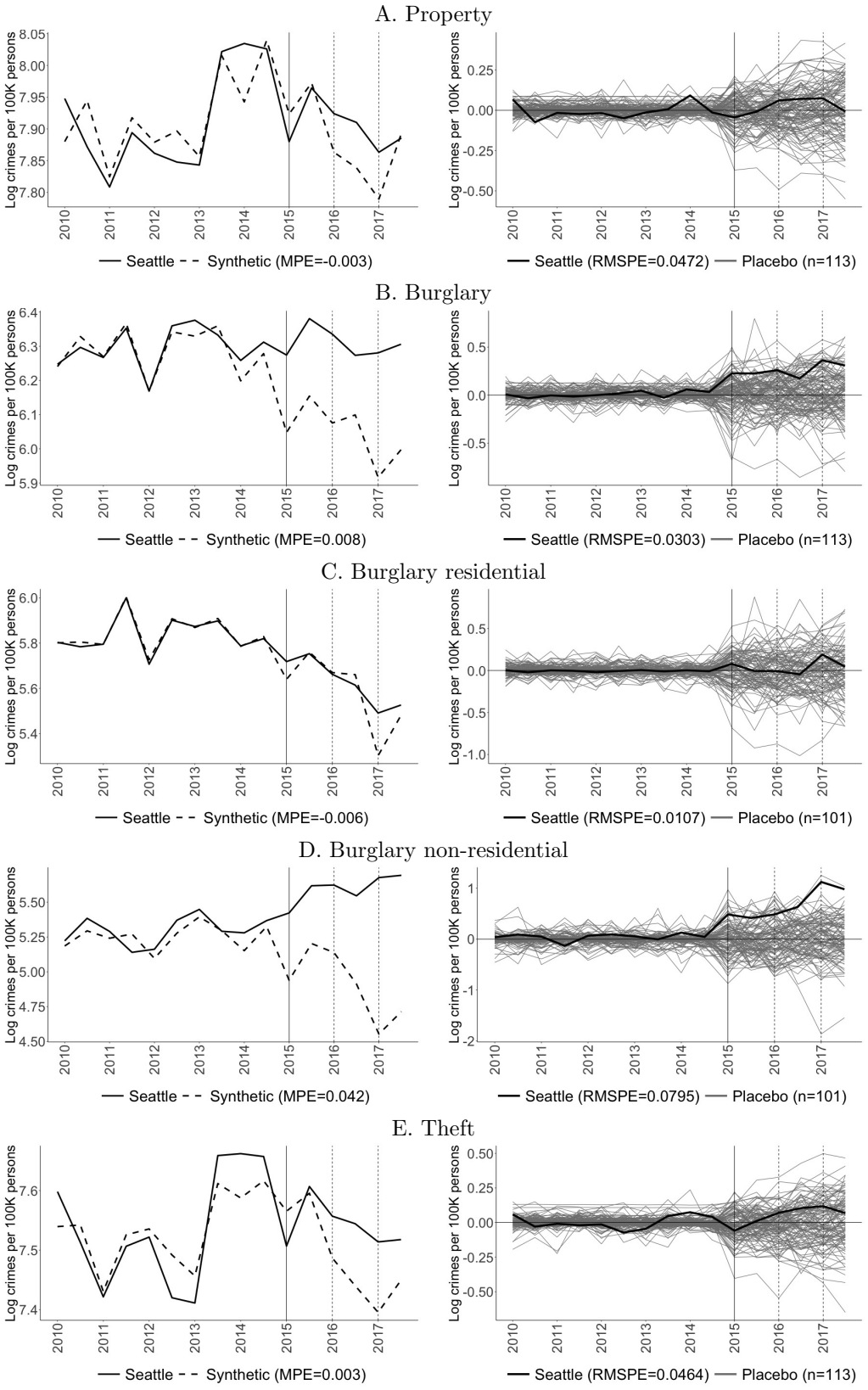
Notes: Each data point is a city over 100,000 persons that experienced a minimum wage increased between 2010 and 2017, including only cities that also reported Part I crimes every month to the FBI-UCR. It differentiates by the law or ordinance that caused the minimum wage increase. Based on Vaghul and Zipperer (2016) and updated to 2017 using online searches.

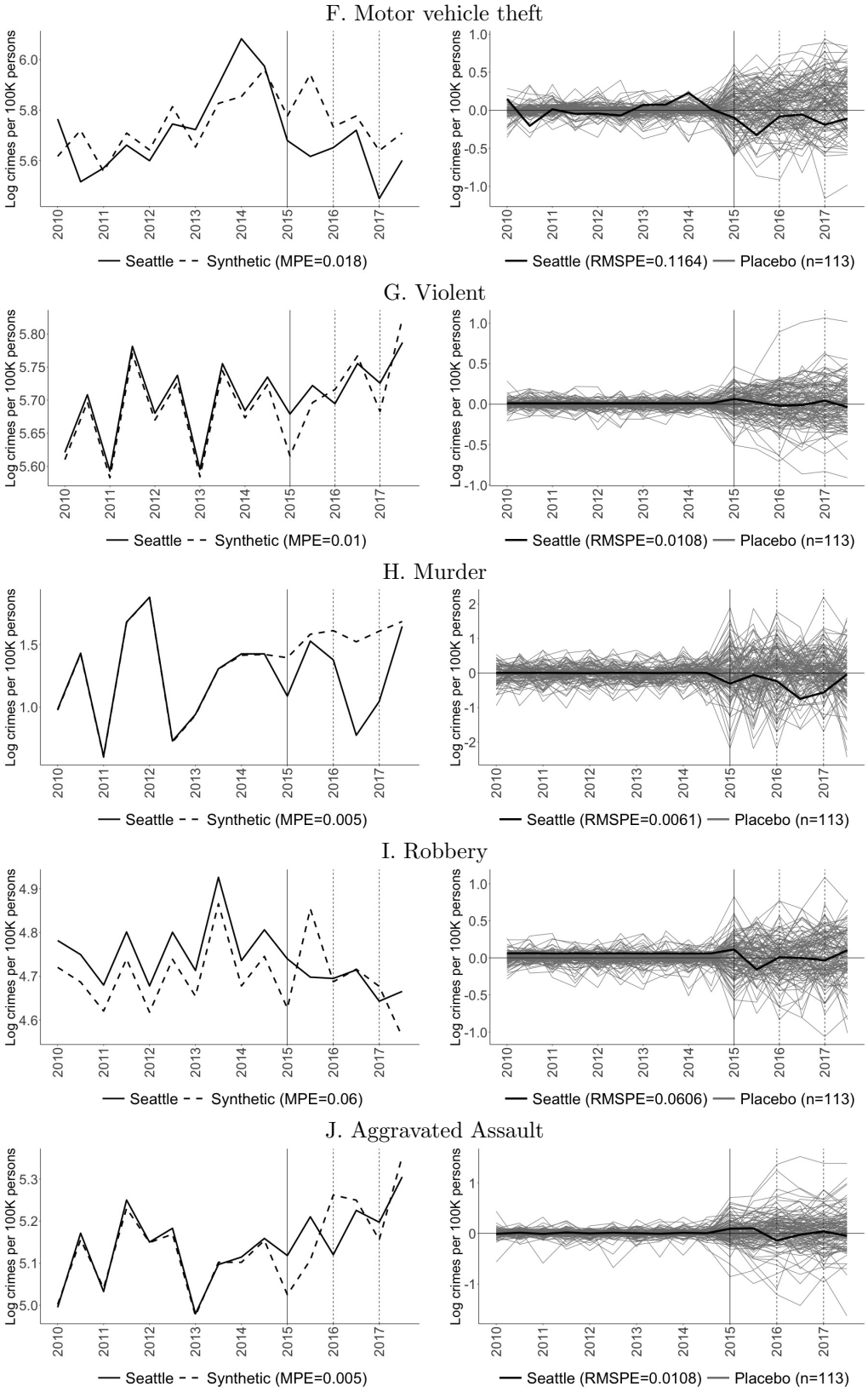
Figure 2: Seattle's income distribution



Notes: Income distribution includes population 15 years and over. The groups show the average share of individuals by income bracket between 2010 and 2014, and 2015 to 2017. Source: U.S Census Bureau's American Community Survey.

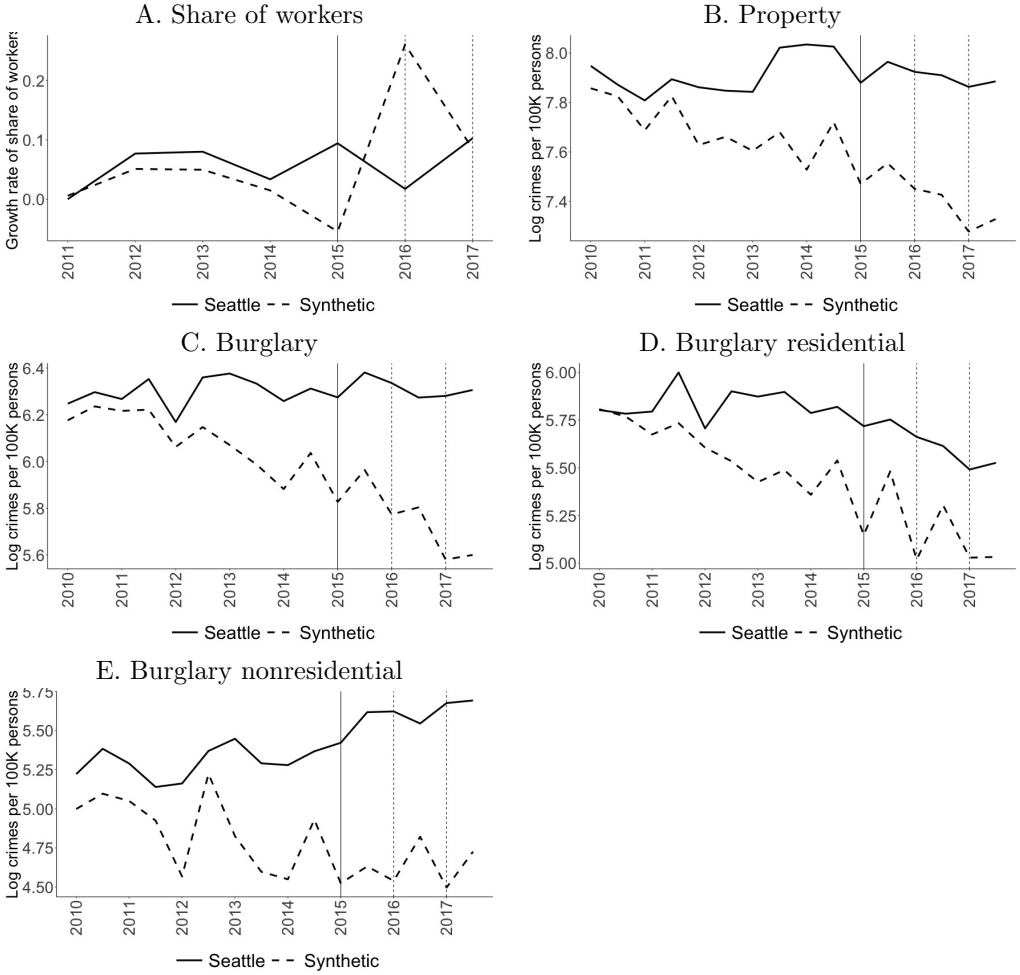
Figure 3: Log Semesterly Crime per 100K Persons. Seattle and Synthetic Seattle





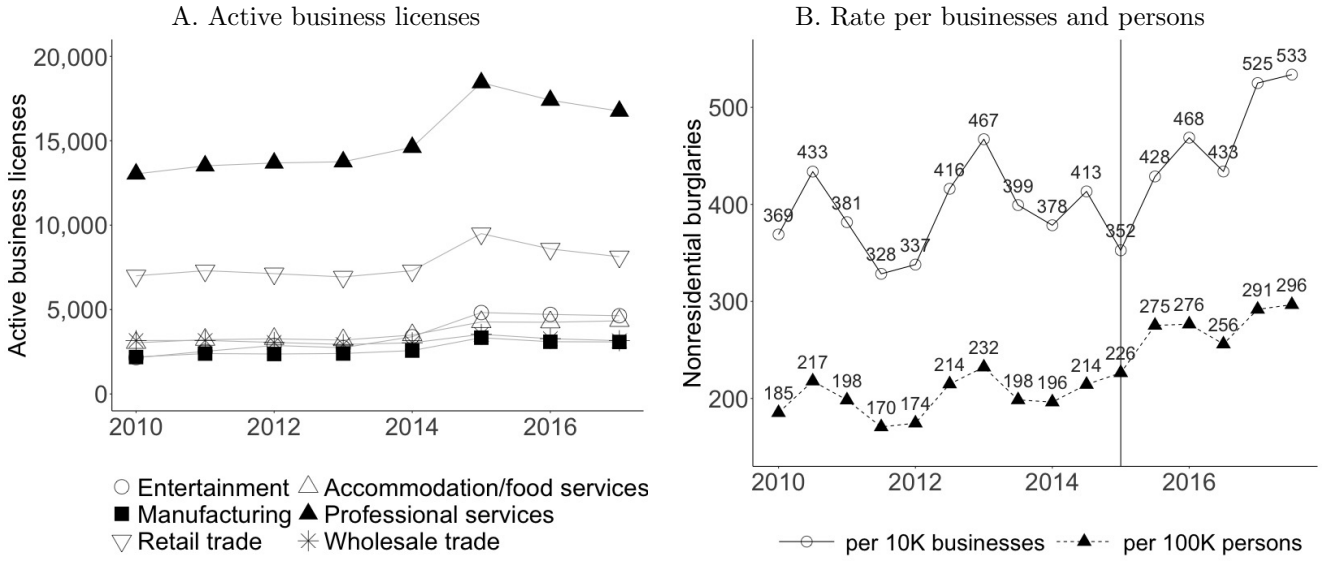
Notes: The synthetic control method includes covariates, as explained in the text. The treatment period starts in 2015S1 (solid vertical line). The second and third minimum wage increases were in 2016S1 and 2017S1 (dashed vertical lines). Pre-treatment Mean Prediction Error (MPE), Root Mean Square Prediction error (RMSPE), and donor pool sample size in parentheses.

Figure 4: Synthetic control estimates using ten donors with the largest average growth rate in the share of workers earning over $55K between 2015 and 2017



Notes: The synthetic control method includes covariates, as explained in the text. The treatment period starts in 2015S1 (solid vertical line). The second and third minimum wage increases were in 2016S1 and 2017S1 (dashed vertical lines). Panel A uses yearly data, and Panels B-D use semesterly data. Donor pool: Asheville, Concord, and High Point cities in North Carolina; Murfreesboro, TN; Odessa, Midland, San Angelo, and Wichita Falls cities in Texas; Salt Lake City, UT; and Lynchburg, VA.

Figure 5: Seattle's active business licenses and nonresidential burglaries

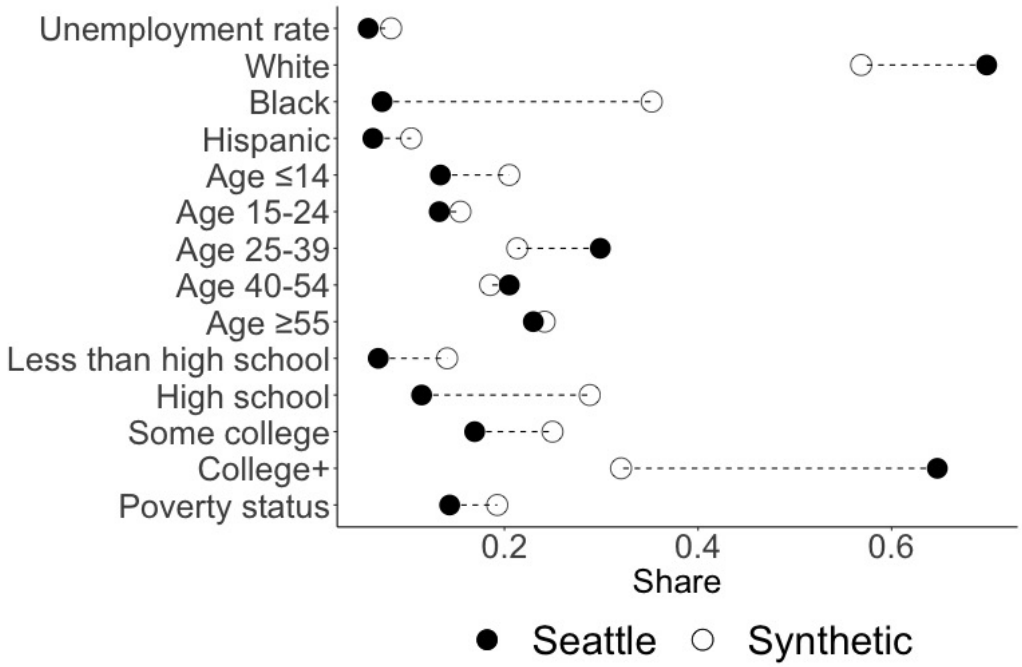


Notes: Panel A shows the number of active business licenses per year and North American Industry Classification System at the two-digit level code: Entertainment refers to sector 71 (Arts, Entertainment, and Recreation), Accommodation and food services is sector 72, Manufacturing includes sectors 31-33, Professional services is sector 54 (Professional, Scientific, and Technical Services), Retail Trade includes sectors 44-45, and Wholesale trade is sector 42. Panel B shows the rate of nonresidential burglaries per 100K persons and 10K active business licenses (sum of the six industries shown in Panel A).

ONLINE APPENDIX

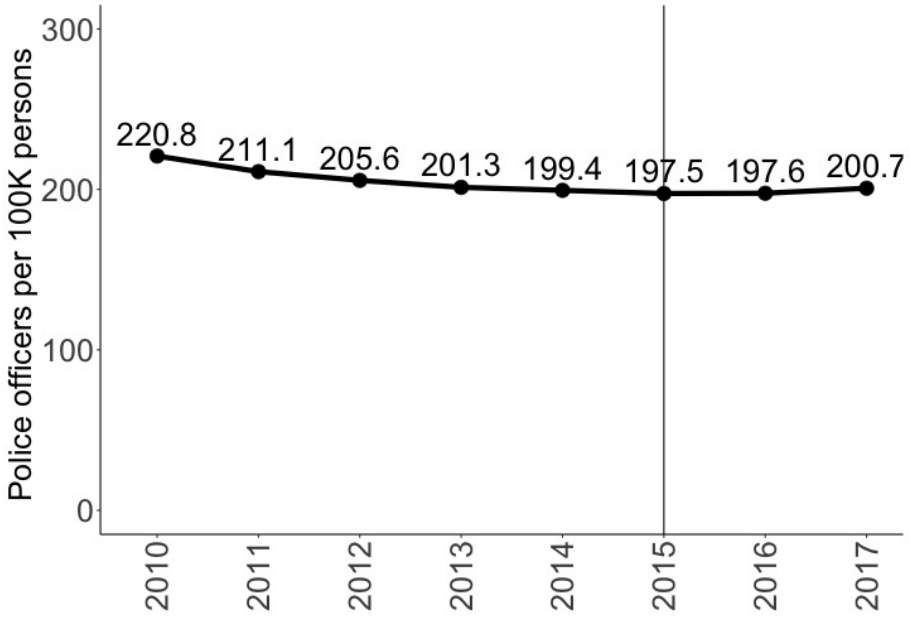
A Appendix: Tables and Figures

Figure A.1: Balancing covariates using burglary synth weights



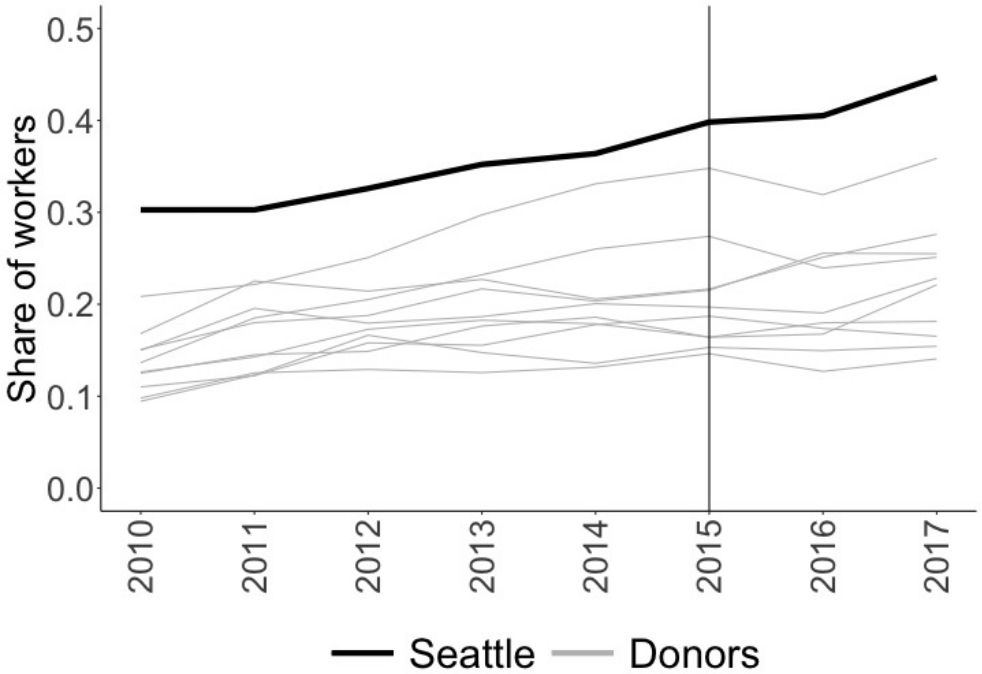
Notes: Balancing on pre-treatment covariates used in the synthetic control method. It uses the weights from the semesterly burglary rate model. A value of 0.1 means ten percent.

Figure A.2: Police officers per 100K persons



Notes: The vertical line marks 2015, year when the minimum wage was implemented.

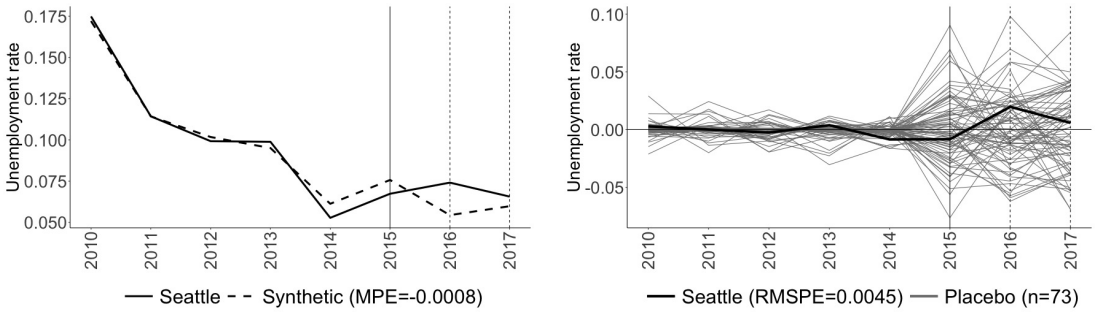
Figure A.3: Share of workers earning over $55,000



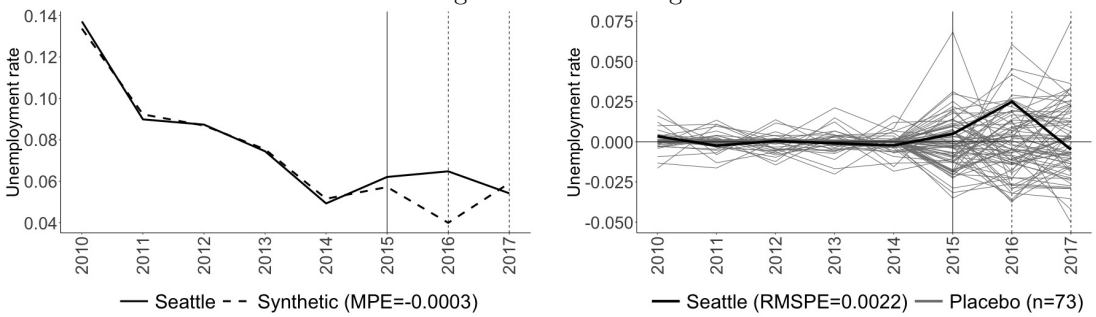
Notes: Raw data used in the synthetic control estimates presented in Figure 4. Donor agencies include Asheville, Concord, and High Point cities in North Carolina; Murfreesboro, TN; Odessa, Midland, San Angelo, and Wichita Falls cities in Texas; Salt Lake City, UT; and Lynchburg, VA. The vertical line marks 2015, the year when the minimum wage was implemented.

Figure A.4: Unemployment rate. Seattle and Synthetic Seattle

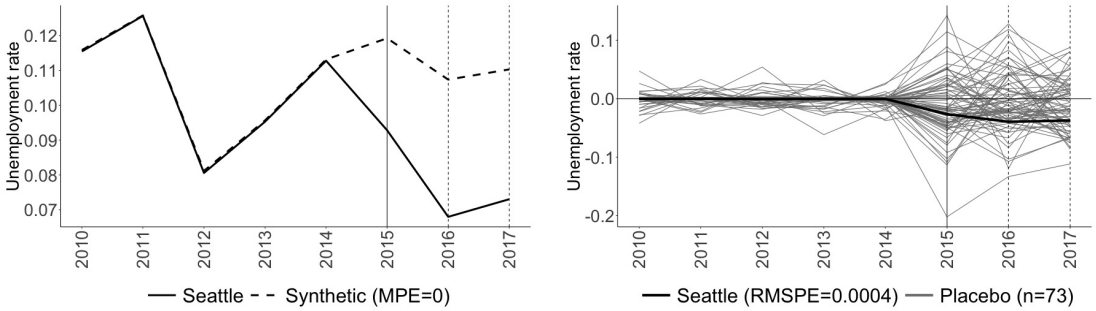
A. Schooling level: High school or less



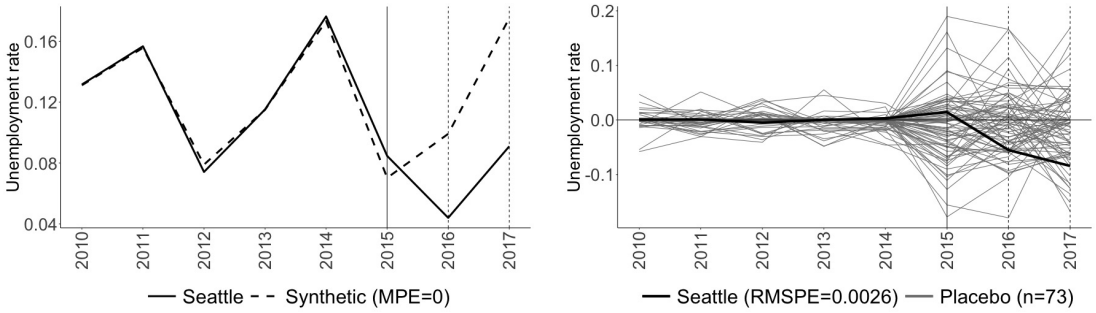
B. Schooling level: Some college or less



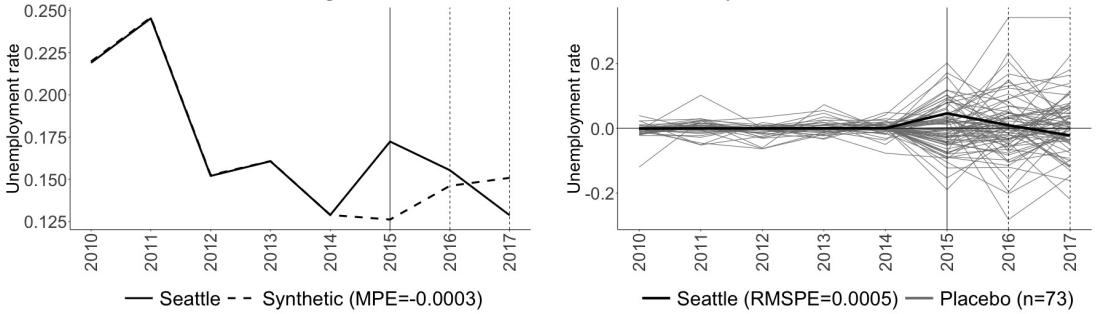
C. Age-Race: White between 16-24 years old



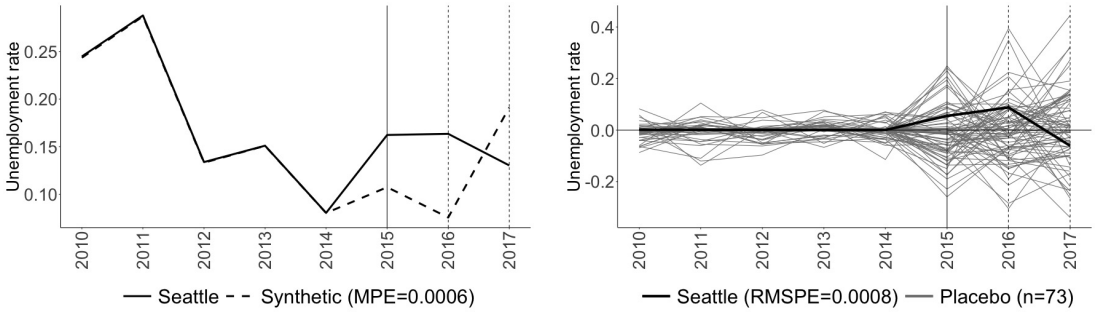
D. Age-Race: White male between 16-24 years old



E. Age-Race: Nonwhite between 16-24 years old



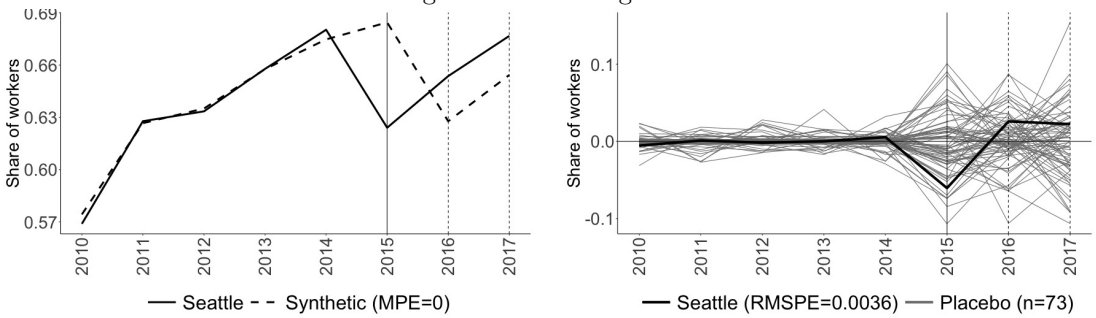
F. Age-Race: Nonwhite male between 16-24 years old



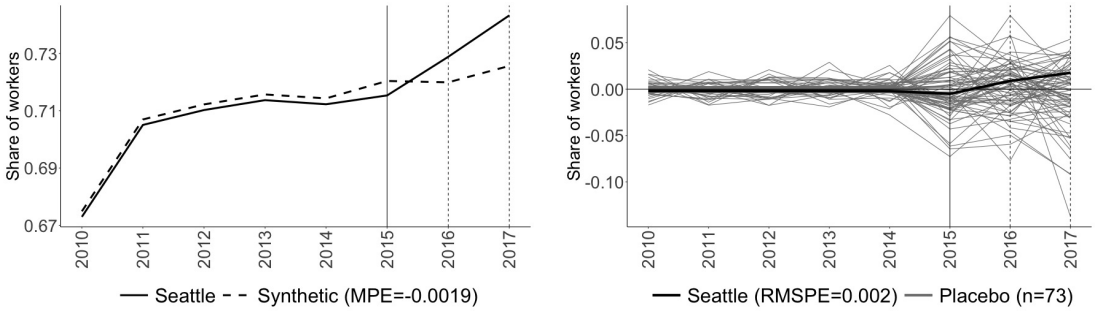
Notes: The synthetic control method includes covariates, as explained in the text. The treatment period starts in 2015 (solid vertical line). The second and third minimum wage increases were in 2016 and 2017 (dashed vertical lines). Pre-treatment Mean Prediction Error (MPE), Root Mean Square Prediction error (RMSPE), and donor pool sample size are in parentheses.

Figure A.5: Share of workers. Seattle and Synthetic Seattle

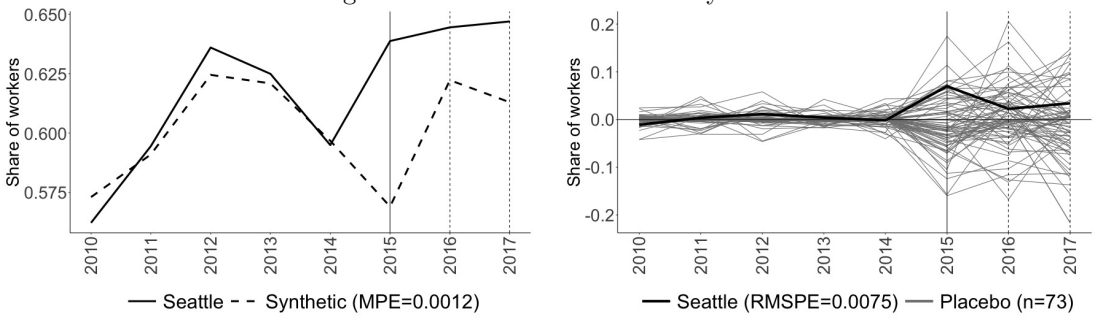
A. Schooling attainment: High school or less



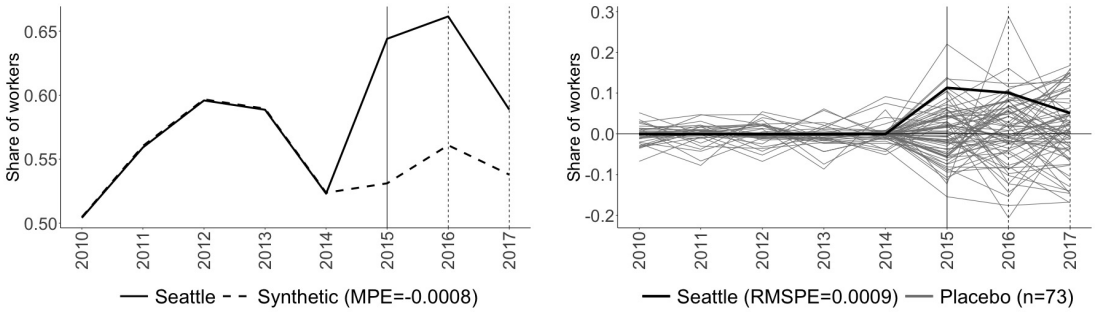
B. Schooling attainment: Some college or less



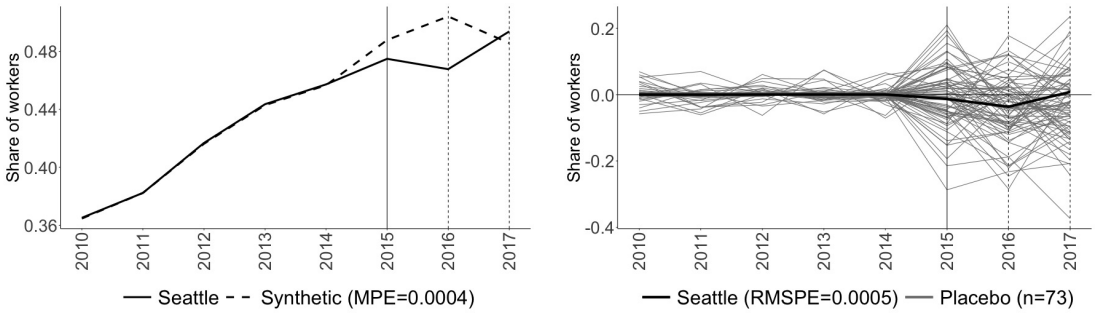
C. Age-Race: White between 16-24 years old



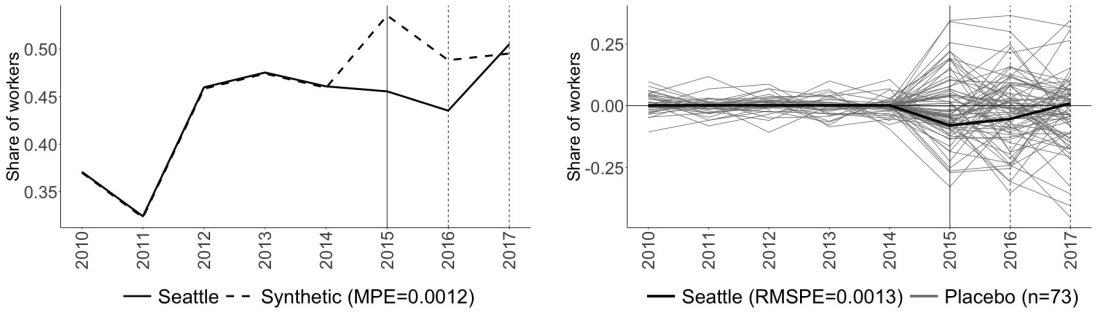
D. Age-Race: White male between 16-24 years old



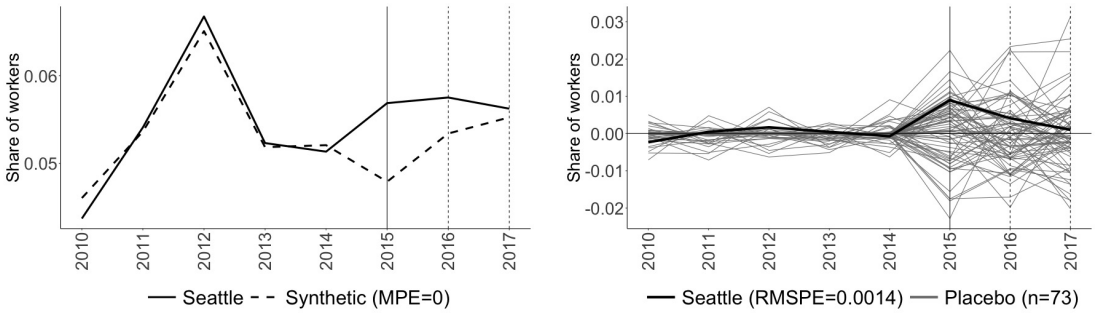
E. Age-Race: Nonwhite between 16-24 years old



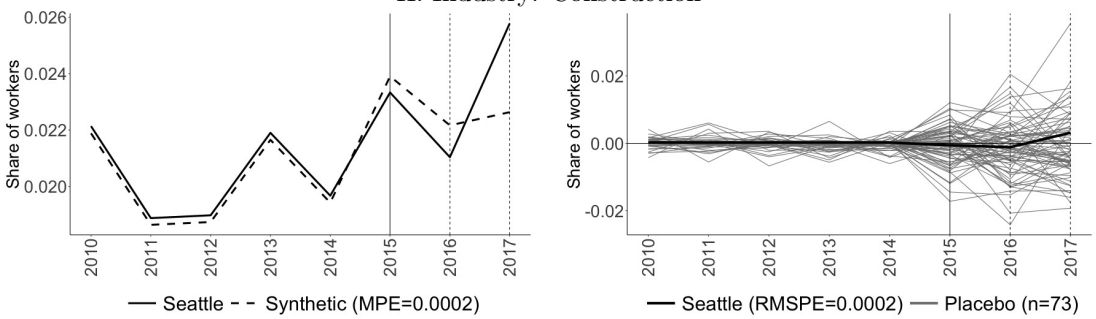
F. Age-Race: Nonwhite male between 16-24 years old



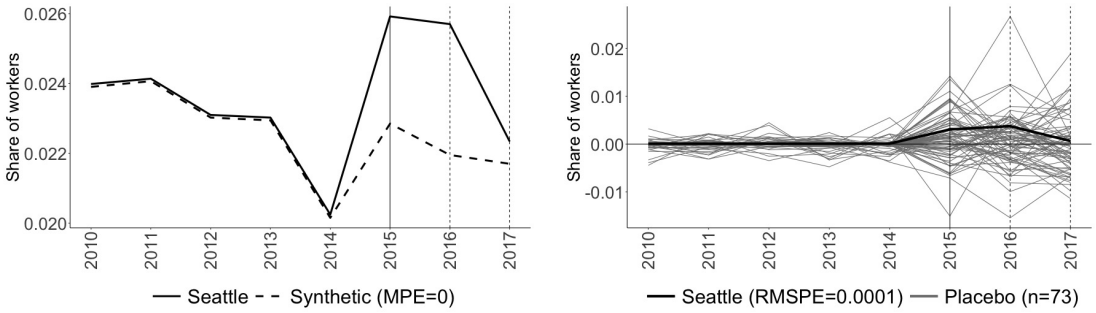
G. Industry: Accommodation and food services



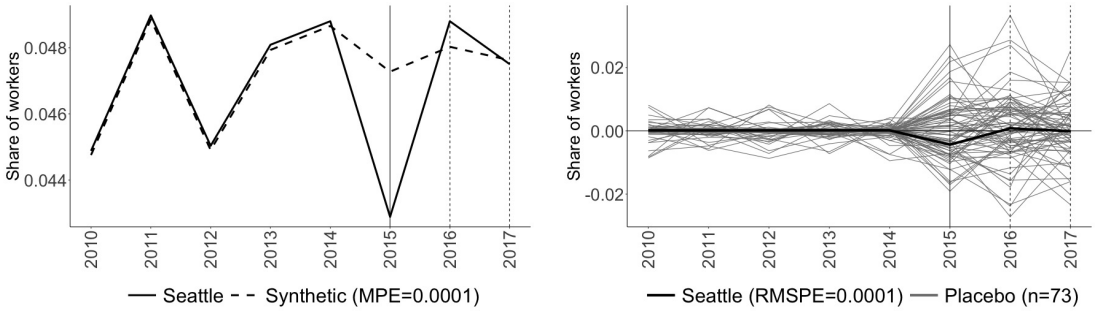
H. Industry: Construction



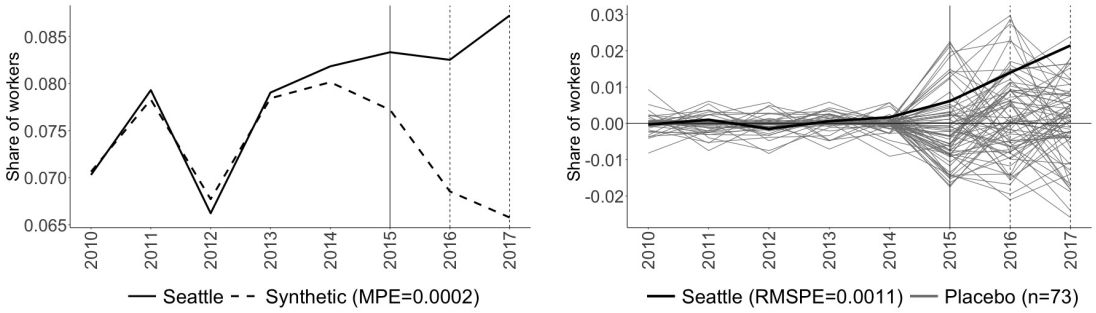
I. Industry: Finance and insurance



J. Industry: Manufacturing



K. Industry: Retail trade

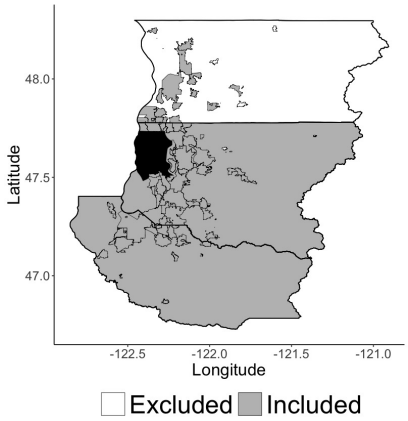


Notes: The synthetic control method includes covariates, as explained in the text. The treatment period starts in 2015 (solid vertical line). The second and third minimum wage increases were in 2016 and 2017 (dashed vertical lines). Pre-treatment Mean Prediction Error (MPE), Root Mean Square Prediction error (RMSPE), and donor pool sample size are in parentheses.

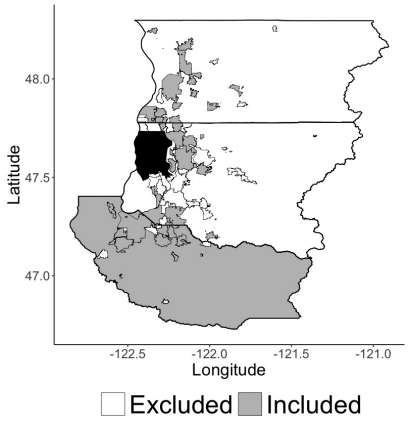
Figure A.6: Agencies included in the regional crime analysis

Adjacent counties

A. Part I crimes

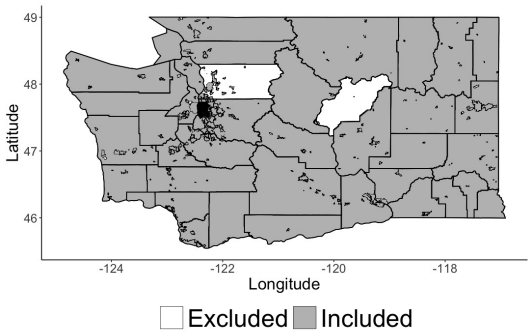


B. Supplement A crimes

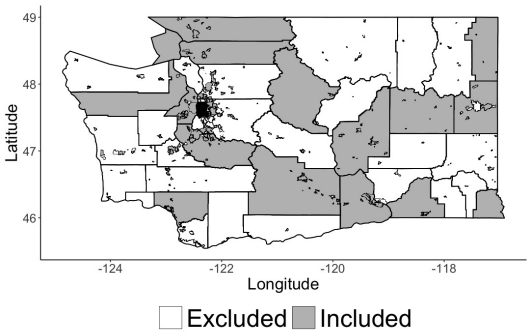


Washington State

A. Part I crimes

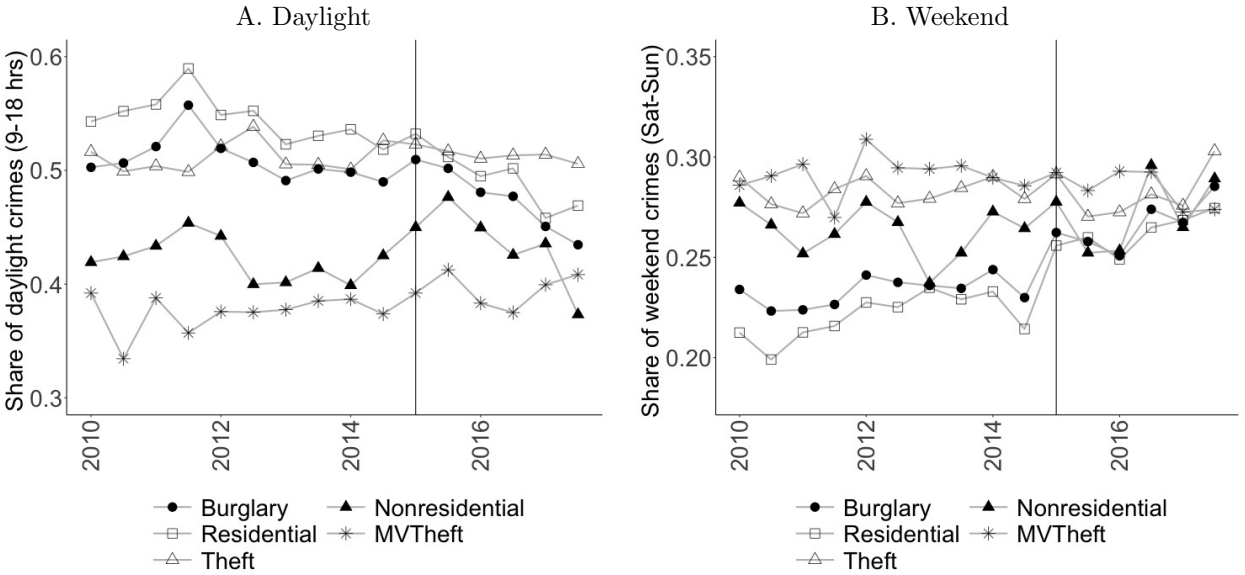


B. Supplement A crimes



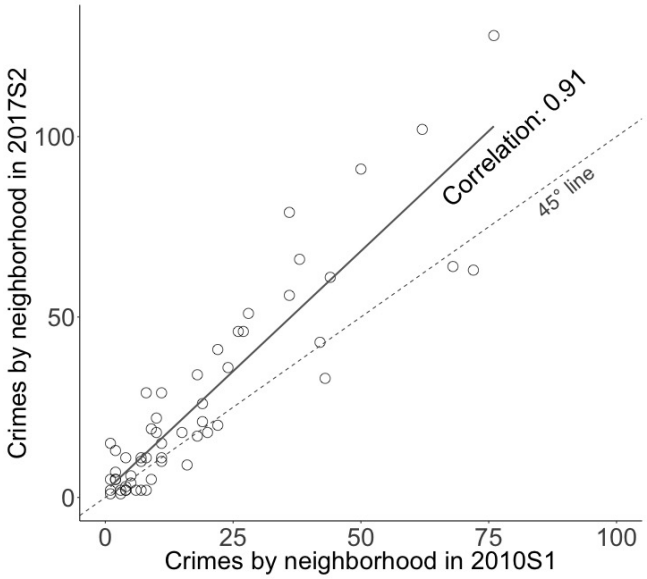
Notes: The boundaries show the U.S. Census Bureau's place and county codes. Seattle is colored in black. The agencies included in the King County Part I and Supplement A crime rates represent 1.3 million and 0.5 million persons (using UCR population values in 2010). The adjacent counties Part I and Supplement A crime rates include 2.7 and 1.8 million people. Meanwhile, Washington State Part I and Supplement A crime rates account for 6.3 and 3.6 million individuals.

Figure A.7: Temporal variation of crime in Seattle



Notes: Panel A shows the share of crimes committed between 9 am and 6 pm. Panel B presents the percentage of crimes committed on Saturday and Sunday.

Figure A.8: Nonresidential burglaries by neighborhood



Notes: Each dot is the sum of nonresidential burglaries reported to the police at the Micro-Community Policing Plans neighborhood (boundaries designed through police-citizen engagement to provide police services and community engagement in Seattle).

B Appendix: Synthetic Controls Methodology

Let the index $j = (1,2,...,J)$ denote the J cities in the United States, meeting our population size and data availability thresholds. The value j=1 corresponds to Seattle, and j=(2,...,J) correspond to each of the other cities that are candidate contributors to the control group (the "donor pool").

We begin by defining Y_0 as a $k \times 1$ vector with elements equal to the seven annual index crime rates and two crime aggregates (violent and property crimes) for Seattle for the 2010-2014 pre-intervention period. Likewise, we define the $k \times J$ matrix Y_1 as a stack of similar vectors for each of the other J cities in the donor pool. The synthetic control method identifies a convex combination of the J cities in the donor pool that best approximates the pre-intervention data vectors for the treated city. Define the $J \times 1$ weighting vector W=(w_1, w_2, ..., w_J)' such that:

$$(A1) \qquad \sum_{i=1}^{J} w_j = 1$$
$$(A2) \qquad w_j \geq 0 \ \text{ for } \ j=(1,...,J)$$

Condition (A1) guarantees that the weights sum to 1 while condition (A2) constrains that the weights are weakly positive. The product Y_1W then gives a weighted average of the pre-intervention vectors for all states in the donor pool (omitting Seattle), with the difference between Seattle and this average given by $Y_0 - Y_1W$. The synthetic control method selects values for the weighting vector, W, that result in a synthetic comparison group that best approximates the pre-intervention crime trend in Seattle. Once the optimal weighting vector $W*$ is computed, both the pre-intervention path as well as the post-intervention values for the dependent variable in "synthetic Seattle" can be tabulated by calculating the corresponding weighted average for each year using the donor cities with positive weights. The post-intervention values for the synthetic control group serve as the counterfactual outcomes for Seattle.

Our principal estimate of the impact of Seattle's minimum wage law on the crime rate uses the pre- and post-treatment values for both Seattle and its synthetic control group to calculate a simple differences-in-differences estimate. Specifically, define $Y_{PRE}^{Seattle}$ as the average value of the violent crime rate for Seattle for the pre-intervention period 2010 through 2014 and $Y_{POST}^{Seattle}$ as the corresponding average for a defined post-treatment period, 2015-2017. Y_{PRE}^{SYNTH} and Y_{POST}^{SYNTH} are the corresponding quantities for Seattle's synthetic control group. Then the synthetic differences-in-differences estimate is given as follows:

$$DD = (Y_{POST}^{Seattle} - Y_{POST}^{SYNTH}) - (Y_{PRE}^{Seattle} - Y_{PRE}^{SYNTH}) \tag{1}$$

To formally test the significance of any observed relative change in Seattle's crime rate, we apply a permutation test suggested by Abadie et al. (2010) and implemented by Bohn et al. (2014) among others to the differences-in-differences estimator. Specifically, for each city in the donor pool that *did not* receive the intervention, we re-compute weights to generate a synthetic control group. Next, we re-compute the synthetic differences-in-differences estimates under the assumption that the other cities, in fact, passed a minimum wage law on the same date as Seattle. Because, the causal effect of the placebo laws is zero by construction, the distribution of these "placebo" difference-in-difference estimates then provides the equivalent of a sampling distribution for the estimate $DD_{Seattle}$.