Local Minimum Wages and Twelfth Grade Enrollment in California

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Abstract

In the second chapter I look at how local minimum wages may affect students' high school enrollment decisions. A new minimum wage may affect enrollment by changing students' perceived payoffs to staying in education and/or working. To assess this, I estimate the effect of sub-state minimum wage policy on twelfth grade enrollment in California using yearly school enrollment data disaggregated by grade. In my preferred specification, I estimate treatment effects using a triple difference-in-differences estimator that accounts for the staggered introduction of local minimum wages. To do so, I exploit variation in local minimum wage policy across grade levels, regions, and time. I find that, except for two cohorts, enrollment typically increased in response to the introduction of a local minimum wage. For the remaining two cohorts, any changes in enrollment after adopting a minimum wage are relatively small and insignificant. Within this specification, I also allow for spillover effects, which are generally small in magnitude and insignificant.

Keywords: City minimum wage, schooling, educational attainment, human capital investment, staggered difference-in-differences, triple DiD

JEL Codes: I21 - Analysis of Education, I26 - Returns to Schooling, J38 - Public Policy

1 Introduction

Currently, California is home to the largest primary and secondary public school system in the US, serving just over 6 million students in the 2020-21 academic year (National Center for Education Statistics 2022). As a result, any policy that affects these students, either directly or indirectly, can potentially affect many people. One such policy is the minimum wage. Although the schooling effects of the minimum wage receive less attention than the employment effects, it is an important issue with a large literature.

A minimum wage may affect students' schooling in several ways, which I discuss in the methodology section. Then main channel is by potentially changing their expected payoffs at various levels of schooling. The direction of this effect is ambiguous and depends on how students perceive their payoffs to change due to the minimum wage.

In 2022 California also has the highest share of students not finishing high school in the US. The NCES reports that in that year approximately 15.3% of the population aged 25 and above do not have a high school diploma. Therefore, any policy, such as the minimum wage, that may incentivize students to drop out of school should be thoroughly analyzed. In addition, the potential secondary effects of such a policy should be taken into consideration when it is proposed.

This paper seeks to expand on the existing literature by examining the introduction of sub-state, or local, minimum wages in California. Local minimum wages have become an increasingly popular policy tool for local governments in the past decade, particularly in California. Prior to 2022 56 sub-state minimum wages have been implemented in the US, with around 32 of these being in California (Vaghul and Zipperer 2016). Despite this, relatively few papers examine the effects of local minimum wages. In this paper I contribute to this growing body of work by examining how local minimum wages have affected twelfth grade enrollment in California between 2010 and 2019.

To date, research on schooling and minimum wages has largely focused on state level implementation. Using sub-state minimum wages have several unique advantages and disadvantages. The primary advantage is that they provide more variation in the minimum wage. In addition, using schools in the same state removes concerns regarding states adopting different policies, although this can be replaced by a similar concern at the city level. Perhaps the largest challenge to using city level data is concerns about migration. I discuss ways in which I control for it in the methodology section.

To estimate the effect of a local minimum wage on enrollment, I use yearly school level enrollment data from 2010 to 2019 from the California Department of Education, disaggregated by grade, and local minimum wage data from both the UC Berkeley Labor Center and Vaghul and Zipperer (ibid.). Because of the staggered adoption of local minimum wages and the likelihood of heterogeneous treatment effects, I estimate the enrollment effects of a local minimum wage using the Callaway and Sant'Anna (2021) Difference-in-Differences (DiD) estimator.

Results suggest that for four of the five treatment-timing cohorts (2013, 2015, 2017, and 2019), a local minimum wage increased twelfth grade school level enrollment. Results from the other cohort (2016) indicate that a local minimum wage decreased enrollment for these schools. This negative effect becomes insignificant when conditioning on pre-treatment city level population. I also use a triple DiD estimator to estimate the treatment effects under likely more plausible assumptions. The results for the triple DiD estimator are similar to conditioning on population with the Callaway and Sant'Anna (CS) DiD estimator, albeit the positive estimates for one cohort (2015) flatten out and become insignificant. Finally, I allow for spillover effects by treatment-timing cohort as well. These results suggest that they are typically small and insignificant with a single exception (2013).

The layout of the paper is as follows. I first present a conceptual framework in which minimum wages may affect students' human capital investment decisions through a variety of channels. Then I discuss the existing literature, focusing particularly on papers with similar methodology. After, I discuss the data that I use for analysis and present summary statistics. Then, in the methodology section I discuss my identification strategy. Finally, I present my findings before concluding the chapter.

2 Conceptual Framework

In this section I first discuss the potential channels through which a minimum wage could potentially affect high school enrollment that have been discussed in the literature. I will present these channels below. An additional point to make here is that I will only focus on the supply side of the market and ignore the effect the minimum wage has on firms. This is because a students' enrollment decision is entirely determined by students' beliefs about the labor market which may or may not reflect actual changes in the labor market.

2.1 Primary Channels

The first two channels that I will discuss are interrelated. First, assume that a student is uncertain about the wage, W, that they would receive at various levels of schooling. Because they do not know the exact wage they would receive, the student forms beliefs about their potential earnings. To capture this uncertainty, define student i's perceived wage distribution for a given level of schooling, S, to be:

$$\tilde{f}_i(W|S).$$

Additionally, define the actual conditional wage distribution for student i as:

$$f_i(W|S).$$

The actual and perceived conditional wage distributions may differ for a variety of reasons. For example, a student may have incorrect beliefs about their ability, the labor market, and/or about discrimination to name a few.[1]

For simplicity, assume there are only two levels of schooling, high and low. As I focus on twelfth grade students in this paper, it is natural to think of the two levels of schooling

[1]This idea that students make decisions based upon their perceptions is not new. For example, see Betts (1996), Dominitz and C. F. Manski (1996) and C. Manski (1993).

as completing high school, where $S = 1$, and not completing high school, where $S = 0$. To further simplify, assume there are only two periods, where in the first period students either stay enrolled in school or work at their no schooling wage and in the second period both types of students work at the wage corresponding to their education level. The perceived payoff to student i with $S = 1$ is thus given by:

$$\tilde{\pi}_i(S = 1) = I_i + \frac{\mathbb{E}[W|S = 1]}{1 + r} - C_i(S = 1), \tag{1}$$

where I_i is student i's family income, W is the wage for the given level of schooling, r is the interest rate, and $C_i(S = 1)$ is the cost of completing schooling. If the student did not complete high school, so that $S = 0$, their perceived payoff would be:

$$\tilde{\pi}_i(S = 0) = \mathbb{E}[W|S = 0] + \frac{\mathbb{E}[W|S = 0]}{1 + r}. \tag{2}$$

In both of these equations expectations for wages are taken over the student's perceived conditional wage distribution, $\tilde{f}_i(W|S)$, because of uncertainty around the wage they will receive until it is realized. In this simple example a student will therefore complete high school if:

$$\tilde{\pi}_i(S = 1) > \tilde{\pi}_i(S = 0),$$

or if

$$\frac{\mathbb{E}[W|S = 1] - \mathbb{E}[W|S = 0]}{1 + r} - (\mathbb{E}[W|S = 0] - I_i) - C_i(S = 1) > 0. \tag{3}$$

Equation 3 illustrates three of the major channels that I will discuss. First, the minimum wage potentially affects high school enrollment by altering students' [expected] future returns to schooling (Cunningham 1981; Leighton and Mincer 1981). This is captured by the first component in Equation 3, the discounted difference between expected wages at both levels of schooling, both of which could potentially be affected by an increase in the minimum wage. It is also worth noting that these expected wages are also not necessarily increasing with the minimum wage; the direction of the effect depends upon how students believe both of their perceived wage distributions will be affected.[2]

The second major channel illustrated in Equation 3 is the opportunity cost of foregoing work to complete high school. This is captured by the second term in Equation 3, the difference between what a student believes they would earn in the initial period if they dropped out of school less the family income they would have received if they stayed in school during the initial period.[3] Similar to the previous channel, both $\mathbb{E}[W|S = 0]$ and I_i could potentially be affected by the introduction of a minimum wage. The expected wage without completing school is perhaps the foremost channel that comes to mind when we think of a minimum wage affecting students' enrollment decisions. Family income, however,

[2]The minimum wage does not need to affect the actual and perceived wage distributions similarly. Students' may have incorrect beliefs about how the minimum wage affects their potential earnings.

[3]I have implicitly assumed that family income is zero for individuals that do not complete high school for simplicity.

is important as well. Depending upon the direction that family income changes due to a minimum wage, it could make it easier or harder for students to stay enrolled in school. There is some evidence that minimum wages increase family income, particularly at the bottom of the income distribution (Dube 2019). This suggests that, *ceteris paribus*, the potential increase in family income would make it easier for lower income students to stay enrolled in school by reducing the opportunity cost.

The final channel that I will discuss in detail is migration (Chaplin et al. 2003).[4] This channel is extremely important in this context because if the minimum wage changes the costs and benefits of migrating, people may move into or out of areas with a minimum wage. If people with children migrate, then we would observe a change in enrollment as well. These changes in enrollment, however, would not be caused by changes in the payoffs to education, which is the focus of this paper. The direction that enrollment is affected through this channel is also ambiguous as it depends upon how migration patterns are affected by the minimum wage and whether the migration is between- or within-states.[5] Both between and within-state migration are problematic, but the latter is particularly worrisome. This is because within-state migration caused by a minimum wage would result in a decrease in enrollment in one area and an increase in another area. If one of these areas has a minimum wage and the other does not, then the effect of the minimum wage on enrollment will appear large in magnitude, but this is misleading because the change in enrollment does not reflect a change in human capital investment decisions by students and would invalidate the identification strategy that I use.

Overall, the effect of the minimum wage on enrollment, and educational outcomes of children and teenagers more generally, is an extremely important issue. It is not clear *a priori* whether the minimum wage will increase or decrease enrollment by changing the payoffs to schooling. Ambiguity in the effect of a minimum wage on enrollment may arise from both of these potential channels discussed above. The total effect of the minimum wage depends upon the direction and magnitude of these two channels in addition to migration. This ambiguity makes this paper, and other research on the topic, which I discuss in the next section below, particularly important because it is not a part of the typical minimum wage discourse among policymakers. Additionally, the research on minimum wages and enrollment typically does not discuss migration as a potentially problematic channel. This is likely not as serious of an issue in those papers as it is here because migration between states due to a minimum wage introduction seems less likely than between cities when there are local minimum wages introduced. In this paper I attempt to eliminate migration as a potential channel so that I can determine whether there are changes to students perceived payoffs caused by the minimum wage. I discuss how I deal with this threat to identification in the methodology section below.

[4] I use migration to refer to both internal and external migration for the state of California.

[5] Lieberman (2021) finds evidence that the introduction of county level minimum wages caused an increase in migration out of these counties, but the effect on enrollment would depend upon the direction of migration for parents.

3 Literature Review

Here I will discuss the growing body of research that aims to estimate how students' schooling decisions change in response to a minimum wage. First, I will provide a general overview of the literature. Then I will discuss several closely related papers in detail. Finally, I will discuss the key differences between this paper and the existing literature.

3.1 General Literature

Early research in this area focused primarily on high school students, although recent papers have started examining higher education as well.[6] For high school students, papers by Cunningham (1981), Ehrenberg and Marcus (1982) and Neumark and Wascher (1995a,b,c, 2003) in the US, and Campolieti et al. (2005) in Canada, estimate the effect of a minimum wage increase on the school/work decisions of teens using multinomial logit models. These papers typically estimate the effect of the minimum wage on the probability of being in four mutually exclusive states: in school and employed, in school and not employed, not in school and employed, and not in school and not employed.[7]

Each of these papers use some variant of the Kaitz index, the ratio of the minimum wage to the average wage, as the minimum wage measure. Aside from two exceptions, these papers find that an increase in the minimum wage reduces schooling for teens. The first exception is Ehrenberg and Marcus (1982), who allow the effect of the minimum wage on education to vary by parental income. They find that the minimum wage causes high income white teens to increase their schooling and low-income white teens to decrease theirs. For nonwhite teens the results are generally insignificant. They propose a potential explanation, differences in the reservation wages by race, but they leave this as an open question for future research. The other exception is Campolieti et al. (2005), who find a that a minimum wage increase had a small positive and statistically insignificant effect on net enrollment for Canadian teens.

Chaplin et al. (2003), Mattila (1982) and Warren and Hamrock (2010) in the US and Landon (1997) in Canada, estimate the effect of the minimum wage on either high school enrollment rates or continuation rates using ordinary least squares or fixed effects estimators. Like others, these papers typically use a continuous measure of the minimum wage; Landon (1997) and Mattila (1982) use a Kaitz index type variable, and Chaplin et al. (2003) use the real minimum wage. Warren and Hamrock (2010), on the other hand, use both continuous and binary measures of the minimum wage, stating that the choice of minimum wage variable did not affect their findings. Landon (1997) finds the minimum wage has a negative effect on the enrollment rate of 16-year-old males and 17-year-old males and females. Chaplin et al. (2003) allow for the minimum wage effect to vary by minimum school leaving age, which varies across states. They find that minimum wage increases have a negative effect

[6] For example, Alessandrini and Milla (2021) and Lee (2020) find students in higher education are also affected by increases in the minimum wage.

[7] The exception to this is Cunningham (1981), who distinguishes between covered and uncovered employment, and full-time and part-time work, resulting in ten states.

on continuation rates, although only when the minimum school leaving age is at or below sixteen. They find no significant effect when the minimum school leaving age is above sixteen. The other two papers find few conclusive results.

These papers have several shortcomings in terms of identification. First, a handful of earlier papers rely solely on variation in the minimum wage either across states *or* across time, thus requiring strong identifying assumptions. Second, other papers use a Two-Way Fixed Effects (TWFE) estimator for identification. The assumptions implicitly buried within these results, often referred to as continuous difference-in-differences estimates, are likely stronger than initially realized (see Callaway, Goodman-Bacon et al. 2021). Third, nearly every paper uses some variation of the Kaitz index as a measure of the strength of the minimum wage. This is problematic because this measure is potentially endogenous as the denominator of the index is the real average wage. This average wage may be correlated with some unobservables that also contribute to students' enrollment decisions, for example the condition of the local labor market.

3.2 Related Literature

Next, I will discuss several key papers in detail. In each of these papers, all but Smith (2021) use similar identification strategies to this project. In this paper, Smith looks at whether an increase in minimum wages in the US causes high schoolers to dropout.[8] Using three data sets, the effect of the minimum wage on dropouts is estimated using two identification strategies. The first strategy uses a two-way fixed effects estimator, similar to much of the existing literature. The second uses a cross-state-border pairing design. Contrary to the earlier studies in this area, Smith consistently finds that minimum wage increases reduce the probability of dropping out for teens from low Socioeconomic Status (SES) backgrounds and has no significant effect on teens from high SES backgrounds.

Card (1992) in the US, Hyslop and Stillman (2007) in New Zealand, and Crawford et al. (2011) and De Coulon et al. (2010) in the UK, also use similar identification strategies to each other. Each of these papers estimate the effect of the minimum wage on schooling using a simple two group, two time period DiD.[9] Card (1992) uses several states and Dallas-Fort Worth, Texas as the comparison group for the 1988 California minimum wage increase, finding that the minimum wage increase reduced enrollment in California. The estimated effect decreases in magnitude and becomes insignificant when fall enrollment is used in place of average enrollment throughout the year. This is potentially due to fall enrollment only capturing changes in enrollment that occur early in the academic year, whereas average enrollment would capture more of the changes that occur later in the academic year. Problematically, the time period that Card examines includes the increase in the mandatory schooling age in California from 16 to 18 years old. As a result, the estimated treatment effect for enrollment reported in this paper likely captures this change in policy as well.

Hyslop and Stillman (2007) examine the 2001 minimum wage reform in New Zealand,

[8]Using dropouts as the dependent variable is similar to using enrollment, with both suffering from similar drawbacks. I will discuss these more below.

[9]I will also abbreviate this as a 2×2 DiD.

which lowered the adult minimum wage eligibility age from twenty to eighteen and raised the youth minimum wage from 60% to 80% of the adult minimum wage. Similar to Card (1992), Hyslop and Stillman (2007) estimate the effect of the minimum wage on enrollment using a DiD estimator, estimating separate treatment effects for 16-17-year-olds and 18-19-year-olds. To do so, Hyslop and Stillman use young adults aged 20-25 as the comparison group. Consistent with much of the existing literature, they find that the minimum wage reduces enrollment.[10] Recently, Alessandrini and Milla (2021) and Lee (2020) have found that students in community college and university are also affected by a minimum wage increase. As a result, using older students as a comparison group is not ideal as the adult minimum wage was also increased at the same time, albeit by a relatively small amount.

Finally, Crawford et al. (2011) and De Coulon et al. (2010) both examine the effect that the 2004 introduction of the 16-17-year-old UK National Minimum Wage (NMW) had on schooling. Both of these papers exploit regional variation, similar to Card (1992), but because the NMW covers the entire UK, there is no regional variation in minimum wage levels to exploit. Instead, the authors rely on variation in average wages by region, using high wage areas as the comparison group for low wage areas. De Coulon et al. (2010) find no evidence that the minimum wage affected schooling decisions. Crawford et al. (2011) find that the introduction of the 16-17-year-old NMW increased the probability of working while in school but had no effect on other outcomes.

Overall, the existing literature is generally in agreement about the effect of the minimum wage on schooling decisions. That is, apart from a few conflicting papers, they appear to generally find a negative effect on high school enrollment. There is some evidence that this effect may vary by schooling level, SES, and the minimum school leaving age. Many of these papers, particularly the earlier ones, rely on strong identifying assumptions that are often vaguely addressed.

Other papers that focus on minimum wages and schooling in the US use variation in minimum wages across states. Instead, I choose to focus on city level minimum wages in California. My reasoning behind this is twofold. First, there are a relatively large number of cities that introduced minimum wage ordinance in a short time period. Second, local minimum wages bite more into a city's wage distribution than the state minimum wage because they are set at a higher level. This is particularly important for large cities with high average wages, where the bite under the state minimum wage would be relatively low. If wages in a city are high enough, then the minimum wage may effectively be nonbinding resulting in a relatively large proportion of the state's population being "treated" even though the minimum wage is unlikely to have a large impact on them.

[10] Pacheco and Cruickshank (2007) criticize the measure of enrollment used by Hyslop and Stillman (2007), which only excludes anyone working over two hours per week. Using a broader measure of enrollment, as well as a continuous measure of the real minimum wage, Pacheco and Cruickshank (2007) find that the minimum wage had a negative effect on the enrollment of 16-19-year-olds. They also find that the introduction of the teen minimum wage, in 1994, appears to have had a positive effect on teenage enrollment.

4 Background

In this section I discuss local minimum wages, focusing particularly on their prevalence in California. I then briefly discuss the educational system and compulsory education in California.

4.1 Local Minimum Wages in California

According to the UC Berkeley Labor Center (2025) approximately 5.6 million workers in California are considered low wage, which they define as earning below $19.69 per hour. Because such a high share of workers are low wage, any minimum wage policy, be it local, state, or federal, has the potential to affect many people's lives. There are also large differences in the share of workers that earn a low wage by race/ethnicity. For example, in 2022 48.7% of Hispanic workers and 37.2% of African American workers were considered low wage by this definition. This is in sharp contrast to the 23.9% of White workers who earn a low wage. Particularly concerning is the share of Hispanic workers that earn a low wage given that they are roughly 40% of the state's population (US Census Bureau 2025).

Local minimum wages have become increasingly prevalent in the United States, and particularly in California (UC Berkeley Labor Center 2021).[11,12] The first local minimum wage in California was introduced in San Francisco in February of 2004, making San Francisco the second city in the US to introduce a local minimum wage. The minimum wage in San Francisco was initially set at $8.50 per hour, $1.75 above the state minimum wage at the time.

In 2013, the adoption of local minimum wages began to increase substantially in both the US and in California. By the end of 2019, there were 52 local minimum wages in place in the US. Of the 52 local minimum wages, 26 were in California. Of these, 20 of the local minimum wages in California were in and around the San Francisco Bay Area. The remaining local minimum wages were in Los Angeles and San Diego County. All cities and places within California without a binding local minimum wage are subject to the state minimum wage. If a local minimum wage is surpassed by the state or federal minimum wage, then the higher minimum wage becomes binding.[13] With the exception of the Los Angeles County all local minimum wages within California are at the city level. The Los Angeles County minimum wage applies solely to unincorporated parts of Los Angeles County.[14]

Local minimum wage ordinances and coverage vary by locality. For example, Los Angeles County, Malibu, and Pasadena have separate minimum wage rates for small and large businesses. The cities of Los Angeles and Santa Monica introduced a third minimum wage rate for hotel workers in addition to separate rates for small and large businesses. Additionally,

[11] I use *local minimum wage* to refer to all sub-state minimum wages and I use *city minimum wage* or *county minimum wage* to refer to minimum wages at a specific level.

[12] Dube and Lindner (2021) provide a general overview of local minimum wages in the US.

[13] The San Diego minimum wage was surpassed by the state minimum wage at the beginning of 2019. Similarly, several other local minimum wages in the Los Angeles area were temporarily surpassed by the state minimum wage from the beginning of 2017 to mid 2017.

[14] Technically, San Francisco is both a city and a county and could be included as well.

the hours of work and age requirements vary by locality as well. Los Angeles, for example, allows for employees aged 14 to 17 to be paid 85 percent of the city minimum wage for their first 160 hours of work. Some cities cover employees for any work done within their boundaries, whereas others require at least two hours of work. Another common aspect of the minimum wage ordinances in California is the practice of indexing the local minimum wages to the CPI so that increases in price levels do not erode the local minimum wage.

4.2 The California Educational System

California is the most populous state with around 39 million people in 2024 (Public Policy Institute of California 2025). As mentioned above, it is also home to the largest public school system in the US, having enrolled approximately 6 million students in the 2021-22 school year. Similar to the state of California, a large share of the students in California public schools are Hispanic. For the 2023-24 school year approximately 56.1% of students were Hispanic, 4.9% African American, and 20.3% White (California Department of Education 2024).

The public school system in California is comprised of three main types of schools: traditional, charter, and magnet. Traditional schools are operated by school districts, a government entity, and draw students from within their district boundaries (California Department of Education 2025a). School districts provide education for primary and secondary school students and can be classified as either an elementary school district if they only serve primary school students, high school district if they serve secondary school students, and unified district if they serve both. The second type of school, charter schools, operate via a school charter and are not subject to all of the state laws that school districts are (California Department of Education 2025b). The final school type is magnet schools. These schools typically focus on a specialized subject and draw students from across district boundaries (California Department of Education 2025c). Because of this a student may not necessarily live where they attend school and so I exclude them from my analysis as it would be difficult to identify which minimum wage would be applicable.

4.3 Compulsory Education in California

In January of 1988 the California minimum school leaving age was raised from 16 to the current minimum of 18 years old. There are several exceptions, for example students aged 16 to 17 may leave school with parental consent upon passing the California High School Proficiency Exam. After a student turns 18, they may drop out at any time. Because of this, I focus on twelfth grade enrollment, as a typical twelfth grade student is 17 or 18 years old. Students in other grades, particularly eleventh, may also be affected by a minimum wage because they can drop out with parental consent or possibly due to noncompliance with compulsory schooling laws.

5 Data

In this section I describe the data sets used for my analysis, including how they were cleaned, in addition to presenting relevant summary statistics.

5.1 Data Sources

For the primary analysis, I combine three data sets from two sources. The first data set, from the UC Berkeley Labor Center, contains publicly available local minimum wage data for the US. Because the focus of this project is local minimum wages in California, I remove all cities and counties in other states. Further, I keep San Francisco, which, after initially introducing their local minimum wage in 2004, subsequently increased it in 2015. This is due to the length of time between the introduction of the minimum wage in San Francisco and when it was increased. To verify the accuracy of the local minimum wage data, I cross-check the local minimum wage rates in the UC Berkeley data set with both local authority websites and the data set compiled by Vaghul and Zipperer (2016). When discrepancies arose, I relied upon information from the local authority's website.

In addition to the minimum wage data, I also use two data sets from the California Department of Education (CDE). The first data set from the CDE contains school-level annual primary enrollment data for public schools in California from 2010 to 2019. This data is collected by the CDE on the first Wednesday in October of each year (Census Day) and contains enrollment numbers by school, grade, race, and gender. I supplement the enrollment data with a second data set from the CDE containing voluntarily self-reported data from schools about their grades offered, school type, and location.

To construct the data set for my analysis below, I first merge the enrollment data with the school characteristics data using the County-District-School (CDS) codes, a unique identifier for schools within California created by amalgamating county, school district, and school codes. Approximately 94.91% of school-year observations in the enrollment data set can be matched to their corresponding school characteristics. Any unmatched observations were dropped as there is no information on the city or level of schooling offered, making it impossible to construct a treatment variable for these schools. Several school CDS codes in the data did not match with the information on the CDE website, primarily due to inconsistencies with district codes. These were manually recoded to match the CDE website prior to merging. Less frequently, some schools' CDS codes did not match with the CDE website due to differences in county codes. These schools were removed from the data.

I remove schools that opened within four years of a Census Day that had no students enrolled in grade twelve and each of the grades between grade twelve and the grade of what would be the first graduating cohort. The reasoning behind this is because schools may open one grade at a time, so that the first cohort of freshmen will be the first graduating class. These schools should not have enrollment coded as zero, but rather it should be missing. Observations with Census Day either before the school opening date or after the school closing date are also removed.

As the focus of this paper is twelfth grade enrollment, schools teaching solely at the elementary (K-5) or middle school (6-8) grades are dropped. Other nontraditional schools, such as those providing inmate education, special education, or adult education are removed along with continuation, virtual-only, and magnet schools. Charter schools are *not* removed.

Finally, I merge the minimum wage data to the combined CDE data set. Schools are then assigned to treatment-timing cohorts depending upon if and when a minimum wage became effective in their city. Schools in cities that never receive a minimum wage are assigned to the never-treated group and schools in cities with a minimum wage are assigned to a treatment-timing cohort depending upon when the minimum wage was enacted in their city. Schools in cities that implemented their minimum wage after Census Day of the previous year and before Census Day of the current year are assigned to the treatment-timing cohort of that year. If a city introduced a minimum wage on Census Day, schools in that city are assigned to the treatment-timing cohort of the following year. The only city that this applies to is Berkeley, who implemented their minimum wage on Census Day of 2014. Additionally, several cities had minimum wages that varied by firm size. For these cities I use the date of the earliest minimum wage to define the treatment-timing cohort.

In defining the treatment-timing cohorts, I define a city as treated or not based upon the city reported in the school characteristics data set. This data set contains information up to 2021, including school closures and merges. Problematically, if a school move locations this would not be observed in the data. Instead, I would only observe their most recently reported city. Because of this, my treatment variable is potentially measured with error, but this is likely small as it seems somewhat rare for a school to move to a different city. There are some schools assigned to incorrect cities. This was typically due to schools not reporting the actual city, but rather a neighborhood within the city. For example, schools reported La Jolla as their city, although they are technically in the city of San Diego. I corrected these when possible, otherwise it was dropped from the data set.

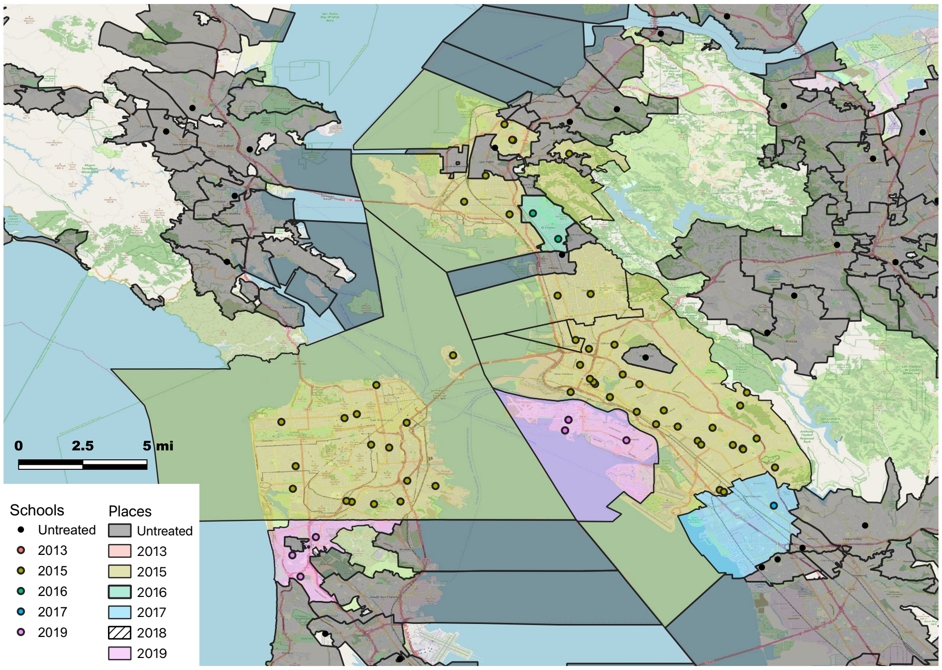
In addition to the main data set I described above, I also use 5-year American Communities Survey (ACS) data tables containing city-level information on population, median salaries, and other local labor market characteristics. This version of the ACS reports 5-year averages. I use this version because I primarily use these variables as time-invariant pre-treatment controls, so the fact that they may not adjust quickly to changes in economic conditions is not a concern.

5.2 Summary Statistics

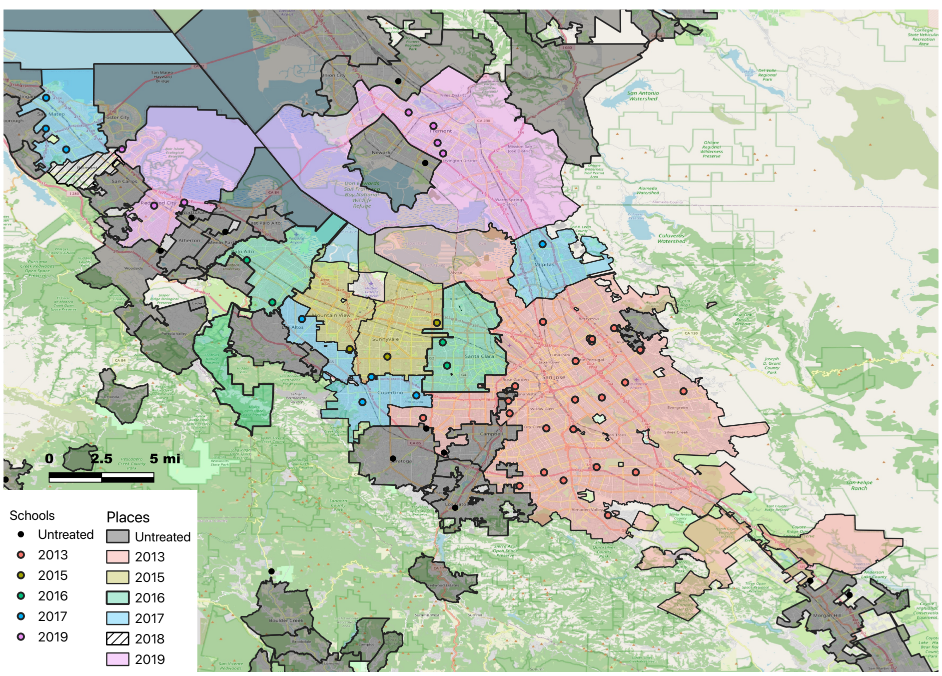
After cleaning, the main data set contains 12,449 observations of 1,442 unique schools in 569 cities and CDPs. Of these cities and CDPs, 44 introduced a minimum wage before 2020, with 25 being in cities (including San Francisco) and the remaining 19 being CDPs, resulting in 298 schools receiving treatment at some point in the panel. All 19 CDPs with a minimum wage are in Los Angeles County. As mentioned above, 26 cities in California received a minimum wage before 2020. The city not included in the main data set is Belmont, which enacted a minimum wage in 2018. Belmont is not included because there are no traditional high schools in the city.

Figure 1 and Figure 2 plot the 44 cities and CDPs in the panel that introduced a minimum

Figure 1: Northern California Schools and Local Minimum Wage Adoption



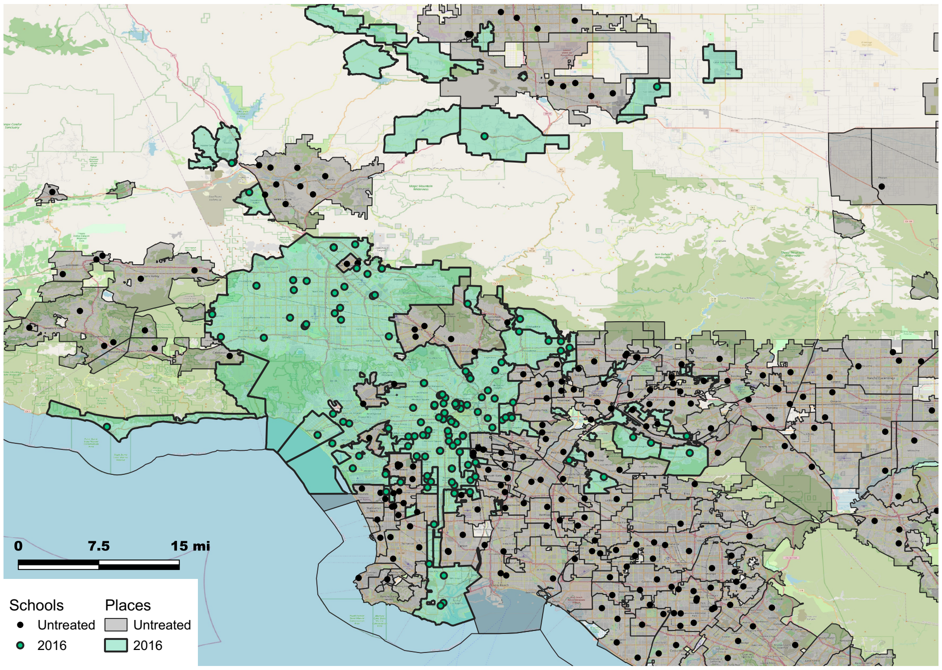
(a) San Francisco



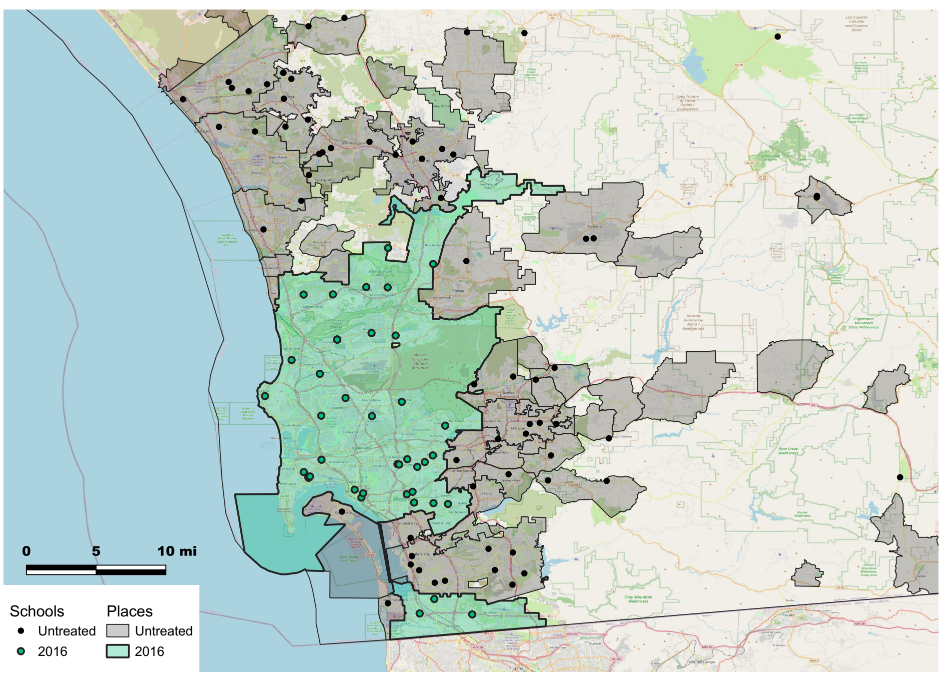
(b) San Jose

[1] These figures plot schools and the geographical boundaries of cities and CDPs in and around San Francisco in the top panel, and San Jose in the bottom panel. Outlined and filled areas are cities and CDPs, with the color corresponding to the treatment-timing cohorts listed in the legend. Each dot represents a school, plotted if it is observed at least once in the main data set.
[2] Created using school data from the CDE, local minimum wage data from the UC Berkeley Labor Center and Vaghul and Zipperer (2016), geographical boundaries from the US Census Bureau, and map data from OpenStreetMap.

Figure 2: Southern California Schools and Local Minimum Wage Adoption



(a) Los Angeles



(b) San Diego

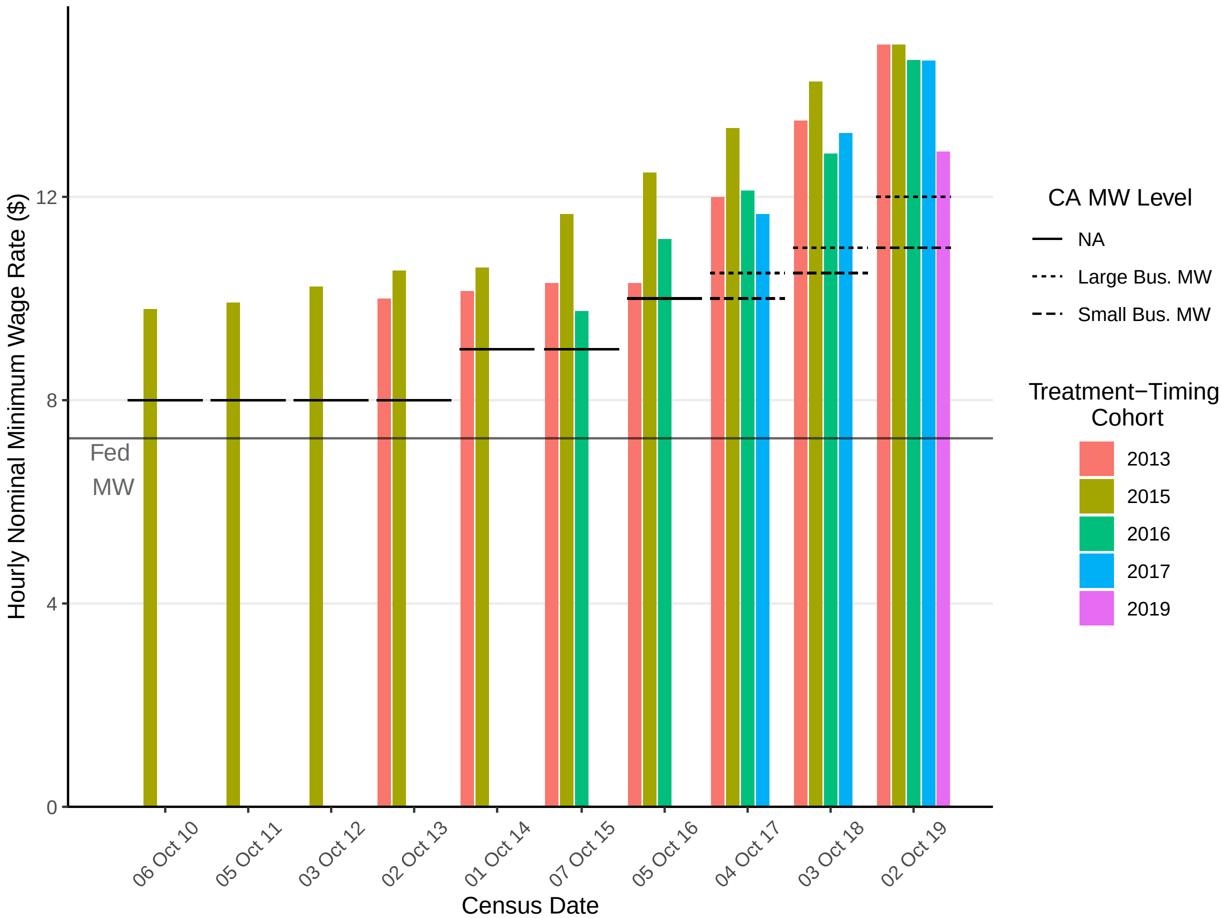
[1] These figures plot schools and the geographical boundaries of cities and CDPs in and around Los Angeles in the top panel, and San Diego in the bottom panel. Outlined and filled areas are cities and CDPs, with the color corresponding to the treatment-timing cohorts listed in the legend. Each dot represents a school, plotted if it is observed at least once in the main data set.
[2] Created using school data from the CDE, local minimum wage data from the UC Berkeley Labor Center and Vaghul and Zipperer (2016), geographical boundaries from the US Census Bureau, and map data from OpenStreetMap.

wage before 2020. Panel 1a plots the schools and place boundaries in and around San Francisco. Panel 1b does the same for San Jose. Similarly, Panel 2a and Panel 2b plot schools and place boundaries near Los Angeles and San Diego, respectively.[15] As can be seen from this figure, these cities are generally clustered around the San Francisco Bay Area or Los Angeles, the only exception being San Diego, the southernmost city highlighted on the map. The fill color of the places and points on the map represents the treatment-timing cohort each city has been assigned to. From this figure we can see that all of the variation in treatment-timing comes from the Bay Area. Table 3 in the appendix lists all cities and places in my data set along with their assigned treatment-timing cohort, adoption date, and when the minimum wage became effective.

Figure 3 plots the nominal local, state, and federal minimum wages on each Census Day from 2010 to 2019. The solid gray line spanning all of the columns is the federal minimum wage, which was constant for the duration of the panel. The solid black lines are the California state minimum wages on each Census Day. In 2017, the California state minimum wage split into two levels with the small business minimum wage covering businesses with 25 employees or less, and the large business minimum wage covering businesses with 26 or more employees. The two state minimum wages are represented in Figure 3 by the dashed lines for the final three Census dates. The figure also plots the local minimum wages, with bars representing the average local minimum wage by treatment-timing cohort on each Census Day in the panel. When taking the average across cities, I weight the nominal minimum wage by twelfth grade enrollment. For cities and CDPs with more than one minimum wage, a simple average was taken prior to this. Note that the 2015 cohort is above the state minimum wage level because of San Francisco's previous minimum wage in the early 2000s. This figure also illustrates the stepwise implementation of local minimum wages, where they are introduced at a relatively low level and increased incrementally over time. This is slightly less obvious, but still present, for both the 2013 and 2015 cohorts because they started with relatively higher average minimum wage levels.

[15]See Figure 12 in the appendix for a map of the entire state of California.

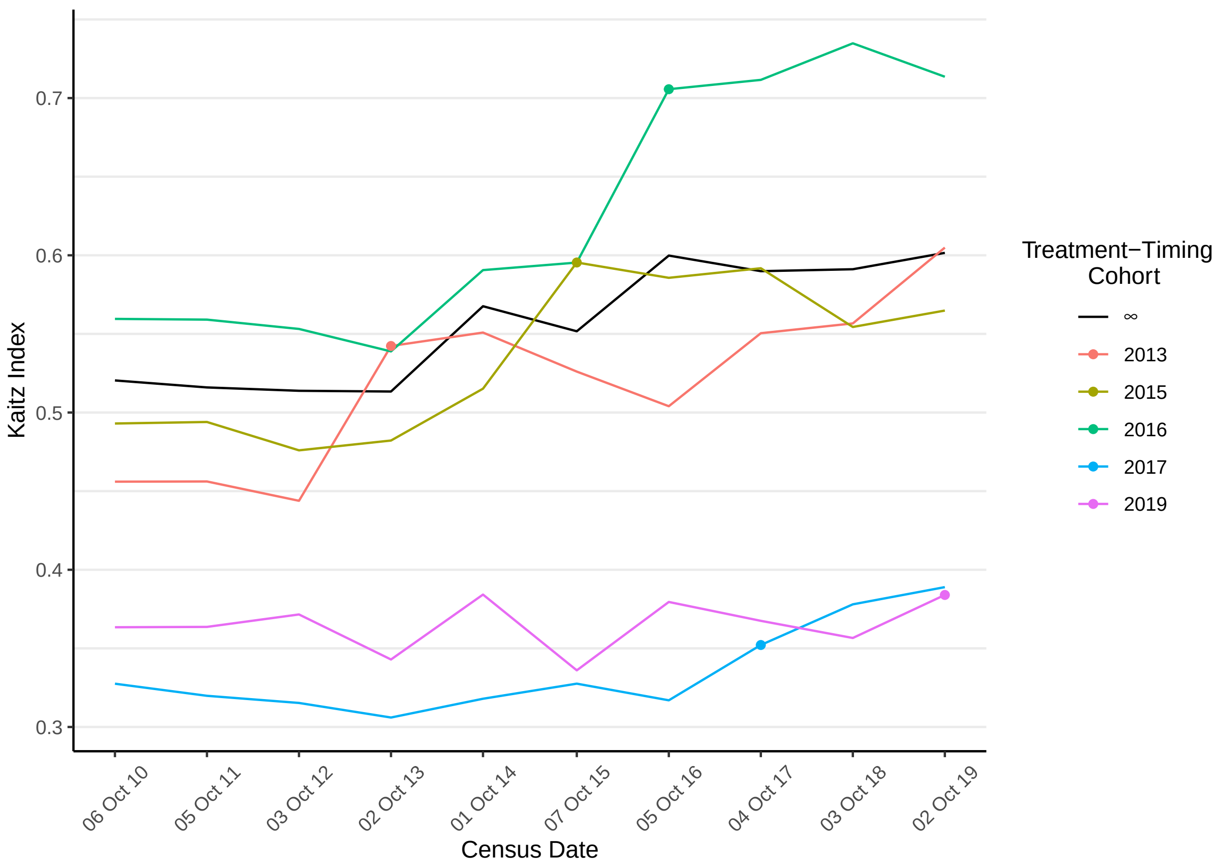
Figure 3: California Nominal Minimum Wages, Census Day 2010-2019



[1] This figure plots the federal minimum wage, state minimum wages, and the average local minimum wage by treatment-timing cohort on each Census Day in the panel. To calculate the average local minimum wages in each treatment-timing cohort, I weight each local minimum wage by twelfth grade enrollment on that day. The horizontal gray line is the federal minimum wage. The horizontal black lines are the state level minimum wages, with dashed lines indicating small and large business minimum wages beginning in 2017. A small business is 25 or fewer employees and a large business is 26 or more. The vertical bars are average local minimum wages on Census Day of a given year by treatment-timing cohort, weighted by twelfth grade enrollment.

[2] Created using local minimum wage data from the UC Berkeley Labor Center and Vaghul and Zipperer (2016), state minimum wage data from the California Department of Industrial Relations, and federal minimum wage data from the US Department of Labor. I exclude the city of Belmont from this figure.

Figure 4: Kaitz Index by Treatment-Timing Cohort, Census Day 2010-2019



[1] This figure plots the Kaitz index by treatment-timing cohort on each Census Day in the panel. The Kaitz index is the minimum wage relative to the median wage. To calculate the Kaitz index for each treatment-timing cohort, I first calculate the Kaitz index for each city and CDP. I then average the Kaitz index across cities and CDPs within a treatment-timing cohort, weighting by twelfth grade enrollment. For places with multiple minimum wages, a simple average was taken prior to computing their Kaitz index. Median wages are estimated by dividing the median salary for a city or CDP by 2,080. This figure only plots cities and CDPs with a school observed in the data set. As a result, Belmont is excluded.

[2] Created using local minimum wage data from the UC Berkeley Labor Center and Vaghul and Zipperer (2016), and state minimum wage data from the California Department of Industrial Relations. For large cities, I use median wage data from the 1-Year ACS data tables. When this is missing, I use data from the 5-Year ACS data tables.

Table 1: Summary Statistics by Year for the Main Data Set

	Year										
	2010	2011	2012	2013	2014	2015	2016	2017	2018	2019	Total
12th Grade Enrollment	305.67	301.58	296.53	291.21	286.81	282.99	277.96	279.41	279.56	277.99	287.53
	(234.01)	(233.01)	(231.39)	(228.71)	(227.68)	(225.28)	(223.31)	(226.73)	(223.96)	(222.63)	(227.66)
12th Grade Dropouts	14.12	13.98	12.54	11.37	8.59	7.35	6.68	.	.	.	10.53
	(20.42)	(21.85)	(19.76)	(18.51)	(15.76)	(13.93)	(12.50)	(.)	(.)	(.)	(17.92)
Post-Treatment	0.00	0.00	0.00	0.01	0.02	0.06	0.20	0.21	0.21	0.22	0.10
	(0.00)	(0.00)	(0.00)	(0.12)	(0.13)	(0.23)	(0.40)	(0.41)	(0.41)	(0.42)	(0.29)
Share FRPM	.	0.53	0.54	0.57	0.56	0.57	0.56	0.60	0.59	0.59	0.57
	(.)	(0.26)	(0.26)	(0.26)	(0.26)	(0.26)	(0.26)	(0.25)	(0.25)	(0.26)	(0.26)
Charter School	0.22	0.23	0.24	0.25	0.26	0.27	0.28	0.29	0.28	0.28	0.26
	(0.42)	(0.42)	(0.43)	(0.44)	(0.44)	(0.44)	(0.45)	(0.45)	(0.45)	(0.45)	(0.44)
Observations	1,136	1,168	1,205	1,232	1,251	1,272	1,293	1,307	1,298	1,297	12,459

This table reports the sample averages and standard deviations, in parentheses, for the main data set by year. Observations are at the school-year level. The final column contains averages and standard deviations for the pooled data set. Twelfth Grade Enrollment is the average number of students enrolled in a school-year on Census Day. Twelfth Grade Dropouts is the average number of dropouts in a school-year. Dropout data is not available for the 2017/18 through 2019/20 academic years. Post-Treatment is a dummy variable equal to one if an observation is in a city or place with an effective local minimum wage and zero otherwise. Share Free-Reduced Price Meal (FRPM) is the average share of students receiving free or reduced-price meals. Data on FRPM is not available for the 2010/11 academic year. Charter school is a dummy variable equal to one if the school is a charter school and zero otherwise.

Figure 4 plots the Census Day Kaitz index, or minimum wage relative to the median wage, over time for each treatment-timing cohort. I estimate median wages by dividing the median salary for a city or place by 2,080. For cities or places with multiple minimum wages, I take a simple average of each Kaitz index prior to plotting. The point along each line, aside from the untreated group, indicates when a treatment-timing cohort became treated. From this figure, we can see that there are essentially two distinct groupings of treatment-timing cohorts. Those with relatively high bite, which include the untreated group and the 2013, 2015, and 2016 treatment-timing cohorts, and those with relatively low bite, which includes the 2017 and 2019 cohorts. Differences in the bite of local minimum wages is one of the potential reasons for heterogeneous treatment effects, something that I consider below.

Table 1 provides sample averages and standard deviations for the main data set by year. Observations in this table are at the school-year level. As we can see in the first and final row, there is a downward trend in average yearly twelfth grade enrollment from 2010 to 2019 accompanied by an increase in the number of schools over the same period. Along with the decrease in enrollment over the duration of the panel, we can also see a decrease in dropouts for the years that the data is available. The post-treatment variable in the third row is a dummy variable equal to one if a school-year observation is in a city or place with a local minimum wage and zero otherwise. By the end of the panel approximately 22% of schools are in cities or places with a local minimum wage. The largest increase in cities or places with a minimum wage is in 2016, where 14% of schools become treated. This is largely due to Los Angeles and San Diego both being treated in 2016, the largest and second largest school districts in California, respectively. The fourth row of the table is the average share of students receiving free or reduced-price meals for each school-year. There is no data available for 2010, but from the 2011 to 2019 there is an upward trend, with the average share of students receiving free lunch increasing by 6 percentage points. The fifth row of the table reports the share of observations that are charter schools, which have also increased from 22% in 2010 to 28% in 2019.

Table 2 reports summary statistics by treatment-timing cohort at various levels of aggregation, with ∞ denoting the never treated group. In the first panel, observations are at the school-year level. Twelfth Grade Enrollment is defined similar to above, but this table highlights differences in enrollment across the treatment-timing cohorts and the control group. We can see that the 2017 cohort is by far the largest, with average enrollment of 508.77. For the remaining cohorts, average enrollment is around 200 to 300. The final row of the first panel, as well as the final row of the other two panels, highlights the difference in size of the treatment-timing cohorts, as I mentioned in the previous paragraph. The second panel in Table 2 reports averages over observations at the school level. Rural is a dummy variable equal to one if a school is in a rural place, which I define as having a population of less than 65,000. We can also see here that the 2017 cohort has the largest share of schools in rural places outside of the control group. The second row contains Southern CA, a dummy variable equal to one if a school is in southern California and zero otherwise. This row tells a similar story to the maps above, that all of the variation in treatment-timing comes from northern California. In the final panel of Table 2 I report averages at the city or CDP level. The only variable in this panel is Adoption Gap, which is the average number of

Table 2: Summary Statistics by Treatment-Timing Cohort

	Treatment-Timing Cohort					
	2013	2015	2016	2017	2019	∞
School-Years						
Twelfth Grade Enrollment	236.01	181.27	225.55	508.77	273.94	302.63
	(181.51)	(190.30)	(211.88)	(140.00)	(179.92)	(229.75)
Share Free/Reduced Lunch	0.46	0.69	0.72	0.22	0.32	0.55
	(0.29)	(0.23)	(0.26)	(0.16)	(0.14)	(0.25)
Observations	205	507	1,659	90	125	9,873
Schools						
Rural	0.00	0.02	0.13	0.44	0.00	0.54
	(0.00)	(0.13)	(0.34)	(0.53)	(0.00)	(0.49)
Southern CA	0.00	0.00	0.97	0.00	0.00	0.48
	(0.00)	(0.00)	(0.17)	(0.00)	(0.00)	(0.50)
Observations	25	55	195	9	15	1,048
Cities/Census Places						
Adoption Gap	125.00	139.86	232.81	147.80	175.25	0.00
	(.)	(83.12)	(111.32)	(79.62)	(111.38)	(0.00)
Observations	1	7	27	5	4	525

This table reports sample averages and standard deviations, in parentheses, by treatment-timing cohort for several variables in the main data set at different levels of aggregation. Untreated observations are denoted by ∞. The number of observations at each level of aggregation are presented in the last row of each panel in the table. The first panel presents Twelfth Grade Enrollment and the Share Free/Reduced Lunch, defined identically to Table 1, but instead of conditioning on year I condition on treatment-timing cohort before calculating the averages. The second panel contains geographical data at the school level. Rural is a dummy variable equal to one if a school is located in a rural place, defined as a city or CDP with a population less than 65,000 people, and zero otherwise. Southern CA is also a dummy variable equal to one if a school is in southern California and zero otherwise. The third panel reports averages over observations at the city or CDP level. Here, Adoption Gap as the average number of days between when a local minimum wage was adopted and when it became effective.

days between when a local minimum wage was adopted and when it became effective. We can see that this is particularly large for the 2016 cohort. This is largely due to San Diego, with a gap of 714 days. San Diego's minimum wage was initially planned for 2015, but this was postponed due to a mayoral veto resulting in a referendum. In Table 4 in the appendix, I present some pre-treatment summary statistics by treatment-timing cohort.

6 Methodology

To estimate the effect of a local minimum wage on twelfth grade enrollment in California I use three estimators, all of which use variation in local minimum wage adoption across cities and over time for identification. Each of these estimators rely on variations of the parallel trends assumption. For each estimator I assume that once a city adopts a minimum wage, they remain treated for the remainder of the panel. This is due to the relatively short length of the panel. For each estimator, I cluster the standard errors at the city level.

Initially, I will estimate the effect of the minimum wage using a Two-Way Fixed Effects (TWFE) estimator that allows for treatment effect dynamics by duration of exposure to a local minimum wage. I estimate the model:

$$Y_{st} = \gamma_s + \lambda_t + \sum_{\substack{\tau=-9,\\ \tau\neq-1}}^{6} \beta_\tau D_{st}^\tau + \varepsilon_{st} \ , \tag{4}$$

where Y_{st} is twelfth grade enrollment for school s in year t, γ_s is a set of school fixed effects, λ_t is a set of year fixed effects, D_{st}^τ is a dummy variable equal to one if a school is τ years from a local minimum wage being implemented and zero otherwise, and ε_{st} is the error term. Aside from the usual assumptions of (unconditional) parallel trends and no anticipation, I also assume that the treatment effect is homogeneous across treatment-timing cohorts for a given event-time (Borusyak et al. 2021; Sun and Abraham 2021).[16] Under these assumptions, the estimated coefficient on the contemporaneous effect and the lags ($\tau \geq 0$) is an average treatment effect on the treated (ATT) type parameter. I omit $\tau = -1$ as the reference period and estimate placebo effects by including 8 leads ($\tau \leq -2$).

Because I am estimating the effect of the minimum wage on enrollment using a binary variable, it is possible that there is treatment effect heterogeneity across cohorts for a given event time period. An obvious reason for this is because of variation in the level of the minimum wage and how much this bites into the earnings distribution as we saw in Figure 4 above. As a result, students may make different educational investment decisions depending upon the level of the local minimum wage and the treatment effect homogeneity assumption is unlikely to hold. This, along with the staggered adoption of local minimum wages, makes it possible that any of the lag or lead coefficients in Equation 4 are "contaminated" by treatment effects from other periods (Sun and Abraham 2021).

[16]This problem is not unique to the dynamic model. The static model estimated by TWFE has similar problems. See Chaisemartin and D'Haultfoeuille (2022) and Roth et al. (2022) for an overview of the recent literature regarding the TWFE estimator with variation in treatment-timing and treatment effect heterogeneity.

Due to the strong assumptions of the TWFE estimator, I also estimate the effect of the minimum wage using the estimator proposed by Callaway and Sant'Anna (2021). The Callaway and Sant'Anna (CS) estimator estimates a generalization of the ATT that allows the treatment effect to vary for each group and time combination. They refer to this parameter as the group-time average treatment effect on the treated or $ATT(g,t)$. Without control variables, estimation of each $ATT(g,t)$ involves subsetting the main data set to include only years $g-1$ and t. For both of these years I then remove all schools except those in treatment-timing cohort g and those that are not-yet or never-treated in year t.[17] Finally, I remove any schools that are not observed in both years. The $ATT(g,t)$ is then estimated by estimating β^{gt} via Ordinary Least Squares (OLS) in:

$$Y_{st} = \alpha^{gt} + \gamma^{gt} G_s + \lambda^{gt} T_t + \beta^{gt} G_s \times T_t + \varepsilon_{st}^{gt} \ , \tag{5}$$

where G_s is a dummy variable equal to one if a school is in treatment-timing cohort g and zero otherwise, and T_t is a dummy variable equal to one if an observation is post-treatment and zero otherwise. Under a parallel trends and no anticipation assumption, the estimates for the coefficient of the interaction term, β^{gt}, are estimates of the $ATT(g,t)$.

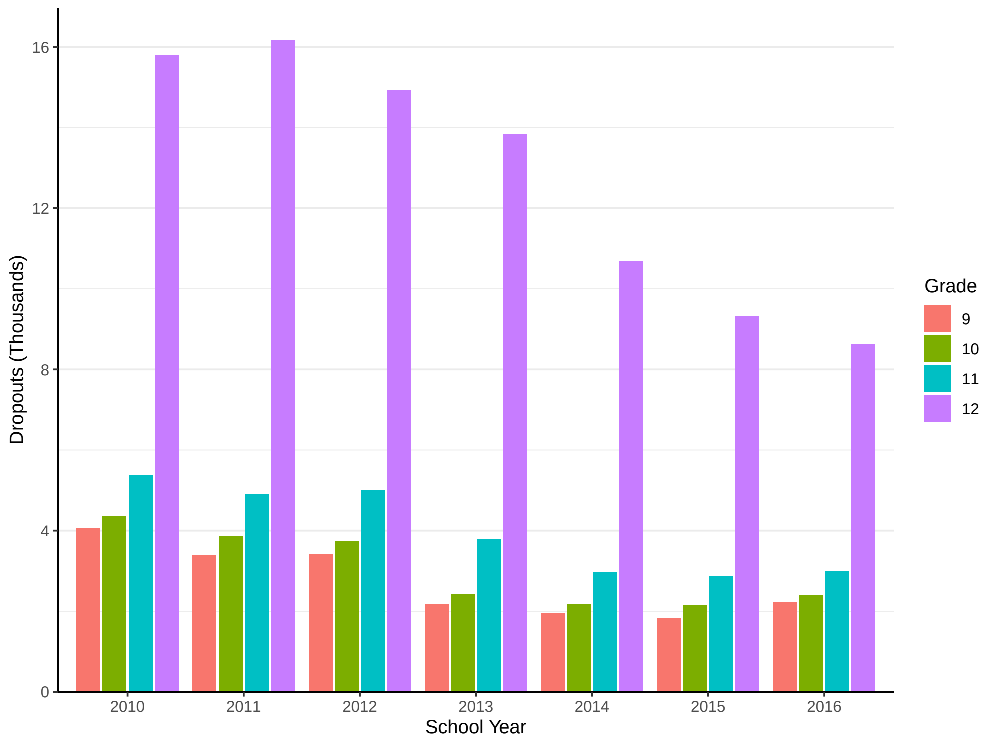
It is possible that parallel trends only holds conditional on covariates. To account for this, I also estimate the $ATT(g,t)$ using the CS estimator with 5-year city-level population for 15-17-year-olds in year $g-1$ as a control variable for each treatment-timing cohort. I use only time-invariant pre-treatment population as it is plausible that city-level population is affected by local minimum wages, resulting a bad control problem (Angrist and Pischke 2009). Further, I use 5-year averages because this minimizes the number of observations I need to drop due to missing population data. Another issue that I encounter is there are some treated observations lacking common support with untreated observations. For example, Los Angeles has the largest population in California and there would not be any similarly sized control group schools. As a result, I scale population by the number of schools in a given city and use this constructed variable as the control that I include.

To estimate each $ATT(g,t)$ while controlling for population, I use the doubly robust DiD estimator proposed by Sant'Anna and Zhao (2020) which Callaway and Sant'Anna (2021) extend to the staggered setting. This DiD estimator models both the outcome and the probability of treatment to obtain estimates of the $ATT(g,t)$. This requires estimating both a conditional expectation function and propensity scores and is unbiased if either is modeled correctly.

The final estimator that I use, which is also my preferred estimation strategy, is a triple DiD. To do so, I create an additional dimension in the data set based upon grade level. The third dimension that I use is average ninth through eleventh grade school-level enrollment. I use the average instead of a single year as an attempt to smooth out any yearly fluctuations in enrollment for a particular grade. This average should be relatively unaffected by the minimum wage because a typical student is not able to drop out without parental permission before twelfth grade. This can be seen in Figure 5, which plots the total number of dropouts by grade and school year across schools in the data set that I use for my analysis below. As

[17] For this estimator, and each one that follows, I use not-yet and never-treated units as controls.

Figure 5: High School Dropouts by Grade and School Year.



This figure plots the total number of dropouts by grade and school year which I calculate using schools in my main data set. The x-axis is the school year and the y-axis is the total number of dropouts (in thousands) across all schools for a given grade, which is represented by the color of the bar. Dropout data is not available for the 2017/18 through 2019/20 academic years.

can be seen from this figure, the number of dropouts for ninth through eleventh grades is markedly lower than it is for twelfth grade in each of the years that data is available.

I then estimate:

$$\begin{aligned} Y_{slt} = \alpha_3^{gt} + \gamma_3^{gt} G_s + \theta_3^{gt} L_l + \lambda_3^{gt} T_t + \delta_3^{gt} G_s \times L_l + \\ \rho_3^{gt} G_s \times T_t + \psi_3^{gt} L_l \times T_t + \beta_3^{gt} L_l \times G_s \times T_t + \varepsilon_{slt,3}^{gt} \ , \end{aligned} \tag{6}$$

where G_s and T_t are defined similar to before and L_l is a dummy variable equal to one if the grade level is twelve and zero otherwise. Also similar to Equation 5, I estimate this model on each group-time subset of the data.

The coefficient of the triple interaction term, β_3^{gt} is the $ATT(g,t)$ under a slightly different variation of the parallel trends assumption in addition to the usual no anticipation assumption. Specifically, we now need to assume that the difference in the average evolution of enrollment between twelfth graders and the average of ninth through eleventh graders is the same in cities with and without a minimum wage.

Under an additional assumption, the triple DiD estimator also more plausibly eliminates between city migration as one of the channels that the minimum wage affects enrollment. To see why, note that we can decompose enrollment in a given school s and year t as the students who were already there, E_{st}, and students who moved to/from the school, M_{st}, so that $Y_{st} = E_{st} + M_{st}$. The change in total enrollment between years t and $g-1$ can similarly be decomposed. Let the change in total enrollment be $\Delta Y_{st}^g = Y_{st} - Y_{s,g-1}$, the change in those that did not move be $\Delta E_{st}^g = E_{st} - E_{s,g-1}$, and those that did move be $\Delta M_{st}^g = M_{st} - M_{s,g-1}$, so that:

$$\Delta Y = \Delta E + \Delta M \ , \tag{7}$$

for each group-time combination where $t \geq g$. I omit the subscripts and superscripts here for notational simplicity. For each g and t with $t \geq g$:

$$\begin{aligned}
\beta_3^{gt} = &\Big(\mathbb{E}[\Delta Y|L=1,S=1] - \mathbb{E}[\Delta Y|L=0,S=1]\Big) - \\
&\Big(\mathbb{E}[\Delta Y|L=1,S=0] - \mathbb{E}[\Delta Y|L=0,S=0]\Big) \\
= &\Big(\mathbb{E}[\Delta E + \Delta M|L=1,S=1] - \mathbb{E}[\Delta E + \Delta M|L=0,S=1]\Big) - \\
&\Big(\mathbb{E}[\Delta E + \Delta M|L=1,S=0] - \mathbb{E}[\Delta E + \Delta M|L=0,S=0]\Big) \\
= &\Big[\Big(\mathbb{E}[\Delta E|L=1,S=1] + \mathbb{E}[\Delta M|L=1,S=1]\Big) - \\
&\Big(\mathbb{E}[\Delta E|L=0,S=1] + \mathbb{E}[\Delta M|L=0,S=1]\Big)\Big] - \\
&\Big[\Big(\mathbb{E}[\Delta E|L=1,S=0] + \mathbb{E}[\Delta M|L=1,S=0]\Big) - \\
&\Big(\mathbb{E}[\Delta E|L=0,S=0] + \mathbb{E}[\Delta M|L=0,S=0]\Big)\Big] \ .
\end{aligned}$$

If one is willing to assume that for both the treated and untreated groups, changes in enrollment due to migration are on average the same across the two grade level partitions (when $L=1$ and $L=0$) for each group-time combination that the $ATT(g,t)$ is identified, or:

$$\mathbb{E}[\Delta M|L=1,S=1] = \mathbb{E}[\Delta M|L=0,S=1]$$

and

$$\mathbb{E}[\Delta M|L=1,S=0] = \mathbb{E}[\Delta M|L=0,S=0] \ ,$$

then:

$$\begin{aligned}
\beta_3^{gt} = &\Big(\mathbb{E}[\Delta E|L=1,S=1] - \mathbb{E}[\Delta E|L=0,S=1]\Big) - \\
&\Big(\mathbb{E}[\Delta E|L=1,S=0] - \mathbb{E}[\Delta E|L=0,S=0]\Big) \ .
\end{aligned} \tag{8}$$

This is essentially just a parallel trends assumption on migration by treatment status. We would need a similar assumption to hold in Equation 5 to purge the treatment effect of migration, although it would be across treated and control cities. This seems less plausible, particularly if there is migration between treated and control cities in response to a minimum wage.

An additional problem is spillovers. Because of the way that I define treatment, it is possible that untreated students live near a city with a minimum wage change their schooling decisions because of their proximity to a treated school. Because of this, I assign schools within 15 miles of a treated school to a spillover treatment-timing cohort based upon which year the nearest treated school was treated. I then re-estimate the main effects in Equation 6 excluding spillover schools from the control group. Then, I estimate Equation 6 for schools in the 15-mile spillover zone, using untreated and schools that are not in the spillover zone as the control group.

It is worthwhile to mention a significant shortcoming of the analysis. Using enrollment levels as the outcome variable is not ideal. With this data, constructing a city-level enrollment rate using population data from the ACS data tables is problematic. It would require using only places observed in the 1-year ACS data tables *or* using 5-year population data as well. Using only 1-year population data is problematic because I would lose 46% of my observations. The other alternative is constructing an enrollment rate using 5-year population data (or some combination of 1-year and 5-year data). In doing so, I lose less than 1% of my data due to missing observations. The problem with doing this to construct an enrollment rate is that changes in population will not be immediately reflected in the population variable because they are averages over such a long time span. As a result, I prefer to control for pre-treatment 5-year population averages using the CS estimator or attempt to eliminate migration as a channel using the triple DiD estimator.

The triple DiD estimator is my preferred estimator for several reasons. Foremost, it does not suffer from the problems that the TWFE estimator does. The CS estimator without controls is also likely problematic in this case. Although it does not suffer from the drawbacks of the TWFE estimator, it also does not deal with migration explicitly. Further, even when controlling for population with the CS estimator, this does not solve the problem of migration as controlling for population and controlling for migration are different. Unfortunately, it is not possible to construct a migration variable to be used in place of the population variable that I use for the same reason that it is not possible to construct an enrollment rate variable, having to rely on 5-year ACS population data means that the changes in population will not be immediately reflected in the constructed migration variable. Because of this, and how the triple DiD estimator can seemingly more plausibly deal with migration, it is my preferred estimator.

7 Results

In this section I present the main findings of my research question: "How have local minimum wages in California affected twelfth grade enrollment?" First, I present the TWFE estimates from Equation 4. After, I present CS estimates from Equation 5 without controls. Then I present the CS estimates with population as a control. Finally, I present the results for the triple DiD in Equation 6 initially without then with spillovers.

7.1 Two-Way Fixed Effects (TWFE) Estimates

Figure 6 plots the TWFE event study estimates of β_τ in Equation 4. This provides a useful comparison to the results using the CS estimator that follow. The red circles are estimated treatment effects from relative time dummy variables post treatment and the blue triangles are placebo estimates from relative time dummies prior to treatment. The vertical lines are 95% confidence intervals, calculated using standard errors clustered at the city level. I also do not bin or trim the distant relative time periods. Instead, I estimate all identified relative time coefficients and only plot the coefficients from event time periods with relatively many observations. These are not balanced in event time, but because a large fraction of treated schools receive treatment on or before 2016 (see Table 1), the coefficients $\beta_{\tau=1}$, $\beta_{\tau=2}$, and $\beta_{\tau=3}$ are estimated using a relatively large number of observations.

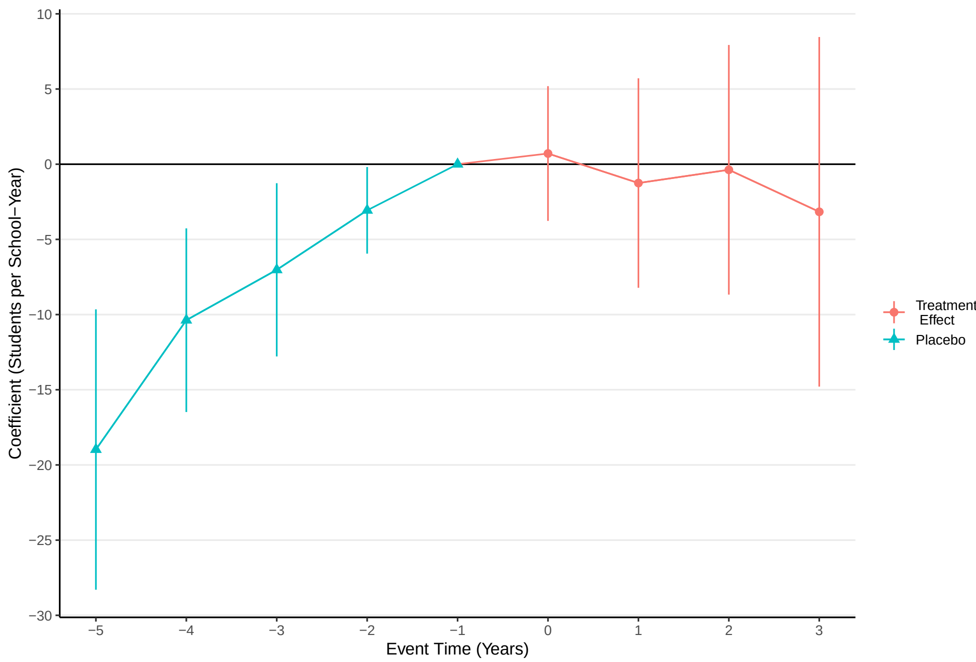
As we can see from Figure 6, the estimated coefficients peak in the initial year following minimum wage adoption, $\tau = 0$, and decrease thereafter. None of the estimated treatment effects are significantly different from zero at the 95% significance level. We can also see that the placebo estimates follow a nearly linear relative time trend prior to treatment. All placebo estimates are significantly different from zero at the 95% level. There are several possible explanations for the significant placebo estimates. First, there may be anticipation prior to treatment occurring. Although a local minimum wage is discussed some time in advance, it seems unlikely that the placebo estimates are all significant solely due to anticipation. Second, there may be differential trends in the outcome variable prior to treatment. Third, the coefficients could be contaminated due to heterogeneous treatment effects as discussed in the previous section. I will investigate these possibilities below.

7.2 Callaway and Sant'Anna (CS) Estimates

To allow for heterogeneous treatment effects, I estimate the effect of the minimum wage on enrollment using the CS estimator from Equation 5.[18] Figure 7 plots the $ATT(g, t)$ estimates, with each panel being a different treatment-timing cohort. The coefficients are plotted as a percent of treatment-timing cohort average pre-treatment enrollment for comparability. As we can see in the figure, for the 2013, 2015, 2017, and 2019 treatment-timing cohorts, the $ATT(g, t)$ estimates are positive and vary in magnitude. The placebo estimates for these cohorts are typically around zero, with all but three placebo estimates being statistically insignificant. Two of the three significant placebo effects come from the 2013 cohort, which is only San Jose. The other significant placebo effect is in the year prior to treatment for the 2019 cohort. The 2016 cohort's estimated treatment effects look much different. All of the estimates are negative and significant. In addition, all of the placebo effects are positive,

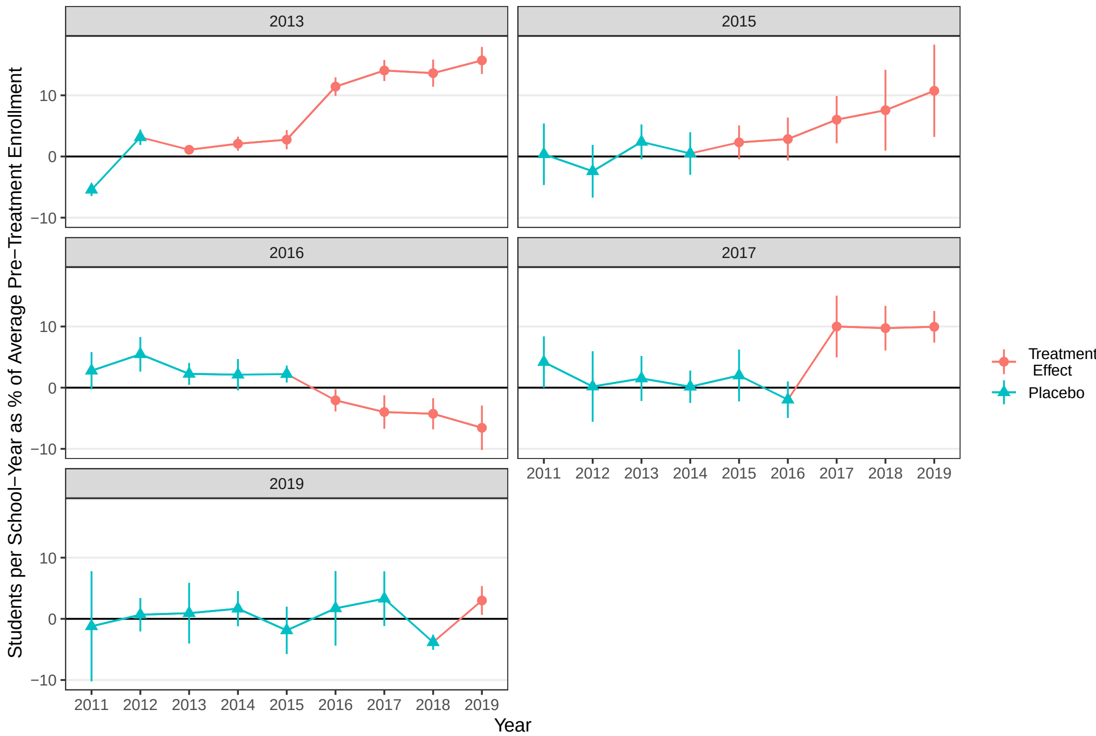
[18] Another potential concern is that adoption of the minimum wage may coincide with schools opening or closing. This could result in changes in school level enrollment, while there is no change in city level enrollment. As a result, I present Figure 13 and Figure 14 in the appendix which report treatment effect estimates using city level enrollment. The former does not include any controls and the later uses city level population as a control. The 2013 cohort is dropped from the figure because it is comprised of a single city, San Jose. The estimates presented here in Figure 7 at the school level are similar to the estimates in the appendix at the city level, suggesting that school openings and closures are not driving the findings.

Figure 6: Minimum Wage and Twelfth Grade Enrollment, TWFE Event Study Estimates



This figure plots TWFE event study estimates from Equation 4, with $\tau = -1$ omitted as the reference period. Each point represents the estimated coefficient and the vertical bars are the corresponding 95% confidence intervals. Blue triangles are estimated placebo effects prior to treatment and red circles are the estimated treatment effects. Standard errors are clustered at the city level. The coefficients are estimated without binning or trimming, but I do not plot estimates from distant relative time periods.

Figure 7: Minimum Wage and Twelfth Grade Enrollment, Callaway and Sant'Anna Estimates



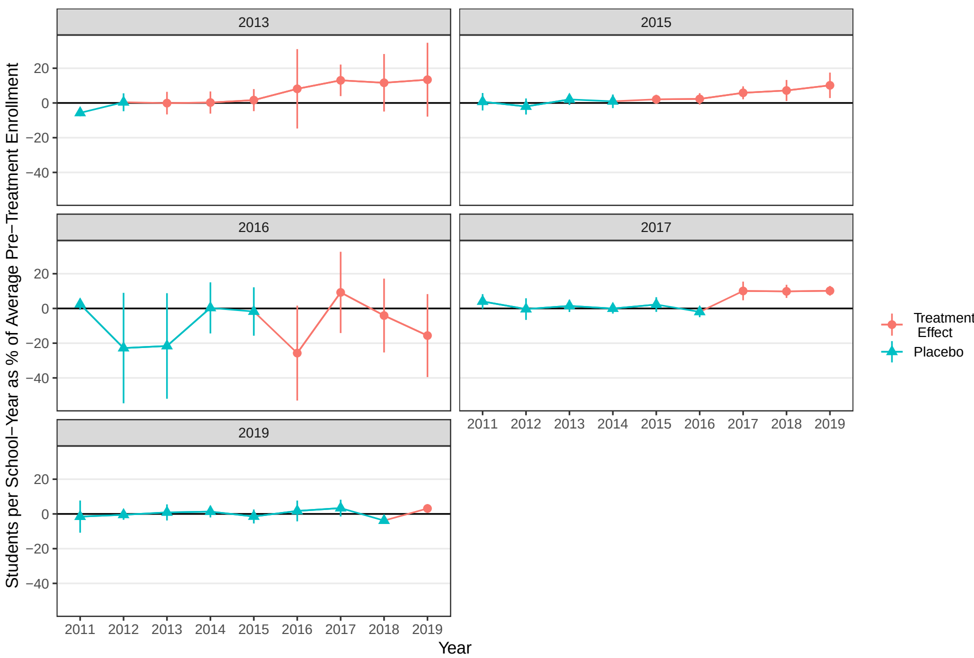
This figure plots the Callaway and Sant'Anna (2021) $ATT(g, t)$ estimates from Section 5 Equation 5. The year at the top of each subplot denotes the treatment-timing cohort. Blue triangles are estimated placebo effects prior to treatment and red circles are the estimated treatment effects. The vertical bars attached to each estimate are the corresponding 95% confidence intervals. All of the parameter estimates are plotted as a percent of their treatment-timing cohort's average twelfth grade pre-treatment enrollment. The estimates in this figure do not include any controls and standard errors are clustered at the city level. See Table 5 in the appendix for the raw coefficients in table format.

with three being statistically significant. Because of the treatment effect heterogeneity across cohorts, the significant pretrends shown in Figure 6 are misleading.

The significant pretrends in Figure 7 may arise due to a violation of the no anticipation assumption or they could suggest a violation of the parallel trends assumption. It is impossible to definitively state which of the two is causing the observed patterns for the 2013, 2016, and 2019 cohort. But it is possible that conditional parallel trends is a better assumption to be making, particularly with population and migration potentially being problematic.

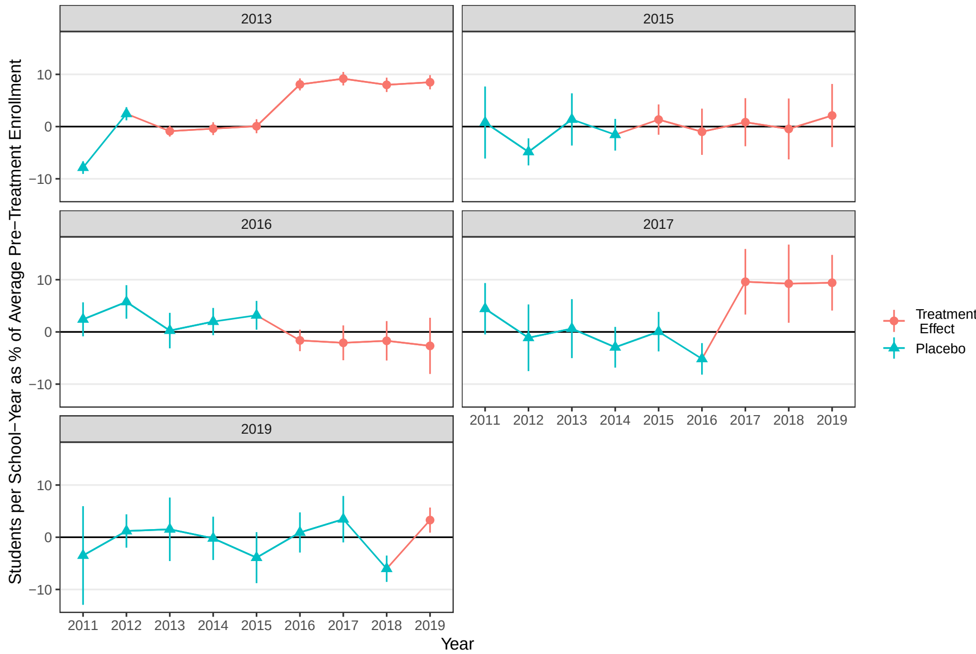
As a result, I estimate the group-time treatment effects conditioning on population aged 15 to 17, relative to the number of schools in the city, and present the results in Figure 8. The results for the 2013, 2015, 2017, and 2019 cohort are similar for both the unconditional and conditional on population estimates. The 2016 cohort results are much different when conditioning on population. The pretrends for the 2013 cohort now appear to be relatively flatter. The magnitude and standard errors in several years has increased, but the significant pretrends have entirely disappeared. Additionally, the only treatment effect that is significant

Figure 8: Minimum Wage and Twelfth Grade Enrollment, Callaway and Sant'Anna Estimates Conditional on Population



This figure plots the Callaway and Sant'Anna (2021) $ATT(g, t)$ estimates using the procedure described in Section 5, conditioning on population aged 15 to 17 relative to the number of schools in the city. The year at the top of each subplot denotes the treatment-timing cohort. Blue triangles are estimated placebo effects prior to treatment and red circles are the estimated treatment effects. The vertical bars attached to each estimate are the corresponding 95% confidence intervals. All of the parameter estimates are plotted as a percent of their treatment-timing cohort's average twelfth grade pre-treatment enrollment. Standard errors are clustered at the city level. See Table 6 in the appendix for the raw coefficients in table format.

Figure 9: Triple Difference-in-Differences



This figure plots the triple DiD $ATT(g, t)$ estimates using the procedure from Section 5 Equation 6. The year at the top of each subplot denotes the treatment-timing cohort. Blue triangles are estimated placebo effects prior to treatment and red circles are the estimated treatment effects. The vertical bars attached to each estimate are the corresponding 95% confidence intervals. All of the parameter estimates are plotted as a percent of their treatment-timing cohort's average twelfth grade pre-treatment enrollment. Standard errors are clustered at the city level. See Table 7 in the appendix for the raw coefficients in table format.

is in the initial year of treatment at the 10% level. Rather than interpreting this as a causal effect (or lack thereof), I interpret this as suggesting that population dynamics are contributing the parameter estimates, at least for one of the cohorts. In the following section, I attempt to control for differences in population levels and migration using my preferred strategy.

7.3 Triple Difference-in Differences (DiD) Estimates

The difference between the 2016 cohort estimates and others raises questions as to whether the identifying assumptions hold or whether they truly have entirely different treatment effects than the other cohorts. In Figure 9 I present estimates of the triple interaction terms β_3^{gt} in Equation 6. As mentioned in the previous section, the identifying assumptions of the triple DiD estimator seem more plausible and it can also eliminate the migration channel under an additional assumption.

As we can see from Figure 9, the placebo and treatment effect estimates are similar to the previous results for the 2013, 2017, and 2019 cohort. For the 2015 cohort, all of the treatment effect estimates have become insignificant and are around zero. The 2016 cohort's placebo estimates are similar to the unconditional CS estimator, but the point estimates of the treatment effects have decreased in magnitude, although they remain negative, they have become insignificant and have a flatter profile.

If the identifying assumptions for this estimator hold, then this suggest that migration, or something else, was contributing to the positive effect for the 2015 cohort and the negative effect for the 2016 cohort. This raises the question as to why there are different responses to treatment. I will discuss this in the discussion subsection below.

7.4 Spillovers

In this subsection, I account for the possibility of spillovers due to a school's proximity to a place with a local minimum wage. To do so, I estimate β_3^{gt} again, excluding schools within 15 miles of a treated school from the control group. I present these results in Figure 10. These results are similar to the main triple DiD results that do not exclude schools in the spillover zone from the control group.

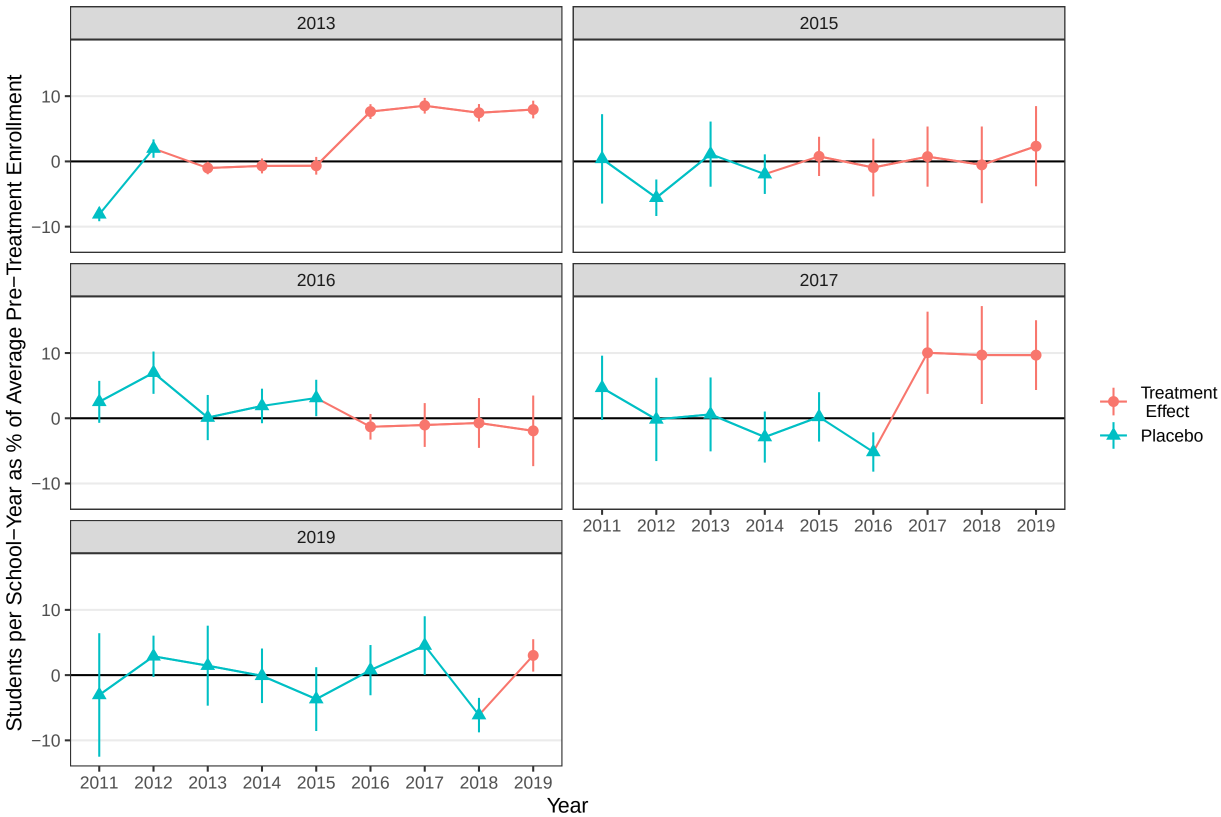
In Figure 11, I estimate the spillover effects for each spillover treatment-timing cohort as I discussed in the methodology section. For the 2015, 2016, 2017, and 2019 cohorts, the estimates are generally insignificant, although the uncertainty around the point estimates varies and is particularly large for the 2017 cohort. Interestingly, the spillover effect estimates for the 2013 cohort are negative. There are several possible reasons for this including differences in the bite of the minimum wage or there is potentially migration from spillover areas to places with a minimum wage that is not being captured by the triple DiD.

7.5 Discussion

From the results presented above, there are significant differences across estimator used and the identifying assumptions made. Perhaps the most interesting comparison is between the CS estimators and the triple DiD estimator. One of the more obvious characteristics between these estimators is how similar the 2017 and 2019 cohorts' estimates are regardless of the estimator used. Also notable, controlling for population or eliminating migration using the triple DiD estimator changes the point estimates for the remaining cohorts. As mentioned above, I take these differences across estimators to suggest that population dynamics are contributing to the point estimates, at least for these three cohorts.

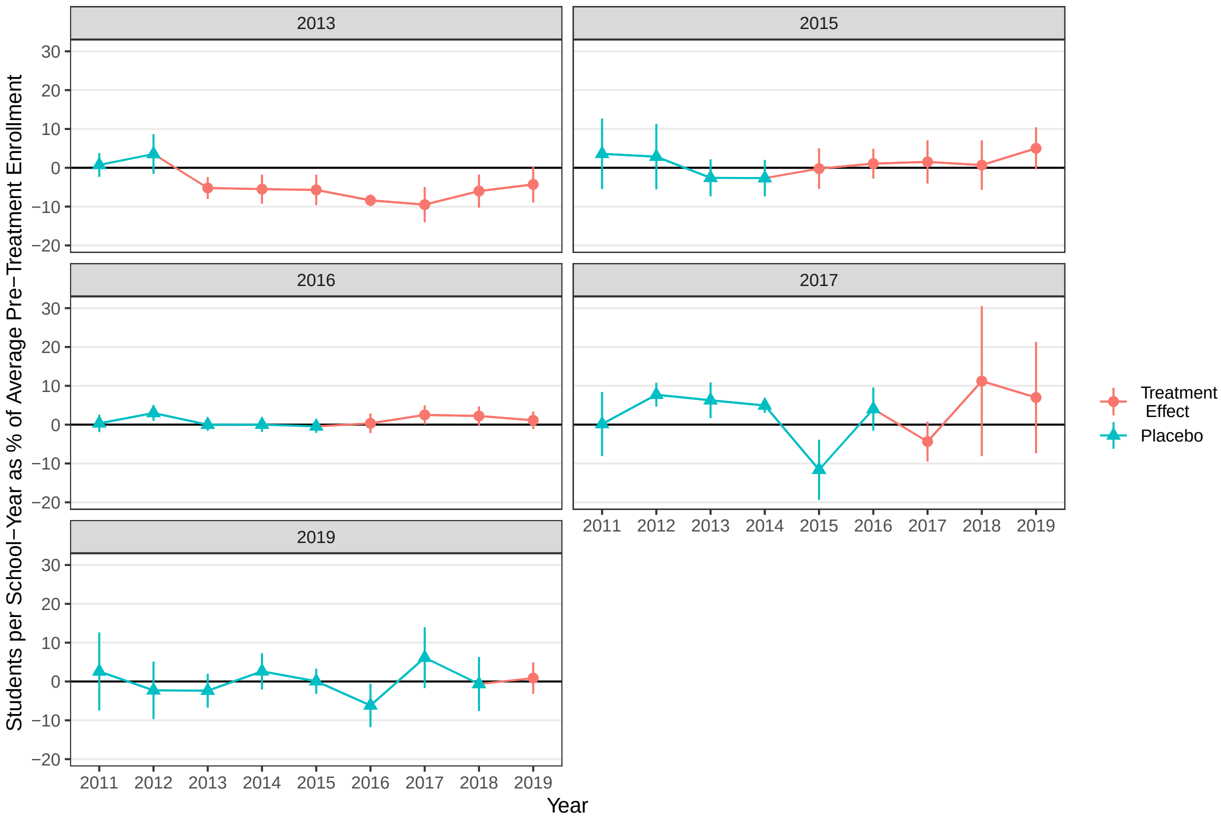
In the remainder of this section, I will discuss the triple DiD estimates as I believe these most plausibly deal with migration. Under the assumptions of the estimator discussed above, the findings suggest that for some treatment-timing cohorts enrollment increased due to the introduction of a local minimum wage. If the migration channel is eliminated using the triple DiD estimator, then these results suggest that for 2017 and 2019 cohorts the payoff to finishing high school increased relative to the payoff to not finishing high school. As mentioned above, research by Dube (2019) suggests that family income increases because of the minimum wage. Whether the student's opportunity cost to schooling changes depends upon whether family income changed by more or less than the expected wage for not completing high school. Whether this contributed entirely to the increase in enrollment for these cohorts is difficult to say, but I will explore this further in the paragraphs that

Figure 10: Main Effect with Spillovers



This figure plots the triple DiD $ATT(g,t)$ estimates using the procedure from Section 5 for schools directly affected by treatment. I exclude schools withing 15 miles of a treated school from the control groups. The year at the top of each subplot denotes the treatment-timing cohort. Blue triangles are estimated placebo effects prior to treatment and red circles are the estimated treatment effects. The vertical bars attached to each estimate are the corresponding 95% confidence intervals. All of the parameter estimates are plotted as a percent of their treatment-timing cohort's average twelfth grade pre-treatment enrollment. Standard errors are clustered at the city level.

Figure 11: Spillover Effect



This figure plots the triple DiD $ATT(g,t)$ spillover estimates using the procedure from Section 5 for schools *not* directly affected by treatment. That is, I assign schools within 15 miles of a treated school to the treatment timing cohort of the nearest treated school. The year at the top of each subplot denotes the treatment-timing cohort. Blue triangles are estimated placebo effects prior to treatment and red circles are the estimated treatment effects. The vertical bars attached to each estimate are the corresponding 95% confidence intervals. All of the parameter estimates are plotted as a percent of their spillover treatment-timing cohort's average twelfth grade pre-treatment enrollment. Standard errors are clustered at the city level.

follow.

The estimates I have presented above also raise the question as to why there are such significant differences across cohorts. For example, the lack of an apparent effect for the 2015 and 2016 cohort and the positive estimates for the 2017 and 2019 cohorts. One potential reason is differences in the Kaitz index. As can be seen in Figure 4, there are two distinct groups, the 2017 and 2019 cohorts have a relatively low bite, and all other cohorts, which have a relatively higher bite. The 2017 and 2019 cohorts, with the relatively low bite, both have consistently positive estimated treatment effects. Conversely, the 2015 and 2016 cohorts, both with relatively high bite, all have relatively small and statistically insignificant treatment effect estimates with the triple DiD estimator (I will discuss 2013 separately below).

Differences in the Kaitz index are highlighting two important features, differences in local minimum wage rates and differences in average income across geographies. In fact, what can be seen from Table 2 is that the 2017 and 2019 cohorts are relatively wealthier, having less students receiving free/reduced-price lunch. There are also racial differences between the 2017 and 2019 cohort and the 2015 and 2016 cohort. Table 8 contains racial shares by treatment-timing cohort. As can be seen from this table the 2015 and 2016 cohorts have larger shares of Hispanic and Black students than the 2017 and 2019 cohorts.

Given the evidence that the minimum wage increases family income at the bottom of the wage distribution and assuming this is the case for the relatively lower income 2015 and 2016 cohorts where student poverty is higher, then many of these students' would see an increase in family income, but this must be roughly offset by an increase in their no schooling expected wage given that there is not much of a change in enrollment either up or down. For the higher income cohorts, 2017 and 2019, family income is unlikely to be affected much by the minimum wage introduction. Therefore, even if their no schooling expected wages increased, this increase must have been by less than the increase in their expected wages having completed high school. These findings are relatively in line with the hypothesis posited by Ehrenberg and Marcus (1982), that low-income students would be negatively affected and that high-income students would be positively affected by a minimum wage. Although in this paper, I do not find any evidence that lower income cohorts are negatively affected by the minimum wage introduction, they appear to be on average no better off than before it.

The only cohort which does not fit into this pattern is the 2013 cohort. Although they have a relatively low share of students receiving free/reduced-price meals, they also have relatively low median wages in 2010. The pattern of estimated treatment effects is also rather curious. The fact that there were no significant effects until three years after the minimum wage introduction makes me question whether the identifying assumptions hold for this group. In fact, both placebo estimates are significant prior to treatment for this cohort. A violation of identifying assumptions could be driven by several factors, for example changing demographics, anticipation of the minimum wage, or a differences post-recession labor market recovery to name a few. Overall, I believe that there is some evidence that the identifying assumptions for this cohort do not hold.

There may also be concerns about the size of the effect estimates. For example, the

2017 cohort experienced an average post-treatment enrollment increase of 9.42%. The point estimates for the 2013 cohort were also relatively large after 2015, approximately a 9.73% increase in enrollment, but as mentioned above I have concerns about identifying assumptions holding for this cohort. These estimates are relatively in line with the recent existing literature which has also found a positive effect. For example, Alessandrini and Milla (2021) finds a 6% increase in community college enrollment following a minimum wage increase in Canada. Smith (2021) finds between a 4% to 10% decrease in the average dropout rate for high schoolers in the US. Although the estimates for some of the cohorts in this paper are large, I do not believe that they are unreasonable considering the findings of the recent literature.

8 Conclusion

In this chapter I have looked at whether local minimum wages cause any change in twelfth grade enrollment in California. To answer this question, I used a variety of related estimators that identify treatment effects under slightly different assumptions. In addition, several of these estimators allow for the variation in treatment-timing and spillovers.

With my preferred estimator, under parallel trends and no anticipation assumptions, I estimate the effect that the introduction of local minimum wages has on twelfth grade enrollment. I have presented evidence that local minimum wages have typically increased or had no effect on twelfth grade enrollment. These findings are in sharp contrast to what the previous literature has typically found in the US using state level minimum wages with few exceptions, notably the recent paper by Smith (ibid.). For the 2017 cohort I find that the introduction of local minimum wages increased enrollment by between 9.24% and 9.61% of pre-treatment average enrollment in each of the years following the minimum wage introduction. For the 2019 cohort I find a more modest effect, an increase in enrollment by approximately 3.29% of average pre-treatment enrollment.

There are several limitations of this paper aside from the usual assumptions necessary for identification with DiD type estimators. One of the main concerns in this paper is the use of enrollment as the outcome. This is a fairly broad measure of schooling that captures other potential mechanisms that, albeit interesting, are not due to changes in schooling behavior *per se*. Perhaps the most problematic is migration. I have presented several estimates that try to deal with changes in population more generally. Several of the estimators used above have attempted to control for the relevant population size or eliminate migration as a potential channel.

A second limitation is the external validity of this study may be limited as I focus on a single state, California. It is not clear if these results would generalize to other states, although Smith (ibid.) provides some evidence that it may. Similarly, the enrollment effects of a local minimum wage may be different than the effects of minimum wages imposed at coarser levels (i.e., state minimum wages) because of potentially heterogeneous responses to minimum wages due to differences in average wages across cities.

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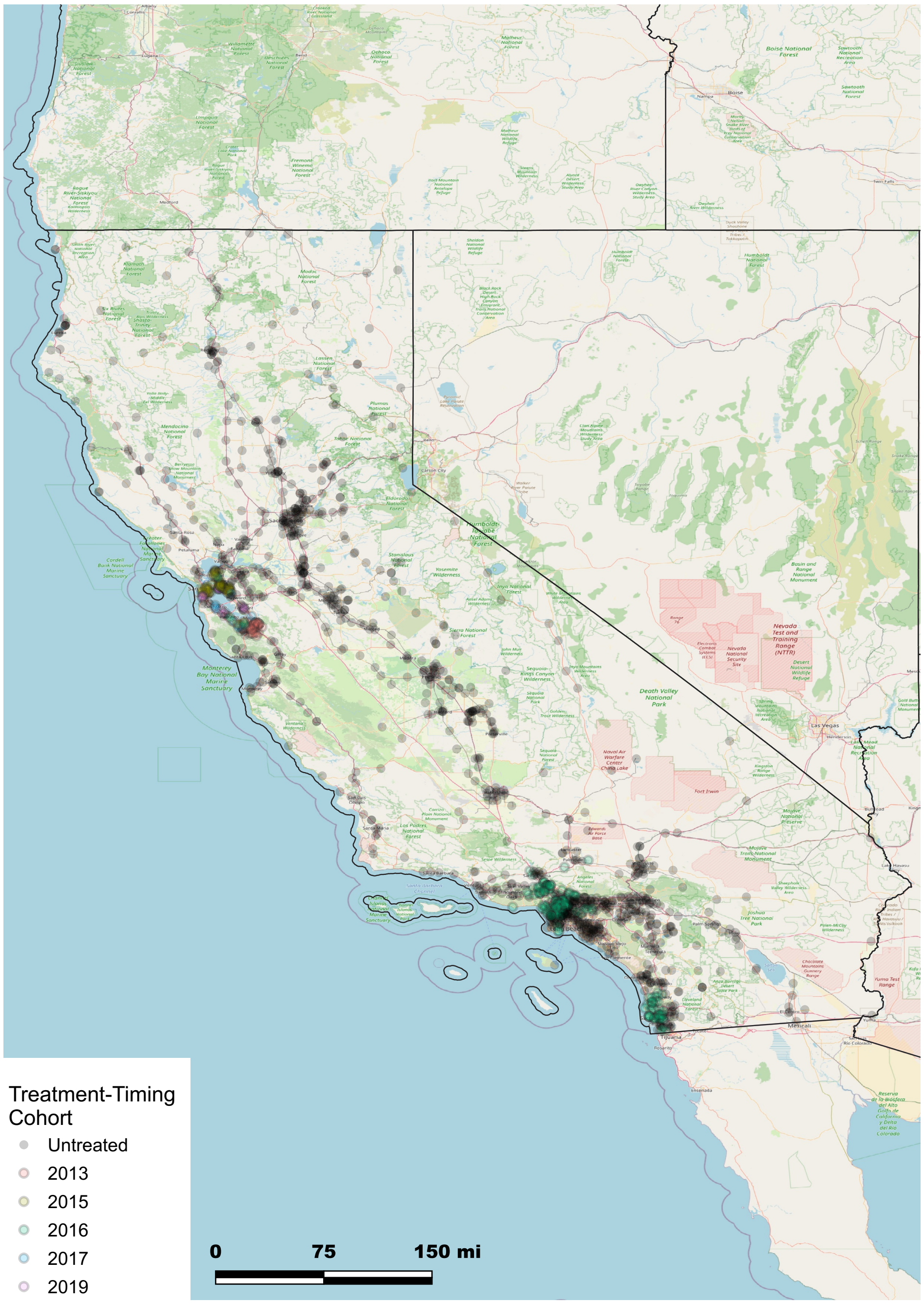
9 Chapter 2 Appendix

Figure 12: Geographic Distribution of California High Schools



Created using data from the California Department of Education, minimum wage data from the UC Berkeley Labor Center and Vaghul and Zipperer (2016), and map data from OpenStreetMap. Each point plots a single school if it is observed in the panel at least once. The color of each dot represents the treatment-timing cohort the school is assigned to.

Table 3: Cities Treatment-Timing Cohort, Minimum Wage Adoption, and Implementation

Treatment-Timing Cohort:	City	Adopted	Effective
2013	San Jose	06/11/12	11/03/13
2015	Berkeley	10/06/14	01/10/14
	Emeryville	02/06/15	01/07/15
	Mountain View	09/10/14	01/07/15
	Oakland	04/11/14	01/03/15
	Richmond	03/06/14	01/01/15
	San Francisco†	04/11/14	01/05/15
	Sunnyvale	28/10/14	01/01/15
2016	Acton*	03/11/15	01/07/16
	Altadena*	03/11/15	01/07/16
	Castaic*	03/11/15	01/07/16
	Del Aire*	03/11/15	01/07/16
	East Los Angeles*	03/11/15	01/07/16
	El Cerrito	17/11/15	01/07/16
	Florence-Graham*	03/11/15	01/07/16
	Hacienda Heights*	03/11/15	01/07/16
	La Crescenta-Montrose*	03/11/15	01/07/16
	Lennox*	03/11/15	01/07/16
	Los Angeles	03/11/15	01/07/16
	Malibu	28/03/16	01/07/16
	Palo Alto	13/11/15	01/01/16
	Pasadena	14/03/16	01/07/16
	Rowland Heights*	03/11/15	01/07/16
	San Diego	28/07/14	11/07/16
	Santa Clara	22/09/15	01/01/16
	Santa Monica	12/01/16	01/07/16
	South San Jose Hills*	03/11/15	01/07/16
	South Whittier*	03/11/15	01/07/16
	Stevenson Ranch*	03/11/15	01/07/16
	Sun Village*	03/11/15	01/07/16
	View Park-Windsor Hills*	03/11/15	01/07/16
	West Athens*	03/11/15	01/07/16
	West Whittier-Los Nietos*	03/11/15	01/07/16
	Westmont*	03/11/15	01/07/16
	Willowbrook*	03/11/15	01/07/16
2017	Cupertino	04/10/16	01/01/17
	Los Altos	27/09/16	01/01/17
	Milpitas	21/02/17	01/07/17
	San Leandro	19/09/16	01/07/17
	San Mateo	15/08/16	01/01/17
2019	Alameda	16/10/18	01/07/19
	Daly City	14/01/19	13/02/19
	Fremont	05/02/19	01/07/19
	Redwood City	09/04/18	01/01/19

Created using data from the UC Berkeley Labor Center and city websites. For cities and places with multiple minimum wages, this table reports the effective date of the earliest minimum wage in the final column.
† San Francisco is the only city or place in the data set with a local minimum wage prior to the effective date listed here.
* Census-Designated Place in Los Angeles County.

Table 4: Summary Statistics in 2010 by Treatment-Timing Cohort

	Treatment-Timing Cohort					
	2013	2015	2016	2017	2019	∞
Unemployment Rate	0.09	0.08	0.08	0.07	0.07	0.09
	(.)	(0.02)	(0.02)	(0.01)	(0.01)	(0.05)
Participation Rate	0.68	0.68	0.64	0.65	0.68	0.62
	(.)	(0.05)	(0.04)	(0.04)	(0.01)	(0.10)
Population 15 to 17	37,885.00	6,510.14	10,161.32	2,617.80	4,491.50	1,983.36
	(.)	(6,768.93)	(32,442.36)	(811.16)	(2,780.85)	(2,966.49)
Median Earnings	38,854.00	41,817.71	36,102.08	58,226.60	42,348.00	31,458.04
	(.)	(10,458.82)	(13,392.01)	(24,993.78)	(6,505.23)	(12,122.17)
Observations	1	7	25	5	4	498

[1] This table reports sample averages and standard deviations, in parentheses, for the main data set for 2010. Observations are at the city level. Untreated observations are denoted by ∞.
[2] Created using data from the ACS 5-year data tables.

Table 5: Callaway and Sant'Anna Estimates

	Treatment-Timing Cohort				
	2013	2015	2016	2017	2019
2011	-13.687***	0.669	6.558*	20.614*	-3.365
	(1.318)	(4.694)	(3.679)	(10.557)	(12.656)
2012	7.910***	-4.411	12.916***	0.917	1.848
	(1.611)	(4.020)	(3.412)	(14.473)	(3.839)
2013	2.770***	4.374*	5.334**	7.399	2.541
	(0.937)	(2.658)	(2.166)	(9.219)	(6.980)
2014	5.249***	0.899	5.023	0.752	4.585
	(1.479)	(3.246)	(3.098)	(6.649)	(4.040)
2015	6.920***	4.219	5.265***	9.796	-5.200
	(2.007)	(2.581)	(1.675)	(10.633)	(5.448)
2016	28.759***	5.208	-4.898**	-9.679	4.711
	(1.944)	(3.278)	(2.190)	(7.497)	(8.576)
2017	35.397***	11.008***	-9.437***	49.212***	9.080
	(2.221)	(3.607)	(3.303)	(12.672)	(6.277)
2018	34.298***	13.837**	-10.105***	47.895***	-10.548***
	(2.850)	(6.160)	(3.074)	(9.146)	(1.747)
2019	39.535***	19.641***	-15.535***	48.993***	8.251**
	(2.839)	(7.037)	(4.384)	(6.486)	(3.303)

[1] This table reports the raw coefficients from Figure 7 prior to rescaling by pre-treatment enrollment. Columns indicate the treatment-timing cohort, g, and rows indicate the year, t. When $t < g$ the estimates are placebos and when $t \geq g$ the estimates are treatment effects. Observations vary by cell.
[2] * significant at 10%, ** significant at 5%, *** significant at 1%.

Table 6: Callaway and Sant'Anna Estimates, Conditional

	Treatment-Timing Cohort				
	2013	2015	2016	2017	2019
2011	-14.360***	1.288	5.779	19.877*	-4.256
	(2.458)	(4.678)	(3.930)	(10.517)	(13.023)
2012	1.022	-3.712	-54.013	-1.817	-1.342
	(6.622)	(4.319)	(38.410)	(15.642)	(4.062)
2013	-0.223	3.715	-51.177	7.204	2.391
	(8.361)	(2.962)	(36.724)	(8.803)	(6.514)
2014	0.608	1.732	0.777	-0.301	3.646
	(8.198)	(3.653)	(17.771)	(7.268)	(4.688)
2015	4.125	3.823	-4.167	11.043	-4.043
	(8.163)	(2.450)	(16.847)	(10.637)	(5.653)
2016	20.501	4.260	-60.879*	-9.647	4.712
	(29.362)	(3.066)	(33.015)	(7.709)	(8.413)
2017	32.802***	10.671***	21.854	49.572***	9.263
	(11.670)	(3.471)	(28.270)	(13.496)	(6.767)
2018	29.263	13.131**	-9.674	48.453***	-10.552***
	(21.279)	(5.656)	(25.683)	(9.514)	(1.898)
2019	33.723	18.569***	-36.992	50.069***	8.640**
	(27.316)	(6.839)	(28.862)	(6.989)	(3.394)

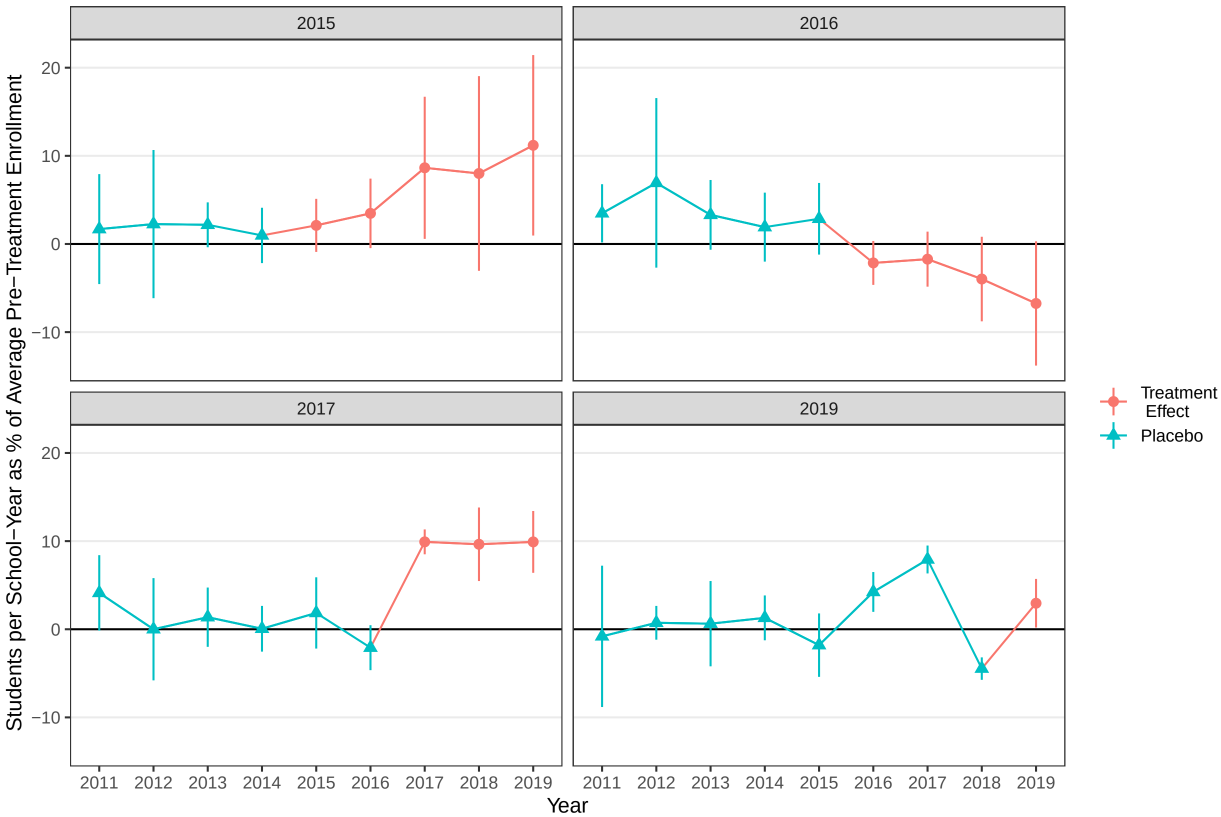
[1] This table reports the raw coefficients from Figure 8 prior to rescaling by pre-treatment enrollment. Columns indicate the treatment-timing cohort, g, and rows indicate the year, t. When $t < g$ the estimates are placebos and when $t \geq g$ the estimates are treatment effects. Observations vary by cell.
[2] * significant at 10%, ** significant at 5%, *** significant at 1%.

Table 7: Triple Difference-in-Differences Estimates

	Treatment-Timing Cohort				
	2013	2015	2016	2017	2019
2011	-19.78***	1.417	5.700	21.84	-9.628
	(-12.82)	(0.22)	(1.45)	(1.77)	(-0.73)
2012	6.171***	-8.859***	13.60***	-5.522	3.288
	(3.82)	(-3.67)	(3.52)	(-0.35)	(0.73)
2013	-2.209	2.505	0.600	3.090	4.188
	(-1.66)	(0.54)	(0.15)	(0.22)	(0.49)
2014	-0.990	-2.852	4.680	-14.47	-0.579
	(-0.63)	(-1.01)	(1.49)	(-1.48)	(-0.10)
2015	0.182	2.450	7.538*	0.234	-10.77
	(0.11)	(0.91)	(2.27)	(0.02)	(-1.57)
2016	20.33***	-1.809	-3.850	-25.41***	2.519
	(13.80)	(-0.44)	(-1.55)	(-3.36)	(0.47)
2017	23.08***	1.531	-4.944	47.30**	9.526
	(13.92)	(0.36)	(-1.23)	(3.00)	(1.53)
2018	20.13***	-0.825	-4.034	45.50*	-16.61***
	(11.42)	(-0.15)	(-0.89)	(2.43)	(-4.69)
2019	21.37***	3.874	-6.323	46.36***	9.049**
	(12.26)	(0.69)	(-0.97)	(3.47)	(2.68)

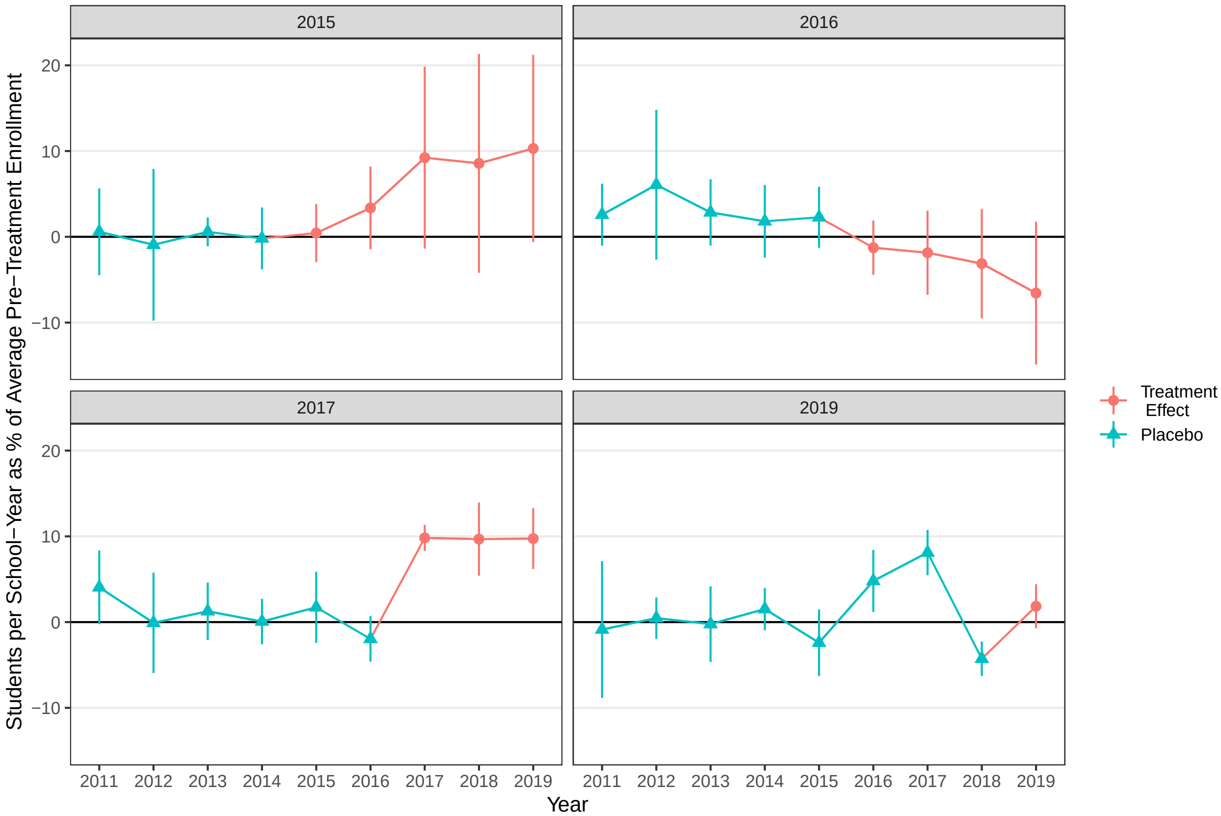
[1] This table reports the raw coefficients from Figure 9 prior to rescaling by pre-treatment enrollment. Columns indicate the treatment-timing cohort, g, and rows indicate the year, t. When $t < g$ the estimates are placebos and when $t \geq g$ the estimates are treatment effects. Observations vary by cell.
[2] * significant at 10%, ** significant at 5%, *** significant at 1%.

Figure 13: City-Level Estimates, Unconditional



This figure plots the Callaway and Sant'Anna (2021) $ATT(g,t)$ estimates similar to Figure 7, but observations are at the city level. The year at the top of each subplot denotes the treatment-timing cohort. The 2013 treatment-timing cohort is excluded as it is composed of a single city. Blue points are estimated placebo effects prior to treatment and red points are the estimated treatment effects. The vertical bars attached to each estimate are the corresponding 95% confidence intervals. All of the parameter estimates are plotted as a percent of their treatment-timing cohort's city level average pre-treatment twelfth grade enrollment. The estimates in this figure do not include any controls and standard errors are clustered at the city level.

Figure 14: City-Level Estimates, Conditional



This figure plots the Callaway and Sant'Anna (2021) $ATT(g,t)$ estimates similar to Figure 14, conditioning on population aged 15 to 17 relative to the number of schools in the city, but observations are at the city level. The year at the top of each subplot denotes the treatment-timing cohort. The 2013 treatment-timing cohort is excluded as it is composed of a single city. Blue points are estimated placebo effects prior to treatment and red points are the estimated treatment effects. The vertical bars attached to each estimate are the corresponding 95% confidence intervals. All of the parameter estimates are plotted as a percent of their treatment-timing cohort's city level average pre-treatment twelfth grade enrollment. Standard errors are clustered at the city level.

Table 8: School Racial Composition by Treatment-Timing Cohort

	Treatment-Timing Cohort					
	2013	2015	2016	2017	2019	∞
Share Hispanic	0.55	0.48	0.68	0.24	0.27	0.48
	(0.31)	(0.27)	(0.28)	(0.13)	(0.16)	(0.27)
Share Black	0.03	0.20	0.09	0.03	0.06	0.05
	(0.02)	(0.18)	(0.13)	(0.05)	(0.05)	(0.07)
Share White	0.19	0.08	0.13	0.22	0.24	0.34
	(0.18)	(0.11)	(0.18)	(0.13)	(0.12)	(0.25)
Share Asian	0.17	0.15	0.05	0.36	0.24	0.06
	(0.21)	(0.19)	(0.08)	(0.21)	(0.15)	(0.10)
Observations	205	507	1659	90	125	9873

[1] This table reports sample averages and standard deviations, in parentheses, by treatment-timing cohort for several school level racial share variables in the main data set. Untreated observations are denoted by ∞. The number of observations is presented in the last row of each panel in the table.