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EFFECTS OF RECENT MINIMUM WAGE POLICIES IN CALIFORNIA AND NATIONWIDE: RESULTS FROM A PRE-SPECIFIED ANALYSIS PLAN

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ABSTRACT

We analyze the impacts of recent city minimum wage increases in California and nationwide, following a pre-analysis plan (PAP) registered prior to the release of data covering two years of minimum wage increases. For California cities we find a hint of negative employment effects. Nationally, we find some evidence of disemployment effects for teens, but not young adults or high school dropouts. City-specific analyses provide limited evidence of adverse effects on the share low-income, but the pooled city analysis does not; the national analysis generally finds no impact on the share low-income, with one exception that may reflect prior trends.

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Introduction

Many U.S. cities have recently increased their minimum wages, especially in California. This paper reports results from carrying out analyses of the impacts of these city minimum wages, as specified in a pre-analysis plan (PAP) that was registered on Open Science Framework prior to the release of data covering two years of minimum wage increases.

Our PAP describes an empirical investigation of the effects of city (and state and county) minimum wages on employment and poverty (and, secondarily, wages and earnings), using data from the American Community Survey (ACS). As of the date of registering this PAP, on September 24, 2019, work had been done using ACS data through 2017. Because we use a lagged minimum wage variable, these data covered the minimum wage variation we study through 2016.[1] This PAP was filed prior to the release of ACS data for 2018, with ACS 1-year summary files for 2018 released on September 26, 2019 and ACS public-use microdata for 2018 released on November 14, 2019. The data for 2019 were released approximately one year later. Our PAP committed to carry out our analyses after the release of the 2018 data (in 2019) and then again after the release of the 2019 data (in 2020). We committed to releasing an initial working paper (Neumark and Yen, 2020) using the 2018 data,[2] and a new revised paper (this one), to be submitted for publication, using the 2019 data as well. These additional two years of data substantially expanded the amount of data available on city minimum wage increases.[3,4]

[1] Throughout, therefore, when we refer to data on minimum wage increases before or after registration of our PAP, we are referring to lagged increases. The reason is explained fully below, and has more to do with the timing of measurement in the ACS data we use than with lagged adjustments to minimum wages.
[2] This was a provisional paper only, not to be submitted for publication.
[3] Moreover, we committed to do the analysis with the 2019 data in two ways – using all of the data (i.e., extending the data used in the analysis through 2017 reported in our PAP), and then isolating the identifying information on the effects of minimum wages to include only the two years of variation that were not available when the PAP was written. The latter avoids any issues of specification search driven by finding particular minimum wage effects in the data available prior to filing the PAP, at the cost of using less data. The tables and figures in the main text use the first approach; the estimates isolating the effects estimated from the post-registration data are reported in Online Appendix A.
[4] All of the code used in this PAP is also registered as part of the PAP. With each update, the code was simply updated to accommodate the additional data, barring any unforeseen complications or errors that we discovered subsequently. In fact, as Neumark and Yen (2020) noted, we discovered a couple of coding errors at the first update. Our resolution was always to change the code to conform with what we said we were going to do, rather than ever modifying the planned analysis.

Most of our analysis focuses on California, where there have been numerous city minimum wages passed in recent years. California is potentially a good setting for a credible research design, because the within-state comparisons account for many other changes, including in-state minimum wages. It is also very important from a policy perspective, because California is, in a sense, ground zero for city minimum wages, with many cities having adopted minimum wages – and high minimum wages – in recent years. Nonetheless, part of the analysis focuses on city minimum wages nationally.

While the effects of minimum wages on employment are the subject of most research and policy debate, the effects on poverty (or low-income families more generally) are of greater interest since the goal of most minimum wage advocates is to reduce poverty and help low-income families. We cover both outcomes. Finally, we also explore the effects on wages and earnings; these analyses are not primary, but help to assess whether minimum wages in fact push up wages.

Related Prior and Current Work

There are some precedents to using pre-specified analysis plans (PAPs) to estimate the effects of minimum wages, with the goal of reducing or eliminating specification searches or data mining that could influence the reported estimates, and there is also some concurrent related work. As David Levine – then editor of *Industrial Relations* – wrote in an introduction to what was supposed to be a mini-symposium on using this approach, "Published results in the social sciences are potentially biased due to researchers' specifications searches. That is, unconscious and conscious biases in specification searches can lead to "author effects," where one team of researchers consistently finds results larger or smaller than another team" (p. 161).[5]

In the one paper resulting from the *Industrial Relations* project, Neumark (2001) pre-committed to a research design to study the effects of the U.S. federal minimum wage increases in October 1996 and September 1997.[6] The project used the October, November, and December CPS files. The earliest

[5] Another component of this mini-symposium was a pre-commitment by the journal to publish the paper based on an evaluation of the research design, to avoid biases introduced by editors' or referees' views of the findings.
[6] The issue of *Industrial Relations* in which this introduction appeared was supposed to be a symposium, but no other invited researchers chose to participate.

relevant file (October 1996) was released to the public at the end of May 1997, and the research design (what we now call a pre-analysis plan) was submitted to the journal before this date. (Data from 1995 are used as the baseline.) The paper concluded that some of the inferences are fragile – perhaps attributable to discarding data except on the minimum wage increases following the pre-specification of the research design. Overall, though, there was evidence of disemployment effects of minimum wages where they would be most expected – for some younger workers (16-24 year-olds but not teens), and for less-educated workers – with the negative effects occurring with a lag.

Campolieti et al. (2006) conducted an analysis of minimum wage increases in Canada which they described as "in the spirit" of Neumark (2001). The authors readily acknowledged that their paper was not a "pure" pre-specified research design, as the paper included earlier data. The sense in which they argued that it was a pre-specified research design was that it committed to following the specifications in Neumark (2001), although it also included a few other specifications that the authors are clear to delineate from the original specifications. Campolieti et al. reported employment elasticities with respect to the minimum wage ranging from −0.17 to −0.44 for youths (aged 20-24 or 16-24), but no statistically significant evidence of disemployment effects for teenagers, and mixed evidence for those with at most a high school education. They indicated the results were quite robust, and that the larger negative estimates resulted from accounting for lagged adjustments.

Wang and Gunderson (2011) did a similar analysis for the effects of minimum wages in China, studying data from 2000-2007, but basing their analysis on an earlier research design. They found negative employment effects in slower-growing regions, larger negative effects in non-state-owned enterprises, and larger negative effects with lags. They found no adverse employment effects in faster-growing regions, and positive effects in state-owned enterprises in these regions. Given that their analysis focused to some extent on regional differences and state- vs. non-state-owned enterprises – issues that were absent from the earlier research design on which their study is based – calling this paper a pre-specified research design (as the title does) might be viewed as more of a stretch than Campolieti et al. (2006).

There is a tradeoff between a pure pre-specified design using post-treatment data from after the research design is specified, and the kinds of analyses in the Campolieti et al. and Wang and Gunderson studies. When researchers commit to a pure pre-specified research design, they are generally throwing out a lot of earlier data, which comes at the cost of precision (and the ability to assess robustness).[7] In contrast, when researchers use earlier data they always have the ability to learn something about how specification choices affect the results – either from their own work or the work of other researchers – at least in earlier data. The idea of Campolieti et al. to use more data but to commit to *someone else's* pre-specified research design is a creative way to try to balance these costs and benefits – not as compelling as a pure pre-specified design, but still potentially more reliable than a paper that presents specifications not drawn directly from pre-specified research designs. Of course, the benefits can be overstated, as subsequent researchers may come up with alternative and potentially more convincing ways to identify minimum wage effects.[8]

Finally, in a recent pair of papers Clemens and Strain (2017, 2019) presented analyses of the employment effects of state minimum wages based on CPS data through 2015 and committed to apply the same analysis to the effects of state minimum wage increases in 2016 (and some in late 2015) through 2019 (in their 2017 paper), and then present evidence through 2017 (in their 2019 paper).[9] The results in the latter paper (the only one that analyzes data subsequent to the pre-specification) indicated that large minimum wage increases reduced employment of low-skilled individuals by just over two percentage points, while the estimated effects of smaller minimum wage increases are more variable, and the evidence for inflation-indexed increases (a recent development in some U.S. states) pointed more to positive effects. They suggested that the differences between larger and smaller increases may also have to do with the timing of effects, as the larger increases occurred earlier and hence the negative

[7] This is not always the case, as there are sometimes new minimum wages implemented, such as in Germany in 2015 and the United Kingdom in 1999.
[8] See, e.g., the arguments in Allegretto et al. (2011) and Dube et al. (2010).
[9] They also noted that their analyses of future data for 2017-2019 might include adaptation to account for other local labor market developments regarding immigration, trade, and technology, while also presenting straightforward extensions of their original pre-specified analyses.

employment effects could reflect lagged effects – something they can only sort out with more data.

Overall, then, all four of these papers (counting the second Clemens and Strain paper only) found some evidence of disemployment effects of minimum wages. The papers differed in the extent to which they used pure pre-specified research designs, however, so one may not want to draw strong conclusions from them about the evidence from such designs.

Our present paper includes key features of some of these prior pre-specified analyses, with some differences. Similar to Clemens and Strain's work and Neumark (2011), our study is a "pure" pre-specified design in the sense that we break out and report results for the "post-registration" minimum wage increases.[10] There are some key differences relative to both their work and the other papers. First, we followed what is becoming standard practice in the experimental literature, and registered our pre-analysis plan (PAP) prior to the availability of data on the effects of a set of minimum wage increases. Second, our focus is different, with a particular emphasis on local minimum wages in the many cities in California that adopted them in the late 2010s. And third, our paper is the first that incorporates analysis of the distributional effects of minimum wages into a pre-specified research design.

City Minimum Wages

Table 1 lists California cities with minimum wages. In all cases, the cities included in Table 1 are large enough to have data in the ACS 1-year files, for which the criterion is a Census place with a population greater than 65,000. The state minimum wage is shown in the top row, followed by information on city minimum wages. We first show the date of the increase or new implementation in a year, if any, followed by the minimum wage level. The table ends in 2018, which will be the last year of minimum wage data used in our analysis. (The same is true for the tables and figures that follow.)

Figure 1 makes clear the rising number of city minimum wages in California. The figure plots the state minimum wage (line) and the value of each city minimum wage. The first city minimum wage was

[10] However, also like their work, we had data on increases just prior to the registration date prior to committing to our research design – a deviation from the kind of pure pre-specified design one increasingly sees in experimental research.

in San Francisco, but beginning in 2015 many more cities jump into the fray – with 14 cities with minimum wages as of the end of 2017, 13 of which increased their minimum wages in 2018. As this figure shows, the additional two years of data we study since filing our PAP provide potentially useful policy variation, since many of the California city minimum wages are very recent.

To give a sense of the share of population covered by minimum wages in different cities, Figure 2 weights each city's minimum wage by the log of the population aged 16 and over, based on an average of ACS data from 2005 to 2018.

Table 2 shows all city and county minimum wages nationally that have been enacted since 2012. The table shows that 2016 saw a large increase in the number of cities with minimum wages, and a number of cities also implemented minimum wages in 2017 or increased their minimum wages. Table 3 shows the longer histories for Santa Fe and San Francisco, which passed minimum wages earlier.

Data Issues

For our analysis of employment and poverty, we use ACS 1-year summary files at the Census-place level, allowing an easy mapping to cities – the level at which most local minimum wages are set. In these 1-year files, we obtain measures of employment, poverty status (the share of the population that is poor or below other thresholds we use, based on the family), earnings of full-time year-round workers, citizenship status, race, age, sex, education, and population at the Census place level. The ACS restricts the 1-year data to Census places with populations of greater than 65,000. On a similar basis, ACS data for our subgroups may be suppressed in certain Census places with low populations of those subgroups for confidentiality concerns and statistical reliability. Census places are either incorporated places (legally bounded entities), such as cities, boroughs, towns, or villages, or Census designated places (CDPs), which are statistical entities that can include unincorporated communities, concentrations of population, housing, and commercial structures, identifiable by name, but not within an incorporated place.[11] For simplicity we always refer to these as cities, except in some cases where we are referring explicitly to how Census

[11] See https://www.census.gov/content/dam/Census/data/developers/understandingplace.pdf.

labels these entities.

There are several considerations for classifying the minimum wage variable. Because the ACS reports in one-year intervals, without month identifiers, the minimum wage must be assigned a value for the year. For continuous measures of the minimum wage, we simply average the minimum wage for the year. Additionally, because of the structure of the ACS data and the timing of minimum wage increases, we use a one-year lag of the minimum wage when estimating effects in the ACS data. Table 1 lists the dates of enactment of California cities' minimum wages. Many cities have changes that take place on July 1, more on January first, and a couple cities on other dates. Thus, if we assign the average minimum wage for the current year (or assign an increase in the current year, when we use a dummy variable), it is possible that a good deal of the data actually come from the period prior to the minimum wage increase.

Similarly, ACS income-related questions refer to the past twelve months. (See Table 4.) For example, when using the ACS 1-year summary files, poverty status is based on the past twelve months. For these types of variables, it is even more likely that the data were generated prior to the current year's minimum wage increase. In one analysis, we study the effects of minimum wages on wages using converted microdata. For this analysis, we attempt to construct a more accurate hourly wage measure as our outcome of interest, but this relies on even more variables that are reported in the past twelve months.

Thus, in our analyses we always one-year lags of the minimum wage variables. Using a one-year lag will reduce the incorrect classification of untreated observations as treated – a classification error that would generate bias towards finding no effect. Of course, if the policy effect occurs precisely in the month of treatment, then misclassification in either direction (temporally) will generate bias towards zero. However, there is in fact some reason – and some past evidence – suggesting that minimum wage effects could occur with a lag.[12] Thus, there is much less likely to be a bias towards finding no effect generated from lagging the minimum wage variable. We believe that, especially due to the nature of the ACS data,

[12] See, e.g., Neumark and Wascher (1992), Neumark et al. (2004), and Cengiz et al. (2019) on employment effects. In an analysis of distributional effects, in standard two-way fixed effects specifications, Dube (2019) finds effects that occur with a three-year lag, although that may be an outlier.

lagging the minimum wage variable is essential.[13]

Finally, we define a relative minimum wage variable based on an average wage in the denominator that is lagged by two years, since average wages are computed using data over the past twelve months. This ensures that the average wage we are using is not directly influenced by the lagged minimum wage, and hence provides a better measure of wage levels and the "bite" of the minimum wage, uninfluenced by minimum wage increases.

California Analysis

City-specific analyses

For the analysis of city minimum wages in California, we first report synthetic control analyses for each city. The analyses cover employment rates for teens (ages 16-19), youths (ages 16-24), and high school dropouts (ages 25-64). We also report the same types of analyses for the share of individuals below 50% of the poverty line, the poverty line, and 150% of the poverty line.[14]

We match on the outcome variable for each analysis in the pre-treatment period for each pre-treatment year. We do not add in matching for additional covariates, as any covariates become irrelevant when using the entire pre-treatment path of the outcome variable (Kaul et., al 2015). Additionally, to take into account the lagged minimum wage effect, we simply lag our treatment year by one in each synthetic control analysis. We report the results for each city in Tables 5A-5M; we report results for each post-treatment year, and the pooled estimate.[15] Pooled estimates are obtained by averaging each post-treatment yearly estimate across the post-treatment years. "Group population" in Tables 5A-5M represents the average sample sizes of the specified group between 2006-2019, in the treated city.[16] Note that Tables 5J-5M cover cities or which there were no minimum wage increases in the data at the time we registered our

[13] This has not, however, been done in recent analyses of local minimum wages using the ACS data (Godøy and Reich, 2019; Clemens and Strain, 2015).
[14] In the ACS, poverty is not calculated for those in group or institutional quarters (such as prisons or dormitories).
[15] Note that there are some blank cells when data were suppressed. Also, San Francisco is omitted from this analysis (as well as that in Tables 6A-7C) because these analyses require pre-treatment observations, which are not available in the data given how early San Francisco implemented its minimum wage.
[16] The only exception is for two cities (Palo Alto and Milpitas) where there is missing data. For these cities, the years are 2011-2019 and 2007-2019, respectively.

PAP.

The inference procedure follows the placebo analysis outlined in Abadie et al. (2010), where we run the synthetic control analysis on each city in the donor pool. (The control cities are listed in the notes to Table 1.) Two examples – for a very large unit (LA County), and a very small unit (Santa Clara) – are shown in Figures 3 and 4.

The results suggest a few conclusions, which we discuss here on a city-by-city basis, and then summarize graphically below.

Turning first to the employment effects, there is sometimes evidence of a negative effect that is sizable (larger than 0.05 in absolute value) but not very precisely estimated and hence rarely statistically significant, especially for the post-registration 2018 data (and less so for 2019): in Berkeley (5A) for teens, youths, and high school dropouts in 2018 and youths in 2019; in Palo Alto (5D) for teens in 2018; in San Diego (5F) for teens in 2018; in Santa Clara (5K) for teens in both years; in Sunnyvale (5H) for teens and youths in 2018 (significant for teens); in Milpitas (5J) for teens and high school dropouts in 2018; and in San Mateo (5M) for teens in 2018.

Second, there are some post-registration estimates that are positive and sizable, mostly statistically insignificant: in Berkeley (5A) for high school dropouts in 2019; for Mountain View (5B) for high school dropouts in 2019 (significant); in Oakland (5C) for high school dropouts in 2018; in Richmond (5E) for teens and youths in both years (significant for youths in 2019) and high school dropouts in 2019; in San Diego (5F) for high school dropouts in 2019; in Sunnyvale (5H) for high school dropouts in 2018 and all three groups in 2019 (significant for youths); in Milpitas (5J) for teens and youths in 2019; in San Jose (5L) for high school dropouts in 2019; and for San Mateo (5M) for teens in 2019.

Third, there are only three pooled estimates (combining minimum wage increase pre- and post-registration) that are statistically significant at the 10% level. All three are positive – for Mountain View

(5B) for youths and high school dropouts, and for Richmond (5E) for youths.[17]

For the effects on being below the poverty line (or 50% or 150% of the poverty line), the estimates are similarly variable and often imprecise, and also vary in sign. However, for the pooled pre- and post-registration data, there are multiple (six) statistically significant estimates consistent with increasing the share low-income: for the 50% threshold in Berkeley (5A) in 2019; for the 150% threshold in Richmond (5E) in 2019; for the 50%, 100%, and 150% thresholds in Santa Clara (5G) in 2018; for the 50% threshold in San Leandro (5K) in 2019. There is only one statistically significant estimate in the direction of reducing the share poor or low income: for the 150% threshold in San Leandro (5K) in 2018.

Thus, there is no clear evidence on the sign of the employment effects in the city-by-city analyses for California. There is a somewhat clearer message on the distributional effects; while there is virtually no evidence of poverty (low-income) reductions, there is some evidence of adverse effects, although it arises in a relatively small number of cases. Overall, the imprecision in the estimates suggests that analysis from data pooled across California cities may be more valuable. We turn to this type of evidence below.

Prior to doing so, however, we report some other summary measures of the results in Tables 5A-5M. Because we have reported results for individual cities, and because minimum wage increases vary in size, it is difficult to interpret the overall evidence. To help interpret the estimates, we also graph the estimated employment effects and poverty effects against the size of the minimum wage increase – computed over the entire post-treatment period. These graphs appear in Figure 5. For employment, if larger minimum wage increases were associated with larger employment declines, the lines would be downward sloping. The lines for teenagers and youths are nearly flat, and the line for high school dropouts is upward sloping. For the poverty thresholds, if a higher minimum wage reduces the share poor

[17] The synthetic control analyses that isolate the post-registration variation (presenting a pooled estimate over the post-registration years) are reported in Online Appendix A. Note there are not alternative tables for the small number of cities for which all of the variation was post registration. In these alternative tables, we report the estimates for each year separately, and also add the estimate of the average effect over only the "post-registration" years (2018-2019), based on 2017 and 2018 minimum wage increases.

or low-income, the lines should be downward sloping. Only the line for the 150% threshold has a noticeable downward slope – and then not by much. The slopes are generally near zero.

Finally, we also graph the estimated employment effects against the poverty effects, to see if larger estimated employment declines are associated with larger increase (or smaller decreases) in poverty. These estimates are also computed over the entire post-treatment period. We show these for all three groups for which we estimate employment effects, but we expect a stronger relationship for the high school dropout employment rates, since the employment effect for this group is more strongly linked to family income.[18] Figure 6A shows these graphs for the poverty threshold, while Figures 6B and 6C show them for the 50% and 150% thresholds as well.

Note that these figures also provide a nice summary of the previous estimates, which span many tables, by showing the employment and poverty estimates by quadrant. Thus we see, for example, that most of the employment estimates for teens are negative or very close to zero. This is less true for young adults and high school dropouts; indeed for the latter, the estimates tend to be more positive. And we see that the estimated effects on the share in poverty and below 50% of the poverty line tend to be positive, while there is no clear direction of the evidence for the share below 150% of the poverty line.

Looking at the relationships between the estimated employment effects and the estimated poverty effects by city, in all cases but one we find a negative relationship between the two estimates; we plot the regression lines. That is, when the estimated employment effect is less positive or more negative, the estimated effect on the share poor (or below the other thresholds) is less negative or more positive (i.e., adverse). Moreover, most of the regressions lines fit to these scatterplots go through a point fairly close to (0,0), implying, for example, that evidence that the minimum wage decreased employment is associated with an increase in the share poor or low-income, and vice versa. Thus, these findings suggest that, although the estimated employment and poverty effects are generally imprecise, we are not just getting

[18] Our PAP inadvertently referred to employment effects for those defined as poor as well. However, we made an earlier decision to omit these results, since poverty and employment cannot be independently defined; this decision was reflected in our pre-registered code.

noise, as we would expect larger employment declines to be more harmful to low-income families. Finally, Figures 6A-6C indicate (and the preceding discussion implies) that there are a number of cities with negative estimated employment effects and estimated increases in the share poor or low-income (the lower-right quadrant).

Pooled analyses

We next conduct empirical analyses that continue to estimate the effects of discrete minimum wage increase events, but in a pooled analysis weighted by city population. This analysis closely follows the specification and approach in Allegretto et al. (2018) (who focused on a small set of cities), except for a few modifications. First, we use a one-year lag modification of the treatment year and corresponding pre- and post-treatment years, for reasons described above. Additionally, we modify the "Jump estimate" to account for partial year implementations, as described below. Finally, we also weight the regression by the population of the group studied. For each outcome, we also show estimates on the subset of observations with one or two additional post-treatment years.[19]

In the tables for this analysis (beginning with Table 6A), the key estimates are highlighted. The "Jump estimate" is the shift in the intercept following the minimum wage increase. (For the year of implementation, if the minimum wage was not implemented on January 1, we set the "dummy" equal to the proportion of the year for which the minimum wage prevailed, instead of 1.) The "Post-treatment trend" estimate is the estimated linear trend in the employment rate subsequent to the initial increase, relative to the initial trend. Tables 6A-6C show the estimates for employment effects. Tables 7A-7C have the same structure, but the outcome is the poverty rate or other low-income thresholds.[20]

For employment, Table 6A shows that the jump estimate is negative for teens for the maximal number of minimum wage increases looking out two or four years, but not three. (These columns have shaded headings; the other columns give the comparable estimates for the shorter-term treatment effects

[19] When we do this, we also show the estimates for the same subsample of observations, without the post-trend term or corresponding observation added, so that one can compare results for the same treatment cities using the different specifications.
[20] See Online Appendix A for parallel analyses that isolate the post-registration variation.

for the observations for which the longer-term treatment effects are defined.) The estimates in column (3), (5), and (6) are significant at the 5% or 10% level based on clustered standard errors but not the bootstrapped p-values. (See the table notes for explanation.)[21] The Post-treatment trend is always negative for teens. It is not statistically significant, except in column (5).

For youths (Table 6B), the corresponding two jump estimates (in columns (1) and (6)) are also negative but not statistically significant. The post-treatment trend is negative for the cities with three or four post-treatment years; one of these estimates is significant at the 10% level based on the clustered standard error, but not the bootstrapped p-value. For both teens and young adults there is some evidence of positive pre-trends associated with adopting a higher minimum wage, which could make it more difficult to detect negative employment effects.

For high school dropouts (Table 6C) the estimated employment effects in the three highlighted columns are also negative in two out of three cases (this time for one and two years post-treatment) but not statistically significant. The post-treatment trends are positive, and in one case significant at the 5% (or less) level based on both inference procedures.

Turning to the poverty thresholds in Tables 7A-7C, there is no consistent evidence of an effect one way or the other. Both the jump effects and post-treatment trends are small, insignificant, and vary in sign. For all three cases there are negative and significant pre-trends associated with minimum wage increases, which could potentially obscure evidence of increases in the shares with low-income.

Overall, then, for employment there is fairly consistent evidence pointing to negative effects for teens and youths, although most of it is not statistically significant. The estimates for high school dropouts are more ambiguous. There is no consistent evidence of an effect on the share below the poverty line or other low-income thresholds.

We next move on to a more standard panel data analysis of the effects of minimum wages, using

[21] We have a fairly large number of groups, but relatively few treated groups (13, 9, or 4 across the columns of Tables 6A-6C and 7A-7C). We read the state of knowledge of how to best calculate the clustered standard errors with a fairly large number of groups but few treated groups as somewhat unsettled, but it is likely that the bootstrapped standard errors would be more accurate (Cameron and Miller, 2015).

a continuous minimum wage variable. In this analysis, we revisit an issue that received more attention in the beginning stages of the new minimum wage research – how to specify the minimum wage variable. Most of earlier work on minimum wages used a ratio of the minimum wage to an average wage (Neumark and Wascher 1992) – often referred to as a "Kaitz index."[22] Typically, specifications using this approach defined the dependent variable in levels rather than logs, so one had to compute an elasticity based on the regression estimate and the means of the dependent variable and the minimum wage variable. More recently, researchers have specified the minimum wage variable in logs – without reference to an average wage – and defined the dependent variable in logs, so that the minimum wage coefficient is the elasticity.

In our view, however, there are reasons – especially in the current context of high minimum wages – to revert to the relative minimum wage specification, and to estimate the specification in levels rather than logs (of a rate, for employment and poverty). Consider first the employment rate. Suppose, as seems simplest, that a change in the minimum wage (relative or absolute) has equal absolute effects regardless of the level of the employment rate. Then using the log of the employment rate can generate quite misleading evidence on the magnitudes of the effects at different "baseline" employment rates. The change from, e.g., 0.9 to 0.8 is much smaller in percentage (or log) terms than change from 0.3 to 0.2, suggesting that that using the level of the employment rate is preferred to the log unless there is a reason the minimum wage has smaller effects at higher levels of the employment rate.

What does this imply in our specific context? In our sample period, the employment rate is higher at the end of the sample period (because of developments since the Great Recession). Large minimum wage increase are also concentrated at the end of the sample period. Together, this implies that using the log of the employment rate could obscure the relationship between the high minimum wage increases at the end of sample period and employment declines – which look smaller, at high employment rates, if we use the log of the employment rate.

Consider next the minimum wage variable. With a relative measure, when the relative measure is

[22] However, the original Kaitz index (Kaitz, 1970) also incorporated information on coverage.

low, a minimum wage increase affects relatively fewer workers, and should have smaller effects on employment. Conversely, when the relative measure is high, a minimum wage increase affects relatively more workers, and hence should have larger effects on the employment rate. Using the log of the relative measure has the opposite effect. When the relative minimum wage is higher, the change in the log of the relative measure is smaller for the same nominal increase in the minimum wage. Thus, using the log of a relative measure obscures the relationship between increases in higher minimum wages (relative to the average wage) and larger employment declines. The same holds for simply using the log of the minimum wage. A $1 increase at a higher minimum wage is smaller in percentage terms, but this may generate larger employment declines. In contrast, using the level of the minimum wage or the level of the relative minimum wage, an equal minimum wage change induces equal changes. This is preferable, but still does not capture the potential for larger employment effects at higher minimum wages, for which one might want to use a convex function of the minimum wage. We do not go this far, but we do revert to using regressions of the levels of the dependent variables (the employment rate, poverty rate, or wages/earnings) on the relative minimum wage (in levels, not logged).

The one issue with using a relative minimum wage variable is that unobserved demand variation that is positively correlated with average wage in the denominator, and with the employment rate, induces a negative relationship. However, the models include an unemployment rate for 25-64 year-olds, which should control for demand variation. Moreover, as noted above, we lag the average wage variable by an extra year.

The estimates in Tables 8 and 9 report results from standard panel data analyses for California. Note that the minimum wage effects in the California analyses, as in the prior analyses, are identified from the within-state variation only. Table 8 is for employment effects, and Table 9 for effects on the shares below different poverty thresholds.[23] In Table 8, the estimated employment effects are always negative. The elasticity is −0.12 for teens, about −0.01 for youths, and −0.03 for high school dropouts,

[23] See Online Appendix A for parallel analyses that isolate the post-registration variation.

although none of the effects are significant. In Table 9, the evidence is stronger. The estimates for the 100% and 150% thresholds point to statistically significant reductions in the share poor or low-income. The elasticities are around −0.24 for the poverty line (the 100% threshold), and −0.13 for the 150% threshold (and −0.17 for the 50% threshold, although that estimate is insignificant).

Compared to the other statewide specifications reported in Tables 6A-6C and 7A-7C, the evidence of negative employment effects is more consistent in Table 8, although in no case is the estimated effect statistically significant. One advantage of Table 8 is that it takes account of the magnitude of the minimum wage increase, while Tables 6A-6C do not. In addition, Tables 6A and 6B report evidence of significant positive pre-trends for teens and youths, which could weaken evidence of disemployment effects in Table 8.

With regard to poverty, in contrast, Table 9 provides quite clear evidence of reductions in the probability of being poor or low-income, whereas Tables 7A-7C generated rather unambiguous evidence of no effect (with estimates small, centered on zero, and varying in sign). However, all three of the prior tables – 7A, 7B, and 7C – showed statistically significant evidence of negative pre-trends prior to these policy changes – i.e., the shares poor or low-income were declining in the cities in California that enacted or increased minimum wages in recent years; this evidence is strongest for the 150% threshold. This suggests that the evidence of reductions in the share poor or low-income in Table 9 may be driven by these negative pre-trends rather than actual reductions caused by the minimum wage increases.[24]

National Analysis

Finally, we conduct more standard national panel data analyses of the effects of local minimum wages. Our panel is constructed from Census places – rather than states, which are the focus of most prior national panel data analyses of the effects of minimum wages. The minimum wage level is defined as the higher of the city, county, or state minimum wage (and the state minimum wage is always the lower

[24] In Online Appendix B, we explore the effects of minimum wages in California cities on wages and earnings. We conclude that the ACS data are most likely not useful for estimating the effects of minimum wages on wages or earnings, because of difficulties in measuring wages in the ACS.

bound).

We begin, to provide a benchmark relative to other literature, with estimates of the effects of state minimum wages in our sample period. We then substitute the local minimum wage, which will be the higher of the state or the local minimum wage. Finally, we add both, in which case we identify the effect of local minimum wages only from the variation that is independent of state minimum wages. In the latter specifications, we enter the state minimum wage (relative to the average wage), and the difference between the city and state minimum wage (also relative to the average wage). In this specification, the estimated coefficient of the state minimum wage is comparable to state minimum wage estimates from state panels. The estimated coefficient of the "city – state" difference is the additional effect of the city minimum wage, and isolates the effects of city minimum wages.

This latter specification is of particular interest in light of concerns raised by Allegretto et al. (2011) and Dube et al. (2010) about the potential correlation between state minimum wages and economic conditions for low-skilled workers.[25] In particular, one response to this potential criticism is to use within-state variation in minimum wages and allow the state minimum wage variation to control for the potential state-level shocks that are correlated with minimum wages (at the state level).[26] If one takes this criticism seriously, then the effects we identify from city minimum wage variation relative to state minimum wage variation might be viewed as more credibly identified. However, we do not want to overstate this; we noted earlier, with respect to the evidence for California only, that there was evidence of negative pre-trends in the shares poor or low-income in the cities in the state that raised minimum wages, relative to other cities in the state. And as discussed in Neumark and Wascher (2017), many approaches to controlling for potential correlations between state minimum wage variation and unmeasured shocks yield

[25] See Neumark et al. (2014a, 2014b), Allegretto et al. (2017), and Neumark and Wascher (2017) for subsequent discussion of these issues.

[26] This parallels the approach in Thompson (2009), although he uses variation in the bindingness of the minimum wage within a state, rather than policy variation. An alternative is to control for state shocks by using within-state variation in the effects of minimum wages on workers directly affected by the minimum wage and low-skilled workers subject to the same shocks (by assumption) but not directly affected by the minimum wage (as in Clemens and Strain (2019), and Clemens and Wither (2019)). One could also saturate the model with state-by-period effects, and hence only identify the effects of the city minimum wages.

evidence of disemployment effects as larger or larger than the standard two-way fixed effects model.

Table 10 reports the estimates for employment.[27] Columns (1)-(3) present the results using the Census place data ("cities"), with state minimum wages assigned. Columns (4)-(6) instead substitute the city minimum wages. Not surprisingly, these estimates are fairly similar, since the prevailing minimum wage in the city is most often the state minimum wage.[28] The estimates indicate a small and insignificant negative employment effect for teenagers, a positive and significant effect for youths (at the 10% level), and a negative but insignificant effect for high school dropouts. The elasticities are generally small.

Columns (7)-(9) include the state minimum wage variable and the city-relative-to-state minimum wage variable. The latter estimates in these specifications isolate the effects of city minimum wages. As we would expect, the estimated effects of the state minimum wage are little changed. The estimated effects of city minimum wages are small for young adults and high school dropouts, but the estimate for teens is statistically significant at the 10% level, with an elasticity of about −0.15.

Note that the teen elasticity is larger (in absolute value) than the estimated effect of state minimum wages, suggesting that city minimum wages may have more adverse employment effects for them. However, they are imprecise, with standard errors on the employment effects of around 0.07 (for teens), suggesting that it is difficult to get enough power to reject the hypothesis of no employment effects for true effects in the lower part of the expected range from other studies that find negative effects of minimum wages (say, around −0.10 elasticities).[29]

Finally, Table 11 reports the estimates for poverty and similar thresholds. Columns (1)-(3) present the results using the Census place data, and provide no clear evidence that state minimum wages affect poverty or low-income shares. The estimates are small and insignificant, although all are negative. In columns (4)-(6), where we substitute the city minimum wage variation, the estimates remain negative, and are statistically insignificant in two of the three cases, with small elasticities (in the range of −0.015

[27] See Online Appendix A for parallel analyses that isolate the post-registration variation.
[28] In the national data, across the different samples and outcomes, only 1.90-1.97% of the observations have the city minimum wage greater than the state minimum wage.
[29] For a summary of preferred estimates from published studies, see Neumark and Shirley (2000).

to −0.041).

In columns (7)-(9) we isolate the effects of city minimum wage variation. In this case, while most of the estimated effects are small and statistically insignificant, the estimate for 150% of the poverty line is statistically significant, with an elasticity of −0.126, consistent with reducing the share with low income.[30] The estimate for extreme poverty (50% of the poverty line) is positive, rather than negative.

Thus, the evidence from the national analysis of city minimum wages suggests there may be some job loss among the least-skilled, although not for all groups, although the evidence is not statistically significant only for teens, and only at the 10% level. There is also evidence that city minimum wages reduced the share below 150% of the poverty line; the estimate for the poverty line is in the same direction, but smaller and insignificant, while the estimate for extreme poverty is the opposite sign. However, recall that Tables 7A-7C indicated that, for California, there was a fairly strong negative pre-trend in the shares poor or low-income (strongest for the 150% threshold), which could generate spurious evidence of reduction in the shares poor or low-income. This is most important for Table 9 – which uses the same California minimum wage increases. However, while Table 11 uses city minimum wages nationwide, California cities still contribute a large share of the variation (across the different samples and outcomes, 33-34 percent of observations with city minimum wage exceeding state minimum wages), and hence the same problem could arise.

Conclusions and Discussion

Our goal in this paper was to pre-specify analyses of city minimum wage increases – mainly in California, but also elsewhere. We registered a pre-analysis plan (PAP) in September 2019, that dictated how we would use American Community Survey (ACS) data released in the fall of 2019 and the fall of 2020 – data that covered 2018 and 2019. Our analyses included city-by-city analyses of minimum wage increases in California, pooled analyses of city minimum wage increases in California, and pooled

[30] The larger poverty-reduction effects the 100% and 150% thresholds are consistent with the more negative effects in columns (4)-(6) vs. columns (1)-(3).

analysis of city (and state) minimum wage increases nationwide.

Our analysis reveals the following evidence:

- Analyzing minimum wage effects on employment for individual cities in California, there is no clear evidence on the sign of the employment effects. There is sometimes evidence of a negative effect that is sizable but not very precisely estimated and hence not statistically significant, and the same is true for positive effects. Overall the synthetic control analysis city by city is inconclusive regarding employment effects.
- In the city-specific analyses, there is some limited evidence pointing to increases in the share low-income, or adverse distributional effects. While there is virtually no evidence of poverty (low-income) reductions, there is some evidence of adverse effects, although it arises in a relatively small number of cases.
- In general, the city-specific estimates are quite imprecise, which is not surprising. We do, though, find suggestive evidence that where the estimated employment effect is negative (and more negative), the estimated change in the share poor or low-income is positive (or more positive).
- In analyses pooling across California cities, there is relatively consistent evidence pointing to job loss, although the estimated effects are not statistically significant. In our analysis of teen and young adult employment, positive pre-trends may mask disemployment effects. Across our different pooled analyses of recent California minimum wages, there is no clear evidence of distributional effects.
- Our national analysis – which covers state and city minimum wages but focuses on the latter – finds evidence of a significant (at the 10% level) negative employment effect of city minimum wages for teens, with an elasticity of −0.15.
- In the national analysis of distributional effects, the evidence points to reductions in the share below 150% of the poverty line, with a significant the estimated elasticity of −0.13. This

evidence, however, may be driven in part by the kinds of pre-trends we found for city minimum wages in California.

One might argue that our analysis is incomplete because it does not explore hypotheses about the results, or potential problems for some of the estimators, suggested by our analyses based on our PAP. For example, one might argue that having raised the issue of pre-trends, we should evaluate models that allow for or control for these – like we did, at least partially, in the pooled California analysis (as planned in the PAP). However, our intention in this paper is to present the results from analyses specified in the PAP, and to leave to other research – and other researchers –explorations of some of the issues or hypotheses that stem from this paper. Pursuing analyses suggested by our results – such as seeing whether the apparent evidence of beneficial distributional effects is in fact spurious – poses the question of which analyses and questions to pursue, which would potentially take us back to authors' decisions, based on the data, influencing which kinds of results are reported.[31]

We are by no means arguing that research of the latter type is not valuable. Indeed, we anticipate that in non-experimental research the use of PAP's will remain very limited. For example, their use in minimum wage research was made at least feasible in the contemporaneous period because it was a safe bet that many more jurisdictions would be raising their minimum wages. Even so, the imprecision of many of our estimates, especially for single or smaller jurisdictions, highlights a potentially serious cost of using only future minimum wage increases in a period following the registration of a PAP. At the same time, the approach we use is feasible for early evidence on newly implemented minimum wage increases (or other policy changes).

[31] Nonetheless, while this is our intention, during the peer-review process we may well get pushed to conduct these additional analyses to present a fuller picture of the evidence on recent minimum wage increases. (We did not have any kind of pre-commitment to publish based on the PAP, unlike the case with the *Industrial Relations* mini-symposium that pre-committed to publish Neumark (2001).) If so, we will be sure to delineate which analyses go beyond those described in the PAP, which is described as good practice for using pre-specified research designs in non-experimental research by Christensen et al. (2019).

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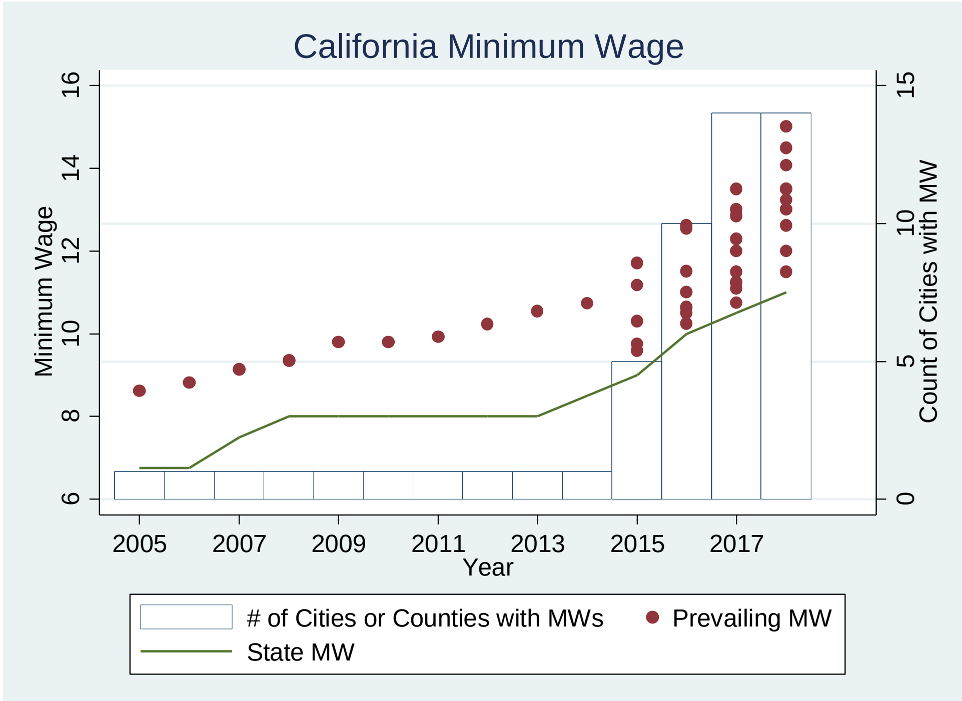
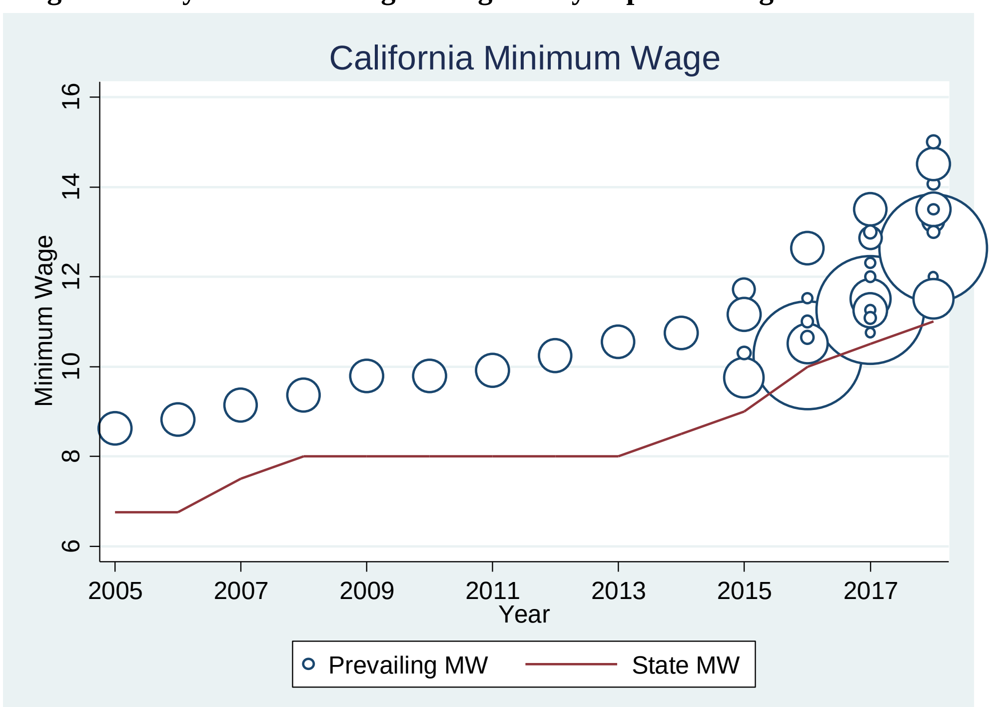
Figure 1: The Evolution of Minimum Wages in California



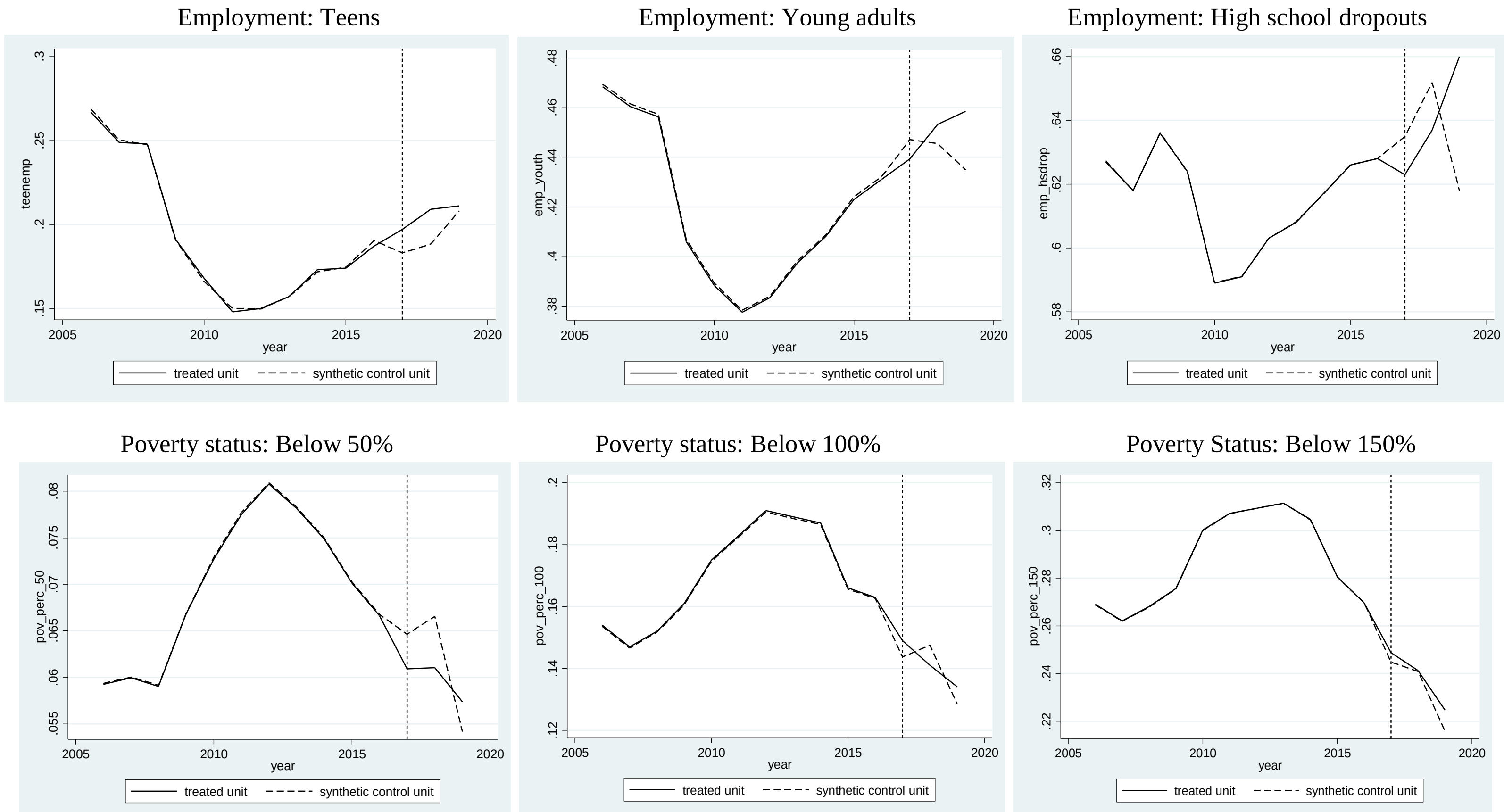
Note: The figure includes Los Angeles County as a single city. It excludes Cupertino, El Cerrito, Emeryville, and Los Altos, because they are too small to appear in the American Community Survey 1-year data (summary files) that we use. In the pre-analysis plan, we erroneously coded the average minimum wage for California in 2014 as $9 instead of $8.50, which has been fixed.

Figure 2: City Minimum Wages Weighted by Population Aged 16 and Over



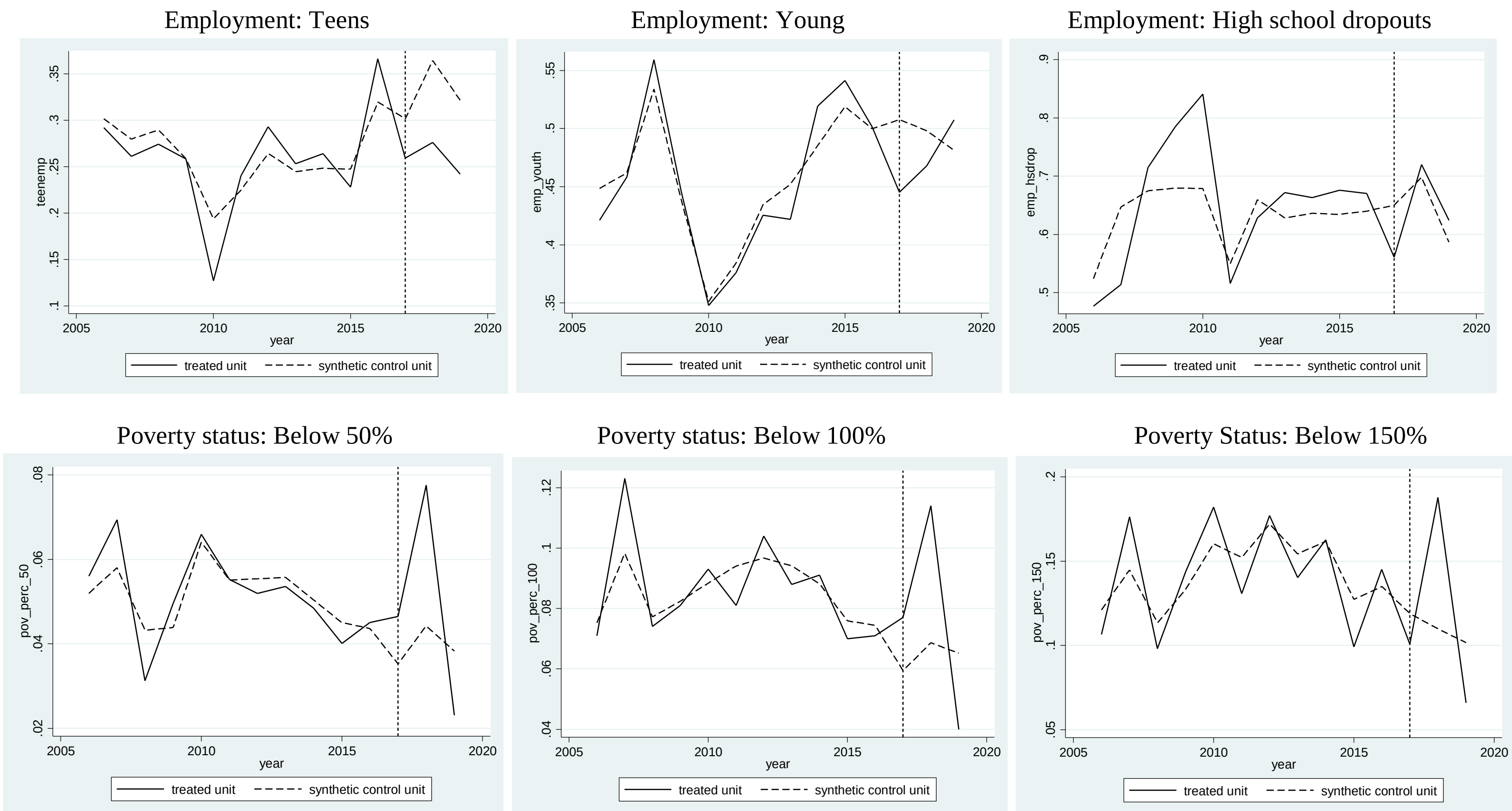
Note: This figure plots city minimum wages with plots weighted by the average population aged 16 and over from 2005 to 2018. In the pre-analysis plan, we erroneously coded the average minimum wage for California in 2014 as $9 instead of $8.50, which has been fixed.

Figure 3: Synthetic Control Estimates, Los Angeles County



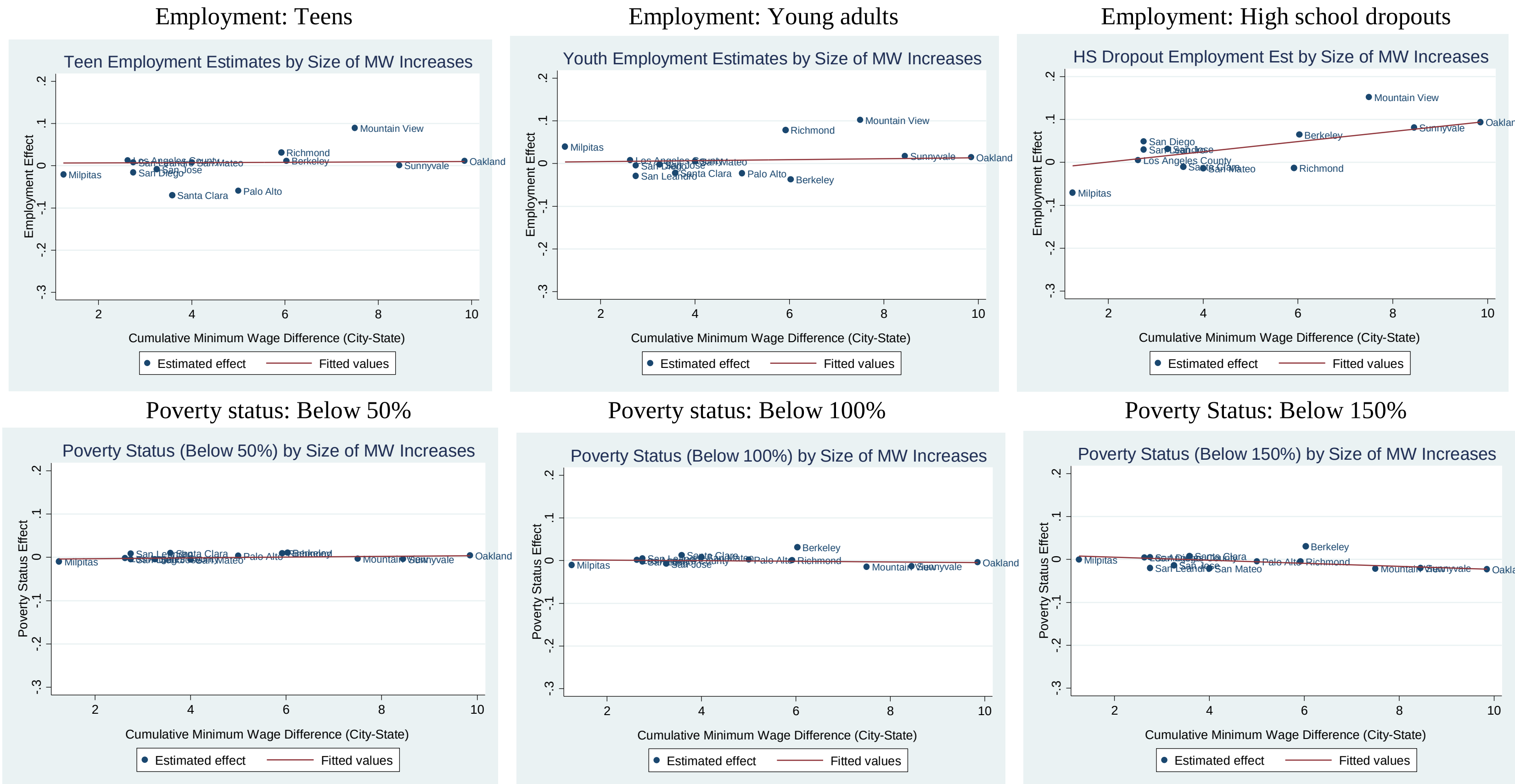
Note: The synthetic control estimation matches on the outcome variables of the pre-treatment period for each pre-treatment year. We lag the treatment year by one to take into account the lagged minimum wage effect.

Figure 4: Synthetic Control Estimates, Santa Clara



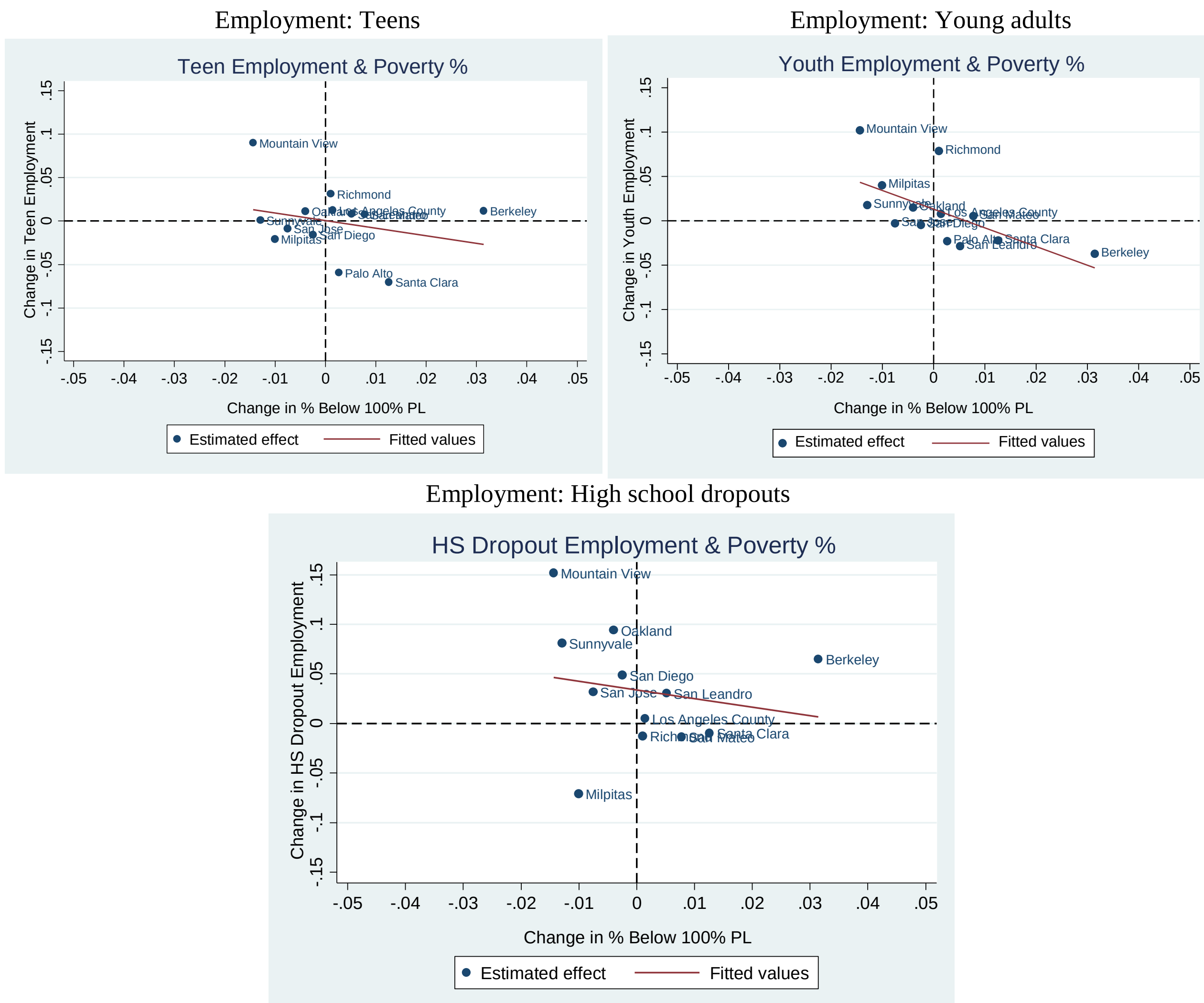
Note: See notes to Figure 3.

Figure 5: Synthetic Control Estimates vs. Minimum Wage Increases, Cumulative



Note: We plot the average synthetic control estimates across all post-treatment years against the total cumulative nominal minimum wage difference (based on one-year lags of the minimum wage) between the city and state minimum wage across all post-treatment years. Slopes of the lines, across the three panels in the top row and then the bottom row, are: 0.000 (p-value 0.910), 0.001 (p-value 0.704), 0.012 (p-value 0.006), 0.001 (p-value = 0.093), −0.001 (p-value = 0.470), and −0.004 (p-value = 0.017).

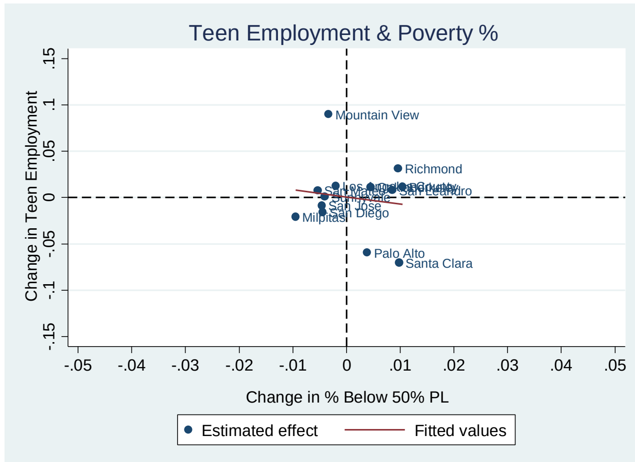
Figure 6A: Synthetic Control Estimates: Employment Effects vs. Poverty Effects (100% Threshold)



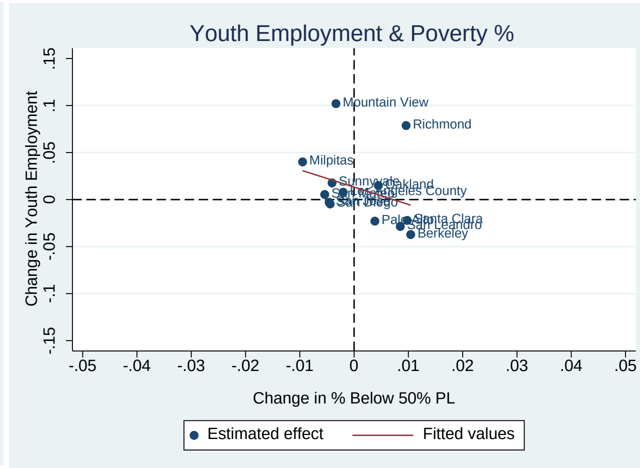
Note: We plot the synthetic control employment estimates against the synthetic control estimates for those below 100% of the poverty line. The estimates are computed across all post-treatment years. Slopes of the lines across the top row and the bottom panel are: −0.875 (p-value = 0.373), −2.11 (p-value =0.023), and −0.877 (p-value = 0.560).

Figure 6B: Synthetic Control Estimates: Employment Effects vs. Poverty Effects (50% Threshold)

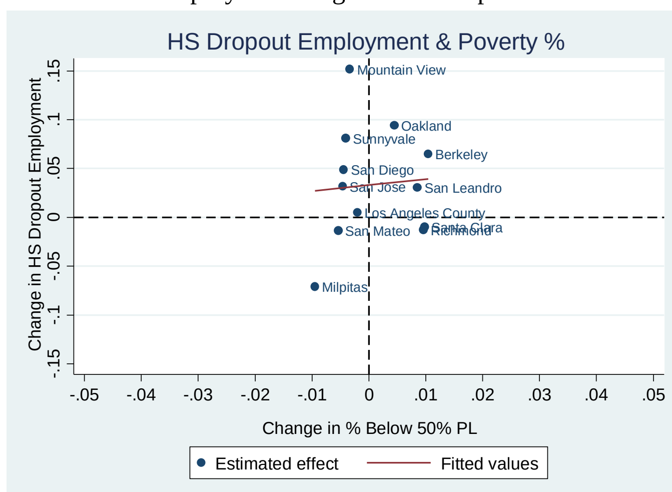
Employment: Teens



Employment: Young adults



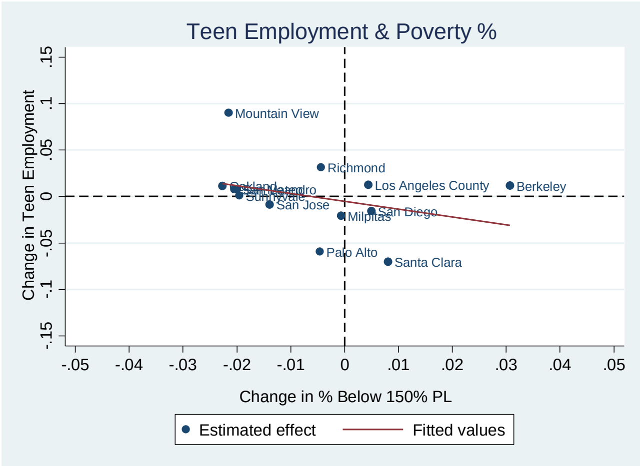
Employment: High school dropouts



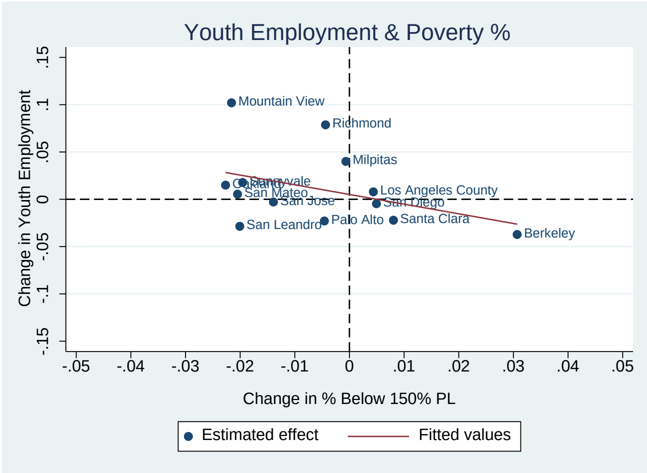
Note: We plot the synthetic control employment estimates against the synthetic control estimates for those below 50% of the poverty line. The estimates are computed across all post-treatment years. Slopes of the lines across the top row and the bottom panel are: −0.758 (p-value = 0.664), −1.842 (p-value = 0.301), and 0.625 (p-value = 0.815).

Figure 6C: Synthetic Control Estimates: Employment Effects vs. Poverty Effects (150% Threshold)

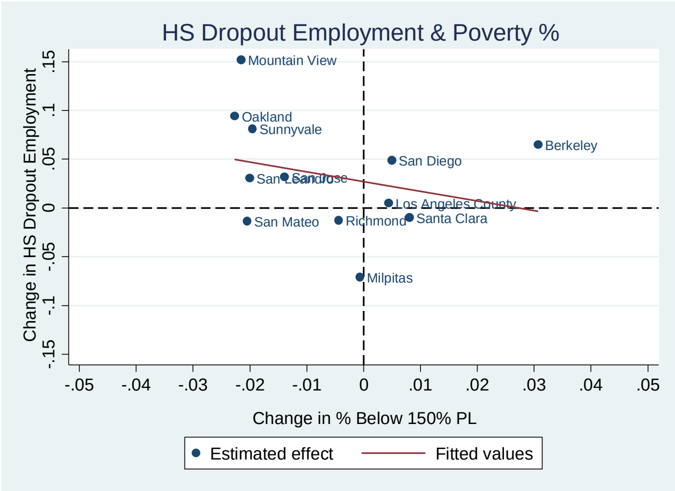
Employment: Teens



Employment: Young adults



Employment: High school dropouts



Note: We plot the synthetic control employment estimates against the synthetic control estimates for those below 150% of the poverty line. The estimates are computed across all post-treatment years. Slopes of the lines across the top row and the bottom panel are: −0.841 (p-value = 0.264), −1.023 (p-value = 0.186), −1.000 (p-value = 0.386).

Table 1: History of CA Minimum Wage Increases (2015-2018)

	2015		2016		2017		2018	
City/County	**Date of increase/ implementation**	**MW**	**Date of increase/ implementation**	**MW**	**Date of increase/ implementation**	**MW**	**Date of increase/ implementation**	**MW**
State of California	No Inc.	9	1/1	10	1/1	10.5	1/1	11
Total New City Minimum Wages	4		5		4		0	
Total City Minimum Wage Increases	1		5		10		13	
Berkeley			10/1	12.53	10/1	13.75	10/1	15
Los Angeles County			7/1	10.5	7/1	12	7/1	13.25
Milpitas					7/1	11	1/1	12
Mountain View			1/1	11	1/1	13	1/1	15
Oakland	3/1	12.25	1/1	12.55	1/1	12.86	1/1	13.23
Palo Alto			1/1	11	1/1	12	1/1	13.5
Richmond	1/1	9.6	1/1	11.52	1/1	12.3	1/1	13
San Diego	1/1	9.75	1/1	10.5	1/1	11.5		
San Francisco	5/1*	12.25	7/1	13	7/1	14	7/1	15
San Jose					7/1	12	1/1	13.5
San Leandro					7/1	12	7/1	13
San Mateo					1/1	12	1/1	13.5
Santa Clara			1/1	11	3/1	11.1	1/1	13
Sunnyvale	1/1	10.3	7/1	11	1/1	13	1/1	15

Notes: In the second and third rows, we report the counts, rather than dates. Cupertino, El Cerrito, Emeryville, and Los Altos have city minimum wages in the time frame, but do not appear in this table because they do not appear in the ACS 1-year summary files. All minimum wages are at the city-level in California, except for Los Angeles County. Census places in Los Angeles County that show up in the ACS 1-year summary files include Alhambra, Baldwin Park, Bellflower, Burbank, Carson, Compton, Downey, East Los Angeles CDP, El Monte, Florence-Graham CDP, Glendale, Hawthorne, Inglewood, Lakewood, Lancaster, Long Beach, Los Angeles (city), Lynwood, Norwalk, Pasadena, Pomona, Redondo Beach, Santa Clarita, Santa Monica, South Gate, Torrance, West Covina, and Whittier. The Census places that appear in the 1-year ACS Summary files but do not have city or county minimum wages in this timeframe, and serve as controls, include Alameda, Anaheim, Antioch, Apple Valley, Arden-Arcade CDP, Bakersfield, Buena Park, Camarillo, Carlsbad, Carmichael CDP, Castro Valley CDP, Chico, Chino, Chino Hills, Chula Vista, Citrus Heights, Clovis, Concord, Corona, Costa Mesa, Daly City, Davis, El Cajon, Elk Grove, Escondido, Fairfield, Folsom, Fontana, Fremont, Fresno, Fullerton, Garden Grove, Hayward, Hemet, Hesperia, Huntington Beach, Indio, Irvine, Jurupa Valley, Laguna Niguel, Lake Elsinore, Lake Forest, Livermore, Lodi, Madera, Manteca, Menifee, Merced, Mission Viejo, Modesto, Moreno Valley, Murrieta, Napa, Newport Beach, Oceanside, Ontario, Orange, Oxnard, Pittsburg, Pleasanton, Rancho Cordova, Rancho Cucamonga, Redding, Redlands, Redwood City, Rialto, Riverside, Roseville, Sacramento, Salinas, San Bernardino, San Buenaventura (Ventura), San Clemente, San Marcos, San Ramon, Santa Ana, Santa Barbara, Santa Cruz, Santa Maria, Santa Rosa, Simi Valley, South San Francisco, Stockton, Temecula, Thousand Oaks, Tracy, Turlock, Tustin, Union City, Upland, Vacaville, Vallejo, Victorville, Visalia, Vista, Walnut Creek, Westminster, Yorba Linda, and Yuba City. Among these cities, Fremont and Redwood City have their first minimum wage increases in 2019 but that is outside our timeframe.

* San Francisco had two minimum wage increases in 2015, one on Jan 1st to $11.05.

Table 2: City/County Minimum Wage Increases Since 2013

		2013		2014		2015		2016		2017		2018		
State	**City/County**	**I/I**	**MW**	**I/I**	**MW**	**I/I**	**MW**	**I/I**	**MW**	**I/I**	**MW**	**I/I**	**MW**	**Notes:**
Total New Minimum Wages		1[a]		1		7		7		12		1		
Total MW Increases		2		3		4		9		16		26		
AZ	Flagstaff									7/1	10.5	1/1	11	
CA	Berkeley							10/1	12.53	10/1	13.75	10/1	15	
CA	Los Angeles County							7/1	10.5	7/1	12	7/1	13.25	26+ Employees
CA	Milpitas									7/1	11	1/1	12	
CA	Mountain View							1/1	11	1/1	13	1/1	15	
CA	Oakland					3/1	12.25	1/1	12.55	1/1	12.86	1/1	13.23	
CA	Palo Alto							1/1	11	1/1	12	1/1	13.5	
CA	Richmond					1/1	9.6	1/1	11.52	1/1	12.3	1/1	13	
CA	San Diego					1/1	9.75	1/1	10.5	1/1	11.5			
CA	San Francisco	1/1	10.55	1/1	10.74	1/1 & 5/1	11.05 & 12.25	7/1	13	7/1	14	7/1	15	
CA	San Jose									7/1	12	1/1	13.5	
CA	San Leandro									7/1	12	7/1	14	
CA	San Mateo									1/1	12	1/1	13.5	Non-profits subject to lower MW
CA	Santa Clara							1/1	11	3/1	11.1	1/1	13	
CA	Sunnyvale					1/1	10.3	7/1	11	1/1	13	1/1	15	
IL	Chicago					7/1	10	7/1	10.5	7/1	11	7/1	12	
IL	Cook County[b]									7/1	10	7/1	11	
MD	Montgomery County[c]			10/1	8.4	10/1	9.55	7/1	10.75	7/1	11.5	7/1	12.25	
ME	Portland							1/1	10.1	1/1	10.68	7/1	10.9	
MN	Minneapolis											1/1	10	100+ employees
NM	Albuquerque	1/1	8.5	1/1	8.6	1/1	8.75			1/1	8.8	1/1	8.95	$1 lower if health/child care provided
NM	Las Cruces					1/1	8.4			1/1	9.2			
NM	Santa Fe	3/1	10.51	3/1	10.66	3/1	10.84	3/1	10.91	3/1	11.09	3/1	11.80	
NY	New York City									1/1[d]	11	1/1[d]	13	
NY	Suffolk County[e]									1/1[d]	10	1/1[d]	11	
NY	Westchester County[f]									1/1[d]	10	1/1[d]	11	
OR	Portland UGB[g]									7/1	11.25	7/1	12	
WA	Seattle					4/1	11	1/1	13	1/1	15	1/1	15.45	
WA	Tacoma							2/1	10.35	1/1	11.15	1/1	12	

Notes: "I/I" denotes date of increase/implementation, except in first two rows, where we report the counts. Cupertino (CA), El Cerrito (CA), Emeryville (CA), Los Altos (CA), Bangor (ME) have city-level minimum wages but are omitted from the ACS 1-year summary files. Prince George's County (MD) and Nassau County (NY) are omitted because they do not have any Census places that are large enough to show up in the ACS 1-year summary files. Bernalillo County (NM) has a county-wide minimum wage ordinance that is different from Albuquerque, but Albuquerque is the only Census place large enough to show up in the ACS 1-year summary files. Santa Fe County (NM) has a county-wide minimum wage ordinance that is different from Santa Fe city, but Santa Fe city is the only Census place large enough to show up in the ACS 1-year summary files.

[a] Santa Fe and San Francisco had their first minimum wage prior to 2013.

[b] Cook County includes Arlington Heights, Cicero, Elgin, Evanston, Palatine, Schaumburg, and Skokie in the ACS data.

[c] Montgomery County includes Bethesda CDP, Gaithersburg, Germantown CDP, Rockville, and Silver Spring in the ACS data.

[d] In NY, the actual reported minimum wage increase date is on 12/31 in the preceding year. We report the minimum wage increase as 1/1 in the following year, because we treat it as such in the data.

[e] Suffolk county includes Brentwood CDP

[f] Westchester County includes Mount Vernon, New Rochelle, and Yonkers.

[g] Oregon in 2016 established three separate geographical guidelines for determining the minimum wage – Portland Urban Growth Boundary (UGB), Standard, and Nonurban counties. Portland Urban Growth Boundary (UGB) contains most of Washington, Clackamas, and Multnomah counties but does not necessarily include the whole county. Non-urban counties include Baker, Coos, Crook, Curry, Douglas, Gilliam, Grant, Harney, Jefferson, Klamath, Lake, Malheur, Morrow, Sherman, Umatilla, Union, Wallowa, and Wheeler. These counties have a minimum wage lower than the standard minimum wage beginning in July 1, 2016. The Portland UGB first has a minimum wage *higher* than the standard minimum wage on July 1, 2017, which is the variation we study. Among the Census places appearing in the ACS, we use Portland UGB's minimum wage for Portland, Beaverton, Gresham, and Hillsboro and the standard minimum wage for Bend, Eugene, Medford, and Salem. (There are some complications here, but this is our best reading of how to treat the corresponding Census places.)

Table 3: History of San Francisco and Santa Fe Minimum Wages

Santa Fe	
Date	**MW**
1/1/2004	8.5
1/1/2006	9.5
1/1/2010	9.85
3/1/2012	10.30
3/1/2013	10.51
3/1/2014	10.66
3/1/2015	10.84
3/1/2016	10.91
3/1/2017	11.09
3/1/2018	11.80
San Francisco	
Date	**MW**
2/23/2004	8.5
1/1/2005	8.62
1/1/2006	8.82
1/1/2007	9.14
1/1/2008	9.36
1/1/2009	9.79
1/1/2011	9.92
1/1/2012	10.24
1/1/2013	10.55
1/1/2014	10.74
1/1/2015	11.05
5/1/2015	12.25
7/1/2016	13
7/1/2017	14
7/1/2018	15

Notes: Santa Fe increase dates for 2010 and earlier are not well documented. These are our best assessments of dates from newspaper articles.

Table 4: Timing of Measurement of ACS Outcomes and Other Data

ACS 1-Year Summary Files (Place level)	
Employment	Contemporaneous
Mean earnings (full-time year-round workers)	Last 12 months
Poverty status	Last 12 months
Demographic data (age, sex, race, marital status, citizenship, education)	Contemporaneous
Public Use Microdata (PUMA level)	
Employment	Contemporaneous
Wage income	Last 12 months
Usual hours worked	Last 12 months
Usual weeks worked	Last 12 months
Poverty status	Last 12 months
Demographic data (age, sex, race, marital status, citizenship, education)	Contemporaneous

Table 5A: Synthetic Control Analyses, Berkeley, 2006-2019

	Teen empl.	Youth empl.	HSDO empl.	Poor (50%)	Poor (100%)	Poor (150%)
	(1)	(2)	(3)	(4)	(5)	(6)
Post-treatment (2017-2019)						
Estimate	0.012	-0.037	0.065	0.010	0.031	0.031
p-value	0.789	0.275	0.218	0.272	0.123	0.235
Donor pool (number of Census places)	68	68	77	80	80	80
RMSPE	0.026	0.053	0.110	0.021	0.016	0.016
Group population (2006-2019)	12,109	30,598	2,537	104,337	104,337	104,337
2017						
Estimate	0.098	0.017	0.105	-0.018	0.019	0.033
p-value	0.246	0.768	0.282	0.173	0.494	0.284
2018						
Estimate	-0.068	-0.073	-0.052	0.004	0.035	0.026
p-value	0.362	0.304	0.500	0.778	0.198	0.407
2019						
Estimate	0.004	-0.055	0.142	0.046***	0.040	0.032
p-value	0.957	0.319	0.128	0.000	0.123	0.259

Notes: The first post-treatment year is one year after the implementation of the minimum wage, because we lag the minimum wage one year. We match on the pre-treatment outcome variable for each pre-treatment year. The p-value is calculated from the placebo inference procedure (Abadie, et al., 2010) where estimates are obtained for each Census place in the donor pool. The group population represents the average population of the specified group (teens, youths, high school dropouts, or population below the poverty thresholds). When there is more than one post-treatment year, as in this table, we report the average effect. Note that for teens and youths three Census places were dropped from the donor pool when compared to the donor pool in the pre-analysis plan owing to missing data for 2018. (This is two more than in our initial update, because of missing data in 2019.) This had no effect on the 2017 estimates, but changed the p-values slightly, relative to the results reported in the pre-analysis plan. Estimates below the highlighted line are based on minimum wage increases after the PAP was registered. We indicate statistical significance at the 10%, 5%, and 1% level with *, **, and ***.

Table 5B: Synthetic Control Analyses, Mountain View, 2006-2019

	Teen empl.	**Youth empl.**	**HSDO empl.**	**Poor (50%)**	**Poor (100%)**	**Poor (150%)**
	(1)	(2)	(3)	(4)	(5)	(6)
Post-treatment (2017-2019)						
Estimate	0.090	0.102***	0.152**	-0.003	-0.014	-0.022
p-value	0.143	0.000	0.026	0.691	0.333	0.346
Donor pool (number of Census places)	69	69	77	80	80	80
RMSPE	0.094	0.088	0.074	0.006	0.011	0.014
Group population (2006-2019)	2,485	7,047	3,591	76,729	76,729	76,729
2017						
Estimate	0.200**	0.172**	0.162*	-0.004	-0.020	-0.042
p-value	0.029	0.014	0.090	0.741	0.494	0.210
2018						
Estimate	-	-	0.008	0.000	-0.005	0.017
p-value	-	-	0.859	0.963	0.741	0.519
2019						
Estimate	-0.020	0.032	0.286**	-0.014	-0.018	-0.040
p-value	0.771	0.629	0.013	0.259	0.432	0.185

Notes: See notes to Table 5A. Mountain View is missing 2013 data, so 2013 is omitted from the pre-treatment match. Mountain View is also missing 2018 data for teens and youths.

Table 5C: Synthetic Control Analyses, Oakland, 2006-2019

	Teen empl.	**Youth empl.**	**HSDO empl.**	**Poor (50%)**	**Poor (100%)**	**Poor (150%)**
	(1)	(2)	(3)	(4)	(5)	(6)
Post-treatment (2016-2019)						
Estimate	0.011	0.015	0.094	0.004	-0.004	-0.023
p-value	0.797	0.667	0.128	0.519	0.753	0.296
Donor pool (number of Census places)	68	68	77	80	80	80
RMSPE	0.015	0.012	0.003	0.004	0.004	0.002
Group population (2006-2019)	17,489	42,517	42,150	398,834	398,834	398,834
2016						
Estimate	-0.024	0.044	0.130	0.019	0.011	-0.014
p-value	0.609	0.551	0.128	0.173	0.642	0.605
2017						
Estimate	0.079	0.084	0.142	-0.009	-0.002	-0.015
p-value	0.304	0.188	0.154	0.420	0.926	0.691
2018						
Estimate	0.009	-0.027	0.078	0.011	-0.023	-0.042
p-value	0.913	0.652	0.333	0.432	0.309	0.222
2019						
Estimate	-0.020	-0.041	0.026	-0.003	-0.001	-0.020
p-value	0.768	0.449	0.846	0.840	0.926	0.469

Notes: See notes to Table 5A. Note that in the PAP, for the 2016 estimate for Poor (50%), there was a rounding error, and the estimate should have been 0.019 (not 0.018 as reported in the pre-analysis plan).

Table 5D: Synthetic Control Analyses, Palo Alto, 2006-2019

	Teen empl.	Youth empl.	HSDO empl.	Poor (50%)	Poor (100%)	Poor (150%)
	(1)	(2)	(3)	(4)	(5)	(6)
Post-treatment (2017-2019)						
Estimate	-0.059	-0.023	-	0.004	0.003	-0.005
p-value	0.272	0.556	-	0.689	0.933	0.822
Donor pool (number of Census places)	80	80	-	89	89	89
RMSPE	0.046	0.047	-	0.007	0.005	0.014
Group population (2011-2019)	2,840	6,028	-	66,051	66,051	66,051
2017						
Estimate	-0.077	-0.088	-	0.015	0.021	0.023
p-value	0.395	0.210	-	0.389	0.456	0.433
2018						
Estimate	-0.070	-0.008	-	0.003	-0.000	-0.007
p-value	0.333	0.926	-	0.789	1.000	0.822
2019						
Estimate	-0.031	0.027	-	-0.007	-0.013	-0.030
p-value	0.765	0.630	-	0.589	0.567	0.356

Notes: See notes to 5A. We match on the pre-treatment outcome variable for each pre-treatment year, starting in 2011 when Palo Alto first appears in the data. We omit estimates for high school dropout employment for Palo Alto, because it is insufficiently reported. Note that for teens and youths three Census places were dropped from the donor pool when compared to the donor pool in the pre-analysis plan owing to missing data for 2019. For poverty estimates, one Census place was dropped.

Table 5E: Synthetic Control Analyses, Richmond, 2006-2019

	Teen empl.	Youth empl.	HSDO empl.	Poor (50%)	Poor (100%)	Poor (150%)
	(1)	(2)	(3)	(4)	(5)	(6)
Post-treatment (2016-2019)						
Estimate	0.031	0.079**	-0.013	0.010	0.001	-0.004
p-value	0.420	0.043	0.872	0.222	0.938	0.815
Donor pool (number of Census places)	68	68	77	80	80	80
RMSPE	0.025	0.029	0.025	0.006	0.009	0.011
Group population (2006-2019)	5,243	13,147	13,204	105,437	105,437	105,437
2016						
Estimate	0.029	0.051	-0.067	-0.001	-0.008	-0.042
p-value	0.551	0.478	0.385	0.889	0.691	0.284
2017						
Estimate	-0.046	0.041	-0.017	0.011	0.008	-0.006
p-value	0.507	0.522	0.795	0.395	0.753	0.901
2018						
Estimate	0.083	0.111	-0.020	0.015	-0.012	-0.042
p-value	0.275	0.188	0.705	0.358	0.519	0.222
2019						
Estimate	0.059	0.112**	0.054	0.013	0.015	0.073*
p-value	0.464	0.029	0.654	0.309	0.494	0.062

Notes: See notes to Tables 5A. Note that the 2016 and 2017 estimates differ from those reported in the pre-analysis plan, due to errors reported there (which failed to capture our final estimates).

Table 5F: Synthetic Control Analyses, San Diego, 2006-2019

	Teen empl.	**Youth empl.**	**HSDO empl.**	**Poor (50%)**	**Poor (100%)**	**Poor (150%)**
	(1)	(2)	(3)	(4)	(5)	(6)
Post-treatment (2016-2019)						
Estimate	-0.016	-0.005	0.049	-0.004	-0.002	0.005
p-value	0.638	0.928	0.397	0.519	0.889	0.815
Donor pool (number of Census places)	68	68	77	80	80	80
RMSPE	0.000	0.002	0.000	0.000	0.000	0.000
Group population (2006-2019)	74,095	195,138	85,902	1,312,502	1,312,502	1,312,502
2016						
Estimate	-0.009	0.021	0.012	-0.004	-0.006	0.008
p-value	0.855	0.797	0.885	0.630	0.765	0.827
2017						
Estimate	-0.001	-0.010	0.064	0.004	-0.001	0.009
p-value	0.971	0.884	0.500	0.704	0.975	0.827
2018						
Estimate	-0.059	-0.042	0.021	-0.013	0.001	0.001
p-value	0.420	0.522	0.692	0.407	0.914	0.975
2019						
Estimate	0.008	0.013	0.099	-0.004	-0.004	0.002
p-value	0.855	0.841	0.346	0.741	0.827	0.963

Notes: See notes to Tables 5A.

Table 5G: Synthetic Control Analyses, Santa Clara, 2006-2019

	Teen empl.	**Youth empl.**	**HSDO empl.**	**Poor (50%)**	**Poor (100%)**	**Poor (150%)**
	(1)	(2)	(3)	(4)	(5)	(6)
Post-treatment (2017 - 2019)						
Estimate	-0.070	-0.022	-0.010	0.010	0.013	0.008
p-value	0.145	0.594	0.846	0.284	0.407	0.654
Donor pool (number of Census places)	68	68	77	80	80	80
RMSPE	0.029	0.020	0.078	0.006	0.009	0.018
Group population (2006-2019)	6,830	15,327	4,565	115,934	115,934	115,934
2017						
Estimate	-0.042	-0.062	-0.089	0.011	0.018	-0.018
p-value	0.536	0.275	0.359	0.358	0.506	0.556
2018						
Estimate	-0.088	-0.030	0.022	0.033**	0.045*	0.078**
p-value	0.232	0.652	0.808	0.049	0.086	0.049
2019						
Estimate	-0.080	0.026	0.038	-0.015	-0.025	-0.036
p-value	0.333	0.638	0.705	0.235	0.284	0.235

Notes: See notes to Table 5A.

Table 5H: Synthetic Control Analyses, Sunnyvale, 2006-2019

	Teen empl.	**Youth empl.**	**HSDO empl.**	**Poor (50%)**	**Poor (100%)**	**Poor (150%)**
	(1)	(2)	(3)	(4)	(5)	(6)
Post-treatment (2016-2019)						
Estimate	0.001	0.018	0.081	-0.004	-0.013	-0.020
p-value	0.957	0.667	0.218	0.568	0.395	0.321
Donor pool (number of Census places)	68	68	77	80	80	80
RMSPE	0.009	0.012	0.039	0.006	0.004	0.007
Group population (2006-2019)	4,887	11,867	6,487	144,561	144,561	144,561
2016						
Estimate	-0.012	0.060	0.079	0.002	-0.006	0.000
p-value	0.812	0.420	0.333	0.790	0.728	1.000
2017						
Estimate	0.052	0.016	0.021	0.007	-0.000	-0.011
p-value	0.435	0.812	0.782	0.556	0.975	0.815
2018						
Estimate	-0.148*	-0.098	0.097	-0.019	-0.043	-0.054
p-value	0.087	0.144	0.167	0.235	0.111	0.136
2019						
Estimate	0.113	0.092*	0.128	-0.006	-0.002	-0.014
p-value	0.116	0.072	0.282	0.667	0.914	0.593

Notes: See notes to Tables 5A.

Table 5I: Synthetic Control Analyses, LA County, 2006-2019

	Teen empl.	**Youth empl.**	**HSDO empl.**	**Poor (50%)**	**Poor (100%)**	**Poor (150%)**
	(1)	(2)	(3)	(4)	(5)	(6)
Post-treatment (2017 - 2019)						
Estimate	0.013	0.008	0.005	-0.002	0.001	0.004
p-value	0.768	0.812	0.923	0.815	0.877	0.790
Donor pool (number of Census places)	68	68	77	80	80	80
RMSPE	0.002	0.000	0.000	0.000	0.000	0.000
Group population (2006-2019)	565,319	1,304,623	1,161,306	9,843,498	9,843,498	9,843,498
2017						
Estimate	0.014	-0.008	-0.012	-0.004	0.005	0.004
p-value	0.855	0.899	0.872	0.790	0.889	0.840
2018						
Estimate	0.021	0.008	-0.015	-0.006	-0.007	0.000
p-value	0.855	0.899	0.795	0.630	0.728	0.975
2019						
Estimate	0.003	0.024	0.042	0.003	0.005	0.009
p-value	0.986	0.652	0.667	0.864	0.753	0.765

Notes: See notes to Table 5A. We use county-level data for Los Angeles rather than providing separate estimates for all 29 of Los Angeles' non-censored Census places.

Table 5J: Synthetic Control Analyses, Milpitas, 2006-2019 – First Minimum Wage in 2017

	Teen empl.	Youth empl.	HSDO empl.	Poor (50%)	Poor (100%)	Poor (150%)
	(1)	(2)	(3)	(4)	(5)	(6)
Post-treatment (2018 – 2019)						
Estimate	-0.021	0.040	-0.071	-0.009	-0.010	-0.001
p-value	0.722	0.431	0.266	0.383	0.605	0.975
Donor pool (number of Census places)	71	71	78	80	80	80
RMSPE	0.046	0.027	0.041	0.012	0.011	0.005
Group population (2007-2019)	3,124	7,479	4,313	70,054	70,054	70,054
2018						
Estimate	-0.123	0.017	-0.093	0.002	0.016	0.025
p-value	0.111	0.750	0.215	0.914	0.432	0.346
2019						
Estimate	0.087	0.063	-0.049	-0.021	-0.036	-0.026
p-value	0.250	0.250	0.582	0.148	0.148	0.272

Notes: See notes to Table 5A. Analyses for Census places in Tables 5J-5M were not reported in the pre-analysis plan because their minimum wages were first implemented in 2017. Due to using a lag of the minimum wage data, we needed to wait for an additional year of ACS data for this Census place when the PAP was registered. Note that for teens and youths three Census places was dropped from the donor pool when compared to the previous release with 2018 data owing to missing data for 2019.

Table 5K: Synthetic Control Analyses, San Leandro, 2006-2019 – First Minimum Wage in 2017

	Teen empl.	Youth empl.	HSDO empl.	Poor (50%)	Poor (100%)	Poor (150%)
	(1)	(2)	(3)	(4)	(5)	(6)
Post-treatment (2018 - 2019)						
Estimate	0.008	-0.029	0.030	0.009	0.005	-0.020
p-value	0.913	0.623	0.615	0.395	0.741	0.370
Donor pool (number of Census places)	68	68	77	80	80	80
RMSPE	0.043	0.010	0.023	0.005	0.017	0.007
Group population (2006-2019)	3,724	9,221	7,692	87,978	87,978	87,978
2018						
Estimate	-0.029	-0.033	0.046	-0.006	-0.008	-0.053*
p-value	0.739	0.594	0.590	0.679	0.679	0.086
2019						
Estimate	0.045	-0.024	0.014	0.023*	0.019	0.013
p-value	0.536	0.710	0.833	0.099	0.370	0.704

Notes: See notes to Table 5A and 5J. Analyses for cities in Tables 5J-5M were not reported in pre-analysis plan because their minimum wages were first implemented in 2017.

5L: Synthetic Control Analyses, San Jose, 2006-2019 – First Minimum Wage in 2017

	Teen empl.	Youth empl.	HSDO empl.	Poor (50%)	Poor (100%)	Poor (150%)
	(1)	(2)	(3)	(4)	(5)	(6)
Post-treatment (2018 - 2019)						
Estimate	-0.008	-0.003	0.032	-0.005	-0.008	-0.014
p-value	0.928	0.986	0.628	0.679	0.642	0.531
Donor pool (number of Census places)	68	68	77	80	80	80
RMSPE	0.000	0.000	0.000	0.002	0.003	0.002
Group population (2006-2019)	48,032	114,720	83,914	974,005	974,005	974,005
2018						
Estimate	-0.010	-0.010	0.013	-0.007	-0.012	-0.005
p-value	0.928	0.870	0.859	0.630	0.593	0.802
2019						
Estimate	-0.007	0.004	0.050	-0.002	-0.003	-0.023
p-value	0.913	0.942	0.603	0.901	0.827	0.358

Notes: See notes to Table 5A and 5J. Analyses for cities in Tables 5J-5M were not reported in pre-analysis plan because their minimum wages were first implemented in 2017.

5M: Synthetic Control Analyses, San Mateo, 2006-2019 – First Minimum Wage in 2017

	Teen empl.	Youth empl.	HSDO empl.	Poor (50%)	Poor (100%)	Poor (150%)
	(1)	(2)	(3)	(4)	(5)	(6)
Post-treatment (2018 - 2019)						
Estimate	0.008	0.005	-0.014	-0.005	0.008	-0.020
p-value	0.942	0.957	0.859	0.654	0.667	0.370
Donor pool (number of Census places)	68	68	77	80	80	80
RMSPE	0.065	0.055	0.073	0.003	0.010	0.012
Group population (2006-2019)	3,605	8,842	5,383	98,803	98,803	98,803
2018						
Estimate	-0.091	-0.003	-0.028	-0.007	0.005	-0.020
p-value	0.203	0.971	0.731	0.593	0.728	0.481
2019						
Estimate	0.107	0.014	0.001	-0.003	0.010	-0.021
p-value	0.145	0.841	1.000	0.864	0.617	0.494

Notes: See notes to Table 5A and 5J. Analyses for cities in Tables 5J-5M were not reported in pre-analysis plan because their minimum wages were first implemented in 2017.

Table 6A: Employment Estimates, Pooled California Cities, 2005-2019 – Teens

Post-Treatment Years	Two	Two	Two	Three	Three	Four
	(1)	(2)	(3)	(4)	(5)	(6)
Number of treated cities	13	9	4	9	4	4
Jump estimate	**-0.005**	**0.002**	**-0.017***	**0.005**	**-0.013***	**-0.024****
Regular SEs	**[0.010]**	**[0.008]**	**[0.009]**	**[0.007]**	**[0.008]**	**[0.010]**
Bootstrap p-values	**0.643**	**0.773**	**0.340**	**0.555**	**0.405**	**0.392**
Pre-treatment trend	0.001	0.001	0.005***, †	0.001	0.005***,†	0.005***,†
Regular SEs	[0.001]	[0.001]	[0.001]	[0.001]	[0.001]	[0.001]
Bootstrap p-values	0.454	0.609	0.066	0.610	0.066	0.066
Post-treatment trend	**-0.000**	**-0.006**	**-0.009**	**-0.009**	**-0.019****	**-0.003**
Regular SEs	**[0.009]**	**[0.008]**	**[0.013]**	**[0.006]**	**[0.008]**	**[0.004]**
Bootstrap p-values	**0.987**	**0.428**	**0.607**	**0.273**	**0.415**	**0.419**
Unemployment (25-64)	0.002	0.021	0.051	0.006	0.042	0.038
	[0.114]	[0.113]	[0.116]	[0.112]	[0.115]	[0.114]
Relative group size	0.165	0.124	0.152	0.122	0.158	0.165
	[0.151]	[0.150]	[0.158]	[0.150]	[0.158]	[0.158]
N	1450	1398	1340	1407	1344	1348

Notes: The table follows the specifications of Allegretto et al. (2018) in estimating an immediate "jump" effect and a "post-trend," with a few modifications. Consistent with lagging the minimum wage one year, we treat the year following the first city-minimum wage implementation as the first post-treatment year. We omit Census places in Los Angeles County and instead include Los Angeles County-level data. Regression includes place and year fixed effects. Standard errors are clustered by place. The bootstrapped p-values are based on the Wild bootstrap as in Allegretto et al. We denote significance based on the usual clustered standard errors with the symbol *, and significance based on the bootstrap with the symbol †. We indicate statistical significance at the 10%, 5%, and 1% level with one, two, or three symbols. Regressions are weighted by the population of the group (teens, youths, or high school dropouts in Tables 6A, 6B, and 6C, respectively, and the population for which poverty status is defined in Tables 7A-7C). In column (4), we only include cities that first implemented minimum wages in 2016 (when t = 0 in 2017). In column (6), we only include cities that first implemented minimum wages in 2015, which also allows for a post-trend for all treated cities – now for an additional year. Columns (2) and (3) run the same specification as column (1) but only including the treatment cities in column (4) and (6) respectively. Column (5) runs the same specification as column (4) but only including the treatment cities in column (6). Note that Table 6 from the PAP is broken into Tables 6A-6C, because we have an additional post-treatment observation and hence more columns to report. The registered code inadvertently omitted the clustering, which has been added to this table.

Table 6B: Employment Estimates, Pooled California Cities, 2005-2019 – Youths

Post-Treatment Years	Two	Two	Two	Three	Three	Four
	(1)	(2)	(3)	(4)	(5)	(6)
Number of treated cities	13	9	4	9	4	4
Jump estimate	**-0.003**	**0.000**	**-0.017^{*}**	**0.007**	**-0.006**	**-0.014**
Regular SEs	**[0.009]**	**[0.008]**	**[0.009]**	**[0.007]**	**[0.009]**	**[0.010]**
Bootstrap p-values	**0.725**	**0.964**	**0.572**	**0.391**	**0.682**	**0.610**
Pre-treatment trend	0.001	0.001	$0.006^{***,\dagger\dagger}$	0.001	$0.006^{***,\dagger\dagger}$	$0.006^{***,\dagger\dagger}$
Regular SEs	[0.001]	[0.001]	[0.002]	[0.001]	[0.002]	[0.002]
Bootstrap p-values	0.342	0.379	0.022	0.396	0.022	0.023
Post-treatment trend	**0.005**	**0.004**	**0.013^{*}**	**-0.005**	**$-0.018^{***,\dagger}$**	**-0.006**
Regular SEs	**[0.005]**	**[0.006]**	**[0.007]**	**[0.006]**	**[0.004]**	**[0.004]**
Bootstrap p-values	**0.333**	**0.527**	**0.268**	**0.520**	**0.098**	**0.293**
Unemployment (25-64)	-0.127	-0.128	-0.107	-0.140	-0.111	-0.118
	[0.110]	[0.111]	[0.109]	[0.111]	[0.109]	[0.109]
Relative group size	0.108	0.096	0.119	0.101	0.121	0.131
	[0.096]	[0.097]	[0.102]	[0.095]	[0.103]	[0.102]
N	1450	1398	1340	1407	1344	1348

Notes: See notes to Table 6A.

Table 6C: Employment Estimates, Pooled California Cities, 2005-2019 – High School Dropouts (HSDO)

Post-Treatment Years	Two	Two	Two	Three	Three	Four
	(1)	(2)	(3)	(4)	(5)	(6)
Number of treated cities	13	9	4	9	4	4
Jump estimate	**-0.011**	**-0.014**	**0.003**	**-0.018**	**0.009**	**0.012**
Regular SEs	**[0.010]**	**[0.010]**	**[0.013]**	**[0.011]**	**[0.013]**	**[0.015]**
Bootstrap p-values	**0.348**	**0.284**	**0.848**	**0.321**	**0.487**	**0.422**
Pre-treatment trend	0.001	0.001	0.001	0.001	0.001	0.001
Regular SEs	[0.001]	[0.001]	[0.002]	[0.001]	[0.002]	[0.002]
Bootstrap p-values	0.302	0.328	0.493	0.316	0.495	0.497
Post-treatment trend	**0.010**	**0.011**	**0.030^{*}**	**$0.016^{***,\dagger\dagger}$**	**0.011^{*}**	**0.006**
Regular SEs	**[0.008]**	**[0.009]**	**[0.017]**	**[0.004]**	**[0.006]**	**[0.009]**
Bootstrap p-values	**0.294**	**0.344**	**0.384**	**0.048**	**0.241**	**0.589**
Unemployment (25-64)	-0.615^{***}	-0.614^{***}	-0.606^{***}	-0.614^{***}	-0.606^{***}	-0.600^{***}
	[0.100]	[0.102]	[0.104]	[0.102]	[0.104]	[0.104]
Relative group size	0.101	0.105	0.087	0.105	0.084	0.094
	[0.082]	[0.083]	[0.088]	[0.082]	[0.087]	[0.086]
N	1450	1448	1391	1457	1395	1399

Notes: See notes to Table 6A.

Table 7A: Poverty Estimates, Pooled California Cities – Below 50% of Poverty Line

Post-Treatment Years	Two	Two	Two	Three	Three	Four
	(1)	(2)	(3)	(4)	(5)	(6)
Number of treated places	13	9	4	9	4	4
Jump estimate	**-0.000**	**0.000**	**0.002**	**-0.001**	**0.003**	**0.003**
Regular SEs	**[0.003]**	**[0.004]**	**[0.004]**	**[0.003]**	**[0.003]**	**[0.002]**
Bootstrap p-values	**0.892**	**0.960**	**0.658**	**0.861**	**0.292**	**0.221**
Pre-treatment trend	-0.001***,†††	-0.001***,†††	-0.002***,††	-0.001***,††	-0.002***,††	-0.002***,††
Regular SEs	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Bootstrap p-values	0.000	0.008	0.023	0.010	0.023	0.023
Post-treatment trend	**0.001**	**-0.000**	**0.002**	**0.002**	**0.000**	**0.000**
Regular SEs	**[0.003]**	**[0.004]**	**[0.008]**	**[0.001]**	**[0.002]**	**[0.001]**
Bootstrap p-values	**0.797**	**0.970**	**0.646**	**0.377**	**0.852**	**0.905**
Unemployment (25-64)	0.164***	0.161***	0.148***	0.161***	0.148***	0.147***
	[0.040]	[0.041]	[0.042]	[0.041]	[0.042]	[0.042]
N	1510	1458	1398	1467	1402	1406

Notes: See notes to Table 6A. Note that Table 7 from the pre-analysis plan is broken into Tables 7A-7C, because we have an additional post-treatment observation and hence more columns to report.

Table 7B: Poverty Estimates, Pooled California Cities – Below 100% of Poverty Line

Post-Treatment Years	Two	Two	Two	Three	Three	Four
	(1)	(2)	(3)	(4)	(5)	(6)
Number of treated places	13	9	4	9	4	4
Jump estimate	**-0.001**	**-0.002**	**-0.001**	**-0.003**	**-0.001**	**-0.001**
Regular SEs	**[0.006]**	**[0.006]**	**[0.006]**	**[0.005]**	**[0.005]**	**[0.004]**
Bootstrap p-values	**0.868**	**0.819**	**0.857**	**0.621**	**0.937**	**0.692**
Pre-treatment trend	-0.001***,†††	-0.001***,†††	-0.002***,††	-0.001***,†††	-0.002***,††	-0.002***,††
Regular SEs	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Bootstrap p-values	0.000	0.002	0.027	0.003	0.027	0.026
Post-treatment trend	**-0.001**	**-0.001**	**0.002**	**0.002**	**-0.001**	**0.000**
Regular SEs	**[0.005]**	**[0.005]**	**[0.009]**	**[0.003]**	**[0.006]**	**[0.002]**
Bootstrap p-values	**0.873**	**0.851**	**0.812**	**0.558**	**0.848**	**0.824**
Unemployment (25-64)	0.356***	0.356***	0.331***	0.358***	0.334***	0.333***
	[0.056]	[0.057]	[0.057]	[0.057]	[0.057]	[0.057]
N	1511	1459	1399	1468	1403	1407

Notes: See notes to Tables 6A and 7A.

Table 7C: Poverty Estimates, Pooled California Cities – Below 150% of Poverty Line

Post-Treatment Years	Two	Two	Two	Three	Three	Four
	(1)	(2)	(3)	(4)	(5)	(6)
Number of treated places	13	9	4	9	4	4
Jump estimate	**-0.005**	**-0.004**	**0.003**	**-0.004**	**0.002**	**-0.001**
Regular SEs	**[0.004]**	**[0.005]**	**[0.004]**	**[0.005]**	**[0.004]**	**[0.005]**
Bootstrap p-values	**0.383**	**0.439**	**0.515**	**0.532**	**0.656**	**0.857**
Pre-treatment trend	-0.002***,†††	-0.002***,†††	-0.003***,†	-0.002***,†††	-0.003***,†	-0.003***,††
Regular SEs	[0.000]	[0.000]	[0.001]	[0.000]	[0.001]	[0.000]
Bootstrap p-values	0.000	0.000	0.079	0.000	0.080	0.077
Post-treatment trend	**0.002**	**0.000**	**-0.009**	**0.001**	**-0.006**	**-0.002**
Regular SEs	**[0.003]**	**[0.003]**	**[0.007]**	**[0.003]**	**[0.006]**	**[0.003]**
Bootstrap p-values	**0.589**	**0.917**	**0.269**	**0.892**	**0.339**	**0.536**
Unemployment (25-64)	0.390***	0.392***	0.373***	0.396***	0.378***	0.376***
	[0.064]	[0.066]	[0.068]	[0.066]	[0.067]	[0.068]
N	1510	1458	1398	1467	1402	1406

Notes: See notes to Tables 6A and 7A.

Table 8: Employment Effects, Pooled California Cities, 2005-2019

	Teens	Youths	HSDO
	(1)	(2)	(3)
MW/average wage	-0.085	-0.012	-0.052
	[0.075]	[0.080]	[0.073]
MW elasticity	-0.120	-0.008	-0.028
N	1259	1259	1275
R^2	0.649	0.724	0.757

Notes: Control variables include unemployment rate of 25-64, relative cohort size of the group (teen (16-19), youths (16-24), and high school dropouts (25-64)), shares U.S. citizens, nonwhite, black, high school graduates, some college graduate, BA or higher, and male. The shares are typically shares of the whole population except for education, which uses 18-to-24 for teen and youth employment regressions and is omitted in the high school dropout regression. We omit Census places in Los Angeles County and instead include Los Angeles County-level data. Regressions includes place and year fixed effects. Standard errors are clustered by place. Regressions are weighted by the population of the group (teens, youths, or high school dropouts). The average wage is defined as the average earnings of full-time year-round workers aged 16 and over, divided by 2087, which is the assumed hours worked. Our MW/average wage measure is a one-year lag of the minimum wage divided by a two-year lag of the average wage. The elasticity is determined by taking the estimate multiplied by the ratio of the average of the MW/average wage to the average employment rate of the group. In the pre-analysis plan, we erroneously coded the average minimum wage for California in 2014 as $9 instead of $8.50 which has been fixed.

Table 9: Effects on Poverty Thresholds, Pooled California Cities, 2005-2019

	Below 50% of poverty line	Below 100% of poverty line	Below 150% of poverty line
MW/average wage	-0.034	-0.111^{***}	-0.102^{**}
	[0.021]	[0.032]	[0.041]
MW elasticity	-0.168	-0.236	-0.129
N	1280	1280	1280
R^2	0.840	0.917	0.949

Notes: See notes to Table 8. The education controls here use the shares high school graduate, some college graduate, and BA or higher for ages 18 and over. We indicate statistical significance at the 10%, 5%, and 1% level with *, **, and ***.

Table 10: Employment Effects of City and State Minimum Wages, 2005-2019

	State minimum wages			*City minimum wages*				*State and city minimum wages*		
	Teens	Youths	HSDO	Teens	Youths	HSDO		Teens	Youths	HSDO
	(1)	(2)	(3)	(4)	(5)	(6)		(7)	(8)	(9)
State MW/average wage	-0.026	0.058*	-0.043				State MW/average wage	-0.024	0.067**	-0.042
	[0.041]	[0.030]	[0.040]					[0.040]	[0.032]	[0.039]
State MW elasticity	**-0.029**	**0.042**	**-0.023**				**State MW elasticity**	**-0.028**	**0.042**	**-0.022**
City MW/average wage				-0.039	0.058*	-0.043	(City MW−state MW)/average wage	-0.132*	0.002	-0.046
				[0.039]	[0.030]	[0.034]		[0.071]	[0.050]	[0.100]
City MW elasticity				**-0.045**	**0.037**	**-0.023**	**City MW elasticity**	**-0.150**	**0.001**	**-0.025**
N	6312	6312	6434	6312	6312	6434		6312	6312	6434
R^2	0.756	0.818	0.811	0.756	0.818	0.811		0.756	0.818	0.811

Notes: See notes to Table 8. The construction of the variables is the same, except we use all Census places nationally and use state, county, and city minimum wages (applying state or county minimum wages to any city for which these are the binding minimum wages). The first three columns report regressions of employment on the state minimum wage over the average state wage. The next three columns use the city minimum wage, or state minimum wage if no city minimum wage exists, over the average city wage. The last three columns use the state and city minimum wages, over the average city wage, with the city minimum wage as defined for the middle three columns. In the pre-analysis plan, we erroneously coded the average minimum wage for California in 2014 as $9 instead of $8.50 and for Maryland in 2006 as $5.05 instead of $6.07, which has been fixed in this iteration. Note that we made one change relative to the pre-analysis plan, substituting (City MW−state MW)/average wage for City MW/average wage in the last three columns. This has no effect on the model fit or the city MW estimates, but implies that one can read the effect of state minimum wage variation off of the State MW/average wage coefficient alone, rather than having to subtract off the city minimum wage effect (which would be redundant since the city MW is defined as the maximum of the two). (In other words, this has no impact on the estimated effects of either city or state minimum wage; it just makes it easier to read these directly from the table.) The state minimum wage elasticity is calculated using only the State MW/average wage variable; this is the correct elasticity for any city in which the state minimum wage binds, which is almost all observations. The city minimum wage elasticity is calculated using the average city minimum wage over the average wage, not the variable based on the average difference between the city and state minimum wage; the coefficient of the (City MW−state MW)/average wage variable is the partial effect on employment of the city minimum wage divided by the average wage, because we control for the state minimum wage. We indicate statistical significance at the 10%, 5%, and 1% level with *, **, and ***.

Table 11: Poverty Effects of City and State Minimum Wages

	State minimum wages			*City minimum wages*				*State and city minimum wages*		
	Below 50% of PL	Below 100% of PL	Below 150% of PL	Below 50% of PL	Below 100% of PL	Below 150% of PL		Below 50% of PL	Below 100% of PL	Below 150% of PL
	(1)	(2)	(3)	(4)	(5)	(6)		(7)	(8)	(9)
State MW/average wage	-0.006	-0.012	-0.023				State MW/average wage	-0.006	-0.011	-0.021
	[0.012]	[0.018]	[0.023]					[0.012]	[0.018]	[0.022]
State MW elasticity	**-0.021**	**-0.020**	**-0.025**				**State MW elasticity**	**-0.021**	**-0.019**	**-0.023**
City MW/average wage				-0.004	-0.016	-0.037*	(City MW−state MW)/average wage	0.005	-0.039	-0.114***
				[0.010]	[0.015]	[0.021]		[0.028]	[0.032]	[0.039]
City MW elasticity				**-0.015**	**-0.027**	**-0.041**	**City MW elasticity**	**0.018**	**-0.068**	**-0.126**
N	6449	6451	6449	6449	6451	6449		6449	6451	6449
R^2	0.884	0.936	0.951	0.884	0.936	0.951		0.884	0.936	0.951

Note: See notes to Table 10. “PL” denotes “poverty line.” The sample size difference for 100% of PL is because some city observations do not have information about 50%/150% of PL.

Online Appendix A: Estimates Isolating Post-Registration Variation

Alternative Table 5A: Synthetic Control Analyses, Berkeley, 2006-2019

	Teen empl.	Youth empl.	HSDO empl.	Poor (50%)	Poor (100%)	Poor (150%)
	(1)	(2)	(3)	(4)	(5)	(6)
Post-treatment (2017-2019)						
Estimate	0.012	-0.037	0.065	0.010	0.031	0.031
p-value	0.789	0.275	0.218	0.272	0.123	0.235
Donor pool (number of Census places)	68	68	77	80	80	80
RMSPE	0.026	0.053	0.110	0.021	0.016	0.016
Group population (2006-2019)	12,109	30,598	2,537	104,337	104,337	104,337
Post-Registration Years (2018-2019)						
Estimate	-0.032	-0.064	0.045	0.025**	0.038*	0.029
p-value	0.565	0.188	0.423	0.049	0.086	0.259
2017						
Estimate	0.098	0.017	0.105	-0.018	0.019	0.033
p-value	0.246	0.768	0.282	0.173	0.494	0.284
2018						
Estimate	-0.068	-0.073	-0.052	0.004	0.035	0.026
p-value	0.362	0.304	0.500	0.778	0.198	0.407
2019						
Estimate	0.004	-0.055	0.142	0.046***	0.040	0.032
p-value	0.957	0.319	0.128	0.000	0.123	0.259

Notes: See notes to Table 5A.

Alternative Table 5B: Synthetic Control Analyses, Mountain View, 2006-2019

	Teen empl.	Youth empl.	HSDO empl.	Poor (50%)	Poor (100%)	Poor (150%)
	(1)	(2)	(3)	(4)	(5)	(6)
Post-treatment (2017-2019)						
Estimate	0.090	0.102***	0.152**	-0.003	-0.014	-0.022
p-value	0.143	0.000	0.026	0.691	0.333	0.346
Donor pool (number of Census places)	69	69	77	80	80	80
RMSPE	0.094	0.088	0.074	0.006	0.011	0.014
Group population (2006-2019)	2,485	7,047	3,591	76,729	76,729	76,729
Post-Registration Years (2018-2019)						
Estimate	-0.020	0.032	0.147**	-0.007	-0.011	-0.012
p-value	0.771	0.629	0.013	0.531	0.444	0.531
2017						
Estimate	0.200**	0.172**	0.162*	-0.004	-0.020	-0.042
p-value	0.029	0.014	0.090	0.741	0.494	0.210
2018						
Estimate	-	-	0.008	0.000	-0.005	0.017
p-value	-	-	0.859	0.963	0.741	0.519
2019						
Estimate	-0.020	0.032	0.286**	-0.014	-0.018	-0.040
p-value	0.771	0.629	0.013	0.259	0.432	0.185

Notes: See notes to Table 5B.

Alternative Table 5C: Synthetic Control Analyses, Oakland, 2006-2019

	Teen empl.	Youth empl.	HSDO empl.	Poor (50%)	Poor (100%)	Poor (150%)
	(1)	(2)	(3)	(4)	(5)	(6)
Post-treatment (2016-2019)						
Estimate	0.011	0.015	0.094	0.004	-0.004	-0.023
p-value	0.797	0.667	0.128	0.519	0.753	0.296
Donor pool (number of Census places)	68	68	77	80	80	80
RMSPE	0.015	0.012	0.003	0.004	0.004	0.002
Group population (2006-2019)	17,489	42,517	42,150	398,834	398,834	398,834
Post-Registration Years (2018-2019)						
Estimate	-0.005	-0.034	0.052	0.004	-0.012	-0.031
p-value	0.942	0.464	0.474	0.667	0.469	0.235
2016						
Estimate	-0.024	0.044	0.130	0.019	0.011	-0.014
p-value	0.609	0.551	0.128	0.173	0.642	0.605
2017						
Estimate	0.079	0.084	0.142	-0.009	-0.002	-0.015
p-value	0.304	0.188	0.154	0.420	0.926	0.691
2018						
Estimate	0.009	-0.027	0.078	0.011	-0.023	-0.042
p-value	0.913	0.652	0.333	0.432	0.309	0.222
2019						
Estimate	-0.020	-0.041	0.026	-0.003	-0.001	-0.020
p-value	0.768	0.449	0.846	0.840	0.926	0.469

Notes: See notes to Table 5C.

Alternative Table 5D: Synthetic Control Analyses, Palo Alto, 2006-2019

	Teen empl.	Youth empl.	HSDO empl.	Poor (50%)	Poor (100%)	Poor (150%)
	(1)	(2)	(3)	(4)	(5)	(6)
Post-treatment (2017-2019)						
Estimate	-0.059	-0.023	-	0.004	0.003	-0.005
p-value	0.272	0.556	-	0.689	0.933	0.822
Donor pool (number of Census places)	80	80	-	89	89	89
RMSPE	0.046	0.047	-	0.007	0.005	0.014
Group population (2011-2019)	2,840	6,028	-	66,051	66,051	66,051
Post-Registration Years (2018-2019)						
Estimate	-0.051	0.009	-	-0.002	-0.007	-0.018
p-value	0.407	0.815	-	0.822	0.756	0.511
2017						
Estimate	-0.077	-0.088	-	0.015	0.021	0.023
p-value	0.395	0.210	-	0.389	0.456	0.433
2018						
Estimate	-0.070	-0.008	-	0.003	-0.000	-0.007
p-value	0.333	0.926	-	0.789	1.000	0.822
2019						
Estimate	-0.031	0.027	-	-0.007	-0.013	-0.030
p-value	0.765	0.630	-	0.589	0.567	0.356

Notes: See notes to 5D.

Alternative Table 5E: Synthetic Control Analyses, Richmond, 2006-2019

	Teen empl.	Youth empl.	HSDO empl.	Poor (50%)	Poor (100%)	Poor (150%)
	(1)	(2)	(3)	(4)	(5)	(6)
Post-treatment (2016-2019)						
Estimate	0.031	0.079**	-0.013	0.010	0.001	-0.004
p-value	0.420	0.043	0.872	0.222	0.938	0.815
Donor pool (number of Census places)	68	68	77	80	80	80
RMSPE	0.025	0.029	0.025	0.006	0.009	0.011
Group population (2006-2019)	5,243	13,147	13,204	105,437	105,437	105,437
Post-Registration Years (2018-2019)						
Estimate	0.071	0.111**	0.017	0.014	0.002	0.015
p-value	0.174	0.029	0.808	0.173	0.938	0.519
2016						
Estimate	0.029	0.051	-0.067	-0.001	-0.008	-0.042
p-value	0.551	0.478	0.385	0.889	0.691	0.284
2017						
Estimate	-0.046	0.041	-0.017	0.011	0.008	-0.006
p-value	0.507	0.522	0.795	0.395	0.753	0.901
2018						
Estimate	0.083	0.111	-0.020	0.015	-0.012	-0.042
p-value	0.275	0.188	0.705	0.358	0.519	0.222
2019						
Estimate	0.059	0.112**	0.054	0.013	0.015	0.073*
p-value	0.464	0.029	0.654	0.309	0.494	0.062

Notes: See notes to Tables 5E.

Alternative Table 5F: Synthetic Control Analyses, San Diego, 2006-2019

	Teen empl.	**Youth empl.**	**HSDO empl.**	**Poor (50%)**	**Poor (100%)**	**Poor (150%)**
	(1)	(2)	(3)	(4)	(5)	(6)
Post-treatment (2016-2019)						
Estimate	-0.016	-0.005	0.049	-0.004	-0.002	0.005
p-value	0.638	0.928	0.397	0.519	0.889	0.815
Donor pool (number of Census places)	68	68	77	80	80	80
RMSPE	0.000	0.002	0.000	0.000	0.000	0.000
Group population (2006-2019)	74,095	195,138	85,902	1,312,502	1,312,502	1,312,502
Post-Registration Years (2018-2019)						
Estimate	-0.026	-0.015	0.060	-0.009	-0.001	0.002
p-value	0.609	0.812	0.410	0.395	0.938	0.951
2016						
Estimate	-0.009	0.021	0.012	-0.004	-0.006	0.008
p-value	0.855	0.797	0.885	0.630	0.765	0.827
2017						
Estimate	-0.001	-0.010	0.064	0.004	-0.001	0.009
p-value	0.971	0.884	0.500	0.704	0.975	0.827
2018						
Estimate	-0.059	-0.042	0.021	-0.013	0.001	0.001
p-value	0.420	0.522	0.692	0.407	0.914	0.975
2019						
Estimate	0.008	0.013	0.099	-0.004	-0.004	0.002
p-value	0.855	0.841	0.346	0.741	0.827	0.963

Notes: See notes to Tables 5F.

Alternative Table 5G: Synthetic Control Analyses, Santa Clara, 2006-2019

	Teen empl.	**Youth empl.**	**HSDO empl.**	**Poor (50%)**	**Poor (100%)**	**Poor (150%)**
	(1)	(2)	(3)	(4)	(5)	(6)
Post-treatment (2017 - 2019)						
Estimate	-0.070	-0.022	-0.010	0.010	0.013	0.008
p-value	0.145	0.594	0.846	0.284	0.407	0.654
Donor pool (number of Census places)	68	68	77	80	80	80
RMSPE	0.029	0.020	0.078	0.006	0.009	0.018
Group population (2006-2019)	6,830	15,327	4,565	115,934	115,934	115,934
Post-Registration Years (2018-2019)						
Estimate	-0.084	-0.002	0.030	0.009	0.010	0.021
p-value						
2017						
Estimate	-0.042	-0.062	-0.089	0.011	0.018	-0.018
p-value	0.536	0.275	0.359	0.358	0.506	0.556
2018						
Estimate	-0.088	-0.030	0.022	0.033**	0.045*	0.078**
p-value	0.232	0.652	0.808	0.049	0.086	0.049
2019						
Estimate	-0.080	0.026	0.038	-0.015	-0.025	-0.036
p-value	0.333	0.638	0.705	0.235	0.284	0.235

Notes: See notes to Table 5G.

Alternative Table 5H: Synthetic Control Analyses, Sunnyvale, 2006-2019

	Teen empl.	Youth empl.	HSDO empl.	Poor (50%)	Poor (100%)	Poor (150%)
	(1)	(2)	(3)	(4)	(5)	(6)
Post-treatment (2016-2019)						
Estimate	0.001	0.018	0.081	-0.004	-0.013	-0.020
p-value	0.957	0.667	0.218	0.568	0.395	0.321
Donor pool (number of Census places)	68	68	77	80	80	80
RMSPE	0.009	0.012	0.039	0.006	0.004	0.007
Group population (2006-2019)	4,887	11,867	6,487	144,561	144,561	144,561
Post-Registration Years (2018-2019)						
Estimate	-0.017	-0.003	0.112	-0.012	-0.023	-0.034
p-value	0.696	0.957	0.115	0.272	0.247	0.210
2016						
Estimate	-0.012	0.060	0.079	0.002	-0.006	0.000
p-value	0.812	0.420	0.333	0.790	0.728	1.000
2017						
Estimate	0.052	0.016	0.021	0.007	-0.000	-0.011
p-value	0.435	0.812	0.782	0.556	0.975	0.815
2018						
Estimate	-0.148*	-0.098	0.097	-0.019	-0.043	-0.054
p-value	0.087	0.144	0.167	0.235	0.111	0.136
2019						
Estimate	0.113	0.092*	0.128	-0.006	-0.002	-0.014
p-value	0.116	0.072	0.282	0.667	0.914	0.593

Notes: See notes to Tables 5H.

Alternative Table 5I: Synthetic Control Analyses, LA County, 2006-2019

	Teen empl.	Youth empl.	HSDO empl.	Poor (50%)	Poor (100%)	Poor (150%)
	(1)	(2)	(3)	(4)	(5)	(6)
Post-treatment (2017 - 2019)						
Estimate	0.013	0.008	0.005	-0.002	0.001	0.004
p-value	0.768	0.812	0.923	0.815	0.877	0.790
Donor pool (number of Census places)	68	68	77	80	80	80
RMSPE	0.002	0.000	0.000	0.000	0.000	0.000
Group population (2006-2019)	565,319	1,304,623	1,161,306	9,843,498	9,843,498	9,843,498
Post-Registration Years (2018-2019)						
Estimate	0.012	0.016	0.014	-0.001	-0.001	0.005
p-value	0.783	0.841	0.821	0.889	0.988	0.864
2017						
Estimate	0.014	-0.008	-0.012	-0.004	0.005	0.004
p-value	0.855	0.899	0.872	0.790	0.889	0.840
2018						
Estimate	0.021	0.008	-0.015	-0.006	-0.007	0.000
p-value	0.855	0.899	0.795	0.630	0.728	0.975
2019						
Estimate	0.003	0.024	0.042	0.003	0.005	0.009
p-value	0.986	0.652	0.667	0.864	0.753	0.765

Notes: See notes to Table 5I.

Alternative Table 6A: Employment Estimates, Pooled California Cities, 2005-2019 – Teens (Isolating Post-Registration Variation)

Post-Treatment Years	Two	Three	Four
	(1)	(4)	(6)
Number of treated cities	13	9	4
Number of treated cities (post-reg)	9	9	4
Jump estimate × Pre-Registration	**-0.002**	**0.003**	**-0.017****
Regular SEs	**[0.010]**	**[0.008]**	**[0.008]**
Bootstrap p-values	**0.788**	**0.711**	**0.328**
Jump estimate × Post-Registration	**-0.039**	**0.009**	**-0.126****
Regular SEs	**[0.027]**	**[0.015]**	**[0.054]**
Bootstrap p-values	**0.250**	**0.607**	**0.452**
Pre-treatment trend	0.001	0.001	0.005***,†
Regular SEs	[0.001]	[0.001]	[0.001]
Bootstrap p-values	0.448	0.608	0.066
Post-treatment trend × Pre-Registration	**-0.005**	**-0.005**	**-0.009**
Regular SEs	**[0.015]**	**[0.014]**	**[0.013]**
Bootstrap p-values	**0.830**	**0.814**	**0.608**
Post-treatment trend × Post-Registration	**0.033**	**-0.011**	**0.036***
Regular SEs	**[0.024]**	**[0.011]**	**[0.019]**
Bootstrap p-values	**0.251**	**0.432**	**0.532**
Unemployment (25-64)	0.001	0.006	0.038
	[0.115]	[0.113]	[0.115]
Relative group size	0.166	0.124	0.158
	[0.151]	[0.149]	[0.158]
N	1450	1407	1348

Notes: See notes to Table 6A. “Pre-Registration” and “Post-Registration” refer to the data from periods before or after the PAP was registered; the post-registration data are for 2018 and 2019. Columns (2), (3), and (5) from Table 6A are dropped because with the distinction between pre- and post-registration years, they no longer provide as sensible a comparison to estimates for the same treatment cities as later columns for the more limited set of treatment cities (e.g., in Table 6A, column (2) for the 9 treatment cities in column (4)), because when we distinguish pre- and post-registration years we can end up with different numbers of pre- and post-registration years even when the number of post-treatment years is the same.

Alternative Table 6B: Employment Estimates, Pooled California Cities, 2005-2019 – Youths (Isolating Post-Registration Variation)

Post-Treatment Years	Two	Three	Four
	(1)	(4)	(6)
Number of treated cities	13	9	4
Jump estimate × Pre-Registration	**-0.000**	**0.004**	**-0.017***
Regular SEs	**[0.009]**	**[0.009]**	**[0.009]**
Bootstrap p-values	**0.989**	**0.711**	**0.570**
Jump estimate × Post-Registration	**-0.017**	**0.008**	**-0.119*****
Regular SEs	**[0.018]**	**[0.015]**	**[0.037]**
Bootstrap p-values	**0.571**	**0.696**	**0.251**
Pre-treatment trend	0.001	0.001	0.006***,††
Regular SEs	[0.001]	[0.001]	[0.002]
Bootstrap p-values	0.324	0.360	0.023
Post-treatment trend × Pre-Registration	**0.021**,†**	**0.021**,†**	**0.013***
Regular SEs	**[0.009]**	**[0.008]**	**[0.007]**
Bootstrap p-values	**0.074**	**0.093**	**0.275**
Post-treatment trend × Post-Registration	**0.013**	**-0.007**	**0.033****
Regular SEs	**[0.010]**	**[0.011]**	**[0.014]**
Bootstrap p-values	**0.519**	**0.762**	**0.384**
Unemployment (25-64)	-0.133	-0.145	-0.122
	[0.111]	[0.113]	[0.110]
Relative group size	0.108	0.103	0.122
	[0.096]	[0.095]	[0.103]
N	1450	1407	1348

Notes: See notes to Table 6A and Alternative Table 6A.

Alternative Table 6C: Employment Estimates, Pooled California Cities, 2005-2019 – High School Dropouts (HSDO) (Isolating Post-Registration Variation)

Post-Treatment Years	Two	Three	Four
	(1)	(4)	(6)
Number of treated cities	13	9	4
Jump estimate × Pre-Registration	**-0.006**	**-0.008**	**0.003**
Regular SEs	**[0.010]**	**[0.011]**	**[0.013]**
Bootstrap p-values	**0.563**	**0.457**	**0.868**
Jump estimate × Post-Registration	**-0.008**	**-0.043***,†††**	**0.027**
Regular SEs	**[0.031]**	**[0.011]**	**[0.056]**
Bootstrap p-values	**0.798**	**0.008**	**0.671**
Pre-treatment trend	0.001	0.001	0.001
Regular SEs	[0.001]	[0.001]	[0.002]
Bootstrap p-values	0.262	0.297	0.489
Post-treatment trend × Pre-Registration	**0.045*****	**0.045*****	**0.030***
Regular SEs	**[0.009]**	**[0.009]**	**[0.017]**
Bootstrap p-values	**0.152**	**0.143**	**0.382**
Post-treatment trend × Post-Registration	**-0.002**	**0.029***,†††**	**-0.001**
Regular SEs	**[0.027]**	**[0.006]**	**[0.026]**
Bootstrap p-values	**0.964**	**0.007**	**0.951**
Unemployment (25-64)	-0.619***	-0.619***	-0.602***
	[0.100]	[0.102]	[0.104]
Relative group size	0.091	0.096	0.092
	[0.082]	[0.082]	[0.086]
N	1500	1457	1399

Notes: See notes to Table 6A and Alternative Table 6A.

Alternative Table 7A: Poverty Estimates, Pooled California Cities – Below 50% of Poverty Line (Isolating Post-Registration Variation)

Post-Treatment Years	Two	Three	Four
	(1)	(4)	(6)
Number of treated places	13	9	4
Jump estimate × Pre-Registration	**-0.001**	**0.000**	**0.002**
Regular SEs	**[0.003]**	**[0.004]**	**[0.004]**
Bootstrap p-values	**0.865**	**0.907**	**0.658**
Jump estimate × Post-Registration	**-0.000**	**-0.003**	**0.003**
Regular SEs	**[0.004]**	**[0.004]**	**[0.007]**
Bootstrap p-values	**0.983**	**0.766**	**0.735**
Pre-treatment trend	$-0.001^{***,\dagger\dagger\dagger}$	$-0.001^{***,\dagger\dagger\dagger}$	$-0.002^{***,\dagger\dagger}$
Regular SEs	[0.000]	[0.000]	[0.000]
Bootstrap p-values	0.001	0.008	0.023
Post-treatment trend × Pre-Registration	**0.000**	**0.000**	**0.002**
Regular SEs	**[0.006]**	**[0.006]**	**[0.008]**
Bootstrap p-values	**0.913**	**0.906**	**0.646**
Post-treatment trend × Post-Registration	**0.001**	**0.003**	**0.000**
Regular SEs	**[0.003]**	**[0.002]**	**[0.003]**
Bootstrap p-values	**0.864**	**0.603**	**0.994**
Unemployment (25-64)	0.164^{***}	0.161^{***}	0.146^{***}
	[0.040]	[0.041]	[0.042]
N	1510	1467	1406

Notes: See notes to Table 6A, 7A, and Alternative Table 6A.

Alternative Table 7B: Poverty Estimates, Pooled California Cities – Below 100% of Poverty Line (Isolating Post-Registration Variation)

Post-Treatment Years	Two	Three	Four
	(1)	(4)	(6)
Number of treated places	13	9	4
Jump estimate × Pre-Registration	**-0.003**	**-0.002**	**-0.001**
Regular SEs	**[0.005]**	**[0.006]**	**[0.005]**
Bootstrap p-values	**0.711**	**0.824**	**0.843**
Jump estimate × Post-Registration	$\mathbf{0.012^{*}}$	**-0.005**	**-0.011**
Regular SEs	**[0.007]**	**[0.007]**	**[0.024]**
Bootstrap p-values	**0.145**	**0.693**	**0.676**
Pre-treatment trend	$-0.001^{***,\dagger\dagger\dagger}$	$-0.001^{***,\dagger\dagger\dagger}$	$-0.002^{***,\dagger\dagger}$
Regular SEs	[0.000]	[0.000]	[0.000]
Bootstrap p-values	0.000	0.002	0.026
Post-treatment trend × Pre-Registration	**-0.002**	**-0.002**	**0.002**
Regular SEs	**[0.007]**	**[0.007]**	**[0.009]**
Bootstrap p-values	**0.637**	**0.644**	**0.805**
Post-treatment trend × Post-Registration	$\mathbf{-0.013^{***}}$	**0.003**	**0.004**
Regular SEs	**[0.005]**	**[0.004]**	**[0.008]**
Bootstrap p-values	**0.110**	**0.646**	**0.709**
Unemployment (25-64)	0.357^{***}	0.359^{***}	0.332^{***}
	[0.056]	[0.057]	[0.057]
N	1511	1468	1407

Notes: See notes to Table 6A, 7A, and Alternative Table 6A.

Alternative Table 7C: Poverty Estimates, Pooled California Cities – Below 150% of Poverty Line (Isolating Post-Registration Variation)

Post-Treatment Years	Two	Three	Four
	(1)	(4)	(6)
Number of treated places	13	9	4
Jump estimate × Pre-Registration	**-0.006**	**-0.005**	**0.003**
Regular SEs	**[0.004]**	**[0.005]**	**[0.004]**
Bootstrap p-values	**0.282**	**0.395**	**0.550**
Jump estimate × Post-Registration	**0.000**	**-0.000**	**-0.020**
Regular SEs	**[0.009]**	**[0.009]**	**[0.030]**
Bootstrap p-values	**0.981**	**0.974**	**0.533**
Pre-treatment trend	$-0.002^{***,\dagger\dagger\dagger}$	$-0.002^{***,\dagger\dagger\dagger}$	$-0.003^{***,\dagger}$
Regular SEs	[0.000]	[0.000]	[0.001]
Bootstrap p-values	0.000	0.000	0.077
Post-treatment trend × Pre-Registration	**-0.003**	**-0.003**	**-0.009**
Regular SEs	**[0.006]**	**[0.006]**	**[0.007]**
Bootstrap p-values	**0.798**	**0.800**	**0.272**
Post-treatment trend × Post-Registration	**-0.002**	**-0.001**	**0.006**
Regular SEs	**[0.007]**	**[0.005]**	**[0.011]**
Bootstrap p-values	**0.813**	**0.851**	**0.610**
Unemployment (25-64)	0.391^{***}	0.397^{***}	0.377^{***}
	[0.064]	[0.066]	[0.068]
N	1510	1467	1406

Notes: See notes to Table 6A, 7A, and Alternative Table 6A.

Alternative Table 8: Employment Effects, Pooled California Cities, 2005-2019

	Teens	Youths	HSDO
	(1)	(2)	(3)
(MW/average wage) × Pre-registration	-0.109	-0.068	-0.050
	[0.079]	[0.085]	[0.076]
MW elasticity (Pre-registration)	-0.154	-0.048	-0.027
MW/average wage × Post-registration	-0.069	0.026	-0.053
	[0.081]	[0.084]	[0.080]
MW elasticity (Post-registration)	-0.097	0.018	-0.029
N	1259	1259	1275
R^2	0.649	0.725	0.757

Notes: See notes to Table 8 and Alternative Table 6A.

Alternative Table 9: Effects on Poverty Thresholds, Pooled California Cities, 2005-2019

	Below 50% of poverty line	Below 100% of poverty line	Below 150% of poverty line
(MW/average wage) × Pre-registration	-0.015	-0.040	-0.015
	[0.024]	[0.033]	[0.040]
MW elasticity (Pre-registration)	-0.075	-0.084	-0.019
MW/average wage × Post-registration	-0.046**	-0.154***	-0.154***
	[0.020]	[0.033]	[0.041]
MW elasticity (Post-registration)	-0.224	-0.327	-0.195
N	1280	1280	1280
R^2	0.841	0.920	0.951

Notes: See notes to Table 9 and Alternative Table 6A.

Alternative Table 10: Employment Effects of City and State Minimum Wages, 2005-2019

	State minimum wages			*City minimum wages*				*State and city minimum wages*		
	Teens	Youths	HSDO	Teens	Youths	HSDO		Teens	Youths	HSDO
	(1)	(2)	(3)	(4)	(5)	(6)		(7)	(8)	(9)
(State MW/average wage) × Pre-registration	-0.047	0.033	-0.095**				(State MW/average wage) × Pre-registration	-0.042	0.031	-0.100**
	[0.044]	[0.031]	[0.042]					[0.042]	[0.031]	[0.045]
State MW elasticity (Pre-registration)	**-0.052**	**0.021**	**-0.052**				**State MW elasticity (Pre-registration)**	**-0.047**	**0.020**	**-0.054**
State MW/average wage × Post-registration	0.002	0.113***	0.021				State MW/average wage × Post-registration	-0.001	0.113***	0.023
	[0.042]	[0.036]	[0.036]					[0.043]	[0.036]	[0.036]
State MW elasticity (Post-registration)	**0.002**	**0.071**	**0.011**				**State MW elasticity (Post-registration)**	**-0.001**	**0.071**	**0.013**
(City MW/average wage) × Pre-registration				-0.057	0.022	-0.096***	(City MW−state MW)/average wage × Pre-registration	-0.063	0.020	-0.139
				[0.042]	[0.029]	[0.036]		[0.120]	[0.120]	[0.218]
City MW elasticity (Pre-registration)				**-0.065**	**0.014**	**-0.053**	**City MW elasticity (Pre-registration)**	**-0.071**	**0.013**	**-0.076**
(City MW/average wage) × Post-registration				-0.017	0.104***	0.027	(City MW−state MW)/average wage × Post-registration	-0.127*	0.037	0.056
				[0.040]	[0.033]	[0.030]		[0.077]	[0.050]	[0.089]
City MW elasticity (Post-registration)				**-0.020**	**0.066**	**0.015**	**City MW elasticity (Post-registration)**	**-0.145**	**0.023**	**0.031**
N	6312	6312	6434	6312	6312	6434		6312	6312	6434
R^2	0.756	0.818	0.812	0.756	0.818	0.812		0.756	0.818	0.812

Notes: See notes to Tables 8, 10, and Alternative Table 6A.

Alternative Table 11: Poverty Effects of City and State Minimum Wages

	State minimum wages			*City minimum wages*				*State and city minimum wages*		
	Below 50% of PL	Below 100% of PL	Below 150% of PL	Below 50% of PL	Below 100% of PL	Below 150% of PL		Below 50% of PL	Below 100% of PL	Below 150% of PL
	(1)	(2)	(3)	(4)	(5)	(6)		(7)	(8)	(9)
(State MW/average wage) × Pre-registration	0.008	0.016	0.010				(State MW/average wage) × Pre-registration	0.008	0.019	0.017
	[0.011]	[0.017]	[0.021]					[0.012]	[0.017]	[0.020]
State MW elasticity (Pre-registration)	**0.031**	**0.028**	**0.011**				**State MW elasticity (Pre-registration)**	**0.031**	**0.033**	**0.019**
State MW/average wage × Post-registration	-0.024*	-0.048**	-0.067***				State MW/average wage × Post-registration	-0.023*	-0.049***	-0.070***
	[0.013]	[0.020]	[0.026]					[0.013]	[0.019]	[0.023]
State MW elasticity (Post-registration)	**-0.090**	**-0.083**	**-0.073**				**State MW elasticity (Post-registration)**	**-0.090**	**-0.085**	**-0.076**
(City MW/average wage) × Pre-registration				0.010	0.015	0.002	(City MW−state MW)/average wage × Pre-registration	-0.026	-0.049	-0.091*
				[0.010]	[0.014]	[0.018]		[0.024]	[0.033]	[0.047]
City MW elasticity (Pre-registration)				**0.038**	**0.026**	**0.003**	**City MW elasticity (Pre-registration)**	**-0.099**	**-0.001**	**-0.100**
(City MW/average wage) × Post-registration				-0.021*	-0.053***	-0.085***	(City MW−state MW)/average wage × Post-registration	-0.002	-0.066*	-0.156***
				[0.011]	[0.016]	[0.021]		[0.031]	[0.035]	[0.035]
City MW elasticity (Post-registration)				**-0.080**	**-0.092**	**-0.093**	**City MW elasticity (Post-registration)**	**-0.008**	**-0.001**	**-0.171**
N	6449	6451	6449	6449	6451	6449		6449	6451	6449
R^2	0.884	0.936	0.951	0.884	0.936	0.951		0.884	0.936	0.951

Note: See notes to Table 10 and Alternative Table 6A.

Online Appendix B: Effects on Wages

In this appendix, we consider evidence on the effects of minimum wages on wages in California cities. This is a common "first stage" analysis in the minimum wage literature. We do not emphasize this analysis, however, because it is actually rather complicated to do this analysis with the public ACS data. For mean earnings, the public ACS data reports estimates for year-round full-time workers aged 16 and over (which we used to construct the denominator of the relative minimum wage variable). However, these data do not allow the construction of earnings measures for demographic subgroups (like teens). Thus, we need to go to the microdata to construct wage (or earnings) measures for our demographic subgroups.

While our focus was measuring wages (or earnings), we decided to construct our other covariates used in this analysis – such as averages for education levels, race, etc. – the same way, defining all variables are defined on a consistent basis.[32] Additionally, we also change the average wage measure to use in the denominator of the minimum wage variable. Previously, the average wage was measured as earnings of aged 16 and over full-time year-round workers in the ACS summary files (divided by 2,087, our assumed hours worked). Since we are using the microdata for the analysis of wages and earnings, we now refine this measure to use earnings of 25-64 year-old, non-high school dropout, full-time year-round workers, divided by their reported hours worked and weeks worked (based on midpoints of the ranges for weeks worked). This allows us to use a wage measure that is even more exogenous to the minimum wage.

The problem is that in the microdata that we now have to use, cities are not identified, but rather observations are classified by PUMA. We therefore use a complex procedure to allocate people to cities based on PUMAs. The complicating factor is that PUMAs do not respect city boundaries, and (as in the example given below) can be larger than cities and hence have to be allocated.

We first constructed a panel dataset by PUMA and year from the ACS individual-level microdata using IPUMS (Ruggles et al., 2019). While IPUMS identifies cities for certain individuals in easily

[32] As we discuss below, this creates challenges for estimates at the city level, which is why we did not do this for the preceding analyses of employment effects and shares poor or low income.

identifiable cities, most individuals (71.32%) are not in identifiable cities, including large cities such as San Diego. While more individuals could be identified using MSAs, this was too large of a geographical area, and inapplicable to Bay Area cities (which have large differences in minimum wage policies), as most of these cities fell into the San Jose metropolitan area. Thus, we use the allocation factors provided by the Missouri Census Data Center (Rice, n.d.) to convert our dataset from PUMAs to Census places.

To convert variables on population sizes, such as the population of teens, youths, and high school dropouts, we use the PUMA-to-Census-place allocation. The purpose of the PUMA-to-Census-place allocation is to reliably convert population totals from these two geographies. For example, to convert the "Alameda County (North)—Berkeley & Albany Cities" teen population to the "Berkeley city" Census place, we take the teen population of the PUMA (14,728 in 2017) and multiply it by the allocation factor 0.859. (See Appendix Table B1.) The assumption we have to make is that the allocation for the teen population is like the allocation for the overall population. While this is a reasonable assumption for teen and youth population, it should be used with more caution for high school dropouts, who are more likely to be geographically segregated (because of income differences).

However, for converting variables that are given as averages for the PUMA, such as earnings and the shares by education level, race, etc., we instead use Census-place-to-PUMA allocations, because in this case we are trying to determine how much weight to put on each PUMA's reported average to construct an average for a Census place. For example, to get average teen earnings for Oakland, we take the weighted average of the four listed PUMAs that cover Oakland, using the allocation factors as weights. (See Appendix Table B2.) For cities with only one PUMA that is larger than the city, the city average will be the same as the PUMA. The assumption here is that the PUMA is representative of the Census place.

We restrict the Census places to be the ones identified in the ACS Summary Files (which we used in our preceding analyses of employment and the shares poor or low-income). We also only use 2012 to 2017, given that PUMA boundaries were different (corresponding to 2000 PUMA definitions) in prior years.

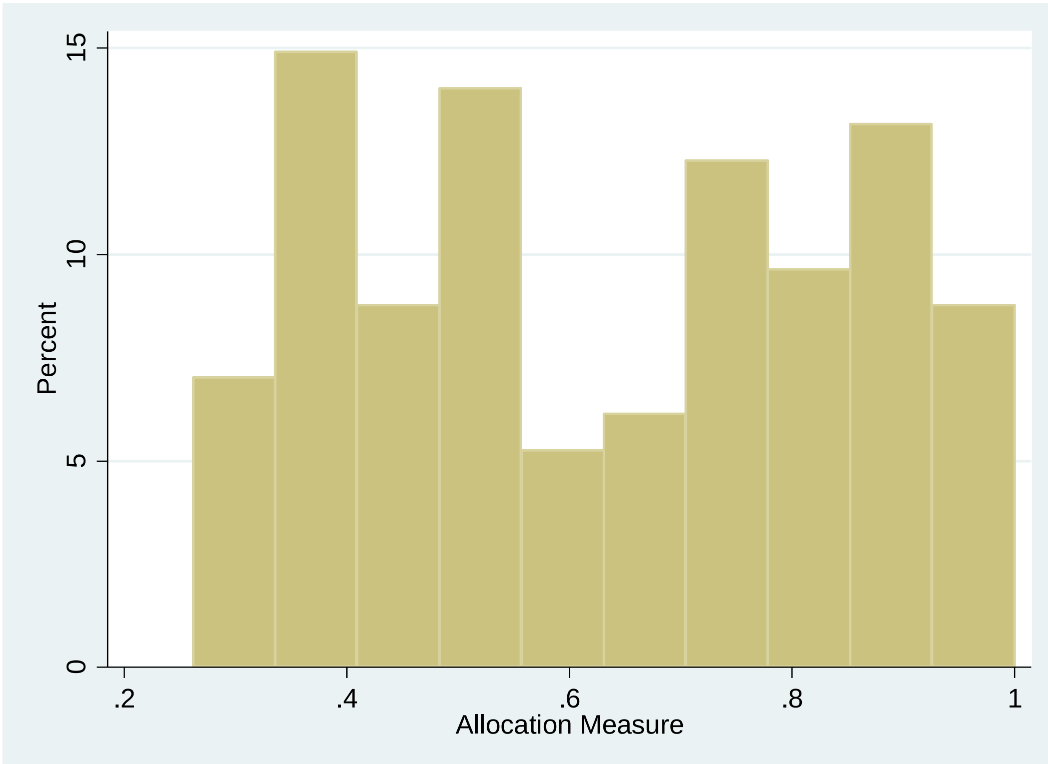
The results for wages and earnings are reported in Appendix Table B3. The estimates are of varying sign, and there is only one significant positive estimate (for earnings for high school dropouts). The case where there is the strongest prediction of positive effects is for wages – because earnings, conditional on work, can still reflect hours effects. Thus, for wages, in particular, the absence of positive effects is unexpected, and not consistent with other studies that have better wage measures (e.g., Neumark et al., 2004; Cengiz et al., 2019).

As stated above, the assumption that the PUMA is representative of the Census place is strong. It may be beneficial to restrict the analysis to a subset of Census places where the match is good. We calculate how well the fit of a Census place to a PUMA is by taking the weighted average of the PUMA-to-Census-place allocation factor, weighted based on the percent of the Census place allocated to that PUMA. An example for Oakland is in Appendix Table B4. Oakland's "allocation measure" – the weighted average of the third column, using the weights in the fourth columns – is 0.942, which places it relatively high in the scale. It makes sense that Oakland has a high measure, because most of the weight is on PUMAs almost entirely within Oakland. The distribution of allocation measures for all Census places is given in Appendix Figure B1.[33]

Based on these calculations, we estimated the models from Appendix Table B3 using only the subset of Census places with an allocation measure above 0.75. These are reported in Appendix Table B5.[34] This does not provide any stronger evidence of positive effects on wages or earnings. We conclude that the ACS data are most likely not useful for estimating the effects of minimum wages on wages or earnings, presumably for the measurement-related reasons discussed above.

[33] The allocation measures are based on 2010 Census data, and remain fixed when we updated the ACS data.
[34] To modify the analyses in Appendix Tables B3 and B5 to use only the minimum wage effects observed in data released after filing the PAP, we introduce interactions between the minimum wage effect and a dummy variable for the 2018 and 2019 data, thus allowing for different effects in the data covered by these two years. These estimates are reported in Alternative Appendix Tables B3 and B5.

Appendix Figure B1: Distribution of Allocation Measures for PUMAs and Census Places



Note: This figure shows the distribution of the constructed allocation measure for all Census places. We calculate how well the fit of a Census place to a PUMA is by taking the weighted average of the PUMA-to-Census-place allocation factor, weighted based on the percent of the Census place allocated to that PUMA.
Sources: Rice (n.d.).

Appendix Table B1: PUMA-to-Census Place Allocations, Alameda County (North) – Berkeley & Albany Cities

PUMA	Census place	Population (2010)	Allocation factor
Alameda County (North)--Berkeley & Albany Cities	Albany city, CA	18,539	0.141
Alameda County (North)--Berkeley & Albany Cities	Berkeley city, CA	112,580	0.859

Source: Rice (n.d.).

Appendix Table B2: Census Place-to-PUMA Allocations, Oakland

Census place	PUMA	Population (2010)	Allocation factor
Oakland city, CA	Alameda County (Northwest)--Oakland (Northwest) & Emeryville Cities	148,011	0.379
Oakland city, CA	Alameda County (Northeast)--Oakland (East) & Piedmont Cities	114,562	0.293
Oakland city, CA	Alameda County (North Central)--Oakland City (South Central)	124,599	0.319
Oakland city, CA	Alameda County (West)--San Leandro, Alameda & Oakland (Southwest) Cities	3,552	0.009

Source: Rice (n.d.).

Appendix Table B3: Effects on Wages and Earnings, (Imputed) Census Place, 2012-2019

	Teens	Youths	HSDO
	(1)	(2)	(3)
Wages			
Minimum wage/average wage	13.596	-5.948	-4.241
	[8.396]	[8.357]	[10.551]
MW elasticity	**0.355**	**-0.139**	**-0.085**
N	690	690	690
R^2	0.264	0.568	0.403
Earnings			
Minimum wage/average wage	-5316.786	-3204.728	21441.435***
	[4190.780]	[6814.604]	[6324.306]
MW elasticity	**-0.270**	**-0.072**	**0.265**
N	690	690	690
R^2	0.465	0.846	0.674

Notes: Constructed from ACS 1-year microdata in California converted from PUMAs to Census places using Rice (n.d.). For Los Angeles County, we simply aggregate or take a weighted average of all the PUMAs that encompass Los Angeles County, since PUMAs in LA County respect county boundaries. Earnings are measured as the average of non-zero wage income last year for the group. Hourly wages are measured as earnings divided by the usual hours worked last year and the usual weeks worked last year. Usual weeks worked are given in intervals, so we assume the median of the interval as the weeks worked. Control variables include unemployment rate of 25-64 year-olds, relative cohort size of the group (teen (16-19), youths (16-24), and high school dropouts (25-64)), shares U.S. citizens, nonwhite, black, high school graduate, some college graduate, BA or higher (with the education shares omitted for the analysis of high school dropouts), and male. The shares are of the relevant group (teens, youths, and high school dropouts). Regressions include place and year fixed effects. Standard errors are clustered by place. Regressions are weighted by the population of the group (teens, youths, or high school dropouts). The average wage is defined as the average earnings of full-time (35+ hours) year-round (50-52 weeks worked) workers, aged 25-64, who are not high school dropouts, divided by the usual hours worked and the weeks worked, which we assumed as 51. The MW/average wage measure is a one-year lag of the minimum wage divided by a two-year lag of the average wage. The elasticity is determined by taking the estimate multiplied by the ratio of the average of the MW/average wage measure to the average employment rate of the group. Note that the results for this table in the pre-analysis plan were based on a specification that inadvertently omitted the U.S. citizenship share (but the table notes correctly noted our intention to include this variable). The results were very similar excluding it, although the effect on earnings for the HSDO column was no longer significant at the 10% level. In the pre-analysis plan, we erroneously coded the average minimum wage for California in 2014 as $9 instead of $8.5, which has been fixed in this iteration. We also miscoded the average wage using high school dropouts rather than non-high school dropouts and used a one-year lag of the state minimum wage rather than the city minimum wage. Finally, the registered code inadvertently omitted the clustering, which has been added in this table. We indicate statistical significance at the 10%, 5%, and 1% level with *, **, and ***.

Appendix Table B4: Measuring PUMA Representativeness of Census Place, Example of Oakland

Census place	PUMA	Population (2010)	Allocation factor (PUMA-to-place)	Allocation factor (place-to-PUMA)
Oakland, CA	Alameda County (Northwest)--Oakland (Northwest) & Emeryville Cities	148011	0.936	0.379
Oakland, CA	Alameda County (Northeast)--Oakland (East) & Piedmont Cities	114562	0.915	0.293
Oakland, CA	Alameda County (North Central)--Oakland City (South Central)	124599	1	0.319
Oakland, CA	Alameda County (West)--San Leandro, Alameda & Oakland (Southwest) Cities	3552	0.022	0.009

Sources: Rice (n.d.).

Appendix Table B5: Effects on Wages and Earnings, (Imputed) Census Place, 2012-2019, Allocation Measure ≥ 0.75

	Teens	Youths	HSDO
	(1)	(2)	(3)
	Wages		
Minimum wage/average wage	-2.042	-9.974	-6.820
	[11.726]	[10.485]	[21.101]
MW elasticity	**-0.053**	**-0.235**	**-0.138**
N	258	258	258
R^2	0.344	0.765	0.414
	Earnings		
Minimum wage/average wage	-7713.946	-8405.238	13849.127
	[6207.967]	[11180.229]	[8522.330]
MW elasticity	**-0.391**	**-0.188**	**0.172**
N	258	258	258
R^2	0.462	0.905	0.725

Notes: See notes to Appendix Table B3.

Alternative Appendix Table B3: Effects on Wages and Earnings, (Imputed) Census Place, 2012-2019 (Isolating Post-Registration Variation)

	Teens	Youths	HSDO
	(1)	(2)	(3)
Wages			
Minimum wage/average wage × Pre-registration	14.488	-4.019	-8.060
	[10.279]	[8.562]	[12.450]
MW elasticity (Pre-registration)	**0.378**	**-0.094**	**-0.162**
Minimum wage/average wage × Post-registration	13.319	-6.720	-1.244
	[8.284]	[8.579]	[10.136]
MW elasticity (Post-registration)	**0.347**	**-0.157**	**-0.025**
N	690	690	690
R^2	0.264	0.569	0.404
Earnings			
Minimum wage/average wage × Pre-registration	-11873.056**	-3053.569	15499.369**
	[4796.408]	[7326.305]	[6469.623]
MW elasticity (Pre-registration)	**-0.603**	**-0.068**	**0.191**
Minimum wage/average wage × Post-registration	-3284.472	-3265.185	26103.320***
	[4231.051]	[6797.706]	[6440.203]
MW elasticity (Post-registration)	**-0.167**	**-0.073**	**0.322**
N	690	690	690
R^2	0.474	0.846	0.676

Notes: See notes to Appendix Table B3. “Pre-Registration” and “Post-Registration” refer to the data from periods before or after the PAP was registered; the post-registration data are for 2018 and 2019.

Alternative Appendix Table B5: Effects on Wages and Earnings, (Imputed) Census Place, 2012-2019, Allocation Measure ≥ 0.75 (Isolating Post-Registration Variation)

	Teens	Youths	HSDO
	(1)	(2)	(3)
Wages			
Minimum wage/average wage × Pre-registration	-4.155	-8.496	-9.749
	[13.534]	[10.354]	[23.454]
MW elasticity (Pre-registration)	**-0.108**	**-0.200**	**-0.197**
Minimum wage/average wage × Post-registration	-0.312	-11.351	-2.449
	[11.398]	[11.091]	[19.784]
MW elasticity (Post-registration)	**-0.008**	**-0.267**	**-0.049**
N	258	258	258
R^2	0.344	0.765	0.414
Earnings			
Minimum wage/average wage × Pre-registration	-15667.526**	-7060.951	8410.332
	[6871.330]	[11136.122]	[8722.488]
MW elasticity (Pre-registration)	**-0.793**	**-0.158**	**0.105**
Minimum wage/average wage × Post-registration	-1204.070	-9658.660	21963.591**
	[6159.174]	[11520.637]	[8986.407]
MW elasticity (Post-registration)	**-0.061**	**-0.216**	**0.273**
N	258	258	258
R^2	0.462	0.905	0.725

Notes: See notes to Appendix Table B3 and Alternative Appendix Table B3.