

**Are the Effects of Minimum Wage Increases Always Small?
New Evidence from a Case Study of New York State**

Forthcoming, *Industrial and Labor Relations Review*

Joseph J. Sabia
San Diego State University

Richard V. Burkhauser
Cornell University

Benjamin Hansen
University of Oregon

January 2012

The authors thank Kosali Simon, Jordan Matsudaira, Brad Schiller, Mick Coelli, and seminar participants at the United States Military Academy, the 2009 Society of Labor Economics meetings, and the 2009 IZA Economics of the Minimum Wage conference for useful comments on an earlier draft of this paper. We also thank Nikki Williams, Lois Brown and Tom Rushmer for excellent editing assistance. We are especially grateful for Charlie Brown's advice in completing the final revisions of this paper. This research was funded, in part, by the Employment Policies Institute. This article was begun while Burkhauser was the R. I. Downing Fellow in Social Economics in the Faculty of Economics and Commerce at the University of Melbourne.

Data from the Current Population Survey Merged Outgoing Rotation Groups used for our analysis are available at <http://www.nber.org/data/morg.html>. Stata do files for the analysis are available from Benjamin Hansen at: Department of Economics, 1285 University of Oregon, Eugene OR 97403; Email: bchansen@uoregon.edu and from Joe Sabia at: San Diego State University, Department of Economics, 5500 Campanile Drive, San Diego, CA 92182-4485; Email: Joe_Sabia@yahoo.com.

Abstract

Using data drawn from the Current Population Survey, we estimate the effect of the 2004-2006 New York State (NYS) minimum wage increase from \$5.15 to \$6.75 per hour on the employment rates of 16-to-29 year-olds without a high school diploma. Difference-in-difference estimates show that the NYS minimum wage increase is associated with a 20.2 to 21.8 percent reduction in the employment of younger less-educated individuals, with the largest effects for those ages 16-to-24. Our estimates imply a median employment elasticity with respect to the minimum wage of around -0.7, large relative to consensus estimates. These findings are robust to our choice of geographically proximate comparison states, the use of a more highly-skilled within-state comparison group, and a synthetic control design approach. Our results provide plausible evidence that state minimum wage increases can have substantial adverse labor demand effects for low-skilled individuals that are outside the consensus elasticity range of -0.1 to -0.3.

While a large body of evidence suggests that minimum wage increases cause adverse employment effects among low-skilled workers (Neumark and Wascher, 2007; 2008), most national studies have found that these effects are relatively modest (elasticities of -0.1 to -0.3), and some case studies of states have found no negative employment effects (Card, 1992; Card and Krueger, 1994). New York State's most recent experience with a large minimum wage increase provides a new and unique set of circumstances to isolate the effect of a minimum wage hike on younger, less-educated, lower-skilled individuals. In 2004, the New York State legislature voted to raise the state minimum hourly wage by nearly 39 percent, from \$5.15 to \$7.15.¹ Between 2004 and 2006, three geographically proximate states—Pennsylvania, Ohio, and New Hampshire—maintained their minimum hourly wage at \$5.15, providing a window to isolate the labor demand effects of New York's minimum wage increase on low-skilled individuals.

¹ The wage increase was implemented in three phases: from \$5.15 to \$6.00 per hour on January 1, 2005, from \$6.00 to \$6.75 on January 1, 2006, and finally, from \$6.75 to \$7.15 on January 1, 2007.

Studying New York State's large minimum wage increase has important advantages over prior state case studies, which have been scrutinized over the choice of counterfactual comparison states. In our case study of New York, we not only examine pre-treatment employment trends for both lower *and* more highly-skilled individuals in our treatment and comparison states, but also exploit the unique circumstance that New York and each of the geographically proximate states raised their minimum wages in the period just *after* the 2004-2006 New York minimum wage hike. This feature allows us to explore whether low-skilled employment trends converge in periods when treatment and comparison states all raise their minimum wages. Moreover, in addition to using geographically proximate states as a comparison group, this study is the first in the minimum wage literature to employ a synthetic control design, in which we generate a synthetic comparison state that most closely resembles the treatment state based on pre-treatment levels and trends in observable economic conditions, but not necessarily geographic proximity to New York. In doing so, we find that Ohio and Pennsylvania account for over half of the weight implied in the creation of our synthetic control group for each of our outcome measures of interest.

Our findings suggest that the effects of a state minimum wage increase on younger, less-educated, lower-skilled individuals may not always be small. Difference-in-difference estimates produce a median employment elasticity of around -0.7 for 16-to-29 year-olds without a high school diploma, larger than consensus estimates. Our employment estimates are largest for 16-to-24 year-olds and are robust to the choice of comparison states, the use of a more highly-skilled within-state comparison group, and the use of a synthetic control group.

I. Literature

The iconoclastic work of Card and Krueger (1994; 1995) caused a major reconsideration of the consequences of minimum wage increases in the economics literature and more generally popularized the use of natural experiments as a way of capturing the marginal effect of policy changes. Since 1995, a substantial number of new studies of the effect of state and Federal minimum wage laws have been undertaken using more precise data and often using natural experiment techniques. Neumark and Wascher (2007; 2008) review over 90 of these studies and conclude that the evidence is “overwhelming” that the least-skilled workers most likely to be affected by minimum wage increases experience the strongest disemployment effects. They place consensus employment elasticities in this new literature in a range from -0.1 to -0.3.

Recently, however, the debate in the literature has been stirred anew by studies questioning the credibility of the estimation strategy used in many national panel studies (see, for example, Addison, Blackburn, and Cotti, 2009; Dube, Lester, and Reich, 2010). These authors argue that the usual panel data techniques of controlling for state and year effects and identifying minimum wage effects from within-state variation in the minimum wages may be flawed due to unobserved state-specific employment trends. To better control for differences in trends that could exist across heterogeneous states, Dube, Lester, and Reich (2010) rely on variation in minimum wages in contiguous counties across state borders, which they argue should have similar employment trends. When the authors use a specification that includes county and time effects, they find a significant negative employment effect associated with the minimum wage, but after controlling for

area-specific time trends within counties, they find little evidence of adverse employment effects in the low-skilled retail and restaurant sectors.

Addison, Blackburn, and Cotti (2009) and Sabia (2009b) estimate the effect of state minimum wage increases on employment in the low-skilled retail sector, and each finds that controlling for state-specific linear time trends reduces the estimated effect of minimum wages on employment. However, while the inclusion of area-specific time trends as additional regressors will control for unmeasured time trends that could be correlated with minimum wage increases and employment, these added controls may also capture important identifying variation, substantially reducing statistical power.

To better isolate the effect of the minimum wage on affected workers, most researchers have focused on narrower, less-skilled, less-educated groups such as teenagers. But even among these individuals there are likely to be sub-groups that are differentially affected (Neumark and Wascher, 2008). While Brown (1999, pp. 2114-2115) and Neumark and Wascher (2007, pp. 61-62) provide a strategy for adjusting employment elasticities for heterogeneous treatment groups using the share of workers affected, recent studies using longitudinal data have tried to isolate the employment effects of the minimum wage by focusing on a treatment group comprised entirely of lower-skilled workers for whom the minimum wage was binding and examining employment transitions for these workers relative to unaffected lower-skilled workers (Currie and Fallick, 1996; Abowd, Kramarz, and Margolis, 2000; Zavodny 2000; Yuen, 2003; Campolieti, Fang, and Gunderson, 2005). This approach produces low-wage demand elasticities for affected workers. However, an important drawback of this approach is that it only measures one set of employment transitions:

“A limitation of the at-risk methodology is that it can assess the effects of the minimum wage increases only on the transition from employment to non-employment... To obtain a complete picture of the minimum wage effect we should also look at the effects of the minimum wage on transitions from non-employment to employment. But this is not possible because there is no wage information on non-employed persons to define an at-risk group.” (Campolieti, Fang, and Gunderson, 2005, p. 84)

Another approach taken to identify those for whom minimum wage increases are binding was taken by Thompson (2009). Using a repeated cross-section of counties drawn from Census data, Thompson (2009) finds that minimum wages increases from 1996 to 2000 had a small, statistically insignificant effect on overall teenage employment. However, when he focuses on more localized labor markets in which the minimum wage was binding—counties where the pre-treatment market-clearing wage for teenagers was below the proposed minimum wage—he finds adverse employment effects that are much larger, with estimated elasticities of -0.3 to -0.4 for all counties and -0.4 to -0.6 for small counties. These findings suggest that failing to define a treatment group for whom the minimum wage is binding may mask or understate adverse employment effects.

In contrast to the above-described large national panel studies, other papers have focused on specific case studies of minimum wages in particular states or cities, generally using a difference-in-difference identification strategy (see, for example, Card, 1992; Card and Krueger, 1994; Kim and Taylor, 1995; Dube, Lester, and Reich, 2010). Case studies have the potential advantage of more adequately approximating the conditions of a natural experiment by relying on more “similar” control states, but are less generalizable.

Card and Krueger (1994) examine the effect of the 1992 minimum wage increase in New Jersey from \$4.25 to \$5.05 per hour on fast food restaurant employment using

Pennsylvania as their control state, and find no evidence of adverse employment effects, and in fact, evidence of positive employment effects. However, the findings of this study have been criticized over both choice of research design (Hamermesh, 1995) and phone survey methodology (Welch, 1995).

Using a similar methodology, Card (1992) uses establishment data from the Bureau of Labor Statistics' unemployment insurance system to estimate the effect of the 1988 California minimum wage hike from \$3.35 to \$4.25 per hour on retail employment. Difference-in-difference estimates suggest no adverse effects of California's minimum wage increase on state retail employment growth. And a recent study of the effects of a minimum wage increase in Illinois on the fast-food industry (Powers, Persky, and Baiman, 2007) also uses a difference-in-differences approach, and finds little very limited evidence of adverse employment effects, but no evidence of positive employment effects as in Card and Krueger (1994).

One key criticism of the identification strategy employed by these authors is that their control states could have had different employment growth trends than their "treatment" state for reasons that are unrelated to the minimum wage (Deere, Murphy, and Welch, 1995; Hamermesh, 1995; Kim and Taylor, 1995; Neumark and Wascher, 1995; Welch, 1995). For instance, Kim and Taylor (1995) find some evidence in County Business Pattern (CBP) data that California's retail sales growth in the late 1980s was much stronger than in the rest of the country, raising concerns that Card's estimates were subject to omitted variable bias.² Along the same lines, Hamermesh (1995) found that

² However, Card and Krueger (1995), note that employment trends were similar in the period prior to the minimum wage hike. Kim and Taylor (1995) do find substantial retail employment effects in their analysis of California data. But Card and Krueger (1995) showed that measurement error in Kim and Taylor's wage measure led to their negative employment effects. Because of limitations in the CBP data, Kim and Taylor

beginning in 1988, employment trends in New Jersey began to diverge significantly from those in Pennsylvania, casting doubt on the findings of Card and Krueger (1994). More generally, Hamermesh (1995) cautions that in these case studies, “any changes in the relative demand shocks...[will] swamp the effect of a higher minimum wage.” (p. 837).

In summary, previous case studies of the minimum wage have tended to find small (or no) adverse employment effects and critiques have highlighted the importance of examining the sensitivity of results to unmeasured trends between treatment and control states.

Our case study contributes to the minimum wage literature in several ways. First, while previous case studies of the minimum wage have estimated industry-wide employment effects, none have focused on employment among low-skilled workers more broadly across sectors as we do. We explore the effect of a large state minimum wage increase on younger high school dropouts, a population of low-skilled workers likely to be affected by this policy. Second, our case study of New York State is unique in that we not only can explore pre-treatment trends in low-skilled employment in both treatment and comparison states prior to the minimum wage increase, but also explore a period just after the hike when all states raised their minimum wages. This will allow us to better explore whether any differential trends attributed to the minimum wage can be explained by pre-existing or subsequent employment trends. And finally, this study is the first in the minimum wage literature to use a synthetic control design to explore the sensitivity of results to the use of an alternate comparison group that is generated to most closely

calculate wages as the ratio of annual industry expenditures to total industry employment. But, as Card and Krueger note, this introduces a negative correlation between wages and employment by construction of the wage measure. When these measurement error concerns are addressed, Card and Krueger (1995) find no retail employment effects.

resemble the treatment state based on pre-treatment trends in observable economic conditions rather than geographic proximity to New York.

II. Data and Methodology

Data. Our primary analysis uses data drawn from pooled monthly cross-sections of the 2004 and 2006 Current Population Survey (CPS). We focus on a group of lower-skilled, less-educated, less-experienced workers that we expect to be affected by minimum wage policy: individuals aged 16-to-29 without a high school diploma or GED. While many studies have focused on teenagers, we expand our “treatment” group to include individuals ages 20-to-29 without a high school diploma for two reasons. First, non-teenage less-educated individuals have increasingly drawn attention from researchers as being affected by minimum wage hikes. For instance, Sabia (2008) explores the effect of minimum wage increases on less-educated single mothers in their prime-age working years and Burkhauser, Couch, and Wittenburg (2000a,b) examine less-educated individuals in their 20s. Second, as Neumark and Wascher (2007) note, older low-wage workers may be of more policy relevance:

“From a policy perspective, the effect of a minimum wage increase on teenagers is arguably of less interest than the effect on low-wage adult workers, both because teenagers are less likely than adults to be permanently low-wage workers and because many teenagers are secondary earners from non-poor families.” (Neumark and Wascher, 2007, p. 61)

While there are policy relevant reasons to include less-educated older individuals in our treatment group—as well as the gains in statistical power from drawing on a larger sample—we also explore heterogeneity of the effects of minimum wage increases by age,

given that we might expect the youngest, least experienced individuals to experience the largest minimum wage effects.

We will first show that the NY minimum wage increase was effective by tracking its impact on the share of 16-to 29 year-old workers without a high school degree earning hourly wages between \$5.15 and \$6.74 per hour and the share earning \$6.75 compared to our control states where the minimum wage remained at \$5.15 per hour over the period of our analysis. We then estimate the impact of the minimum wage on employment as defined as whether the respondent was working in the previous week.

Identification. Our first identification strategy is a difference-in-difference approach, similar to that used by Card (1992) and Card and Krueger (1994). We restrict the sample to individuals aged 16-to-29 without a high school degree in the years 2004 and 2006 and estimate:

$$E_{ist} = \alpha + \beta_1 MW_{st} + \theta_s + \tau_t + \varepsilon_{ist} \quad (1)$$

where E_{ist} is an indicator for whether respondent i residing in state s at time t was employed in the last week, MW_{st} is an indicator equal to one if the individual lives in New York in 2006 and zero otherwise, θ_s is a time-invariant state effect that captures any unmeasured differences in states that are fixed across time, and τ_t is a year effect that captures a time trend common to all states.³ The key parameter of interest in the above models is β_1 , the difference-in-difference (DD) estimator. The estimate of β_1 will only be unbiased if unmeasured employment trends are similar in the treatment and comparison states. Thus, our choice of comparison states is critical.

³ We also augment equation (1) with a vector of socio-demographic controls including age, age-squared, marital status, race, sex, number of own children under age 18 in the family, whether the respondent lives in a standard metropolitan statistical area (SMSA), month dummies, and years of schooling completed. Estimating this model via probit produces results that are qualitatively similar to those reported in the paper.

We begin by using low-skilled individuals in border or geographically proximate states to form a comparison group. In the first two columns of Appendix Table 1, we present information on average wage rates, unemployment rates, unionization rates and industrial composition in New York and the geographically proximate states during the period just prior to New York State’s minimum wage increase (2002-2004). We find that the characteristics of our selected geographically proximate comparison states (column 2) generally more closely approximate the characteristics of New York (column 1) than the national averages for the United States as whole (column 3) or all of the states which had a \$5.15 minimum over the 2004-2006 period (column 4). More specifically, the wage rates, occupation mix and industrial composition were quite similar between New York and the comparison states, while New York’s unemployment rate was slightly higher.⁴

The key concern with a difference-in-difference approach is whether the choice of comparison group serves as an appropriate counterfactual. While state fixed effects will control for fixed differences between New York and the comparison states, unmeasured trends may differ. Our first approach to explore whether unmeasured trends differ between treatment and comparison states is to examine whether minimum wage effects are observed for more highly-skilled individuals who should be largely unaffected by minimum wage increases. We select a more highly-skilled comparison group for which treatment and control individuals share common support on age, individuals ages 20-to-29 who received a high school degree or more, and estimate a difference-in-difference-in-difference (DDD) model of the following form:

$$E_{ist} = \alpha + \beta_1 A_{ist} * MW_{st} + \beta_2 A_{ist} + \beta_3 MW_{st} + \theta_s + \tau_t + \beta_4 \theta_s * A_{ist} + \beta_5 \tau_t * A_{ist} + \varepsilon_{ist} \quad (2)$$

⁴ The unionization rate for prime-age males was also higher in New York State, but New York has the highest unionization rate (21.4 percent) in the nation, compared to the national average of 12.3 percent.

where A_{ist} is an indicator variable coded equal to one if the respondent is a 16-to-29 year-old without a high school degree and equal to zero if the respondent is a member of the more highly skilled within-state comparison group. The key parameter of interest in (2), β_1 , is the difference-in-difference-in-difference estimator.⁵

As a second test of the credibility of our difference-in-difference approach, we conduct a set of falsification tests in which we examine employment trends just prior to and just after the 2004-2006 New York minimum wage increase. The absence of differential employment trends between lower-skilled workers in the treatment and comparison states during these periods would lend support to attributing any differential employment trend during the 2004-2006 period to the minimum wage increase.

Finally, rather than rely on geographically proximate comparison states, we explore the robustness of our findings to the creation of a synthetic control group. This approach utilizes factors that are likely predictors of changes in employment rates and the wages—average hourly wages for prime-age (age 25 to 54) male workers, the unemployment rate for prime-age male workers, industrial mix, occupation composition, and the unionization rate for prime-age male workers—to generate a synthetic control state. Using levels and pre-treatment trends in the above factors, weights are chosen from a set of donor states—in our case, all of the states with a \$5.15 minimum wage from 2002-2006—to construct a synthetic control group whose labor market characteristics

⁵ A second concern with using more highly-educated or experienced individuals as a control group is the possibility that these workers are affected by the minimum wage. If the minimum wage increases, the demand for higher-skilled workers may be affected if low- and high-skilled workers are gross substitutes or complements. If the substitution effect dominates the scale effect, then DDD estimates could overstate the effect of the minimum wage on low-skilled workers, because the estimate will reflect both the rising demand for high-skilled workers and the falling demand for low-skilled workers. If the scale effect dominates, the opposite is true. Thus, the DDD estimate will provide an unbiased estimate of the effect of the minimum wage to the extent that the minimum wage does not affect the demand for higher-skilled workers.

most closely resemble those of the treatment state (see Abadie, Diamond, and Hainmueller, 2010).⁶

The synthetic control group is obtained by aggregating the micro data into a panel of outcomes and labor market characteristics for both the treatment state and the potential donor states. With this panel of states, weights are optimally chosen to generate a data series whose outcomes and labor market characteristics most closely mirror those of the treatment state. Our geographic-based comparison approach, which follows much of the natural experiment literature, can be seen as a special case of the synthetic control approach. In the former case, we weight each of the geographically proximate states by their relative population size (because we weight the regressions) while all other states receive a weight of zero. In the latter, we allow observable economic conditions such as: average hourly wages for prime-age male workers, the unemployment rate for prime-age male workers, industrial composition, and occupation mix, to optimally choose weights to generate a synthetic comparison group.

In summary, the synthetic control approach offers (i) an additional comparison group with which to estimate the labor demand effects of New York State' minimum wage, and (ii) a purely data-driven method to examine the credibility of our choice of geographically proximate states as a comparison group. Should our synthetic control design yield similar estimates and generate weights which in large part support our ex-ante chosen counter-factual group, this would add additional credibility to our identification strategy.

⁶ The donor states for our analysis are all states which had a \$5.15 minimum wage in 2005, namely Alabama, Arkansas, Colorado, Georgia, Idaho, Indiana, Iowa, Kansas, Kentucky, Maryland, Michigan, Missouri, Montana, Nebraska, Nevada, New Mexico, North Carolina, North Dakota, Texas, Utah, Virginia, and West Virginia as well as Pennsylvania, Ohio, and New Hampshire.

IV. Results

All estimates below are weighted by the relevant state population and bootstrapped standard errors are corrected for clustering on the state (Bertrand, Duflo and Mullainathan, 2004).⁷

Wage Effects. In Table 1 we examine the effect of the minimum wage hike on the distribution of wages of employed 16-to-29 year-olds without a high school degree. For workers who report being paid hourly, their wage rate is directly reported from their current job. For those who are not paid hourly, wage rates are calculated as the ratio of weekly earnings to weekly hours in the past week.

Table 1 shows the wage distribution of these low-skilled workers in New York and the geographically proximate comparison states in 2004 and 2006. The first row of Panel I shows that approximately one-third (33.6 percent) of less-educated 16-to-29 year-old workers in New York earned hourly wages between \$5.15 and \$6.74 per hour in 2004

⁷ Inference in the presence of serial correlation has posed a problem in hypothesis testing for some time. While early work focused on time-series applications (see Newey and West, 1987; Andrews and Monahan, 1992; Kiefer and Volgalsang, 2005), more recent research has focused on panel datasets or repeated cross-sections, including Bertrand, Marianne, Duflo and Mullainathan (2004) and Cameron, Gelbach, and Miller (2008). Specifically, Cameron, Gelbach, and Miller (2008) consider several bootstrap approaches and find that bootstraps based on asymptotic refinements perform better or average than bootstraps which lack such higher-order properties. While the Wild bootstrap was the method of choice in the Cameron, Gelbach, and Miller (2008) study, its power quickly falls to zero in expectation as the number of available clusters shrinks. For this reason we use the bootstrap tested by Cameron, Gelbach and Miller (2008) to calculate standard errors. For each bootstrap replication b we estimate $\hat{\beta}_b$. After collecting B replications, we estimate $\hat{\sigma}_b^2 = \frac{\sum_{b=1}^B (\hat{\beta}_b - \bar{\beta})^2}{B}$ where $\bar{\beta} = \frac{\sum_{b=1}^B \hat{\beta}_b}{B}$, resampling within groups to replicate the inherent correlation present in the data. The square root of the bootstrap variance yields a standard error which can be compared to standard Gaussian critical values. This approach fares reasonably well in the Cameron, Gelbach, and Miller (2008) study, and proves to continue to have power when the number of clusters is small and other bootstrap methods lose power.

and would be directly affected by the minimum wage hike.⁸ By 2006 (row 2 of Panel I), the share of less-educated 16-to-29 year-old workers earning between \$5.15 and \$6.74 per hour declined substantially. The share who earned wages between \$5.15 and \$5.99 per hour fell from 0.127 in 2004 to 0.044 in 2006, and the share who earned between \$6.00 and \$6.49 per hour fell from 0.165 to 0.096.⁹ We also find evidence that the share of low-skilled New Yorkers earning \$6.75 per hour rose from 0.017 in 2004 to 0.068 in 2006. In contrast, there was little change in the share of less-educated workers earning low wages in comparison states between 2004 and 2006 (Panel II).

In Panel III, we show difference-in-difference (DD) estimates of the share of low-skilled workers that fell in each wage category. We find that the 2004-2006 New York minimum wage increase is associated with a 6.6 percentage-point decline in the share of low-skilled workers that earned hourly wages between \$5.15 and \$5.99 and a 6.7 percentage-point decline in the share of workers that earned hourly wages between \$6.00 and \$6.49 per hour. There was also a statistically significant 4.3 percentage-point increase in the share of low-skilled workers earning \$6.75 per hour. We find no evidence of “spillover effects,” whereby workers without high school degrees earning above the

⁸ Workers earning less than \$5.15 per hour are assumed to be employed in jobs that are not covered by the state or federal minimum wage, such as tipped employees. However, our estimated wage effects may understate the full wage effect of the change in the state minimum wage law as we do not estimate the effect of the minimum wage change on tipped workers (from \$3.30 to \$4.60 per hour). Moreover, Schiller (1994a, b) argues that the full adverse employment effects of minimum wages may be understated if the minimum wage induces previously employed workers in covered jobs to move into uncovered jobs. However, in New York, we find little evidence that the minimum wage affects the share of workers earning under \$5.15 per hour, presumably in uncovered jobs.

⁹ However, the share of workers earning between \$6.50 and \$6.74 per hour remained fairly steady between 2004 and 2006. In fact, in 2006, just over 20 percent earned wages less than \$6.75, which could suggest (i) lagged enforcement effects, (ii) a shift in employment toward the “uncovered” sector not covered by state minimum wages, or (iii) reporting error in hourly wages. For example, it may be the 6.5 percent of wage earners reporting wages between \$6.50 and \$6.74 are actually earning the minimum wage.

minimum wage (e.g. those earning hourly wages between \$6.76 and \$7.99) receive a wage boost as a result of the minimum wage hike.

In Table 2, we explore whether there were heterogeneous effects of the minimum wage on wages by age, and whether more highly-skilled workers, who should not be affected by the minimum wage, were affected. The first row of Table 2 shows that the minimum wage increased log wages of low-skilled workers by 0.095, an implied elasticity of approximately 0.305 (column 5, row 1). When we disaggregate 16-to-29 year-olds by age (rows 2-4), we find the strongest evidence for wage effects for younger individuals ages 16-to-24, but less evidence that minimum wages affected the wages of 25-to-29 year-old dropouts. This is consistent with the hypothesis that the minimum wage binds more for younger workers; for instance, 52.3 percent of New York's employed teenagers (ages 16-to-19) without a high school degree earned between \$5.15 and \$6.74 per hour compared to 19.6 percent of 20-to-24 year-old dropouts, and 9.8 percent of 25-to-29 year-old dropouts.

Finally, in row 5, we find no evidence that the minimum wage increased the wages of more highly-skilled 20-to-29 year-olds with a high school degree or more. These finding suggests that the wage effect we attribute to the minimum wage are not explained by differing wage trends across treatment and control states that exist for reasons unrelated to the minimum wage.

Employment Effects. Difference-in-difference estimates of the effect of the New York minimum wage increase on employment are shown in Table 3; these trends are also shown in Figure 1. The first two columns of row (1) show that the employment rates of low-skilled New Yorkers fell from 0.362 to 0.291, a decline of 7.1 percentage-points

(19.6 percent) from 2004 to 2006. In the comparison group (columns 3 and 4), the employment rate of comparably aged and educated individuals actually *rose* slightly. The difference-in-difference estimate suggests that the minimum wage increase from \$5.15 to \$6.75 per hour led to a 7.6 percentage-point decline in employment rates (column 5). When observable controls are added to the model, this effect declines to 7.3 percentage-points (column 6).

Using the mean employment rate of low-skilled 16-to-29 year-old New Yorkers in 2004 (0.362), this implies that the 31.1 percent minimum wage hike was associated with an 20.2 percent employment decline ($-0.073/0.362$). This represents an employment elasticity with respect to the minimum wage of -0.648 , which is large relative to consensus estimates, which tend to range from -0.1 to -0.3 (Neumark and Wascher, 2008; Brown 1999).¹⁰

In rows (2)-(4) of Table 3, we present difference-in-difference estimates of the employment effects of the minimum wage by age. Consistent with the evidence in Table 2, we find the largest employment effects for younger individuals ages 16-to-24, for whom the minimum wage was more binding. Adjusted difference-in-difference estimates suggest that estimated employment elasticities are largest for teenagers (-0.892)

¹⁰ As noted, our estimated elasticity represents an employment elasticity with respect to the minimum wage for all 16-to-29 year-old dropouts. Brown (1999; pp. 2114-2116) and Neumark and Wascher (2007; pp. 61-62) provide a method for adjusting these elasticities to obtain employment elasticities for affected individuals. Neumark and Wascher (2007; p. 61) note that to obtain a minimum wage elasticity for affected workers β^A , one can divide the overall elasticity by the share of affected individuals. In our sample, 33.6 percent of 16-to-29 year-old New York workers without a high school diploma earned wages between \$5.15 and \$6.75 in 2004. Thus, $\beta^A = -0.648/0.336 = -1.93$. Moreover, to obtain an uncompensated low-wage demand elasticity (Brown 1999, pp. 2114-2115; Neumark and Wascher 2007, p. 62), we estimate $\eta = \beta [\Delta \ln w_m / \Delta \ln w^*] / 0.336$, where $\Delta \ln w_m$ is the percent change in the minimum wage (0.311) and $\Delta \ln w^*$ is the proportional wage increase among those with hourly wages between \$5.15 and \$6.75 that would be required to raise their wages to \$6.75, assuming full coverage and full compliance. We estimate $\Delta \ln w^* = 0.154$ in our sample of 16-to-29 year-olds without a high school diploma. Thus, our estimate of $\eta = \beta [\Delta \ln w_m / \Delta \ln w^*] / 0.336 = -3.91 = 6.01\beta$. Our estimated uncompensated demand elasticity is also large relative to consensus estimates.

and decline with age (-0.844 for 20-to-24 year-old dropouts and -0.373 for 25-to-29 year old dropouts).¹¹

Could the differences in low-skilled employment trends we observe in the 2004-2006 simply capture differential employment trends in New York and the comparison states that have little to do with the minimum wage increase? The descriptive evidence in Figure 2 suggests that in contrast to Figure 1, employment trends among more highly-skilled individuals did not diverge between New York and the comparison states during the 2004-2006 period.

Difference-in-difference estimates for our more highly skilled comparison group are shown in row (5) of Table 3. The results confirm the trends observed in Figure 2, and suggest that in contrast to younger high school dropouts, employment trends for 20-to-29 year-olds with a high school degree or more were statistically similar in New York and the comparison states. These findings support the hypothesis that the minimum wage induced the divergence in employment trends during the 2004-2006 period.

In the first row of Table 4, we examine whether the difference-in-difference estimates of employment effects for low-skilled workers are significantly different from those for more highly skilled workers using a triple-difference approach. We find that the New York State minimum wage increases reduced the relative employment of lower-skilled to higher-skilled individuals relative to the lower-skilled to higher-skill employment trend in geographically proximate states. We obtain an estimated elasticity with respect to the minimum wage of -0.693 for 16-to-29 year-old high school dropouts,

¹¹ Sabia and Burkhauser (2010) show that these results hold for white 16-29 year-old dropouts, for whom pre-treatment (2004) employment levels were nearly identical (0.42 in New York compared to 0.43 in the geographically proximate states).

an elasticity that is once again large relative to consensus estimates (Neumark and Wascher, 2008).^{12,13}

The remaining rows of Table 4 show difference-in-difference-in-difference estimates by age. Again, we continue to find evidence that the largest adverse employment effects are found for individuals ages 16-to-24 (rows 2 and 3) and are much smaller for individuals ages 25-to-29 (row 4).

To further test whether our employment elasticities are larger for populations for which the minimum wage is more binding, we examine whether difference-in-difference estimates of employment effects are larger for sub-populations with a relatively greater share of workers that earned wages between \$5.15 and \$6.74 per hour. In Table 5, we define 12 sub-groups of lower-skilled and more highly-skilled individuals disaggregated by age and education and present difference-in-difference estimates of the employment effects for each sub-group. Consistent with the results above, we find the largest adverse employment effects for those individuals with larger shares of affected workers. For instance, for teenagers ages 16-to-19 without a high school diploma, we obtain an employment elasticity with respect to the minimum wage of -0.791 compared to a (statistically insignificant) elasticity of 0.071 for 30-to-34 year-olds with more than a high school degree. Following Card (1992), we regress our difference-in-difference estimates of employment effects for each sub-group on the share of New York workers in

¹² In Appendix Table 2, we estimate the effects of the first and second phases of the New York State minimum wage increase separately. DD estimates show a negative relationship between the minimum wage and employment in each period.

¹³ Sub-groups of our highly-skilled population could be directly affected by the minimum wage. As Table 5 shows, 11.6 percent of 20-24 year-old workers with a high school degree, but not a college degree, earned hourly wages between \$5.15 and \$6.74. Thus, the use of this control group may produce lower-bound estimates of the impact of the minimum wage. We experimented with other within-state control groups: 25-to-29 year-old college graduates and 30-to-54 year-olds with more than a high school education. The results are comparable to those presented above (see column 1 of Appendix Table 3).

each sub-group who earned hourly wages between \$5.15 and \$6.74 per hour in 2004. We obtain an estimated correlation of -0.212 with a standard error of 0.094 (final row), consistent with the hypothesis of greater adverse employment effects for populations with relatively larger shares of affected workers.¹⁴

Pre- and Post-Treatment Trends. In Table 6, we explore whether employment trends for low-skilled workers were similar in New York relative to the comparison states in the pre- and post-treatment periods. Row (1) of Table 6 reproduces the difference-in-difference estimates first shown in Table 3 for low-skilled workers ages 16-to-29 and the more highly-skilled comparison group during the 2004-2006 minimum wage “window.” As discussed above, the decline in low-skilled employment in New York are strongest for younger less-educated individuals ages 16-to-24 (columns 3 and 4) and do not extend to more highly-skilled individuals (column 5). In the second row, we find no evidence that low-skilled or high-skilled employment trends in New York were significantly different from their trends in the comparison states during the 2002-2004 pre-treatment period.¹⁵

In the third row of Table 6, we examine the period just after the 2004-2006 minimum wage hike (2006-2007) when each of the comparison states as well as New York raised its minimum wage. On January 1, 2007, Pennsylvania raised its minimum

¹⁴ We also experimented regressing our difference-in-difference estimates of employment effects for each group on a new variable, *WAGEGAP*, equal to the difference between each New York worker’s wage in 2004 and \$6.75 for those who earned between \$5.15 and \$6.75 per hour, and equal to 0 for unaffected individuals, following Linneman (1982), Currie and Fallick (1996), and Campolieti, Fang, and Gunderson (2005). We obtained an estimated correlation of -1.09 with a standard error of 0.324, again consistent with expectations that populations with more affected workers experience larger adverse employment effects.

¹⁵ While low-skilled employment trends were statistically equivalent in New York State and the comparison states, we were concerned about the small increase in employment in New York State during the 2003-2004 period, perhaps due to firms anticipating the effects of a minimum wage increase and hiring more low-skilled workers for short-term jobs. Thus, we also experimented with using the alternate baseline years 2002 and 2003. These results, shown in columns (2) and (3) of Appendix Table 3 produced slightly smaller estimated elasticities of around -0.466 to -0.762 when using 2003 as the baseline year, and -0.257 to -0.395 when using 2002 as the baseline year.

wage from \$5.15 per hour to \$6.15, Ohio raised its minimum wage from \$5.15 per hour to \$6.85, and New York raised its minimum wage from \$6.75 per hour to \$7.15. And on July 24, 2007, the Federal minimum wage increased from \$5.15 to \$5.85 per hour, affecting workers in New Hampshire. Given that minimum wages rose in both treatment and control states, the relative employment trend of low-skilled workers should not be declining faster in New York than in the comparison states. This is confirmed in columns (1)-(4), row (3) of Table 6. Finally, column (5) of Table 6 shows that higher-skilled employment trends also did not differ in New York versus the comparison states in any of the years.

Synthetic Control Approach. The difference-in-difference estimates presented above rely on geographically proximate states to provide a counterfactual trend for low-skilled individuals. We next explore a synthetic control design approach, where we select from donor states that had minimum wages at \$5.15 per hour between 2002 and 2006 to create a synthetic state that most closely resembles the treatment state based on labor market characteristics. This offers an objective data-driven method to select states as a counter-factual group appropriately reweighted to most closely resemble the treatment state. The observable state characteristics used to create the synthetic control state are: average hourly wages for prime-age male workers, the unemployment rate for prime-age male workers, industrial mix, occupation composition, and the unionization rate for prime-age male workers.

To create our synthetic control group, we follow Abadie, Diamond, and Hainmueller (2010) and estimate regressions of each of our outcome measures (wages and employment) on: average hourly wages for prime-age male workers, the

unemployment rate for prime-age male workers, industrial mix, occupation composition, and the unionization rate for prime-age male workers. We then used the t-statistics (rescaled to sum to one) to generate weights to place on each regressor. Appendix Table 4 shows the resultant weights generated for each independent variable. We find that the state economic characteristics that most often receive the largest weights are the prime-age male wage rate and unemployment rate. Utilizing each of these characteristics and their respective weights, a synthetic control state is chosen as a weighted average of all-states which had a \$5.15 minimum wage in the pre-treatment window.

Table 7 presents the weights estimated for each state in the pre-treatment period (2004) leading up to the minimum wage changes in 2005 and 2006 for each of the relevant outcome measures—the share of 16-to-29 year-olds earning hourly wages between \$5.15 and \$6.75 per hour, the share earning \$6.75 per hour, and the employment ratio. For the pre-treatment period (2004) using share employed as the outcome variable, only four states received a positive weight—Ohio and Pennsylvania receive 38 and 29 percent respectively, while Maryland receives 27 percent and Michigan receives 6 percent. Notably, in our synthetic control design that does not include geographical proximity to New York as a factor, two of our geographically proximate states account for two-thirds of the weight implied in the creation of the synthetic control group for each of the outcome measures of interest.

As shown in Appendix Table 1, when we compare characteristics of our synthetic state (column 5) to all other columns, we find that the synthetic comparison state is more similar to New York State on most pre-treatment (2004) levels of unemployment, wages, unionization, and many measures of industrial composition.

Figure 3 compares the employment trends of 16-to-29 year-olds without a high school diploma in New York with the geographically proximate states as well as the synthetic control state during the 2000-2007 period.¹⁶ The pre-treatment trend for the synthetic control state is remarkably similar to that observed for the geographically proximate comparison states.

In the first row of Table 8, we find that low-skilled employment trends were statistically equivalent in New York and the synthetic state in the pre-treatment (2002-2004) period. In the remaining rows of Table 8, we also find that trends in the prime-age unemployment rate, prime-age average male wage rate, the share employed in the service sector, and the share in durable manufacturing were statistically equivalent in New York State and the synthetic state.

Table 9 shows difference-in-difference estimates of the wage and employment effects of the New York State minimum wage increase using the synthetic control group as our counterfactual. This exercise produces estimates similar in magnitude to those obtained using geographically proximate states as the comparison group.¹⁷ Using the synthetic control group, we find that the increase in New York State's minimum wage is associated with a 11.0 percentage-point decrease in the share of workers ages 16-to-29 without a high school diploma earning between \$5.15 and \$6.74 per hour, a 4.2 percentage-point increase in the share earning \$6.75 per hour, and a 7.9 percentage point decrease in employment for 16-to-29 year-olds without a high school diploma (elasticity

¹⁶ Employment means in each year were chosen using the weights estimated with employment as the dependent variable over the 2004 window. Similar weights and results are obtained using a longer pre-treatment window from 2000-2004.

¹⁷ Note that the synthetic control design is designed for continuous time-series without interruption for the prediction period. As in the previous analyses, we have used only 2006 in the treatment period calculations. The estimates are similar when 2005 is included the post-treatment analysis.

= -0.701).^{18,19} Consistent with our findings in Table 4, the estimated employment elasticity is largest for younger individuals ages 16-to-19 (-1.010) and smallest for older dropouts ages 25-to-29 (-.314).

VI. Conclusion

Using a difference-in-difference approach, we find robust evidence that raising the New York minimum wage from \$5.15 to \$6.75 per hour significantly reduced employment rates of less-skilled, less-educated New Yorkers. Our estimates show that employment among all less-educated 16-to-29 year-olds fell by 20.2 to 21.8 percent, implying a median elasticity of around -0.7, large relative to consensus estimates. Our findings are robust to our choice of geographically proximate comparison states, the use of more highly-skilled within-state control group, and a synthetic control design approach. These findings provide plausible evidence that large state minimum wage increases can have substantial adverse labor demand effects for younger less-experienced, less-educated individuals that are well outside the consensus range of -0.1 to -0.3 found in the literature.

¹⁸ Abadie, Diamond, and Hainmueller (2010) suggest using placebo groups to construct confidence intervals for hypothesis testing. Although such methods could provide exact permutation tests, one difficulty is that population sizes vary across states. As New York State is one of the largest states in the U.S., other placebo states will have more noise in both population and sample estimates. The additional noise in these placebo states would suggest that our hypothesis tests in the synthetic control design method are likely conservative in nature. In order to calculate test and confidence intervals, we utilize placebo groups chosen from the other states which did not alter their minimum wage between 2004 and 2006, and introduce a placebo law change in 2005. The point estimate of these placebo effects is utilized to construct a confidence interval utilizing the 5th and 95th percentile. As noted above, the placebo estimated effects exhibit additional noise due to their smaller sample size. To address this, we tested for a difference utilizing a difference-in-difference model with the synthetic and treatment states, and compared that test-statistic for the treatment with the test-statistics for placebo states. Doing so more appropriately reflects the additional noise in the smaller states.

A limitation of our difference-in-difference approach is that we are only able to estimate contemporaneous minimum wage effects. A number of studies (Neumark and Wascher, 1994; Baker, Benjamin, and Stranger, 1999; Burkhauser, Couch, and Wittenburg, 2000a, b; Neumark, 2001; Campolieti, Gunderson, and Riddell, 2006; Sabia 2009a) have emphasized the importance of allowing lagged minimum wages to affect contemporaneous employment, because firms may not respond instantaneously to changes in minimum wage policy. In fact, Baker, Benjamin, and Stranger (1999) suggest that one reason why Card and Krueger (1994, 1995) did not find evidence of adverse employment effects from minimum wage increases is that they did not allow for lagged policy effects. Thus, our contemporaneous effects may understate the full long-run labor demand effects of New York State's minimum wage increase.

References

Abadie, Alberto, Alexis Diamond and Jens Hainmueller. 2010. "Synthetic Control Methods for Comparative Case Studies of Aggregate Interventions: Estimating the Effect of California's Tobacco Control Program." *Journal of the American Statistical Association*, Vol. 105, pp. 493-505.

Abowd, John M., Francis Kramarz, and David N. Margolis. 2000. "Minimum Wages and Youth Employment in France and the United States." NBER Chapters, in: *Youth Employment and Joblessness in Advanced Countries*, NBER Inc., pp. 427-472.

Addison, J.T., M.L. Blackburn, and C.D. Cotti. 2009. "Do Minimum Wages Raise Employment? Evidence from the US Retail-Trade Sector." *Labour Economics*, Vol. 16 (4), pp. 397-408.

Andrews, Donald W.K. and J. Christopher Monahan. 1992, "An Improved Heteroskedasticity and Autocorrelation Consistent Covariance Matrix Estimator." *Econometrica*, Vol. 60, pp. 953-966.

Baker, M., Benjamin, D., and Stranger, S. 1999. "The Highs and Lows of the Minimum Wage Effect: A Time-Series-Cross-Section Study of the Canadian Law." *Journal of Labor Economics*, Vol. 17(2), pp. 318-350.

Bertrand, Marianne, Esther Duflo and Sendhil Mullainathan. 2004. "How Much Should We Trust Difference-in-Difference Estimates." *Quarterly Journal of Economics*, Vol. 119, pp. 249-275.

Burkhauser, Richard V., Kenneth A. Couch, and David C. Wittenburg. 2000a. "A Reassessment of the New Economics of the Minimum Wage." *Journal of Labor Economics*, Vol. 18(4), pp. 653-681.

Burkhauser, Richard V., Kenneth A. Couch, and David C. Wittenburg. 2000b. "Who Minimum Wage Increases Bite: An Analysis Using Monthly Data from the SIPP and CPS." *Southern Economic Journal*, Vol. 67 (1), pp. 16-40.

Cameron, A. Colin, Jonah B. Gelbach, Douglas L. Miller. 2008. "Bootstrap-Based Improvements for Inference with Clustered Errors." *The Review of Economics and Statistics*, Vol. 90, pp. 414-427.

Campolieti, M., Gunderson, M., and Riddell, C. 2006. "Minimum Wage Impacts from a Prespecified Research Design: Canada 1981-1997." Vol. 45(2), pp. 195-216.

Campolieti, Michele, Tony Fang, and Morley Gunderson. 2005. "Minimum Wage Impacts on Youth Employment Transitions." *Canadian Journal of Economics*, Vol. 38(1), pp. 81-104.

- Card, David. 1992. "Do Minimum Wages Reduce Employment? A Case Study of California 1987-1989." *Industrial and Labor Relations Review*, Vol. 46, pp. 22-37.
- Card, David and Alan B. Krueger. 1994. "Minimum Wages and Employment: A Case Study of the Fast-Food Industry in New Jersey and Pennsylvania." *American Economic Review*, Vol. 84, pp. 772-793.
- Card, David and Alan B. Krueger. 1995. "Myth and Measurement: The New Economics of the Minimum Wage." Princeton, NJ: Princeton University Press.
- Currie, Janet and Bruce C. Fallick. 1996. "The Minimum Wage and the Employment of Youth: Evidence from the NLSY." *Journal of Human Resources*, Vol. 31(2), pp. 404-428.
- Deere, Donald, Kevin M. Murphy, and Finis Welch. 1995. "Reexamining Methods of Estimating Minimum Wage Effects: Employment and the 1990-1991 Minimum Wage Hike." *American Economic Association Papers and Proceedings*, Vol. 85, pp. 232-237.
- Dube, A., Lester, T.W., and Reich, M. 2010. "Minimum Wage Effects Across State Borders: Estimates Using Contiguous Counties." *Review of Economics and Statistics*. Vol. 92(4), pp. 945-64.
- Hamermesh, Daniel S. 1995. "Review Symposium: Myth and Measurement: The New Economics of the Minimum Wage." *Industrial and Labor Relations Review*, Vol. 48, pp. 835-838.
- Hotz, Joseph V., Charles Mullin, and John Karl Scholz. 2002. "The Earned Income Tax Credit and the Labor Market Participation of Families on Welfare," Mimeo, UCLA.
- Kiefer, Nicholas M. and Timothy J. Vogelsang. 2005. "A New Asymptotic Theory for Heteroskedasticity-Autocorrelation Robust Tests." *Econometric Theory*, Vol. 21, pp. 1130-1164.
- Kim, Taell and Lowell J. Taylor. 1995. "The Employment Effect in Retail Trade of California's Minimum Wage Increase." *Journal of Business and Economic Statistics*, Vol. 13, pp. 175-182.
- Linneman, Peter. 1982. "The Economic Impacts of Minimum Wage Laws: A New Look at an Old Question." *Journal of Political Economy*, Vol. 90, pp. 443-469.
- Neumark, David and William Wascher. 1995. "The Effect of New Jersey's Minimum Wage Increase on Fast-Food Employment: A Re-Evaluation Using Payroll Records." National Bureau of Economic Research Working Paper #5224.
- Neumark, David. 2001. "The Employment Effects of Minimum Wages: Evidence from a Prespecified Research Design," *Industrial Relations*, Vol. 40(1), pp. 121-144.

Neumark, David, Mark Schweitzer, and William Wascher. 2004. "Minimum Wage Effects throughout the Wage Distribution." *Journal of Human Resources*, Vol. 39(2), pp. 425-450.

Neumark, David, Mark Schweitzer, and William Wascher. 2005. "The Effects of Minimum Wages on the Distribution of Family Incomes: A Non-Parametric Analysis." *Journal of Human Resources*, Vol. 40(4), pp. 867-894.

Neumark, David, and William Wascher. 2007. "Minimum Wages and Employment" in *Foundations and Trends in Microeconomics*, Vol 3. No. 1-2, pp. 1-182.

Neumark, David and William Wascher. 2008. "Minimum Wages." MIT Press: Cambridge, MA.

Newey, Whitney K. and Kenneth D. West. 1987, "A Simple, Positive Semi-Definite, Heteroskedasticity and Autocorrelation Consistent Covariance Matrix." *Econometrica*, Vol. 55, pp. 703-708.

Powers, Elizabeth T., Joe Persky, and Ron Baiman. 2007. "Impacts of the Illinois Minimum Wage: Employment, Hours and Labor Substitution in the Fast Food Industry." Unpublished paper, University of Illinois at Urbana-Champaign.

Sabia, Joseph J. and Richard V. Burkhauser. 2008. "The Employment and Distributional Consequences of Minimum Wage Increases: A Case Study of New York State." Employment Policies Institute.

Sabia, Joseph J. 2008. "Minimum Wages and the Economic Wellbeing of Single Mothers." *Journal of Policy Analysis and Management*, Vol. 27(4), pp. 848-866.

Sabia, Joseph J. 2009a. "Identifying Minimum Wage Effects in State Panels: Evidence from the Current Population Survey." *Industrial Relations*, Vol. 48(2), pp. 311-328.

Sabia, Joseph J. 2009b. "The Effects of Minimum Wage Increases on Retail Employment: New Evidence from Monthly CPS Data." *Journal of Labor Research*, Vol. 30(1), pp. 75-97.

Schiller, Bradley R. 1994a. "State Minimum Wage Laws: Youth Coverage and Impact." *Journal of Labor Research*, Vol. 15(4), pp. 317-329.

Schiller, Bradley R. 1994b. "Below-Minimum Wage-Workers: Implications for Minimum-Wage Models." *The Quarterly Review of Economics and Finance*, Vol. 34(2), pp. 131-143.

Thompson, Jeffrey P. 2009. "Using Local Labor Market Data to Re-Examine the Employment Effects of the Minimum Wage," *Industrial and Labor Relations Review*, Vol. 62(3), pp. 343-366.

Welch, Finis R. 1995. "Review Symposium: Myth and Measurement: The New Economics of the Minimum Wage." *Industrial and Labor Relations Review*, Vol. 48, pp. 842-849.

Yuen, Terence. 2003. "The Effect of Minimum Wages on Youth Employment in Canada: A Panel Study." *Journal of Human Resources*, Vol. 38(3), pp. 647-672.

Zavodny, Madeline. 2000. "The Effect of the Minimum Wage on Employment and Hours." *Labour Economics*, Vol. 7(6), pp. 729-750.

Table 1. Difference-in-Difference Estimates of the Effect of the New York State Minimum Wage Hike on the Wage Distribution of Workers Aged 16-to-29 without a High School Degree

	<i>Hourly Wage Rate</i>								
	< \$5.15	\$5.15- \$5.99	\$6.00- \$6.49	\$6.50- \$6.74	\$6.75	\$6.76- \$7.25	\$7.26- \$7.99	\$8.00- \$10.00	> \$10.00
	<i>Panel I: New York</i>								
2004	0.082 (0.275)	0.127 (0.334)	0.165 (0.372)	0.044 (0.205)	0.017 (0.128)	0.139 (0.347)	0.068 (0.253)	0.220 (0.415)	0.138 (0.346)
2006	0.033 (0.179)	0.044 (0.205)	0.096 (0.296)	0.065 (0.247)	0.068 (0.252)	0.144 (0.352)	0.079 (0.270)	0.281 (0.450)	0.191 (0.394)
	<i>Panel II: Comparison States (PA, OH, NH)</i>								
2004	0.085 (0.279)	0.167 (0.373)	0.171 (0.377)	0.069 (0.253)	0.014 (0.120)	0.107 (0.309)	0.068 (0.252)	0.256 (0.412)	0.102 (0.303)
2006	0.053 (0.225)	0.150 (0.358)	0.171 (0.377)	0.068 (0.251)	0.022 (0.146)	0.124 (0.330)	0.072 (0.259)	0.213 (0.410)	0.126 (0.333)
	<i>Panel III: Difference-in-Difference Estimates</i>								
Diff-in-Diff Estimates for Each Wage Category	-0.018 (0.012) [1,898]	-0.066** (0.033) [1,898]	-0.067* (0.039) [1,898]	0.021 (0.022) [1,898]	0.043** (0.020) [1,898]	-0.012 (0.026) [1,898]	0.005 (0.011) [1,898]	0.065 (0.042) [1,898]	0.029 (0.023) [1,898]

*** Significant at the 1% level ** Significant at the 5% level * Significant at the 10% level

Notes: Estimates are obtained using data from the 2004 and 2006 Current Population Survey Outgoing Rotation Groups from respondents aged 16-to-29 without a high school degree who were employed in the last week. All estimates are weighted. For workers paid hourly, hourly wages are coded as reported; for workers not paid hourly, hourly wage rates are calculated as the ratio of weekly earnings to weekly hours. The final row shows difference-in-difference estimates; bootstrapped standard errors corrected for clustering on the state are in parentheses and sample sizes are in brackets.

Table 2. Difference-in-Difference Estimates of the Effect of the New York State Minimum Wage Hike on the Log Wages of Low-Skilled and Higher-Skilled Workers

	New York State		Comparison States (PA, OH, NH)		Diff-in-diff (5)
	2004 (1)	2006 (2)	2004 (3)	2006 (4)	
16-to-29 Year-Olds w/out HS Degree	1.99 (0.391) [332]	2.11 (0.362) [260]	1.93 (0.401) [695]	1.96 (0.423) [611]	0.095** (0.041) [1,898]
<i>Elasticity</i>					0.305
16-to-19 Year-Olds w/out HS Degree	1.84 (0.378) [178]	1.96 (0.247) [131]	1.82 (0.370) [500]	1.84 (0.341) [444]	0.104** (0.048) [1,253]
<i>Elasticity</i>					0.334
20-to-24 Year-Olds w/out HS Degree	2.06 (0.316) [86]	2.23 (0.452) [64]	2.11 (0.308) [114]	2.16 (0.360) [90]	0.128 (0.097) [354]
<i>Elasticity</i>					0.412
25-to-29 Year-Olds w/out HS Degree	2.12 (0.371) [68]	2.24 (0.343) [65]	2.25 (0.411) [81]	2.30 (0.551) [77]	-0.032 (0.048) [291]
<i>Elasticity</i>					-0.103
20-to-29 Year-Old HS Grads	2.48 (0.578) [1,352]	2.57 (0.548) [1,212]	2.37 (0.522) [2,478]	2.44 (0.514) [2,552]	0.026 (0.028) [7,594]
<i>Elasticity</i>					0.084

*** Significant at the 1% level ** Significant at the 5% level * Significant at the 10% level

Notes: Estimates obtained using data from the 2004 and 2006 Current Population Survey Outgoing Rotation Groups. All estimates are weighted. Columns (1)-(4) present means with standard deviations in parentheses and sample sizes are in brackets. Column (5) shows difference-in-difference estimates with bootstrapped standard errors corrected for clustering on the state in parentheses.

Table 3. Difference-in-Difference Estimates of the Effect of the New York State Minimum Wage Hike on the Employment of Low-Skilled and Higher-Skilled Individuals

	New York State		Comparison States		Diff-in-diff	Adjusted Diff-in-diff
	2004	2006	2004	2006		
	(1)	(2)	(3)	(4)	(5)	(6)
	<i>Mean Employment</i>		<i>Mean Employment</i>			
16-to-29 Year-Olds w/out HS Degree	0.362 (0.481) [989]	0.291 (0.454) [916]	0.409 (0.482) [1,765]	0.414 (0.483) [1,499]	-0.076*** (0.029) [5,169]	-0.073*** (0.028) [5,169]
<i>Elasticity</i>					-0.675	-0.648
16-to-19 Year-Olds w/out HS Degree	0.260 (0.439) [685]	0.196 (0.397) [659]	0.357 (0.479) [1,383]	0.356 (0.479) [1,198]	-0.064** (0.032) [3,925]	-0.072** (0.036) [3,925]
<i>Elasticity</i>					-0.791	-0.890
20-to-24 Year-Olds w/out HS Degree	0.537 (0.500) [176]	0.430 (0.497) [148]	0.524 (0.499) [224]	0.560 (0.498) [170]	-0.124 (0.077) [718]	-0.141** (0.071) [718]
<i>Elasticity</i>					-0.742	-0.844
25-to-29 Year-Olds w/out HS Degree	0.604 (0.491) [128]	0.620 (0.488) [109]	0.603 (0.491) [158]	0.671 (0.472) [131]	-0.053 (0.034) [526]	-0.070 (0.051) [526]
<i>Elasticity</i>					-0.282	-0.373
20-to-29 Year-Old HS Grads	0.694 (0.461) [2,082]	0.700 (0.452) [1,844]	0.759 (0.428) [3,422]	0.754 (0.430) [3,503]	0.010 (0.009) [10,851]	0.005 (0.005) [3,176]
<i>Elasticity</i>					0.046	0.023

*** Significant at the 1% level ** Significant at the 5% level * Significant at the 10% level

Notes: Estimates obtained using data from the 2004 and 2006 Current Population Survey Outgoing Rotation Groups. All estimates are weighted. Columns (1)-(4) present means with standard deviations in parentheses and sample sizes are in brackets. Column (5) shows difference-in-difference estimates with bootstrapped standard errors corrected for clustering on the state in parentheses.

Table 4. Difference-in-Difference-in-Difference Estimates of the Effect of the New York State Minimum Wage Hike on the Employment of Low-Skilled Individuals, by Age

(1) <i>Treatment Group: Aged 16-29</i> Without a HS Degree	-0.078* (0.043) [16,020]
<i>Elasticity</i>	-0.693
(2) <i>Treatment Group: Aged 16-to-19</i> Without a HS Degree	-0.077** (0.039) [14,776]
<i>Elasticity</i>	-0.953
(3) <i>Treatment Group: Aged 20-to-24</i> Without a HS Degree	-0.148* (0.078) [11,569]
<i>Elasticity</i>	-0.887
(4) <i>Treatment Group: Aged 25-to-29</i> Without a HS Degree	-0.071 (0.061) [11,377]
<i>Elasticity</i>	-0.378

*** Significant at the 1% level ** Significant at the 5% level * Significant at the 10% level

Notes: Estimates obtained using data from the 2004 and 2006 Current Population Survey Outgoing Rotation Groups. All estimates are weighted. Bootstrapped standard errors corrected for clustering on the state are in parentheses and sample sizes are in brackets. Adjusted difference-in-difference-in-difference models include controls for age, age-squared, marital status, race, sex, number of own children under 18 in the family, whether residing in an SMSA, education, and month dummies. The comparison states in each specification are Pennsylvania, Ohio, and New Hampshire.

Table 5. Examining the Relationship between the Magnitude of Minimum Wage Effects and the Share of Affected Workers

Sub-Group	Share New York Workers Earning \$5.15-\$6.74 in 2004	Diff-in-Diff	Employment Elasticity
<i>No High School Degree</i>			
16-to-19 year-olds	0.523	-0.064** (0.032)	-0.791
20-to-24 year-olds	0.196	-0.124 (0.077)	-0.743
25-to-29 year-olds	0.098	-0.053 (0.034)	-0.283
<i>At Least High School Degree but No Bachelors</i>			
20-to-24 year-olds	0.116	-0.025 (0.032)	-0.135
25-to-29 year-olds	0.048	-0.005 (0.034)	-0.123
<i>More than a High School Degree</i>			
30-34 year-olds	0.032	0.017 (0.022)	0.071
35-39 year-olds	0.025	0.019 (0.021)	0.080
40-44 year-olds	0.024	0.024 (0.020)	0.096
45-49 year-olds	0.026	-0.026 (0.020)	0.103
50-54 year-olds	0.024	0.006 (0.022)	0.025
<i>At Least a Bachelor's Degree</i>			
20-24 year-olds	0.051	-0.025 (0.032)	-0.135
25-29 year-olds	0.010	0.026 (0.023)	0.103
Regression of Diff-in-Diff Estimate on Share Earning \$5.15-\$6.74 in 2004		-0.212** (0.094)	

*** Significant at the 1% level ** Significant at the 5% level * Significant at the 10% level

Notes: Estimates are obtained using data from the 2004 and 2006 Current Population Survey Outgoing Rotation Groups. The comparison group for each difference-in-difference estimate is the sub-group in column (1) residing in the geographically proximate states of Pennsylvania, Ohio, and New Hampshire. All estimates are weighted. Standard errors corrected for heteroskedasticity are in parentheses.

Table 6. Difference-in-Difference Estimates of Employment Trends in the Pre- and Post-Treatment Periods

	16-to-29 year-olds w/out HS Degree	16-to-19 year-olds w/out HS Degree	20-to-24 year-olds w/out HS Degree	25-to-29 year-olds w/out HS Degree	20-to-29 year-old HS Grads
	(1)	(2)	(3)	(4)	(5)
Minimum Wage Window: 2004-2006	-0.073** (0.028) [5,169]	-0.072** (0.036) [3,925]	-0.141** (0.071) [718]	-0.070 (0.051) [526]	0.005 (0.005) [3,176]
Falsification Window I: 2002-2004	0.038 (0.027) [5,633]	0.027 (0.024) [4,222]	-0.018 (0.090) [805]	0.108 (0.082) [606]	-0.004 (0.007) [11,389]
Falsification Window II: 2006-2007	0.008 (0.009) [4,798]	-0.014 (0.013) [3,716]	-0.104 (0.086) [611]	0.131 (0.095) [471]	0.002 (0.006) [10,517]

*** Significant at the 1% level ** Significant at the 5% level * Significant at the 10% level

Notes: Estimates obtained using data from the 2002-2007 Current Population Survey Outgoing Rotation Groups. All difference-in-difference estimates are weighted, with bootstrapped standard errors corrected for clustering on the state/heteroskedasticity-corrected standard errors in parentheses. Sample sizes are in brackets. All models include controls for age, age-squared, marital status, race, sex, number of own children under 18 in the family, whether residing in an SMSA, education, and month dummies.

Table 7. Weights Implied by Synthetic Control Design Method

State	Weights for Earning \$5.15-6.74 Regression	Weights for Earning \$6.75 Regression	Weights for Employment Regression
Colorado	8.1	9.6	0.0
Maryland	16.0	14.8	27.2
Michigan	4.7	0.0	6.1
Nevada	15.1	20.1	0.0
Ohio	9.1	0.0	38.0
Pennsylvania	50.1	51.5	28.8
Virginia	0.0	4.0	0.0
Total	100	100	100

Notes: Synthetics weights calculated using age group 16-29 of high school drop outs. Other states receiving zero weight which also had a \$5.15 minimum wage include the following: Alaska, Arkansas, Georgia, Idaho, Indiana, Kansas, Kentucky, Montana, Nebraska, New Hampshire, New Mexico, North Carolina, North Dakota, Texas, Utah, West Virginia and Wyoming.

Table 8. Placebo Tests of New York State versus Synthetic Control State in Pre-Treatment (2002-2004) Period

	Pre-Treatment (2002-2004) Synthetic Control Estimates
Employment of 16-to-29 year-olds without HS diploma	0.047 (0.039)
Prime-Age Male Unemployment Rate	-0.005 (0.008)
Prime-Age Male Average Wage Rate	-0.470 (0.299)
Share Service Industry	-0.004 (0.007)
Share Durable Manufacturing	-0.004 (0.006)

*** Significant at the 1% level ** Significant at the 5% level * Significant at the 10% level

Notes: Difference-in-difference estimate are calculated using 2002 as the pre-period and 2004 as the post-treatment period. For the synthetic control time series and New York, the CPS data are aggregated into a quarterly time series. The weights used to generate the synthetic series were those generated utilizing employment as an outcome in 2004.

Table 9. Synthetic Control Design Difference-in-Difference Estimates of the Effect of the New York State Minimum Wage Hike on Wages and Employment

Variable	Dependent Variable: Earns \$5.15-6.74	Dependent Variable: Earns \$6.75	Dependent Variable: Employed
(1) Treatment Group: Aged 16-29 Without a HS Degree	-0.110, -2.42 (-0.16, 0.22) {-2.90,1.91}	0.042*, 2.44* (-0.02, 0.02) {-1.06,0.77}	-0.079*, -2.57* (-0.05, 0.07) {-1.50,1.50}
(2) Treatment Group: Aged 16-19 Without a HS Degree	-0.194*, -3.07* (-0.18, 0.16) {-1.98, 1.54}	0.069*, 3.5* (-0.04, 0.05) {-1.24, 1.10}	-0.081*, -1.90* (-0.06, 0.04) {-1.44, 1.09}
(3) Treatment Group: Aged 20-24 Without a HS Degree	-0.067, -0.79 (-0.27, 0.29) {-2.22, 2.66}	-0.027, -0.79 (-0.06, 0.04) {-1.27, 1.67}	-0.082, -1.14 (-0.23, 0.24) {-1.80, 3.21 }
(4) Treatment Group: Aged 25-29 Without a HS Degree	0.042, 0.72 (-0.33, 0.24) {-3.25, 2.13}	0.051, 1.60 (-0.07, 0.07) {-1.19, 1.10}	-0.059, -0.76 (-0.14, 0.15) {-2.0, 1.12}
Point Estimate, Test Statistic (Placebo Confidence Interval) {Placebo Test Critical Values}			

*** Significant at the 1% level ** Significant at the 5% level * Significant at the 10% level

Notes: Difference-in-difference estimate are calculated using 2004 as the pre-period and 2006 as the post-treatment period. For the synthetic control time series, the CPS data are aggregated into a quarterly time series, with 2004 establishing the synthetic control group weights. Placebo confidence interval and test statistics are simulated using all states which also had a \$5.15 minimum wage between 2004-2006 with a placebo law change introduced at the beginning of 2005.

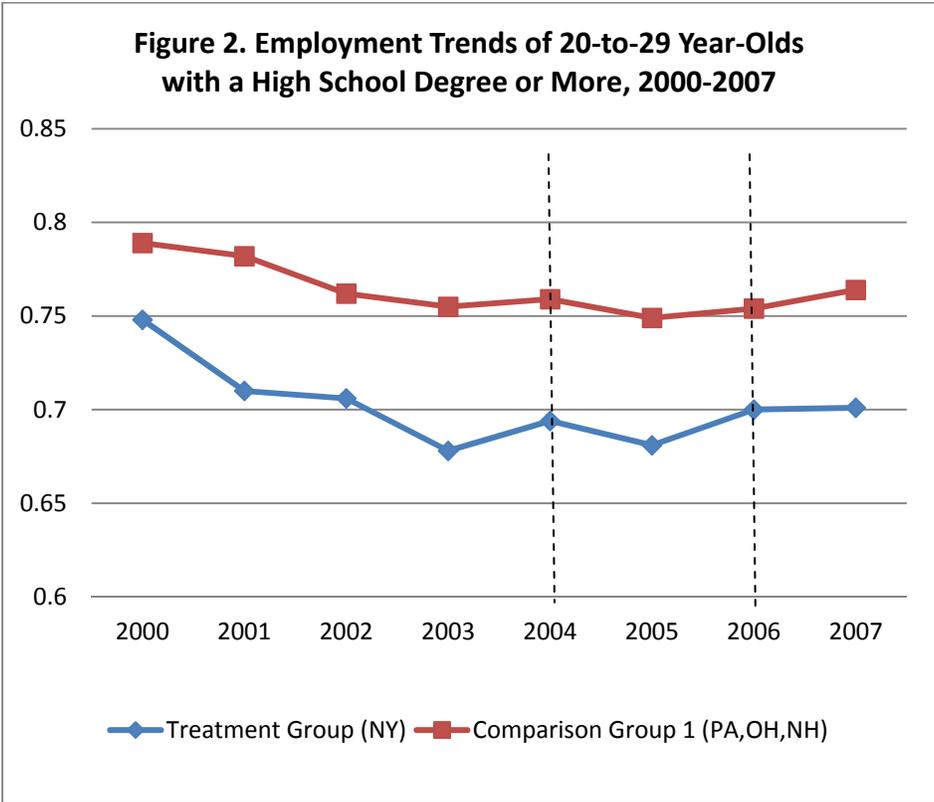
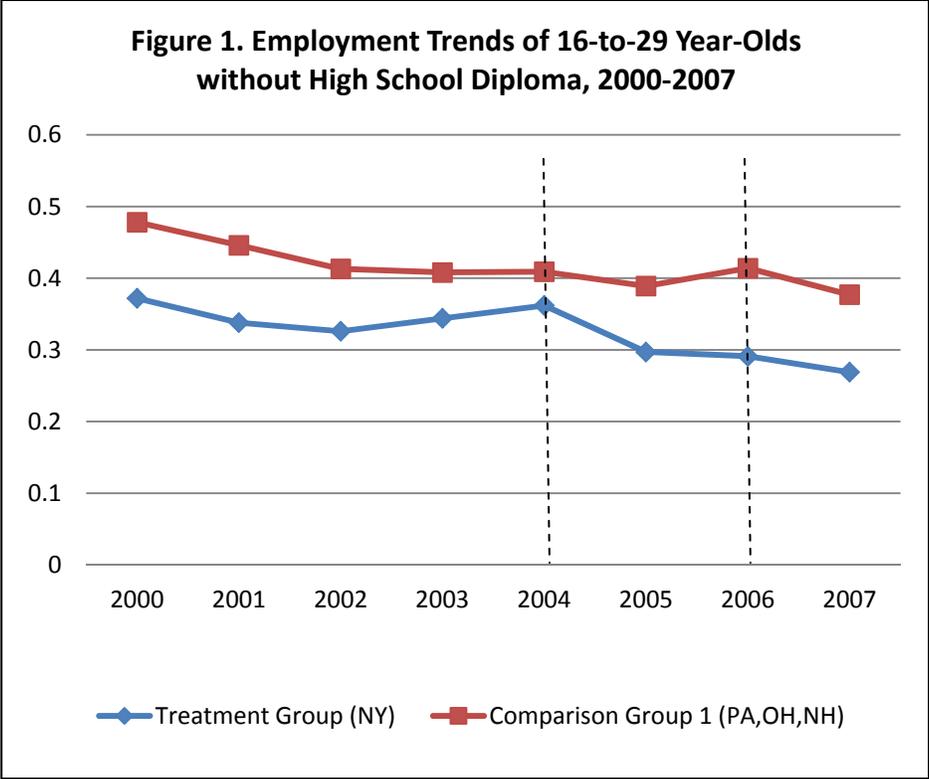
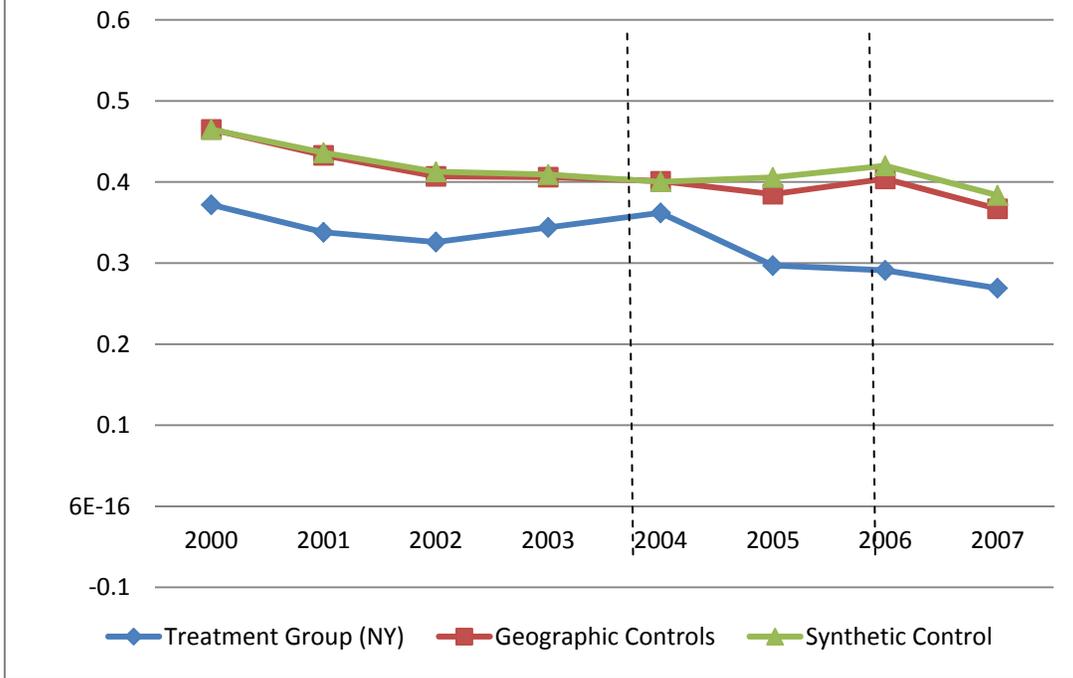


Figure 3. Employment Trends of 16-to-29 Year-Olds without High School Diploma, 2000-2007



Appendix Table 1. Summary Characteristics of NY and Counterfactual Groups in 2004					
	<i>New York</i>	<i>PA, OH, NH</i>	<i>All of US (outside NY)</i>	<i>States with \$5.15 MW</i>	<i>Synthetic Control Group</i>
Wages	15.9	15.5	15.3	15.0	16.0
Unemployment Rate	4.8	5.1	4.4	4.4	4.6
Agriculture, Fishing, etc.	0.6	1.3	1.7	1.8	1.0
Mining	0.1	0.3	0.4	0.5	0.2
Construction	6.5	6.8	7.9	8.1	7.1
Durable Manufacturing	3.5	5.3	4.4	4.8	4.5
Non-Durable Manufacturing	4.7	9.5	7.5	8.0	8.3
Wholesale Trade	2.6	3.1	3.2	3.1	2.9
Retail Trade	11.3	11.6	11.9	12.1	11.6
Transportation	4.7	4.1	4.0	4.0	3.9
Utilities	0.6	0.7	0.8	0.8	0.7
FIRE	11.3	8.3	9.3	8.7	8.5
Services, Professional and other	49.2	44.9	44.2	43.5	45.7
Public Administration	4.9	3.7	4.4	4.4	5.4
Management	12.9	13.1	14.0	13.6	13.9
Professional	21.8	19.9	19.5	19.4	20.8
Service	19.5	17.0	16.8	16.6	17.3
Sales and Office	25.6	25.3	25.7	25.2	25.1
Construction and Maintenance	8.4	9.5	10.7	11.0	9.3
Production	5.6	8.2	6.9	7.6	7.2
Transportation	5.6	7.0	6.3	6.6	6.3
Unionization Rate	21.4	15.3	10.6	9.6	13.8
Share of Population that are 16-to-29 Year-Olds w/out HS Diploma	8.0	8.3	8.7	9.1	7.9
Share of Labor Force that are 16-to-29 Year-Olds without HS Diploma	5.0	5.7	5.6	6.0	5.3
Labor Force Participation Rate of 16-to-29 Year-Olds without HS Diploma	45.1	52.3	47.9	50.3	51.8
Unemployment Rate of 16-to-29 Year-Olds without a HS Diploma	16.9	18.7	17.1	17.1	18.3

Notes: Estimates are obtained using data from the 2004 Current Population Survey Outgoing Rotation Group. This table contains characteristics of New York State, the geographically proximate comparison states (Pennsylvania, Ohio, and New Hampshire), all states other than New York State, states other than New York State with a \$5.15 minimum wage in 2004, and the synthetic control group.

Appendix Table 2. Difference-in-Difference Estimates of First (2005) and Second (2006) Phases of New York State Minimum Wage Hike on Less-Educated 16-to-29 Year-Olds

	First Phase from \$5.15 in 2004 to \$6.00 in 2005	Second Phase from \$6.00 in 2005 to \$6.75 in 2006 ¹
	(1)	(2)
Effect of Minimum Wage Increase on Employment of 16-to-29 Year-Olds without HS Degree	-0.045* (0.027) [5,345]	-0.031** (0.015) [5,006]

*** Significant at the 1% level ** Significant at the 5% level * Significant at the 10% level

Notes: Estimates in columns (1) and (2) are obtained using data from the 2004 and 2005 Current Population Survey Outgoing Rotation Groups. Estimates in columns (3) and (4) are obtained using data from the 2005 and 2006 Current Population Survey. All estimates are weighted. Bootstrapped standard errors corrected for clustering on the state are in parentheses and sample sizes are in brackets. All models use PA, NH, and OH as control states.

¹Note that in 2005, the NYS minimum wage was \$6.00 per hour, while in the control states it was \$5.15.

Appendix Table 3. Robustness of DDD Estimates to Choice of Baseline Year

	Baseline Year = 2004 (1)	Baseline Year = 2003 (3)	Baseline Year = 2002 (5)
<i>Comparison Group I: 25-to-29 year-old college grads</i>	-0.097** (0.047) [7,226]	-0.090* (0.052) [7,375]	-0.041 (0.048) [7,398]
<i>Elasticity</i>	-0.863	-0.762	-0.352
<i>Comparison Group I: 20-to-29 year-olds with ≥ HS Degree</i>	-0.078* (0.046) [16,020]	-0.055* (0.030) [16,932]	-0.030 (0.025) [16,526]
<i>Elasticity</i>	-0.693	-0.466	-0.257
<i>Comparison Group I: 30-to-54 year-olds with > HS Educ</i>	-0.080* (0.042) [27,030]	-0.059* (0.032) [27,796]	-0.046 (0.029) [28,251]
<i>Elasticity</i>	-0.711	-0.500	-0.395

*** Significant at the 1% level ** Significant at the 5% level * Significant at the 10% level

Notes: Estimates obtained using data from the 2004 and 2006 Current Population Survey Outgoing Rotation Rotation Groups. All estimates are weighted. Bootstrapped standard errors corrected for clustering on the state are in parentheses and sample sizes are in brackets. Adjusted difference-in-difference-in-difference models include controls for age, age-squared, marital status, race, sex, number of own children under 18 in the family, whether residing in an SMSA, education, and month dummies.

Appendix Table 4. Weights on Labor Market Characteristics used in the Construction of our Synthetic Control State for various Outcomes			
	Dependent Variable: Earns \$5.15-6.74	Dependent Variable: Earns \$6.75	Dependent Variable: Employed
Wages	6.9	16.4	8.9
Unemployment	5.9	1.5	25.1
Agriculture, Fishing, etc.	2.2	4.7	6.9
Mining	4.3	0.8	1.7
Construction	4.5	0.8	0.3
Durable Manufacturing	4.2	3.1	2.4
Non-Durable Manufacturing	3.8	3.9	2.4
Wholesale Trade	4.6	6.3	2.7
Retail Trade	3.8	6.2	1.7
Transportation	4.6	6.3	1.7
Utilities	0.4	4.7	1.7
FIRE	5.3	2.3	5.2
Services, Professional and other	4.4	3.9	3.1
Public Administration	3.3	5.5	2.4
Management	4.3	4.7	0.4
Professional	5.0	4.7	12.3
Service	4.6	3.7	0.7
Sales and Office	5.4	4.7	3.8
Construction and Maintenance	5.1	3.9	7.9
Production	4.6	3.8	1.0
Transportation	4.6	6.3	3.1
Unionization Rate	4.2	1.6	6.2
Total	100	100	100

Notes: Estimated weights are obtained using the 2004-2006 Current Population Survey Merged Outgoing Rotation Groups.